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# PREDICTING CRIME THROUGH INCARCERATION: THE IMPACT OF RATES OF PRISON CYCLING ON RATES OF CRIME IN COMMUNITIES

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# ABSTRACT

The purpose of this project has been to estimate the impact of "prison cycling"—the flow into and out of prison--on crime rates in communities, with special concern about areas that have high rates of prison cycling. In this work, we explicitly hypothesized that: (1) there would be a positive impact of neighborhood reentry rates on neighborhood crime rates, controlling for neighborhood characteristics; (2) there would be a positive effect of neighborhood removal rates (admissions) on neighborhood crime rates, controlling for neighborhood characteristics; (3) the effect of the rate of both removal and reentry on the neighborhood crime rate would depend upon the level of removal and reentry (tipping point); and (4) the effect of the rate of both removal and reentry on crime the neighborhood crime rate would depend upon the level of concentrated disadvantage in the neighborhood (interaction effect).

To complete the proposed work, we compiled datasets on prison admissions and releases that would be comparable across places and geocoded and mapped those data onto crime rates across those same places. The data used were panel data. The data were quarterly or annual data, depending on the location, from a mix of urban (Boston, Newark and Trenton) and rural communities in New Jersey covering various years between 2000 and 2012. Census tract characteristics come from the 2000 Census Summary File 3.

The crime, release, and admission data were individual level data that were then aggregated from the individual incident level to the census tract level by quarter (in Boston and Newark) or year (in Trenton). The analyses centered on the effects of rates of prison removals and returns on rates of crime in communities (defined as census tracts) in the cities of Boston, Massachusetts, Newark, New Jersey, and Trenton, New Jersey, and across rural municipalities in New Jersey. Our analytic strategy, was one of analytic triangulation. Through the data collection associated with this project, we amassed a uniquely comprehensive crime and incarceration dataset over time – arguably one of the most comprehensive assembled to date. This dataset allowed us to model the relationship between crime and incarceration using a range of techniques (fixed effects panel models, Arrellano-Bond estimations, and vector auto-regression) taking advantage of each and being partially freed of the limitations of any one.

We gave considerable attention to the problem of modeling. As might be expected, different models often provide different results. The most parsimonious models provide small standard errors with significant results, but there are sometimes sign changes when new control variables are added, suggesting instability in the modeling strategy. By contrast, the most stable results are provided by fixed effects models that, while intuitively attractive, have the disadvantage of large standard errors. When we use this analytic approach, we achieve results that, we believe, are more reliable.

Overall, our work finds strong support for the impact of prison cycling on crime. It seems that such cycling has different effects in different kinds of neighborhoods, consistent with the idea of a "tipping point" but more clearly expressed as an interaction between crime policy and type of neighborhood. The results in Tallahassee, Boston, and Trenton provide consistent support for this idea. In Newark, as a result of the city's limited variability in neighborhood disadvantage, we failed to find the same pattern. Further research will investigate whether this neighborhood interaction holds in other sites. It will also enable us to think about how neighborhood change over time affects the prison cycling-crime relationship. Do neighborhoods that improve start to benefit from incarceration policy? In contrast, does current incarceration policy become a factor that inhibits neighborhood improvement?

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## Executive Summary

While America's prison population has declined slightly over the last couple of years, the number of prisoners still remains very high. In 2011, some 1,598,780 individuals were incarcerated in state and federal prisons (representing a decrease of nearly 1% from 2010) (Carson & Sabol, 2012). Although relatively small, these recent decreases represent a noteworthy change from the more than quadrupling of prison populations between 1980 and 2010 (Clear and Frost, 2014). Nevertheless, incarceration rates remain particularly high for minorities – especially black males – residing in disadvantaged urban neighborhoods (Clear, 2007a; Western, 2006). In the poorest communities, the level of concentration is substantial; as many as one-fifth of adult men are incarcerated on any given day (Lynch & Sabol, 2004). Since most inmates stay in prison for only a few years at a time, the removal and release of imprisoned men has become a prevalent feature of life in these impoverished places.

Over the last several decades, an increasing number of scholars have examined whether high incarceration rates might directly or indirectly lead to increased crime in communities (e.g. Rosenfeld, Wallman, & Fornango, 2005; Veiraitis, Kovandzic, & Marvell, 2007; Hipp, Petersilia, & Turner, 2010). In 1998, Rose and Clear first proposed what has since been referred to as the coercive mobility thesis. Clear had previously argued that incarceration, when conceived of as a crime control policy, might backfire and actually increase crime. He identified at least three reasons why we might expect a backfire effect: (1) recruitment of increasing numbers of young people to replace those incarcerated offenders; (2) the diminishing deterrent effect of incarceration as more and more people experience prison, and (3) the effects that removing people from communities might have on social factors (broken families, increasing inequality, and social disorder) related to crime in those communities. This was an early exposition of the thesis that Rose and Clear would then develop focusing primarily on Clear's third effect of incarceration – its impact on the fabric of communities. Rose and Clear (1998) suggested that high incarceration rates could be viewed as a form of "coercive mobility" that damages local network structures and undermines informal social control. The central claim of the coercive mobility thesis was that high rates of prison cycling (simultaneous removals from and returns to prison) could potentially increase crime in disadvantaged communities because those communities would be unable to absorb (or counter) the effects of cycling. With an already weakened neighborhood capacity for informal social control, these prison removals and returns likely further damaged the already fragile fabric of these communities leading to increases in rates of crime.

The purpose of this project has been to estimate the impact of "prison cycling"—the flow into and out of prison--on crime rates in communities, with special concern about areas that have high rates of prison cycling. It is well documented that the increase in incarceration nationally over the last 40 years was a factor in the decade-long crime drop seen at the national level, though the size of that impact is much debated among social scientists. It is equally well-known that the rate of incarceration of an area's residents also has a range of impacts on the crime rates of these "prison cycling" areas. Studies have generally shown that:

- As the number of people returning to a neighborhood from prison increases, the crime rate in that area tends to increase; and
- As the number of people removed from a neighborhood and sent to prison increases in an area, problems such as poverty, broken families, and juvenile delinquency tend to increase.

While prison cycling thus seems to potentially impact local areas in ways that are at cross purposes to national crime trends, the precise measure of that impact—how the flow in and out of prison contributes to changes in local crime rates—is not known. Moreover, while a growing body of work now investigates the impact of prison cycling on impoverished inner city areas, there has been no comparable effort to investigate these effects in rural areas. This project sought to fill these knowledge gaps by pursuing three objectives:

1. Partnering with local and state police and parole agencies to gather and compile an extended time series of crime and incarceration data, one decade in duration, mapped to the census block level for Newark, New Jersey, Trenton, New Jersey, and Boston, Massachusetts.
2. Through partnerships with local and state police and parole and corrections agencies, we gathered an extended time series of crime and incarceration data, one decade in duration, mapped to the census block level for rural areas of New Jersey.

3. Using panel analysis with Vector Autoregressive (VAR) modeling, we modeled the impact of rates of prison cycling, both removal and reentry, on changes in rates of crime rates in those local (neighborhood) areas, controlling for a range of community factors.

In the years since Rose and Clear first articulated the coercive mobility thesis, there have been a number of attempts to test the thesis both directly and indirectly (for a more comprehensive review of this work, see (Clear, 2008; Frost & Clear, 2012a; Frost & Gross, 2012). Those tests have produced often conflicting results, with some finding support for the thesis, others reporting partial support, and still more failing to find support. There have been only a few direct tests of the coercive mobility thesis, which focuses on the simultaneous *removal from and return to* communities. More recently referred to as prison cycling, this pattern of removal and returns more fully captures the type of coercive residential mobility Rose and Clear hypothesized would be disruptive to the fabric of communities and to patterns of crime in those communities.

Clear, Rose, and colleagues (Clear, Rose, Waring, & Scully, 2003) offered the first test of their thesis when they examined the relationship between imprisonment and crime in communities in Tallahassee, Florida. Using neighborhood-level rates of incarceration across Tallahassee in 1995 and rates of crime in 1996, Clear et al. controlled for a variety of neighborhood-level characteristics and modeled the effect of incarceration rates in one year on crime in the following year. Their test offered preliminary support for the coercive mobility thesis. Rates of incarceration increased rates of crime, and this was true on both sides of the equation (e.g. in terms of prison admissions and releases). The effect of prisoner releases was, as expected, positive and linear – as people were released from prison to neighborhoods, crime in those neighborhoods went up. The effect of admissions, however, was more complicated. When estimated using a quadratic for neighborhood incarceration rates, they found a curvilinear relationship between prison admissions and crime rates. When prison admission rates in a community were low, each additional admission to prison had a fairly small by negative effect of crime in those communities. In other words, a small number of admissions to prison from a community caused crime to go down in those communities, but as the number of admissions to prison increased and eventually hit a tipping point, the sign flipped and additional admissions to prison actually caused crime to go up. They concluded that in those neighborhoods disproportionately affected by incarceration (those that experience the highest levels of prison cycling), both prison admissions and releases were damaging to the community. Moreover, without exception, these communities hardest hit by incarceration were those that were characterized by fairly significant levels of concentrated disadvantage to begin with. In other words, in high incarceration neighborhoods, prison cycling increased crime making already fragile neighborhoods worse.

Clear et al.'s finding has since been partially replicated in a number of additional studies that have assessed the impact of incarceration in one year on crime in the next. Several of these were unpublished studies (Bhati, Lynch, & Sabol, 2005; George, LaLonde, & Schuble, 2005; Powell, Peterson, Krivo, Bellair, & Johnson, 2004), providing partial support across diverse cities (Columbus, Baltimore, Cleveland, and Chicago) and among different populations of offenders removed and returned from prisons (women, drug offenders, etc.).

Studies that have limited their analyses to the effects of incarceration in one year on crime in the next suffer from several methodological problems, including the inability to account for simultaneity bias. In an attempt to overcome some of the methodological limitations of early tests of the coercive mobility thesis, two other studies have examined the impact of rates of incarceration on rates of crime over a more extended period of time, and both of those have used extended time series of the original Tallahassee data. First Waring, Scully and Clear expanded the original Tallahassee dataset to include data from 1994 through 2002, ran models similar to those used in Clear et al.'s earlier test of the thesis, and found essentially the same results. Just as Clear et al. had found, Waring and colleagues documented linear and curvilinear relationships between rates of incarceration and rates of crime in Tallahassee communities (Waring, Clear, & Scully, 2005). Prison releases increased crime in those communities and prison admissions, at low levels of prison cycling, decreased crime but, at higher levels, increased crime.

More recently, economist Geert Dhondt estimated the effect of adding prisoners on crime per capita and the effect of prison cycling on crime per capita using a panel of neighborhoods in Tallahassee, Florida for the period 1995 to 2002. Dhondt also finds evidence to support the contention that the high levels of prison cycling are associated with increasing crime rates in marginalized neighborhoods, while this effect is not found in other neighborhoods. Looking more closely at the issues of race and class, Dhondt finds that while marginalized neighborhoods experience slightly higher crime rates, they are faced with much higher incarceration rates. In Black neighborhoods in particular, prison admissions are an order of magnitude higher in comparison with non-Black neighborhoods even though underlying crime rates are not very different.

These studies provide growing evidence that high rates of incarceration heavily concentrated in the most disadvantaged communities might actually make exacerbate the problem of crime in those communities. In keeping with findings from former work, in this project we explicitly hypothesized that:

1. There would be a positive impact of neighborhood reentry rates on neighborhood crime rates, controlling for neighborhood characteristics.
2. There would be a positive effect of neighborhood removal rates (admissions) on neighborhood crime rates, controlling for neighborhood characteristics.
3. The effect of the rate of both removal and reentry on the neighborhood crime rate would depend upon the level of removal and reentry (tipping point).
4. The effect of the rate of both removal and reentry on crime the neighborhood crime rate would depend upon the level of concentrated disadvantage in the neighborhood (interaction effect).

It is important to note that the coercive mobility model assumes that between the removal from and/or return to communities, some mediating changes occur. Those mediating changes include things like increasing inequality, more broken families, decreases in levels of informal social control, and increasing social disorder. While our models have no direct measures of those mediating changes, they include a time lag to allow some of those changes to occur. A full test of the model would require measures of those mediating changes, but gathering those was beyond the scope of the proposed work.

To complete the proposed work, we needed to compile datasets on prison admissions and releases that would be comparable across places and map those data onto crime rates across those same places. The original electronic data files for this project were sourced from nine different state and local criminal justice agencies, four in Massachusetts (MA) and five in New Jersey (NJ), including: Boston Police Department (MA), Suffolk County House of Correction (MA), Massachusetts Department of Correction (MA), Massachusetts Parole Board (MA), Newark Police Department (NJ), Trenton Police Department (NJ), New Jersey Department of Corrections (NJ), New Jersey State Police (NJ), and New Jersey State Parole Board (NJ).

Each agency provided individual level data on offenders (incarceration data) or offenses (crime data), in most cases, from their data information systems. State agencies provided state-wide data and local agencies provided local data. These original individual level data files were then cleaned and relevant street address elements were geocoded. Once all individual level data had been geocoded the various admissions, release, and crime incident files were then merged with census data (from the 2000 decennial census – Summary File 3) and aggregated to the block, block group, and census tract level.

The data assembled for and used in this project are panel data. The data are quarterly or annual data, depending on the location, from a mix of urban (Boston, Newark and Trenton) and rural communities in New Jersey covering various years between 2000 and 2012. Census tract characteristics come from the 2000 Census Summary File 3. The Summary File 3 is a collection of tabulations down to the Block level provided by the Census Bureau. It is the only publicly available source of Census data for Census tracts.

The crime, release, and admission data were then aggregated from the individual incident level to the census tract level by quarter (in Boston and Newark) or year (in Trenton). This choice of the level of aggregation (census tract vs. block group) and time period (quarterly vs yearly) was driven by a desire to simultaneously balance three criteria. The first criterion was to find a small geographic area, ideally a block or block group, but a census tract is small enough to still be considered a neighborhood. The second criterion was to use a time frame short enough to minimize the simultaneity bias. Lastly, the data needed to have sufficient variation in releases and admissions across neighborhood by time-period. Ideally, we could have used smaller geographic areas and time periods as demanded by criteria one and two. But at the same time we needed to have sufficient variation, which in these data, the smaller geographic areas and time-periods do not have. Constrained by these three criteria we decided to use quarterly data at the census tract level (for all but Trenton).

The analyses centered on an analysis of the effects of rates of prison removals and returns on rates of crime in communities (defined as census tracts) in the cities of Boston, Massachusetts, Newark, New Jersey, and Trenton, New Jersey, and across rural municipalities in

New Jersey. Our analytic strategy, was one of analytic triangulation. Through the data collection associated with this project, we amassed a uniquely comprehensive crime and incarceration dataset over time – arguably one of the most comprehensive assembled to date. This dataset allowed us to model the relationship between crime and incarceration using a range of techniques (fixed effects panel models, Arrellano-Bond estimations, and vector auto-regression) taking advantage of each and being partially freed of the limitations of any one.

After establishing a preference for fixed effects regressions for these analyses, the first analyses we conducted sought to compare what we found in Boston to what had been found in earlier analyses of Tallahassee data. Tallahassee has been the site of several tests of the coercive mobility thesis, and each of those tests has provided support for the thesis. In these analyses of Boston data, we find that in neighborhoods with low concentrated disadvantage, there is no evidence of coercive mobility. Although the estimated persistence of crime rates from quarter to quarter is roughly the same across the two categories of neighborhoods (low and high concerntrated disadvantage), the effect of releases is negative and insignificant, and the effect of admissions is also negative but significant. Thus in neighborhoods with low levels of disadvantage, cycling in and out of prison reduces the crime rate. In the census tracts with higher than the median concentrated disadvantage, the signs on both releases and admissions flip. The effect on crime of releases is positive but not significant and the effect on crime of admissions is positive and significant. What we find is that one additional admission increase the crime rate by 0.367 the following quarter, an effect size that is in the neighborhood of one quarter of the lag of total crime. Thus, while in census tracts below the median concentrated disadvantage incarceration reduces crime, in neighborhoods above the median concentrated disadvantage incarceration increases crime.

We then turned to an analysis of this relationship between rates of incarceration and rates of crime across our three cities. The results for Boston and Trenton were each broadly consistent with the coercive mobility thesis. In the overall models, though the release rate was statistically insignificant at conventional levels in both cities, the coefficients are positive on both releases and admissions. In Boston, a one unit increase in the admission rate is associated with a 0.485 unit increase in the crime rate, on an average crime rate of 13.49 per 1000 per quarter. In Trenton, a one unit increase in the annual admission rate is associated with an increase in the annual crime rate of 8.3, on an annual rate of 22.49. In Newark, on the other hand, increases in both the admission and release rates are associated with decreases in the crime rate. A one unit increase in the quarterly release rate is associated with a 0.176 unit decrease in the quarterly crime rate, on an average crime rate of 13.06.

The coercive mobility argument, though, is a tipping point argument. There are particular types of neighborhoods, and particular types of situations where this effect should occur. If we think of Boston and Newark as a continuum of neighborhood types, then Boston would have low and medium disadvantage neighborhoods, while Newark will have largely high disadvantage neighborhoods. That is, based on statistics like median tract income or share of a tract in poverty, there is almost no overlap between Newark and Boston. Newark's highest income neighborhoods have roughly the same median income as Boston's median neighborhood. Further, Boston goes through a period of significant changes in many neighborhoods during the time period covered by the data, while Newark is largely stagnant.

This pattern suggested to us that, if we divided Newark and Boston, respectively, into high and low disadvantage neighborhoods, that we should see coercive mobility in the high disadvantage neighborhoods in Boston as has been demonstrated already, we may see it in the low disadvantage neighborhoods in Newark, but we should not see it in the low disadvantage neighborhoods in Boston nor the high disadvantage neighborhoods in Newark. Put simply, this is very close to the pattern we see. As seen earlier, the coefficients on releases and admissions were negative in low disadvantage neighborhoods in Boston, while they were positive in high disadvantage neighborhoods. In Newark, both coefficients were negative in low disadvantage neighborhoods. They were also negative in high disadvantage neighborhoods, though they are closer to zero, reflecting relative lack of variation in crime rates across the high disadvantage neighborhoods in Newark. In other words, we realized that high disadvantage neighborhoods in Newark have been so stagnant and high crime rates are so entrenched there that, although they are high cycling neighborhoods, the estimated effect of cycling is weaker.

The findings in Boston and Trenton provide clear support for the coercive mobility. In neighborhoods with high concentrated disadvantage prison cycling increases crime, while in neighborhoods with low concentrated disadvantage it does not. While prison releases have a negligible impact on crime, removal of residents has a strong positive impact on crime in disadvantaged neighborhoods, but a small negative impact on crime in those not struggling with disadvantage. (Rural New Jersey did not contain enough quarterly prison admissions and releases to support an analysis.)

We gave considerable attention to the problem of modeling. As might be expected, different models often provide different results. The most parsimonious models provide small standard errors with significant results, but there are sometimes sign changes when new control variables are added, suggesting instability in the modeling strategy. This is, we think one of the problems Lynch and Sabol encountered in their modeling approach in Baltimore. By contrast, the most stable results are provided by fixed effects models that, while intuitively attractive, have the disadvantage of large standard errors. When we use this analytic approach, we achieve results that, we believe, are reliable.

Though the results generally provide support for the coercive mobility hypothesis in Boston and Trenton, the analysis in Newark leads to a different outcome and, we believe, provides further theoretical insights as well as directions for further research.

For Newark the coercive mobility effects we expected to find did not hold. While reentry from prison was positively associated with crime rates, removal from the neighborhood was negatively associated with later crime. We believe there are interrelated theoretical and data explanations for why the coercive mobility model did not hold here. Newark neighborhoods, measured by indicators of social disadvantage, are very different than those of Tallahassee, Boston, and Trenton. All Newark census tracts have a very high degree of concentrated disadvantage, meaning that Newark does not have much neighborhood variation, as the other three cities do. This result is not entirely inconsistent with a theory of coercive mobility. If prison cycling does not have a linear effect on crime rates, but rather there is a tipping point with higher rates of cycling, could also be a point of saturation. After such saturation, increases in cycling would not contribute to further destabilization of the neighborhood, because it is already extremely destabilized. Cycling would not be a further destabilizing factor for these neighborhoods, because current destabilizing factors in Newark are powerful enough that they overshadow cycling. There is a data analytic explanation of the results in Newark, as well. We estimate the effect of cycling on crime by assessing variation between neighborhoods and variation over time. In Newark this variation is simply not there to estimate.

We ultimately conclude that our work finds strong support for the impact of prison cycling on crime. It seems that such cycling has different effects in different kinds of neighborhoods, consistent with the idea of a “tipping point” but more clearly expressed as an interaction between crime policy and type of neighborhood. The results in Tallahassee, Boston, and Trenton provide consistent support for this idea. In Newark, as a result of the city’s limited variability in neighborhood disadvantage, we fail to find the same pattern. Further research will investigate whether this neighborhood interaction holds in other sites. It will also enable us to think about how neighborhood change over time affects the prison cycling-crime relationship. Do neighborhoods that improve start to benefit from incarceration policy? In contrast, does current incarceration policy become a factor that inhibits neighborhood improvement?

We believe our results hold strong implications of incarceration policy. In particular, we think this work calls attention to the need for place-based correctional programming and policing strategies. If neighborhood context is such an important determinant of policy outcomes, then neighborhood-specific intervention seem warranted. In particular, interventions that are designed to ameliorate the destabilizing impact of prison cycling on informal social control—especially families and children—seem promising areas for experimentation. At a minimum, criminal justice strategies that increase prison cycling in these locations—such as drug sweeps producing a large number of arrests, and gang interventions based on arrests—seem to be potentially self-defeating.

The research undertaken to complete this project has informed a vibrant area of inquiry. As the number incarcerated in the United States approached, then exceeded, two million people, a widespread and multidisciplinary interest in the social effects of mass incarceration grew alongside. In recent years, there has been rapid growth in quantity and quality of work related to the impacts of incarceration on children, families, and neighborhoods. Distinguished scholars across the country are currently working on further our understanding of these issues. This work speaks directly to this growing inter-disciplinary literature, and in particular to a developing controversy regarding the effects of incarceration as it has concentrated among residents of our poorest communities.

## INTRODUCTION

While America's prison population has declined slightly over the last couple of years, the number of prisoners incarcerated in prisons across the United States still remains very high. In 2011, almost 1.6 million individuals were incarcerated in state and federal prisons (representing a decrease of nearly 1% from 2010) (Carson & Sabol, 2012). The recent decreases in rates of incarceration across many of the states represents a noteworthy change from the consistent annual increases that resulted in the more than quadrupling of prison populations between 1980 and 2010. Despite some early indications that prison population growth has certainly slowed, and may be beginning to reverse course (Clear & Frost, 2014), incarceration rates are particularly high for minorities – especially black males – residing in disadvantaged urban neighborhoods (Clear, 2007b; Western, 2007) and will remain so for the foreseeable future (Clear, 2007a; Mauer & Ghandnoosh, 2013; Western, 2006). In the poorest communities, the level of concentration is substantial; as many as one-fifth of adult men are incarcerated on any given day (James P. Lynch & William J. Sabol, 2004). Since most inmates stay in prison for only a few years at a time, the removal and release of imprisoned men has become a prevalent feature of life in these impoverished places.

The movement of people going into and coming back from prison is thought to put strains on a neighborhood and its residents in the way it affects networks, families, and children (Clear, 2007b; Patillo, Weiman, & Western, 2004). This process is not evenly distributed across places, but, rather, is highly concentrated, creating specific areas where there are continuously relatively large numbers of people being removed to prison and then later returned. The cumulative impact of this concentrated incarceration in communities is only beginning to be understood. Of particular interest is the impact of the flow in and out of neighborhoods on crime in those neighborhoods. While there is a large body of work estimated the impact of the size of the stock population on crime rates, there is much less work investigating the impact of the flow in and out of prison on crime rates for places with high rates of this kind of flow.

The growth in incarceration has not been evenly distributed across American society, but rather has concentrated among young, black males from impoverished inner-city neighborhoods. More than half of the adult prisoners are under 35 years old and almost 9 in 10 are under 45. Over 90% are men, and nearly half are African-American (Harrison & Beck, 2006). Cooccurrence of these demographic characteristics concentrates incarceration even further. Of black men in their late 20s, one in eight is currently behind bars (Harrison and Beck, 2006:10). Well more than half of all black high-school dropouts born between 1965 and 1999 have been (or will go) to prison; overall, one in five black males will be imprisoned sometime during adulthood (Western, 2006: 25-27).

In high incarceration places, imprisonment—especially of men in the prime parenting age groups—permeates the context, influencing institutional aspects of community life such as labor markets and housing, and interpersonal aspects such as family functioning and parenting (Braman, 2004; Clear, 2007b). Racial and economic segregation in cities concentrates incarceration in the poorest black neighborhoods. In some urban areas, more than one in six black adult males of ages 20 to 44 are in prison (Sabol & Lynch, 2003), and in sections of Brooklyn, one in seven youth aged 18-24 enter prison or jail each year (Cadora, 2007).

There is a well-established literature on how incarceration affects the individuals who go to prison (see, for example, (Gendreau, Goggin, & Cullen, 1999; Liebling & Maruna, 2006)). The ripple effects of high rates of individual-level incarceration are substantial. Across time, as different men cycle through confinement, family after family is affected by imprisonment. Rose and her colleagues (2000) report, for example, that in some Tallahassee neighborhoods, it seems almost every family has a family member imprisoned within a five year period. Braman's (2004) study of a District of Columbia neighborhood, LeBlanc's (2004) study of the South Bronx, and Venkatesh's (2006) study of New York City housing projects report similarly high, nearly ubiquitous, family-level experiences with incarceration of a loved one (Braman, 2004; LeBlanc, 2004; Venkatesh, 2006). There is a new and important literature on the way incarceration affects marriage (Darity, Myers Jr., Carson, & Sabol, 1994; Huebner, 2005, 2007), families (Murray, 2005; Western, 2006; Western, Lopoo, & McLanahan, 2004) and children (Aaron & Dallaire, 2010; Gabel & Shindledecker, 1993; Murray & Farrington, 2008; Murray, Farrington, Sekol, & Olsen, 2009) in various deleterious ways. This rapidly growing literature demonstrates the importance of incarceration as a dynamic affecting a range of community-level attributes.

Over the last decade, an increasing number of scholars have examined whether high incarceration rates might directly or indirectly lead to increased crime in communities (Hipp, Petersilia, & Turner, 2010; Rosenfeld, Wallman, & Fornango, 2005; Vieraitis, Kovandzic, &

Marvell, 2007). Rose and Clear (1998) suggested that high incarceration rates could be viewed as a form of "coercive mobility" that damages local network structures and undermines informal social control (Rose & Clear, 1998). The central claim of the coercive mobility thesis is that high rates of prison cycling (simultaneous removals from and returns to prison) increases crime in disadvantaged communities because those communities are unable to absorb (or counter) the effects of cycling. With an already weakened neighborhood capacity for informal social control, these prison removals and returns further damage the already fragile fabric of these communities leading to increases in rates of crime. In their analysis of prison admissions and releases to 80 neighborhoods in Tallahassee, Florida, Clear et al. (2003) showed that high rates of prison cycling generate increased crime rates net neighborhood levels of concentrated disadvantage (Clear, Rose, et al., 2003).

## STATEMENT OF THE PROBLEM

The purpose of this project has been to estimate the impact of "prison cycling"—the flow into and out of prison--on crime rates in communities, with special concern about areas that have high rates of prison cycling.

It is well documented that the increase in incarceration nationally over the last 40 years was a factor in the decade-long crime drop seen at the national level, though the size of that impact is much debated among social scientists (Zimring, 2007). It is equally well-known that the rate of incarceration of an area's residents also has a range of impacts on the crime rates of these "prison cycling" areas. Studies have generally shown that:

- As the number of people returning to a neighborhood from prison increases, the crime rate in that area tends to increase (Drakulich, Crutchfield, Matsueda, & Rose, 2012; Hipp & Yates, 2009); and
- As the number of people removed from a neighborhood and sent to prison increases in an area, problems such as poverty, broken families, and juvenile delinquency tend to increase (Clear, 2007b).

While prison cycling thus seems to potentially impact local areas in ways that are at cross purposes to national crime trends, the precise measure of that impact—how the flow in and out of prison contributes to changes in local crime rates—is not well understood. Moreover, while a growing body of work now investigates the impact of prison cycling on impoverished inner city areas, there has been no comparable effort to investigate these effects in rural areas. This project sought to fill these knowledge gaps by pursuing three objectives:

1. Through partnerships with local and state police, parole and correctional agencies, we sought to compile an extended time series of crime and incarceration data, one decade in duration, mapped to the census block level for the *cities* of Newark, New Jersey, Trenton, New Jersey, and Boston, Massachusetts.
2. Through partnerships with state police and parole and corrections agencies, we sought to compile an extended time series of crime and incarceration data, one decade in duration, mapped to the census block level for *rural* areas of New Jersey.
3. Using panel analysis with Vector Autoregressive (VAR) modeling, we modeled the impact of rates of prison cycling, both removal and reentry, on changes in rates of crime rates in those local (neighborhood) areas, controlling for community factors.

## REVIEW OF RELEVANT LITERATURE

Despite recent declines in rates of incarceration across many places in the United States, incarceration rates here remain the highest in the world with the U.S. imprisoning at least 20% of the world's incarcerated population. Systematic annual increases in prison populations between 1972 and 2010 produced what is commonly referred to as "mass incarceration:" a prison population of more than 1.6 million people at the start of 2010 (West, Sabol, & Greenman, 2010). Because of its scale, mass incarceration has become a social force to be reckoned with, and, without question, our unprecedented reliance on incarceration has had some fairly substantial effects on individuals, on families, and on communities. Some of these effects might have been positive (meaningful reductions in crime where they have occurred), but others have been demonstrably negative. In recognition of this, scholars in recent years have turned to teasing out some of those effects and beginning to try to understand the social ledger of incarceration (Sampson, 2011). We begin with an overview of that work.

Our overview is meant to illustrate the patterns of studies of prison, rather than offer a comprehensive investigation. Such an examination is certainly going to emerge from the upcoming National Academy of Sciences review of mass incarceration, now underway (National Research Council's Committee on Causes and Consequences of High Rates of Incarceration, 2014; National Research Council and Institute of Medicine, 2013). Instead, we provide a summary of the line of argument that has emerged from studies of incarceration, to show that the current paper fits well into the logic of the trajectory of current work. We review studies of incarceration in order to make the point that this field has moved from studies of individual-level effects to that of community-level effects. It is consistent with that trajectory to now ask how community contexts shape community-level effects.

## Studies of Incarceration

Prisons serve multiple purposes, including providing punishment for individuals who commit serious crimes. Incarceration rates, however, are also influenced by crime control concerns. The scale of imprisonment is linked to the extent to which society believes that expanding the use of prison sentences will reduce crime. Incapacitation and deterrence are the key mechanisms through which incarceration would achieve crime reductions.

Incarceration is typically thought of as a crime control strategy – the more people a place incarcerates, the less crime that place should expect to experience. Indeed, although the effects are admittedly quite modest, research has consistently demonstrated both deterrent and incapacitation effects associated with incarceration (Blumstein, Cohen, & Nagin, 1978; Bushway & Paternoster, 2009; Nagin, 1998, 2013a; Reuter & Bushway, 2007). The estimated size of incapacitation and deterrent effects though have varied quite dramatically from study to study (for an excellent review see (Stemen, 2007) see also (Bhati, 2007; Piquero & Blumstein, 2007), with some arguing that we should give up on our quest to estimate incapacitation effects entirely (Miles & Ludwig, 2007) and others arguing that "when deterrence effects are unpacked, it is clear that sanction threats are not universally efficacious as the magnitudes of the deterrent effects range from none to seemingly very large" (Durlauf & Nagin, 2011).

At risk of over-simplification, the existing evidence on the crime control efficacy of incarceration generally suggests modest deterrent and incapacitation effects (Blumstein et al., 1978; Bushway & Paternoster, 2009; Nagin, 1998, 2013a; Reuter & Bushway, 2007). A series of empirical studies suggested that increases in the incarceration rate were associated with decreases in the crime rate during the 1990s (see, e.g., Levitt, 2004; Spelman, 2008; Witt & Witte, 2000). In their analysis of prison population trends and crime rates between 1972 and 2000, Liedka, Piehl, and Useem (2006) revealed similar crime reduction impacts but concluded that as the prison population has grown, its crime-control benefits have diminished. Other studies suggest very small or no impacts of increased imprisonment on crime rates during this same time period (Western, 2005; Kovandzic & Vieraitis, 2006).

As incarceration rates climbed from the early 1970s right through the 1990s, despite fairly significant drops in crime over the latter decade, scholars began to try to better understand the *reciprocal* relationship between crime and incarceration. Understanding that relationship is not as straightforward as it would seem because as crime is occurring, people are being simultaneously housed in prisons, being removed from communities to go to prisons, and being returned to communities from prisons. Untangling these simultaneous effects has proven challenging, but theoretical developments have suggested that unpacking the effects of prison cycling on crime will likely prove instrumental to better understanding the total societal costs of incarceration (Sampson, 2011).

Social scientific examination of the impact of incarceration has become more sophisticated over the years. Early studies of imprisonment sought to understand how going to prison affected those who went there. As the proliferation of prison grew—as mass incarceration became the American penal reality—studies began to consider the effect of large-scale imprisonment. Quite naturally, this meant that social scientists investigated the way growing numbers of people exposed to prison affected the communities they came from and returned to. We review studies of effects of incarceration on individuals and studies of effects of incarceration on communities in turn below.

## Studies of the Impact of Incarceration on Individuals

The earliest studies of the effects of incarceration sought to learn the impact of imprisonment on people who go to prison. Two types of studies have dominated that literature: studies of changes in rates of recidivism due to prison programs and studies of rates of incapacitation that arise from incarceration. In general, the more sophisticated the study, the smaller the effect it has found.

Any summary of studies of prison programming must begin with the realization that prison is, itself, an intervention, regardless of any other programming a person might experience while behind bars. Of course, it is quite difficult to estimate the effect of imprisonment due to selection: people who are sent to prison represent, on the whole, a greater risk of recidivism than do those who receive a non-prison sanction. Thus it is not surprising that base expectancy studies find that people who go to prison experience higher recidivism rates than those who do not. The question is, how does going to prison affect the recidivism rate? Because there have been no randomized trials to determine the effect of prison as an intervention, most conclusions about this have been seen as informed speculation. Recently, however, two studies have been published that shed light on this question. Cullen and colleagues (Cullen, Jonson, & Nagin, 2011) have performed a meta-analysis of prison sentencing studies, and they conclude that going to prison slightly increases one's chances of new crime, compared to non-prison sanctions. Snodgrass and colleagues (Snodgrass, Blokland, Haviland, Nieuwbeerta, & Nagin, 2011) used a propensity score approach with instrumental variables on a very large sample of Dutch prisoners, and arrived at a very similar conclusion. These two studies confirm what most penologists have long believed, that the experience of prison does not convince people to become law abiding (Bales & Piquero, 2012; Lerman, 2009; Massoglia & Uggen, 2007, 2010; Nagin, 2013b; Nagin, Cullen, & Jonson, 2009).

If prison itself does not reduce the propensity to criminal behavior, what about intervention programs in the prison context? Here again, the field is dominated by poorly designed studies that do not avail themselves of confident conclusions.[1] Recent meta-analyses suggest, however, that prison intervention programs do not fare much better than prison itself (Rubak, 2005; Tong & Farrington, 2006).  Uniformly, meta-analytical approaches show the effect of prison programs to be quite small or non-existent, even though those same programs might have sizeable effects when offered in the community (Don A. Andrews & Dowden, 2005; D.A. Andrews et al., 1990; MacKenzie, 2000, 2006; Tong & Farrington, 2006).

[1] For a more comprehensive review of correctional research, and some suggestions for new directions, see Frost and Clear (Frost & Clear, 2012b).

Disappointing results of studies of intervention effects have often been counterbalanced by claims of quite sizeable crime prevention effects through incapacitation. The story of incapacitation has been told in some detail in several places (Spelman, 1994; Zimring & Hawkins, 1995). Early studies of incapacitation uncovered substantial frequencies of pre-prison criminality for people who were sent to prison and thus estimated quite large crime prevention effects of their stay in prison (Greenwood & Abrahamse, 1982; Zedlewski, 1987). It soon became apparent that these studies overestimated the incapacitative effects of confinement. Later studies identified problems with the simple incapacitation model on which these studies relied (Auerhahn, 2003; Miles & Ludwig, 2007; Spelman, 1994) and more sophisticated models began to show much more limited effects (DeFina & Hannon, 2010; Vieraitis et al., 2007). Simply put, the story of incarceration as an individual-level intervention follows a straight-forward pattern. Because imprisonment is such an extreme punishment, the effects are presumed to be quite significant. Initial studies using weak designs confirm this presumption. As studies become more sophisticated, however, the impacts they uncover become smaller and smaller. In the end, imprisonment may have a wide range of social and personal effects on individuals, but in terms of crime prevention, the value of prison is surprisingly limited.

For the United States, however, any useful contemporary understanding of the impact of imprisonment has to begin with an understanding of "mass incarceration:" the fact that we have experienced four decades of incarceration growth and, today, lead the world's democracies in imprisonment. Recent studies of mass incarceration have documented the concentration of imprisonment among poor people of color (Alexander, 2010) and disadvantaged neighborhoods (Clear, 2007b). The stark reality of mass incarceration as a major force in poor minority communities has led people to investigate how high levels of incarceration has affected those individuals and communities.

## Collateral Effects of Incarceration

In places where incarceration rates are especially high, most aspects of the community context are influenced by incarceration.[2] Not only are the individuals removed from the

[2] Here we can only introduce a small sample of the body of literature that has accumulated in this area. For more comprehensive reviews see (Clear, 2007b; Mauer & Chesney-Lind, 2003)

community affected, but so are their spouses, their children and their extended families. These individual and family level effects may, when aggregated across space and time – as they typically are in high incarceration places – produce relatively profound community-level effects. The communities themselves experience the consequences of incarceration with demonstrable effects in the labor market, in housing, and in the relative distribution of inequality and disadvantage.

The prison growth experienced for close to four decades concentrated among young men of color, and, due in large part to ongoing residential segregation, it especially concentrated in the communities in which they reside. The concentration of incarceration in poor urban communities that are home to large minority populations has had a range of lasting effects on those communities. The list of effects is long and the research documenting those effects is persuasive. For individuals and families, concentrated incarceration has reduced the rate of marriage, especially among African Americans (Huebner, 2005, 2007); it has further damaged the job prospects of individuals whose prospects were already quite limited (Holzer, 2009), it has been a cause of increased economic strain on families (Geller, Garfinkel, & Western, 2011; Sabol & Lynch, 2003; Wildeman & Western, 2010); it has damaged the life chances of children of people who go to prison, especially by increasing their risks of involvement in the juvenile justice system, damaging their school prospects, and serving as a risk factor in mental illness (Farrington, Coid, & Murray, 2009; Murray & Farrington, 2008; Murray et al., 2009; Wakefield & Wildeman, 2011; Wildeman, 2009); and it has served as a source of increasingly negative attitudes toward the justice system (Rose & Clear, 2004). At the community level, it has weakened labor markets, especially by weakening the earning power of people who cycle through the prison system (Holzer, 2009; Sabol & Lynch, 2003; Western & Pettit, 2005) and contributed to problematic health outcomes, including higher rates of STDs (Thomas & Sampson, 2005; Thomas & Torrone, 2006) and increases in the number of teen-age births (Thomas, Torrone, & Browning, 2010).

Policy makers and social scientists alike have begun to criticize the policy of mass incarceration, largely drawing upon these community-level effects. Some or all of these effects, or consequences, of concentrated incarceration might be acceptable – or more palatable – if incarceration had demonstrable crime prevention benefits. But, the incapacitation and deterrent effects of incarceration are, at best, modest (Reuter & Bushway, 2007) and in recent years we have seen an increasing number of studies indicating that high incarceration, concentrated in impoverished communities, leads to more, not less, crime (Clear, Rose, et al., 2003; DeFina & Hannon, 2010; Hannon & DeFina, 2012; Renauer, Cunningham, Feyerherm, O'Connor, & Bellatty, 2006).

## The Coercive Mobility Thesis

In 1998, Rose and Clear first proposed what has since been referred to as the coercive mobility thesis. Clear had previously argued that incarceration, when conceived of as a crime control policy, might backfire and actually increase crime. He identified at least three reasons why we might expect a backfire effect: (1) recruitment of increasing numbers of young people to replace those incarcerated offenders; (2) the diminishing deterrent effect of incarceration as more and more people experience prison, and (3) the effects that removing people from communities might have on social factors (broken families, increasing inequality, and social disorder) related to crime in those communities. This was an early exposition of the thesis that Rose and Clear would then develop focusing primarily on Clear's third effect of incarceration – its impact on the fabric of communities.[3]

In the years since Rose and Clear first articulated the thesis, there have been a number of attempts to test the thesis both directly and indirectly (for a more comprehensive review of this work, see (Clear, 2008; Frost & Clear, 2012a; Frost & Gross, 2012). Those tests have produced often conflicting results, with some finding support for the thesis, others reporting partial support, and still more failing to find support. We briefly review work that has most directly tested the thesis.[4]

There have been only a few direct tests of the coercive mobility thesis, which focuses on the simultaneous *removal from and return to* communities. More recently referred to as prison cycling, this pattern of removal and returns more fully captures the type of coercive residential mobility Rose and Clear hypothesized would be disruptive to the fabric of communities and to patterns of crime in those communities.

[3] Rose and Clear originally situated the coercive mobility thesis in the social disorganization tradition, but it is important to note that the thesis does not necessarily require a social disorganization orientation.
[4] For more comprehensive reviews of these literatures, see (Frost & Clear, 2012a; Frost & Gross, 2012)

Clear, Rose, and colleagues (Clear, Rose, et al., 2003) offered the first test of their thesis when they examined the relationship between imprisonment and crime in communities in Tallahassee, Florida. Using neighborhood-level rates of incarceration across Tallahassee in 1995 and rates of crime in 1996, Clear et al. controlled for a variety of neighborhood-level characteristics and modeled the effect of incarceration rates in one year on crime in the following year. Their test offered preliminary support for the coercive mobility thesis. Rates of incarceration increased rates of crime, and this was true on both sides of the equation (e.g. in terms of prison admissions and releases). The effect of prisoner releases was, as expected, positive and linear – as people were released from prison to neighborhoods, crime in those neighborhoods went up. The effect of admissions, however, was more complicated. When estimated using a quadratic for neighborhood incarceration rates, they found a curvilinear relationship between prison admissions and crime rates. When prison admission rates in a community were low, each additional admission to prison had a fairly small by negative effect of crime in those communities. In other words, a small number of admissions to prison from a community caused crime to go down in those communities, but as the number of admissions to prison increased and eventually hit a tipping point, the sign flipped and additional admissions to prison actually caused crime to go up. They concluded that in those neighborhoods disproportionately affected by incarceration (those that experience the highest levels of prison cycling), both prison admissions and releases were damaging to the community. Moreover, without exception, these communities hardest hit by incarceration were those that were characterized by fairly significant levels of concentrated disadvantage to begin with. In other words, in high incarceration neighborhoods, prison cycling increased crime making already fragile neighborhoods worse.

Clear et al.'s finding has since been partially replicated in a number of additional studies that have assessed the impact of incarceration in one year on crime in the next. Several of these were unpublished studies (Bhati et al., 2005; George et al., 2005; Powell et al., 2004), providing partial support across diverse cities (Columbus, Baltimore, Cleveland, and Chicago) and among different populations of offenders removed and returned from prisons (women, drug offenders, etc.). In one of the few published replications, Renauer et al. used data from 94 neighborhoods in Portland, Oregon to examine the relationship between removals for incarceration in 2000 on crime in 2001, and found similar evidence of a curvilinear relationship between incarceration and crime. Their findings while not significant for rates of property crime, were significant for rates of violent crime and therefore provided partial support for the coercive mobility thesis. More recently, using data from Seattle, Drakulich and colleagues (Drakulich et al., 2012) similarly found that “concentrations of returning prisoners are associated with a reduced capacity for collective efficacy, the fostering of social situations conducive to criminal behavior, and higher levels of violent crime” (p. 513-14).

One other work deserves explicit recognition, because it represented the most significant challenge to the findings of direct tests of the coercive mobility thesis to date. Lynch and Sabol attempted to find a coercive mobility effect through examining the relationship between crime and incarceration across neighborhoods in Baltimore, Maryland (James P. Lynch & William J. Sabol, 2004; James P. Lynch & William J. Sabol, 2004). To account for simultaneity bias, they ran models that included an instrumental variable (the portion of the drug arrest rate that could not be explained by the index crime rate) and models that did not. In the models that most closely replicated earlier tests of the coercive mobility thesis (e.g. non-instrumented models), Lynch and Sabol observed a coercive mobility effect – as prison admissions increased, so did crime in high incarceration communities. In other words, the non-instrumented models replicated what Clear et al. had found in Tallahassee. With the instrumental variable included in the model, however, the sign flipped and crime in communities appeared to go down as admissions to prison increased. In the instrumented models, prison admissions were negatively associated with crime rates as one might expect them to be if incarceration had an incapacitation or deterrent effect.

Lynch and Sabol also assessed the impact of incarceration on community organization and informal social control capacities and found that while incarceration had both positive and negative impacts on the capacities of communities, higher rates of incarceration were associated not only with lower rates of crime, but also with increased participation in community organization. Lynch and Sabol’s findings are particularly important because they challenged some of the key assumptions of the coercive mobility thesis and suggested that some of the observed relationships in earlier coercive mobility work might have been spurious.

Studies that have limited their analyses to the effects of incarceration in one year on crime in the next suffer from several methodological problems, including the inability to account for simultaneity bias. We know that, for a given probability of being caught, an increase in crime causes an increase in cycling. In particular, an increase in the flow into prison. It may not cause an increase in the flow out of prison if individuals released from prison do not return to the neighborhood they started in. Reverse causality may be an issue as well. That is it may be the case that prison cycling also increases crime. If this is true, then a "virtuous" cycle is created where crime becomes highly concentrated in certain neighborhoods as cycling in the past causes crime now.

In an attempt to overcome some of the methodological limitations of early tests of the coercive mobility thesis, two other studies have examined the impact of rates of incarceration on rates of crime over a more extended period of time, and both of those have used extended time series of the original Tallahassee data. First Waring, Scully and Clear (2005) expanded the original Tallahassee dataset to include data from 1994 through 2002, ran models similar to those used in Clear et al.'s earlier test of the thesis, and found essentially the same results. Just as Clear et al. had found, Waring and colleagues documented linear and curvilinear relationships between rates of incarceration and rates of crime in Tallahassee communities (Waring et al., 2005). Prison releases increased crime in those communities and prison admissions, at low levels of prison cycling, decreased crime but, at higher levels, increased crime.

More recently, economist Geert Dhondt estimated the effect of adding prisoners on crime per capita and the effect of prison cycling on crime per capita using a panel of neighborhoods in Tallahassee, Florida for the period 1995 to 2002 and found robust evidence for the coercive mobility thesis (Dhondt, 2012). Dhondt finds evidence to support the contention that the high levels of prison cycling are associated with increasing crime rates in marginalized neighborhoods, while this effect is not found in other neighborhoods. Looking more closely at the issues of race and class, Dhondt finds that while marginalized neighborhoods experience slightly higher crime rates, they are faced with much higher incarceration rates. In Black neighborhoods in particular, prison admissions are an order of magnitude higher in comparison with non-Black neighborhoods even though underlying crime rates are not very different.

## HYPOTHESES

Building on the work briefly summarized in the literature review above, researchers and theorists have suggested that highly concentrated incarceration would negatively impact public safety in places. Meares and Fagan (2007), Travis (2005) and Lynch and Sabol (2004a, 2004b) (among others) posited crime-enhancing effects that flow through relationships between the men who cycle through the prison and back into the neighborhood and those who live in the neighborhood. Rose and Clear (1998) used a revision of Bursik and Grasmick's (1993) theoretical work built on the social disorganization tradition to show how high levels of incarceration, concentrated in poor places, would be expected to produce a "tipping point" at which incarceration would cause crime in a community to go up rather than down. Analyzing data from Tallahassee (Clear, Rose, Waring and Scully 2003), Clear and colleagues found evidence of what they called a “coercive mobility” effect, in which the reentry of people from prison had a positive linear effect on crime at the neighborhood level in the year they returned, while rates of removal (in the preceding year) had a curvilinear impact, driving up crime at the higher levels after a certain “tipping point” (Clear, Rose, et al., 2003). This effect has since been replicated in Portland (Renauer, Cunningham and Feyerherm, 2004), Cleveland and Baltimore (Lynch et al., 2001) and partially replicated in Columbus (Powell et al., nd). The coercive mobility model was also replicated in Chicago for female prisoners (Lalonde and George, 2003).

These studies provide growing evidence that high rates of incarceration heavily concentrated in the most disadvantaged communities might actually make exacerbate the problem of crime in those communities. In keeping with findings from former work, we explicitly hypothesize that:

- There will be a positive impact of neighborhood reentry rates on neighborhood crime rates, controlling for neighborhood characteristics.
- There will be a positive effect of neighborhood removal rates (admissions) on neighborhood crime rates, controlling for neighborhood characteristics.
- The effect of the rate of both removal and reentry on the neighborhood crime rate will depend upon the level of removal and reentry (tipping point).

The original Clear/Rose thesis, and virtually all of the work that has followed has predicted that both the removals from the community and the returns to the community from prison could have negative effects on the ability of that community to self-regulate, but crucially all of that work has either explicitly or implicitly made a tipping point argument. Nobody argues that low levels of removals or returns will damage a community. In fact, it makes sense to argue that at low levels the removals of offenders would have a small, but perhaps difficult to isolate crime prevention effect, and that the return of those few offenders would also have a negligible effect. The key issue is whether high levels of prison cycling affect community level processes, and this by definition means communities with high level of disadvantage, already. If there is an effect, it is entirely plausible that it would not be linear, because the impact of the prison cycling multiplies across network to become a dense property of high disadvantage neighborhoods.

- The effect of the rate of both removal and reentry on crime the neighborhood crime rate will depend upon the level of concentrated disadvantage in the neighborhood (interaction effect).

It is important to note that the coercive mobility model assumes that between the removal from and/or return to communities, some mediating changes occur.

Increasing Removal Rates ➡ Mediating Changes ➡ Increased Crime Rates

Increasing Return Rates ➡ Mediating Changes ➡ Increased Crime Rates

Those mediating changes include things like increasing inequality, more broken families, decreases in levels of informal social control, and increasing social disorder. While our models have no direct measures of those mediating changes, they include a time lag to allow some of those changes to occur. A full test of the model would require measures of those mediating changes, but gathering those was beyond the scope of the proposed work.

Although the current study does not capture those mediating changes and therefore cannot provide a full test of the model, it represents a significant advance over previous work testing the coercive mobility thesis because it represents the most robust test of the thesis to date across three quite different cities with data spanning almost a decade in some sites. The extensive data we collected also allowed us to model the relationship between rates of prison cycling and rates of crime much more carefully with a lag to allow for the mediating changes to have occurred. Our ability to use quarterly data across two of the three sites also allows us to demonstrate that simultaneity is not as big a concern as had been previously assumed.

# Methods

## Data

To complete the proposed work, we needed to compile datasets on prison admissions and releases that would be comparable across places and map those data onto crime rates across those same places. Although this sounds relatively uncomplicated, we quickly learned the data collection for this project would be anything but straightforward. We needed comprehensive crime data and incarceration data (prison admissions and prison releases) across each of our sites, and we needed these data to be geocoded to the street address.

The original electronic data files for this project were sourced from nine different state and local criminal justice agencies, four in Massachusetts (MA) and five in New Jersey (NJ), including: Boston Police Department (MA), Suffolk County House of Correction (MA), Massachusetts Department of Correction (MA), Massachusetts Parole Board (MA), Newark Police Department (NJ), Trenton Police Department (NJ), New Jersey Department of Corrections (NJ), New Jersey State Police (NJ), and New Jersey State Parole Board (NJ).

## Data Cleaning

Upon receipt of datafiles from one of our partner agencies, any personal identifying information in each file was removed and replaced with unique study identification numbers before subsequent cleaning, geocoding, and analysis. Below, general descriptions of cleaning and geocoding methods are followed by detailed accounts of the number of cases lost and retained during the cleaning and geocoding phases and used in the final analyses.

As was anticipated, the original data files from the criminal justice agencies varied in quality and completeness. For example, some data files contained concatenated address or offense data. Lengthy strings of information needed to be parsed into separate fields before cleaning and sorting to remove duplicate entries, identify cases with complete (or nearly complete) addresses, and filter out any extraneous data points (e.g., stray entries or rows of invalid data). Other data files were missing key address fields. For example, zip codes were frequently missing, incomplete, or clearly incorrect. City names were often incorrectly spelled. The goal was to prepare files with the maximum number of entirely unique entries containing addresses that could be successfully geocoded. After cleaning, all files were submitted to Justice Mapping (JM), a partner organization, for geocoding.

## Data Geocoding

Prior to geocoding, the research team made sure that each data file contained fields for a street address, city, state, and zip code (when available) for geocoding. Our partner, Justice Mapping Inc., geocoded and mapped the crime and incarceration datasets that did not already have geocodes (both the Boston and Newark police departments provided data that already contained geocodes). Once they received an original datafile, Justice Mapping conducted a visual inspection of the address fields, checking that all of the necessary fields were present and assessing the cleanliness and consistency of the data (e.g., does every record have a zip code?, how many different ways is Newark spelled?, is the street address field formatted correctly?, does every record have a building number?, are there records missing addresses?). Records without a mappable address were removed (e.g., blank, missing city and zip, missing house number, marked as "homeless", etc.). Larger issues that consist across the dataset, if any, were fixed (rename "Newk" abbreviation to "Newark) – although most of the data sets did not have these issues. Addresses were then geocoded using the Geocode.com service (formerly owned by TeleAtlas, now owned by TomTom). The service assigns a code to each address (see Table 1 for a list of all codes) and returns the latitude and longitude, State FIPS, County FIPS, Census Tract, and Census Block Group IDs for each address. Only matches with a code of 1 to 5 were used. A match code of 1 indicates a match to the primary name of the street segment, a match code of 2 indicates an intersection match "from end of the segment from the first street in the address," a match code of 3 indicates an intersection match "to end of the segment from the first street in the address," and a match code of 5 indicates a match to alternate or secondary name on a segment.

For a case to be included in this study, it had to have yielded a match code of 1-5 (almost all of these were matches to the primary street segment (1), but some were matches to an intersection or to an alternate or secondary name on segment (2, 3, or 5). If a case yielded any other result, it was considered an unsuccessful match and excluded from the analyses. In Table 2, we indicate the match type counts for each of the datasets that Justice Mapping geocoded as part of this project. As w, the geocoding match rates were generally quite good (when calculated from the total number of cases submitted hit rates ranged from 73 – 84%) but fall short of the

85% that Ratcliffe has argued is acceptable as a minimum hit rate for geocoding crime point data (Ratcliffe, 2004).[5] More specific information related to the geocoding hit rate of each dataset is provided in relation to the discussion of that dataset below.

### Table 1: Geocoding Match Codes

| Match Code | Description |
|---|---|
| 1 | Match to primary name of street segment |
| 2 | Intersection match to the "from end of the segment from the first street in the address" |
| 3 | Intersection match to the "to end of the segment from the first street in the address" |
| 5 | Match to alternate or secondary name on segment |
| 6 | Match to placeholder point |
| 7 | Match to alternate or secondary name of placeholder point |
| 10 | Invalid state abbreviation |
| 11 | Invalid locality name |
| 12 | Street name failed to parse due to unrecognized format |
| 14 | Street name could not be found in requested locality |
| 15 | Address range for input house number did not exist on give street |
| 16 | More than one segment with adequate address range |
| 17 | Unable to match intersection |
| -1 | Invalid input address sent to server (incorrect number of fields) |
| -10 | Server or network error prevented the address from geocoding |

[5] We further discuss the implications of this hit rate and what it means for research of this type and for agencies that are the repositories of original criminal justice data in the discussion section of this report.

### Table 2: Justice Mapping Geocoding Declination Reports – For All Data Geocoded

| | | MA DOC Admissions | MA DOC Releases | SHOC Admissions | SHOC Releases | NJ DOC Admissions | New Jersey Parole |
|---|---|---|---|---|---|---|---|
| **Pre-Geocoding** | | | | | | | |
| | Submitted to JM | 38,743 | 27,924 | 10,722 | 7,772 | 137,227 | 108,385 |
| | Dropped by JM Prior to Geocoding | 3,362 | 2,106 | 1,162 | 614 | 9,934 | 17,676 |
| | Remainder for Coding | 35,381 | 25,818 | 9,560 | 7,158 | 127,293 | 90,719 |
| **Match Code** | | | | | | | |
| 1 | ***Match to primary name of street segment*** | ***30,029*** | ***23,172*** | ***7,768*** | ***5,818*** | ***111,499*** | ***79,142*** |
| 2 | ***Intersection match to the "from end of the segment from the first street in the address"*** | ***0*** | ***0*** | ***0*** | ***0*** | ***3*** | ***6*** |
| 3 | ***Intersection match to the "to end of the segment from the first street in the address"*** | ***0*** | ***0*** | ***0*** | ***0*** | ***43*** | ***19*** |
| 5 | ***Match to alternate or secondary name on segment*** | ***227*** | ***173*** | ***94*** | ***85*** | ***1,835*** | ***1,426*** |
| 6 | Match to placeholder point | 0 | 0 | 0 | 0 | 0 | 0 |
| 7 | Match to alternate or secondary name of placeholder point | 0 | 0 | 0 | 0 | 0 | 0 |
| 10 | Invalid state abbreviation | 1 | 0 | 15 | 12 | 199 | 40 |
| 11 | Invalid locality name | 21 | 18 | 200 | 49 | 1,848 | 3,322 |
| 12 | Street name failed to parse due to unrecognized format | 3 | 2 | 32 | 5 | 8 | 565 |
| 14 | Street name could not be found in requested locality | 4,119 | 1,847 | 857 | 705 | 7,647 | 3,629 |
| 15 | Address range for input house number did not exist on give street | 527 | 388 | 186 | 146 | 3,112 | 1,469 |
| 16 | More than one segment with adequate address range | 450 | 216 | 408 | 338 | 1,094 | 719 |
| 17 | Unable to match intersection | 4 | 2 | 0 | 0 | 5 | 7 |
| -1 | Invalid input address sent to server (incorrect number of fields) | 0 | 0 | 0 | 0 | 0 | 0 |
| -10 | Server or network error prevented the address from geocoding | 0 | 0 | 0 | 0 | 0 | 0 |
| | Unspecified | 0 | 0 | 0 | 0 | 0 | 20 |
| **Overall Hit Rate (cases matched/cases originally submitted)** | | **78%** | **84%** | **73%** | **76%** | **83%** | **74%** |

*Notes: Only those data fields with match codes 1-5 (in bold italics) were assigned geocodes and included in the analyses.*

## Research Sites and Partners

To carry out this project, we developed working partnerships with the Boston, Newark, and Trenton police departments, the New Jersey State Police, and with Massachusetts and New Jersey correctional and parole agencies. In total nine justice agencies (four police departments, two departments of correction, two state parole agencies, and one county correctional facility) provided data for this project. Each of the agencies, and the type of data they provided, are described in the sections that follow.

It is crucial to note that in the sections that follow, we are explaining how we got to the data that we actually used for the project. In Table 2 above, we report on ALL data that were provided and then geocoded. In all sections that follow, we are distinguishing data collected from data used to describe how we got to our final datasets. The descriptions below are describing the various data declination tables provided in Table 3.

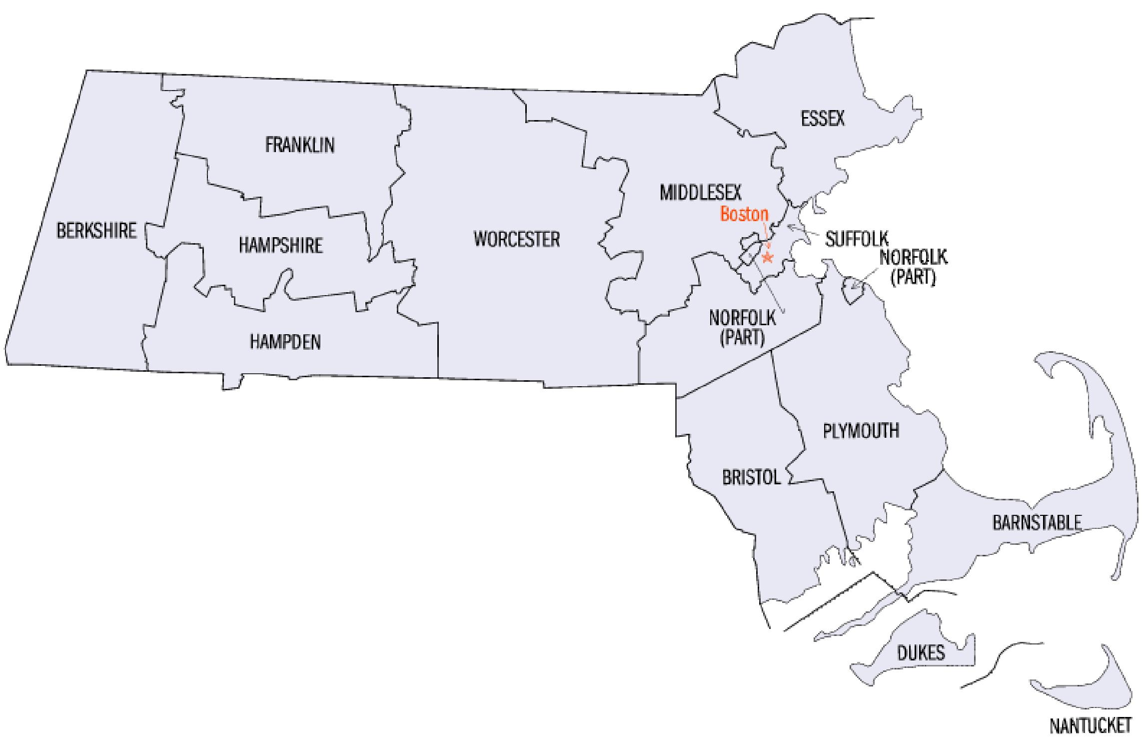
### Boston, Massachusetts

The prison admission and release rates for Boston, Massachusetts were derived from data provided by three different partner agencies. In Massachusetts, sentenced prisoners serve out their sentences in two different types of correctional facilities. Offenders sentenced to more than two and a half years serve their sentences in state prisons, while offenders sentenced to one day up to two and a half years serve their time in "houses of correction" administered by the county (Boston is entirely in Suffolk County). Although counties often maintain both a jail (for unsentenced inmates) and a house of correction (for sentenced inmates), in many counties across Massachusetts, the county house of correction incarcerates both unsentenced and sentenced inmates. In Suffolk County, the Nashua Street jail houses unsentenced inmates (but only to its rated capacity of 700 pre-trial detainees) and the House of Correction handles any overflow from the jail (Suffolk County Sheriff's Department, 2013). The Suffolk County House of Correction (locally known as "South Bay") therefore houses both unsentenced and sentenced inmates.

As a result of this unique structure in Massachusetts, three distinct agencies maintain the data required for this project. The Massachusetts Department of Correction (MDOC) maintains records for all offenders sentenced to and released from state prisons (and this generally includes data on only those sentenced to more than 2.5 years). The Suffolk County House of Correction maintains records for all offenders sentenced to 1 day to 2.5 years from Suffolk County (which includes all of Boston). The Massachusetts Parole Board is an independent state-wide agency that separately maintains basic data on all offenders admitted to a county or state correctional institution for at least 60 days, and maintains more detailed data on all of those who are ultimately released to parole.

It is important to note that although Boston is entirely in Suffolk County (and therefore most admissions from and releases to Boston addresses will be processed through the Suffolk County correctional facilities), the neighboring city of Cambridge is in Middlesex County (to the west of Boston), and our data for those sentenced to between 1 and 2½ years cannot capture those admitted to Middlesex County facilities from Boston addresses or released to Boston from Middlesex County facilities. The same is true for the counties neighboring Suffolk to the north (Essex) and to the south (Norfolk) and more distant neighboring counties (see Figure 1).

**Figure 1: Massachusetts County Map**




Source: http://quickfacts.census.gov/qfd/maps/massachusetts_map.html (Last accessed: 12/2/2013)

*Massachusetts Department of Corrections*

In Massachusetts, offenders sentenced to more than two and a half years are incarcerated in one of eighteen Massachusetts Department of Correction (MA DOC) facilities. The MA DOC is the state-wide correctional agency comprising eighteen facilities of four different security levels (maximum, medium, minimum and pre-release) located across the state. The average inmate population per day under DOC jurisdiction (which includes the populations serving time in Massachusetts DOC facilities and in facilities outside of Massachusetts DOC) was 11,819 in 2012. According to their annual report, there were 3,216 criminally sentenced offenders admitted to, and 3,550 released from, the custody of the Department of Correction in 2012 (Massachusetts Department of Correction, 2012, 2013).

The Massachusetts Department of Correction (MA DOC) provided prison admission and release data between January 2000 and September 2011 for all offenders admitted to and released from Massachusetts prisons.[6] The MA DOC data included information on admissions to (from 2000-2011) and releases from (for years 2000-2011). These data were provided in seven separate files: three for the admissions (admission offenses (ADM), unique commitments (UC), and last known addresses (LKA)) and four for the releases (release offenses (REL), unique commitments (UC), last known addresses (LKA), and release addresses (RA)). Working with these data was particularly complicated because, as noted by the MA DOC when the files were provided:

> *The last known addresses files includes data which is self-reported by the inmate at the time of admission and an inmate may have multiple addresses within this file because an inmate address file is updated with each admission. The release address file may also contain multiple commitment numbers because an inmate address file is updated with each release. There is an insert date within all the address files, which may represent the date the data entered into IMS. The address files can be matched to the admission and release files via the commitment number; however, there is no clear-cut process for linking the addresses to the corresponding admission or release row of data by dates.*

In other words, MA DOC updates addresses every time there is an address worth updating and provided those addresses in separate file (LKAs and RAs). A single commitment could have

[6] We would like to thank the Massachusetts Department of Correction for providing these data and Rhiana Kohl, Linda Griffin, and Jessica Simes in particular for their assistance in the process of compiling and providing the data for this project.

many last known addresses (up to 11 in the data provided) and several release addresses and these were dated by year in the LKA file and RA file, but often these dates did not match up with the admission or release date (and were not even in the same year). In other instances a new commitment did not require an LKA update because the LKA presumably stayed the same. At the extremes, you have some offenders with many commitments and just one address (that might have been entered into the system long before the current commitment or release), and others with just one commitment but many addresses (sometimes entered outside the range of the current commitment). The data structure made merging the files more complicated than simply matching on commitment number and merging last known addresses. We ultimately developed a merging strategy for dealing with this that involved assuming that the LKA or RA was the address associated with our admission or release as long as it matched (or pre-dated) the date of the current admission or release.

The original MA DOC admissions files contained 37,418 admission offense records (of which 34,550 were associated with unique commitments) and the releases files contained 30,205 release records (of which 29,362 were associated with unique commitments). Records from years 2000 to 2002 were removed entirely due to the extent of missing and unusable address data in the release files and because the Suffolk County House of Correction, which provided the supplementary data for those offenders admitted to and released for sentences of one to two and half years, could only provide data starting in 2003 (see below). Records from the partial year (2011) were also excluded. This resulted in MA DOC data files containing 27,006 admissions and 20,347 releases between 2003 and 2010.

The admission and release files contained the inmate governing offense data, which represented the offense that carried the longest maximum sentence. These governing offenses were then broken down by the MA DOC into one of the following five offense types: (1) drug, (2) person, (3) property, (4) sex, and (5) other crimes, as well as into categories of “violent” or “non-violent” offenses. The last known addresses in the files were self-reported by the inmates at the time of admission or release.

Prior to sending a file for geocoding, data with missing and un-useable addresses were removed. Of the 27,006 admission records from the MA DOC for 2003-2010, 25,901 (or 96% of all records) had data in the address field after merging LKAs and were sent for geocoding. Of the

20,347 release records from the MA DOC for 2003-2010, 18,789 (or 92% of all records) had data in the address field after merging RAs or LKAs and were sent for geocoding.

Not all of the data sent to Justice Mapping could be geocoded (a general description of the geocoding process and the types of matches that could result was included in a section above).[7] For the MA DOC data, Justice Mapping was able to successfully geocode 20,941 admission records and 15,835 release records for 2003 through 2010 (see Table 3 below). In other words, from original data to geocoded data for MA DOC data for 2003-2010, our data retention rate was approximately 78% (20,941/27,006) for admissions and approximately 78% for releases (15,835/20,347).

The MA DOC provided *state-wide* data: data for offenders admitted from or released to Boston addresses were then isolated after geocoding. In total, 3,647 offenders were admitted from Boston addresses over the 2003-2010 period and 3,286 offenders were released to Boston addresses over the period. In other words, among those for whom we could geocode an address, roughly 17% (3,647/20,941) are admitted to DOC custody from Boston addresses and approximately 21% (3,286/15,835) are released from DOC custody to a Boston address. These admissions to and releases from DOC facilities to Boston addresses were the only data ultimately included in the analyses. Table 3 below reports on overall data declination for MA DOC files from 2003-2010, as well as final resulting data files once MA DOC data were combined with Suffolk County House of Correction data.

### *Suffolk County House of Correction*

Although there are some exceptions, generally speaking, in Suffolk County (Boston), unsentenced offenders await trial in the Suffolk County “Nashua Street” Jail and sentenced offenders serve sentences of one day to two and a half years in the Suffolk County “South Bay”

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[7] Refer to Table 2 for a comprehensive report on the numbers of cases by match code from geocoding (for all records sent to Justice Mapping).

House of Correction.[8] Both facilities are run by the Suffolk County Sheriff's Office, but the data for this project were provided by the House of Correction.[9]

The Suffolk County House of Correction is comprised of seven buildings and primarily houses adult males and females convicted of crimes and sentenced to 2.5 years or less. When necessary, the Suffolk County House of Correction also houses overflow populations from the Suffolk County jail. Based on the facility's classification system, inmates are assigned to one of 32 separate housing units (comprising approximately 674 cells and 1892 beds). In 2010, the average daily inmate population fluctuated between 1,700 and 1,800 inmates (Suffolk County Sheriff's Department, 2011).

The Suffolk County House of Correction (HOC) provided basic data for all offenders admitted to or released from the HOC between 2003 and 2010. These data were then filtered to exclude all unsentenced inmates and then further filtered to include only those sentenced to more than one year (to make the Massachusetts data on prison cycling more comparable to the data that would be collected in other states where a prison sentence is typically a sentence of more than one year).

In total, the Suffolk County HOC provided data on 63,058 admissions and 51,331 releases between 2003 and 2010. The vast majority of those were records for offenders admitted for less than one year. After isolating those offenders sentenced to more than one year, 9,685 admission records remained, of which 9,267 had an entry in the address field. For releases, we additionally had to isolate valid release types, including only those released to parole, released to streets, or released at end of sentence. Of the original 51,331 release records, 6,540 were records for offenders sentenced to more than one year and actually released, of which 6,254 had an entry in the address field.

For 2003-2010, 9,267 admission records and 6,254 release records were sent to Justice Mapping for geocoding. Justice Mapping was able to geocode 7,753 admissions and 5,538

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[8] Exceptions can occur when an inmate is housed in a different county for security reasons, or when an inmate has already served time in a state prison. In Massachusetts, under some circumstances, an inmate who has served time in a state facility can be transferred to the state from the county.

[9] We would like to thank the Suffolk County Sheriff's Office for providing these data and, in particular, Gerald Walsh, Robert Gaudet, and Annie Mui for their assistance in providing access to and compiling the data.

releases (See Table 3 below). After isolating the relevant population, our general "hit rate" for geocoding HOC admissions, was approximately 80% (7,753/9,685) and our general "hit rate" for geocoding HOC releases was approximately 85% (5,538/6,540).

From these geocoded admission and release records, 6,092 of the 7,753 admissions (or close to 79%) were admitted from addresses within the city of Boston. 4,360 of the 5,538 geocoded release records (also close to 79%) were released to addresses within the city of Boston.[10] These admissions to and releases from the Suffolk County HOC to Boston addresses were the only data ultimately included in the analyses. Table 3 below reports on overall data declination for MA DOC and Suffolk County HOC files from 2003-2010, as well as final resulting admission and release data files once MA DOC data were combined with Suffolk County HOC data. When MA DOC and Suffolk County HOC data for admissions and releases were combined, there were a total of 9,739 Boston admissions and a total of 7,646 Boston releases between 2003 and 2010 included in the analyses reported below (See Table 3).

### *Massachusetts Parole Board*

The Massachusetts Parole Board (MPB) is responsible for making parole release determinations inmates who are eligible for parole as well as for supervising those who have been granted parole. The seven members of the Parole Board are each appointed by the Governor of Massachusetts and these Parole Board members hold hearings both at the Parole Board headquarters and in the correctional facilities around the state to determine whether or not inmates should be granted parole (Massachusetts Parole Board, 2012). According to the MPB's report on parole decisions, in 2012, 6,694 release hearings were held (which includes release hearings from both the Massachusetts Department of Correction and the various county Houses of Correction) (Massachusetts Parole Board, 2013). From the 6,694 release hearings held, the

[10] The overlap between admissions and releases to the city of Boston is not surprising given the House of Correction reported that the release address very often is the admission address (whether or not it is actually the case that the offender went back to the same address). This was also the case with the New Jersey Department of Correction, which reported that the NJ DOC typically does not even ask about the address upon release for those released from DOC facilities. Indeed the release address data from New Jersey were of such questionable quality that we opted to use New Jersey Parole data for releases even though this meant that we would only be capturing the rates of release of paroled offenders. The veracity of the address data, and the release addresses in particular, is a major issue for this type of work and is discussed in more detail in the implications for future research section of this final report.

Parole Board granted parole to 3,770 inmates, which yields a 56% paroling rate for that year. Additionally, rescission hearings are also held by the Parole Board, which occur in the time after the release hearing and before the parole release date, if an inmate's behavior during that time warrants an additional hearing. The inmate's parole could be withdrawn, postponed or reactivated, based on the Parole Board's decision. In 2012, 191 rescission hearings were held, which also includes rescission hearings from both the Department of Correction and the Houses of Correction. Out of these 191 rescission hearings, 110 resulted in the granting of parole. Lastly, revocation hearings are held when parolees have violated their conditions of parole. In 2012, 459 revocation hearings were held, which included revocation hearings from both the Department of Correction and the House of Correction. Out of these 459 hearings, 306 of them resulted in parole revocation (Massachusetts Parole Board, 2013).

We initially assumed that the Massachusetts Parole Board (MPB) would be the best research partner for the Boston, Massachusetts request because the MPB collects data on prison admissions and releases for all inmates admitted to and released from both state prisons and county houses of correction serving sentences of at least 60 days (as these offenders might ultimately be eligible for parole). We therefore requested that the Parole Board provide address data on prison and house of correction admissions and releases for those serving sentences of more than one year (2000-2010); however, the Parole Board was not able to provide address data for admissions, and could only provide reliable address data for those actually released to parole.[11] The benefit of the parole data is that the addresses at release would have been verified by the agency prior to release (this makes these data more reliable than department of correction or House of Correction data which relies entirely on the self-report of the inmate without any further verification).

The original datafiles received from the Massachusetts State Parole Board contained data on 51,138 offenders released to parole between January 2000 and May 2011.The parole data included release addresses in a single "home plan" field that included string data that then had to be delimited. Home plans often included multiple addresses as new addresses are entered each time the parolee's home plan changes. We retained the initial home plan as the "release" as this

[11] We would like to thank the Massachusetts Parole Board, and in particular, Josh Wall, Stephanie Geary, Dave Quinlan, and Shawna Andersen for their assistance in providing and assembling the data for this project.

first entry typically represents the address at first release. The home plans also included street addresses and cities and/or neighborhoods, but no zipcode. Once delimited the data were filtered to include only those released to Boston addresses.[12] Of the 51,137 releases, 41,893 were released to an address that was not in Boston, 1,074 were in custody/transferred, or deported, 465 had no home plans provided, 394 were deemed duplicate entries (same birth date, release date, and address). A handful of cases were removed for other anomalies, resulting in a final MPB dataset of 6,922 released to Boston addresses between 2000 and mid-2011. These data were then sent to Justice Mapping for geocoding. Justice Mapping was able to successfully geocode 6,415 of those addresses (for a geocoding match rate of 92.6%), of which 4,802 occurred between 2003 and 2010 (the years for which we have consistent corrections data for Massachusetts).

Although we geo-coded the Boston parole data, as demonstrated in Table 3 below, the 4,802 geocoded parole addresses between 2003 and 2010 represent approximately 63% of the total releases we could model if we use the combined MA DOC and Suffolk County HOC data (by year the percent of total varies from a low of 54% in 2009 to a high of 72% in 2003).[13] In all of the primary analyses included in this report, we ultimately used the combined MA DOC and Suffolk County HOC data for Boston releases as these data capture both parole releases and end of sentence data.

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[12] Included as Boston addresses were any addresses that explicitly identified either "Boston" or one of the following Boston neighborhoods in the initial entry in the home plan field: Allston, Brighton, Back Bay, Beacon Hill, Charlestown, Chinatown, Dorchester, Mattapan, East Boston, Fenway/Kenmore, Hyde Park, Jamaica Plain, Mission Hill, North End, Roslindale, Roxbury, South Boston, South End, and West Roxbury. We have included a map of Boston's various neighborhoods as Figure 2.

[13] It is crucial that we note that these are not necessarily the same offenders, so the 63% of the total does NOT mean that 63% of the offenders released between 2003 and 2010 were paroled. Because we could not match offenders across datasets, we are simply reporting that were we to use parole data for the analyses, we would have been capturing at best 63% of the number of geocoded releases that we can capture if we use the combined DOC and SHOC data for releases. The choice to substitute data quantity could certainly come at the cost of a decline in quality of the data (because as noted above, the MSP verifies release addresses for parolees, and the DOC and HOC do not, and because the MPB data can catch releases to Boston regardless of which state or county correctional facility the offender was released from). We ran all models reported below using only parole releases (rather than all MA DOC and SHOC releases combined), and the results were substantively the same (see Tables 28 and 29).

### Table 3: Massachusetts Correctional Data Declination

**Massachusetts Department of Correction: Admissions**

| **Description of Cases** | **Totals Cases** | **2003** | **2004** | **2005** | **2006** | **2007** | **2008** | **2009** | **2010** |
|---|---|---|---|---|---|---|---|---|---|
| | | Year | | | | | | | |
| Original data files | 27,006 | 2,923 | 2,895 | 3,556 | 3,458 | 3,628 | 3,657 | 3,430 | 3,459 |
| *After removing blank or unusable addresses* | *25,901* | *2,808* | *2,825* | *3,446* | *3,296* | *3,534* | *3,477* | *3,190* | *3,325* |
| Geocoded cases | 20,941 | 2,295 | 2,246 | 2,790 | 2,627 | 2,833 | 2,851 | 2,593 | 2,706 |
| **Admitted from Boston addresses** | **3647** | **399** | **375** | **510** | **475** | **489** | **483** | **454** | **462** |

**Massachusetts Department of Correction: Releases**

| **Description of Cases** | **Totals Cases** | **2003** | **2004** | **2005** | **2006** | **2007** | **2008** | **2009** | **2010** |
|---|---|---|---|---|---|---|---|---|---|
| | | Year | | | | | | | |
| Original data files | 20,347 | 2,364 | 2,435 | 2,390 | 2,395 | 2,561 | 2,715 | 2,759 | 2,728 |
| *After removing blank or unusable addresses* | *18,789* | *1,808* | *2,251* | *2,208* | *2,183* | *2,363* | *2,614* | *2,691* | *2,671* |
| Geocoded cases | 15,835 | 1,440 | 1,898 | 1,850 | 1,800 | 1,975 | 2,249 | 2,272 | 2,351 |
| **Released to Boston addresses** | **3,286** | **308** | **393** | **379** | **349** | **430** | **446** | **482** | **499** |

**Suffolk County House of Correction: Admissions**

| | **Total Cases** | **2003** | **2004** | **2005** | **2006** | **2007** | **2008** | **2009** | **2010** |
|---|---|---|---|---|---|---|---|---|---|
| | | Year | | | | | | | |
| Original data files | 63,058 | 5,993 | 6,805 | 7,063 | 9,018 | 8,859 | 9,034 | 8,250 | 8,036 |
| *After isolating inmates sentenced (> 1 year)* | *9,685* | *1,072* | *1,233* | *1,290* | *1,442* | *1,302* | *1,321* | *1,105* | *920* |
| *After removing blank or unusable addresses* | *9,267* | *1,038* | *1,180* | *1,250* | *1,379* | *1,246* | *1,262* | *1,040* | *872* |
| Geocoded cases | 7,753 | 897 | 996 | 1,027 | 1,142 | 1,031 | 1,038 | 866 | 756 |
| **Admitted from Boston addresses** | **6,092** | **718** | **789** | **804** | **880** | **802** | **815** | **680** | **604** |

**Suffolk County House of Correction: Releases**

| | Total Cases | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|---|---|---|---|---|
| Original data files | 51,331 | 4,326 | 5,165 | 4,974 | 7,647 | 7,603 | 7,400 | 6,948 | 7,268 |
| *After isolating inmates sentenced (> 1 year)* | *6,914* | *924* | *897* | *851* | *853* | *915* | *851* | *848* | *775* |
| *After determining valid release type* | *6,540* | *887* | *850* | *818* | *800* | *847* | *812* | *797* | *729* |
| *After removing blank or unusable addresses* | *6,254* | *845* | *814* | *794* | *767* | *808* | *768* | *764* | *694* |
| Geocoded cases | 5,538 | 724 | 741 | 706 | 684 | 716 | 670 | 675 | 622 |
| **Released to Boston addresses** | **4,360** | **599** | **589** | **552** | **528** | **551** | **524** | **528** | **489** |

(Year)

**Massachusetts Parole Board Releases**

| | Total | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|---|---|---|---|---|
| Geocoded Parole Releases | 4802 | 651 | 704 | 612 | 594 | 581 | 544 | 546 | 570 |
| Geocoded MA DOC + SHOC Releases | 7646 | 907 | 982 | 931 | 877 | 981 | 970 | 1,010 | 988 |
| Percent | 62.80 | 71.78 | 71.69 | 65.74 | 67.73 | 59.23 | 56.08 | 54.06 | 57.69 |

**TOTAL BOSTON ADMISSIONS AND RELEASES INCLUDED IN ANALYSES**

Year

| | Total Cases | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|---|---|---|---|---|
| Massachusetts DOC Admissions | 3,647 | 399 | 375 | 510 | 475 | 489 | 483 | 454 | 462 |
| Suffolk County HOC Admissions | 6,092 | 718 | 789 | 804 | 880 | 802 | 815 | 680 | 604 |
| **TOTAL ADMISSIONS** | **9,739** | **1,117** | **1,164** | **1,314** | **1,355** | **1,291** | **1,298** | **1,134** | **1,066** |
| | | | | | | | | | |
| Massachusetts DOC Releases | 3,286 | 308 | 393 | 379 | 349 | 430 | 446 | 482 | 499 |
| Suffolk County HOC Releases | 4,360 | 599 | 589 | 552 | 528 | 551 | 524 | 528 | 489 |
| **TOTAL RELEASES** | **7,646** | **907** | **982** | **931** | **877** | **981** | **970** | **1,010** | **988** |

### *Boston Police Department*

The Boston Police Department's (BPD) jurisdiction includes all of the city of Boston, Massachusetts, and all of the municipality-owned property within the municipality's boundaries. BPD is composed of twelve districts, not including the Headquarters, that encompass areas such as, downtown, Charlestown, East Boston, Roxbury, Mattapan, South Boston, Dorchester, the South End, Brighton, West Roxbury, Jamaica Plain and Hyde Park. In 2010, the BPD was comprised of 2,090 sworn officers and 787 civil personnel (Boston Police Department, 2011).

Table 4 reports on index crime rates for Boston reported to the Federal Bureau of Investigation (FBI) through the Uniform Crime Reports (UCR). For all cities, we asked policing agencies to provide index crime data with incident location fields for geocoding based on UCR violent offenses (murder/manslaughter, rape, robbery, aggravated assault) and property offenses (burglary, larceny/theft, and motor vehicle theft). The crime categories used in this study are similar, but not identical to, the index crime categories used in the UCR. The BPD incident data we received were collected internally for Compstat, crime analysis, and other administrative purposes. The definitions of particular crime categories can vary slightly in accordance with Massachusetts State legal definitions and, as such, vary from what is requested by the FBI through the UCR program.

**Table 4: UCR Violent and Property Crime Rates, 2010 (Boston)**

| Agency | Population | Violent Crime rate | Property crime rate |
|---|---|---|---|
| Boston Police Department | 617,594 | 942 | 3,340 |

*Notes: Rates are the number of reported offenses per 100,000 population.*
*Sources: Compiled using UCR Data Online Tool. Date of download: Dec 06, 2013. FBI, Uniform Crime Reports, prepared by the National Archive of Criminal Justice Data*

The crime data for the city of Boston for this project were provided by the Boston Police Department (BPD).[14] The data provided by the BPD had already been cleaned and geocoded internally by the police department and included all index (violent and property) crime reported

[14] We would like to thank the Boston Police Department and, in particular, former Police Commissioner Edward F. Davis, Chief of Staff Sharon Hanson, David Carabin, Rich Laird, and James Portolese for providing the official Boston Police Department crime incident data used in these analyses.

between 2000 and 2010. This data file needed no additional work and was included in its entirety in the analyses reported throughout this final report.

## New Jersey

### *New Jersey State Parole Board*

The New Jersey State Parole Board (NJSPB) members and staff conduct more than 20,000 parole hearings per year, and use input from victims to decide parole matters. According to NJSPB reports, in 2012, 3,793 state inmates were granted parole and 1,600 county inmates were granted parole. The Parole Board also held 2,368 revocation hearings (for both adult and juvenile inmates), and 2,000 of those hearings resulted in revocation. In addition to holding parole decision and revocation hearings, there are a number of community programs that work in conjunction with the parole board to provide transitional and rehabilitative assistance to parolees including, Stages to Enhance Parolee Success (STEPS), Reentry Substance Abuse Program (RESAP), Community Resource Centers (CRC), the Mutual Agreement Program (MAP) and others.

The New Jersey Parole Board provided electronic records of addresses of people released from prison and received in the state parole system for the years 1999-2010.[15] Although New Jersey State Parole has data on all prisoners received in the state prison system, the Parole Board could not verify the reliability of address data for those who had "maxed out" of prison. The New Jersey parole data were therefore used for only parole releases.

### *New Jersey Department of Corrections*

The New Jersey Department of Corrections (NJDOC) is comprised of state correctional institutions, county jails, and halfway houses. There are thirteen major correctional institutions that are overseen by the Department of Corrections: seven adult male facilities, one female

[15] We would like to thank New Jersey State Parole and, in particular, Leonard Ward and Michael Ostermann for providing the official parole data used in these analyses.

facility, one sex offender facility, and a central reception/intake unit (New Jersey Department of Corrections, 2011).

The New Jersey Department of Corrections provided electronic records by year for all prisoners admitted to prison between 2000 and 2010 and for all prisoners released from prison between 2000 and 2010.[16] The addresses in the datafiles provided were self-reported by the inmates at the time of admission or release, or were retrieved from court documents. The New Jersey State Parole Board (NJSPB) also provided data for this project. NJSPB data include a type of release indicator allowing us to distinguish parole releases from DOC max-out releases. The NJSPB data were used for all New Jersey release analyses reported in this final report.

Prior to sending a file for geocoding, data with missing or unusable addresses were removed. Of the 144,016 admission records received from the NJDOC for 2001-2010, 137,227 (or 95% of all records) had usable data in the address field and were sent for geocoding. Of the 120,171 release records from the NJSPB for 2001-2010, 79,000 (or 66% of all records provided) were records for paroled offenders and all of these records were sent to Justice Mapping for geocoding.[17]

Not all of the data sent to Justice Mapping could be geocoded (descriptions of the geocoding process and the types of matches that could result is included in a section below).[18] For the NJ DOC data, Justice Mapping was able to successfully geocode 111,499 admission records for 2001 through 2010. In other words, from original data to geocoded data for NJ DOC admissions data for 2001-2010, our data retention rate was approximately 77%

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[16] Although we requested an additional two years of data (for 2011 and 2012) to allow for the extension of the panel for Newark and Trenton (as both police agencies could only provide more recent data but could do so up to the present date), the NJDOC would not allow that request to be submitted as an addendum instead requiring (1) that it be treated as a new request (due to changes in the composition of their Research Review Board) and (2) that we begin the entire data request review process all over again. Because the review process for our first request to the NJDOC took just under a year to complete, we concluded that there was not enough time remaining in the term of our project to go through this lengthy process a second time.

[17] The other 41,171 cases represented max-outs, and although address data were present for many of these offenders, NJSPB had not verified release addresses for offenders unless they were released to parole. These data were therefore excluded, and analyses on New Jersey releases were restricted to those who had been released to parole supervision. Later in the report, we discuss the possible implications of using parole only data for estimating the effects of releases.

[18] Refer to Table 2 for a comprehensive report on the numbers of cases by match code from geocoding (for all records sent to Justice Mapping).

(111,499/144,016). From original data to geocoded data for NJSPB releases, our data retention rate was approximately 81% (64,362/79,000).

The NJDOC provided *state-wide* data: data for offenders admitted from Newark, Trenton, or rural addresses were then isolated after geocoding. In total, 4,727 offenders were admitted from Newark addresses over the 2007-2010 period and 923 offenders were admitted from Trenton addresses over the 2009-2010 period. In other words, among those for whom we could geocode an address, roughly 11% (4,727/41,687) were admitted to DOC custody from Newark addresses and approximately 5% (923/19,689) were admitted to DOC custody from a Trenton address. These admissions to DOC facilities to Newark and Trenton addresses were the only data ultimately included in the analyses.

The NJSPB also provided *state-wide* data: data for offenders released to Newark, Trenton, or rural addresses were then isolated after geocoding. In total, 3,285 offenders were admitted from Newark addresses over the 2007-2010 period and 557 offenders were released to Trenton addresses over the 2009-2010 period. In other words, among those for whom we could geocode an address, roughly 13% (3,285/26,300) were released by parole to a Newark addresses and approximately 5% (557/12,137) were released by parole to a Trenton address. These parole releases to Newark and Trenton addresses were the only data ultimately included in the analyses.

Table 5 below reports on overall data declination for the complete NJ DOC files from 2001-2010, as well as final resulting Newark and Trenton data counts for the relevant years included in the analyses. We have far more years of data than we were able to use as we needed to match the correctional data to the crime data and the police departments could typically only provide very recent data (2007-2010 for Newark, and 2009-2010 for Trenton).

## Table 5: New Jersey Correctional Data Declination

**New Jersey Department of Corrections Admissions**

| | Total Cases | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Original data files | 144,016 | 15,516 | 15,866 | 15,454 | 15,219 | 14,980 | 14,644 | 14,370 | 13,416 | 12,373 | 12,178 |
| *After removing unusable addresses* | *137,227* | *14,237* | *14,975* | *14,788* | *14,450* | *14,241* | *14,083* | *13,879* | *12,918* | *11,942* | *11,714* |
| Geocoded cases | 111,499 | 10,998 | 11,936 | 11,973 | 11,857 | 11,597 | 11,451 | 11,330 | 10,668 | 9,853 | 9,836 |
| **Admitted from Newark addresses** | **4,727** | | | | | | | **1,307** | **1,195** | **1,153** | **1,072** |
| **Admitted from Trenton addresses** | **923** | | | | | | | | | **411** | **512** |

**New Jersey Parole Releases***

| | Total Cases | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Original data files | 120,171 | 14,672 | 12,968 | 12,331 | 12,703 | 12,277 | 12,553 | 10,489 | 10,170 | 11,071 | 10,937 |
| *Max-outs (excluded)* | *41,171* | *5,197* | *4,612* | *4,511* | *4,487* | *4,584* | *4,591* | *2,109* | *2,104* | *4,460* | *4,516* |
| *Parole Releases* | *79,000* | *9,475* | *8,356* | *7,820* | *8,216* | *7,693* | *7,962* | *8,380* | *8,066* | *6,611* | *6,421* |
| *After removing unusable addresses* | *79,000* | *9,475* | *8,356* | *7,820* | *8,216* | *7,693* | *7,962* | *8,380* | *8,066* | *6,611* | *6,421* |
| Geocoded Cases | 64,362 | 7,888 | 6,428 | 5,640 | 5,784 | 5,906 | 6,416 | 6,697 | 7,466 | 7,060 | 5,077 |
| **Release to Newark Addresses** | **3,285** | | | | | | | **817** | **924** | **892** | **652** |
| **Released to Trenton Addresses** | **557** | | | | | | | | | **287** | **270** |

*Note: New Jersey Parole also provided data for 2000, 2011, and 2012. Because we did not have corresponding DOC admission data for these years, we have not included them in this table. These data were cleaned and geocoded.

### *Newark Police Department*

The Newark Police Department (NPD) serves the city of Newark, New Jersey. Newark Police Department is comprised of four precincts. In 2011, there were reported to be 1,095 sworn police officers in the department. The Police Director is the highest-ranking employee of the department. Directly under the Police Director are the Deputy Police Director, Chief of Staff, and the Chief of Police – each in charge of several other divisions. The Chief of Police oversees the Support Services Bureau, the Detective Bureau, and the Operations Bureau (which is composed of the patrol units in the four precincts). Also in 2011, 3,360 violent crime incidents were reported to NPD and 11,152 nonviolent (property) crime incidents were reported (State of New Jersey Division of State Police, 2012).

Table 6 reports provides violent and property crime rates for Newark reported to the FBI through the Uniform Crime Reports. As described above, we asked policing agencies to provide geocodable crime data based on UCR violent offenses (murder/manslaughter, rape, robbery, aggravated assault) and property offenses (burglary, larceny/theft, and motor vehicle theft).[19] The data we received were collected internally by the Newark Police Department for Comstat and usually at the incident level. Their definitions of crime categories varied slightly from what is requested by the FBI through the UCR program. For Comstat reporting purposes, the Newark Police Department uses definitions of offenses derived from the New Jersey Penal Code.

**Table 6: UCR Violent and Property Crime Rates, 2010 (Newark, NJ)**

| Agency | Population | Violent Crime rate | Property crime rate |
|---|---|---|---|
| Newark Police Department | 277,140 | 1,041 | 3,323 |

*Notes: Rates are the number of reported offenses per 100,000 population.*
*Sources: Compiled using UCR Data Online Tool. Date of download: Dec 06, 2013. FBI, Uniform Crime Reports, prepared by the National Archive of Criminal Justice Data*

[19] We would like to thank the Newark Police Department, and Eric Piza in particular for his assistance in compiling and assembling (and reassembling) the data for this project.

The Newark Police Department ultimately provided data for all property and violent crimes reported between 2007 and 2011.[20] These data were geo-coded by the Newark police department and were then used to calculate neighborhood-specific crime rates for all neighborhoods within Newark's city limits (see Table 9 for a detailed data declination summary).

### *The Trenton Police Department*

The Trenton Police Department serves the city of Trenton, New Jersey. The Trenton Police Department recently terminated its use of the districts approach, so it is no longer broken up into separate districts. The department was consisted of 238 sworn police officers in 2011. In that same year, there were 1,211 violent crime incidents reported and 2,683 nonviolent property crime incidents reported (State of New Jersey Division of State Police, 2012). Table 7 provides violent and property crime rates for Trenton reported to the FBI through the Uniform Crime Reports.

**Table 7: UCR Violent and Property Crime Rates, 2010 (Trenton, NJ)**

| Agency | Population | Violent Crime rate | Property crime rate |
|---|---|---|---|
| Trenton Police Department | 84,913 | 1,411 | 2,963 |

*Notes: Rates are the number of reported offenses per 100,000 population.*
*Sources: Compiled using UCR Data Online Tool. Date of download: Dec 06, 2013. FBI, Uniform Crime Reports, prepared by the National Archive of Criminal Justice Data*

The Trenton Police Department ultimately provided data for all property and violent crimes reported between 2009 and 2011.[21] These data were geo-coded by Justice Mapping and were then used to calculate neighborhood-specific crime rates for all neighborhoods within Trenton's city limits (see Table 9 for a detailed data declination summary).

[20] The Newark Police Department initially provided data for reported crime between 1999 and 2010, but it was later determined that the data provided for 1999-2006 were not comparable to the data for 2007-2011 and in some years excluded large numbers of reported offenses (auto-theft for example), so we ultimately concluded that it was best to use the more recent, more accurate data and included only the data for 2007-2011.

[21] We would like to thank the Trenton Police Department, and Frank Korchick in particular, for assistance with providing and compiling the data used in this project.

### *New Jersey State Police*

New Jersey offered an interesting opportunity to look at the impact of prison cycling in rural areas. Several hundred local municipal police departments serve incorporated areas in New Jersey, but there remain some 89 municipalities for which there are no general police services (see Table 8). In these more rural areas, general police services are provided by the New Jersey State Police.[22] While some of these more rural communities are quite wealthy, pockets (particularly in the Northwest and Southern part of the state) suffer from entrenched poverty, not unlike that which is seen in inner-cities. These rural New Jersey communities experience substantially higher suicide rates than other areas of the state (Frassinelli, 2008). We were interested in whether the processes of prison cycling work the same way in these rural areas as they do in the urban areas.

The New Jersey State Police provided statewide data on reported violent and property crime.[23] We then geocoded these data and include only crimes that occurred in one of the unincorporated (rural) municipalities in New Jersey.

[22] The New Jersey State Police (NJSP) have statewide law enforcement jurisdiction, provide support to other law enforcement agencies, if requested, and are also responsible the protection of for state, national, or international officials while they are in the state. Additionally, the New Jersey State Police oversees and patrols the New Jersey highways (State of New Jersey Division of State Police, 2012).

[23] We would like to thank the Special Projects Unit of the New Jersey State Police, and Superintendent Colonel Rick Fuentes, Captain Kevin Dunn, and Sargent Algiri, in particular, for their assistance with providing and compiling the data used in this project.

### Table 8: Rural Municipalities in New Jersey

Alexandria Township
Allamuchy Township
Alloway Township
Andover Borough
Bass River Township
Bethlehem Township
Blairstown Township
Bloomsbury Borough
Branchville Borough
Buena Vista Township
Chesterfield Township
Commercial Township
Corbin City
Deerfield Township
Dennis Township
Downe Township
Eagleswood Township
East Amwell Township
Elmer Borough
Estell Manor City
Fairfield Township
Farmingdale Borough
Fieldsboro Borough
Folsom Borough
Frankford Township
Franklin Township
Fredon Township
Frelinghuysen Township
Frenchtown Borough
Glen Gardner Borough
Green Township
Greenwich Township
Hainesport Township
Hampton Borough
Hampton Township
Hardwick Township
Harmony Township
Holland Township
Hope Township
Hopewell Township
Kingwood Township
Knowlton Township
Lafayette Township
Lawrence Township
Lebanon Borough
Liberty Township
Mannington Township
Mansfield Township
Maurice River Township
Milford Borough
Millstone Borough
Millstone Township
Montague Township
New Hanover Township
North Hanover Township
Oldmans Township
Oxford Township
Pemberton Borough
Pilesgrove Township
Pittsgrove Township
Port Republic City
Quinton Township
Rocky Hill Borough
Roosevelt Borough
Sandyston Township
Shamong Township
Shiloh Borough
Shrewsbury Township
South Harrison Township
Southampton Township
Springfield Township
Stockton Borough
Stow Creek Township
Sussex Borough
Tabernacle Township
Union Township
Upper Deerfield Township
Upper Freehold Township
Upper Pittsgrove Township
Upper Township
Victory Gardens Borough
Walpack Township
Wantage Township
Washington Township
Weymouth Township
White Township
Woodbine Borough
Woodland Township
Wrightstown Borough

## Table 9: New Jersey Crime Data Declination

**Newark Police Department: Reported Index Crime**

| | Total | 2007 | 2008 | 2009 | 2010 | 2011 |
|---|---|---|---|---|---|---|
| Original data files | 64,223 | 12,760 | 12,792 | 11,651 | 12,898 | 14,122 |
| *After removing blank addresses* | *63,613* | *12,662* | *12,741* | *11,606* | *12,677* | *13,927* |
| Geocoded Cases | 63,613 | 12,662 | 12,741 | 11,606 | 12,677 | 13,927 |
| **Included in Analyses** | **48,438** | **12,200** | **12,530** | **11,314** | **12,394** | |
| ***Nonviolent Index Crimes*** | ***37,890*** | ***9,837*** | ***9,821*** | ***8,754*** | ***9,478*** | |
| ***Violent Index Crimes*** | ***10,548*** | ***2,363*** | ***2,709*** | ***2,560*** | ***2,916*** | |

**Trenton Police Department: Reported Index Crime**

| | Total | 2009 | 2010 | 2011 | 2012 |
|---|---|---|---|---|---|
| Original data files | 13,444 | 3,376 | 3,711 | 3,979 | 2,378 |
| *Submitted for geocoding* | *13,444* | *3,376* | *3,711* | *3,979* | *2,378* |
| Geocoded Cases | 11,610 | 2,888 | 3,207 | 3,456 | 2,059 |
| **Included in Analyses** | **6,095** | **2,888** | **3,207** | | |

**New Jersey State Police: Reported Crime**

| | Total | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|---|---|---|---|---|
| Original data files | 220,560 | 16,169 | 18,257 | 19,247 | 26,068 | 29,076 | 28,780 | 27,205 | 27,915 |
| *After removing blank addresses* | *197,533* | *14,350* | *16,164* | *16,862* | *23,474* | *25,962* | *26,038* | *24,336* | *24,966* |
| *After removing cases not in NJSP jurisdiction* | *141,835* | *13,409* | *15,050* | *15,474* | *16,630* | *15,726* | *16,378* | *15,732* | *16,423* |
| Submitted for geocoding | 141,835 | 13,409 | 15,050 | 15,474 | 16,630 | 15,726 | 16,378 | 15,732 | 16,423 |
| Geocoded cases | 56,536 | 5,684 | 6,136 | 6,218 | 6,289 | 6,144 | 6,510 | 6,198 | 6,591 |
| **Included in analyses** | **49,770** | **5,684** | **6,136** | **6,218** | **6,289** | **6,144** | **6,510** | **6,198** | **6,591** |

## The Research Process

In order to conduct this research, we set out to accomplish two explicit tasks.[24] The first task involved collecting the data from relevant agencies and assembling a comprehensive data set that would enable us to build a foundation for the analytic tasks. The second task, modeling prison cycling effects on crime, was to help us understand the nature of the impact of imprisonment flow on crime rates in areas, and particularly in those areas with high rates of flow.

- Task 1. Build a crime/incarceration time series dataset for Newark, New Jersey and Boston, Massachusetts.

To accomplish this task, we sought to create a ten-year incarceration (removals and returns) and crime time series dataset spanning from 2001-2010. Assembling panel data of this type would then allow the use of fixed effect models and provide sufficient data points to enable us to investigate potential spatial non-independence. We had thought that creation of this dataset would be facilitated greatly by two facts. First, we had been under the impression that parole files with addresses at prison admission and addresses for prison release were maintained electronically and are available back to the late 1990s. Second, we were quite confident that crime reports to the police identifying the offense (for index offenses), the date of offense, and the location at which the offense occurred would have already been maintained by police departments based on the reporting requirements of the UCR.

The incarceration portion of the dataset was to include: (1) address at time of admission to prison; (2) address at time of release from prison; (3) crime for which the person was sentenced; (4) length of sentence; and (5) length of stay. If we had been able to include detailed offense related data, we would have been able to develop a better understanding of the potentially differential impact of cycling people involved in different types of offending into and

[24] We initially proposed three tasks, but given data limitations, we were only able to accomplish two of them. The third task in the original proposal involved testing a series of hypotheses regarding the impact of the characteristics of those who are removed and returned on rates of crime. We had hoped to conduct a series of analyses to see how the crime and sentence information influenced rates of crime at the local community level, which would have enabled us to address such questions as whether removing serious offenders also have the same unintended consequences as removing less serious offenders. When we wrote the initial proposal, we were not expecting the data collection process to be as arduous and unwieldy as it became. Although some of our correctional datasets included offense and sentence data, these were inconsistently provided and in some instances could not be accurately merged (because of a lack of a unique identifier across datasets).

out of communities. For example, one could speculate that churning large numbers of drug offenders into and out of prisons might have a different impact on crime in the community than churning primarily property offenders. Moreover, one would expect that churning property offenders might have a larger impact on property crime than it would on violent crime. The result would have been a time-ordered dataset that included horizontal and vertical data, promoting a more detailed analysis of incarceration's effects across urban locations in Massachusetts and New Jersey.

On both accounts, our expectations exceeded the reality of what we would be able to acquire for this project. Both parole agencies that we worked with (Massachusetts and New Jersey) had statewide data on admissions and releases to correctional facilities and these data tended to include quite detailed offense-related variables, but each could only provide reliable release address data for those released to parole and neither could provide admission address data. As a result, we had to establish relationships with state and local correctional facilities whose data were not always as comprehensive or as complete as the parole agencies.[25] For example, while (at least in theory) parole verifies addresses, the Departments of Correction and the county correctional facility that we acquired data from relied on self-report data on admission and (sometimes) at release. Moreover, the release data from the New Jersey Department of Correction did not include a city field, and very few of those addresses included zipcodes, so we were not able to use these data. For New Jersey releases, we had to use only Parole data and theses data only included those actually released to parole (excluding all max outs). This is only an issue if there is a good reason to believe that the patterns of releases for paroled offenders are systematically different from the patterns of releases for offenders who max-out. Although we had no reason to believe this would be the case, we took advantage of having both types of data in Massachusetts and ran the models for Boston using all releases and using parole only releases.

[25] As explained above, we also secured prison release data from the Massachusetts Parole board. Although the Parole Board maintains address data on only those who are ultimately released to parole, we collected these data because the parole board verifies addresses at release and we harbored a lingering concern that the release addresses collected by the Department of Corrections and the House of Correction might not be particularly accurate. In Massachusetts, we were able to take advantage of having address data for *all* releases (from the MA DOC and the SHOC) and for *parole only* releases (from the MPB). As demonstrated in a section that follows, the results of the analyses using parole release data were substantially the same as those using MADOC and SHOC data, and given the latter provide a more complete picture of returns, we have used these data in all analyses reported here.

The results of those analyses of parole only releases, presented in a section below, were substantially the same as the results of the analyses for all offenders (discussed in all models above). We therefore felt more confident in our New Jersey results that included only parolee data for releases.

Other than the Boston police department, which had clean geocoded data spanning the entire decade, the police departments we worked with tended to have only recently begun maintaining electronic data that included each of the elements we needed. These departments could provide UCR summary data that had been submitted but these data did not include the individual level address detail we needed to map crime.

- Task 2. Analyze the impact of incarceration on crime using alternative modeling strategies

For our second major task, we proposed to address the modeling challenges of simultaneity and spatial non-independence by incorporating the data assembled in a series of models analyzing the role of prison cycling on overall neighborhood-specific crime rates. Specifically, we developed panel models assessing the impact of prison cycling on the rate of crime as well as on rates of violent and property crime separately. Recognizing that spatial autocorrelation is often a concern in this type of work, we employed fixed effects models that use within neighborhood variation to estimate the coefficients, plus we investigated neighborhood specific time trends. As explained in more detail below, the effect of correlation across neighborhoods is taken care of by the neighborhood specific time trends. Then, we cluster the standard errors on the census tract, which accounts for autocorrelation within the neighborhood.

Quarterly and annual models were constructed for two of the cities (Newark, New Jersey and Boston, Massachusetts). This enabled us to see if the effects are immediate, lagged at less than annual levels, or essentially large scale (and thus felt across 12-month intervals). The comparison of these models provided a basis for understanding the sensitivity of the incarceration-crime connection to model specification. It also allowed us to provide good estimates of the central problem at hand: what is the separate impact of removal to prison, on the one hand, and reentry from prison, on the other, on general and specific rates of crime. Ultimately, as discussed below, we settled on quarterly data whenever possible, as we believe that many of the simultaneity issues frequently discussed in the literature on neighborhood crime rates are not present in models that high periodicity data.

The data assembled for and used in this project are panel data. The data are quarterly or annual data, depending on the location, from a mix of urban (Boston, Newark and Trenton) and rural communities in New Jersey covering various years between 2000 and 2012. Census tract characteristics come from the 2000 Census Summary File 3. The Summary File 3 is a collection of tabulations down to the Block level provided by the Census Bureau. It is the only publicly available source of Census data for Census tracts.

The crime, release, and admission data are then aggregated from the individual incident level to the census tract level by quarter (in Boston and Newark) or year (in Trenton). This choice of the level of aggregation (census tract vs. block group) and time period (quarterly vs yearly) was driven by a desire to simultaneously balance three criteria. The first criterion was to find a small geographic area, ideally a block or block group, but a census tract is small enough to still be considered a neighborhood. The second criterion was to use a time frame short enough to minimize the simultaneity bias. Lastly, the data needed to have sufficient variation in releases and admissions across neighborhood by time-period. Ideally, we could have used smaller geographic areas and time periods as demanded by criteria one and two. But at the same time we needed to have sufficient variation, which in these data, the smaller geographic areas and time-periods do not have. Constrained by these three criteria we decided to use quarterly data at the census tract level (for all but Trenton). With only three years of data for Trenton, nothing is gained by using quarterly data.

## Analytic Strategy

While previous research has demonstrated that it is clear that prison cycling is a factor in local crime rates, it is not clear exactly how these rates affect neighborhood crime levels, and there are substantial modeling issues that needed to be addressed in developing an estimate of the effect. Testing the coercive mobility thesis presents a number of challenges involving specification and model assumptions. We address each of these in more detail in turn.

### Simultaneity

The major empirical issue that must be overcome is simultaneity. Holding the level of enforcement fixed, an increase in crime clearly "causes" an increase in incarceration. We posit that the reverse is also true, namely, that an increase in cycling—admissions to and releases from prison—also increases crime.

The problem of simultaneity is not easily resolved. There are two primary reasons the simultaneity bias exists. The first problem is an inability to control for all of the determinants of crime. If we could control for all of the determinants of crime with a fully developed causal model of crime, then the coefficient on cycling would likely not be biased by simultaneity. While we are able to control for the effects of a host of neighborhood-level and individual-level attributes measured at the neighborhood level, it is clear that even with these controls we are far short of a full causal model of crime. However, even if we could control for all determinants of crime, simultaneity may still exist due to the timing of data collection. Data aggregated over long time periods, for example a year, likely include people who both committed a crime and were incarcerated in the same time period. This too leads to a simultaneity bias, as it means that an increase in incarceration is related to an increase in crime by definition.

Thus, we need an alternative approach to the causal model that takes account of simultaneity in a non-causal manner, in this case, both by aggregating the data over short time periods (a quarter), and by employing estimation techniques not typically used for neighborhood level analysis of crime in Criminology. There are two general options for estimating the effect of cycling on crime. One may either utilize an estimation technique that removes the simultaneity, or one may employ a technique that uses the simultaneity to its benefit. We propose to estimate two types of models in which simultaneity is removed in order to uncover underlying effects, and two types in which it is employed in the model to estimate effects. We suggest that the strength of our overall design is this triangulation of approaches—using multiple alternative methods in addition to substantially improved data sources to estimate the effects of prison cycling on crime should lead to a better understanding of the actual nature of the relationship.

### *Two approaches to removing simultaneity*

***Fixed-effects models.*** When using panel-data, the most common technique for removing simultaneity due to unobservable covariates in most contexts is fixed-effects. The fixed-effect allows each unit of observation to have its own mean, or intercept, which is explicitly estimated. The fixed effect absorbs the impact of all factors that make one neighborhood different from another neighborhood, on average. What distinguishes the fixed effects model from the more commonly used random effects model is the fact that the fixed effects model allows for any arbitrary correlation between the variables included in the model and the fixed effect (i.e. the intercept), whereas random effects models at the very least require that the correlation between the intercept and the variables is known.

This issue will be discussed in more detail below, but before proceeding a note on terminology is warranted. For the purposes of this discussion, we consider a random effects model to be any model that uses group specific random intercepts, as opposed to the group specific fixed intercepts used in a fixed effects model. Importantly, this means that the very large majority of hierarchical linear and other mixed effects models are random effects models because, although they frequently include a fixed overall intercept, they almost invariably use only random group specific intercepts.

Fixed-effects only remove simultaneity due to the presence of unmeasurable, time-invariant neighborhood specific characteristics that may cause both crime and cycling. Because cycling and crime evolve through time, there are likely unmeasurable characteristics associated with both cycling and crime that also evolve through time. To capture these characteristics, a neighborhood specific time-trend is added. The time-trend captures all unmeasurable characteristics of the neighborhoods that change through time. By making it neighborhood specific, we allow each neighborhood to have its own time-path.

Second, we used lags of cycling rather than concurrent cycling. That is, rather than estimating a structural equation regressing crime in time period t on cycling in time period t, we estimated crime in time period t on cycling in periods t-1, t-2,...,t-n. Because it is impossible for someone to be incarcerated for a crime in period t-1 that was committed in period t, the second component of the simultaneity bias is removed.

It should be noted that the inclusion of both fixed-effects and a neighborhood specific time trend changed the source of identifying variation for the estimated coefficient on cycling. Without fixed effects or a time-trend, the source of identifying variation comes from both variation across neighborhoods in a given year, and across years within a neighborhood. The inclusion of a fixed effect absorbs all variation across neighborhoods in a given year, changing the implicit question from "is there an overall relationship between cycling and crime" to "does a deviation in cycling from the neighborhood mean cause crime in that neighborhood to deviate from its mean?" Without fixed effects, the coefficient on cycling captures both the hypothesized causal relationship between cycling and crime, and the mean differences across neighborhoods in both crime and cycling.

However, this still leaves the possibility that different neighborhoods are on fundamentally different trajectories, which could result in a common trend for cycling and crime, but has nothing to do with a causal relationship running from cycling to crime. The neighborhood specific time-trend captures this. It changes the implicit question from "does a deviation in cycling from the neighborhood mean cause crime in that neighborhood to deviate from its mean?" to "does a deviation in cycling from the overall neighborhood trend—including both the mean and time-path—cause crime to deviate from its overall trend?" This is a very rigorous test of the hypothesis because the combination of the fixed-effect and time-trend will absorb a tremendous amount of variation in the dependent variable (crime rates).

***Instrumented models.*** The second of the proposed estimation strategies, also aimed at removing simultaneity bias, is an instrumental variables approach, in which we include in the model an instrumental variable that is strongly correlated with cycling, but not correlated with the error term in the regression for crime—that is, the instrument is uncorrelated with the unmeasurable determinants of crime that are captured by the error term. The intuition here is that, if one can find a variable that causes cycling but has no direct effect on crime, then one can use variation in this variable to cause exogenous "shocks" to cycling.

Two measures have been used previously as instruments for the crime-incarceration problem. Lynch and Sabol (2004) used the residual from a regression model for drug arrests as an instrument in their study of incarceration and crime in Baltimore. Defina and Hannon (2009) used the proportion of the incarcerated population that is female as a proxy variable for these criminal justice dynamics and an index of "conservativeness" of state government on a national-level time series of state incarceration rates. These authors argue that their instruments meet the criteria for instrumentation, namely that they satisfy two basic conditions: the instrument is strongly related to the incarceration rate, and yet is uncorrelated with the error in the structural model of crime.

We did not use instrumental variable techniques because, in our opinion, we have not yet found a good instrument that varies across neighborhoods and time within a city. We further do not think the instruments used to date are actually good instruments. Specifically, an instrument must have only one possible channel of effect on crime, through the variable that is instrumented. If there is any other way that the instrument can determine the dependent variable (Spelman's instruments), or if the instrument affects the dependent variable in a fundamentally different way than the variable being instrumented (Levitt's instrument), then it is not a good instrument. We believe this to be the case with the instruments used in Lynch and Sabol (2004) and Defina and Hannon (2009) as well.

But we have been able to make use of recently developed econometric methods for producing quantitative instruments when theoretically driven instruments are unavailable. An alternative method for instrumentation is to use predicted cycling instead of actual cycling. The most widely accepted modern incarnation of this technique for panel data is the Arellano-Bond dynamic estimation technique (Arellano and Bond, 1991). The Arellano-Bond technique uses lagged values of the endogenous variable as instruments for the endogenous variable. Thus, we use lagged values of cycling as instruments for cycling in period t. The advantage of this technique is that we do not need to search for a valid instrument, the instrument is itself a product of the estimation technique. Second, because lagged values of the endogenous variable are not correlated with the error term in period t, we know that it meets the criteria for serving as a valid instrument (Cameron and Trivedi, 2005). Third, it allows us to include both fixed effects and lagged values of the dependent variable without introducing Nickell bias.[26]

[26] Although it is frequently done, lagged values of the dependent variable cannot be used at the same time as fixed effects because the lagged dependent variable is correlated with the error term when fixed effects are present, biasing the coefficients. This is referred to as Nickell bias.

There are several disadvantages to this technique which drive us to ultimately lean most heavily on the OLS results. First, as with instrumental variables more generally, is that if lagged values of cycling are not a good predictor of current cycling the resulting standard errors will be large. Second, if the variables being instrumented have a high degree of serial correlation, then the lags will also be correlated with the error term, resulting in an instrument that is not exogenous. Finally, Arellano-Bond regressions are notoriously unstable. It is not uncommon for small changes in specification to result in large changes in estimated coefficients and/or standard errors.

### *Two approaches retaining simultaneity*

***Vector Auto-Regression***. An alternative strategy to instrumentation is Vector Auto-Regression (VAR) (Enders, 2003; Stock & Watson, 2001). VAR is a technique designed to be used specifically in situations where there are two or more simultaneously determined variables, and the causal relationship between the two is needed. Thus, given the hypothesis, it is the natural estimation strategy to turn to. In other words, VAR is an estimation technique that does not seek to remove the simultaneity, but instead uses the simultaneity to estimate the coefficients in question.

VAR is a simultaneous equations technique. In this context, there is one equation for crime, one equation for releases, and one equation for admissions. Each equation models the dependent variable as a function of lags of the dependent variable of that equation, plus lags of the dependent variables of the other two equations. The equations are then estimated simultaneously using maximum likelihood techniques. Unfortunately, because panel VAR methods are still very new, there is not currently an accepted method for including exogenous explanatory variables in the model.

There are two main advantages to VAR for this analysis. The first is that, as stated, there is no need to remove the simultaneity between crime and cycling because VAR uses this feature to estimate the coefficients. Second, it provides an important validity check for the data. If the fixed-effects with time-trend regression shows a positive causal relationship running from cycling to crime, one possible reason for its existence is that the positive bias resulting from simultaneity issues is still present. VAR allows an assessment of the extent of reverse causality.

This second advantage of VAR is referred to as "Granger Causality," which requires some special modifications for panel data (Love & Zicchino, 2006). At its essence, Granger Causality is a test of whether one variable tends to change chronologically before another variable (Granger, 1969). For example, if cycling tends to increase before crime (i.e. is statistically significant in a regression of crime on lags of cycling and lags of crime), but crime does not tend to increase before cycling (i.e. crime is insignificant in a regression of cycling on lags of crime and cycling), then cycling Granger-causes crime. If cycling increases before crime, and crime increases before cycling, than we have mutual causation, and so on. The test itself is essentially a series of modified Chow tests of the joint significance of the lagged values of crime and cycling in each equation.

There is, however, one major obstacle to using VAR, called the unit-root problem. A variable has a unit root when there is no tendency for that variable to return to a fixed mean; it has a unit root with float if it has no tendency to return to a fixed trend. Formally, a variable has a unit root when the random error from period t-1 passes fully into period t. The problem this creates is fairly straightforward. Assume that crime and cycling both experience a random, but unrelated, shock in period t (for example, a prison is southern New Jersey decides to release all of its inmates in Newark, and Newark has a police strike). This would result in a simultaneous increase in both cycling and crime in Newark. But, the relationship is spurious; the positive correlation is not due to the causal impact of the release of inmates in Newark. The test for this is called an augmented Dickey-Fuller test.

The ideal situation is if either both crime and cycling do not have a unit root, or they both have unit roots. If neither have a unit root, than VAR can be used exactly how it is described above. If they both have unit roots, then two strategies can be employed. The first strategy is to difference both crime and cycling, meaning to subtract crime in period t-1 from crime in period t, and the same for cycling. Then, the effect of a change in cycling (crime) on the change in crime (cycling) is estimated.

The alternative is to estimate an error correction model, and test for co-integration (Engle & Granger, 1987). An error correction model estimates whether two random variables (variables with unit roots) tend to return to a common trend. They may periodically deviate from each other, but eventually they always return. If this is true, then they are co-integrated. A good analogy is two children tied together with a rubber band. They can do all they want to try and run away from each other, but the force of the rubber band will always pull them back together. It turns out that, according to the unit-root tests for panel data, our data do not have a unit root problem and VAR can be estimated.

***Selecting a theoretically clean measure of crime as an outcome.*** A final approach to this problem is to model the impact of adult incarceration on juvenile arrests rather than all crime. This approach is thought to be conceptually clean of the endogeneity problems of modeling adult incarceration and adult crime, because it is not plausible to think that juvenile arrests rates will cause an increase in adult incarcerations. This approach has been used in one recent study (Taylor et al, in press) producing results that indicate that the impact of incarceration rates on juvenile crime rates are time-dependent. Our data do not allow us to distinguish between crimes committed by juveniles and adults, respectively, so this approach is not an option for us.

## Addressing (serial and spatial) Autocorrelation

Simultaneity is not the only problem in work of this nature. Another common problem in empirical estimations of crime at the neighborhood level is autocorrelation, both within a neighborhood through time (i.e. serial correlation), and across neighborhoods that are geographically close to each other (i.e. spatial autocorrelation). These two issues each, separately, violate the independence assumption that makes inference possible from OLS regressions. However, the two types of correlation must be dealt with in separate ways.

To address serial correlation, we cluster the standard errors on the census tract. This procedure adjusts the standard errors to account for any arbitrary form of correlation within the census tract. Clustering on the census tract, however, assumes that the residuals are conditionally independent across clusters. Spatial correlation is absorbed by the neighborhood fixed effects and the year fixed effects. The use of neighborhood fixed effects forces the estimation of both coefficients and the standard errors to come from variation within the census tract, while the year fixed effects absorb all year to year variation that is common to all census tracts.

We experimented with multi-way clustering by clustering the standard errors on both the census tract and the year. This absorbs any arbitrary form of correlation both within the census tract through time and across census tracts at a given point in time, though the procedure had no meaningful impact on the standard errors. This strongly suggests that the tract and year fixed effects remove concerns about spatial autocorrelation, if they are present.

Of course, serial and spatial autocorrelation can also be addressed using random effects models. However, the primary reason for using fixed effects it to eliminate omitted variable bias, not autocorrelation. The fact that fixed effects also helps alleviate spatial autocorrelation is an incidental benefit of the model, but is not the reason for using the model in the first place.

### Summary

Our strategy, then, was one of analytic triangulation. Through the data collection associated with this project, we amassed a uniquely comprehensive crime and incarceration dataset over time – arguably one of the most comprehensive assembled to date. This dataset allowed us to model the relationship between crime and incarceration using a range of techniques, taking advantage of each and being partially freed of the limitations of any one.

## RESULTS

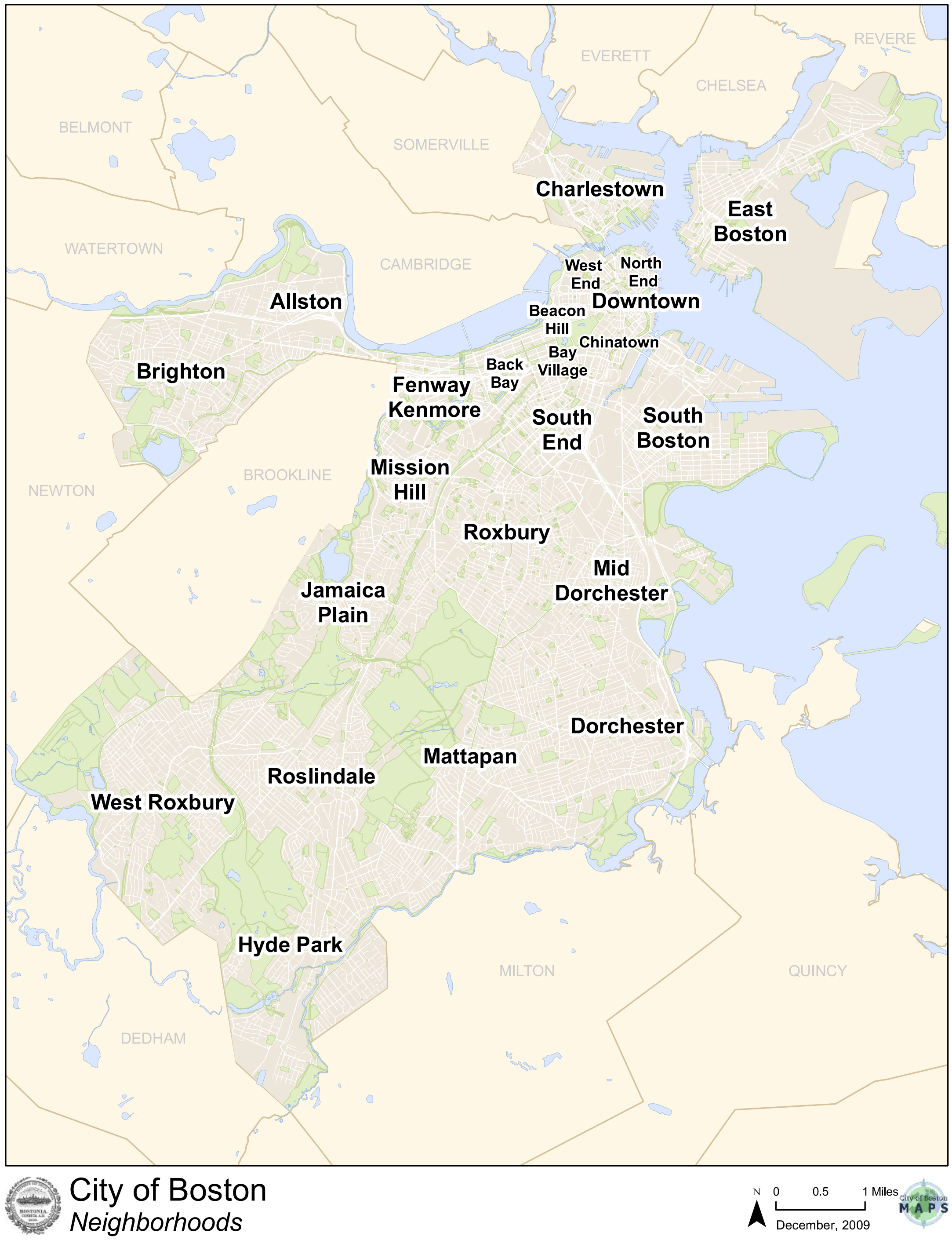
### Descriptives

We start with some basic descriptive statistics that help to frame some of the analyses that follow. Because so much of our project focused on mapping phenomena across places, we describe our places – and how the data map across those places – in some detail before turning to the more sophisticated analytic results that begin to explain some of what we are seeing.

### Boston, MA

In many ways, the city of Boston made for an ideal site in which to test the impact of rate of prison cycling on rates of crime in communities because Boston is very much a collection of relatively distinct and diverse communities. In fact, Boston characterizes itself as “a city of neighborhoods” each with “its own personality and distinct appeal” (www.cityofboston.gov/neighborhoods/). Figure 2 is a map of Boston neighborhoods provided on the City of Boston’s website that helps give a sense of place and an orientation to the crime, prison cycling, and concentrated disadvantage maps that follow.

## Figure 2: Boston Neighborhoods




**Map Source:** City of Boston. Available online: http://www.cityofboston.gov/neighborhoods/ (Last Accessed 12/04/2013)

Table 10 provides the descriptive statistics for the most recent year of data used for Boston. In this table, the data are quarterly census tract data for 2010 (the tracts themselves were based on the 2000 decennial census). We report only 2010 because it is the end year of data in all sites and gives a sense of how the sites compare to each other now. We report full panel descriptive statistics in later tables.

In the table below, total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Violent crime is the sum of homicide, rape, robbery, and aggravated assault. Nonviolent crime is the sum of burglary, larceny theft, and motor vehicle theft. Admissions and releases are sums of counts admissions and releases respectively for the tract over the quarter. The cycling variable sums the admissions and releases together over the quarter.

The data used to compile the concentrated disadvantage index were drawn from the 2000 decennial census. Based on the census tracts designations associated with the 2000 decennial census (which was used in this project for reasons described below), there were 156 census tracts across the city of Boston.[27] Concentrated disadvantage is an index that consists of the sum of four variables (the index has a theoretical range of 0-4): the share of households headed by a single parent in a tract, the tract poverty rate, the tract unemployment rate, and the share of the tract population that is black.[28] Residential Mobility is the share of the census tract that moved within last 5 years.

On average, there are approximately 156 crimes per census tract per year in Boston, the large majority of which are nonviolent crimes (122.5 nonviolent crimes per census tract per year versus 33.9 violent crimes). This translates into an average yearly total crime rate of 45.08, a violent crime rate of 9.88 and a nonviolent crime rate of 35.2 per 1,000 residents. There are an average of 6.15 admissions and 5.83 releases per tract – for rates of 1.83 and 1.76, respectively,

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[27] In 2000, there were technically 157 census tracts in Boston. However, that count includes the harbor islands where no one lives. There were 156 census tracts that actually had residents.

[28] We tried several different versions of the concentrated disadvantage index: the sum of shares, the sum of standardized shares, and an index based on a factor analysis. Ultimately, we chose the sum of shares because it allows for quantitative comparison of disadvantage across cities. However, it turns out that the choice of index construction does not matter much for either the qualitative or the quantitative results. The correlation between the three indexes within a city is about 0.95. We did not experiment with alternative variables in the index, though this is something we intend to do in the future.

per 1000.  The standard deviation of both the crime and cycling variables indicates the wide dispersion of the data.

**Table 10: Boston Descriptives**

| | Min | Max | Mean | Std. Dev. | *N* |
|---|---|---|---|---|---|
| Total Crime | 24.00 | 1088.00 | 156.46 | [126.82] | 156 |
| Violent Crime | 0.00 | 132.00 | 33.90 | [27.11] | 156 |
| Nonviolent Crime | 18.00 | 956.00 | 122.55 | [110.21] | 156 |
| Admissions | 0.00 | 33.00 | 6.15 | [6.16] | 156 |
| Releases | 0.00 | 49.00 | 5.83 | [7.42] | 156 |
| Total Crime Rate | 7.06 | 331.51 | 45.08 | [38.26] | 156 |
| Violent Crime Rate | 0.00 | 40.95 | 9.88 | [8.30] | 156 |
| Nonviolent Crime Rate | 5.08 | 291.29 | 35.20 | [32.96] | 156 |
| Admission Rate | 0.00 | 24.57 | 1.83 | [2.39] | 156 |
| Release Rate | 0.00 | 36.49 | 1.76 | [3.30] | 156 |
| Concentrated Disadvantage | 0.09 | 1.67 | 0.67 | [0.44] | 156 |
| Residential Mobility | 0.25 | 0.91 | 0.51 | [0.14] | 156 |

*Notes*: Based on 2010 crime and incarceration data and 2000 census data. The concentrated disadvantage index used 2000 decennial census data from Summary File 3 and includes the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Residential Mobility, also drawn from Summary File 3 for the 2000 census, is the share of the census tract that moved within last 5 years. Rates are measures per 1,000 residents.

In Boston, the concentrated disadvantage variable had a mean of .67 and a standard deviation of 0.44.  Given that concentrated disadvantage has a theoretical range of 0 to 4, a mean of 0.67 and a standard deviation of 0.44 indicates that Boston has relatively few high disadvantage neighborhoods (the low mean) and a relatively wide dispersion of the characteristics across tracts (the high standard deviation relative to the mean).

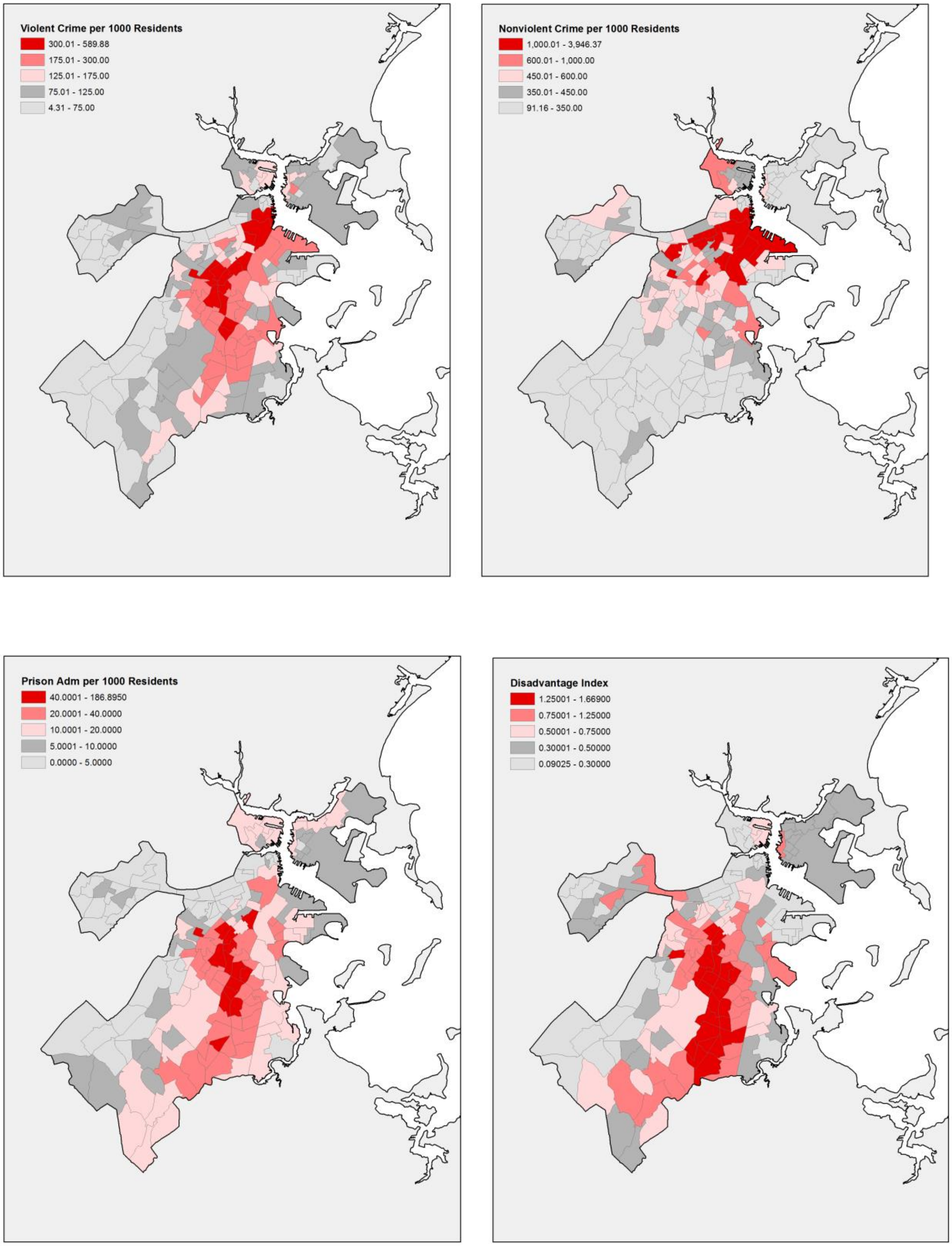
As noted above, residential mobility is measured by the share of the census tract that moved into the tract within the last five years.  The mean of 0.51 indicates the relatively high rate of neighborhood turnover in Boston, driven in part by the student population.

The maps in Figure 3 visually display the dispersion of crime (violent and nonviolent separately), prison admissions, and concentrated disadvantage across the city of Boston. Viewing these maps one after the other reveals some striking observations. First the dispersion of violent and nonviolent crime map somewhat differently from one another across the city. Violent crime concentrates in the corridor from north to south that runs from the Downtown section of the city down into Roxbury. The violent crime rate exceeds 300 violent crimes per 1,000 residents in the downtown section, through Chinatown and the South End, and into the Mission Hill, Roxbury, and the northernmost sections of Dorchester and Mattapan. By contrast, the primary hot-spots for nonviolent crime in Boston (where the nonviolent crime rate exceeds 1,000 per 1,000 residents) cluster in the Downtown, Back Bay, South End, and South Boston sections of the city. Second and perhaps more importantly, the map for prison admissions bear surprisingly little resemblance to the maps for violent and particularly non-violent crime. Prison admissions cluster heavily through the Roxbury, Dorchester, and Mattapan sections of the city with rates of prison admissions across all three communities typically exceeding 20 admissions per 1,000 residents. Perhaps most importantly, the map of prison admissions resembles far more closely the map of the dispersion of concentrated disadvantage than it does the maps of either violent or non-violent crime. The map for concentrated disadvantage shows that the areas suffering most from concentrated disadvantage (with index scores of 1.25 or greater) are all quite clearly within the boundaries of the Roxbury, Dorchester and Mattapan sections of the city.

The divergence in the distribution of prison admissions and crime rates across Boston neighborhoods can be explained, in part, by differences in daily ambient populations relative to the residential populations in the Downtown, Back Bay, and South End neighborhoods. While there are some pockets of disadvantage (e.g. the Villa Victoria and Lenox public housing projects in the South End), the residential populations tend to be more affluent and, most importantly, much smaller in number relative to the very large number of non-residents who work, shop, dine, and take advantage of the many entertainment venues in these neighborhoods. The high crime rates in these neighborhoods might reflect criminal opportunities generated by these ambient populations. Specifically, crime problems in these neighborhoods are characterized by robbery, larceny, and auto theft victimizations of non-residents as well as assaults among college students and other young adults frequenting the many bars in mostly non-residential areas (Braga, Hureau, & Papachristos, 2011b).[29]

[29] Interested readers should consult Braga, Hureau, and Papachristos (2011) for a more detailed account of the distribution of robbery problems across Boston neighborhoods.

## Figure 3: Violent Crime, Property Crime, Prison Admissions, and Concentrated Disadvantage (Boston, MA)



## Newark, New Jersey

Table 11 provides the descriptive statistics for the most recent year of data used in the analysis for Newark. In this table, the data are quarterly census tract data for 2010 (the identification of the tracts themselves was based on the 2000 decennial census).

In the table below, and in all analyses that follow, total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Violent crime is the sum of homicide, rape, robbery, and aggravated assault. Nonviolent crime is the sum of burglary, larceny theft, and motor vehicle theft. Admissions and releases are sums of counts admissions and releases respectively for the tract over the quarter. The cycling variable sums the admissions and releases together over the quarter.

The data used to compile the concentrated disadvantage index were drawn from the 2000 decennial census. Based on the census tracts designations associated with the 2000 decennial census, there are 90 census tracts across the city of Newark. Concentrated disadvantage is an index that consists of the sum of four variables (the index has a theoretical range of 0-4): the share of households headed by a single parent in a tract, the tract poverty rate, the tract unemployment rate, and the share of the tract population that is black. Residential Mobility is the share of the census tract that moved within last 5 years.

In 2010, on average, there were approximately 138 crimes per census tract in Newark, the large majority of which were nonviolent crimes (105 nonviolent crimes per census tract versus 32 violent crimes). This translates into an average yearly total crime rate of 52.59, a violent crime rate of 13.17 and a nonviolent crime rate of 39.42 per 1,000 residents. There were an average of 11.91 admissions and 7.24 releases per quarter per tract – for rates of 4.70 and 2.97, respectively, per 1000. The standard deviation of both the crime and cycling variables indicates the wide dispersion of the data.

In Newark, the concentrated disadvantage variable had a mean of 1.32 and a standard deviation of 0.53. Given that concentrated disadvantage has a theoretical range of 0 to 4, a mean of 1.31 and a standard deviation of 0.53 indicates that Newark clearly has some fairly disadvantaged neighborhoods and a relatively narrow dispersion of the characteristics across tracts (the low standard deviation relative to the mean).

As noted above, residential mobility is measured by the share of the census tract that moved into the tract within the last five years. The mean of 0.45 indicates there is a relatively high rate of neighborhood turnover in Newark.

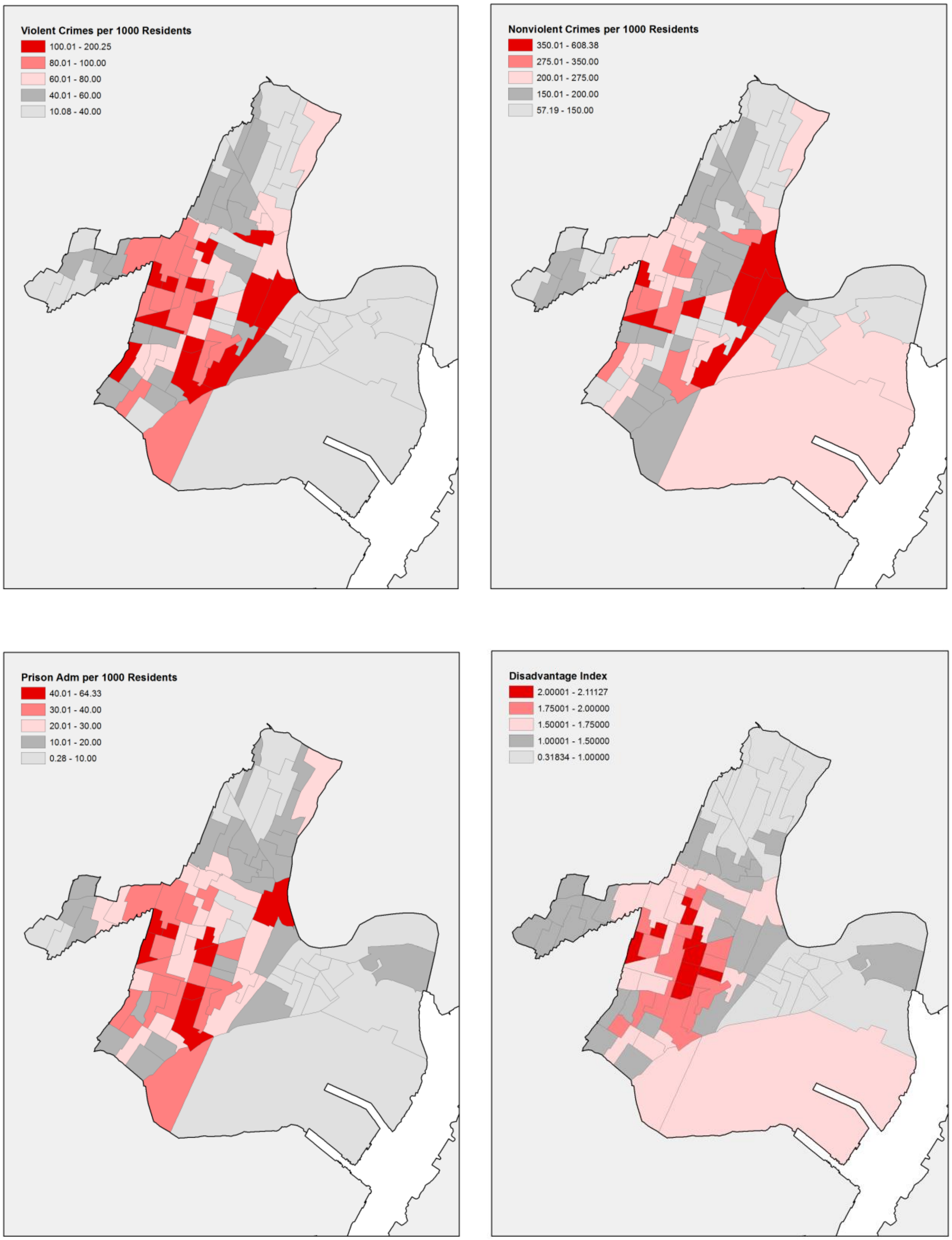
**Table 11: Newark Descriptives**

| | Min | Max | Mean | Std. Dev. | *N* |
|---|---|---|---|---|---|
| Total Crime | 36.00 | 343.00 | 137.56 | [55.64] | 90 |
| Violent Crime | 4.00 | 70.00 | 32.33 | [14.39] | 90 |
| Nonviolent Crime | 31.00 | 276.00 | 105.22 | [45.62] | 90 |
| Admissions | 0.00 | 36.00 | 11.91 | [7.63] | 90 |
| Releases | 0.00 | 38.00 | 7.24 | [6.03] | 90 |
| Total Crime Rate | 10.09 | 147.14 | 52.59 | [28.04] | 90 |
| Violent Crime Rate | 1.20 | 51.96 | 13.17 | [9.15] | 90 |
| Nonviolent Crime Rate | 8.69 | 119.98 | 39.42 | [20.48] | 90 |
| Admission Rate | 0.00 | 14.61 | 4.70 | [3.37] | 90 |
| Release Rate | 0.00 | 28.46 | 2.97 | [3.51] | 90 |
| Concentrated Disadvantage | 0.32 | 2.11 | 1.32 | [0.53] | 90 |
| Residential Mobility | 0.25 | 0.65 | 0.45 | [0.08] | 90 |

*Notes*: Based on 2010 crime and incarceration data and 2000 census data. The concentrated disadvantage index used 2000 decennial census data from Summary File 3 and includes the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Residential Mobility, also drawn from Summary File 3 for the 2000 census, is the share of the census tract that moved within last 5 years. Rates are measures per 1,000 residents.

The maps in Figure 4 display the spatial distribution of violent and nonviolent crime across Newark. Very high violence and very high property crime census tracts, defined here as those areas that experience more than 100 violent crimes per 1,000 residents and more than 350 nonviolent crimes per 1,000 residents, are similarly distributed among four of Newark's five political wards. The downtown and proximate neighborhoods represent the highest crime areas in the East Ward. In Newark's Central Ward, the Springfield / Belmont and Seventh Avenue neighborhoods experience very high levels of crime. Most of the West Ward, especially the Westside area and neighborhoods along Avon Avenue, experience very high levels of violent crime. Finally, the Chadwick Village section of Clinton Hill represents the most violent area of the South Ward during the study time period. Clinton Hill also experiences high levels of property crime. Like Boston, the distribution of prison admissions per 1000 residents closely matches the spatial distribution of concentrated disadvantage across Newark with substantial overlap in particular census tracts in the Central, West, and South wards. Newark's downtown also generates very high levels of crime. While the downtown area is not characterized by an overall high concentration of disadvantage, it does have a very high level of prison admissions per 1000 residents. However, there are public housing projects in areas proximate to the downtown that seem likely to represent a disproportionate share of prison admissions during the study time period.

**Figure 4: Violent Crime, Property Crime, Prison Admissions, and Concentrated Disadvantage (Newark, NJ)**



## Trenton, New Jersey

Table 12 provides the descriptive statistics for the most recent year of data used for Trenton. In this table, the data are annual data across the tracts in 2010 (the tracts themselves were based on the 2000 decennial census). In the table below, total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Violent crime is the sum of homicide, rape, robbery, and aggravated assault. Nonviolent crime is the sum of burglary, larceny theft, motor vehicle theft, and arson. Admissions and releases are sums of counts admissions and releases respectively for the tract over the year. The cycling variable sums the admissions and releases together over the year.

The data used to compile the concentrated disadvantage index were drawn from the 2000 decennial census. Based on the census tracts designations associated with the 2000 decennial census (which was used in this project for reasons described below), there are 38 census tracts across the city of Trenton, 33 of which have usable data. Concentrated disadvantage is an index that consists of the sum of four variables (the index has a theoretical range of 0-4): the share of households headed by a single parent in a tract, the tract poverty rate, the tract unemployment rate, and the share of the tract population that is black. Residential Mobility is the share of the census tract that moved within last 5 years.

Trenton had about 97 crimes per tract in 2010, 78 of which were nonviolent and 19 of which were violent. This translates into a crime rate of 31.13 per tract per 1000, a violent crime rate of 6.22, and a nonviolent crime rate 24.92. There were 14.18 admissions and 7.42 releases per tract per year, translating into an admission rate of 4.49 per 1000 and a release rate of 2.31.

In Trenton, the concentrated disadvantage variable had a mean of .81 and a standard deviation of 0.58. Given that concentrated disadvantage has a theoretical range of 0 to 4, a mean of 0.81 and a standard deviation of 0.58 indicates that Trenton has relatively few high disadvantage neighborhoods (the low mean) and a relatively even dispersion of the characteristics across tracts (the low standard deviation). As noted above, residential mobility is measured by the share of the census tract that moved into the tract within the last five years. The mean of 0.38 indicates the relatively high rate of neighborhood turnover in Trenton.

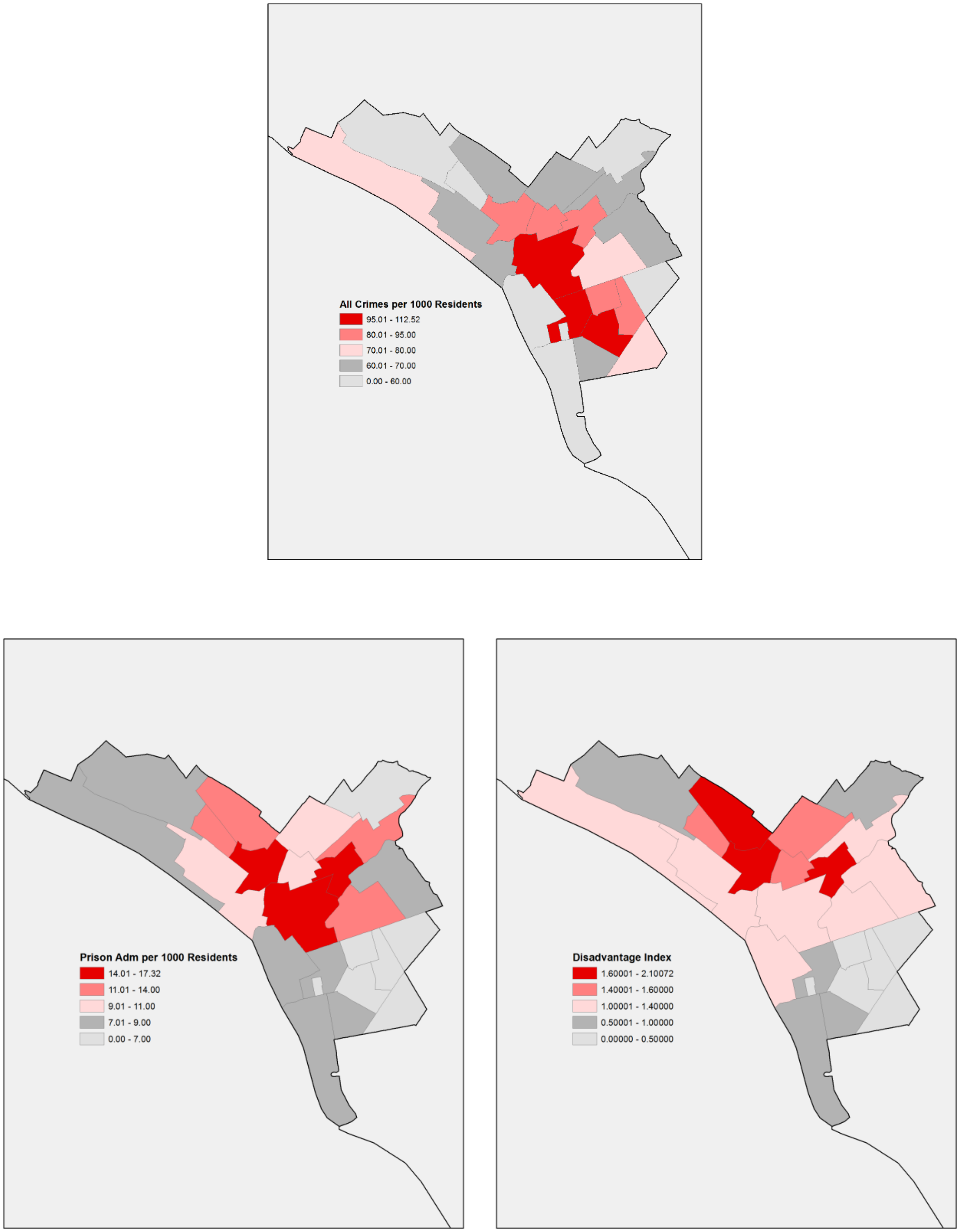
**Table 12: Trenton Descriptives**

| | Min | Max | Mean | Std. Dev. | *N* |
|---|---|---|---|---|---|
| Total Crime | 0.00 | 287.00 | 97.18 | [82.82] | 33 |
| Violent Crime | 0.00 | 64.00 | 18.94 | [16.78] | 33 |
| Nonviolent Crime | 0.00 | 223.00 | 78.24 | [66.88] | 33 |
| Admissions | 0.00 | 43.00 | 14.18 | [11.52] | 33 |
| Releases | 0.00 | 58.00 | 7.42 | [10.21] | 33 |
| Total Crime Rate | 0.10 | 65.06 | 31.13 | [19.57] | 29 |
| Violent Crime Rate | 0.00 | 15.97 | 6.22 | [4.50] | 29 |
| Nonviolent Crime Rate | 0.00 | 50.70 | 24.92 | [15.43] | 29 |
| Admission Rate | 0.10 | 9.94 | 4.49 | [2.57] | 29 |
| Release Rate | 0.29 | 13.40 | 2.31 | [2.59] | 29 |
| Concentrated Disadvantage | 0.00 | 2.10 | 0.81 | [0.58] | 33 |
| Residential Mobility | 0.00 | 0.56 | 0.38 | [0.16] | 33 |

*Notes*: Based on 2010 crime and incarceration data and 2000 census data. The concentrated disadvantage index used 2000 decennial census data from Summary File 3 and includes the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Residential Mobility, also drawn from Summary File 3 for the 2000 census, is the share of the census tract that moved within last 5 years. Rates are measures per 1,000 residents.

The maps in Figure 5 visually display the dispersion of crime, prison admissions, and concentrated disadvantage across the city of Trenton. The highest crime neighborhoods, defined here as those with at least 95 crimes per 1,000 residents, concentrate just south of Trenton's downtown area.  These crime hot spots include the areas around the train station and areas and extend through the Mill Hill, Greenwood / Hamilton, and Chambersburg neighborhoods. Neighborhoods with high levels of prison admissions per 1,000 residents tend to overlap with some of these high crime areas (e.g. Greenwood / Hamilton) but also include East Trenton, North Trenton, and the Pennington / Prospect neighborhoods. The most disadvantaged neighborhoods in East Trenton, North Trenton, and the Pennington / Prospect neighborhoods.  It is worth noting that these disadvantaged neighborhoods are not only characterized by higher levels of prison admissions but also experience modest levels of crime (60 – 70 crimes per 1,000 residents). However, crime problems are more intensive in the commuter and commercial areas south of the downtown.

## Figure 5: Crime, Prison Admissions, and Concentrated Disadvantage (Trenton, NJ)



### Rural New Jersey

Table 13 provides the descriptive statistics for the most recent year of data used for rural New Jersey. In this table, the data are quarterly census tract data for 2010 (the tracts themselves were based on the 2000 decennial census). In the table below, and in all analyses that follow, total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Violent crime is the sum of homicide, rape, robbery, and aggravated assault. Nonviolent crime is the sum of burglary, larceny theft, and motor vehicle theft. Admissions and releases are sums of counts admissions and releases respectively for the tract over the quarter. The cycling variable sums the admissions and releases together over the quarter.

**Table 13: Rural New Jersey Descriptives**

| | Min | Max | Mean | Std. Dev. | *N* |
|---|---|---|---|---|---|
| Total Crime | 0.00 | 318.00 | 20.59 | [43.72] | 145 |
| Violent Crime | 0.00 | 86.00 | 5.06 | [11.92] | 145 |
| Nonviolent Crime | 0.00 | 232.00 | 15.52 | [32.67] | 145 |
| Admissions | 0.00 | 43.00 | 3.04 | [5.35] | 145 |
| Releases | 0.00 | 58.00 | 1.87 | [5.24] | 145 |
| Total Crime Rate | 0.00 | 46.51 | 6.58 | [11.20] | 145 |
| Violent Crime Rate | 0.00 | 23.26 | 1.82 | [4.01] | 145 |
| Nonviolent Crime Rate | 0.00 | 32.67 | 4.75 | [7.82] | 145 |
| Admission Rate | 0.00 | 9.94 | 0.76 | [1.32] | 145 |
| Release Rate | 0.00 | 13.40 | 0.45 | [1.22] | 145 |
| Concentrated Disadvantage | 0.06 | 1.65 | 0.29 | [0.30] | 145 |
| Residential Mobility | 0.21 | 0.81 | 0.37 | [0.10] | 145 |

*Notes*: Based on 2010 crime and incarceration data and 2000 census data. The concentrated disadvantage index used 2000 decennial census data from Summary File 3 and includes the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Residential Mobility, also drawn from Summary File 3 for the 2000 census, is the share of the census tract that moved within last 5 years. Rates are measures per 1,000 residents.

The data used to compile the concentrated disadvantage index were drawn from the 2000 decennial census. Concentrated disadvantage is an index that consists of the sum of four variables (the index has a theoretical range of 0-4): the share of households headed by a single parent in a tract, the tract poverty rate, the tract unemployment rate, and the share of the tract population that is black. Residential Mobility is the share of the census tract that moved within last 5 years.

Rural New Jersey has an average of approximately 21 crimes per tract in 2010, with 15 nonviolent crimes and 5 violent crimes. There were 3 admissions and 1.87 releases per tract. This yields census tracts rates per 1000 of 6.58 for total crime, 1.82 for violent crime, 4.75 for nonviolent crime, 0.76 for admissions, and 0.45 for releases. The mean of concentrated disadvantage is 0.29 and the standard deviation is 0.30, implying that rural New Jersey has generally low disadvantage but does have wide dispersion in disadvantage.

## Four Site Comparison

Descriptive statistics of the key variables in the analysis for each of the four areas – Boston, Newark, Trenton, and unincorporated New Jersey – are provided in Table 14 for the last year in the sample of each respective location.[30] With the exception of Trenton, data are aggregated to tract by quarter counts, and rates are calculated per 1000 residents. In Trenton, data are year by tract instead.

Perhaps surprisingly, Boston has the highest average annualized average number of crimes per tract. Newark, however, has the lowest standard deviation indicating that, while Boston may have some very high crime neighborhoods it also has a considerable number of low crime neighborhoods, Newark has a much more uniform distribution of crime across neighborhoods. The very large number of census tracts that have no reported crimes in a quarter drives the very high standard deviation of total crime in rural New Jersey. Comparing crime rates across cities still results in Newark having the highest annualized crime rate, followed by Boston, Trenton, and unincorporated New Jersey.[31] Unlike crime and crime rates, releases and admissions and the corresponding rates are considerably lower in Boston than in either Newark or Trenton, with Newark having the highest annualized rate. Finally, the index of concentrated disadvantage, which is the sum of the share of households with a single parent, the unemployment rate, the poverty rate and the share of individuals who identify as black, is considerably higher in Newark than the other locations, and lowest in unincorporated New

[30] The Boston, Newark, and Trenton data are balanced panels. Rural NJ is an unbalanced panel because the low geocoding hit rate results in a lot of zeroes. Therefore, Table 13 reports a different number of tracts for rural NJ than Table 14. Table 14 has the total number of tracts observed at any point in time, Table 13 has the number of tracts observed in 2010.

[31] Table 35 reports the official UCR crimes rates across all three cities in 2010. Tables 4, 6 and 7 report on these rates for Boston, Newark and Trenton respectively.

Jersey. In the maps that follow the table, we provide visuals that map crime, prison admissions, and concentrated disadvantage to allow for greater comparison across our sites (see Figures 6-8).

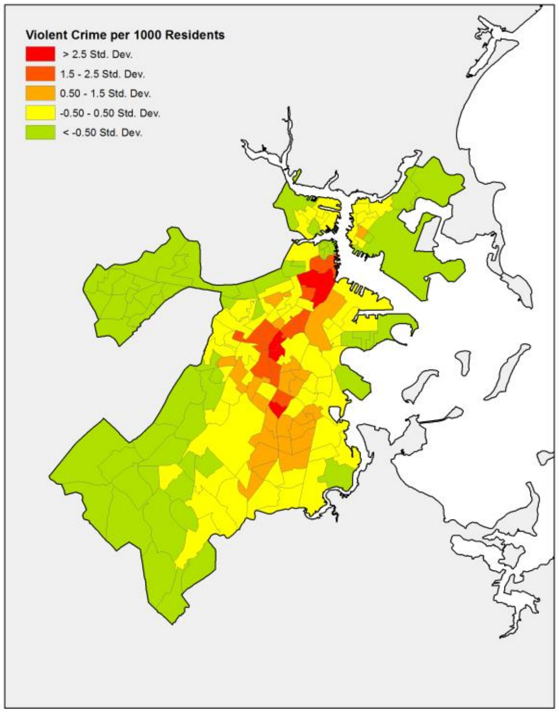
**Table 14: Descriptive Statistics – Four Site Comparison**

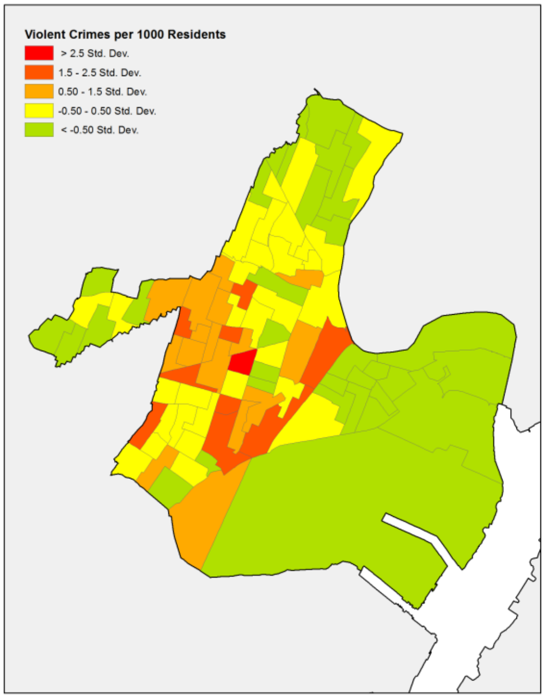
| | Boston | Newark | Trenton | Rural NJ |
|---|---|---|---|---|
| Total Crime | 46.66 | 33.57 | 92.35 | 18.43 |
| | [39.30] | [15.96] | [77.79] | [40.60] |
| Releases | 1.42 | 2.28 | 7.76 | 1.99 |
| | [1.96] | [5.76] | [9.10] | [4.21] |
| Admissions | 1.77 | 3.28 | 12.88 | 3.19 |
| | [2.17] | [2.65] | [10.39] | [5.67] |
| Total Crime Rate | 13.45 | 12.75 | 25.90 | 5.69 |
| | [11.99] | [7.41] | [19.46] | [10.53] |
| Release Rate | 0.43 | 0.94 | 2.18 | 0.48 |
| | [0.81] | [2.75] | [2.40] | [0.97] |
| Admission Rate | 0.54 | 1.31 | 3.57 | 0.79 |
| | [0.80] | [1.21] | [2.54] | [1.47] |
| Concentrated Disadvantage | 0.67 | 1.32 | 0.83 | 0.28 |
| | [0.44] | [0.53] | [0.56] | [0.28] |
| N | 4992 | 1440 | 66 | 1170 |
| Tracts | 156 | 90 | 33 | 247 |
| Population (2010 census) | 617,594 | 277,140 | 84,913 | 319,865* |

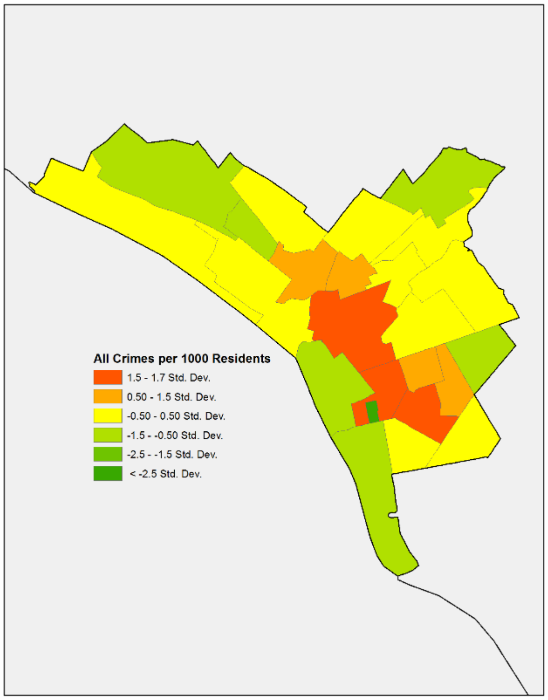
Note: Table 14 reports the full sample averages at the unit of analysis for the areas (tract-quarters for Newark, Boston, and Rural NJ, and tract-years for Trenton).

## Figure 6: Violent Crime Rates for Boston and Newark, and Total Crime for Trenton (2010)

**Boston** (centered); **Newark** (bottom left); **Trenton** (bottom right)

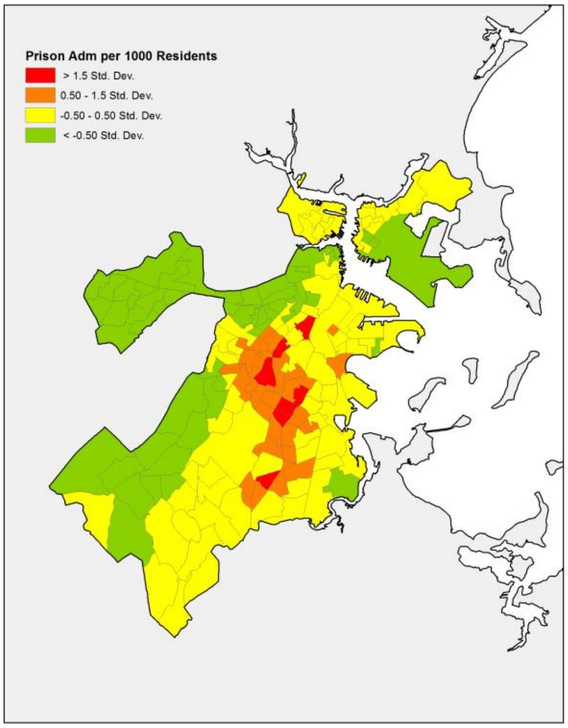


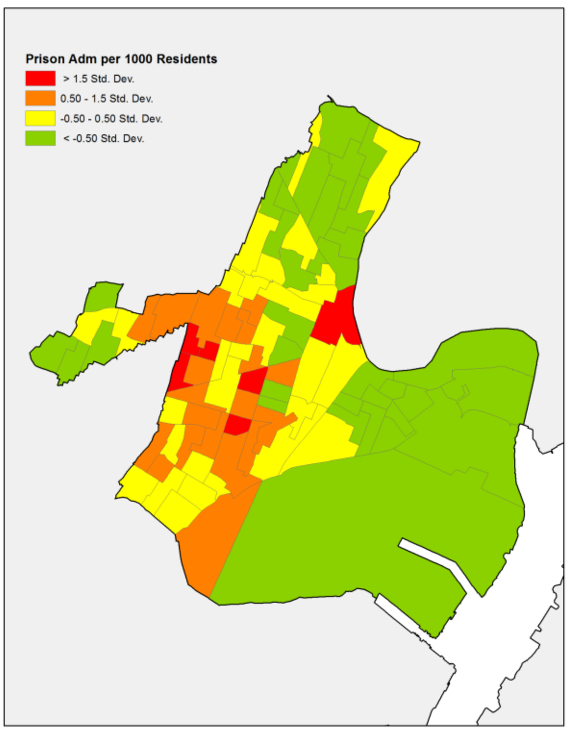



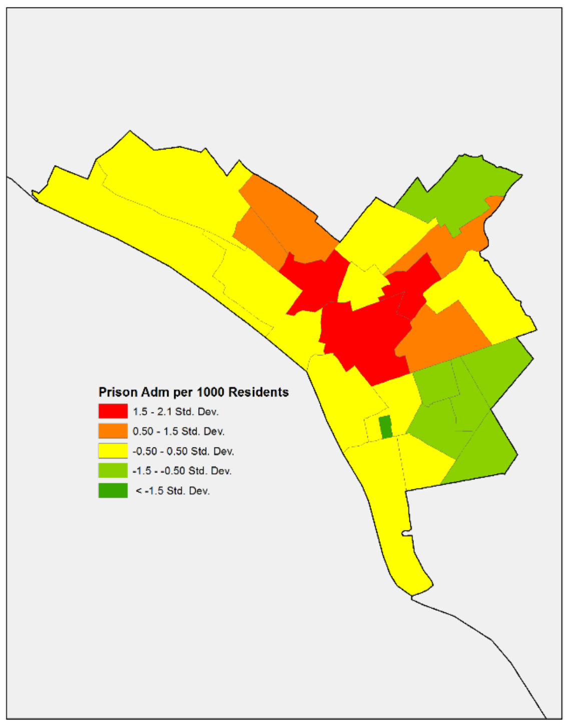



## Figure 7: Prison Admission Rates – Boston, Newark, and Trenton

Boston (centered); Newark (bottom left); Trenton (bottom right)

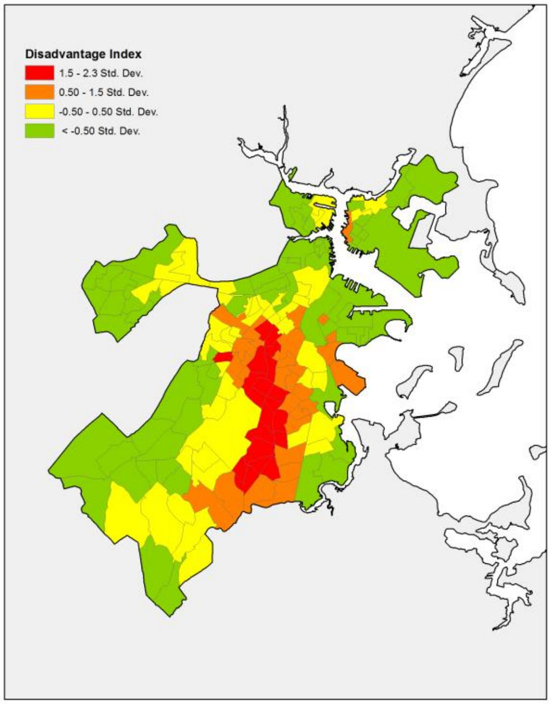


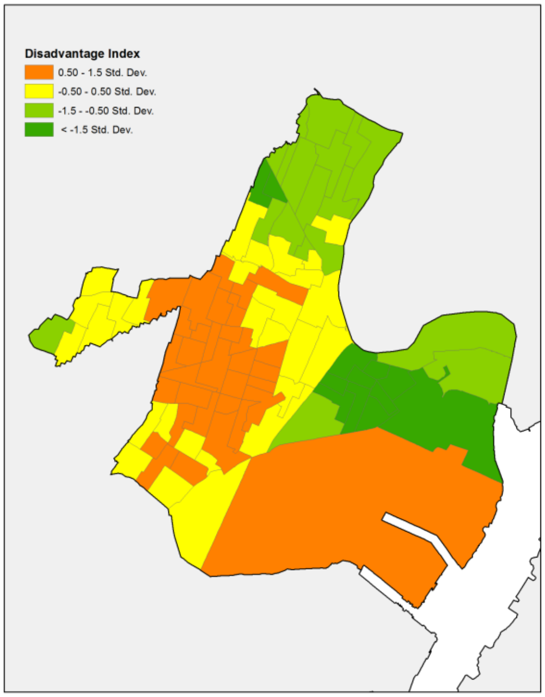



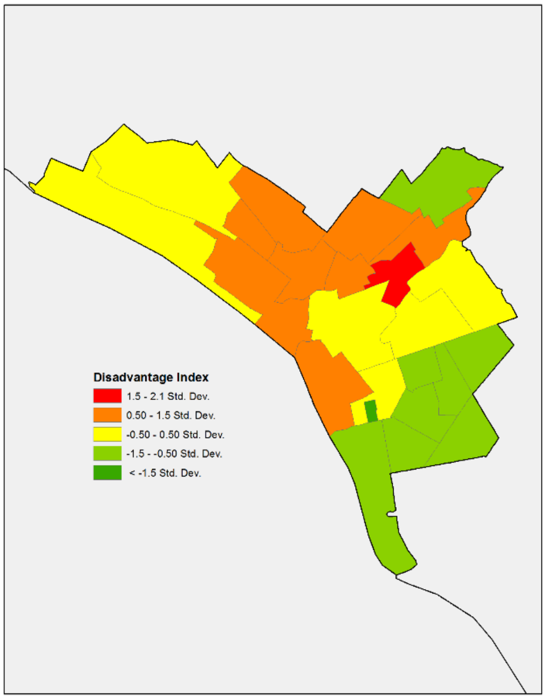



## Figure 8: Concentrated Disadvantage – Boston, Newark, and Trenton

Boston (centered); Newark (bottom left); Trenton (bottom right)



## Random Versus Fixed Effects

Omitted variable bias, which is one of the key reasons for the simultaneity problem in this context, is pervasive in the empirical social sciences. It is simply impossible to control for all determinants of a given outcome. When there are group specific omitted variables,[32] the two most common methods for eliminating omitted variable bias are random and fixed effects models. Conceptually, the two methods accomplish similar things, namely, they absorb the impact of unobserved determinants of the outcome of interest. In other words, they control for "unobserved effects." The conditions under which each approach can be used, however, are quite different. In particular, failure to demonstrate that the necessary conditions for random effects are met can lead to very misleading results.[33]

The basic unobserved effect model applied to neighborhoods is given in equation 1, where $t$ indexes time and $c$ indexes the neighborhood. However, equation 1 is equally applicable to any data structure where the full sample can be broken down into groups, and each group has at least two observations. The key distinction between random and fixed effects models is how the unobserved effect, $\delta_c$, is handled. In random effects, $\delta_c$ is treated as a random, and is therefore a part of the error term and not explicitly estimated. In fixed effects, $\delta_c$ is assumed to be non-random, and is treated as a parameter to be explicitly estimated.

$$y_{ct} = \alpha + \beta X_{ct} + \delta_c + u_{ct} \tag{1}$$

Random effects models are very appealing and pervasive in the literature on neighborhood crime. Probably most importantly for neighborhood level data, they allow the researcher to include both time-varying and time-invariant variables in the estimation model in a straightforward manner. This has appeal when combining U.S. Census data for neighborhood level characteristics with yearly crime data. Random effects models also have important efficiency properties. When random effects are appropriate, the coefficients from the random

[32] Any situation where there are repeated observations on the same unit of observations the dataset has groups. In other words, all panel datasets have groups as there are repeated observations through time on the same unit of observation, but cross-section datasets have groups also. The fixed versus random effects debate applies equally in both situations.

[33] For an excellent review of the mechanics of random effects and fixed effects models, in addition to a discussion of when to use each model, see Wooldridge (2010) and Cameron and Trivedi (2005). For a less technical presentation, see Wooldridge (2013). The discussion here draws heavily on all three resources (Cameron & Trivedi, 2005; Wooldridge, 2010, 2013).

and fixed effects models will be the same, however the standard errors of the random effects model will likely be smaller. The critical issue is whether a random effects model is either technically or intuitively applicable to a given situation. This issue is almost universally ignored in empirical criminology research on neighborhood level issues. Note, importantly, that Hierarchical Linear Models are random effects models, and thus the discussion that follows applies more or less equally to both HLM and more traditional random effects estimations.

Before proceeding to a discussion of the general applicability of random effects models to neighborhood level research, a brief intuitive review of random effects models is helpful. The primary empirical issue when there are group level unobserved effects is caused by the fact that the constant of a regression model is the estimate of the average of the dependent variable when all other variables are equal to zero. Estimating a single intercept model when there are group level average differences in the dependent variable can lead to biased coefficients as the coefficients must account for both the within group association between $X$ and $y$ in equation 1, and the across group average difference in the dependent variable due to group level observed or unobserved characteristics, the association between $y$ and $\delta$ in equation 1. Both random and fixed effects models attempt to remove this bias by giving each group its own intercept, that is by accounting for the existence of $\delta$.

The critical assumption behind random effects is that $\delta_c$, which again reflect average group level differences in the dependent variable, is uncorrelated with $X_{ct}$. If this necessary condition is not fulfilled, then two issues arise which makes inference from the random effects regression invalid, and frequently significantly misleading. First, this assumption helps assure that the coefficients are unbiased, thus if the assumption is violated then the coefficients may be biased. Second, because random effects can generate (much) smaller standard errors than fixed effects, one may draw a misleading conclusion about the significance of the estimated $\beta_i$'s.

Our assertion is that, even if the model passes the diagnostic tests for the applicability of random effects, random effects models are not applicable to neighborhood level data, especially not longitudinal data. Our assertion is based on the seemingly non-controversial observation that unobserved neighborhood characteristics are likely to be correlated with the observed characteristics, that is, $\mathrm{Corr}(X_{ct}, \delta_c) \neq 0$.

For lack of a better term, neighborhoods have "character." This character is a function of the people who choose to live there, the businesses that choose to locate there, the type of housing and other amenities, both observable and unobservable. Average differences in many important outcomes tend to be persistent through time, and determined by a set of neighborhood characteristics that are not observable but are correlated with many things that are measurable and observable, which also determine neighborhood crime levels.

This is precisely the situation where fixed effects models are applicable and random effects models are not. Unlike random effects models and their variants where at the very least the correlation structure between the observed and unobserved variables must be known34, fixed effects models are robust to any arbitrary correlation between the fixed effect and the observed variables. There are three distinct ways to estimate fixed effects models, but they all have the same intuitive understanding. Essentially, fixed effects models explicitly estimate an intercept for each group, as opposed to random effects where the random intercept is part of the error term. The time-invariant (a point to be discussed later) characteristics of the neighborhood are absorbed by the fixed effect, removing the bias on the coefficients. And, assuming proper adjustments are made to the standard errors, the standard errors are closer to correct as well.

Fixed effects models are not a panacea; they come with their own drawbacks and potential pitfalls. First, and the most likely reason why so many turn to random effects models, fixed effects models absorb all time-invariant characteristics. It is impossible to estimate a coefficient on a time-invariant variable entered directly into a fixed effects model as it is wiped out by the fixed effect.35 Second, fixed effects models are more data intensive because of the large number of parameters to be estimated. Third, some researchers are concerned about incidental parameters bias, a problem that is believed to arise when one estimates too many

[34] The classic presentation of random effects requires that the correlation between the unobserved effect and the dependent variables is zero. Correlated random effects models relaxes this assumption, but it is still necessary to know, and be able to specify, the correlational structure between the independent variables and the unobserved effect.

[35] As mentioned, there are three different ways to estimate fixed effects models. Using the first-difference approach, time-invariant characteristics are absorbed because the difference between x_t and x_t-1 is always zero. Using the mean-difference approach, time-invariant characteristics are absorbed because the standard deviation is zero. Using the dummy variable approach, the coefficient on the time-invariant characteristics cannot be estimated because it is perfectly collinear with the fixed effect.

parameters, especially in non-linear and limited dependent variable models. We will discuss the second two briefly, before turning to a more in depth discussion of the first.

Fixed effects models are data intensive, there is no getting around this fact. In addition to the coefficients on the dependent variables, the fixed effect for each group absorbs one degree of freedom each. This is one of the reasons why fixed effects models have larger confidence intervals. The second reason they are data intensive is because the source of variation in the model is substantially reduced because the fixed effect absorbs all of the variation across groups. This latter point means that the data either need to have many observations per group so there is sufficient within group variation, or the data need to have a large number of groups so that there are sufficient degrees of freedom to make inference based on a large number of potentially small within group changes. In general, fixed effects models perform better with a smaller number of larger groups than with a larger number of small groups. For example, it is generally better to have 60 months of data on 100 neighborhoods than 6 months of data on 1000 neighborhoods as the former only has 100 fixed effects to estimate while the latter has 1000.

The incidental parameters problem is the possibility that one can introduce bias to a model by estimating a very large number of parameters that are not all necessary. However, the downside is that not estimating the fixed effects when fixed effects are appropriate results in omitted variable bias, a problem that is of much greater concern than incidental parameters bias. Further, incidental parameters bias is of most concern with non-linear, limited dependent variable, and count data models. But, as there is evidence to believe that fixed effects are not appropriate with a number of commonly used models in this category, this problem is of secondary concern.

### Using Fixed Effects Models

The classic presentation of a fixed effects model is the dummy variable approach, where $\delta_c$ in equation 1 represents a set of dummy variables, one for each neighborhood with one neighborhood omitted if the model includes a constant. The dummy variable approach also makes clear that fixed effects absorb C-1 degrees of freedom. The dummy variable approach is commonly referred to as “unconditional fixed effects.”

As mentioned above, the primary limitation of this estimation technique is that the dummy variable for the neighborhood is perfectly collinear with any time-invariant characteristics of the neighborhood. This poses a particular problem when combining time-varying with time-invariant data sources. In some sense this does not matter, the fixed effect will absorb all of the time-varying information anyway. However, unless the time-invariant characteristics are very highly correlated with each other, the regression model will perform better if both the fixed effect and the neighborhood characteristics are included.

A simple extension of equation 1, shown in equation 2, accomplishes this. The term $\sum_t(\gamma_t Z_c)$ is an interaction term between dummy variables for time period and the time-invariant neighborhood characteristics. Notice that the direct effect of each of the respective time-invariant characteristics are omitted, as they are perfectly collinear with the fixed effects. The interaction terms, however, are not perfectly collinear with the neighborhood fixed effect and thus can be included in the model. This allows the researcher to control for important neighborhood characteristics. Unfortunately, this approach does not result in easily interpretable coefficients on the neighborhood characteristics, as all that can be observed is how the effect of the time-invariant characteristic varies through time. This approach cannot be used if one is interested in the coefficient on one of the time-invariant characteristics.

$$y_{ct} = \alpha + \beta X_{ct} + \sum_t(\gamma_t Z_c) + \delta_c + u_{ct} \qquad (2)$$

A related issue, but not one specific to fixed effects, is dealing with time-varying unobserved characteristics. This is generally dealt with using a time-trend. There are two types of time-trends: an overall time trend and a neighborhood specific time trend. The overall time trend is useful when there is a broad trend that all the neighborhoods follow, and little difference across neighborhoods in trend. The overall time trend can, alternatively, be accounted for using time-period dummy variables, which is helpful if the overall pattern through time does not follow a clear pattern.

When using time-invariant neighborhood characteristics a neighborhood specific time-trend may be preferable, if the dataset is large enough. This results in equation 3, where $\rho_c$ accounts for the neighborhood specific time trend. Results based on equation 3 have an intuitively appealing interpretation, especially if the time-invariant characteristics are observed at the beginning of the time-period under analysis. In the next section, we estimate equation 3 and compare the results to more commonly used estimation techniques in neighborhood level data.

$$y_{ct} = \alpha + \beta X_{ct} + \sum_t (\gamma_t Z_c) + \delta_c + \rho_c + u_{ct} \qquad (3)$$

## Estimations

The next section presents the results of the estimation of equation 3, and compares the results to a selection of random effects regressions.

## Regression Results

Tables 15 and 16 report the results of estimating equation 3 with OLS (column 1) and a Generalized Linear Model (column 2). In addition, a selection of commonly used random effects models are also estimated, where effect of the $\sum_t (\gamma_t Z_c)$term has been replaced with $\gamma Z_c$reflecting the fact that time-invariant characteristics can be estimated directly in random effects models. In the fixed effects regressions, the standard errors are clustered on the census tract to adjust the standard errors for within tract correlation in the residuals. In both tables, marginal effects are reported. For the OLS and GLM models, the dependent variable is the total crime rate and the key independent variables are the release rate and the admissions rate. For the Poisson and negative binomial regressions, the dependent variable is total crime and the key independent variables are releases and admissions. Finally, the log of the tract population is included in the Poisson and negative binomial regressions, implying that the coefficient on releases and admissions can be interpreted as a change in the rate.

Our specific choice of estimation technique was driven by a combination of popularity in the literature and theoretical applicability. Crime is a count variable, meaning, it is impossible for it take on values less zero, it can only take integer values, and the observed data may not increase in units of 1. Crime rates also cannot be less than zero, however they are not limited to integer values. This suggests that count data models should be used for total crime, while OLS and its variants are more applicable for crime rates. The use of the GLM model with the log link function is motivated by the fact that crime rates are approximately log-normally distributed, implying that a regression strategy based on a logarithmic form is more appropriate, however we cannot take the natural log of the release and admission rate because it contains a significant number of zeroes. The GLM model solves this problem. The Poisson and Negative Binomial regressions are the most common count data models, with the latter used when the data are overdispersed. Because there is not a generally accepted method for testing for overdispersion, we report the results of both models. Our preferred specification is OLS with fixed effects, as recent Monte-Carlo results suggest that non-liner models are inconsistent when using fixed effects (Green 2004).

**Table 15: Model Comparisons for Releases and Admissions with Total Crime as the Dependent Variable (Boston)**

| | OLS FE | GLM FE | Poisson FE | Neg. Bin. FE | OLS RE | Poisson RE | Neg. Bin. RE |
|---|---|---|---|---|---|---|---|
| $\text{Admissions}_{t-1}$ | 0.144 | 0.144 | 0.003 | 0.006 | 0.345* | 0.014*** | 0.018*** |
| | [0.214] | [0.210] | [0.007] | [0.007] | [0.210] | [0.004] | [0.007] |
| $\text{Releases}_{t-1}$ | -0.198 | -0.198 | 0.005 | 0.006 | -0.260 | -0.002 | 0.001 |
| | [0.235] | [0.231] | [0.006] | [0.006] | [0.222] | [0.004] | [0.007] |
| Constant | -0.812 | -5.835 | 1.890 | 2.213 | 16.933*** | -0.420 | 0.961* |
| | [9.940] | [11.031] | [1.511] | [2.999] | [4.154] | [0.857] | [0.569] |
| *N* | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 |

**Table 16: Model Comparisons for Releases and Admissions with Total Crime as the Dependent Variable (Newark)**

| | OLS FE | GLM FE | Poisson FE | Neg. Bin. FE | OLS RE | Poisson RE | Neg. Bin. RE |
|---|---|---|---|---|---|---|---|
| $\text{Admissions}_{t-1}$ | 0.086 | 0.086 | 0.002 | 0.002 | 0.162 | 0.004 | 0.005 |
| | [0.232] | [0.224] | [0.010] | [0.010] | [0.237] | [0.006] | [0.009] |
| $\text{Releases}_{t-1}$ | -0.179** | -0.179** | -0.004 | -0.003 | -0.148** | -0.003 | -0.003 |
| | [0.087] | [0.084] | [0.004] | [0.004] | [0.072] | [0.003] | [0.004] |
| Constant | -39.376 | -50.54 | -8.902 | -10.527 | 6.154 | 0.209 | 0.507 |
| | [67.591] | [68.871] | [7.186] | [9.406] | [7.242] | [0.858] | [0.816] |
| *N* | 1350 | 1350 | 1350 | 1350 | 1350 | 1350 | 1350 |

*Notes for Tables 15 & 16*: Data are quarterly counts of crime by census tract. Dependent variable is total crime, measured as counts for Poisson and Negative Binomial regressions, and rate per 1000 people for OLS and GLM regressions. Releases and admissions are counts for Poisson and Negative Binomial regressions, and rates per 1000 for OLS and GLM regressions. Additional controls come from the 2010 Census Summary File 3 and include concentrated disadvantage, share of the census tract population that is black, the share of the population that are renters, quarter dummies, a tract specific linear time trend, and the log of census tract population for the Poisson and Negative Binomial Regressions. Standard errors are in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

Column 1 is the preferred estimation technique. In summary, this is for three primary reasons. First, random effect models are unlikely to be appropriate with neighborhood level data because the random effects will be correlated with the independent variables. Second, as Green (2002) shows, several commonly used limited and count dependent variable models generate inconsistent coefficients when used in conjunctions with fixed effects. And third, OLS regressions are easy to estimate and interpret.

Some care must be taken when comparing the magnitudes of the coefficients across the various models. For the OLS and GLM models, the coefficients on releases and admissions, respectively, represent the effect of a one unit change in the release (admissions) rate on the total crime rate. In the Poisson and negative binomial regressions, the coefficient represents the effect of a one unit change in the release (admissions) rate on total crime.[36]

Instead of comparing coefficients across different estimation methods, it is more informative to compare coefficients across random effect and fixed effects models within the same basic technique. A clear pattern emerges. In Boston, the coefficient on releases is never statistically significant. However, the coefficient on admissions is statistically significant with random effects, but insignificant with fixed effects. Comparing coefficients across analogous estimation techniques (e.g. OLS fixed effects to OLS random effects) demonstrates that the lack of significance in the fixed effects regression is not due to coefficients being smaller, as in some cases the effect size is actually larger for the fixed effects regression.

For Newark, a somewhat different and surprising pattern emerges. The coefficient on admissions is never significant, though is generally larger for the random effects regressions. The estimated effect of releases is again slightly larger for the fixed effects regressions, and is statistically significant with both fixed and random effects. Although the coefficient on releases is statistically from each other across OLS fixed and random effects, this is a situation where it is less clear cut that one should use fixed effects, though we still believe that it is unlikely that the assumptions necessary for inference from a random effects regression to be valid hold.

[36] The inclusion of the log of the census tract population in the count models allows the coefficients to be interpreted as changes in the rate, despite the fact that the variables themselves are counts.

## The Two City Comparison: Boston and Tallahassee

After establishing the preference for fixed effects regressions for this analysis, the first analyses we conducted sought to compare what we found in Boston to what had been found in earlier analyses of Tallahassee data. Tallahassee has been the site of several tests of the coercive mobility thesis, and each of those tests has provided support for the thesis.

The descriptive statistics of the key variables for the Boston sample are provided in Table 17. Here, the statistics are quarterly census tract statistics reflecting the full panel for analysis which runs from 2003 to 2010 due to timing of the releases and admissions data for Boston. On average, there are approximately 47 crimes per census tract per quarter in boston, the large majority of which are nonviolent crimes. This translates into an average quarterly total crime rate of 13.45, a nonviolent crime rate of 10.34 per 1,000 residents, and a violent crime rate of 3.10. There is an average of 1.42 release and 1.77 admissions per quarter per tract – a rate of 0.43 and 0.54, respectively, per 1,000 residents. The standard deviation of both the crime and cycling variables indicates the wide dispersion of the data.

**Table 17: Boston Descriptive Statistics (Entire Panel)**

| | Mean | Std. Dev. | *N* |
|---|---|---|---|
| Total Crime | 46.66 | [39.30] | 4992 |
| Violent Crime | 10.73 | [9.02] | 4992 |
| Nonviolent Crime | 35.93 | [34.12] | 4992 |
| Releases | 1.42 | [1.96] | 4992 |
| Admissions | 1.77 | [2.17] | 4992 |
| Total Crime Rate | 13.45 | [11.99] | 4992 |
| Violent Crime Rate | 3.10 | [2.73] | 4992 |
| Nonviolent Crime Rate | 10.34 | [10.38] | 4992 |
| Release Rate | 0.43 | [0.81] | 4992 |
| Admission Rate | 0.54 | [0.80] | 4992 |
| Concentrated Disadvantage | 0.67 | [0.44] | 4992 |
| Residential Mobility | 0.51 | [0.14] | 4992 |

*Notes*: Data are quarterly census tract statistics from 2003-2010. Total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Concentrated disadvantage is the sum of share of single parents, the poverty rate, the unemployment rate, and the share black. Rates are measures per 1000 residents. Residential Mobility is the share of the census tract that moved within last 5 years.

Concentrated disadvantage is an index that consists of the sum of four variables: the share of households headed by a single parent in a tract, the tract poverty rate, the tract unemployment rate, and the share of the tract population that is black. The variable has a mean of 0.67 and a standard deviation of 0.44. Given that concentrated disadvantage has a theoretical range of 0 to 4, a mean of 0.67 and a standard deviation of 0.44 indicates that Boston has relatively few high disadvantage neighborhoods (the low mean) and a relatively wide dispersion of the characteristics across tracts (the high standard deviation relative to the mean). Residential mobility is measured by the share of the census tract that moved into the tract within the last five years. The mean of 0.51 indicates the relatively high rate of neighborhood turnover in Boston, driven in part by the student population.

Table 18 reports the correlations between the key variables. There is prima facie evidence for the validity of the coercive mobility thesis: both the release and admission rates, respectively, are positively correlated with census tract crime rates. However, admission and release rates are more highly correlated with concentrated disadvantage than with crime. This is consistent with Dhondt (2012) findings in Tallahassee neighborhoods where crime rates are weakly correlated with incarceration (r=0.31) but the percentage of the Black population (r=0.69) or the percentage of the population with no high school diploma (r=0.68) are highly correlated with prison admissions.

This simultaneously suggests the need for regression analysis and tells us where to look for coercive mobility. Given that the core coercive mobility thesis is about the disruption of informal social control caused by incarceration, it is extremely unlikely that coercive mobility will exist in neighborhoods with low levels of cycling. This is different than claiming that cycling and crime are positively correlated; instead it is a tipping point argument, where only at higher levels of cycling can we see an increase in crime caused by incarceration. The coercive mobility thesis simply should not hold in low cycling neighborhoods. The positive correlation between cycling and concentrated disadvantage suggests a way to categorize neighborhoods in a way that separates those where coercive mobility should hold from those where it should not. This is similar to Dhondt (2012) where this reasoning was tested by splitting the neighborhoods by low and high incarceration, low and high presence of female- headed households, and the percentage of the population which is Black.

**Table 18: Correlations between Key Variables in Boston**

| | Total | Nonviolent | Violent | Release | Admission | Conc. Dis. |
|---|---|---|---|---|---|---|
| Total Crime Rate | 1 | | | | | |
| Nonviolent Crime Rate | 0.98 | 1 | | | | |
| Violent Crime Rate | 0.67 | 0.51 | 1 | | | |
| Release Rate | 0.35 | 0.29 | 0.44 | 1 | | |
| Admission Rate | 0.26 | 0.18 | 0.47 | 0.61 | 1 | |
| Concentrated Disadvantage | 0.12 | -0.01 | 0.56 | 0.4 | 0.52 | 1 |

*Notes*: Data are quarterly census tract statistics from 2003-2010. Total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Concentrated disadvantage is the sum of share of single parents, the poverty rate, the unemployment rate, and the share black. Rates are measures per 1000 residents. Mobility is the share of the census tract that moved within last 5 years.

Table 19 reports the regression results for Boston. Only the key coefficients are reported, though all regressions also include concentrated disadvantage, residential mobility, year dummies, and quarter dummies as additional controls, where the quarter dummies control for seasonality in crime rates and the year dummies control for city wide trends in crime rates.[37] The first two regressions do not include tract fixed effects, while the remaining six do. As mentioned previously, standard errors are clustered on the census tract for all regressions.

It is also worth commenting again on the choice of functional form. It is entirely likely that the relationship between cycling and crime is more complicated than how we are modelling it here. We did experiment with quadratic and cubic forms in both releases and admissions with mixed benefits. Although the relationship is non-linear, it is not strongly non-linear, meaning that the additional terms did not result in a range of either admissions or releases where the basic qualitative relationship (positive or negative) reversed itself. The strength of the association has periods where it increases and decreases, but maintains the same overall effect as the linear models. Similarly, adding additional lags of the cycling variables to more adequately explore the dynamics of the relationship may yield interesting nuances, but our cursory investigation suggests it does not alter the basic relationships seen below.

[37] We also tested models that include the tract specific time-trend discussed in the analytical methods section. In Boston, the inclusion of tract specific time trends has almost no quantitative impact, and does not change the qualitative results at all, thus we chose to omit them in this analysis.

### Table 19: Regression Results (Boston)

| | Full Sample | Fixed Effects | Bottom 1% | Bottom 95% | High/Low | High Dis. | Low Dis. |
|---|---|---|---|---|---|---|---|
| Release Rate$_{t-1}$ | 4.237** | -0.196 | 0.011 | 0.055 | -0.188 | -0.66 | 0.015 |
| | [1.996] | [0.237] | [0.098] | [0.147] | [0.256] | [0.715] | [0.094] |
| Admission Rate$_{t-1}$ | 1.860** | 0.143 | 0.204 | 0.022 | 0.197 | -0.316 | 0.304 |
| | [0.811] | [0.214] | [0.207] | [0.158] | [0.219] | [0.332] | [0.244] |
| *N* | 4836 | 4836 | 4805 | 4619 | 4741 | 1612 | 1612 |
| R-square | 0.197 | 0.918 | 0.887 | 0.817 | 0.907 | 0.94 | 0.812 |
| Fixed Effects | No | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: Regressions are OLS regressions with standard errors clustered on the census tract. The dependent variable is the total crime rate per 1000. Additional controls include Concentrated Disadvantage, residential mobility, year dummies, and quarter dummies. Total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Concentrated disadvantage is the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Mobility is the share of the census tract that moved within last 5 years. Standard errors are reported in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

Column 1 reports the baseline results with the cycling variables – the lag of releases and admissions, respectively – in addition to the other controls. The coefficient on the release rate is statistically and large, implying that a 1 unit increase in the release rate is associated with a 3.92 increase in the total crime rate. The coefficient on admissions is not signficant, but is still quite large. Although most regression models for crime include a lag of the dependent variable as an explanatory variable, we omit it here because the lag is correlated with the fixed effects, which biases the coefficients.

Column 2 introduces census tract fixed effects. The failure to control for unobserved time-invariant census tract characteristics, which is what the fixed effects accomplish, has a significant impact on the coefficients on both cycling and the lag of total crime. The coefficient on releases becomes negative and insignificant. The coefficient on admissions is 10 percent the size it used to be, remaining statistically insignificant.

As is typical, the respective distributions of crime rates, release rates, and admission rates are very right skewed. This raises concerns about the influence of outliers; columns 3 through 5 address this problem in different ways. Columns 3 and 4 trim total crime by discarding the top 1 percent and 5 of neighborhoods by average total crime over the entire period. Both the effect of releases and admissions on total crime are sensitive to outliers. In the full model, the estimated impact of releases on total crime is insignificant and negative, while it is positive but still insignificant when outliers are discarded. The effect of admissions increases slightly when the top 1 percent of census tracts are discarded, but decreases considerably when the top 5 percent of total crime neighborhoods are discarded. Given the considerable sensitivity of admissions to excluding outliers in total crime, column 5 uses a joint hypothesis for excluding outliers: observations that are in both the top 5 percent of total crime and the bottom 5 percent of admissions, or the bottom 5 percent of total crime and the top 5 percent of admissions. In other words, we discard high admission, low crime neighborhoods and low admission, high crime neighborhoods. Here, the coefficient on releases decreases relative to the results in columns 3 and 4, but is quite similar to the full sample, while the coefficient on admissions increases in magnitude considerably and but again remains insignificant. This result implies that the average census tract exhibits a mild form of coercive mobility, but that there are a small number of tracts with the opposite pattern.

If considering only columns 2 to 5, one would likely conclude that the coercive mobility thesis is present, but not robust. However, implicit in the coercive mobility thesis is the notion that there should be a tipping point. A neighborhood should be able to withstand a low level of cycling regardless of the characteristics of the neighborhood, but marginalized neighborhoods should have more difficulty coping with cycling. This suggests that the proper test of the coercive mobility thesis is to divide the sample into neighborhoods where cycling should and should not be a problem. We use the index of concentrated disadvantage to divide the sample into three groups based on their level of disadvantage, and estimate the same model within the low and high group, discarding the middle Because concentrated disadvantage is based on a set of related but not completely overlapping variables, dividing the sample based on concentrated disadvantage could result in groups of neighborhoods that are qualitatively unalike. Discarding the middle group minimizes this concern because the resulting groups have largely either low or high values on most or all of the components of concentrated disadvantage.

The results of this exercise are in columns 6 and 7 of Table 19. In column 6, the neighborhoods with low concentrated disadvantage, there is no evidence of coercive mobility. The effect of releases and admissions, respectively, are negative and insignificant. Thus in neighborhoods with low levels of disadvantage, cycling in and out of prison reduces the crime rate. Column 7 presents the results of the census tracts in the top one-third of the concentrated disadvantage distribution. What is striking is that the signs on both releases and admissions flip. The effect on crime of releases is positive but not significant and the effect on crime of admissions is positive and significant. Further, though the cycling variables are insignificant for both low and high disadvantage neighborhoods, the coefficients for the low disadvantage neighborhoods are statistically significantly larger than those for the low disadvantage neighborhoods. This is fairly strong support for the coercive mobility thesis, and strong evidence that the results are not driven by simultaneity bias, otherwise at least admissions should always have a positive coefficient.

### Table 20: Tallahassee Replication with Boston Data

| | Baseline | Cycling |
|---|---|---|
| Log Population | -1.522** | -1.662** |
| | [0.702] | [0.667] |
| Concentrated Disadvantage | 4.327*** | 4.020*** |
| | [0.765] | [0.842] |
| Residential Mobility | 4.152 | 5.125 |
| | [3.345] | [3.236] |
| Release Rate | | -0.092 |
| | | [0.310] |
| Admission Rate | | 1.620** |
| | | [0.708] |
| Admit Rate Squared | | -0.639* |
| | | [0.342] |
| Admit Rate Cubed | | 0.046* |
| | | [0.024] |
| *N* | 4836 | 4836 |

*Notes*: Data are yearly census tract statistics from 2003-2010 for Boston. Model specification based on Clear et al. (2003). Regressions are negative binomial regressions with standard errors clustered on the census tract. Average marginal effects are reported. Significance levels: *10%, ** 5%, and ***1%.

The regressions in Table 19 are broadly consistent with the original regressions in Clear et. al. (2003), however, they are estimated in a completely different way. Specifically, Clear et al. (2003) estimate a generalized linear model with a two year panel using yearly data, and a cubic in admissions. As a robustness check, although we strongly prefer the estimation technique in table 19, we replicate the estimation technique in Clear et. al. (2003) as closely as possible in Table 20. The results, contained in table 20, are qualitatively the same. The effect of releases is insignificant, while the effect of admissions is cubic where high and low levels of admissions are positively associated with crime, while medium levels of admissions are negatively associated with crime.

## The Three-City Comparison: Boston, Newark, Trenton

The baseline regression results for all three cities, those from estimating equations 2, are given in Table 21. The results for Trenton are broadly consistent with the coercive mobility thesis. The results for Boston, as discussed above, are not consistent with the coercive mobility thesis overall, but are within high disadvantage neighborhoods. Though the release rate is statistically insignificant at conventional levels in both cities, the coefficient is for Trenton. In Trenton, a one unit increase in the annual admission rate is associated with an increase in the annual crime rate of 7.6, on an annual rate of 27.36. In Newark, on the other hand, increases in release rate are associated with a statistically significant decrease in total crime. A one unit increase in the quarterly release rate is associated with a 0.178 unit decrease in the quarterly crime rate, for an average quarterly crime rate of 13.06. The admissions rate in Newark is associated with a very small and statistically significant increase in the total crime rate in Newark. Note that the magnitudes of the coefficients between Newark and Boston on the one hand, and Trenton on the other, are not directly comparable because Trenton are annual data while Newark and Boston are quarterly.

**Table 21: Baseline Regression (Boston, Newark, and Trenton)**

| | Boston | Newark | Trenton |
|---|---|---|---|
| Release Rate$_{t-1}$ | -0.196 | -0.178** | 2.304 |
| | [0.233] | [0.086] | [1.429] |
| Admission Rate$_{t-1}$ | 0.143 | 0.053 | 7.613*** |
| | [0.211] | [0.214] | [2.309] |
| *N* | 4836 | 1350 | 33 |
| R-square | 0.181 | 0.103 | 0.677 |

*Notes*: Regressions are OLS regressions with standard errors clustered on the census tract. The dependent variable is the total crime rate per 1000. Additional controls include Concentrated Disadvantage, residential mobility, year dummies, and quarter dummies. Total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Concentrated disadvantage is the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Mobility is the share of the census tract that moved within last 5 years. Standard errors are reported in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

## Further Investigation

As mentioned above, the coercive mobility argument is a tipping point argument. There are particular types of neighborhoods, and particular types of situations where this should occur. If we think of Boston and Newark as a continuum of neighborhood types, then Boston would have low and medium disadvantage neighborhoods, while Newark will have largely high disadvantage neighborhoods. That is, based on statistics like median tract income or share of a tract in poverty, there is almost no overlap between Newark and Boston. Newark's highest income neighborhoods have roughly the same median income as Boston's median neighborhood. Further, Boston goes through a period of significant changes in many neighborhoods during the time period covered by the data, while Newark is largely stagnant. This pattern suggests that, if we divide Newark and Boston, respectively, into high and low disadvantage neighborhoods, that we should see coercive mobility in the high disadvantage neighborhoods in Boston as has been demonstrated already, we may see it in the low disadvantage neighborhoods in Newark, but we should not see it in the low disadvantage neighborhoods in Boston nor the high disadvantage neighborhoods in Newark. Put simply, this is very close to the pattern we see (See Table 22).

**Table 22: High and Low Disadvantage Neighborhoods (Boston and Newark)**

| | Boston | | Newark | |
|---|---|---|---|---|
| | Low | High | Low | High |
| Release Rate$_{t-1}$ | -0.660 | 0.015 | -0.330* | -0.273* |
| | [0.704] | [0.093] | [0.173] | [0.141] |
| Admission Rate$_{t-1}$ | -0.316 | 0.304 | -0.479 | -0.070 |
| | [0.326] | [0.240] | [0.456] | [0.198] |
| *N* | 1612 | 1612 | 450 | 450 |
| R-square | 0.246 | 0.168 | 0.141 | 0.13 |

*Notes*: Regressions are OLS regressions with standard errors clustered on the census tract. The dependent variable is the total crime rate per 1000. Additional controls include Concentrated Disadvantage, residential mobility, year dummies, and quarter dummies. Total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Concentrated disadvantage is the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Mobility is the share of the census tract that moved within last 5 years. Standard errors are reported in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

As seen earlier, the coefficients on releases and admissions are negative in low disadvantage neighborhoods in Boston, while they are positive in high disadvantage neighborhoods.[38] In

[38] The coefficients on releases and admissions are different for Boston than those presented above in Tables 15 and 16. This is because we do not include the lag of the dependent variable in the three city regressions. There is some concern that it is inappropriate to include both lags of the dependent variable and fixed effects in the same regression. We include the lag in earlier results because this is what the discipline is used to seeing. We exclude the lag here to illustrate the effect of the bias. Intuitively, the problem is that a lag of the dependent variable is correlated with the fixed effects, leading to biased

Newark, both coefficients are negative in low disadvantage neighborhoods. They are also negative in high disadvantage neighborhoods, though they are closer to zero, reflecting relative lack of variation in crime rates across the high disadvantage neighborhoods. In other words, high disadvantage neighborhoods in Newark have been so stagnant and high crime rates are so entrenched that, although they are high cycling neighborhoods, the estimated effect of cycling is weaker.

## The Effect of Prison Cycling on Violent and Property Crime Rates

For the discussion above, we conducted all analyses using total crime (measured as index crime) as the dependent variable. Total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, motor vehicle theft, and arson. We also conducted the analyses for Boston (Tables 23 and 24), Newark (Tables 25 and 26) and Trenton (Table 27) separately for violent and property crimes. Violent crime is the sum of homicide, rape, robbery, and aggravated assault. Property crime is the sum of burglary, larceny/theft, motor vehicle theft, and arson.

In each of the tables that follows: data are quarterly counts of crime by census tract. The dependent variable is either violent crime or property crime (as specified in the title), measured as counts for Poisson and Negative Binomial regressions, and rate per 1000 people for OLS and GLM regressions. Releases and admissions are counts for Poisson and Negative Binomial regressions, and rates per 1000 for OLS and GLM regressions. Additional controls come from the 2010 Census Summary File 3 and include concentrated disadvantage, share of the census tract population that is black, the share of the population that are renters, quarter dummies, a tract specific linear time trend, and the log of census tract population for the Poisson and Negative Binomial Regressions.

**Table 23: Model Comparisons for Releases and Admissions with Violent Crime as the Dependent Variable (Boston)**

| | OLS FE | GLM FE | Poisson FE | Neg. Bin. FE | OLS RE | Poisson RE | Neg. Bin. RE |
|---|---|---|---|---|---|---|---|
| Admission Rate$_{t-1}$ | 0.077 | 0.077 | 0.009 | 0.010 | 0.153* | 0.019** | 0.023** |
| | [0.098] | [0.096] | [0.010] | [0.012] | [0.089] | [0.008] | [0.009] |

coefficient (Angrist & Pischke, 2009). The coefficients for Boston are different, but the qualitative story remains unchanged.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Release Rate$_{t-1}$ | 0.045 | 0.045 | 0.019* | 0.019** | 0.064 | 0.014* | 0.015 |
| | [0.059] | [0.058] | [0.010] | [0.009] | [0.050] | [0.008] | [0.010] |
| Constant | 4.561 | 3.123 | 2.710 | 2.747 | 5.874*** | -1.933** | -0.616 |
| | [3.102] | [3.351] | [2.432] | [3.825] | [0.942] | [0.915] | [0.815] |
| *N* | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 |

*Notes*: For OLS and GLM models, the dependent variable is in rates. For Poisson and Negative Binomial, the dependent variable is in levels. Standard errors are in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

## Table 24: Model Comparisons for Releases and Admissions with Property Crime as the Dependent Variable (Boston)

| | OLS FE | GLM FE | Poisson FE | Neg. Bin. FE | OLS RE | Poisson RE | Neg. Bin. RE |
|---|---|---|---|---|---|---|---|
| Admission Rate$_{t-1}$ | 0.067 | 0.067 | 0.002 | 0.005 | 0.213 | 0.012** | 0.017** |
| | [0.153] | [0.150] | [0.008] | [0.008] | [0.149] | [0.005] | [0.008] |
| Release Rate$_{t-1}$ | -0.244 | -0.244 | 0.000 | 0.002 | -0.295 | -0.006 | -0.004 |
| | [0.211] | [0.208] | [0.008] | [0.006] | [0.200] | [0.004] | [0.008] |
| Constant | -5.373 | -8.958 | 1.367 | 1.496 | 11.068*** | -0.607 | 0.843 |
| | [7.867] | [8.793] | [1.745] | [3.241] | [3.502] | [0.871] | [0.575] |
| *N* | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 |

Notes: For OLS and GLM models, the dependent variable is in rates. For Poisson and Negative Binomial, the dependent variable is in levels. Standard errors are in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

## Table 25: Model Comparisons for Admissions and Releases with Violent Crime as the Dependent Variable (Newark)

| | OLS FE | GLM FE | Poisson FE | Neg. Bin. FE | OLS RE | Poisson RE | Neg. Bin. RE |
|---|---|---|---|---|---|---|---|
| Admission Rate$_{t-1}$ | -0.079 | -0.079 | -0.017 | -0.017 | -0.003 | -0.009 | -0.005 |
| | [0.075] | [0.072] | [0.014] | [0.016] | [0.072] | [0.013] | [0.014] |
| Release Rate$_{t-1}$ | -0.022 | -0.022 | 0.002 | 0.001 | -0.009 | 0.002 | 0.002 |
| | [0.037] | [0.036] | [0.007] | [0.007] | [0.030] | [0.006] | [0.007] |
| Constant | -10.533 | -13.873 | -17.612 | -17.532 | -7.739*** | -3.064*** | -1.542 |
| | [20.175] | [20.552] | [12.189] | [17.123] | [2.861] | [1.065] | [1.094] |
| *N* | 1350 | 1350 | 1350 | 1350 | 1350 | 1350 | 1350 |

*Notes*: For OLS and GLM models, the dependent variable is in rates. For Poisson and Negative Binomial, the dependent variable is in levels. Standard errors are in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

## Table 26: Model Comparisons for Admissions and Releases with Property Crime as the Dependent Variable (Newark)

| | OLS FE | GLM FE | Poisson FE | Neg. Bin. FE | OLS RE | Poisson RE | Neg. Bin. RE |
|---|---|---|---|---|---|---|---|
| Admission Rate$_{t-1}$ | 0.165 | 0.165 | 0.009 | 0.008 | 0.236 | 0.011 | 0.012 |
| | [0.210] | [0.202] | [0.012] | [0.012] | [0.217] | [0.007] | [0.011] |
| Release Rate$_{t-1}$ | -0.157*** | -0.157*** | -0.004 | -0.004 | -0.126*** | -0.004 | -0.002 |
| | [0.058] | [0.056] | [0.004] | [0.004] | [0.048] | [0.003] | [0.005] |
| Constant | -28.843 | -36.667 | -6.119 | -7.322 | 13.618** | 0.438 | 0.837 |
| | [58.583] | [59.661] | [8.036] | [9.730] | [5.682] | [0.902] | [0.859] |
| *N* | 1350 | 1350 | 1350 | 1350 | 1350 | 1350 | 1350 |

*Notes*: For OLS and GLM models, the dependent variable is in rates. For Poisson and Negative Binomial, the dependent variable is in levels. Standard errors are in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

**Table 27: Regressions by Type of Crime (Total, Property, and Violent) for Trenton**

| | Total | Nonviolent | Violent |
|---|---|---|---|
| Release Rate$_{t-1}$ | 2.304 | 1.916* | 0.388 |
| | [1.429] | [1.104] | [0.377] |
| Admission Rate$_{t-1}$ | 7.613*** | 5.951*** | 1.662** |
| | [2.309] | [1.785] | [0.609] |
| Constant | -9.295 | -7.411 | -1.883 |
| | [5.912] | [4.568] | [1.560] |
| *N* | 33 | 33 | 33 |
| R-Square | 0.677 | 0.692 | 0.548 |

*Notes*: Regressions are OLS regressions. The dependent variables are the total crime rate, the property crime rate, and the violent crime rate respectively per 1000. Additional controls include Concentrated Disadvantage, residential mobility, and year dummies. Concentrated disadvantage is the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Mobility is the share of the census tract that moved within last 5 years. Standard errors are reported in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

For both Newark and Boston, the results for nonviolent crime is broadly similar to the results for total crime, which is to be expected given the large share of nonviolent crime in total crime. In Boston, the coefficient on the release rate is negative for violent crime, but it remains insignificant in both cases. In Newark, the coefficient on the admissions rate is positive for violent crime but negative for nonviolent crime. Both admissions and releases show a stronger effect on violent crime than on nonviolent crime in Newark. Overall, however, the evidence that cycling has a different impact on different crime rates is not strong.

## Parole Only Releases

As explained above in the data section, for New Jersey releases, we had to use only New Jersey State Parole data and these data only included those actually released to parole (excluding all max outs). We recognized that this would only be an issue if there is a good reason to believe that the patterns of rates of releases for paroled offenders were systematically different from the patterns of rates of releases for offenders who max-out.[39] Although we had no reason to believe that there would be systematic differences in the rates at which paroled and max-out offenders

[39] Some recent research using New Jersey parole data suggests that there are some significant differences in re-offense rates for those release to parole versus those who max out and are released unconditionally (Ostermann, 2011).

were released to particular neighborhoods, we took advantage of having both types of data in Massachusetts and ran the models for Boston using all releases and using parole only releases (see Tables 28 and 29).

### Table 28: Model Comparisons for Cycling with Total Crime as the Dependent Variable Using Parole Only Data (Boston)

| | OLS FE | GLM FE | Poisson FE | Neg. Bin. FE | OLS RE | Poisson RE | Neg. Bin. RE |
|---|---|---|---|---|---|---|---|
| Cycle Rate$_{t-1}$ | -0.044 | -0.044 | -0.001 | 0.000 | -0.024 | 0.000 | 0.001 |
| | [0.066] | [0.065] | [0.001] | [0.001] | [0.065] | [0.001] | [0.002] |
| Constant | -1.135 | -6.272 | 1.763 | 2.159 | 17.288*** | -0.370 | 1.050* |
| | [9.965] | [11.084] | [1.513] | [3.040] | [4.091] | [0.857] | [0.569] |
| *N* | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 |

*Notes*: For OLS and GLM models, the dependent variable is in rates. For Poisson and Negative Binomial, the dependent variable is in levels. Standard errors are in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

### Table 29: Model Comparisons for Releases and Admissions with Total Crime as the Dependent Variable Using Parole Only Data (Boston)

| | OLS FE | GLM FE | Poisson FE | Neg. Bin. FE | OLS RE | Poisson RE | Neg. Bin. RE |
|---|---|---|---|---|---|---|---|
| Admission Rate$_{t-1}$ | 0.147 | 0.147 | 0.004 | 0.006 | 0.350 | 0.015*** | 0.019*** |
| | [0.217] | [0.213] | [0.007] | [0.007] | [0.215] | [0.004] | [0.007] |
| Release Rate$_{t-1}$ | -0.300 | -0.300 | -0.006 | -0.004 | -0.423 | -0.013*** | -0.015* |
| | [0.406] | [0.398] | [0.007] | [0.009] | [0.379] | [0.004] | [0.009] |
| Constant | -1.012 | -6.057 | 1.715 | 2.092 | 17.298*** | -0.432 | 0.960* |
| | [9.878] | [10.964] | [1.513] | [3.028] | [4.070] | [0.859] | [0.570] |
| *N* | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 | 4836 |

*Notes*: For OLS and GLM models, the dependent variable is in rates. For Poisson and Negative Binomial, the dependent variable is in levels. Standard errors are in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

The results of the analyses of parole only releases, presented below, were substantially the same as the results of the analyses for all offenders (discussed in all models above). We therefore felt more confident in our New Jersey results that included only parolee data.

## Unincorporated (Rural) New Jersey

We also estimated the same basic set of models for unincorporated (rural) New Jersey as we did for Boston and Newark. The baseline model using the full sample is reported in Table 30, is our preferred OLS fixed effects specification. The effect of releases on total crime is positive, not statistically significant, and in the ballpark of that found in Boston. The coefficient on admissions is negative, statistically significant , and has a considerably larger effect than that found in either Newark or Boston.[40]

**Table 30: Baseline Regression (Rural New Jersey)**

| | OLS FE |
|---|---|
| Release $Rate_{t-1}$ | 0.159 |
| | [0.338] |
| Admission $Rate_{t-1}$ | -0.426** |
| | [0.168] |
| *N* | 813 |
| R-Square | 0.069 |

*Notes*: Regressions are OLS regressions. The dependent variables are the total crime rate, the property crime rate, and the violent crime rate respectively per 1000. Additional controls include Concentrated Disadvantage, residential mobility, and year dummies. Concentrated disadvantage is the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Mobility is the share of the census tract that moved within last 5 years. Standard errors are reported in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

We did not perform the same set of robustness checks with this data as we did with either the Newark or Boston data. The primary reason for this is because the release and admissions data in unincorporated New Jersey is fairly thin. Dividing the sample by high and low disadvantage neighborhoods results in very few low disadvantage neighborhoods with any admissions or releases at all. This also implies that one should take the rural results with a grain of salt. This suggests that the prison cycling argument is indeed an urban argument relevant to densely populated places, and not a spatial argument related to geographical areas. Taking data quality problems into account, the results in rural New Jersey are qualitatively more similar to the results found in Newark than those found in Boston.

## Arellano-Bond

As discussed above, one of the critical issues in estimating neighborhood level crime rates is dealing with persistent and systematic neighborhood heterogeneity. We have already argued that

[40] We should note that the crime data for rural New Jersey were not particularly good. Likely because of the way in which state police record crime data (often noting just a street name, marker, or intersection), the geocoding match rate was significantly lower for the New Jersey State Police dataset than for any other dataset.

fixed effects models are the most appropriate in this context. However, in addition to the difficulty that fixed effects present for estimating the coefficients on time-invariant variables, it is improper to use a lagged value of the dependent variable at the same time as fixed effects. Given the frequent use of lagged dependent variables in neighborhood level analyses of crime, this is an apparent draw back.

The Arellano-Bond (Arellano and Bond 1991) dynamic panel data estimation technique provides a method to overcome this problem. Using a lagged dependent variable with fixed effects introduces correlation with the error term, which makes the estimated coefficients inconsistent. Under a set of fairly weak assumptions, it is possible to use higher period lags of the dependent variable as an instrument for the first period lag, resulting in consistent estimation of the coefficients. It should be noted, however, that Arellano-Bond models are particularly sensitive to specification, so should always be interpreted with caution.

### Table 31: Arrellano-Bond Regression (Boston)

| | Boston | | |
|---|---|---|---|
| | All | Low | High |
| Release Rate$_{t-1}$ | -0.228 | -0.97 | -0.122 |
| | [0.167] | [0.725] | [0.148] |
| Admission Rate$_{t-1}$ | -0.063 | -0.525 | 0.081 |
| | [0.256] | [0.426] | [0.292] |
| *N* | 4680 | 1560 | 1560 |

*Notes*: Data are quarterly census tract statistics from 2003-2010. Regressions are Arellano-Bond regressions with standard errors clustered on the census tract. Three lags of the release rate and admissions rate, respectively, are used as instruments. The dependent variable is the total crime rate per 1000. Additional controls include Concentrated Disadvantage, residential mobility, year dummies, and quarter dummies. Total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Concentrated disadvantage is the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Mobility is the share of the census tract that moved within last 5 years. Standard errors are reported in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

### Table 32: Arrellano-Bond Regression (Newark)

| | Newark | | |
|---|---|---|---|
| | All | Low | High |
| Release Rate$_{t-1}$ | -0.135 | -0.301 | -0.237* |
| | [0.095] | [0.216] | [0.133] |
| Admission Rate$_{t-1}$ | 0.033 | -0.637 | -0.093 |
| | [0.192] | [0.426] | [0.178] |
| *N* | 1260 | 420 | 420 |

*Notes*: Data are quarterly census tract statistics from 2007-2010. Regressions are Arellano-Bond regressions with standard errors clustered on the census tract. Three lags of the release rate and admissions rate, respectively, are used as instruments. The dependent variable is the total crime rate per 1000. Additional controls include Concentrated Disadvantage, residential mobility, year dummies, and quarter dummies. Total crime is the sum of homicide, rape, robbery, aggravated assault, burglary, larceny/theft, and vehicle theft. Concentrated disadvantage is the sum of the share of single parents, the poverty rate, the unemployment rate, and the share black. Mobility is the share of the census tract that moved within last 5 years. Standard errors are reported in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

Tables 31 and 32 show the results of an Arellano-Bond estimation for Boston and Newark, respectively, replicating the results in Tables 21 and 22. The results are qualitatively similar to those reported for the standard fixed effects model in Table 21, though they differ quantitatively. First, for Boston, the coefficient on both the release rate and the admissions rate are negative in both models. . For Newark, the coefficient on releases negative in both cases, while the coefficient on admissions is very small but positive using the Arellano-Bond model but was previously slightly negative.

Dividing the sample into low and high disadvantage neighborhoods also yields a similar qualitative pattern but a different quantitative pattern. For Boston, the coefficient on releases remains negative in both low and high disadvantage neighborhoods, but again is insignificant in both categories, while with fixed effects it is positive in high disadvantage neighborhoods but also insignificant. The results for Newark are again very similar across the two estimation techniques, with the coefficient on releases negative in both high and low disadvantage neighborhoods, and the coefficient on admissions negative and insignificant in both high and low disadvantage neighborhoods. It is important to note that part of the difference in significance may be due in part to differences in how the standard errors are calculated across the two techniques. While, as discussed, the standard errors for the fixed effects models are robust to serial correlation, heteroskedasticity, and neighborhood level clustering, the Arellano-Bond standard errors are only robust to serial correlation and heteroskedasticity. Although clustering problems typically results in standard errors that are too small, sometimes the opposite can occur.

In summary, the results across the two estimation techniques are broadly similar to each other. The differences that do arise are limited to coefficients that are neither statistically different from zero nor from each other. In situations where the specific magnitude of a coefficient is of secondary importance to its sign and significance, we favor the fixed effects model. This is due primarily to its familiarity, ease of interpretation, and ease of implementation. Although most commonly used statistical packages have programs to estimate Arellano-Bond models, models of this type are still an active area of research and their statistical properties are not well understood. As Greene (2002) showed for a number of non-linear models, when using fixed effects one needs to use extra caution, as a number of models that were previously thought to be well-behaved appear to have problems with fixed effects.

## Panel Vector Auto-Regression

As suggested above, the frequency with which our data is collected combined with the use of fixed effects regressions likely removes any concern about reverse causation. And, the fixed effects should absorb much of the remaining simultaneity bias, as suggested by the difference in the coefficients between the fixed and random effects models. However, absent a suitable instrument, we have no other method of assessing the potential impact of the remaining endogeneity bias.

As an alternative, we estimate a series of panel vector autoregression models. As discussed above, panel vector autoregression (PVAR) is a simultaneous equations estimation technique that, rather than trying to remove the effect of bias in the face of simultaneously determined variables, uses the simultaneity to estimate the relationship between a potentially large set of variables. Typically, one would be able to use both endogenous and exogenous variables within a VAR model. Unfortunately, to the author's knowledge there is no existing estimation technique that allows one to include exogenous variables in a PVAR setup, so the models that follow include only the three (potentially) endogenous variables: the crime rate, the admissions rate, and the release rate. It is the admissions rate, in particular, that receives so much attention as it is definitely the case that, given a sufficiently long time frame and sufficiently large geographic area, an increase in crime leads to an increase in admissions.

Tables 33 and 34 report the results of the PVAR estimations. The PVAR models regress each of the endogenous variables of lags of the given variable, plus lags of all of the other endogenous variables. If the lags of the variable are statistically significant, they are said to cause the dependent variable in the sense that they tend to change chronologically before the dependent variable. We first deseasonalized each of the variables to remove the effect of quarterly variation due only to seasons. We then verified that crime rates and release rates do not have unit roots using a variant of the Augmented Dickey-Fuller test for panel data where the data have been demeaned to account for the fixed effect and the test allows for drift.[41] We then estimate, simultaneously, each of the three endogenous variables as a function of three lags of the variable plus three lags of the other two variables.

Table 33 shows the results for Boston. Consistent with the results from the fixed effects models presented earlier, there is considerable persistence in neighborhood level crime rates as evidenced by the significant coefficients on the lags of total crime in the total crime equation. Also consistent with previous results, neither the admission rate nor the release rate have a statistically significant impact on total crime. This equation is, however, essentially identical to

[41] For all three variables – total crime rate, release rate, and the admissions rate – we reject the null that the variable has a unit root in both Boston and Newark at all conventional levels of significance.

what was estimated earlier, so it is no surprise that the results are broadly the same. The other two equations – for admissions and releases, respectively – are more important. Table 34 shows the results for Newark, which are broadly similar. Here, we see that the total crime rate does not have a statistically significant impact on either admissions or releases. This is strong evidence that our data do not suffer from an endogeneity problem.

Tables 33 and 34 also split the sample into high and low disadvantage neighborhoods for Boston and Newark, respectively. Again, looking at the equations for admissions and releases, there is no evidence of endogeneity. The lags of the total crime rate are not statistically significant in any of the regressions.

### Table 33: PVAR Results (Boston)

| | Full Sample | | | Low Disadvantage | | | High Disadvantage | | |
|---|---|---|---|---|---|---|---|---|---|
| | Total Crime | Admissions | Releases | Total Crime | Admissions | Releases | Total Crime | Admissions | Releases |
| Crime Rate$_{t-1}$ | 0.501*** | 0.018 | -0.03 | 0.466*** | 0.002 | 0.000 | 0.504*** | 0.028 | -0.05 |
| | 0.103 | 0.017 | 0.026 | 0.040 | 0.001 | 0.001 | 0.041 | 0.041 | 0.041 |
| Admit Rate$_{t-1}$ | 0.743 | 0.213* | -0.10 | 0.359 | 0.104*** | 0.002 | 0.664 | 0.189** | -0.02 |
| | 0.700 | 0.118 | 0.194 | 0.444 | 0.045 | 0.039 | 0.546 | 0.088 | 0.142 |
| Release Rate$_{t-1}$ | 0.693 | 0.220 | -0.11 | 0.216 | 0.065 | -0.05 | 0.552 | 0.195 | -0.00 |
| | 1.092 | 0.187 | 0.303 | 0.563 | 0.050 | 0.051 | 0.956 | 0.161 | 0.261 |
| Crime Rate$_{t-2}$ | 0.146** | 0.004 | -0.02 | 0.111*** | -0.00 | 0.000 | 0.157 | 0.006 | -0.03 |
| | 0.065 | 0.010 | 0.016 | 0.031 | 0.001 | 0.001 | 0.104 | 0.017 | 0.026 |
| Admit Rate$_{t-2}$ | 0.470 | 0.175* | -0.00 | 0.528 | 0.042 | 0.009 | 0.339 | 0.161** | 0.073 |
| | 0.543 | 0.095 | 0.131 | 0.382 | 0.037 | 0.033 | 0.424 | 0.070 | 0.087 |
| Release Rate$_{t-2}$ | 0.737 | 0.248 | -0.11 | 0.518 | 0.091 | 0.030 | 0.630 | 0.236 | -0.05 |
| | 0.910 | 0.168 | 0.221 | 0.484 | 0.057 | 0.051 | 0.801 | 0.148 | 0.183 |
| Crime Rate$_{t-3}$ | 0.267*** | 0.012 | -0.02 | 0.219*** | 0.000 | -0.00** | 0.282** | 0.018 | -0.03 |
| | 0.072 | 0.013 | 0.019 | 0.035 | 0.001 | 0.001 | 0.116 | 0.021 | 0.032 |
| Admit Rate$_{t-3}$ | 0.045 | 0.148*** | 0.032 | 0.754*** | 0.086 | 0.070** | -0.16 | 0.131*** | 0.081 |
| | 0.329 | 0.059 | 0.083 | 0.363 | 0.037 | 0.033 | 0.264 | 0.047 | 0.062 |
| Release Rate$_{t-3}$ | 0.169 | 0.101 | -0.03 | 0.989*** | 0.133*** | -0.00 | -0.07 | 0.063 | 0.029 |
| | 0.672 | 0.114 | 0.190 | 0.492 | 0.052 | 0.046 | 0.594 | 0.098 | 0.168 |
| N | 6232 | 6232 | 6232 | 3152 | 3152 | 3152 | 3080 | 3080 | 3080 |

*Notes*: Data are quarterly counts of crime by census tract. Standard errors are in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

## Table 34: PVAR Results (Newark)

| | Full Sample | | | Low Disadvantage | | | High Disadvantage | | |
|---|---|---|---|---|---|---|---|---|---|
| | Total Crime | Admissions | Releases | Total Crime | Admissions | Releases | Total Crime | Admissions | Releases |
| Crime Rate$_{t-1}$ | -0.13 | -0.00 | 0.290*** | -0.14 | -0.01 | 0.212* | -0.14 | -0.00 | 0.376*** |
| | 0.18 | 0.026 | 0.093 | 0.322 | 0.027 | 0.114 | 0.168 | 0.041 | 0.126 |
| Admit Rate$_{t-1}$ | -0.53** | 0.399*** | 0.234 | -0.84 | 0.318*** | 0.307 | -0.49** | 0.418*** | 0.281 |
| | 0.23 | 0.056 | 0.194 | 0.587 | 0.067 | 0.221 | 0.233 | 0.071 | 0.249 |
| Release Rate$_{t-1}$ | -0.15 | 0.035* | -0.49*** | -0.50 | 0.038 | -0.74*** | -0.13 | 0.035 | -0.46*** |
| | 0.11 | 0.019 | 0.105 | 0.605 | 0.044 | 0.236 | 0.100 | 0.022 | 0.123 |
| Crime Rate$_{t-2}$ | -0.17 | -0.00 | 0.190** | -0.18 | -0.01 | 0.138* | -0.18 | 0.002 | 0.249** |
| | 0.12 | 0.019 | 0.074 | 0.208 | 0.017 | 0.083 | 0.123 | 0.032 | 0.105 |
| Admit Rate$_{t-2}$ | -0.42* | 0.266*** | 0.473*** | -0.63 | 0.192*** | 0.418* | -0.40 | 0.281*** | 0.516** |
| | 0.25 | 0.05 | 0.174 | 0.744 | 0.061 | 0.238 | 0.258 | 0.057 | 0.210 |
| Release Rate$_{t-2}$ | -0.05 | 0.058** | -0.42*** | -0.38 | 0.043 | -0.64** | -0.03 | 0.061* | -0.39*** |
| | 0.11 | 0.027 | 0.088 | 0.562 | 0.053 | 0.255 | 0.101 | 0.031 | 0.099 |
| Crime Rate$_{t-3}$ | -0.12 | -0.00 | 0.185*** | -0.15 | -0.01 | 0.156** | -0.11 | -0.00 | 0.226** |
| | 0.11 | 0.019 | 0.065 | 0.206 | 0.018 | 0.076 | 0.103 | 0.029 | 0.091 |
| Admit Rate$_{t-3}$ | -0.43** | 0.122*** | 0.469*** | -0.98* | 0.208*** | 0.475** | -0.35* | 0.104** | 0.500*** |
| | 0.197 | 0.038 | 0.137 | 0.571 | 0.049 | 0.233 | 0.199 | 0.046 | 0.170 |
| Release Rate$_{t-3}$ | -0.06 | 0.028 | -0.42*** | -0.27 | 0.024 | -0.62*** | -0.04 | 0.028 | -0.40*** |
| | 0.106 | 0.022 | 0.081 | 0.536 | 0.047 | 0.219 | 0.103 | 0.024 | 0.087 |
| N | 1456 | 1456 | 1456 | 729 | 729 | 729 | 727 | 727 | 727 |

*Notes*: Data are quarterly counts of crime by census tract. Standard errors are in brackets. Significance levels: * 10%, ** 5%, and *** 1%.

In total, the results support our assertion that fixed effects combined with the sampling frame of our data remove concerns about simultaneity bias. There is no empirical evidence from the PVAR regressions suggesting that there is a statistically significant relationship running from crime to either releases or admissions, holding lagged values of releases and admissions fixed.

## CONCLUSIONS

The purpose of this study was to determine the impact of "prison cycling"—that is, removing people from the community into prison and then, some time later, returning them to the community—on crime rates. In designing this study, we relied heavily on the idea of "coercive mobility," first presented by Clear (1996) and Rose and Clear (1998) that incarceration can backfire by increasing crime instead of decreasing crime. Coercive mobility holds that the forced removal and reentry of prisoners (prison cycling) in and out of neighborhoods destabilizes the foundation for informal social control in those poor neighborhoods. The empirical test of this idea (Clear et al. 2003) found evidence of a "tipping point," whereby levels of prison entry were associated with a later increase in crime in high prison cycling neighborhoods.

The Clear et al. (2003) test of the theory used two years of data covering all neighborhoods in the city of Tallahassee. Lynch and Sabol (2004) have criticized their model because of the endogeneity of incarceration and crime. Their analysis of crime and incarceration data in Baltimore using an instrumental variable approach finds support for the critique. Dhondt (2012) on the other hand, argued that a stronger test of the model would be provided by splitting neighborhoods in the sample by differing levels of disadvantage and using a longer time series. Doing so, he finds strong support for the coercive mobility thesis in Tallahassee neighborhoods with high concentrated disadvantage but not in those with low concentrated disadvantage.

We sought to enter the debate about the impact of high rates of incarceration on crime at the neighborhood level by gathering crime and incarceration data in Newark (NJ), Trenton, Boston, and Rural New Jersey over a longer series of years. Doing so proved difficult, given problems in data quality and availability. However, we were able to construct useful data series by combining datasets from multiple sources in these locations.

We gave considerable attention to the problem of appropriate statistical modeling. As might be expected, different models often provide different results. The most parsimonious models provide small standard errors with significant results, but there are sometimes sign changes when new control variables are added, suggesting instability in the modeling strategy. This is, we think, one of the problems Lynch and Sabol (2004) encountered in their modeling approach in Baltimore. By contrast, the most stable results are provided by fixed effects models that, while intuitively attractive, have the disadvantage of large standard errors. When we use this analytic approach, we achieve results that, we believe, are reliable.

The findings in Boston and Trenton provide clear support for the coercive mobility.  In neighborhoods with high concentrated disadvantage prison cycling increases crime, while in neighborhoods with low concentrated disadvantage it does not. While prison releases have a negligible impact on crime[42], removal of residents from communities to go to prisons has a strong positive impact on crime in disadvantaged neighborhoods, but a small negative impact on crime in those not struggling with disadvantage. (Rural New Jersey did not contain enough quarterly prison admissions and releases to support an analysis.)

However, though the results provides support for the coercive mobility hypothesis in Boston and Trenton, the analysis in Newark leads to a different outcome and, we believe, provides further theoretical insights as well as directions for further research. For Newark, the coercive mobility relationships do not hold. While reentry from prison is positively associated with crime rates, removal from the neighborhood is negatively associated with later crime.

There are interrelated theoretical and data reasons that may explain why the coercive mobility model does not hold here.  Newark neighborhoods, measured by indicators of social disadvantage, are very different than those of Tallahassee, Boston, and Trenton. All Newark census tracts have a very high degree of concentrated disadvantage, meaning that Newark does not have much neighborhood variation, as the other three cities do. This result is not entirely inconsistent with a theory of coercive mobility. If prison cycling does not have a linear effect on

[42] The finding regarding releases is somewhat a variance with current literature. We believe this finding needs to be better understood with additional studies. To be sure, our release data had numerous address coding problems that may contribute to the finding of no effect. Since all of the non-parole correctional agencies explicitly indicated that they cannot verify the address provided as a “release” address was actually the address at release, we cannot be confident that the release finding would hold up with better data.

crime rates, but rather there is a tipping point with higher rates of cycling, could also be a point of saturation. After such saturation, increases in cycling would not contribute to further destabilization of the neighborhood, because it is already extremely destabilized. Cycling would not be a further destabilizing factor for these neighborhoods, because current destabilizing factors in Newark are powerful enough that they overshadow cycling.

There is a data analysis explanation of the results in Newark, as well. We estimate the effect of cycling on crime by assessing variation between neighborhoods and variation over time. In Newark this variation is simply not there to estimate.

Thus, our work finds strong support for the impact of prison cycling on crime. It seems that such cycling has different effects in different kinds of neighborhoods, consistent with the idea of a "tipping point" but more clearly expressed as an interaction between crime policy and type of neighborhood. The results in Tallahassee, Boston, and Trenton provide consistent support for this idea. In Newark, as a result of the city's limited variability in neighborhood disadvantage, we fail to find the same pattern. Further research will investigate whether this neighborhood interaction holds in other sites. It will also enable us to think about how neighborhood change over time affects the prison cycling-crime relationship. Do neighborhoods that improve start to benefit from incarceration policy? In contrast, does current incarceration policy become a factor that inhibits neighborhood improvement?

This leads to an equally important limitation of our work with clear implications for future work in this area. As noted in several places throughout this report, the coercive mobility model assumes that some mediating changes occur between the removal from and/or return to communities. Those mediating changes include things like increasing inequality, more broken families, decreases in levels of informal social control, and increasing social disorder. While our models had no direct measures of those mediating changes, they included a time lag to allow some of those changes to occur. While we explicitly noted that a full test of the model would require measures of those mediating changes, and that gathering those was beyond the scope of the proposed work, we recognize just how important this next step is. Indeed, part of Lynch and Sabol's critique of the work that has tested the coercive mobility thesis to date has been that it assumes a bunch of things are occurring between time 1 and time 2 without being able to directly demonstrate that these things actually have occurred. Indeed in their own work (Lynch and

Sabol, 2004), they used Baltimore data to explore some of these mediating changes and found evidence for the underlying mechanisms wanting.

Given this work has provided the most sophisticated evidence to date that there is growing evidence for a coercive mobility effect, future research should clearly provide a more sophisticated test of the full model that could account for these mediating changes and try to demonstrate that underlying processes assumed by the thesis are indeed at work.

## Discussion

Most of the work to date on the effect of incarceration treats characteristics of neighborhoods as a variable, but does not consider the characteristics of the cities within which those neighborhoods are nested. The characteristics of neighborhoods that are accounted for include poverty and racial composition. Then the city is analyzed as a whole, looking at the effects of the neighborhood characteristics on citywide patterns of crime and incarceration. But clearly cities also vary on the nature of neighborhood characteristics contributing to citywide crime/incarceration patterns.

Our three cities provide an excellent example. A word of caution: because of methodological differences in data collection by each respective police department, the crime rates are not directly comparable with UCR crime rates, nor are they directly comparable across cities, though they are internally consistent within each city. For point of reference, we provided crime statistics from the UCR reports for each city earlier in report (Tables 4, 6, and 7) and provide a summary table below (Table 35). Table 36 provides descriptive data on neighborhood and justice system data for Boston, Newark and Trenton.

**Table 35: UCR Violent and Property Crime Rates, 2010 (Boston, Newark, and Trenton)**

| Agency | Population | Violent Crime rate | Property crime rate |
|---|---|---|---|
| Boston Police Department | 617,594 | 942 | 3,340 |
| Newark Police Department | 277,140 | 1,041 | 3,323 |
| Trenton Police Department | 84,913 | 1,411 | 2,963 |

*Notes*: Rates are the number of reported offenses per 100,000 population.
*Sources*: Compiled using UCR Data Online Tool. Date of download: Dec 06, 2013. FBI, Uniform Crime Reports, prepared by the National Archive of Criminal Justice Data

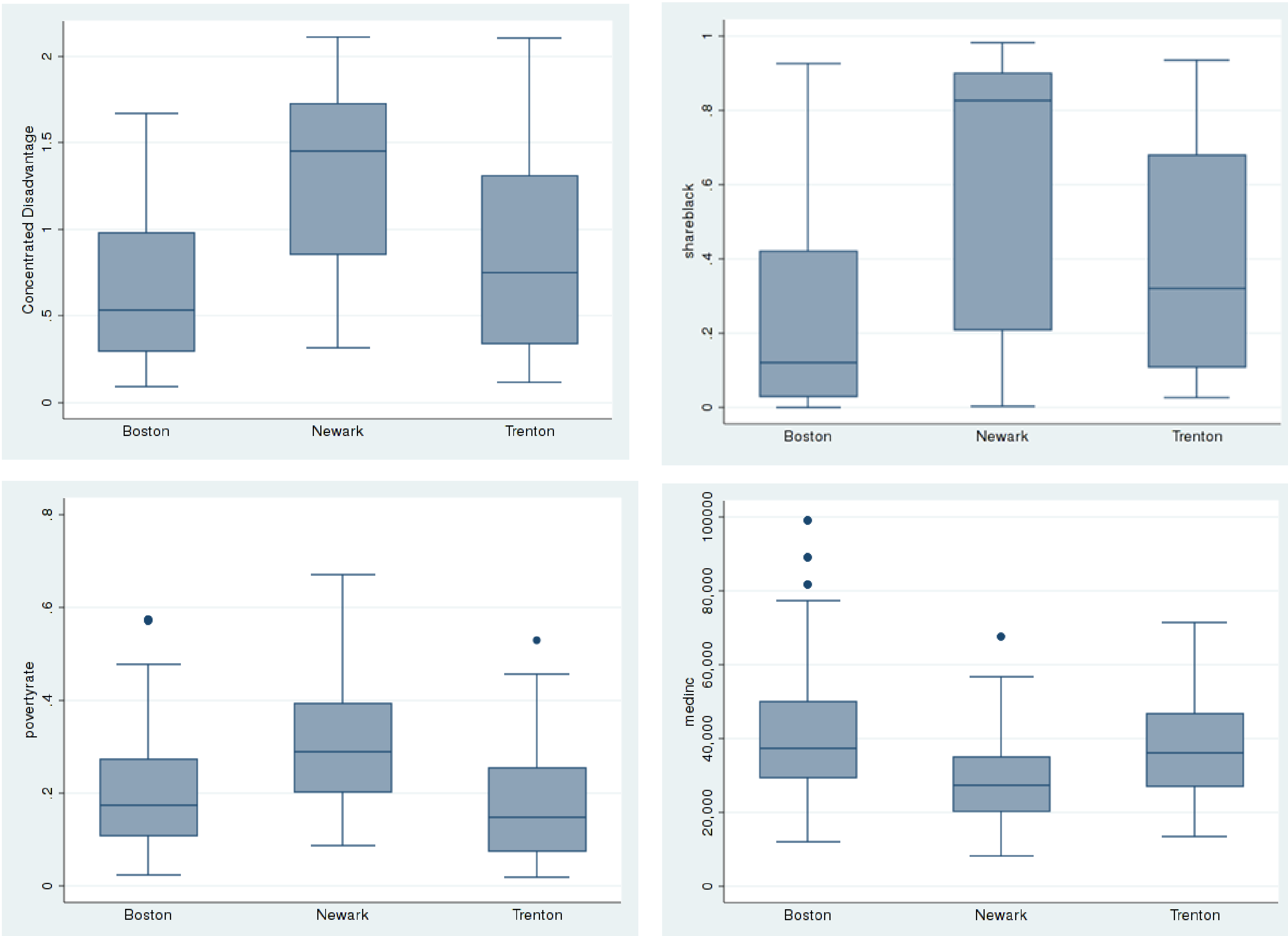
**Table 36: Descriptive Statistics (Boston, Newark, and Trenton)**

| | Boston | Newark | Trenton |
|---|---|---|---|
| Total Crime | 46.66 | 33.57 | 92.35 |
| | [39.30] | [15.96] | [77.79] |
| Releases | 1.42 | 2.28 | 7.76 |
| | [1.96] | [5.76] | [9.10] |
| Admissions | 1.77 | 3.28 | 12.88 |
| | [2.17] | [2.65] | [10.39] |
| Total Crime Rate | 13.45 | 12.75 | 25.90 |
| | [11.99] | [7.41] | [19.46] |
| Release Rate | 0.43 | 0.94 | 2.18 |
| | [0.81] | [2.75] | [2.40] |
| Admission Rate | 0.54 | 1.31 | 3.57 |
| | [0.80] | [1.21] | [2.54] |
| Concenetrated Disadvantage | 0.67 | 1.32 | 0.83 |
| | [0.44] | [0.53] | [0.56] |
| N | 4992 | 1440 | 66 |
| Tracts | 156 | 90 | 33 |
| Population (2010 census) | 617,594 | 277,140 | 84,913 |

The critical message of this table is two-fold. First, the standard deviation of both total crime and the total crime rate in Boston and Trenton is much higher than in Newark. Given that the UCR statistics indicate a much higher overall crime rate in Newark than in either Trenton or Boston, the low standard deviation indicates that while both Boston and Trenton have neighborhoods with comparatively low levels of crime, Newark does not. The second observation is that the extent of disadvantage, a statistic that is comparable across cities, is much higher in Newark. Further, the standard deviation relative to the mean is much lower in Newark than either Trenton or Boston, again indicating a degree of homogeneity in Newark that does not exist in either Trenton or Boston.

Figure 9 provides a clearer representation of the same point. Panel A shows the level and distribution of disadvantage across the three cities. Note that while there is considerable overlap between Boston and Trenton, there is almost no overlap between Newark and either Boston or Trenton. The distribution of neighborhood incomes (Panel D) follows a similar pattern: the median neighborhood income in Boston and Trenton is similar while the median income in both Boston and Trenton is higher than the $90^{th}$ percentile income in Newark. Finally, we see in panel B that there is a much higher concentration of neighborhoods dominated by a single race, African American in this case, in Newark than in either Trenton or Boston.

**Figure 9: Concentrated Disadvantage, Black Population, Median Income, and Poverty Rates (Boston, Newark, Trenton)**



This difference should have implications for citywide statistical analysis. In places like Tallahassee, Boston, and Trenton, there is substantial neighborhood variation on measures of social disadvantage, and as a result there would be more variation in the level of criminal justice involvement and its affects at the neighborhood level. In contrast, for Newark, there is less variation—the right-hand tail of the distribution of neighborhoods does not exist. Thus, citywide analyses will look different, because a search for a "tipping point" will have difficulty estimating the right-hand side. In other words, we would expect Newark to look different from Tallahassee, Boston, and Trenton, because of the effects of neighborhood composition.

## Implications for Policy and Practice

We believe our results hold strong implications for incarceration policy. In particular, we think this work calls attention to the need for place-based correctional programming. If neighborhood context is such an important determinant of policy outcomes, then neighborhood-specific intervention seem warranted. In particular, interventions that are designed to ameliorate the destabilizing impact of prison cycling on informal social control—especially families and children—seem promising areas for experimentation. At a minimum, criminal justice strategies that increase prison cycling in these locations—such as drug sweeps producing a large number of arrests, and gang interventions based on arrests—seem to be potentially self-defeating. We turn to some of those implications below.

### Implications for Corrections: Place-Based Approaches

One of the most important contributions of contemporary criminological research is the empirical demonstration of the recurrent importance of "place" to understanding the nature of crime (Weisburd, Groff, & Yang, 2012). This has meant that our understanding of crime is now heavily guided by context—what the communities are like where crimes occur and how that context facilitates crime (or does not). The implications of this idea are only now beginning to be felt in the wider criminal justice reform movement.

The concept of "community justice" connotes a host of justice system reforms that take advantage of the community-level dynamics of crime and justice (Clear, Cadora, Bryer, & Swartz, 2003; Tucker & Cadora, 2003). These include parole and probation supervision strategies that take account of community (Morash, 2010), and also community development strategies that treat community as a target (Clear, 2011; La Vigne, Neusteter, Lachman, Dwyer, & Nadeau, 2010; Subramanian & Tublitz, 2012). Although this is not the place for an extended discussion of these initiatives, our research suggests that all of these approaches have an empirical foundation, and to the extent that they can affect community-level variables, they have promise as crime prevention strategies.

## Implications for Policing Communities

Policing strategies and tactics, unfortunately, vary by race and social class. Crime hot spots, illicit drug activity, and violent crime tend to cluster in low-income, minority neighborhoods (Sampson, Raudenbush, & Earls, 1997; Weisburd et al., 2012). The pressure on the police to keep crime rates low is usually greatest in high-crime neighborhoods and, therefore, these places receive increased police enforcement attention. Given the growing popularity of broken windows policing approaches to controlling urban crime problems (Kelling & Coles, 1996), low-income, minority residents often face increased levels of police-initiated contacts for minor offenses (such as loitering and public drinking) and traffic enforcement activities to reduce more serious crimes by searching for guns, drugs, and persons with outstanding warrants. Regrettably, the "hit rate" for these increased police-citizen contacts can be extremely low and the majority of persons who are inconvenienced (and sometimes offended) by these stops are innocent, low-income minority residents (Rosenbaum, 2006). Many of these citizens already tend to feel disenfranchised from government and do not have easy access to legal remedies when they feel mistreated or their civil liberties are being jeopardized.

Residents of many high-crime areas seem to be very ambivalent about aggressive enforcement. Survey research suggests that many residents are willing to give up their civil liberties to achieve an enhanced sense of security (Rosenbaum, 1993). Residents want the police to deal with problem situations and problem people. It is unclear, however, how long residents living in high-crime areas will tolerate aggressive enforcement actions as the primary strategy to deal with crime problems. Residents may support aggressive enforcement up to the point where it directly affects them, their family, or their friends, who frequently end up in jail and prison or report being mistreated by the police (Rosenbaum, 2006). Moreover, aggressive enforcement strategies, such as "zero tolerance" policing, can drive a wedge between the police and communities, as the latter can begin to feel like targets rather than partners (Weitzer & Tuch, 2006).

Aggressive enforcement efforts in minority communities also seem likely to contribute to disproportionate minority confinement by plausibly generating negative effects on the rest of the criminal justice system. For instance, a recent evaluation of the adverse system side effects of Operation Sunrise, a widely publicized, geographically-targeted drug enforcement strategy in Philadelphia, found that initiative strained the local judicial system by generated a high volume of arrests that resulted in a significant increase in fugitive defendants (Goldkamp & Vilcica, 2008). Short-term crime gains produced by particular types of enforcement-oriented policing initiatives (as Operation Sunrise did in Philadelphia; see (Lawton, Taylor, & Luongo, 2005)) could undermine the long-term stability of specific neighborhoods through the increased involvement of mostly low-income minority men in the criminal justice system. As described elsewhere in this report, the large-scale cycle of arrest, removal, and return has damaged the familial and community relationships that hold neighborhoods together and worsened the prospects for employment, income, marriage, and responsible parenting for African-American men, in particular (Clear, 2007b; Petersilia, 2003; Travis, 2005; Western, 2006).

Our findings in Boston and Trenton provide strong support for the idea that, in neighborhoods with high concentrated disadvantage, prison cycling increases crime. Police crime strategies that are primarily arrest-focused seem to exacerbate the problems that arise from concentrated incarceration in these disadvantaged neighborhoods. Clear (2007) suggests community problem-solving strategies that engage residents and take alternative preventive approaches to recurring crime problems are well positioned to reduce crime without increasing arrests. Problem-oriented policing strategies that modify the underlying conditions, situations, and dynamics that cause crime to cluster at specific places could serve as "harm reduction" strategies that also generate positive community perceptions (Braga, 2008). Clear (2007) also suggests that focused deterrence strategies designed to change the behavior of high-risk offenders through strategic enforcement and creating a grounded sense of the community's norms and values against criminal behavior can also be applied to reduce crime without exacerbating incarceration problems (see also (Kennedy, 2011)).

Boston serves as an important example of the potential crime control efficacy of preventive policing strategies that reduce the need to arrest, prosecute, and incarcerate offenders. In response to an increase in violent crime during the early to mid-2000s, the Boston Police Department (BPD) implemented two community justice policing strategies. The BPD implemented a revitalized Operation Ceasefire focused deterrence program that focused criminal justice, social service, and community-based resources on halting outbreaks of gun violence among feuding street gangs. The BPD also launched its Safe Street Teams initiative that used community problem-solving techniques to control violent hot spot locations in Boston. Controlled evaluations of both programs suggest immediate violence reduction impacts (Braga, Hureau, & Papachristos, 2011a, 2013). Influenced by these programs and other innovations, violent UCR Index crimes in Boston decreased by 30% between 2006 (7,512 incidents) and 2012 (5,265 incidents). Equally impressive, total arrests decreased by 37% during the same time period (from 24,745 arrests in 2006 to 15,625 arrests in 2012).

Preventive policing strategies offer an approach to crime prevention that can increase public safety while decreasing the human and financial costs of imprisonment for Americans (Durlauf & Nagin, 2011). If preventive policing was to become the central focus of police, rather than the arrest and apprehension of offenders, we would likely see at the same time a reduction of prison populations and an increase in the crime control effectiveness of the police. There is no reason why police need to rely on aggressive enforcement strategies that can distance them from communities they serve and potentially cause more harm to the disadvantaged communities that need them the most.

## Implications for Further Research – Overcoming Methodological Challenges

In addition to implications for policy and practice, this project has led us to several recommendations for future research avenues. One of the major limitations of this project, and most of those that have preceded it, concerns the availability and quality of the data. At several points across the life of this project, we were forced to make changes to our original design, and reach out to new sites due to issues with data availability and quality.

In our initial proposal, we had proposed to test the coercive mobility thesis across three cities in New Jersey (Camden, Newark, and Trenton) over a period spanning at least a decade. Weeks into our project we realized just how overly ambitious unfeasible this was given the lack of the availability of data. In 2011, Camden laid off almost half of its police force and in December 2012 announced plans to lay off its entire force and replace it with a newly configured county police department (Chang, 2012). Although the Camden Police Department had agreed to partner with us for this project, we very quickly realized that Camden would not be in a position to provide data for our project.

In Trenton and Newark, securing a dataset that isolated UCR offenses and included information on the date, the offense, and the location of the offense proved difficult. Both agencies offered access to "calls for service" databases, but these databases were massive, and included hundreds of thousands of calls for service that had nothing to do with index crime. Moreover, we recognized that very frequently what came in as robbery call in a calls for service database would not ultimately get counted as a robbery for the purposes of UCR reports. In both cities, more advanced data systems had only recently been developed, or had recently been changed, and so consistent data over time was an issue. Data choices had to be made, and internal consistency of data over time trumped quantity of data. Although we had hoped for a decade of data in both cities, we ultimately truncated the period of coverage in each reflecting what the police department could reliably provide.

And policing data were relatively easy to work with compared to the correctional data. One of the biggest surprises over the course of this project was how little good data there are around released offenders. Each of the correctional agencies that we approached indicated that although they might have an address for a released offender, they could not verify that the address was actually a release address as released offenders were not consistently asked for an address at release. Although all of the correctional agencies noted that they now had data systems that allowed for multiple addresses for each offender (both on this stay and subsequent stays), some of the correctional agencies indicated that more recent addresses might have overwritten previous addresses at some points over the decade. Parole agencies maintained relatively more extensive data on addresses that generally indicated both the address and the date at which that address was effective, which allowed for a much more careful matching of addresses with releases. Where DOC data were used, decisions about which address to use as the release address had to be made. In New Jersey, there were so many issues with missing data in the DOC data that we opted to focus on parole only releases. In Massachusetts, we were able to take advantage of relatively more complete data for both paroled offenders and for all releases, enabling us to run models using both types of data and to determine that the results were substantively similar. This has important implications for future research. Since there are serious concerns about the veracity of release address data that can be provided for released offenders by correctional facilities (at least in our two states), in many ways, it makes sense to focus on the places to which paroled offenders return (since these are, at least in theory, verified release addresses).

A related concern arose around the geocoding hit rates given the quality of the data that were provided by the various agencies, particularly the correctional agencies. Most of the agencies were able to provide address data, these data were of varying quality. Many of the agencies used non-standard ways of entering addresses, with some using a single field to record an abbreviated version of the address. The address data, where provided, often required an incredible amount of work to clean them up and some of those data were un-recoverable. Several of the agencies did not record the complete address – most frequently leaving out the zip-code. While we were still able to geocode these datasets, the omission of important address data undoubtedly affected the hit-rate. While not unacceptable, the geocoding hit rates in some cases fell short of the recommended 85% for crime-related data. A practical implication of this work would be to find ways to encourage, and possibly fund, correctional agencies to either routinely geo-coding of the address data that they collect or, at a minimum, to record addresses in a complete and standardized manner.

With the advent (and increasing popularity) of big data analytics, we are optimistic that the data situation will improve over time. There were already substantial improvements made across the period of study we had initially intended to use (2000-2010), with most agencies collecting useable data by 2007. For our policing agencies, the advent of COMPSTAT has meant that police more routinely geocode their own data, making these data eminently more useful for researchers looking to engage in spatial analysis of crime patterns. Correctional agencies have less of an incentive for geocoding and tend to have little interest in mapping their offender populations. Indeed, a couple of the correctional agencies that we worked with indicated that they were not routinely recording the address at release as they are no longer the responsible agency at that point. Even if these data are not particularly useful for the agencies generating them, some relatively basic standardization of recording address data that could later be geocoded by researchers would enable far more sophisticated research that could begin to answer some of the questions that are surely important to these agencies. Although not interested in geocoding per se, correctional agencies generally are interested in measuring their outcomes, usually by reference to recidivism rates, and knowing more about the released populations – and where they went – is certainly a crucial first step in understanding how patterns of release are related to crime and future offending.

We therefore think that while our substantive findings are provocative and should generate additional interest in continuing to explore the complicated relationship between rates of crime and rates of prison cycling, one of the most important contributions we have made through this work has been to demonstrate just how challenging taking on such work can be. As criminal justice agencies continue to update their data systems and improve their data collection efforts, we are optimistic that work like this will become less fraught with challenges and will generate the reliable spatial data necessary for more comprehensive understanding of crime and place..

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