Hanging Out with the Usual Suspects: Neighborhood Peer Effects and Recidivism

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Abstract

Social interactions within neighborhoods, schools and detention facilities are important determinants of criminal behavior. However, little is known about the degree to which neighborhood peers affect successful community re-entry following incarceration. This paper measures the influence of pre-incarceration social networks on recidivism by exploiting the fact that peers may be locked up when a prisoner returns home. Using detailed arrest and incarceration data that includes residential addresses for offenders, we find consistent and robust evidence that a former inmate is less likely to reoffend if more of his peers are held captive while he reintegrates into society.

Keywords: crime, recidivism, peer effects, social spillovers, social interaction

JEL classification codes: C31, J10, K42, Z13

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Recidivism is a costly failure of the criminal justice system and is often attributed to difficulties among released offenders in establishing stable employment and housing as well as other personal obstacles such as substance abuse, mental health disorders, and financial obligations (Visher and Travis 2003). These reintegration challenges are both mitigated and exasperated by the social environment upon re-entry into society (Sampson 2011). Supportive peers, family, and other positive role models prevent reoffending, but relationships with criminally active individuals promote recidivism and can erode the efficacy of programs that directly address employment, housing, and health challenges for those recently incarcerated.

An emerging literature documents the negative influence of criminally active peers in a variety of settings. Research finds that inmates who are more likely to interact in the same detention facility affect each other's post-release criminal activity (Bayer et al. 2009, Ouss 2011, Drago and Galbiati 2012, Damm and Gorinas 2013, Stevenson 2015).[1] Crime is also affected by peer influences within schools and neighborhoods (Deming 2011, Billings et al. 2014, Case and Katz 1991, Ludwig et al. 2001, Kling et al. 2005, Ludwig and Kling 2007, Corno 2015, Kirk 2015)[2] with residential proximity enhancing the within-school peer effects (Billings et al. 2016). The influence of criminals in a neighborhood can be long-lasting—Damm and Dustmann (2014) find that growing up amongst criminally active neighbors impacts later-life convictions around immigrants in Denmark. At an aggregate level, social interactions within neighborhoods can help explain variation in crime rates across space and time through a social multiplier mechanism (Glaeser et al. 1996).

Despite the evidence documenting the criminal influence of peers and growing concern about the high costs of recidivism, little is known about the effect of pre-incarceration social networks on successful prisoner re-entry for several reasons. First, social connectivity is difficult to measure and data on social relationships is rare.[3] Moreover, identifying the causal relationship between neighborhood peers and recidivism is complex given the presence of endogenous

[1]Research on prison gangs suggests that prison peer groups persist after release (Skarbek 2014).

[2]Kirk (2015) also focuses on neighborhoods and released prisoners, finding higher rates of recidivism associated with higher parolee concentration in Louisiana neighborhoods following Hurricane Katrina in 2005. The main challenge of examining peers and recidivism in the context of Katrina is the large-scale changes in neighborhoods that coincided with and influenced the concentration of parolees across neighborhoods.

[3]Impressively, Corno (2015) administered a survey among hundreds of homeless individuals in Milan, Italy, asking each to self report up to five "best friends" to measure social networks.

relationships (Manski 1993, 2000). Changes in the presence of criminals in the neighborhood may affect and be affected by recidivism through endogenous social interactions and contextual effects ("social effects"), but the same factors that underlie changes to the number of criminals in the neighborhood such as police enforcement and employment opportunities also influence recidivism ("correlated effects").[4]

Using administrative arrest and incarceration records from Charlotte, North Carolina, we provide novel evidence of the relationship between neighborhood peers and recidivism in a setting not unlike the one faced by hundreds of thousands of offenders who exit jails and prisons each year in the United States.[5] We use pre-incarceration residential information to both obtain a proxy for the neighborhood of re-entry as well as to count the number of criminal peers who are absent from the neighborhood when an individual returns home. We rely on the fact that the majority of prisoners return to their pre-incarceration locations due to many factors including financial constraints, housing discrimination, and the presence of family and social support networks.[6] We then exploit the variation in social interactions at the time of release which arise from the pre-release flow of neighborhood criminal peers into prison or jail. In our setting, the presence of criminals in the neighborhood is constantly shifting as similar criminals may have different incarceration experiences due to idiosyncratic factors such as random judge/courtroom assignment (Mueller-Smith 2015, Aizer and Doyle 2015), the timing of arraignment (Danziger et al. 2011) and random variation in the probability that a criminal act is cleared by an arrest. Through a series of balance and placebo tests, we show that conditional on neighborhood- and time-fixed effects, variation in the number of peers incarcerated at the time of release is driven by factors plausibly unrelated to unobserved determinants of recidivism.

[4] Manski (1993) defines endogenous effects as "the propensity of an individual to behave in some way varies with the behavior of the group;" exogenous contextual effects as "the propensity of an individual to behave in some way varies with the exogenous characteristics of the group;" and correlated effects as "individuals in the same group tend to behave similarly because they have similar individual characteristics or face similar institutional environments."

[5] Carson and Golinelli (2013) estimates 637,400 inmates were released from state prisons in 2012 (not including those released from county jails or juvenile detention facilities).

[6] The Post Release Supervision (PRS) program in North Carolina can also restrict released offenders to remain in their county of residence as a special condition of supervision which limits mobility outside Charlotte. The NC PRS guidelines were accessed at http://www.interstatecompact.org/LinkClick.aspx?fileticket=dhABP8c-DfU%3D&tabid=1289&portalid=0&mid=4391 [Date Accessed: Dec. 15, 2016]. As discussed in Section 2, we find that over 50% of people who recidivate within one year report the same post-incarceration residential address as that reported for the pre-incarceration arrest.

Our use of the number of peers incarcerated provides a measure of potential peer interactions and is similar to other papers (Billings et al. 2016, Jacobson 2004) that measure the relationship between potential peers and criminal activity using changes in cohort composition.

Overall, we find consistent and robust evidence that a released offender is less likely to reoffend if more of his neighborhood peers are incarcerated at the time of release. These peer effects increase in the degree of connectivity as measured by pre-incarceration residential proximity, past criminal relationships, and demographic (e.g. age, race, gender) similarity. We find that a decrease in the presence of one neighborhood peer with similar demographic characteristics (age within one year, same race, same gender) is associated with a 2.4 percentage point decrease in the probability of arrest during the first year post-release (approximately a 5% decrease relative to the mean rate of recidivism). The estimated effect of an absent peer is much larger for peers linked to the released offender for specific crimes in the past—the incarceration of a former criminal partner during the first year post-release is associated with a 12 percentage point decrease in the probability of recidivism. Across different types of criminals, we find evidence that less serious criminals and those who may be aging out of crime are the most heavily influenced by the presence of neighborhood criminal peers. We also find larger effects among drug offenders.

Our main results can be interpreted broadly as "peer effects", which include a role for direct social interactions, social learning and congestion externalities. While we cannot separately identify the role of each of these mechanisms, our stronger results for more similar peers and for those committing drug crimes lend support for a greater contribution from direct interaction and social learning rather than congestion externalities. A congestion externality mechanism may predict lower rates of recidivism with more peers incarcerated since there could be a higher probability of apprehension given a crime[7]—however, our estimated effects are not impacted by the inclusion of controls for police enforcement in the neighborhood at the time of release, suggesting a small role for this particular mechanism in explaining our results. Furthermore, we document a negative association between the total number of individuals incarcerated within a

[7] We find a positive correlation between neighborhood incarceration levels and crime clearance rates in a panel data model for Census Block Group 2000 (CBG) neighborhoods suggesting the presence of congestion externalities.

neighborhood and crime rates, with the largest effects in high-incarceration neighborhoods. This pattern is consistent with the existence of social multiplier effects.[8]

Our results contribute to a large literature that evaluates other determinants of recidivism such as post-release employment. It is well known that individuals experience low rates of employment following imprisonment. Since unemployment increases an ex-prisoners social time in the neighborhood, social interactions may be an important mechanism behind a growing number of studies finding a connection between local labor market conditions and recidivism (Schnepel 2016, Yang 2016, Wang et al. 2010, Raphael and Weiman 2007, Sabol 2007). Schnepel (2016) finds reductions in recidivism associated with increases in manufacturing and construction opportunities but not in other lower-wage jobs available to released prisoners. While expected wages differ across these opportunities, certain jobs could facilitate more positive social interactions (or discourage negative social interactions). For example, a released offender working on a construction site typically starts work early and engages in physically exhausting work—these job characteristics may prevent interactions with criminal peers in the neighborhood compared with a job in retail or food services. In fact, Redcross et al. (2012) speculate that differences in the social environment across randomized controlled trials evaluating re-entry employment programs may explain differential effects on recidivism outcomes.[9]

Our findings speak directly to the role of location for recently released inmates. More often than not, released offenders have no choice but to return to their old neighborhoods due to such issues as insufficient money, housing discrimination, and post-release supervision requirements. Policies that provide assistance to live further away from other criminals (similar to the Moving to Opportunity program) or more strictly enforce limited interaction between former criminals may lower recidivism. Another policy that may prevent negative social interactions is electronic monitoring. For example, Di Tella and Schargrodsky (2013) document large reductions in

[8] We interpret this pattern as evidence of a social multiplier effect since the pattern is not consistent with purely an incapacitation mechanism given prior research documenting diminishing marginal returns to incarceration (Vollaard 2013, Johnson and Raphael 2012, Owens 2009). With diminishing returns, we would expect smaller marginal impacts in neighborhoods with higher levels of incarceration.

[9] This point is highlighted in a discussion of recent evaluations by Raphael (2014). More recently, Cook et al. (2015) does not find large differences in recidivism in a re-entry program which combined pre-release social services with employment re-entry programs. It is possible that the post-release social environment differed across treatment and control groups given the increased participation in group therapies among treated individuals.

recidivism for offenders under electronic monitoring compared with those who are sent to prison. These effects may be a result of preventing the formation of criminogenic relationships within prisons, but they could also be due to preventing the monitored offender from hanging out on the street or in places where criminal peers congregate.

The remainder of the paper is structured as follows: Section 2 describes our administrative dataset of criminals. Section 3 outlines our empirical strategy to identify the role of peer effects on reoffending. Section 4 presents and discusses the estimated effects of peer concentration on recidivism. Finally, Section 5 provides some concluding remarks and further discussion of the policy implications of our results.

2. Data Description

Our main sample focuses on adults sentenced in Mecklenburg County, NC who are released from prison or jail between January 1, 2000 through December 31, 2009.[10] We combine administrative records from the Mecklenburg County arrest registry, Mecklenburg County Jail intake and release, and data from the North Carolina Department of Public Safety on state prisoners. All data is matched using first and last name as well as date of birth. Given the similar administrative nature of these datasets, the match rate across these datasets is high with over 95% of jail or prison records linked to an arrest record. Beginning in 2005, the registry of offenders can be matched to criminal incident records, allowing us to identify individuals who commit crimes together, therefore linking individuals with their criminal partners.

The arrest registry data provides individual names, demographic information, details on the nature of the arrest charges, the time and date of arrest, and information on the location of residence at the time of arrest. The offenders' residential address is typically ascertained from personal identification or secondary verification from law enforcement at the time of arrest and is provided in full address form from which we geocode the pre-incarceration residential locations of released offenders. One limitation of our analysis is that we cannot include individuals who

[10] Even though arrest records are available from 2010-2016, the sheriff's department stopped providing the address field for arrest records as part of publicly available data in 2010. We are able to examine post January 1, 2010 rearrests since we do not need location-of-residence information.

have inaccurate or missing location information in their pre-incarceration arrest record in our estimation sample or in our counts of neighborhood peers incarcerated at the time of release. Most of our analysis uses this residential information to determine the concentration of peers within small neighborhood definitions – individuals residing within 1km of one another. Among released inmates meeting our sample criteria, we are able to match approximately 80% to a pre-incarceration residential address using arrest records.[11]

Our primary estimation sample includes individuals 18 through 65 years of age incarcerated for at least three months and less than 10 years. Overall, our primary estimation sample includes 14,696 re-entry observations among nearly 11,000 unique individuals. Figure 1 details the distribution of months incarcerated for this sample.[12]

Table 1 indicates that nearly half of the individuals released from prison or jail are arrested within one year of release. We use the probability of arrest within one year as our primary measure of recidivism. A similar fraction is reincarcerated within one year.[13] The majority of our sample is black (79%) and male (92%). The average offender in our estimation sample is 32 years old, has been incarcerated for 11.3 months, and has 5.6 prior arrests at the time of release.

We use the pre-incarceration residential location of offenders as a proxy for their location post-release. To check whether this is a valid assumption, we plot the distance between the pre- and post-incarceration residential location for the subsample of offenders who are rearrested within one year of release and who have a residential location recorded for their post-release arrest (5,783 of released offenders meet this criteria).[14] As shown in Figure 2, the majority of released offenders provide the same residential address during the post-release arrest as the pre-release arrest. Approximately 75% of those rearrested provide an address within five kilometers of the pre-incarceration location. While we can only observe this lack of mobility

[11] Given higher rates of mobility and concerns about immigration status, Hispanic offenders are less likely to provide accurate or any address information to law enforcement officers. Results are consistent when excluding Hispanic offenders.

[12] We see a large number of offenders with relatively short incarceration spells—46% serving six months or less and 74% serving a year or less.

[13] Note that reincarceration does not require an arrest since under some forms of post release supervision offenders may be reincarcerated after a violation of the terms of supervision, which will not always correspond to a recorded arrest.

[14] Since this subsample is for only those offenders who are rearrested, we automatically lose at least half of our original sample.

for those who recidivate, it is reassuring for our estimates that the majority of offenders return to the same neighborhood.

2.1. Defining Criminal Peers

While other groups of peers may exert influence (such as schoolmates and workmates), our analysis focuses on measuring the effects of peers who are most likely to influence the behavior of offenders released from prison—those individuals who are themselves involved in the criminal justice system. Ideally our measure of neighborhood peers would include everyone who is criminally active, but our data and identification strategy limit us to examining the influence of peers who have an incarceration experience themselves. [15]

We are able to calculate multiple measures of neighborhood criminal peers using pre-incarceration residential addresses and detailed demographic information about offenders in Charlotte-Mecklenburg County. To construct a variable that measures the presence of criminal peers in the neighborhood at the time of release, we count the number of individuals who are incarcerated at the time of release and match those to a residential address within a specified distance (our primary focus is addresses within one-kilometer) from a released offender.[16] To focus on potential peers who are criminally active in the neighborhood around the same time, we also require that the peer was arrested no earlier than two years prior to the focal offender's arrest date.[17] We refer to this group as *peers incarcerated* at the time of release.[18] We then decompose the total number of neighborhood peers incarcerated into those who share various demographic and criminal history characteristics with the released offender of interest.[19]

[15] This measure also has the benefit of providing estimates of the influence of a portion of the population known to criminal justice authorities and who can be influenced by policies in the criminal justice system.

[16] If there is more than one pre-incarceration arrest for potential peers, we use the minimum distance from the focal offender's pre-incarceration arrest address to determine the distance. A robustness check confirms that results are similar if we only use the potential peer's address associated with the arrest closest to the incarceration spell or extend the time period for which we include prior offenders as criminal neighbors.

[17] As discussed and presented in the Appendix, results are smaller but similar in magnitude when we allow individuals arrested anytime prior to the focal offender's release date to contribute to our measure of peers incarcerated.

[18] Using counts of peers incarcerated as our main measure of peers is consistent with the idea that greater concentration of peers provides more opportunities for social interactions. Cohort or peer groups size has been used as a measure of peer environment in a number of papers in the crime and education literature (Jacobson 2004, Billings et al. 2016, Hoxby 2000).

[19] We examine results for alternative definitions of peers through varying neighborhood definitions, race/ethnicity

Each variable measuring the number of peers incarcerated is constructed to represent the number of peers in jail or prison during the first year post-release, in other words, during the first year of re-entry. To avoid any simultaneity bias, peers are only included in these measures if they are incarcerated on the day when an individual is released from jail or prison.[20] We then calculate the number of people in jail or prison during the first post-release year, allowing peers released during that year to contribute fractionally. Figure 3 helps illustrate this calculation. Suppose Offender i is released from prison on January 1, 2005. On this release date, three individuals (Peer A, B and C) who were the same age and arrested in the same neighborhood are incarcerated. Peer A and Peer B both entered prison while Offender i was incarcerated, while Peer C was in jail prior to Offender i's incarceration. Peer A was released from prison on July 1, 2005 while Peer B and C remain incarcerated until early 2006. In this example, our *Peers Incarcerated* variable would equal 2.5 for an outcome window of one year since Peer A contributes 0.5 (half of a year) and Peer B and C each contribute a full year. Results are similar when we use alternative definitions (i.e. not allowing Peer A to contribute partially to our measure).[21]

3. Empirical Methodology

To assess the influence of criminal peers on the criminal activity of an individual released from jail or prison, we estimate the following model:

$$\text{Recid}_{ijt} = \beta_0 + \beta_1 \text{Peers Incarcerated}_{ijt} + \mathbf{X}_i'\alpha + \gamma_j + \delta_t + \epsilon_{ijt} \tag{1}$$

match, and the relevant age window.

[20] Without this restriction, the behavior of the released individual could directly influence the criminal behavior of individuals in the neighborhood and thus our measure of criminal peers.

[21] These alternative results are discussed in Appendix Section A.1 and presented in Appendix Table A.2. Later results also highlight that our main estimates are similar when we control or exclude cases like Peer C—individuals incarcerated prior to Offender i's entry in jail.

where Recid_{ijt} is an indicator variable equal to one if individual i, released in neighborhood j at time period t, recidivates within a one year of release from incarceration.[22] We present results for both rearrest and reincarceration definitions of recidivism. As described in Section 2.1, our key variable of interest, $\mathrm{Peers\ Incarcerated}_{ijt}$, measures the number of i's neighborhood peers incarcerated (in jail or prison) during the first re-entry year where neighborhoods are primarily defined as a one-kilometer radius surrounding i's pre-incarceration residential location. We decompose the total number of neighbors incarcerated into groups of increasingly similar peer groups. Our preferred model focuses on a measure of peers that includes those within one year of age and peers of the same race and gender. For all models that include more narrowly defined peers, we also include a variable measuring all of the other neighbors incarcerated who do not meet the specified classification of peers based on attribute similarity. Individual demographic and prior criminal histories are included as part of vector $\mathbf{X_i}$.[23] To account for unobserved neighborhood determinants of criminal activity and any shocks common to a particular time period, we also include fixed effects for Census Block Group (CBG) by offender attributes (γ_j) and year-by-month-of-release fixed effects (δ_t). All specifications allow for arbitrary correlation in unobservables within CBG areas.[24]

The variation in $\mathrm{Peers\ Incarcerated}_{ijt}$ conditional on the control variables provides a measure of the concentration of criminal peers at the time an offender is released. A key assumption is that greater peer concentration of criminals (less peers incarcerated at the time of release) increases the likelihood of interactions (directly or indirectly) with active criminals in the neighborhood. Our strategy is similar to recent papers that measure the impact of variation in the types of juveniles sharing a correctional facility (Stevenson 2015, Bayer et al. 2009) or the stock of criminals in a neighborhood assigned to immigrants (Damm and Dustmann (2014)). Similar to these studies, we quantify the peer environment using changes in the levels and characteristics of peers.

[22]We later discuss results using alternative time windows for measuring recidivism.

[23]We include controls for gender, race, age at incarceration exit, type of offense associated with the incarceration spell, number of months incarcerated, time between arrest and incarceration, number of prior months incarcerated, and number of prior arrests.

[24]There are 363 unique CBGs in our primary estimation sample.

3.1. Identification Concerns

To enable a causal interpretation of our estimates for β_1 in Equation 1, we need the variation in $\mathit{Peers\ Incarcerated}_{ijt}$ to be "as good as random" conditional on the control variables and fixed effects included. Our estimates would be biased if there were unobserved determinants of post-release reoffending that are correlated with our measures of social influence. We assess the potential influence of such factors in the following paragraphs.

Since offenders are not randomly assigned to neighborhoods, our estimates may be affected by offenders prone to high rates of recidivism selecting into neighborhoods in which a large proportion of their peers are at risk for incarceration. To limit this type of sorting, we use pre-incarceration addresses and thus our estimates capture an intent-to-treat (ITT) effect of neighborhood criminal peer concentration on recidivism. Any differential post-release sorting will attenuate our estimated effects. However, pre-incarceration sorting could also influence our estimated effects if individuals more prone to recidivism sort into certain types of neighborhoods. To account for neighborhood-level determinants of recidivism, all of our specifications include location fixed effects, which limits any systematic bias from certain neighborhoods.[25] To the extent that the type of neighborhood changes over time, we ensure that our results are robust to the inclusion of neighborhood-specific time trends (linear, quadratic, and CBG-by-year fixed effects).

To support the validity of our identification assumptions we present a balancing test in Table 2 which investigates whether our key regressors of interest are correlated with observable characteristics. Across our attribute-specific measures of $\mathit{Peers\ Incarcerated}_{ijt}$, we cannot reject the null hypothesis that the coefficients on the individual's criminal history characteristics are jointly equal to zero.[26] In each of the specifications we also include fixed effects for CBGs interacted with attributes specific to the peer measure and controls for the time window used to

[25] When we define peers based on offender attributes, we include CBG interacted with released offender attribute fixed effects to control for the average numbers of peers within a neighborhood, which will vary by peer groups defined along different attributes (e.g. race, gender, etc.)

[26] The large number of neighborhood fixed effects and individual covariates may lead one to be concerned about the amount of variation in incarcerated peers used to identify effects on recidivism. Table 2 highlights how much peer variation is explained by our covariates based on the R^2 at the bottom of the table. We find between 70% and 80% of peer variation is explained by our controls and thus there is still substantial variation in peers that is unrelated to the individual and neighborhood attributes that predict recidivism.

capture potential criminal peers in the neighborhood.[27] Table 2 provides strong evidence that the variation in our regressor of interest is plausibly exogenous to unobserved determinants of offender behavior since we find that very important predictors of criminal recidivism are not correlated with the presence of criminal peers at the time of release.

For individuals arrested together or around the same time, a less serious offender (who also may be less likely to reoffend) is more likely to be released before more serious offenders. This pattern could induce correlation between the number of peers incarcerated at the time of release and the probability of recidivism, and therefore negatively bias our estimates. However, we do not observe any systematic correlation between characteristics of more serious offenders (e.g. higher severity of crime) and the number of peers incarcerated in our balance test results in Table 2. We also provide further evidence that our estimates are not biased by such a mechanism through robustness checks that exclude certain types of peers such as former partners in crime and those who enter prison around the same time when calculating our primary regressor.[28]

Another potential identification concern arises from the fact that individuals in our estimation sample could impact our measure of peers incarcerated at the time they themselves go to jail or prison. Specifically, the incarceration of an individual in our sample could affect the number of neighborhood peers going to jail through such a mechanism as the offender cooperating with the police to facilitate arrests of known associates. On the other hand, the removal of a criminal from the neighborhood can reduce the probability of incarceration among peers through a social interaction effect.[29] We present a series of robustness checks in Section 4 that include controls for neighborhood peers incarcerated at the time of entry and neighborhood peer flows

[27] We allow those who are arrested and incarcerated in the neighborhood of our released offender from two years prior up to the release date to contribute to our measure for neighborhood peers. Therefore, individuals who are incarcerated for longer time periods or have a larger gap between arrest and incarceration will have a longer window to capture potential peers, and our measure of peers incarcerated at the time of release will be mechanically related to this time window. To evaluate whether this relationship influences our main results, we provide a robustness check where we exclude controls for the amount of time spent incarcerated and the time between arrest and incarceration. We find that our primary estimate is unchanged by the exclusion of these controls. We also test whether our results are robust to alternative time windows used to calculate neighborhood criminal peers. These robustness checks are discussed in Appendix A.1.

[28] These models are discussed in Section 4 and presented in Appendix Table A.2.

[29] For example, suppose an individual in our estimation sample (person A) is a gang leader and thus very influential in the neighborhood. Person A is arrested and incarcerated which has a direct crime-reducing effect on the criminal activity of his peers in the neighborhood. Thus, less of A's peers are incarcerated during A's sentence and are more likely to be around when A gets out. Person A's criminality is then correlated with the concentration of criminal peers at the time of release which could influence our estimate.

into jail during the period of incarceration to mitigate any concerns about the patterns driving our results.[30]

One may be concerned that the peer environment at the time of release influences whether a released offender returns to his or her original neighborhood. We can partially evaluate this concern using the subsample of released offenders who do recidivate to examine how the concentration of neighborhood peers and the attributes of the released offender affect the post-release residential location of the offender upon rearrest. We report the results from a specification where we replace the dependent variable with the distance between a released offenders pre- and post-incarceration residential addresses in Table A.1. We do not find that the concentration of criminal peers has a significant effect on mobility for this selected subsample. While this finding is not a clean test of the effect of peer concentration on mobility, it does provide us some assurance that our results are not driven by offender mobility motivated by the presence (or lack thereof) of criminal peers at the time of release.

Finally, we are also concerned that criminal enforcement could affect our estimates. Law enforcement is an important determinant of arrest and incarceration rates and may be influenced by the concentration of criminals within a neighborhood. We begin by including controls for general policing patterns by including detailed place- and time-specific fixed effects. To test whether neighborhood and temporal variation in enforcement influences our results, we include measures of the arrest clearance ratio for each neighborhood using geocoded reported crime and arrest data in Charlotte to proxy for neighborhood-level enforcement. We discuss these results in Section 4. Furthermore, it is unlikely that the presence of criminal peers in narrowly defined neighborhoods heavily influences differences in enforcement across neighborhoods. One last issue is that our control variables for general policing based on reported crimes and clearance rates may not capture the targeted patrolling of recently released criminals. However, we find the strongest peer effects for less serious criminals (i.e. those with no prior arrests, shorter prison sentences) who are less likely to receive additional attention by police officers, which helps to mitigate concerns about targeted enforcement.

[30] Even with these controls, we still have variation in peers incarcerated due to variation in the timing of peers released.

4. Results

4.1. Peers and Recidivism

Table 3 presents our estimates of Equation 1 for our sample of released offenders across two outcomes: arrest and incarceration within one year of release. We start by estimating the influence of the total number of peers incarcerated within the-kilometer neighborhood and then subsequently report results from regressions each redefining our primary measure of neighborhood peers incarcerated to isolate the effects of increasingly similar criminal peers based on offender attributes.

Overall, we find that an increase in the number of neighborhood criminal peers who are locked up at the time of release reduces the probability of recidivism. These effects are larger for more similar peers and are strongest when we define peers based on very close connections such as offenders who share the same pre-incarceration residential address, the same surname, and especially a prior history of former criminal partnership.[31] This pattern of increasing influence based on the similarity of attributes also holds for residential proximity. Figure 4 shows that in addition to similar attributes exacerbating peer effects, smaller definitions of neighborhoods (<1km) generate stronger peer effects.

To focus our analysis, we primarily discuss results from our "baseline" model, in which we use one-year arrest and incarceration outcome variables and define criminal peers as having a pre-incarceration residence within one-kilometer, being within one year of age, and possessing the same race/ethnicity and gender. Results for our preferred specification are presented in Panel 5 of Table 3. We find that one additional peer incarcerated at the time of release decreases the probability of arrest (incarceration) by 2.4 (1.9) percentage points within the first year post-release. For more narrow definitions of peers such as potential family members and former criminal partners, we find estimates implying a more than 10 percentage point decrease in the

[31] Another dimension of connection used in related studies starting with Bayer et al. (2009) is shared time incarcerated at the same detention facility. Unfortunately, our data does not allow us to observe the specific facility of incarceration. Likely, similarities between the types of criminals and neighborhoods of residence predict assignment to similar detention facilities, which would strengthen peer influences along those observable dimensions of our data.

probability of recidivism (nearly a 25% effect relative to the mean rate of recidivism).[32] These results are consistent in models using alternative outcomes and for alternative definitions of our key regressor: peers incarcerated (see Appendix Tables A.2 and A.3).[33]

To test our identification assumptions, we include three placebo specifications. In Table 4, we estimate the influence of the number of peers and criminal partners incarcerated one year prior to release (Panel 2) and one year post release (Panel 3) as well as randomly assign released offenders to a pre-incarceration residential location (Panel 4). Any unobserved factors driving our estimates should also be strongly correlated with the lag and lead pseudo-release dates. We find no evidence of any such factors. Expanding on the one-year lead and lag placebo checks, Figure 5 presents estimated effects for placebo release dates each month over the year before and after the actual release date. We only observe a statistically significant effect using the actual release date.[34] The fact that coefficients are similar prior to the release date, but drop in value at the actual release date is consistent with our models measuring the influence of the stock of criminal peers in the neighborhood at the time of release and not the influence of other neighborhood conditions. Furthermore, in Panel 4 of Table 4, we find no relationship between peers incarcerated at the time of release in a randomly assigned location on recidivism, which should alleviates any concerns about the influence of unobserved factors correlated with the time of release.[35]

Since we identify social interactions from fluctuations in the number of individuals incarcerated, we are aware that variation in our primary regressor of interest could be influenced by a persistent (or cyclical) pattern of neighborhood incarceration rates, crime waves, and

[32] Our results for peers defined as same neighborhood, age, race, and gender are not driven by high-impact peers since results are similar when we exclude peers based on same building, same family or former criminal partners (Appendix Table A.3).

[33] Our results are robust to redefining peers incarcerated over the first year post-release to only those incarcerated the entire year instead of allowing for fractional comparisons; allowing individuals arrested in the same neighborhood more than two years prior to count as neighborhood peers; defining neighborhoods with CBG designations rather than one-kilometer concentric circles; and excluding very closely connected peers such as those living in the same building and former criminal partners. See Appendix Section A.1 for a more thorough description and discussion of these robustness checks.

[34] Note that the estimated coefficients on placebo dates is consistently negative due to the positive correlation between the peers incarcerated at the placebo release date and the number incarcerated at the actual release date.

[35] All of our specifications include fixed effects for the year and month of release which control for county-wide exit conditions. Our results are also robust to time-of-entry fixed effects as reported in Appendix Table A.4.

police responses to the crime waves. To assess the influence of such factors on our results, we implement a series of robustness checks (see Table 5) and compare each to the baseline result presented in Column 1.

Column 2 of Table 5 evaluates whether our estimate is robust to the inclusion of controls for peer incarceration at the time of entry into prison. Our results are clearly not driven by a persistent effect of criminal peer concentration in a neighborhood at time of entry since we find a similar (albeit, slightly smaller) estimated effect in this model. Column 3 controls for the total number of neighborhood criminal peers entering prison during the focal individual's incarceration.[36] The goal of this control is to capture any bias from the influence of the incarceration of those in our estimation sample on the neighborhood concentration of criminal peers. We find consistent effects across these specifications which mitigates concerns about the relationship between the construction of our estimation sample and our primary measure of neighborhood criminal peer concentration during the post-release period.

The specifications presented in Columns 4 and 5 of Table 5 control for measures of neighborhood crime and enforcement (the fraction of crimes solved) just before and after release from jail or prison. Again, these controls do not influence our results, providing assurance that our baseline estimates are not biased by any neighborhood-specific crime waves or changes in police enforcement. To assess the influence of other potential confounding factors, such as changes to neighborhoods over time, we ensure that our results are also robust to alternative fixed effects and neighborhood-specific linear and quadratic time trends.[37]

So far, we have focused on one-year follow-up periods in measuring outcomes and the presence of peers, but results across different post-release time periods may help illuminate the mechanisms driving our results. First, Figure 6 plots estimated effects over various time windows. These estimates are scaled by the mean rate of recidivism for each time interval so that estimated effects are more comparable. The presence of criminal peers appears to be a weaker determinant of recidivism during the first few months after release, but the effect strengthens when the time horizon is pushed beyond seven months. This pattern suggests that it may take some time

[36] This measure differs from our main variable of peers incarcerated because it only captures the flow of criminals to jail, but does not account for the release of offenders.

[37] These robustness checks are presented in Appendix Table A.4.

to re-establish peer relationships or that it takes time for the negative peer influences to affect the behavior of a released offender.

To the extent that social interactions are an important mechanism behind the estimated impact of the presence of criminal peers on reoffending, we expect to see fewer crimes with partners when there are more potential partners (neighborhood peers) incarcerated. While imprecisely estimated due to a smaller sample size and a limited number of rearrests involving partners, we present estimates in Appendix Table A.5 (Columns 5 and 6) which do suggest that offenders released during times with more neighborhood peers incarcerated are less likely to be arrested for a crime with a partner. The estimate presented in Column 6 of Appendix Table A.5 shows nearly a 50% decrease in the probability of recidivism with a partner from the same neighborhood relative to the mean of this outcome. These estimates are only suggestive and not statistically different from zero, but they are consistent with criminal peers impacting collaborative criminal activity.

Our initial results provide strong evidence that the concentration of criminal peers in a neighborhood during re-entry exerts a causal influence on recidivism, but questions remain as to which types of offenders and types of peers are driving these effects which we will address in the following section.

4.2. Heterogeneous Effects

We begin by evaluating whether results vary across specific criminal types for both the released offenders as well as for peers. The first panel of Table 6 presents results for our measure of peers incarcerated for offender groups indicated by column title. These specifications split offenders by the types of crimes for which they were initially incarcerated. As evident from the pattern of results, we find that criminal peers have the strongest influence on drug offenders. However, these estimates are not statistically significant. Panel 2 splits our measure of peers incarcerated based on the type of crime for which neighborhood peers are incarcerated. The pattern of effects in Panel 2 implies the largest influence from drug offending peers, which is consistent with the nature of drug crimes involving more direct social interactions than other crime types. In fact, Billings et al. (2016) document that drug crimes contain the largest share of arrests with criminal partners. We also estimate negative effects for peers who engage in violent and property crimes, but effects are smaller in magnitude.

In Tables 7 and 8, we summarize heterogeneous effects across a range of demographic and criminal history characteristics. These models estimate effects for the full sample and interact indicators for the various groups of interest with the regressor for neighborhood peers incarcerated. Analyzing effects by age at the time of exit in the first panel of Table 7, we find that released offenders between the ages of 25 and 45 are the most strongly influenced by neighborhood criminal peer concentration. The influence of incarcerated peers on recidivism is not statistically significant for young offenders (between 18 and 25) or for older offenders (more than 45 years). The fact that our results are not strongest for the youngest cohorts is somewhat surprising and highlights the role of peers beyond young adulthood. In Panels 2 and 3 of Table 7, we find larger effects for released offenders who are black as well as those who are male, which is not surprising given the significant representation of these groups in our estimation sample.

Results in Table 8 suggest that the presence of criminal peers has a greater effect on those under post-release supervision as well on less serious criminals. For our estimation sample, individuals convicted of certain felonies are required to have nine months of post-release supervision upon exit from prison. Slightly less than 5% of released offenders in our sample are under this type of supervision at the time of release, and requirements could include random drug screenings, employment, and victim restitution payments. While our estimates are less precise, we find a larger influence of peer presence among the individuals under supervision. Peers likely exert influence over the ability of individuals to meet the various supervision requirements.[38]

We find slightly larger peer effects for released offenders with shorter sentences and no prior incarcerations. The third panel of Table 8 present results by quartiles of predicted risk of recidivism (rearrest within one year) based on all of the control variables except our peers incarcerated regressors of interest. We find larger effects for those in the lower half of the

[38] We test whether our results are driven by technical violations in Columns 1 and 2 of Appendix Table A.5. Our estimates decline in magnitude suggesting that technical violations are affected by the presence of peers, but estimates remain statistically significant and similar in the effects relative to the mean recidivism rate for an outcome requiring a non-technical offense.

distribution of predicted risk of recidivism but also find effects for those in the highest risk quartile.[39] This pattern suggests an inverse-U relationship, which may reflect a larger peer influence among less habitual offenders but also the potential for social multiplier effects among high-risk offenders who are released in neighborhoods with higher levels of criminal peers. In the next section, we will discuss and explore a social multiplier effect looking at the aggregate relationship between neighborhood incarceration and criminal activity.

4.3. Neighborhood Incarceration and Criminal Activity

Since we find strong evidence that the concentration of criminals in the neighborhood influences recidivism through a social interaction effect, we expect non-linear effects at the aggregate level due to a social multiplier. We assess whether there is evidence of a social multiplier effect through evaluating the aggregate relationship between neighborhood incarceration rates and neighborhood crime outcomes where we define a neighborhood as a census block group (CBG). We use this as a descriptive exercise to investigate neighborhood correlations therefore we do not claim identification of a causal relationship between neighborhood incarceration rates and neighborhood crime outcomes using this methodology.

We create a panel of CBG neighborhoods based on each year-quarter in our study period (Quarter 1, 2000 through Quarter 4, 2009) and estimate the following standard panel data regression:

$$\ln(Crime_{jt}) = \beta_0 + \beta_1 \text{Neighbors Incarcerated}_{jt} + \gamma_j + \delta_t + \sum \beta_j \text{quarter}_t + \epsilon_{jt} \quad (2)$$

where the dependent variable is the natural log of the number of crimes reported in neighborhood j during quarter q; the regressor of interest is the total number of neighborhood residents (as indicated by pre-incarceration residential addresses) who are incarcerated for the entire time period t[40]; and the other terms represent fixed effects for neighborhoods (γ_j), year-quarter time

[39] We regress our arrest outcome on all control variables except those measuring the number of peers incarcerated at the time of release and obtain the predicted probability of recidivism. We then rank individuals according to their predicted risk of recidivism and divide into quartiles representing 25 percentile groups.

[40] To avoid any mechanical relationships between crime outcomes and neighbors incarcerated, we only base our count of neighbors incarcerated on criminals that are incarcerated for the entire year-quarter of interest.

periods (δ_t), and a neighborhood-specific time trend ($\sum \beta_j \mathtt{quarter}_t$). We also estimate this model with the neighborhood crime clearance rate as the dependent variable to test whether the number of neighbors incarcerated is potentially associated with police effectiveness.

We find a negative relationship between people incarcerated and the number of reported crimes in a neighborhood and report this result in the first panel and first column of Table 9. An increase of five neighbors incarcerated during the quarter (approximately one standard deviation in this measure) is associated with a 3% decrease in neighborhood crime. Panel 2 highlights the non-linear nature of this relationship with the largest and most significant effects occurring in neighborhoods that are in the top two quartiles of the distribution of neighborhood incarceration rates. These results are in line with a social multiplier effect—greater drops in crime are observed in neighborhoods with higher rates of incarceration. A decrease in crime within the neighborhood is also expected through an incapacitation mechanism. However, we would expect the effects from incapacitation to be linear or even diminish with respect to higher neighborhood incarceration rates based on prior work that demonstrates diminishing marginal returns to incarceration (Vollaard 2013, Johnson and Raphael 2012, Owens 2009).

Changes in the number of neighbors incarcerated may also impact the effectiveness of enforcement due to congestion externalities. In other words, fewer criminals present in the neighborhood could increase the probability of arrest and apprehension because the police can focus more time on a smaller number of reported crimes and can search among a smaller pool of suspects. Using the same panel data model described above, we replace the dependent variable with one measuring the crime clearance rate. This rate is calculated by dividing the number of reported crimes that are cleared either administratively (no evidence of the reported crime) or by arrest by the total number of reported crimes. We report results from this regression in the second column of Table 9. As expected, we find a positive effect of the number of neighbors incarcerated on crime clearance. One standard deviation decrease in the number of peers incarcerated is associated with a 0.6 percentage point increase in crime clearance. This represents approximately a 2% increase and is consistent with enforcement becoming more effective when the neighborhood is less congested with active criminals. As previously mentioned in our discussion of robustness checks, we do not find the change in the effectiveness of enforcement to be an important mechanism behind the influence of peers incarcerated on recidivism.[41]

5. Discussion and Conclusion

Our results provide strong evidence that neighborhood concentration of criminal peers has a significant and non-trivial effect on the probability that a released offender recidivates. These results are consistent across a number of different models that vary in how we define peers as well as our inclusion of controls for measures of neighborhood crime and policing. Aggregate neighborhood crime models highlight non-linear effects of the stock of criminal peers on neighborhood crime consistent with a social multiplier effect. All of our results together suggest a strong role for endogenous social interaction effects. Support for the importance of social interaction and social learning is demonstrated by the fact that our largest effects are found among peers defined as same residence or family and former criminal partners. The strong peer influence among drug offenders—a crime type that involves more partnerships and gang activity—further supports the important role of social effects in determining recidivism. In our setting, we do not find a large effect from congestion externalities on the relationship between criminal peer concentration and recidivism.

The transition from prison back into a community is undoubtedly a dynamic social process. The social environment can affect the probability of a successful transition in a variety of ways. Our results suggest that an environment with less negative peer influence can reduce the high rates of recidivism. However, designing policies to discourage social interactions with "the usual suspects" is very difficult. Policy solutions that expand housing opportunities to areas away from a released offender's old neighborhood may be effective, but these policies may also reduce positive social interactions such as in the form of supportive friends and family. The effectiveness of group homes and re-entry programs depend on the types of interactions facilitated. Based on the strong social effects observed, we advocate for evaluations of re-entry programs to incorporate measures of the effects of programs on both positive and negative

[41] Our estimates are robust to the inclusion of neighborhood crime clearance rates both prior and post release in our primary models of interest (Columns 4 and 5 of Table 5).

social interactions within the community. Increases in interactions with positive role models through re-entry mentoring programs or decreases in interactions with criminally active peers using electronic monitoring could potentially help reduce the damaging influence of criminally active peers in the neighborhood.

Reducing barriers to employment for released prisoners may also limit social interactions with criminal peers in the neighborhood but the effectiveness of a job might depend on the social environment at work. Despite little evidence of productivity differences between applicants with and without criminal records (Lundquist et al. 2016, Minor et al. 2016), the majority of employers are averse to hiring former inmates and therefore most employment opportunities are heavily concentrated within a small number of firms (Agan and Starr 2016, Doleac and Hansen 2016, Raphael 2014, Holzer et al. 2006).[42] Just as social interactions are important determinants of offender behavior, social interactions and learning between firms about the productivity differentials could increase employment opportunities at firms with low concentrations of former inmates. These new opportunities will reduce social interactions with criminal peers both inside and outside of the workplace compared with the typical experience of a released offender.

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[42]While recent efforts to increase employment opportunities through the removal of questions about prior felony convictions on applications (known as "Ban-the-Box" policies) appear to increase opportunities for individuals with criminal records, evidence suggests the policies also decrease opportunities for individuals from demographic groups with high rates of offending through a statistical discrimination mechanism (Agan and Starr 2016, Doleac and Hansen 2016). In other words, without an ability to screen applicants based on criminal records, employers averse to hiring from this group may rely on other characteristics correlated with a criminal history.

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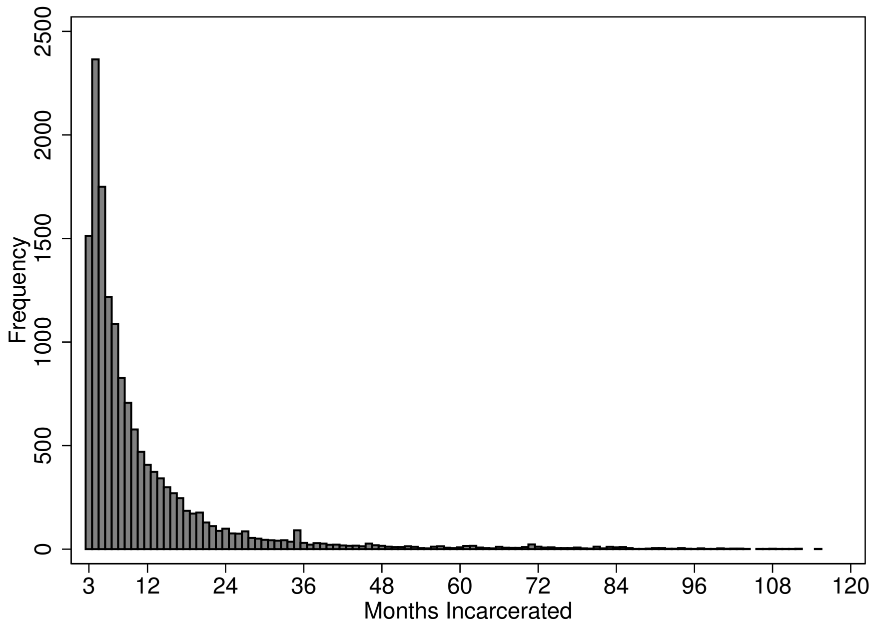
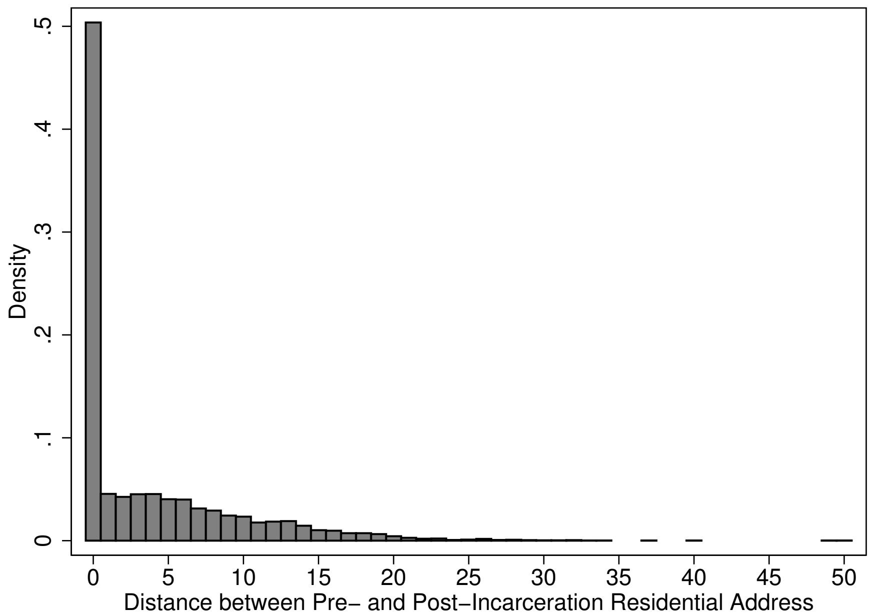
Figure 1: Months Incarcerated Histogram



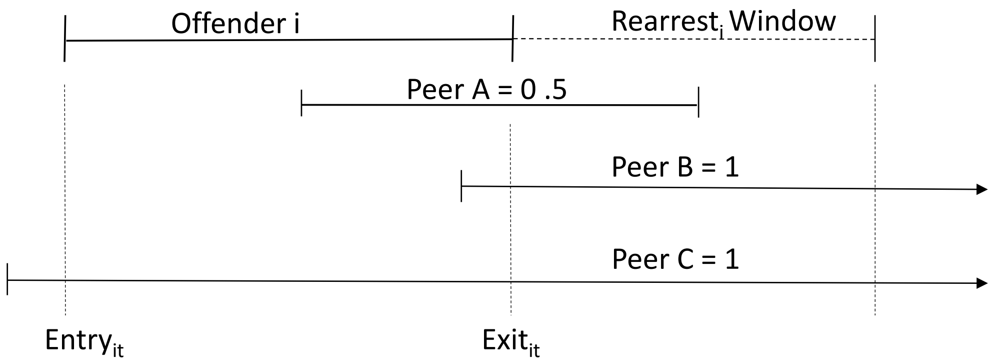
This figure plots the distribution of months incarceration in jail or prison for our sample. Our estimation sample only includes those incarcerated for at least 3 months. General sample construction notes from Table 1 apply.

Figure 2: Distance (km) between Pre- and Post-Incarceration Residential Address



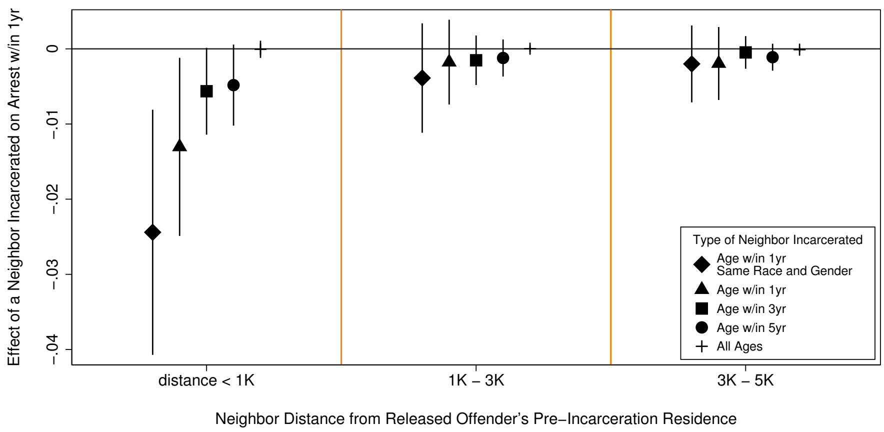
This figure plots the distance in kilometers between the pre- and post-residential addresses recorded for those in our sample who are rearrested within one year of release and report a valid residential address at the time of release. 50% of these individuals have the same residential address recorded for the pre- and post-incarceration arrest. General sample construction notes from Table 1 apply.

Figure 3: Construction of Peers Incarcerated Measure



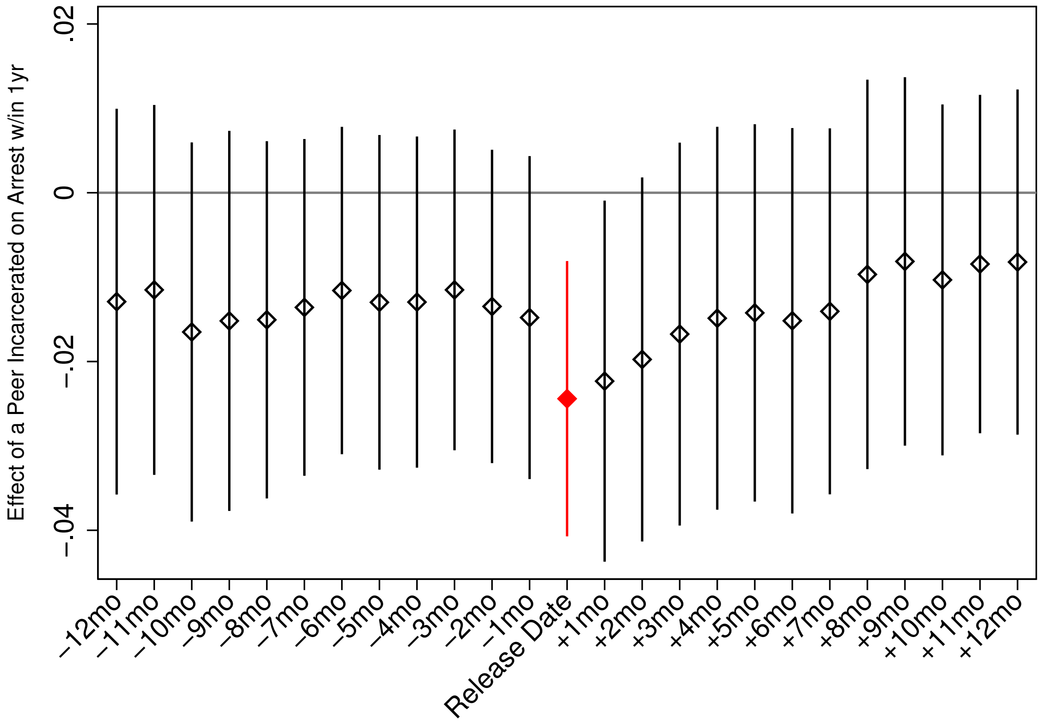
This figure provides a visual of how we create our primary regressor of interest: *Peers Incarcerated.* We measure the total number of person-years of peers who are incarcerated at the time each individual in our sample is released.

Figure 4: Peers Incarcerated Effects by Distance Bands and Attribute Similarity



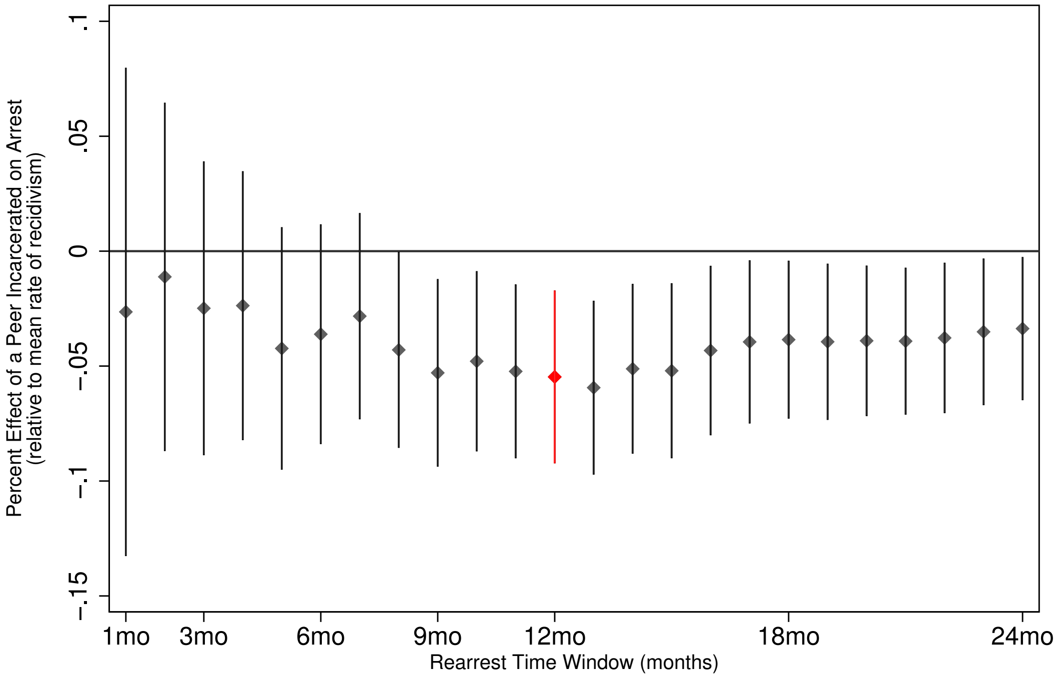
This figure provides the estimated coefficient (and 95% confidence interval) of a one person increase in the number of peers incarcerated during the first year post-release. We vary the definitions of peers based on demographic attributes (age, race, and gender) and distance bands away from the pre-incarceration residential address of individuals in our estimation sample. Each point in the figure represents a result from a separate regression. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Figure 5: Estimated Effects of Peers Incarcerated (w/in 1K, Age w/in 1 yr, Same Race and Gender) Using Placebo Release Dates



This figure provides the estimated coefficient (and 95% confidence interval) of a one person increase in the number of peers incarcerated during the first year post-release where peers are defined as individuals with residential addresses within 1K, age within one year, and of the same race and gender. Each point represents the estimated effects of peers incarcerated on recidivism where peers incarcerated is defined using a placebo exit date for each month during the year prior and post the actual exit date. The estimate in red represents the estimated effect using the correct date to define peers incarcerated. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Figure 6: Estimated Effects of Peers Incarcerated (w/in 1K, Age w/in 1 yr, Same Race and Gender) Using Alternative Time Windows



This figure plots the estimated effect of the number of peers (w/in 1k, same age, race, and gender) incarcerated on rearrest within time horizons which increase incrementally by a month at a time. Our key regressor of interest measuring the number of peers incarcerated also is based on the rearrest outcome window. Each coefficient plotted is from a separate specification and is adjusted to represent the percent effect relative to the mean rate of recidivism for the time window. For example, for the 6 month model, we regress an indicator for arrest within 6 months of release on the peers incarcerated during the first 6 year of the re-entry period. We obtain an estimate and then divide by the mean rate of rearrest within 6 months. We do this to more easily compare the effects across the alternative time frames examined. The coefficient plotted at 12 months (colored in red) represents our baseline estimate of a 5 percent decrease relative to the mean rate of rearrest within one year post-release. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table 1: Summary statistics

	Mean	Std dev	Min	Max
Recidivism Outcomes				
Arrested w/in 1yr	0.445	(0.497)	0.000	1.000
Property Crime	0.107	(0.310)	0.000	1.000
Violent Crime	0.127	(0.332)	0.000	1.000
Drug Crime	0.127	(0.333)	0.000	1.000
Reincarcerated w/in 1yr	0.461	(0.499)	0.000	1.000
Key Peer Variables				
Total Peers (w/in 1K) Incarcerated	19.287	(15.856)	0.000	110.625
Age w/in 5yr	5.755	(5.599)	0.000	43.984
Age w/in 3yr	3.610	(3.739)	0.000	28.921
Age w/in 1yr	1.248	(1.539)	0.000	12.819
Age w/in 1yr, Same Race, Same Gender	0.924	(1.393)	0.000	12.819
Age w/in 1yr, Same Race, Same Gender, Same Parcel	0.042	(0.275)	0.000	5.619
Same Parcel, Same Surname	0.012	(0.103)	0.000	1.638
Former Criminal Partners Incarcerated*	0.048	(0.218)	0.000	2.551
Age at Release	32.150	(9.843)	18.000	65.000
Offender Demographic Characteristics				
Black	0.786	(0.410)	0.000	1.000
Hispanic	0.060	(0.238)	0.000	1.000
Female	0.076	(0.265)	0.000	1.000
Pre-Incarceration Criminal History				
Total Prior Arrests (Since 1998)	5.655	(6.258)	0.000	89.000
Incarcerated for Property Crime	0.280	(0.449)	0.000	1.000
Incarcerated for Violent Crime	0.180	(0.385)	0.000	1.000
Incarcerated for Drug Crime	0.174	(0.379)	0.000	1.000
Incarcerated for Technical Crime	0.107	(0.310)	0.000	1.000
Incarcerated for Other Crime	0.258	(0.437)	0.000	1.000
Incarceration Characteristics				
Total Months Incarcerated (County Jail + State Prison)	11.334	(12.817)	3.000	115.000
Fraction with Any Time in State Prison	0.397	(0.489)	0.000	1.000
Percent of Incarceration in State Prison (Remainder in County Jail)	0.315	(0.417)	0.000	1.000
Fraction with Post Release Supervision	0.045	(0.207)	0.000	1.000
Observations	14,696			

This table presents summary statistics for our dependent variables (recidivism outcomes), various measures of the number of neighborhood peers incarcerated (key peer variables), and other background characteristics. *Total peers (w/in 1K) incarcerated* indicates the number of peers located within 1 kilometer of a given offender's pre-incarceration address. Our estimation sample includes released offenders aged 18 through 65 who were sentenced in Charlotte-Mecklenberg County, served at least 3 months in jail or prison, and released from a Charlotte-Mecklenberg County Jail or a NC State Prison between January 1, 2000 and January 1, 2010. *Data on the number of former partners incarcerated is based on those released between January 1, 2005 and January 1, 2010 due to the availability of data.

Table 2: Balance Test

	(1) Peers w/in 1K Incarcerated	(2) Peers= Age w/in 5yr	(3) Peers= Age w/in 3yr	(4) Peers= Age w/in 1yr	(5) Peers= Age w/in 1yr Same Race Same Gender
Age at Release	−0.005	–	–	–	–
	(0.007)	–	–	–	–
Black	0.659***	0.170	0.117	0.026	–
	(0.229)	(0.155)	(0.108)	(0.054)	–
Hispanic	1.053***	0.372	0.204	0.060	–
	(0.346)	(0.264)	(0.179)	(0.094)	–
Female	−0.377	−0.103	−0.068	−0.057	–
	(0.248)	(0.195)	(0.133)	(0.070)	–
Total Prior Arrests (Since 1998)	−0.022*	−0.007	−0.006	−0.000	−0.002
	(0.011)	(0.009)	(0.007)	(0.004)	(0.004)
Incarcerated for Property Crime	0.173	0.044	0.040	0.008	0.010
	(0.252)	(0.198)	(0.141)	(0.062)	(0.070)
Incarcerated for Drug Crime	0.128	0.070	0.009	−0.028	−0.005
	(0.203)	(0.140)	(0.110)	(0.058)	(0.076)
Incarcerated for Violent Crime	−0.076	−0.009	−0.019	−0.000	0.015
	(0.309)	(0.223)	(0.176)	(0.083)	(0.098)
Incarcerated for Technical Crime	0.267	0.206	0.162	0.079	0.114
	(0.217)	(0.186)	(0.127)	(0.072)	(0.094)
Severity of Crime (Scale from 1-11)	−0.035	−0.021	−0.013	−0.005	−0.006
	(0.044)	(0.033)	(0.025)	(0.011)	(0.013)
Fraction with Post Release Supervision	0.393	0.678*	0.529*	0.099	0.002
	(0.516)	(0.401)	(0.302)	(0.161)	(0.190)
Percent of Inc. in State Prison	−0.087	−0.037	−0.045	−0.034	−0.062
	(0.157)	(0.115)	(0.080)	(0.043)	(0.054)
Observations	14,696	14,696	14,696	14,696	14,696
Test of joint sig.: F-stat	2.44	0.91	0.97	0.55	0.42
Test of joint sig.: p-value	0.0045	0.5269	0.4704	0.8684	0.9068
R^2	0.823	0.830	0.796	0.678	0.731

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table tests whether observable characteristics are significant predictors of our key regressors of interest. Each column represents a different specification with the dependent variable being a measure of neighborhood (within 1K) peer incarceration during the first year following release from incarceration for each individual in our estimation sample. In addition to the variables listed, each specification includes neighborhood-by-attribute fixed effects (CBG by age in Columns 1 through 4 and CBG by age, race and gender in Column 5) and year-by-month of release fixed effects. We also include a measure of the time window used to count peers (from two years prior to the individual's arrest to the date of incarceration exit) since we allow criminal peers to be defined as those arrested w/in 1k from two years prior to each released offender's arrest until the date of incarceration exit. For each estimation, we report the F-statistic and associated p-value for a joint test of significance of all the explanatory variables listed. We cannot reject that all of the reported estimated coefficients are jointly equal to zero in every specification except the first, providing support that our peer measures are unrelated to observed characteristics which are important predictors of recidivism.

Table 3: Peer Effects by Attribute Similarity

	(1) Arrested w/in 1yr	(2) Re-incarcerated w/in 1yr
1. Peers Incarc ($\leqslant 1km$)	−0.000	0.000
	(0.001)	(0.001)
2. Peers = w/in 1K, Age w/in 5 yr	−0.005*	−0.005*
	(0.003)	(0.003)
3. Peers Incarc w/ Age $\pm 3yr$	−0.006*	−0.006*
	(0.003)	(0.003)
4. Peers Incarc w/ Age $\pm 1yr$	−0.013**	−0.009
	(0.006)	(0.006)
5. Peers Incarc w/ Age $\pm 1yr$, Same Race-Gender	−0.024***	−0.019**
	(0.008)	(0.007)
6. Peers Incarc w/ Age $\pm 1yr$, Same Race-Gender-Building	−0.062**	−0.047*
	(0.028)	(0.027)
7. Peers Incarc w/ Same Building & Surname	−0.109***	−0.120***
	(0.031)	(0.032)
8. Peers Incarc w/ Former Criminal Partners*	−0.123*	−0.060
	(0.073)	(0.069)
Dep. Var (mean)	0.445	0.461
Observations	14,697	14,697

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors robust to arbitrary within-CBG correlation in parentheses.

General Estimation Note: All regressions include controls for gender, race, age at incarceration exit, type of offense associated with the incarceration spell, number of months incarcerated, time between arrest and incarceration, number of prior months incarcerated, and number of prior arrests. We focus on effects of peers incarcerated of similar age, race, and gender, but always include a variable measuring the other neighbors incarcerated in our specifications. Thus, in each model, we highlight the effects from likely peers while controlling for other criminal neighbors incarcerated. We also include year by month of release fixed effects as well as Census Block Group 2000 (CBG) by peer attribute fixed effects. Our estimation sample is defined in Table 1.

This table presents results for specifications varying the definition of peers. We start with incarcerated individuals arrested while residing within one-kilometer (residential address) of the released offender between two years prior to the focal offender's initial arrest date and the offender's release date. The second through fifth rows estimate the influence of peers with increasingly similar characteristics (indicated by the description of each panel). The sixth row restricts proximity to the same building parcel address. The seventh row defines peers as individuals within the building who also have the same last name (a proxy for same family). Finally, the eighth row defines peers as former criminal partners.

* Due to the availability of the partnership arrest data, these results are based on a subsample of individuals entering prison on or after Jan. 1, 2006. For this model, we include 5,400 observations

Table 4: Placebo Tests

	(1) Arrested w/in 1yr	(2) Re-incarcerated w/in 1yr
1. Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated	−0.024***	−0.019**
	(0.008)	(0.007)
Mean of Dep. Var.	0.445	0.461
Observations	14,696	14,696
2. **LAG 1YR**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated	−0.013	−0.005
	(0.012)	(0.010)
Mean of Dep. Var.	0.444	0.465
Observations	13,248	13,248
3. **LEAD 1yr**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated	−0.005	0.005
	(0.009)	(0.009)
Mean of Dep. Var.	0.444	0.457
Observations	12,746	12,746
4. **RANDON LOC**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated	−0.007	−0.003
	(0.009)	(0.008)
Mean of Dep. Var.	0.445	0.461
Observations	14,696	14,696

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table reports results from three placebo (or falsification) specifications. The first panel reports baseline results previously reported in Table 3 for comparison. The second and third panels present estimates from models in which all peer variables are measured either 1 year prior (LAG) or 1 year post (LEAD) the actual release date of each individual observation in our sample. The fourth panel randomly assigns a pre-incarceration location (from the set of all pre-incarceration locations for our sample) to each released offender and calculates peer values based on the randomly assigned location. Panel 2 limits the sample to those entering prison after Jan. 1, 2001 and Panel 3 limits the sample to those exiting prison prior to Jan. 1, 2009 since all peer variables are based on counts one year past the actual exit date. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table 5: Robustness of Results to Controls for Entry Conditions, Neighborhood Crime Trends and Enforcement

	(1) Baseline Results	(2) Peers Inc. at Entry	(3) Total Peers Enter During Inc.	(4) Nbhd Crime Vars Pre Release	(5) Nbhd Crime Vars Post Release
Peers = 1K, Age 1yr, Race, Gender					
Peers Incarcerated	−0.024***	−0.020**	−0.022**	−0.024***	−0.022**
	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)
Peers Incarcerated at Entry		−0.002			
		(0.006)			
Peers Entering During Inc.			0.000		
			(0.001)		
Nbhd crime vars (3mo prior to release)					
Nbhd Reported Crimes				−0.000	
				(0.000)	
Nbhd Clearance Rate				0.100	
				(0.150)	
Nbhd crime vars (3mo post release)					
Nbhd Reported Crimes					−0.000
					(0.000)
Nbhd Clearance Rate					−0.020
					(0.149)
Observations	14,696	14,300	14,303	14,363	13,950

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table presents results from specifications testing the plausible exogeneity of our key regressors of interest. Column 1 provides our baseline results for comparison. To test whether our results are sensitive to the conditions at the time of prison or jail entry, Column 2 reports estimates including the number of peers incarcerated at the time of entry as an additional regressor in our baseline model. Column 3 includes a control for the number of peers who enter prison while the focal individual is incarcerated. Column 4 include measures of criminal activity and the crime clearance rate during the 3 months prior to the release date of the focal individual to assess whether changes in local conditions or enforcement are driving our results. Column 5 includes controls for the number of crimes and crime clearance ratio for the first three months post-release. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table 6: Heterogeneous Effects by Types of Criminals

	(1) All	(2) Property	(3) Violent	(4) Drugs	(5) Other
1.**By Released Offender Type:**					
Peers Incarcerated	−0.025***	−0.020	−0.035	−0.079	−0.012
	(0.008)	(0.037)	(0.109)	(0.079)	(0.038)
2 .**By Released Offender Type & Peer Type:**					
Peers Incarcerated for Prop Crimes	−0.022	0.029	−0.172	0.093	−0.039
	(0.024)	(0.109)	(0.272)	(0.186)	(0.096)
Peers Incarcerated for Violent Crimes	−0.011	−0.034	−0.047	0.001	0.015
	(0.015)	(0.057)	(0.204)	(0.145)	(0.068)
Peers Incarcerated for Drug Crimes	−0.041**	−0.074	−0.038	−0.138	0.042
	(0.018)	(0.087)	(0.194)	(0.136)	(0.081)
Peers Incarcerated for Other Crimes	−0.030**	0.006	0.004	−0.135	−0.058
	(0.014)	(0.055)	(0.160)	(0.139)	(0.064)
Mean of Dep. Var.	0.445	0.529	0.477	0.418	0.376
Observations	14,696	4,126	2,653	2,561	5,362

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table presents estimates effects specific to different types of released offenders based on the crime for which they were incarcerated. The first column presents estimates for our entire release sample. The second through fifth columns split by the type of crime for which each released offender was incarcerated. The types of crimes included in each category are as follows: property includes auto theft, burglary, fraud/forgery, and larceny; violent includes assault, homicide, and rape; drug includes any drug possession or distribution offense; and other captures all other crimes not listed in the other three categories such as technical violations, driving offenses, trespassing, vandalism, and disorderly conduct. Panel 1 presents estimated effects of our primary peer variable of interest (peers incarcerated defined by being w/in 1km and 1 year of age, same race and same gender). In Panel 2 we separate the peers incarcerated variable by the type of crime the peers were incarcerated for and include the four separate regressors for peers incarcerated in one specification for each column. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table 7: Heterogeneous Peer Effects by Offender Demographic Characteristics

	(1) Arrested w/in 1yr	(2) Re-incarcerated w/in 1yr
1. **BY AGE**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated * $18 \leqslant$ Age < 25	−0.015	−0.007
	(0.012)	(0.012)
Peers Incarcerated * $25 \leqslant$ Age < 35	−0.030*	−0.018
	(0.016)	(0.015)
Peers Incarcerated * $35 \leqslant$ Age $\leqslant 45$	−0.048***	−0.042**
	(0.018)	(0.019)
Peers Incarcerated * $45 \leqslant$ Age $\leqslant 65$	−0.031	−0.035
	(0.036)	(0.030)
Mean of Dep. Var.: 18 to 25	0.528	0.544
Mean of Dep. Var.: 25 to 35	0.410	0.428
Mean of Dep. Var.: 35 to 45	0.435	0.449
Mean of Dep. Var.: 45 to 65	0.374	0.393
2. **BY RACE**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated * Black	−0.025***	−0.018**
	(0.008)	(0.007)
Peers Incarcerated * Non-Black	0.017	−0.015
	(0.093)	(0.093)
Mean of Dep. Var.: Black	0.486	0.500
Mean of Dep. Var.: Non-Black	0.293	0.317
3. **BY GENDER**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated * Male	−0.025***	−0.019**
	(0.008)	(0.007)
Peers Incarcerated * Female	0.039	−0.082
	(0.252)	(0.260)
Mean of Dep. Var.: Male	0.450	0.467
Mean of Dep. Var.: Female	0.378	0.387
Observations	14,696	14,696

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table presents estimates allowing for effects of our key *Peers Incarcerated* regressor to vary by certain characteristics of the released offender. We separately identify the effect by age in panel 1, race in panel 2, and gender in panel 3. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table 8: Heterogeneous Peer Effects by Offender Criminal Backgrounds

	(1) Arrested w/in 1yr	(2) Re-incarcerated w/in 1yr
1. **BY PAROLE**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated * No Post Release Superv.	−0.023***	−0.018**
	(0.009)	(0.008)
Peers Incarcerated * Post Release Superv.	−0.045**	−0.032
	(0.023)	(0.021)
Mean of Dep. Var.: No Post Release Superv.	0.449	0.468
Mean of Dep. Var.: Post Release Superv.	0.370	0.332
2. **BY INC LENGTH**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated * Incarcerated $\leqslant$ 6mo	−0.033**	−0.027**
	(0.013)	(0.011)
Peers Incarcerated * Incarcerated $>$ 6mo	−0.020**	−0.014
	(0.009)	(0.009)
Mean of Dep. Var.: Incarcerated $\leqslant$ 6 months	0.484	0.510
Mean of Dep. Var.: Incarcerated $>$ 6 months	0.410	0.419
3. **BY PRIOR INC**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated * No Prior Inc	−0.030**	−0.028**
	(0.013)	(0.012)
Peers Incarcerated * Prior Inc	−0.021**	−0.013
	(0.010)	(0.009)
Mean of Dep. Var.: No Prior Incarceration	0.306	0.317
Mean of Dep. Var.: Prior Incarceration	0.539	0.559
3. **BY RECID RISK**: Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated * 1st Quartile Risk	−0.030**	−0.029*
	(0.015)	(0.015)
Peers Incarcerated * 2nd Quartile Risk	−0.032**	−0.018
	(0.015)	(0.014)
Peers Incarcerated * 3rd Quartile Risk	−0.011	−0.015
	(0.015)	(0.015)
Peers Incarcerated * 4th Quartile Risk	−0.028**	−0.015
	(0.014)	(0.011)
Mean of Dep. Var.: Q1 Recid Risk	0.219	0.255
Mean of Dep. Var.: Q2 Recid Risk	0.368	0.394
Mean of Dep. Var.: Q3 Recid Risk	0.497	0.504
Mean of Dep. Var.: Q4 Recid Risk	0.694	0.691
Observations	14,696	14,696

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table presents estimates allowing for effects of our key *Peers Incarcerated* regressor to vary by certain characteristics of the released offender. We separately identify the effect by whether the CBG associated with a released offender is above or below median in terms of reported crime rates in panel 1; length of incarceration (above or below 6 months) in panel 2; whether the released offender had any prior incarceration or not in panel 3; and by predicted risk quartile in panel 4. To obtain the predicted risk, we regress our outcome variable on all controls with the exception of the peers incarcerated variables and then rank the predicted risk scores over a uniform distribution. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table 9: Aggregate Neighborhood Relationship Between Incarceration Rates and Crime

	(1) ln(Crime)	(2) Crime Clearance Rate
Total CBG neighbors incarcerated	-0.0060^{***}	0.0012^{**}
	(0.0016)	(0.0005)
Mean of Dep. Var.	4.004	0.311
Total CBG neighbors incarcerated - First Quartile	−0.0004	−0.0027
	(0.0110)	(0.0037)
Total CBG neighbors incarcerated - Second Quartile	−0.0042	0.0012
	(0.0056)	(0.0016)
Total CBG neighbors incarcerated - Third Quartile	-0.0085^{***}	0.0019^{**}
	(0.0027)	(0.0008)
Total CBG neighbors incarcerated - Fourth Quartile	-0.0053^{**}	0.0008
	(0.0021)	(0.0006)
Mean of Dep. Var.: First Quartile	3.069	0.271
Mean of Dep. Var.: Second Quartile	3.885	0.301
Mean of Dep. Var.: Third Quartile	4.232	0.336
Mean of Dep. Var.: Fourth Quartile	4.565	0.323
Observations	11,751	11,751

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table presents estimates form a CBG-by-quarter panel data set tracking the number of individuals incarcerated for the entire quarter and quarterly reported crimes (Columns 1) and crime clearance rates (Column 2). The regressor of interest only includes those who are in prison or jail at the beginning and end of the quarter and thus does not include any individuals committing crimes in the neighborhood during the quarter. The dependent variable in the first column is the natural log of the reported CBG crimes per quarter. The dependent variable in the second column is the fraction of crimes which are cleared by arrest. The second panel interacts the number of neighbors incarcerated with an indicator for each quartile of the distribution of individuals incarcerated across CBGs to estimate heterogeneous effects across neighborhoods with higher or lower incarceration rates. The quartiles are calculated by ranking the average number of individuals incarcerated in a neighborhood over our time frame adjusted for the number of people per square mile (population density) in the CBG. All specifications include fixed effects for neighborhood (CBG) and year-quarter as well as a CBG-specific time trend. The mean of our regressor of interest in the first panel *Total CBG neighbors incarcerated* is 5.77 and the standard deviation is 6.22.

A. Appendix Materials – NOT FOR PUBLICATION

A.1. Alternative outcomes and definitions of peers incarcerated

This section describes results from robustness checks and variations from our baseline model. Table A.4 provides results varying the level of fixed effects and area-specific trends used in our specifications. Overall, results are quite consistent and robust to the choice of these model specifications.

For further robustness checks, we assess the sensitivity of variations to our criteria used to calculate our measure of peers incarcerated in Table A.2 as described in detail in Section 2. First, we limit peers to just those individuals that are incarcerated for the entire year post-release year rather than allowing peers incarcerated for part of the year to contribute partially to our variable of interest as illustrated in Figure 3. Using this "all-or-nothing" criteria excludes the potential influence from peers who are incarcerated at the time of release but themselves are released during the post-release year, so it is not surprising that our estimated effect decreases in magnitude. Our baseline measure does not include individuals who were arrested more than 2 years prior to the released offender's pre-incarceration arrest in peer calculations. We relax this restriction and allow individuals who are tied to the same pre-incarceration neighborhood going back to the beginning of our arrest records in 1998 to contribute to our variable of interest and report these results in Column 3 of Table A.2. Again, results are significant but smaller in magnitude due to the inclusion of less relevant (or less influential) peers who may not have been active in the neighborhood of the focal released offender. Column 4 reports results from a specification in which we only allow individuals who were associated with the neighborhood immediately prior to their incarceration and do not use information from other arrests within the window from two years prior to arrest and the date of release. We find smaller and less precise results for this more restrictive definition of a neighborhood peer. Finally, we redefine neighborhoods as Census Block Groups (CBGs) instead of 1km cocentric cirles around pre-incarceration residential addresses and report estimated effects in Column 5 of Table A.2 and find smaller and less precise estimates.[43] In total,results presented in Table A.2

[43]The average size of a CBG 2000 neighborhood in our sample is approximately 1.4 square km, so would capture

are consistent but slightly reduced and less precise as we include less relevant peers or peers that are less likely to still be living in the neighborhood.

Table A.3 first evaluates whether key types of peers drive our results by excluding certain types of peers from the regressor of interest in our baseline specification. First, we assess the influence of peers who are incarcerated around the same time by excluding those who enter prison within one month (either before or after) our sample of released offenders. As discussed in Section 3.1, this exercise provides reassurance that our estimates are not picking up some systematic pattern whereby less-serious offenders are released earlier than more-serious offenders. We find very similar effects excluding those entering prison around the same time from our calculation.

Due to the strong influence of same-address peers (Panel 6 of Table 3), same-address and surname peers (Panel 7 of Table 3), and former criminal partners (Panel 8 of Table 3) we estimate our baseline model excluding these types and report results in Columns 2 through 4 of Table A.3. We are reassured that our estimates are robust to the exclusion of these influential peers suggesting that other peers with similar attributes (age, race, and gender) also influence the recidivism rates of our estimation sample. We then assess whether baseline results are robust to excluding controls mechanically related to the potential number of neighborhood criminal peers. As described in Section 2.1, we count as peers those who are arrested (and then incarcerated) who are arrested with a residential location within 1km of our focal individual's pre-incarceration location within a window stretching 2 years before our focal individual's arrest date and the day of release. Thus, for those who have longer incarceration spells or who have larger gaps between the arrest date and incarceration release date will have a longer window to capture criminals as neighborhood peers. For this reason, we exclude these variables from the Balance Test in Table 2. In Column 5 of Table A.3, we exclude control variables mechanically related to the our measure of *Peers Incarcerated* due to the window of time used to count peers in the neighborhood. These controls are the number of months incarcerated and the time between arrest and incarceration for each released offender. Our results do not change

a neighborhood in between a 1km and 2km circle. We explored the spatial definitions of our neighborhoods further in Figure 4. Results highlight the importance of using our narrow spatial definition of neighborhood given that defining neighborhoods as 1-3km and 3km-5km provide limited and imprecise effects of peers incarcerated on recidivism.

when we exclude these controls, eliminating any concerns about bias driven by the mechanical relationship between the time used to capture peers and our key regressor of interest. In our baseline model, we focus on the effect of neighborhood (1km) peers who are within one-year of age, of the same race and gender and also include a regressor measuring the effect of other neighborhood criminal peers (not of the same age, race, and gender). While we prefer to include these measures of other peers since their inclusion allows us to identify the effects of relevant peers while accounting for the presence of other neighborhood criminals, we evaluate whether our estimates are robust to the exclusion of this control in Column 6 of Table A.3. Our estiamted effects are slightly attenuated since our measure of relevant peers will be correlated with the presence of less relevant peers, but remain significant and similar in magnitude. In Column 6 of Table A.3

In Table A.5 evaluate the effects of our baseline measure of peers incarcerated over the first year post-release on alternative recidivism outcomes. First, results presented in Columns 1 and 2 estimate our baseline model but estimate the effects on recidivism measures which do not involve arrests for technical violations. Technical arrests include those for bond termination, probation violation, or parole violation. These results suggest that technical violations may influence recidivism (which is suggested by stronger effects for those under post release supervision in Table 8), but also provide assurance that our primary results are not driven by these effects.

Columns 3 and 4 of Table A.5 report effects for outcomes counting the number of arrests or days incarcerated within one year instead of a dichotomous indicator for arrest or incarceration. Estimates are less precise, but similar in magnitude—approximately a 5 percent decrease in recidivism relative to the mean of the outcome variable.

Table A.1: Correlation between *Peers Incarcerated* variables and post-incarceration mobility for reoffending subsample

	Distance (km) btw Pre- and Post-Incarceration Residential Location
Pre-incarceration Neighborhood Peers Incarcerated	
Age w/in 1yr, Same Race, Same Gender	−0.048
	(0.073)
Other Peers (w/in 1K) Incarcerated	−0.014
	(0.011)
Other Control Variables	
Black	0.099
	(0.367)
Hispanic	−1.577**
	(0.698)
Female	0.273
	(0.333)
Age at Release	−0.008
	(0.010)
Any Time in State Prison	0.085
	(0.423)
Post Release Parole Supervision	0.472
	(0.600)
Percent of Incarceration in State Prison (Remainder in County Jail)	0.074
	(0.474)
Total Prior Arrests (Since 1998)	−0.017
	(0.013)
Incarcerated for Property Crime	0.207
	(0.274)
Incarcerated for Drug Crime	−0.364
	(0.223)
Incarcerated for Violent Crime	0.465
	(0.373)
Severity of Crime (Scale from 1-11)	0.054
	(0.052)
Dep. Var - Mean	3.825
Dep. Var - Median	0.477
Observations	5,627

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table presents the relationship between the distance between the pre- and post-incarceration residential addresses for the subsample of individuals who recidivate and whose rearrest contains a residential address. A variable measuring the distance between addresses (in km) is created and regressed on our measures of pre-incarceration neighborhood peers incarcerated and other control variables. The coefficients reported are all from a single specification. Also included in the specification, but not reported, are fixed effects for the number of months incarcerated, the year-month of release, and CBG neighborhood definitions. Results with more detailed fixed effects (such as CBG by attribute) are similar to those reported above. Other general estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table A.2: Robustness Checks - Distance and Time Peer Definitions

	(1)	(2)	(3)	(4)	(5)
	Baseline Results	Redefine Peers: Inc Full Year	Redefine Peers: Longer Time Window	Redefine Peers: Pre-Inc Distance	Redefine Peers: w/in CBG
Peers = 1K, Age 1yr, Race, Gender					
Peers Incarcerated	-0.024^{***}	-0.020^{**}	-0.017^{**}	-0.019^{*}	-0.019
	(0.008)	(0.010)	(0.007)	(0.011)	(0.013)
Observations	14,696	14,696	14,696	14,696	14,696

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table provides results from specifications varying the way in which we define peers. Column 1 provides our baseline results for comparison. As described in Section 2, we allow peers incarcerated for part of the first post-release year to contribute a fractional amount to our primary *Peers Incarcerated* measure. Column 2 presents the estimated effect when we only count those peers which are incarcerated for the entire post-release year. Our primary measure of peers requires that an individual is arrested w/in 1K of the focal offender's pre-incarceration residential address and this arrest occurs not earlier than 2 years prior to the focal offender's arrest in this neighborhood and not later than the focal offender's release date. In Column 3, we relax the floor of this time window allowing individuals who were arrested while living in the same neighborhood more than two years prior to count within the released offender's peer group. Finally, we count as peers individuals who have many arrests but at least one of their arrests is w/in 1K of the focal individuals address. In Column 4, we restrict the peer group only to those who were w/in 1K with the residential address just prior to their own incarceration spell. Column 5 estimates our primary specification but redefines neighborhoods to a Census Block Group rather than a 1km circle surrounding the released offenders pre-incarceration address. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table A.3: Robustness Checks - Exclude Certain Types of Peers, Exclude Key Control Variables

	(1) Exclude Peers Entering Prison w/in 1mo	(2) Exclude Same Bldng Peers	(3) Exclude Same Bldng and Surname Peers	(4) Exclude Former Partner Peers	(5) Exclude Time Inc. Controls	(6) Exclude Other Peer Inc. Controls
Peers = 1K, Age 1yr, Race, Gender						
Peers Incarcerated	−0.025***	−0.022**	−0.023***	−0.025	−0.024***	−0.020***
	(0.009)	(0.009)	(0.008)	(0.031)	(0.008)	(0.007)
Observations	14,696	14,692	14,692	5,400	14,696	14,696

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table provides results from specifications excluding certain types of peers from our baseline group (within 1km and 1 year of age, same race and gender) to assess whether specific types are driving our main results. Column 1 presents results from a model in which peers who enter prison within one month of the focal individual are not included in the peers incarcerated calculation. Column 2 presents the estimated effect when we exclude those peers who are matched to the same parcel address. Column 3 presents estimates when we exclude peers who have the same surname and parcel address (a proxy for family members). Column 4 presents results excluding former criminal partners from our primary peer measure on the data post-2005 due to the availability of partnership data. Column 5 presents results where we exclude control variables mechanically related to the our measure of *Peers Incarcerated* due to the window of time used to count peers in the neighborhood. We count as peers those who are arrested (and then incarcerated) who are arrested with a residential location within 1km of our focal individual's pre-incarceration location within a window stretching 2 years before our focal individual's arrest date and the day of release. Thus, for those who have longer incarceration spells or who have larger gaps between the arrest date and incarceration release date will have a longer window to capture criminals as neighborhood peers. For this reason, we exclude these variables from the Balance Test in Table 2. We test whether our baseline results are robust to excluding these time measure here in Column 5 to ensure that this relationship is not biasing our estimated effects. Column 6 excludes the control we include in our baseline model for all other types of peers incarcerated (those not of the same age, race, and gender). General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table A.4: Robustness Checks - Alternative Fixed Effects and Neighborhood-Specific Time Trends

	(1)	(2)	(3)	(4)	(5)
Peers = 1K, Age 1yr, Race, Gender					
Peers Incarcerated	-0.018^{***}	-0.024^{***}	-0.024^{***}	-0.025^{***}	-0.025^{***}
	(0.006)	(0.008)	(0.008)	(0.009)	(0.009)
Observations	14,696	14,696	14,696	14,696	14,696
CBG-Age FE	✓	-	-	-	-
CBG-Age-Race-Gender FE	-	✓	✓	✓	✓
Year-Month of Entry FE	-	-	✓	-	-
CBG-specific linear trend	-	-	-	✓	✓
CBG-specific quadratic trend	-	-	-	-	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table provides results from specifications with various levels of location fixed effects and location-specific time trends. Our primary results focus on peers within 1K, age within 1 year, and of the same race and gender. For these specifications, we include fixed effects for each combination of CBG, age at release, race, and gender. These baseline results are presented in Column 2. Column 1 includes only CBG by age at release fixed effects. Columns 3 includes year-by-month of incarceration entry fixed effects. Columns 5 and 5 present estimated coefficients from models which include CBG-specific time trends to assess whether any unobserved trends within different locations may be driving our results. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table A.5: Effects by Alternative Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Arrested Non Tech. Violation w/in 1yr	Re-incar. Non Tech. Violation w/in 1yr	Num of Arrests w/in 1yr	Num of Incar Days w/in 1yr	Rearrest for Partnership Crime	Rearrest for Partnership Crime w/in 1km
Peers = 1K, Age 1yr, Race, Gender						
Peers Incarcerated	−0.022**	−0.016**	−0.049	−2.594	−0.009	−0.009
	(0.008)	(0.008)	(0.030)	(1.622)	(0.026)	(0.010)
Mean of Dep. Var.	0.416	0.434	0.920	40.957	0.096	0.016
Observations	14,696	14,696	14,696	14,696	5,400	5,400

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

This table presents estimated effects for alternative outcomes. Each coefficient represents an estimate from a separate specification. The first two columns report estimated effects on our arrest and re-incarceration outcomes excluding from the rearrest and reincarceration probabilities those whose only have arrests during the first year post-release for technical violations. Technical violations include bond termination, probation violation or parole violation. Columns 3 and 4 report results from models with continuous outcomes (number of arrests and number of days incarcerated) rather than dichotomous recidivism indicators. Column 3 reports our estimate of the effect of similar peers incarcerated at the time of release on the number of arrests within the first year post-release. Column 4 reports the estimated effect on the total number of days incarcerated during the first post-release year. The final two columns report estimated effects of our key peer regressor of interest on the probability of a recidivating through a crime committed with a partner. First, Column 5 reports the estimated effect of a neighborhood peer incarcerated on being rearrested with any partner. Column 6 restricts the outcome to being rearrested with a partner from the 1km neighborhood. General estimation notes from Table 3 and sample construction notes from Table 1 apply.