Hanging Out with the Usual Suspects: 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 reentry following a prison or jail sentence. This paper measures the influence of pre-incarceration social networks on recidivism by exploiting the fact that peers may themselves be locked up and away from the neighborhood when a prisoner returns home. Using detailed arrest and incarceration data that includes residential addresses for offenders in Charlotte, North Carolina, 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. These peer effects are increasing in the degree of social connectivity as measured by residential proximity, past criminal relationships and attribute (e.g. age, race, gender) similarity. We find that one less criminal peer of the same age, race, and gender in the neighborhood over the first year post-release is associated with a five percent decrease in the probability of arrest.

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

JEL classification codes: C31, J10, K42, Z13

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Recidivism represents a costly failure of the criminal justice system and is often attributed to difficulties in establishing stable employment and housing and to other personal obstacles such as substance abuse, mental health disorders, or financial obligations (Visher and Travis 2003). These reintegration challenges are both mitigated and exasperated by the social environment upon reentry (Sampson 2011). Supportive peers, family, and other positive role models prevent reoffending, but relationships with criminally active individuals promote reoffending and can erode the efficacy of programs directly addressing employment, housing, and health outcomes.

An emerging literature documents the negative influence from offending 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 (Deming 2011, Billings et al. 2014) and neighborhoods (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 among immigrants in Denmark. All together, 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 this evidence documenting the criminal influence of peers and an increasing awareness of the costs of recidivism, little is known about the effect of pre-incarceration social networks on successful prisoner reentry for several reasons. First, identifying social networks is difficult without detailed information on offender locations or surveys measuring social connectivity.[3] Further, identifying the causal relationship between neighborhood peers and recidivism is complex given the presence of social and correlated effects (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 recidivism is also affected by the same factors underlying changes to the number of criminals in the neighborhood such as police enforcement or employment opportunities ("correlated effects").[4]

Using administrative arrest and incarceration records from Charlotte, North Carolina, we provide novel evidence of the relationship between neighborhood criminal peers and recidivism in a setting not unlike that faced by the hundreds-of-thousands of offenders exiting 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 reentry as well as to count the number of criminal peers who are not in the neighborhood when an individual returns home. We exploit the fact

[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.

[4]Manski (1993) defines endogenous effects as "the propensity of an individual to behave in some way varies with the behaviour 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).

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 also exploit variation in social interactions at the time of release arising from the pre-release flow of neighborhood criminal peers into prison or jail. Through a series of balance and placebo tests, we show that once we control for a detailed set of neighborhood and offender characteristics, variation in the number of peers incarcerated at the time of release is driven by factors plausibly unrelated to unobserved determinants of recidivism.

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 his release. These peer effects are increasing in the degree of connectivity as measured by pre-incarceration residential proximity, past criminal relationships and attribute (e.g. age, race, gender) similarity. We find that a decrease in the presence of one peer (about one standard deviation) who lives within one kilometer, is within one year of age and of the same race/ethnicity and gender is associated with a 2.4 percentage point decrease in the probability of arrest during the first year post-release (approximately a five percent decrease relative to the mean rate of recidivism). Our estimated social interaction effect on recividism is much larger for individuals who were tied to the same crime—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 or social spillovers, 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 narrowly defined peers and for those committing drug crimes lend support to 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 affected 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 neighborhood and reported crimes with the largest effects in neighborhoods with the highest levels of incarceration which is consistent with the existence of social multiplier effects.[8]

Our results contribute to a large literature evaluating other determinants of recidivism such as post-release employment. It is well known that individuals experience low rates of employment

[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 of 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 those who recidivate within one year report the same post-incarceration residential address as that reported for the pre-incarceration arrest.

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

[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.

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 is waking up early and engaging in physically exhausting work—these job characteristics may prevent interactions with criminal peers in the neighborhood compared to a job in retail or food services. In fact, Redcross et al. (2012) speculate that differences in the nature of social interactions across recent randomized controlled trials evaluating reentry 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 neighborhood due to insufficient money, housing discrimination, or even post release supervision requirements. Given the potentially damaging influence of neighborhood criminal peers, we should continue to expect high rates of recidivism in neighborhoods with a lot of criminal peers. Policies that provide housing assistance away from other criminals (similar to the Moving to Opportunity) or more strictly enforce interaction among former criminals may lower recidivism. Another policy that may prevent negative social interactions is electronic monitoring. Di Tella and Schargrodsky (2013) document large reductions in recidivism for offenders under electronic monitoring compared to those who are sent to prison. These effects may be partially attributed to preventing the formation of criminogenic relationships within prisons. Electronic monitoring could also be used to limit negative social interactions in the neighborhood by 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 incarcerated peers on reoffending. Section 4 presents and discusses 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 (city of Charlotte) 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

[9] This point highlighted in a discussion of recent evaluations by Raphael (2014). More recently, Cook et al. (2015) did not find large differences in recidivism in a reentry program which combined pre-release social services with employment reentry progrms. 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.

[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.

with over 95% of jail or prison records matching an arrest record. Beginning in 2005, the registry of offenders was also linked to criminal incident records, allowing us to identify individuals committing crimes together to link 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 as well as information on the location-of-residence at the time of arrest. The location-of-residence 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 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. Among released prison and jail inmates meeting our sample criteria, we are able to match approximately 80% to pre-incarceration residential address information using the arrest records.[11]

Our primary estimation sample includes individuals 18 through 65 years of age incarcerated for at least 3 months.[12] 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. We see a large number of offenders with relatively short incarceration spells and given that we need to observe individuals post-release, we do not include individuals incarcerated more than 10 years. Our sample is predominately comprised of individuals serving relatively short incarceration spells with 46% serving 6 months or less and 74% serving a year of less.

Table 1 indicates that nearly half of the individuals released from prison or jail are arrested within one year from release. We use the probability of arrest within one year as our primary measure of recidivism. A similar fraction is reincarcerated within one year. Consistent with prison populations in general, 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 that post-release arrest (5,783 of released offenders meet this criteria). 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 5 km of the pre-incarceration location. While we can observe this lack of mobility only for those who recidivate, it is reassuring for our estimates that the majority of offenders return to the same neighborhood.

[11] Not surprisingly 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.

[12] Even though an individual is considered an adult as of age 16 in North Carolina, we focus our analysis on individuals at least 18 years of age to avoid any confounding effects due to school attendance which is based on residential address.

2.1. Defining Criminal Peers

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. While other groups of peers may exert influence (such as schoolmates and workmates), our sample of adults leaving jails and prisons are also influenced by their criminally active neighbors. One limitation to our approach is that we cannot measure the influence of peers who are not arrested and incarcerated nor those who are not criminally active but also impart criminal peer effects.

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. In order to construct a variable measuring 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, who are matched to a residential address within a specified distance (our primary focus is on a distance of within 1K) from a released offender, and who were arrested no earlier than two years prior to the focal offender's arrest date.[13] We refer to this group as *neighbors incarcerated* at the time-of-release. We then decompose the total number of neighbors incarcerated into those who share various demographic and criminal history characteristics with the released offender.[14]

Each variable measuring the number of peers incarcerated is constructed to represent the number incarcerated in jail or prison during the first year post-incarceration. To avoid any simultaneity bias, peers only contribute to these measures if they are incarcerated on the day when an individual is released from jail or prison. We then calculate the fraction of the first post-release year which each peer remains in prison and sum these fractions over each individual release observation. 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).[15]

In order to identify peer effects in this setting, we need a sufficient amount of variation in peers incarcerated over time within small spatial definitions of neighborhoods conditional on all of the controls included. We find that 70 to 80 percent of the variation in our key peer measures is explained by our detailed set of control variables and fixed effects for Census Block Groups (CBG) and time periods of release.[16] There is still a substantial amount of variation not

[13]If there are more than one pre-incarceration arrests for potential peers, we use the minimum distance from the focal offender's pre-incarceration arrest address to determine the distance. In a robustness check, we test whether results are similar if we only use the potential peer's address associated with the arrest closest to the incarceration spell.

[14]We examine results for alternative definitions of peers through varying neighborhood definitions, race/ethnicity match, and the relevant age window.

[15]These alternative results are discussed in Appendix Section A.1 and presented in Appendix Table A.1. 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.

[16]The fraction of variation explained by our controls for various peer measures is reported at the bottom of

explained by the fixed effects and detailed controls indicating that there is enough variation left to identify the influence of neighborhood criminal peers in this setting.

3. Empirical Methodology

In order to assess the influence of criminal peers on released offender's behavior, we estimate the following model:

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

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 since his release from incarceration. We present results for both rearrest and reincarceration definitions of recidivism.[17] As described in Section 2.1, our key variable of interest, $\text{Peers Incarcerated}_{ijt}$, measures the number of i's neighborhood peers incarcerated (in jail or prison) during the first reentry year where neighborhoods are defined as a 1km circle surrounding i's pre-incarceration residential location as reported in the arrest records. We decompose the total number of neighbors incarcerated into groups of increasingly similar peer groups. Our preferred model focuses on a measure of peers which includes those within one-year of age, and of the same race and gender. For all models which 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 $\mathbf{X}_i$. To account for unobserved neighborhood determinants of criminal activity and any shocks common to a particular time period, we include CBG-by-offender-attribute fixed effects (γ_j) and year-by-month fixed effects for the incarceration exit date (δ_t). All specifications allow for arbitrary correlation in unobservables within census block group (CBG) areas.[18]

3.1. Identification Concerns

In order to apply a causal interpretation to our estimates of β_1 in Eq. 1, we need the variation in $\text{Peers Incarcerated}_{ijt}$ to be "as good as random" conditional on the control variables and fixed effects included. Our estimates will be biased if there exist unobserved determinants of post-release re-offending that are correlated with our measures of social influence. We assess the potential influence of these factors in a variety of ways.

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 of jail or incarceration. In order 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 post-release differential 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

Table 2.

[17] We also present and discuss results over alternative time frames in Section A.1 and Table A.5.

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

of neighborhoods. To account for neighborhood-level determinants of recidivism, all of our specifications include location fixed effects, which will limit any systematic bias from certain neighborhoods.[19] 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) and also test whether neighborhood crime rates prior to release predict the number of peers or partners present at release.

In order to support the validity of our identification assumption 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 $\mathtt{Peers\ Incarcerated}_{ijt}$, we cannot reject the null hypothesis that the coefficients on the individual's criminal history characteristics are jointly equal to zero. 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 capture potential neighborhood criminal peers.[20] 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.

Another potential identification concern arises from the fact that entry into our estimation sample could impact our measure of peers incarcerated. One can imagine this feedback effect due to sample construction positively or negatively affecting our measure of neighborhood criminal peers incarcerated at the time of release. First, the incarceration of an individual in our sample could affect the number of neighborhood peers going to jail through a mechanism by which the offender cooperates 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 for peers in the neighborhood through a social interaction effect.[21]

Since each individual in our sample has the same effect of removing one criminal from the neighborhood, the main concern is if individual predictors of recidivism correlate with any of the effects of our sample individual on neighborhood peers. Table 2 provides strong evidence that this is not a concern along observable individual offender attributes. Since unobserved attributes such as being a gang leader or an exceptionally social criminal may not be captured in our offender attributes, we present a series of robustness checks in Section 4 that include controls for peer stock at the time our focal individual enters jail and peer flows into jail (where individuals may be released prior to when we measure peers) while our focal individual is

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

[20] We allow those who are arrested and incarcerated in the neighborhood of our a released offender from two years prior up to the release date to contribute to our neighborhood peers measure. Thus, 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.

[21] 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 themselves 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.

incarcerated.[22] These controls do not change our main results and should capture any positive or negative effects of sample construction on our measure of peers.

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 and discuss these results in the following Section 4. Further, it is unlikely that differences in enforcement across neighborhoods are heavily influenced by the presence of criminal peers in such a narrowly defined neighborhoods. An additional concern 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 by police officers. However, we do not think our latter results substantiates this as an important factor since we find the strongest peer effects for less serious (i.e. no prior arrests, shorter prison sentences) criminals who are less likely to receive additional attention by police officers.

4. Results

4.1. Peers and Recidivism

Table 3 presents our estimates of Eq. 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 1km 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 a reduction in the presence of neighborhood criminal peers 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 those who share the same pre-incarceration residential address, the same address and surname, and even those having a prior history of former criminal partnership.[23] This pattern of increasing influence based on attribute similarity is also reflected in Figure 4, as is the importance of residential proximity.

To focus our analysis, we primarily discuss results from a our "baseline" model where we use one-year arrest and incarceration outcome variables and define criminal peers as those whose pre-incarceration residence is within 1km, are within one year of age, and are of the same race/ethnicity and gender. We present results for alternative outcomes and for alternative

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

[23] Another dimension of connection used in related studies starting with Bayer et al. (2009) is based of shared time incarcerated at the same detention facility. Unfortunately, our data does not allow us to explore same facility peers. Likely, closeness in types of criminal and neighborhood of residence predict assignment to similar detention facilities which would strengthen peer influences along those observable dimensions of our data.

definitions of our key peers incarcerated regressor in Appendix Tables A.1 and A.2.[24] Results for our preferred specification are presented in Panel 5 of Table 3—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 likely family members or former criminal partners, we find effects implying a larger than 10 percentage point decrease in the probability of recidivism associated with a reduction of the presence of one criminal peer for the first year post-release (nearly a 25% effect relative to the mean rate of recidivism).[25]

To test our identification assumptions, we include three placebo specifications. In Table 4, we estimate the influence of the number of peers and partners incarcerated one-year prior to release (Panel 2), 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 measures. We find no evidence of any such factors. To illustrate these placebo checks varying the release date, 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.[26] 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 alleviating any concerns about the influence of unobserved factors at the time-of-release.[27]

Since we identify social interactions from fluctuations in the number of individuals incarcerated, we are concerned that variation in our primary regressor of interest could be influenced by a persistent (or cyclical) pattern of neighborhood incarceration rates, crime waves and police responses to the crime waves. To assess the influence of such factors on our results, we implement a series of robustness checks in Table 5 and compare each to the baseline result presented in Column 1.

First, 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. While our estimate is slightly smaller than our baseline estimate, our results are clearly not driven by a persistent effect of neighborhood criminal peer concentration conditions at entry. Column 3 controls for the total number of neighborhood criminal peers entering prison during the focal individual's incarceration. This measure is intended to capture any bias from the influence of the incarceration of those in our estimation sample on the neighborhood concentration of criminal peers (as described in Section 3.1). We find consistent effects across these specifications eliminating concerns about the relationship between the construction of our estimation sample and our primary measure

[24]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; restricting our definition of peers only to those whose arrest immediately preceding the incarceration spell is within the 1km neighborhood; defining neighborhoods with CBG designations rather than 1km 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.

[25]Our results for peers defined as same neighborhood, age, race, and gender are not driven by these high-impact peers as results are similar when we exclude peers based on same building, same family or former criminal partners (Appendix Table A.2.

[26]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.

[27]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.3.

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 cleared by arrest) just before and after release from incarceration. 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 levels of fixed effects and neighborhood-specific linear and quadratic time trends.[28]

Our initial results provide strong evidence that the concentration of neighborhood criminal peers during the reentry period exerts a causal influence on criminal recidivism. Questions remain as to which types of offenders and types of peers are driving these 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 each 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 the greatest influence of peers among those who were incarcerated for drug offenses. However, these estimates are not statistically significant due to the smaller sample sizes. 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 among drug offender peers which is a result 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 violent and property criminal peers, but effects are smaller in magnitude.

We estimate heterogeneous effects across a range of demographic and criminal history characteristics in Tables 7 and 8. These models estimate effects for the full sample and interact indicators for the various groups of interest with the neighborhood peers incarcerated regressor. Analyzing effects by the age at the time of exit in the first panel of Table 7, we find the strongest response to neighborhood criminal peer concentration among released offenders between the ages of 25 and 45. The influence of incarcerated peers on recidivism is not statistically significant for young offenders (between 18 and 25) or for older offenders (greater than age 45). The fact that results are not limited to our youngest cohorts is somewhat surprising and highlights a role for peers beyond young adults who are usually more active in criminal gang activity. 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 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 as less serious criminals. For our estimation sample, individuals convicted of certain felonies are required to have 9 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 which can involve a wide range of requirements such as random drug screenings, employment, and victim restitution payments. While our estimates are less precise, we find a larger influence of peer presence for the individuals under supervision. Peers likely exert influence over the ability of individuals to meet the various supervision

[28]These robustness checks are presented in Appendix Table A.3.

requirements.[29]

We find slightly larger peer effects for released offenders with shorter sentences and no prior incarcerations. The third panel of Table 8 and Figure 6 present results by quartiles of predicted risk of recidivism (rearrest w/in 1 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 distribution of predicted risk of recidivism, but also find effects for those in the highest risk quartile.[30] 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. We will discuss and explore a social multiplier effect looking at the aggregate relationship between neighborhood incarceration and criminal activity in the next section.

4.2. 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 groups (CBG). We use this as a descriptive exercise to investigate neighborhood correlations and 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:

$$ln(Crime_{jt}) = \beta_0 + \beta_1 Neighbors\ Incarcerated_{jt} + \gamma_j + \delta_t + \sum \beta_j quarter_t + \epsilon_{jt} \tag{2}$$

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 the pre-incarceration residential addresses) who are incarcerated for the entire time period t[31]; and the other terms represent fixed effects for neighborhoods (γ_j), year-quarter time periods (δ_t), and a neighborhood-specific time trend ($\sum \beta_j quarter_t$). 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 neighbors incarcerated and the number of reported crimes in the neighborhood and report this estimated effect in the first panel and first column of Table 9. An increase of 5 neighbors incarcerated during the quarter (approximately one standard

[29] We test whether our results are driven by technical violations in Columns 5 and 6 of Appendix Table A.4. 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.

[30] 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.

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

deviation in this measure) is associated with a 3% decrease in neighborhood crime. Panel 2 highlights the nonlinear 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 suggestive of a social multiplier effect—greater declines in crime are observed in neighborhoods with higher rates of incarceration. A decrease in crimes 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 demonstrating 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, a lower number of 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 number incarcerated is associated with a 0.6 percentage point increase in crime clearance. This represents approximately a 2 percent 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 this change in the effectiveness of enforcement to be an important mechanism behind the influence of peers incarcerated on recidivism.[32]

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. Together, all of our results suggest, but cannot disentangle, a role of what Manski (1993) labeled "social effects" including both endogenous social interaction and exogenous contextual effects. Support for the importance of social interaction and social learning is given by our larger estimated effects for peers defined as same residence, family and former criminal partners. The larger peer influence of drug offenders, a crime type that involves more partnerships and gang activity, further supports the important role of social effects in determining recidivism. We do not find a large effect from congestion externalities on the relationship between criminal peer concentration and recidivism in our setting.

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.

[32] 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).

Our results suggest that an environment with less negative peer influences can reduce the high rates of failed prisoner reentry. However, designing policies to discourage social interactions with "the usual suspects" is very difficult. Policy solutions that expand housing opportunities away from a released offender's 'old' neighborhood may also be effective, but these policies may also reduce positive social interactions with supportive friends and family. The effectiveness of group homes and reentry programs could depend on the types of interactions that are facilitated by the facilities and programs. Based on the strong social effects we observe, we advocate for evaluations of reentry programs to try to incorporate survey-based measures of the effects of programs on both positive and negative social interactions within the community. Increases in interactions with positive role models through reentry 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 should limit social interactions with neighborhood criminal peers. 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. Given recent evidence contrary to most employers' perceptions of productivity differences between those with and without criminal records (Lundquist et al. 2016, Minor et al. 2016), social interactions/learning between firms could eventually improve employment outcomes for those facing employment obstacles. These social channels are particularly important for discriminatory employers with limited prior experience with ex-prisoners.

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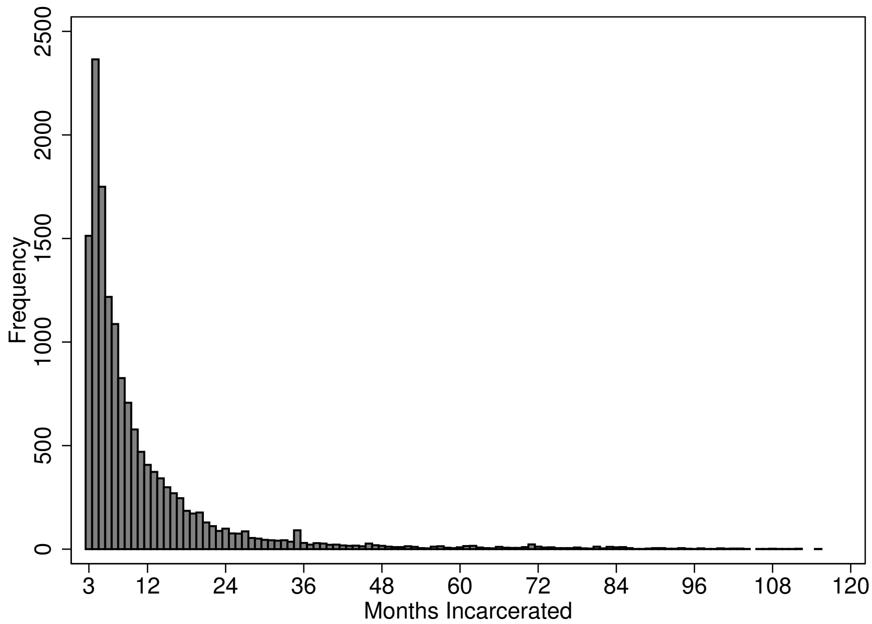
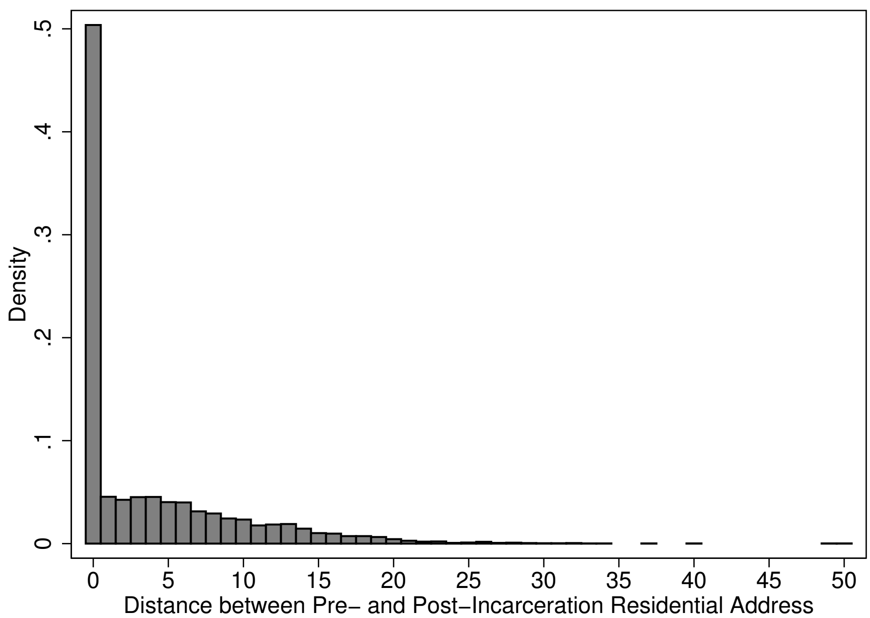
Figure 1: Months Incarcerated Histogram



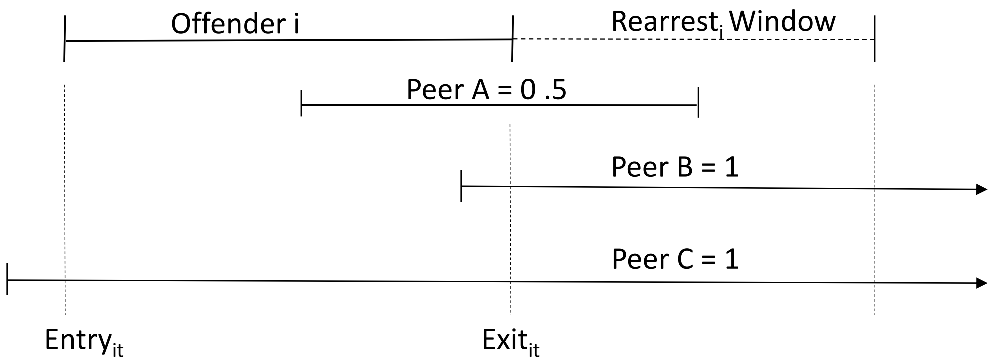
This figure plots the distribution of months incarceration in jail or prison for our sample. Our estimation sample only includes those incarcerated for at least 3 months. General sample construction notes from Table 1 apply.

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



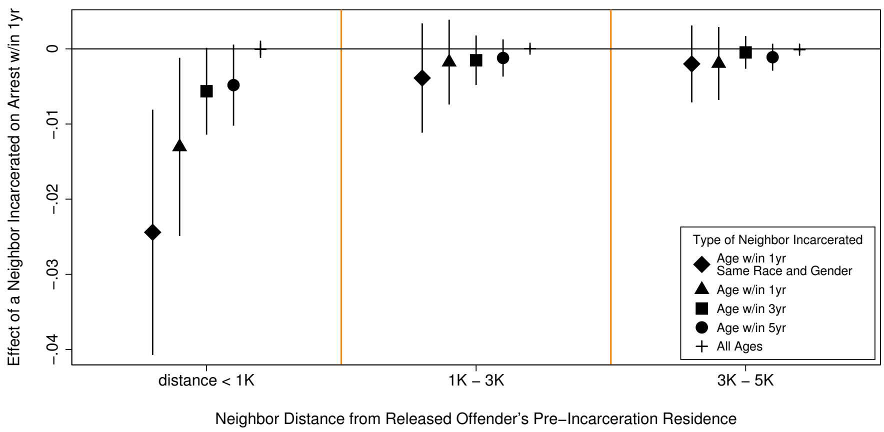
This figure plots the distance in kilometers between the pre- and post-residential addresses recorded for those in our sample who are rearrested within one year of release and report a valid residential address at the time of release. 50% of these individuals have the same residential address recorded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

Figure 3: Construction of Peers Incarcerated Measure



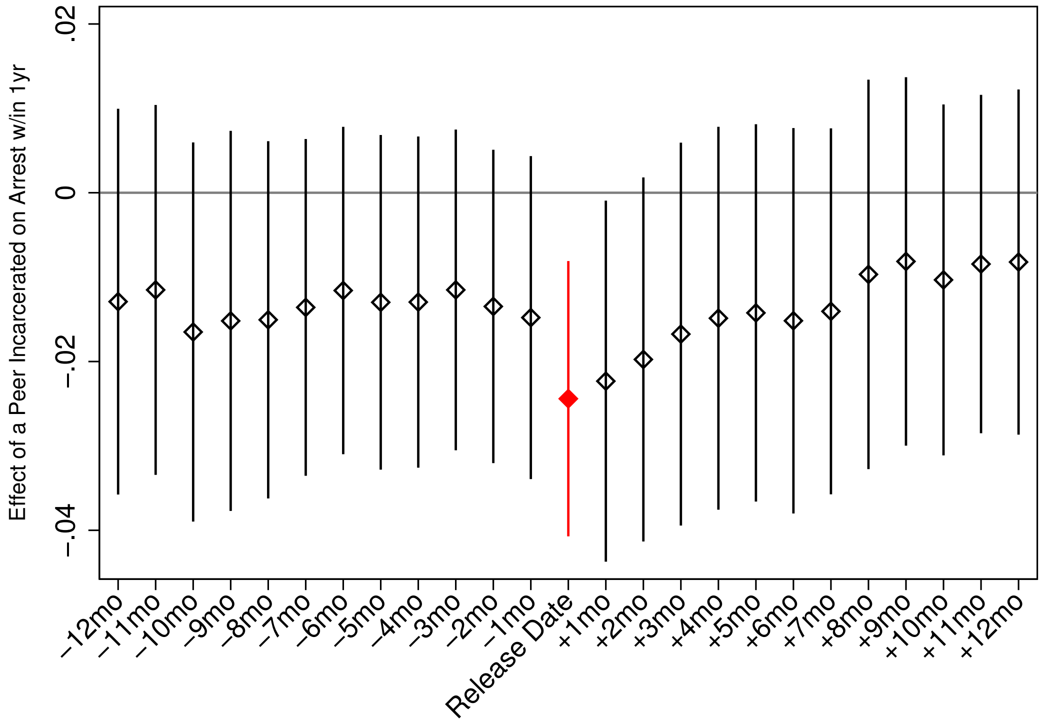
This figure provides a visual of how we create our primary regressor of interest: *Peers Incarcerated*. We measure the total number of person-years of peers who are incarcerated at the time each individual in our sample is released.

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



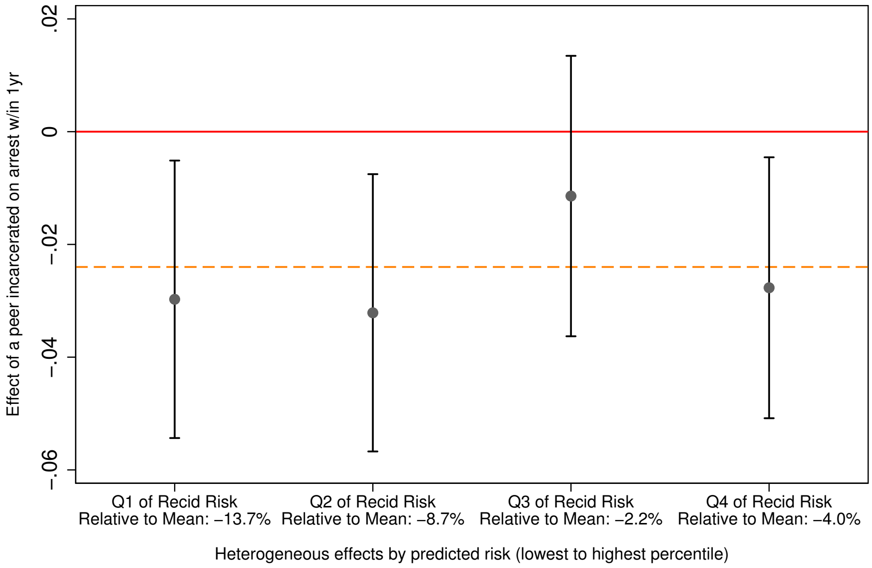
This figure provides the estimated coefficient (and 95% confidence interval) of a one person increase in the number of peers incarcerated during the first year post-release. We vary the definitions of peers based on demographic attributes (age, race, and gender) and distance bands away from the pre-incarceration 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 1KM, 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: Effect of Peers Incarcerated by Predicted Risk of Recidivism



This figure plots the estimated effect of the number of peers (w/in 1k, same age, race, and gender) incarcerated over the first year post-release by quartiles of the predicted risk of recidivism. The figure plots the effects presented in the third panel of Table 8. We estimate the predicted risk of recidivism using all regressors in our primary estimation sample excluding the peer incarceration variables. The effect sizes relative to the mean rearrest probabilities for each quartile are described under each coefficient. 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. 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 = w/in 1K		
Peers Incarcerated	−0.000	0.000
	(0.001)	(0.001)
2. Peers = w/in 1K, Age w/in 5 yr		
Peers Incarcerated	−0.005*	−0.005*
	(0.003)	(0.003)
3. Peers = w/in 1K, Age w/in 3 yr		
Peers Incarcerated	−0.006*	−0.006*
	(0.003)	(0.003)
4. Peers = w/in 1K, Age w/in 1 yr		
Peers Incarcerated	−0.013**	−0.009
	(0.006)	(0.006)
5. Peers = w/in 1K, Age w/in 1 yr, Same Race and Gender		
Peers Incarcerated	−0.024***	−0.019**
	(0.008)	(0.007)
6. Peers = Age w/in 1 yr, Same Race, Gender, and Building		
Peers Incarcerated	−0.062**	−0.047*
	(0.028)	(0.027)
7. Peers = Same Building and Surname		
Peers Incarcerated	−0.109***	−0.120***
	(0.031)	(0.032)
Mean of Dep. Var.	0.445	0.461
Observations	14,696	14,696
8. Peers = Former Criminal Partners		
Peers Incarcerated	−0.123*	−0.060
	(0.073)	(0.069)
Dep. Var (mean)	0.449	0.479
Observations	5,400	5,400

* 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 decompose the total number of *Neighbors Incarcerated* into two groups: those who are likely peers and those who are of different age, race, or gender. 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 1 km (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 panels estimate the influence of peers with increasingly similar characteristics (indicated by the description of each panel). The sixth panel restricts proximity to the same building parcel address. The seventh panel defines peers as individuals within the building who also have the same last name (a proxy for same family). Finally, the eighth panel 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.

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. We present estimates for each lag and lead month leading up to the actual release date in Figure 5. 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 exogeniety 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.3 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.1 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.1. 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.1 and find smaller and less precise estimates.[33] In total,results presented in Table A.1 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.2 first evaluates whether key types of peers drive our results by excluding very influential peers from the regressor of interest in our baseline specification. 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.2. 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

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

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.2, 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 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.2. 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.2

In Table A.4 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 through 6 of Table A.4 report effects for outcomes counting the number of arrests or days incarcerated within one and two years instead of a dichotomous indicator for arrest or incarceration within one year. Results for the one year outcome are less precise, but similar in magnitude—each imply a 3 to 5 percent decrease in recidivism relative to the mean of the outcome variable. Effects for outcomes in a two year follow up period are also not precise but indicate increasing numbers of arrests and days of incarceration for those with more criminal peers present during the first year post-release.

We choose to focus on one-year recidivism outcomes and use a measure that captures the total number of peers incarcerated over this post-release time period as this is a critical time period—the majority of those who recidivate do so within the first year post-release. We explore shorter time windows of one, three, six, and nine months in Table A.5. In the first panel of results, the key regressor is measured relative to the outcome time period (e.g. the regressor in Column 1 measures the number of peers incarcerated over the first month post-release). While not statistically significant, the impact of peers incarcerated is fairly consistent across columns when comparing the estimated magnitude of the effect to the mean of the outcome variable considered. For a one month time window, the estimated effect is not statistically significant, but implies a 3 percent decrease in the probability of arrest within one month. Similarly, using a six month time window for the outcome and to define the regressor of interest yields a 4 percent decrease in recidivism from the mean (Column 3). In the second panel of results, we hold the outcome window fixed at one year and vary the time period used to measure our key regressor of interest. Effects increase as we measure peer presence over a longer time period. However, the presence of peers over the first month of reentry exerts a significant influence on recidivism within the first year. Overall, it seems that conditions at entry matter and our results are not specific to a one-year time window.

Table A.1: Robustness Checks - Distance and Time Peer Definitions

	(1) Baseline Results	(2) Redefine Peers: Inc Full Year	(3) Redefine Peers: Longer Time Window	(4) Redefine Peers: Pre-Inc Distance	(5) 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.2: Robustness Checks - Exclude Same Address and Former Partners from Peer Definitions, Exclude Key Control Variables

	(1) Baseline Results	(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	-0.024^{***}	-0.022^{**}	-0.023^{***}	-0.025	-0.024^{***}	-0.020^{***}
	(0.008)	(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 provides our baseline results for comparison. 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.3: 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.4: 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	Num of Arrests w/in 2yr	Num of Incar Days w/in 2yr
Peers = 1K, Age 1yr, Race, Gender						
Peers Incarcerated	−0.022**	−0.016**	−0.049	−2.594	−0.069	−6.851
	(0.008)	(0.008)	(0.030)	(1.622)	(0.062)	(5.038)
Mean of Dep. Var.	0.416	0.434	0.920	40.957	1.684	105.669
Observations	14,696	14,696	14,696	14,696	14,696	12,745

* 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. Columns 1 and 2 present effects of peers incarcerated on rearrest outcomes over shorter time windows. The time frame for the measurement of our key regressors is based on that of the outcome, but we still allow for fractional contributions to the *Peers Incarcerated* measures as we vary the time window. For example, a peer incarcerated for the first 15 days following the focal offender's release would contribute 0.5 to *Peers Incarcerated First Month*; 0.083 to *Peers Incarcerated First 6 Months*. 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 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. General estimation notes from Table 3 and sample construction notes from Table 1 apply.

Table A.5: Effects by Various Definitions of Outcome Variable

	(1) Arrested w/in 1mo	(2) Arrested w/in 3mo	(3) Arrested w/in 6mo	(4) Arrested w/in 9mo	(5) Arrested w/in 1yr
Peers = 1K, Age 1yr, Race, Gender					
Peers Incarcerated	−0.002	−0.004	−0.011	−0.020**	−0.024***
	(0.003)	(0.006)	(0.007)	(0.008)	(0.008)
Mean of Dep. Var.	0.063	0.173	0.297	0.379	0.442
Observations	14,696	14,696	14,696	14,696	14,696
	Arrested w/in 1yr	Arrested w/in 1yr	Arrested w/in 1yr	Arrested w/in 1yr	Arrested w/in 1yr
	Peers Inc 1mo	Peers Inc 3mo	Peers Inc 6mo	Peers Inc 9mo	Peers Inc 1yr
Peers = 1K, Age 1yr, Race, Gender					
Peers Incarcerated	−0.016***	−0.019***	−0.022***	−0.023***	−0.024***
	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)
Mean of Dep. Var.	0.445	0.445	0.445	0.445	0.445
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 presents estimated effects for rearrest outcomes over various time windows as well as over different time windows for our key *Peers Incarcerated* explanatory variable in the first panel. The second panel of results estimates the effect of peer incarcerated for different time lengths over the first year post-release but keeps the outcome variable fixed at a one year window. Each coefficient represents an estimate from a separate specification. Our regressors still allow for fractional contributions. For example, a peer incarcerated for the first 15 days would contribute 0.5 to the one month peers incarcerated measure in Column 1. The estimates in the fifth column correspond to our baseline estimates. General estimation notes from Table 3 and sample construction notes from Table 1 apply.