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

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Abstract

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

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

JEL classification codes: C31, J10, K42, Z13

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

An emerging literature documents the negative influence of criminally active peers in a variety of settings. Research finds that inmates who are more likely to interact in the same detention facility affect each other's post-release criminal activity (Bayer et al. 2009, Ouss 2011, Drago and Galbiati 2012, Damm and Gorinas 2013, Stevenson 2017).[1] Crime is also affected by peer influences within schools and neighborhoods (Deming 2011, Billings et al. 2014, Case and Katz 1991, Ludwig et al. 2001, Kling et al. 2005, Ludwig and Kling 2007, Kirk 2015, Corno 2017)[2] with residential proximity enhancing the within-school peer effects (Billings et al. 2019). The influence of criminals in a neighborhood can be long-lasting—Damm and Dustmann (2014) find that growing up amongst criminally active neighbors impacts later-life convictions around immigrants in Denmark. At an aggregate level, social interactions within neighborhoods can help explain variation in crime rates across space and time through a social multiplier mechanism (Glaeser et al. 1996). While much of this literature focuses on social interactions which increase criminal propensity, Stuart and Taylor (2017) find that areas with higher levels of social connectedness/cohesion due to historical migration patterns have less crime than otherwise expected.

Despite the evidence documenting the criminal influence of peers and growing concern about the high costs of recidivism, little is known about the effect of pre-incarceration residential social networks

[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.

on successful prisoner re-entry for several reasons. First, social connectivity is difficult to measure and data on social relationships is rare.[3] Moreover, identifying the causal relationship between neighborhood peers and recidivism is complex given the presence of endogenous relationships (Manski 1993, 2000). Changes in the presence of criminals in the neighborhood may affect and be affected by recidivism through endogenous social interactions and contextual effects ("social effects"), but the same factors that underlie changes to the number of criminals in the neighborhood such as police enforcement and employment opportunities also influence recidivism ("correlated effects").

Two contemporaneous working papers do provide some evidence of reductions in criminal offending associated with the incarceration of siblings, criminal partners, or individuals within the same criminal networks. Bhuller et al. (2018) document substantial declines in criminal offending following the incarceration of a sibling or individual in the same criminal network in Norway and Philippe (2017) also finds evidence that the incarceration of a former criminal partner reduces reoffending in France. Our results are consistent with these findings but our focus is on the US context as well as the broader neighborhood peer environment where the role of criminal gangs, criminogenic role models and norms about illegal activity all contribute to criminal recidivism.

Using administrative arrest and incarceration records from Charlotte, North Carolina, we provide novel evidence of the relationship between neighborhood peers and recidivism in a setting not unlike the one faced by hundreds of thousands of offenders who exit jails and prisons each year in the United States.[4] We use pre-incarceration residential information to both obtain a proxy for the neighborhood of re-entry as well as to count the number of criminal peers who are absent from the neighborhood when an individual returns home. We primarily use census block designations to define neighborhoods and also provide results for larger areas (census block groups and census tracts). We rely on the fact that the majority of prisoners return to their pre-incarceration locations due to many

[3] Impressively, Corno (2017) 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] 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).

factors including financial constraints, housing discrimination, and the presence of family and social support networks.[5] We then exploit the variation in social interactions at the time of release which arise from the pre-release flow of neighborhood criminal peers into prison or jail. In our setting, the presence of criminals in the neighborhood is constantly shifting as similar criminals may have different incarceration experiences due to idiosyncratic factors such as random judge/courtroom assignment (Mueller-Smith 2015, Aizer and Doyle Jr 2015), the timing of arraignment (Danziger et al. 2011) and random variation in the probability that a criminal act is cleared by an arrest. Through a series of balance and placebo tests, we show that conditional on neighborhood- and time-fixed effects, variation in the number of peers incarcerated at the time of release is driven by factors plausibly unrelated to unobserved determinants of recidivism.

Overall, we find consistent and robust evidence that a released offender is less likely to reoffend if more of his neighborhood peers are incarcerated at the time of release. These peer effects increase in the degree of connectivity as measured by pre-incarceration residential proximity, past criminal relationships, and demographic (e.g. age, race, gender) similarity. We find that a decrease in the presence of one neighborhood peer with similar demographic characteristics (age within one year, same race, same gender) is associated with a 3.8 percentage point decrease in the probability of arrest during the first three months post-release (approximately a 20% decrease relative to the mean rate of recidivism). A one person increase represents approximately a two-standard deviation change.[6] While this size of change is large and extrapolates past the typical variation in our sample, we focus on a whole-person incremental change since this variation is more intuitive when considering the size of an individual's peer group. The estimated effect of an absent peer is even larger for peers living at the same residential address or linked to the released offender for specific crimes in the past. Across different types of criminals, we find evidence that less serious criminals and

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

[6]For a one-standard deviation change in the presence of neighborhood peers at the time of release, we find a 10% decline in 3 month recidivism rates.

those between 25 and 34 years old are the most heavily influenced by the presence of neighborhood criminal peers. Heterogeneous effects by age are particularly interesting since they suggest that young offenders aged 18 through 24, who have the highest recidivism rates, are less sensitive to the presence of neighborhood peers upon release. This result is consistent with younger individuals having less established neighborhood networks or networks that erode faster with time apart. Strong peer effects among young offenders detained within the same facility could also diminish the role of neighborhood networks for this age group (Bayer et al. 2009, Stevenson 2017).

Our main results can be interpreted broadly as "peer effects", which include a role for direct social interactions, social learning and congestion externalities. While we cannot separately identify the role of each of these mechanisms, our stronger results for more similar peers lend support for a greater contribution from direct interaction and social learning rather than congestion externalities. A congestion externality mechanism may predict lower rates of recidivism with more peers incarcerated since there could be a higher probability of apprehension given a crime—however, our estimated effects are not impacted by the inclusion of controls for police enforcement in the neighborhood at the time of release, suggesting a small role for this particular mechanism in explaining our results. Further, when isolating effects for certain types of neighborhood peers based on shared attributes, we control for all other neighborhood peers incarcerated which will also absorb the influence of any congestion externality channel. We also cannot isolate direct social interactions from a social learning mechanism. An additional peer incarcerated at the time of release decreases the presence of peers in the community but also represents a potential increase in social interaction and learning in the detention facility. However, similar results for peers entering incarceration before and after an individual in our estimation sample along with results suggesting that an increase in criminal peers in the community (peers released just prior) suggest that direct social interaction plays a key role in our results.

Our study follows a literature that documents strong peer effects within juvenile detention facilities (Bayer et al. 2009, Stevenson 2017). While it is hard to compare effect sizes given the different contexts, the general magnitude of our effects are not unusual given those found in these prior studies.

For example, Bayer et al. (2009) document a 10% increase in the probability of a future felony drug offense if a juvenile with a history of drug offending is exposed to a one standard deviation increase in other drug offenders while detained; Stevenson (2017) finds that a one standard deviation increase in the risk score of detention facility peers leads to a 16% increase in the probability of re-incarceration within one year from release. Our primary estimate that implies a 10% decline in the probability of re-arrest for a one-standard deviation increase in neighborhood peers incarcerated is in the same range as these studies and is also in line with recent work documenting the role of school-aged peers on crime as a young adult. Billings et al. (2014) find that a one standard deviation increase in higher-risk peers increases arrests by 8% among minority males, and Billings and Hoekstra (2019) document a 3% increase in adult arrests following a one standard deviation increase in the number of high-risk middle school peers.[7]

Our results contribute to a large literature that evaluates other determinants of recidivism such as post-release employment and advances our understanding of what factors can reduce offending soon after release. It is well known that individuals experience low rates of employment following imprisonment. Since unemployment increases an ex-prisoners social time in the neighborhood, social interactions may be an important mechanism behind a growing number of studies finding a connection between local labor market conditions and recidivism (Agan and Makowsky 2018, Schnepel 2017, Yang 2016, Wang et al. 2010, Raphael and Weiman 2007, Sabol 2007). Our estimates imply a larger decline in recidivism following a one standard deviation decrease in the presence of criminal peers than the decrease in recidivism following a one standard deviation increase in relevant employment opportunities, low-skill or miminum wages, and EITC program generosity.[8] More broadly, Redcross et al. (2012) speculate that differences in the social environment across randomized controlled trials evaluating re-entry employment programs may explain differential

[7] Even though the study area and focus on criminal peer effects is similar to Billings et al. (2019) and Billings and Hoekstra (2019), our sample and focus is different since we focus on adult recidivism among individuals incarcerated while examine the relationship between school peers and criminal activity among juveniles and young adults.

[8] Schnepel (2017) finds a 4% decline in recidivism associated with a one SD increase in construction employment opportunities; Yang (2016) finds a 7% decline in recidivism associated with a one SD increase in low-skill wages; Agan and Makowsky (2018) find a 4% decline in recidivism associated with a one SD increase in the minumum wage and a 5% decline in female recidivism following a one SD increase in EITC.

effects on recidivism outcomes.[9] Furthermore, we add to evidence that conditions at the time of release can have an immediate impact on behavior. Munyo and Rossi (2015) document a reduction in reoffending on the first day of release following a large increase in the amount of cash provided to released prisoners in Uruguay. While we do not focus on behavior on the first day since release, we find a strong influence from initial neighborhood conditions on behavior in the first few months following release.

Our findings speak directly to the role of location for recently released inmates. More often than not, released offenders have no choice but to return to their old neighborhoods due to such issues as insufficient money, housing discrimination, and post-release supervision requirements. Policies that provide assistance to live further away from other criminals (similar to the Moving to Opportunity program) or more strictly enforce interaction between former criminals may lower recidivism. Another policy that may prevent negative social interactions is electronic monitoring. For example, Di Tella and Schargrodsky (2013) document large reductions in recidivism for offenders under electronic monitoring compared with those who are sent to prison. These effects may be a result of preventing the formation of criminogenic relationships within prisons, but they could also be due to preventing the monitored offender from hanging out on the street or in places where criminal peers congregate.

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

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

2. Data Description

Our main sample focuses on adults sentenced in Mecklenburg County, NC who are released from prison or jail between January 1, 2005 through June 30, 2010.[10] We combine administrative records from the Mecklenburg County arrest registry, Mecklenburg County Jail intake and release, and data from the North Carolina Department of Public Safety on state prisoners. All data is matched using first and last name as well as date of birth. Given the similar administrative nature of these datasets, the match rate across these datasets is high with over 95% of jail or prison records linked to an arrest record. We also link the registry of offenders to detailed records of criminal incidents that allow us to identify individuals who commit crimes together, therefore linking individuals with their criminal partners.

The arrest registry data provides individual names, demographic information, details on the nature of the arrest charges, the time and date of arrest, and information on the location of residence at the time of arrest. The offenders' residential address is typically ascertained from personal identification or secondary verification from law enforcement at the time of arrest and is provided in full address form from which we geocode the pre-incarceration residential locations of released offenders. One limitation of our analysis is that we cannot include individuals who 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 inmates meeting our sample criteria, we are able to match approximately 80% to a pre-incarceration residential address using arrest records.[11]

Our primary estimation sample includes individuals at least 18 years of age incarcerated for at least one full calendar month between January 2005 through June 2010. Overall, our primary estimation sample includes 17,361 re-entry observations among nearly 13,000 unique individuals. Figure 1

[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 after June 2010. We are able to examine post June 2010 rearrests since we do not need location-of-residence information.

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

details the distribution of the time spent incarcerated for this sample.[12]

When an individual is arrested they are booked and then depending on the nature of the offense, they may be released within a few hours and thus never enter the jail/prison system or held for longer periods in a county jail facility or transferred into a state prison. Our data captures all stints of incarceration in jail or prison and treats almost all transfers from county jail to state prison as a single incarceration lasting the entire span of jail and prison. For more serious crimes, arrestees are often arraigned in court and released upon payment of bond or serve time until a scheduled court appearance. Since our data only contains incarceration information and not court information, we treat pre-trial incarceration time spans the same as post sentencing incarceration spells. For our purposes, this difference should not matter for exposure to peers but does capture some qualitative differences in the type of offender and thus interpretation of our estimates. This process creates substantial variation in the relationship between the time of criminal activity and the time an individual is released from incarceration.[13]

Table 1 indicates that nearly one-quarter of the individuals released from prison or jail are arrested within three months of release. We use the probability of arrest within 3 months as our primary measure of recidivism. The majority of our sample is black (72%) and male (90%). The average offender in our estimation sample is 33 years old, has been incarcerated for 6 months, and has nearly 7 prior arrests at the time of release.

We use the pre-incarceration residential location of offenders as a proxy for their location post-release. To check whether this is a valid assumption, we plot the distance between the pre- and post-incarceration residential location for the subsample of offenders who are rearrested within one year of release and who have a residential location recorded for their post-release arrest.[14] As shown in Figure 2, the majority of released offenders provide a residential address within one kilometer during the post-release arrest as the pre-release arrest. Approximately 75% of those rearrested

[12] We see a large number of offenders with relatively short incarceration spells—just over 75% serving six months or less and nearly 90% serving a year or less.

[13] We document this variation in Appendix Figure A.5 where we demonstrate substantial variation in the time between arrest and incarceration exit for our estimation sample.

[14] Approximately 35% of the released offenders in our estimation sample meet this criteria.

provide an address within five kilometers of the pre-incarceration location. While we can only observe this lack of mobility for those who recidivate, it is reassuring for our estimates that the majority of offenders return to the same neighborhood.

Our primary specification defines neighbourhood as a census block since the spatial scale of criminal relationships is often quite small based on the existing literature. Using the same study area, Billings et al. (2019) find a sharp drop off in number of criminal partners for distances greater than 1km. Since our sample of released offenders may be quite different, we plot a histogram measuring the distance between the residential address of two offenders linked to the same reported crime (partners) in Appendix Figure A.1. This figure highlights that the spatial scale of criminal peer networks is quite concentrated within a small area for our estimation sample.

2.1. Defining Criminal Peers

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

We are able to calculate multiple measures of neighborhood criminal peers using pre-incarceration residential addresses and detailed demographic information about offenders in Charlotte-Mecklenburg County. To construct a variable that measures the presence of criminal peers in the neighborhood at the time of release, we count the number of individuals who are incarcerated during the full calendar month in which an individual in our estimation sample is released and are tied to the

[15]This measure also has the benefit of providing estimates of the influence of a portion of the population known to criminal justice authorities and who can be influenced by policies in the criminal justice system. We also report results from a specification where our key measure of peers incarcerated is the ratio of neighborhood peers incarcerated at the time of release to the total number of criminals tied to that neighborhood that have ever been incarcerated in Table 6.

same pre-incarceration neighborhood (our primary specification defines neighborhood as a census block).[16] To avoid any simultaneity bias, peers are only included in these measures if they are already incarcerated when an individual is released from jail or prison.[17] To focus on potential peers who are criminally active in the neighborhood around the same time, we also require that the peer was incarcerated no earlier than two years prior to the focal offender's incarceration entry date.[18] We refer to this group as *neighborhood peers incarcerated* at the time of release. We then decompose the total number of neighborhood peers incarcerated into those who share various demographic and criminal history characteristics with the released offender of interest.[19]

To help illustrate the construction of our key independent variables, suppose Offender A spends six months in jail entering on January 15, 2008 and released on June 15, 2008. Our primary regressor of interest counts the number of individuals tied to the same pre-incarceration neighborhood as Offender A who are incarcerated during the entire month of June 2008. This includes individuals who entered incarceration prior to Offender A's entry in January as well as those who enter incarceration during Offender A's sentence before May 31, 2008. Our measure of neighborhood peers incarcerated captures initial re-entry conditions and can include individuals released any time after July 1, 2008. While we are most interested in the average impact of the number of neighborhood criminal peers not present at the time of release, we investigate many alternative definitions including splitting peers by whether they enter incarceration before or after Offender A as well as restricting our measure only to peers incarcerated for the entirity of our outcome time windows (e.g. peers who are incarcerated for the entire 3 month period of July, August and September following release of Offender A in June).

[16] We use calendar month as the primary unit of time to simplify the creation of a monthy panel dataset tracking incarceration and reoffending for our estimation sample. The effect of peers incarcerated for the entire calendar month of release may vary by whether an individual is released towards the beginning or end of the month which we investigate in Table 6.

[17] Without this restriction, fluctuations in the presence of neighborhood peers are influenced by changing neighborhood conditions which also affect the recidivism of the released individual. Moreover, the behavior of the released individual also can directly affect the behavior of individuals in the neighborhood.

[18] For example, if an individual in our sample was incarcerated in March 2005 and released in January 2006, we would allow individuals to contribute to our neighbourhood peers incarcerated measure if they (a) were still incarcerated during January 2006, and (b) entered incarceration between March 2003 and December 2006.

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

Table 1 presents summary statistics for our measures of neighborhood peers incarcerated at the time of release from prison for our estimation sample. On average, each individual has 1.6 neighbors incarcerated when released and 0.1 peers incarcerated of a similar age, race and gender (our key regressor).[20] Appendix Figure A.2 plots raw survival rates for released individuals with no peers (of similar age, race and gender) versus those with at least one neighborhood peer incarcerated at the time of release. On average, we see evidence of lower rates of recidivism for those released with their peers incarcerated. However, this figure is descriptive and does not control for offender characteristics or for differences in recidivism risk across neighborhoods.

3. Empirical Methodology

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

$$\texttt{Recid}_{ijt} = \beta_0 + \beta_1 \texttt{Nbhd Peers Incarcerated}_{ijt} + \mathbf{X_i'}\alpha + \gamma_j + \delta_t + \epsilon_{ijt} \tag{1}$$

where $\texttt{Recid}_{ijt}$ is an indicator variable equal to one if individual i, living prior to incarceration in neighborhood j, and released at time period t, is arrested within three months of release from incarceration. We present results using various definitions of recidivism including longer time horizons (6 months, one year), the number of future arrests, as well as the number of days incarcerated. As described in Section 2.1, our key variable of interest, $\texttt{Nbhd Peers Incarcerated}_{ijt}$, measures the number of i's neighborhood peers incarcerated (in jail or prison) during the month of release where neighborhoods are primarily defined as residing in the same census block based on the pre-incarceration residential locations of inmates. We decompose the total number of neighbors

[20] At the census block level (our primary unit for neighborhoods), 40 percent (1427 out of 3575) of our census blocks have variation in the total number of incarcerated peers in the census block and 10 percent (370) have variation in the total number of incarcerated peers of a similar age, race and gender.

incarcerated into groups of increasingly similar peer groups. Our preferred model focuses on a measure of peers that includes those within one year of age and peers of the same race and gender. For all models that include more narrowly defined peers, we also include a variable measuring all of the other neighbors incarcerated who do not meet the specified classification of peers based on attribute similarity. Individual demographic and prior criminal histories are included as part of vector $\mathbf{X_i}$.[21] To account for unobserved neighborhood determinants of criminal activity and any shocks common to a particular time period, we also include fixed effects for neighborhood (γ_j) and year-by-month-of-release fixed effects (δ_t). We report standard errors that are cluster-robust at the census-tract level and within individuals (two-way clustering) for all specifications.[22]

3.1. Identification Concerns

To enable a causal interpretation of our estimates for β_1 in Equation 1, we need the variation in $\texttt{Peers Incarcerated}_{ijt}$ to be "as good as random" conditional on the individual control variables, time and neighborhood fixed effects included. Our estimates would be biased if there exist unobserved determinants of post-release reoffending that are correlated with our measures of social influence. We assess the potential influence of such 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 for incarceration. To limit this type of sorting, we use pre-incarceration addresses and thus our estimates capture an intent-to-treat (ITT) effect of neighborhood criminal peer concentration on recidivism.[23] Any differential post-release sorting will likely attenuate our estimated effects, but it is plausible that pre-incarceration neighborhood networks persist even for

[21] We include indicators for gender, race, age at incarceration exit, type of offense associated with the incarceration spell, the number of prior arrests, and prior incarceration spells.

[22] We primarily define neighborhoods by census blocks for which there are 3,575 in our sample. However, we cluster at the census tract level to allow for correlation across neighborhoods in similar areas. There are 231 census tracts in our sample. We also allow for correlation within individuals over time for those with multiple reentry episodes.

[23] Since not all individuals will return to the same neighborhood, our estimate could underestimate the impact of peers. However, we hesitate to recommend any rescaling of our estimates since we do not observe whether individuals in our estimation sample return to the same neighborhood.

individuals who relocate following incarceration. Importantly, we do not find that our measure of peers incarcerated is correlated with the post-incarceration residential location for the selected subsample of individuals who are rearrested and who report a residential address at the time of rearrest (see results in Appendix Table A.4). However, pre-incarceration sorting could also influence our estimated effects if individuals more prone to recidivism sort into certain types of neighborhoods. To account for neighborhood-level determinants of recidivism, all of our specifications include location fixed effects, which limits any systematic bias from certain neighborhoods. 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.

To support the validity of our identification assumptions we present a balancing test in Table 2 which investigates whether our key regressors of interest are correlated with a predicted reoffending risk measure based on all observable characteristics. For each individual in our estimation sample, we obtain the predicted probability of reoffending using all of the control variables in the vector $\mathbf{X_i}$ from equation 1. We then report the estimated correlation between $\texttt{Nbhd Peers Incarcerated}_{ijt}$ and this predicted reoffending risk conditional on the neighborhood and time fixed effects. Across nearly all of our attribute-specific measures of $\texttt{Nbhd Peers Incarcerated}_{ijt}$ and specifications varying the window upon which we measure reoffending , we cannot reject the null hypothesis that our measure of peers incarcerated is uncorrelated with the predicted risk of recidivism. 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 the reoffending risk based on observable characteristics is not correlated with the presence of criminal peers at the time of release. We do find a marginally significant relationship between peers incarcerated and the predicted risk of recidivism for two of our specifications which is likely spurious since we conduct 18 seperate hypothesis tests and expect a few to randomly be statistically different from zero. We also provide an alternative balance test in Table A.1 where we evaluate the correlation between demographic and criminal history observable characteristics and our key peers incarcerated regressor of interest and find further support that our key regressor is not correlated with key determinants of recidivism,

such as prior criminal histories.[24]

Another potential identification concern arises from the fact that individuals in our estimation sample could impact our measure of peers incarcerated at the time they themselves go to jail or prison. Specifically, the incarceration of an individual in our sample could affect the number of neighborhood peers going to jail through such a mechanism as the offender cooperating with the police to facilitate arrests of known associates. On the other hand, the removal of a criminal from the neighborhood can reduce the probability of incarceration among peers through a social interaction effect.[25] To mitigate this concern, we present a series of estimates in Section 4 that highlight similar effects even when we limit our measure of peers incarcerated to only individuals who entered prison before or after the month an individual in our estimation sample enters prison.[26]

To further support the exogeneity of our peers incarcerated measure, we evaluate whether there is any systematic relationship between the timing of release and the timing of a peer's incarceration. For each individual in our estimation sample, we calculate the distance (in weeks) between release and when a neighborhood peer was incarcerated and plot the histogram of this distance in Appendix Figure A.4. We do not find evidence of a systematic relationship between the timing of a peer incarceration and an individual's release which helps alleviate concerns about underlying neighborhood conditions driving our results. For example, if our key regressor was driven by peers who are incarcerated just prior to an individual's release, then we would be concerned that the underlying neighborhood conditions may be influencing our regressor and our reoffending outcome.[27]

[24] In Table A.1, we only find a significant correlation between fixed observable characteristics race and gender and our regressor of interest. This is expected given disproportionately higher incarceration rates for males and minority individuals. A potential identification concern arises if unobservable determinants of recidivism are correlated with these demographic characteristics. However, these concerns are mitigated by similar results from models holding all fixed unobservables within location-race-gender groups through including fixed effects for census block interacted with race and gender which has been added as part of our robustness checks reported in Table 4

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

[26] We also demonstrate that the severity of the pre-incarceration crime is not systematically related to the number of peers incarcerated at the time of release in Appendix Table A.5 which would be expected if less serious criminals are released when their more severe criminal partners remain incarcerated.

[27] Appendix Figure A.4 exhibits a downward sloping patter after 4 weeks since, to be included, a neighborhood peer

Finally, we are also concerned that criminal enforcement could affect our estimates. Law enforcement is an important determinant of arrest and incarceration rates and may be influenced by the concentration of criminals within a neighborhood. We control for general crime and policing patterns by including detailed place- and time-specific fixed effects. To assess whether neighborhood and temporal variation in crime and enforcement influence our results, we include measures of the pre- and then post-release crime rates and arrest clearance ratio for each area. We discuss these results in Section 4.[28] One last issue is that our control variables for general policing based on reported crimes and clearance rates may not capture the targeted patrolling of recently released criminals. The fact that we find the strongest peer effects for less serious criminals (i.e. those incarcerated for less than 6 months) who presumably are less likely to receive additional attention by police officers helps limit concerns about targeted enforcement.

4. Results

4.1. Peers and Recidivism

Table 3 presents our estimates of Equation 1 for our sample of released offenders across three separate windows used to measure recidivism. We start by estimating the influence of the total number of peers incarcerated within our census block 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.

We do not find that an increase in the total number of neighborhood criminal peers who are locked up at the time of release reduces the probability of recidivism. However, we do find large and

must be incarcerated when i is released. This pattern starts after 4 weeks because the time unit used in our main analysis is at the monthly level. So for an individual released at the end of the month in our estimation sample, we only count peers who are incarcerated for that entire month and we would not have observations for those entering one week prior release for our estimation sample.

[28] We also estimate a model with neighborhood clearance rate as the dependent variable and do not find a significant correlation with peers incarcerated in our baseline model. These results are presented in Appendix Table A.5

significant effects for more similar peers that are increasing in the degree of attribute similarity.[29] This pattern of increasing influence based on the similarity of attributes also holds for residential proximity. Figure 3 shows that larger definitions of neighborhoods (census block groups or census tracts) generate weaker peer effects than estimates using neighborhoods defined by census block.

To focus our analysis, we primarily discuss results from our "baseline" model, in which we use an indicator for rearrest within 3 months from release as the outcome variable and define neighborhood peers as having a pre-incarceration residence within the same census block, being within one year of age, and possessing the same race/ethnicity and gender. Results for our preferred specification are presented in the fifth row of Table 3.[30] We find that one additional peer incarcerated at the time of release decreases the probability of arrest (incarceration) within 3 months by 3.8 percentage points—over a 17% decline relative to the mean recidivism rate. These results are consistent in models using alternative outcomes and for alternative measures of peers incarcerated.[31] Estimates of the impact of an additional neighborhood peer incarcerated even more closely linked by the same residential address or having co-offended with the released individual in the past are even stronger, but decline in precision.[32]

To evaluate the plausibility of our identification assumptions, we estimate effects in placebo specifications where we vary our primary measure of neighborhood peers incarcerated by month for a year prior and a year post the actual release date. Any unobserved factors driving our estimates should also be strongly correlated with the lag and lead pseudo-release dates. Each coefficient presented in part (a) of Figure 4 is from a separate specification that is identical to Equation 1 with the following exception: instead of measuring $\texttt{Nbhd Peers Incarcerated}_{ijt}$ at the time of

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

[30] We report results for these preferred specifications that display coefficients for observable covariates in Appendix Table A.2.

[31] Our results are robust to redefining peers incarcerated over the three months post-release to only those incarcerated the entire period instead of only measuring the number incarcerated for the month-of-release as presented in Table 6.

[32] Only a small fraction of our estimation sample are released during a month with a former partner incarcerated, but the magnitude suggests effects similar and perhaps slightly larger to those for neighborhood peers similar in demographic characteristics.

release from prison t, we use a measure of neighborhood peers incarcerated in another month, $\text{Nbhd Peers Incarcerated}_{ij,t-m}$, where m varies from -12 to $+12$.[33] For example, the estimated coefficient at $t-6mo$ estimates the impact of peers incarcerated 6 months prior to i's actual release date on the probability of i recidivating within 3 months. We find similar effects in the immediate surrounding months but this is due to a correlation between the neighborhood peers incarcerated during the actual month of release and those in the nearby months. This is demonstrated by estimates in panel (b) of Figure 4. Results presented in panel (b) replicate the specifications described for panel (a) but add an additional explanatory variable, $\text{Nbhd Peers Incarcerated}_{ijt}$.[34] In other words, these specifications now control for the relevant peers incarcerated measure at the time of release and evaluate the relationship between peers incarcerated during the placebo months and recidivism outcomes. Here, the estimated coefficient at $t-6mo$ measures the impact of peers incarcerated 6 months prior to i's actual release date on the probability of i recidivating within 3 months while holding constant the number of peers incarcerated at i's actual release date. Here, we find no systematic effect of neighborhood peers incarcerated in months surrounding the timing of release.

Since we identify social interactions from fluctuations in the number of individuals incarcerated, we are aware that variation in our primary regressor of interest could be influenced by a persistent (or cyclical) pattern of neighborhood incarceration rates, crime waves, and 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 4.

The specifications presented in specifications (1) and (2) presented in the first row of Table 4 control for measures of neighborhood crime and enforcement (the fraction of crimes solved) just before and after release from jail or prison. These controls do not influence our results, providing assurance

[33] Panel (a) of Figure 4 plots $\widehat{\beta}_1^m$ from the 25 regressions of the equation $\text{Recid}_{ijt} = \beta_0 + \beta_1^m \text{Nbhd Peers Incarcerated}_{ij,t-m} + \mathbf{X_i'}\alpha + \gamma_j + \delta_t + \epsilon_{ijt}$, where we vary m from -12 to 12.

[34] Panel (b) of Figure 4 plots $\widehat{\beta}_1^m$ from the 25 regressions of the equation $\text{Recid}_{ijt} = \beta_0 + \beta_1^m \text{Nbhd Peers Incarcerated}_{ij,t-m} + \beta_2 \text{Nbhd Peers Incarcerated}_{ijt} + \mathbf{X_i'}\alpha + \gamma_j + \delta_t + \epsilon_{ijt}$, where we vary m from -12 to 12. When $m = 0$ we report the coefficient for the variable measuring peers incarcerated in the month of release, $\widehat{\beta}_2$ as $\text{Nbhd Peers Incarcerated}_{ij,t-0} = \text{Nbhd Peers Incarcerated}_{ijt}$.

that our baseline estimates are not biased by any neighborhood-specific crime waves or changes in police enforcement. We also find similar effects if we limit our sample to only one observation per individual by choosing the first release that we observe in specification (3). 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 neighborhood-specific linear and quadratic time trends in specifications (4) and (5) of Table 4. The second row of Table 4 reports a similar point estimate for specifications including year-by-census block group fixed effects (6); controlling flexibly for timing of incarceration entry with year-by-month of entry fixed effects (7); identifying off variation within groups defined by neighborhood (census block), race and gender categories (8); including individual fixed effects defined by unique combinations of name and date-of-birth (9); and, marginal effects from a logit specification rather than our primary linear probability model specification (10).

So far, we have primarily focused on three-month follow-up periods in measuring outcomes and estimate the impact of peers incarcerated at the time of an inmate's release, but results across different post-release time periods may help illuminate the mechanisms driving our results. The presence of criminal peers appears to be a stronger determinant of recidivism during the first few months after release and becomes less important for longer time horizons as reported in the second and third columns of Table 3. We find that one additional neighbor incarcerated at the time of release is associated with a 2.6 percentage point decline in the probability of arrest within one year post-release, a 6% decline from the rate of rearrest of 42%.[35] These results suggest that peers at the time of release matter most for behavior around the time of release and that other factors may be more important determinants of recidivism in the longer term.

Table 5 reports our primary estimate across alternative definitions of the dependent variable. Peers incarcerated at the time of release significantly affects the types of recidivism which we expect to be most influenced by social influences (property, drug, and technical violations). We find no effect of the number of neighborhood peers incarcerated on violent recidivism among our estimation sample.

[35]This pattern is reflected in Appendix Figure A.3 where we split time into 3 month intervals and evaluate the impact of peers incarcerated at the time of release for each quarter interval from one year prior to release to 2 years after release. We find the effect is strongest during the quarter of release and gradually diminishes over the first year.

We also find similar effects if we redefine our binary recidivism outcome variable to equal either the number of arrests or the number of days incarcerated in the second row of Table 5.

Finally, we test whether our results are driven by the way we measure our key regressor of interest (peers incarcerated during the month of release) in Table 6. In all prior results discussed, we focus on the effect of the raw number of individuals incarcerated at the time of release without adjusting for the total number of potential criminal peers in the neighborhood. While we cannot observe all potential peers, we can calculate the total number of neighborhood peers ever incarcerated prior to an individual's release. This provides a proxy for the pool of potential criminal peers in the neighborhood.

In the first column of Table 6, we redefine our key regressor of interest as the ratio of the peers incarcerated at the time of release to the total potential peers (those previously tied to the neighborhood and incarcerated). Here, a one unit increase can be interpreted as the effect of being released when the entire pool of potential criminal peers are incarcerated at the time of release. We find effects that are approximately 20% higher for this variation compared to measuring peers incarcerated with a raw count.

Since our measure of peers incarcerated at the time of release is defined by those incarcerated for the entire calendar month, the effect of peers incarcerated may vary by whether an individual is released towards the beginning or end of the month. As an extreme example, our measure of peers incarcerated for someone released on the first of the month captures individuals who are absent for atleast the entire first month post-release; conversely our measure captures individuals absent for atleast a one day for an individual released on the last day of the month. We investigate whether the timing of release influences our results in the second and third columns of Table 6 by splitting our sample into those who were released in the first half of the month (column 2) and those released in the second half (column 3). While we loose statistical precision due to the smaller sample, we find slightly larger effects for those released in the first half of the month for whom the independent variable measures peers incarcerated for several weeks following release. This result is not surprising

given a larger dose of peer absence for those released earlier in the month. However, these estimated effects are not statistically distinguished and the magnitudes suggest similar effects across both those released early or late in a month.

We find similar effects when we measure the influence of peers incarcerated for the entire reoffending window in specification (4) of Table 6. This result further demonstrates that the number of peers released during the reoffending window (the difference between the number incarcerated at the time of release and at three months) does not exert substantial downward pressure on our baseline estimates. Finally, we assess whether our results are robust to only counting peers who were arrested within one year of the focal individual's arrest and find similar effects in specification (5) of Table 6.

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

4.2. Heterogeneous Effects

We begin by evaluating whether results vary across specific criminal types for both the released offenders as well as for peers. In Tables 7, we summarize heterogeneous effects across a range of demographic characteristics and criminal histories.

These models estimate effects for the full sample and interact indicators for the various groups of interest with the regressor for neighborhood peers incarcerated. Analyzing effects by age at the time of exit in the first row of Table 7, we find that released offenders between the ages of 25 through 34 are the most strongly influenced by neighborhood criminal peer concentration. For this group, one additional neighborhood peer incarcerated at the time of release is associated with a 30% decline in the probability of rearrest within three months.[36] Surprisingly, incarcerated peers do not appear

[36]The standard deviation of the key peers incarcerated regressor is lower within the younger age groups (0.32 for 18-24; 0.34 for 25-34; 0.49 for 35-44; and 0.81 for 45 plus) For the 18-24 and 25-34 age groups, a one person increase in peers incarcerated of similar age, race and gender is a change of approximately three standard deviations.

to influence recidivism among young offenders (between 18 and 25) which suggests other factors influence reoffending for this group with the highest rate of recidivism. While some estimates are not statistically significant, the magnitudes indicate that peers incarcerated are likely important across different demographic groups defined by gender, race and ethnicity. Estimated effects for released offenders who are black as well as those who are male are similar to our full sample results, which is not surprising given the significant representation of these groups in our estimation sample.

Results in the final two rows of Table 7 suggest that the presence of criminal peers has a greater effect on those incarcerated for shorter periods of time in the county jail. The influence of neighborhood peers may diminish as offenders spend more time incarcerated potentially becoming less connected to neighbors and more connected to fellow inmates.

The final row (specifications 16 through 20) split our estimation sample by the types of crimes for which they were initially incarcerated. As evident from the pattern of results, we find that criminal peers have the strongest influence on property and drug offenders. This is consistent with the nature of these types of crimes involving more direct social interactions than other crime types. In fact, Billings et al. (2019) document that drug crimes contain the largest share of arrests linked to group crimes.

Table 8 estimates effects by different types of peers incarcerated for our primary measure of peers (residing in the same census block neighborhood prior to incarceration, of similar age, race and gender). First, we evaluate whether peers exert a differential influence based on whether they entered incarceration before or after a released individual. We find no differential effect which suggests the primary mechanism driving our results is a change in social interactions at the time of release and not a social learning mechanism since those peers from the neighborhood incarcerated just prior to release may inform inmates of the neighborhood conditions. Other specifications in Table 8 define peers incarcerated by the length of incarceration among the neighborhood peers or the type of crime prior to incarceration. Here, we find slightly larger effects among peers who are likely more serious criminals (incarcerated for more than 6 months) and for those involved in property or drug offending.

Finally, specifications (10) through (12) in the fourth row of Table 8 estimate the impact of peers who are incarcerated for similar types of crime as the focal individual in our estimation sample. For those incarcerated for property crimes, we find a strong impact of a decrease in the presence of property offending peers on reoffending rates.

While the focus of our analysis is on a variable that measures the absence of criminal peers in the neighborhood at the time of release, we are also able to evaluate the effect of increases in the number of criminal peers by estimating the impact of plausibly exogenous flows of individuals back into the neighborhood on recidivism for individuals in our estimation sample. We create a new variable measuring the number neighborhood peers released just prior to release for each individual in our estimation sample. Overall, we find that an increase in the presence of neighborhood peers at the time of release from neighborhood peers released just prior (within three months) to each individual in our estimation sample increases the probability of arrest. For the specification in the first panel of Appendix Table A.3, one additional neighborhood peer is associated with a 2.1 percentage point increase in recidivism within three months. While we reject a test as to whether this effect is symmetric to an additional peer incarcerated, in the second panel of Appendix Table A.3, we split the measure of neighborhood peers released into those released one month prior and those released two or three months prior. For those released in the month prior to the release of our focal individual, we find a 2.9 percentage point increase in recidivism and cannot reject a null hypothesis that effects of peers released and peers incarcerated are symmetric.

We also evaluate whether there are heterogeneous effects by different levels of peers incarcerated and different types of neighborhoods in the Appendix. Overall, we do not find evidence that effects are non-linear. In other words, the effect of two peers incarcerated at the time of release is approximately twice the effect of only one peer incarcerated (see Appendix Table A.6). We also do not find evidence that effects are heterogeneous across different types of neighborhoods split by crime, recidivism or incarceration levels (see Appendix Table A.7).

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. All of our results together suggest a strong role for endogenous social interaction effects. Support for the importance of social interaction is demonstrated by the fact that our largest effects are found among peers defined as same residence or family and former criminal partners. The strong peer influence among property and drug offenders—crime types that involve more partnerships—further supports the important role of social effects in determining recidivism.

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

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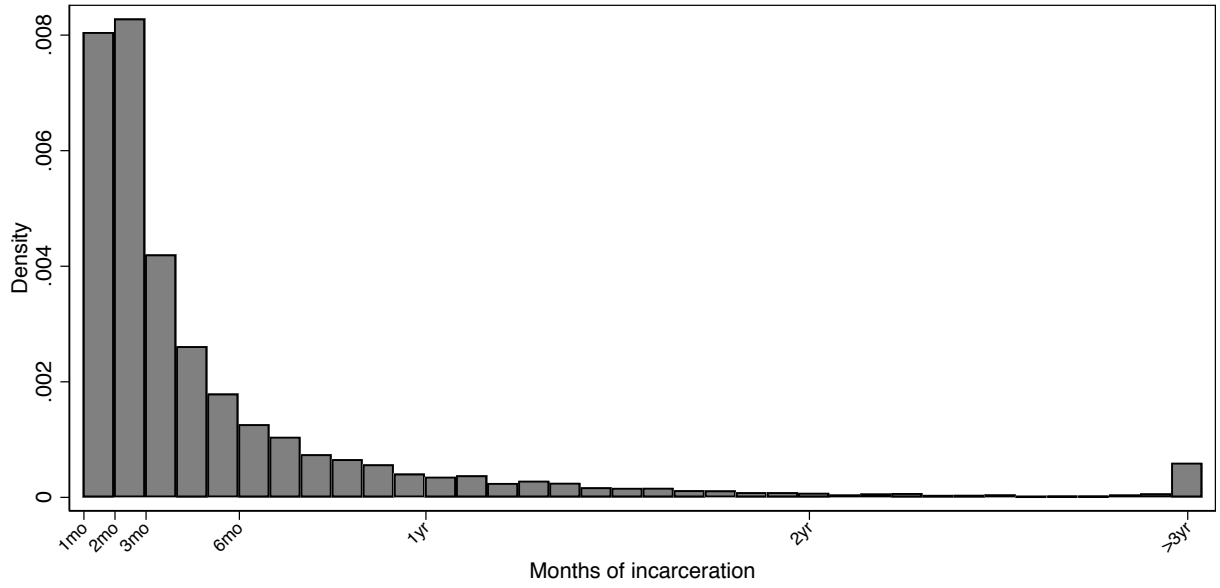
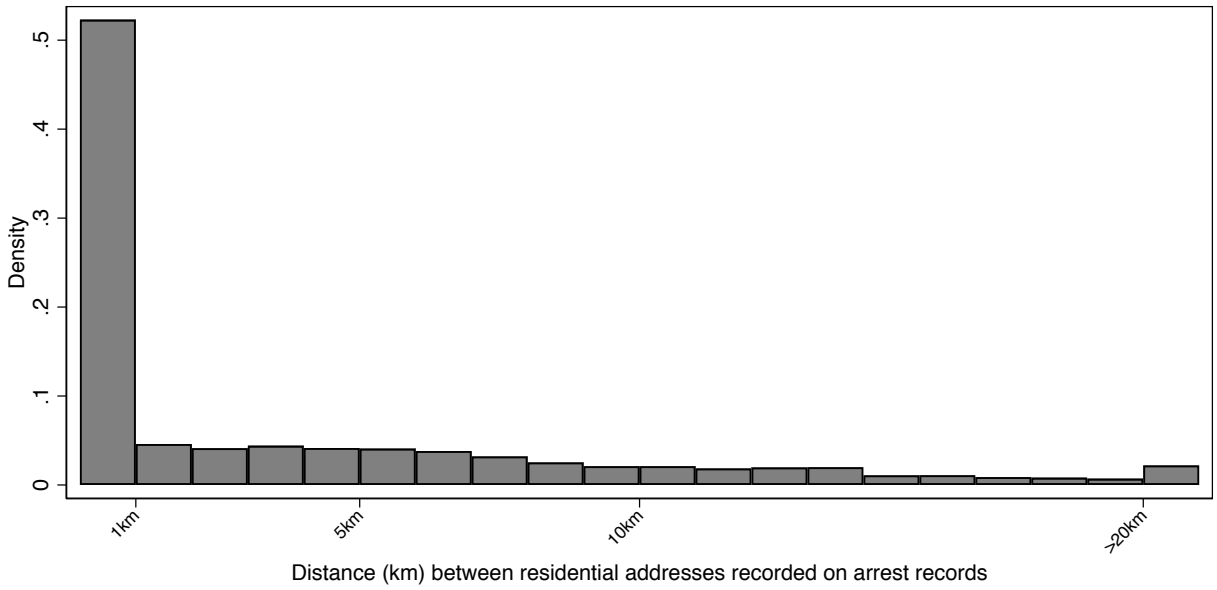
Figure 1: Days incarcerated histogram



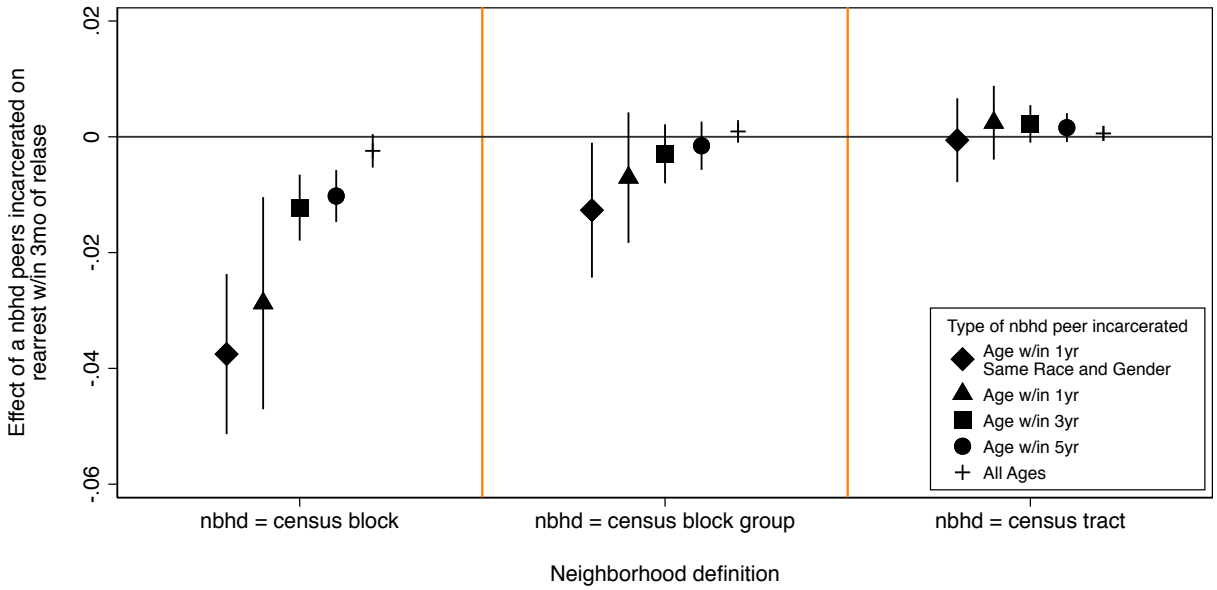
This figure plots the distribution of days incarcerated in jail or prison for our sample. Our estimation sample only includes those incarcerated for at least one full calendar month. General sample construction notes from Table 1 apply.

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 a residential address recorded for the pre- and post-incarceration arrest within one kilometer of their pre-incarceration residential address. General sample construction notes from Table 1 apply.

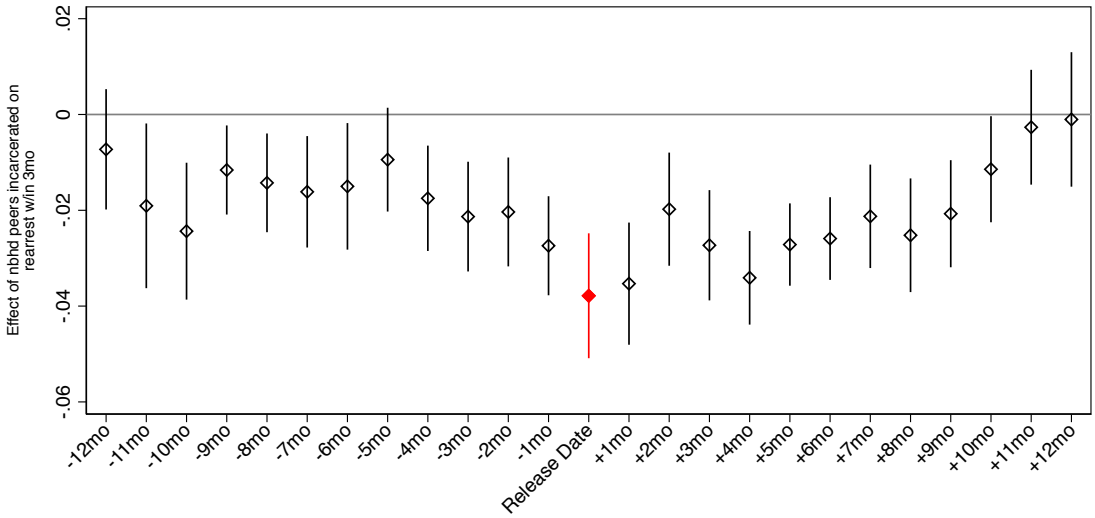
Figure 3: Estimated effects of neighborhood peers incarcerated by distance bands and attribute similarity



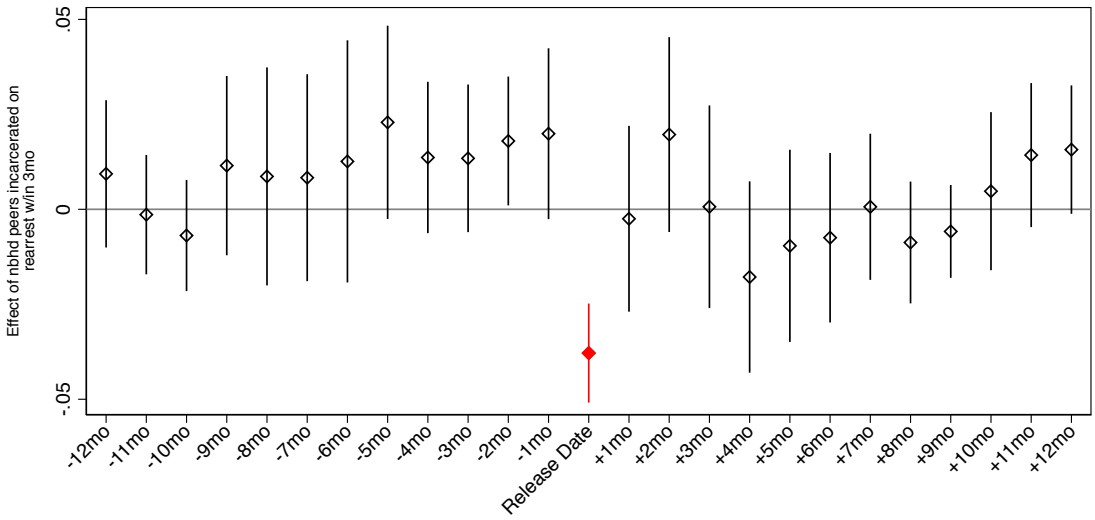
This figure provides the estimated coefficient (and 95% confidence interval) of a one person increase in the number of peers incarcerated at the time of release. We vary the definitions of peers based on demographic attributes (age, race, and gender) and distance bands away from the pre-incarceration 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 4: Estimated effects of neighborhood peers incarcerated (age w/in 1 yr, same race & gender) using placebo release dates

(a) Only include neighborhood peers incarcerated during placebo release month



(b) Include neighborhood peers incarcerated for actual and placebo release months



These figures provide the estimated coefficients (and 95% confidence interval) of a one person increase in the number of peers incarcerated during the month of release where peers are defined as individuals with residential addresses within the same census block, age within one year, and of the same race and gender. Each point represents the estimated effects of peers incarcerated on rearrest within three months 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 with a solid (rather than hollow) diamond at t=0 represents the estimated effect using the correct date to define peers incarcerated. Panel A only includes the placebo peers incarcerated variables; Panel B controls for peers incarcerated at the actual release month. 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				
Rearrest w/in 3mo	0.220	(0.414)	0	1
Rearrest w/in 6mo	0.324	(0.468)	0	1
Rearrest w/in 12mo	0.420	(0.494)	0	1
Nbhd peers incarcerated, nbhd=census block				
Nbhd peers incarcerated	1.687	(3.936)	0	38
Nbhd peers incarcerated: age w/in 5yr	0.559	(1.586)	0	23
Nbhd peers incarcerated: age w/in 3yr	0.367	(1.103)	0	19
Nbhd peers incarcerated: age w/in 1yr	0.161	(0.589)	0	11
Nbhd peers incarcerated: age w/in 1yr, same race & gender	0.107	(0.480)	0	9
Nbhd peers incarcerated: age w/in 1yr, same race & gender & building	0.051	(0.394)	0	9
Nbhd peers incarcerated: former partners	0.004	(0.070)	0	3
Nbhd peers incarcerated, larger nbhd areas				
Nbhd peers incarcerated: nbhd = census tract	16.647	(11.904)	0	63
Nbhd peers incarcerated: nbhd = census block group	7.546	(6.352)	0	40
Demographic characteristics				
Black	0.722	(0.448)	0	1
Hispanic	0.123	(0.328)	0	1
Female	0.098	(0.297)	0	1
Age at release	32.968	(10.274)	18	80
Incarceration characteristics				
Total months incarcerated (county jail + state prison)	6.106	(9.705)	2	131
Fraction with any time in state prison	0.116	(0.320)	0	1
Fraction with post release supervision	0.017	(0.129)	0	1
Prior criminal history				
Total prior arrests (since 1998)	6.969	(7.230)	1	114
Total prior incarceration spells	0.918	(1.355)	0	14
Observations	17,361			

This table presents summary statistics for our dependent variables (recidivism outcomes), various measures of the number of neighborhood peers incarcerated, and other background characteristics. Our estimation sample includes released adult offenders who were sentenced in Charlotte-Mecklenberg County, served at least 1 full calendar month in jail or prison, and released from a Charlotte-Mecklenberg County Jail or a NC State Prison between January 1, 2005 and June 30, 2010.

Table 2: Balance Test

	(1) Predicted Rearrest w/in 3mo	(2) Predicted Rearrest w/in 6mo	(3) Predicted Rearrest w/in 12mo
Nbhd peers incarcerated	−0.0003	−0.0002	−0.0003
	(0.0014)	(0.0015)	(0.0013)
Nbhd peers incarcerated: age w/in 5yr	−0.0016	−0.0009	−0.0003
	(0.0020)	(0.0019)	(0.0016)
Nbhd peers incarcerated: age w/in 3yr	−0.0028*	−0.0016	−0.0008
	(0.0016)	(0.0016)	(0.0016)
Nbhd peers incarcerated: age w/in 1yr	−0.0035*	−0.0015	−0.0001
	(0.0019)	(0.0029)	(0.0034)
Nbhd peers incarcerated: age w/in 1yr, same race & gender	0.0001	0.0039	0.0078
	(0.0021)	(0.0042)	(0.0052)
Nbhd peers incarcerated: age w/in 1yr, same race & gender & building	0.0007	0.0029	0.0060
	(0.0014)	(0.0027)	(0.0038)
Nbhd peers incarcerated: former partners	0.0030	−0.0051	0.0029
	(0.0177)	(0.0230)	(0.0259)
Mean of dependent var.	0.220	0.324	0.420
Observations	17,361	17,361	17,361

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table tests whether our key regressors of interest measuring the number of neighborhood peers incarcerated are correlated with observable determinants of recidivism. We first predict our recidivism outcome (probability of rearrest within a certain time frame) as a function of all observable demographic and criminal history characteristics. We then model the relationship between this predicted recidivism probability and our key variables of interest including census block fixed effects and report the estimated effects in the table above. We also provide an alternative balance test in Table A.1 where we report the correlation between demographic and criminal history observable characteristics and our key peers incarcerated regressor of interest.

Table 3: Effects of neighborhood peers incarcerated at time of release by attribute similarity

	(1) Rearrest w/in 3mo	(2) Rearrest w/in 6mo	(3) Rearrest w/in 12mo
Nbhd peers incarcerated	−0.002*	−0.000	0.005**
	(0.001)	(0.002)	(0.002)
Nbhd peers incarcerated: age w/in 5yr	−0.010***	−0.008***	−0.004
	(0.002)	(0.002)	(0.003)
Nbhd peers incarcerated: age w/in 3yr	−0.012***	−0.009***	−0.008*
	(0.003)	(0.003)	(0.004)
Nbhd peers incarcerated: age w/in 1yr	−0.029***	−0.021***	−0.016**
	(0.009)	(0.006)	(0.006)
Nbhd peers incarcerated: age w/in 1yr, same race & gender	−0.038***	−0.031***	−0.026***
	(0.007)	(0.006)	(0.007)
Nbhd peers incarcerated: age w/in 1yr, same race & gender & building	−0.051***	−0.038***	−0.032***
	(0.005)	(0.008)	(0.012)
Nbhd peers incarcerated: former partners	−0.048	−0.035	−0.011
	(0.035)	(0.048)	(0.053)
Mean of dependent var.	0.220	0.324	0.420
Observations	17,361	17,361	17,361

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

General Estimation Note: All regressions include indicator variables for gender, race, age (in years) at the time of exit, type of offense associated with the incarceration spell, number of months incarcerated, number of prior arrests, and number of prior incarceration spells. Neighborhoods are defined by census block delineations unless otherwise noted. When splitting the neighborhood peers by attribute type, specifications also include a variable measuring the number of other types of neighbors incarcerated (e.g. the effect of *Nbhd peers age w/in 1yr, same race, same gender* is from a specification that includes a variable measuring all of the other neighborhood peers incarcerated). We also include year-by-month of release fixed effects as well as neighborhood fixed effects (census block). Our estimation sample is defined in Table 1.

Each coefficient reported is from a separate specification with the outcome variable indicated by the column heading and the key regressor of interest described in the row heading. All other control variables are as described in the *General Estimation Note* above. We start with incarcerated individuals who are tied to the same census block neighborhood. The second through sixth rows estimate the influence of peers with increasingly similar characteristics (indicated by the variable description in each row). Full model results for the italicized row focusing on neighborhood peers of similar age, race and gender are reported in Appendix Table A.2.

Table 4: Robustness to alternative specifications

	Dep. variable = Rearrest w/in 3mo Nbhd peers incarcerated: age w/in 1yr, same race & gender				
	(1) Include local area crime rate 3mo pre-release	(2) Include local area crime rate 3mo post-release	(3) Include only one obs. per unique ind.	(4) Include local area linear trend	(5) Include local area linear & quad. trend
Nbhd peers incarcerated	−0.038*** (0.007)	−0.039*** (0.007)	−0.031** (0.014)	−0.041*** (0.009)	−0.040*** (0.010)
Mean of dependent var.	0.220	0.220	0.220	0.220	0.188
Observations	17,361	17,319	12,949	17,361	17,361
	(6) Include local area-by-year fixed effects	(7) Incl. year-mo of incar entry fixed effects	(8) Include local area-by-race-gender fixed effects	(9) Include individual fixed effects	(10) Logit
main					
Nbhd peers incarcerated	−0.042*** (0.010)	−0.039*** (0.007)	−0.038*** (0.007)	−0.035*** (0.013)	−0.030*** (0.006)
Mean of dependent var.	0.220	0.220	0.220	0.220	0.226
Observations	17,361	17,361	17,361	17,361	16,829

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents estimated impact of neighborhood peers incarcerated within one year of age and of the same race and gender on rearrest within 3 months of release for different specifications: The first and second specifications in the first row (specifications (1) and (2)) include measures of community (at the census block group level) crime and clearance rates around the time of release: Column (1) adds crime and clearance rates during the month prior to release and Column (2) adds the crime and clearance rates for the first 3 months post release (the time window of our dependent variable). Column (3) limits the sample to only the first incarceration exit for each unique individual. The next four specifications evaluate the robustness of results to controls for area-level trends and other time-specific effects: Column (4) includes census block group-specific linear time trends as controls and Column (5) adds a census block group–specific quadratic term. Column (6) includes census block group-by-year fixed effects, flexibly controlling for arbitrary annual trends at the census block group level. We use census block group area level controls instead of block-level since the number of census blocks prevents interactions with other control variables given our sample size. Column (7) adds fixed effects for the year-month of incarceration entry to our baseline specification. Column (8) modifies the baseline specification with census block fixed effects to include fixed effects for census block group interacted with race and gender categories and Column (9) includes individual fixed effects (based on full name and birth date variables). Finally, Column (10) reports the average marginal effects from a logit specification. All other general estimation notes from Table 3 apply.

Table 5: Alternative recidivism outcomes

	Nbhd peers incarcerated: age w/in 1yr, same race & gender					
	(1) Rearrest w/in 3mo Any	(2) Rearrest w/in 3mo Property	(3) Rearrest w/in 3mo Violent	(4) Rearrest w/in 3mo Drugs/Alc.	(5) Rearrest w/in 3mo Tech. Viol.	(6) Rearrest w/in 3mo Other
Nbhd peers incarcerated	−0.038***	−0.014**	0.001	−0.007*	−0.008***	−0.020***
	(0.007)	(0.006)	(0.005)	(0.004)	(0.002)	(0.005)
Mean of dependent var.	0.220	0.045	0.043	0.047	0.032	0.093
Observations	17,361	17,361	17,361	17,361	17,361	17,361
	(7) No. of rearrests w/in 3mo	(8) No. of rearrests w/in 6mo	(9) No. of rearrests w/in 12mo	(10) Days incar w/in 3mo	(11) Days incar w/in 6mo	(12) Days incar w/in 12mo
Nbhd peers incarcerated	−0.055***	−0.086***	−0.114***	−1.061***	−1.965***	−3.132**
	(0.010)	(0.013)	(0.019)	(0.287)	(0.556)	(1.244)
Mean of dependent var.	0.304	0.538	0.871	6.770	18.025	39.977
Observations	17,361	17,361	17,361	17,361	17,361	17,361

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents estimated impact of neighborhood peers incarcerated within one year of age and of the same race and gender on the following alternative definitions of recidivism: probability of rearrest within 3 months for different types of crime (specificaitons (2) through (6)); number of arrests within 3, 6, and 12 months since release (specifications (7) through (9)); days of incarceration within 3, 6, and 12 months since release (specifications (10) through (12)) . All specifications use census block delineations to define neighborhoods. All general estimation notes from Table 3 apply.

Table 6: Robustness to alternative definitions of neighborhood peers incarcerated

	(1) Peers inc. as a ratio	(2) Released first half of mo.	(3) Released second half of mo.	(4) Peers inc. at 3mo	(5) Peers arrest w/in 1yr
Nbhd peers incarcerated: age w/in 1yr, same race & gender	−0.046*	−0.051	−0.039	−0.035***	−0.033**
	(0.024)	(0.034)	(0.039)	(0.010)	(0.013)
Mean of dependent var.	0.220	0.233	0.203	0.220	0.220
Mean of key independent var.	0.045	0.044	0.045	0.058	0.065
Observations	17,361	9,612	7,749	17,361	17,361

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents specifications that assess robustness to potential measurement issues of our key regressor of interest: neighborhood peers incarcerated within one year of age, and of the same race and gender. The first column measures the key independent variable as the ratio of potential criminal peers incarcerated dividing the number incarcerated at the time of release by the total number of individuals previously incarcerated from the same neighborhood with the same demographic characteristics; the second and third columns estimate effects separately by the timing of release (first half of month in column 2 and second half of month in column 3) since our measure of peers incarcerated at the time of release only counts those incarcerated for the entire month of release; the fourth column estimates the impact of the number of peers incarcerated for the entire 3 month recidivism window on the 3 month recidivism outcome instead of our preferred measure capturing those incarcerated during the month of release; finally, column 5 reports estimates from a specification which redefines peers to include only those who are arrested within one year of the individual in our estimation sample.

Table 7: Heterogeneous effects by demographic and criminal history characteristics

	Dep. variable = Rearrest w/in 3mo Nbhd peers incarcerated: age w/in 1yr, same race & gender				
	(1) **Baseline**	(2) Age 18-24	(3) Age 25-34	(4) Age 35-44	(5) Age $\geqslant$ 45
Nbhd peers incarcerated	−0.038*** (0.007)	0.007 (0.025)	−0.065*** (0.014)	−0.053*** (0.012)	−0.048*** (0.009)
Mean of dependent var.	0.220	0.252	0.186	0.232	0.218
Observations	17,361	4,440	5,757	4,414	2,750
	(6) Male	(7) Female	(8) White	(9) Black	(10) Hispanic
Nbhd peers incarcerated	−0.041*** (0.008)	−0.222* (0.133)	−0.064 (0.085)	−0.038*** (0.006)	−0.020 (0.012)
Mean of dependent var.	0.219	0.229	0.211	0.250	0.049
Observations	15,661	1,700	2,694	12,535	2,132
	(11) Incar. for $<$ 6mo	(13) Incar. for $>$ 6mo	(14) Incar. in County Jail	(15) Incar. in State Prison	(15) Any prior Incar.
Nbhd peers incarcerated	−0.042*** (0.007)	−0.022* (0.013)	−0.042*** (0.007)	−0.007 (0.020)	−0.052*** (0.007)
Mean of dependent var.	0.240	0.164	0.230	0.141	0.287
Observations	12,696	4,665	15,345	2,016	8,050
	(16) Incar. for Prop. Crime	(17) Incar. for Viol. Crime	(18) Incar. for Drugs/Alc.	(19) Incar. for Tech. Viol.	(20) Incar. for Other Type
Nbhd peers incarcerated	−0.075*** (0.015)	−0.009 (0.014)	−0.064*** (0.012)	−0.029* (0.017)	−0.046*** (0.014)
Mean of dependent var.	0.306	0.233	0.204	0.187	0.189
Observations	2,499	4,661	2,721	2,273	5,207

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents estimated impact of neighborhood peers incarcerated within one year of age and of the same race and gender on the probability of recidivism within 3 months since release for sub samples split by: the age of release (row 1), gender or race (row 2), and length or type of incarceration (rows 3 and 4). All specifications use census block delineations to define neighborhoods. All general estimation notes from Table 3 apply.

Table 8: Heterogeneous effects by types of neighborhood peers incarcerated

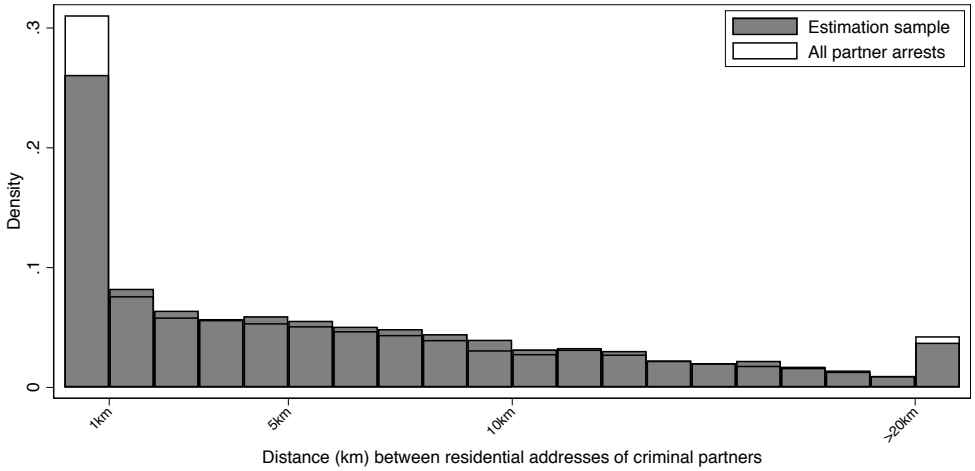
	Dep. variable = Rearrest w/in 3mo Nbhd peers incarcerated: age w/in 1yr, same race & gender (w/ mods in col. heading)		
	(1) **Baseline**	(2) Peers Incar. Before Entry	(3) Peers Incar. After Entry
Nbhd peers incarcerated	−0.038*** (0.007)	−0.036*** (0.007)	−0.039*** (0.012)
Mean of dependent var.	0.220	0.220	0.220
Mean of nbhd peers inc. var	0.107	0.050	0.057
Observations	17,361	17,361	17,361
	(4) Peers Incar. for $<$ 3mo	(5) Peers Incar. for $\geqslant$ 3mo and $<$ 6mo	(6) Peers Incar. for $\geqslant$ 6mo
Nbhd peers incarcerated	−0.039*** (0.008)	−0.025* (0.014)	−0.042*** (0.014)
Mean of dependent var.	0.220	0.220	0.220
Mean of nbhd peers inc. var	0.070	0.014	0.023
Observations	17,361	17,361	17,361
	(7) Peers Incar. for Prop. Crime	(8) Peers Incar. for Viol. Crime	(9) Peers Incar. for Drugs/Alc.
Nbhd peers incarcerated	−0.055*** (0.011)	−0.034** (0.014)	−0.054*** (0.018)
Mean of dependent var.	0.220	0.220	0.220
Mean of nbhd peers inc. var	0.020	0.036	0.016
Observations	17,361	17,361	17,361
	(10) Incar. for Prop. Crime & Peers Incar. for Prop. Crime	(11) Incar. for Viol. Crime & Peers Incar. for Viol. Crime	(12) Incar. for Drugs/Alc. & Peers Incar. for Drugs/Alc.
Nbhd peers incarcerated	−0.086*** (0.023)	0.021 (0.034)	−0.045 (0.077)
Mean of dependent var.	0.311	0.233	0.196
Mean of nbhd peers inc. var	0.026	0.043	0.020
Observations	3,207	4,668	2,251

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents estimated impact of neighborhood peers incarcerated within one year of age and of the same race and gender on the probability of recidivism within 3 months since release for different types of neighborhood peers as indicated by each column heading. All specifications use census block delineations to define neighborhoods. All general estimation notes from Table 3 apply.

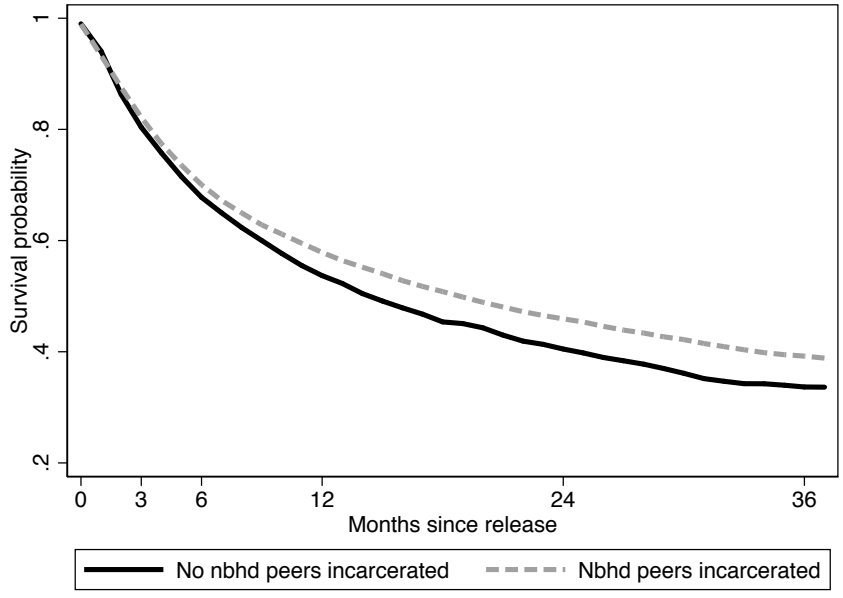
A. Appendix

Figure A.1: Distance (km) between residential addresses of criminal partners



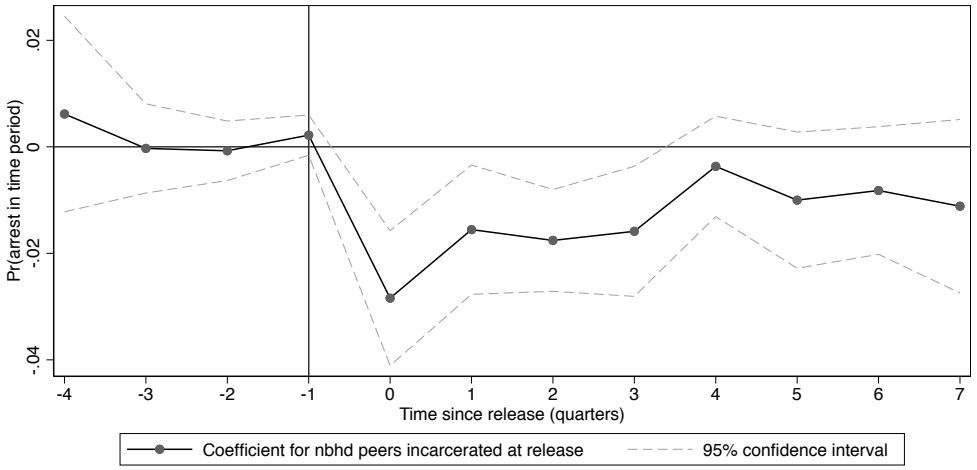
This figure plots the distance in kilometers between residential addresses of criminal partners in the partnership arrest data provided. We plot the distribution for individuals from our estimation sample that match with the partnership arrest data provided by the Charlotte Mecklenburg police as well as all partners in the police data.

Figure A.2: Survival rates for individuals released with neighborhood peers incarcerated vs. individuals released without any neighborhood peers incarcerated



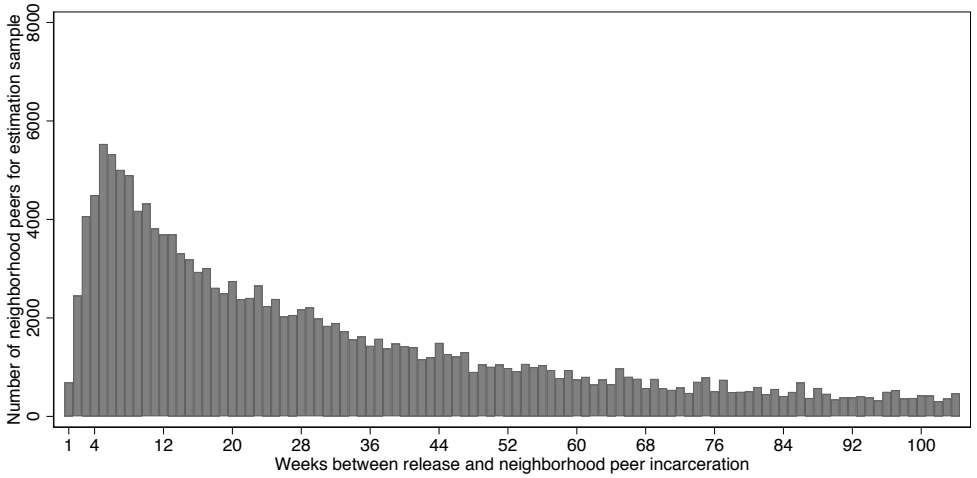
This figure plots the raw survival rate defined as the proportion of the total sample not rearrested for two different groups: individuals in our estimation sample released with at least one neighborhood peer incarcerated (dashed line) and individuals in our estimation sample released with no neighborhood peers incarcerated (solid line).

Figure A.3: Event study estimates of the effect of neighborhood peers incarcerated at time of release



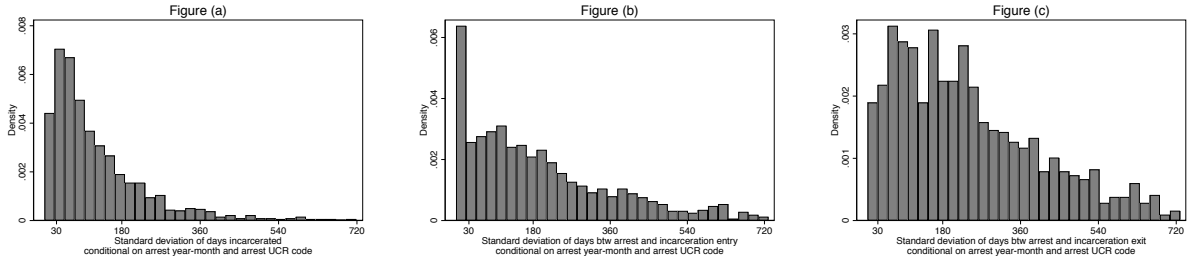
This figure plots the effect of our key regressor of interest, neighborhood peers incarcerated of similar age, race and gender, on the probability of arrest during time periods relative to release from incarceration. As expected, we see no effect on arrest while individuals are incarcerated and the largest impact in the first three months post-release.

Figure A.4: Relationship between timing of release and timing of peer incarceration



This figure plots a histogram of the number of weeks in between a peer's incarceration entry and the timing of release for individuals in our estimation sample. For each individual in our estimation sample, we calculate the distance between release and entry for every peer who is incarcerated and is included in our key measure of neighborhood peers incarcerated.

Figure A.5: Variation in time between arrest date and release from incarceration



This figure the standard deviation (in days) conditional on the type of offense and the year-month of arrest between (a) incarceration entry and exit; (b) arrest date and incarceration entry; and, (c) arrest date and incarceration exit.

Table A.1: Alternative Balance Test

	Dependent variable = *Nbhd peers incarcerated: age w/in 1yr, same race & gender*		
	No location FE	Census block FE	Census block race/ gender FE
Black	0.077**	0.086**	
	(0.037)	(0.044)	
Female	–0.111**	–0.111***	
	(0.043)	(0.027)	
Age at release	0.005	0.004	0.005
	(0.006)	(0.007)	(0.008)
Age at release squared	–0.000	–0.000	–0.000
	(0.000)	(0.000)	(0.000)
Time in state prison	–0.001	0.002	0.009
	(0.008)	(0.010)	(0.011)
Post release supervision	0.013	0.006	–0.008
	(0.026)	(0.025)	(0.027)
Total prior arrests (since 1998)	0.003	–0.001	0.000
	(0.003)	(0.001)	(0.001)
Total prior incarceration spells	0.004	0.001	–0.007
	(0.003)	(0.003)	(0.006)
Total months incarcerated (county jail + state prison)	–0.000	0.001	0.001
	(0.001)	(0.000)	(0.000)
Incarcerated for property crime	0.022	0.003	0.004
	(0.022)	(0.006)	(0.007)
Incarcerated for violent crime	0.015**	0.001	0.004
	(0.007)	(0.008)	(0.007)
Incarcerated for drug/alc. crime	0.014	0.006	0.013
	(0.011)	(0.011)	(0.012)
Total number of former criminal partners	0.003	0.002	0.005
	(0.003)	(0.003)	(0.005)
Observations	17,361	17,361	17,361
R^2	0.022	0.356	0.421

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table tests whether observable characteristics are correlated with our key regressor of interest measuring the number of neighborhood peers incarcerated. All specifications include fixed effects for year-by-month of release. The first column does not include any location fixed effects. The second column adds census block fixed effects and the third adds census block interacted with race and gender fixed effects. We find a significant correlation between fixed demographic characteristics of race and gender and the number of peers incarcerated which is driven by higher incarceration rates among male and minority populations. A potential identification concern arises if unobservable determinants of recidivism are correlated with these demographic characteristics. However, these concerns are mitigated by similar results from models holding all fixed unobservables within location-race-gender groups through including fixed effects for census block interacted with race and gender as reported in Table 4.

Table A.2: Full model results for primary specification

	Rearrest w/in 3mo	Rearrest w/in 6mo	Rearrest w/in 12mo
Nbhd peers incarcerated, nbhd=census block			
Nbhd peers incarcerated: age w/in 1yr, same race & gender	−0.037***	−0.030***	−0.025***
	(0.005)	(0.007)	(0.007)
Other nbhd peers incarcerated	−0.000	0.003	0.008***
	(0.002)	(0.002)	(0.002)
Demographic characteristics			
Black	0.041***	0.077***	0.119***
	(0.011)	(0.012)	(0.013)
Female	0.013	−0.007	−0.017
	(0.012)	(0.015)	(0.016)
Age at release	−0.012***	−0.020***	−0.022***
	(0.002)	(0.002)	(0.003)
Age at release squared	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)
Criminal justice characteristics			
Time in state prison	−0.064***	−0.069***	−0.044***
	(0.012)	(0.013)	(0.014)
Post release supervision	−0.001	0.017	0.061*
	(0.027)	(0.031)	(0.036)
Total prior arrests (since 1998)	0.013***	0.016***	0.018***
	(0.001)	(0.001)	(0.002)
Total prior incarceration spells	0.004	0.011***	0.011***
	(0.004)	(0.004)	(0.004)
Total months incarcerated (county jail + state prison)	−0.001***	−0.002***	−0.003***
	(0.000)	(0.000)	(0.001)
Incarcerated for property crime	0.094***	0.106***	0.106***
	(0.010)	(0.011)	(0.011)
Incarcerated for violent crime	0.039***	0.049***	0.066***
	(0.010)	(0.010)	(0.011)
Incarcerated for drug/alc. crime	0.002	0.005	0.029**
	(0.012)	(0.013)	(0.012)
Total number of former criminal partners	0.020***	0.025***	0.030***
	(0.004)	(0.005)	(0.004)
Observations	17,361	17,361	17,361
R^2	0.291	0.326	0.349

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents the results for all control coefficients for our the specifications reported in Table 3. Note that the estimated impact of neighborhood peers differs slightly from the relevant row in Table 3, since in our main specifications we control flexibly for several covariates by including dummy variables for each integer value of age at exit, number of prior arrests, number of prior incarceration episodes, specific UCR codes for the arrest leading to incarceration, length (in months) of the incarceration spell, and the total number of former criminal partners. In the specification reported here we only include continuous measures for these variables to allow for the assessment of the correlations between these variables and the dependent variables of interest..

Table A.3: Effects of peers released from incarceration

	(1) Rearrest w/in 3mo	(2) Rearrest w/in 6mo	(3) Rearrest w/in 12mo
(A) Nbhd peers incarcerated: age w/in 1yr, same race & gender	–0.042***	–0.035***	–0.028***
	(0.009)	(0.007)	(0.007)
(B) Nbhd peers recently released (w/in 3mo): age w/in 1yr, same race & gender	0.021**	0.024**	0.009
	(0.009)	(0.010)	(0.008)
p-value of test (A)=-(B)	0.027	0.210	0.022
Effects by timing of peer release			
(A) Nbhd peers incarcerated: age w/in 1yr, same race & gender	–0.042***	–0.035***	–0.027***
	(0.009)	(0.007)	(0.007)
(B) Nbhd peers recently released (w/in 1mo): age w/in 1yr, same race & gender	0.029**	0.030*	0.031**
	(0.014)	(0.017)	(0.015)
(C) Nbhd peers recently released (2 to 3mo): age w/in 1yr, same race & gender	0.016	0.020*	–0.003
	(0.011)	(0.011)	(0.008)
p-value of test (A)=-(B)	0.324	0.769	0.807
Observations	17,361	17,361	17,361

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents the results for the estimated impact of our key neighborhood peers incarcerated regressor also including a measure of the number of neighobhood peers released from incarceration just prior to the release of an individual in our estimation sample. We present these estimates to evaluate whether increases in the presence of criminal peers at the time of release have similar impacts as the lack of peers due to their incarceration at the time of release. We test whether the effects are symmetric and report the p-value of this test beneath each specification.

Table A.4: Effect of neighborhood peers incarcerated on relocation to same neighborhood for subsample of individuals rearrested post-release

	Distance btw rearrest location and pre-incar. location		
	Total distance km	Distance $<$ 5km	Distance $<$ 1km
Nbhd peers incarcerated: age w/in 1yr, same race & gender	–0.029	0.009	0.016
	(0.130)	(0.012)	(0.013)
Mean of dependent var.	3.623	0.714	0.714
Observations	6,208	6,208	6,208

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents the results for a model that evaluates the correlation between our key regressors measuring neighborhood peers incarcerated at the time of release and the change in residential location pre- and post-incarceration. We only observe post-incarceration location for individuals who are rearrested and who have a residential address reported by police, so these results are based on a selected subsample. Each column title indicates the residential mobility regressor in terms of distance in kilometers between the pre- and post-incarceration residential addresses reported in the arrest data. Each specification includes controls from our baseline specification described in Table 3.

Table A.5: Additional balance check: relationship between neighborhood peers incarcerated and neighborhood crime clearance rates and pre-incarceration crime severity

	(1) Severity of pre-incarceration offense	(2) Crime clearance rate in census block group
Nbhd peers incarcerated: age w/in 1yr, same race & gender	−0.021	
	(0.065)	
Nbhd peers incarcerated: nbhd = census block group		0.002
		(0.001)
Mean of dep. var	8.299	0.193
Observations	17,200	17,319

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents the results for a model that evaluates the correlation between our key regressors measuring neighborhood peers incarcerated at the time of release and the severity of pre-incarceration crime in the first column and the crime clearance rate in the first column to help address endogeneity concerns. Each specification includes controls from our baseline specification described in Table 3 with the following exceptions: the specification reported in the first column excludes fixed effects for UCR crime codes since the dependent variable (severity of crime) is mechanically related to crime offense codes (and crime severity is missing for 119 observations); for the second column the dependent variable is the ratio of arrests to reported crimes (the crime clearance rate) and is measured at the census block group so we also adjust our measurement of the key regressor (neighborhood peers incarcerated at the time of release of similar age, race and gender) and location fixed effects to be measured at the census block group level.

Table A.6: Non-linear effects of neighborhood peers incarcerated

	(1) Rearrest w/in 3mo	(2) Rearrest w/in 6mo	(3) Rearrest w/in 12mo
(a) 1 nbhd peer incarcerated (relative to zero)	−0.030**	−0.030*	−0.022
	(0.014)	(0.016)	(0.016)
(b) 2 nbhd peers incarcerated (relative to zero)	−0.064***	−0.083***	−0.071**
	(0.023)	(0.031)	(0.036)
(c) 3 or more nbhd peers incarcerated (relative to zero, avg = 4.2)	−0.181***	−0.134***	−0.113***
	(0.035)	(0.029)	(0.031)
p-value of test: (b)=2*(a)	0.929	0.585	0.573
p-value of test: (c)=4.2*(a) p-value	0.313	0.878	0.749
Mean of dep. var	0.220	0.324	0.420
Observations	17,361	17,361	17,361

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents the results for a model that splits our key neighborhood peers incarcerated regressor by the number of individuals. The first measures the effect of one peer incarcerated relative to zero; the second measures the effect of two peers incarcerated relative to zero; the third coefficient measures the effect of three or more relative to zero peers incarcerated (only 121 individuals in our estimation sample have three or more peers incarcerated at the time of release and the average number among this group is 4.2 peers incarcerated). We report p-values from tests where the null hypothesis is linear effects (e.g. the effect of two peers incarcerated is twice as large as one). All other details of the specifications are as described in Table 3.

Table A.7: Heterogeneous effects by neighborhood

	(1) Effects split by Nbhd crime levels	(2) Effects split by Nbhd recid levels	(3) Effects split by Nbhd incar. levels
(a) Above median nbhd: peers incarcerated	−0.041***	−0.036***	−0.038***
	(0.007)	(0.008)	(0.007)
(b) Below median nbhd: peers incarcerated	−0.025**	−0.049***	−0.046
	(0.013)	(0.014)	(0.064)
p-value of test: (a)=(b)	0.289	0.445	0.904
Mean of dep. var, above median	0.240	0.267	0.224
Mean of dep. var, below median	0.198	0.127	0.181
Observations	17,361	17,361	17,361

* p < 0.1, ** p < 0.05, *** p < 0.01. Two-way cluster-robust standard errors within census tracts and individuals are reported in parentheses.

This table presents the results for the outcome of arrest within 3 months allowing for heterogeneous effects across different types of neighborhoods. In the first column, we split our treatment variable by whether an individual is tied to a neighborhood above or below median crime rates. The second specification splits treatment by whether the individual is tied to a neighborhood above the median recidivism rate for our estimation sample. The third specification allows for different effects across neighborhood incarceration levels as measured by the number of individuals in our estimation sample released into a particular neighborhood over the analysis period. We report the p-value of a test of equality between the coefficients and means for each group. All other details of the specifications are as described in Table 3.