

Residential Relocation and Recidivism

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Abstract

This study examines whether moving away from an old neighborhood can reduce recidivism among recently released prisoners. On one hand, returning to an old neighborhood and connecting to networks of family and friends may support successful reentry and lower recidivism; while, on the other hand, returning to an old neighborhood may increase the risk that parolees become re-embedded in prior networks of criminal peers and family members and fall back on old habits of crime and substance use. To examine whether residential relocation after release from prison results in reduced recidivism, a randomized experiment was conducted, in which voluntary inmates paroled to a community correction center (CCC) in Pennsylvania were assigned to either an experimental group that went to a CCC in a neighborhood that was different from the neighborhood that they resided in before incarceration, or a control group that went to a CCC in their old neighborhood. Data were obtained from the Pennsylvania Department of Corrections, Pennsylvania Board of Probation and Parole, and the Pennsylvania State Police. Our results suggest that relocation reduces certain recidivism outcomes, especially rearrest and reincarceration following rearrest, at least in the relatively short term. We also find certain groups of parolees, especially older as well as drug-abusing parolees and those with less social attachment, tend to benefit more from relocation. We discuss policy implications and future research directions.

Introduction

Prisoners' successful reentry into society is a shared goal across the entire criminal justice system because it translates into a set of improved outcomes such as a reduction in recidivism, improved public safety, and cost efficiency. The current research addresses the question of whether relocating to a neighborhood that is different from the one that released prisoners lived in prior to incarceration facilitates successful reentry measured in terms of reduced recidivism. The research on this question is not clear. While it is documented that returning to an old neighborhood and connecting to the support networks of family and friends may be important for successful reentry and lower recidivism, there is some evidence that returning to the old neighborhood may increase the risk that released prisoners become re-embedded in prior networks of criminal peers and family members. A better understanding of the relationship between residential relocation upon release from prison and recidivism has important implications for reentry policies in general, and more specifically for the community corrections system in Pennsylvania. If allowing released prisoners to relocate to a new neighborhood results in reduced recidivism, such relocation options should be encouraged and integrated into reentry preparation prior to release. Because a large number of released prisoners in Pennsylvania go through Community Corrections Centers (CCC) as they transition back to the community – and there is some evidence that transition through CCCs tend to result in higher recidivism – developing a policy, which allows released prisoners to move to a CCC in a new neighborhood, has the potential to reduce recidivism and improve the efficiency of Pennsylvania's community corrections system. Those who voluntarily relocate after being released from prison may be systematically different from those who choose to return to their old neighborhood. For example, offenders who are relocating may be more motivated to desist from

crime and relocation is part of their strategy to desist. This selection process makes it difficult to estimate the effects of relocation based on observation data. In order to address the selection problem, the current research utilizes a randomized experiment, in which eligible paroled offenders are randomly assigned to CCCs in either a new neighborhood or an old neighborhood. In this report, we begin with a discussion of mass incarceration and the problem of high recidivism rates, both at the national level and in Pennsylvania. Next, we discuss the existing knowledge about the relationship between reentry outcomes and whether released prisoners return to their home neighborhood. We then describe the data and methodology that we use, and finally present the results and discuss their implications.

Background

Parole and the growth of prison population

As states and the federal government are moving toward reducing prison populations, expanded use of post-prison community supervision is a crucial component of reform efforts (Charles Colson Task Force on Federal Corrections, 2016; Clement, Schwarzfeld, and Thompson, 2011; La Vigne et al., 2014; Silber, Subramanian, and Spotts, 2016). Nearly 600,000 individuals are released from state prisons annually (Carson and Anderson, 2016), and roughly 70% of those released from prisons are placed under parole supervision (Carson and Anderson, 2016; Carson and Golinelli, 2013). Once on parole supervision, parolees must abide by a variety of conditions. While the actual conditions vary across jurisdictions and types of offenses (e.g., sex offenders may be required to attend sex offender therapy), typical conditions that parolees must comply with include (but are not limited to) regular reporting to their parole officer, curfew, payments of restitution and supervision fees, drug testing, and certain travel restrictions

(Ruhland et al., 2016). If any of these conditions are violated (technical violation), or if they are convicted of a new crime, the parolee can be recommitted to prison.

Despite the recent stabilization after four decades of continuous growth (Carson and Anderson, 2016), the U.S. incarceration rate is exceedingly high, both historically and comparatively in the world (National Research Council [NRC], 2014). As the prison population experienced this dramatic growth, the population which was released from prison – and then supervised on parole – also grew. The number of recommitted parolees due to recidivism and parole violations is also expected to increase. Parole violators represent an increasing fraction of all admissions to prison (Blumstein and Beck, 2005). In 1978, parole violators accounted for only 16% of the total state prison admissions (Carson and Golinelli, 2013). In 2015, over a quarter of state prison admissions were violators of parole and other conditional releases (Carson and Anderson, 2016).¹ Incarcerating an offender costs approximately \$29,000 per year (Pew, 2009), and states now spend over \$50 billion of general funds on corrections, most of which on prisons (NRC, 2014; Pew, 2011). The cost of supervising an offender on parole is on average \$2,750 a year (Pew, 2009). Given the large cost differential, efforts to reduce the risk of violations and parolee reincarceration are crucial to control correctional costs (Pew, 2009, 2011).

Pennsylvania context

Mirroring the national trend, the prison population of Pennsylvania has experienced decades of steady increase and recent slowdowns (Bureau of Justice Statistics, 2015; Carson and Anderson, 2016). In fact, in 2015, Pennsylvania recorded the largest one-year decline in the

¹ This is not necessarily to say that parole violators are the driver of prison growth. As prison population has grown, so have the number of parolees and the number of parole violators returned to prison (Pfaff, 2011).

prison population over the last 40 years (Carson and Anderson, 2016; Pennsylvania Governor's Office, 2016). With the strong momentum for correctional reforms under way in Pennsylvania, it is important to identify areas of vulnerabilities and improvement. Based on the most recent statistics, Pennsylvania has the second largest parolee population in the country, only smaller than Texas (Kaeble and Bonczar, 2016) and the highest parole rate (Watts et al., 2016).² In 2015, nearly a half of all admissions to the Pennsylvania Department of Corrections (PADOC) were for parole violations, and the number of parole violator admissions has been on the rise in the last 10 years (46% increase between 2005 and 2015), continuing to be an important driver of the state's prison population (PADOC, 2015) and costing the state over \$200 million each year (CSG, 2016). An internal survey conducted by PADOC of other state correctional agencies revealed that bed days occupied in prison for parole violations (i.e., number of parole violators recommitted annually x length of stay in prison for a parole violation) is higher in Pennsylvania than for any other states surveyed except for Texas and California.³ One key element in reducing this costly revolving door is Community Corrections Centers (CCC), which are halfway houses for recently released parolees. About half of all parolees in the state transition through CCCs, and evaluations of CCCs indicate that the parolees who go through CCCs have significantly higher recidivism (Council of State Governments [CSG], 2016; Latessa et al., 2009; PADOC, 2013), making the improvement of parolee outcomes through CCCs a priority.

² The large parole population in Pennsylvania is in part due to a large prison population (Carson and Anderson, 2016) but also a change in reporting methods in 2010, which now includes those previously classified as probationers (Glaze and Bonczar, 2011). However, even before the 2010 change, Pennsylvania had the third largest parolee population (Glaze, Bonczar, and Zhang, 2010).

³ Recommited parole violators spend on average 6-12 months in prison (PBPP, 2016). Certain less serious parole violators are placed in secure Technical Parole Violator Centers (PVC), rather than prisons. In 2015, similar numbers of parole violators are placed in PVCs and state prisons (CSG, 2016).

Recidivism and desistance

Recidivism rates among released prisoners are persistently high – two-thirds are rearrested and a half are re-incarcerated within three years of release from state prisons (Beck and Shipley, 1997; Durose et al., 2014; Langan and Levin, 2002). Similarly high recidivism rates are reported for Pennsylvania (PADOC, 2013). There are several explanations for high recidivism. Those who are incarcerated tend to have a long array of risk factors for offending, even *before* the incarceration experience, such as low educational attainment, poor employment history, substance abuse, and mental illness (Bushway, 2006; Petersilia, 2003). Those who are incarcerated on average also carry extensive criminal history records (10.6 arrests, 4.9 convictions prior to incarceration; Durose et al., 2014). The extant research on criminal career and life-course (e.g., Blumstein, et al., 1985; Brame, et al., 2003; Farrington, 1987; Laub and Sampson, 2003; Moffitt, 2006; Piquero, et al., 2003; Bushway, Nieuweerta, and Blokland, 2011) and corrections (e.g., Durose et al., 2014; Gendreau, Little, and Goggin, 1996; NRC, 2008; PADOC, 2013), suggests a strong positive relationship between past and future offending.

While these studies lend support to high recidivism and reentry failures, and more generally the continuity in offending, this is only half the story. The changes in the life course of offenders, either through external life events, such as marriage and employment (Sampson and Laub, 1993; Sampson, et al., 2006; Uggen, 1999, 2000; Warr, 1998), or more internal, cognitive transformation (Giordano, Cernkovich, and Rudolph, 2002; Maruna, 2001; Paternoster and Bushway, 2009), can affect their future offending trajectories and facilitate desistance. Research also suggests that the neighborhood context in which reentry occurs affects recidivism (Visher and Travis, 2003). In an early study on parolee recidivism, Burgess and others found that the type of neighborhood prisoners lived in and paroled to predict recidivism (Burgess, 1929;

Tibbitts, 1931). Recent scholarship also suggests that returning to disadvantaged neighborhoods (Hipp, Petersilia, and Turner, 2010; Kubrin and Stewart, 2006; Mears et al., 2008) or neighborhoods with a high concentration of other reentering offenders (Chamberlain and Wallace, 2016; Kirk, 2015) is associated with higher recidivism.

The aspect of the reentry context that the current research focuses on is the *dyadic*-level indicator of whether the neighborhood prisoners are released to a neighborhood they are from. While the economic and social disadvantage of the neighborhood can impact offenders in general, and certain offenders may be more susceptible to the impact than others (Gottfredson and Taylor, 1986), the offender-neighborhood pair (dyad) can be an important determinant of reentering prisoners' recidivism. Relocation does not necessarily situate offenders in better and more resourced neighborhoods, but rather breaks the connection between the offender-neighborhood dyad. The underlying theory for relocation is that prisoners' offending behavior is closely tied to their home neighborhood and their familiarity with the environment, and that neutral features of the neighborhood, such as places (e.g., a street corner, house, park) or people can take on unique meaning for offenders from that neighborhood. In other words, the neighborhood context could be criminogenic for one reentering prisoner who calls the neighborhood home, while at the same time, being in the same neighborhood could be beneficial for another prisoner who is new to the neighborhood.⁴

⁴ More generally, consideration of situation is important for behavioral prediction (Mischel, 1968; Mischel and Schoda, 1995; also see Horney, 2006). Even when the home and new neighborhoods are equally plagued by violence, drug activities, and lack of legitimate employment opportunities, familiarity itself may act as an additional risk factor of "falling back" into old pattern of criminal behavior (Fader, 2013). One juvenile offender in Jamie Fader's ethnography, who moved to a new neighborhood in Philadelphia describes, "I don't know the drug dealers and they don't know me and that's how I keep it" (Fader, 2013: 24).

Potential Benefits and Risks of Going “Home”

Where do they go and why?

Based on the Urban Institute’s prisoner reentry studies in Maryland, Ohio, Illinois, and Texas, roughly 50 to 60% of those who are released from prisons returned to the neighborhood that they lived in before they went to prison (La Vigne, Shollenberger, and Debus, 2009; La Vigne, Visher, and Castro, 2004; Visher and Courtney, 2007; Visher, La Vigne, and Travis, 2004). In an early study of released federal prisoners, Glaser (1969) reported that 45% returned to the same residence and over 80% of the prisoners returned to the same city. Recent studies, however, point to a more modest tendency of prisoners returning to the former neighborhoods. For example, among paroled prisoners in Michigan, only about a third return to an address within half a mile of their pre-prison address (Harding, Morenoff, and Herbert, 2013). Massoglia and colleagues (2012) also report that about 20 percent return to and stay in their former neighborhoods. A surprisingly low tendency of prisoners to move back to their former neighborhood may be due to the mobility of their family members, whom they plan to live with (Harding, Morenoff, and Herbert, 2013), rather than their voluntary decision to move away. In the highly socioeconomically disadvantaged neighborhoods and households, from which most prisoners originate, housing insecurity and instability is common (Desmond, 2012) and incarceration also contributes to the remaining family’s housing insecurity (Geller and Franklin, 2014).

Realities of being back home

Upon release, returning prisoners back to the area of former residence is often a default option in our reentry and parole policy (Glaser, 1969; Petersilia, 2003; for a PA policy, see

PADOC, 2008:4-5). On one hand, returning to the same neighborhood often translates to the releasees being reunited with their support network, most importantly their family members (Naser and La Vigne, 2006; Nelson, Deess, and Allen, 1999; Travis, 2005). Released prisoners receive a variety of tangible and emotional support from their family members, including housing, employment, and financial assistance, and close family relationships and strong family support are associated with positive reentry outcomes such as getting a job and staying clean from drugs (Berg and Huebner, 2011; Harding et al., 2014; Naser and La Vigne, 2006; Visher et al., 2004; Sullivan et al., 2002; Western et al., 2015). Importantly, however, the influence of family, and similarly romantic partners, in reentry can be negative as well as positive, and prisoners' family environment is often complicated (Harding et al., 2016; Wyse, Harding, and Morenoff, 2014). Studies document that family members themselves could have extensive criminal histories and substance abuse problems. Among the released prisoners in Maryland, about 40% of them reported having at least one relative serving time in prison and about 70% of them reported having at least one family member with a drug or alcohol problem (Visher et al., 2004).

Similar to the role of family, friends could also have either positive or negative influences on reentry outcomes. Among the released prisoners in Maryland (Visher et al., 2004), the most common channel for gaining employment after release from prison was through friends.⁵ Although friends could provide job referrals or emotional support (Nelson et al., 1999), it is equally likely that reuniting with old friends, particularly those with whom drugs were shared and other crimes committed, could increase the likelihood of recidivism (Berg and Cobbina,

⁵ Despite the prevailing notion that reentry prisoners are isolated from networks that can lead to employment and other resources and opportunities (e.g., Western, 2006), recent literature suggests otherwise (Sugie, 2014).

2016; Nelson et al., 1999; Visher et al., 2004; see also Hagan, 1993). Returning to the old neighborhood is often a risk factor because the same environment of temptations and other criminogenic forces that led to the crime is still there when they get back (Clear, 2007; Reitz, 2004). Even though some returning prisoners try to avoid those friends with negative influences, it is often difficult to stay away from them completely as they live in the same neighborhood, and constantly avoiding people they know could result in emotional distress and loneliness (Berg and Cobbina, 2016; Nelson et al., 1999).⁶

Review of empirical observations and theory development

Competing narratives on the consequences of moving back to the old neighborhood are not new. In the early twentieth century, urbanization and increasing social problems led to the characterization of the city and its conditions as the roots of crime. The Chicago School sociologists discovered differential levels of crime associated with different areas of the city (Shaw and McKay, 1942). In this context, not surprisingly, being paroled to an urban area was considered a risk. For example, based on the cases of youths who were paroled from an Illinois reformatory, Tibbitts (1931) found that the highest rates of parole violations were found among those paroled to rooming house districts in the high-delinquency transition area and the lowest rates were found among those paroled to rural and farm communities. This finding was framed as indicative of neighborhood effects on parolee recidivism but also suggestive of potential benefits of relocating parolees out of the city. Tibbitts also noted that for a parolee whose

⁶ Avoiding old people, places, and things is particularly difficult in rural reentry, in which offenders return to an area with smaller populations with frequent interactions (Ward, 2015). Returning to the old neighborhood may carry different meanings. They want people in the old neighborhood to see that they have changed (Uggen and Wakefield, 2005). For some others, especially those who intend to desist, their old neighborhoods provide a reminder of their past and helps strengthen their resolve to change (Leverentz, 2010).

criminality is closely tied to his criminal associates (e.g., gang), reform can result from “paroling him into an unfamiliar neighborhood” (19). Based on data of those who were paroled from two penitentiaries in Illinois, Clark (1951), however, found that returning to a community of the same size as the one where they lived prior to incarceration was associated with lower violation rates, compared to being paroled to a community of a different size. Clark (1951) reasoned that post-release adjustment might be more difficult in an unfamiliar environment.⁷ Glaser (1969:248) noted that among federal prisoners, one of the most common reasons for moving away from their old neighborhood was “contact with criminal or disorderly friends or relatives in the community whom they could not readily avoid, and with whom they engaged in the behavior which lead to their violation.” Based on a sample of juveniles who were released from imprisonment in the Netherlands, Buikhuisen and Hoekstra (1974) found that those who returned to their pre-prison residential address were more likely to experience recidivism than those who moved to another neighborhood. Buikhuisen and Hoekstra (1974) also found that those who came from a stable family environment did not benefit from relocation as much as those who came from an unstable family environment. These early studies on the reentry environment mirror today’s competing narratives about the effects of post-incarceration relocation. Early research also suggests possible heterogenous effects of relocation, depending on the strength of positive and negative social ties in the old neighborhood.

⁷ This dilemma of relocation is echoed by an early commentary on parole in Pennsylvania, suggesting “a man be placed in a different environment from the one that nurtured him in a life of crime, so that new stimuli can effect a change from an anti-social to a social attitude in his behavior. But a man released from an institution with ten dollars in his pocket and no job, with no definite plans for his future, is like a fish out of water. He endeavors to leap right back to his original habitat, if he can possibly do so... it is almost impossible to coerce him into trying a new stamping ground” (Kolakoski and Broecker, 1932:434-435).

The strongest early call for the post-prison relocation came from Jacobs (1976). Based on extensive qualitative work in the Illinois prison system (Jacobs, 1977), Jacobs (1976) points out that released prisoners, especially gang members, are reintegrated with family and friends who are often responsible for initiating and sustaining prisoners' criminal career from the beginning. He also criticizes the naïve emphasis placed on prisoners' maintaining social ties during and after incarceration, a major focus of the community-oriented corrections at that time (President's Commission on Law Enforcement and Administration of Justice, 1967). He writes (p. 193):

“The image of an inclusive normative community into which an ex-offender can be reintegrated does not fit the city blocks comprising the inner-city slum. Even if such areas comprise integrated normative communities supportive of crime-free careers, it is surely a mistake to assume that the convicted felon, by definition alienated from conventional community life, will now be ‘reintegrated’ with it in a new crime-free relationship. The opposite conclusion seems more predictable.”

With no empirical evidence for the theorized benefits of relocation at the time of his writing, Jacobs (1976) called for a rigorous experimental evaluation of prisoner relocation. The closest to experimental data on this come from research by Kirk (2009, 2012). Using a dislocation of prisoners following Hurricane Katrina as a natural experiment, Kirk (2009, 2012) found that those who moved to a new neighborhood after release had significantly lower recidivism than those who returned to the neighborhood they resided in before incarceration. He attributes a separation from the potentially criminogenic former neighborhood and social contexts as the underlying mechanism for reduced recidivism. In other words, for released prisoners, residential relocation serves as an instrument to knife off their prior context (Kirk,

2012; also see Maruna, 2001, Maruna and Roy, 2007).⁸ Moving away from an environment associated with negative behavioral routines and stigma is often seen as a change strategy. There are qualitative accounts of relocation as an attempt to desist among offenders (Fader, 2013; Laub and Sampson, 2003; Shaw, 1930; Sommers et al., 1994). Aside from the involuntary relocation due to a natural disaster, relocation is often initiated by offenders themselves because they recognize risk factors in their home environment. Self-initiated relocation, as a strategy Bottom (2013) calls *situational self-binding*, is not uncommon precisely because “offenders know their environments, routines, and vulnerabilities better than anyone else” (Schaefer et al., 2016:126) and thus relocation is an attempt to avoid environments that can draw them back to crime and recidivism.

The act of relocation to avoid recidivism and relapse into negative behaviors is mental as much as physical. Risk of relapse and recidivism may be tied to offenders’ memory of environments, in which they previously occupied. In a study of long-term prisoners in Canada, Munn (2009) presents that life after prison often involves navigating geographic spaces that are attached to place memory – the people, social interactions, and emotions as remembered by the prisoners. Released prisoners try to avoid certain places because of the negative place memories and attempt to associate themselves with new places for positive future memories. Residential change as a catalyst for a change is also prominent in the literature on substance abuse, where attempts to break away from the substance using world through physical relocation are referred to as the “geographic cure” (Cloud and Granfield, 2001; Maddux and Desmond, 1982; Waldorf, 1970; Waldorf et al., 1992). Those who abuse substances develop behavioral patterns or routines

⁸ There are other examples of forced relocations induced by natural disasters generating positive impacts, despite the disruptive nature of the disasters. For example, students displaced by Hurricane Katrina improved their school test scores (Sacerdote, 2012). A dislocation due to a volcanic eruption in Iceland improved economic situations of younger evacuees (Nakamura, Sigurdsson, and Steinsson, 2016).

over time, and certain environmental cues can trigger cravings of drugs, which can lead to relapse (Siegel, 1999). These environmental cues include “people, places, and things” such as encountering friends or partners who they used drugs with, visiting places and street corners where they bought or used drugs, and seeing drug paraphernalia or handling cash (Beck et al., 1993; Wallace, 1989). Extant evidence points to the benefits of such relocation on subsequent substance use (Genberg et al., 2011; Maddux and Desmond, 1982; Rachlis et al., 2010).⁹ Because drug use is common among prisoners and their drug-abusing life is intertwined with their criminal behavior (Mumola and Karberg, 2006),¹⁰ a geographic cure is relevant for prisoners and other serious offender populations (Berg and Cobbina, 2016; Sommers, Baskin, and Fagan, 1994; Travis and Petersilia, 2001).

Reconciling relocation and residential mobility research

When one considers the effects of residential relocation, it is important to distinguish relocation as a means to break away from negative influences and residential moving in general. There is rich inter-disciplinary literature on the association between residential mobility and a wide array of negative outcomes, including increased risk of suicide, mortality, school dropout, substance use, and delinquency and violence (for a recent review, see Gillespie, 2017).¹¹

⁹ There is a discussion in the Alcoholics Anonymous (A.A.) community about the merits and perils of changing “people, places, and things” and doing “geographicals”, which often involves a residential change to make a fresh start. The negative view on changing environment comes from the contention that “wherever you go, there you are” and recovery is only possible through self change (Dilkes-Frayne et al., 2017). Maruna (2001) also found similar skeptical views toward relocation or environment avoidance strategies among offenders.

¹⁰ This is by no means limited to those who are incarcerated for drug offenses. Nearly half of violent offenders and over 60% of property offenders report using drug the month before the offense, and one in three property offenders in state prisons committed their crime for drug money (Mumola and Karberg, 2006).

¹¹ At the neighborhood level, residential mobility is an important aspect of social disorganization that is associated with higher levels of crime and violence (Sampson, Morenoff, and Gannon-Rowley, 2002).

Especially for children, as Coleman (1988) argues, residential moves can disrupt social ties that connect children, family, school, and community, resulting in a loss of social capital. Consistent with Coleman's social capital argument, Sampson and Laub (1993), based on the Gluecks data, find that frequent residential moves are associated with higher delinquency.¹² While other research also suggests that mobility is associated with higher delinquency (e.g., Haynie and South, 2005), most recent research indicates such associations could be largely spurious, and can be explained by selection such that mobile youths are characterized by risk factors for both high mobility and delinquency (Gasper, DeLuca, and Estacion, 2010; Porter and Vogel, 2014; Vogel, Porter, and McCuddy, 2017). This highlights the difficulty of recovering causal relationships from observational data, where residential moves are endogenous.

In an early analysis from the Cambridge Study in Delinquent Development, a longitudinal survey of boys from an inner-city neighborhood of London, Osborn (1980) finds a positive relationship between frequent moving and delinquency, while he also finds that moving out of London was associated with lower delinquency. In other words, "moving *around* and moving *away*" (emphasis added) have opposite effects on delinquency (Osborn, 1980:57). This is not necessarily contradictory, as the moving away may be an indication of an attempt to separate from negative context and ties, while moving around is more of an indication of endogenous disruption in positive social capital. Vogel et al (2017) find that long-distance moves (outside the sending counties) lead to lower delinquency for youths with prior delinquency but such moves have no impact on youths without prior delinquency involvement. This suggests that for those with prior crime and delinquency, which could be at least in part attributable to their

¹² It is well documented that released prisoners often experience residential instability (Harding, Morenoff, and Herbert, 2013; Warner, 2015), and there is some evidence that frequent moving is associated with higher recidivism among released prisoners (Steiner, Makarios, and Travis, 2015).

criminogenic influences of their neighborhood, long-distance relocation benefits by separating them from those negative influences.

The benefits of moving away, specifically when the neighborhood of origin is a criminogenic risk, is consistent with the evaluation of the Gautreaux residential mobility program. The court-ordered desegregation program moved low-income, mostly black families in the Chicago metropolitan area during the 1970s-90s, some of whom were placed within the city, while others were placed in suburban communities (Rubinowitz and Rosebaum, 2000). The evaluation of the Gautreaux program found that boys in the families who moved to suburban communities were less likely to be arrested for a drug, theft, or violent offense, compared to those who stayed within the city (Keels, 2008).¹³ Contrasting this finding with another well researched mobility program, the Moving to Opportunity (MTO) program is informative. Beginning in the 1990s, following the encouraging results of Gautreaux, MTO randomly assigned low-income families living in public housing in five major U.S. cities to lower-poverty neighborhoods using housing vouchers. On crime outcomes, earlier evaluations (4 to 7 years after random assignment) found that the program reduced violent and property crime arrests for female youth but increased property crime arrests for male youth (Kling, Ludwig, and Katz, 2005). A longer-term evaluation (10 years after random assignment), however, found that the initial effects, both adverse and protective, largely faded away (Sciandra et al., 2013).¹⁴ One potentially important explanation for the divergent results from Gautreaux and MTO is the

¹³ It is interesting that for girls, suburban movers were more likely to be convicted of drug, theft, or violent offense (Keels, 2008). The results for boys should be more weight given that a larger proportion of boys are arrested and convicted.

¹⁴ Interestingly, a recent study found that MTO improved adulthood outcomes, including income and college attendance, for those who moved when they were young (before age 13), while moving had slightly negative impacts on those who were older adolescents (age 13 to 18) when they moved (Chetty, Hendren, and Katz, 2016).

degree of physical and social separation brought by the relocation. Most (over 80%) of the MTO treatment group moved less than 10 miles, while few Gautreaux suburban families made such short moves (Rosenbaum and Zuberi, 2010). Short physical distances meant limited separation from the former neighborhood and its negative social context (Keels, 2008; Sampson, 2008; Sharkey and Sampson, 2010). Using data from the Project on Human Development in Chicago, Sharkey and Sampson (2010), concur with the Gautreaux finding that adolescents who move outside of the city of Chicago are less likely to be engaged in violent behavior.

These existing accounts and studies suggest that the presence and direction of the effects of residential relocation is unclear. This is mainly because the effects of relocation are difficult to isolate in studies that rely on observational data where certain released prisoners choose to move while others choose to go back. Thus, the possibility that it is not relocation but certain individual characteristics that affect recidivism cannot be ruled out. It is also difficult to form expectations about the direction of relocation effects given the potentially competing consequences of returning to an old neighborhood. However, past research on offender and substance abuser relocation, residential mobility, and housing mobility programs collectively indicates that when offenders' criminality and criminal history is linked to their home neighborhood, moving away from the neighborhood with substantial geographic separation, can lead to lower recidivism. Kirk's (2009, 2012) natural experiment that takes advantage of forced relocation provides one creative way to overcome this problem, but as Kirk suggests, a more policy applicable and methodologically convincing approach would be to conduct a randomized experiment.

Data and Methodology

Experimental design and data sources

The current research is based on a randomized control field experiment that assigns voluntary parolees to an experimental condition of moving to Community Corrections Centers (CCCs), which are halfway houses, in a new neighborhood and a control condition of returning to CCCs in the former neighborhood. Thus, individuals who are released from their incarceration and are placed on parole through CCCs in Pennsylvania constitute the potential population that can participate in the relocation experiment.¹⁵ State prisoners who are paroled in Pennsylvania are either released directly to their private residences or CCCs. Approximately half of Pennsylvania state prisoners are paroled through CCCs (CSG, 2016; Wetzel et al., 2012). The decision to release parolees to the centers or not is made by the parole board based on factors, including whether or not an individual has an approved place to stay after release (“home plan”) as well as individual’s risk that may indicate a need for additional supervision (Wetzel, 2012). Typically, those are paroled to a CCC stay in the facility for several months.

The recruitment of these CCC-bound prisoners for the experiment ran from March, 2013 to December, 2015. The experiment focused on those parolees who resided before incarceration in the following three geographic areas that “send” the largest numbers of offenders to state prisons: Philadelphia County, Allegheny County (of which Pittsburgh is a part), and Dauphin County (of which Harrisburg is a part). The three areas are separated by a considerable distance (Allegheny County in the very West of the state, Philadelphia in the very East, and Dauphin in the middle), ensuring a substantial separation from the old context for the parolees in the experimental group.¹⁶ This addresses the concern in previous relocation experiments that

¹⁵ These centers include Community Corrections Facilities (CCF), which are contract facilities that provide similar services to state-run CCCs.

¹⁶ The distance between the two closest major cities Philadelphia and Harrisburg is approximately 100 miles, and the furthest pair of major cities Pittsburgh and Philadelphia is over 300 miles apart. The

relatively short distances between the origin and destination neighborhoods lead to diminished benefits of relocation (Sampson, 2008; Sharkey and Sampson, 2010) and the lack of separation fails to sever the criminogenic connections from the original neighborhoods (Kirk, 2012). Additionally, the three relatively populous areas have multiple CCCs that are equipped to provide programming that are required for certain parolees, such as the violence prevention program and alcohol and drug treatment. This allows the experiment to be as inclusive as possible.

The CCC-bound parolees were approached by case managers/counselors at their institutions with a consent form, which includes questions about the inmates' willingness to relocate and their home county (whether it is one of the three target counties). The questions can be found in Appendix A. In order to prevent selection bias, we identified, in advance, parolees with special constraints that make them ineligible for the experiment before the random assignment. We consulted with the PADO staff in the Bureau of Community Corrections (BCC), which oversees the CCC system, regarding the movability of parolees. As a result of this consultation, we determined a set of ineligibility criteria for the participation in the experiment. We created a spreadsheet-based random assignment generator for the BCC's Central Referral Unit (CRU), which is in charge of assigning prisoners to CCCs, to keep records of potential experiment participants, their eligibility outcomes, and the random assignment outcomes. The determined ineligibility criteria included being female, an arsonist or a sex offender, home county ineligible (not one of the three target counties), severe medical issues, and severe mental health issues. All the ineligibility conditions are determined due to the fact that inmates with these conditions can be placed in only a few centers and that would prevent random assignment.

geographic separation is thus much more significant than past residential mobility programs (Rosenbaum and Zuberi, 2010).

Within the spreadsheet, the CRU is also allowed to list other reasons for ineligibility (e.g., federal detainer, requesting relocation to specific counties or requesting not to be placed in specific counties). Those who were deemed eligible were randomly assigned to either an experimental group who were placed in a CCC in one of the areas that is different from the area that they resided in before incarceration, or a control group who were placed in a CCC in the area they were from. On the spread-sheet form, a random number was generated for each eligible case and there was an equal likelihood of the case being assigned to the experimental or control groups.¹⁷ We employed a rolling enrollment strategy for the experiment. As a new eligible parolee was identified, he was randomly assigned to the experimental or control groups.

The data on a wide range of individual characteristics of parolees came from the PADOc, including demographics, sentencing (e.g., incarceration crime), in-prison records (e.g., visitation, misconduct), and assessment (e.g., risk, substance abuse, mental health). Every court-committed inmate went through PADOc's Diagnostic and Classification Center (DCC) to complete tests and interviews for assessment areas, including security, medical, mental health, psychological education, and substance abuse. Data on prior incarceration came from PADOc, while arrest history data come from the Pennsylvania State Police (SPS). The complete list of variables is in Appendix B. The post-release address data came from the Pennsylvania Board of Probation and Parole (PBPP). The outcome of interest is recidivism after being released from prison, measured as a new arrest and a reincarceration (return to a Pennsylvania state prison). The rearrest data come from the PSP, and the reincarceration data come from PADOc.

¹⁷ A random number is generated by pressing a pre-programmed button. For each case, the random number generator can be pressed only once to prevent bias in the assignment procedure. After the random number is generated, a county is assigned automatically and then a particular CCC is selected manually within the assigned county.

Estimating the intent-to-treat effects

In this study, the main quantity of interest is the “intent-to-treat” (ITT) effects that capture the effects of being assigned to the treatment condition (relocation). The probability of recidivism was modeled as a function of a treatment indicator (a binary variable that indicates whether an individual is assigned to the experimental or control group) and other covariates using a linear probability model (LPM). Because perfect randomization removes selection bias (i.e. the characteristics of parolees are *balanced* between the experimental and control groups), the difference in proportion of recidivists between the groups captures the average causal effect of the treatment. The difference in proportions can be obtained from a regression on a treatment indicator without control variables. Although the baseline (pre-random assignment) values of covariates are uncorrelated with the treatment indicator,¹⁸ and thus controlling for them in a regression model does not affect the expected value of an estimator of the treatment indicator, controlling for them can generate more precise estimates (lower the standard error) of the causal effect (Angrist and Pischke, 2008).¹⁹ As shown below, the choice of LPM is to present consistent interpretation of estimates between the ITT models and the treatment on the treated models using the instrumental variable approach. Using logit models in place of LPM does not qualitatively change our main findings.

Another issue we should consider is the cohort effect. The recruitment and random assignment occurred over an extended period of time (2013-2015), and there are longer post-release data available for earlier cohorts. During the study period, there might have been changes

¹⁸ For the age variable, age at the time of eligibility determination (pre-randomization) is included instead of age at release, which could have been affected by the treatment assignment.

¹⁹ Even though improved precision is the reason for including covariates, controlling for covariates that explain little of the outcome variation can lead to larger standard errors by reducing the degrees of freedom (Duflo, Glennerster, and Kremer, 2007).

in factors that influence recidivism, aside from relocation, such as parole supervision and DOC practice and policy, and the economy.²⁰ Thus, in order to minimize the confounding cohort effects, we included fixed effects for the years when the experiment eligibility was determined (see Ludwig et al., 2008 for a similar issue in the MTO evaluation).

Subpopulation treatment heterogeneity

While those who voluntarily relocate are qualitatively different from those who return to the old neighborhood, in terms of their recognition of the criminogenic influences in the old neighborhood and their desistance motivation, it is possible that relocation is still more effective for some relocating parolees than others. For example, the age-crime curve demonstrates a steady decline in criminal activity after a peak in the late teens and young-adult period, and aging is one of the most powerful predictors of desistance (Farrington, 1986; Hirschi and Gottfredson, 1983; Laub and Sampson, 2003; Sampson and Laub, 1993). In an experimental study of employment and recidivism, Uggen (2000) found that employment was associated with lower recidivism but only for older offenders, and employment was not associated with recidivism for younger offenders. Thus, age may be associated with the potential role of relocation as a turning point in the life-course of released prisoners. In order to investigate the differential impacts of relocation by age, interaction terms between the treatment assignment indicator and the indicators for the quartiles of prisoner age distribution.²¹

Another source of heterogeneity in the effects of relocation is the strength of social attachment and support. As discussed earlier, while the role of family and friends in the reentry

²⁰ For example, based on unemployment rates, employment situation has been steadily improving post 2008 recession in Pennsylvania, which could have affected parolee recidivism in our sample.

²¹ The quartiles are < 27.64, >= 27.64 & < 33.03, >= 33.03 & < 40.80, and >= 40.80.

process is complicated, support from conventional family and friends can be an important facilitator of more successful reintegration (e.g., Berg and Huebner, 2011; Travis, 2005; Western et al., 2015). For those with strong social support in their former neighborhood, relocation may mean a separation from the source of support and lead to more negative consequences. It is worth noting that, given the participants of the experiment expressed their willingness to relocate, they may have considered possible consequences of relocation, including being separated from friends and family. This self-selection into the willing group may mitigate the negative impact of this relocation. In order to assess the heterogeneity, we use two indicators for the presence of social support: being married and being visited during incarceration. Marriage is uncommon for prisoners (Western, 2006), and like other contemporary parole populations (for example, Michigan parolees in Morenoff and Harding, 2011), only about 10 percent of the current sample are married. A long line of research supports marriage as a protective factor against offending, likely through multiple mechanisms, including increased informal social control and changed daily routines (Bersani, Laub, and Nieuwbeerta, 2009; Horney, Osgood, and Marshall, 1995; Laub and Sampson, 2003; Sampson and Laub, 1993; Sampson, Laub, and Wimer, 2006). If marriage is what prevents prisoners from returning to crime, it is possible that relocation can diminish the marriage effect and increase recidivism by creating a distance between prisoners and their spouse.

Another indicator of social support we consider is visitation. It has been long known that prisoners who maintain family ties through visits and other means have lower recidivism (e.g., Glaser, 1964; Hairston, 1988; Homer, 1979). Recent scholarship continues to support the beneficial role of visitation in reducing recidivism (e.g., Bales and Mears, 2008; Cochran, 2014; Mears et al., 2012), and prisoners who receive family visits tend to have better post-release

family support (La Vigne et al., 2005). Although the prior literature tends to view visits as a means to help prisoners maintain outside social ties, it is important not to equate visits with the universe of social ties that prisoners have. States typically have policies in place to approve visitors in advance and often exclude individuals who have ever been involved in any correctional system or have criminal records in general (Boudin, Stutz, and Littman, 2013; for PA visitation policies, see PADO, 2009). Because of these policies as well as the self-selection of visitors, visitors likely constitute a positive sphere of prisoners' social networks. Thus, relocating parolees who have received visits while incarcerated could mean separating them from social support and could increase recidivism. The interaction terms between the treatment indicator and the indicator of whether one is married as well as the indicator of whether one has received any visits while incarcerated were included to investigate this potential heterogeneity.

Lastly, prisoners with substance abuse problems may respond differently to relocation. Returning to the same neighborhood could pose an especially high risk for those with substance use history (Morenoff and Harding, 2014; Travis, 2000). Prior research suggests that environmental familiarity plays an important role in where drug users' purchase drugs. Based on interviews of crack cocaine users in Philadelphia, Pettway (1995) finds that users tend to buy drugs in their residential neighborhood because of safety and familiarity, and even when they engage in activities outside of their neighborhood, they return to their own neighborhoods to purchase drugs (see also similar findings from Camden, NJ in Johnson, Taylor and Ratcliffe, 2013). It is thus not surprising that for those who are in the process of recovery from drug dependency, one of the strategies to reduce the risk of relapse is to engage in a geographic cure – avoid the people, places, and things that are associated with drug use and to move away from the drug, drug scene, drug-using networks, both socially and physically (McIntosh and McKeganey,

2000; Waldorf, 1983). There is empirical evidence that such residential relocation is associated with lower substance use (Genberg et al., 2011; Maddux and Desmond, 1982; Rachlis et al., 2010).²² In order to examine whether the effects of relocation vary by the presence of preexisting substance abuse problems, we run models that include interactions between the treatment assignment indicator and indication of drug problems.

For each of the subpopulations, we further perform pairwise comparisons (e.g., compare recidivism of movers and stayers among parolees older than 40) to better understand heterogeneous treatment effects.²³

Estimating the treatment on the treated effects

By using all members of the treatment and control groups, who are equivalent on average as a result of random assignment, ITT estimates avoid selection bias. The ITT estimates of the relocation effects, however, do not differentiate within the treatment group those who adhered to the assigned treatment status and those who did not. In other words, some of those who were assigned to the treatment group (to be relocated) did not actually relocate for some reason. While this non-compliance may dilute the effects of relocating, the ITT estimates are important policy quantities, as criminal justice programs and social programs in general are often voluntary and non-compliance is inevitable (Angrist, 2006; Gartin, 1995; Heckman, Smith, and Taber, 1998; Ludwig, Duncan, and Hirschfield, 2001).²⁴ Thus, provided that other aspects of generalizability

²² The benefit of relocation is likely more for those who are addicted to illicit drugs than those with alcohol problems because the former is more closely tied to a subculture of drug users (Cloud and Granfield, 2001). Among correctional populations, especially prisoners, substance abuse most likely involves illicit drugs (the current sample is not an exception, as seen in Table 1).

²³ The pairwise comparisons are conducted using Stata command `pwcompare`.

²⁴ While criminal justice programs may uniquely enjoy higher compliance rates within the treatment group because the receipt of programs or sanctions may be mandatory, as in the case of Minneapolis

are taken into account, ITT estimates provide a realistic expectation for the impact of offering voluntary relocation outside of the current experiment (Sciandra et al., 2013).

While the ITT estimates are of policy relevance, the impact of relocation derived from those who actually relocated is a quantity of equal importance. Thus, we estimate the treatment on the treated (TOT) by comparing the outcomes of would-be compliers in the control group and the actual compliers of the treatment group. In order to estimate TOT, we take advantage of the fact that random assignment influences the probability of *receiving* the treatment. If we assume that would-be non-compliers in the control group are similar to the non-compliers in the treatment group, we calculate the TOT effects with two-stage least squares by using the random assignment as an instrument for receiving the treatment.²⁵

Non-compliance within the treatment group can arise for different reasons. First, some of those who were assigned to the treatment group did not end up going to a center for various reasons, including withdrawal (being paroled to an approved home plan instead), detainer status (being turned over to the authority to serve another sentence), and non-reporting (absconding).²⁶ There are also those in the treatment group who did not go to a center in the assigned county. Some degree of treatment group's non-compliance is also expected given that the actual relocation is not forced even if they were assigned to the treatment group.²⁷ Those who changed

Domestic Violence Experiment, compliance within the control group still may be imperfect (Angrist, 2006).

²⁵ In a model without covariates, this is equivalent to the ITT effect divided by the proportion of those in the treatment group who experienced the treatment (Bloom, 1984).

²⁶ There is one inmate in the control group who appeared to have completed the sentence (maxed-out) while waiting for release and thus did not go to a center. Based on the search of PADOC death records, there is none in either treatment or control group who has died before release. Seven inmates (5 in the treatment group and 2 in the control group) were not released at the time when we completed data collection, and thus they are excluded from recidivism analysis.

²⁷ Among those who were assigned to the treatment group, 77% appears to have actually relocated, while among those who were assigned to the control group, 86% moved to the control group areas.

their mind about relocating could inform DOC/PBPP and request a transfer to a center in non-treatment counties, either before they are actually relocated or sometime after they are relocated.²⁸

It is important to note that the TOT effects are estimated assuming that those who are assigned to the control group do not receive the treatment.²⁹ This assumption is met under our definition of treatment as relocation through the current CCC-based relocation program. Thus, those in the control group who move on their own are different from those in the treatment group who relocated, because it is possible and even likely that the control group members' relocation destination is determined by a variety of personal and family factors, not through the randomization process.

The indicator of treatment compliance was created based on records from both PADOCC and PBPP.³⁰ If the PADOCC records indicate that the parolees were actually admitted to the halfway houses within the assigned county, the compliance indicator coded as 1, otherwise 0. This indicator is further refined in light of the PBPP address records. For example, the compliance indicator is switched to 0 for parolees who appear compliant based on DOC records but have out-of-state addresses or addresses (ZIP codes) outside of the assigned county recorded by PBPP.

Time spent

²⁸ The consent form includes this point and potential program participants are aware that they are allowed to decide not to relocate anytime during the course of the program.

²⁹ Similar treatment of noncompliance in the control group is found in the MTO experimental evaluation (Kling et al., 2005)

³⁰ The home county is based on the information entered by the referral unit, which is mostly consistent with the parolees' response to the question about the area they lived in prior to incarceration that they consider home.

As noted earlier, those who were randomly assigned to the treatment group were not forced to move to or stay in the treatment CCCs, and they were allowed to opt out of the experiment at any time. Additionally, after being successfully discharged from CCCs, parolees were not required to reside in a particular area. They were allowed to move to housing in either the treatment area or in their former neighborhood or anywhere else. For these reasons, among those who complied with the treatment assignment, the length of stay in the assigned area varies. Thus, understanding the relationship between the length of relocated parolees' stay in the treatment area and recidivism would be of policy value. If a longer exposure to the area away from home increasingly reduces recidivism, correctional and parole agencies may incentivize released prisoners not only to relocate but also to stay longer in the destination area.

Despite its possible value, it is challenging to estimate such a “dose-response” relationship. This is because the duration is not manipulated by the experiment, and it is unlikely to be exogenous. For example, for those with shorter duration in the treatment area may represent those who are successfully discharged from halfway houses early and are thus of lower recidivism risk. Even if we account for some of the differences between parolees with shorter and longer duration through covariate adjustment, there may be still remaining unobserved differences that are unaccounted for. Thus, the results we report on the treatment duration should be considered only supplementary to the main results on ITT and TOT estimates.

The length of stay in the treatment area is the number of months between the release date and the last date on which the address within the treatment county is recorded by PBPP.³¹ In

³¹ If the parolee has multiple addresses recorded, the date the compliant address (within the assigned county) was last recorded was used to calculate the length. For some parolees, this is the date before the compliance status changes (i.e., the next recorded address is outside the assigned county), while for others, this is the last recorded address. If the parolee has a single recorded address, the date the address was recorded was used.

examining the duration effects, we control for the recidivism-free time, in addition to all the individual covariates. The time spent in the treatment area is directly affected by recidivism. Those who have a longer stay by definition have not experienced recidivism (at least in the form of reincarceration), and a longer stay can be a product of a variety of observable and unobservable characteristics of parolees (such as their resolve to desist). A long line of research on recidivism (Durose et al., 2014; Hoffman and Stone-Meierhoefer 1977; Kitchener, Schmidt, and Glaser, 1977; Maltz, 1984; NRC, 2008; Schmidt and Witte, 1988) and more recently on redemption (Blumstein and Nakamura, 2009; Bushway et al., 2011; Kurlychek et al., 2006, 2007; Soothill and Francis, 2009) indicates a strong negative correlation between the length of recidivism-free period and recidivism risk. Thus, comparing recidivism rates of those with a longer stay and those with a shorter stay would not generate the impact of duration but a mixture of the impacts of duration and other factors that affect both duration and recidivism. We gauge the duration effects by comparing the treatment and control groups who have the same recidivism-free length (e.g., three months) and by varying the treatment group's length of stay (e.g., changing from one month to two months).

Results

Descriptive characteristics of the sample

The data compiled from the consent forms and the spread-sheet form contained 14,302 unique individuals.³² Approximately 19% (2,553) of them expressed their willingness to relocate, and approximately 25% (641) of the willing inmates were determined to be eligible to participate

³² This is the number after duplicate data points have been removed.

in the experiment.^{33, 34} The total of 647 inmates were randomly assigned, and 336 inmates were assigned to the treatment group and 311 inmates were assigned to the control group.³⁵ Table 1 shows the average characteristics of those who were willing to relocate vs. those who were not, those who were eligible vs. those who were not (among the willing), and those who were assigned to the experimental group vs. the control group (among the eligible). On average, those who were willing to relocate are older, more likely to be incarcerated for violent and sex crimes, incarcerated for the first time but for a longer time. The willing prisoners also had a smaller number of prior arrests and lower risk score but a larger number of misconducts, and were less likely to be visited. They tend to have a lower prevalence of drug problems, but have a higher prevalence of mental health problems. The eligibility screening altered the sample composition in several aspects. For example, consistent with the exclusion factors of sex offender and severe mental health issues, those who were incarcerated for sex crimes were completely excluded and the prevalence of prisoners with mental health issues was reduced in the eligible group. Also, eligible prisoners were significantly younger and less white as a group than ineligible prisoners. The eligible group also shows multiple indicators of high risk, such as more prior arrests and misconducts, higher risk score and more STG members. Lastly, there were no statistically significant differences between the experimental and control groups, providing support for effective randomization.

³³ It was found that 105 individuals who expressed their willingness to relocate on the consent forms were left out of the spread-sheet form, and as a result, their eligibility status remained unknown.

³⁴ More than half of ineligible cases are due to home county ineligibility. About a quarter of ineligible cases are due to the prisoners being sex offenders. Other than the standard ineligibility criteria, the most common ineligibility reason was prisoners' request to move to specific areas or CCCs.

³⁵ Two individuals were determined ineligible but appeared to have entered the randomized assignment (assigned to the control group). Lastly, 36 individuals apparently stated not willing to relocate on the consent forms but their eligibility was assessed, and 5 of them were deemed eligible and randomly assigned and the rest (31) were deemed ineligible.

ITT estimates

Table 2 presents the main ITT estimates. The estimates are displayed by the length of follow-up to accommodate the different timings of release from prison during the study period (2013-2015). The estimates from rearrest outcome models in Column (1) indicate that relocation results in modest reductions in the likelihood of rearrests, from 3 percent within 3 months to 7 percent within a year. It is also evident that the reductions in rearrest are not sustained beyond a year. The recidivism reductions are absent in the reincarceration models, as shown in Column (2). The signs of the estimates are inconsistent across follow-up lengths, and the estimates are also noisier than the rearrest estimates.

The finding that relocation seems to affect rearrest but not reincarceration warrants further investigation. There are possibly multiple explanations for this. One is that arrest captures more of criminal behavior and relocation is helping parolees refrain from committing new crimes, while relocated and control parolees are equally vulnerable to reincarceration that can be driven by parole violations that are a noisy indicator of criminal behavior. Another possible explanation is that the risk of police arrest is affected by where offenders are from and where they may have had prior interaction with local law enforcement, and thus be known to them.³⁶ Parole agencies often work together with law enforcement, and information shared between them about where returning prisoners live, work, and engage in criminal activity could enhance surveillance (Jannetta and Lachman, 2011; Lutze, 2014; Travis, Davis, and Lawrence, 2012; for such partnerships in Pennsylvania, see PBPP, 2016, p. 20-21). Local law enforcement agencies may not have knowledge about parolees who are not from the area and thus such surveillance

³⁶ Some offenders may be motivated to initiate post-prison relocation to avoid, in their view, police harassment in their former neighborhood (Glaser, 1969).

may be less intensive and result in a lower possibility of arrest for relocated parolees (Kirk, 2012). This latter scenario, enforcement effects, suggests that relocation does not necessarily lead to better public safety, but it is an artifact of how the outcome (arrest) is generated.

The comparison of self-report offending and official records indicates that prior record or prior contact with the police may predict official response (Krohn et al., 2010) and rearrest may over-count crimes committed by those known to the police (Travis and Visser, 2005:6). In the absence of self-report offending data that may be less sensitive to the enforcement effects (Osborn, 1980), we test this possibility by examining the impact of relocation on 1) serious arrests that are defined here as arrests for violent and sexual crimes and 2) reincarceration following rearrest. Using violent and sexual arrests may reduce the influence from police discretion and how “known” the parolees are to local law enforcement (Uggen, 2000), and reincarceration following rearrest can also capture relatively serious arrests (reincarcerations due to technical parole violations, without preceding police rearrest, are not part of the outcome).

The estimates for these two additional outcomes are shown in Columns (3) and (4) of Table 2. For the violent and sexual arrests, the estimates for relocation are similar to the general arrest outcomes, negative for all follow-up periods and significant for 6 and 9 months. The estimates for new crime reincarceration are also all negative but stronger than the overall arrest outcomes. Given that the substantive pattern did not change from the he arrest outcome overall, there is no evidence that the arrest results are driven by the variation in police behavior.

Subpopulation estimates

The ITT estimates for the subpopulation analysis are presented in Tables 3a-d. As introduced earlier, the three dimensions characterizing the subpopulations are: age, social ties

(marriage and visitation), and substance abuse. On the conditioning effects of age (Table 3a), the results suggest that the oldest group (over 40) benefits significantly more from relocation, especially in comparison with the youngest group, although the effects are not always consistent and differentiable from other age groups. The results on marital status (Table 3b) indicate whether relocating parolees are married or not is not associated with relocation effects in any statistically distinguishable manners. For some of the outcomes, the effects are statistically significant for those who are not married, although this may be due to a larger proportion of parolees not married. However, those who were visited during incarceration experience higher risk of recidivism if they relocate. It is also possible the marital status variable may be a less reliable indicator of social ties and support post release given existing research suggesting that incarceration increases the risk of marital dissolution (Apel et al., 2010; Lopoo and Western, 2005). The results on visitation (Table 3c) provide a stronger pattern. The effects of relocation are stronger and more of them are statistically significant among those who are not visited. Thus, overall, social ties seem to have dampening or somewhat negative effects on the benefits of relocation. The results on substance abuse status (Table 3d) suggest that the relocation effects are stronger for substance abusing parolees, especially for the outcomes of reincarceration due to new crime.

Time spent estimates

Because recidivism risk changes with the length of recidivism-free period, experimental data, in which different lengths of stay in the new neighborhood are randomized across relocated

parolees would be necessary to estimate causal effects of changing lengths of stay.³⁷ Without such experimental data, the results presented in Table 3 only attempt to account for the impact of recidivism-free length by holding the length constant between the treatment and control groups. The results indicate the relative strength of recidivism-free length over and beyond the length of time spent in the treatment area. For both rearrest and reincarceration outcomes, among those who stay recidivism free for over 5 months, there is no statistically significant treatment effects, suggesting that any recidivism-reducing impact of relocation is washed by the stronger consequence of remaining recidivism free. Although there is more evidence of beneficial relocation within 4 months, the patterns are not consistent. For example, among those who stay recidivism-free for the first 3 months, the likelihood of rearrest in the next 3 months is lower to a greater extent for relocated parolees who stay in the treatment area for at least a month but less than two months, compared to less than a month. However, parolees who stay in the treatment area for at least 2 months but less than 3 months do not experience stronger relocation effects than those who stay for at least a month but less than two months. Thus, the observed patterns are not consistent with the expectation that the longer parolees stay in the treatment area, the stronger the positive relocation effects become. There are possibly many reasons why staying longer in the treatment does not translate into better outcomes. It is important to remember that relocation was facilitated through halfway houses, and thus staying in and leaving the treatment area is likely tied to staying and leaving the assigned halfway house. It is possible, for example, that parolees remain at the halfway houses longer because they are experiencing parole supervision difficulties and their home plan has not been approved.

³⁷ This is similar to estimating the dose-response relationship between incarceration length and recidivism. Without a random assignment of prison time, the relationship is confounded by age (Nagin et al., 2009).

TOT estimates

The TOT results are shown in Table 4. The estimates are similar but somewhat stronger than the ITT results. For example, within 9 months from release, relocated parolees had about 8 percentage points lower rearrest rates compared to the control group, while relocation-offered parolees had about 6 percentage points lower rearrest rates compared to the control group. Similar to the ITT estimates, the effects of relocation appear to be stronger on reincarceration for new crime. Within a year, relocation reduced new crime reincarceration by 12 percentage points, while being assigned to relocation reduced such reincarceration by about 9 percentage points (ITT effects). Because the significant majority (approximately 77%) of relocation-assigned parolees did appear to have relocated, it may not be surprising that the ITT and TOT estimates do not differ much.

Conclusions and Discussion

This study examined whether moving to a new neighborhood different from the one that released prisoners lived in prior to incarceration facilitates successful reentry, in terms of reduced recidivism. Prior studies and theory point to conflicting expectations about the relationship between returning to an old neighborhood and recidivism. In order to examine whether residential relocation after release from prison results in reduced recidivism, this study used a randomized experiment, in which voluntary inmates paroled to a community correction center (CCC) in Pennsylvania are assigned to either an experimental group that went to a CCC in a neighborhood that is different from the neighborhood that they resided in before incarceration, or a control group that went to a CCC in their old neighborhood. The results indicate a modest,

time-limited benefits of relocation, and in particular the benefits appear stronger among substance abusing offenders and those who may not have strong social attachment. The results also show that the effects of relocation are stronger in rearrest rather than reincarceration outcomes. While this provides some support for the effectiveness of relocation, the fadeout issue requires a careful examination of relocation as a correctional policy option.

Possible explanations

In the life-course literature, there are competing mechanisms put forth to explain desistance process. Structural explanations suggest that structurally-induced turning points (e.g., job and marriage) facilitate desistance (Sampson and Laub, 1993; Laub and Sampson, 2003), while others put more emphasis on internal change or cognitive transformation (Giordano, Cernkovich, and Rudolph, 2002; Maruna, 2001; Paternoster and Bushway, 2009). Because the relocation experiment is only applied to willing, and likely more motivated offenders, the current research does not directly support one or the other (which would perhaps require relocation experiment on *all* released prisoners). However, the current findings do suggest that structural assistance like residential relocation can facilitate a behavioral change at least in a short term (Maruna and Roy, 2007; Visher and Travis, 2003).

Restructuring one's pattern of living seems to help abstinence from substance use (Vaillant, 1988), and parole supervision and the use of halfway houses have the potential to achieve this. Even though the separation from the old neighborhood is likely to be "most crucial during the first few weeks after release when the challenges for ex-offenders are substantial" (Kirk, 2012:336), the facilitation of short-term separation through relocation does not seem sufficient to make a more permanent change in the lives of offenders. There may be several

explanations for the lack of long-term benefits. First, while relocating to a new area may initially help parolees, especially those with substance abuse problems, stay away from crime and drugs, maintaining the new lifestyle is often difficult. Those in the recovery process know that “drugs are everywhere” and have the skills and knowledge to find drugs even in the new and unfamiliar neighborhood they have relocated to (Leverentz, 2010: 666).³⁸

Also, while relocation may provide a new environment that is relatively void of familiar criminogenic forces (known criminal associates, street corners and other places associated with old habits, family members engaged in illicit activities), the new environment is also relatively void of familiar forces that support conditional activities, such as conventional models among family members, local social service agencies.³⁹ Going through the reentry process devoid of social support systems and away from former social connections can be a lonely and may be conducive to recidivism (Clear, 2007; Harding et al., 2016; Leverentz, 2010). A recent study of reentry in Boston suggests that older prisoners and prisoners with mental health and addiction tend to fall into social isolation (Western et al., 2015). It is a dilemma that the population that is most susceptible to isolation is also the population that is probably strongly attached to the criminogenic networks over their life course. For them, relocation can help sever ties with the criminogenic networks and stay out of trouble but simultaneously isolate them physically and socially. Research on housing mobility programs also indicate that strong attachment to movers’

³⁸ Physical relocation to avoid problem people and situations can be one way to maximize the chances of successful reentry. Also important is the capacity to cope with problems. Life after prison is likely to be riddled with problems, and those who succeed and those who fail both experience problems, and those who end up failing are those with more conventional coping and problem-solving skills (Bucklen and Zajac, 2009). Thus an avoidance strategy is not a cure but can be incorporated into the problem-solving tool kit that prisoners can acquire.

³⁹ One parolee who participated in the relocation experiment expressed concern that he may not be able to receive certain services (e.g., employment assistance, health care) from local county agencies because he is not from the area. He also pointed out that if he is in the old neighborhood, he could relatively easily access day-to-day help, such as food and clothes from his family members.

former neighborhood and their feeling of isolation in their new neighborhood would make relocation less successful (Boyd et al., 2010; Briggs, 1997).⁴⁰

Lastly, the apparent difficulty of making the impact of relocation “stick” may largely reflect the sheer riskiness of our prisoner sample. Despite the growth in incarceration in recent decades, “the process leading to imprisonment is still a highly selective one” (Loeffler, 2013:141). Those who end up in prison have accumulated disadvantage over their life-course, through the cascading effects of their contact with the criminal justice system, exposure to neighborhood disadvantage, family and school failures (Sampson and Laub, 1993, 1997). Nearly half of the current sample of parole releases are prison “veterans” (Rosenfeld, 2008) who have prior incarceration experience in Pennsylvania, and they are more prone to recidivism and more likely to return to prison (Rhodes et al., 2016). Relocation can be part of the effort to start reversing their cumulative momentum toward recidivism and reentry difficulties, but it may be still incomplete as a solution.

The divergence between the outcomes of rearrest and reincarceration is worth our attention. In Pennsylvania and anywhere else, supervision intensity likely plays an important role in parolee reincarcerations. Prior research suggests that closer surveillance of parolees can result in reincarceration without significantly preventing new criminal behavior (Grattet and Lin, 2016; Hamilton and Campbell, 2013; MacKenzie, 2006; Petersilia and Turner, 1993; Piehl and LoBuglio, 2005). Although both rearrest and reincarceration are a function of underlying parolee

⁴⁰ Stanley, the youthful offender in Clifford Shaw’s (1930) *The Jack-Roller*, illustrates the relationships between residential move and social ties. After being released from incarceration, Stanley was placed in a foster home. Because of the relatively short distance between his new residence and his former neighborhood, he often returned to the old neighborhood and maintained social ties that facilitated his early delinquency. What finally helped Stanley break away from the past was quality employment and the social bonds that he established with the foster family. Thus, certainly geographic separation from past negative influences contributed to his eventual desistance but the creation of the new prosocial context was necessary for desistance to set in.

behavior and system behavior (policing, supervision practices and intensity), the system behavior is likely a stronger determinant of reincarceration than of rearrest (Kurlychek, 2011; Rudes, 2012; Wright and Rosky, 2011).⁴¹ The relocation effects are mostly limited to rearrest and return to prison after a new arrest. This is consistent with the argument that reincarceration is a noisy proxy for new criminal behavior, and even if relocation truly prevents criminal behavior such effectiveness may not be reflected in reincarceration reductions.

Generalizability, heterogeneity, and possible limitations

Due to the randomized assignment, the current estimates of ITT or TOT should have high internal validity. However, the estimates are limited to the experimental sample, and it is important to assess the external validity. There are multiple layers of selection into the sample, including the assignment to the community corrections centers, willingness to relocate, and the eligibility for relocation. The use of halfway houses during post-prison parole supervision varies across states, and Pennsylvania is one of states with extensive use (others include Ohio and New Jersey). The assignment to halfway houses is determined by PADO and PBPP, based on the consideration of home plan (post-release housing arrangement) and additional factors, such as assessed risk and needs. Thus, those with stable housing history and family support, and lower levels of recidivism risk and treatment needs, may be screened out of the center placement and thus the current findings may have little applicability to.

⁴¹ It is important to note that some of the parolees returned to prison with the status of “technical violators” have committed a new crime (along with other violations of supervision conditions) but their cases were preliminarily handled by the parole board rather than the police and the court. In Pennsylvania, approximately half of parolee reincarcerations are linked to some occurrence of criminal activity, which includes conviction of a new crime as well as a failure to notify parole agents of an arrest (Bucklen, 2005; Kaeble and Bonczar, 2016; Kramer, Silver, and VanEseltine, 2008).

The various ineligibility criteria also limit the generalizability of the current findings. For example, sex offenders, those with severe medical or mental health issues, and those who were not from the target counties were not eligible for relocation. This may be a common limitation in state agency driven field experiments (see, for example Duwe, 2012), but this is a realistic policy constraint when one considers implementation of similar relocation options in other jurisdictions.

The current findings on the effects of relocation are solely based on willing parolees, and may not be generalizable to unwilling ones. As the analysis on willingness above suggests, those who are willing to relocate are different from those who are not willing to relocate on observed characteristics but also likely to be different on unobserved characteristics, which may determine recidivism outcomes. Given these differences, the forced residential relocation for all parolees is not a policy option that the current research can help inform.

Also, it is important to note that the use of halfway houses as a vehicle of relocation has both advantages and limitations. The main advantage is the practicality. Any number of states already use halfway houses or residential reentry centers (Camp et al., 2003, Center for Behavioral Health Services & Criminal Justice Research, 2013), so there is an existing infrastructure to relocate released prisoners without expensing new state resources involving private-market housing (as in MTO, for example). Using halfway houses, however, also led to some of the ineligibility criteria because not all halfway houses can accommodate parolees with certain conditions, such as mental health issues. Another limitation is the potentially criminogenic environment of halfway houses. Research on the effectiveness of halfway houses has generated mixed findings (MacKenzie, 2002), and recent review evidence seems to be tilting

toward null or criminogenic (MacKenzie and Farrington, 2015; Ndrecka, 2014).⁴² In Pennsylvania in particular, the recent evaluation findings are consistent with criminogenic influences of halfway houses (PADOC, 2013). If some halfway houses are more criminogenic than others, and if the treatment halfway houses are systematically different from the control halfway houses, the recidivism of untreated parolees may be a poor representation of the counterfactual outcome. Housing relocation efforts for former prisoners that are not through halfway houses, such as privately funded housing subsidy, are under way and the initial findings are promising (Kirk et al., 2017).

Lastly, the current sample is limited to Pennsylvania and the three areas selected as the origin and destination sites. All three are urban centers of Pennsylvania. A quarter of court-committed prisoners and approximately 40% of the state's total prison population are from the three counties (PADOC, 2015), so by no means the extent of generalizability is small. However, it is still the case that the results are not easily applicable to those from suburban and rural counties and relocation to such areas.

There is possible heterogeneity in the response to relocation that is unobserved in the current study. One source of heterogeneity may be the neighborhood effects. Neighborhood characteristics are known to influence the outcomes of crime in general (Sampson, 2011) as well as recidivism (Hipp, Petersilia, and Turner, 2010; Kubrin and Stewart, 2006). In other words, the current estimates are a function of the relocation effects and the "destination effects" (Sharkey and Sampson, 2010). Previous studies on the effects of residential relocation have some indications for this potential heterogeneity. The MTO evaluations suggest that relocating to less

⁴² Recent scholarly work suggests that halfway houses may be associated with reductions in certain recidivism events (parole revocations) but not others (rearrest and reconviction) (Hamilton and Campbell, 2014) or the effectiveness may be limited to high-risk offenders (Lowenkamp and Latessa, 2005)

impoverished neighborhoods leads to a reduction in violent crime, although the beneficial effects attenuate over time (Kling et al., 2005; Sciandra et al., 2013). Among substance abusers, relocating to neighborhoods with better qualities (e.g., disadvantage, economic conditions) is associated with lower substance use (Cooper et al., 2013; Genberg et al., 2011). One of the potential mechanisms for the beneficial effects of relocation may be changes in social networks. Linton et al. (2016) found that relocating to a neighborhood with less economic disadvantage led to less association with drug/alcohol using social circles. However, this evidence should be weighed against indications that relocation (even to less disadvantaged neighborhoods) can result in difficulty building new relationships and less emotional support and sense of isolation (Clampet-Lundquist, 2004; Curley, 2009).

One potential limitation of the current research is possible interference between the experiment participants. The valid causal inference relies on the stable unit treatment value assumption (SUTVA), which assumes that for any individual in the study, the potential response to the intervention (treatment or control) is not affected by the treatment assignment of that individual and any other individual under study (Angrist, Imbens, and Rubin, 1996; Rubin, 1980; see also Gangl, 2010; Sobel, 2006).⁴³ In the context of the relocation experiment, violations of this assumption include social interactions between inmates in the study.⁴⁴ It is not directly testable, but the communication between inmates in the study is not likely because of relatively small proportion of inmates in each prison who participated in the experiment. On average across prisons, 5% of inmates who were in the process of release to halfway houses and thus were

⁴³ To be clear, STUVA is not a unique concern to experiments but equally problematic for non-experimental studies (Berk, 2005).

⁴⁴ Potential violations of this assumption has been a concern with field experiments, including the Moving to Opportunity experiment (Clampet-Lundquist and Massey, 2008; Ludwig et al., 2008; Sampson, 2010; Sobel, 2006).

approached for relocation participated in the experiment.⁴⁵ Further, inmates near release constitute a small portion of the total inmate population in each facility, making direct communication between participating inmates even less likely. Another possible violation of STUVA is the John Henry effects, referring to the control group's behavioral response to their treatment assignment. For example, those who are assigned to the control group may feel even more committed to stay out of trouble because they wished to relocate to turn their life around but were denied the option. Another scenario could be that those who were assigned to the control group may feel offended by the perceived failure of the system to honor their relocation choice and may become less committed to desist. If the former scenario exists, the current relocation estimates are conservative ones, while the latter scenario would result in overestimates. Future research is needed to further investigate the mechanisms of how inmates receive and spread information about these research-based opportunities through official channels (prison staff) as well as through informal inmate social networks (Kreager et al., 2016).

Policy options

Although the voluntary nature of the current relocation program limits its scalability, reflecting on history, there is a good reason for why relocation remains voluntary. During the early era of U.S. corrections, banishment, exile and “warning out” laws were used to remove offenders from the community, to preserve safety of the sending community but also resources (Rothman, 1971). Although banishment laws were initially justified by the presumed benefits of relocation, the fact that banishment meant ostracizing offenders from their community and denying provision of assistance and supervision damaged its legitimacy (Armstrong, 1963;

⁴⁵ The smallest proportion is 1% and the largest proportion is 14%.

Miller, 1957). A more formal policy, referred to as “conditional termination” or “conditional pardon”, which required prisoners to sign a pardon agreement with a condition that they leave the state and never return was also criticized on similar grounds (Lynch, 1965; Miller 1957).⁴⁶

Given the evidence of fadeout, providing relocated parolees with aftercare, possibly through parole and/or halfway houses, may be particularly useful. This aftercare assistance can include locating and maintaining housing and employment and forming new social relationships in their newly adopted environment (Waldorf, 1970; Westermeyer, 1989). Past housing mobility programs indicate that pre- and post-relocation support services, which connect movers to social services in the community, help develop support networks, and help identify employment and post-halfway house housing options could be helpful (Boyd et al., 2010; Varady and Kleinhaus, 2013).

Lastly, although individually-oriented correctional interventions and rehabilitation efforts tend to dominate current correctional research and practice, some scholars direct our attention to environmental and situational risk factors and opportunities of recidivism (Cullen, Eck, and Lowenkamp, 2002; Schaefer, Cullen, and Eck, 2015; Cullen, Jonson, and Mears, 2017; see also Clear, 1996). Engaging parolees in the process of developing reentry plan and a roadmap to desistance, including the option of residential relocation, makes sense as “offenders know their environments, routines, and vulnerabilities better than anyone else” (Schaefer et al., 2016:126). Allowing released prisoners to relocate when appropriate seems to be a promising strategy that warrants further research and refinement, especially an addition of aftercare and support for relocated parolees.

⁴⁶ Avoiding states dumping problem cases on another state was an impetus for the creation of the Interstate Compact for the Supervision of Probationers and Parolees in 1937 (Bennett, 1937).

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Tables and Figures

Table 1. Comparison of Inmate Characteristics across Groups (sample size in parentheses)

	Willing	Not Willing	Eligible	Not Eligible	Treatment	Control
Age at Release	38.06* (2,402)	35.98 (11,464)	35.21* (634)	39.26 (1,671)	34.84 (331)	35.62 (309)
White (0/1)	.40 (2,550)	.42 (11,749)	.20* (641)	.47 (1,804)	.21 (336)	.20 (311)
Married (0/1)	.14* (2,550)	.12 (11,749)	.12 (641)	.14 (1,804)	.14 (336)	.10 (311)
Children (0/1)	.68 (2,550)	.68 (11,749)	.69 (641)	.69 (1,804)	.69 (336)	.68 (311)
High School Graduate (0/1)	.58 (2,550)	.58 (11,748)	.53* (641)	.59 (1,804)	.53 (336)	.52 (311)
Court Commitment (0/1)	.80* (2,550)	.73 (11,749)	.75* (641)	.82 (1,804)	.77 (336)	.73 (311)
Commitment Crime: Violent (0/1)	.28* (2,550)	.25 (11,749)	.37* (641)	.25 (1,804)	.37 (336)	.37 (311)
Commitment Crime: Sex Crime (0/1)	.12* (2,550)	.02 (11,749)	.00* (641)	.17 (1,804)	.00 (336)	.00 (311)
Commitment Crime: Property (0/1)	.23* (2,550)	.25 (11,749)	.19* (641)	.24 (1,804)	.20 (336)	.17 (311)
Commitment Crime: Drugs (0/1)	.25* (2,550)	.31 (11,749)	.30* (641)	.22 (1,804)	.29 (336)	.31 (311)
Commitment Crime: Public Order (0/1)	.12* (2,550)	.16 (11,749)	.14 (641)	.11 (1,804)	.14 (336)	.14 (311)
No Prior Incarceration (0/1)	.52* (2,549)	.49 (11,748)	.46* (641)	.55 (1,803)	.47 (336)	.44 (311)
Time Served (Years)	3.81* (2,402)	2.68 (11,464)	3.78 (634)	3.88 (1,671)	3.58 (331)	3.97 (309)
Number of Prior Arrests	7.17* (2,548)	8.39 (11,749)	8.68* (640)	6.60 (1,803)	8.47 (336)	8.91 (319)
Number of Misconducts	2.30* (2,550)	1.29 (11,749)	3.23* (641)	2.01 (1,804)	2.86 (336)	3.57 (311)
Visitation (0/1)	.54* (2,550)	.60 (11,749)	.58* (641)	.52 (1,804)	.59 (336)	.56 (311)
STG (0/1)	.07* (2,552)	.05 (11,749)	.09* (641)	.07 (1,806)	.09 (336)	.09 (311)
Risk Score	5.70* (2,548)	5.88 (11,749)	6.25* (640)	5.49 (1,803)	6.22 (336)	6.28 (310)
Substance Abuse Problems (Alcohol Only) (0/1)	.03 (2,508)	.03 (11,681)	.01* (635)	.03 (1,769)	.01 (335)	.02 (306)
Substance Abuse Problems (Drugs) (0/1)	.68* (2,508)	.74 (11,681)	.68 (635)	.66 (1,769)	.69 (335)	.68 (306)
Mental Health Problems (0/1)	.51* (2,550)	.45 (11,749)	.41* (641)	.54 (1,804)	.40 (336)	.43 (311)

IQ	90.61 (2,550)	90.65 (11,748)		89.45* (641)	90.96 (1,804)	89.10 (336)	89.89 (311)
Religious Affiliation (0/1)	.35 (2,550)	.34 (11,749)		.26* (641)	.38 (1,804)	.27 (336)	.25 (311)

Note: * indicates that the differences are statistically significant at the .05 level.

Table 2. ITT estimates of relocation

	(1) Rearrest	(2) Reincarceration	(3) Violent/Sex crime rearrest	(4) Reincarceration following rearrest
3 months	-0.031** (0.015)	0.012 (0.023)	-0.011 (0.008)	-0.000 (0.007)
6 months	-0.049* (0.025)	-0.012 (0.033)	-0.041** (0.014)	-0.028* (0.016)
9 months	-0.064* (0.034)	0.021 (0.039)	-0.047** (0.019)	-0.066** (0.024)
12 months	-0.073* (0.039)	-0.003 (0.044)	-0.032 (0.022)	-0.094** (0.030)
15 months	-0.053 (0.045)	-0.001 (0.050)	-0.029 (0.027)	-0.106** (0.035)
18 months	-0.035 (0.053)	-0.041 (0.055)	-0.040 (0.031)	-0.126** (0.040)
21 months	-0.041 (0.061)	-0.073 (0.063)	-0.047 (0.035)	-0.104** (0.046)
24 months	-0.046 (0.077)	-0.007 (0.077)	-0.064 (0.045)	-0.103* (0.058)

Notes: The estimates are from linear probability models adjusted for the covariates in Table 1 and indicator variables for the eligibility determination year. Age at the time of eligibility determination is used instead of age at release, as prison release occurs after randomization. Standard errors in parentheses are robust for heteroscedasticity. * $p < .1$; ** $p < .05$.

Table 3a. ITT estimates of relocation by age

	Rearrest						
	Age I	Age II	Age III	Age IV	Age II – Age I	Age III – Age I	Age IV – Age I
3 months	0.019 (0.032)	-0.053* (0.032)	-0.022 (0.029)	-0.077** (0.028)	-0.072 (0.045)	-0.041 (0.044)	-0.096** (0.044)
6 months	-0.046 (0.058)	-0.051 (0.045)	-0.014 (0.056)	-0.102** (0.041)	-0.005 (0.073)	0.031 (0.081)	-0.056 (0.070)
9 months	-0.008 (0.081)	-0.074 (0.051)	-0.058 (0.071)	-0.131** (0.055)	-0.066 (0.095)	-0.049 (0.108)	-0.123 (0.097)
12 months	0.002 (0.089)	-0.063 (0.074)	-0.100 (0.076)	-0.150** (0.071)	-0.065 (0.115)	-0.102 (0.118)	-0.152 (0.113)
	Reincarceration						
3 months	0.073 (0.051)	0.036 (0.045)	-0.010 (0.050)	-0.055 (0.042)	-0.037 (0.067)	-0.083 (0.073)	-0.128* (0.066)
6 months	0.073 (0.065)	-0.087 (0.075)	0.027 (0.065)	-0.084 (0.062)	-0.161 (0.099)	-0.046 (0.094)	-0.158* (0.090)
9 months	0.160** (0.080)	-0.010 (0.085)	0.043 (0.074)	-0.122 (0.076)	-0.170 (0.117)	-0.118 (0.110)	-0.282** (0.110)
12 months	0.085 (0.091)	-0.006 (0.092)	0.004 (0.082)	-0.100 (0.086)	-0.091 (0.129)	-0.082 (0.124)	-0.185 (0.126)
	Violent/ Sex crime rearrest						
3 months	0.001 (0.017)	0.015 (0.014)	-0.017 (0.022)	-0.028 (0.017)	-0.003 (0.017)	-0.018 (0.028)	-0.029 (0.024)
6 months	-0.040 (0.030)	-0.002 (0.007)	-0.063* (0.036)	-0.055** (0.027)	0.037 (0.032)	-0.023 (0.047)	-0.015 (0.039)
9 months	-0.026 (0.053)	-0.007 (0.009)	-0.094** (0.044)	-0.056* (0.029)	0.019 (0.053)	-0.068 (0.069)	-0.030 (0.059)
12 months	-0.014 (0.058)	0.017 (0.027)	-0.082* (0.046)	-0.041 (0.040)	0.031 (0.063)	-0.068 (0.077)	-0.027 (0.069)
	Reincarceration following rearrest						
3 months	0.028 (0.017)	-0.008 (0.019)	0.001 (0.004)	-0.022 (0.018)	-0.035 (0.026)	-0.027 (0.018)	-0.049** (0.024)
6 months	-0.033 (0.041)	-0.060** (0.029)	0.022 (0.034)	-0.047* (0.025)	-0.027 (0.051)	0.055 (0.054)	-0.014 (0.047)
9 months	-0.039 (0.062)	-0.106** (0.040)	-0.004 (0.044)	-0.126** (0.043)	-0.067 (0.073)	0.035 (0.077)	-0.087 (0.075)
12 months	-0.086 (0.072)	-0.108** (0.052)	-0.036 (0.060)	-0.166** (0.050)	-0.022 (0.086)	0.050 (0.095)	-0.081 (0.088)

Notes: Age I: < 27.64; Age II: >= 27.64 & < 33.03; Age III >= 33.03 & < 40.80; Age IV: >= 40.80. The estimates are from linear probability models adjusted for the covariates in Table 1 and indicator variables for the eligibility determination year. Standard errors in parentheses are robust for heteroscedasticity. * $p < .1$; ** $p < .05$.

Table 3b. ITT estimates of relocation by marital status

	Rearrest		
	Married	Not married	Married – Not married difference
3 months	-0.034 (0.050)	-0.031* (0.016)	-0.002 (0.054)
6 months	-0.103 (0.067)	-0.043 (0.027)	-0.061 (0.072)
9 months	-0.159* (0.087)	-0.053 (0.036)	-0.105 (0.095)
12 months	-0.147 (0.115)	-0.065 (0.042)	-0.082 (0.124)
	Reincarceration		
3 months	-0.064 (0.068)	0.022 (0.025)	-0.086 (0.073)
6 months	-0.092 (0.097)	-0.001 (0.035)	-0.091 (0.105)
9 months	-0.159 (0.113)	0.099 (0.042)	-0.205* (0.121)
12 months	-0.184 (0.129)	0.023 (0.046)	-0.207 (0.137)
	Violent/Sex crime rearrest		
3 months	-0.011 (0.040)	-0.011 (0.008)	0.000 (0.041)
6 months	-0.049 (0.054)	-0.040** (0.013)	-0.009 (0.055)
9 months	-0.069 (0.065)	-0.045** (0.020)	-0.024 (0.068)
12 months	-0.044 (0.083)	-0.030 (0.023)	-0.014 (0.087)
	Reincarceration following rearrest		
3 months	-0.025 (0.032)	0.003 (0.007)	-0.028 (0.032)
6 months	-0.031 (0.053)	-0.028* (0.017)	-0.003 (0.057)
9 months	-0.067 (0.074)	-0.067** (0.026)	0.000 (0.079)
12 months	-0.083 (0.094)	-0.096** (0.031)	0.013 (0.099)

Notes: The estimates are from linear probability models adjusted for the covariates in Table 1 and indicator variables for the eligibility determination year. Age at the time of eligibility determination is used instead of age at release, as prison release occurs after randomization. Standard errors in parentheses are robust for heteroscedasticity. * $p < .1$; ** $p < .05$.

Table 3c. ITT estimates of relocation by visitation

	Rearrest		
	Visited	Not visited	Visited – Not visited difference
3 months	0.000 (0.018)	-0.076** (0.025)	0.076** (0.031)
6 months	-0.025 (0.033)	-0.086** (0.039)	0.061 (0.051)
9 months	-0.040 (0.044)	-0.103** (0.051)	0.063 (0.067)
12 months	-0.077 (0.053)	-0.071 (0.058)	-0.007 (0.079)
	Reincarceration		
3 months	0.029 (0.026)	-0.011 (0.042)	0.039 (0.050)
6 months	-0.004 (0.040)	-0.024 (0.055)	0.020 (0.068)
9 months	0.033 (0.049)	0.004 (0.064)	0.029 (0.080)
12 months	0.012 (0.055)	-0.025 (0.070)	0.037 (0.088)
	Violent/Sex crime rearrest		
3 months	0.003 (0.009)	-0.031** (0.015)	0.034* (0.018)
6 months	-0.029 (0.018)	-0.058** (0.022)	0.029 (0.029)
9 months	-0.049* (0.027)	-0.046* (0.025)	-0.003 (0.037)
12 months	-0.028 (0.031)	-0.037 (0.030)	0.009 (0.042)
	Reincarceration following rearrest		
3 months	0.011 (0.008)	-0.016 (0.015)	0.027 (0.017)
6 months	0.002 (0.023)	-0.070** (0.023)	0.072** (0.033)
9 months	-0.033 (0.032)	-0.115** (0.035)	0.081* (0.048)
12 months	-0.050 (0.039)	-0.157** (0.044)	0.106* (0.059)

Notes: The estimates are from linear probability models adjusted for the covariates in Table 1 and indicator variables for the eligibility determination year. Age at the time of eligibility determination is used instead of age at release, as prison release occurs after randomization. Standard errors in parentheses are robust for heteroscedasticity. * $p < .1$; ** $p < .05$.

Table 3d. ITT estimates of relocation by substance abuse status

	Rearrest		
	Substance abuse	No substance abuse	Substance abuse-No substance abuse difference
3 months	-0.047** (0.020)	0.002 (0.020)	-0.049* (0.029)
6 months	-0.073** (0.028)	-0.000 (0.050)	-0.073 (0.057)
9 months	-0.093** (0.039)	-0.006 (0.061)	-0.087 (0.072)
12 months	-0.111** (0.047)	0.003 (0.069)	-0.113 (0.083)
	Reincarceration		
3 months	0.008 (0.028)	0.020 (0.041)	-0.012 (0.049)
6 months	-0.025 (0.040)	0.015 (0.056)	-0.040 (0.068)
9 months	-0.003 (0.048)	0.076 (0.066)	-0.080 (0.081)
12 months	-0.005 (0.053)	-0.00 (0.075)	-0.005 (0.090)
	Violent/Sex crime rearrest		
3 months	-0.011 (0.011)	-0.012 (0.010)	0.000 (0.016)
6 months	-0.042** (0.018)	-0.038* (0.021)	-0.005 (0.028)
9 months	-0.050** (0.023)	-0.043 (0.031)	-0.007 (0.037)
12 months	-0.045 (0.028)	-0.003 (0.031)	-0.042 (0.040)
	Reincarceration following rearrest		
3 months	-0.004 (0.010)	0.009 (0.010)	-0.013 (0.014)
6 months	-0.044** (0.018)	0.005 (0.029)	-0.049 (0.033)
9 months	-0.109** (0.028)	0.028 (0.041)	-0.137** (0.049)
12 months	-0.140** (0.036)	0.007 (0.049)	-0.147** (0.060)

Notes: The estimates are from linear probability models adjusted for the covariates in Table 1 and indicator variables for the eligibility determination year. Age at the time of eligibility determination is used instead of age at release, as prison release occurs after randomization. Standard errors in parentheses are robust for heteroscedasticity. * $p < .1$; ** $p < .05$.

Table 4. Estimates of relocation by time spent (all lengths of time are in months)

Follow-up	Recidivism-free	Time spent	Estimates
			Rearrest
3 months	2	<1	-0.001 (0.035)
3 months	2	>=1, <2	-0.041** (0.019)
6 months	2	<1	0.008 (0.073)
6 months	2	>=1, <2	-0.099** (0.030)
6 months	3	<1	-0.011 (0.065)
6 months	3	>=1, <2	-0.069** (0.029)
6 months	3	>=2, <3	-0.017 (0.040)
6 months	4	<1	0.014 (0.064)
6 months	4	>=1, <2	-0.049* (0.026)
6 months	4	>=2, <3	0.004 (0.038)
6 months	4	>=3, <4	-0.035* (0.021)
6 months	5	<1	-0.023 (0.017)
6 months	5	>=1, <2	-0.020 (0.015)
6 months	5	>=2, <3	0.005 (0.025)
6 months	5	>=3, <4	-0.012 (0.014)
6 months	5	>=4, <5	0.068 (0.081)
			Reincarceration
Follow-up	Recidivism-free	Time spent	Estimates
3 months	2	<1	-0.044** (0.018)
3 months	2	>=1, <2	-0.041** (0.019)
6 months	2	<1	-0.145** (0.046)
6 months	2	>=1, <2	-0.045 (0.061)
6 months	3	<1	-0.113** (0.046)

6 months	3	$\geq 1, < 2$	-0.095** (0.033)
6 months	3	$\geq 2, < 3$	0.010 (0.063)
6 months	4	< 1	-0.066 (0.043)
6 months	4	$\geq 1, < 2$	-0.050* (0.027)
6 months	4	$\geq 2, < 3$	0.030 (0.055)
6 months	4	$\geq 3, < 4$	-0.019 (0.051)
6 months	5	< 1	-0.024 (0.019)
6 months	5	$\geq 1, < 2$	-0.021 (0.016)
6 months	5	$\geq 2, < 3$	0.038 (0.049)
6 months	5	$\geq 3, < 4$	0.023 (0.050)
6 months	5	$\geq 4, < 5$	-0.011 (0.021)

Table 5. TOT estimates of relocation

	(1) Rearrest	(2) Reincarceration	(3) Violent/Sex crime rearrest	(4) Reincarceration following rearrest
3 months	-0.040** (0.018)	0.015 (0.029)	-0.014 (0.010)	-0.000 (0.009)
6 months	-0.059** (0.030)	-0.015 (0.039)	-0.050** (0.017)	-0.034* (0.019)
9 months	-0.078* (0.040)	0.025 (0.047)	-0.058** (0.023)	-0.081** (0.029)
12 months	-0.090* (0.047)	-0.004 (0.053)	-0.039 (0.027)	-0.115** (0.036)
15 months	-0.066 (0.054)	-0.001 (0.060)	-0.036 (0.032)	-0.132** (0.042)
18 months	-0.042 (0.061)	-0.049 (0.064)	-0.047 (0.036)	-0.151** (0.046)
21 months	-0.050 (0.070)	-0.090 (0.074)	-0.056 (0.040)	-0.127** (0.054)
24 months	-0.055 (0.085)	-0.008 (0.085)	-0.077 (0.050)	-0.122* (0.064)

Notes: The estimates are from two-stage least squared models adjusted for the covariates in Table 1 and indicator variables for the eligibility determination year. Age at the time of eligibility determination is used instead of age at release, as prison release occurs after randomization. Standard errors in parentheses are robust for heteroscedasticity. * $p < .1$; ** $p < .05$.

Appendix A: Consent form questions

Question 1: Which area did you live in before your current incarceration that you consider to be home? Please only pick one.

- Philadelphia
- Dauphin (Harrisburg area)
- Allegheny (Pittsburgh area)
- Somewhere else

Question 2: If you are paroled to a Community Corrections Center, would you be willing to go to a center in a county in Pennsylvania that is different from the county where you are from (your answer to Question 1)? For example, if you are from the Harrisburg area, you may go to a center in Philadelphia. It's important that your answer should NOT be based on your personal preference for any particular place, such as another place where you have family or friends.

- YES, I would be willing to go to a Community Corrections Center different from my home area in Pennsylvania
- NO, I would not be willing to go to a Community Corrections Center different from my home area in Pennsylvania

Appendix B: List of covariates and coding/descriptions

Category	Variable	Coding/Description
Age	Age at Release	Age when released from prison
	Age at Eligibility Determination	Age when CRU determines relocation eligibility
Race	White	1 White; 0 Otherwise
Marital Status	Married	1 Married; 0 Otherwise
Children	Children	1 Any children; 0 Otherwise
Education	High School Graduate	1 Completed Grade 12 or equivalent; 0 Otherwise
Criminal/ Misconduct History	Number of Prior Arrests	Number of arrests prior to the current incarceration
	No Prior Incarcerations	1 No previous incarcerations in PA; 0 Otherwise
Current Incarceration	Number of Misconducts	Number of all prior institutional misconducts in PA
	Time Served (years)	Numbers of years spent on the current incarceration
	Commitment Crime: Violent	1 Violent crime; 0 Otherwise
	Commitment Crime: Sex Crime	1 Sex crime; 0 Otherwise
	Commitment Crime: Property	1 Property crime; 0 Otherwise
	Commitment Crime: Drugs	1 Drug crime; 0 Otherwise
	Commitment Crime: Public Order	1 Public order crime; 0 Otherwise
	Court Commitment	1 Current incarceration is court commitment; 0 Otherwise
	Visitation	1 Visited at least once; 0 Otherwise
	Risk Score	PADOC Risk Screen Tool (RST) score (0-9)
Assessment	Substance Abuse Problems (Alcohol Only)	Assessed by DOC's Drug and Alcohol Treatment Specialists; 1 Alcohol problem only; 0 Otherwise
	Substance Abuse Problems (Drugs)	Assessed by DOC's Drug and Alcohol Treatment Specialists; 1 Drug problem; 0 Otherwise
	Mental Health Problems	1 Mental health problem (history or current); 0 Otherwise
	STG	1 Security Threat Group affiliation; 0 Otherwise
	IQ	IQ score
	Religious Affiliation	1 Any religious affiliation; 0 Otherwise