

# RESIDENTIAL CHANGE AS A TURNING POINT IN THE LIFE COURSE OF CRIME: DESISTANCE OR TEMPORARY CESSATION?\*

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*Many former prisoners return home to the same residential environment, with the same criminal opportunities and criminal peers, where they resided before incarceration. If the path to desistance from crime largely requires knifing off from past situations and establishing a new set of routine activities, then returning to one's old environment and routines may drastically limit an ex-prisoner's already dismal chances of desisting from crime. This study tests these ideas by examining how forced residential migration caused by Hurricane Katrina affected the likelihood of reincarceration among a sample of ex-prisoners originally from New Orleans, LA. Property damage from the hurricane induced some ex-prisoners who otherwise would have moved back to their former neighborhoods to move to new neighborhoods. Findings from an instrumental variables survival analysis reveal that those parolees who moved to a new parish following release were substantially less likely to be reincarcerated during the first 3 years after release than those ex-offenders who moved back to the parish where they were originally convicted. Moreover, at no point in the 3-year time period was the hazard of reincarceration greater for those parolees who moved than for those who returned to the same parish.*

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According to John Laub and Robert Sampson (2003: 149), offenders desist from crime in response to structurally induced turning points, such as work, marriage, and military service. Such turning points serve as catalysts for a sustained behavioral change by providing an opportunity for individuals to knife off from their past—to separate from the former contexts, situations, and criminal associates that facilitated their criminal behavior.

If criminal behavior is inextricably tied to social context, then by separating individuals from those contexts associated with their previous criminality, residential change may be one way to reduce offending and foster desistance. Recent research buttresses this point. In prior research that serves as a foundation for the present study, I used the neighborhood destruction in New Orleans, LA from Hurricane Katrina as a natural experiment to investigate the effects of residential change on recidivism among ex-prisoners during the first year after release (Kirk, 2009). I found substantial reductions in the rates of reincarceration among ex-offenders who moved away from their former parishes. In fact, for every 10 miles parolees moved, the risk of reimprisonment dropped by 1 percent. Sharkey and Sampson (2010) found that among Chicago adolescents who moved to a different neighborhood within the city, the likelihood of violent offending increased. However, moving outside of Chicago reduced violent behavior among adolescents.<sup>1</sup> Interestingly, the characteristics of destination neighborhoods, such as racial-ethnic composition and poverty, failed to explain these changes. Sharkey and Sampson (2010: 671) suggested that moving beyond the city helps reduce violence, in part, because of the physical distance from old neighborhoods and because it means severing ties to those city institutions—namely, the Chicago Public Schools—that may facilitate criminal behavior.

Still unresolved in the prior research is whether the benefits of residential change are permanent or fleeting. For instance, during the first few months after release, moving may make it harder for ex-offenders to gain access to criminal opportunities than if they had returned to their old neighborhoods. Yet it is likely that they would eventually uncover criminal opportunities, especially if they were residing in a neighborhood bereft of social controls and legitimate opportunities for work. Likewise, although moving may fragment individuals' ties to criminal peers, they may soon meet new

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1. This distinction between the effects of suburban versus intracity moves is consistent with research from the Gautreaux housing mobility program, a court-ordered residential desegregation program that resulted from a 1976 U.S. Supreme Court ruling against the U.S. Department of Housing and Urban Development (Sharkey and Sampson, 2010). Male youths from the Gautreaux families who moved to the Chicago suburbs were less likely to be arrested for drug, theft, and violent offenses than male youths who moved internally within Chicago (Keels, 2008).

criminal peers. They also may move back to their former neighborhoods and may reunite with criminal peers. Finally, scrutiny by law enforcement may initially be minimal when an ex-offender moves to a new area, thereby lessening the likelihood of sanctioning even if an offender is committing the same number of crimes in a new neighborhood as he or she would in the old one. Eventually, though, police may become aware of his or her reputation or transgressions and increase their scrutiny of the ex-offender's behavior. Empirically, then, any gap in the likelihood of reincarceration during the first year after release between ex-prisoners who return to home neighborhoods and those who move may evaporate over time.

This study therefore asks the following question: Does residential change provide a temporary cessation to criminal behavior, or does it serve as a catalyst for desistance and true behavioral change? As in my prior work (Kirk, 2009), this study uses a natural experiment made possible by Hurricane Katrina to gauge the effect of residential change on criminal behavior, while minimizing the potential for selection bias. The destruction of many neighborhoods in the New Orleans metropolitan area induced some ex-prisoners to move to new parishes who otherwise would have moved back to their old residences had it not been for the hurricane. Therefore, to estimate the effect of residential change, I can compare reincarceration rates among those parolees induced to move with their counterparts pre-Katrina who did not move.

In contrast to my previous work (Kirk, 2009), this study looks longer term. By using a survival analysis, it estimates the effect of moving on the hazard of reincarceration through 3 years after release from prison. Abbott (2001: 245) contended that "what makes a turning point a turning point rather than a minor ripple is the passage of sufficient time 'on a new course' such that it becomes clear that direction has indeed been changed." Therefore, to determine more conclusively whether residential migration caused by Hurricane Katrina produced turning points in the life course of crime for former prisoners, it is necessary to follow the same individuals over time, in this case, 3 years.

## RESIDENTIAL CHANGE AS A TURNING POINT

Sampson and Laub's (2005) work highlighted several commonalities among turning points that foster desistance from crime. They (2005: 17-8) noted:

What appears to be important about institutional or structural turning points is that they all involve, to varying degrees, (1) new situations that "knife off" the past from the present, (2) new situations that

provide both supervision and monitoring as well as new opportunities of social support and growth, (3) new situations that change and structure routine activities, and (4) new situations that provide the opportunity for identity transformation . . . [I]nvolvement in institutions such as marriage, work, and the military reorders short-term situational inducements to crime and, over time, redirects long-term commitments to conformity.

At a theoretical level, residential change would seem to fit these four commonalities. First, as prior research has suggested, residential moves may be turning points in the life course of crime, but the moves must include enough physical separation to allow an individual truly to “knife off” from his or her criminal past and prior criminal associates (Kirk, 2009; Sampson, 2008; Sharkey and Sampson, 2010). In contrast, moves in the housing mobility demonstration, Moving to Opportunity (MTO), often were of a short distance (Sampson, 2008).<sup>2</sup> Findings from MTO reveal that over time male youths who moved showed a greater likelihood of engaging in problem behavior than control group youths (Kling, Ludwig, and Katz, 2005). Perhaps MTO did not provide enough of a catalyst for behavioral change because the new residences were generally close enough to former neighborhoods that criminogenic social networks were not fragmented and the daily routines of the movers did not change appreciably (Sampson, 2008).

Second, residential change may produce new opportunities for supervision, monitoring, and social support. For instance, separating from a prior neighborhood context and criminal associates may provide a constructive environment for a parolee to repair the familial social bonds that are crucial to desistance.

Third, residential change may restructure routine activities through several means. Individuals may move to more socioeconomically advantaged areas than where they resided previously, thus making structured activities

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2. MTO is a program sponsored by the U.S. Department of Housing and Urban Development, which was initiated in 1994 in Baltimore, Boston, Chicago, Los Angeles, and New York (Katz, Kling, and Liebman, 2001; Kling, Liebman, and Katz, 2007). The question driving the MTO studies is whether an individual would behave differently, in terms of crime and other individual outcomes, if he or she lived in a nonpoor neighborhood instead of a poor neighborhood. MTO families were randomly assigned to one of three groups: 1) an experimental group, who received a housing voucher that had to be used in areas with less than 10 percent poverty as well as relocation assistance; 2) a Section 8 comparison group, who received a geographically unrestricted housing voucher but did not receive relocation assistance; and 3) a control group that received no change in housing assistance. Researchers used a comparison of individual behaviors across these three groups to make claims about neighborhood effects.

such as employment more likely. Additionally, criminal opportunities may be nearly ubiquitous, even in destination locations, but moving may provide a short-term disruption in an individual's access to them. This disruption may be enough for an ex-offender to think twice about engaging in criminal conduct once an opportunity does develop. Relatedly, separating drug addicts from environments associated with previous drug use may be particularly important for altering behavioral patterns (Kirk, 2009; Vaillant, 1988). Indeed, the self-help principles underlying programs such as Narcotics Anonymous and Alcoholics Anonymous stress the necessity of avoiding the people and places associated with previous substance use in order to abstain (see Leverentz, 2010; Maruna and Roy, 2007). It is easily conceivable that many formerly incarcerated individuals, because of limited housing opportunities (Travis, 2005), will end up residing in neighborhoods with ready access to drugs. Yet residential change may provide the foundation for true behavioral change for addicts by providing an alternative set of daily routines and situations.

Finally, residential change may provide an opportunity for identity transformation. Giordano, Cernkovich, and Rudolph contended (2002: 1000; see also Maruna, 2001) that "[t]he environment can thus provide a kind of scaffolding that makes possible the construction of significant life changes," including a new identity.

Clifford Shaw's (1966) life history of "Stanley" in *The Jack-Roller* is a good example of the link between residential change and identity transformation. Stanley had long struggled with feelings of inferiority as a result of the "criminal" label he had so deeply internalized. Indeed, shortly before his release from incarceration at 17 years of age, Stanley despaired that he "had lost all hope of regeneration" (Shaw, 1966: 160). What helped pave the way for his identity transformation from a delinquent boy into a more conventional, law-abiding adult was a change in peer associations facilitated by a new area of residence, gainful employment at a hospital laboratory, and the nurturing he received from his maternal landlady. After his spell of incarceration at 17 years of age, Stanley moved 4 to 5 miles away from where he grew up and approximately 9 miles from where he engaged in much delinquency. His former neighborhoods were close enough so that severing ties with his delinquent peers was a somewhat gradual process, but they were far enough away to make it possible to establish new group relationships and routine activities.

In sum, residential change would seem to satisfy many of the consequential aspects of turning points, particularly the necessity of knifing off from past contexts and changing routine activities. The mere act of moving is not likely as important as the degree of move: The more separation an individual has from his or her criminal past, the better. Thus, moving next door may have little effect on behavioral change, but moving to an entirely

different county or parish may allow an individual truly to knife off from the past and therefore lead to long-term behavioral change.

## THE SOCIOECONOMIC CONTEXT OF PRISONER REENTRY

The sparse but growing body of research on the neighborhood context of prisoner reentry reveals that contextual characteristics of reentry destinations, such as neighborhood disadvantage, resource deprivation, and a lack of access to social service providers, influence the likelihood of recidivism. Kubrin and Stewart (2006) found that ex-offenders in Oregon who resided in disadvantaged neighborhoods were significantly more likely to be rearrested within 12 months than were those who resided in more affluent neighborhoods. Mears et al. (2008) found a positive association between county-level resource deprivation and reconviction from violent offending among male offenders released from Florida prisons. Hipp, Petersilia, and Turner (2010) found that multiple dimensions of neighborhood context predicted reincarceration among a sample of California parolees, including concentrated disadvantage and the number of social service providers within 2 miles of parolees.

In this study, I seek to determine whether residential change has an effect on recidivism net of the characteristics of destination (i.e., postrelease) locations. If residential change leads to a lesser likelihood of reincarceration, the question is whether it is because the destination locations of movers have relatively more socioeconomic resources (e.g., jobs and social service providers) relative to where the stayers reside, and/or because residential change facilitates knifing off the past and acquiring new situations, regardless of whether movers are residing in resourced areas. By providing a fresh start, moving still may be beneficial for desistance even if the new location is disadvantaged and under-resourced.

## THE CURRENT STUDY

This study tests the following relationships between residential change and reincarceration:

*Hypothesis 1:* A change of residence can serve as a turning point in the life course of crime, thereby lessening the likelihood of reincarceration for parolees who reside in a geographic area different from where they resided before incarceration.

*Hypothesis 1A (Hazard):* The probability of reincarceration in each calendar-quarter, conditional on the fact that the parolee had

not yet been reincarcerated, will be lower for parolees who reside in a parish different from where they resided before incarceration (movers) than for parolees who returned to the same parish (stayers).

*Hypothesis 1B (Survival):* The cumulative risk of reincarceration during the first 3 years after release will be lower for movers than for stayers.

## DATA AND RESEARCH DESIGN

I drew the analytic sample from Louisiana prisoners originally convicted in the New Orleans metropolitan area—specifically Orleans, Jefferson, Plaquemines, St. Bernard, and St. Tammany parishes—which sustained substantial housing damage during Hurricane Katrina. In Orleans Parish, 72 percent of housing units suffered property damage from the hurricane (U.S. Department of Housing and Urban Development, 2006). In St. Bernard Parish and Plaquemines Parish, 80 percent of housing units were damaged, whereas 70 percent were damaged in St. Tammany Parish, and 53 percent were damaged in Jefferson Parish.

Sampled prisoners were released from Louisiana correctional facilities in two separate time periods. A first cohort ( $n = 1,538$ ) comprises all releases from a Louisiana prison to parole supervision between September 2001 and February 2002. This time period allows for a 3-year observation window of reincarceration that still ends before Hurricane Katrina. A second cohort ( $n = 1,370$ ) consists of all parolees released immediately after Hurricane Katrina (from September 2005 to February 2006). The sample used in this analysis is the same as used in my prior examination of the 1-year recidivism rate (Kirk, 2009), with one exception. I previously included an additional cohort, released in 2003 and 2004. That middle cohort is excluded from the present analysis of the 3-year reincarceration rate because the 3-year observation window overlaps with Hurricane Katrina.

Individual-level data on parolees come from administrative records maintained by the Louisiana Department of Public Safety & Corrections and the Division of Probation and Parole (DPP). These data provide information on the reincarceration—the dependent variable—of parolees in Louisiana state facilities and in local, other state, and federal facilities. Information on non-Louisiana incarcerations, as well as on incarcerations in a local facility, is captured in the DPP case management system in a field for supervision level. The dependent variable represents the hazard of reincarceration for each quarter, from 1 to 12, after release.

The treatment condition is *residential change*, measured by a binary variable indicating whether parolees initially moved to a different parish

after incarceration from where they were originally convicted. This variable equals 0 if parolees returned to the same parish as where they were convicted and 1 if they moved to a different parish. I use interparish moves to measure residential change to ensure that, for those who moved, the new residential location is a considerable distance from their old neighborhood. This approach allows me to assess the effect of a substantial separation from one's old context.

Because I lack data on the extended residential histories of parolees, the focus in this study is on place of residence immediately after release from incarceration. It is possible and even likely that some parolees moved to another parish or back to their original parish during the course of their parole, if approved by their parole officer. Still, moving away from a former parish may serve as a catalyst for sustained behavioral change even if ex-offenders ultimately end up back in their old neighborhoods. Separation from prior contexts is arguably most crucial during the first few weeks after release when the challenges for ex-offenders are substantial (such as finding a job and housing, navigating social services, and finding substance abuse treatment providers) (Maruna and Roy, 2007; National Research Council, 2007).

Analyses also include six individual-level control variables designed to account for potential differences in prison releases pre/post-Katrina. I measure parolee race with a binary variable (Black equals 1, otherwise 0). Black parolees compose 75.1 percent of the sample, with Whites making up 24.7 percent. Other races make up .2 percent of the sample. I also include measures of gender (male equals 1), marital status, age at time of release, time served in prison, and first release. Males compose 87.3 percent of the sample, and 10.2 percent of the sample were married at the time of release. On average, prisoners were 33 years of age at the time of release. Averaged time served was 2.1 years. Controlling for time served is necessary to account for any differences between cohorts in the average severity of prior offending. *First release* is a binary variable indicating whether the parolee was released from his or her first term of incarceration (equals 1) or from a second or greater term (0). First releases account for 70.9 percent of the sample. I compared the pre/post-Katrina cohorts on all of these individual-level characteristics. Only for first releases (pre-Katrina: 76.3 percent first releases; post-Katrina: 64.7 percent) and race (pre-Katrina: 77.0 percent Black; post-Katrina: 72.9 percent) were there statistical differences across cohorts. I control for differences across cohorts in statistical models.

For the purposes of estimating the effect of residential change on reincarceration net of the socioeconomic conditions of destination parishes, I include the following statistical controls in analyses, all measured at the parish level at the time point in which a given parolee was released from prison: segregation, income inequality, the unemployment rate, average

weekly wages, and fair market rents. Measures of segregation (dissimilarity) and income inequality are drawn from annual sociodemographic estimates produced by Esri (2006). I use 2002 estimates for the pre-Katrina cohort and 2006 estimates for the post-Katrina cohort. Data on unemployment and wages come from the Louisiana Department of Labor, and they are measured during the calendar-quarter in which the parolee was released from prison. From data compiled by the U.S. Department of Housing and Urban Development, I use estimates of fair market rents in 2002 for the pre-Katrina cohort and in 2006 for the post-Katrina cohort. Figures are adjusted to year 2000 U.S. dollars. Because of sizable correlations among these aspects of socioeconomic context, I combined these five measures into one variable (*socioeconomic conditions*) through principal components analysis.

The implications of Hurricane Katrina for changes in the criminal justice system in Louisiana are many (Garrett and Tetlow, 2006; Roman, Irazola, and Osborne, 2007). For instance, because of the depopulation of New Orleans and the geographic shift in where parolees fresh out of prison resided after Katrina, an increase occurred in the ratio of parole officers to parolees in New Orleans (Louisiana Department of Public Safety & Corrections, 2005, 2007). Arguably, then, parolees in New Orleans after Katrina faced greater scrutiny than their pre-Katrina counterparts. Given that increased scrutiny is associated with higher recidivism rates (Turner and Petersilia, 1992), the increased ratio of parole officers to parolees post-Katrina may have resulted in higher levels of reincarceration. Therefore, using parish-level data from the Supreme Court of Louisiana, the Uniform Crime Reports (UCR) Program, and DPP, I developed pre- and post-Katrina measures of judge caseloads (in 2002 and 2006 for the pre- and post-Katrina cohorts, respectively), the likelihood of arrest after the commission of a crime (UCR arrests divided by reported crimes, based on 2001 and 2005 data for the respective cohorts), average parole contacts per parole officer during the quarter in which a parolee was released from prison, and the reincarceration rate in the parolee's parole district during the 6 months prior to when he or she was released from prison. I combined these measures via principal components analysis and labeled the component *criminal justice operations*.

## ANALYTIC STRATEGY

Conceptually, I seek to determine what would happen to the behavior of the same parolee under two different circumstances: he or she moved to a new parish or stayed in the same parish. Yet, in reality, it is only possible to observe one of these two potential outcomes for an individual at a given point in time (i.e., either the individual moved or did not). If, instead, one

simply compares reincarceration rates for movers and stayers, the estimate of the effect of moving may be biased because of omitted confounding variables related to the reasons why an individual moves. One solution to this problem of omitted variables is the use of instrumental variables (IVs).

#### INSTRUMENTAL VARIABLES AND A NATURAL EXPERIMENT

One key assumption of the IV framework is that the instruments and the outcome variable are unrelated, except through the treatment condition (Angrist and Krueger, 2001). This is known as the exclusion restriction. If we can find a variable that is 1) correlated with the treatment variable, yet is 2) uncorrelated with the dependent variable except through the treatment variable, we can use such a variable as an instrument. A common criticism of using IVs is that it is difficult to satisfy this second condition. However, we can have more confidence that the instrument and outcome are related only through the treatment if that instrument derives from a random force of nature like a hurricane. In this case, a variable representing the natural occurrence is used as a predictor (instrument) of the key explanatory variable (the treatment), and then the outcome variable is regressed on the *predicted* treatment measure. This approach removes the spurious correlation between the treatment variable and omitted variables. An IV remedies the issue of omitted variables by using only a portion of the variability in the treatment variable that is uncorrelated with omitted variables to estimate the causal relation between the treatment and the outcome (see Angrist and Krueger, 2001; Angrist and Pischke, 2009; Bushway and Apel, 2010; Kirk, 2011).<sup>3</sup>

In this study, I use five IVs to predict the treatment condition. These IVs represent the interaction of the time period of release from prison—pre- versus post-Hurricane Katrina—and parish of conviction. Because property damage from Hurricane Katrina varied by parish, the likelihood of a parolee returning to the same parish where he or she was originally convicted was influenced by the extent of housing destruction in the parish. The first instrument (pre- vs. post-Katrina release and Orleans conviction) equals 1 for all parolees who were released after Katrina *and* who were originally convicted in Orleans Parish. It equals 0 otherwise. I construct similar instruments for parolees originally convicted in Jefferson, Plaquemines, St. Bernard, and St. Tammany parishes, respectively. In sum, I assume that

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3. Per this discussion, I do not use Hurricane Katrina to conduct a longitudinal pre/post analysis; rather, I use the induced treatment caused by Katrina as a counterfactual scenario to answer the question of what would happen to a parolee's likelihood of recidivism if he moved to an entirely different parish instead of returning to his original parish as he would have done had it not been for the hurricane.

whether parolees migrate to a different parish depends on whether they were released from prison before or after Hurricane Katrina *and* where they were originally convicted. I also employ a sensitivity analysis that uses different instruments, which I describe in the subsequent section.

#### INTEGRATING INSTRUMENTAL VARIABLES AND SURVIVAL ANALYSIS

With recidivism rates so high in the United States, we know that most released offenders will end up back in confinement within just a few years (Langan and Levin, 2002). Therefore, just as it is important to investigate *whether* an individual will recidivate, *when* recidivism occurs is an interesting question in its own right. Survival models are a statistical method well suited to investigate such questions. My data file is structured as a person-period data set with up to 12 observations for each parolee to correspond to 12 quarters of observations during the 3-year follow-up period. If an offender recidivated prior to the end of 3 years, or if he or she successfully finished parole in less than 3 years without a reincarceration, then the data are censored and there will be no further observations in the data file beyond the quarter in which he or she was reincarcerated or exited parole.

Recall the hypothesis that the hazard of reincarceration will be lower for movers during every quarter of observation, and cumulatively over time. In contrast, a plausible alternative hypothesis is that there is initially a difference in the hazard of reincarceration between movers and stayers during the first year after incarceration, yet no difference after a year as movers become associated with new criminal peers, begin to draw the attention of the police in their new neighborhood, or even move back to their old neighborhoods. Crucially, then, survival models allow one to assess the potential for the timing of any effects of residential change. If residential change has only a temporary effect on reincarceration, any initial difference in the hazard of reincarceration between movers and stayers would disappear over time. In fact, if the relatively more recidivism-prone parolees are merely delayed in their reincarceration because of a residential change, then there may be a catch-up period in which the movers have a higher hazard of reincarceration than stayers (e.g., years 2 and 3) after initially having a lower hazard (year 1).<sup>4</sup> Those stayers most at risk of reincarceration would have recidivated in the first year, whereas movers

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4. This type of scenario might explain the results from the MTO experiment, where initially problem behavior was lower for males in the experimental group than in the control group. However, after 4 to 7 years, the experimental group had more problem behavior (Katz, Kling, and Liebman, 2001; Kling, Ludwig, and Katz, 2005). In this case, the hazard of problem behavior may have been lower for the experimental group soon after random assignment, but then it reversed course as time since randomization increased.

at greatest risk of recidivating may have simply delayed their eventual reincarceration until year 2. If true, this temporary delay arguably would demonstrate that residential change is not a turning point on the path to desistance.

For the first stage of the two-stage IV estimation, I model the treatment variable, residential change, as a function of the five instrumental variables  $Z_i$ , and a vector of control variables  $X_i$  measured at the point when the parolee was released from prison, where  $i$  references the parolee:

$$\text{ResChange}_i = \alpha_0 + \sum_{p=1}^P \beta_p Z_{pi} + \sum_{q=1}^Q \beta_q X_{qi} \quad (1)$$

Even though I use an exogenous natural occurrence to predict treatment assignment, in the absence of random assignment, it is not necessarily the case that treatment and control groups are identical. Descriptive results revealed cohort differences in the proportion of first releases and racial composition. Thus, it is beneficial to include statistical controls  $X_i$  for parolee characteristics to account for observed differences across groups. Also included as controls are measures of socioeconomic conditions and criminal justice operations. An assumption of the IV estimation is that Hurricane Katrina is exogenous to reincarceration because, after controlling for socioeconomic conditions and criminal justice operations, the hurricane is unrelated to reincarceration except through residential change (i.e., the exclusion restriction).

The second stage of the two-stage estimation process models the hazard of reincarceration at each quarter as a function of the predicted treatment variable from the first stage and a vector of control variables  $X_i$ . The subscript  $i$  references the parolee and  $j$  references the calendar-quarter:

$$\begin{aligned} \text{Reincarceration}_{ij} = & \alpha_0 + \alpha_1(\text{Quarter}_j - 1) + \alpha_2(\text{Quarter}_j - 1)^2 \\ & + \beta_1(\text{ResChange}_i) + \sum_{p=2}^P \beta_p X_{pi} \end{aligned} \quad (2)$$

In this equation, the linear and quadratic *Quarter* terms are centered on 1 to facilitate interpretation of the intercept; it is interpreted as the hazard of reincarceration for stayers during the first calendar-quarter after release from prison. Without centering, the intercept would refer to the hazard at quarter 0, which is not meaningful.

## RESULTS

Table 1 offers a simplified estimation, excluding control variables, of the effect of residential change on reincarceration. It is simplified because

**Table 1. Wald Estimator of the Difference in the 3-Year Reincarceration Rate between Movers and Stayers**

Cohort	Proportion Moved	Proportion Reincarcerated
Post-Katrina	.50	.47
Pre-Katrina	.25	.51
Difference	.25	-.04
Causal Estimate (standard error)		-.148 (.073)

NOTES:  $N = 2,908$ . The causal estimate of the difference in reincarceration is computed as follows:

$$\frac{E(\text{Reincarceration} | \text{Post-Katrina}) - E(\text{Reincarceration} | \text{Pre-Katrina})}{[E(\text{Moved} | \text{Post-Katrina}) - E(\text{Moved} | \text{Pre-Katrina})]}$$

I assume for this analysis that Hurricane Katrina is only related to reincarceration through its effect on residential change, not through alternative paths of socioeconomic and criminal justice changes. This analysis is meant to illustrate how the natural experiment can be used to estimate the effect of residential change.

As the table shows, 50 percent of the post-Katrina cohort moved to a new parish after release from prison compared with 25 percent of their pre-Katrina counterparts. This table illustrates that Hurricane Katrina induced an additional 25 percent of parolees to move. Given this exogenously induced shift, I am able to estimate the effect of migrating to a different parish on reincarceration for those 25 percent of parolees who otherwise would not have moved had it not been for Hurricane Katrina.

Table 1 also shows an unadjusted difference of 4 percent between the groups in the proportion reincarcerated. To estimate the causal effect of residential change on reincarceration, I adjust this difference by the difference between groups in moving, as shown in the second-to-last row in table 1. This is known as the Wald estimator, the simplest of the IV estimators (Angrist and Pischke, 2009). The Wald estimator is computed as follows:  $[E(Y|Z = 1) - E(Y|Z = 0)] / [E(S|Z = 1) - E(S|Z = 0)]$ , where  $Y$  is the dependent variable,  $Z$  is the instrument, and  $S$  is the treatment variable.<sup>5</sup> Findings reveal an estimated effect of residential change on reincarceration after

5. Although the proportions in table 1 can be computed without estimating a regression, to produce the standard error of the causal estimate in the table, I simply estimate the following bivariate regression in accord with the formula for the Wald estimator:  $E(Y|Z) = \alpha + \rho E(S|Z)$ .

3 years of .148; almost 15 percent more stayers are reincarcerated than those otherwise similar individuals who moved because of the hurricane.

#### TIME-INDEPENDENT EFFECTS OF RESIDENTIAL CHANGE

The next analysis estimates the full IV model shown in equations 1 and 2. The specification used in table 2 and figure 1 assumes that residential change has an identical effect in every calendar-quarter. This specification is termed the *proportionality assumption* because the odds of reincarceration for one group are proportional to the other group—i.e., the odds of reincarceration for stayers are a magnification of the odds for movers (Singer and Willett, 2003). I test the suitability of this assumption in the next section.

A common issue with IV analyses is what is known as the “weak instruments” problem. If a treatment variable and the instruments used in analyses are only weakly correlated (or completely unrelated), then the estimator will produce inconsistent IV estimates, such that the IV estimate of the causal effect will not be close to the true causal effect (Angrist and Pischke, 2009; Bound, Jaeger, and Baker, 1995). The first-stage coefficients in table 2 reveal that being released from prison post-Katrina (relative to pre-Katrina) is positively and significantly associated with migrating to a new parish for those offenders originally from Orleans, Jefferson, Plaquemines, and St. Tammany parishes (which compose 96 percent of the releases). Additionally, an *F* test reveals that the instruments are significantly correlated with the treatment variable ( $F = 32.52$ ; d.f. = 5, 50;  $p < .001$ ). Given the strong association between the instruments and the treatment variable, it is appropriate to proceed to results from the second stage of the two-step estimation.

The second model in table 2 presents IV probit results of the duration until reincarceration. Findings reveal that males are more likely to be reincarcerated than females. Age is negatively related to reincarceration, as is time-served in prison. First releases from prison are substantially less likely to recidivate than repeat offenders, which is not surprising given that offenders with multiple prior incarcerations have drastically higher rates of recidivism than first releases (e.g., Rosenfeld, Wallman, and Fornango, 2005). Consistent with recent research by Sharkey and Sampson (2010), who found that the socioeconomic conditions of destination neighborhoods fail to explain why violence declined among adolescents who moved outside of Chicago, the results in table 2 reveal that socioeconomic context has little direct influence on reincarceration (although, per the first-stage results, it does influence why people move).

Turning to the main finding of the study, the results reveal a significant, negative relationship between residential change and the hazard of

**Table 2. Two-stage IV Probit Estimates, Hazard of Reincarceration**

Variables	First Stage Predicting Treatment		Second Stage Predicting Reincarceration	
	Coefficient	Robust Standard Error	Coefficient	Robust Standard Error
Instruments				
Post-Katrina Release × Orleans	.620 ***	(.060)		
Post-Katrina Release × Jefferson	.380 ***	(.064)		
Post-Katrina Release × Plaquemines	.473 **	(.164)		
Post-Katrina Release × St. Bernard	.353	(.227)		
Post-Katrina Release × St. Tammany	.266*	(.123)		
Treatment				
Residential Change			-.206 **	(.067)
Time				
Quarter			.057 ***	(.015)
Quarter <sup>2</sup>			-.011 ***	(.002)
Individual Level				
Black	-.001	(.061)	.008	(.035)
Male	-.016	(.016)	.196 ***	(.043)
Married	.018	(.018)	-.003	(.045)
Age at Release	.000	(.001)	-.010 ***	(.002)
Time Served	.005*	(.003)	-.010*	(.006)
First Release	-.005	(.017)	-.213 ***	(.033)
Context and Criminal Justice System				
Jefferson Parish	.037	(.369)	-.118***	(.033)
Plaquemines Parish	.365	(.294)	-.065	(.143)
St. Bernard Parish	.250	(.455)	.044	(.091)
St. Tammany Parish	-.163	(.377)	-.137 * *	(.057)
Socioeconomic Conditions	-.215***	(.034)	-.014	(.019)
Criminal Justice Operations	.004	(.035)	.020	(.022)
Intercept	.183	(.208)	-.857 ***	(.080)

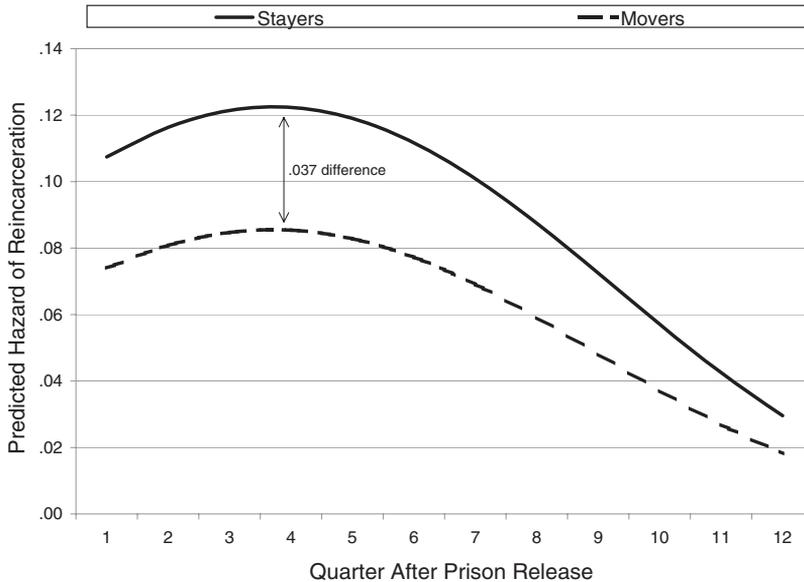
NOTES:  $N = 15,827$  (up to 12 calendar-quarter observations per parolee).

\* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$  (one-tailed test).

reincarceration.<sup>6</sup> Individuals who migrated to a different parish are substantially less likely to be reincarcerated than stayers.

6. The results in table 2 were estimated via a two-stage probit model. Rhodes (2010) observed that IV estimates provided by a two-step estimator may be inconsistent when estimated with censored data. The data used in this analysis are censored because reincarceration is observed only for a portion of the sample. Rhodes suggested an alternative to the two-step estimator to ensure consistency of the causal estimate when using censored data. In the additional supporting information (which can be found in the listing for this article in the Wiley Online Library at <http://onlinelibrary.wiley.com/doi/10.1111/crim.2012.50.issue-2/issuetoc>), I replicate the analysis from table 2 using this alternative strategy, and find that inferences are robust to whether I use Rhodes's strategy or the two-stage probit used for the model in table 2.

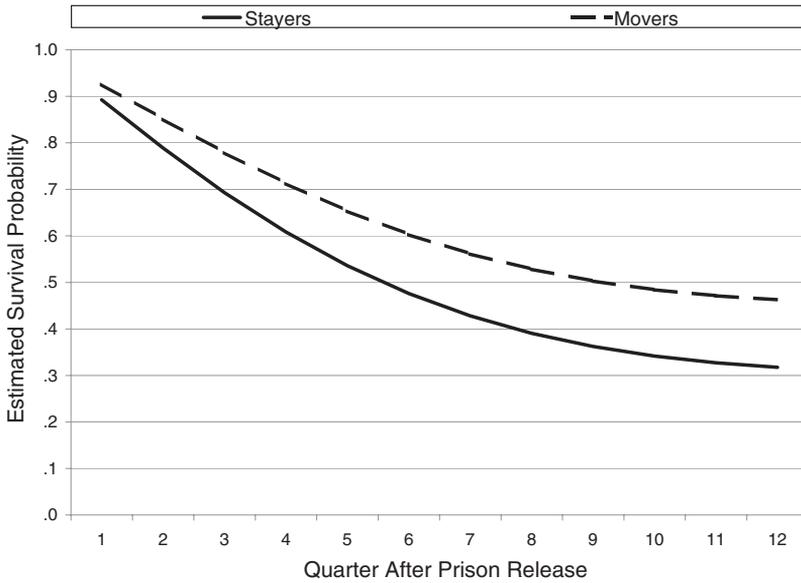
**Figure 1. Effect of Residential Change on the Hazard of Reincarceration**



Using coefficients from the second-stage results, figure 1 depicts the predicted hazard function for stayers (top curve) versus movers (bottom curve) during the first 3 years (by quarter) after release. The coefficients for the quarter-year indicators (added to the intercept) can be used to assess the hazard of reincarceration within each given quarter after prison release for stayers. I use the coefficient for the measure of *residential change* to assess the difference in the hazard of reincarceration between movers and stayers. All predictor variables besides time and the treatment condition are held at their mean values.

Figure 1 demonstrates that in the first quarter, the predicted probability of reincarceration for the average offender who moved is slightly more than .07. For those parolees who returned to the same parish after incarceration, the predicted probability of reincarceration is nearly .11. In accord with a long line of criminological research (e.g., Beck and Shipley, 1989; Langan and Levin, 2002; Schmidt and Witte, 1988), the hazard of reincarceration peaks within 1 year after release. Because of the proportionality assumption of this model, between quarters 3 and 4 is not only when the two curves peak, but also when the difference between the two groups peaks, with

**Figure 2. Estimated Survival, by Place of Residence After Incarceration**



a gap equal to .037. The predicted hazard of reincarceration is lowest for both groups in the last quarter when the difference between the curves is narrowest.

Figure 2 depicts the estimated survival rate for ex-offenders; in this case, “survival” refers to the ability of a parolee to avoid reincarceration. It illustrates the cumulative risk of reincarceration over the entire follow-up period. The estimated survival probability for a given time period is computed as the product of the estimated survival probability from the previous time period and 1 minus the estimated hazard probability for the current period:

$$\hat{S}(t_j) = \hat{S}(t_{j-1})[1 - \hat{h}(t_j)] \tag{3}$$

In support of hypothesis 1B, this analysis reveals that an estimated 46 percent of movers avoided reincarceration compared with 32 percent of stayers. Most reincarceration occurs within 1 year of prison release: 29 percent of movers and 39 percent of stayers are reincarcerated within 1 year (1 minus the survival probability for each group).

**Table 3. Time-Varying Effects of Residential Change, Hazard of Reincarceration**

Variables	Coefficient	Robust Standard Error
Treatment		
Residential Change	-.219*	(.099)
Residential Change × Quarter	.004	(.019)
Time		
Quarter	.056 * **	(.016)
Quarter <sup>2</sup>	-.011 * **	(.002)
Individual Level		
Black	.008	(.035)
Male	.196 * **	(.043)
Married	-.003	(.045)
Age at Release	-.010 * **	(.002)
Time Served	-.010*	(.006)
First Release	-.213 * **	(.033)
Context and Criminal Justice System		
Jefferson Parish	-.118 * **	(.033)
Plaquemines Parish	-.066	(.143)
St. Bernard Parish	.044	(.091)
St. Tammany Parish	-.137 * *	(.057)
Socioeconomic Conditions	-.014	(.019)
Criminal Justice Operations	.020	(.022)
Intercept	-.853 * **	(.086)

NOTES:  $N = 15,827$  (up to 12 calendar-quarter observations per parolee).

\* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$  (one-tailed test).

### TIME-INDEPENDENT VERSUS TIME-VARYING EFFECTS

The results in table 2 reveal that, on average across the 12 calendar-quarters, movers have a lesser likelihood of reincarceration than stayers. Because of model specification, these results do not demonstrate whether the effect changes with time. For example, the hazard curves in figure 1 may actually cross and reverse positions if the effect of residential change on reincarceration was allowed to vary with time. I now turn to an analysis that investigates whether the effect of residential change on reincarceration depends on how long someone has been out of prison.

Table 3 presents model coefficients from IV probit results of the duration until reincarceration, where the model now specifies a linear interaction between the treatment condition and time. It is the specification of this interaction that allows me to examine whether the effect of residential change varies over time. The results reveal a nonsignificant relationship between the interaction term and the hazard of reincarceration, with a coefficient of virtually zero. This finding suggests that the effect of residential change on recidivism does not vary with time. Thus, the results support the

**Table 4. Model Fit Statistics, Proportional and Nonproportional Hazard Models of Reincarceration**

Models	Main Effect	Interaction with Residential Change	BIC
Proportional Hazards			
1. General	12 Dummies	None	9,498.2
2. Linear	Quarter	None	9,465.5
3. Quadratic	Quarter + Quarter <sup>2</sup>	None	9,433.0
4. Cubic	Quarter + Quarter <sup>2</sup> + Quarter <sup>3</sup>	None	9,435.0
Nonproportional Hazards			
5. General	12 Dummies	12 Dummies	9,595.9
6. Linear/Linear	Quarter	Quarter	9,475.2
7. Quadratic/Linear	Quarter + Quarter <sup>2</sup>	Quarter	9,442.6
8. Quadratic/Quadratic	Quarter + Quarter <sup>2</sup>	Quarter + Quarter <sup>2</sup>	9,451.8
9. Cubic/Linear	Quarter + Quarter <sup>2</sup> + Quarter <sup>3</sup>	Quarter	9,444.7
10. Cubic/Quadratic	Quarter + Quarter <sup>2</sup> + Quarter <sup>3</sup>	Quarter + Quarter <sup>2</sup>	9,454.0
11. Cubic/Cubic	Quarter + Quarter <sup>2</sup> + Quarter <sup>3</sup>	Quarter + Quarter <sup>2</sup> + Quarter <sup>3</sup>	9,462.9

NOTES:  $N$  (for each model) = 15,827 (up to 12 calendar-quarter observations per parolee). The preference ordering of models is determined by the Bayesian Information Criterion (BIC) on the basis of predictive utility. The model with the lowest BIC is decided as the one having the best prediction-fit.

conclusion that the odds of reincarceration for movers are proportional to the odds for stayers (with the movers having a lower estimated hazard), and that the hazard functions for movers and stayers do not cross. I do not find evidence that the effect of moving on reincarceration disappears over time.

Of course, there are numerous alternative specifications for incorporating an interaction between time since release and the treatment condition (residential change). Table 4 presents model fit statistics across a variety of alternative specifications. I use the Bayesian Information Criterion (BIC; Schwarz, 1978) to compare model fit on the basis of predictive utility. The model with the lowest BIC has the best prediction-fit over the population of respondents. Table 4 reveals that model 3, which is the proportional hazard model that was presented in table 2, has the lowest BIC (9,433.0) of all the models specified and thus provides the best fit to the data. In no case does a nonproportional hazard model with an interaction between time and residential change provide a better fit to the data. In support of hypothesis 1A, moving does not merely prompt a temporary cessation in criminal behavior; rather, residential change seems to be a catalyst for true behavioral change.

## SENSITIVITY ANALYSIS

The results thus far demonstrate that residential migration has a substantial effect on desistance from crime. Yet, the macrolevel shock from Hurricane Katrina may have affected reincarceration by mechanisms other than residential migration. If we do not account for these other mechanisms, it is possible that the treatment effect may be misestimated. To account for the effect of Katrina on other factors related to reincarceration, the analysis in table 2 included controls for socioeconomic characteristics and the operation of the criminal justice system of postrelease locations. For instance, if the hurricane undermined the ability of parole officers to scrutinize the behavior of their parolees, then the likelihood of reincarceration may have declined after Katrina not because proportionally more parolees moved away from old parishes, but because parole practices changed. By including a control for criminal justice system operations, I have attempted to account for the possibility that the police, parole officers, and courts may have become less watchful of or punitive toward ex-offenders in the wake of Katrina.

Despite statistical controls for criminal justice operations and socioeconomic conditions, the possibility exists that there are additional, unmeasured paths by which Hurricane Katrina affected reincarceration, which could make the reported estimates too conservative. For instance, neighborhoods in parishes affected by Katrina may have become more socially disorganized after the hurricane, with the instability associated with the hurricane undermining the capacity of neighborhood residents to engage in informal social control. In this sense, we would expect that reincarceration would increase after the hurricane.

The results may, in contrast, have overestimated the treatment effect of migration. For instance, although I control for temporal changes in the *scrutiny* of parolees by parole officers (through the measure of criminal justice operations), I have no measure of changes in leniency post-Katrina. Yet, restricting analyses to observations from only the post-Katrina cohort may be one way to alleviate confounding from unmeasured pre-to-post-Katrina changes such as leniency.

I justify my use of Hurricane Katrina as an instrument because it was an exogenous source of variation that influenced where affected individuals live. Just as prisoners released from incarceration immediately after the hurricane were far less likely to return home to their old parishes than those released pre-Katrina, it also is true that those individuals released in the first month after Katrina were drastically more likely to change parishes than individuals released in the ensuing months. In September 2005, 69 percent of individuals released from prison who were originally convicted in the New Orleans metropolitan area moved to a new parish. By

October, 53 percent moved to a new parish, and by November, this number declined to 48 percent. In August 2006, 1 year after the hurricane, 46 percent of parolees moved to a new parish. In addition, substantial geographic variation occurred in the patterns of residence. Because property damage was not uniformly severe across the New Orleans metropolitan area, it was relatively easier for ex-prisoners to move back to some parishes than to others. Given these patterns, it is possible to exploit the geographic variation in property damage and variation in when offenders were released from prison to conduct an analysis of reincarceration solely for those individuals released in the first year after Hurricane Katrina. I attempt to control for the macro-level shock of Hurricane Katrina, and therefore unmeasured mechanisms that explain reincarceration, by conducting a sensitivity analysis with a sample composing only those parolees released from prison post-Katrina (September 2005 to August 2006).

Table 5 presents IV results based on a specification that uses an interaction between time period of release—the first month post-Katrina (September) versus 2–12 months post-Katrina—and parish of conviction to create five IVs. The first-stage results reveal that the five instruments are indeed predictive of residential change.<sup>7</sup> The combination of release just after the hurricane and parish of conviction thus predict whether a released offender returned to the parish where he or she was convicted. As in the previous analyses displayed in table 2, the second-stage results demonstrate that individuals who migrated to a different parish are significantly and substantially *less* likely to be reincarcerated.

Figure 3 depicts the estimated survival function based on the coefficients presented in table 5. The results reveal that an estimated 54 percent of movers avoided reincarceration during the first 3 years after their release from incarceration compared with 28 percent of stayers.

In sum, in an attempt to minimize bias from the fact that the shock of Hurricane Katrina may have affected reincarceration in more ways than just through residential migration patterns of parolees (such as by increasing social disorganization or by prompting greater leniency on the part of police or parole staff), I conducted a sensitivity analysis solely with ex-offenders released from prison post-Katrina. Presumably if Katrina had altered conditions conducive to reincarceration (either positively or negatively), then comparing movers and stayers released only post-Katrina is one way to reduce the potential confounding influence of unmeasured predictors related to the devastation from Hurricane Katrina. In both the

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7. To use instruments sufficiently strong to differentiate between movers and stayers, I exclude parish main effects (i.e., the four parish dummy variables) from the analysis. Conceptually then, I compare reincarceration between movers from specific parishes with the average stayer across all parishes.

**Table 5. Sensitivity Analysis, Hazard of Reincarceration, Post-Katrina Sample Only**

Variables	1st Stage Predicting Treatment		2nd Stage Predicting Reincarceration	
	Coefficient	Robust Standard Error	Coefficient	Robust Standard Error
Instruments				
Sept Release × Orleans	.320 **	(.133)		
Sept Release × Jefferson	.176	(.192)		
Sept Release × Plaquemines	.538 ***	(.097)		
Sept Release × St. Bernard	.339	(.265)		
Sept Release × St. Tammany	-.159	(.112)		
Treatment				
Residential Change			-.376*	(.209)
Time				
Quarter			.054 **	(.022)
Quarter <sup>2</sup>			-.011 ***	(.002)
Individual Level				
Black	.065	(.054)	.081*	(.048)
Male	.001	(.034)	.151 ***	(.037)
Married	.026	(.035)	-.031	(.058)
Age at Release	.000	(.001)	-.010 ***	(.002)
Time Served	.016 **	(.005)	-.004	(.005)
First Release	-.068*	(.033)	-.234 ***	(.037)
Context and Criminal Justice System				
Socioeconomic Conditions	.029	(.075)	.053 **	(.018)
Criminal Justice Operations	.180 ***	(.051)	.037	(.043)
Intercept	.383 ***	(.112)	-.858 ***	(.133)

NOTES:  $N = 15,141$  (up to 12 calendar-quarter observations per parolee).

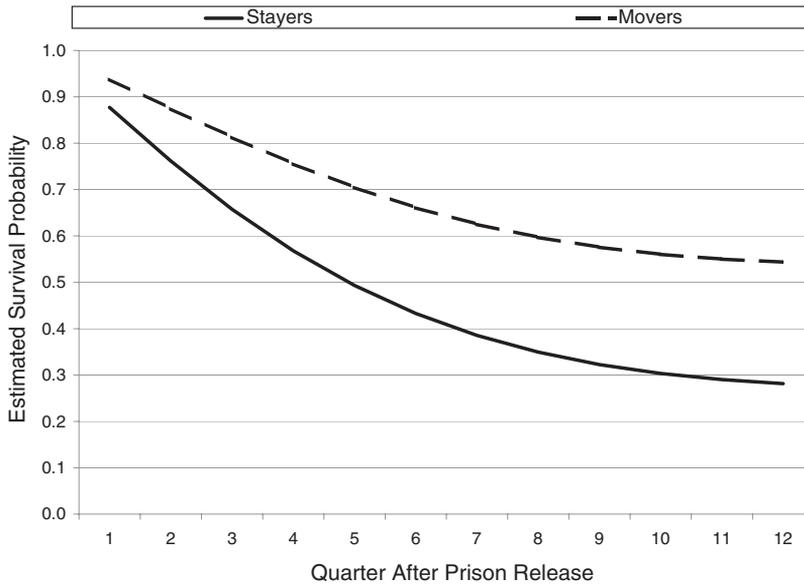
\* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$  (one-tailed test).

main and sensitivity analyses, the estimated 3-year survival rate for movers is substantially greater than for stayers (a .14 gap by quarter 12 in figure 2 and .26 in figure 3). The results across all analyses reveal that residential change is negatively related to the likelihood of reincarceration.

## DISCUSSION

The most direct conclusion of this study is that residential change provides an opportunity for sustained behavioral change among ex-offenders. Three years out of prison, those ex-offenders who moved to a new parish after release had substantially lower rates of reincarceration than those who returned to the same parish. Given that a proportional hazards model fits the data better than a nonproportional model with a time-treatment

**Figure 3. Estimated Survival, by Place of Residence After Incarceration (Post-Katrina Sample Only)**



interaction, I do not find evidence that the effect of moving was merely temporary. Rather, the findings reveal that the gap in reincarceration between stayers and movers holds cumulatively over time and with respect to the hazard of reincarceration at each consecutive quarter during the 3-year observation period.

My findings support Laub and Sampson's (2003) life-course theory of desistance that emphasized the importance of knifing off from past situations and contexts. Although the socioeconomic context of prisoner reentry remains important to consider, my findings suggest that separating oneself from prior contexts improves an ex-offender's prospects of desisting, net of the socioeconomic conditions of destination locations. Just as Sharkey and Sampson (2010) concluded, the relevance of place is not just found in socioeconomic conditions. Even if individuals move to neighborhoods bereft of economic opportunities, they may still fair better than if they had returned home to a similarly disadvantaged neighborhood. Thus, knifing off is a turning point. Related to my earlier discussion, perhaps the MTO housing mobility experiment found few positive results for reducing problem behavior among male youths because moves were generally of such minimal distance that physical separation did not occur (Sampson, 2008).

Residential change is an important step in reducing recidivism, but it is not the only step. Some 54 percent of the movers were reincarcerated within 3 years.<sup>8</sup> What might account for this fact, despite the turning point of residential change? Lack of social support is one prospect. For instance, some participants in Leverentz's (2010) study of female ex-prisoners stressed that positive social supports are perhaps even more important for achieving behavioral change than severing ties with negative influences. In this sense, moving away from former neighborhoods *with* social supports (e.g., family) may be the best way to reduce the likelihood of reincarceration. Additional factors leading to recidivism include a lack of drug treatment and employment. Laub and Sampson (2003) found that men who desisted commonly had structured daily routines that included meaningful activity. In some instances, moving to unfamiliar areas may produce idle time without meaningful activity. Residential change may be particularly meaningful for the process of desistance if it prompts a change in daily routines and provides a platform for meaningful daily activity. Thus, knifing off—whether through residential change or other turning points such as joining the military—may be a necessary precursor to desistance from crime, but it may not alone ensure desistance.

Ex-prisoners tend to be geographically concentrated within resource-deprived sections of metropolitan areas, most often returning to the same general areas where they resided before incarceration. For instance, research by the Urban Institute reveals that more than half of prisoners released from Illinois prisons return to the city of Chicago, and one third of those returning to Chicago are concentrated in just six community areas (La Vigne et al., 2003). These patterns are anything but random. The reasons ex-prisoners return home are many, and they include family and social ties as well as familiarity and attachment to particular places. Institutional mechanisms are also at work. In most states, prisoners released onto parole are legally required to return to their county of last residence, thus contributing to a return to old neighborhoods (National Research Council, 2007). In addition, barriers to securing housing, such as the denial of public housing and vouchers to certain classes of felons, mean that many former prisoners will return to their home neighborhoods even when they do not want to because they have nowhere else to go. Given that residential change fosters the path to behavioral change, removing the institutional barriers to moving elsewhere may enhance public safety in aggregate, and it may even reduce incarceration costs from recidivism in the process.

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8. Recall that the reincarceration measure used in this analysis includes confinement in a state prison as well as in local and federal facilities. The high rates of reincarceration of this study are partly a function of the inclusiveness of this definition of reincarceration.

More research is needed to confirm the findings and to translate the implications from a unique natural experiment to a real-world policy environment. One avenue for future research would be to conduct a controlled trial that randomly assigns voluntary parolee participants to either an experimental group of movers or a control group of stayers. Random assignment would make it possible to isolate the effects of residential change on recidivism.

A second avenue for future research is to conduct a series of qualitative interviews with current and former parolees to uncover the precise mechanisms, such as changes in daily activities and peer relationships, by which residential change leads to lower recidivism. If the theoretical rationale for this study is sound, then we should find that stayers are more likely to fall back into their old routines, whereas movers will have changed who they associate with and the activities they engage in daily. Alternatively, residential change for some individuals may lead to an improvement in socioeconomic context relative to where they lived in the past and may thus reduce an individual's likelihood of criminal behavior. For instance, employment opportunities may be more plentiful in a destination neighborhood relative to an origin neighborhood. Data limitations in this study prevented such an assessment of within-individual change in socioeconomic conditions of residence; yet such an analysis would be useful for determining exactly why residential change leads to a lesser likelihood of recidivism.

Although much work remains on disentangling causal mechanisms, the results suggest that residential change should be added to the list of known turning points that foster desistance from crime. It also is one correlate of desistance that policymakers can tangibly promote, through housing incentives and greater flexibility in parole residency restrictions. Moreover, residential change may facilitate other turning points such as marriage and employment by keeping ex-offenders away from their previous environments during the challenging days and weeks after release. If ex-offenders can "survive" through this difficult period, then obtaining a job and establishing (or renewing) intimate relationships may be more likely. Knifing off through residential change, then, may be a crucial first step in a sequence of turning points that characterize the process of desistance from crime.

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## SUPPORTING INFORMATION

The following supporting information is available for this article:

**Table S1.** Estimating the Effect of Residential Change on Reincarceration with Rhodes's (2010) Method

**Table S2.** Estimated Treatment Effect, by Estimation Strategy

Supporting Information may be found in the online version of this article.

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