A Natural Experiment on Residential Change and Recidivism: Lessons from Hurricane Katrina

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Ex-prisoners tend to be geographically concentrated in a relatively small number of neighborhoods within the most resource deprived sections of metropolitan areas. Furthermore, many prisoners return "home" to the same criminogenic environment with the same criminal opportunities and criminal peers that proved so detrimental prior to incarceration. Yet estimating the causal impact of place of residence on the likelihood of recidivism is complicated by the issue of selection bias. In this study, I use a natural experiment as a means of addressing the selection issue and examine whether the migration of ex-prisoners away from their former place of residence will lead to lower levels of recidivism. In August 2005, Hurricane Katrina ravaged the Louisiana Gulf Coast, damaging many of the neighborhoods where ex-prisoners typically reside. The residential destruction resulting from Hurricane Katrina is an exogenous source of variation that influences where a parolee will reside upon release from prison. Findings reveal that moving away from former geographic areas substantially lowers a parolee's likelihood of re-incarceration.

> For the first time in my life, I didn't have the feeling that I had to go to Coxsackie, to Woodburn, and then to Sing Sing. I had the feeling now that anything could happen, anything that I decided to do. It seemed a little bit crazy, but I even had the feeling that if I wanted to become a doctor or something like that, I could go on and do it. This was the first time in my life that I'd had that kind of feeling, and getting out of Harlem was the first step toward that freedom.
>
> — Claude Brown (1965:178)

Research reveals that ex-prisoners tend to be geographically concentrated in a relatively small number of neighborhoods within metropolitan areas, often returning to the same criminogenic neighborhoods where they resided prior to incarceration (La Vigne et al. 2003). Alarmingly, a majority of ex-prisoners will be back in confinement within three years (Langan and Levin 2002). Yet it may be premature to implicate geographic context as a culprit in this vicious cycle. Estimating the causal impact of place of residence on the likelihood of recidivism is complicated by the issue of selection bias—that is, the possibility that some unmeasured characteristic of individuals influences both where they live and their criminal behavior and may account for any relation between place of residence and recidivism. Individuals with a high propensity toward criminal offending, and therefore a high likelihood of recidi-

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vism, might self-select into certain geographic contexts, and the characteristics of these contexts might have little causal bearing on individuals' behavior. Moreover, parolees who return "home" following incarceration to the same neighborhoods where they resided prior to incarceration might be fundamentally different on unobserved characteristics relative to parolees who move to different neighborhoods following incarceration. Without accounting for such processes of residential selection, observed correlations between recidivism and residential migration likely reflect the effect of unmeasured individual or family characteristics that motivated residential choice. Consequently, empirical estimates of the effects of residential migration on recidivism may be biased and inconsistent. If scholars of social context and "neighborhood effects" are to make causal statements about the impact of place of residence or residential change on a given outcome measure, they must address the issue of selection bias by separating out selection effects from true contextual effects.

In this study, I use a natural experiment as a means of minimizing the potential for selection bias in a study of recidivism. Hurricane Katrina and the tragic events that followed have implications for prisoner reentry,[1] and, in particular, the selection of residence for those prisoners released from incarceration following Katrina.[2] In Orleans Parish in Louisiana, 71.5 percent of housing units suffered some damage following Hurricane Katrina, with 56 percent of housing units significantly damaged (U.S. Department of Housing and Urban Development 2006). The extent of housing destruction was similar in adjacent parishes. In both St. Bernard Parish and Plaquemines Parish, 80 percent of housing units were damaged, while 70 percent were damaged in St. Tammany Parish and 53 percent were damaged in Jefferson Parish.

The tragedy of Katrina presents an opportunity—a natural experiment—to assess the effects of residential change on recidivism. The residential destruction resulting from Hurricane Katrina is an exogenous source of variation that influences where a parolee will reside upon release from prison. In the absence of complete data on why a given parolee moves to one geographic area versus another, to estimate the causal effect of residential migration (i.e., the treatment) it is advantageous to have an exogenous source of variation that substantially influences this treatment.

This study aims to answer the following research questions: First, how has the geographic distribution of parolees released from Louisiana prisons changed following Hurricane Katrina? Second, if there has been a geographic displacement of parolees following the hurricane, has this hurricane-induced migration had any negative, or even beneficial, effects on the likelihood of recidivism?

If Claude Brown's (1965) tale of residential change and desistance from crime generalizes to the parolees in Louisiana induced to move because of Hurricane Katrina, we may find that Katrina actually led to positive outcomes for this particular slice of the population. The lesson from Katrina may be that residential change leads to turning points in the lives of parolees.

THE GEOGRAPHIC CONTEXT OF PRISONER REENTRY

The Bureau of Justice Statistics reports that presently more than 700,000 prisoners are released from state and federal facilities each year (Sabol and Couture 2008); estimates suggest that up to half of these releasees will have been in prison before (Langan and Levin 2002). By some estimates, two-thirds of former prisoners in the United States are rearrested within three years of prison release, and half are re-incarcerated (Langan and Levin 2002). These figures should not be separated from the geographic context to which prisoners return. Ex-prisoners tend to be geographically concentrated within the most resource deprived sections of metropolitan areas,

[1] "Prisoner reentry" refers to the process of leaving prison and returning to the community (National Research Council 2007).

[2] Hurricane Katrina initially developed on August 23rd, 2005. It made its first landfall on the 25th near Ft. Lauderdale, Florida as a Category 1 hurricane. The storm then moved into the Gulf of Mexico, becoming a Category 5 hurricane by the 28th. On August 29th, Katrina made its second landfall, in southern Louisiana, and then moved inland toward New Orleans. Levee walls on the Industrial Canal in New Orleans breached from the storm a couple of hours later, and additional levee breaches followed soon after.

often returning to the same neighborhoods where they resided prior to incarceration. For instance, research by the Urban Institute on reentry in Illinois reveals that over 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 six communities are among the most economically disadvantaged in the city. Given that many prisoners return home to their old neighborhoods, it is important to develop a theoretical understanding for why this fact is consequential.

PLACE OF RESIDENCE, RESIDENTIAL MIGRATION, AND RECIDIVISM

The alarming rates of recidivism in the United States demonstrate that there is much continuity in criminal behavior over the life course. Sampson and Laub (1993) explain such continuity by the absence of social controls (e.g., stable employment and a healthy marriage). Yet Sampson and Laub also argue that continuity in behavior is not inevitable; change is possible, despite the deleterious consequences that the mark of a criminal record has for future life events. Crucial to understanding the mechanisms underlying changes in criminal behavior is the notion of turning points, defined as "consequential shifts that redirect a process" (Abbott 1997:101).

Hurricane Katrina may have induced turning points in the lives of former prisoners for a number of different reasons. First, Katrina was a macro-level shock to the Gulf Coast region. As life-course researchers have shown, historical shocks such as the Great Depression and wars have profound consequences for the life course of affected individuals (Elder 1974). A second, but no less important, implication of Hurricane Katrina is residential change. Widespread property destruction in the Gulf Coast region had an immense impact on residential relocation.

In a study of the life course of crime through age 70, Laub and Sampson (2003:149) contend that, "offenders desist [from crime] in response to structurally induced turning points that serve as the catalyst for sustaining long-term behavioral change." Based on interviews with 70-year-old desisters, they find that residential change is often a fundamental turning point leading to desistance from crime.[3] Similarly, in an analysis of data from the Cambridge Study of Delinquent Development, Osborn (1980) finds that delinquents who subsequently moved away from London were significantly less likely to be reconvicted of a crime than were delinquents who stayed in London. Laub and Sampson, as well as Osborn, find that a change of residence allows individuals to separate from past situations and criminal peers, thus eliminating some of the factors contributing to the persistence of criminal behavior.

Claude Brown's (1965) memoir, *Manchild in the Promised Land,* illustrates the benefits of separating from criminogenic environments. During his troubled youth, Brown committed countless crimes, used and sold a variety of drugs, was expelled from school, asserted his dominance over both the Wiltwyck School for Boys and the Warwick State Training School, and was even shot during a burglary. Yet after this antisocial childhood, Brown was able to desist from crime while many of his peers ultimately went to prison, became heroin addicts, or died (or some combination thereof). One sure reason why Brown avoided such a fate is because he did not take to heroin as did many of his peers (Brown did not enjoy his brief experimentation with the drug). Yet another compelling reason why he avoided a life of crime is change of residence. As he entered adulthood, Brown made a conscious decision to move away from Harlem and the peers, routines, and temptations of a familiar environment:

> One thing began to scare me more than anything else about jail. This was the fact that if I went to jail and got that sheet on me, any time I decided that I didn't want to go the crime way, that I wanted to do something that was straight, I'd have a lot of trouble doing it behind being in jail. I didn't want that sheet on me, and I knew if I kept hanging around Harlem I was going to get busted. (P. 177)

By moving to Greenwich Village, Brown both physically and mentally separated himself from many of the criminogenic influences in his life.

[3] Desistance refers to the causal process that supports the termination of criminal offending (Laub and Sampson 2003).

There are a number of potential mechanisms by which residential change may lead to a turning point in the life course of crime, including a change in association with criminal peers and a change in one's routine activities. Association with criminal peers may influence an individual's criminality, in part, through a contagion process whereby individuals learn the motivations and techniques for crime. Association with criminal peers may also be consequential to crime because such associations provide access to criminal opportunities. With respect to the former causal mechanism, contagion models of crime generally posit that the likelihood of criminal and antisocial behavior increases with exposure to others who engage in similar behavior. As Warr (2002:3) convincingly demonstrates, "Criminal conduct is predominantly social behavior. Most offenders are imbedded in a network of friends who also break the law, and the single strongest predictor of criminal behavior known to criminologists is the number of delinquent friends an individual has."

If crime is in fact a behavior learned and facilitated through group interaction, then it follows that removing individuals from their criminogenic social networks should reduce their likelihood of engaging in criminal behavior. Research evidence supports such a contention. For instance, Warr (1993) finds that exposure to delinquent peers declines with age, and this explains the decline in crime with age. In a later work, Warr (1998) dissects the reasons why marriage is such a powerful predictor of desistance from crime, arguing that it is because marriage often disrupts peer relationships. He finds that marriage reduces the likelihood that an individual will associate with delinquent peers, and it alters the amount of time individuals spend with delinquent peers.

In addition to a learning process, peers may influence an individual's criminal conduct through several other mechanisms, including the creation of criminal opportunities (Laub and Sampson 2003; Warr 2002). Association with delinquent peers does not necessarily mean that an individual is learning the motivations and techniques for committing crime; rather, it may just mean that the person is exposed to more opportunities for committing crime than would be available in the absence of criminal peers. For instance, an individual may use a criminal associate to fence stolen goods or to purchase drugs. Separating individuals from criminal networks via residential change may thus reduce their criminal behavior by eliminating opportunities for engaging in crime.

Besides altering peer associations and any corresponding criminal opportunities, residential change may lead to desistance by disrupting routine activities and daily temptations that are conducive to criminal conduct. For example, the cue-reactivity paradigm in addiction research suggests that drug addicts are vulnerable to relapse in the presence of familiar environmental stimuli (Carter and Tiffany 1999; Wikler 1948). Through a process of classical conditioning, drug addicts come to associate certain stimuli with the use of a drug. These stimuli can trigger physiological reactions, including an intense craving for drugs. Addicts are thus more likely to relapse in environments associated with prior drug use. Given that over half of prisoners have some form of drug dependence, and that reduced consumption of drugs is one key factor leading to desistance from crime (Mumola and Karberg 2006; National Research Council 2007), residential change may lower the likelihood of recidivism by separating drug addicts from familiar contexts associated with past drug use.

The life-course literature suggests that to decrease the probability of recidivism, it would be beneficial to separate parolees from their criminal past and their peers. Yet, sorting out the causal consequences of residential migration and peer effects requires a research design that explicitly addresses the issue of selection bias. While I am unaware of any studies that expressly account for selection bias in investigations of the association between residential migration and recidivism, several studies of residential mobility have found that selection bias can be quite consequential for estimates of neighborhood effects. As but one example, with data from the Moving to Opportunity (MTO) housing mobility experiment, Ludwig and Kling (2007) examine the effects of neighborhood conditions on the likelihood of arrest for a violent crime. In their analysis, Ludwig and Kling compare nonexperimental results estimated through ordinary least squares (OLS) with experimental results based on instrumental variable (IV) estimates. For male youths, they find substantially lower treatment effects of exposure to neighborhood violence on arrest through IV estimates than for OLS estimates. Specifically, the size of the treatment effect declined by nearly 40 percent after accounting for the differential sorting of youth into neighborhoods. Accounting for selection is thus vital to uncovering unbiased estimates of the effects of moving on recidivism.

In this study, I address the first part of a two-part causal story. I seek to uncover whether separating individuals from their former criminogenic environments reduces their likelihood of recidivism. I do this through a research design that expressly accounts for selection bias. If residential migration does reduce the likelihood of recidivism, the second part of the causal story is to investigate why. Two potential answers worthy of future exploration are changes to routine activities and peer associations.

DATA AND RESEARCH DESIGN

This study exploits the benefits of a natural experiment in order to characterize the geographic distribution of prisoner reentry in Louisiana and assess the repercussions of geographic displacement from Hurricane Katrina on the probability of re-incarceration. The analytic sample is drawn from prisoners released from Louisiana correctional facilities. Because my interest is in residential displacement due to Hurricane Katrina, I restrict analyses to those prisoners who resided in affected metropolitan areas prior to incarceration. Accordingly, the analytic sample includes only ex-prisoners who were committed to prison from Orleans Parish and the four parishes adjacent to Orleans (Jefferson, Plaquemines, St. Bernard, and St. Tammany). These areas sustained substantial housing damage in the days following Hurricane Katrina. Therefore, the residential options of prisoners released post-Katrina are significantly different than their options if they had been released prior to the hurricane, resulting in some measure of geographic displacement. Finally, I also restrict the sample to exclude sex offenders. Given the nature of their offense, sex offenders face a number of constraints on their residency choices upon release from prison.

For the purposes of descriptive analyses, I construct three cohorts of prison releasees, two of which were released from prison prior to Hurricane Katrina and one released afterward. The first cohort is made up of releases from a Louisiana prison to parole supervision anytime between September 2001 and February 2002 (hereafter called the 2001 to 2002 cohort).[4] The second cohort consists of releases between September 2003 and February 2004 (the 2003 to 2004 cohort), while the third cohort consists of releases onto parole supervision between September 2005 and February 2006 (the post-Katrina cohort). This study uses two pre-Katrina cohorts to more fully establish whether Hurricane Katrina altered prior trends in parolee migration. Sample sizes equal 1,538, 1,731, and 1,370 for the 2001 to 2002, 2003 to 2004, and post-Katrina cohorts, respectively.

I use three varieties of data in this study: (1) individual-level data on parolees from the Louisiana Department of Public Safety & Corrections (DPS&C) and the Division of Probation and Parole (DPP), (2) zip code and parish-level characteristics from the U.S. Department of Housing and Urban Development, the Louisiana Department of Labor, and ESRI,[5] and (3) Louisiana criminal

[4] I use the term "parole supervision" to refer to all release mechanisms from incarceration that result in a term of supervision for the returning prisoner. Releases to supervision in Louisiana are generally made through parole board action or through the diminution of the sentence via good time credit. "Parole supervision" encompasses both types of releases. Roughly 90 percent of prisoners released each year from Louisiana prisons are released onto parole supervision (in contrast to unconditional releases, which do not require post-incarceration supervision). To distinguish my analytic sample of paroled releases from unconditional releases, I use the term "parolee."

[5] ESRI is a developer of geographic information systems and associated software applications, including ArcGIS. ESRI also compiles and distributes an assortment of geographically referenced data, including yearly demographic estimates of the U.S. population at county and zip code levels. I use these estimates to produce measures of racial segregation and household income. To derive post-Katrina estimates, ESRI (2006a, 2006b) augmented their standard methodology used to compute intercensal demographic estimates by incorporating data from the Federal Emergency Management Agency (FEMA) on property damage and applications for assistance, data from the United States Postal Service National Change of Address file, and data from the American

justice system data from the Supreme Court of Louisiana, DPS&C, DPP, and the Uniform Crime Reports.

Given that macro-level social and economic conditions in Louisiana changed drastically immediately following Hurricane Katrina, one must control for such temporal changes to isolate the effect of residential change on recidivism. The statistical models include controls for segregation, average household income, the unemployment rate, average weekly wages, and fair market rents. Massey and Denton (1993) argue that racial segregation has been instrumental in creating and perpetuating "underclass" geographic areas characterized by poverty and social isolation. To the extent that underclass areas are associated with high rates of crime, whether from a lack of informal social control or a lack of police protection, temporal changes in segregation may influence temporal changes in the likelihood of recidivism (Massey and Denton 1993; Sampson and Wilson 1995). Low income levels, as well as poverty, are related to crime by a number of mechanisms. Specifically, average income is negatively related to social disorganization (Shaw and McKay 1942) and economic strain (Merton 1938), which are positively predictive of crime. Unemployment may have contrasting effects on recidivism. High unemployment rates may lead to an increase in recidivism as individuals turn to crime in the absence of legitimate employment opportunities (Cantor and Land 1985; Grogger 1998). However, while criminal motivation may increase with the unemployment rate, criminal opportunity may decline. Homes are more likely to be occupied during the day, and potential victims are less likely to be away from home or their neighborhoods, when unemployment is high (Cantor and Land 1985). Declining wages and rising rents, by increasing deprivation, may also be related to recidivism (Blau and Blau 1982; Merton 1938).

Hurricane Katrina also had many implications for temporal changes in Louisiana's criminal justice system (for detailed discussions, see Garrett and Tetlow 2006; Roman, Irazola, and Osborne 2007). Given Katrina's impact on the criminal justice system, particularly in Orleans and adjacent parishes, it is vital to account for temporal variation in the operation of the justice system to draw causal inferences about the effect of residential change on recidivism. The analyses thus include control variables related to parole practices, court operations, and the probability of arrest given the commission of a crime.

Individual-Level Variables

Re-incarceration. Re-incarceration refers to whether a parolee returned to a Louisiana prison for a new criminal conviction or a parole violation within one year of prison release. This is the primary measure of recidivism used in the study. In addition to estimating the effect of migration on the combined measure of re-incarceration, I also perform separate analyses for new criminal convictions and parole revocations.

Re-incarcerated or detained. I rely on Louisiana data in this study, but a proportion of the release cohorts from Louisiana may be committed to prison in another state or in the federal system. However, since the three cohorts include only those prisoners released to parole supervision, and parolees are required to remain in Louisiana and have periodic visits with their parole officer, the commitment of parolees to other state correctional systems may be limited. Information on non-Louisiana incarcerations, as well as incarcerations in a local facility (as opposed to state facilities), is captured in the DPP case management system in a field for supervision level. A parolee's supervision level is marked as "detained" if that parolee is confined in another jurisdiction.[6] In the interest of

Red Cross on housing unit damage. Estimates for 2006 reflect data current as of February 2006. Data used in this study are published in annual editions of the *Community Sourcebook of County Demographics* and the *Community Sourcebook of ZIP Code Demographics.*

[6] Information on other jurisdictional detention is obtained if the DPP directly transfers a given parolee to the other jurisdiction, or through a process of tracking absconders. In the event that a parolee absconds and misses a periodic visit with a parole officer, the officer may decide to issue a warrant for the parolee through the FBI's National Crime Information Center (NCIC) and through local law enforcement. To track absconders, DPP parole offi-

determining whether my inferences are sensitive to the operationalization of recidivism, I re-estimate my statistical model with a second measure of recidivism. This second, more inclusive, measure combines the re-incarceration measure described previously with information on other detentions and indicates whether a given parolee was re-incarcerated in a Louisiana prison within one year or was detained in another jurisdiction.

DIFFERENT PARISH FROM CONVICTION. This is a binary treatment variable indicating whether parolees moved to a different parish following incarceration relative to where they were originally convicted. This variable equals zero if parolees returned to the same parish where they were convicted, and one if they migrated to a different parish.

DISTANCE OF MIGRATION. This is a continuous treatment variable indicating how far released parolees reside relative to where they were originally convicted. This variable equals zero if parolees return to the same parish where they were convicted. Otherwise, it is computed as the Euclidean distance (in miles) between the centroid of the parish where a parolee was originally convicted and the centroid of the zip code where the parolee resided upon release from prison.

POST-KATRINA RELEASE. This is a binary variable indicating whether the parolee was released from prison following Hurricane Katrina. This variable is used as an instrument in analyses.

FIRST RELEASE. This is a binary variable indicating whether parolees were released from their first term of incarceration (equals one) or from their second or greater term (zero). Parolees as a whole are a heterogeneous group with widely varying rates of recidivism (National Research Council 2007). Parolees released from their first term of imprisonment have, on average, substantially lower rates of recidivism than do parolees with multiple prior incarcerations (Rosenfeld, Wallman, and Fornango 2005). Controlling for prior incarcerations is thus vital in a study of recidivism.

The study employs five additional individual-level measures as correlates of recidivism: race, gender, age at time of release, marital status, and time served. Black parolees make up 74.2 percent of the sample, with Whites making up 25.7 percent. Other races make up .1 percent of the sample. I use a binary indicator *(Black)* in analyses to compare Black versus White–other race categories. *Male* is a binary indicator of gender. Males make up 87 percent of the sample. *Married* is a binary variable indicating the marital status of parolees at the time of their release (not married equals zero). Ten percent of the sample was married at the time of release. *Time served* refers to the amount of time a parolee served in prison (in years or fraction thereof) until release.[7] Controlling for time served is crucial to account for any differences between cohorts in the average severity of prior offending. If the likelihood of prisoners being released from custody once they were eligible declined in the post-Katrina period, relative to the pre-Katrina period, accounting for time served will capture differences across release cohorts in prior offending severity.

CONTEXTUAL-LEVEL VARIABLES[8]

DISSIMILARITY. Dissimilarity *(D)* is a measure of the evenness of population distribution

cers also search the National Law Enforcement Telecommunications System (NLETS) and local warrant systems. If absconders are detained in a local or non-Louisiana facility, information in the DPP data system is updated accordingly.

[7] Time served is highly associated with the offense of conviction (e.g., prisoners in Louisiana convicted of violent offenses serve more time relative to other offenses). In the interest of minimizing collinearity, I use *time served* as a control in the analyses, but I do not use indicators of offense of conviction.

[8] Research shows that the level of aggregation (e.g., census tract, zip code, county) used in studies of contextual effects influences inferences derived from the association between a given contextual characteristic and a dependent variable (Hipp 2007). In this study, I use zip code and parish levels of aggregation. I do so partially because measures such as unemployment and wages are reflective of metropolitan-wide economic conditions, and also because

(Duncan and Duncan 1955). In the case here, it is a measure of segregation between Blacks and Whites, reflecting their relative distributions across zip codes within each parish. *D* can range in value from 0, indicating complete integration, to 100, indicating complete segregation. For the 2001 to 2002 cohort, I use an estimate of *D* for a parolee's parish of residence calculated from the 2002 ESRI data, while for the 2003 to 2004 and post-Katrina (i.e., 2005 to 2006) cohorts I use estimates of *D* from the 2004 and 2006 data, respectively.

AVERAGE HOUSEHOLD INCOME. This is a measure of the average household income (in 2000 adjusted dollars) in the zip code in which a parolee resides. For the 2001 to 2002 cohort, I use the 2002 ESRI household income estimates, and I use 2004 and 2006 estimates for the 2003 to 2004 and post-Katrina cohorts, respectively.

UNEMPLOYMENT RATE. This is a measure of the parish unemployment rate in parolees' parish of residence in the quarter during which they were released from prison. Data are drawn from the Louisiana Department of Labor.

AVERAGE WEEKLY WAGE. This is a measure of the average weekly wage (in 2000 adjusted dollars) in the parish where parolees reside for the quarter during which they were released from prison. Data are drawn from the Louisiana Department of Labor.

FAIR MARKET RENT. This is a measure of the average fair market rent in the parish where a parolee resides, taken from data compiled by the U.S. Department of Housing and Urban Development. For the 2001 to 2002 cohort, I use the fair market rent average in 2002, while for the 2003 to 2004 and post-Katrina cohorts, I use rent averages from 2004 and 2006, respectively. All figures are adjusted to 2000 dollars.

CRIMINAL JUSTICE SYSTEM VARIABLES

AVERAGE PAROLE CONTACTS. This measure is drawn from the DPS&C Quarterly Statistical Performance Report. Greater scrutiny influences the likelihood that a parolee will get caught for violating conditions of parole (Turner and Petersilia 1992), yet increased scrutiny may also have a deterrent effect on parolees that lowers the likelihood of recidivism. It is thus important to control for temporal variations in average parole contacts (i.e., the total contacts parole officers have across their entire caseloads), as well as between parole district variation, to account for these contrasting influences of scrutiny on recidivism. For analyses, I use the average number of contacts made across parole officers in a parole district during the quarter in which parolees were released from prison. Contacts include those that occurred within the parole office, as well as field contacts to a parolee's residence or workplace.

JUDGE CASELOADS. This measure derives from the Supreme Court of Louisiana's annual report. There is a vast body of research examining the relation between judge caseloads and criminal case dispositions (e.g., Heumann 1975). One key issue in this regard is whether large caseloads, and the need to process large numbers of defendants, pressure courts toward the use of plea bargaining and result in lenient sentencing practices (Eisenstein, Flemming, and Nardulli 1988). Recent research (see, e.g., Johnson 2005; Ulmer and Johnson 2004) reveals that judge caseloads are inversely related to the likelihood of incarceration and the severity of criminal sentences. I thus include a control for judge caseloads given that such caseloads likely influence whether a convicted offender is sentenced to a term of imprisonment or some other sanction, such as probation. For the 2001 to 2002 cohort, I use the number of cases per judge in 2002 in a parolee's parish of residence; for the 2003 to 2004 and post-Katrina cohorts, I use caseload figures from 2004 and 2006, respectively.

UCR ARRESTS PER CRIME. I use this measure, computed from the FBI's Uniform Crime Reports, to control for the temporal and geographic variation in the likelihood of getting arrested for a Part I offense (i.e., murder, rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, or arson) given the commission of such a crime. For the 2001 to 2002

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monthly, quarterly, and yearly time series data are not available at a lower level of aggregation.

cohort, I construct this measure using the ratio of Part I arrests per reported Part I crimes in 2001 for a parolee's parish of residence; for the 2003 to 2004 and post-Katrina cohorts, I use the 2003 and 2005 ratios, respectively.

ANALYTIC STRATEGY

Analyses follow two paths to coincide with the objectives of the study. First, I expect that prisoners released soon after Katrina faced severely constrained opportunities to reside in New Orleans, resulting in far fewer returns to the New Orleans metropolitan area than in the pre-Katrina period and therefore fewer returns to former parishes. This expectation leads to Hypothesis 1:

Hypothesis 1: Because of hurricane-related destruction, significantly fewer parolees moved to New Orleans upon release from prison during the time period immediately following Hurricane Katrina, relative to the pre-Katrina period, and significantly fewer returned to the parish they inhabited prior to incarceration.

To examine this hypothesis, I geocode and map the post-release addresses for the three separate cohorts of parolees, and I provide illustrations of how the geographic distribution of prisoner reentry changed post-Katrina.

With regard to my second research question, concerning the impact of residential change on recidivism, I expect that separating parolees from their former residential environments will be beneficial with respect to desisting from crime. Induced migration due to Hurricane Katrina allows for a separation between parolees and their criminal past, thus reducing the likelihood of re-incarceration. These propositions yield Hypothesis 2:

Hypothesis 2: The likelihood of re-incarceration is lower when parolees reside in a geographic area different from where they resided prior to incarceration.

To test Hypothesis 2, I compare the likelihood of re-incarceration within one year of release from prison for parolees who resided in the same parish upon release as where they were originally convicted versus parolees who moved to a different parish. Before describing the specific statistical analyses that will be performed, it is first relevant to highlight the benefits of using a natural experiment to examine the influence of residential migration on recidivism. There are numerous potential threats to internal validity, but in studies of residential migration, the threat of selection bias is of particular concern, where selection refers to the process of assigning individuals to conditions (i.e., treatment versus control groups). One approach to remedy the threat of selection bias is simply to measure more characteristics of individuals and more fully specify statistical models with those measures; in essence, the strategy is to subset the data such that two or more individuals are identical except with respect to the treatment condition. The assumption with such an approach is that selection into treatment and control groups is ignorable (i.e., assignment to control and treatment groups is random) after conditioning on the observed characteristics of parolees. The threat of omitted variables is still probable, however, even with extensive use of statistical controls.

An alternative approach to alleviating the threat of selection bias is the use of instrumental variables. With an IV approach, a variable (or variables) that is unrelated to the outcome variable is used as a predictor (i.e., instrument) of the key explanatory variable (i.e., the treatment), and the outcome variable is then regressed on the predicted treatment measure. Conceptually, this approach removes the spurious correlation between the explanatory variable and unobserved characteristics, in this case unobservable characteristics of parolees. An IV remedies the issue of omitted variables by using only that portion of the variability in the treatment variable that is uncorrelated with omitted variables to estimate the causal relation between the treatment and outcome. The key criticism of this approach is that the assumption about the lack of relation between the instrument and the outcome variable may be problematic. However, the use of an instrument derived from a natural experiment obviates this issue. We can have more confidence that the instrument and outcome variable are unrelated if the instrument derives from a random force of nature like a hurricane (for a discussion, see Angrist and Krueger 2001). This assumption is known as the exclusion restriction—that is, $\text{cov}(Z_i,u_i) = 0$, where Z_i

is the instrument and u_i is the model error that includes the omitted variables—but it is not directly verifiable (Angrist, Imbens, and Rubin 1996). The validity of inferences from an IV analysis depends on the appropriateness of this assumption. I address potential violations of the exclusion restriction in the Discussion and Appendix.

Using Hurricane Katrina as an exogenous source of variation that influences where a parolee resides, I combine a natural experiment with an instrumental variable approach to provide a consistent estimate of the effect of moving to a different parish (i.e., the treatment) on re-incarceration (Angrist et al. 1996).[9] Angrist and Krueger (2001:77) note that, "instrumental variables provide an estimate for a specific group—namely, people whose behavior can be manipulated by the instrument." In the present context, Hurricane Katrina affected the residential behavior of those parolees who could not or would not have moved away from the New Orleans metropolitan area following incarceration, but it did not affect parolees who would have moved even in the absence of the hurricane.[10] In short, using an IV approach allows me to compute an estimate of the effect of migrating to a *different* parish for parolees who otherwise would have moved back to the *same* parish, had Hurricane Katrina not occurred (this is the Local Average Treatment Effect, or LATE). I do not provide an estimate of the effect of moving to a different parish on re-incarceration for parolees who would have moved regardless of the hurricane.

I specify in Equations 1 and 2 the two-stage estimation process with the IV technique. The first equation models the key explanatory variable S_i (different parish from conviction) as a function of an instrumental variable Z_i (post-Katrina release) and a vector of control variables X_i used to account for any observed differences between the treatment and control groups. I assume that where parolees reside depends, in part, on whether they were released from prison before or after Hurricane Katrina:[11]

$$S_i = Z_i\theta_1 + X_i\pi + \xi_i \qquad (1)$$

The second-stage of the two-stage estimation process models the dependent variable Y_i (re-incarceration) as a function of the predicted S_i from Equation 1 and a vector of control variables X_i:[12]

$$Y_i^* = \alpha\,\hat{S}_i + X_i\beta + u_i \qquad (2)$$

where

$$Y_i = 0 \text{ if } Y_i^* < 0,\ Y_i = 1 \text{ if } Y_i^* \geq 0, \text{ and } u_i \sim N(0,1).$$

The coefficient α is the key parameter of interest and will be used to determine whether migrating to a new parish upon release influences a parolee's likelihood of recidivism.

[9] Consistency means that the IV estimate converges to the population parameter as the sample size increases.

[10] As an example, Angrist (1990) uses Vietnam-era draft lotteries as an instrument to assess the effect of military service on future earnings. Some individuals joined the military voluntarily, and some joined because of the draft. Angrist's IV estimates of the effects of military service on future earnings are applicable for draftees but not volunteers.

[11] I estimate Equations 1 and 2 in Stata using the *ivprobit* function. Via the *cluster* estimation command with the *ivprobit* function, I adjust model standard errors to account for the clustering (i.e., interdependence) of parolees within parishes. Note that the *ivprobit* function treats the endogenous treatment variable S_i in Equation 1 as continuous, and a linear regression model is used to predict treatment assignment. As Angrist and Krueger (2001:80) reveal, using a linear regression model to produce the first-stage estimates generates consistent second-stage estimates even when the endogenous treatment variable is binary (as is the case here). Stata do-files for all models estimated in analyses are available from the author on request.

[12] With a true randomized experiment, it is assumed that treatment and control groups are balanced (i.e., equivalent) with respect to observable and unobservable characteristics, aside from random variation. Even though I use an exogenous source of variation—a hurricane—to predict treatment assignment, in the absence of random assignment it is not necessarily the case that treatment and control groups are identical. It is thus beneficial to include statistical controls (X_i) for key parolee characteristics, as well as for contextual and criminal justice covariates, to account for any observed differences across groups. The benefit of statistical controls is a gain in statistical efficiency (i.e., a reduction in sampling variance).

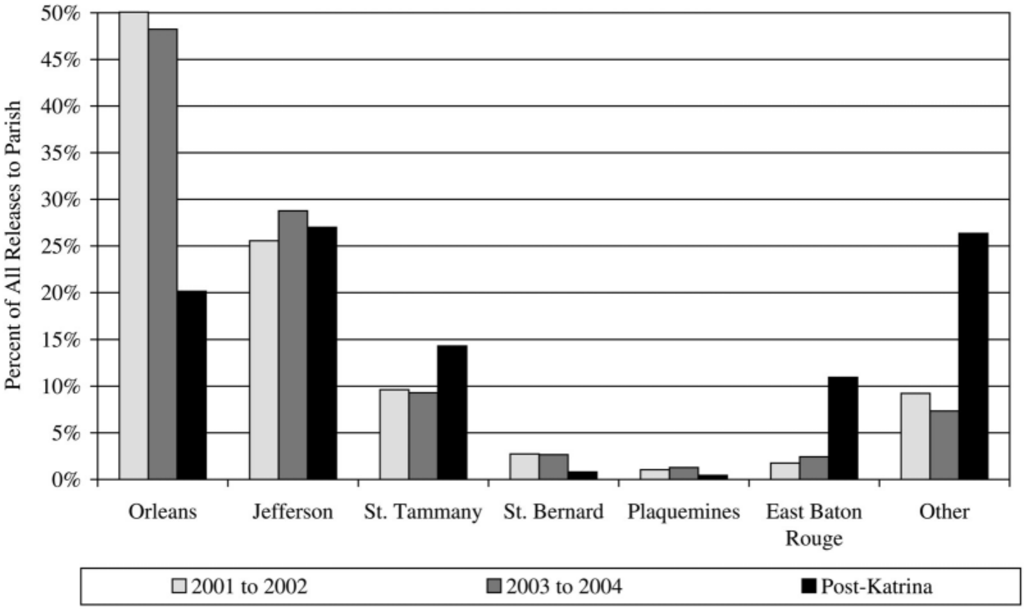

Figure 1. Parish of Release for Prisoners Committed from the Five-Parish Area

RESULTS

HYPOTHESIS 1

Figure 1 provides a snapshot of the geographic redistribution of parolees post-Katrina. This figure shows which parish parolees resided in immediately after their exit from prison. I use data from two pre-Katrina cohorts to establish the extent to which Hurricane Katrina prompted a shift in the geographic distribution of parolees. The release figures from 2001 to 2002 and 2003 to 2004 reveal that roughly 50 percent of prisoners convicted in the five-parish area (i.e., Orleans, Jefferson, Plaquemines, St. Bernard, and St. Tammany) subsequently returned to New Orleans. Post-Katrina, this number drops to 20 percent. In the post-Katrina period, proportionally more parolees opted to reside in Baton Rouge (2 percent before Katrina versus 11 percent after), and many other parolees dispersed throughout the state. This figure demonstrates that the proportion of newly released prisoners (who were originally committed from the New Orleans metropolitan area) who returned to the five-parish area upon release declined drastically immediately following Katrina. This finding provides support for Hypothesis 1. Moreover, recall that one benefit of using an instrument derived from a natural experiment is to assure that the instrument is correlated with the treatment condition (i.e., migration) but otherwise unrelated to the outcome variable (i.e., re-incarceration). Figure 1 suggests that the time period of release (pre- versus post-Katrina) is highly correlated with the treatment condition (see the Appendix for a detailed assessment of the assumptions of the IV framework).

Table 1 contrasts the place of residence across the three cohorts through a cross-tabulation of the proportion of members of each cohort who moved to the same parish following incarceration as where they were originally convicted, relative to the proportion who migrated to a different parish. Results reveal that prior to Hurricane Katrina, roughly three-quarters of parolees returned to their parish of conviction upon release from prison. Post-Katrina, this distribution changed significantly (Chi-Square = 286.65, $p < .001$), with 49.9 percent of parolees returning to the same parish and 50.1 percent migrating to a different parish. This finding provides additional support for Hypothesis 1.

HYPOTHESIS 2

Results thus far reveal that there has been a geographic redistribution of prisoner reentry

Table 1. Post-incarceration Place of Residence

	Post-incarceration Residence	
Cohort	Same Parish	Different Parish
2001 to 2002 (N = 1,538)	75.5%	24.5%
2003 to 2004 (N = 1,731)	75.2%	24.8%
Post-Katrina (N = 1,370)	49.9%	50.1%

Note: "Same" means the proportion of parolees in a given cohort who moved to the same parish following incarceration as where they were originally convicted. "Different" refers to the converse. Chi-Square = 286.65, $p <$ .001.

post-Katrina. Whether the residential migration underlying this redistribution is causally related to the likelihood of recidivism is an empirical question, which I will address through an IV analysis. Before moving onto the IV results, it is necessary to address the topic of treatment noncompliance, so as to clarify precisely which type of treatment effect is estimated in this study. Recall that in the present case the treatment S_i is represented by a given parolee residing in a *different* parish upon release relative to where he was convicted prior to incarceration. Perfect treatment compliance would represent the situation where all parolees released post-Katrina ($Z = 1$) moved to a different parish ($S = 1$), and all parolees released pre-Katrina ($Z = 0$) moved to the same parish as where they were originally convicted ($S = 0$). As revealed in Table 1, this situation certainly does not hold; 49.9 percent of parolees released post-Katrina ($Z = 1$) moved to the same parish ($S = 0$), and 24.7 percent of parolees from the two pre-Katrina cohorts ($Z = 0$) moved to a different parish ($S = 1$). While we can assume that the *assignment* to treatment is ignorable (i.e., conditional on the instrument Z and the other covariates from Equation 1, assignment to control and treatment groups is random), a consequence of noncompliance is that the *receipt* of treatment is nonignorable (Angrist et al. 1996). If this is the case, simply computing the difference between the pre- and post-Katrina cohorts on recidivism will not provide an unbiased or consistent estimate of the *average causal effect* of migrating to a different parish on recidivism. Yet, by using IV methods, I can compute a consistent estimate of the effect of migrating to a different parish on re-incarceration for parolees who would not have moved had it not been for Hurricane Katrina (i.e., an estimate of LATE).[13] Results in the ensuing analyses represent the LATE estimate.

Table 2 presents the IV probit results of re-incarceration (a detailed assessment of the assumptions of the IV framework is presented in the Appendix). Focusing on the first column of results (Re-incarceration), males are more likely than females to be re-incarcerated, and married parolees are less likely to be re-incarcerated. Age and time served in prison are negatively related to re-incarceration. As expected, first releases from prison are significantly and substantially less likely to recidivate than are repeat offenders (this relation will be explored in greater detail in Table 3). With respect to the contextual-level and criminal justice system covariates, only fair market rent is significantly associated with re-incarceration. In this case, higher rents correspond to a lower likelihood of re-incarceration, which may indicate that areas with higher rent have relatively more informal social control and surveillance, and therefore less crime.

Turning to the main finding of the study, results show that individuals who migrated to

[13] Through the IV method, I adjust the recidivism difference between the pre- and post-Katrina cohorts for the fact that not all post-Katrina parolees moved to a different parish and not all pre-Katrina parolees returned to the same parish. This adjustment yields the Local Average Treatment Effect, which is computed as follows (Angrist et al. 1996):

$$\{E[Y_i \mid Z_i = 1] - \{E[Y_i \mid Z_i = 0]\} \, / \\ \{E[S_i \mid Z_i = 1] - \{E[S_i \mid Z_i = 0]\} = \alpha \, ,$$

where Y_i is the outcome measure of re-incarceration, Z_i is the instrument (post-Katrina release), and S_i is the key explanatory variable (different parish from conviction).

a different parish were significantly and substantially *less* likely to be re-incarcerated within one year of release from prison. With respect to the marginal effect, the probability of re-incarceration is .15 lower for parolees who did not move back to the parish where they were originally convicted, relative to parolees who did return (net of individual, contextual, and criminal justice system correlates). The 95 percent confidence interval of the marginal effect ranges from –.082 to –.221. The predicted probability of re-incarceration for male parolees who returned to the same parish as where they were convicted is .265. By contrast, the predicted probability for males released to a different parish is .110. As noted, these inferences pertain to the local average treatment effect for compliers—that is, the effect of migrating to a different parish for parolees who otherwise would have moved back to the same parish had Hurricane Katrina not occurred.

Re-incarceration typically results from one of two paths: a new criminal conviction or a parole revocation due to violations of the conditions of parole (e.g., a failed drug test). To determine if the significant treatment effect of migration identified in the first column of results is robust to these two different forms of recidivism, I re-estimated the model using parole revocation and new conviction as dependent variables. The second and third columns in Table 2 reveal that individuals who migrated to a different parish were significantly less likely to be re-incarcerated, whether through parole revocation or a new criminal conviction.

Given that offenders with multiple prior incarcerations have drastically higher rates of recidivism than do first releases (Rosenfeld et al. 2005), it may be the case that repeat offenders are relatively immune to the apparent benefit of residential migration. The risk factors that propel repeat offenders toward crime, and the disadvantages associated with the mark of a criminal record, may render the influence of a change of residence essentially meaningless. Table 3 explores this possibility by estimating Equations 1 and 2 separately by group.

Table 2. Instrumental Variable Probit Estimates of Re-incarceration

	Re-incarceration		Parole Revocation		New Conviction	
	Coef.	Robust SE	Coef.	Robust SE	Coef.	Robust SE
Different Parish from Conviction	–.597	(.156)***	–.586	(.151)***	–.481	(.226)*
Individual-Level						
Black	–.004	(.084)	–.009	(.090)	.024	(.066)
Male	.163	(.054)**	.132	(.063)*	.316	(.109)**
Married	–.205	(.056)***	–.236	(.051)***	–.019	(.098)
Age at release	–.008	(.002)***	–.008	(.002)***	–.009	(.003)**
Time served	–.034	(.013)**	–.031	(.015)*	–.045	(.007)***
First release	–.304	(.076)***	–.320	(.085)***	–.127	(.046)**
Context and Criminal Justice System						
Unemployment rate	.067	(.072)	.037	(.085)	.198	(.129)
Average weekly wage	–.003	(.005)	–.004	(.005)	.004	(.006)
Average household income	.003	(.002)	.003	(.002)	.007	(.005)
Dissimilarity	.021	(.040)	.021	(.044)	–.025	(.073)
Fair market rent	–.008	(.004)*	–.007	(.004)*	–.011	(.006)*
Average parole contacts	–.005	(.012)	–.009	(.011)	.017	(.022)
Judge caseloads	.000	(.001)	.000	(.001)	–.004	(.002)
UCR arrests per crime (parish)	–.244	(.245)	–.407	(.508)	.017	(.108)
Intercept	.382	(.402)	.436	(.469)	–1.260	(.663)*
N	4,639		4,514		3,817	

Note: Re-incarceration includes both parole revocations and new convictions. The instrument Z_i is a binary indicator of the release period (pre- versus post-hurricane). The coefficient and standard error for average household income are multiplied by 1,000. Coefficients and standard errors for all other context and criminal justice system measures, except UCR arrests per crime, are multiplied by 10. Significance tests are calculated from robust standard errors. The analytic sample for the Parole Revocation model excludes new conviction recidivists. The sample for the New Conviction model excludes parole revocation recidivists.
* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$ (one-tailed test).

Table 3. IV Probit Estimates of Re-incarceration, First Release versus Repeat Offenders

	First Release		Repeat Offenders	
	Coef.	Robust SE	Coef.	Robust SE
Different Parish from Conviction	–.684	(.195)***	–.499	(.202)**
Individual-level				
Black	.044	(.084)	–.141	(.110)
Male	.208	(.069)**	.075	(.072)
Married	–.191	(.063)**	–.230	(.093)**
Age at release	–.008	(.004)*	–.009	(.003)**
Time served	–.035	(.014)**	–.042	(.047)
Context and Criminal Justice System				
Unemployment rate	–.013	(.088)	.204	(.185)
Average weekly wage	.001	(.005)	–.007	(.004)*
Average household income	.002	(.002)	.006	(.003)*
Dissimilarity	–.003	(.045)	.056	(.053)
Fair market rent	–.009	(.005)*	–.007	(.008)
Average parole contacts	–.008	(.007)	.000	(.025)
Judge caseloads	.000	(.001)	.000	(.001)
UCR arrests per crime (parish)	–.482	(.636)	–.136	(.108)
Intercept	.134	(.376)	.410	(.693)
N	3,275		1,364	

Note: The instrument Z_i is a binary indicator of the release period (pre- versus post-hurricane). The coefficient and standard error for average household income are multiplied by 1,000. Coefficients and standard errors for all other context and criminal justice system measures, except UCR arrests per crime, are multiplied by 10. Significance tests are calculated from robust standard errors.
* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$ (one-tailed test).

Results reveal that all parolees, whether first releases or those with multiple prior incarcerations, are significantly less likely to be re-incarcerated if they migrate to a different parish. While the likelihood of re-incarceration within one year differs for these two groups, with a probability of .174 for first releases and a probability of .270 for repeat offenders, the marginal effect of migration is equivalent for the groups (.15).

Sensitivity Analyses

To assess whether findings are robust to different operationalizations of the recidivism measure, the treatment variable, the sample, and the instrumental variables, I perform a series of sensitivity analyses, with results presented in Table 4.[14] First, I re-estimate Equations 1 and 2 with an alternative measure of recidivism, which combines indicators of re-incarceration and detainers. Recall that detainers refer to incarcerations in another jurisdiction (i.e., federal, another state, or local). Results in the first column of Table 4 reveal that there is some variation in the correlates of this second measure of recidivism relative to the re-incarceration model in Table 2, but I still find that parolees who migrate to a different parish relative to where they were originally convicted are significantly less likely to recidivate (re-incarcerated or detained) within one year of prison release. The probability of re-incarceration or detention is .08 lower for parolees who did not move back to the parish where they were originally convicted, relative to parolees who did return. The decline in the treatment effect of migration relative to the .15 effect from the re-incarceration model in Table 2 is likely due to the detained measure containing relatively more minor forms of detention (e.g., short-term stays in jail following an arrest). Residential migration thus appears to be more consequential for desistance from serious forms of criminal offending. Claude Brown's (1965) memoir is instructive in this regard. After moving to Greenwich Village,

[14] For the four different sensitivity analyses, only one model specification is varied at a time (e.g., I do not test alternate measures of recidivism and treatment at the same time).

Table 4. Sensitivity Analyses: Instrumental Variable Probit Estimates of Re-incarceration

	(1) Alternate Outcome Re-incarceration/Detained		(2) Alternate Treatment Re-incarceration (Distance)		(3) Alternate Sample Re-incarceration (Orleans)		(4) Alternate IV Re-incarceration (5 IVs)	
	Coef.	Robust SE	Coef.	Robust SE	Coef.	Robust SE	Coef.	Robust SE
Treatment								
Different parish from conviction	–.217	(.073)**			–.388	(.207)*	–.534	(.188)**
Distance of migration (miles)			–.004	(.001)***				
Individual-Level								
Black	.047	(.084)	.021	(.058)	–.358	(.092)***	–.001	(.088)
Male	.240	(.040)***	.172	(.050)***	.203	(.074)**	.165	(.058)**
Married	–.112	(.049)*	–.242	(.065)***	–.344	(.128)**	–.209	(.052)***
Age at release	–.013	(.002)***	–.008	(.003)***	–.006	(.003)*	–.008	(.002)***
Time served	–.007	(.006)	–.034	(.013)**	–.043	(.012)***	–.035	(.012)**
First release	–.225	(.047)***	–.320	(.071)***	–.210	(.053)***	–.303	(.073)***
Context and Criminal Justice System								
Unemployment rate	–.167	(.059)**	.001	(.050)	.220	(.179)	.041	(.083)
Average weekly wage	.005	(.005)	–.007	(.004)	–.017	(.008)*	–.003	(.005)
Average household income	–.001	(.002)	.002	(.002)	.011	(.006)*	.003	(.002)*
Dissimilarity	.003	(.029)	.028	(.030)	.062	(.083)	.018	(.039)
Fair market rent	–.001	(.002)	–.006	(.003)*	–.001	(.006)	–.007	(.004)
Average parole contacts	.011	(.013)	–.001	(.011)	–.002	(.023)	–.004	(.010)
Judge caseloads	.000	(.001)	.000	(.001)	–.004	(.002)**	.000	(.001)
UCR arrests per crime (parish)	–.169	(.104)	–.260	(.310)	.014	(.117)	–.254	(.263)
Intercept	–.122	(.465)	.357	(.392)	.654	(.658)	.315	(.314)
N	4,639		4,639		2,207		4,639	

Note: In Model 4, “5 IVs” denotes that the model was estimated with five instruments measuring the interaction between time period of release (pre- versus post-Katrina) and parish of conviction (Orleans, Jefferson, Plaquemines, St. Bernard, or St. Tammany). The coefficient and standard error for average household income are multiplied by 1,000. Coefficients and standard errors for all other context and criminal justice system measures, except UCR arrests per crime, are multiplied by 10. Significance tests are calculated from robust standard errors. For Model 3, the sample is restricted to parolees originally convicted in Orleans Parish.

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

Brown avoided the serious forms of criminality that had led to his detention in juvenile reform institutions. However, he did not abstain from criminal behavior altogether (e.g., he frequently used marijuana and occasionally got into fights). Residential migration may thus lead to a de-escalation to less serious forms of offending, rather than a total cessation from criminal behavior.

To assess the sensitivity of results to the type of treatment variable used, I re-estimated Equations 1 and 2 with an alternative treatment measure. For analyses performed up to this point, I used a binary treatment measure indicating whether parolees moved to a different parish relative to where they were originally convicted. As an alternate treatment, I use a measure of distance indicating how far parolees moved relative to the parish where they were originally convicted (i.e., distance of migration). In this sense, the measure of distance is analogous to a treatment dosage, and the model reveals whether the level of dosage affects the likelihood of re-incarceration. Results in the second set of columns in Table 4 once again demonstrate the significant effect of migration on re-incarceration. In this case, the marginal treatment effect of migration is equivalent to a .01 reduction in the likelihood of re-incarceration for every 10 miles parolees migrate away from their prior parish.

Recall that the sample used in this study is made up of prisoners originally convicted in metropolitan areas affected by Hurricane Katrina (i.e., Orleans, Jefferson, Plaquemines, St. Bernard, and St. Tammany parishes). As a third sensitivity analysis, I restrict the sample to individuals who were convicted in Orleans Parish and re-estimate Equations 1 and 2 with this restricted sample. Given that Orleans Parish typically has a higher crime rate than the other parishes (Federal Bureau of Investigation 2007), residential migration away from criminogenic areas may be particularly advantageous for individuals convicted in Orleans Parish, while less advantageous for individuals convicted in lower crime areas. The third model in Table 4 reveals that there is a significant negative effect of migration on re-incarceration for parolees who were originally convicted in Orleans Parish, although the treatment effect is smaller relative to the full sample (the marginal effect of treatment is .15 for the full sample and .11 for the Orleans Parish sample). For comparison purposes, I also estimated the model excluding the Orleans convictions but including individuals convicted in the four other parishes, and I found a marginal treatment effect of .19 (results available from the author on request). In sum, I find a negative effect of migration regardless of the pre-incarceration location of the parolee, although the size of the treatment effect varies by parish. One potential reason for this divergence may be related to distance. If the likelihood of recidivism declines with distance from the original parish, then the lower marginal effect of treatment for parolees convicted in Orleans Parish may be due to the fact that movers did not migrate very far when they left the parish (at least relative to movers from other parishes). Such a scenario is a worthy topic for future investigation.

As a final sensitivity analysis, I re-estimated Equation 1 using an alternative specification of instrumental variables. As described earlier, property damage from Hurricane Katrina varied by parish; St. Bernard and Plaquemines parishes suffered the most damage, while Jefferson Parish had the least amount of damage. As depicted in Figure 1, the likelihood of a parolee moving to a given parish post-incarceration was influenced by the extent of housing destruction in the parish. Given that both the time period of release from prison (pre- versus post-Katrina) and the geographic variation in property damage affect whether parolees will return to the same parish where they were originally convicted, I use an interaction between time period of release and parish of conviction as an alternative specification of instrumental variables.[15] With this specification, I assume that

[15] In essence, I have five instruments. The first instrument (post-Katrina release and Orleans conviction) equals 1 for all parolees who were released following 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. I entered all five instruments into Equation 1 simultaneously. One key benefit of estimating a model with more instruments than endogenous regressors (i.e., treatments) is that I can test the IV assumption that the dependent variable and instruments are unrelated (see the Appendix for details).

whether parolees migrate to a different parish or not depends on whether they were released from prison before or after Hurricane Katrina *and* where they were originally convicted.

Findings presented in the fourth model in Table 4 are consistent with inferences derived in Table 2. Parolees who migrated to a different parish were significantly less likely to be re-incarcerated, such that the marginal effect of re-incarceration is .14 lower for parolees who did not move back to the parish where they were originally convicted, relative to parolees who did return.

In summary, findings suggest that moving away from former places of residence serves some benefit for parolees. A change of residence allows individuals to separate from their peers and the temptations that contributed to their criminality in the first place. This finding holds for both first releases and repeat offenders, and it holds under alternative specifications of the dependent variable, treatment variable, sample, and instruments.

DISCUSSION

Selection bias is very much an issue with research on both residential migration and neighborhood effects. The innovation of this study has been to use a natural experiment to investigate a theoretical question whose answer has largely eluded researchers because of selection—just how consequential is a change of residence to behavioral outcomes such as crime? In the absence of perfect information on why a given individual moves to one place of residence versus another, it is difficult to answer this question because omitted variables may introduce bias into estimates of the effects of migration. While tragic, the residential destruction and migration resulting from Hurricane Katrina present a unique opportunity for understanding how place and migration affect outcomes such as crime and recidivism.

This study addresses whether separating individuals from their former residential environments reduces their likelihood of recidivism. My findings support this assertion. In particular, the marginal effect of migrating to a different parish upon release from prison is a .15 decline in the probability of re-incarceration. This finding holds for both first releases from incarceration and parolees with multiple prior incarcerations.

We must consider whether such findings are internally valid. Recall my discussion of the exclusion restriction, which states that the instrument and the outcome variable are unrelated except indirectly through the effect of the instrument on the causal treatment. The validity of the IV analysis rests on the assumption that any effect of the time period of release (pre- versus post-Katrina) on the likelihood of re-incarceration must be captured through the effect of time period on residential migration. Tests presented in the Appendix provide support for the assumption that the instruments and the re-incarceration measure are not significantly correlated. While I cannot completely rule out the possibility of violating the exclusion restriction, my findings suggest that even with such a violation, the potential bias would not wholly eliminate the sizable treatment effect observed in this study.

The next step in this causal story is to further investigate why there is such a powerful treatment effect from changing place of residence. For instance, the causal mechanism underlying the effect of migration may be related to social ties. Parolees who migrate to a different parish may be more likely to sever ties with criminal peers than would parolees who return to the same parish. They would therefore have less opportunity and provocation for engaging in crime. While I reason that changes to peer associations and routine activities explain why migration is consequential to recidivism, I attempted to account for several alternative explanations for the reduced likelihood of recidivism among parolees who migrated. For instance, judges may be less likely to convict or sentence an offender to prison and parole officers may be less likely to revoke the parole of parole violators in destination parishes, relative to the original parishes. By controlling for court, parole, and police practices in my statistical models, I attempted to determine if migration has a causal effect on recidivism after consideration of the variation in criminal justice practices across space and time.

Similarly, to account for the possibility that parolees who migrated were more likely to abscond and leave Louisiana, I examined the effect of migration not only on re-incarcerations in Louisiana (i.e., Table 2), but also on detentions in local, federal, or other state facilities (i.e., the first column in Table 4). Still, the potential for unmeasured variation in the operation of the criminal justice system across space and time presents a possible alternative explanation for the effect of migration on recidivism (in contrast to the hypothesized mechanisms of changes in peer association and routine activities). Exploration of mechanisms underlying the causal effect of migration is an important avenue for future research.

Yet, even before the full causal story bears out, the findings established thus far with respect to residential migration and recidivism have significant policy implications. If separating ex-prisoners from their former residential environments actually benefits those prisoners without sacrificing public safety, then a logical next step is to consider how to disperse the population of ex-prisoners on a large scale. Research by the Urban Institute suggests that substantial proportions of returning prisoners would welcome the opportunity to move away from their former criminogenic environments (Visher and Farrell 2005). For instance, in a sample of recently released males from the Illinois Department of Corrections, Visher and Farrell (2005) find that 45 percent of returning prisoners explicitly expressed a desire to move to a different neighborhood than where they lived prior to prison. Over half of this group wished to live in a different neighborhood to avoid drugs and the other temptations and troubles of familiar settings.

Yet many ex-prisoners still end up moving back to their former counties and neighborhoods despite an expressed interest to avoid such places. One prime reason is because of legal barriers. In most states, prisoners released to some form of parole supervision are legally required to return to their county of last residence (National Research Council 2007). Criminal justice policies in most states are designed to return prisoners to the same familiar surroundings where they got into trouble with the law in the first place. In Louisiana, there are no such geographic restrictions. The findings from Louisiana presented in this study suggest that allowing ex-prisoners to move to different parishes or counties will reduce recidivism. It may thus be fruitful for states to reconsider the residency restrictions imposed on returning prisoners.

As noted, however, the sizable treatment effect of moving found in this study is only applicable to parolees who moved to a different parish post-Katrina who otherwise would have moved back to where they lived before. To fully explore the policy implications of this finding, we must understand why prisoners return to their former parishes and neighborhoods even in a state like Louisiana where there are no legal barriers preventing parolees from moving to a different parish. We know from the Urban Institute research that another key reason why released prisoners move back to their former neighborhoods is to be close to family (Visher and Farrell 2005). To capitalize on the apparent benefits of residential migration, it may thus be necessary to both remove legal barriers preventing ex-prisoners from moving and provide ex-prisoners and their families with opportunities and incentives to move away from old neighborhoods. While forcing ex-prisoners to move away from their old neighborhoods is neither realistic nor ethical, providing opportunities for ex-prisoners to move away from old neighborhoods is a policy prescription that may be worth pursuing.

Before enacting policy initiatives to promote residential change among the parole population, more research must be done to both validate the findings demonstrated in this study and uncover the mechanism underlying the treatment effect. With respect to the former, replicating the current study may prove challenging given that the research design is based on a natural experiment. One possibility for future research is to contrast recidivism rates of states that require prisoners to return to their county of last residence with states that do not. A second approach would be to identify states that changed the residency restrictions imposed on released prisoners and then compare recidivism rates before and after the changes were made.

Two particular extensions to the current study would add another layer of nuance to understanding the relation between migration and recidivism. First, results presented in Table 4 reveal a negative relationship between distance of migration and recidivism, yet it remains unclear just how far parolees must move to separate themselves from past situations and criminal associates. For Claude Brown (1965), the roughly seven miles between Harlem and Greenwich Village was sufficient to separate him from the daily criminal temptations of his former social environment. The key question is whether there is a distance threshold after which parolees have a substantial decline in the likelihood of recidivism. Moreover, does a parolee's likelihood of desisting from crime increase with each increment after this threshold, or does it remain approximately the same after passing the threshold? Further research is needed to answer such questions. Second, as Abbott (1997:89) notes, "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." To more definitively conclude that Hurricane Katrina and the resulting migration produced turning points in the life course of crime for many former prisoners, it is thus necessary to follow up with the same cohorts of individuals in future analyses to determine if the observed differences in recidivism across cohorts remain after a sufficient passage of time (e.g., three to five years).

In addition to these various opportunities to further investigate the relation between residential migration and recidivism, other worthy research topics include the repercussions of macro-structural changes due to Katrina and the psychological strain associated with such a tragedy. With respect to macro-structural change, Katrina not only influenced residential decisions at the individual-level but also the macro-level distribution of parolees and criminals more generally. De-concentrating the spatial clustering of criminals, just like de-concentrating poverty, may attenuate the cultural disorganization that characterizes so many urban neighborhoods, as well as reverse the deleterious consequences of social isolation (Sampson and Wilson 1995). With respect to psychological strain, one plausible hypothesis is that strain associated with the hurricane would increase the likelihood of recidivism. The fact that I found a sizable treatment effect despite the likely psychological strain from Katrina means that the effect of migration may in fact be underestimated. Still, the psychological impact of Hurricane Katrina may bear upon recidivism, and it is a worthy topic of investigation.

While there are plenty of opportunities for future research on the effects of Katrina, as well as the relation between migration, place of residence, and recidivism, findings thus far suggest that successful prisoner reentry and reintegration depend on providing opportunities for prisoners to separate from their criminogenic past. Such a finding provides initial support for the life-course propositions outlined earlier. If criminal peers and familiar criminogenic contexts causally influence criminal behavior, separating individuals from their peers and familiar contexts should lead to a reduced likelihood of criminal behavior. That is precisely what I find. In a practical sense, to lessen the likelihood of recidivism and to foster the path to desistance, it is beneficial to separate ex-prisoners from their criminal past.

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APPENDIX: ASSUMPTIONS OF THE IV FRAMEWORK

We must consider potential violations of the IV framework before concluding that the treatment effect of residential migration described in Tables 2, 3, and 4 is in fact valid. As mentioned previously, one key assumption 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. While the exclusion restriction is not directly verifiable, it is possible to indirectly assess this assumption in overidentified models where there are more instruments than endogenous regressors. Specifically, for the results presented in Table 2, models are just identified—that is, the model has the same number of instruments as endogenous variables (one). Yet with five instruments for the fourth model estimated in Table 4, the model is overidentified. It is thus possible to indirectly test the exclusion restriction—that is, the instruments are uncorrelated with the model error—by correlating the residuals from the estimation of Equation 2 with the excluded instruments. I use Hansen's (1982) *J* statistic (see also Baum, Schaffer, and Stillman 2003) to test the joint null hypothesis that the instruments are in fact valid instruments (i.e., they are uncorrelated with the error term).[16] The J statistic for the fourth model estimated in Table 4 equals 5.996 ($p = .200$). I thus fail to reject the null hypothesis, thereby providing necessary support for the exclusion restriction.

A second key assumption of the IV framework is that the covariance between the treatment and instrument differs from zero: $\text{cov}(S_i, Z_i) \neq 0$. If the instrument (i.e., post-Katrina release) does not affect residential migration, then use of Katrina as an instrument is inappropriate. Results from Figure 1 support the assumption that the treatment and instrument covary, in that the spatial distribution of the parole population changed following Hurricane Katrina. However, even if the treatment and instrument are significantly associated, problems may arise in the second-stage estimation (Equation 2) if they are only weakly related (i.e., a nonzero yet small correlation). This is known as the weak instruments problem (Staiger and Stock 1997). A primary consequence is that weak instruments produce inconsistent IV estimators (Bound, Jaeger, and Baker 1995). Additionally, a weak instrument increases standard errors of the IV estimates and therefore affects hypothesis testing.

To assess the potential for a weak instrument, it is useful to examine the explanatory power of the instrument in the first-stage (i.e., Equation 1) of the two-stage approach. For the re-incarceration model from Table 2, to assess the instrument's explanatory power, I use an F-test of the significance of the instrument. Results reveal that the instrument, post-Katrina release, is significantly correlated with the treatment variable ($F = 60.25$; df = 1, 46; $p < .001$). Yet, a statistically significant F statistic may still represent bias in the IV estimator, so the F statistic must be compared against a suitable threshold. Staiger and Stock (1997) suggest that an F statistic below 10 is indicative of a weak instrument. In the case here, the instrument post-Katrina release is statistically significant and far exceeds Staiger and Stock's threshold. Such findings alleviate concerns over violating assumptions of the IV approach.[17]

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[16] Tests of IV assumptions are available in Stata through the *ivreg2* function.

[17] Another common strategy for assessing weak instruments is to examine the partial R^2 of the first-stage equation, which isolates the squared partial correlation between the instrument and the endogenous regressor (Bound et al. 1995). The magnitude of the bias of IV estimators increases as the partial R^2 approaches zero. First-stage results from the re-incarceration model in Table 2 reveal a partial R^2 of .173.

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