The Effect of Hurricane Katrina on Prisoner Reentry in Louisiana: A Natural Experiment

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Lessons from Hurricane Katrina:
A Natural Experiment of the Effect of Residential Change on Recidivism

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ABSTRACT

Ex-prisoners tend to be geographically concentrated in a relatively small number of neighborhoods within the most resource deprived sections of metropolitan areas, often returning to the same neighborhoods where they resided prior to incarceration. Yet estimating the causal impact of place of residence on the likelihood of recidivism is typically complicated by selection bias—the possibility that some unmeasured characteristic of prisoners influences both where they live and their criminal behavior, and may therefore account for any relation between place of residence and recidivism. In this study, I utilize a natural experiment as a means of addressing the selection issue. In August 2005, Hurricane Katrina ravaged the Louisiana Gulf Coast, damaging many of the neighborhoods where ex-prisoners typically reside. Given the neighborhood residential destruction resulting from Hurricane Katrina, it is unclear whether the resulting geographic displacement of returning prisoners has had any adverse, or even beneficial, effects on the reintegration of former prisoners. Through the use of a natural experiment, this study seeks to establish whether the dispersal of ex-prisoners away from their former place of residence will lead to lower levels of recidivism. Findings suggest that moving away from former geographic areas substantially *lowers* an ex-prisoner'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 (*Manchild in the Promised Land*, 1965:178)

INTRODUCTION

If the incarceration rate in the United States remains stable at current levels, 6.6 percent of U.S. residents born at the beginning of the millennium will spend time in prison during their lifetimes, including an expected 33 percent of black males (Bonczar 2003). Roughly 5.6 million individuals in the United States, or 1 out of every 37 adults, have served time in prison (Bonczar 2003), with 1.4 million individuals currently serving time in state prisons and another million in federal and local facilities (Sabol and Harrison 2007).

The repercussions of mass incarceration become apparent when considering the number of prisoners likely to be released each year for the foreseeable future. A study of nationwide prisoner reentry trends conducted by the Bureau of Justice Statistics estimates that 95 percent of state prison inmates will be released from prison at some point during their lifetimes, with an average length of stay in prison under three years (Bureau of Justice Statistics 2007; Hughes and Wilson 2004). The Bureau of Justice Statistics also reports that nearly 700,000 prisoners were released from state and federal facilities in 2005 (Sabol and Harrison 2007), and estimates suggest that up to half of these releasees have been in prison before (Langan and Levin 2002). In fact, by some estimates, two-thirds of returning prisoners in the United States are re-arrested within three years of prison release, and half are re-incarcerated (Langan and Levin 2002). These figures

should not be separated from the social context in which prisoners return. Recent 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 neighborhoods where they resided prior to incarceration (La Vigne, Mamalian, et al. 2003). It is also true that the neighborhoods where ex-prisoners often return are bereft of economic opportunities and characterized by concentrated poverty and high crime rates.

The facts of prisoner reentry seem clear: substantial numbers of prisoners are released each year from U.S. prisons, and a large proportion of these ex-prisoners return to the same disadvantaged, criminogenic context where they resided prior to incarceration.[1] Most of these ex-prisoners will be back in confinement within three years. 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—i.e., the possibility that some unmeasured characteristic of prisoners influences both where they live and their criminal behavior, and may therefore account for any relation between place of residence and recidivism. It may be the case that individuals with a high propensity towards criminal offending and therefore a high likelihood of recidivism simply select into certain geographic contexts, and that the characteristics of these contexts have little causal bearing on individuals' behavior. If scholars of social context and "neighborhood effects" are to make any kind of causal statements about the impact of place of residence or residential mobility on a given outcome measure, they must address the issue of selection bias by separating out selection effects from any true neighborhood effects.

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

In this study, I utilize a natural experiment as a means of minimizing the potential for selection bias. Specifically, Hurricane Katrina and the tragic events that followed have implications for prisoner reentry, and in particular, the geographic context of reentry.[2] For instance, data released by the Federal Emergency Management Agency (FEMA) reveal that in Orleans Parish, 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). While the tragedy of Hurricane Katrina has certainly been felt by members of all racial, ethnic, and socioeconomic groups, research reveals that members of those social groups with the least resources were affected most by the storm (Logan 2006). In the city of New Orleans, 29.2 percent of the population in damaged areas had incomes below the poverty line in comparison to 24.7 percent of the population in unaffected areas, and 10.4 percent of the population in damaged areas were unemployed relative to 7.2 percent in unaffected areas (Logan 2006). Furthermore, the population of damaged areas was 75 percent black compared to 46.2 percent in unaffected areas. Thus, the impacts of Katrina were felt the most in those New Orleans neighborhoods marked by poverty, unemployment, and high proportions of black population, and it is in these types of urban neighborhoods where prisoners disproportionately reside upon release from prison (e.g., La Vigne, Mamalian, et al. 2003; Clear, Waring, and Scully 2005).

Given the neighborhood residential destruction resulting from Hurricane Katrina,

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[2] Hurricane Katrina initially developed on August 23rd, 2005 near the Bahamas, and made its first landfall on the 25th near Ft. Lauderdale as a Category 1 hurricane. The storm then moved into the Gulf of Mexico, and rapidly intensified, becoming a Category 5 storm by the 28th. At roughly 6:10am on August 29th, Katrina made its second landfall, in southern Louisiana, and then moved inland towards New Orleans. One of the levees in New Orleans breached from the storm a few hours later, and another levee breached the next day.

it begs the question of where ex-prisoners resided in the period immediately following the hurricane if many of the New Orleans metropolitan area neighborhoods were uninhabitable. It also remains unclear whether the likely geographic displacement of returning prisoners because of Hurricane Katrina had any adverse, or even beneficial, effects on the reintegration of former prisoners. For instance, one hypothesis would be that hurricane-related destruction produced barriers for prisoners to reunite with their *families* and *social support networks*, thus making recidivism more likely. On the other hand, if prisoners were also blocked from reuniting with their *criminal peers*, recidivism may be less likely.

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

The paper proceeds as follows. As a starting point I situate the study by describing the social and geographic context of prisoner reentry in the United States. I then outline theoretically why social context is consequential for prisoner reentry and recidivism. After that, I address two empirical objectives. I first describe the geographic pattern of prisoner reentry in Louisiana before and after Hurricane Katrina by mapping the addresses upon release for returning prisoners. I then estimate the causal effect of hurricane-induced geographic displacement on the likelihood of recidivism among ex-prisoners.

THE SOCIAL CONTEXT OF PRISONER REENTRY

Ex-prisoners tend to be geographically concentrated in a relatively small number of neighborhoods within the most resource deprived sections of metropolitan areas. 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, Mamalian, et al. 2003). These six communities areas (Austin, East Garfield Park, Englewood, Humboldt Park, North Lawndale, West Englewood) are among the most economically and socially disadvantaged in the city. As but one example, in 2000 the poverty rate in North Lawndale was 41.7 percent in comparison to a city-wide average of 16.6 percent, and the unemployment rate stood at 25.8 percent relative to 10.1 percent city-wide (Northeastern Illinois Planning Commission 2002). In Maryland, nearly 60 percent of released prisoners return to the city of Baltimore, with 30 percent of those returning to Baltimore concentrated in just six neighborhoods (La Vigne, Kachnowski, et al. 2003). As with Chicago, released prisoners to Baltimore tend to reside in the most disadvantaged sections of the city. Similar patterns of concentrated reentry into urban areas have also been found in Georgia (La Vigne and Mamalian 2004), Ohio (La Vigne and Thomson 2003), and Texas (Watson et al. 2004).

As with prisoner reentry, prison admissions predominate from a select number of neighborhoods within metropolitan areas. For instance, Lynch and Sabol (2001) find that a mere 3 percent of the census block groups in Cuyahoga County in Ohio (which includes Cleveland) account for over 20 percent of the state's prison population, with an expected 350 to 700 ex-prisoners returning to those very same block groups each year following

prison release. Thus, prison admissions derive from select communities, prison releases concentrate in those same communities, and most prisoners return "home" to the same neighborhoods where they resided prior to incarceration.

Given that prisoner reentry tends to concentrate into mostly impoverished neighborhoods in metropolitan areas, and that most prisoners "return home" to their old neighborhoods, it is important to develop a theoretical understanding for why these findings are consequential. Insights from the "neighborhood effects" literature and the desistance from crime literature provide such a theoretical foundation.

PLACE OF RESIDENCE AND RECIDIVISM

In their review of the neighborhood effects literature, Jencks and Mayer (1990) describe a number of causal models which suggest why neighborhood conditions affect various individual outcomes. One particular model, the contagion model, offers some clues for the means by which a prisoner's residential neighborhood influences her/his likelihood of recidivism. Contagion models focus on the influence of neighborhood peers, and 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, "[C]riminal 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."

Sutherland's (1947) differential association theory is a prominent version of the contagion model. Sutherland posits that criminal behavior is learned through interaction

in intimate social groups. This proposition helps explain one of the most robust findings in the sociology of crime, namely that the bulk of crimes, particularly for youths, are carried out in groups (Shaw and McKay 1931; Warr 2002). Sutherland also argues that the process by which individuals learn criminal behavior is influenced by the broader social organization in which they are embedded, including the organization of neighborhoods. Put simply, in some neighborhoods individuals are more likely to be exposed to other individuals who spread information about the techniques for committing crimes and the motives and rationalizations for committing crimes.

If crime is in fact contagious, and is primarily a group phenomenon, then it follows that removing individuals from their criminogenic social networks should reduce their likelihood of engaging in criminal behavior.[3] Research evidence supports such a contention. For instance, Warr (1998) dissects the reasons why marriage is such a powerful predictor of desistance from crime, and argues 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 alters the amount of time individuals spend with delinquent and non-delinquent peers. Moreover, his analysis reveals that the association between marriage and criminal behavior disappears once controlling for peer relationship characteristics. The importance of Warr's study is two-fold: first, it reveals that criminal behavior is a function of peer influence, and second, it demonstrates that one way to minimize criminal behavior is to separate individuals from their peers.

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[3] Note there is a distinction between peer influence and group offending. The former refers to mechanisms by which one's peers influence her/his behavior, while the latter refers to the fact that much criminal behavior takes place in groups. Criminal behavior may still occur in groups even if group members are not exerting "influence" over each other. Thus, separating an individual from a criminal network may reduce her/his criminal behavior by two mechanisms: 1) by severing the peer influence process, and 2) by eliminating the allure of engaging in group behavior.

Of course, marriage is not the only mechanism which can disrupt networks of criminal peers. In a study of the life-course of crime from childhood 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 that leads to desistance from crime. 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 delinquents who stayed in London. Osborn investigates whether the lower level of reconviction among movers is a function of changing attitudes, or a function of changing circumstances and eliminating temptations, and finds support for the latter assertion. Thus, in both the Laub and Sampson study and that of Osborn, authors find that a change of residence allows individuals to separate from past situations and criminal peers, thus eliminating some of the factors which contribute to the persistence of criminal behavior.

In the popular literature, perhaps the most illustrative example of the benefits of separating from criminogenic environments and criminal peers is found in Claude Brown's (1965) semiautobiographical account, *Manchild in the Promised Land*. The lessons of crime and delinquency began at an early age for Brown. As a 5-year-old, his peers taught him how to skip school and to avoid truant officers, and how to steal. 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 a very deviant 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 and other residents of mid-century Harlem. Brown did

not enjoy his brief experimentation with the drug. Yet another very good 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 routines and temptations of such a

familiar environment. He reflects (1965),

> 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, he was able to both physically and mentally separate

from many of the criminogenic influences in his life.

While there are strong theoretical reasons to expect that peers influence criminal

behavior, and that a separation from peers contributes to desistance from such behavior,

alternative explanations need to be considered. Glueck and Glueck (1950:164) argue that

delinquents simply seek out other delinquents, just as "birds of a feather flock together."

Similarly, Gottfredson and Hirschi (1990) argue that the association between peers and

crime is in fact a spurious relationship, and that low self-control is an antecedent

predictor of both crime and an individual's likelihood of associating with delinquent

peers. The question is one of causal order and the potential for selection bias: do criminal

peers causally influence an individual's behavior, or do criminals simply select other

criminals as their friends?

There are numerous potential threats to internal validity, but one of particular concern for the design of a study of peer effects or neighborhood effects is the threat of selection bias. Selection refers to the process of assigning individuals to conditions (i.e., treatment versus control groups). Selection bias may occur when an unobserved or unmeasured characteristic of an individual influences both where s/he lives and the outcome under study, and may therefore account for any relation between place of residence and the outcome. Similarly, selection bias may occur when an unmeasured characteristic of an individual influences her/his selection into a peer group and the outcome under study, and may therefore account for any relation between peer effects and the outcome.

Few empirical studies have directly addressed the issues of endogeneity and selection bias with respect to peer influence on criminal or delinquent behavior.[4] Of the exceptions, Case and Katz (1991) find support for the contagion argument. Specifically, using an instrumental variables approach to address selection, they find that youth residing in neighborhoods where other youths are engaging in crime and other acts of delinquency have an increased likelihood of engaging in those same behaviors. They also note, "our finding that the estimated effects of family background variables are not much reduced by the inclusion of neighbors' variables suggests that our estimates of neighborhood effects do not largely reflect the possibility that neighbors' variables may proxy for mismeasured family background variables" (Case and Katz 1991:22-23). In other words, neighborhood effects are not an artifact of the systematic sorting of families into neighborhoods.

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[4] The peer/contagion effect is endogenous in the sense that, "the propensity of an individual to behave in some way varies with the prevalence of that behaviour in some reference group containing the individual" Manski 1993:531).

In contrast to the Case and Katz study (1991), Evans, Oates, and Schwab (1992) find no support for peer effects on teenage pregnancy and school dropout after controlling for the individuals' selection into particular peer groups. These authors use simultaneous equations to estimate peer effects, factoring in that to some extent individuals choose their peers on the basis of household residency decisions. Results demonstrate that peer influences on teenage pregnancy and school dropout disappear after consideration for the endogeneity of peer groups.

In sum, there are strong theoretical reasons to expect that peers influence criminal behavior, though empirical evidence on peer effects has been mixed. The theoretical implication derived from the contagion and desistance literatures is that to lessen the likelihood of recidivism, it is beneficial to separate ex-prisoners from their criminal past and their peers. Sorting out the causal consequences of neighborhood effects and peer effects requires a research design that explicitly addresses the issue of selection bias. In this study I attempt to answer the first part of a two part causal story. I seek to uncover *whether* separating individuals from their former criminogenic environment reduces their likelihood of recidivism. If separation does reduce the likelihood of recidivism, the second part of the causal story is to investigate *why*. One potential answer is contagion. I save the investigation of *why* for future analyses.

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 the displacement due to Hurricane Katrina, I restrict analyses to those prisoners who resided in affected metropolitan areas prior to incarceration. Accordingly, the analytic sample only includes 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. Thus, for those prisoners released since Hurricane Katrina, arguably their residential choices are significantly different than 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. Because of this, I opt to exclude these individuals from the study.

For the purposes of analyses, I construct three cohorts of prison releasees, two of which were released from prison prior to Hurricane Katrina and one released afterwards. The first cohort is comprised of *first-releases* from a Louisiana prison to parole supervision anytime from September 2001 to April 2002 (hereafter called the 2001-2002 cohort).[5] By *first-releases*, I refer to individuals who were released from their first stint of incarceration in a Louisiana prison. The second cohort consists of first-releases onto

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[5] 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 by the Louisiana Department of Public Safety & Corrections 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). Prisoners to be released onto parole supervision are required to provide the Division of Probation and Parole a residence plan prior to release, which includes the parish and address where they will reside. Prisoners in Louisiana are not required to return to the same parish where they were convicted. To distinguish my analytic sample of paroled releases from unconditional releases, throughout the analysis I will utilize the term "parolee."

parole supervision between September 2003 and April 2004 (the 2003-2004 cohort). The third cohort consists of first-releases onto parole supervision between September 2005 and April 2006 (the post-Katrina cohort). The selection of years for the 2003-2004 cohort is designed to allow for a one-year observation period for re-incarceration, which still occurs prior to events associated with Hurricane Katrina.

My rationale for only sampling parolees released from their *first* term of imprisonment is that parolees as a whole are a very heterogeneous group with widely varying rates of recidivism (National Research Council 2007). More specifically, parolees released from their first term of imprisonment have, on average, substantially lower rates of recidivism than parolees with multiple prior incarcerations. Rosenfeld and colleagues (2005) find that first-releases from incarceration accumulate between 18 and 25 percent fewer subsequent arrests during the three years immediately following incarceration relative to those ex-prisoners who had at least one prior incarceration. Given that I have only limited information on criminal history, I restrict my sample to first-releases in order to ensure equivalence of cohorts and to ensure that I am not omitting relevant predictors of variation in recidivism (i.e., criminal history). Another consequence of restricting my sample to first releases is that later cohorts will *not* contain any of the same parolees as earlier cohorts.

Data utilized in this study are of three varieties: a) individual-level data on parolees from the Louisiana Department of Public Safety & Corrections (DPS&C) and the Division of Probation and Parole (DPP)[6], b) zip code and parish-level characteristics from the U.S. Department of Housing and Urban Development, the Louisiana

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[6] DPS&C data is maintained in the Corrections and Justice Unified Network (CAJUN) database, and DPP data is maintained in the Case Management System.

Department of Labor, and ESRI, and c) Louisiana criminal justice system data from the

Supreme Court of Louisiana, DPS&C, DPP, and the Uniform Crime Reports.

Given that social and economic conditions in Louisiana changed drastically

immediately following Hurricane Katrina, it is necessary to control for such temporal

changes in order to make causal statements about the relation between residential change

and recidivism. As but one example of temporal change, estimates a little over one year

after Hurricane Katrina placed the population of Orleans Parish at just 191,139 residents

(as of October 2006; Louisiana Public Health Institute 2006). Estimates two years after

the storm placed the population at 264,969 residents (ESRI 2007). In contrast, the

estimated population of Orleans Parish just prior to Hurricane Katrina was 437,186

residents (Brookings Institution 2006). ESRI provides yearly demographic estimates of

the U.S. population at both the county/parish level and zip code level. I utilize these

demographic 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 FEMA on property damage and applications for assistance, from the United

States Postal Service National Change of Address file, and from the American Red Cross

on estimated housing unit damage.[7] ESRI estimates for 2006 reflect data current as of

January and February of 2006 (ESRI 2006b).

Similarly, the implications of Hurricane Katrina for temporal changes in the

criminal justice system in Louisiana are many (for detailed discussions, see Garrett and

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[7] Description of the ESRI methodology can be found in the cited references, and from the following website: http://www.esri.com/data/community_data/demographic/methodology.html. Data used in this study are available for purchase from ESRI, and are also published in annual editions of the *Community Sourcebook of County Demographics* and the *Community Sourcebook of ZIP Code Demographics*.

Tetlow 2006; Roman, Irazola, and Osborne 2007). Most obviously, many justice officials

evacuated from New Orleans in the wake of the storm, and some did not return. The

Orleans Parish District Attorney's office flooded, and office staff were forced to work

from temporary locations following the storm. The Orleans Parish courthouse also

flooded. Makeshift courts were setup in prisons and other facilities, but the result was

ultimately a delay in legal proceedings in the Orleans Parish Criminal District Court.

Given the impact of Katrina 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 in order to draw any kind of causal inference about the effect of residential

change on recidivism. Thus, I include control variables in analyses 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 given 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.

RE-INCARCERATED OR DETAINED. While I rely upon Louisiana data in this study, it

may be true that a proportion of the release cohorts from Louisiana will 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, the commitment of parolees to other

state correctional systems may be limited, given that parolees are required to remain in

Louisiana and make periodic visits with their parole officer. Information on non-

Louisiana incarcerations, as well as incarcerations in a local facility (as opposed to state),

is captured in the DPP case management system in a field for supervision level. A parolee's supervision will be marked as "detained" if that parolee is confined in another jurisdiction.[8] In the interest of 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. In other words, this secondary measure of recidivism indicates whether a given parolee was re-incarcerated in a Louisiana prison within one year, or was detained in another jurisdiction.

Different Parish as Conviction. This is a binary variable indicating whether the parolee moved to a different parish following incarceration relative to where s/he was originally convicted. In contrast to some other states, prisoners in Louisiana are <u>not</u> required to return to the same parish where they were convicted or last resided. I geocoded the address of residence for a given parolee immediately following her/his release from prison, and compared this to the parish of conviction. This variable equals zero for those parolees who returned to the same parish as where s/he was convicted, and one for parolees who moved to a different parish.

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 to follow.

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[8] 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 her/his 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 officers also search the National Law Enforcement Telecommunications System (NLETS) as well as local warrant systems to identify the location of absconders. If absconders are detained in a local or non-Louisiana facility, information in the DPP data system is updated accordingly.

The study includes five additional individual-level measures as correlates of recidivism, all derived from prisoner data in the DPS&C CAJUN database system: race, gender, age at time of release, marital status, and time served. Black parolees comprise 72.7 percent of the sample, with whites making up 27.2 percent. Other races make up 0.1 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, with female as the reference category. *Married* is a binary variable indicating the marital status of the parolee at the time of her/his release (not married equals zero). *Time served* refers to the amount of time a parolee served in prison (in years or fraction thereof) until release.[9]

CONTEXTUAL-LEVEL VARIABLES

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

Average Household Income (Sqrt). This is a measure of the average household income (in 2000 adjusted dollars) in the zip code in which a given parolee resides. For the 2001-2002 cohort, I use the 2002 ESRI household income estimates, and for the

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

2003-2004 and 2005-2006 cohorts I use respective estimates from the 2004 and 2006

data. To reduce the skewness of the measure, I utilize the square root of income in the

probit models estimated to follow.

Unemployment Rate. This is a measure of the parish unemployment rate in a

parolee's parish of residence in the quarter during which the prisoner was released from

prison. Data derive 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 a given parolee resides for the quarter during which

s/he was released from prison. Data derive from the Louisiana Department of Labor.

Fair Market Rent. This is a measure of the average fair market rent in the parish

where a given parolee resides. Data derive from the U.S. Department of Housing and

Urban Development (HUD). For the 2001-2002 cohort, I use the fair market rent in 2002

for a given parolee's parish of residence, while for the 2003-2004 and 2005-2006 cohorts

I utilize rent averages from 2004 and 2006, respectively. All figures are adjusted to 2000

dollars.

CRIMINAL JUSTICE SYSTEM VARIABLES

Average Parole Contacts. This measure derives from the DPS&C Quarterly

Statistical Performance Report, which provides data on the operations of the correctional

and parole systems, including information on the average number of interpersonal

contacts made per parole officer in a given parole district. Research suggests that there is

an inverse relationship between parole officer caseload and recidivism, because smaller

caseloads lead to more contacts with parolees and more scrutiny (Turner and Petersilia

1992). Thus, it is vital to control for temporal variations in the average parole contacts, as well as between parole district variation. The measure used in analyses is the average number of contacts made by parole officers in the parolee's parole district during the quarter in which s/he was released from prison. Contacts include those which occurred within the parole office as well as field contacts to the parolee's residence or workplace.

Judge Caseloads. This measure derives from the Louisiana Supreme Court's annual report, which provides data on the operations of the Louisiana court system. Each year, the Supreme Court reports the number of cases filed in each District court as well as the number of judges. There is a vast body of research which examines the relation between judge caseloads and criminal case dispositions (e.g., Dixon 1995; Feeley 1979; Heumann 1975). One key issue in this regard is whether large caseloads pressure courts towards the use of plea bargaining and result in lenient sentencing practices because of the need to process large numbers of defendants (e.g., Eisenstein, Flemming, and Nardulli 1988; Nardulli 1979). 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. Thus, I 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 or parole. For the 2001-2002 cohort, I use the number of cases per judge in 2002 in a given parolee's parish of residence, while for the 2003-2004 and 2005-2006 cohorts, I use caseload figures from 2004 and 2006, respectively.

UCR Arrests per Crime. This measure derives from the FBI's Uniform Crime Reports, and is used 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, and arson) given the commission of such a crime. For the 2001-2002 cohort, I construct this measure using the ratio of Part I arrests per reported Part I crimes in 2001 for a given parolee's parish of residence, while for the 2003-2004 and 2005-2006 cohorts I use the 2003 and 2005 ratios, respectively.

ANALYTIC STRATEGY

Analyses follow two paths to coincide with the objectives of the study. In the first set of analyses, I test two hypotheses. 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 fewer returns to former neighborhoods. This expectation leads to Hypothesis 1a:

Hypothesis 1a. Because of hurricane-related destruction, significantly fewer parolees moved to New Orleans following incarceration 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.

However, I also expect that the structural barriers that lead to a concentration of prisoner reentry into the most impoverished urban neighborhoods will result in ex-prisoners migrating to other disadvantaged urban areas besides those found in New Orleans. In addition, the residential patterns of prisoner reentry are likely affected by the increased competition for housing resulting from the forced migration of Louisiana Gulf Coast residents to geographic areas unaffected by Hurricane Katrina (and Hurricane Rita). These observations lead to Hypothesis 1b:

Hypothesis 1b. The <u>types</u> of geographic areas where parolees typically reside, in terms of poverty and economic conditions, are similar or even more disadvantaged for the post-Katrina period relative to the pre-Katrina period.

To examine these hypotheses, I geocode and map the post-release addresses for the three separate cohorts of parolees, and provide illustrations of how the geographic distribution of prisoner reentry changed post-Katrina. To address Hypothesis 1b specifically, I compare the three parolee cohorts with respect to the five measures of social and economic context described previously.

With regards 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. In other words, I propose that the forced migration due to Hurricane Katrina serves some benefit in that it allows for a separation between parolees and their criminal past, thus reducing the likelihood of re-incarceration. These propositions yield Hypotheses 2a and 2b:

Hypothesis 2a. The proportion of ex-prisoners re-incarcerated for a new conviction or parole violation is significantly lower in the post-Katrina period.

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

Hypotheses 2a and 2b are related, but do address slightly different questions. Hypothesis 2a tests the "Katrina Effect"—i.e., the difference in the likelihood of re-incarceration for those prisoners released post-Katrina relative to those released pre-Katrina. In contrast, Hypothesis 2b is focused specifically on the impacts of residential change on recidivism. The distinction lies in the fact that some parolees post-Katrina did

return to the parish where they resided prior to incarceration. In other words, while Katrina is consequential to residential change following imprisonment, not everyone moved to a different parish.

I employ two different modeling strategies to address these hypotheses: an intention-to-treat analysis (ITT) to address Hypothesis 2a and an instrumental variables analysis (IV) to address Hypothesis 2b. For the ITT analysis, I provide an estimate of the difference between the post-hurricane (the treatment group) and two pre-hurricane cohorts (the control group) on the likelihood of re-incarceration. In the language of counterfactual reasoning, my interest is in estimating the recidivism outcome for a given parolee if s/he was released from prison for the first time following Hurricane Katrina versus the counterfactual condition of being released from prison for the first time prior to Katrina. For each specific parolee, however, we can only observe one outcome. Thus, it is not possible to directly calculate the causal effect of Hurricane Katrina for a specific prisoner. Given this restriction, one alternative is to use an exogenous source of variation to assign comparable individuals to control and treatment groups, and then estimate the treatment effect. In this case, the control individuals serve as the counterfactual.

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. Without random assignment, it is not necessarily the case that treatment and control groups are identical. Thus, it is beneficial to include statistical controls for parolee characteristics and other relevant factors in statistical models in order to account for any differences. In equation form, re-

incarceration is estimated as a function of the release cohort (i.e., the ITT effect, Z_i) and a

set of controls:

$$Y_i = Z_i\theta_1 + X_i\beta + \varepsilon_i \tag{1}$$

where

Y_i is a dichotomous variable indicating whether parolee i was re-incarcerated within
one year of release;
X_i is a vector of control variables used to account for any observed differences
between the pre- and post-hurricane cohorts;
Z_i is a binary indicator of treatment assignment, which in this case is the release
cohort (i.e., the *Post-Katrina Release* binary variable).

Because the treatment and control groups are defined by the time period of prison

release (i.e., before or after the hurricane), in essence the ITT estimate provides

inferences as to whether there are any differences in re-incarceration between the two

groups. While informative, the ITT estimates provide no information on causal

mechanisms. In other words, it is not possible to draw conclusions as to *why* the pre-

versus post-Katrina cohorts differ on recidivism. However, combining the natural

experiment method used to estimate the ITT with an IV approach allows for a greater

examination of the mechanisms by which the geographic displacement of prisoner

reentry influences recidivism. Thus, to test Hypothesis 2b I compare the likelihood of re-

incarceration with 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. Here, the causal mechanism is residential mobility.

In addition to the exploration of causal mechanisms, combining a natural

experiment with an IV approach allows me to further control for selection bias. With a

natural experiment alone, a simple comparison of the treatment and control groups may

be biased by unknown or unmeasured factors which influence residential mobility

(besides whether the parolee was released pre- or post-Katrina). Combining the natural experiment with an IV approach can provide an unbiased estimate of the effect of moving to a different parish on re-incarceration (Angrist, Imbens, and Rubin 1996). 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 then the outcome variable is 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 prisoners. 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. In other words, we can have more confidence that the instrument and outcome variable are unrelated if that 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—i.e., $cov(Z_i, u_i) = 0$—and is not directly verifiable (Angrist et al. 1996). The validity of inferences from an IV analysis depends upon the appropriateness of this assumption. In the *Discussion* section following the presentation of results, I address potential violations of the exclusion restriction.

To test Hypothesis 2b, the treatment condition is whether, upon release from prison, a parolee moved to a parish different from where s/he was originally convicted. Thus, the control group is comprised of prisoners who returned to the same parish upon release, while the treatment group consists of prisoners who moved to a different parish upon release. As Angrist and Krueger (2001) demonstrate, an IV remedies the issue of

omitted variables (e.g., the fact that I lack complete information for why a given parolee selects one parish of residence versus another) 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. They note (Angrist and Krueger 2001:77), "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 did not affect those parolees who would have moved even in the absence of the hurricane.[10] In short, use of an IV approach allows me to compute an estimate of the effect of moving to a *different* parish for those parolees who otherwise would have moved back to the *same* parish instead had Hurricane Katrina not occurred (this is termed 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 those parolees who would have moved regardless of the hurricane.

The two structural equations specified to follow outline the two-stage estimation process with the IV technique. The first equation models the key explanatory variable S_i, a binary measure indicating whether parolee i resides in a *different* parish upon release relative to where s/he was convicted prior to incarceration, as a function of an instrumental variable Z_i and a vector of control variables X_i. The instrument Z_i is a binary indicator of the release cohort (post-Katrina versus otherwise). It is assumed that where a

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[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 who joined the military did so voluntarily, and some did so because of the draft. His IV estimates of the effects of military service on future earnings are applicable for draftees but not volunteers. In other words, he estimates the effects of military service on the earnings of draftees, yet does not capture the effect of military service on earnings for volunteers.

parolee resides depends, in part, upon whether s/he was released from prison before or after Hurricane Katrina.

$$S_i = Z_i\theta_1 + X_i\pi + \xi_{it} \tag{2}$$

The second stage of the two-stage estimation process models the dependent variable (Y_i), a dichotomous variable indicating whether parolee i was re-incarcerated within one year of release, as a function of the predicted S_i from Eq.2 and a vector of control variables X_i:

$$Y_i = \alpha S_i + X_i\beta + u_i \tag{3}$$

The coefficient α is the key parameter of interest, and will be used to draw inferences as to whether returning to one's old parish upon release influences a parolee's likelihood of recidivism.[11]

RESULTS

HYPOTHESES 1A AND 1B

To examine Hypotheses 1a and 1b, I present a series of tables as well as a descriptive figure to compare and contrast the geographic patterns of prisoner reentry pre- and post-Katrina. Figure 1 provides a snapshot of the geographic redistribution of parolees post-Katrina. More specifically, this figure reveals in which parish parolees reside immediately upon their exit from prison. The release figures from 2001-2002 and 2003-2004 reveal that roughly 45 percent of prisoners convicted in the 5-parish area (i.e., Orleans, Jefferson, Plaquemines, St. Bernard, and St. Tammany) subsequently returned to New Orleans. Post-Katrina, this number drops to 21 percent. In the post-Katrina period, proportionally more parolees opted to reside in Baton Rouge (2 percent before Katrina

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[11] I estimate equations (2) and (3) in STATA using the *ivprobit* function. Model standard errors are adjusted to account for the clustering of parolees within parishes.

versus 8 percent after), and many other parolees dispersed throughout the state, with

sizable numbers residing in Lafayette and Lake Charles. This figure demonstrates that the

proportion of newly released prisoners (who were originally committed from the greater

New Orleans metropolitan area) who returned to the 5-parish area upon release declined

drastically immediately following Katrina. This finding provides support for Hypothesis

1a. Moreover, recall that one benefit of using an instrument derived from a natural

experiment is to assure that said instrument is correlated with the treatment condition

(i.e., moving) but otherwise unrelated to the outcome variable (i.e., re-incarceration).

Results presented in Figure 1 suggest that the time period of release (pre versus post-

Katrina) is highly correlated with the treatment condition.[12]

[FIGURE 1 ABOUT HERE]

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 moved 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 = 155.383, $p < 0.001$),

with 55 percent of parolees returning to the same parish and 45 percent moving to a

different one. This finding provides additional support for Hypothesis 1a.

[TABLE 1 ABOUT HERE]

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[12] A key assumption of the IV framework is that the covariance between the treatment and instrument differs from zero: $cov(S_i, Z_i) \neq 0$. Results from Figure 1 support this assumption. The substantial correlation between the instrument and the treatment is also confirmed via the ivprobit estimation—equations (2) and (3) are estimated jointly, and the correlation between *Post-Katrina Release* and *Different Parish as Conviction* is positive and highly significant ($p < 0.001$).

To examine Hypothesis 1b, Table 2 presents a descriptive summary of the geographic areas where the respective cohorts reside. This table displays the percentages of parolees within each cohort which fall within a given category along the distributions of the five measures I utilize to describe social and economic context.

As noted previously, the impacts of Hurricane Katrina were felt proportionally more in those New Orleans neighborhoods marked by poverty and high proportions of black population. Consequently, demographic shifts following Hurricane Katrina have altered the evenness of the population distribution in the city of New Orleans and the wider metropolitan area. Population estimates from the ESRI data as well as enrollment figures from New Orleans public schools (Brookings Institution and Greater New Orleans Community Data Center 2007) reveal that black residents have been relatively less likely to return to New Orleans post-Katrina than white residents. Specifically, data reveal that prior to the hurricane, black residents comprised roughly 68 percent of Orleans Parish residents and whites made up 27 percent. Post-Katrina, the share of black population has declined to 44 percent and the share of white population has increased to 50 percent. Estimates also suggest that Latino groups have migrated to the metropolitan area in substantial numbers in the post-Katrina period. Given that a sizable portion of parolees post-Katrina still reside in the metropolitan area, the demographic shifts in New Orleans impact the likelihood that parolees will reside in an area characterized by segregation. Findings presented in Table 2 support this assertion. Post-Katrina parolees are significantly more likely to reside in an area characterized by segregation and less likely to reside in an integrated area relative to their pre-Katrina counterparts.

[TABLE 2 ABOUT HERE]

Trends in the unemployment rate and weekly wages combine to produce offsetting economic consequences for returning prisoners. Reports suggest that the New Orleans metropolitan area lost roughly 4,000 firms in the year following Katrina, equating to 15 percent of the regional employer base (Brookings Institution and Greater New Orleans Community Data Center 2007). At the same time, the labor force size plummeted with the evacuation of the metropolitan area; yet, it has been steadily increasing since the first anniversary of the storm. Unemployment rates in the New Orleans metropolitan area initially spiked following the storm, reaching 15.8 percent in November 2005 (according to Department of Labor data). Yet owing to the decline in the labor force and the availability of jobs to rebuild the metropolitan region, unemployment declined to as low as 3.8 percent by December 2006, well below the national average and pre-Katrina levels. Spurred by a shortage of workers in key industries such as construction and hospitality, the average weekly wage in the New Orleans metropolitan area increased significantly following Katrina. For instance, construction wages increased by 41 percent one year after Katrina, and wages for professional and technical services jumped 53 percent (Brookings Institution and Greater New Orleans Community Data Center 2007). What have these trends meant for prisoners released soon after Hurricane Katrina? Data presented in Table 2 reveal that post-Katrina parolees resided in areas with significantly higher wages than those individuals released from prison pre-Katrina. At the same time, a significantly higher unemployment rate characterized the post-Katrina context for parolees. The average household income (by zip code) remained similar pre- and post-Katrina, which is partially a function of the offsetting effects of unemployment and wage increases.

With fair market rents, temporal patterns of prisoner reentry reflect the fact that the supply of rental housing in the New Orleans metropolitan area declined significantly following Katrina due to property destruction, while at the same time the demand for rental units from displaced residents significantly increased (Brookings Institution and Greater New Orleans Community Data Center 2007). HUD data reveal that the average fair market rent in Orleans Parish rose a staggering 39 percent from 2005 to 2006, from $676 for a two-bedroom apartment to $940. Rents also rose in other geographic areas where parolees concentrated post-Katrina. For instance, rents climbed 18.4 percent in Baton Rouge, from $608 to $720 for a two-bedroom apartment. The consequence is that post-Katrina parolees tended to reside in geographic areas characterized by significantly higher rents than their counterparts released prior to Katrina.

Taken as a whole, findings presented in Tables 1 and 2 as well as Figure 1 suggest that prisoners released post-Katrina may be residing in different geographic areas, but these areas are similar to the types of geographic contexts where parolees concentrated pre-Katrina. In other words, the geographic distribution of prisoner reentry changed, but the qualities of social and economic context did not. If anything, the socio-economic context of prisoner reentry became slightly more disadvantaged following the hurricane. Such findings provide support for Hypothesis 1b.

HYPOTHESES 2A AND 2B

Figure 2 examines whether parolees who returned to the same parish as where they were originally convicted were any more or less likely to recidivate relative to parolees who moved to different parishes. While re-incarceration has been declining in more recent

years, it is also true that for all three cohorts those parolees who returned to the same parish as where they were originally convicted were more likely to be back in prison within one year.

[FIGURE 2 ABOUT HERE]

Table 3 demonstrates the effect of Hurricane Katrina on re-incarceration (i.e., the ITT results). Black parolees, on average, are more likely to be re-incarcerated within one year than parolees of other race and ethnicity. As expected, males are more likely to be re-incarcerated than females. Those parolees who are married are less likely to be re-incarcerated. Thus, as documented in numerous studies (e.g., Laub and Sampson 2003; Warr 1998), marriage has a controlling influence on criminal behavior, and is a significant contributor to desistance from crime. Age is negatively related to re-incarceration, and so is time served. With respect to the contextual-level and criminal justice system covariates, only fair market rents are significantly associated with re-incarceration. In this case, higher rents correspond to a greater likelihood of re-incarceration, which may indicate that parolees are turning to crime in order to keep up with rising costs of housing.

[TABLE 3 ABOUT HERE]

The key parameter of interest in Table 3 is that for *Post-Katrina Release*. Results reveal that parolees released following Katrina are significantly and substantially *less* likely to be re-incarcerated within one year of release. Translating these findings to a marginal effect, the probability of re-incarceration within one year of prison release is 0.09 lower for parolees released post-Katrina relative to parolees in the 2001-2002 cohort and the 2003-2004 cohort (net of individual, contextual, and criminal justice system

correlates). The predicted probability of re-incarceration for male parolees released prior to Katrina is 0.204, while the predicted probability for males released after the hurricane is 0.113. These findings provide support for Hypothesis 2a.

Results thus far reveal that there has been a geographic redistribution of prisoner reentry post-Katrina and that recidivism declined. Whether these two processes are causally related 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. 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 s/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—i.e., 55 percent of parolees released post-Katrina ($Z = 1$) moved to the same parish ($S = 0$), and 26.1 percent of parolees across the two pre-Katrina cohorts ($Z = 0$) moved to a different parish ($S = 1$). Thus, while it is adequate to assume that the *assignment* to treatment is ignorable (i.e., conditional on the instrument Z and the other covariates from equation (2), 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-Katrina and post-Katrina cohorts on recidivism will not provide an unbiased estimate of the *average causal effect* of moving to a different parish on recidivism. Yet, through the use of IV methods, I am able to compute an unbiased estimate of the effect of moving to a different

parish on re-incarceration for those parolees who otherwise would not have moved had it not been for Hurricane Katrina (i.e., an estimate of LATE).[13] Results reported to follow represent the LATE estimate.

Table 4 presents the IV probit results of re-incarceration. With the exception of the coefficient for race, the associations between individual-level covariates and re-incarceration are similar to findings presented in Table 3. The key parameter of interest is that for *Different Parish as Conviction*, which is a binary measure indicating whether a given parolee resides in a different parish upon release relative to where s/he was convicted prior to incarceration. Results show that those individuals who moved to a different parish were significantly and substantially *less* likely to be re-incarcerated within one year of release. With respect to the marginal effect, the probability of re-incarceration within one year of prison release is 0.15 lower for parolees who did not move back to the parish where they were originally convicted relative to parolees who did (net of individual, contextual, and criminal justice system correlates). The 95 percent confidence interval of the marginal effect ranges from -0.081 to -0.219. The predicted probability of re-incarceration for male parolees who returned to same parish as where they were convicted is 0.240. In contrast, the predicted probability for males released to a different parish is 0.085. As noted, these inferences pertain to the local average treatment effect for compliers—i.e., the effect of moving to a *different* parish for those parolees

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[13] In essence, through the IV method, I adjust the recidivism difference between the post-Katrina cohort versus pre-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):
$\{E[Y_i \mid Z_i = 1] - \{E[Y_i \mid Z_i = 0]\} / \{E[S_i \mid Z_i = 1] - \{E[S_i \mid Z_i = 0]\} = \alpha$. With perfect treatment compliance, the denominator equals one and the treatment effect α simply equals the Intention-to-Treat (ITT) effect.

who otherwise would have moved back to the *same* parish instead had Hurricane Katrina

not occurred.

[TABLE 4 ABOUT HERE]

SENSITIVITY ANALYSIS

To assess whether findings are robust to a different operationalization of recidivism, I re-

estimate equations (2) and (3) with a combined measure of re-incarceration and detainers.

Results are presented in Table 4. While there is some variation in the correlates of this

second measure of recidivism relative to the previous model, I still find that those

parolees who return 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

release. The probability of re-incarceration or detention within one year of prison release

is 0.098 lower for parolees who did not move back to the parish where they were

originally convicted relative to parolees who did.

In sum, findings suggest that moving away from former places of residence serves

some benefit for parolees in that a change of residence allows individuals to separate

from their peers and the temptations that contributed to their criminality in the first place.

DISCUSSION

Selection bias is very much an issue with research on "neighborhood effects" and

residential mobility. The innovation of this study has been to utilize a natural experiment

in order to investigate a theoretical question whose answer has largely eluded researchers

because of selection—just how consequential are place of residence and residential

mobility 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 problematic to answer questions like this one. While tragic, the residential destruction and migration resulting from Hurricane Katrina present a unique opportunity for understanding how place and mobility affect outcomes such as crime and recidivism.

In this study I have addressed *whether* recidivism declined post-Katrina and *whether* separating individuals from their former criminogenic environment reduces their likelihood of recidivism. Findings support both assertions. In particular, the marginal effect of moving to a different parish upon release from prison is nearly a 0.15 decline in the probability of re-incarceration.

Whether such a finding is internally valid as well as generalizable to other settings is critical to consider. With respect to validity, 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. In other words, the validity of my 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 mobility. The instrument should not be related to any other factors besides residential mobility that are predictive of re-incarceration. Results presented in Table 4 reveal that none of the contextual and criminal justice system covariates included in the analysis, which are likely correlated with Hurricane Katrina, are predictive of incarceration. In other words, my instrument has little partial effect on re-incarceration through these covariates because none of these covariates are predictive of re-incarceration.

While I have utilized a variety of data in order to measure as many relevant predictors of recidivism as possible, it is plausible that there are unobserved changes in the post-Katrina context which are correlated with both my instrument and re-incarceration, thus biasing my results. One such unobservable factor may be neighborhood collective efficacy (Sampson, Raudenbush, and Earls 1997). If Hurricane Katrina is correlated with temporal variation in collective efficacy and, in turn, collective efficacy is related to re-incarceration, then I have violated the exclusion restriction and may therefore have bias in the findings presented in Table 4. Unfortunately, I am unaware of any studies which have examined the relation between collective efficacy and recidivism, and therefore have little guidance as to whether collective efficacy is correlated with recidivism. Still, it is instructive to point out that while collective efficacy is predictive of neighborhood rates of crime (Morenoff, Sampson, and Raudenbush 2001; Sampson et al. 1997), a number of recent studies reveal that collective efficacy is not related to arrest or violence *at the individual-level* (Kirk 2008; Sampson, Morenoff and Raudenbush 2005). Moreover, research generally supports the notion that geographic units, particularly at the parish or county level, are far more internally heterogeneous than externally differentiable (Cook, Shagle, and Degirmencioglu 1997). I use this discussion to suggest that even if some unobserved contextual factor, such as collective efficacy, is correlated with my instrument, it is very likely the case that it has at most a modest effect on re-incarceration. In sum, I cannot rule out the possibility of violating the exclusion restriction, but I suggest that even with such a violation the potential bias would not wholly eliminate the sizable treatment effect observed in Table 4 (i.e., the 0.15 difference

in the probability of re-incarceration for those parolees who returned to the same parish as where they were convicted relative to those who moved to a different one).

As for the generalizability of my findings, one likely critique of the research design employed in this study is that it derives from a tragic and devastating disaster, which will hopefully never occur again. Thus, is the context of post-Katrina Louisiana so unique that inferences are generalizable to just this one period and place in time? If we focus on the repercussions of forced or induced migration, then I think the lessons from Katrina easily translate to other settings. For instance, forced migration has become all too commonplace among public housing residents, as nearly 80,000 severely distressed housing units have been demolished in the United States since the enactment of HOPE VI program by Congress in 1992 (Turner et al. 2007). While I am unaware of any studies that specifically address the impact of forced migration through public housing demolition on recidivism, the study of educational impacts of such demolitions provides a blueprint for future research on forced migration (see, e.g., Jacob 2004). Thus, if we focus broadly on causal mechanisms—i.e., induced migration and residential mobility— then the lessons from Katrina offer some clues to the likely impacts of public housing demolition for criminality. More generally, I have unraveled a causal story of migration and mobility which were spurred by Hurricane Katrina, but there are plenty of other examples of forced or induced migration which likely have implications for criminal behavior.

The next step in this causal story is to further investigate *why* there is such a powerful treatment effect from changing place of residence. Yet, even before the full causal story bears out, findings established thus far with respect to residential mobility

and recidivism have significant policy implications. If separating ex-prisoners from their old neighborhoods actually benefits those prisoners (in terms of reintegration) 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 Courtney 2006; Visher and Farrell 2005). For instance, in a sample of 400 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, with over half of this group desiring to do so in order to avoid drugs and the other temptations and troubles of familiar neighborhood 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). Thus, 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. Findings from Louisiana presented in this study suggest that allowing ex-prisoners to move to different parishes or counties will reduce recidivism. Thus, it may be fruitful for states to reconsider the residency restrictions imposed upon returning prisoners. Yet, as noted, the sizable treatment effect of moving found in this study is applicable to just those parolees who moved to a different parish post-Katrina who otherwise would have moved back to the where they

lived before. To fully explore the policy implications of this finding, it is necessary to 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 same Urban Institute research that another key reason why released prisoners move back to their former neighborhoods is to be close to family and friends (Visher and Farrell 2005). Thus, to capitalize on the apparent benefits of changing place of residence, it may be necessary to both remove legal barriers for ex-prisoners to move as well as provide ex-prisoners and their families with opportunities and incentives to move away from old neighborhoods. In other words, forcing ex-prisoners to move away from their old neighborhoods is not realistic or ethical. But providing opportunities for ex-prisoners to move away from old neighborhoods is a policy prescription that may be worth pursuing.

In advance of enacting policy initiatives to promote residential change among the parole population, more research needs to be undertaken to both validate the findings demonstrated in this study and to uncover the mechanism underlying the treatment effect (i.e., *why* it is that mobility to new surroundings has such a consequential impact on the likelihood of recidivism). With respect to the former, replicating the current study may prove challenging given that the research design is based upon a natural experiment. That said, similar investigations could be carried out in other geographic areas which have been affected by hurricanes and other natural disasters. Another possibility for further research is to contrast recidivism rates of states which require prisoners to return to their county of last residence versus those states which do not. A third approach would be to identify states which changed the residency restrictions imposed upon released prisoners

and then compare recidivism rates before and after such changes were made. Finally, a fourth worthwhile effort would be to explore the impacts of migration from the aforementioned housing demolitions on recidivism.

While more research on mobility, place of residence, and recidivism is needed, findings thus far suggest that successful prisoner reentry and reintegration depends upon providing opportunities for prisoners to separate from their criminogenic past. Such a finding provides support for the propositions of the contagion and desistance literatures outlined previously. If criminal peers and familiar criminogenic contexts causally influence criminal behavior, then we should see that 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 and their peers.

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Table 1. Comparison of Post-Incarceration Place of Residence

| | **Post-Incarceration Residence** | |
Cohort	Same Parish	Different Parish
2001-2002	75.5%	24.5%
2003-2004	72.4%	27.6%
2005-2006	54.8%	45.2%

Notes: "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 = 155.383, $p < 0.001$.

Table 2. Percent of Ex-Prisoners by Category: Social and Economic Context

		2001-2002	2003-2004	2005-2006	Chi-Square	*P* value
Dissimilarity	**0 to 25**	14.3%	12.5%	9.6%		
	26 to 50	80.9%	82.4%	80.2%	51.681	<0.001
	51 to 100	4.7%	5.2%	10.2%		
Average Household Income	**< $25,000**	29.0%	28.3%	27.3%		
	$25 - $50,000	67.9%	69.5%	70.0%	3.884	0.422
	> $50,000	3.1%	2.2%	2.7%		
Unemployment Rate	**0 to 5.0**	36.9%	53.3%	28.1%		
	5.1 to 10.0	63.1%	46.7%	44.5%	1,081.720	<0.001
	10.1+	0.0%	0.0%	27.4%		
Average Weekly Wage	**< $500**	11.8%	8.8%	8.1%		
	$500 - $750	87.7%	91.1%	68.8%	709.383	<0.001
	> $750	0.5%	1.0%	23.1%		
Fair Market Rent	**< $500**	7.0%	5.8%	17.0%		
	$500 - $750	93.0%	94.2%	11.0%	3,335.079	<0.001
	> $750	0.0%	0.0%	71.9%		

Note: Average Household Income, Average Weekly Wage, and Fair Market Rent all adjusted to 2000 dollars.

Table 3. Intention-to-Treat Probit Estimates of Re-Incarceration

	Re-Incarceration		
	Coef.	Robust Std. Err.	
Post-Katrina Release	-0.383	(0.086)	***
Individual-Level			
Black	0.082	(0.035)	*
Male	0.259	(0.065)	***
Married	-0.210	(0.063)	***
Age at Release	-0.009	(0.004)	*
Timeserved	-0.040	(0.011)	***
Context and Crim. Justice System			
Unemployment Rate	-0.051	(0.081)	
Avg. Weekly Wage	-0.006	(0.005)	
Avg. Household Income (Sqrt)	0.000	(0.005)	
Dissimilarity	-0.021	(0.035)	
Fair Market Rent	0.015	(0.004)	***
Ave. Parole Contacts	-0.001	(0.006)	
Judge Caseloads	-0.001	(0.001)	
UCR Arrests per Crime (Parish)	-0.424	(0.466)	
Intercept	-1.040	(0.332)	**

Notes: * $p \le 0.05$ ** $p \le 0.01$ *** $p \le 0.001$

Coefficients and standard errors for all Context and Criminal Justice System measures except *UCR Arrests per Crime* are multiplied by 10. Significance are calculated from robust standard errors.

Table 4. Instrumental Variable Probit Estimates of Re-Incarceration

	Re-Incarceration			Re-Incarceration/Detainer		
	Coef.	Robust Std. Err.		Coef.	Robust Std. Err.	
Different Parish as Conviction	-0.664	(0.179)	***	-0.282	(0.087)	***
Individual-Level						
Black	0.050	(0.053)		0.098	(0.052)	
Male	0.236	(0.082)	**	0.317	(0.052)	***
Married	-0.169	(0.054)	**	-0.096	(0.055)	
Age at Release	-0.009	(0.003)	**	-0.016	(0.002)	***
Timeserved	-0.038	(0.013)	**	-0.011	(0.006)	
Context and Crim. Justice System						
Unemployment Rate	-0.019	(0.080)		-0.116	(0.065)	
Avg. Weekly Wage	-0.001	(0.006)		0.005	(0.006)	
Avg. Household Income (Sqrt)	0.011	(0.008)		-0.003	(0.009)	
Dissimilarity	0.013	(0.042)		0.009	(0.037)	
Fair Market Rent	-0.005	(0.005)		-0.002	(0.004)	
Ave. Parole Contacts	0.008	(0.006)		0.014	(0.005)	*
Judge Caseloads	0.000	(0.001)		0.000	(0.001)	
UCR Arrests per Crime (Parish)	-0.473	(0.474)		-0.093	(0.127)	
Intercept	-0.345	(0.353)		0.978	(0.331)	**

Notes: * $p<=0.05$ ** $p<=0.01$ *** $p<=0.001$
The instrument Z_i is a binary indicator of the release period (pre-hurricane versus post-hurricane). Coefficients and standard errors for all Context and Criminal Justice System measures except *UCR Arrests per Crime* are multipled by 10. Significance tests are calculated from robust standard errors.

Figure 1. Parish of Release
For Prisoners Committed from 5-Parish Area
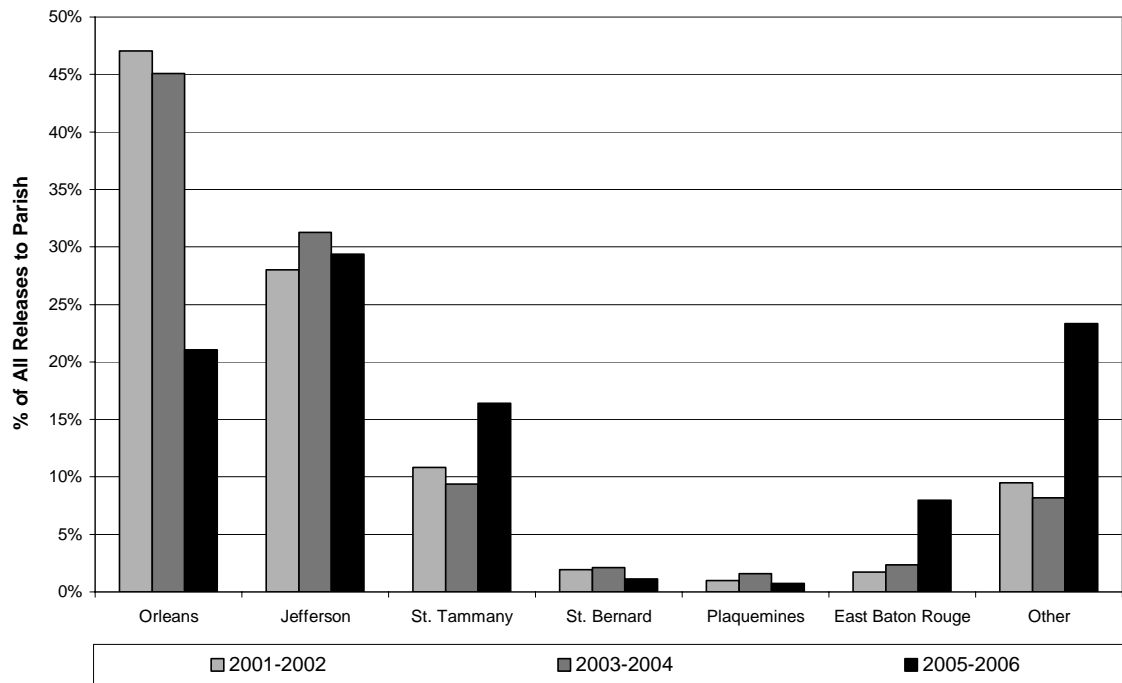
Figure 2. One-Year Re-Incarceration Rate
Prisoners Released to the Same Parish vs. a Different Parish