According to Laub and Sampson (2003), desistance from crime is made possible by “knifing off”—“offenders desist in response to structurally induced turning points that serve as the catalyst for sustaining long-term behavioral change” (p. 149). What turning points create are new situations that allow individuals to knife off the past, in part, by changing those routine activity patterns that led to trouble with the law prior to incarceration (Sampson and Laub, 2005). This idea is straightforward, but the corresponding intervention is extraordinarily complex, as the crippling expenditures on imprisonment and parole and the alarming recidivism rates in the United States clearly reveal.1

How is knifing off achieved? Laub and Sampson (2003) examine the importance of marriage and military service as turning points in the life course that enabled men in their sample to knife off from their past and then desist from crime. Marriage, in part, promotes desistance from crime because it produces changes in individual’s routine activities, including reduction of time spent in unstructured activities and in association with criminal peers (see also Warr, 1998). Yet the marriage–crime association may be spurious. For instance, Gottfredson and Hirschi (1990) offer a competing explanation for the observed negative relationship between marriage and crime found in countless studies. They contend that criminals are shortsighted and that the low level of self-control associated with criminality also causes individuals to discount the long-term benefits of commitments like marriage in favor of short-term gratifications. Ultimately, the marriage–crime association may be spurious, with each
explained by self-control. Failing to account for self-control may lead to biased inferences about the effect of marriage on desistance from crime.

Military service is a striking example of knifing off from the past. Military service in World War II afforded many of the formerly institutionalized individuals in Laub and Sampson’s study (2003; see also Sampson and Laub, 1996) the opportunity to desist from crime. The G.I. Bill provided opportunities for education and socioeconomic advancement following the war, and military service also facilitated desistance by separating individuals from the criminogenic contexts and stigmas associated with their earlier juvenile delinquency. Here again, though, there may be unmeasured characteristics that are related to both crime and military service, particularly voluntary service, that create a spurious association between the two. These characteristics may include education and socioeconomic status as well as impulsivity and risk-seeking behavior (Wright et al., 2005).

The challenge then to estimate the causal effects of knifing off from past criminogenic influences is to either understand and measure the mechanisms by which individuals select into certain environments and institutions (e.g., marriage, military service, work, place of residence), or design away such confounding influences through an approach like experimentation where equivalent individuals are randomly assigned into contrasting contexts. The goal of this chapter is to highlight the challenges of testing Laub and Sampson’s (2001, 2003) theory of desistance—and of testing criminological theories more generally—and to offer one framework for estimating causal effects in criminological research net of selection effects. I confront the issue of selection bias through research design, by means of instrumental variables (IVs). IV techniques are a standard tool in the economics literature, yet have seen limited (although growing) application in criminology. This chapter will demonstrate the utility of IV techniques for testing criminological theory.

This chapter proceeds as follows. First I provide a brief introduction to IVs, focusing attention on the use of IVs to resolve the issue of omitted variable bias. In the interest of brevity, I do not offer a full technical explication of IV methods and its many uses or a detailed literature review of the use of IVs in criminological research. Econometric textbooks and several recent publications already do this (Angrist, 2006; Angrist and Pischke, 2009; Bushway and Apel, 2010). Rather my focus is on explicating the main conceptual and methodological rationale for employing IV methods to test theory. IVs provide a remedy to the issue of selection
bias by using only that portion of the variability in an independent variable (e.g., marriage or military service) that is uncorrelated with omitted variables (e.g., self-control) to estimate the causal relationship between the independent variable and the dependent variable. With an introduction to IV methods in hand, in the second part of the chapter, I turn to an empirical application of IV methods. I use IVs to estimate the causal effect of an underexplored form of knifing off—residential change—on the likelihood of recidivism.

A Primer on IVs

Estimating the causal effect of a correlate of crime such as place of residence on the likelihood of criminal behavior 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 therefore account for any relation between place of residence and recidivism. The same logic can be applied to other commonly recognized correlates of crime. For instance, unobserved characteristics of individuals may be related to the acquisition of criminal peers as well as individual’s behavior, thus rendering the relationship between criminal peers and crime spurious (see, e.g., Glueck and Glueck (1950) and Kornhauser (1978) for critiques of the peer influence hypothesis). Put simply, it may be the case that individuals with a high propensity toward criminal offending self-select into certain geographic contexts (or certain peer groups) and that the characteristics of these contexts have little causal bearing on individual’s behavior.

More technically, a key assumption of standard regression models is that a given treatment or independent variable is uncorrelated with the model’s error term. This is known as the exogeneity assumption. By contrast, the problem of endogeneity occurs when an independent variable is correlated with the error term in a regression model. Why might they be correlated? Two key reasons are simultaneity and measurement error (Angrist and Krueger, 2001; Bushway and Apel, 2010). A third reason in accord with the discussion above is omitted variables. Omitted variables will be captured in the error term of a model. Making valid causal inferences becomes problematic if that error term and the treatment variable are correlated because of an omitted variable. This can be seen in Figure 9.1, in which \( T \) denotes the observed treatment variable and \( X_o \) denotes the unobserved (omitted) variable that is correlated with \( T \). If the model was specified correctly to include the omitted variable, it would look as follows:
If we omit the confounding variable $X$, however, then the coefficient for the treatment effect ultimately yields the following:

$$
Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \epsilon_i. \quad (9.1)
$$

The difference between $\beta_1^*$ and $\beta_1$ (i.e., $\beta_1^* = \beta_1$) is the omitted variable bias. The estimated coefficient for the treatment variable $T$ absorbs the effect of the omitted variable $X_0$. There are two scenarios where $\beta_1^* = \beta_1$, thus meaning there is no omitted variable bias. If there is no association between the dependent variable and the unobserved variable (i.e., $\beta_2 = 0$), or no association between the treatment variable and the unobserved variable (i.e., $\gamma_1 = 0$), then the unobserved variable is not a confounding covariate. If the unobserved variable is related to the dependent and treatment variables, however, then the estimated effect of the treatment variable on the dependent variable will be biased.

The omitted variable bias can be positive or negative; positive bias results in an overestimation of the treatment effect ($\beta_1$) and negative bias leads to an underestimation. If both the effect of the omitted variable on the outcome (i.e., $\beta_1^*$) and the correlation between the omitted variable and treatment are positive (i.e., $\rho_{XT}$), then the bias will be positive.
resulting in an overestimation of the treatment effect. The same is true if the effect of the omitted variable is negative and the correlation with the treatment variable is negative. If, however, either the effect of the omitted variable or the correlation with the treatment variable is positive whereas the other is negative, then the bias will be negative, and the coefficient estimate of the treatment effect will be underestimated. For instance, in an analysis of the effect of a criminal record on wages, omission of a variable on education may lead to an underestimation of the effect of a criminal record—because the correlation between a criminal record and education is likely negative whereas the effect of education on wages is positive. By contrast, omission of a variable measuring the immigration status of an individual may lead to an overestimation of the effect of a criminal record—because both the correlation between a criminal record and immigrant status and the effect of immigrant status on wages are negative (see Kirk and Laub (2010) for a review of research on the effect of immigration on crime).

Suppose we can find a variable that is (1) correlated with the treatment variable yet is (2) uncorrelated with the dependent variable $Y$ except through the treatment variable. Such a variable can be used as an instrument (denoted by $Z$ in Figure 9.1B). An IV ($Z$) for $T$ is one solution to the problem of omitted variable bias. With an IV approach, a variable that satisfies the two conditions above 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. This approach removes the spurious correlation between the treatment variable and omitted variables. As noted previously, 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.

One central challenge to using IVs to resolve the issue of omitted variable bias is uncovering an observed variable that is correlated with the treatment variable yet unrelated to the outcome variable (except indirectly through the treatment variable). Despite the challenge of meeting these conditions, there are plenty of examples in the research literature. In a study of the effect of incarceration length on subsequent labor market outcomes, Kling (2006) argues that the random assignment of criminal cases to judges serves as an IV for incarceration length. In this case, Kling capitalizes on the fact that some judges are more lenient in their sentencing decisions than others. It is assumed that the judge doing the sentencing ($Z$) affects incarceration length ($T$), yet is
uncorrelated with subsequent labor market outcomes ($Y$) except indirectly through the treatment variable. Lochner and Moretti (2004) use changes in state compulsory school attendance laws as an IV for testing the effect of years of schooling on arrest and incarceration, assuming that the timing of the changes in law is unrelated to the outcome variables. In a noncriminological example, Angrist (1990) uses Vietnam-era draft lotteries as an instrument to assess the effect of military service on future earnings. In all these examples, the authors make convincing cases that the respective IVs are related to the treatment effect but are unrelated to the outcome variable except indirectly through the treatment variable.

Note that in all three examples the instrument is derived from some form of a natural experiment. This does not always have to be the case, but again, a key challenge with the use of IVs is finding a variable that is only indirectly related to the outcome (through the treatment). However, the use of an instrument derived from a natural experiment obviates this issue. We can have more confidence that the instrument and the outcome variable are unrelated (except through the treatment condition) if that instrument derives from a random assignment process (Angrist and Krueger, 2001). This assumption is known as the exclusion restriction—that is, $\text{cov}(Z_i, \epsilon_i) = 0$. Random assignment of cases to judges creates an exogenous source of variation, which ultimately influences sentence length. Change in policy, as in the Lochner and Moretti (2004) study, is a common example of a natural experiment. Randomized lotteries, whether related to military drafts, school choice, housing programs, or some other kind of assignment process, are useful as instruments because a convincing case can be made that the random lottery is unrelated to the outcome variable (earnings in the case of Angrist’s (1990) study) except through the treatment variable. In the empirical example presented in the next section, I truly capitalize on the “natural” part of natural experiments by using a random force of nature—a hurricane—as an instrument that affects where people live.

There are several methods to implement an IV analysis, the most common being a two-step estimator (e.g., two-stage least squares). The first stage models the treatment variable $T_i$ as a function of an IV $Z_i$ and a vector of control variables $X_i$ used to account for observed differences between the treatment and control groups:

$$T_i = Z_i \theta_1 + X_i \beta + \zeta_i \quad (9.3)$$
The second stage of the two-stage estimation process models the dependent variable $Y_i$ as a function of the predicted treatment variable from the first stage and a vector of control variables $X_i$. Including statistical controls leads to greater statistical efficiency (i.e., a smaller standard error for the treatment effect). The coefficient $\alpha$ represents the treatment effect.

$$Y_i = \alpha \hat{T}_i + X_i \beta + u_i \quad (9.4)$$

A common concern with the IV approach is what is known as the weak instruments problem. If a treatment variable and the instrument used in analyses are only weakly correlated (or completely unrelated), then the two-step estimator just described will produce inconsistent IV estimates (Bound et al., 1995). In other words, the IV estimate of the causal effect will not be close to the true causal effect (Angrist and Pischke, 2009). Additionally, a weak instrument increases standard errors of the IV estimates and therefore affects hypothesis testing. Generally, a strong correlation between the treatment and instrument and a large sample size will measurably improve consistency. Because of the potential for weak instruments, it is imperative to examine the first stage of the results to determine the extent to which the instrument and treatment variable are correlated. In the empirical example reported to follow, I present the results from the first stage of the estimation to examine the explanatory power of the instrument.

Rhodes (2010) suggests an alternative to the two-step estimator in order to ensure consistency of the causal estimate when using censored data (see also Abbring and van den Berg, 2005; Abbring and Heckman, 2008). Censored data are common in the study of recidivism because the observation period often ends before some of the study participants recidivate. In Rhodes’s strategy, first, the probability of treatment is estimated as a function of an IV $Z_i$ and a vector of control variables $X_i$ as in equation (9.3). The predicted probability of treatment for each group is then computed from model coefficients and a user-specified value for predictor variables (e.g., the sample mean). This predicted probability is used in subsequent analysis. Second, the dependent variable $Y_i$ is modeled as a function of the IV $Z_i$ and the vector of control variables $X_i$ (by contrast, in the two-step estimation, $Y_i$ is modeled as a function of the predicted treatment variable $T_i$). The predicted probability of the outcome, in this case for re-incarceration, is computed with model coefficients and user-specified values of predictor variables. Third, the
predicted probability of re-incarceration is regressed on the predicted probability of treatment using a least-squares regression model. If there are different sample sizes per group, particularly if the samples are small, then it is advantageous to use a weighted least-squares regression in this step (i.e., weight by the sample size of the treatment and control groups). The estimated treatment effect is inferred from the parameter estimate from this final model. In the empirical example below, I employ both the two-step and Rhodes’s IV estimation strategies.

**Estimating the Causal Effect of Knifing Off**

Ex-prisoners often return to the same criminogenic neighborhoods where they resided prior to incarceration (La Vigne et al., 2003). Not surprisingly, a majority of former prisoners are back in confinement within three years (Langan and Levin, 2002). If Laub and Sampson’s (2003) theory of desistance is correct, then it reasons that those ex-prisoners who knife off from their past by moving some distance from where they used to live should have a reduced likelihood of recidivism. Yet estimating the causal effect of place of residence on the likelihood of recidivism is extraordinarily difficult because of selection bias. Differences in recidivism between individuals who moved to new neighborhoods versus individuals who moved back to their former neighborhoods following incarceration may not result from residential mobility; rather, recidivism differences may simply be due to the fact that individuals who moved are different from individuals who did not. Movers may have fewer familial bonds that tie them to former places of residence, or they may have the savings and income necessary to relocate. Presumably these factors are also correlated with recidivism. Thus, an observed correlation between place of residence and recidivism may be biased because of omitted variables that influence both where someone lives and their criminal behavior.

In this empirical example, I utilize an exogenous source of variation from a natural experiment as an IV to provide a consistent estimate of the effect of residential change on the likelihood of recidivism. In August 2005, Hurricane Katrina and the associated flooding and property damage devastated the Gulf Coasts of Louisiana and Mississippi. In New Orleans, more than 70 percent of housing units suffered some damage following Hurricane Katrina, and 56 percent of housing units suffered significant damage (U.S. Department of Housing and Urban Development, 2006). 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 an ex-prisoner moves to one geographic area versus another, to estimate the causal effect of residential migration on recidivism, it is advantageous to have an exogenous source of variation that induced residential mobility (i.e., the treatment) and which can then be used as an IV. The research question this empirical example attempts to answer is whether knifing off, in the form of residential change, lessens a parolee’s likelihood of recidivism. I expect that induced residential change due to Hurricane Katrina allows for a separation between parolees and their criminal past, thus reducing the likelihood of re-incarceration. To test this argument, I compare the monthly hazard of re-incarceration during the first six months following prison release for parolees who resided in the same parish upon release as where they were originally convicted versus parolees who moved to a different parish.

**Data and Research Design**

The analytic sample is drawn from male prisoners released from Louisiana correctional facilities who were originally convicted of their crime in New Orleans (i.e., Orleans Parish). For those prisoners released soon after Hurricane Katrina, their residential choices were significantly different than if they had been released prior to the hurricane, resulting in geographic displacement.

In analyses, I include three cohorts of prison releasees, two of which were released from prison prior to Hurricane Katrina and one released afterward. I use two pre-Katrina cohorts to more fully establish that Hurricane Katrina altered prior geographic patterns of parolee residence. The first cohort comprises males released from a Louisiana prison to parole supervision anytime from September 2001 to February 2002. The second cohort similarly consists of releases between September 2003 and February 2004, and the third cohort consists of releases onto parole supervision between September 2005 and February 2006 (the post-Katrina cohort). Sample sizes equal 711, 768, and 495 for the 2001–2, 2003–4, and post-Katrina cohorts, respectively.

Data utilized in this study are of three types: (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, and (3) Louisiana criminal justice system data from the Supreme Court of Louisiana, DPS&C, DPP, and the Uniform Crime Reports.
Given that macrolevel social and economic conditions in Louisiana changed drastically immediately following Hurricane Katrina, it is necessary to control for such temporal changes to isolate the effect of residential change on recidivism. Controls included in statistical models include segregation, average household income, the unemployment rate, average weekly wages, and fair market rents. Similar to the effect on social and economic conditions, the implications of Hurricane Katrina for temporal changes in the criminal justice system in Louisiana are many (Garrett and Tetlow, 2006; Roman et al., 2007). Given the effect of Katrina on the criminal justice system, 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. Thus, I include control variables in analyses related to parole practices, court operations, and the probability of arrest given the commission of a crime.

My dependent variable, re-incarceration, measures whether a parolee returned to a Louisiana prison for a new criminal conviction or a parole violation within a given month during the first six months following the release from prison. As will be described in greater detail below, analyses will utilize survival analysis, so the dependent variable represents the hazard of re-incarceration at each month from one to six. Simply, it is the probability of re-incarceration in a given month conditional on the fact that the parolee had not yet been re-incarcerated.

The treatment variable used in analyses is labeled different parish as conviction. This is a binary variable indicating whether the parolee moved to a different parish following incarceration relative to where they were originally convicted. This variable equals zero if the parolee returned to the same parish as where they were convicted and one if they moved to a different parish.

The instrument used in analyses is post-Katrina release, which is a binary variable indicating whether the parolee was released from prison following Hurricane Katrina (equals one) or before (zero).

In addition to the contextual and criminal justice system controls already described, the analyses employ five individual-level control variables: race (black equals one, otherwise zero), age at the time of release, marital status, time served in prison, and first release.\textsuperscript{6} Time served refers to the amount of time a parolee served in prison (in years or fraction thereof) until release.\textsuperscript{7} Controlling for time served is necessary to account for any differences between cohorts in the average severity of prior offending. First release is a binary variable indicating whether the parolee was released from their first term of incarceration (equals one) or from their second or greater term (zero).
In prior work examining the effects of residential migration on recidivism (Kirk, 2009), I estimated the likelihood of re-incarceration at any point during the first year following release from prison. Yet in addition to examining the prevalence of re-incarceration, it is also informative to estimate the time to re-incarceration. With recidivism rates so high in the United States, we know that most released offenders will end up back in confinement within just a few years (Langan and Levin, 2002). Just as it is important to investigate whether an individual will recidivate, it is useful for understanding the path to recidivism to examine why some individuals recidivate soon after their release from prison whereas others may not recidivate for some time. When recidivism occurs is an interesting question in its own right. Thus, in this empirical example, I use an IV in a survival analysis to estimate the effect of moving on the monthly hazard of recidivism. My data file is structured as a person–period data set with up to six observations for each parolee. In this case, the estimation outlined in equations (9.3) and (9.4) would simply be augmented to include a notation for month, with binary variables added to the model to represent each month from one to six. Data will be right-censored if an individual was never re-incarcerated, or if they were re-incarcerated sometime after the six-month observation period used in this study.

Before turning to the results, it is necessary to confront one final methodological issue. Why is the use of IVs advantageous even in the presence of a natural experiment? Why doesn’t a simple comparison of recidivism outcomes for pre- versus post-Katrina parolees yield a valid causal effect of residential migration on recidivism? The answer is related to treatment noncompliance and to the causal mechanism of interest. As noted, the treatment of interest in this example is the effect of moving to a different parish upon release from prison relative to where an offender was convicted. Perfect treatment compliance would represent the situation where all parolees released post-Katrina moved to a different parish, and all parolees released pre-Katrina moved to the same parish as where they were originally convicted. By contrast, in the sample of male parolees used in the analyses below, 38 percent of parolees released post-Katrina returned to the same parish and 17 percent of parolees from the two pre-Katrina cohorts moved to a different parish. As in many natural and social experiments, the treatment was diluted in the sense that some individuals released post-Katrina moved back to their old parishes (see Angrist, 2006).

Why is the issue of noncompliance consequential? Although it is adequate to assume that the assignment to treatment is ignorable, a consequence of noncompliance is that the receipt of treatment is nonignorable
Measuring Crime and Criminality

(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. Put simply, because of noncompliance, the group receiving treatment (i.e., those who moved) may not be equivalent to the control group (i.e., those who did not move). Yet through the use of IV methods, I can compute a consistent estimate of the effect of migrating to a different parish on re-incarceration for those parolees who otherwise would not have moved had it not been for Hurricane Katrina. This effect is known as the local average treatment effect (LATE) (see Angrist (2006) for a more extensive discussion).

Results

In terms of descriptive evidence, during the six-month follow-up of male parolees post-incarceration, 9 percent of ex-offenders who migrated to a different parish from where they were originally convicted (i.e., movers) were re-incarcerated, whereas 13 percent of parolees who returned to the same parish where they were convicted (i.e., stayers) were re-incarcerated. Clearly, there is some initial evidence that residential change leads to lower rates of recidivism, yet these descriptive results do not account for selection bias. Thus, I turn to the IV estimates to examine the effect of residential migration net of selection effects.

Table 9.1 shows the first-stage IV results (i.e., equation (9.3)), which regresses the binary treatment indicator of residential change on the IV and a vector of control variables. The coefficient for post-Katrina release is positive and highly significant, indicating that the time period during which a prisoner was released from prison in Louisiana (i.e., pre- versus post-Katrina) substantially influences whether they returned to the same parish where they were originally convicted or moved to a different parish. An F-test can be used to assess the fit of the model and to assess the instrument’s explanatory power. An F-statistic below ten is indicative of a weak instrument (Staiger and Stock, 1997). Results reveal that the instrument, post-Katrina release, is significantly correlated with the treatment variable ($F = 56.30; df = 1, 43; p < 0.001$). Given the strong association between the instrument and the treatment variable, I now proceed to results from the second stage of the two-step estimation (i.e., equation (9.4)).

Table 9.2 presents the IV probit results of the duration until re-incarceration. Results show that those individuals who migrated to a different parish are significantly and substantially less likely to be re-incarcerated.
### Table 1. First Stage of the IV Probit Estimates, Predicting Residential Migration

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coef.</th>
<th>Std. Err.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post-Katrina Release</td>
<td>0.531</td>
<td>(0.071) ***</td>
</tr>
<tr>
<td>Individual-Level</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>-0.117</td>
<td>(0.025) ***</td>
</tr>
<tr>
<td>Married</td>
<td>0.014</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Age at Release</td>
<td>-0.001</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Time Served</td>
<td>0.005</td>
<td>(0.001) ***</td>
</tr>
<tr>
<td>First Release</td>
<td>0.004</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Context and Crim. Justice System</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unemployment Rate</td>
<td>0.196</td>
<td>(0.037) ***</td>
</tr>
<tr>
<td>Avg. Weekly Wage</td>
<td>-0.031</td>
<td>(0.010) **</td>
</tr>
<tr>
<td>Avg. Household Income</td>
<td>0.013</td>
<td>(0.007) *</td>
</tr>
<tr>
<td>Dissimilarity</td>
<td>0.089</td>
<td>(0.037) *</td>
</tr>
<tr>
<td>Fair Market Rent</td>
<td>-0.007</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Avg. Parole Contacts</td>
<td>-0.026</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Judge Caseloads</td>
<td>0.002</td>
<td>(0.001)</td>
</tr>
<tr>
<td>UCR Arrests per Crime (Parish)</td>
<td>-0.031</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Month following Release (versus 1)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month 2</td>
<td>0.000</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Month 3</td>
<td>0.001</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Month 4</td>
<td>0.002</td>
<td>(0.001) *</td>
</tr>
<tr>
<td>Month 5</td>
<td>0.003</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Month 6</td>
<td>0.003</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Intercept</td>
<td>2.127</td>
<td>(0.495) ***</td>
</tr>
</tbody>
</table>

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). The coefficient and standard error for Avg. Household Income is 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.
Table 2. Second Stage of the IV Probit Estimates of the Hazard of Re-Incarceration

<table>
<thead>
<tr>
<th></th>
<th>Robust Coef.</th>
<th>Std. Err.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Different Parish as Conviction</td>
<td>-0.615</td>
<td>(0.184) ***</td>
</tr>
<tr>
<td>Individual-Level</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>-0.241</td>
<td>(0.081) **</td>
</tr>
<tr>
<td>Married</td>
<td>-0.397</td>
<td>(0.351)</td>
</tr>
<tr>
<td>Age at Release</td>
<td>-0.001</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Time Served</td>
<td>-0.063</td>
<td>(0.019) ***</td>
</tr>
<tr>
<td>First Release</td>
<td>-0.210</td>
<td>(0.035) ***</td>
</tr>
<tr>
<td>Context and Crim. Justice System</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unemployment Rate</td>
<td>0.060</td>
<td>(0.241)</td>
</tr>
<tr>
<td>Avg. Weekly Wage</td>
<td>-0.019</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Avg. Household Income</td>
<td>0.011</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Dissimilarity</td>
<td>0.046</td>
<td>(0.059)</td>
</tr>
<tr>
<td>Fair Market Rent</td>
<td>-0.008</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Avg. Parole Contacts</td>
<td>0.000</td>
<td>(0.013)</td>
</tr>
<tr>
<td>Judge Caseloads</td>
<td>-0.005</td>
<td>(0.002) *</td>
</tr>
<tr>
<td>UCR Arrests per Crime (Parish)</td>
<td>-0.007</td>
<td>(0.122)</td>
</tr>
<tr>
<td>Month following Release (versus 1)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month 2</td>
<td>0.677</td>
<td>(0.114) ***</td>
</tr>
<tr>
<td>Month 3</td>
<td>0.788</td>
<td>(0.090) ***</td>
</tr>
<tr>
<td>Month 4</td>
<td>0.873</td>
<td>(0.091) ***</td>
</tr>
<tr>
<td>Month 5</td>
<td>0.876</td>
<td>(0.109) ***</td>
</tr>
<tr>
<td>Month 6</td>
<td>0.997</td>
<td>(0.086) ***</td>
</tr>
<tr>
<td>Intercept</td>
<td>-0.627</td>
<td>(0.579)</td>
</tr>
</tbody>
</table>

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). The coefficient and standard error for Avg. Household Income is 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 coefficients for the month indicators (added to the intercept) can be used to assess the hazard of recidivism within each given month following incarceration. For instance, as depicted in Figure 9.2, in the first month following incarceration, the hazard of re-incarceration for the average male offender who moved (holding the control variables at their means) is roughly 0.001. For those parolees who returned to the same parish following incarceration, the probability of re-incarceration is 0.003. In the second month, the likelihood of re-incarceration given that the parolee was not re-incarcerated in the first month is 0.004 for the treated group and 0.020 for the control group. Following the second month, the gap between the two groups stabilizes to a difference of around 0.025–0.030. This gap represents the treatment effect of moving to a different parish. After six months, an estimated 4 percent of movers were back in prison versus 14 percent of stayers (net of control variables). Whether the size of the gap between the two hazard functions remains stable is a research question worthy of investigation. The treatment effect may dissipate over time as those parolees who moved establish new criminal networks and become readily more aware of criminal opportunities in their new neighborhoods. The gap could also shrink because the police in the mover’s new place of residence become relatively more aware.
of their behavior and reputation and therefore scrutinize their behavior to a similar degree as would police in the mover’s old neighborhood (see Kirk (2011) for further discussion and analysis of the longer term effects of moving).

Table 9.3 compares the monthly hazard rates from the two-step *ivprobit* model with Rhodes’s (2010) method. This table reveals that the estimated gap between the hazard functions displayed in Figure 9.2, which was produced based on *ivprobit* results in Stata, is virtually the same using Rhodes’s estimation strategy.\(^\text{10}\) Findings are robust to how the IV survival model is estimated. The hazard of re-incarceration for each month during the first six months of post-incarceration is significantly and substantially lower for movers.

### Concluding Thoughts

Estimation of treatment or causal effects in criminological research is routinely hindered by the issue of selection bias. There are several methods available to researchers to parse out selection effects when attempting to estimate the causal effect of a treatment on an outcome (see Gangl (2010) for an extended review of the various approaches for addressing selection bias). One common approach, using the selection of place of residence as an example, is to introduce individual and family characteristics as control variables in regression models to account for the nonrandom process by which individuals select where to live.
However, even with extensive measurement of characteristics known to be related to residential choice, unobserved factors may still create omitted variable bias. Another potential solution to selection bias is the use of propensity-score matching. With this approach, control and treatment cases are matched according to a propensity score. The propensity score is defined as the probability that a given individual receives the treatment (e.g., moves to a new neighborhood) given all that we observe about them. In essence, matching via propensity scores is a data-reduction strategy that turns a multidimensional matching problem (i.e., because of numerous statistical controls in a regression model) into a match based on a single variable (i.e., dimension). This statistical adjustment is used to account for differences between treatment and control groups, and matched cases are then compared on the outcome variable to produce an estimated treatment effect. The logic of propensity-score matching is akin to a case-control design in which control subjects are selected with similar observable features to those that received the treatment. Yet because cases in the treatment and control groups are matched only on observed characteristics, there is still the potential for omitted variable bias in the estimation of the effect of a treatment. Both of these approaches to selection—statistical controls and propensity scores—may help minimize selection bias, but the extent depends upon the measurement of confounding variables.

IV methods take a different approach to addressing selection. With an IV approach, a variable (or variables) that is unrelated to the outcome is used as an independent variable to predict the treatment variable, and then the outcome variable is regressed on the predicted treatment variable. Conceptually, this approach removes the spurious correlation between the treatment variable and omitted variables. Rather than trying to eliminate omitted variable bias by attempting to measure all potential omitted variables, the IV approach provides an estimate of a treatment effect by using only that portion of the variability in the treatment variable that is uncorrelated with the omitted variables.

In the empirical example above and in related work on the same project (Kirk, 2009, 2011), I have used IVs to address whether separating individuals from their former residential environment reduces their likelihood of recidivism. Findings strongly suggest that it does. Thus, “knifing off”—independent of selection effects—does appear to lower the likelihood of recidivism among those individuals who moved.

This research has exploited a natural disaster to construct an IV. As noted, use of an instrument from a natural occurrence provides
justification that the instrument is exogenous—that is, it is uncorrelated
with the model’s error term. Such an exogenous source of variation does
not necessarily have to come from a natural disaster. The relationship
between residential change and recidivism could also be studied through
other natural experiments. For instance, one could identify states that
changed the residency restrictions imposed upon released prisoners.
In this case, the timing of the change in residency restrictions could
serve as an IV for residential migration. Similarly, one could contrast
recidivism rates of states that require prisoners to return to their county
of last residence versus those states that do not, using differences in
state residency restrictions for parolees as an instrument for residential
change (see Apel et al. (2008) for a criminological application that uses
differences in state laws as an IV).

As way of conclusion, let me emphasize that IV methods are no
panacea. They crucially rely upon two conditions: that the instrument
is correlated with the treatment variable, yet it is uncorrelated with the
dependent variable except indirectly through the treatment variable.
When these conditions can be met, IVs provide a powerful remedy for
the issue of selection bias that plagues so many tests of criminological
theory.

Notes

1. Two-thirds of returning prisoners in the United States are rearrested within
three years of prison release and half are re-incarcerated (Langan and Levin,
2002).
2. For example, there is likely to be simultaneous causation between the size of the
prison population and crime rates, in which the prison population size influences
the crime rate while at the same time crime rates influence the size of the prison
population. To resolve this simultaneity issue in an investigation of the effect of
changes in the prison population on crime, Levitt (1996) uses prison overcrowding
litigation as an IV for changes in the prison population. Tita and colleagues (2006)
use IVs to alleviate the issue of measurement error in an investigation of the causal
effect of crime on housing values. Because crime is typically underreported to the
police, there will be measurement error in such an analysis. As a remedy, Tita et
al. (2006) use murder, which is generally measured with accuracy, as an IV for the
violent crime rate.
3. Conceptually, suppose we wish to determine what would happen to the criminal
behavior of the same individual under two different circumstances (e.g., they moved
to a new neighborhood or they stayed in the same neighborhood). Yet in reality
we observe only one of these two potential outcomes for an individual at a given
point in time (i.e., either they moved or did not). Given that only one outcome is
observed, randomization is one strategy to estimate an average causal effect of
moving, by comparing the likelihood of criminal behavior across equivalent groups
where one group receives an experimental treatment (i.e., moves). In the absence
of an experiment, we can capitalize on an exogenous source of variation (i.e., an
IV) to induce treatment.
4. The commands \texttt{ivreg} and \texttt{ivprobit} can be used to implement a two-step estimator in Stata, and the \texttt{mfx} command can be used to calculate the marginal (i.e., treatment) effect based on the \texttt{ivreg} or \texttt{ivprobit} results. IV models can be estimated in SAS using the \texttt{Proc Syslin} command. The two stages can be estimated separately by the analyst if desired (i.e., the analyst can manually save the predicted values from stage one and insert these values into stage two). However, if the two stages are estimated separately, the standard errors from the second stage will be incorrect, unless they are manually adjusted (see Gelman and Hill (2007) for a procedure). If unadjusted, the standard errors from the second stage do not reflect uncertainty present at the first stage of the model. The need for some kind of manual adjustment to the standard errors can be eliminated by using one of the standard IV commands in Stata or SAS that automatically calculates the correct standard errors for the user.

5. The sample excludes sex offenders.

6. Analyses are restricted to male parolees, so there is no control for gender.

7. 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.

8. In a similar vein, the widely lauded Moving to Opportunity (MTO) housing mobility program suffered from considerable treatment noncompliance (see, e.g., Ludwig et al., 2001; see also Clampet-Lundquist and Massey, 2008). Research results may be biased by selection because of unobserved characteristics of families that led them to drop out of the program or to never “take up” entry into the program once randomly assigned. Part of the reason why some families did not or could not move is because of the difficulty of finding housing in low-poverty neighborhoods (e.g., finding landlords who would accept housing vouchers). With MTO, assignment to treatment is ignorable (i.e., assignment to control and treatment groups is random), but the receipt of treatment is not. Given this, MTO researchers have, in some instances, examined intention-to-treat estimates, which provide information on the effect of being offered the opportunity to move with an MTO voucher regardless of whether this offer is ever utilized (e.g., Ludwig et al., 2001). For the purposes of evaluating public policy, determining the effect of the offer to move is useful, yet it does not demonstrate the actual effect of mobility (as would estimation of the LATE).

9. I used the \texttt{ivreg2} function in Stata to perform this test for a weak instrument.

10. Again, the potential issue that Rhodes (2010) describes is that IV estimates provided by a two-step estimator may be inconsistent because of the complexities of working with censored data. The data used in this analysis are censored because the event (i.e., re-incarceration) is observed only for a portion of the sample.

Bibliography


“Residential Change as a Turning Point in the Life Course of Crime: Desistance or Temporary Cessation?” Working paper. Austin, TX: Department of Sociology, University of Texas at Austin, 2011.


