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ASYMMETRICAL CAUSATION AND CRIMINAL DESISTANCE

CHRISTOPHER UGGEN* AND IRVING PILLAVIN**

Although criminologists have long been concerned with desistance or cessation from crime,¹ tests of theory are typically based on etiological investigations.² Desistance studies, in contrast, have historically been used for program evaluations, undertaken by professionals in social work, clinical psychology, and corrections. We argue that theory and research on desistance is absolutely critical to advancing scientific and policy goals. We do not attempt to break new theoretical or empirical ground in this paper, but instead present a systematic explication of the argument for desistance research.

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¹ See SHELDON GLUECK & ELEANOR T. GLUECK, 500 CRIMINAL CAREERS 257 (1930) [hereinafter GLUECK & GLUECK, 500 CRIMINAL CAREERS]; SHELDON GLUECK & ELEANOR T. GLUECK, LATER CRIMINAL CAREERS 98 (1937) [hereinafter GLUECK & GLUECK, LATER CRIMINAL CAREERS]; EDWIN S. SUTHERLAND, PRINCIPLES OF CRIMINOLOGY 357-58 (3rd ed. 1939).

² For exceptions, see Liliana E. Pezzin, *Earnings Prospects, Matching Effects, and the Decision to Terminate a Criminal Career*, 11 J. OF QUANTITATIVE CRIMINOLOGY 29, 29-30 (1995); Neil Shover & Carol Y. Thompson, *Age, Differential Expectations, and Crime Desistance*, 30 CRIMINOLOGY 89, 89-90 (1992); Thomas Meisenhelder, *An Exploratory Study of Exiting from Criminal Careers*, 15 CRIMINOLOGY 319, 319-20 (1977); Thomas Meisenhelder, *Becoming Normal: Certification as a Stage in Exiting from Crime*, 3 DEVIANT BEHAVIOR 137, 146-47 (1982).

I. CAUSAL ASYMMETRY, CRIME, AND DESISTANCE

A. THE GROWING UNIVERSE OF POTENTIAL DESISTERS

Though ex-offenders are socially marginalized in America, they are no longer a statistically marginal group. As of December 31, 1997, American state and federal prisons held 1,244,554 prisoners, with over 500,000 additional inmates held in local jails.³ Despite recent increases in mandatory minimum and mandatory life sentences, almost all of these prisoners will eventually rejoin civil society.⁴ Each year, several hundred thousand releasees pour into the general population, with over 400,000 entering parole in 1997.⁵ In fact, over the past twenty-five years the trends in prison release closely mirror rising incarceration rates. Figure 1 plots the number of U.S. prisoners incarcerated and the number of prisoners released each year.⁶ As the figure indicates, the two data series are correlated quite closely ($r=.98$). In fact, more prisoners were released in 1996 than were incarcerated in 1986 and more than twice as many were released in 1996 as were incarcerated in 1976. Some of these released prisoners will resume crime and others will desist from crime—they will temporarily or permanently cease offending—yet little is known about the desistance process. This paper argues that a research program to identify the causes of desistance will advance both scientific and policy concerns.

Despite a longstanding concern within the discipline, criminologists today devote relatively little attention to deriving theoretical understanding of the desistance process. This is because criminological theory and research are primarily concerned with questions of etiology, or the causes of crime.

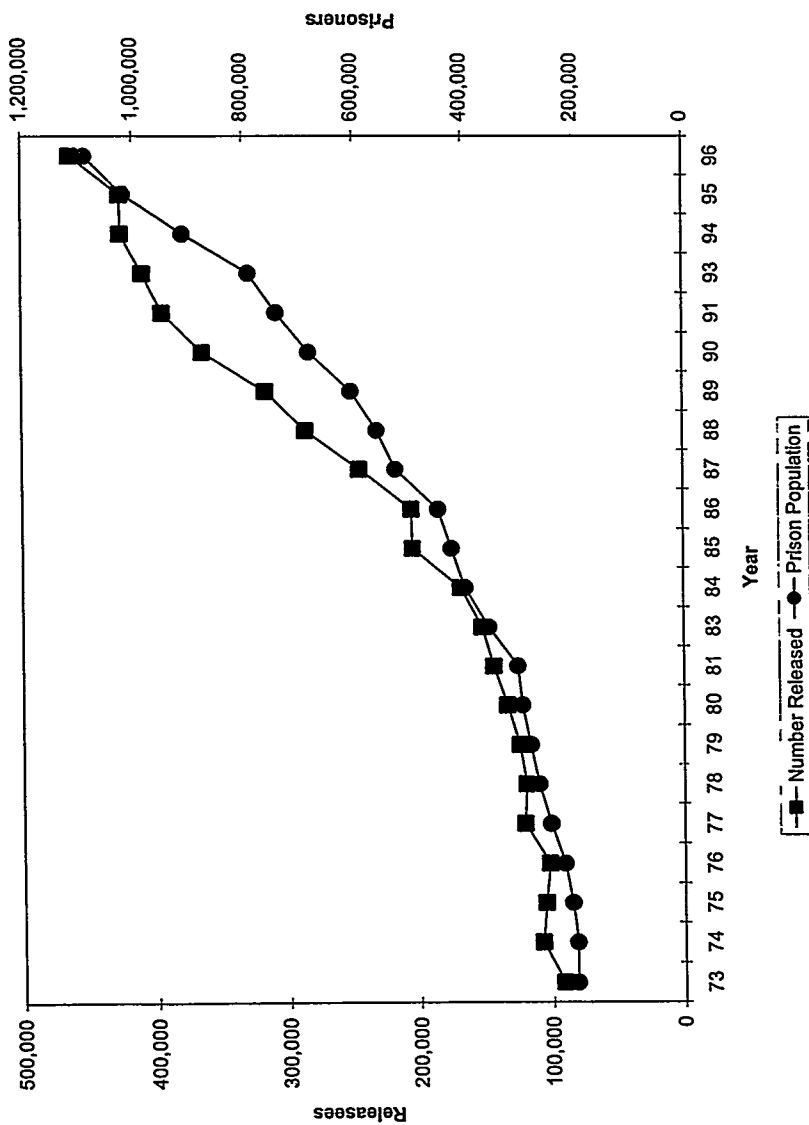
³ Darrell K. Gilliard & Allen J. Beck, *Prisoners in 1997*, in BUREAU OF JUSTICE STATISTICS BULLETIN 1 (1998).

⁴ JOHN IRWIN & JAMES AUSTIN, *IT'S ABOUT TIME: AMERICA'S IMPRISONMENT BINGE* 161 (2d ed. 1997).

⁵ THOMAS P. BONCZAR, U.S. DEP'T OF JUSTICE, PROBATION AND PAROLE POPULATIONS 4 (1998).

⁶ The data in Figure 1 are taken from SOURCEBOOK OF CRIMINAL JUSTICE STATISTICS 1995, at 560 (1996) published by the U.S. Department of Justice Bureau of Justice Statistics for the years 1973-1996. These data may also be found in the Bureau of Justice Statistics series CORRECTIONAL POPULATIONS IN THE UNITED STATES 76 (1996).

Figure 1: Prisoners and Releases by Year



Discipline-based criminologists such as sociologists, psychologists, and economists have focused their attention on etiological research addressing individual involvement in crime or community crime rates. Unfortunately, the study of these phenomena is conceptually complex, fraught with daunting methodological barriers, and in many (though not all) ways, without policy relevance. Desistance research may prove more rewarding for theory and policy, in part because it is more manageable conceptually and methodologically.

In this paper, we are less concerned with why people commit crime than with the conditions promoting social reintegration and desistance from crime. We make the following assertions: (1) that the causes of desistance likely differ from the causes of crime; (2) that knowledge of the true causes of desistance will be easier to obtain than knowledge of the true causes of crime; and (3) that it will be possible to translate scientific knowledge about desistance into specific policy interventions. We begin with a general discussion of crime and causality, then present the case for and against desistance research.

B. CRIMINOLOGICAL PROBLEMS POSED BY THE RUBIN/HOLLAND CAUSAL MODEL

As social scientists researching crime and conformity, we often set out to make causal inferences. Temporal order, statistical association, and lack of spuriousness are generally accepted as minimal criteria for establishing causality in the social sciences,⁷ though these standards are rarely approached in practice. In the study of delinquency, for example, we continue to debate the putative causes: are delinquent friends causes or consequences of involvement in crime? Does family alienation precede or follow from one's involvement with delinquent peers? Definitive answers to such questions are elusive because of both conceptual confusion over the meaning of causality and

⁷ See Paul F. Lazarsfeld, *Foreword to* HERBERT HYMAN, *SURVEY DESIGN AND ANALYSIS* at xiv (1955); TRAVIS HIRSCHI & HANAN C. SELVIN, *DELINQUENCY RESEARCH: AN APPRAISAL OF ANALYTIC METHODS* 52-53 (1967); but see Margaret Mooney Marini & Burton Singer, *Causality in the Social Sciences*, in *SOC. METHODOLOGY* 347, 376-79 (Clifford C. Clogg ed., 1988); and Huw Price, *Agency and Causal Asymmetry*, 101 *MIND* 501, 511 (1992), on temporal order.

operational difficulties implementing critical tests of theoretical propositions.

Rubin⁸ and Holland⁹ offer a statistical model of causal inference that lays bare the obstacles to establishing causation in criminology. The Rubin/Holland model highlights the difficulties in establishing the causes of crime and illustrates the potential for desistance research. The central prescription of the model is to seek the effects of manipulable causes rather than to trace the causes of observed effects. They arrive at this conclusion in the following manner:

Holland defines a true causal effect of some factor T on response variable Y for individual unit U as

$$Y_T(U) - Y_C(U)^{10}$$

This difference implies that the effect of any cause T must always be assessed in relation to some other cause, the counterfactual condition *not-T* or C. The fundamental problem of causal inference, in criminological research as elsewhere, is that it is *impossible* to observe the value of $Y_T(U)$ and $Y_C(U)$ on the same individual person or "unit." If a respondent is employed at age fifteen and commits delinquency at age sixteen, for example, we cannot determine whether she *would have* committed delinquency had she not been working. Because we can only observe one condition per unit, we face a missing data problem for the counter-factual condition. Holland distinguishes between scientific and statistical solutions to this problem. Both have been applied in criminological research.

1. *Scientific Solutions*

Among the scientific solutions, one can attempt to assure (or simply assume) that each individual unit is identical (assuming *unit homogeneity*) and submit unit one to treatment T and

⁸ Donald B. Rubin, *Estimating Causal Effects of Treatments in Randomized and Non-randomized Studies*, 66 J. EDUC. PSYCHOL. 688, 689-90 (1974).

⁹ Paul W. Holland, *Statistics and Causal Inference*, 81 J. AM. STAT. ASS'N 945, 946 (1986).

¹⁰ *Id.* at 947.

unit two to treatment C. If so, then the true causal effect is easily obtained as:

$$Y_T(U_1) - Y_C(U_2)$$

Alternatively, if it is reasonable to believe that prior exposure does not affect subsequent response, the scientist could expose the *same* unit to each treatment in succession (assuming *temporal stability* and *causal transience*).

$$Y_{T(\text{TIME } 2)}(U_1) - Y_{C(\text{TIME } 1)}(U_1)$$

Although such assumptions may be reasonable in laboratory work, they are rarely justified in criminological research: humans are not identical in all relevant respects, and human responses are neither constant over time nor unaffected by previous exposures. To continue the previous example, we cannot assume that a working teen is identical in all relevant respects to a non-working teen, for workers may be more ambitious, less impulsive, or more opportunistic than non-workers. Nor can we assume that exposure to work in eighth grade will have the same effects as exposure to work in tenth grade, or, for that matter, that working in eighth grade will not affect one's response to non-work in tenth grade. Therefore, as social scientists we must often rely on a statistical solution to the inference problem.

2. Statistical Solutions

The Rubin/Holland statistical solution is to find the expected value (E) of the *average* causal effect T, of T (relative to C) over a population U, or

$$E(Y_T - Y_C) = T$$

which can be expressed as

$$T = E(Y_T) - E(Y_C)$$

To continue the earlier example, one could deviate the average number of crimes (or the proportion committing a crime) among workers from the average number of crimes among non-workers. This replaces the unobservable causal effect of T on a specific unit with an estimate of the average causal effect of T over a population of units. Unfortunately, this approach breaks down in practice because an important assumption is unlikely to hold.

3. The Mean Independence Assumption

For the statistical solution to hold, we must assume *mean independence*: the mean values on Y for the T group and the C group must be independent of the selection or assignment mechanism (S) that determines whether Y_T or Y_C is observed for any given unit. In general, mean independence fails in criminology and other social sciences and the observed average treatment effect $T_{(OBS)}$ is not equivalent to the true treatment effect T .¹¹

$$T_{(OBS)} = E(Y_T|S=T) - E(Y_C|S=C)$$

The mean independence assumption for groups is thus analogous to the unit homogeneity assumption for units. Both allow the comparison of observed quantities with latent or unobserved quantities. Only when Y_T and Y_C are both mean independent of S , however, does

$$E(Y_T) = E(Y_T|S=T) \quad \text{and} \quad E(Y_C) = E(Y_C|S=C)$$

So that

$$T = T_{(OBS)} = E(Y_T) - E(Y_C)$$

In short, the two conditional means $E(Y_T|S=T)$ and $E(Y_C|S=C)$ must be independent for them both to equal the un-

¹¹ Holland distinguishes between a "prima facie" cause T_{π} and the true average causal effect T . *Id.* at 949.

conditional mean $E(Y_T)$.¹² If the selection mechanism S is randomized assignment to employment, this is a reasonable assumption for the work and crime example above. If S is *self*-selection, however, this assumption is invalid: those that self-select into employment are likely to have lower crime means than those that self-select out of employment. If selection is partially determined by "ambition" and ambition is associated with crime, for example, the conditional distributions are unequal and the mean independence assumption breaks down. This is simply one important variant of the more general omitted variable problem that biases parameter estimates. Criminology is particularly vulnerable to violations of mean independence, however, because the selection processes into levels of our independent and dependent variables are so poorly understood.

¹² This point becomes clear when expressed in terms of conditional probabilities. The expected value for the treatment group is composed of two parts, the mean if assigned to treatment and the mean if assigned to control. The first quantity is the product of the expected value of the mean for the treatment group and the probability of selection into that group. The second quantity is the unobserved mean for the counterfactual (the mean the controls would have had, were they assigned to treatment) multiplied by the probability of selection into the counterfactual, or control status.

$$E(Y_T) = E(Y_T|S=T)P(S=T) + E(Y_T|S=C)P(S=C)$$

Similarly, the expected value for the control group is:

$$E(Y_C) = E(Y_C|S=C)P(S=C) + E(Y_C|S=T)P(S=T)$$

The expected values for both treatment and control groups are thus weighted averages of the observed and counterfactual conditions. By substituting these quantities into the expression $T = E(Y_T) - E(Y_C)$, we obtain

$$T = E(Y_T|S=T)P(S=T) + [E(Y_T|S=C)P(S=C)] - E(Y_C|S=C)P(S=C) - [E(Y_C|S=T)P(S=T)]$$

Of course, we only observe Y_T when a unit selects into the treatment condition and we only observe Y_C when a unit selects into the control condition, computing $T_{(obs)}$ as follows:

$$T_{(obs)} = E(Y_T|S=T) - E(Y_C|S=C)$$

To determine the true rather than the observed causal effect, we need information about the unknown (and unknowable) counterfactual conditions. If the mean of Y is independent of the assignment mechanism S , however, then

$$E(Y_T|S=T) = E(Y_T|S=C) = E(Y_T) \text{ and } E(Y_C|S=C) = E(Y_C|S=T) = E(Y_C),$$

so that

$$T = T_{(obs)} = E(Y_T) - E(Y_C)$$

4. *Selection Mechanisms and Strong Ignorability*

When researchers cannot control the assignment mechanism (S), as in observational studies of delinquency, causal inference requires “strong ignorability” to assure mean independence.¹³ For a selection mechanism to be strongly ignorable: (1) all factors related to both the treatment and non-treatment condition must be included as covariates; (2) all units must have a non-zero probability of assignment to treatment and non-treatment conditions; and (3) a unit’s response to treatment must not be affected by either the assignment mechanism or the treatments other units receive.¹⁴ These conditions require much more exhaustive data and much more painstaking analysis than are usually conducted in etiological studies of crime and delinquency. *Each* variable of causal interest must be fastidiously examined. In the example above, we would have to pay at least as much attention to the process by which young people find jobs as we would to the effects of jobs on crime. We would also need theory and data regarding factors such as peer associates, success or failure in school, parental supervision, and myriad other indicators likely to be related to both processes. In sum, Berk’s contention that strong ignorability may be a “pipe dream” in observational research is particularly true for the study of crime.¹⁵

II. IMPLICATIONS FOR THEORY AND POLICY ON CRIME

To determine causality, then, the model requires at minimum a random or strongly ignorable assignment mechanism and treatments that can (at least in principle) be manipulated. We argue that these conditions are unlikely to hold in etiological research on crime. In desistance analysis, however, both random selection mechanisms and manipulable interventions are much more feasible: a researcher can randomly assign to (or deny from) offenders a range of treatment modalities—a range

¹³ Paul R. Rosenbaum & Donald B. Rubin, *Reducing Bias in Observation Studies Using Subclassification on the Propensity Score*, 79 J. AM. STAT. ASS’N. 516, 522 (1984).

¹⁴ Richard A. Berk, *Causal Inference for Sociological Data*, in HANDBOOK OF SOCIOLOGY 155, 161 (Neil J. Smelser ed., 1988).

¹⁵ *Id.* at 165.

of putative causes. She then can observe the effects of these causes relative to some other cause, presumably assignment to a control condition.

A. DISADVANTAGES OF NON-EXPERIMENTAL ETIOLOGICAL RESEARCH

In nonexperimental social research, we typically attempt to assure mean independence using covariate adjustment for factors likely to be related to both the putative cause and the selection process. As Sobel points out, if researchers can "name and measure the covariates that account for assignment" to a condition, then its conditional average effects are estimable from the data.¹⁶ Unfortunately, we often do not know which factors must be statistically controlled to render the selection mechanism strongly ignorable. Even when we can identify such variables, they are subject to measurement error. Until we have much more refined conceptual models of selection into our *independent* variables, a safer alternative is to manipulate them as part of the research design.

As regards the *dependent* variables, there remains confusion over the appropriate domain of behavior in etiological research: how do we compare studies of involvement in minor crime with those investigating more serious, sustained, or socially harmful criminal behavior? How can we make causal interpretations when studies fail to distinguish entrance from continuation in crime, particularly since entrance appears to be virtually universal? The primary problem is one of endogeneity—isolating the true effects of factors that cannot be manipulated by the researcher.

I. Accounting for Selection

How can criminologists account for the selection mechanism and make causal interpretations when etiological studies of crime are beset by endogeneity? Perhaps the most promising

¹⁶ Michael E. Sobel, *An Introduction to Causal Inference*, 24 *SOC. METHODS & RESEARCH* 353, 367 (1996).

approach is to examine within-person changes in offending.¹⁷ Still, even models that purport to control for individual differences can only account for *stable* within-unit differences. For example, it is common in econometrics to adjust for unobserved heterogeneity in pooled time-series analysis with fixed-effects models in which each variable is expressed as a deviation from its mean value. Unfortunately, such estimators are inappropriate when crime is not exogenously determined or a reciprocal causal relationship is suspected.¹⁸ If the past level of the dependent variable (crime) has a causal effect on subsequent levels of the independent variable (employment), this violates the assumption of "strict exogeneity."¹⁹ Therefore, although good longitudinal data and analytic techniques enable researchers to statistically control for unmeasured stable differences, these models only correct for selectivity and omitted variable biases to the extent that the relevant unobservables are person-specific and fixed over time. Ethnographic and interview data, however, suggest that this assumption may be mistaken: offenders' perceptions and normative orientations evolve with age and life-course transitions, as does the social and historical context in which they find themselves.²⁰

2. Manipulable Treatments

Although etiological studies have led to elegant tests of criminological theory,²¹ their implications suggest interventions that are often unworkable for public policy. Neither the re-

¹⁷ See Julie Horney et al., *Criminal Careers in the Short-term: Intra-Individual Variability in Crime and its Relation to Local Life Circumstances*, 60 AM. SOC. REV. 655, 657 (1995).

¹⁸ David R. Johnson, *Alternative Methods for the Quantitative Analysis of Panel Data in Family Research: Pooled Time-Series Models*, 57 J. MARRIAGE & FAM. 1065, 1071 (1994).

¹⁹ *Id.*; see also Michael P. Keane & David E. Runkle, *On the Estimation of Panel-Data Models With Serial Correlation When Instruments Are Not Strictly Exogenous*, 10 J. BUS. & ECON. STAT. 1, 3 (1992).

²⁰ See NEIL SHOVER, *GREAT PRETENDERS: PURSUITS AND CAREERS OF PERSISTENT THIEVES* 124 (1996).

²¹ See, e.g., John Hagan et al., *Class in the Household: A Power-Control Theory of Gender and Delinquency*, 92 AM. J. OF SOC. 788 (1987); Allen E. Liska & Mark D. Reed, *Ties to Conventional Institutions and Delinquency: Estimating Reciprocal Effects*, 50 AM. SOC. REV. 547 (1985); Ross L. Matsueda, *Testing Control Theory and Differential Association: A Causal Modeling Approach*, 47 AM. SOC. REV. 489 (1982).

searcher nor the state has the ethical or constitutional license to radically alter, say, the personality, parental background, neighborhood, or associates of youth identified as "pre-delinquent"²² who have yet to violate the law. In light of these problems, programs aiming to prevent the onset of delinquency have been excoriated in the evaluation literature.²³ There are few strong treatments that a researcher can administer in good conscience, leaving little room for bold experimentation among such a vulnerable population. Even well-intentioned efforts to increase opportunities for youth in high-risk groups may have unintended stigmatizing or otherwise deleterious consequences.²⁴

3. *Causal Asymmetry*

Etiological research has provided many well-established empirical generalizations, such as the relation between law violation and gender, age, urban residence, mobility, parental attachment, school success, and moral beliefs.²⁵ Unfortunately, these findings do not translate neatly into a set of causal maxims or concrete policy prescriptions to prevent the onset of delinquency or crime. Moreover, we have little reason to believe that manipulating these factors will *alter* offending behavior once it has begun. The failure of programs as wide-ranging as family therapy, remedial education, reference group alteration, and psychological counseling suggests that either the presumed cause is misidentified or that symmetrical causation does not apply. Of course, the failure of particular policies does not rule out the possibility that more intensive, more costly, more fo-

²² SHELDON GLUECK & ELEANOR T. GLUECK, IDENTIFICATION OF PREDELINQUENTS: VALIDATION STUDIES AND SOME SUGGESTED USES OF THE GLUECK TABLE 1, 9 (Sheldon Glueck & Eleanor T. Glueck eds., 1972).

²³ One otherwise evenhanded review of programs to prevent or control delinquency concludes that "Prevention projects don't work and they waste money, violate the rights of juveniles and their families, inspire bizarre suggestions and programs, and fail to affect the known correlates of urban delinquency." RICHARD J. LUNDMAN, PREVENTION AND CONTROL OF JUVENILE DELINQUENCY 245 (1993).

²⁴ See HOWARD BECKER, OUTSIDERS 35 (1963); LARRY L. ORR ET AL., DOES TRAINING FOR THE DISADVANTAGED WORK? 131-33 (1996).

²⁵ For reviews of this literature see JOHN BRAITHWAITE, CRIME, SHAME, AND REINTEGRATION 54-97 (1989); MICHAEL GOTTFREDSON & TRAVIS HIRSCHI, A GENERAL THEORY OF CRIME 123-53 (1990); JAMES Q. WILSON & RICHARD J. HERNSTEIN, CRIME AND HUMAN NATURE (1985).

cused, or more invasive treatments may indeed have the intended prophylactic result. Therefore, we must continue seeking innovative and creative policy interventions. This paper argues, however, that we must simultaneously begin a more systematic investigation of the causes of desistance.

4. *Manipulating Initial Conditions*

We currently lack the information to determine whether the causes of crime and desistance are symmetrical. The situation is analogous to the apparent temporal asymmetry in radiative phenomena observed by Popper: we understand why a stone tossed into a still pond will produce outgoing concentric waves on the water's surface, and yet it would strike us as remarkable to witness a confluence of *incoming* concentric waves arriving at just the right moment to launch a stone out of the pond and into one's hand.²⁶ This could signal some asymmetry in the laws of thermodynamics *or* it could simply reflect our relative capacity to manipulate the initial conditions that give rise to each phenomenon. That is, in the existing world it is much easier to toss a rock into a pond than to engineer the sort of elaborate experiment necessary to reverse the process.²⁷ For the study of crime and desistance, the situation is analogous: as we argue below, the conditions likely to engender desistance are much more amenable to manipulation than the conditions likely to cause crime.

B. ADVANTAGES OF DESISTANCE ANALYSIS

Desistance studies—of drug use, welfare receipt, retirement, and other phenomena in addition to crime—are much better situated to guide social policy than etiological studies. The primary advantage of desistance analysis stems from the ability to randomly assign a target group to a truly exogenous treatment. Experimental evaluations examining desistance or recidivism outcomes may provide results that directly translate into policy

²⁶ See Karl R. Popper, *The Arrow of Time*, 177 NATURE 538 (1956).

²⁷ See Price, *supra* note 7, at 504-05.

prescriptions.²⁸ Moreover, when interventions are tightly linked to theory, desistance analysis can provide the sort of critical tests of criminological theory that have eluded etiological researchers.

1. *Temporal Order*

Why is this best accomplished in a study of desistance? First, and perhaps most importantly, the desistance researcher works in concert with time rather than against it. Although twentieth-century physics have challenged traditional conceptions of temporal order in causality,²⁹ the arrow of causation typically remains aligned with the arrow of time in the social sciences. In terms of the Rubin/Holland model, desistance research seeks the effects of manipulable causes—interventions in the hands of the researcher—rather than seeking to reconstruct the putative causes of observed effects. Too often, criminological researchers attempt to enumerate *all* possible causes of crime in a fruitless attempt to statistically deconstruct naturally occurring assignment mechanisms. By “controlling for” myriad causes, the argument goes, researchers hope to render selection strongly ignorable and get clean estimates of all of them. Instead, we believe with Holland³⁰ and Sobel³¹ that we should carefully examine the effect of *particular* causes by consciously manipulating them.

2. *A License to Intervene*

The difficulties in etiological research are a matter of both logical and pragmatic considerations—of both causal sequencing and the authority to act. Where and how (and *when*) can we best craft and apply an intervention so that we may examine its consequences? Our ability to isolate the true causal effect of critical etiological factors such as parents, schools, and neigh-

²⁸ PETER H. ROSSI ET AL., MONEY, WORK, AND CRIME: EXPERIMENTAL EVIDENCE 277 (1980); JUDITH M. GUERON & EDWARD PAULY, FROM WELFARE TO WORK 29 (1991); ORR ET AL., *supra* note 24, at 211.

²⁹ Price, *supra* note 7, at 512.

³⁰ Holland, *supra* note 9, at 959 (1986).

³¹ Sobel, *supra* note 16, at 373.

borhoods is constrained by our inability to manipulate the selection mechanisms guiding their allocation. For both social scientific research and for policy purposes, manipulation of these factors is unacceptably invasive in a democratic society. The researcher conducting a desistance study has a more legitimate and expansive *license to intervene* in the lives of participants. This means that she can provide, or perhaps more importantly, deny a particular program to a treatment and a control group. Of course constitutional and ethical concerns do not disappear in desistance programs; some inmates may be falsely accused and imprisoned, the victims of an imperfect criminal justice system. Nevertheless, participants have all been convicted of crimes, or in the case of juveniles, adjudicated delinquent. We may certainly provide or deny employment, for example. Although we do not advocate random assignment to marriage, we can certainly manipulate causal interventions that are designed to enhance marital quality or stability through financial, psychological, or behavioral counseling and support.

3. *Target Efficiency*

Finally, desistance strategies are better able to concentrate resources on specific target groups likely to benefit from them. Primary prevention programs, in contrast, must cast a much wider net that is liable to include a large number of non-offenders (or offenders who commit only trivial offenses). For policy, this means that programs designed to foster desistance may bring a greater return to investments in released offenders—a greater potential to reduce the social harm associated with recidivism. For science, this means that desistance analysis offers potentially greater efficiencies in design and statistical analysis.

C. REAL AND IMAGINED DISADVANTAGES OF DESISTANCE ANALYSIS

1. *Devising Theoretically Relevant Interventions*

In our view, the major weakness in existing desistance research has been the lack of theoretically derived interventions. Typically researchers assess the effects of a program, such as ju-

venile probation, without sufficient attention to the *mechanisms* thought to link that program with criminal behavior. Such a program could be based on deterrence theory, in which supervision and sanctions play a large role. Alternatively, it could be based on modeling or learning theory, in which the affective ties between the caseworker and the probationer are emphasized. Moreover, policymakers and practitioners generally prefer to individualize rather than standardize treatments—to do whatever appears to work without regard to theory.³² The result of such interventions is often uninterpretable. It is also difficult, though not impossible, to make valid inferences about the effects of mediating social-psychological processes that occur after randomization.³³ Despite these barriers, it is certainly possible to structure theoretically derived interventions and to distinguish between such disparate theories as those found in the labeling, deterrence, differential association, and social control traditions.

2. *External Validity*

The primary weakness of previous desistance analyses for policy purposes has been external validity or generalizability. Many interventions and evaluations have been based on pilot programs that drew subjects from rather narrow officially-defined offender subgroups, such as heroin users in New York City.³⁴ Nevertheless, this difficulty is neither unique to nor inherent in investigations of desistance. A well-designed employment program and a long-term follow-up on a national probability sample of prison releasees, for example, would certainly constitute a viable research project. The results of this type of study would generalize to a broad population of interest and could offer specific policy recommendations.

³² DAVID J. ROTHMAN, CONSCIENCE AND CONVENIENCE: THE ASYLUM AND ITS ALTERNATIVES IN PROGRESSIVE AMERICA 43-81 (1980).

³³ Sobel, *supra* note 16, at 373.

³⁴ LUCY FRIEDMAN, U.S. DEP'T. HEALTH, EDUC., & WELFARE, THE WILDCAT EXPERIMENT: AN EARLY TEST OF SUPPORTED WORK IN DRUG ABUSE REHABILITATION (1978).

3. Secret Deviance

The desistance researcher must typically rely on officially-defined deviant populations. Yet prison populations clearly represent a subset of all law violators and the desistance process may differ for those who have never been formally labeled. One might speculate, for example, that the effects of adult social bonds to work and family differ among those formally labeled in comparison to those not stigmatized by official deviant status.

III. PAST, PRESENT, AND FUTURE DESISTANCE ANALYSIS

A. EARLY DESISTANCE MODELS

We are not the first to make these points: criminologists have long suspected that the causes of crime and the causes of desistance may be asymmetrical. In tracing the history of theory and research on desistance, the first task is to arrive at a meaningful definition. The concept has a long lineage in criminology, though few theorists or researchers used the term until the 1970s.³⁵ Sheldon and Eleanor Glueck spoke of "maturation," "cessation," or "reformation" as a process quite different from the genesis of delinquency or criminality.³⁶ Sutherland used the term "reformation" as well,³⁷ both as a putative justification for punishment³⁸ and as a constructive process of character reorganization.³⁹ The latter process, Sutherland argued, could be explained by his differential association theory of crime and his group conflict theory of the criminal code. Reformation for Sutherland results when the offender assimilates the values and culture of the group responsible for the laws, or when that group "assimilates the criminal."⁴⁰

Though David Matza's theory of delinquency and drift⁴¹ was never intended to explain the behavior of the "compulsive or

³⁵ MARVIN WOLFGANG ET AL., *DELINQUENCY IN A BIRTH COHORT* 44 (1972).

³⁶ GLUECK & GLUECK, *500 CRIMINAL CAREERS*, *supra* note 1, at 257; GLUECK & GLUECK, *LATER CRIMINAL CAREERS*, *supra* note 1, at 98, 106.

³⁷ SUTHERLAND, *supra* note 1, at 357.

³⁸ *Id.*

³⁹ *Id.* at 363-64.

⁴⁰ *Id.* at 364.

⁴¹ DAVID MATZA, *DELINQUENCY AND DRIFT* 27 (1964).

committed delinquent,"⁴² much less the adult criminal, it provides conceptual tools that generalize to the study of desistance among these groups.⁴³ "Maturational reform," Matza's term for the age-graded desistance process, connotes a life-course explanation of crime and desistance.⁴⁴ Matza's "drift" theory is founded on an episodic view of crime: "delinquency is a status and delinquents are incumbents who *intermittently* act out a role."⁴⁵ Most delinquent youth spend long periods of time in a state of desistance that is only occasionally punctuated by delinquent activities. Over time, with age and increasing work and family responsibilities, most will eventually enter a permanent state of desistance.⁴⁶

More recently new models of desistance have been developed based on economic choice,⁴⁷ or choice plus a combination of sociological and social-psychological indicators.⁴⁸ In these models, situational factors such as negative criminal experiences and life events such as marriage cause offenders to re-evaluate their readiness to commit crime. Although these theories are usually tested with observational data, many of the mechanisms might easily be translated into manipulable interventions such as financial aid to releasees.

B. THE CAREER PARADIGM

Today, the concept of desistance from crime is most commonly associated with the study of "chronic offenders,"⁴⁹ "career

⁴² *Id.* at 30.

⁴³ *Id.* at 81.

⁴⁴ *Id.* at 22-26.

⁴⁵ *Id.* at 26.

⁴⁶ ROBERT J. SAMPSON & JOHN H. LAUB, CRIME IN THE MAKING: PATHWAYS AND TURNING POINTS THROUGH LIFE 217-30 (1993); NEAL SHOVER, GREAT PRETENDERS 119, 125 (1996).

⁴⁷ See Pezzin, *supra* note 2, at 33.

⁴⁸ Ronald V. Clark & Derek B. Cornish, *Modeling Offenders' Decisions: A Framework for Research and Policy*, in 6 CRIME AND JUSTICE: AN ANNUAL REVIEW OF RESEARCH 147, 168 (M. Tonry & N. Morris eds., 1985); Maurice Cusson & Pierre Pinsonneault, *The Decision to Give Up Crime*, in THE REASONING CRIMINAL 72, 74 (D.B. Cornish & R.V. Clarke eds., 1986); Shover & Thompson, *supra* note 2, at 93.

⁴⁹ WOLFGANG ET AL., *supra* note 35 at 88.

criminals," and the criminal career perspective more generally.⁵⁰ The career paradigm suggests that criminal activities follow distinct patterns and introduces concepts such as age of "onset," "persistence," and "desistance" to describe these patterns.⁵¹

The career concept has provoked a rancorous debate surrounding desistance. As Wolfgang et al. acknowledge,⁵² and Hirschi and Gottfredson criticize,⁵³ the concept of desistance appears to imply a criminal career. For Hirschi and Gottfredson, the term "career" connotes specialization in particular offenses and escalation in offense severity over time.⁵⁴ Since empirical research generally reveals versatility rather than specialization in offending⁵⁵ and decreasing severity rather than escalation with age,⁵⁶ Hirschi, Gottfredson, and other critics of the paradigm argue that no new insights are gained by conceiving of crime as a career.⁵⁷ Proponents of the career perspective, in contrast, maintain that no escalation in seriousness or specialization is implied by the term "career" and that the predictors of participation differ empirically from the predictors of onset and desistance.⁵⁸

This paper does not engage the criminal career debate, except insofar as it bears on the question of symmetric causation. If crime is conceptualized as a social event in the life course,⁵⁹ desistance can be conceptualized in a number of ways: (1) as

⁵⁰ Alfred Blumstein et al., *Criminal Career Research: Its Value for Criminology*, 26 CRIMINOLOGY 1, 1-36 (1988).

⁵¹ Michael Gottfredson & Travis Hirschi, *Science, Public Policy, and the Career Paradigm*, 26 CRIMINOLOGY 37, 40 (1988).

⁵² WOLFGANG ET AL., *supra* note 35, at 251.

⁵³ Travis Hirschi & Michael Gottfredson, *Age and the Explanation of Crime*, 89 AM. J. SOCIOLOGY 552-84 (1983).

⁵⁴ *Id.* at 574-78.

⁵⁵ Michael J. Hindelang, *Age, Sex, and the Versatility of Delinquent Involvements*, 18 SOC. PROBS. 522, 522 (1971); Malcolm W. Klein, *Offence Specialisation and Versatility among Juveniles*, 24 BRIT. J. CRIMINOLOGY 185, 185 (1984).

⁵⁶ SHELDON GLUECK & ELEANOR T. GLUECK, *JUVENILE DELINQUENTS GROWN UP* 89 (1940).

⁵⁷ GOTTFREDSON & HIRSCHI, *supra* note 25, at 241 (1990).

⁵⁸ Alfred Blumstein et al., *Longitudinal and Criminal Career Research: Further Clarifications*, 26 CRIMINOLOGY 57, 60 (1988).

⁵⁹ John Hagan & Alberto Palloni, *Crimes as Social Events in the Life Course: Reconceiving a Criminological Controversy*, 26 CRIMINOLOGY 87, 87-100 (1988).

simple non-crime, a behavioral state of indeterminate duration characterized by the absence of criminal events; (2) as non-crime conditional on prior commission of crime (e.g. one must first offend in order to desist); or, (3) as non-crime forever, a more-or-less permanent behavioral state characterized by the absence of criminal events. In this paper we have been primarily concerned with the latter two conceptions of desistance.

C. FUTURE DESISTANCE ANALYSIS

1. *Randomized Experimentation*

We have already learned a great deal from the few controlled experiments conducted with ex-offenders,⁶⁰ though such studies have historically examined rather circumscribed economic questions. To unravel the causes of desistance, further randomized experimentation for both juvenile and adult offenders is crucial. We therefore conclude on a programmatic note. We need not think of desistance programs as narrowly limited to existing correctional practices or evaluation research at the individual level. Desistance research can reasonably and profitably examine a diverse range of questions at the situational, group, or even the societal level of analysis.

2. *Comparative Work*

Comparative research on desistance is prompted by empirical differences across nations in factors such as the relation between age and desistance. In Japan, for example, the peak age of offending is earlier in adolescence and the decline in the late teen years is steeper than in the United States.⁶¹ The rationale for comparative analysis stems from potential American exceptionalism with regard to deviant behavior. The United States is

⁶⁰ KENNETH J. LENIHAN, WHEN MONEY COUNTS: AN EXPERIMENTAL STUDY OF PROVIDING FINANCIAL AID AND JOB PLACEMENT SERVICES TO RELEASED PRISONERS 1 (1976); IRVING PILIAVIN & ROSEMARY GARTNER, THE IMPACT OF SUPPORTED WORK ON EX-OFFENDERS 132 (1981); PETER H. ROSSI ET AL., MONEY, WORK AND CRIME: EXPERIMENTAL EVIDENCE 277-83 (Academic Press 1st ed. 1980).

⁶¹ Yutaka Harada, *Adjustment to School, Life Course Transitions, and Changes in Delinquent Behavior in Japan*, in 4 CURRENT PERSPECTIVES ON AGING AND THE LIFE CYCLE 35, 37 (Z.S. Blau & John Hagan eds., 1995).

clearly the most punitive of the advanced Western societies,⁶² with an incarceration rate that exceeds Germany's rate by a factor of seven and Japan's rate by a factor of sixteen.⁶³ This creates disproportionately large officially-defined deviant populations. Yet the United States lacks institutional mechanisms for integrating delinquents and reintegrating adult offenders into the social and economic fabric of civil society.⁶⁴ Many attribute Japan's low crime rates to cultural traditions of shaming and apology,⁶⁵ but quite apart from Japanese culture, a more rationalized school-to-work transition facilitates the integration of youth into adult society.⁶⁶ Experimental (or quasi-experimental) pilot programs in U.S. communities that are modeled on German or Japanese structures might better isolate their causal effects, as well as the residual effects of enduring cultural differences. Comparative analysis may also qualify, extend, or refine generalizations based on the American case.

IV. CONCLUSION

The goal of this paper has been to examine desistance among criminal offenders, rather than to plumb the limits of desistance research more generally. We believe that the same principles underlying desistance research on crime are likely to hold true for a wide range of deviant role-exits, such as substance use, homelessness, welfare receipt, non-traditional careers, perhaps even social-physiological phenomena such as eating disorders.

We have motivated this discussion of desistance with the Rubin/Holland causal model to highlight the extraordinary barriers to causal inference in etiological investigations. We believe it is virtually impossible to *assure* mean independence in

⁶² NILS CHRISTIE, *CRIME CONTROL AS INDUSTRY* 30-31 (Routledge 2d ed. 1994); ELLIOTT CURRIE, *CONFRONTING CRIME: AN AMERICAN CHALLENGE* 28-29 (1985).

⁶³ Marc Mauer, *AMERICANS BEHIND BARS: THE INTERNATIONAL USE OF INCARCERATION* 1995, at 4 tbl.1 (1997).

⁶⁴ BRAITHWAITE, *supra* note 25, at 179.

⁶⁵ *Id.* at 61-65.

⁶⁶ James E. Rosenbaum et al., *Market and Network Theories of the Transition from High School to Work: Their Application to Industrialized Societies*, 16 *ANNUAL REV. OF SOC.* 263, 282-85 (1990).

observational work on the causes of crime, despite the often brilliant methodological work-arounds devised by criminologists. If mean independence fails and we cannot account for selection into levels of our most critical independent variables, then the assumption of "strong ignorability" also fails. Under these conditions, any statistical solution to the fundamental problem of causal inference is unwarranted. Thus, we remain skeptical of the findings of etiological investigations.

As an alternative, we outline a case for increased theoretical and empirical work on the problem of cessation or desistance from crime. Desistance research better accounts for selectivity because the treatment is more often within the hands of the researcher. Moreover, we have greater ethical and constitutional license to intervene among admitted or convicted offenders and are thus able to administer stronger treatments to target groups. Among the challenges to learning more from desistance research are devising theoretically meaningful interventions and expanding and ensuring external validity. Our intent is not to discourage efforts for juveniles, but simply to acknowledge that we currently lack effective technologies for this population. More optimistically, desistance research could offer critical tests of existing theory, the potential for new breakthroughs, and concrete policy guidance.

Clearly, our advocacy of desistance research is not intended to suggest that it has yet to be undertaken. Rather the problem is that most such efforts are seriously flawed in terms of research design, sampling strategy, and theoretical justifications for the interventions being assessed.⁶⁷ Such investigations may be valuable for the administration of correctional or social services but are essentially without any implication for desistance theory or policy.

On the other hand, there are several desistance-oriented interventions whose evaluations have documented their content, suggested their crime control potential and, in some cases, specified the offenders for whom the interventions might be

⁶⁷ See Ted Palmer's excellent review of this literature and suggestions for future research in *A PROFILE OF CORRECTIONAL EFFECTIVENESS AND NEW DIRECTIONS FOR RESEARCH* 10-21, 67-178 (1994).

most effective. A partial list of these programs includes the National Supported Work Demonstration,⁶⁸ the Baltimore Life Study,⁶⁹ the multisystemic therapy program,⁷⁰ and the "Fixing Broken Windows" police intervention.⁷¹ There are also a few studies (and specifications of theories) that imply the desistance possibilities of yet-to-be implemented crime control interventions. Among these are the recent investigations of Rosenbaum and Popkin⁷² (suggesting possible desistance effects of residential change and its attendant opportunities) and Sampson and Laub (addressing the impact of the GI Bill on the criminal behavior of its beneficiaries).⁷³

That the degree of effectiveness of these interventions has not yet been definitively established is not surprising since, with few exceptions, they have received little systematic study following their initial exploration. This has been particularly true of interventions providing offenders with material assistance, training, or opportunities.⁷⁴ Though the reasons for this apparent neglect have received little discussion, two considerations seem relevant. Such interventions are likely to be expensive and politically sensitive, the latter a consequence of the potential costs as well as the reluctance of citizens and politicians to "reward" those who prey upon society's conventional members. Thus, if these programs are shown to increase the probability of desistance from crime among offenders, policy makers face a potential dilemma. Should they support full implementation of these programs they may face accusations that they reward the unwor-

⁶⁸ MANPOWER DEMONSTRATION RESEARCH CORP., SUMMARY AND FINDINGS OF THE NATIONAL SUPPORTED WORK DEMONSTRATION 133 (1980).

⁶⁹ Charles Mallar & Craig Thornton, *Transitional Aid for Released Prisoners: Evidence from the LIFE Experiment*, 13 J. HUM. RESOURCES, Spring 1978, at 208, 233.

⁷⁰ SCOTT HENGGELER & CHARLES BORDUIN, FAMILY THERAPY AND BEYOND 55 (1990).

⁷¹ GEORGE KELLING & CATHERINE COLES, FIXING BROKEN WINDOWS 23 (1996).

⁷² James Rosenbaum & Susan Popkin, ECONOMIC AND SOCIAL IMPACTS OF HOUSING INTEGRATION 1 (1990).

⁷³ Robert Sampson & John Laub, *Socioeconomic Achievement in the Life Course of Disadvantaged Men: Military Service as a Turning Point, Circa 1940-1965*, 61 AM. SOC. REV. 347, 363 (1996).

⁷⁴ More punitive interventions, such as mandatory arrest for domestic abusers, are frequently replicated. Lawrence W. Sherman & Richard A. Berk, *The Specific Deterrent Effects of Arrest for Domestic Assault*, 49 AM. SOC. REV. 261, 268 (1984).

thy for their criminal behavior; conversely, should they oppose implementation, they may be criticized for withholding proven crime control measures. Given the alternatives, policy makers may turn to the evaluation of more politically acceptable experiments based on such factors as increased police manpower, greater use of arrest, and longer prison sentences. Although the merits of these policy measures cannot be dismissed out-of-hand, their promise seems no greater than and their costs no less than the desistance strategy we suggest.

The merits of a desistance strategy apply as well to the advancement of academic knowledge as they do to urgent policy imperatives. Perhaps the most fundamental asymmetry in the study of crime and desistance is in our ability to intervene and thus isolate the respective causes of these phenomena. If so, desistance research could further John Dewey's goal of moving social science from a "passive and accumulative" stance to an active and productive one.⁷⁵ For Dewey, as for Rubin and Holland, "As far as we intentionally do and make, we shall know."⁷⁶

⁷⁵ John Dewey, *Social Science and Social Control*, THE NEW REPUBLIC, July 29, 1931, at 276.

⁷⁶ *Id.* at 277.