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SHAWN D. BUSHWAY

Traditionally, criminologists have sought to understand how unemployment can lead to crime. Recently, however, a group of criminologists have begun to consider ways in which crime itself might lead to employment problems. One idea, advanced by Hagan, is that youths involved in crime do not develop the necessary social and human capital necessary to succeed in the legal labor market. A second idea, advanced by Sampson and Laub, is that the formal sanctions of the criminal justice system lead employers to avoid individuals who might otherwise succeed in the labor market. This article tests the competing implications of these two theories by using the method of “differences in differences” on the National Youth Survey. The evidence from this article suggests that arrest can lead to minor problems in the labor market above and beyond the impact of current or past criminal activity.

Traditionally, criminologists have measured the impact of employment problems on criminal activity (Chiricos 1987). More recently, criminologists have also begun to consider the possibility that the causality is bidirectional. Discussions of how crime itself might lead to employment problems have their roots in developmental theories that attempt to explain the dynamic process by which past offending may lead to future offending.

One possibility is that offending weakens social bonds, which, in turn, increases later adult crime. Thornberry (1987) formalized this idea of a reciprocal relationship between offending and factors that may influence future offending in his interactional theory. He states,

[A] behavioral trajectory is established that predicts increasing involvement in delinquency and crime. The initially weak bonds lead to high delinquency

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involvement, the high delinquency involvement further weakens the conventional bonds (represented, during adolescence, by attachment to parents, commitment to school, and belief in conventional values), and in combination both of these effects make it extremely difficult to reestablish bonds to conventional society at later ages. (P. 883)

Hagan (1993) notes that this process, which he calls "social or criminal embeddedness," leaves delinquent youths without the necessary human and social capital to successfully participate in legal employment when full-time employment becomes realistic. As a result, Hagan claims that any correlation between contact with the criminal justice system and adult employment problems will be spurious, the leftover impact of the interaction between the offender and agents of adolescent social control during adolescence. Hagan bases this claim on the observation that criminal activity peaks for most people during mid- to late adolescence, before employment is an important factor in an individual's life.

In contrast to Hagan, Sampson and Laub (1997) and Thornberry (1987) claim that important behavioral change can be caused during adulthood by active interaction between the offender and adult institutions like employment. Sampson and Laub (1997) specifically claim that formal labeling by the criminal justice system in adulthood will directly cause employers to exclude adult ex-offenders from employment opportunities. Such exclusion leads to job instability, which has been shown to increase offending (see Cook 1975; Farrington et al. 1986; McCarthy and Hagan 1992; Needels 1996; Sampson and Laub 1993; Uggen 1996).

In contrast to both of the above theories, economist Gary Becker (1968) views the decision to participate in crime as a time-allocation decision. People who are criminally active find criminal activity more profitable than legal labor market activity. As a result, they are not as committed to the legal labor market as their noncriminal counterparts and therefore have more unstable job histories. Any link between criminal activity and job instability is noncausal.

The differences between these three theories, although subtle, have both theoretical and policy implications. From a theoretical perspective, Hagan is claiming that the dynamic causal process by which crime is generated is essentially over by late adolescence. Sampson and Laub's story is more dynamic because the interactional processes that generate crime essentially continue through adulthood. As an economist, Becker is mute on the process by which criminal proclivities are formed.

From a policy perspective, both Hagan's and Becker's theories imply that policymakers should not be worried about the employer use of criminal history records. Yet, if Sampson and Laub are right, employer actions actually cause employment problems that would otherwise not occur. As a result,
policymakers might want to consider more carefully the consequences of granting employers access to criminal history records.

Using data from the National Youth Survey (NYS), this article seeks to shed light on the debate by testing for the relationship between criminal behavior, arrest, and job stability. The empirical predictions about the job stability of young delinquents from these three theoretical ideas are remarkably clear and distinct. Hagan's theory implies that only prior criminal activity should matter, and Becker's theory implies that only current criminal activity should matter, whereas Sampson and Laub's theory puts the weight on involvement with the criminal justice system (arrests in the NYS), net of either past or current criminal involvement. Using the method of "differences in differences," the results suggest that arrest, rather than past or current criminal involvement, is responsible for the increase in job instability following arrest.

PREVIOUS EMPIRICAL RESEARCH

Labeling theory predicts that formal labels from social agents in the criminal justice system can "type" or "cast" an individual as "essentially" deviant, even if the individual is an otherwise nondeviant individual (Garfinkel 1956; Lemert 1951; Matza 1969; Paternoster and Iovanni 1989; Scheff 1966). This label of deviant can take on a kind of "master" status that drives the reaction of control agents and peers to the individual, despite the existence of other secondary, more positive characteristics. According to Becker (1963),

[1]he status of deviant (depending on the kind of deviance) is this kind of master status. One receives this status as a result of breaking a rule, and the identification proves to be more important than most others. One will be identified as a deviant first, before other identifications are made. The question is raised: "What kind of person would break such an important rule?" And the answer is given: "One who is different than the rest of us, who cannot or will not act as a moral human being and therefore might break other important rules." The deviant identification becomes the controlling one. (Pp. 33-34)

Becker implies here that one consequence of being typed as essentially deviant is that one becomes, in the eyes of others, generally less trustworthy, as one who "might break other important rules." An employer who uses the existence of a criminal history record to exclude an individual from consideration for a job, without consideration of the applicant's other merits, would be effectively labeling an individual in the manner described above.
Not all uses of a criminal history record by employers can be accurately described as labeling. Employers can make use of criminal history information as a signal in a relative assessment about the qualifications of an individual for a job (Jensen and Giegold 1976). Examples of cases where employers would interpret a criminal history record as a signal rather than a label include restricting convicted drunk drivers from jobs as truck drivers or restricting an individual arrested multiple times (but not convicted) for child molestation from working in a child care center. Case law in New York actually limits the use of criminal history records to cases where the qualifications for the job have a demonstrable link to past offenses (Belair 1988).

Although, no doubt, this more subtle use of criminal history records does take place in the hiring process of some employers, evidence suggests that employers primarily use a criminal history record to make judgments about the general or essential character of an applicant in the manner described above by Becker (1963) (Finn and Fontaine 1985). According to a survey by Hulsey (1990), employers who will not hire ex-offenders are most concerned about the general “trustworthiness” of the ex-offenders rather than anything specifically related to the offense or the job in question. In addition, federal legislation explicitly denies employment to ex-felons from any job in the financial sector, as well as in other areas such as child care and private investigation (Bainbridge 1985). Most states also have legislation mandating screening on the basis of criminal history records for literally hundreds of jobs, either directly or through licensing requirements for “good moral character” (Burton, Cullen, and Travis 1987). A court case from 1898 (Hawker v. New York, 170 U.S. 189), cited as precedent in recent cases, makes it clear that the state “may make the record of a conviction conclusive evidence of the fact of the violation of the criminal law and the absence of the requisite good character” (emphasis added, as cited by Rubin 1971:3).

Although many of the affected jobs have educational requirements beyond the educational attainment of the typical ex-offender (Cook 1975), restrictions on the basis of “moral character” clauses are also found for licenses for jobs well within the reach of the ex-offender population, such as barbers, apprentice electricians, and plumbers (Dale 1976; Downing 1984; Sampson and Laub 1997).

Survey evidence also demonstrates that employers sometimes refuse employment to an individual solely on the basis of his or her criminal history record (e.g., Boshier and Johnson 1974; Buikhuisen and Dijkstra 1971; Tromanhauser 1976). Perhaps the widest-known study was conducted by Schwartz and Skolnick (1964), who circulated job applications that varied only in that a percentage of them contained information about an assault charge. Although the application was for an unskilled labor position
(dishwasher), the applications of convicted individuals faced a statistically significant lower probability of generating a positive response than did the applications of nonoffenders. More recently, an employer survey in five major U.S. cities for relatively unskilled jobs by Holzer (1996) found that roughly 65 percent of all employers would not knowingly hire an ex-offender (regardless of the offense), and between 30 percent and 40 percent actually checked the criminal history records of their most recently hired employee.

This kind of survey evidence only proves that some employers label potential employees on the basis of their criminal history record. It does not prove that such labeling results in significant job instability over and above what would otherwise occur. A delinquent youth with poor self-control might have problems maintaining stable employment absent any action by employers (Gottfredson and Hirschi 1990). Or, problems with job stability following an arrest could actually be the result of the criminal activity itself, as noted in the introduction. Hence, any definitive study on the impact of contact with the criminal justice system on employment must control for time-stable individual differences, as well as past and current criminal involvement. The best way to do that is with longitudinal data that have multiple observations of behavior over time.

Hagan’s research on this question, using Farrington’s London data, found that although self-reported delinquency at ages 18 and 19 had an impact on the probability of experiencing a spell of unemployment at ages 21 and 22, the number of official convictions at ages 18 and 19 did not have an impact. Hagan controls for individual heterogeneity with a long list of observed measures of individual behavior. Hagan (1993) interprets this result to mean that “the son’s behavioral embeddedness in youth crime more than official convictions . . . is influencing unemployment at this stage” (p. 482).

As a counterexample, Sampson and Laub’s (1993) reanalysis of the Glueck data found that, controlling for excessive drinking, sex, age, race, and delinquency, length of incarceration as a juvenile had a significant impact on job stability for the delinquent subsample at ages 17 through 25 and ages 25 through 32. Self-reported delinquency and arrests per year free had no impact on job stability. This result is consistent with the hypothesis that official contact with the criminal justice system, at least serious contact, has a negative impact on job stability.

The finding that incarceration has a significant negative impact on employment is consistent with results from two studies by Freeman (1991a, 1991b). Using the National Longitudinal Survey of Youth (NLSY), he found that having been jailed in 1980 led to fewer weeks of employment during the follow-up period (1980-1988). Controls for individual heterogeneity were limited to age, education, and region, and controls for criminal activity were
limited to questions about current alcohol and drug abuse. He found no
evidence that arrest or conviction led to problems in the labor market. In the
Boston Youth Survey of out-of-school youths, Freeman was able to use better
controls for individual heterogeneity (age, race, education, living arrange-
ments, public housing, marital status, religious attendance, household size)
as well as slightly better controls for criminal activity (current alcohol use and
gang membership). Once again he found that jail time reduced the probability
of employment by between 15 and 30 percentage points from a mean of 87 percent.
Conviction and probation also had an impact on employment probabilities in
this second study, although the effect was smaller (employment probabilities
decreased by between 6 and 10 percentage points).

The finding that incarceration, rather than lesser contact with the criminal
justice system like arrest or conviction, has an impact on employment is
somewhat problematic. Incarceration, especially length of incarceration, may
convey information about the seriousness of offending or other aspects of an
individual's character that could by itself contribute to job instability (Smith
and Paternoster 1990). In addition, the process of incarceration could actually
change an individual into a less stable employee. The idea that employer
response to formal labels leads to subsequent job instability would be stronger
if an effect was found for arrest or conviction.

Thornberry and Christenson's (1984) study of the reciprocal relationship
between unemployment and crime using the follow-up sample from the 1945
Philadelphia cohort did, in fact, find that arrest itself led to more time
unemployed in the next two years. However, unlike the above studies, no
controls were included for criminal activity or observed individual heteroge-
neity. As a result, it is not possible to know whether the actual arrest led to
the employment problems (formal labeling) or employment problems were
the result of individual differences and/or criminal involvement.

*Controls for Unobserved Heterogeneity*

The above studies attempt to control for individual heterogeneity with
observed measures only. It is also possible to control for unobserved individ-
ual differences. There are two generally available methods, fixed-effect
estimation and random-effect estimation. Fixed-effect estimation is the more
general, less restrictive model (Hsiao 1986). The relationship between ar-
rest/conviction and employment is identified on the basis of the change in an
individual's employment patterns *after* contact with the criminal justice
system. It controls for all static factors, unobserved and observed. The
technique assumes that because individual specific characteristics are time
constant, any change in employment outcomes from one period to the next
should be the result of factors about an individual or his or her environment that change over time, such as an arrest.

Random-effect estimation, in contrast, is identified on assumptions about the distribution of the unobserved heterogeneity in the population. It controls only for those unobserved factors that are uncorrelated with the included exogenous variables. In this context, this means that if criminal propensity, intelligence, or other unobserved factors are correlated with arrest, this method will not control for these factors, and the arrest measure could be biased. Despite this disadvantage, it does have the advantage of being more practical for use with dichotomous dependent variables.

There are several articles that make use of these techniques to control for unobserved heterogeneity. Sampson and Laub (1993) implemented random-effect estimation on the Glueck data and still found that incarceration, rather than criminal offending, leads to employment problems. Waldfogel (1994) also used random-effect estimation on 2,200 federal fraud and larceny offenders from the Federal Probation and Parole Sentencing and Supervision (FPSS) research file. This data set has employment data before and after an individual was convicted. No nonarrested individuals are included in the sample, so the conviction effect is estimated on the basis of the assumption that employment probabilities would not have changed except for the conviction. He found a negative, significant impact of federal fraud (but not larceny) convictions on the probability of employment. Conviction and probation led to a reduction of employment probabilities of 2 to 5 percentage points. Prison time for federal fraud and larceny convictions led to a reduction in employment probabilities of 6 to 9 percentage points. This result is comparable with Freeman’s results for conviction of a drop of 6 to 10 percentage points.

Grogger (1995), in contrast, used fixed-effect estimation on a longitudinal sample of 343,714 people created by matching criminal history records with quarterly employment data in California from 1980 to 1984. A comparison group of 2,901 individuals was composed of people who offended from 1985 to 1988 and were, therefore, nonoffenders during the study period. These people were younger and less likely to be employed than the offenders in the sample. Arrest and conviction in general caused no decline in employment probability over the next six quarters. Probation also had no impact. Property arrests and multiple arrests did, however, lead to a decline of between 2 and 6 percentage points in employment probabilities. Like Freeman, Grogger also found that the impact increases with incarceration. The effect of jail time decreased from a decline of 8 percentage points in employment probability the next quarter to a decline of 4 percentage points six quarters later. The impact of prison was far more severe, with a high decline of 24 percentage.
points in employment probabilities the quarter after the arrest. These results are very similar to Freeman’s incarceration results, but questions about their reliability remain because the offenders are most likely incarcerated during the period of observation.

Although each of the three preceding studies do control for unobserved heterogeneity, the samples only include individuals who have been, or will be, arrested. This limits the ability to construct a convincing “but for arrest” story. Also, the latter two studies do not include measures for past or current criminal activity, a key component of the current discussion.

One study that has both a good comparison group and controls for criminal activity is a study by Nagin and Waldfogel (1995), which used Farrington’s British youth cohort, previously studied by Hagan (1993). In their study, Nagin and Waldfogel concentrate on the change in wages and employment stability from ages 17 to 19 for categories of youths that were divided by criminal history record and criminal involvement. In this way, they measure the effect of a conviction on employment above and beyond the effect of criminal involvement alone. This version of fixed-effect estimation, known as “difference in differences,” is more intuitive than traditional fixed effects. It will be explained more thoroughly in the following section.

The results of their analysis are striking—the conviction effect on employment stability is strong and negative, whereas current and past criminal activity by itself had no effect on employment stability. For example, conviction led to two more weeks of unemployment (from a mean of about 2.5 weeks) for those individuals who were already criminally active at age 17, compared with their counterparts who were criminally active at both points in time but had not been convicted. This suggests that the conviction, rather than criminal offending itself, explains the change in employment outcomes after a conviction.

Further work is needed to substantiate the results of the Nagin and Waldfogel (1995) article. The remainder of this article presents the first longitudinal study with repeated observations of criminality to use a representative, contemporary sample of out-of-school U.S. youths, the National Youth Survey (NYS).

METHOD

Study Design

The analysis uses a panel data set collected by Delbert Elliot and David Huizinga at the Behavioral Research Institute, University of Colorado. The
NYS used a probability sample of households in the continental United States. The 1,725 participating youths, aged 11 to 17 at the time of the initial interview in 1977, were selected to be representative of the total youth population of the same age in the U.S. Census Bureau (Elliot et al. 1983). The youths were interviewed annually from 1976 to 1980, and then at three-year intervals beginning in 1983. Only the data from the first seven waves are publicly available. A wide range of education (e.g., years of schooling, grade point average [GPA]), socialization (e.g., family involvement), delinquency, and labor market performance variables were collected. See Elliot et al. (1983) for a complete discussion of the data set.

This study will deal with first arrests during the period from 1983 to 1986 for individuals who were out of school in both years. It is important that individuals be out of school in both periods because the jobs of out-of-school youths tend to be categorically different both in type (full-time versus part-time) and purpose (money for necessities versus extra spending money) from the jobs of youths who have not finished their schooling (Williams, Cullen, and Wright forthcoming). Thornberry and Christenson (1984) make a similar distinction in their work.

Women were not used because there were only four first arrests for the 191 women who report being out of school in both periods without being arrested before 1983. This is too few people to allow for estimation of the separate impact of arrest on employment for women, yet it seems unwise to assume that arrest affects women the same way as men.

In a related sampling issue, the sample also excludes African Americans. The NYS arrest data are from self-reports rather than official records. According to Weis (1986), official records collected for the NYS, but not released to the public, show that self-reports and official records are consistent for all groups but African American men. African American men apparently underreport arrests by a factor of three in the NYS. This is consistent with findings from other surveys (Weis 1986).

Grogger (1992) faced similar problems in his analysis using the NLSY. In the California data using official records, arrests explained fully two-thirds of the employment differential between the races. However, in the NLSY, as in the NYS, African Americans and Whites self-report similar levels of arrest. As a result, the estimate of the impact of arrest on employment was biased. To avoid this problem in the current study, African American men are excluded from the sample.

After excluding women and African American men, there are 178 White men who were out of school and in the workforce in both periods and not arrested by 1983. The men average 21 years of age in 1983. Sample attrition is a minor issue. Elliot et al.(1983) report a 13 percent loss for the first five
waves, with no selective loss relative to self-reported delinquency. However, 20 percent of all individuals with a self-reported arrest by 1983 did not answer the survey in 1986, whereas only 8.6 percent of all those without an arrest in 1983 did not answer the survey in 1986. Although troublesome, it is not directly problematic for this article because the analysis concentrates on people arrested for the first time between 1983 and 1986.

Measures

This article will use only first-time adult arrests that occur during the time period from 1984 to 1986. The arrests cover a range of offenses from the somewhat trivial (underage drinking) to the very serious (rape), but most of the offenses are not serious. In fact, only 2 percent of the entire White male sample in the NYS (or 8.3 percent of all arrestees) had been arrested for a serious crime by 1986 (FBI Index I plus fraud). This fact makes this study somewhat unique, because most other studies concentrate on serious offenses. A finding in this study that first-time adult arrests lead to employment instability will be especially strong given the relatively minor nature of the arrests. The nature of the arrests also mitigates against the lack of accurate disposition data in the NYS. As in Grogger (1992), findings of job instability following cases that lead to incarceration are suspect because of possible exclusion from the labor market. However, because most cases are relatively minor first offenses, it is likely that incarceration rarely occurred.

Two measures of employment stability will be used: number of weeks worked at the major source of employment over the last year (hereafter known as job length) and a proxy for job stability. An individual will be considered stable if he has worked only one job this year for 40 weeks or more, or he has worked more than one job but has a full year at one job. This specification of job stability is based on social control theory, which stresses the importance of social bonds formed on the job. These bonds have a better chance of forming when an individual has been employed for a long period of time at one place. Unfortunately, these measures are not directly comparable with measures used in previous studies because of constraints in the data. Wave 6, the first year the survey was administered after a three-year gap, only included questions about one job, although a full employment history is available at all other waves. As a result, it is not possible to determine number of weeks unemployed, as in Thornberry and Christenson (1984) or Nagin and Waldfogel (1995). In addition, a person is considered unemployed only if he has not worked all year. It is therefore also not possible to construct a measure of unemployment similar to most other studies.
These two measures are highly correlated. This is to be expected because anyone with less than 40 weeks of work at one job will be considered unstable. Despite the high correlation, both measures will be analyzed separately. Although the stability measure captures in some sense the “right” concept (the overall job stability during the past year), the discrete nature of the measure could be problematic given the analytic method, which depends on changes in outcomes. The job length measure, which is continuous, should provide more information about changes.

The potentially confounding impact of criminal activity will be measured with a dichotomous measure of criminal activity in the past year. An individual will be considered criminally active if he reported doing any of the following relatively minor delinquent acts: carried a weapon, stole less than $50, sold marijuana, hit a parent, sold hard drugs, or took a vehicle; or if he reported doing any one of the following serious acts in the past year: stole a vehicle, stole more than $50, bought stolen goods, had sex with someone against that person’s will, broke into a building, attacked someone, or used force on someone. Finally, a man was considered to be criminally active in 1986 if he had been arrested in the past year. On average, individuals who have committed at least one seriously delinquent act compose about 50 percent of the overall criminally active group.

**Analysis Strategy**

The “difference in differences” approach outlined by Nagin and Waldfogel (1995) will be used to isolate the effect of an arrest and past and current criminal activity on labor market performance for youths who are out of school for the period between 1983 and 1986. This approach controls for individual heterogeneity, which might be responsible for any observed differences in labor market performance between the arrested and nonarrested individuals. The approach has two components. The first component is the difference in job stability measures for those people who are arrested for the first time in the period between 1983 and 1986. This difference would have been caused at least in part by the arrest or a change in offending patterns.

As noted by Waldfogel (1994), this difference by itself will not tell researchers the true impact of an arrest because job patterns will change as individuals age. This sample averages 21 years of age in 1983 and is just settling into more permanent employment. One would not expect people to have the same employment outcomes at 21 as they do at 24. This fact is what makes the comparison group so important. Without adequate controls, the researcher is not sure if the observed change is due to the arrest or other time-varying factors such as criminal activity, marriage, or place of residence.
The second component of the differences-in-differences method, then, takes advantage of the comparison group by comparing the difference in outcomes for the arrested individuals with the difference in outcomes for a comparison group of individuals who have not been arrested but are otherwise similar to the arrested individuals.

By using this approach, the general population nature of the NYS is exploited to full advantage. Recall that Waldfogel (1994) and Grogger (1995) both struggled with the lack of true comparison groups because their studies used samples of offenders only. The impact of an arrest will be the difference between the differences in the employment outcomes for 1983 and 1986 for the arrestee group and the comparison, nonarrestee group; hence the name “differences in differences.”

This method will be operationalized by splitting the sample into two groups of people: (1) those who have never been arrested and (2) those who have been arrested for the first time between 1983 and 1986. The effect of an arrest on someone who has never been arrested is simply the difference in the labor market outcomes for the arrestees less the difference in labor market outcomes for the nonarrestees. In the following discussion, let \( y_i^t \) denote a specific measure of job performance for an individual of arrest status \( i \) at time \( t \). Formally, the effect of arrest on previously nonarrested individuals is then

\[
[y_{\text{arrestee}}^{86} - y_{\text{arrestee}}^{83}] - [y_{\text{nonarrestee}}^{86} - y_{\text{nonarrestee}}^{83}].
\]

This can also be done in a regression format in which the difference in labor market outcomes is regressed on a dummy variable set equal to 0 if the person is never arrested, and 1 if the person is arrested by 1986.

Controls for other things besides arrest that may change during the same time period will be included in the second stage of the analysis, using the regression format. The most relevant candidate here is criminal activity. Rational choice theory predicts that contemporaneous criminal activity affects employment stability. This possibility can be measured by including changes in criminal activity in the model. Changes in marital status and place of residence that occurred at the same time period as the arrest will also be included.

Because each of the factors is a dichotomous variable, change must be captured by dummies representing changes in states. For criminal activity, people are divided into four groups: chronic offenders (people who report offending in both periods), desistors (people who report offending in 1983 but not in 1986), initiators (people who report offending in 1986 but not in 1983), and abstainers (people who do not report offending in either period). The abstainers are the omitted group. Note that each of these groups will have people who have been arrested. For example, someone who has been arrested in 1984 but did not report criminal activity in 1983 or 1986 will be considered
an abstainer. When both offense categories and arrest are included in the model, it is up to the model to identify which pattern of behavior best explains changes in the employment outcomes. For marriage, people are divided into four groups: those who are married in both periods (the omitted group), those who are never married, those who marry sometime after 1983, and those who separate after 1983. For place of residence, people are divided into nine groups representing all the possible combinations of movement from urban, suburban, and rural locations. Those who stay in a rural area are the omitted group in the model. This model, with changes modeled for all variables, is essentially the same as the traditional fixed-effect approach.

What makes the differences-in-differences approach different from the traditional fixed-effect approach conceptually, however, is the requirement that the two groups look comparable in the first time period on the dependent variable of interest. This requirement is meant to deal with possible differences in relevant characteristics between the two groups. Yet, the fact that the two groups might vary on characteristics such as age, place of residence, or even criminal activity in 1983 is technically not a problem, because these are time-stable characteristics whose impact should be eliminated by the process of differencing. But this is only true if the characteristics have the same impact in both time periods, something that might not be true in periods of great change, like early adulthood.

Consider the case where the members of the arrested group have a more criminally active past than members of the nonarrested group. What if those without a criminal past have better ties to conventional society, and these ties (social and human capital) help these individuals land on more stable career trajectories, as predicted by Hagan (1993). This trajectory might include more time spent on the job in 1986 than in 1983. Hence, in relative terms, the lack of criminal activity had a bigger impact in 1986 than it did in 1983. As a result, a finding that the arrested group experiences declines in job stability relative to the nonarrested group could still be the result of the prior criminal activity. This same confounding could occur for any other time-stable characteristic.

This problem is ameliorated somewhat if the two groups have similar levels of job stability in 1983 before the arrest occurs. If prior criminal involvement (or any other time-stable characteristic) varies between the two groups, but these factors appear not to affect job stability in 1983, it seems less likely that prior involvement will then affect what happens between 1983 and 1986.

Less likely, but still possible. Because prior criminal involvement is the key rival hypothesis to the hypothesis that arrest leads to changes in employment stability, the third model to be estimated will actually include measures
for past criminal involvement and other prior behavior in the difference regression. This approach explicitly allows the initial levels of these variables to affect changes in job stability. If the impact of arrest does not change when these measures are added, it seems unlikely that the causal impact of past criminal offending is being captured by the arrest measure.

Although Hagan (1993) stipulates that only adolescent criminal involvement is important, it would be difficult to limit the criminal involvement measure to adolescence in the NYS given the age range of the NYS. For example, looking at crime at age 17 would require that wave 1 data be used for those originally aged 17 in wave 1 and the year before wave 6 for those who were aged 11 in wave 1. This latter information is not available. Furthermore, the time span between the activity and the measured employment outcomes in 1986 would then depend on each individual's age. For example, if the employment outcomes from 1983 to 1986 are used, the difference between offense and last employment outcome would range from 5 to 11 years. It seems wiser to concentrate on a fixed amount of time between offense and employment outcomes. In this model, a measure of criminal activity in 1980, when the median age of the sample was 18, will be used to predict employment changes from 1983 to 1986. The testable hypothesis is whether or not an arrest sometime between 1984 and 1986 will still affect employment stability after the inclusion of controls for criminal activity in 1980. Age, place of residence, and marital status in 1983 will also be included. The omission of other relevant factors not measured by the NYS, like alcohol use, will be discussed in the conclusion.

RESULTS

The averages for a variety of individual characteristics in 1983 for the nonarrestees and the arrestees are given in Table 1. The job lengths for the two groups were very similar in 1983, with each group working on average about 33 weeks at their major job in 1983. The job stability measure is also similar in 1983 for the two groups, 39.22 percent of the nonarrestees versus 32 percent of the arrestees were employed in “stable jobs.” As noted above, this similarity makes the test of the impact of an arrest using the differences-in-differences method more robust. Given that there is no difference in 1983, it becomes difficult to argue that any preexisting difference between the two groups, rather than arrest, is responsible for the change that occurs between 1983 and 1986.

Nonetheless, it is also important to note that GPA in 1976, highest grade completed, and age are also very similar between the two groups. But not all characteristics are similar between the two groups. The nonarrested individu-
### TABLE 1: Comparison of Group Means in 1983 of Individual Characteristics

<table>
<thead>
<tr>
<th>Individual Characteristic</th>
<th>Non-Arrestees</th>
<th>Arrested by 1986</th>
<th>t Test on Means</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of observations</td>
<td>153</td>
<td>25</td>
<td></td>
</tr>
<tr>
<td>Job length (weeks)</td>
<td>33.31</td>
<td>33.64</td>
<td></td>
</tr>
<tr>
<td>Job stability (%)</td>
<td>39.22</td>
<td>32.00</td>
<td></td>
</tr>
<tr>
<td>GPA(^a) in 1976 (1-5)</td>
<td>3.55</td>
<td>3.44</td>
<td>0.67</td>
</tr>
<tr>
<td>Age</td>
<td>21.37</td>
<td>20.92</td>
<td>1.09</td>
</tr>
<tr>
<td>Highest school grade completed</td>
<td>11.89</td>
<td>11.88</td>
<td>0.025</td>
</tr>
<tr>
<td>% committed &quot;criminal&quot; act in 1980</td>
<td>38.56</td>
<td>56</td>
<td>1.63(^*)</td>
</tr>
<tr>
<td>% committed &quot;serious&quot; criminal act in 1980</td>
<td>14.38</td>
<td>20</td>
<td>0.73</td>
</tr>
<tr>
<td>Number of offenses in 1980</td>
<td>3.94</td>
<td>8.56</td>
<td>1.86(^**)</td>
</tr>
<tr>
<td>Number of offenses in 1983</td>
<td>4.50</td>
<td>7.76</td>
<td>-1.21</td>
</tr>
<tr>
<td>% living in rural areas</td>
<td>32.7</td>
<td>12.0</td>
<td>2.09(^**)</td>
</tr>
<tr>
<td>% living in urban areas</td>
<td>3.5</td>
<td>32.0</td>
<td>-0.91</td>
</tr>
<tr>
<td>% married</td>
<td>21.6</td>
<td>8.0</td>
<td>1.58(^*)</td>
</tr>
</tbody>
</table>

NOTE: Test is for the difference of group means in 1983. The null hypothesis is that the groups are the same. Groups are categorized by arrest and criminality status in both 1983 and 1986 as described in the text. In this table and in Table 2, the comparisons for means are done with a t test. The comparisons for proportions were done with a z test for proportions.

\(a\). GPA = grade point average.

\(^*\) Test statistic is significant at the 10 percent level (one-tailed test).

\(^**\) Test statistic is significant at the 5 percent level (one-tailed test).

als were significantly more likely to live in rural areas and be married than the arrestees. Either of these facts could contribute both to lower arrest rates and better employment outcomes. In addition, only 38.56 percent of the nonarrestees were criminally active in 1980, whereas 56 percent of the arrestees were criminally active in 1980. This difference, although only significant at the 10 percent level (one-tailed test), seems substantial. Even though there is no difference in the participation in serious acts, the average number of more general delinquent acts is also significantly different for the two groups. Clearly, Hagan’s theory must be taken seriously. Those who get arrested for the first time after 1983 are more criminally active before the arrest. So, subsequent declines in employment outcomes, relative to the group with lower criminal activity, could be the result of prior criminal involvement, and not the result of arrest. Hence, the NYS presents the same inferential conundrum about the cause of the decline found in other studies and highlighted in the theoretical debate.

As a baseline, it is important to compare employment outcomes for the two groups after the arrest occurs. This analysis is not without controls. The fixed effect implicit in the differences-in-differences approach and the initial comparability of the job stability measures in 1983 between the two groups
provides some assurance that time-stable factors such as initial criminal activity have been taken into account. This comparison is given for job length in Figure 1.

Despite the fact that the two groups had nearly identical job lengths in 1983, the groups are now significantly different in 1986. The nonarrestees increase their employment by 1986 to an average of 42.84 weeks, for an increase of 9.53 weeks. The arrested individuals, on the other hand, only work on average 36.16 weeks at their major job in 1986, for an increase of 2.53 weeks. Arrested individuals clearly did not see the same increase in employment at their major job as those people who had not been arrested. The difference-in-difference result of −7 weeks (2.53-9.53) is given in the first column of Table 2. It is statistically significant at the 5 percent level. Hence, it appears that an arrest leads to job instability, even though arrested individuals saw a net increase in employment at their major job over that period.

The comparison of the two groups for those in so-called stable jobs is given in Figure 2. In 1986, a full 69.28 percent of the nonarrestees were now employed in “stable jobs” (for an increase of 30.06 percentage points), but only 48 percent of the arrestees were so employed (for an increase of 16 percentage points). Because this measure is dichotomous, the difference-in-difference estimate will have three levels: −1 for someone who had a stable job history in 1983 but not in 1986, 0 for someone who had the same type of history in both periods, and 1 for someone who had an unstable history in 1983 but a stable history by 1986. Ordinary least squares (OLS) estimation on this model will force the transition from each state to have the same relationship with arrests, although these are qualitatively different outcomes. Performing ordered probit estimation on these outcomes eliminates this problem, although the coefficient on arrest can no longer be interpreted as the traditional differences-in-differences coefficient.

The results for the ordered probit on the job stability measure in model 1 are also reported in Table 2. The impact of arrest on job stability is negative but not significant. So, in the first model, arrest has a nonspurious impact on job length but does not appear to affect job stability. The lack of statistical power, despite the objectively large difference, could be caused in part by the relative lack of information available in the dichotomous measure and the small sample size.

Of course, this first simple model does not control for other things that change besides arrest during the 1983 to 1986 time period. Model 2 attempts to address this concern by controlling for changes in marital status, residential status, and offending status. This step explicitly tests the time allocation idea implied by rational choice theories of crime. If people who are criminally active are simply choosing to spend time committing crimes rather than
Figure 1: Job Length by Arrest Status: 1983-1986
Figure 2: Job Stability by Arrest Status: 1983-1986
working, the effect of arrest on job length and stability should lessen or even disappear. The results are given in Table 2.

In the job length equation, with controls for changes in married status, residence, and offending, the coefficient on arrest is actually stronger than without controls. There is now a 10.78 week “difference in difference” in the number of weeks worked at the major job. This difference is significant at the 5 percent level. This result appears to suggest that arrest, and not current criminal activity (or marriage or changes in residence), is the causal agent in the decline in job length during this time period.

Although this is the key result, the coefficients on the other independent variables are also interesting. Staying unmarried is the only significant married variable. It actually leads to a significant 9.21 weeks’ increase in weeks worked because unmarried individuals start out with a very low number of weeks worked at one job in the first time period, relative to their always-marital counterparts. Moving from a rural setting to the suburb is the only change in residence variable that is significant. It appears that these movers stay at their jobs for less time in the suburbs than those who stay in a rural setting.

Finally, the offending variables have some interesting results. Desistors actually do worse than those who never offended, whereas initiators and chronic offenders do somewhat better. Only the difference between abstainers and chronic offenders is statistically significant. Part of this effect is caused by the fact that, much like in the marriage equation, abstainers are working for longer periods than the chronic offenders in 1983. The chronic offenders and the abstainers are not comparable in 1983, yet the chronic offenders are doing almost as well as the abstainers by 1986. In fact, the chronic offenders are doing better than any other offending group, even better than the desistors. The desistors start out like the chronic offenders but hardly improve at all; they improve less than the initiators. This result is counterintuitive and has no clear explanation. Despite this odd result, arrest still has the most significant impact on job length.

The job stability equation once again has a poorer fit. The marriage variables follow the same pattern as in the job length equation. The arrest and offending variables are all in the predicted direction but are not significant.

Whereas any time-stable characteristics that affect the levels in both years were controlled for in the above analysis, time-stable characteristics that affect trajectories of change were not. And although the number of weeks worked and percentage of individuals in stable jobs were similar for the two groups in 1983, they do differ on some measures of past behavior, such as criminal activity in 1980, marriage, and place of residence. Hagan’s theory of criminal embeddedness explicitly predicts that past criminal behavior will
TABLE 2: Effect of Arrest and Initial Values of Relevant Covariates on Job Length and Job Stability: “Differences in Differences” (standard errors in parentheses)

<table>
<thead>
<tr>
<th>Group Variable (N of Group)</th>
<th>Job Length Model 1</th>
<th>Job Length Model 2</th>
<th>Job Stability Model 1</th>
<th>Job Stability Model 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept 1</td>
<td>9.52 (1.59)</td>
<td>7.35 (4.58)</td>
<td>−1.29 (0.13)</td>
<td>−1.93 (0.33)</td>
</tr>
<tr>
<td>Intercept 2</td>
<td>−7.00** (4.23)</td>
<td>−10.78** (3.43)</td>
<td>−0.11 (0.25)</td>
<td>−0.59* (0.27)</td>
</tr>
<tr>
<td>Arrested (25)</td>
<td>3.30 (4.55)</td>
<td>9.21** (2.26)</td>
<td>0.78** (0.50)</td>
<td>0.55** (0.28)</td>
</tr>
<tr>
<td>Stay unmarried (97)</td>
<td>−10.31 (10.46)</td>
<td>−12.36** (5.79)</td>
<td>1.39** (0.64)</td>
<td>0.10 (0.36)</td>
</tr>
<tr>
<td>Get separated (4)</td>
<td>10.33 (9.46)</td>
<td>−0.36 (6.25)</td>
<td>0.91 (0.29)</td>
<td>0.17 (0.39)</td>
</tr>
<tr>
<td>Move from country to suburb (19)</td>
<td>−0.56 (7.38)</td>
<td>−0.56 (7.38)</td>
<td>−0.52 (0.53)</td>
<td>0.059 (0.32)</td>
</tr>
<tr>
<td>Stay in city (33)</td>
<td>−4.69 (5.09)</td>
<td>−4.69 (5.09)</td>
<td>−0.77 (0.32)</td>
<td>0.059 (0.32)</td>
</tr>
<tr>
<td>Move from suburb to country (9)</td>
<td>−6.36 (4.39)</td>
<td>−6.36 (4.39)</td>
<td>−0.14 (0.29)</td>
<td>−3.37 (0.28)</td>
</tr>
<tr>
<td>Criminal desistors (24)</td>
<td>1.23 (3.87)</td>
<td>1.23 (3.87)</td>
<td>6.39** (2.03)</td>
<td>−0.21 (0.24)</td>
</tr>
<tr>
<td>Chronic offenders (39)</td>
<td>6.39** (3.87)</td>
<td>6.39** (3.87)</td>
<td>0.19 (0.19)</td>
<td>21.49* (0.19)</td>
</tr>
</tbody>
</table>

Fit measure (F-test) $F = 2.74^*$ $F = 2.058$ LLR = 0.19 LLR = 21.49*

NOTE: LLR = log likelihood ratio test.
* Significant at the 10 percent level (one-tailed test). ** Significant at the 5 percent level (one-tailed test).

affect employment outcomes. This hypothesis is tested in model 3, in which all the initial levels, personal characteristics, and behavior are added to the model. If Hagan is correct, the magnitude of the coefficient on arrest will lessen or disappear.
TABLE 3: Effect of Arrest and Initial Values of Relevant Covariates on Job Length and Job Stability: "Differences in Differences" (standard errors in parentheses)

<table>
<thead>
<tr>
<th>Measured Effect</th>
<th>Job Length Model</th>
<th>Job Stability Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept 1</td>
<td>-21.24</td>
<td>-1.09</td>
</tr>
<tr>
<td></td>
<td>(22.24)</td>
<td>(1.35)</td>
</tr>
<tr>
<td>Intercept 2</td>
<td>-</td>
<td>0.18</td>
</tr>
<tr>
<td></td>
<td>(1.35)</td>
<td>(0.26)</td>
</tr>
<tr>
<td>Arrest in 1984-1986</td>
<td>-8.32**</td>
<td>-0.16</td>
</tr>
<tr>
<td></td>
<td>(4.26)</td>
<td>(0.26)</td>
</tr>
<tr>
<td>Married in 1983</td>
<td>-7.11**</td>
<td>-0.57**</td>
</tr>
<tr>
<td></td>
<td>(3.94)</td>
<td>(0.25)</td>
</tr>
<tr>
<td>Living in urban area 1983</td>
<td>-2.01</td>
<td>-0.35</td>
</tr>
<tr>
<td></td>
<td>(3.75)</td>
<td>(0.23)</td>
</tr>
<tr>
<td>Living in rural area 1983</td>
<td>-0.048</td>
<td>-0.13</td>
</tr>
<tr>
<td></td>
<td>(3.75)</td>
<td>(0.21)</td>
</tr>
<tr>
<td>Age in 1983</td>
<td>0.14</td>
<td>0.012</td>
</tr>
<tr>
<td></td>
<td>(0.80)</td>
<td>(0.048)</td>
</tr>
<tr>
<td>Highest grade completed</td>
<td>1.61**</td>
<td>-0.01</td>
</tr>
<tr>
<td></td>
<td>(0.91)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>GPA in 1976</td>
<td>2.40</td>
<td>0.018</td>
</tr>
<tr>
<td></td>
<td>(1.89)</td>
<td>(0.12)</td>
</tr>
<tr>
<td>Criminally active in 1983</td>
<td>5.07*</td>
<td>-0.16</td>
</tr>
<tr>
<td></td>
<td>(3.11)</td>
<td>(0.26)</td>
</tr>
<tr>
<td>Fit measure (F-test)</td>
<td>$F = 2.03^{**}$</td>
<td>LLR = 7.99</td>
</tr>
</tbody>
</table>

NOTE: GPA = grade point average, LLR = log likelihood ratio test. * Significant at the 10 percent level (one-tailed test). ** Significant at the 5 percent level (one-tailed test).

The results for the job length measure are given in the second column of Table 2. Once again, the effect of arrest on job length is actually slightly larger than the effect without the controls for individual characteristics. This result reaffirms the claim that arrest does have an independent causal impact on job length.

The criminality variable itself is unexpectedly positive. If the one-tailed test is done in this direction, the result is significant at the 10 percent level. Although unexpected, the result is consistent with the above findings that chronic offenders start out with very little time spent on the job in a year, and they show significant gains in 1986 relative to abstainers. Alternate specifications were tried with number of crimes in 1980, participation in 1979, and participation in serious crimes only in 1980. Arrest continued to have the same impact in each case, although past criminal activity was no longer significant in any of these alternative specifications. The initial result seems to be driven primarily by relatively chronic minor offending.
Marital status in 1983 is the only other initial observed time-varying variable that is significant in the job length equation. Having been married already in 1983 results in a smaller increase in weeks worked at one job from 1983 to 1986. This result captures the fact that the 35 men who were married in 1983 worked on average about nine weeks more than their unmarried counterparts in 1983, a difference that is statistically significant at the 1 percent level.

The results for the fraction of individuals in stable jobs are given in the fourth column of Table 2. The effect is similar to the effect without controls, but still insignificant. Marriage is the only significant coefficient, again because men who were married in 1983 were much more likely to already have settled into stable jobs in 1983 than their unmarried counterparts. It seems safe to conclude that there is more evidence for the story that arrest matters more than criminal offending. Although only one of the two variables has a significant arrest coefficient, neither model supports the hypothesis that criminal offending matters. The results are most supportive of the Sampson and Laub story of active stigmatization by employers, which leads to greater instability for arrested individuals.

**CONCLUSION**

There is much anecdotal evidence that employers do (and have for a long time) screened individuals on the basis of their criminal history record. There is also plenty of evidence that contact with the criminal justice system is correlated with problems in the labor market. The question addressed in this article is if the stigma of an arrest causes actual harm to the applicants, who might be so “embedded” in criminal activity as to be unprepared for or ultimately uninterested in meaningful employment.

The question is addressed by looking at the impact of relatively minor first arrests on job stability using a representative U.S. sample to control for both observed criminal activity and unobserved time-stable factors using a form of fixed-effect estimation known as “differences in differences.” Two measures of employment were used: a continuous measure of job length and a discrete measure of job stability.

The models using the job length measure are supportive of the claim that formal contact with the criminal justice system, rather than criminal activity itself, directly damages job prospects. The coefficient on arrest was significant, even when controls for offending, married state, age, and place of residence were added. In fact, the size of the effect became larger when controls were added. In the second model, arrest decreased the longest time spent at a job by 10.78 weeks, which is large compared with the 1986 average
for the sample of 41.89 weeks worked at a major job. This 25.73 percent difference is comparable with the 32.8 percent decrease in months worked after a conviction for 19-year-olds in the Nagin and Waldfogel (1995) study.  

In contrast, the models for the discrete measure of job stability were uniformly poor. Neither offending nor arrest had an impact on job stability. This result weakens somewhat the conclusions one can draw from the study as a whole. It also highlights the weakness of the fixed-effect approach when applied to discrete outcomes. Random-effect models, despite their restrictive assumptions, might be the only realistic alternative for useful models with discrete dependent variables.

Neither random-effect nor fixed-effect models, however, will ever completely eliminate the possibility that the results are merely the result of omitted variable bias. Time-varying factors that affect both employment outcomes and offending behavior could be responsible for the observed impact of arrest on job length. On one hand, the nonarrested group looked similar to the arrested individuals before the arrest. Perhaps there are not that many time-varying factors, besides the arrest, that differ dramatically between the two groups. On the other hand, because the groups were not randomly assigned to arrest, it will never be possible to know for sure that other factors do not vary between the two groups in a meaningful way. The only way to solve this problem, other than to randomly arrest people, would be to include additional appropriate measures in the model.

For example, in the present case, some of the arrests are alcohol related. As a result, it is possible that alcohol abuse has caused both the arrests and the employment difficulties. To the extent to which the effect of alcohol abuse is stable over time, the differencing will control for this possibility. Yet, if alcohol abuse changes over time, it is possible that the arrest will capture the effect of the alcohol abuse.

This example underscores the main limitation of a study of this type. Absent information directly from employers, it will never be possible to know that the arrest, and not other factors, actually leads to the employment problems faced by an individual. That conclusion must be inferred from the available data and the model that attempts to control for other possibilities. To that point, this study, even more so than the Nagin and Waldfogel (1995) study, has the advantage of good controls for individual heterogeneity and a good comparison group. The cost of this approach in this case is small sample size and a sample that includes only relatively minor criminal behavior.

It is important to place the results within the context of other research. At this point, this study, along with studies by Sampson and Laub (1993) and Nagin and Waldfogel (1995), shows that arrest leads to job instability even when strong controls for offending and unobserved heterogeneity are in-
cluded in the model. Several other studies, without strong controls for individual differences or criminal offending, also show an impact of contact with the criminal justice system on job stability (Freeman 1991a, 1991b; Waldfogel 1994). There is only one study (Hagan 1993) that found that offending rather than contact with the criminal justice system leads to employment problems, and that study does not have controls for unobserved heterogeneity. The growing evidence is beginning to support the conclusion that contact with the criminal justice system leads directly to problems in the labor market.

The next obvious question is whether this increased instability leads to increased crime. This possibility clearly needs more research. Although evidence suggests that job instability leads to additional criminal activity, no study currently ties increased criminal activity directly to the job instability created by an arrest. At least in the short term, evidence from the NYS suggests that criminal activity does not significantly increase after an arrest. Individuals who were criminally active in 1983 and arrested by 1986 show a decrease in the mean (and median) number of self-reported criminal acts from 1983 to 1986. In contrast, those individuals who were criminally active in both periods without an arrest show an increase in the mean (and median) number of self-reported criminal acts from 1983 to 1986. This study's reliance on the last two waves of publicly available data from the NYS prevents any more detailed follow-up. Wave 8 data might provide a unique opportunity to study an individual's response to job instability brought on by an arrest.

NOTES

1. The Glueck data are a study, titled Unraveling Juvenile Delinquency, begun in 1940 by Sheldon and Eleanor Glueck. The study involved 500 officially delinquent boys and 500 nondelinquent boys from Boston. The boys were followed for 18 years with a 92 percent follow-up rate. Sampson and Laub resurrected the Glueck data in 1987.

2. This latter category is needed to catch a small number of people who apparently work more than one job at a time.

3. The measures have a Pearson correlation coefficient of .767 in 1983 and .839 in 1986; the change between 1983 and 1986 is correlated at .736.

4. Age is a continuous measure, but the change will be the same for all individuals (3 years). The effect will be captured by the constant.

5. Calling criminal activity in 1983 a time-stable characteristic might seem confusing, yet an individual's criminal involvement in 1983 is the same in 1983 as it is in 1986. This is different from an individual's current criminal activity, which clearly will vary from 1983 to 1986.

6. Criminal activity in 1979 also was used. The results are qualitatively similar to the results reported using the 1980 measure and are available upon request.

7. This result is less pronounced when only serious offenses are used.
8. The fact that the effect is smaller for an arrest than for a conviction is also supportive of previous findings, although one must be careful when making comparisons across such different studies.

REFERENCES


