THE EFFECT OF ARRESTS ON THE EMPLOYMENT AND EARNINGS OF YOUNG MEN*

JEFFREY GROGGER

Many young men commit crime, and many are arrested. I estimate the effect of arrests on the employment and earnings of arrestees, using a large longitudinal data set constructed by merging police records with UI earnings data. I find that the effects of arrests are moderate in magnitude and rather short-lived.

I. INTRODUCTION

A large proportion of young men commit crime, and many of them are arrested. Wolfgang, Figlio, and Sellin [1972] estimated that almost 35 percent of all young men in Philadelphia were arrested by age eighteen. Tillman [1987] reported that one-third of all men in California were arrested, for a crime punishable by a jail term, at least once between the ages of 18 and 30.

Tabulations from the National Longitudinal Survey of Youth (NLSY) show that arrests and subsequent labor market outcomes are strongly negatively correlated. Among men who had been arrested prior to 1980, annual earnings averaged $7047 (in $1980) between 1980 and 1984. For men without prior arrest records, annual earnings were 15 percent higher, averaging $8083.[1]

At first glance, therefore, it would appear that young criminals pay heavily for their crimes in terms of their future market earnings. Indeed, one might question whether the existence of widespread youth crime in the face of such substantial market penalties was consistent with forward-looking behavior on the part of optimizing agents.

An important question is whether this strong negative correlation reflects causation; that is, whether being arrested actually causes the arrestee's earnings to fall. An alternative hypothesis is that the negative association arises simply due to unobserved characteristics of workers that are correlated both with crime and labor market outcomes. The goal of this paper is to answer this

    1. Details on the sample are provided below.

question by estimating the effects of arrest and prosecution on the employment and earnings of young male arrestees.

Conceptually, I am attempting to estimate what would have happened to an arrestee if he had not been arrested. This counterfactual state obviously cannot be observed directly, although if arrests occurred at random, then arrestees and nonarrestees would be the same on average ex ante, and estimation would be simple. In fact, arrests are not assigned at random, but rather arise through a complex process involving many unknown and unobservable variables. My approach to estimation is first to choose a suitable comparison group, then to control statistically for any time-invariant, individual-specific unobservable characteristics.

To do this, I have constructed a dataset by merging longitudinal arrest records from the California Department of Justice with longitudinal earnings records for the period 1980–1986 from the California Employment Development Department (EDD).[2] Although all sample members are arrested at least once, their first arrests occur at different times. I limit my analysis to employment and earnings data from 1980 to 1984. This lets me construct a comparison sample from individuals first arrested in 1985 or later, which I compare with a treatment sample of individuals first arrested in 1984 or earlier.

Other researchers have analyzed questions related to mine. Lott [1990] and Waldfogel [1994] attempted to estimate the effect of convictions, primarily for "white-collar" offenses, on the annual earnings of prime-age males. Freeman [1992] has reported estimates of the effects of arrest and particularly jail sentences on young men's future employment. I have previously examined the relationship between youth employment and arrests as well [Grogger 1992]. This paper substantially extends my earlier work by reporting estimated earnings effects as well as employment effects, by considering the effects of convictions and jail sentences as well as arrests, and by allowing different types of arrests to have different effects.

II. THE DATA

A. The California Adult Criminal Justice Statistical System

The criminal justice data used in this study come from a random sample of arrestee records in the California Justice

Department's Adult Criminal Justice Statistical System (ACJSS). The ACJSS is the central repository of individual arrest records in California, and contains information on every arrest for a jailable offense committed by individuals over seventeen years of age in the state.[3] This includes all felonies, many of which are serious crimes, as well as some misdemeanors, which are usually less serious. The file links arrests to arrestees by fingerprints, providing longitudinal criminal justice records. All records in the ACJSS include the date of the arrest and the charges on which it was made.

About 70 percent of the ACJSS records also include information on subsequent prosecution, including the disposition of the arrest and any resulting sentence.[4] Prosecution information includes the date of disposition, whether a conviction was obtained, and if so, the type of sentence imposed. There is no information, however, on the length of any jail sentence imposed, nor on time actually served.

For this study, a random sample of male arrestees born in 1956, 1958, 1960, and 1962 was drawn from the ACJSS. Arrest records begin to accumulate at age eighteen, and extend through February 1987. For those born in 1956, therefore, arrest records cover fourteen complete years (1973–1986). For the 1962 cohort, by comparison, I have seven years of data (1980–1986).

B. Unemployment Insurance Earnings Data

Each quarter all employers covered by unemployment insurance laws must report the quarterly earnings of each of their employees to their state's unemployment insurance agency.[5] These data are used to determine benefits when a UI applicant files a

3. This definition excluded most drunk driving charges for most of the sample period. It excludes all other traffic infractions, as well as trivial charges such as vagrancy or loitering.
4. An arrest may be disposed of in one of several ways. First, the case may be dropped by police before formal charges are filed. Next, a prosecutor or judge may dismiss charges on such grounds as lack of evidence. If charges are not dropped or dismissed, the suspect may plead guilty, or plead innocent and stand trial to be found guilty or acquitted by a jury. A conviction may result from either a guilty plea or guilty verdict, and is followed by sentencing. For the most part, criminal sentences are imposed in the form of fines, jail or prison sentences, probation (a period of nonincarcerative court supervision), or some mix of the three.
5. The Bureau of Labor Statistics [1989] has estimated that in 1987 the UI system, together with the Unemployment Compensation for Federal Employees program, covered 99 percent of all wage and salary jobs, and 90 percent of all jobs, in the United States. Excluded from UI coverage are self-employed persons, military personnel, federal government employees, some state and local government employees, and domestic workers earning less than $1000 per quarter. While most states also exclude agricultural workers, almost all such workers are covered under California UI laws [U. S. Department of Labor 1988].

claim. In general, EDD retains individual earnings data for only the most recent five calendar quarters. At the time the data for this study were collected, twenty-eight quarters of data fortuitously were available pending the resolution of a court case against EDD. The sample constructed for this study contains quarterly earnings data from the first quarter of 1980 to the fourth quarter of 1986.

C. Merging the ACJSS and UI Data

The ACJSS sample was merged with the UI records by Social Security numbers (SSN's) and last name. SSN's were available for about 80 percent of the sample. Those for whom no SSN could be found were disproportionately Hispanic. About 80 percent of those arrestees for whom SSN's existed were matched to earnings records, yielding an overall match rate of just over 60 percent.

D. The Analysis Sample

All individuals in the merged sample were arrested at least once between 1973 and early 1987. It proved impractical to draw a nonarrested control sample from EDD's files, because EDD's records do not contain even rudimentary background information such as age or sex. Therefore, a control group would have included prime-age males, men nearing retirement, and women, all of whose labor market behavior differs markedly from that of young men. The cost of drawing a random control sample of UI records therefore was not justified by what little stood to be learned from such an undifferentiated set of data.

Nonetheless, the sample design is quite suitable for my purpose, which is to estimate the effect of arrests on the earnings and employment of arrestees. Although all sample members eventually are arrested, these arrests occur at different times. In particular, first arrests occur at different times.

For estimation I use only earnings and employment data from 1980–1984. My treatment sample consists of individuals who were first arrested in 1984 or earlier. As a comparison sample I take individuals whose first arrest occurred after 1984. These individuals' pre-1984 earnings are unaffected by their arrests, since an arrest logically can affect only postarrest employment and earnings. Each individual contributes one observation to the sample for each quarter from 1980 to 1984.

E. Variables Used in Estimation

Table I reports summary statistics of the sample so constructed. Earnings per quarter are in constant 1980 dollars, deflated by the Consumer Price Index for California. The employment variable is binary, equal to one if the individual had positive earnings in the quarter and zero otherwise.

The first two rows of Table I show mean earnings and employment. Mean earnings are low, only $1182 per quarter. This may be due to substantial joblessness in the sample, since mean quarterly employment is only 0.54.

Arrests and prosecutions are coded as dummy variables. The main arrest indicator is equal to one in quarters when the sample member was arrested and zero otherwise. Two variables interact

TABLE I

DATA MEANS

Time-varying variables	
Quarterly earnings ($1980)	1182
	(1706)
Employed	0.541
Arrested	0.071
Arrested more than once	0.009
Property arrest	0.022
Convicted	0.030
Unknown disposition	0.026
Disposition in progress	0.028
Probation	0.006
Jail/probation	0.013
Jail	0.003
Prison	0.002
Sentence missing	0.005
Age (in quarters)	93.07
Observations	343,714
Time-invariant characteristics	
Black	0.153
Hispanic	0.199
Born in 1958	0.211
Born in 1960	0.223
Born in 1962	0.364
Number of individuals	24,551
Number in comparison sample	2901

Note. Standard deviations in parentheses.

with the main arrest indicator: one indicates that the individual was arrested more than once in a given quarter, and one indicates that he was arrested for a property offense (robbery, burglary, larceny/theft, or auto theft). I hypothesize that employers may find more active criminals, or those inclined to steal, particularly poor prospects for employment. If so, then the effects of multiple or property arrests may exceed the effects of other arrests.[6]

The third through fifth lines of Table I summarize these arrest indicators. Approximately 7 percent of the sample was arrested in any given quarter and about 13 percent ($= 0.009/0.071$) of these were arrested at least once more during the same quarter. Roughly 30 percent of all arrests were for property crimes.

The outcome of any subsequent prosecution may affect the extent to which arrests affect employment or earnings. My prosecution variables are binary indicators of disposition and sentencing outcomes. The conviction indicator equals one in any quarter in which the individual was convicted of a crime. The unknown disposition indicator equals one in quarters when the sample member was arrested but no disposition information is available. The conviction indicator would interact with the main arrest variable if all dispositions occurred in the same quarter as the arrest. However, since some dispositions take longer, I construct an indicator of dispositions in progress, equal to one in quarters between arrest and disposition. This indicator also picks up decreases in employment or earnings caused by pretrial detention, which is unobservable. Since all arrests result in either a conviction, an unknown disposition, or an acquittal, the omitted disposition category is acquittal.

I also include dummy variables for four types of sentences. Probation sentences stipulate a time of nonincarcerative court supervision. I refer to another particularly common type of sanction as a jail/probation sentence; these sentences stipulate a relatively short period of time in jail to be followed by a longer period on probation. Regular jail sentences and prison sentences are also coded in the data. A regular jail sentence imposes a period of no more than twelve months incarceration, and is served at a county jail. Prison sentences are typically longer than one year, and are served at a state prison. Since the maximum length of jail sentences is known, I can determine whether jail sentences have

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6. In preliminary analyses I allowed drug arrests to have an additional effect as well. The estimates were insignificant, though, so I do not report them here.

behavioral effects on employment and earnings, or whether negative jail effects simply are attributable to the enforced withdrawal of the arrestee from the labor market. The maximum prison sentence, on the other hand, is unavailable, and for many offenses may exceed the length of the sample period. The estimated effect of prison sentences on employment and earnings therefore may have little behavioral content. Finally, many convictions lack sentence data. I construct an indicator equal to one if the sentence is missing as a control variable. The omitted sentence category refers to a nonincarcerative, nonprobation sentence, such as a fine or community service.

I also include several demographic variables. The bottom panel of Table I indicates that about 15 percent of the sample was black and 20 percent Hispanic, where white (the omitted group), black, and Hispanic are mutually exclusive categories. As is true in most arrest data, blacks are overrepresented in this sample relative to the general population. The proportions from each birth cohort are also shown in Table I. For reasons unrelated to this study, the 1962 birth cohort was sampled at twice the rate of other cohorts. The average age of the sample was 93 quarters, or just over 23 years.

Before proceeding with the estimation, it is instructive to compare the employment and earnings of the analysis sample with a more general sample of the population. The NLSY is well-suited for this purpose, since it began with a random sample of youths aged 14 to 21 in 1979. Three of the cohorts from my sample thus are included in the NLSY, and the 1956 cohort is only two years older than the oldest NLSY respondents. To improve the age match even further, I restrict attention to NLSY respondents who were seventeen or over in 1979. In 1980 the NLSY included a crime module with questions about past offenses, arrests, and sentences. I code as an arrestee anyone who reported a past arrest.

Table II compares annual earnings and employment data from my analysis sample with the NLSY sample for 1980–1984. In my analysis sample, annual earnings are simply the sum of quarterly earnings. The annualized employment indicator equals one if the sample member had any earnings over the year. In the NLSY data, reported annual earnings are deflated by the CPI. The employment variable is binary, equal to one if the respondent's annual earnings were positive. The NLSY data were weighted to obtain estimates representative of the entire population of the relevant age group.

Employment rates vary across the two samples. They are roughly constant over time in the NLSY, with arrestees' employ-

TABLE II
EMPLOYMENT AND EARNINGS OF MEN IN ANALYSIS AND NLSY SAMPLES

| | Analysis sample | | NLSY | |
	Comparison	Treatment	Nonarrestees	Arrestees
A. Employment				
1980	0.57	0.75	0.89	0.87
1981	0.62	0.76	0.88	0.86
1982	0.63	0.71	0.89	0.87
1983	0.69	0.71	0.86	0.85
1984	0.76	0.73	0.88	0.87
	Analysis sample		NLSY	
	Comparison	Treatment	Nonarrestees	Arrestees
B. Earnings ($1980)				
1980	3000	3867	5349	5287
1981	3723	4360	6484	5870
1982	3987	4236	8248	7272
1983	4735	4651	9403	7773
1984	5865	5360	10,931	9031

Note. NLSY sample limited to respondents eighteen or older in 1980. NLSY arrestees are respondents who indicated that they had previously been charged with a crime at the 1980 interview. Estimates from NLSY are weighted.

ment rates about one or two percentage points below the nonarrestees. In my analysis sample, the treatment group's employment rate was also roughly constant over time, though at a level roughly ten to fifteen percentage points below the NLSY arrestees. Part of this difference may be accounted for by under-the-table or other uncovered earnings, which are excluded from EDD records but presumably included in the NLSY respondents' self-reports.

My comparison sample generally has lower employment than any other group. This may be due to age differences. By definition, comparison sample members were first arrested in 1985 or later. Since crime is largely the pursuit of youth, members of the later birth cohorts were more likely to be included in this group than were those from the earlier cohorts. Though the 1962 birth cohort made up 35 percent of the treatment sample, it made up 49 percent of the comparison sample.

Within- and between-sample comparisons of earnings are similar: the NLSY shows higher earnings than my sample, and my comparison sample generally has lower earnings than my treat-

ment sample. In general, it appears that my arrestee sample was drawn from the lower tail of the youth employment and earnings distributions.

III. THE ECONOMETRIC MODEL

Having described my sample, I turn now to specifying an empirical model. At a minimum the model should satisfy two requirements. First, it should allow arrests and prosecution to affect both current and future labor market outcomes. Second, it should exploit the longitudinal structure of the data to provide controls for unobserved variables correlated with arrests that may bias the estimates.

A distributed lag model meets these requirements. Let X_{it} denote the vector consisting of individual i's main arrest indicator and the various arrest and prosecution interactions as of quarter t. Let y_{it} denote the outcome measure, either employment or earnings, of individual i at time t. Let Z_{it} denote a vector of control variables including the disposition-in-progress indicator, race and birth cohort dummies, a quadratic in age, and quarter dummies. The model is then

$$(1) \qquad y_{it} = \sum_{j=0}^{m} X_{it-j}\beta_j + Z_{it}\delta + \mu_i + \epsilon_{it},$$

where $\beta = [\beta_0', \ldots, \beta_m']'$, and δ are parameters to be estimated. In particular, β_j gives the effect of arrest and prosecution j periods ago on current employment or earnings, which is assumed to be the same for all individuals.

Both μ_i and ϵ_{it} are unobservable. The term μ_i is specific to each individual and invariant over time. As in more familiar settings, it captures differences across individuals in such fixed characteristics as tastes, schooling, and innate ability.[7] Here, μ_i also captures differences across individuals in the average amount of time spent

7. Given the age range of the sample, schooling actually may vary over the sample period. Unfortunately, I have no measures of educational attainment or enrollment in this data set, though the NLSY sample described above may provide some useful information. Between 1980 and 1985 average educational attainment among arrestees in that sample increased only 0.4 years, compared with 0.9 years for nonarrestees. Enrollment rates among arrestees therefore were rather low. My sample is slightly older than the NLSY sample, and so may have had even lower enrollment rates. As a very rough control for college attendance, I constructed a dummy variable equal to one in quarters when sample members were less than 23 years old. Its coefficient in regressions similar to those reported below was significant though small, and the other estimates were not affected by its inclusion.

on crime in each quarter, which presumably reflect differences in criminal returns. The term ϵ_{it} is a random disturbance, assumed uncorrelated with μ_i. Experimentation revealed that a lag length of $m = 6$ included all significant effects of arrest and prosecution. Columns (1) and (3) of Table III contain ordinary least squares estimates of the model in equation (1) for earnings and employment, respectively.[8]

While OLS does not account for the special error structure of this model, nonetheless it will provide consistent estimates if μ_i and ϵ_{it} are uncorrelated with all included lags of X_{it}. The OLS estimates give the appearance of substantial and persistent effects of arrests and of jail sentences. The main arrest effects appear fairly large, and the incremental effects of the multiple and property arrest interactions are even larger.

Of course, OLS gives consistent estimates only if μ_i is uncorrelated with the arrest vector X_{it}. On the other hand, if mean levels of (unobserved) criminal activity vary across individuals, and the probability of arrest increases with the amount of time devoted to crime, then μ_i and X_{it} are correlated, and OLS estimates are inconsistent. To solve this problem, I apply the fixed-effects estimator to equation (1). This amounts to treating the μ_i terms as fixed, then taking deviations from individual means to eliminate them, and applying OLS to the transformed model. Columns (2) and (4) of Table III report fixed-effects (FE) estimates of the earnings and employment models.

IV. RESULTS

The substantial differences between the OLS and FE estimates suggest that the individual effects μ_i indeed are correlated with the arrest indicators, and hence that the OLS estimates are inconsistent. For this reason, I focus on the FE estimates.

Beginning with the earnings equation reported in column (2) of Table III, the main arrest effects are mostly statistically significant but relatively modest in magnitude. An arrest leads to a decrease in earnings in the current period of $42, or about 4 percent of mean earnings. Moreover, because the contemporaneous arrest indicator may reflect in part the earnings loss caused by unobservable pretrial confinement, it actually may exaggerate any behavioral effect of an arrest on earnings. No such exaggeration is

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8. The unknown disposition, missing sentence, and disposition-in-progress variables were included in each specification, but I do not report their coefficients in order to save space.

present in the lagged values of the arrest indicator, however, because the disposition-in-progress variable is included in the regression. From periods 2 to 6 following the arrest, the effect of the arrest falls slightly, averaging about 2 to 3 percent of the sample mean. After six quarters the effect is insignificantly different from zero.

Individuals arrested more than once in a given quarter experience an additional drop in earnings of about $70.[9] In one quarter this effect falls to $33, after which it is insignificant. Property arrestees incur a slightly larger and longer-lived incremental earnings penalty. One explanation of these results is that employers may consider multiple and property arrestees to be particularly bad prospects for employment. In particular, employers might view property arrests as indicating the arrestee's intentions for the employer's own establishment.

The next set of variables measures the effect of convictions on the arrestee's earnings. These coefficients are generally positive but not significantly different from zero. At first glance it may seem surprising that arrests rather than convictions lead to lower earnings, since after all, the arrestee is at least legally considered innocent until he is proved guilty. Fortunately, Sullivan's [1989] ethnographic study of inner-city youths provides an explanation for this apparently puzzling finding.

Sullivan followed about twenty young men from three different New York City neighborhoods for about two years. During that time, most of them committed crimes, and many were arrested. His accounts illustrate how arrestees may find it difficult to conceal their arrests from their employers, or even be terminated without directly admitting to being arrested. One problem for the arrestee is that an arrest may entail a period of incarceration before he is able to post bail. It may also result in several legal hearings that require the arrestee's attendance, even if charges are ultimately dropped. Thus, the arrestee may be faced with the choice of informing his employer of the arrest in order to get the needed time off, or of concealing the arrest and missing work without a coherent explanation.

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9. An individual with two arrests in one quarter therefore experiences an earnings reduction of $112. Note that if the two arrests occurred in different quarters, then the arrestee would have earnings reductions of $42 in each quarter. The incremental penalty occurs only if the second arrest takes place in the same quarter as the first. I tested for interactions between a single arrest in a given quarter and the cumulative number of arrests, but none of the interactions was significant.

TABLE III
REGRESSION RESULTS

Variable	Earnings (1) OLS	Equation (2) FE	Employment (3) OLS	Equation (4) FE
Arrested				
Lag: 0	−155	−42	−0.009	0.014
	(16)	(11)	(0.005)	(0.004)
1	−120	−33	−0.008	0.011
	(17)	(14)	(0.005)	(0.005)
2	−103	−34	−0.016	−0.001
	(17)	(14)	(0.005)	(0.005)
3	−96	−29	−0.021	−0.004
	(17)	(14)	(0.005)	(0.005)
4	−88	−29	−0.015	0.001
	(17)	(14)	(0.005)	(0.005)
5	−63	−14	−0.017	−0.002
	(17)	(13)	(0.005)	(0.005)
6	−64	−23	−0.012	0.000
	(17)	(12)	(0.005)	(0.005)
Arrested more than once in quarter				
Lag: 0	−179	−70	−0.102	−0.055
	(33)	(24)	(0.010)	(0.008)
1	−109	−38	−0.102	−0.062
	(34)	(25)	(0.010)	(0.009)
2	−93	−36	−0.076	−0.042
	(33)	(25)	(0.010)	(0.009)
3	−66	−11	−0.066	−0.031
	(33)	(25)	(0.010)	(0.009)
4	−56	5	−0.054	−0.017
	(33)	(25)	(0.010)	(0.009)
5	−47	19	−0.051	−0.016
	(33)	(25)	(0.010)	(0.009)
6	−51	10	−0.030	0.005
	(34)	(24)	(0.010)	(0.008)
Property arrest				
Lag: 0	−213	−86	−0.051	−0.023
	(23)	(17)	(0.007)	(0.006)
1	−175	−70	−0.068	−0.046
	(23)	(17)	(0.007)	(0.006)
2	−148	−39	−0.050	−0.023
	(23)	(17)	(0.007)	(0.006)
3	−139	−30	−0.050	−0.025
	(23)	(17)	(0.007)	(0.006)
4	−123	−11	−0.044	−0.017
	(23)	(17)	(0.007)	(0.006)

TABLE III
(CONTINUED)

Variable	Earnings (1) OLS	Equation (2) FE	Employment (3) OLS	Equation (4) FE
5	−134	−20	−0.036	−0.008
	(23)	(17)	(0.007)	(0.006)
6	−116	−3	−0.040	−0.011
	(23)	(16)	(0.007)	(0.006)
Convicted				
Lag: 0	87	51	0.026	0.006
	(59)	(41)	(0.017)	(0.015)
1	48	38	0.034	0.021
	(59)	(44)	(0.017)	(0.015)
2	10	14	0.004	−0.002
	(58)	(45)	(0.017)	(0.016)
3	−13	−13	0.008	0.001
	(59)	(46)	(0.018)	(0.016)
4	23	40	−0.001	0.005
	(61)	(47)	(0.018)	(0.017)
5	−20	11	0.004	0.001
	(63)	(48)	(0.019)	(0.017)
6	−27	37	−0.006	−0.000
	(66)	(48)	(0.020)	(0.017)
Probation				
Lag: 0	30	−60	0.038	0.014
	(66)	(47)	(0.020)	(0.017)
1	65	−40	0.033	0.005
	(67)	(50)	(0.020)	(0.017)
2	100	−34	0.050	0.010
	(66)	(51)	(0.020)	(0.018)
3	122	−1	0.047	0.010
	(68)	(53)	(0.020)	(0.018)
4	87	−59	0.042	0.003
	(71)	(55)	(0.021)	(0.019)
5	97	−45	0.031	−0.004
	(73)	(56)	(0.022)	(0.019)
6	81	−63	0.038	0.000
	(77)	(56)	(0.023)	(0.019)
Jail/probation				
Lag: 0	−322	−221	−0.075	−0.050
	(61)	(43)	(0.018)	(0.015)
1	−271	−184	−0.096	−0.076
	(61)	(46)	(0.018)	(0.016)
2	−206	−131	−0.025	−0.013
	(61)	(47)	(0.018)	(0.016)

TABLE III
(CONTINUED)

Variable	Earnings (1) OLS	Equation (2) FE	Employment (3) OLS	Equation (4) FE
3	−167	−93	−0.033	−0.022
	(63)	(49)	(0.019)	(0.017)
4	−215	−147	−0.026	−0.014
	(65)	(50)	(0.019)	(0.017)
5	−177	−114	−0.031	−0.014
	(68)	(51)	(0.020)	(0.017)
6	−200	−136	−0.030	−0.013
	(71)	(52)	(0.021)	(0.018)
Jail				
Lag: 0	−313	−190	−0.096	−0.048
	(70)	(49)	(0.021)	(0.018)
1	−301	−179	−0.130	−0.082
	(70)	(53)	(0.021)	(0.019)
2	−276	−146	−0.107	−0.062
	(71)	(55)	(0.021)	(0.020)
3	−256	−121	−0.080	−0.034
	(73)	(57)	(0.022)	(0.020)
4	−293	−157	−0.073	−0.028
	(76)	(59)	(0.023)	(0.020)
5	−291	−167	−0.084	−0.043
	(80)	(61)	(0.024)	(0.021)
6	−304	−172	−0.082	−0.036
	(84)	(62)	(0.025)	(0.021)
Prison				
Lag: 0	−447	−263	−0.243	−0.149
	(77)	(54)	(0.023)	(0.019)
1	−554	−335	−0.333	−0.235
	(78)	(58)	(0.023)	(0.020)
2	−587	−345	−0.318	−0.220
	(80)	(61)	(0.024)	(0.021)
3	−600	−324	−0.297	−0.189
	(84)	(65)	(0.025)	(0.022)
4	−636	−327	−0.232	−0.116
	(87)	(68)	(0.026)	(0.023)
5	−605	−258	−0.248	−0.117
	(92)	(71)	(0.027)	(0.024)
6	−658	−271	−0.218	−0.076
	(98)	(74)	(0.029)	(0.025)
Age	90	89	0.024	0.024
	(7)	(9)	(0.002)	(0.003)
Age^2 ($\times$ 100)	−33	−31	−0.011	−0.011
	(4)	(4)	(0.001)	(0.002)

TABLE III
(CONTINUED)

Variable	Earnings (1) OLS	Equation (2) FE	Employment (3) OLS	Equation (4) FE
Black	−372		−0.092	
	(8)		(0.002)	
Hispanic	22		0.047	
	(7)		(0.002)	
R^2	0.066	0.020	0.032	0.008
Sample size	343,714	343,714	343,714	343,714

Notes. Standard errors are in parentheses. In addition to variables shown, all regressions include disposition-in-progress and quarter dummies, and lags 0–6 of the unknown disposition and missing sentence indicators. Regressions in columns (1) and (3) also include birth cohort dummies.

Employers informed of an employee's arrest may terminate the employee for obvious reasons having to do with dishonesty. They may, however, dismiss the employee for the purely economic motive of avoiding the costs associated with the employee's repeated absenteeism. Sullivan documents one case where a youth chose to conceal his arrest and was fired for his repeated unexplained absences.

Legal hearings that follow an arrest also may affect the job market status of unemployed arrestees. Sullivan documents several cases where youths decided not to search for work when faced with a series of hearings. Whether employed at the time of arrest or not, the arrestee may end up with a spotty work record. Presumably, this would make him less attractive to prospective employers.

Moving on to the sentencing variables in Table III, it appears that probation has no significant effect on the arrestee's earnings. Incarcerative sentences, in contrast, have substantial effects, though much of this effect simply may represent the enforced withdrawal of the arrestee from the labor market. Jail/probation and jail sentences stipulate a maximum incarceration period of one year. Since the fifth and sixth lags of the sentence indicators are significantly negative, I conclude that these sentences affect postrelease earnings as well. Prison sentences also have a substantial impact on earnings. Since the maximum term for such sentences is potentially quite long, however, these effects may result merely from the arrestee's enforced exclusion from the labor market.

The FE estimates of the employment equation are reported in column (4) of Table III. The main effect of an arrest on employment

is less negative than its effect on earnings. The first two lags are in fact significantly positive, though the coefficients are quite small in magnitude. Apparently, the effect of an arrest on earnings arises from either a reduction in hours worked or a reduction in wages.

Multiple and property arrests have incremental effects on employment that are qualitatively similar to their effects on earnings. For the most part, the effects of conviction and sentencing are similar too. It is interesting to note, however, that probation sentences actually raise postsentence employment significantly. Indeed, this makes sense: employment is often stipulated as a condition of probation, and the penalty for breaking such conditions is to be sent to jail.

Overall, the main impression conveyed by the estimates is that the effects of an arrest on employment and earnings are moderate in magnitude and fairly short-lived. This finding contrasts with many previous researchers who have noted a strong negative correlation between arrests and labor market outcomes. The difference is that my fixed-effect estimation exercise is an attempt to isolate the causal effects of an arrest on the arrestee's employment and earnings. These causal effects are not the only means by which correlation between labor market measures and arrest records may arise, however: an individual's propensity to commit crime (and therefore, presumably, his lifetime arrest record) may be correlated with unobservable factors that also cause his employment and earnings to be low.

To make this distinction clearer, consider a rather stylized example. Suppose that schooling, which is unobserved in my data, is highly correlated with labor market productivity. Then individuals with little schooling receive low wage offers. They might determine that their time is best spent committing crime, which over time would lead them to have a spotty employment record and more arrests than someone with more education. Lifetime arrests therefore will be correlated with lifetime employment, but the correlation arises from unobserved schooling rather than from any causal effects of the arrests.

Figures I and II attempt to distinguish graphically between the causal effects of arrest and unobserved heterogeneity correlated with both arrest records and labor market outcomes. I graph in Figure I the simulated earnings paths of two individuals. The first has one arrest over the entire sample period, and the second has a total of four arrests, including one for a property offense.
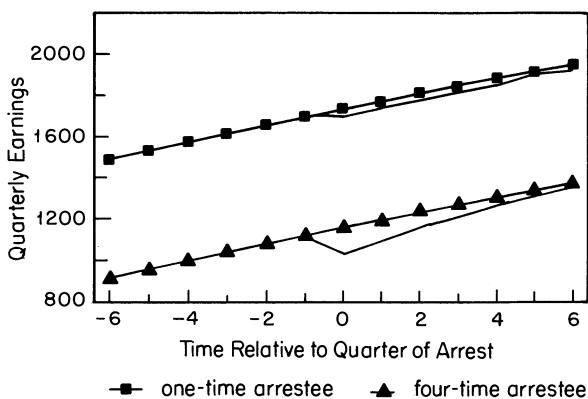
FIGURE I
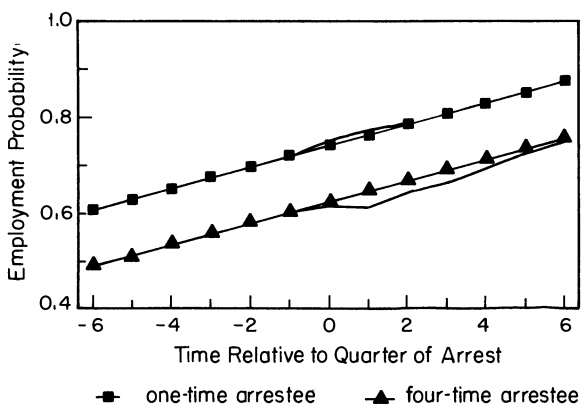Simulated Effect of Arrest on Quarterly Earnings

FIGURE II
Simulated Effect of Arrest on Employment

The top set of lines in the figure depicts two earnings trajectories for the one-time arrestee. For both, the intercept is $1488, the mean quarterly earnings over the sample period among all one-time arrestees in the treatment sample. The line with symbols shows the earnings path of a one-time arrestee over a thirteen-quarter period during which he is *not* arrested. The slope of the earnings path is calculated from the age coefficients at the bottom of column (2) of Table III, assuming that the individual was 23 years old in quarter 0. The path is generally unremarkable: over a three-year horizon, earnings grow roughly linearly with age.

The line without symbols shows what would happen to this individual's earnings path if his one arrest were to occur in quarter 0. It is constructed by adding the FE estimates in column (2) of Table III to the baseline earnings path. As a result of the arrest, earnings fall by $42 in quarter 0, and then return smoothly to the baseline trajectory. After the end of six quarters the arrestee is back on the earnings path he would have experienced if he had not been arrested.

The bottom set of lines shows similar earnings trajectories for the four-time arrestee. The slope of the baseline earnings path for this individual is the same as that for the one-time arrestee: both are based on the age coefficients in column (2) of Table III. There are two important differences between the simulated earnings trajectories of these two workers, however. First, the line without symbols in this case depicts the effect of a property arrest. Because the effect of a property arrest is given by the sum of the main arrest coefficient and the property arrest coefficient from Table III, the initial effect of the property arrest is larger than the effect of the nonproperty arrest that was depicted above. Nonetheless, the effect is fairly modest, amounting to $128 in quarter 0 and vanishing after six quarters. Second, the intercept for the four-time arrestee is only $916, which is the mean quarterly earnings among all four-time arrestees who had one property arrest. Thus, the baseline earnings trajectory is $572 below that of the one-time arrestee. Figure II depicts similarly constructed employment trajectories, from which one draws similar conclusions.

The figures illustrate that for each type of arrestee, the effect of a given arrest is fairly small and dies off relatively quickly. The differences in the levels of the baseline trajectories, however, depict the sizable differences in mean earnings that exist between the two types of arrestees irrespective of the timing of the arrests. From the pictures it seems fairly clear that the permanent differences in mean earnings are considerably greater than the sum of the effects of a few arrests: most of the difference in mean earnings paths, and therefore most of the negative correlation between earnings and total arrests, reflects not the causal effects of the arrests, but rather unobserved heterogeneity that is correlated with the length of the worker's arrest record.[10]

10. One might think that average demographics and risk of jail would vary between one- and four-time arrestees, so that differences in mean earnings would overstate the difference attributable to unobserved heterogeneity. After adjusting

Note that the comparison of one- and four-time arrestees is not meant necessarily to be representative: only one-fifth of the sample was arrested four times or more. It does illustrate, however, that much of the correlation between arrests and labor market outcomes does not reflect causation. It shows graphically that the effects of an arrest are small and short-lived, in contrast to the large and permanent differences in earnings and employment that arise due to unobservable factors that are correlated with the arrest record.

Before concluding, I consider a specification issue. About half of the comparison sample was first arrested in 1985. If increased criminal activity (and therefore risk of arrest) today arises from transitory employment or earnings shocks in the recent past, then including the 1985 first-time arrestees in the comparison sample may result in a model misspecification. This is because their pre-1985 employment and earnings will be too low due to the negative transitory shocks, from which it follows that the difference between their earnings and the treatment sample's earnings will be too small. The resulting estimates therefore will be biased toward zero.

A test for this problem can be conducted by dropping the 1985 first-time arrestees, and using only the 1986 first-time arrestees as the comparison sample. If the resulting estimates are more negative than those obtained using the full comparison sample, then I would suspect that the model was misspecified. Similarity between the two sets of estimates, on the other hand, would provide evidence for the null hypothesis of no misspecification.[11] In fact, the two sets of estimates were nearly identical. There is therefore no evidence of this type of misspecification.

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for race, birth cohort, and the number of jail and prison sentences over the sample period, the mean earnings difference between groups is still $400, or 27 percent of mean earnings for one-time arrestees. I conclude that comparing unconditional means does not give a misleading picture.

11. This test is analogous to Ashenfelter and Card's [1985] specification test in the context of a program evaluation model. In their case, all individuals in the treatment sample are "treated" at the same time. To test whether transitory earnings shocks induce treatment sample members to undergo treatment, they change the base year against which pretreatment earnings are measured. In my case, treatment sample members are treated throughout the 1980–1984 sample period, so there is no natural base year. Comparison sample members first arrested in 1985 eventually received the same treatment as the treatment sample, but in the year following the end of the sample period. Dropping this entire group from the sample thus provides a test for my case of whether transitory labor market shocks induce sample members to undergo treatment.

V. CONCLUSIONS

The primary conclusion of this paper is that the effects of arrests on employment and earnings are moderate in magnitude and rather short-lived. My analysis indicates that most of the negative correlation between arrest records and labor market success stems from unobserved characteristics that jointly influence crime and labor market behavior, rather than from the causal effects of arrests.

This finding helps resolve an apparent conflict between theory and observation. The cross-sectional correlation between earnings and arrest records is strongly negative, suggesting that the market penalty for committing crime is quite severe. Indeed, unless the risk of arrest were quite small, the occurrence of widespread crime in the face of such large market penalties would seem to cast doubt on whether youth crime could be explained by a model of optimizing behavior. In fact, recent research shows that arrest risks are fairly large: Freeman [1992] estimates that young men are arrested about once for every six crimes they commit, and Boland, Mahanna, and Sones [1992] report that about 30 percent of all felony arrests result in a conviction and jail time for the defendant. In a world where arrests have small and short-lived consequences, however, and most of the correlation between arrests and earnings is due to unobserved heterogeneity, widespread crime well may be consistent with optimizing behavior.

My results also indicate that jail terms have short-lasting effects, a finding at odds with Freeman [1992], who, based on an analysis of the NLSY, concluded that jail terms had substantial long-term effects on earnings and employment. The differences in our results may be due in part to differences in the measures of jail spells available in our samples. The NLSY provides an indicator each year of whether the respondent was interviewed in jail. Jail spells so sampled will tend to be longer than the average jail sentence, because only jail terms lasting longer than one year (the interval between interviews) are sampled with certainty. My sample, in contrast, records all jail sentences both long and short. Therefore, it may be that long sentences have long-lasting effects, while the typical sentence has only a shorter effect. Since my data do not include an explicit measure of time served, however, I am unable to test this hypothesis directly.

Finally, note that even though arrests may have small effects, nevertheless other criminal incentives may have important effects

on youth labor market behavior. Indeed, it seems likely that declining youth wages, together with improved criminal prospects brought about by the greater trade in drugs, would provide strong inducements to commit crime at the expense of working on the market. This important issue, which is difficult to study due to inherent problems of measurement, remains open.

UNIVERSITY OF CALIFORNIA, SANTA BARBARA

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