

THE IMPACT OF YOUTH CRIMINAL BEHAVIOR ON ADULT EARNINGS

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Abstract

This paper presents estimates of the effect of youth contact with the criminal-justice system on labor-market earnings as an adult. Our research differs from previous studies in three important ways. First, the existing literature has primarily examined the extent to which adult criminal participation affects wages while we analyze the effect of criminal participation when young on adult wages. Second, we use a representative sample of the entire male population rather than data that include only offenders. Last, in contrast to most studies, which examine the effect on income for only a few years, our study follows labor-market outcomes for more than ten years.

We use data from the National Longitudinal Survey of Youth, which provides detailed, self-reported information on criminal background and socioeconomic and demographic status as of 1980, to specify and estimate a model of the determinants of earnings in 1983 and 1989. Our empirical results imply that having been convicted of a crime when young (prior to 1980) reduces 1983 earnings by at least 12%. However, having been charged—but not convicted—of an offense as a youth has no statistically significant effect on future earnings. As expected, having a criminal case that was adjudicated in juvenile court is associated with a lower labor market penalty than having a criminal case adjudicated in adult court. Juvenile court adjudication of a criminal charge reduces 1983 earnings by at least 9%, while having the charge decided in adult court lowers earnings by about 14%. The magnitudes of these earnings effects persist over the subsequent six years.

JEL Codes: J3 Wages, Compensation, and Labor Costs; K4 Legal Procedure, the Legal System and Illegal Behavior

Keywords: Crime, Illegal Behavior, Earnings, Wages

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I. Introduction

It is well known that young people are more likely to engage in illegal activity than are older individuals. However, few studies have examined how youth criminal participation affects adult labor-market outcomes. Instead, the literature has focused on how adult criminal activity affects adult outcomes. The extent to which illegal behavior engaged in when young influences adult outcomes is less clearly understood. For example, does criminal activity as a youth persistently affect subsequent labor-market performance, or are its effects relatively short-lived? We analyze this question by estimating the impact of youth criminal background on adult labor-market earnings, using panel data from a ten-year period.

Existing research presents conflicting evidence about the labor-market sanctions arising from prior contact with the criminal-justice system [Western, Kling, and Weiman (2001)]. Lott (1990, 1992a, 1992b) examined the earnings of adult federal offenders, and concluded that the post-conviction reduction in income they experienced is statistically significant and largest for high-income offenders. Lott argued that the most important aspect of society's sanction against criminals is the reduction in their legitimate earnings upon returning to the labor force. Waldfogel (1994b) also studied adult federal offenders, and found that a first-time conviction lowered employment probabilities and depressed legitimate income. These effects were largest for offenders whose pre-conviction jobs required some form of trust.

Conversely, other studies have found that the labor-market effects of a criminal

background are modest in both magnitude and duration. Grogger (1995), using a sample of male arrestees from California, concluded that (i) earnings and employment effects are relatively short-lived; (ii) convictions have little effect on future earnings; and (iii) probation has no effect on arrestees' subsequent earnings. Waldfogel (1994a) also addressed the persistence of labor-market penalties for criminal participation and found that, prior to their current conviction, ex-offenders earned less and were less likely to work at legitimate jobs than first-time offenders. These earnings and employment gaps increased with the number of prior convictions. Western and Becket (1999) concluded that prior incarceration has a small, but persistent, effect on the subsequent probability of employment. However, they found that having been charged or convicted as a youth has no effect.

Nagin and Waldfogel (1998) found that criminal participation increases observed wages shortly after conviction. They argued that a prior conviction reduces access to career jobs offering stable, long-term employment, and relegates offenders to spot-market jobs that have higher pay initially but do not offer stable employment or steadily rising wages. Consequently, a first-time conviction has a positive effect on income for offenders under age 25 and an increasingly negative earnings impact for offenders over age 30. Nagin and Waldfogel (1995) studied about 300 London offenders, and concluded that prior criminality alone has no effect on job stability or earnings, whereas a criminal conviction increases both job instability and pay. This result is consistent with their subsequent finding that conviction increases the income of young offenders but also increases employment instability.

Our study differs from the previous literature in two other important ways. First,

our sample is representative of the young-adult, male population. In contrast, most other studies have confined their analyses to the evaluation of labor-market outcomes for offenders.² If, however, offenders are systematically different from non-offenders, previous results may be affected by sample-selection bias. Second, the longitudinal nature of our data allows us to examine the extent to which labor-market penalties for prior criminal activities persist over workers' early careers. Prior studies have typically examined the effect of criminal background on legitimate income for only a few (usually no more than three) years after conviction. In contrast, our study follows labor-market earnings for at least ten years after data were collected about prior contact with the criminal-justice system.

Joseph's (2001) research is most similar to ours, and a comparison between this paper and his helps to distinguish our approach. Joseph analyzed the effect of arrests on earnings, using the National Youth Survey, and found that having been arrested lowers earnings between 18 and 26 percent. There are three noteworthy differences between Joseph's study and ours. First, while Joseph focuses on the effect of arrests on earnings, we estimate the separate effects of arrests *and* convictions. Second, Joseph included both arrests that occurred in youth (when an individual was less than 22 years of age) and arrests that happened later in life. In contrast, we report estimates of the effects of contacts with the criminal justice system that occurred before most individuals began their adult labor market careers. Thus, the arrests and convictions we study occurred between the ages of 14 and 21. Finally, although the data set employed by Joseph covers several years, the fact that the arrests he observes could have occurred at any time means

² Grogger (1992) estimated the effect of conviction on employment in one regression that used data from non-offenders.

that, for some individuals, only a short amount of time passes between their arrest and the end of the sample period. In our study, however, a minimum of nine years passed since the recorded arrest or conviction, allowing us to conduct a longer-term evaluation.

We find that individuals who were convicted of a crime as youths experience a 12 percent reduction in earnings when they are young adults, holding constant various human-capital characteristics like education and work experience. However, those who were charged, but not convicted, of a criminal offense when young suffer no reduction in early-career earnings, *ceteris paribus*. Young adults who had one or more criminal cases adjudicated in juvenile court earned 9 percent less than their non-offender counterparts, and adjudication in adult court reduces earnings by an additional 5 percent. These estimated effects are found to persist over the subsequent six years. In addition, individuals who had contact with the criminal-justice system as youths complete fewer years of schooling and accumulate less work experience as young adults. Because schooling and experience increase future earnings, the estimated partial effect on earnings of a criminal background underestimates its total effect.

II. Data

We use data on males from the 1980, 1984, and 1990 waves of the National Longitudinal Survey of Youth (NLSY), a stratified random sample of individuals who were between 14 and 22 years old in 1979. The 1980 NLSY survey included a special section detailing the respondents' self-reported participation in delinquent and criminal activities. This section of the survey provides detailed information about each respondent's history of criminal charges and convictions, the nature of any offenses

committed, and whether adjudication of a criminal case was in juvenile or adult court. We combine this information with standard demographic and labor-market data to specify and estimate conventional earnings equations that are augmented by a variety of criminal-participation variables. The 1984 and 1990 surveys record labor-market earnings for 1983 and 1989, respectively.

Our empirical work uses two samples: one includes individuals through the 1984 wave of the NLSY, and the second includes individuals through the 1990 wave. To construct the first data set, we omit all individuals who were younger than 21 at the time of the 1984 interview, because many were still in school or just beginning their labor-market experiences.³ Furthermore, observations were deleted for individuals reporting zero weeks of work or zero income and those responding inappropriately.⁴ Finally, we deleted people who were students during the week of the interview.⁵ There are 2,897 respondents with complete records for all variables of interest in 1984.

The 1990 data set was constructed by imposing the same restrictions used to create the 1984 data, with the exception of age. We did not impose an age restriction for the 1990 sample because the respondents to the survey were not then of typical school-going age. There are 3,280 respondents with complete records for all variables of interest

³ We also ran, but do not report, regressions that do not impose this restriction. The estimated effects of the criminal-participation variables were slightly larger in these regressions.

⁴ Missing observations are those defined as REFUSAL, DON'T KNOW, INVALID SKIP, or NONINTERVIEWS. Variables also include the code VALID SKIPS, but this is not necessarily a missing observation. For example, VALID SKIPS for the variables ADLTCRT, NUMCHAR, and NUMCNVC reflect those not charged or convicted of crimes. These valid skips are recoded as zeros. This reduces the sample from 12,686 to 5,400. Of those remaining, 16.7% report having been charged with a crime and 9.9% report having been convicted.

⁵ This is done using a variable in the NLSY called Employment Status Recode (R15199), which reflects employment status during the week of the interview. Individuals coded "Going to School" were deleted.

in 1990. Thus, the 1990 sample is larger than the 1984 sample because the age restriction is relaxed. We adjusted 1989 income to constant 1983 dollars. Table 1 contains the summary statistics for the two samples.

III. Model

We estimate the model

$$\ln(Y_{it}) = \mathbf{a} + \mathbf{b}_1 C_{i0} + \mathbf{b}_2 F_i + \mathbf{b}_3 V_{it} + \mathbf{e}_{it} \quad (1)$$

where Y_{it} is annual earnings in 1983 or 1989, C_{i0} is a set of criminal-participation variables for each person i , as of the interview year 1980, F_i is a vector of time-invariant individual characteristics, such as race, ethnicity, age and AFQT score⁶, V_{it} is a vector of characteristics that vary over time, such as educational attainment, marital status, work experience, union-membership status, and whether or not the respondent lives in a Metropolitan Statistical Area, and \mathbf{e}_{it} is an individual-specific error term.

We use four alternative measures of a youth's contact with the criminal-justice system: (i) a dummy variable indicating whether the individual had been charged with a crime; (ii) a dummy variable indicating whether the individual had been convicted of a crime; (iii) a pair of dummy variables indicating, respectively, whether an individual had been charged but not convicted, and whether he had been convicted; and (iv) a pair of dummy variables denoting whether an individual's criminal case was adjudicated in juvenile or adult court. We estimate these four specifications for both the 1984 and 1990 samples, and therefore report eight sets of estimates of our model of earnings.

⁶ AFQT denotes the normalized score on the Armed Forces Qualification Test, administered in 1980 to over 90% of the NLSY panel, and measures pre-market skills.

Because unobserved characteristics that lead to high wages and employment probabilities also reduce participation in criminal activity, estimates that do not control for these will be biased toward finding the expected negative relationship between youth criminal participation and adult earnings. Several papers have attempted to control for such unobserved heterogeneity in a variety of ways. Grogger (1995) chose a comparison group for the California arrestees comprising his sample to control statistically for time-invariant, individual-specific, unobservable characteristics. Waldfogel (1994b) and Lott (1992a, 1992b) estimated differences between pre- and post-conviction income as a function of changes over time in criminal participation.

Unfortunately, because the NLSY records criminal participation only in the initial year (1980), we do not observe changes in criminal participation, and therefore cannot control for unobserved heterogeneity with a fixed-effects, panel-data model. Instead, we deal with heterogeneity in two ways. First, the NLSY contains an extensive set of demographic variables that allow us to control for many observed individual characteristics. One of these variables, AFQT, is frequently omitted from earnings regressions and, as a proxy for ability, captures much of the heterogeneity that would otherwise be unobserved. Grogger (1995) pursued a similar strategy by incorporating various demographic variables, but he excluded AFQT. Grogger also noted a problem with the NLSY arrest data: blacks and whites have the same number of self-reported arrests, on average. In most other samples, however, the arrest rate for blacks is about three times that of whites. Second, the full model specification in (1) includes many characteristics over which individuals have some degree of choice, these are captured in V_{it} above. Because educational attainment, marital status, and work experience are

functions of prior criminal activity, however, some of the effect of youth criminal behavior on adult earnings is absorbed by the coefficients on these variables. Consequently, the estimate of b_1 in the full specification provides a lower bound on the total effect of youth criminal background on adult earnings.

Our analysis is limited to young adults who reported positive labor-market earnings. However, both Freeman (1991) and Grogger (1992) found that having a criminal record when young reduces the probability of legal employment as an adult. Consequently, by restricting our sample to employed individuals, we further underestimate the total effect of youth criminal background on adult earnings, inclusive of its effect on employment status.

IV. Empirical Results

We begin the empirical analysis by estimating the raw, unadjusted difference in adult earnings between individuals who, when young, had formal contact with the criminal justice system (criminal charges and/or convictions) and those who did not. This estimated difference does not control for either fixed, pre-market traits that affect adult earnings (such as race or ability) or for other human-capital variables (like schooling and work experience) that help determine adult earnings but could also be affected by youth criminal activity. We obtain this raw difference by estimating a bivariate regression in which the dependent variable is either 1983 or 1989 log annual income.

Table 2 contains ordinary least-squares estimates of four bivariate regressions of 1983 log annual earnings against each of the four alternative measures of youth criminal background. Column 1 indicates that individuals who were charged with a crime when

young (whether or not convicted) earned approximately 27% less in 1983, on average, than individuals who were not criminally charged. Of course, because this regression does not control for observed (and unobserved) differences in characteristics that affect earnings, this point estimate is equivalent to a simple difference-in-means. The bivariate regression results reported in column 2 imply that young adults convicted of a crime as youths earned about 29% less in 1983, on average, than those who were not. As expected, the coefficient on having been convicted is larger than the one reported in column 1 on having been charged. Column 3 shows that youths who were charged but not convicted of a criminal offense earned approximately 21% less as young adults than individuals with no criminal charges against them, while persons convicted of crimes when young earned about 31% less as young adults than did those who had no criminal convictions. In column 4, finally, youths whose criminal charges were adjudicated in juvenile (adult) court experienced a 27% (26%) decrease in 1983 earnings compared with uncharged individuals.

Table 3 reports the results of estimating the same four specifications, but with 1989 earnings as the dependent variable, and shows the same general results — the coefficients on the criminal-sanction variables are uniformly negative, substantial in size, and significantly different from zero. The estimates of the coefficients on the variables indicating having been charged and convicted of a crime when young are slightly higher than those from the results with 1983 earnings as the dependent variable.

An analysis of the effect of youth criminal background on adult earnings must assign to (observable) pre-market characteristics some of the explanatory power for differences in subsequent earnings between youthful offenders and non-offenders.

Inherent skill (or ability or aptitude), along with ethnicity and age, are important determinants of labor-market earnings that are unaffected by subsequent individual choices but may be correlated with criminal behavior when young.

Tables 4 and 5 report least-squares estimates of the effect of each of our four alternative measures of youth criminal activity, controlling for the above-mentioned pre-market variables, on 1983 and 1989 earnings, respectively. The point estimate in column 1 implies that, holding ethnicity, skill, and age constant, individuals who were charged with a crime when young earned almost 29% less in 1983 than those who were not. The magnitude of the coefficient on CHARGED is smaller in this specification than in the simple bivariate model because, in the latter, the estimated coefficient captures effects on subsequent earnings that are more properly attributed to the pre-market variables included here. As expected, the estimated coefficient on BLACK is negative and significantly different from zero, and implies that blacks earn about 32% less than whites, holding pre-market skills and age constant. However, this model specification is extremely parsimonious, and does not control for variables such as education and work experience that are typically included in earnings regressions and that are correlated with race. In contrast, the estimate of the HISPANIC coefficient is small and not significantly different from zero. The estimated coefficients on AFQT and AGE are positive and significantly different from zero, as expected.

Column 2 reports the results of estimating the same specification discussed above, with criminal background now represented by a dummy variable indicating whether or not the individual was convicted of a crime as a youth. The coefficient estimate on CONVICTED is positive, significantly different from zero, and somewhat larger than the

estimated coefficient on the CHARGED variable reported in column 1. The estimated coefficients on the included pre-market variables are virtually identical to those in column 1.

Individuals convicted of a crime when young were also charged with that crime, of course, so it is of interest to calculate the marginal effect on earnings of having been convicted of a youthful crime, given that one has been charged with the crime. The estimates in column 3 indicate that someone who was charged but not convicted earns about 22% less than his uncharged counterpart. However, an individual who was charged and subsequently convicted experienced a 34% reduction in 1983 labor-market earnings. Therefore, the marginal impact of a prior conviction on 1983 earnings is about -11.5% [$-33.9 - (-22.4)$], *ceteris paribus*.

Finally, the data permit us to distinguish between the subsequent earnings effects of a criminal charge that was adjudicated in juvenile court rather than in adult court. Column 4 reports the empirical results for this specification, and shows that individuals whose criminal cases were handled in juvenile court earned approximately 20% less than those having had no contact when young with the criminal-justice system. However, those youths whose cases were adjudicated in adult court experienced a 36% reduction in 1983 earnings. This large difference in coefficient estimates may reflect one or both of the following phenomena: (i) because of the confidentiality of juvenile-court proceedings, the “scarring” or “signaling” aspects of criminal charges handled in that setting are less than in cases dealt with in open, adult court; (ii) youths who commit crimes of such severity that they are tried in adult court are systematically different from their juvenile-court counterparts in ways that adversely affect subsequent labor-market

earnings. As before, the 1989 results for the criminal-sanction variables are very similar to the 1983 findings.

The results reported in Tables 4 and 5 control only for the exogenous pre-market variables that, along with youth criminal background, affect the subsequent earnings of young adults. However, the model on which these estimates are based is an under-specified representation of the process determining such earnings. In particular, this specification excludes variables such as schooling and work experience which proxy human-capital investment affecting earnings as a young adult. To redress this shortcoming, we specify a model of earnings incorporating additional variables that are exogenous to earnings but whose values are determined in part by choices made after adolescence.

Tables 6 and 7 report the results of this more richly specified earnings model. Because we include both schooling and work experience in this regression and use a sample of males for whom post-schooling work experience is, on average, highly continuous, we excluded age from these regressions. The estimated coefficients on the pre-market variables HISPANIC and AFQT are very similar to those from the more parsimonious specification reported in Table 4. Interestingly, the size of the coefficient on BLACK is reduced by almost three-fifths after controlling for the post-adolescence explanatory variables, suggesting considerable heterogeneity among the black population with respect to these additional observable determinants of earnings.

The signs, sizes, and significance levels of the coefficients on the additional explanatory variables in column 1 conform to standard results reported in the empirical earnings literature. In particular, the coefficients on schooling (grades completed), marital

status, location of residence, and union-membership status are positive and significantly different from zero. Additional weeks of work experience increase earnings, but at a decreasing rate. Individuals who were charged with a crime when young earned approximately 11.4% less in 1983 than their non-charged counterparts, and this adverse earnings effect is significantly different from zero. However, the size of the criminal-background discount on adult earnings is lowered by about three-fifths with the inclusion of the additional observable influences on adult earnings. We interpret this reduction in the estimated effect of youth criminal background to mean that a portion of the *total* effect of having been charged when young with a criminal offense is now being attributed to variables — such as labor-market experience and years of completed schooling — that are themselves affected by adolescent criminal activity. As a consequence, the estimated coefficient on CHARGED is a downward-biased estimate of the true effect of a youthful criminal charge on subsequent earnings. This downward bias offsets to an unknown degree the upward bias in the estimated effect associated with any individual heterogeneity arising from omitted (unobservable) variables that are correlated with both youth criminal background and adult earnings.

In column 2 the point estimate of the coefficient on CONVICTED is slightly higher than that on CHARGED, reported in the previous column, and is significantly different from zero. As before, the model specification in column 3 permits us to separate out the marginal effect of being convicted when young of a criminal offense from the effect of having been charged but not convicted. The point estimates of the coefficients on both criminal-participation variables are substantially lower than before, again suggesting that the total effects of these variables are being attributed partly to post-

adolescent individual characteristics that are, in turn, affected by youth criminal behavior. The evidence from estimates of this specification implies that an individual charged with a crime when young experiences about a 9% reduction in earnings as a young adult, *ceteris paribus*, while the marginal effect on earnings of a conviction, having been charged, is $-12.8 - (-8.8) = -4.0\%$.

Column 4 reports the results of estimating the model with dummy variables indicating adjudication of any criminal case(s) in adult or juvenile court. Again, the point estimate of the coefficient on the adult-court variable is substantially lower than the estimated coefficient on the juvenile-court variable (-0.131 versus -0.095). Moreover, the magnitudes of both coefficients are lower in this estimated regression than in the more parsimonious model reported in Table 4, as expected.

Compared with the 1983 results, the estimated effects on 1989 earnings of being black, living in an urban area, being a union member, previous work experience, and being married are smaller, while the estimated return to schooling is substantially larger. Examining the estimates of the coefficients on the variable CHARGED in column 1 across the three tables, we find essentially no difference in the magnitudes of the estimated effects on 1983 and 1989 earnings. The point estimate of the effect on 1989 earnings of having been convicted is slightly higher than its effect on 1983 earnings for each of the model specifications.

Our main finding is that both arrests and convictions that occur in youth reduce earnings in adulthood. The point estimate of the earnings reduction varies between 9% and 13% in the most richly specified models, and this negative effect persists for at least ten years. Our conclusions are consistent with the findings reported by Lott (1990),

Waldfoegel (1994b), and Joseph (2001) of a substantial labor-market penalty for recent criminal activity. Our results stand in contrast, however, to those reported by Waldfoegel (1994a) and, especially, Grogger (1995) that the effects of prior criminal convictions on future earnings and employment are short-lived. A full accounting for these disparate findings is beyond the scope of the present discussion. Nevertheless, we draw attention to the fact that our sample includes non-offenders as well as those with a criminal background, and that we estimate the earnings effects of prior criminal participation over an extended period of time.

V. Conclusion

This paper uses data from a stratified random sample of the young-adult, male population to estimate the effect of youth criminal arrests, charges, and convictions on subsequent labor-market earnings. We separate the effects of having been charged with a criminal offense but not convicted, from having been charged and convicted, and compare the estimated effects over time. We conclude that the effect on earnings of having only been charged with a crime decreases over time, while the effect of having been convicted rises slightly in two of the three model specifications. Having been charged, but not convicted, decreases earnings by between 5% and 8%, while conviction when young permanently lowers adult earnings by at least 12%. Having one's case adjudicated in a juvenile court lowers adult earnings by at least 9%, while adjudication in an adult court lowers earnings an additional 5%.

In relating our results to those reported in the previous literature, we highlight two distinguishing features of our study. First, we analyzed a stratified random sample of the

young-adult, male population that included non-offenders as well as those who engaged in criminal activity in the past. As a result, our findings are applicable to young men drawn from the entire population rather than only ex-offenders. Second, we asked a slightly different, but equally important, question than has occupied much of the previous literature: does *youthful* criminal behavior reduce *adult* earnings? Our answer to this question is affirmative, and we find that the negative effect persists over a ten-year period. The fact that our results depart from the findings of several previous studies may simply reflect crucial differences in the nature of the questions asked and the data employed.

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TABLE 1
Summary Statistics

Variable	Number	Mean	St. Dev.	Min.	Max.
1984 Data					
Age	2897	23.65	1.76	21	27
Income	2897	11,237	8,210	25	75001
Black	2897	0.23	0.42	0	1
Hispanic	2897	0.14	0.35	0	1
AFQT89	2897	45.07	29.90	1	99
SMSA	2897	0.76	0.42	0	1
Grade	2897	12.46	2.14	2	20
Married	2897	0.33	0.47	0	1
Experience (weeks)	2897	206	77	2	312
Experience ²	2897	48,452	29,837	4	97344
Union Member	2897	0.21	0.41	0	1
Charged	2897	0.18	0.39	0	1
Just Charged	2897	0.09	0.29	0	1
Convicted	2897	0.11	0.31	0	1
Adult Court	2897	0.10	0.29	0	1
Juvenile Court	2897	0.09	0.28	0	1
1990 data					
Age	3280				
Income	3280	17,963	12,133	40	138204
Black	3280	0.25	0.43	0	1
Hispanic	3280	0.16	0.36	0	1
AFQT89	3280	42.87	30.39	1	99
SMSA	3280	0.79	0.41	0	1
Grade	3280	12.92	2.47	3	20
Married	3280	0.52	0.50	0	1
Experience (weeks)	3280	435.31	132.20	3	624
Experience ²	3280	206,963	107,009	9	389376
Union Member	3280	0.20	0.40	0	1
Charged	3280	0.14	0.35	0	1
Just Charged	3280	0.07	0.25	0	1
Convicted	3280	0.08	0.28	0	1
Adult Court	3280	0.06	0.24	0	1
Juvenile Court	3280	0.08	0.27	0	1

TABLE 2**The Effect of Criminal Participation on 1983 Wages: Bivariate Regression**

Variable	(1)		(2)		(3)		(4)	
	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat
Charged	-0.268	-5.40						
Convicted			-0.288	-4.62	-0.308	-4.94		
Just Charged					-0.208	-3.09		
Adult Court							-0.263	-4.03
Juvenile Court							-0.273	-3.97
Intercept	9.009	426.42	8.991	444.10	9.012	422.82	9.009	426.34
Num. of Obs.	2,897	2,897	2,897	2,897	2,897	2,897	2,897	2,897
F-Statistic	29.11		21.38		15.49		14.56	
Adj. R ²	0.0096		0.0070		0.0091		0.0093	

Note: Dependent variable is the natural log of 1983 income.

TABLE 3**The Effect of Criminal Participation on 1989 Wages: Bivariate Regression**

Variable	(1)		(2)		(3)		(4)	
	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat
Charged	-0.279	-6.78						
Convicted			-0.325	-6.33	-0.339	-6.59		
Just Charged					-0.184	-3.25		
Adult Court							-0.220	-3.67
Juvenile Court							-0.324	-6.11
Intercept	9.594	626.43	9.583	644.63	9.596	622.05	9.594	626.50
Num. of Obs.	3,280	3,280	3,280	3,280	3,280	3,280	3,280	3,280
F-Statistic	45.90		40.09		25.40		23.86	
Adj. R ²	0.0135		0.0118		0.0147		0.0138	

Note: Dependent variable is the natural log of 1989 income.

TABLE 4**The Effect of Criminal Participation on 1983 Wages: Fixed Regressors Only**

Variable	(1)		(2)		(3)		(4)	
	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat
Charged	-0.286	-5.99						
Convicted			-0.314	-5.26	-0.339	-5.64		
Just Charged					-0.224	-3.49		
Adult Court							-0.362	-5.76
Juvenile Court							-0.202	-3.06
Black	-0.322	-6.50	-0.315	-6.36	-0.324	-6.56	-0.324	-6.56
Hispanic	0.030	0.54	0.028	0.50	0.026	0.47	0.026	0.47
AFQT	0.121	5.83	0.124	5.99	0.119	5.73	0.119	5.73
Age	0.119	11.27	0.119	11.24	0.120	11.39	0.120	11.39
Intercept	6.267	25.04	6.251	24.93	6.242	24.94	6.242	24.94
Num. of Obs.	2,897	2,897	2,897	2,897	2,897	2,897	2,897	2,897
F-Statistic	66.92		65.09		56.48		56.39	
Adj. R ²	0.1022		0.0996		0.1031		0.1029	

Note: Dependent variable is the natural log of 1983 income.

TABLE 5**The Effect of Criminal Participation on 1989 Wages: Fixed Regressors Only**

Variable	(1)		(2)		(3)		(4)	
	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat
Charged	-0.271	-7.11						
Convicted			-0.322	-6.81	-0.336	-7.08		
Just Charged					-0.160	-3.08		
Adult Court							-0.308	-5.48
Juvenile Court							-0.245	-5.02
Black	-0.162	-4.73	-0.158	-4.61	-0.163	-4.76	-0.162	-4.74
Hispanic	0.061	1.61	0.059	1.54	0.058	1.53	0.061	1.59
AFQT	0.265	17.89	0.268	18.19	0.265	17.88	0.265	17.89
Age	0.047	7.94	0.045	7.69	0.047	7.93	0.0485	7.98
Intercept	8.571	64.65	8.596	64.90	8.575	64.73	8.551	63.65
Num. of Obs.	3,280	3,280	3,280	3,280	3,280	3,280	3,280	3,280
F-Statistic	142.02		141.03		119.41		118.47	
Adj. R ²	0.1770		0.1760		0.1781		0.1769	

Note: Dependent variable is the natural log of 1989 income.

TABLE 6

The Effect of Criminal Participation on 1983 Wages: Full Specification

Variable	(1)		(2)		(3)		(4)	
	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat
Charged	-0.114	-2.73						
Convicted			-0.117	-2.26	-0.128	-2.46		
Just Charged					-0.088	-1.58		
Adult Court							-0.131	-2.41
Juvenile Court							-0.095	-1.66
Black	-0.125	-2.81	-0.123	-2.77	-0.126	-2.82	-0.125	-2.81
Hispanic	-0.024	-0.50	-0.024	-0.51	-0.025	-0.53	-0.024	-0.51
AFQT	0.124	5.42	0.123	5.36	0.123	5.40	0.124	5.43
SMSA	0.149	3.98	0.145	3.86	0.148	3.94	0.149	3.97
Grade	0.017	1.73	0.019	1.95	0.017	1.74	0.017	1.73
Married	0.325	9.40	0.324	9.38	0.325	9.40	0.325	9.40
Experience	0.012	12.47	0.012	12.44	0.012	12.48	0.012	12.47
Experience ²	0.000	-6.40	0.000	-6.35	0.000	-6.41	0.000	-6.40
Union	0.282	7.18	0.284	7.25	0.282	7.18	0.281	7.18
Intercept	6.783	44.47	6.751	44.65	6.783	44.47	6.782	44.46
Num. of Obs.	2,897	2,897	2,897	2,897	2,897	2,897	2,897	2,897
F-Statistic	151.03		150.68		137.28		137.29	
Adj. R ²	0.3412		0.3407		0.3411		0.3411	

Note: Dependent variable is the natural log of 1983 income.

TABLE 7

The Effect of Criminal Participation on 1989 Wages: Full Specification

Variable	(1)		(2)		(3)		(4)	
	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat	Coeff.	T-stat
Charged	-0.117	-3.39						
Convicted			-0.140	-3.28	-0.145	-3.36		
Just Charged					-0.049	-1.04		
Adult Court							-0.148	-2.99
Juvenile Court							-0.093	-2.09
Black	-0.091	-2.86	-0.090	-2.83	-0.091	-2.86	-0.091	-2.85
Hispanic	0.037	1.09	0.037	1.07	0.037	1.06	0.037	1.08
AFQT	0.129	7.36	0.129	7.35	0.130	7.37	0.130	7.38
SMSA	0.119	4.10	0.115	3.94	0.116	3.97	0.119	4.09
Grade	0.061	9.35	0.062	9.56	0.061	9.39	0.061	9.35
Married	0.228	9.26	0.229	9.32	0.228	9.28	0.228	9.26
Experience	0.006	11.93	0.006	11.83	0.006	11.84	0.006	11.96
Experience ²	0.000	-7.58	0.000	-7.49	0.000	-7.50	0.000	-7.60
Union	0.161	5.49	0.161	5.49	0.161	5.49	0.161	5.50
Intercept	7.019	56.38	7.011	56.43	7.025	56.24	7.014	56.31
Num. of Obs.	3,280	3,280	3,280	3,280	3,280	3,280	3,280	3,280
F-Statistic	169.51		169.40		154.10		154.16	
Adj. R ²	0.3395		0.3393		0.3393		0.3394	

Note: Dependent variable is the natural log of 1989 income.