# NEW ESTIMATES OF THE EFFECT OF MARIJUANA AND COCAINE USE ON WAGES

ROBERT KAESTNER*

Using the 1984 and 1988 waves of the National Longitudinal Survey of Youth, this study provides an update of several previous cross-sectional estimates of the effect of illicit drug use on wages, as well as the first longitudinal estimates of that effect. The cross-sectional results, which are generally consistent with the surprising findings of previous research, suggest that illicit drug use has a large, positive effect on wages. The longitudinal estimates, which control for unobserved heterogeneity in the sample, are mixed: among men, the estimated wage effects of both marijuana and cocaine use are negative, but among women, the effect of cocaine use remains positive and large. Because the longitudinal model is imprecisely estimated, however, those results are inconclusive.

The adverse physical and psychological effects of illicit drug use have been well documented in the medical literature and widely publicized through an extensive public and private media campaign. Given the public's knowledge of the negative health consequences of illicit drug use, it is not surprising that illicit drug use is also commonly believed to adversely affect the social and economic aspects of users' lives. Despite these perceptions, a large segment of the U.S. population engages in illicit drug use. In a 1991 survey of 18–34-year-olds, for example, approximately 60% of the respondents reported having used illicit drugs at some time in their lives, and approximately 23% reported using illicit drugs within the previous year (NIDA 1991).

One important focus of efforts to reduce illicit drug use has been the labor market. Concern that the ability of our workers is being seriously impaired by illicit drug use has spurred sizable expenditures to correct the problem. The fundamental goal of most drug prevention programs is to eliminate *all* illicit drug use, a goal that appears unrealistic in view of the fact that illicit drugs continue to be widely used despite extensive dissemination of information about their harmful effects. Thus, the merit of a "zero tolerance" drug policy depends critically on the effect of illicit drug use on the individual. If illicit drug use harms the user regardless of the particular drug used and even when the amount of the drug consumed is small, a zero tolerance policy is clearly defensible; otherwise, the wisdom of such a policy is questionable.

To date, little evidence of the type that would satisfy most economists has been pro-

*The author is Assistant Professor of Economics at Rider College and Faculty Research Fellow of the National Bureau of Economic Research. He thanks Andrew Gill for comments on an earlier draft. A data appendix with additional results, and copies of the computer programs used to generate the results presented in this paper, are available upon request to Robert Kaestner, Department of Economics, Rider College, Lawrenceville, NJ 08648.

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duced to justify the extent and scope of the current drug prevention effort, particularly in the labor market. Since earnings are one of the best and most accessible measures of labor market success, several recent studies, using data from the National Longitudinal Survey of Youth (NLSY), have examined the impact of illicit drugs on the wages of young adults. In general, the evidence presented in these papers, directly contradicting the commonly held belief that drug use has an adverse impact on labor market outcomes, indicates that drug use has a *positive* effect on wages. The explanation most often suggested for this finding is the probable existence of unobserved heterogeneity in the sample; individuals who use drugs may also, for reasons unobserved by the researcher, be more productive than average.

The previous studies on this subject have all been cross-sectional in nature, and none of them, despite their careful execution, have been able to fully control for unobserved characteristics that could strongly affect the observed relationship between illicit drug use and wages. The purpose of this paper is to partially fill this gap in the literature by estimating, for the first time, a longitudinal or fixed effects model of drug use and wages. To the extent that some of the potentially important unobserved characteristics that have been omitted from previous studies are individual-specific and time-invariant, a fixed effects wage model will provide better estimates of the relationship between illicit drugs and wages. In addition, using the NLSY data of 1988, I provide the most up-to-date cross-sectional estimates of the effect of illicit drug use on the wages of young adults yet published.

## Illicit Drug Use and Wages

There have been several studies of the effects of illicit drug use on wages, and all of them have used the 1984 wave of the NLSY. Gill and Michaels (1992), using a switching regression framework, examined the effect of (1) use of any illicit drug and (2) use of hard drugs (cocaine and heroin) on the wages of a combined sample of men and women. They found a positive effect of drug use on wages, resulting primarily from differences in the return to unobserved characteristics. In a 1991 study (Kaestner 1991), I examined the wage effect of both lifetime and recent use of marijuana and cocaine, using a simultaneous equations model in which drug use and the wage were jointly determined. I found that both marijuana use and cocaine use positively affected wages of all four demographic groups chosen on the basis of gender and age. I also estimated a switching regression model, and found positive wage effects of cocaine and marijuana use consistent with Gill and Michaels's (1992) results. Using a model that was somewhat unconventional in terms of economic theory, Kandel and Davies (1990) estimated simple OLS wage regressions, and found no significant effect of marijuana or cocaine use on the wages of employed men.[1] Finally, Register and Williams (1992) examined the effect of marijuana and cocaine use on the wages of men, including the effect of on-the-job use and long-term use, using an instrumental variables approach. They found a positive wage effect of general marijuana use, but negative effects of on-the-job marijuana use and long-term marijuana use. They found no significant effect of cocaine use on the wage.

All of these previous studies started with the premise that illicit drug use results in deterioration of an individual's physical and psychological well-being, and consequently also decreases a person's productive capabilities. Drug use and wages were hypothesized to be negatively related.

The observed relationship between illicit drug use and wages could very well be positive, however, if illicit drugs are a normal good, since wages are an important determinant of income. Indeed, this hypothesis has received some empirical support. Sickles and Taubman (1991) reported findings suggesting that individuals with higher earnings ca-

[1]Kandel and Davies (1990) estimated a model in which the 1985 wage was the dependent variable, and all independent variables were measured as of 1984. Included among the independent variables were the 1984 wage and drug use, both of which were treated as exogenous. In addition, the authors made no attempt to correct for selection bias due to the labor force participation decision.

pacity have a greater involvement in illicit drug use. Kaestner (1991) and Register and Williams (1992) explicitly recognized the potential income effect, and estimated a structural model of illicit drug use and wages. A second reason for estimating a structural model of drug use and wages is found in Gill and Michaels (1992) and Kaestner (1991). Those studies cast the basic consumer problem in a household production framework, in which the interdependence of drug use and wages resulted from the wage being the price of time used in producing goods.[2] Given these considerations, in this paper I use the following two-equation model to estimate the effect of illicit drug use on wages:

$$(1) \qquad W_{it} = f(X_{it}, D_{it}, E_i, V_{it})$$

and

$$(2) \qquad D_{it} = g(Z_{it}, W_{it}, E_i, U_{it}).$$

In equation (1), $W$ is the natural logarithm of the wage, $X$ is a vector of exogenous variables affecting the wage, $D$ is a measure of the quantity of illicit drug use, $E$ is an unobserved person-specific effect, $V$ is an error term, $i$ indexes individuals, and $t$ indexes time. Equation (2) has similarly defined variables, with $Z$ being a vector of exogenous variables that affect drug use, and $U$ the error term.

## Empirical Model

Before empirical estimates of the model can be obtained, two issues need to be addressed. First, the functional form of equations (1) and (2) needs to be specified. For now, I assume these functions are linear. Second, the exogenous variables for each equation need to be chosen. For equation (1), these will include several human capital variables (for example, education and experience), demographic variables (such as age, race, and marital status), geographic measures (such as region), and family/personal background variables (such as household composition at age 14 and self-esteem scale). The exogenous variables chosen for equation (2) will include all of those in equation (1), plus a measure of non-earned income, the frequency of religious attendance in 1979, the current number of dependent children of various ages, and the number of illegal acts committed in 1979. These last four variables will identify the parameter estimates of equation (1). I chose them based on the results of my 1991 study, which tested this set of variables for the over-identifying restrictions they impose. The parameters of equation (2) are under-identified, and therefore only the reduced form version of this equation will be estimated.

Both cross-sectional and panel data (fixed effect) estimates of the model will be obtained, and the implementation strategy will be the same in both cases. First, reduced form estimates of equation (2) will be obtained using the entire sample, and from these estimates a predicted drug use measure will be calculated. Next, estimates of the wage equation (1) will be obtained for a sample of employed individuals using the predicted drug use variable in place of its actual value, and correcting for the sample selectivity introduced by examining only employed individuals.[3] The Heckman (1976) two-step procedure will be used to correct for the potential sample selectivity bias.[4]

[2]The two papers do not treat the problem in exactly the same way. In my 1991 study, I provided only a simple model similar to that found in Stigler and Becker (1977). In this model, the wage is simply the price of time used to produce consumption goods, including drug consumption. Gill and Michaels (1990) presented a much more detailed model, in which the role played by the wage, though similar, is more complex. In their model, drug time is being produced, and the wage becomes part of the full price.

[3]This strategy is different from that found in Kaestner (1991). In that paper, I obtained the reduced form estimates of equation (2) using only employed persons, and a selectivity correction was applied to this equation as well as the wage equation (1). The results are little affected by which of these strategies is chosen.

[4]In computing the standard errors for this model, it is important to take account of the fact that several of the right-hand-side variables (specifically, drug use and sample selection terms) are predicted values. Murphy and Topel (1985) outlined the procedure for calculating the exact standard errors for a simple version of this type of model. The current case is non-standard, given the inclusion of the selectivity terms. In the cross-sectional regressions, the standard errors are those that correct for the inclusion of the selection correction

The choice of the fixed effects specification of the model is based on the expectation that the unobserved personal characteristics that influence the wage will also be correlated with the other variables in the model, particularly the drug use measures. Thus, this specification is preferred over the alternative random effect model, which assumes that the individual effect is uncorrelated with the other variables. The Chamberlain (1982) "correlated random effects" model, which allows for the correlation between the unobserved effect and the other explanatory variables, is identical to the fixed effects specification when the model is linear in the parameters, as is the current model. The specification of the fixed effects model used in this paper, however, is somewhat unconventional and a departure from what is usually found in the literature. For simplicity, assume that equation (1) can be written as follows for period $t$:

$$\text{(1a)} \qquad W_{it} = b_0 + b_1 X_i + b_2 Z_{it} + b_3 E_i + V_{it},$$

and for period $t-1$,

$$\text{(1b)} \qquad W_{it-1} = a_0 + a_1 X_1 + a_2 Z_{it-1} + b_3 E_i + V_{it-1}.$$

The $X$ are exogenous variables that are time-invariant (such as race), and the $Z$ are exogenous variables that are time-varying (such as illicit drug use). The $E$ in the above equations are the unobserved person-specific characteristics, which are assumed to be time-invariant. The standard way to obtain unbiased estimates of the parameters of the model is to take the difference of equations (1a) and (1b), and run an OLS regression. Furthermore, it is usually assumed that the $a_i = b_i = b$, which yields the following:

$$\text{(1c)} \qquad W_{it} - W_{it-1} = b\,(Z_{it} - Z_{it-1}) + (V_{it} - V_{it-1}).$$

Note that the unobserved person effect has been eliminated by taking the difference. An alternative specification allows the $a_i$ and $b_i$ to differ and estimates the following:

$$\text{(1d)} \qquad W_{it} - W_{it-1} = (b_0 - a_0) + (b_1 - a_1)\, X_i + b_2 Z_{it} - a_2 Z_{it-1} + (V_{it} - V_{it-1}).$$

Although not common, specifying a model in which the parameter values change over time is a theoretically sound idea. For example, if the degree of racial discrimination changes over time, the impact of race on the wage will also change. Similar arguments can be made for the remaining variables, including the human capital variables such as education and experience.[5]

There are several benefits associated with specifying the model as in equation (1d). First, equation (1d) imposes the fewest restrictions on the model. The only restriction imposed in equation (1d) is that the effect of the unobserved person-specific characteristics ($b_3$) is the same in both time periods. Second, given the way the quantity of drug use is measured (described in detail below), the differencing of the drug use measure presents problems, and is not intuitively appealing.[6] Thus, including the level of drug use from each year of data in the model

term, but ignore the fact that drug use is a predicted value. For the panel data estimates, in which there are two correction terms and two predicted drug use measures, the generalized correction for heteroscedasticity suggested by White (1980) is used to estimate the variance-covariance matrix. The latter procedure remains incorrect, but it should represent an improvement over OLS estimates.

[5]To test the restrictions implied by equation (1c), I performed a series of Wald tests. The Wald test is appropriate in this case because the F-test assumes homoscedasticity. The variance-covariance matrix used for the Wald test incorporated the White (1980) correction for heteroscedasticity, and is only an approximation of the true estimate.

Although not all of the restrictions could be rejected, many were found to be invalid. In addition, the results of the Wald tests differed by gender, with more of the restrictions being rejected in the equations for women than in those for men.

[6]The drug use measures are discrete, categorical variables, and taking first differences of these measures would also result in a discrete measure. The most appropriate estimation procedure for categorical variables of this type is an ordered probit (logit) model (Sickles and Taubman 1991; Kaestner 1991). In the cross-sectional model, this methodology is rejected, due to the severe collinearity between the predicted drug use categories that are used in the two step procedure; a person with a high probability of being a heavy cocaine user also has a high probability of being a moderate cocaine user. In the panel data analysis, a fixed effects model of an ordered categorical variable would need to be estimated, and that task is beyond the scope of this paper (Chamberlain 1984).

facilitates estimation, since standard methods can be used to obtain the predicted values of the level of drug use that are used in the second stage of estimation. The predicted values of drug use will be derived from the reduced form estimates obtained from a model that includes all exogenous variables from both time periods.[7] Third, taking differences of any qualitative variable (such as marital status) results in an arbitrary ordering being imposed on the data, and using the levels avoids this problem.

In addition, the panel data estimates will be obtained from a sample of individuals employed in both of the available time periods. The exclusion of those who were not employed in both of the periods introduces a potential sample selection bias that I address by including separate correction terms associated with the labor force participation decision in each year. Using an unrestricted model also facilitates the incorporation of the sample selection terms, since these variables enter the model in levels. The reduced form labor force participation equation will also be obtained using all the exogenous variables from both time periods.[8]

A potential drawback of this procedure is that the time-varying variables are often highly collinear, and this collinearity will affect the precision with which the parameters are estimated. Alternative models that use the differenced form of the drug use variables are estimated to test this hypothesis as it pertains to the drug use variables, which are the variables of interest in this paper. In addition, several of the variables that are technically time-varying will be treated as time-invariant, since there is very little variation over time in these measures. These variables include the respondent's age, since all respondents aged about four years between the two surveys, and the respondent's region of residence.

## Data

The data used in the analysis come from the National Longitudinal Survey of Youth (Center for Human Resource Research 1990). In its starting year, 1979, the survey sample consisted of approximately 12,000 youths aged 14–21. The survey has been updated each year since 1979, with a broadening array of purposes and questions. The questions elicit detailed information on respondents' labor market experience, family and personal background, and illicit drug use. Central to the purposes of this paper are the questions related to the respondent's illicit drug use. In 1984, and again in 1988, respondents were asked questions about their lifetime and recent use of several illicit drugs, most notably marijuana and cocaine.[9]

Several selection criteria were established to eliminate sources of heterogeneity: the sample included only those respondents who were at least 18 years old in 1984, were living independently or with their parents, but not in jail or other temporary quarters (such as a dormitory), and who were not enrolled in school, in the military, or self-employed. In addition, those observations with missing data were deleted. These restrictions resulted in samples of approximately 7,800 individuals in 1984 and 7,200 in 1988. A matched sample of individuals present in both years, who numbered approximately 5,700, was also created. Definitions and descriptive statistics of the variables used in the analysis can be found in the appendix.

The illicit drug use questions are limited in two major respects. First, as was suggested in Mensch and Kandel (1988), there may be some under-reporting in the NLSY1984 data, particularly with regard to cocaine use. The exact nature of the under-reporting is not

[7]The standard errors reported in the text are from an OLS regression, and they ignore the fact that there are several predicted values among the right-hand-side variables.

[8]The labor force participation model is estimated separately for each year, but includes all the exogenous variables in the model. For example, the respondent's education in both of the years under consideration will be included in the estimates of the probability of working in period 1. This specification can be interpreted as a reduced form version of the Chamberlain (1980) random effects probit model, and thus accounts for unobserved heterogeneity in the selection equation.

[9]The 1988 NLSY survey limited the illicit drug use questions to include only marijuana and cocaine.

known, but Mensch and Kandel (1988) suggested that under-reporting is more common among relatively light users of illicit drugs than among heavier users, and more pronounced among women and minorities than among men and non-minorities. Although the present analysis accounts for a simple (that is, random) type of measurement error, under-reporting remains a potential problem.[10]

The second problem related to the drug use questions is the absence of a measure of quantity of use; only the frequency of drug use is measured. Although frequency and quantity have been shown to be highly correlated, the two measures are clearly not equivalent (Stein et al. 1988). In fact, Stein et al. (1988) reported finding that the quantity of drug use was a more powerful predictor of problems associated with illicit drug use than was frequency of use. In addition, the frequency of use was interval-coded with relatively large groupings (see Table 1).

Table 1 is a frequency distribution of illicit drug use for the sample under examination, and presents the unweighted data by gender. One finding of note in Table 1 is the relatively large increase between 1984 and 1988 in the percentage of respondents reporting some lifetime use of cocaine. In 1984, about 19.6% of the male sample and 12.7% of the female sample reported some prior cocaine use; by 1988 the respective figures were 32.6% and 21.2%. The observed increase in the initiation into cocaine use over this age range is consistent with previous studies (Kandel and Logan 1984; Raveis and Kandel 1987). The surprising finding is that the number of respondents who had used cocaine in the previous 30 days decreased between 1984 and 1988, whereas the number of people who had tried cocaine increased by approximately 66% over that period. The relatively high levels of lifetime cocaine use, compared to past 30 day use, imply that there were many individuals who experimented with cocaine but neither regularly used it nor became addicted to it.

Initiation into marijuana use largely ceased over the age range observed for this sample, as evidenced by the relatively small increase in the prevalence of lifetime marijuana use, and the dramatic decline in the number of respondents who reported using marijuana during the past 30 days, between 1984 and 1988.

The figures in Table 1 imply a general decline in illicit drug use consistent with the data from the recent National Institute on Drug Abuse (NIDA) household surveys. In general, marijuana use was much more common than cocaine use, and the proportion of users who reported relatively heavy marijuana use was much greater than the proportion reporting heavy cocaine use. It is also apparent that men had a greater frequency of use than women. Finally, the frequency distribution of illicit drug use for employed individuals (not shown) is very similar to that reported in Table 1 (Kaestner 1994).

The levels of reported drug use in the 1988 NLSY survey are very similar to those reported in the 1988 National Household Survey (NHS) on Drug Abuse (National Institute on Drug Abuse 1988). The sample of respondents used in this study had an age range of 23-32 in 1988, and 32.6% of the men in that sample reported having used cocaine at some time in their lives, slightly higher than the 32.3% figure reported in the NHS survey for a similarly aged (26–34) group of men.[11] 70.6% of the men in this sample reported having used marijuana at some time, compared to 68.1% of men in the NHS. Also consistent with the NHS results are the responses of women in the present sample. Of the women in this sample—who, like the men, were aged 23–32—21.2% reported having used cocaine at some time and 58.9%

[10]The empirical strategy is to use a Two Stage Least Squares (2SLS) estimation procedure, and thus the drug use measures will be replaced by their respective predicted values. This procedure is appropriate due to both the simultaneity and measurement error problems.

[11]The NHS numbers would be expected to be higher, since the NHS sample was somewhat older than the sample examined here, and therefore had a greater chance of having initiated use. In addition, the NLSY oversamples blacks and respondents from the South, two groups that have reported levels of illicit drug use below that of the entire population (Kozel and Adams 1985).

Table 1. Distribution of the Total Sample by Gender and Frequency of Drug Use.

| Year | Lifetime Frequency of Cocaine Use | Men N | Men % | Women N | Women % | Lifetime Frequency of Marijuana Use | Men N | Men % | Women N | Women % |
|---|---|---|---|---|---|---|---|---|---|---|
| 1984 | 0 | 2930 | 80.4 | 3655 | 87.3 | 0 | 1107 | 30.4 | 1804 | 43.1 |
| | 1–9 | 360 | 9.9 | 286 | 6.8 | 1–9 | 916 | 25.1 | 1161 | 27.7 |
| | 10–39 | 165 | 4.5 | 135 | 3.2 | 10–39 | 385 | 10.6 | 481 | 11.5 |
| | 40–99 | 93 | 2.6 | 60 | 1.4 | 40–99 | 356 | 9.8 | 274 | 6.5 |
| | 100–999 | 73 | 2.0 | 37 | 0.9 | 100–999 | 444 | 12.2 | 296 | 7.1 |
| | 1000+ | 23 | 0.6 | 15 | 0.4 | 1000+ | 436 | 12.0 | 172 | 4.1 |
| 1988 | 0 | 2204 | 67.4 | 3084 | 78.8 | 0 | 962 | 29.4 | 1608 | 41.1 |
| | 1–2 | 366 | 11.2 | 292 | 7.5 | 1–2 | 457 | 14.0 | 725 | 18.5 |
| | 3–9 | 249 | 7.6 | 222 | 5.7 | 3–9 | 433 | 13.2 | 451 | 11.5 |
| | 10–39 | 236 | 7.2 | 191 | 4.9 | 10–39 | 455 | 13.9 | 514 | 13.1 |
| | 40–99 | 106 | 3.2 | 72 | 1.8 | 40–99 | 271 | 8.3 | 227 | 5.8 |
| | 100+ | 109 | 3.3 | 51 | 1.3 | 100+ | 692 | 21.2 | 387 | 9.9 |
| **Year** | **Past 30 Day Frequency of Cocaine Use** | **Men N** | **Men %** | **Women N** | **Women %** | **Past 30 Day Frequency of Marijuana Use** | **Men N** | **Men %** | **Women N** | **Women %** |
| 1984 | 0 | 3458 | 94.9 | 4057 | 96.9 | 0 | 2622 | 72.0 | 3613 | 86.3 |
| | 1–2 | 89 | 2.4 | 71 | 1.7 | 1–2 | 243 | 6.7 | 200 | 4.8 |
| | 3–5 | 41 | 1.1 | 24 | 0.6 | 3–5 | 196 | 5.4 | 113 | 2.7 |
| | 6–9 | 30 | 0.8 | 17 | 0.4 | 6–9 | 158 | 4.3 | 78 | 1.9 |
| | 10–19 | 17 | 0.5 | 15 | 0.4 | 10–19 | 194 | 5.3 | 82 | 2.0 |
| | 20–39 | 7 | 0.2 | 2 | 0.0 | 20–39 | 130 | 3.6 | 61 | 1.5 |
| | 40+ | 2 | 0.1 | 2 | 0.0 | 40+ | 101 | 2.8 | 41 | 1.0 |
| 1988 | 0 | 3122 | 95.5 | 3818 | 97.6 | 0 | 2770 | 95.5 | 3590 | 91.8 |
| | 1–2 | 73 | 2.2 | 60 | 1.5 | 1–2 | 113 | 2.2 | 123 | 3.1 |
| | 3–5 | 33 | 1.0 | 22 | 0.6 | 3–5 | 104 | 1.0 | 64 | 1.6 |
| | 6–9 | 20 | 0.6 | 1 | 0.0 | 6–9 | 77 | 0.6 | 34 | 0.9 |
| | 10–19 | 12 | 0.4 | 7 | 0.2 | 10–19 | 102 | 0.4 | 41 | 1.0 |
| | 20–39 | 4 | 0.1 | 2 | 0.1 | 20–39 | 62 | 0.1 | 27 | 0.7 |
| | 40+ | 6 | 0.2 | 2 | 0.1 | 40+ | 42 | 0.2 | 33 | 0.8 |

*Source:* Figures are derived from the National Longitudinal Surveys of Labor Market Experience—Youth Cohort (Center for Human Resource Research 1990).

reported having used marijuana at some time, rates that closely match the corresponding figures of 21.0% and 56.2% for the 26–34-year-old women in the NHS. This finding raises questions about the extent of under-reporting in the NLSY, and particularly about whether there was in fact substantial under-reporting in 1984, as suggested by Mensch and Kandel (1988). Furthermore, Sickles and Taubman (1991) reported findings from an unpublished NLS study that suggest, contrary to Mensch and Kandel's criticism, that the self-reports in the NLSY are reliable.

For both marijuana and cocaine, five separate measures of drug use were used in the cross-sectional analyses: a linear measure of lifetime use that takes on values of 0–5, corresponding to the categories in Table 1; a linear measure of lifetime use that uses the midpoints of the categories observed in Table 1; two similar measures for past 30 day use; and a dummy variable indicating a relatively heavy amount of lifetime use and non-zero past 30 day use.[12] It should be noted that the intervals used to code the illicit drug use responses

[12]Heavy use of cocaine is defined as lifetime use of 40 or more times, and heavy use of marijuana is defined as lifetime use of 100 or more times.

changed between the two surveys. The estimates for the drug use equations were obtained by OLS methods for the linear measures, and by a probit regression model for the dummy variable indicating heavy use.

For the longitudinal models, the coding intervals used to group the data according to lifetime drug use were standardized between the two years, and only the simple (nonmidpoint) linear measures were used in the analysis.[13] These variables took on values from 0 to 4, indicating the following frequencies of use: 0, 1–9, 10–39, 40–99, and 100 or more times. In addition, a differenced version of this linear lifetime measure was created, and this variable took on values ranging from 0, for no change in use, to 4, for changes in reported lifetime use exceeding 99 (that is, in effect, reports of 100 or more instances of use in the past four years). Since lifetime drug use cannot decline, all analyses were estimated twice—once with consistency of response imposed on the data by using the 1984 value if the value reported in 1988 was below that in 1984, and a second time with no such adjustment of contradictory values. The results do not differ according to which method is used, and the results reported in the text are for data in which consistency of the response has been imposed.

As noted above, the grouped nature of the data limits the value of this differenced variable, since much of the variation in use over time is unobservable. For example, individuals who were in the highest category of use in 1984 will never be observed to have an increase in use. To better differentiate between types of users, two additional variables were created to be used in conjunction with this differenced measure of lifetime use. The first is a dummy variable indicating no reported use in either survey, and the second is a dummy variable indicating initiation into use between the two surveys.[14] Measures created for past 30 day drug use were similar, except that the two additional dummy variables were not used in the analysis of the effect of recent drug use.

### Cross-Sectional Results

One purpose of this paper is to use the 1988 NLSY survey year information to update the previous cross-sectional estimates of the effect of illicit drug use on wages. The model used to generate these estimates closely follows that in my 1991 study (Kaestner 1991), with only a few minor modifications.[15] Table 2 lists the parameter estimates of the effect of illicit drug use on the wage, and a full set of estimates is provided in the appendix. Five separate models were estimated for each gender group and both years of data. For example, lifetime cocaine use and past 30 day cocaine use were not entered into the same model, due to the high degree of collinearity between the two predicted measures of illicit drug use. The same reasoning would also apply to the other models in which separate drug use measures were entered.

The parameter estimates associated with the 1988 survey year data are generally similar to those for 1984. In fact, the estimates of the effect of illicit drug use on the wage are large, positive, and frequently significant in both years, across both gender groups, and for both types of drugs. The only negative effect observed among the estimates is associated with heavy cocaine use (as defined in this paper) among the 1988 female sample. This coefficient has a very low level of significance, and its importance should be dis-

[13]The large intervals used in the NLSY make it difficult to use midpoints. There is little information on the nature of the true distribution of drug users within intervals, and any estimate of the mean within the interval would be ad hoc. In addition, estimating the mean of the open-ended interval would also be error-ridden. Because of these concerns, I used the untransformed data.

[14]The predicted values of these variables are obtained by maximum likelihood probit methods. The probits are estimated independently of each other.

[15]The differences are related to aspects of the sample, exogenous variables, and estimation strategy. First, in this study, individuals living in temporary quarters have been deleted, as have individuals with missing information related to the measure of self-esteem. Next, age and AFQT test score enter the model as quadratics. Finally, as mentioned in note 3, the reduced form drug estimates were obtained using the entire sample, as opposed to only the employed.

Table 2. Cross-Sectional Estimates of the Effect of Illicit Drug Use on the Natural Logarithm of the Wage. (Standard Errors in Parentheses)

| | | 1984 | | 1988 | |
|---|---|---|---|---|---|
| *Drug Type/Model* | | *Men* | *Women* | *Men* | *Women* |
| | | **Marijuana Use** | | | |
| 1 | Lifetime Use | .042** | .022 | .031 | .044* |
| | (0,1,2,3,4,5) | (.022) | (.024) | (.028) | (.028) |
| 2 | Lifetime Use | .0002** | .0002 | .001 | .002 |
| | (midpoints) | (.0001) | (.0002) | (.001) | (.001) |
| 3 | Heavy Use | .161 | .166 | .424** | .246 |
| | (0,1) | (.123) | (.173) | (.192) | (.286) |
| 4 | Past 30 Day Use | .065* | .052 | .034 | .153 |
| | (0,1,2,3,4,5,6) | (.003) | (.047) | (.060) | (.122) |
| 5 | Past 30 Day Use | .011+ | .010 | .006 | .012 |
| | (midpoints) | (.006) | (.008) | (.012) | (.017) |
| | | **Cocaine Use** | | | |
| 1 | Lifetime Use | .096* | .124* | .046 | .153*** |
| | (0,1,2,3,4,5) | (.052) | (.066) | (.044) | (.051) |
| 2 | Lifetime Use | .001* | .002** | .003 | .010** |
| | (midpoints) | (.001) | (.001) | (.002) | (.004) |
| 3 | Heavy Use | .012 | .449 | 1.033** | –.361 |
| | (0,1) | (.276) | (.364) | (.418) | (.509) |
| 4 | Past 30 Day Use | .313** | .515** | .315 | .493 |
| | (0,1,2,3,4,5,6) | (.142) | (.227) | (.221) | (.573) |
| 5 | Past 30 Day Use | .098** | .169** | .077* | .055 |
| | (midpoints) | (.046) | (.079) | (.048) | (.144) |
| Observations | | 2852 | 2619 | 2907 | 2724 |

*Notes:* In all models the actual value of drug use is replaced by its predicted value. In models 1, 2, 4, and 5, the prediction method was an OLS regression. In model 3, a probit procedure was utilized, and the dummy variable was replaced by the predicted probability. All models include the inverse mills ratio associated with the Heckman two step sample selection correction.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

counted accordingly. The coefficient on heavy cocaine use for men in 1988, however, has a somewhat unbelievably large, positive effect, which is statistically significant. In general, the results listed in Table 2 contradict prior expectations regarding the effect of illicit drug use on the wage, and are consistent with results that have been previously reported. The results for the 1984 survey data are virtually the same as those I reported three years ago (Kaestner 1991), in the study that provides the framework for the analysis used here.

Respondents in the cross-sectional sample from 1988 were, on average, four years older than those in the 1984 sample. Therefore, if the detrimental effects of drug use are cumulative, the potential for observing a negative relationship between illicit drug use and wages should be greater in 1988 than in 1984. The results reported above, however, do not support this hypothesis. For men, the magnitude and significance of the results tend to diminish somewhat across years, but for women the opposite is true. Moreover, the changes in magnitude may be explained by slight differences between the units of measurement of the drug use variables in the two survey years. On the other hand, drug use may have adverse effects on wages that become noticeable only over a period longer than four years.

### Fixed Effect Estimates

The cross-sectional results are surprising, and most observers would probably consider them inadequate as a description of the true relationship between drug use and wages. The cross-sectional estimates are questionable primarily due to the possibility that there are unobserved characteristics that have been omitted from the analysis that are correlated with both drug use and the wage. To address this problem, I created a limited panel of data consisting of a matched sample of respondents present in both years and implemented a fixed effects estimator. The model used to generate these estimates is specified above, and is a relatively unrestrictive version of a model of first differences.

Before examining the fixed effects estimates, note that the cross-sectional models of Table 2 were re-estimated using the matched sample. If attrition in the sample is an important source of potential bias, the cross-sectional results using the matched sample should differ from those reported in Table 2. On the other hand, if attrition is not a problem, the estimates from a cross-sectional analysis should be similar. In fact, when the cross-sectional models were re-estimated on the matched sample, the results (not shown) differed very little. The signs associated with the drug use coefficients were identical, and the magnitudes of the drug effects were very close to those reported in Table 2. The standard errors associated with the estimates, however, were larger, and the effects were therefore reduced in significance.

There were two exceptions to these generalizations, and both involved the effect of past 30 day marijuana use on the female wage. In 1984 and 1988 the estimates of this effect were substantially smaller for the matched sample than for the full sample. Thus, to the extent that the cross-sectional and panel data estimates differ, that difference does not stem from changes in the sample, except possibly for the effect of past 30 day marijuana use on the wages of women.

Table 3 lists the parameter estimates associated with the illicit drug use measures; a complete set of results is contained in the appendix. For simplicity, the discussion of the results will be in terms of the effect of illicit drug use on the level of the wage, even though the underlying model is of wage changes.

*Drug use by men.* All of the estimates of the effect of illicit drug use on wages of men are negative. They are not statistically significant, however, and the large size of the standard errors associated with the parameter estimates underscores the need to approach these estimates cautiously. The magnitudes of these estimates are substantial, but they are difficult to interpret given the way illicit drug use is measured. For example, a one-unit increase in cocaine use over the preceding four-year period would be expected to reduce an individual's wage anywhere from 2% (model 2) to 22% (model 3 for a new user), and a one-unit increase in marijuana use would be expected to reduce the wage by between 9% (model 2) and 52% (model 3 for a new user).

The problem lies in the interpretion of what is meant by a one-unit increase, since the groupings into drug use categories were somewhat irregular. The specification of the illicit drug use variables in model 3 helps clarify the interpretation. For both marijuana and cocaine, an increase in use that is also associated with an individual's initiation into use has a more adverse impact on an individual's wage than an increase in use for a previous user.[16] In the case of cocaine, approximately 80% of all observed increases in use—531 male respondents, or 21.3% of the total male sample—were for people initiating use, with over half of these cases having a total reported use of only 1 to 9 times over the four-year period. For marijuana, approximately 40% of the observed increases in use—589 respondents, or 23.6% of the total male sample—were for individuals who initiated use during this period, and about a third of these cases were individuals who reported use of only 1 to 9 times during the period.[17]

[16]The coefficients on the change in use and initiation into use variables should be considered in an additive fashion when deriving the total wage effect of initiation into use. The total effect is not, however, the simple sum of the two estimates.

[17]The figures on initiation into use of marijuana that are reported by the matched sample indicate that more

Table 3. Fixed Effects Estimates of the Effect of Illicit Drug Use on the Natural Logarithm of the Wage. (Standard Errors in Parentheses)

| Drug Type/Model | | Cocaine Use: Men | Cocaine Use: Women | Marijuana Use: Men | Marijuana Use: Women |
|---|---|---|---|---|---|
| 1 | Lifetime Use 1984 (0,1,2,3,4) | –.224<br>(.428) | .828*<br>(.428) | –.079<br>(.284) | –.277<br>(.277) |
| | Lifetime Use 1988 (0,1,2,3,4) | –.137<br>(.275) | .607*<br>(.316) | –.086<br>(.219) | –.254<br>(.271) |
| 2 | Change in Lifetime Use, 1988–84 | –.024<br>(.155) | .276<br>(.223) | –.093<br>(.208) | –.250<br>(.270) |
| 3 | Change in Lifetime Use, 1988–84 | –.183<br>(.317) | .538<br>(.380) | –.032<br>(.219) | –.129<br>(.399) |
| | New User | –.225<br>(.586) | .748<br>(.687) | –.523<br>(.530) | –.812<br>(.928) |
| | Never Used | –.274<br>(.338) | .521**<br>(.255) | .130<br>(.238) | –.159<br>(.206) |
| 4 | Past 30 Day Use, 1984 (0,1,2,3,4,5,6) | –.106<br>(.467) | .159<br>(.343) | –.096<br>(.105) | .024<br>(.130) |
| | Past 30 Day Use, 1988 (0,1,2,3,4,5,6) | –.094<br>(.405) | .525<br>(.700) | –.186<br>(.152) | .086<br>(.252) |
| 5 | Change in Past 30 Day Use, 1988–84 | –.101<br>(.409) | .166<br>(.343) | –.051<br>(.106) | –.002<br>(.112) |
| Observations | | 1858 | 1623 | 1858 | 1623 |

*Notes:* In all models the actual value of drug use is replaced by its predicted value, and the prediction method was an OLS regression. Note that the signs associated with the 1984 drug variable coefficients have been reversed, since the regression package assumes that the model is additive. All models include the inverse mills ratio for each year.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

It is not known whether the negative effect associated with the initiation measure of drug use is due primarily to the phenomenon of initiation itself, or to initiation into heavy use over a relatively short period of time. If the latter reason accounts for the negative effect, we would expect this effect to be greater in the case of marijuana, since a larger proportion of the individuals who started using marijuana than of those who started using cocaine became relatively heavy users. This expectation is in fact supported, as illustrated in Table 3.

There is, however, one anomalous result regarding the male sample. The wages of those men who never used cocaine is expected to be 27% lower than the wages of similar individuals who previously used cocaine, but did not increase their use during the four-year period between 1984 and 1988. Since most individuals reported a low level of lifetime use, this result implies that individuals who experimented with cocaine fared better than those who did not. Initiation into cocaine use over this period, however, does have a negative effect on the wage, as does an increase in use. Thus, those individuals who tried cocaine while relatively young, but did not increase their use afterward, are expected to have the highest wage, even compared to non-users.

*Drug use by women.* The findings for the female sample are qualitatively different from those for the male sample. Cocaine use appears to have had a large positive effect on women's wages. A one unit increase in cocaine use increased the wage by between 28% (model 2) and 75% (model 3, new user). These effects are extremely large, and in some cases reach commonly accepted levels of significance. The results obtain both for

initiation into marijuana use took place than is implied in Table 1.

lifetime cocaine use and for past 30 day cocaine use. The model 3 estimates for women differ sharply from those for the male sample. These estimates suggest that the women who tried cocaine at a relatively young age tended to have a lower wage than women who either never used cocaine or initiated use between 1984 and 1988. Over 83% of all observed increases in use—447 respondents, or 14.1% of the total female sample—were individuals who first tried cocaine during this period, and over 60% of these individuals reported lifetime use of 1–9 times by 1988. Thus, very few of the observed increases in use are among relatively heavy users. Initiation into cocaine use during this period did not adversely affect women's wages.

The effect of marijuana use was negative for the female sample, although past 30 day marijuana use does have a positive coefficient. As was the case for the other estimates reported in Table 3, the standard errors associated with these estimates are large, and result in relatively low levels of significance.

## Conclusion

This study has had two purposes: first, to update previous cross-sectional estimates of the effect of illicit drug use on the wage, using data from the 1988 wave of the NLSY; and second, using both the 1984 and 1988 NLSY, to provide a set of longitudinal estimates of the effect of illicit drug use on the wage. The longitudinal estimates are preferred, since in theory this methodology controls for potentially important unobserved individual characteristics that cause the cross-sectional estimates to be biased. I performed both analyses separately by gender.

In regard to the first objective, my estimates of the effect of illicit drug use on the wage using the 1988 NLSY data are consistent with those I obtained in 1991 using the 1984 NLSY data. Both sets of estimates indicate large, positive, statistically significant effects of illicit drug use on the wage, for both gender groups and for both marijuana and cocaine use.

These findings raise several disturbing questions related to drug prevention policy. Much time and money has been and continues to be invested in deterring and preventing drug use, and this investment is based on the proposition that illicit drug use adversely affects users. Few economists would be willing to argue that illicit drug use is somehow beneficial for young people, and is a characteristic that is rewarded in the labor market. Thus, an explanation of the cross-sectional results that would confirm our commonly held beliefs would be comforting. The most commonly invoked explanation of that kind has been that important unobserved characteristics that are positively correlated with both wages and illicit drug use underlie the empirical relationship found in cross-sectional models.

The longitudinal estimates presented in this paper, however, provide only partial support for that explanation. To some extent, the large standard errors associated with the fixed effects estimates can be interpreted to mean that there are really no effects of illicit drug use on the wage that are significantly different from zero. Unfortunately, the standard that must be imposed to draw that conclusion may be too stringent for this type of empirical analysis. If the sign and magnitude of the effects of illicit drug use on the wage are examined, the evidence is mixed. Among the male sample, illicit drug use tended to be negatively related to wages—a finding in accord with most people's expectations, and one that would support continued vigilance against drug use. Similarly, among the female sample, lifetime marijuana use appears to have been negatively associated with the wage; but recent marijuana use (use within the previous 30 days) had a positive effect, and, more important, cocaine use (both recent and lifetime) was positively related to the wage, and had quite a large impact.

There are two possible explanations for the results reported in this paper. The mixed nature of the preferred estimates—those obtained using the longitudinal data—implies that there is a wide range of wage effects among people who consume the same amount of drugs. Thus, illicit drug use may be a highly idiosyncratic phenomenon that has a variety of consequences that depend on the individual, the type of drug, or some combination of the two. The adverse physical and psychological consequences of drug use are known to be quite individual-specific, and a given level of use may therefore lead to a wide variety of labor market outcomes. Another possibility is that some drug users choose jobs in which their drug use has the least impact on their productivity, but others do not. In addition, the drug use measures incorporated in the analysis were extremely crude, and exactly what they measure may differ drastically from person to person. These considerations may explain why the observed wage effects of illicit drug use differ for men and women and are influenced by the type of drug and timing of its use.

The second possible explanation for the results obtained in this paper is that the current analysis, although a significant improvement on past work, is still inadequate. The fixed effects methodology used in this paper controls for characteristics that do not change over time, but among a sample of young adults in their twenties, there may be few characteristics that remain constant. These shortcomings of the analysis point to refinements that should be made in future studies to allow the identification of important personality traits and patterns of drug use that affect the wage.

I believe this study has taken an important new step in the analysis of the effect of illicit drug use. It is the first analysis to exploit longitudinal data, and therefore controls for unobserved person-specific effects that are important determinants of the wage. It has provided some important insight into the effect that these unobserved factors have on the relationship between illicit drug use and wages, and it has provided a foundation for future work on that subject.

**APPENDIX TABLE 1**

**DESCRIPTIVE STATISTICS OF VARIABLES USED IN THE ANALYSIS**

| | *Men, 1988* | | *Women, 1988* | | *Men, 1984* | | *Women, 1988* | |
|---|---|---|---|---|---|---|---|---|
| *Variable* | *Mean* | *Std. Dev.* | *Mean* | *Std. Dev.* | *Mean* | *Std. Dev.* | *Mean* | *Std. Dev.* |
| Age | 27.420 | 2.240 | 27.580 | 2.268 | 23.270 | 2.257 | 23.33 | 2.219 |
| Black | 0.260 | 0.439 | 0.271 | 0.445 | 0.250 | 0.433 | 0.252 | 0.434 |
| Hispanic | 0.164 | 0.370 | 0.154 | 0.361 | 0.158 | 0.365 | 0.157 | 0.364 |
| AFQT | 64.300 | 23.330 | 65.100 | 20.950 | 63.670 | 23.220 | 65.190 | 21.110 |
| Experience | 7.379 | 2.804 | 6.131 | 3.228 | 3.860 | 2.010 | 3.265 | 2.143 |
| Education | 12.680 | 2.394 | 12.780 | 2.266 | 12.170 | 2.058 | 12.370 | 2.036 |
| No. Life Employers | 7.430 | 4.174 | 6.269 | 3.945 | 4.889 | 3.000 | 4.099 | 2.782 |
| No. Children 0–2 | 0.162 | 0.401 | 0.203 | 0.426 | 0.115 | 0.342 | 0.227 | 0.460 |
| No. Children 3–5 | 0.185 | 0.450 | 0.331 | 0.559 | 0.087 | 0.328 | 0.245 | 0.495 |
| No. Children 6–18 | 0.187 | 0.537 | 0.513 | 0.853 | 0.039 | 0.244 | 0.144 | 0.452 |
| Health | 0.040 | 0.196 | 0.051 | 0.220 | 0.033 | 0.180 | 0.050 | 0.217 |
| Never Married | 0.455 | 0.498 | 0.328 | 0.470 | 0.670 | 0.470 | 0.505 | 0.500 |
| Sep-Divorce | 0.105 | 0.306 | 0.163 | 0.370 | 0.053 | 0.225 | 0.101 | 0.301 |
| Religious Attendance | 3.111 | 1.679 | 3.419 | 1.696 | 2.983 | 1.655 | 3.358 | 1.687 |
| Self-Esteem | 32.360 | 4.095 | 31.980 | 4.124 | 32.370 | 4.039 | 32.060 | 4.156 |
| Rotter Index | 8.684 | 2.371 | 8.820 | 2.396 | 8.654 | 2.389 | 8.827 | 2.407 |
| No. Illegal Acts | 11.800 | 31.250 | 5.051 | 18.260 | 12.750 | 36.400 | 4.949 | 17.870 |
| Two Parents at Age 14 | 0.775 | 0.418 | 0.748 | 0.434 | 0.773 | 0.419 | 0.756 | 0.430 |
| Live at Home | 0.213 | 0.409 | 0.139 | 0.346 | 0.455 | 0.498 | 0.324 | 0.468 |
| Non-Earned Income | 7645 | 13033 | 13500 | 16335 | 10057 | 14360 | 11613 | 13599 |
| Missing Income | 0.184 | 0.388 | 0.156 | 0.363 | 0.185 | 0.389 | 0.160 | 0.367 |
| Urban | 0.792 | 0.406 | 0.781 | 0.413 | 0.784 | 0.411 | 0.796 | 0.403 |
| Northeast | 0.184 | 0.388 | 0.178 | 0.382 | 0.185 | 0.389 | 0.176 | 0.381 |
| North-Central | 0.243 | 0.429 | 0.236 | 0.425 | 0.250 | 0.433 | 0.234 | 0.423 |
| South | 0.378 | 0.485 | 0.403 | 0.490 | 0.364 | 0.481 | 0.397 | 0.489 |
| Observations | 3270 | | 3912 | | 3644 | | 4188 | |

*Definitions: Age:* age in years. *Black:* indicates respondent is black. *Hisp:* indicates respondent is hispanic. *AFQT:* combined score on armed forces qualifications test. *Experience:* actual years of labor market experience. *Education:* highest grade completed. *No. Life Employers:* number of previous employers. *No. Children 0–2:* number of dependent children less than 3 years of age. *No. Children 3–5:* number of dependent children 3-5 years of age. *No. Children 6–18:* number of dependent children 6-17 years of age. *Health:* indicates respondent is limited in activity. *Never Married:* indicates respondent was never married. *Sep-Divorce:* indicates respondent is separated or divorced. *Religious Attendance:* frequency of religious attendance as of 1980. *Self-Esteem:* psychological scale measuring feelings of self-worth. *Rotter Index:* psychological scale measuring a person's locus of control. *No. Illegal Acts:* number of illegal acts respondent reported in 1980. *Two Parents at Age 14:* respondent had two-parent household at age 14. *Live at Home:* respondent resides in family home. *Non-Earned Income:* all non-earned income of respondent's household. *Missing Income:* indicates income variable is missing. *Urban:* indicates respondent lives in urban area. *Northeast:* indicates respondent lives in Northeast region. *North-Central:* indicates respondent lives in North-Central region. *South:* indicates respondent lives in the South.

**APPENDIX TABLE 2**

**CROSS-SECTIONAL PARAMETER ESTIMATES FROM THE WAGE MODEL**

| | Men, 1988 | | Women, 1988 | |
|---|---|---|---|---|
| *Variable* | *Coefficient* | *Std. Error* | *Coefficient* | *Std. Error* |
| Constant | 1.721 | 1.594 | 0.628 | 0.505 |
| Age | −0.033 | 0.117 | 0.085 | 0.032 |
| Age Squared | 0.000 | 0.002 | −0.002 | 0.001 |
| Black | −0.024 | 0.027 | 0.061 | 0.029 |
| Hispanic | 0.023 | 0.028 | 0.090 | 0.031 |
| AFQT | 0.004 | 0.004 | −0.003 | 0.004 |
| AFQT Squared | −0.000 | 0.000 | 0.000 | 0.000 |
| Experience | 0.106 | 0.046 | −0.005 | 0.035 |
| Experience Squared | −0.004 | 0.002 | 0.001 | 0.001 |
| Education | −0.019 | 0.036 | −0.050 | 0.035 |
| Education Squared | 0.003 | 0.002 | 0.002 | 0.002 |
| Exper × Education | −0.000 | 0.002 | 0.003 | 0.002 |
| AFQT × Education | −0.000 | 0.000 | 0.001 | 0.000 |
| No. Life Employers | −0.016 | 0.003 | −0.021 | 0.004 |
| Health | −0.067 | 0.050 | −0.037 | 0.043 |
| Never Married | −0.111 | 0.025 | −0.033 | 0.028 |
| Sep–Divorce | −0.103 | 0.037 | 0.004 | 0.032 |
| Self-Esteem | 0.006 | 0.003 | 0.007 | 0.002 |
| Rotter Index | −0.007 | 0.004 | −0.006 | 0.004 |
| Two Parents at Age 14 | 0.001 | 0.023 | 0.013 | 0.021 |
| Live at Home | −0.136 | 0.031 | −0.121 | 0.027 |
| Urban | 0.122 | 0.078 | 0.159 | 0.084 |
| Northeast | 0.033 | 0.096 | 0.192 | 0.101 |
| North–Central | −0.117 | 0.089 | 0.038 | 0.092 |
| South | −0.025 | 0.083 | 0.005 | 0.087 |
| Urban × Northeast | 0.073 | 0.100 | −0.090 | 0.105 |
| Urban × South | −0.084 | 0.084 | −0.058 | 0.089 |
| Urban × North–Central | 0.064 | 0.089 | −0.103 | 0.094 |
| Lifetime Cocaine | 0.046 | 0.044 | 0.153 | 0.051 |
| Inverse Mills Ratio | 0.273 | 0.161 | −0.118 | 0.079 |
| Adjusted R–Square | .239 | | .293 | |
| Observations | 2907 | | 2724 | |

*Notes:* Estimates from other models are available from the author upon request. The reported standard errors are only approximate, since they do not take into account the predicted nature of illicit drug use.

**APPENDIX TABLE 3**

**PARAMETER ESTIMATES FROM THE FIXED EFFECT MODEL OF WAGES**

| | *Men* | | *Women* | |
|---|---|---|---|---|
| *Variable* | *Coefficient* | *Std. Error* | *Coefficient* | *Std. Error* |
| Constant | 0.301 | 1.734 | 2.301 | 1.529 |
| Black | –0.084 | 0.051 | 0.038 | 0.043 |
| Hispanic | 0.042 | 0.066 | 0.144 | 0.062 |
| AFQT | –0.000 | 0.001 | 0.001 | 0.001 |
| Self-Esteem | –0.004 | 0.004 | 0.005 | 0.003 |
| Rotter Index | –0.007 | 0.006 | 0.008 | 0.006 |
| Two Parents at Age 14 | –0.095 | 0.038 | 0.017 | 0.029 |
| Age | –0.037 | 0.161 | –0.231 | 0.128 |
| Age Squared | 0.000 | 0.003 | 0.004 | 0.003 |
| Education 1984 | –0.143 | 0.197 | –0.017 | 0.108 |
| Education Sq 1984 | 0.007 | 0.007 | 0.002 | 0.004 |
| Education 1988 | –0.137 | 0.172 | –0.019 | 0.109 |
| Education Sq 1988 | 0.008 | 0.006 | 0.003 | 0.004 |
| Experience 1984 | 0.003 | 0.001 | 0.004 | 0.001 |
| Experience Sq 1984 | 0.000 | 0.000 | –0.000 | 0.000 |
| Experience 1988 | 0.003 | 0.002 | 0.001 | 0.002 |
| Experience Sq 1988 | 0.000 | 0.000 | 0.000 | 0.000 |
| No. Life Employers 1984 | 0.017 | 0.013 | –0.013 | 0.013 |
| No. Life Employers 1988 | 0.014 | 0.010 | –0.007 | 0.010 |
| Urban | 0.111 | 0.101 | 0.183 | 0.096 |
| Northeast | 0.130 | 0.124 | 0.495 | 0.132 |
| North–Central | 0.156 | 0.111 | –0.278 | 0.116 |
| South | 0.100 | 0.118 | 0.246 | 0.100 |
| Urban × Northeast | –0.061 | 0.123 | –0.379 | 0.130 |
| Urban × North–Central | –0.158 | 0.106 | –0.253 | 0.113 |
| Urban × South | –0.138 | 0.120 | –0.238 | 0.099 |
| Live at Home 1984 | –0.036 | 0.032 | –0.027 | 0.032 |
| Live at Home 1988 | –0.080 | 0.039 | –0.132 | 0.038 |
| Never Married 1984 | –0.028 | 0.042 | –0.062 | 0.051 |
| Sep–Divorce 1984 | 0.115 | 0.067 | 0.039 | 0.048 |
| Never Married 1988 | –0.020 | 0.042 | –0.072 | 0.053 |
| Sep–Divorce 1988 | –0.044 | 0.083 | 0.125 | 0.061 |
| Health 1984 | 0.059 | 0.070 | –0.084 | 0.064 |
| Health 1988 | –0.041 | 0.050 | –0.053 | 0.064 |
| Lifetime Cocaine 1984 | –0.224 | 0.428 | 0.828 | 0.428 |
| Lifetime Cocaine 1988 | –0.137 | 0.275 | 0.607 | 0.316 |
| Inverse Mills Ratio 1988 | 0.489 | 0.270 | 0.162 | 0.161 |
| Inverse Mills Ratio 1984 | –0.008 | 0.171 | –0.095 | 0.105 |
| Adjusted R–Square | .078 | | .079 | |
| Observations | 1858 | | 1623 | |

*Notes:* Estimates from other models are available upon request from the author. The standard errors reported are based on the method proposed by White (1980).

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