Employer Drug Screening and Employment Outcomes

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March 2008

Preliminary – Please Do Not Cite without Author's Permission

Abstract

Beginning the in 1980s, US employers began drug testing employees and job applicants in large numbers. Today, 50% of employees in the US work for firms that conduct some form of drug testing, and 80% of these screen new hires. This paper investigates the labor market impacts of this large policy change. I incorporate drug testing into a standard Roy model of labor market sorting and derive a limited set of predictions concerning sorting across the testing and non-testing sectors. I then identify three key periods in the life of this policy: an early period in which testing was rare (the pre-period), a transition period, and the current high-testing period (the post-period). Using Current Population Survey microdata spanning 1980 to 1999, I test the model's predictions empirically and extend the analysis to dimensions on which the model is silent. Consistent with the model's predictions, I find that groups with high use rates are underrepresented in the testing sector prior to testing and that employment of non-users increased in the testing sector following the advent of drug testing. I also find that average log wages fell in the testing sector in the post-period and that they rose in the non-testing sector. Finally, I find a number of large and significant changes in relative labor market outcomes across demographic groups, particularly for youth, minorities, and less skilled workers. (JEL Codes: J31, J38, J32, J71, M5)

I thank Princeton University for financial support during the writing of this paper. I am particularly indebted to Shawn Moulton for his outstanding research assistance throughout this project. I also thank Giselle Kolenic for her excellent work on the early stages of this project. This paper has benefited from the comments of seminar participants at the University of Notre Dame and the University of Connecticut.

I. **Introduction**

In the 1980s, U.S. employers began requiring drug tests of their employees and job applicants on a large scale. Writing in their comprehensive 1994 report on workplace drug testing, the National Research Council remarks that "[i]n a period of about 20 years, urine testing has moved from identifying a few individuals with major criminal or health problems to generalized programs that touch the lives of millions of citizens. It has given rise to a[n]… industry that was unimagined just 10 years ago. Tens of millions of urine specimens are analyzed every year…" (National Research Council, 1994, p. 180). According to a nationally representative survey conducted semi-annually by the U.S. Department of Health and Human Services, 50% of employees in the U.S. now work for firms that conduct some form of applicant or employee drug testing.[1] These numbers make employer drug testing arguably the largest demand side intervention in the U.S. drug market.

The advent of employer drug testing also constitutes a large scale intervention in the U.S. labor market. Despite the large size of this intervention, its labor market effects have not yet been evaluated. Moreover, there are reasons to believe that the effects of employer drug testing differed across segments of the labor force. Employers in all major industrial sectors practice testing, but it is most common in manufacturing, transportation, and sectors with dangerous work (Hartwell et al. 1996). Drug usage rates also differ dramatically across demographic groups. Young workers are several times more likely to have used drugs in the last month than older workers. Usage rates among men are about twice those of women, and blacks are more likely to use than whites (NSDUH, author's calculations). If drug users are slow or unable to eliminate drug use during likely testing periods, then differences in testing intensity across industries combined with differences in usage rates across groups could result in a new sorting of users and non-users across industries and employment states.

[1] This is the National Survey on Drug Use and Health (NSDUH), formerly called the National Household Survey on Drug Abuse.

Drug testing in the labor market takes several forms: testing of applicants for a job; testing employees for cause—such as following an accident or on suspicions of drug use; as part of a regular testing program or routine medical exam; and random testing of employees.[2] Of these, testing job applicants is by far the most common form of employer drug screening. Few firms have testing programs that exclude this component, and many firms only test applicants. A Conference Board Survey found that among firms that test in some fashion, 92% do job applicant testing (Conference Board, 1990). Among individuals reporting that their employer drug tests in the HHS data cited above, 80% report that their firm conducts pre-employment testing. The consequences for testing positive are also relatively severe for job applicants. Testing firms in the Conference Board survey reported that a positive drug test "virtually guarantees" an applicant will not be hired. Of these, a quarter bar applicants who fail the test from any future employment with the firm. The remaining three-quarters require a waiting period of several months before reapplication. This contrasts with the treatment of current employees who test positive. These individuals are typically sent for counseling and re-tested at a later date. It is rare for an employee to be fired after only one positive test.[3]

In this paper, I incorporate drug testing by firms and drug use by workers into a standard Roy model and derive implications for how the introduction of drug testing may impact the sorting of workers from different demographic groups into testing and non-testing sectors. I combine information from Bureau of Labor Statistics surveys, the National Survey on Drug Use and Health, and the Current Population Survey (CPS) to provide empirical evidence on the model's predictions as well as on dimensions that are outside the scope of the model. I provide reduced-form estimates

[2] Employers are much less likely to test for alcohol except in situations where testing follows an accident or other precipitating cause.
[3] This is due at least in part to legal protections that are extended to employees but not applicants. (National Research Council, 1994, Appendix A.) An exception is the military, which has a zero-tolerance drug policy explored in Mehay and Pacula (1999).

of the impact of three periods of employer drug testing on labor market outcomes. Using CPS microdata spanning 1980 to 1999, I examine changes in outcomes within and across demographic groups and industries as drug testing prevalence increased nationally.

The results suggest that employer drug testing has had a complex impact on the U.S. labor market. Consistent with one of the model's predictions, I find that a number of demographic groups with low drug use rates are overrepresented in the testing sector prior to the introduction of testing. I find that log wages decline in the testing sector (high testing industries or jobs) and rise in the non-testing sector after the introduction of testing, although changes in either direction are possible within the model. Conversely, wage variance rose in the testing sector and fell in the non-testing sector. I also provide evidence that testing led to a number of changes in relative outcomes across demographic groups, although for the time being the model is silent on the direction relative changes should take. I find that testing improved employment and wages for black youth, and improved access to jobs in high testing industries for less skilled white men. While youth employment overall declined under testing, I find that the youth who are employed find better quality jobs. On the other hand, employment of Hispanics in high testing industries and high quality jobs has declined, as have their wages.

II. Background on Drug Testing, Drug Use and Employer Screening

A. Three Periods of Employer Drug Testing

Drug testing differs from other forms of employer screening of job applicants in that it requires the collection and analysis of a physical specimen. In almost all cases, this involves the collection of a urine specimen by a third party within a specified time frame after receiving a job offer.[4] The most common testing kits screen for 5 to 10 different types of drugs, including opiates, cocaine, marijuana, PCP, and amphetamines. These also include the active ingredients in prescription painkillers, for which applicants may be required to provide a doctor's verification that these have indeed been prescribed. A drug test "failure" typically requires a positive result at both the initial screening phase and in a second, confirmatory test of the same specimen, usually conducted by a specialty lab using more sophisticated measures.[5] Contrary to some popular claims, the tests used in the initial screening phase have low rates of false positives—about 2%. The confirmatory tests are highly accurate and are not considered subject to false positives or false negatives.[6]

A bigger concern for employers is the rate of false negatives in the screening phase. While it is true that an industry has evolved to help individuals pass drug tests, the main threats to test validity are high rates of false negatives that occur even in the absence of evasion efforts by tested individuals.[7] False negative rates average 20% over the five main drug classes but are highest for marijuana—over 40%(U.S. Department of Justice, 1991).[8] However, a large number of false negatives are due to generous cutoff levels established by the National Institutes on Drug Abuse rather than to technological limitations in the screening methods.[9] A second significant source of false negatives is lax oversight in testing facilities. A government study found numerous lapses in

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[4] Drug tests using other specimens, including blood and hair, are available but almost all employers use urinalysis as their mode of testing. Many employers outsource this collection and analysis to third party firms, but some larger employers have in-house medical departments who conduct the tests.

[5] Roughly 70% of employers order a confirmatory test in the event of a positive initial screen (Conference Board, 1990).

[6] The Supreme Court has ruled that the gas chromatography and mass spectrometry (GC/MS) procedures used in these second tests are highly accurate and admissible as evidence (Tunnell, 2004).

[7] Most efforts to substitute a urine specimen or to supply one that has been adulterated in order to conceal drug use could be easily detected by monitors at the collection site. (National Research Council, Ch.6, 1994.)

[8] DOJ sampled over 2400 individuals held in the California criminal justice system, who presumably have little access to "masking compounds" and other evasion techniques. Using the confirmatory GC/MS procedures to establish a sample's true drug content, the DOJ researchers evaluated the accuracy of several standard screening tests. The experiment found high rates of false negatives among samples known to be drug-positive.

[9] These cutoffs are binding for federally mandated testing programs, but non-mandated private employers are not bound by them. The National Research Council report notes that detection rates are higher among firms not bound by NIDA guidelines (Ch. 3, 1994).

testing protocol at collection sites for the federally-mandated DOT drug testing program, suggesting that cheating is indeed possible at many of these facilities (Government Accountability Office, 2007).

The arrival of drug testing in the labor market in the early 1980s was driven by a combination of three factors: a small number of somewhat sensational workplace accidents in which drugs were found to have played a role; the development of accurate and relatively inexpensive screening devices; and rising public anxiety over the prevalence of drugs in society, which in turn led to the creation of federal incentives for workplace drug testing. [10] Table 1 lists major legal and policy developments involving workplace drug testing. One of the first private sector employers to do so was Greyhound Buslines, in 1983. It was in this same time period that Federal Railroad Administration and the U.S. Customs Service also instituted regular drug testing of their employees. Both were sued, and the conditions under which employers could require drug tests were highly uncertain until both lawsuits were resolved in the Supreme Court in 1989. The constitutionality of testing was particularly unclear in cases where the state was the employer or the tests were legally mandated. The early 1980s were thus a period in which small numbers of employers—albeit typically large ones—began requiring drug tests of their employees in an atmosphere of uncertainty. A 1990 Conference Board survey of large firms found that of those that tested employees, 12% had been sued over the practice and another 24% had been required to engage in arbitration. Employers who were smaller, averse to legal action, or for whom the costs of employee drug use were less significant were unlikely to have instituted testing in a period in which the legality of such practices was highly uncertain.

[10] Facts in this paragraph are taken from Tunnell (2004), Ch. 1; National Research Council (1994) Ch. 6 and Appendix A. Prior to the 1980s, only the military had instituted a drug testing policy for its employees. Even this was not comprehensive; rather the military required only that soldiers pass a drug test before they would be sent home from Vietnam (Tunnell, 2004). The Navy began widespread drug testing in 1982 with other branches following shortly thereafter.

By the late 1980s, the rights of employers to conduct blanket testing of job applicants and a number of conditions under which they could require current employees to undergo testing had been established in the courts. Examining Table 1 it is clear that by the 1990s, the major cases related to drug testing concerned limits on the right of the government to test its employees and the expansion of testing into other spheres, such as schools.[11] During the late 1980s, states also began to pass guidelines regulating the use of testing (DeBernardo and Nieman, 2006; National Research Council, 1994). Such guidelines further clarified the legal environment facing employers.[12] Finally, in 1987, Ronald Regan signed an executive order requiring that federal agencies adopt testing to establish "drug free workplaces." The 1988 Drug Free Workplace Act went further, requiring that federal contractors adopt comprehensive anti-drug policies. Employee and applicant drug testing was clearly in the spirit of this legislation. Thus, the late 1980s constitute a turning point after which employers begin implementing drug testing programs in increasing numbers.[13]

Recognizing increasing employer interest in these tests, the Bureau of Labor Statistics (BLS) conducted a survey in 1988 to gauge the extent of drug testing practices among U.S. employers (U.S. Department of Labor, 1989).[14] Unfortunately, the survey data themselves are not publicly available. The main findings of the report are summarized in Table 2, in the column headed "1988." A follow up to the BLS survey was conducted by outside researchers in 1993 (Hartwell, et. al. 1996). The

[11] The courts have made clear that the right of the government to conduct testing is regulated by the Fourth Amendment, which applies to government mandated employee drug testing as well as forms such as pre-bail drug testing of criminal defendants. This is in contrast to the ability of non-mandated, private sector employers to test which is not constitutionally limited.

[12] Unfortunately, these state level policies do not provide useful identifying variation in this context. For one, the guidelines do not mandate that employers drug test. They simply clarify the conditions under which employers can test. This has an ambiguous effect on testing. On the one hand, less uncertainty about the legality of testing would likely encourage it. On the other hand, employers in states with guidelines may have felt greater legal scrutiny than those in states with no guidelines, thereby discouraging testing. Consistent with this, the Department of Labor Survey (1989) reports that employers were less likely to test in states with guidelines but more likely to be considering testing.

[13] 1988 is also the year that the nation's largest manufacturer of employer drug tests, Quest Diagnostics, begins reporting its "Drug Testing Index," in which they report annual percentages of positive drug tests in their labs.

[14] The sampling units in the BLS survey were establishments, rather than firms, but the results are largely generalizable to firms. BLS conducts quarterly surveys of U.S. employment establishments and has well developed procedures for generating representative samples of establishments.

main findings of that report are summarized in the column headed "1993." A number of regularities in drug testing prevalence are apparent in both surveys. Larger employers are more likely to test than smaller employers; there is wide variation in rates of establishment testing across industries; and there is variation across regions of the U.S., with larger shares of establishments testing in the South and Midwest than in the Northeast or West.[15]

Comparing the two columns, it is obvious that the share of testing employers increased dramatically in the period between the surveys. This is particularly true of smaller establishments. The share of those with fewer than 250 employees testing rose roughly three-fold, and establishments with 250-1000 employees doubled their rates of testing. Even the largest establishment category, those with 1000+ employees, increased its testing share from roughly 50% to 70%. Direct comparisons of shares in the industry and region cells are somewhat misleading due to changes in the sampled universe across the surveys.[16] According to Hartwell, however, the share of establishments with 50 or more employees testing in 1988 was 0.16. This rose to 0.48 by 1993, or a three-fold increase for this group overall.

There has been no follow up to the 1993 survey, but comparable statistics can be computed using the National Survey on Drug Use and Health (NSDUH).[17] The NSDUH began surveying households in 1979, but questions about the drug testing policies of a respondent's employer were first included in 1997. The questions are then asked in every survey wave until 2006, with only two exceptions. To best match the establishment data, I calculated the shares of employed respondents replying that their employer practiced some form of drug testing. The final column of Table 2

[15] The regional differences in testing seem likely to be related to differences in industry composition across states, but without the underlying survey data I cannot test this directly.

[16] In the 1993 survey, the sample was limited to establishments with 50 or more employees. Since small employers are much less likely to test (as is obvious in the 1988 figures), increases in the shares of testing employers by industry and region are driven in part by this sample adjustment.

[17] A 1995 survey by Hartwell and coauthors asked employers about alcohol testing but included some questions about drug testing policies (Hartwell et al. 1998).

reports these shares overall and by industry.[18] In both cases, the NSDUH shares indicate that drug testing increased either not at all or only very modestly in the period following the 1993 survey. Closer inspection of the shares at an annual frequency shows that they are highly stable over the 1997 to 2006 period (See Figure 1). The rapid expansion of employer drug testing appears to have ended in the early 1990s as testing stabilized at this new, higher level.

The most accurate publicly available data on changing drug testing practices is that collected in Table 2.[19] Based on this, and the policy history in Table 1, I divide my data into three periods: pre-1988, 1988-1993, and 1994 onwards. The first period corresponds to the low testing regime. Employer drug testing was not entirely absent in this period, but its practice had not begun to approach the levels of the commonplace that it would several years later. The second period covers the years of rapid increase in the prevalence of employer testing, and the third period represents the new, high-testing regime. I discuss the empirical methods I use to identify the effects of expanding drug testing in the next section.

B. Patterns of Drug Use

In contrast to problems with measuring the intensity of drug testing by employers, accurate measures of drug use are available back to 1979 in the NSDUH. The NSDUH also provides an additional picture of the prevalence of employer drug testing over the last decade. Figure 1 shows that 50% of employed 22-49 year olds in the NSDUH work for employers who conduct some form of drug testing, and 80% of those work for employers who test job applicants. These figures align well with those in the second wave of establishment data and confirm that testing rates are highly stable over this period.

[18] The BLS surveys omitted establishments in the agriculture and government sectors. Industry testing rates can be calculated for these in the NSDUH.
[19] Restricted use or privately held data may provide annual information on testing rates at the subnational level. I am investigating access to this type of data.

Figures 2 through 6 show the main patterns of drug use in the U.S. population.[20] Most figures focus on marijuana use, since this is the drug most commonly detected in positive drug screens (Tunnell, 2004; Quest Diagnostics, 2008). Figure 2 shows that past-month marijuana use among 22-49 year olds closely tracks past-year use, and differences in these levels are comparatively modest. About 17% of this population used marijuana in the past year, and about 11% in the last month. Note that among chronic users, marijuana can be detected in standard urine tests up to roughly a month after the last use.

Figures 3a and b compare race/ethnicity and gender group use patterns across two age groups, ages 18-21 and 22-49. Use rates for all groups were stable or declining over the 1990s but increasing since 2000. Despite these long-run trends, there are stable group differences in marijuana use over the entire post-1987 period. The biggest difference is across genders—use rates among men are generally nearly double that for women. The other major demographic difference in use rates is across age groups. Women ages 18-21 are about twice as likely to have used marijuana in the past month than women ages 22-49. Among young men, about 25% report using marijuana in the past month. That figure is only 15% among men ages 22-49. Racial differences in use rates are not nearly as large. Among the older age group, blacks are somewhat more likely to use than whites, but the difference is not large. This difference is actually reversed in the younger group.

Figures 4a and b show marijuana use rates by education group and age. As we have already seen, levels of marijuana use are much higher for younger respondents. Among the younger group, educational differences in use rates are less pronounced than among older workers, suggesting that use rates decline more for more educated individuals as they age.[21] Figure 4b shows that the education differential is largest between college graduates and all other education levels in the older

[20] Prior to 1987, the NSDUH was conducted at intervals of several years and sampled a much smaller number of individuals than in later years.
[21] The noisy measures for college graduate use rates in the 18-21 year old age group are due to the fact that very few individuals in this group have completed college.

group of workers. Interestingly, this differential only emerges after 1990 but is stable thereafter, even during the period of rising use rates after 2000.

Figures 5a and b show that marijuana use rates among the unemployed are roughly double those of the employed. Use among the employed declined in the 1990s but increased after 2000, following the general pattern, while rates among the unemployed were fairly stable. Finally, Figures 6a and b show use rates (in the past month) for other illicit drugs and misuse of legal narcotics. The detection window for these drugs in urine is only a few days to a week after the last use. The use of these drugs is much more stable over the period of the data, particularly in the pre-2000 period. While use of these drugs did increase somewhat after 2000, following the same pattern as marijuana use in this period, the post-1979 decline is totally absent from this class of drugs. By the end of the data, use of non-marijuana drugs occurs at only somewhat lower rates than marijuana, although at the outset of the survey use of marijuana far outpaced that of other drugs.

Data on drug test failures are less readily available than those on drug use. The main source of publicly available information on drug test failure rates comes from Quest Diagnostics, a general medical testing company that is one of the nation's largest suppliers of drug test kits and urinalysis services. In 1988, Quest began publishing drug test positivity rates on an annual basis in their Drug Testing Index. The data of course do not represent a random sample, and it is important to note that the index makes no adjustments for changes in Quest's client base.

Nevertheless, the index makes several important points. First, the number of tests performed in the U.S. annually is very large. Quest reports conducting 8.4 million tests in 2007, and this only represents a share of all tests performed nationally. Second, rates of drug test failure are non-zero. The overall failure rate was 3.8% in 2007, with slightly higher rates among job applicants (as opposed to testing of current employees) and in jobs where testing was not federally mandated for safety reasons (Quest Diagnostics, 2008). Figure 7 shows that there is also considerable geographic variation in failure rates, with the worst-performing areas on this measure reporting failure rates in the range of 5.5-16% in 2007. Finally, the data suggest a substantial decline in the rates of positivity. In 1988, the index reports a failure rate of 13.6%. This declined to 8.8% by 1991 and to 5.0% by 1997. Failure rates then declined steadily, reaching a low of 3.8% in 2007. These figures match up well with a limited number of comparable statistics in published reports (National Research Council, 1994). Because of their limited and non-representative nature, the fact that failure rates are declining in these data does not imply either that drug use is declining or that detection rates are falling.

C. A Roy Model of the Employment Effects of Industry Drug Testing

In this section, I incorporate drug use on the part of workers and drug testing on the part of firms into a standard, two-sector Roy model as developed in Heckman and Sedlacek (1985) and Heckman and Honore (1990). A Roy model is applicable to this setting, and it generates some clear predictions that might not be obvious ex ante.

Suppose firms can be divided into two sectors, the testing sector and the non-testing sector, so named because of the practices they will adopt when drug testing becomes available. (I briefly postpone discussion of why one sector and not the other would adopt testing.) Individuals are endowed with a vector of sector-specific skills $\mathbf{s} = (s_T, s_N)$, denoting skills in the testing and non-testing sectors, respectively. Workers can apply for employment in either sector and move between them costlessly at any time.

The key modification that I make to the standard Roy model is to assume that testing sector skills are negatively affected by an individual's drug use. For simplicity, I assume that drug use sets testing sector skills to zero, so that $\mathbf{s}$ becomes the following:[22]

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[22] This simplification is similar to a more general specification: $s_T(D_i) = s_T - D_i f(s_T)$ where $f' > 0, f'' > 0, \lim_{\mu \to -\infty} f = 0, and\ \lim_{\mu \to \infty} f = \infty$. In both cases the absolute productivity loss from drug use is larger for more able individuals and becomes negligible toward the very bottom of the productivity distribution. It is also similar to

(1) $\boldsymbol{s} = (s_T, s_N; D_i) = \begin{pmatrix} \begin{cases} s_T \text{ if } D_i=0 \\ 0 \text{ if } D_i=1 \end{cases} \\ s_N \end{pmatrix}$

Drug use D_i is unobservable before the advent of testing. I also assume that it is independent of $\mathbf{s}$.[23] Testing sector firms anticipate that the total output yield from hiring a given set of workers—some of whom use drugs—is lower than it would be if there was no drug use. Since firms have no information about which hires are more likely to use drugs, they simply deflate offered wages by a constant probability of drug use. Thus testing sector firms offer wages equal to an applicant's expected marginal productivity given the possibility of drug use: $w_T = k_T(1-p)s_T$ where $k_T(1-p) = \pi_T(p)$ and p is the rate of drug use in the population.[24] Non-testing firms offer wages equal to expected (and realized) marginal productivity: $w_N = \pi_N s_N$ where π_N is a constant. $\pi_T(p)$ and π_N are then the sector-specific skill prices in a standard Roy model.

Assuming workers choose their sector of employment to maximize wages, the size of the testing sector is determined by the following:

(2) $p(T) = \Pr(\pi_T(p)s_T \geq \pi_N s_N)$

I assume that wages in the two sectors are log-normally distributed.[25] As a result, (2) becomes the following:

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assuming that drug use is associated with a small probability of a large productivity loss such as that caused by a serious workplace accident or a large theft from the firm, which could be expressed $s_T = s_T + D_i * \varepsilon * loss$.

[23] While this is certainly a assumption, the limited evidence available suggests that detecting drug use from information other than drug tests is extremely difficult. Other methods of ascertaining drug use among job applicants without resorting to drug tests (using detailed personality testing targeted to detect drug use) have been found to have fairly low correlations with actual use and high rates of false positives (National Research Council, Ch. 6, 1994). If drug use were closely related to underlying skills, we might expect alternative methods of detecting it to prove more useful. Also, the Conference Board study reports that supervisors are commonly advised not to try to guess at drug use among their employees but rather to look for specific changes in performance before ordering testing (Conference Board, 1990).

[24] This assumes that total output is a function of the sum of individual worker productivities and does not otherwise depend on their combination. If testing sector firms have market power while the non-testing sector is perfectly competitive, this can provide a rationale for the adoption of testing in the former. Firms with market power make some positive profits from each non-using worker and would therefore like to screen out drug users. Assuming that testing sector firms have market power would not substantively alter the conclusions of the model and would be consistent with the evidence on firm size and industry mix of testing versus non-testing firms in Table 2.

[25] Heckman and Honore (1990) show that the main results of the (log-normal) Roy model are robust to the less restrictive assumption of log concavity in $\varepsilon_T - \varepsilon_N$.

(3) $p(T) = \Pr(\ln k_T + \ln(1-p) + \mu_T + \varepsilon_T) \geq \ln \pi_N + \mu_N + \varepsilon_N)$,

where $\ln s_j \sim N(\mu_j, \sigma_j)$ so that $\ln s_j = \mu_j + \varepsilon_j$ for j = T, N. Note that at this point an individual's drug use does not affect the wages he expects to receive in either sector since only population drug use is relevant for wage setting in the testing sector.

Drug testing introduces a signal into this environment. Following what is known about the validity of drug tests, I assume that firms who require drug tests of their applicants receive a signal t_i of drug use with the following properties[26]:

(4) $$t_i = 1 \Rightarrow D_i = 1$$
$$t_i = 0 \Longrightarrow E(D_i \mid post\ testing\ regime) = \tilde{p}$$

Of course, $E(D_i \mid pre\ testing\ regime) = p$. I assume detection is independent of **s** within the using population that is tested, and that a constant fraction δ of users is detected by the tests. For any applicant population with a given use rate (i.e. drug use patterns are not affected by the introduction of testing), increasing the probability of detection has an unambiguously negative effect on the probability that a hired applicant is a drug user. To see this, let N_0 denote the number of non-users in the population; N_1 is the number of users. For $\delta, \delta' \epsilon [0,1]$ and

$p(\delta) = {(1-\delta)N_1}\big/{N_0 + (1-\delta)N_1}$, simple algebra shows that $p(\delta) > p(\delta') \ \forall \ \delta < \delta'$, which would imply $p > \tilde{p}$.

It is, however, possible that changes in the applicant pool may change the drug use rate in this population such that $p < \tilde{p}$. This is possible even if drug use in the underlying population from which applicants are drawn is unchanged. This is because the introduction of testing affects the drug user's sector choice in an ambiguous way. Drug users now enter the testing sector only if the following inequality holds:

[26] These are consistent with low rates of false positives and high rates of false negatives in the drug screens commonly used by employers.

(5) $(1-\delta)[\ln k_T + \ln(1-\tilde{p}) + \mu_T + \varepsilon_T)] \geq \ln \pi_N + \mu_N + \varepsilon_N$

This is just the post-testing version of Equation (3) for drug users. The change from p to $\tilde{p}$ increases the term in brackets relative to the term on the right side of the inequality, but the addition of the (1-δ) term reduces the bracketed term in a way that makes the overall change in the share of drug users applying to the testing sector ambiguous. For non-users, the post-testing inequality omits the (1-δ) term since they face no possibility of detection and therefore earn the bracketed term with certainty if they enter the testing sector.

More formally, the assumption of log normality provides an explicit expression for the employment share of the testing sector for the two groups (Heckman and Sedlacek, 1985). For non-users:

(6) $pr(T) = P(\ln w_T \geq \ln w_N) = \Phi(c_T)$

$$where\ c_T = \frac{[\ln \frac{\pi_T(p)}{\pi_N} + \mu_T - \mu_N]}{\sigma^*}$$

$$and\ \sigma^* = \sqrt{var(\varepsilon_T - \varepsilon_N)}$$

For this non-users, the introduction of testing raises $\pi_T(p)$ and leaves all other terms unchanged, thereby unambiguously increasing $pr(T)$.

For users, the c_T term becomes the following:

(7) $c_T = \frac{[(1-\delta)\ln k_T + (1-\delta)\ln(1-\tilde{p}) + (1-\delta)\mu_T - \ln \pi_N - \mu_N]}{\sigma_U^*}$

For users, the change in the numerator from the pre- to post-testing state is ambiguous, consistent with the intuition already described. The ambiguity in the share of users selecting the testing sector following the policy change means that the change in the overall share of the population selecting the testing sector is also ambiguous.

Now suppose that in addition to $\mathbf{s}$ and D_i, individuals possess an observable characteristic M_i which takes the values 0 and 1, representing demographic groups. I assume that the distribution of $\mathbf{s}$ does not vary across the M groups.[27]

Rates of drug use differ across demographic groups, and firms are aware of these population differences in use.

(8) $E(D_i|M_i = 1) = p_{M1} > p_{M0} = E(D_i|M_i = 0)$

This in turn implies that expected productivity conditional on an individual's observed M differs across groups, even though the underlying productivity distribution is the same. Firms in the testing sector will therefore offer higher wages to members of group M_0 than they will to those of M_1 conditional on the individuals having the same s_T. Using the formula in (6), it is clear that these differences in use rates imply that $\Pr(T|M_i = 1) < \Pr(T|M_i = 0)$ prior to testing. This means that the testing sector share of employment will be lower in group M_1 than M_0 in the pre-testing period, even if there are no underlying productivity differences across the groups.

The model so far has provided a limited set of predictions: (1) the share of non-users employed in the testing sector should increase after the advent of testing; and (2) the testing sector shares of employment should be lower for groups with higher drug use rates in the pre-testing period. Both of these are readily testable.

Even with the assumption of log normality in wages, the Roy model is unable to generate unambiguous predictions about post-testing changes in several quantities of interest. These include the mean and variance of log wages within sectors and demographic groups. The ambiguous effect of a single-sector price change on these quantities is apparent in the formulas for them provided in Heckman and Sedlacek (1985). In the standard model, additional assumptions are required about the

[27] See Autor and Scarborough (2008) for a discussion of evidence that the variance of productivity does not differ empirically across racial groups. They make the same assumption about variance in their model. The assumption that the mean of productivity is invariant across groups can easily be relaxed at the expense of some of the testable implications that follow.

covariance of the disturbance terms to generate clear predictions. The ambiguity is compounded in the drug testing setting because the price change induced by testing is not equal across the using and non-using segments of the population, and therefore even the size of the testing sector is unclear without additional assumptions about how the skill price change and detection jointly affect the sectoral choices of drug users.

Rather than add assumptions to the model, I turn to empirical analysis to determine the changes in a number of quantities on which the model is currently unable to provide clear guidance.

III. Assessing the Impact of Pre-Employment Drug Testing

A. Microdata Sources

I draw on microdata from two sources. The bulk of the analysis uses microdata on individuals ages 18 to 55 from the IPUMS versions of the March Current Population Surveys.[28] The March CPS surveys, as is widely known, contain the richest set of employment variables in the monthly CPS. The resulting data set includes representative, annual cross sections of prime aged individuals in the U.S. population over the period 1980 to 1999. I truncate the data set in 1999 because the industry variable that I use to classify workers into high and low testing industries undergoes a significant change in coding the following year. A concern with using the CPS data over this period is the major redesign of the survey that was implemented in 1994. I find, however, that this is unlikely to cause problems for my analysis. See Appendix A for a discussion.

I supplement the analysis with data from the NSDUH. The NSDUH is a survey of a nationally representative sample of individuals aged 12 and older first conducted in 1979. It is currently conducted annually although the survey was semi-annual between 1979 and 1987. The sample size has increased considerably over the years. The 1979 sample contained roughly 7200

[28] King et. al. (2004), on the web at cps.ipums.org/cps.

individuals and grew to include over 55,000 individuals in 2006.[29] It is the definitive source of data on drug use in a representative population.[30] The NSDUH contains detailed information on respondent drug use histories (using primarily retrospective questions) and, in later years, on employer drug testing practices.[31] Both have already been documented in the figures. All NSDUH analysis and statistics are unweighted. While the NSDUH contains rich information on drug use, I rely primarily on the CPS for several reasons. First, the NSDUH was conducted only semi-annually and on much smaller samples for the entire pre-period making it difficult to construct the non-linear time trends I use as controls. Second, it is not possible to construct exact hourly wages from NSDUH data. Finally, the NSDUH does not include any geographic identifiers, which prevents the inclusion of geographic controls and, more unfortunately, any study of geographic differences in testing intensity among employers.

Descriptive statistics on the CPS sample are given in Table 3. Race/ethnicity is measured using indicators for Black and Hispanic. Other non-white races are not separately identified in the CPS until the latter part of my sample period. As a result, the omitted race/ethnicity category in most specifications is properly called "whites, Asians and Native Americans," although I will refer to the group simply as "whites." I also create a dummy variable to indicate young workers, those ages 18-25; these constitute nearly a quarter of the sample. Education is measured using four categories: high school dropouts; high school graduates; those with 1-3 years of post-secondary education (some college); and college graduates. Table 3 shows the share of the sample in the latter two groups, combined.[32]

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[29] The growth in sample was non-monotonic; for example, the smallest cross-section (the 1994 survey) contained only 4300 observations.

[30] The NLSY79 asks about drug use but only surveys a limited set of cohorts.

[31] Beginning in 2005, the survey adopted a partial rotating panel, which should allow for construction of some drug use measures using longitudinal information rather than respondent recall.

[32] I do not include marital status in any of the specifications since it is likely endogenous to education and age.

Table 3 also summarizes a number of employment outcomes. Employment itself is measured as a dummy variable indicating employment at the time of the March survey. Its mean gives the rate of employment in the population overall. For employed workers, I also observe their industry of employment, and this is the basis of the high testing industry employment variable.[33] I classify workers as employed in a high testing industry if they worked in mining, transportation, communications and utilities, government or wholesale trade. These industries all had establishment testing rates of over 50% in the 1993 BLS survey. The table shows that the high testing sector employs about 30% of currently employed workers when defined at the industry level. For comparison, I also use an alternative, job-level (industry x occupation cell) definition of the high testing sector in some estimates. The NSDUH data allows me to calculate employer drug testing rates for 150 jobs over the 1998-2006 period.[34] I then matched these to the CPS data over the entire 1980-1999 period on the basis of industry and occupation and defined the high testing sector to be jobs with above sample median testing rates. The mean of this measure (not reported) is higher than the industry-only measure, at 0.47 conditional on employment.

Hourly wages are constructed by dividing wage and salary income earned last year by the product of weeks worked last year and usual weekly hours. Wages are adjusted to 1990 levels using the CPI-U. For individuals who worked at all in the preceding year, I observe pension and group health plan coverage. I classify workers in this universe as either covered or not covered by these benefits.[35] Coverage rates for both benefits are somewhat higher than 50% among individuals who worked in the previous year.

[33] The universe for the industry variable is actually workers who worked at any time in the last five years. I limit this to workers who were employed at the time of the survey.
[34] 10 industry categories by 15 occupation categories.
[35] The universe of the group health questions changed over time, and the wording of the questions was modified slightly. It is possible to adjust the coding of the group health coverage variable in the IPUMS data (INCLUGH) to account for the universe changes over time. It is not possible to correct for changes in the question wording. However, the question becomes somewhat more selective over time in terms of who is classified as having group health coverage, suggesting that trends in later periods should be toward decreasing coverage.

B. Estimating Equations

A lack of publicly available data on the prevalence of drug screening at the sub-national level, together with the absence of meaningful state regulation of testing, present significant challenges to the evaluation of this policy. The three periods of employer drug testing—low, transition, and high—offer a way forward. I create dummy variables for the three phases of drug testing history, with *p8893* and *p9499* indicating the transition (1988 to 1993) and high (1994 to 1999) testing periods, respectively. 1980 to 1987 is the omitted period, or pre-period.

The low and high testing periods correspond to the model's pre- and post-testing regimes, and high and low testing sectors are defined above. These components, together with microdata on demographics and employment outcomes, supply the empirical dimensions necessary for testing the predictions of the model. The main estimating equation I use for these tests is the following:

Eqn. 1

$$y_{ist} = \alpha_1 \Gamma_{\mathrm{ist}} + \alpha_2 \tilde{\Gamma}_{ist} p8893 + \alpha_3 \tilde{\Gamma}_{ist} p9499 + \alpha_4 \tilde{\Gamma}_{ist} t + \alpha_5 \tilde{\Gamma}_{ist} t^2$$
$$+ \alpha_{61} p8893 + \alpha_{62} p9499 + \theta_s + \varepsilon_{ist}$$

y_{ist} represents an individual level outcome. Γ is a vector of individual demographic characteristics (Black, Hispanic, female, age, age-squared and four education dummies); $\tilde{\Gamma}$ is the same except age is entered as only a dummy variable for ages 18-25; *t* is a linear time trend. The interactions $\tilde{\Gamma}$of with the quadratic allow for non-linear trends in testing sector employment that are specific to each demographic group. *p8893* and *p9499* are dummy variables for the transition and post-testing periods, respectively, and θ_s is a set of state of residence fixed effects.

I first assess the model's prediction that the share of non-users employed in the high testing sector should increase after the introduction of testing. I use a variant of Equation 1 that includes a indicator variable for non-users (equal to one if *i* did not use any illicit drug in the past month) in the Γ vectors. The model predicts that the interaction of non-use with *p8893* and *p9499* will be positive.

I examine the model's second prediction—that the likelihood of employment in the testing sector should be lower in the pre-period for groups with high rates of drug use—by estimating Equation 1 with y_{ist} equal to the testing sector employment dummy. In this case, α_1 gives the relative probabilities that members of different demographic groups are employed in that sector in the pre-period. The model predicts negative coefficients for groups with higher relative use rates.

I also use the specification in Equation 1 to assess the impact of drug testing on a range of outcomes for which the theoretical model provides no clear predictions. First, I look for changes in the association of personal characteristics with employment outcomes in the post-testing periods relative to the pre-period. α_2, α_3 and α_6 contain the main coefficients of interest here. The α_6 coefficients indicate the average change in the outcome variable that accompanied the transition and high testing periods. The α_2 and α_3 coefficients show how this change differed across demographic and education groups in the transition and high testing periods, respectively.

Second, I estimate Equation 1 for several dependent variables that are not part of the theoretical model. These include a general employment dummy variable and dummies for group health plan and pension coverage. The last two are meant to measure employer or job quality. They also proxy for employer size. Table 2 showed a clear relationship between employer size and the likelihood of drug testing. Unfortunately, the March CPS does not ask about firm size until 1988, so it is impossible to compare the sorting of workers across firm sizes pre- and post-drug testing. Instead, I examine how inclusion in these two benefit categories changes over the three periods. Since health and pension coverage are more likely to be offered by larger employers with more developed human resources departments, I consider these useful measures of how sorting of workers across high and low testing (and possibly "good" and "bad") employers may have changed with the advent of drug testing. I estimate Equation 1 as a probit for these four left hand side variables. I also estimate Equation 1 as a log hourly wage equation.

α_4 and α_5 are coefficients on demographic and education group specific quadratic time trends. These are important controls, although to conserve space their coefficients will not be reported. The nature of the historical variation in testing prevalence does not allow me to control flexibly for background labor market trends using year dummies. Instead, I include a quadratic time trend and its interactions with all demographic and education groups to account for smooth changes in the labor market outcomes for all groups under study. Compared to other trends in the labor market during this time—like the eroding minimum wage and changing skill demands—changes in drug testing prevalence were relatively discrete. This is particularly true of the pre-testing period, when testing rates were very low and stable, and the high-testing periods, when rates were high and stable. Thus we should expect the effects of rising levels of drug testing to appear as period effects, rather than smooth trends. Moreover, the major changes in the U.S. wage structure that occurred over the 1980s and 1990s are fairly well-approximated by group specific quadratics (Katz and Murphy, 1992; Autor, Katz and Kearney, 2008). This increases our confidence that any separate period effects are related to the advent of widespread employee and applicant drug testing.

An important non-linearity that is not likely to be well-approximated by the quadratics is cyclical changes in the labor market over this period. Appendix Figure B graphs the annual unemployment rate for the sample. It is obvious that there are major business cycle movements in each of the three time periods under study. These are unlikely to "cancel out" in the analysis—for example, the pre- and transition periods both contain a recession and recovery, but the post period only contains a recovery. (The pre-period recession is also more severe than the transition period recession.) Most worrying for the purposes of this study is the fact that the effects of business cycle fluctuations are much more severe for some groups of workers than for others, particularly for blacks, women, young workers and less educated workers (Clark and Summers, 1980). It is therefore possible that the estimated group specific period effects from Equation 1 will be confounded by business cycle variation across the periods.

I deal with this problem in two ways. First, in my analysis of the results from Equation 1, I impose a high bar for concluding that a given change in labor market outcomes can be attributed to changing drug testing policy. Specifically, I require that the period effects in both the transition and high-testing period have significant, same direction coefficients. That is, α_2 and α_3 must both be significant for a given demographic group. Second, I re-estimate Equation 1 including the unemployment rate as a control as well as its interaction with all demographic and education groups.

IV. Results and Robustness Checks

A. Empirical Tests of the Roy Model Predictions

Table 4 tests the first of the model's predictions: that the share of non-users employed in the testing sector should increase after the introduction of testing. It presents results from probit models in which the dependent variable is employment in the high testing sector, conditional on being employed. These models are estimated using NSDUH data, since this is the only source of information on whether an individual worker is a drug user. The column of the table shows that non-users were more likely than users to be employed in the testing sector in the pre-period. Coefficient on the interactions of non-users with the post-period dummies shows that, consistent with the model's prediction, employment of non-users increased in the testing sector following the introduction of testing.[36] The increase was substantial. The probability of testing sector employment rose by roughly 5 percentage points. I conclude that the first prediction of the model is supported by the data: the share of non-users employed in the testing sector increases after testing is introduced. The remaining columns estimate the model separately for four demographic groups and add

[36] Industry was not reported in the NSDUH in the first two waves, so the pre-period data consist only of observations from the 1982 and 1985 waves.

interactions of non-user with gender and age, plus triple interactions of these variables with the post-period dummies. These estimates suggest that the increased testing sector employment of non-users in the post-periods is driven by increased employment of non-using blacks and non-using white and Hispanic youth in that sector. The model is silent on how testing sector employment should change for users, but coefficients on the period dummies alone in the second and third columns of Table 4 show that it increased after the introduction of testing with this change largely coming from increased employment of less educated whites.

Table 6 shows results from estimating the probit and OLS models of Equation 1 using the full CPS sample from Table 3. The model in the second column of Table 6 provides the second empirical test of the model's predictions: demographic groups with lower drug use rates should be relatively more likely to be employed in the testing sector in the pre-testing period. The column shows estimates from a probit model of employment in a high testing industry. The main effects from this model (not reported in the table) indicate whether testing sector employment differed significantly across demographic groups in the pre-period. The relevant coefficients and standard errors are the following: 0.023 (0.005) on Black; 0.050 (0.005) on Hispanic; 0.025 (0.001) on Age; -0.169 (0.002) on Female; -0.008 (0.002) on Dropout; -0.061 (0.002) on Some College; and -0.164 (0.003) on College Graduate. All are significant at the 1% level or better.

A number of the results accord with the model's predictions. Older workers, who have lower drug use rates, are much more likely to be employed in the high testing sector prior to the introduction of testing. Hispanics are also more likely to be in the high testing sector, and dropouts less likely, which accords with the relative drug use rates in these groups. Other results do not line up with the model's predictions. In particular, college graduates and women have much lower drug use rates than less educated workers and men, respectively, but both are markedly less likely to be employed in the high testing sector. Blacks have use rates that are fairly similar to those of whites, but they are significantly more likely to work in the testing sector although the difference is small. Drug use differences are of course not the only factor driving the sorting of demographic groups across sectors. But the model does provide a rationale for higher testing sector employment of some groups—particularly older and Hispanic workers.

B. The Impact of Employer Drug Testing on Other Outcomes

Average within-sector wages and their variance are central to the Roy model, although it makes no clear prediction about how a change in relative skill price will affect those quantities. It is, however, possible to examine these changes empirically in the drug testing context. Table 5 reports the mean and variance of adjusted log wages for the two sectors in both pre- and post-testing periods.[37] Calculations were done defining the sectors at both the industry and job levels, but the sector definition made little difference to the results. In the columns headed by year spans, each cell reports average log wages and their standard deviation for a given demographic group working in one of the two sectors over that time period. In the columns labeled "Tests," the top number is the p-value on a two sided t-test of the difference in mean wages between pre- and post-periods. The number in parentheses is the p-value on a two sided F-test that ratio of wage variances in the two periods is 1.

Looking down the columns labeled 1980-1987, we see that the high-testing sector had higher average wages and lower wage variance than the low-testing sector in this period. Both overall and within demographic groups, average wages decline in the high-testing sector and rise in the low-testing sector from the pre- to the post-period. Wage variance, on the other hand, tends to rise in the high-testing sector and fall in the low-testing sector. P-values for both the t- and F-tests show

[37] Log wages were regression adjusted using a variant of Equation 1 that omits the period variables and their interactions. Adjusted log wages are residuals from this regression.

that these changes are highly significant in almost every instance.[38] The observed changes in the high-testing sector are consistent with a scenario in which drug testing reduces selection on productivity in this sector, lowering average log wages and increasing wage variance.

Changes in several other quantities of interest are outside the scope of the model; these include changes in relative labor market outcomes across demographic groups. I examine these changes empirically beginning in Table 6. The first column reports estimates from a probit with employment as the dependent variable. The interactions of the period effects with an indicator for young workers (ages 18 to 25) show that employment of youth was significantly lower in both post-testing periods than in the pre-period. The magnitude of the difference is similar in both post-periods. As outlined above, I interpret this as evidence that employer drug testing lowered employment rates for young workers. The pre-period dependent variable means for the sample are given at the bottom of each column of estimates. These indicate that the magnitude of the decline is large. A decline of 0.15 from a mean employment rate of 0.72 is economically significant, but in this case the mean employment rate for youth alone is likely much smaller than 0.72, indicating an effect of even larger magnitude.

The second column in Table 6 shows that the probability of employment in a high testing industry (among the employed) increased substantially in the two post-periods. More educated workers, however, were insulated from this shift. Thus employment for less educated workers in high testing industries increased relative to employment in low testing industries. It is important to remember that these results imply that employment of less educated workers was growing in the post-periods in industries identified as high testing *ex ante*.[39] The model was unclear on the impact that testing would have on the overall size of the testing sector, but these results show that it

[38] The results for the variance comparison were unchanged when alternative methods of comparing standard deviations that are robust to non-normality were used.
[39] This is not the same as increases in the likelihood of facing employer testing, which I cannot measure directly in the CPS.

increased. Again, the magnitude of these shifts is large, with the probability of employment in high testing industries among the less educated in the post-periods reaching levels more than 50% above the mean pre-period value.

The probits for group health and pension coverage show that while there were no shifts in health coverage that were sustained over both post-periods, pension coverage increased substantially for younger workers. Finally, the log hourly wage equation shows real wage declines for the average worker in both post-testing periods. These are large, on the order of 4-7%. While young workers were largely insulated from these declines, Hispanic workers suffered even larger real wage declines for a total wage decrease of 6-12% among this group.

Tables 7a through 7d run the same specifications on restricted subsets of the overall sample. Specifically, 7a shows the results from a sample of less educated whites only; 7b for more educated whites; 7c for Blacks and 7d for Hispanics. This strategy is equivalent to running the Table 6 specifications with a complete set of interactions with the relevant demographic group. Both allow me to examine whether changes within specific demographic groups in the post-testing periods differ from those observed in the overall sample. I report results from separate estimations by demographic group because interpreting triple interactions is unwieldy in this context.

Among less educated whites (those with a high school diploma or less), Table 7a shows that employment rates decreased for young workers in both post-periods. The point estimates in this subgroup exceed those in Table 6. As before, the probability of employment in high testing industries increased in the post-periods, but among less educated whites this shift was restricted to men as indicated by the interactions of female with the post-period dummies. There were no robust effects on group health coverage, but pension coverage increased for young workers. Real wages declined (weakly) for this group in both post-periods but again the wages of young workers were unaffected.

Table 7b shows results for more educated whites, those with at least some post-secondary education. Results for this group would seem to constitute an important robustness check, since they are less likely to be affected by changes in employer drug testing practices. Consistent with this prior, Table 7b shows no employment or high testing industry employment effects in the post-periods for this group. On the other hand, both group health and pension coverage increase among the young in this group. The real wages of more educated whites overall decline in both post-periods, with smaller relative declines for college graduates and young workers. I discuss the implications of these findings for my interpretation in the section on robustness checks.

Tables 7c and 7d show results for Blacks and Hispanics, respectively. Among Blacks, the post-testing periods saw large and significant employment declines for dropouts but similarly large and significant employment gains for youth. The post-testing periods saw no employment changes among Hispanics. In contrast to whites, the probability of employment in a high testing industry was unchanged for the average Black and average Hispanic, although as was the case with whites, more educated workers in these groups saw declines in their probabilities of employment in high testing industries.[40] The probability of group health coverage declined for the average worker among both Blacks and Hispanics, although Hispanics with 1-3 years of college were insulated from this decline. Both groups saw relative increases in pension coverage for younger workers.[41] Both groups experience large and significant decreases in real wages for the average worker in both post-periods. Young Hispanic workers, but not young Blacks, were insulated from these.

C. Robustness Checks

As noted above, the estimates for more educated whites in Table 7b constitute a preliminary robustness check. More educated workers are less likely to work in high testing industries (see the

[40] For Hispanics, this was only true of college graduates.
[41] Among Blacks this was also an absolute increase, as average pension coverage was unchanged in the post-periods. Among Hispanics, young workers were simply buffered from the declines in pension coverage that affected the average worker.

pre-period means in Tables 7a through 7d) and employers with large shares of college graduate employees are less likely to test (Hartwell et al, 1996). The fact that more educated white workers experience no employment shifts either overall or across industries in Table 7b is consistent with their lower exposure to employer drug testing. It also suggests that the employment shifts identified for other groups are more likely linked to changes in employer drug testing than to general period effects relevant for all workers.

On the other hand, Table 7b shows that the young in this group experienced a number of other changes in their relative labor market outcomes. Their pension and group health coverage rates and their wages all rose relative to older, more educated white workers. In the cases of the two benefits variables, coverage rates rose for younger workers in this group while rates for older workers were unchanged. Given the large differences in rates of drug use across older and younger workers—even among the more educated—it is not surprising to see relative changes in employment outcomes across age groups among the more educated following the advent of employer drug testing.

The more puzzling result from the perspective of a robustness analysis is in the final column of Table 7b. Coefficients on the period dummies show that real hourly wages declined relative to a quadratic trend in both post-periods for more educated white workers. College graduates and younger workers were partially, but not entirely, insulated from these declines. If the period dummies had been positive and significant, this would have been cause for immediate concern that the dummies were simply reflecting well-known changes in real wage inequality across skill groups over the 1980-1999 period.[42] It is reassuring that this is not the case. This suggests that these secular shifts are adequately captured by the included quadratic time trends.

[42] In this case, the period dummies would be zero or negative for less educated groups (as they are in Tables 9a, 9c and 9d) but positive for more educated workers. See figures in Autor and Dorn (2008).

On the other hand, it is a bit of a surprise to find that the rise of employer drug testing lowered real wages for more skilled workers. Before dismissing the result, it is important to recall that the period effects were not negative in all instances across Tables 7a through 7d. In particular, real wages only decline robustly for the average Hispanic and the average more educated white worker. Real wage declines for Blacks only appeared in one period, and the declines for less educated whites were only weakly significant. If employers had favored Hispanics and more educated whites in the pre-testing period because of their perceived lower rates of drug use, then these groups might experience wage declines in the post-periods as they face competition from a wider group of workers that now includes non-using Blacks and less educated whites.

An important robustness check is to add the aggregate unemployment rate and its interactions with all demographic characteristics as controls. In Tables 6, 7a, 7c, and 7d, the only results affected by the inclusion of the unemployment rate and its interactions (UR specification) are the employment declines for youth in Tables 6 and 7a—those in the total sample and for less educated whites. When the UR specification is estimated on the sample of more educated whites from Table 7b, the relative benefit and wage improvements for young workers disappear, as do the relative wage increases for college graduates in the post-periods. The post-period decline in average real wages for this group is robust to the UR specification. Other coefficients of interest are very similar to those reported when the unemployment rate is added.[43]

In short, with the exception of the findings specific to young, white workers, all key results are robust to the UR specification. Why then is this not the baseline specification? I find that the coefficient on the interaction of the aggregate unemployment rate with a young worker dummy is wrong-signed and significant in the total and less educated white samples, implying strongly countercyclical employment for this group. This conflicts with what is known about youth

[43] In a few instances, some shifts are statistically significant in both post-periods in the UR specification that were only significant in one post-period in the non-UR specification.

employment and the cyclicality of employment among recent labor force entrants. (Clark and Summers, 1980; See also Aaronson, Park and Sullivan, 2006.) Appendix Figure B shows that unemployment rates in the pre-period were markedly higher than in either of the post-periods. It may be that in the case of less educated whites, this pattern in the unemployment rates is confounded with the period effects. I prefer the specification that excludes the unemployment rate for this reason. For most results this makes no difference. Among young, less educated white workers, omitting the UR controls potentially biases the employment effects toward zero, as both post-periods have relatively low levels of unemployment (i.e. comparatively good business cycle performance), which would tend to elevate youth employment in the post-periods relative to the pre-period.

A final possibility for examining the robustness of these results is to look for more pronounced effects in states with high shares of their employment high-testing industries relative to states with smaller employment shares in such industries.[44] However, this exercise produced no important differences across the two groups. This is likely because there is little variation in state-level shares of employment in high-testing industries. These shares range from a low of 0.18 to a high of 0.38, but the min-max spread is somewhat deceiving. Within the 25 highest-testing states, the mean share is 0.34 with a standard deviation of 0.02; within the bottom 25 states the mean is 0.27 with a mean of 0.03. This is simply not a large enough difference to detect differential period effects across these two groups of states.

V. Conclusion

This paper examined the impact of widespread employer drug testing of employees and particularly applicants on an array of labor market outcomes. I incorporated drug testing by firms

[44] I also examined robustness of a subset of the results to an alternative set of time periods: 1980-1984 as the pre-period; 1985-1987 as a first transitional period; 1988-1993 as the second transitional period; and 1994-1999 as the post period.

and drug use by workers into a standard Roy model and derived implications for how the introduction of drug testing may impact the sorting of workers from different demographic groups into testing and non-testing sectors. I then combined information from Bureau of Labor Statistics surveys, the National Survey on Drug Use and Health, and the Current Population Survey (CPS) to provide empirical evidence on the model's predictions as well as on dimensions regarding which the model makes no unambiguous prediction.

Limited data on the prevalence of employer drug testing meant that the empirical work was restricted to analysis of changes in national-level outcomes between the pre- and post-drug testing periods.[45] I used CPS microdata spanning 1980 to 1999 to examine changes in outcomes within and across demographic groups and industries as drug testing prevalence increased nationally. Given the predominant role of pre-employment screening in drug testing programs, combined with penalties for failure than have clear connections to labor market outcomes, I attribute the reduced form changes I observe primarily to the advent of pre-employment testing.

The results suggest that employer drug testing has had a complex impact on the U.S. labor market. Consistent with the model's predictions, I find that demographic groups with low drug use rates are overrepresented in the testing sector prior to the introduction of testing and that non-users' share of employment in the testing industry increases after the introduction of testing. I also find that log wages decline in the testing sector (high testing industries or jobs) and rise in the non-testing sector after the introduction of testing, although changes in either direction are possible within the model. Conversely, wage variance rose in the testing sector and fell in the non-testing sector. I also provide evidence that testing led to a number of changes in relative outcomes across demographic groups, although the model is silent on the direction relative changes should take. I find that testing improved employment and wages for black youth, and improved access to jobs in high testing

[45] State level data on changes in drug testing intensity over time exist in private corporate data and possibly in some federal oversight agencies. I hope to incorporate one or both sources in future versions.

industries for less skilled white men. While youth employment overall declined under testing, I find that the youth who are employed find better quality jobs. On the other hand, employment of Hispanics in high testing industries and high quality jobs has declined, as have their wages.

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Table 1: Drug Testing Policy and Practice Timeline: Key Dates

Year	Decision/Policy	Summary
1972 and 1973	The Drug Abuse Prevention, Treatment and Rehabilitation Acts of 1972 and 1973	The Acts prohibit the denial of federal civilian employment based on prior drug use except for certain sensitive positions. It has been unsuccessfully argued that these acts prohibit drug testing to identify applicants and employees who are using drugs. (6)
1986	Executive Order 12564	Required that all federal agencies adopt drug-testing programs with the goal of creating a "drug free workplace." (3)
1987	Section 503, Title V, Public Law 100-71	Permitted drug testing of federal employees provided that certain parameters were met. (7)
1987	Mandatory Guidelines for Federal Workplace Drug Testing Programs	The Department of Health and Human Services established standards for drug testing laboratory certification and for federal employee drug testing programs. (1)
1987	First Pre-Employment Drug Testing Guidelines	Enacted in five states (CT, IA, MN, UT, VT) (9)
1988	Drug Free Workplace Act of 1988	Required all companies with federal contracts worth $25,000 or more to implement drug-free workplace policies. (3)
1988	Department of Transportation Regulations	Required DOT-regulated industries to create drug testing programs for applicants and employees in safety sensitive positions. (7)
1989	National Treasury Employee's Union v. von Raab, 86-1879	Supreme Court upheld the government's right to require drug tests for certain U.S. Customs Service employees. (1)
1989	Skinner v. Railway Labor Executives' Association, 87-1555	Supreme Court upheld mandatory blood and urine tests for railroad workers involved in accidents. (1)
1989	Transportation Institute v. United States Coast Guard, 727 F. Supp. 648	The United States District Court, DC held that required job applicant testing, among other testing policies, did not violate the Fourth Amendment. (5)
1989	Harmon v. Thornburgh (878 F.2d 484, D.C. Cir. 1989)	Antitrust lawyers could not be subjected to the drug testing program for current Justice Department employees because there was not a sufficient link between their work and drug use to justify the invasion of privacy. (3)
1991	Americans with Disabilities Act	Applicants with prior drug or alcohol use are protected under the ADA, but current illegal drug users are not. Drug testing is not considered a medical exam under the ADA. (3)
1991	Omnibus Transportation Employee Testing Act (OTETA)	The Act unified and consolidated drug testing standards for DOT-regulated industries. (3)
1991	Willner v. Thornburgh (928 F.2d 1185, D.C. Cir. 1991)	Upheld a requirement that applicants to the Justice Department's Antitrust Division to submit to a drug test because the privacy invasion was not unreasonable given that such testing was regularly occurring in the private sector. (3)
1995	Vernonia Sch. Dist. 47J v. Acton (94-590), 515 U.S. 646 (1995)	Upheld a program that required students participating in interscholastic athletics to submit to drug testing. (4)
1997	Chandler v. Miller (96-126), 520 U.S. 305 (1997)	Struck down a Georgia statute requiring candidates for designated state offices to prove that they have taken a urinalysis drug test with a negative test result within 30 days before qualifying for nomination or election. (4)

Notes: Sources: (1) Ackerman in Drug testing issues and options (edited by Coombs); (2) Coombs and West; (3) Normand; (4) Cornell University Law School Supreme Court Collection; (5) Westlaw; (6) Angarola in Coombs; (7) Walsh and Trumble in Coombs; (8) Jacobson; (9) De Bernardo and Nieman (2006).

Table 2: Share of Establishments with a Drug Testing Program

<table>
<tr><th></th><th>1988</th><th>1993</th><th>1997-2006</th></tr>
<tr><td>Total</td><td>3.2</td><td>48.4</td><td>0.46</td></tr>
<tr><td>By Establishment Size</td><td></td><td></td><td></td></tr>
<tr><td>1-9</td><td>0.8</td><td>-</td><td>0.21</td></tr>
<tr><td>10-49</td><td>6.4</td><td>-</td><td>0.39[a]</td></tr>
<tr><td>50-99</td><td>12.4</td><td>40.2</td><td>0.50[b]</td></tr>
<tr><td>100-249</td><td>17.2</td><td>48.2</td><td rowspan="2">0.66</td></tr>
<tr><td>250-499</td><td>29.7</td><td rowspan="2">61.4</td></tr>
<tr><td>500-999</td><td>30.6</td><td rowspan="3">0.75</td></tr>
<tr><td>1000-4999</td><td>41.8</td><td rowspan="2">70.9</td></tr>
<tr><td>5000+</td><td>59.8</td></tr>
<tr><td>By Industry</td><td></td><td></td><td></td></tr>
<tr><td>Mining</td><td>21.6</td><td rowspan="2">69.6</td><td>0.86</td></tr>
<tr><td>Construction</td><td>2.3</td><td>0.44</td></tr>
<tr><td>Durable Mfg.</td><td>9.9</td><td rowspan="2">60.2</td><td rowspan="2">0.70</td></tr>
<tr><td>Non-durable Mfg.</td><td>9.1</td></tr>
<tr><td>Transportation</td><td>14.9</td><td rowspan="2">72.4</td><td rowspan="2">0.72</td></tr>
<tr><td>Communic.,Utilities</td><td>17.6</td></tr>
<tr><td>Wholesale trade</td><td>5.3</td><td rowspan="2">53.7</td><td>0.60</td></tr>
<tr><td>Retail trade</td><td>0.7</td><td>0.43</td></tr>
<tr><td>FIRE</td><td>3.2</td><td>22.6</td><td>0.40</td></tr>
<tr><td>Services</td><td>1.4</td><td>27.9</td><td>0.36</td></tr>
<tr><td>Agriculture</td><td>-</td><td>-</td><td>0.22</td></tr>
<tr><td>Government</td><td>-</td><td>-</td><td>0.61</td></tr>
<tr><td>By Region</td><td></td><td></td><td></td></tr>
<tr><td>Northeast</td><td>1.9</td><td>33.3</td><td>-</td></tr>
<tr><td>Midwest</td><td>3.8</td><td>50.3</td><td>-</td></tr>
<tr><td>South</td><td>3.9</td><td>56.3</td><td>-</td></tr>
<tr><td>West</td><td>2.8</td><td>46.8</td><td>-</td></tr>
</table>

Notes: Data for 1989 are from U.S. Department of Labor (1989), Tables 1 and 2. Data for 1993 are from Hartwell et. al. (1996) Table 1. Numbers in both columns refer to the share of establishments with any kind of drug testing. Note that because the 1993 sample excludes establishments with fewer than 50 employees, some of the increase in total and industry level testing shares is due to dropping a part of the sample where testing is less prevalent. Data for 1997-2006 are average shares of 22-49 year old employees in the NSDUH reporting that their employer conducts some form of drug testing.

a This number is for establishments with 10-24 employees.

b This number is for establishments with 25-99 employees.

Table 3: Descriptive Statistics for the March CPS Sample, 1979-1999

Variable	Mean	Observations
Age	34.94	1615570
Employed	0.75	1615570
Employed in high test ind.	0.31	1203938
Real hourly wage ($1990)	10.14	1341860
Log real hourly wage	2.11	1256882
In wage sample	0.73	1615570
Covered by group health	0.55	1341860
Covered by pension	0.52	1341858
Female	0.52	1615570
Black	0.10	1615570
Hispanic	0.12	1615570
Any postsecondary	0.44	1615570
Young (ages 18-25)	0.23	1615570

Notes: Data are from the IPUMS version of the annual March CPS surveys. Sample is restricted to those ages 18-55.

Table 4: Probability of Employment in High Testing Industries among Drug Users and Non-Users

Sample:	Whole Sample	Less Educ. Whites	More Educ. Whites	Blacks	Hispanics
Non-user (NU)	0.014	0.104	0.078	0.015	0.048
	(0.001)**	(0.001)**	(0.000)**	(0.000)**	(0.000)**
p8893	0.029	0.168	-0.059	-0.014	0.032
	(0.005)**	(0.022)**	(0.031)+	-0.034	-0.071
p9499	0.03	0.161	-0.056	-0.045	0.116
	(0.010)**	(0.044)**	-0.053	-0.039	-0.074
Non-user x p8893	0.044	-0.038	0.093	0.052	0.059
	(0.006)**	(0.013)**	(0.017)**	(0.019)**	(0.031)+
Non-user x p9499	0.053	-0.025	-0.015	0.103	-0.019
	(0.007)**	(0.025)	(0.017)	(0.032)**	(0.019)
NU x Female		-0.075	-0.041	-0.031	0.067
		(0.000)**	(0.000)**	(0.000)**	(0.000)**
NU x Young		-0.094	-0.068	0.018	-0.143
		(0.001)**	(0.000)**	(0.000)**	(0.000)**
Female x p8893		-0.072	-0.048	0.034	0.088
		(0.022)**	(0.028)+	(0.086)	(0.039)*
Young x p8893		-0.075	-0.09	0.042	-0.213
		(0.034)*	(0.065)	(0.048)	(0.041)**
Female x p9499		0.022	-0.104	0.036	0.03
		(0.044)	(0.051)*	(0.09)	(0.04)
Young x p9499		-0.015	-0.038	-0.02	-0.207
		(0.065)	(0.083)	(0.059)	(0.038)**
NU x Female x p8893		0.062	-0.043	-0.02	-0.092
		(0.019)**	(0.016)**	(0.036)	(0.032)**
NU x Young x p8893		0.086	0.131	-0.07	0.093
		(0.026)**	(0.031)**	(0.023)**	(0.056)+
NU x Female x p9499		0.008	-0.024	-0.017	-0.081
		(0.045)	(0.027)	(0.017)	(0.031)*
NU x Young x p9499		0.06	0.132	-0.04	0.11
		(0.016)**	(0.038)**	(0.046)	(0.012)**
Dropouts x NU		-0.026		0.139	-0.024
		(0.000)**		(0.000)**	(0.000)**
Some college x NU				0.066	0.132
				(0.000)**	(0.000)**
College grad x NU			-0.122	0.052	-0.124
			(0.000)**	(0.000)**	(0.000)**
Observations	89015	21516	28014	18299	21186

Notes: Data are from NSDUH, 1985-1999 (industry not reported in 1979 and 1982 waves), restricted to individuals ages 18-64 and employed at the time of the survey. Each column contains estimates from separate estimation of a probit model. Coefficients are reported as marginal effects. Standard errors clustered on year are in parentheses. All specifications include controls for female, youth, and education group as well as interactions of these with a quadratic time trend. Controls for education group x period and education group x period x non-user are also included.

Table 5: Mean and Variance of Log Wage Residuals by Sector, Demographic Group, and Testing Regime

Testing intensity measured at…		Industry Level			Job Level		
		1980-1987	1994-1999	Tests	1980-1987	1994-1999	Tests
Group	**Intensity**						
Overall	*High*	0.176	0.133	0.00	0.094	0.056	0.00
		(0.543)	(0.546)	(0.04)	(0.605)	(0.588)	(0.00)
	Low	-0.055	-0.038	0.00	-0.100	-0.055	0.00
		(0.763)	(0.666)	(0.00)	(0.796)	(0.690)	(0.00)
Ages 18-25	*High*	0.160	0.119	0.00	0.068	0.035	0.00
		(0.555)	(0.592)	(0.00)	(0.579)	(0.630)	(0.00)
	Low	-0.020	-0.012	0.00	-0.077	-0.036	0.00
		(0.661)	(0.649)	(0.03)	(0.705)	(0.674)	(0.00)
Ages 25+	*High*	0.179	0.134	0.00	0.102	0.060	0.00
		(0.540)	(0.540)	(0.98)	(0.613)	(0.579)	(0.00)
	Low	-0.068	-0.044	0.00	-0.108	-0.059	0.00
		(0.797)	(0.670)	(0.00)	(0.826)	(0.694)	(0.00)
Women	*High*	0.180	0.133	0.00	0.091	0.523	0.00
		(0.574)	(0.550)	(0.00)	(0.656)	(0.585)	(0.00)
	Low	-0.024	-0.019	0.05	-0.065	-0.035	0.00
		(0.767)	(0.574)	(0.00)	(0.781)	(0.656)	(0.00)
Men	*High*	0.173	0.132	0.00	0.095	0.058	0.00
		(0.527)	(0.544)	(0.00)	(0.575)	(0.590)	(0.00)
	Low	-0.089	-0.059	0.00	-0.145	-0.080	0.00
		(0.758)	(0.696)	(0.00)	(0.812)	(0.732)	(0.00)
Whites	*High*	0.183	0.137	0.00	0.098	0.059	0.00
		(0.551)	(0.540)	(0.00)	(0.618)	(0.582)	(0.00)
	Low	-0.057	-0.039	0.00	-0.103	-0.055	0.00
		(0.788)	(0.673)	(0.00)	(0.823)	(0.698)	(0.00)
Blacks	*High*	0.154	0.130	0.00	0.076	0.044	0.00
		(0.499)	(0.577)	(0.00)	(0.537)	(0.616)	(0.00)
	Low	-0.045	-0.037	0.15	-0.087	-0.054	0.00
		(0.576)	(0.640)	(0.00)	(0.608)	(0.669)	(0.00)
Hispanics	*High*	0.131	0.107	0.00	0.076	0.049	0.00
		(0.504)	(0.559)	(0.00)	(0.560)	(0.600)	(0.00)
	Low	-0.045	-0.033	0.03	-0.092	-0.054	0.00
		(0.662)	(0.643)	(0.00)	(0.677)	(0.657)	(0.00)

Notes: Data are from March CPS, 1980-1999, restricted to individuals ages 18-65 and employed at the time of the survey. In columns headed by years, top number in cell is mean of log wage residual over the relevant time period, demographic group, and testing sector; number in parentheses is standard deviation of same. Log wage residuals computed as described in text (Footnote 38). In columns labeled "Tests," top number is p-value on two sided t-test of difference in means between pre- and post-periods; number in parentheses is the p-value on the two sided F-test that ratio of variances in the two periods is 1.

Table 6: Changes in the Role of Personal Characteristics in Employment Outcomes following Drug Testing Expansions

Dependent Variable:	Employed	Employed in High Test Ind.	Covered by Group Health	Covered by Pension	Log Real Hourly Wage
DO*p8893	-0.004	-0.01	-0.02	-0.01	0.012
	[0.004]	[0.006]	[0.004]**	[0.004]**	[0.006]+
SC*p8893	-0.001	-0.01	-0.009	0.002	-0.001
	[0.004]	[0.002]**	[0.006]	[0.005]	[0.006]
CG*p8893	-0.007	-0.021	-0.009	-0.009	0.011
	[0.006]	[0.009]*	[0.010]	[0.006]	[0.006]+
DO*p9499	-0.014	-0.004	-0.018	-0.01	0.019
	[0.010]	[0.011]	[0.012]	[0.006]	[0.016]
SC*p9499	0.008	-0.017	-0.004	0.012	-0.015
	[0.009]	[0.003]**	[0.010]	[0.010]	[0.007]+
CG*p9499	0.005	-0.029	-0.003	-0.002	0.016
	[0.010]	[0.015]+	[0.012]	[0.011]	[0.014]
Black*p8893	0.00	-0.011	-0.028	-0.001	0.007
	[0.008]	[0.008]	[0.007]**	[0.008]	[0.004]+
Hispan.*p8893	0.006	-0.001	-0.018	-0.013	-0.022
	[0.005]	[0.006]	[0.013]	[0.012]	[0.009]*
Female*p8893	-0.014	-0.003	-0.008	-0.007	0.001
	[0.010]	[0.004]	[0.011]	[0.006]	[0.007]
Young*p8893	-0.016	0.008	0.016	0.066	0.048
	[0.004]**	[0.008]	[0.010]	[0.016]**	[0.010]**
Black*p9499	-0.013	-0.016	-0.031	-0.032	-0.015
	[0.012]	[0.021]	[0.022]	[0.028]	[0.009]
Hispan.*p9499	-0.008	-0.003	-0.027	-0.007	-0.037
	[0.008]	[0.009]	[0.019]	[0.017]	[0.017]*
Female*p9499	-0.012	-0.013	-0.006	-0.008	0.011
	[0.014]	[0.008]+	[0.020]	[0.011]	[0.012]
Young*p9499	-0.015	0.01	0.028	0.057	0.045
	[0.007]*	[0.011]	[0.013]*	[0.020]**	[0.017]*
p8893	0.026	0.015	-0.022	0.00	-0.04
	[0.017]	[0.004]**	[0.011]*	[0.006]	[0.020]+
p9499	0.019	0.026	-0.016	-0.016	-0.075
	[0.027]	[0.008]**	[0.025]	[0.018]	[0.036]*
Observations	1615455	1203845	1341756	1341754	1182042
DV Mean	0.72	0.33	0.59	0.49	2.09

Notes: Data are from March CPS 1979-1999, IPUMS version. Dependent variable mean is calculated for the pre-period only, 1979-1987. Sample is individuals ages 18-55. Wage equation is further restricted to those with positive earnings within the 3rd and 97th percentiles of the real wage distribution in the overall sample. Specifications in columns 1-4 are estimated via probit, and coefficients are reported as marginal effects. Implied probabilities are reported. Column 5 specification estimated via OLS. All specifications include a quadratic time trend, and interactions of the time trend components with all demographic variables. R-squared on the wage equation (which includes a constant) is 0.31. ** indicates significance at the 1% level, * at 5%, and + at 10%.

Table 7a: Changes in the Role of Personal Characteristics in Employment Outcomes - Less Educated Whites Only

Dependent Variable:	Employed	Employed in High Test Ind.	Covered by Group Health	Covered by Pension	Log Real Hourly Wage
DO*p8893	0.003	-0.012	-0.02	-0.013	0.019
	[0.006]	[0.006]*	[0.005]**	[0.005]**	[0.007]*
DO*p9499	-0.009	0.001	-0.025	-0.031	0.003
	[0.018]	[0.014]	[0.013]+	[0.011]**	[0.018]
Female*p8893	-0.012	-0.01	-0.015	-0.008	0.003
	[0.012]	[0.004]*	[0.015]	[0.009]	[0.011]
Young*p8893	-0.038	0.003	-0.013	0.046	0.037
	[0.010]**	[0.008]	[0.009]	[0.014]**	[0.009]**
Female*p9499	0.002	-0.034	-0.018	-0.018	0.02
	[0.015]	[0.006]**	[0.026]	[0.013]	[0.016]
Young*p9499	-0.042	0.006	-0.015	0.04	0.038
	[0.014]**	[0.016]	[0.011]	[0.021]+	[0.013]**
p8893	0.031	0.019	-0.01	0.005	-0.038
	[0.019]	[0.004]**	[0.015]	[0.007]	[0.021]+
p9499	0.019	0.032	0.004	-0.006	-0.074
	[0.027]	[0.007]**	[0.028]	[0.015]	[0.037]+
Constant					0.977
					[0.031]**
Observations	660740	471857	535711	535710	470896
DV Mean	0.69	0.36	0.56	0.45	1.97

Notes: Data is described in Table 7 notes, but sample is further restricted to whites with schooling attainment of less than or equal to a high school diploma. R-squared on the wage equation is 0.26. ** indicates significance at the 1% level, * at 5%, and + at 10%.

Table 7b: Changes in the Role of Personal Characteristics in Employment Outcomes - More Educated Whites Only

Dependent Variable:	Employed	Employed in High Test Ind.	Covered by Group Health	Covered by Pension	Log Real Hourly Wage
CG*p8893	-0.003	-0.011	0.004	-0.007	0.018
	[0.003]	[0.011]	[0.005]	[0.005]	[0.007]*
CG*p9499	-0.001	-0.013	0.009	-0.009	0.041
	[0.005]	[0.017]	[0.006]	[0.010]	[0.011]**
Female*p8893	-0.015	-0.001	-0.014	-0.013	0.003
	[0.009]+	[0.008]	[0.010]	[0.010]	[0.008]
Young*p8893	-0.012	0.007	0.039	0.068	0.05
	[0.005]**	[0.011]	[0.018]*	[0.018]**	[0.017]**
Female*p9499	-0.018	-0.007	-0.012	-0.007	0.009
	[0.015]	[0.011]	[0.017]	[0.015]	[0.010]
Young*p9499	-0.009	0.012	0.064	0.054	0.05
	[0.009]	[0.023]	[0.024]**	[0.022]*	[0.026]+
p8893	0.019	0.006	-0.037	0.002	-0.048
	[0.011]+	[0.006]	[0.012]**	[0.007]	[0.023]+
p9499	0.023	0.01	-0.031	-0.005	-0.097
	[0.019]	[0.011]	[0.023]	[0.017]	[0.035]*
Constant					0.702
Observations	612317	505735	549079	549078	474886
DV Mean	0.81	0.27	0.65	0.57	2.28

Notes: Data is described in Table 7 notes, but sample is further restricted to whites with at least some post-secondary schooling. R-squared on the wage equation is 0.28. ** indicates significance at the 1% level, * at 5%, and + at 10%.

Table 7c: Changes in the Role of Personal Characteristics in Employment Outcomes – Blacks Only

Dependent Variable:	Employed	Employed in High Test Ind.	Covered by Group Health	Covered by Pension	Log Real Hourly Wage
DO*p8893	-0.033	-0.02	-0.011	-0.011	-0.002
	[0.015]*	[0.019]	[0.027]	[0.012]	[0.023]
SC*p8893	-0.006	-0.033	-0.016	0.006	0.027
	[0.018]	[0.013]*	[0.015]	[0.014]	[0.015]+
CG*p8893	0.00	-0.022	0.026	-0.025	0.015
	[0.017]	[0.012]+	[0.027]	[0.016]	[0.024]
DO*p9499	-0.062	-0.013	0.022	0.027	0.092
	[0.023]**	[0.026]	[0.037]	[0.021]	[0.036]*
SC*p9499	0.015	-0.088	0.001	-0.003	0.037
	[0.027]	[0.019]**	[0.045]	[0.042]	[0.025]
CG*p9499	0.019	-0.047	0.056	-0.079	0.008
	[0.040]	[0.020]*	[0.034]	[0.026]**	[0.049]
Female*p8893	-0.011	-0.01	0.03	0.015	-0.01
	[0.016]	[0.010]	[0.014]*	[0.015]	[0.018]
Young*p8893	0.045	0.017	0.041	0.109	0.068
	[0.011]**	[0.021]	[0.033]	[0.022]**	[0.017]**
Female*p9499	-0.02	0.01	0.022	0.016	-0.03
	[0.024]	[0.014]	[0.022]	[0.026]	[0.023]
Young*p9499	0.085	0.03	0.082	0.096	0.039
	[0.019]**	[0.030]	[0.047]+	[0.026]**	[0.024]
p8893	0.016	0.017	-0.079	-0.019	-0.032
	[0.032]	[0.010]	[0.017]**	[0.017]	[0.020]
p9499	-0.016	0.024	-0.088	-0.058	-0.084
	[0.052]	[0.023]	[0.053]+	[0.061]	[0.036]*
Constant					0.866
					[0.047]**
Observations	153838	99915	114749	114749	106531
DV Mean	0.62	0.37	0.60	0.52	1.95

Notes: Data is described in Table 7 notes, but sample is further restricted to Blacks. R-squared on the wage equation is 0.27. ** indicates significance at the 1% level, * at 5%, and + at 10%.

Table 7d: Changes in the Role of Personal Characteristics in Employment Outcomes – Hispanics Only

Dependent Variable:	Employed	Employed in High Test Ind.	Covered by Group Health	Covered by Pension	Log Real Hourly Wage
DO*p8893	-0.001	0.005	-0.011	0.013	0.005
	[0.010]	[0.011]	[0.011]	[0.010]	[0.022]
SC*p8893	0.009	0.024	0.037	0.027	0.011
	[0.011]	[0.015]	[0.013]**	[0.020]	[0.012]
CG*p8893	-0.014	-0.035	-0.011	0.003	-0.014
	[0.012]	[0.012]**	[0.040]	[0.031]	[0.020]
DO*p9499	0.002	-0.022	-0.009	0.05	0.031
	[0.017]	[0.016]	[0.021]	[0.017]**	[0.031]
SC*p9499	-0.005	-0.001	0.032	0.041	0.005
	[0.018]	[0.023]	[0.018]+	[0.042]	[0.017]
CG*p9499	0.008	-0.083	-0.044	0.047	0.004
	[0.019]	[0.015]**	[0.052]	[0.044]	[0.043]
Female*p8893	-0.028	0.031	-0.017	0.004	-0.009
	[0.017]	[0.019]	[0.012]	[0.010]	[0.016]
Young*p8893	-0.005	0.006	0.028	0.076	0.076
	[0.011]	[0.012]	[0.008]**	[0.015]**	[0.012]**
Female*p9499	-0.034	0.014	0.001	0.003	0.009
	[0.019]+	[0.028]	[0.023]	[0.015]	[0.023]
Young*p9499	-0.028	-0.007	0.018	0.088	0.101
	[0.024]	[0.018]	[0.012]	[0.031]**	[0.018]**
p8893	0.036	-0.01	-0.05	-0.032	-0.063
	[0.020]+	[0.014]	[0.010]**	[0.007]**	[0.023]*
p9499	0.018	0.027	-0.049	-0.063	-0.133
	[0.035]	[0.020]	[0.020]*	[0.021]**	[0.037]**
Constant					1.113
					[0.046]**
Observations	188560	126338	142217	142217	129729
DV Mean	0.64	0.36	0.55	0.40	1.95

Notes: Data is described in Table 7 notes, but sample is further restricted to non-Black Hispanics. R-squared on the wage equation is 0.24. ** indicates significance at the 1% level, * at 5%, and + at 10%.

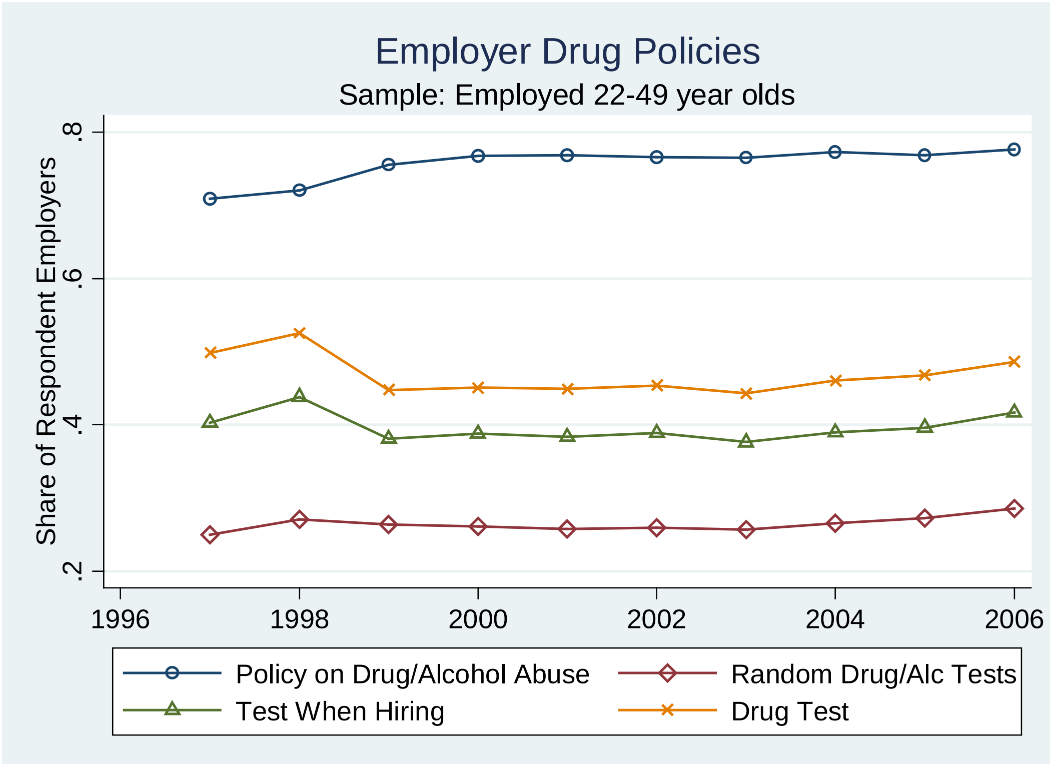


Figure 1. Source: National Survey on Drug Use and Health (NSDUH). Shares of employed respondents, ages 22-49, reporting that their employer adheres to one or more of the listed anti-drug policies.

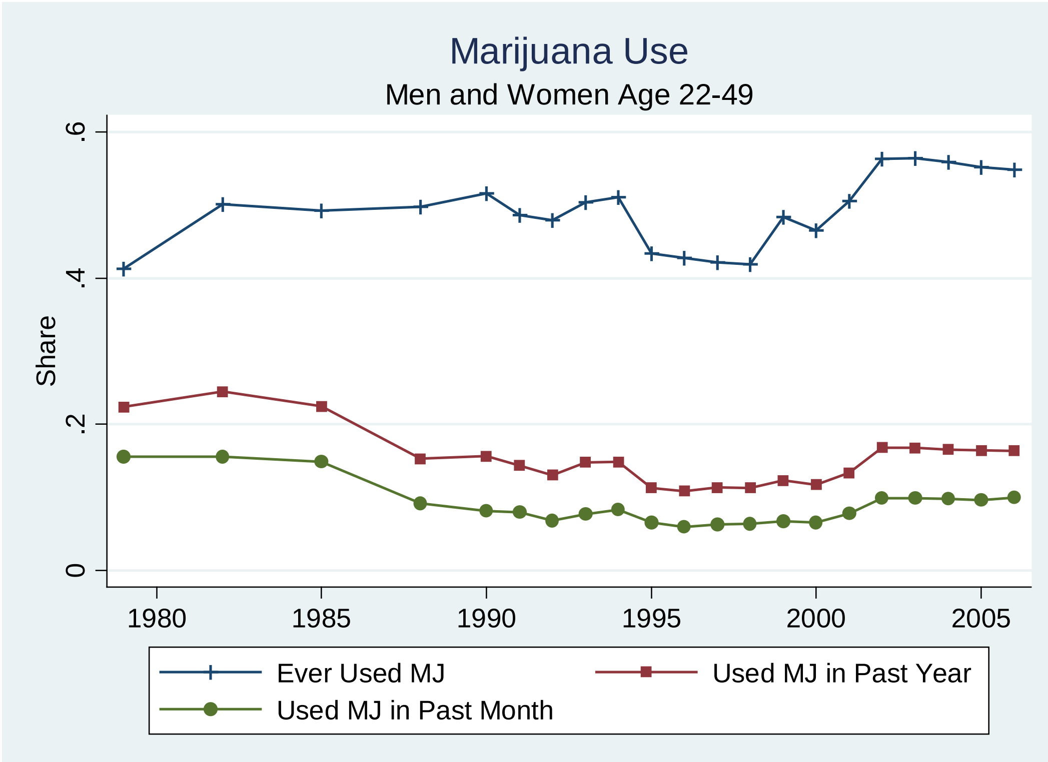
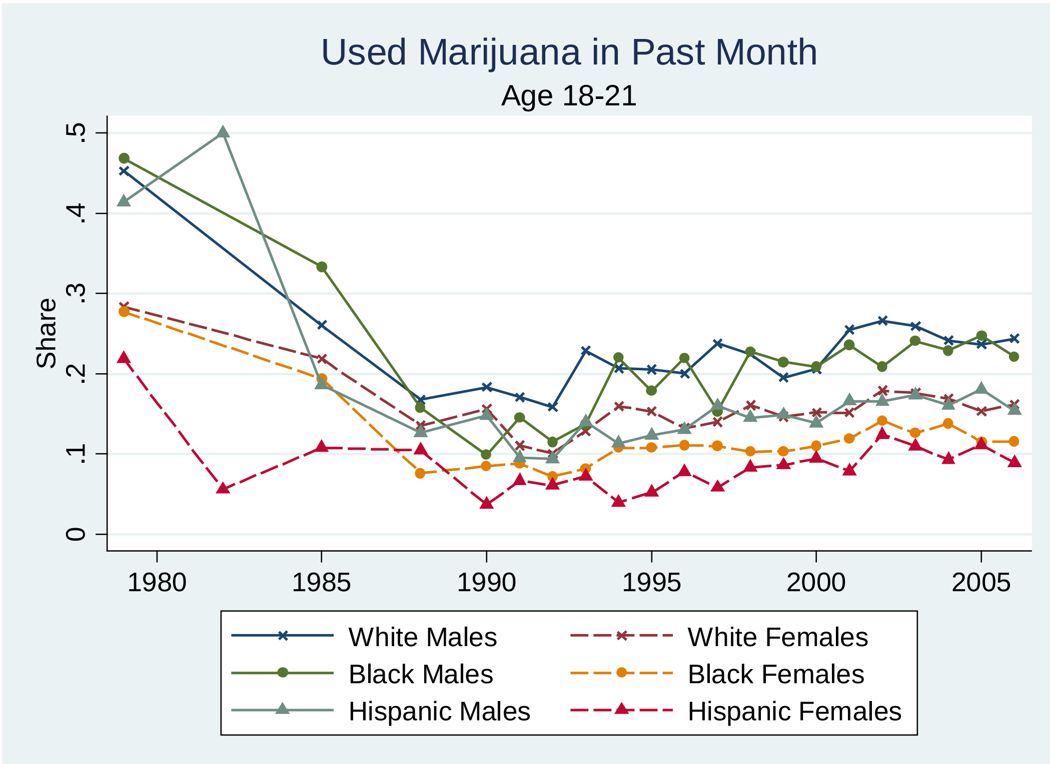
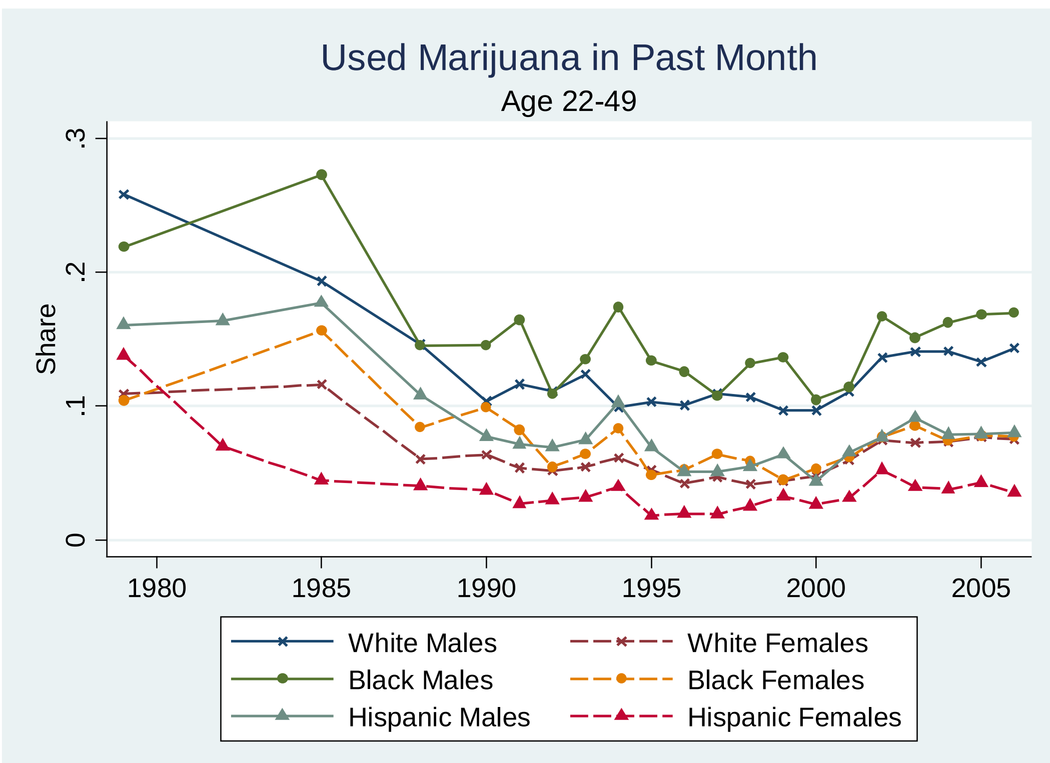


Figure 2. Source: NSDUH. Shares of respondents ages 22-49 reporting marijuana use in various intervals.

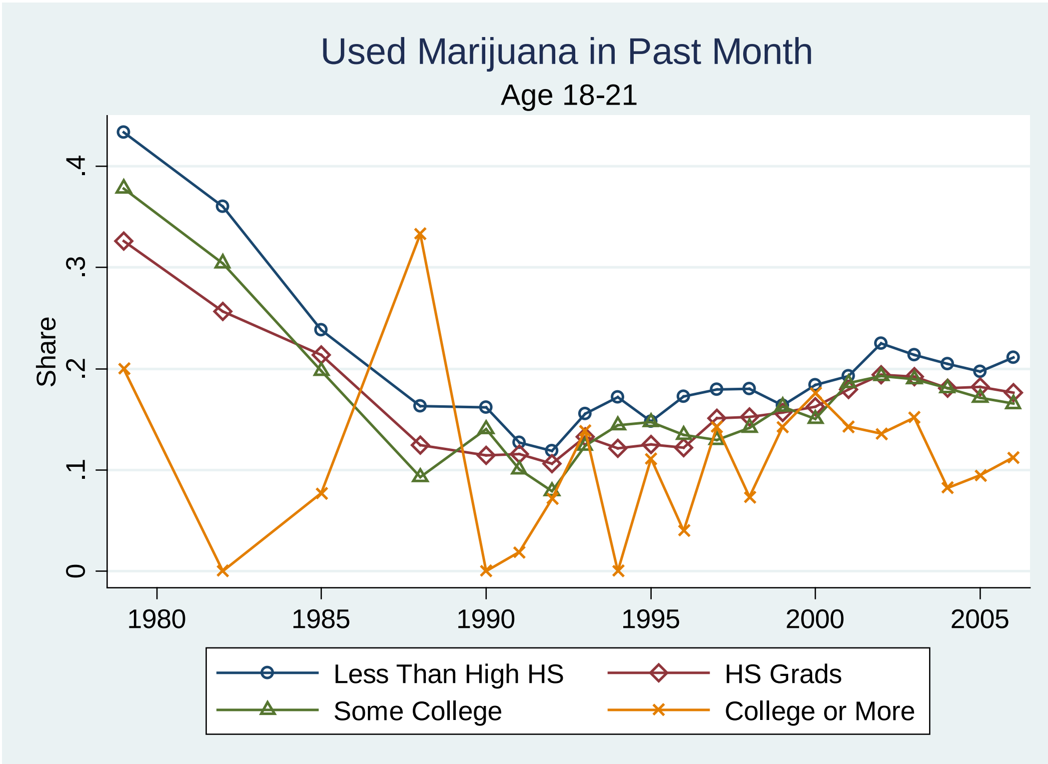


3a.

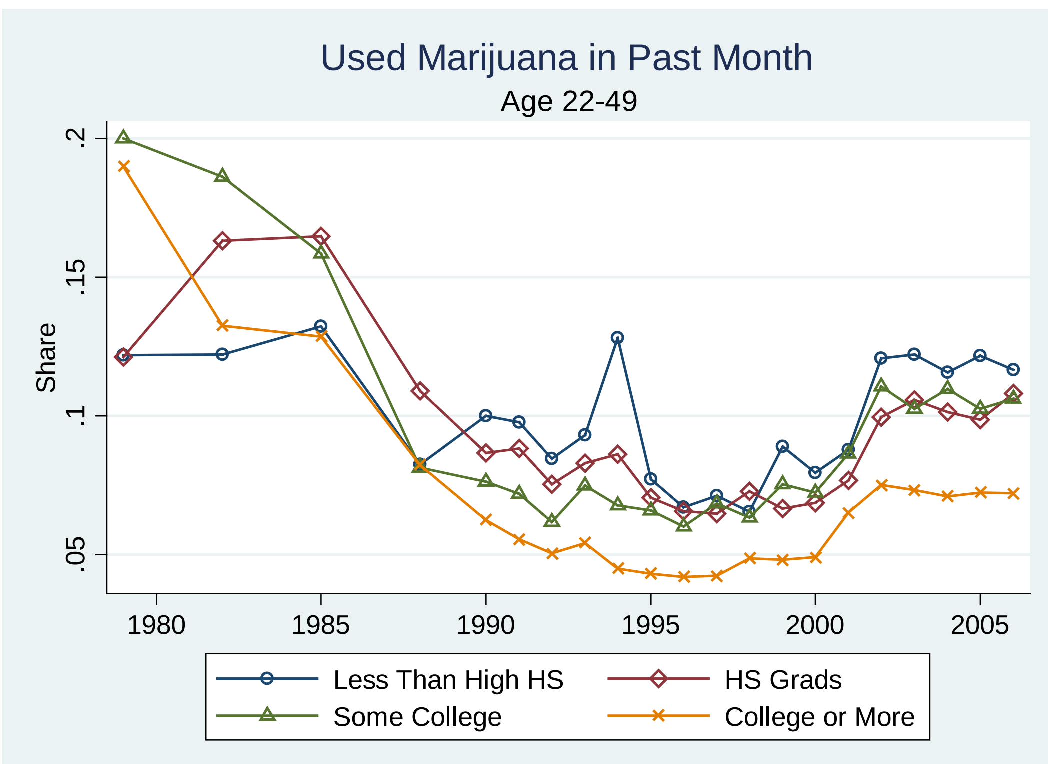


3b.

Figures 3a and 3b. Source: NSDUH. Shares of respondents reporting any past month marijuana use by race/ethnicity, age and gender.

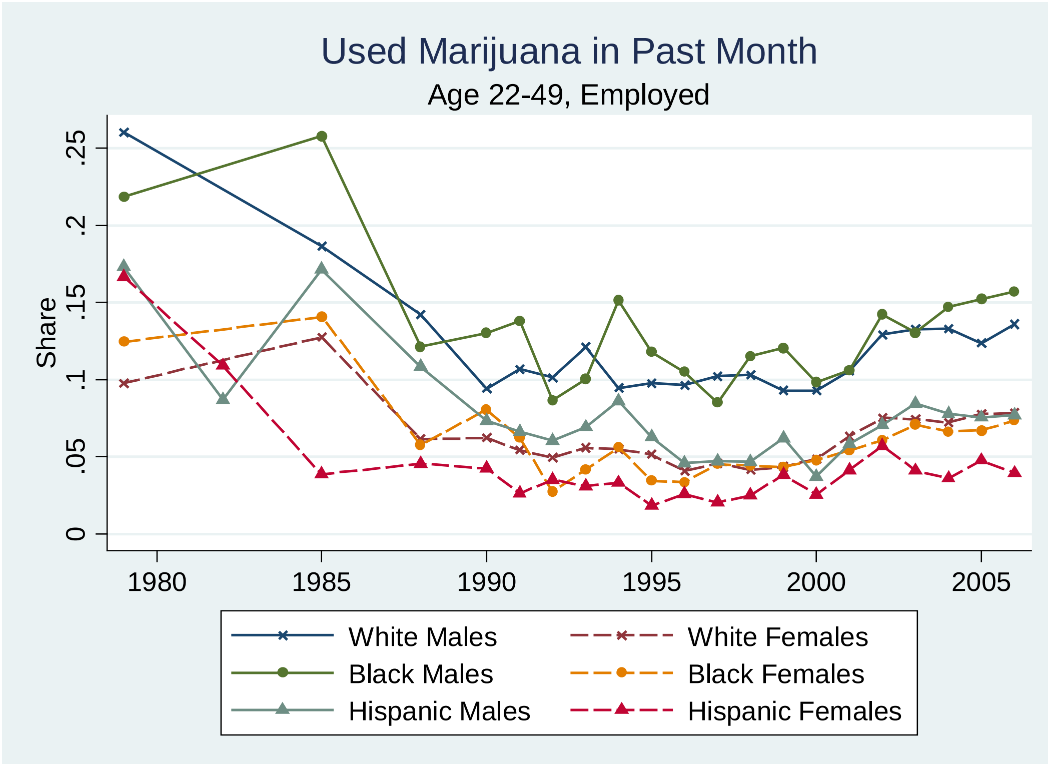


4a.

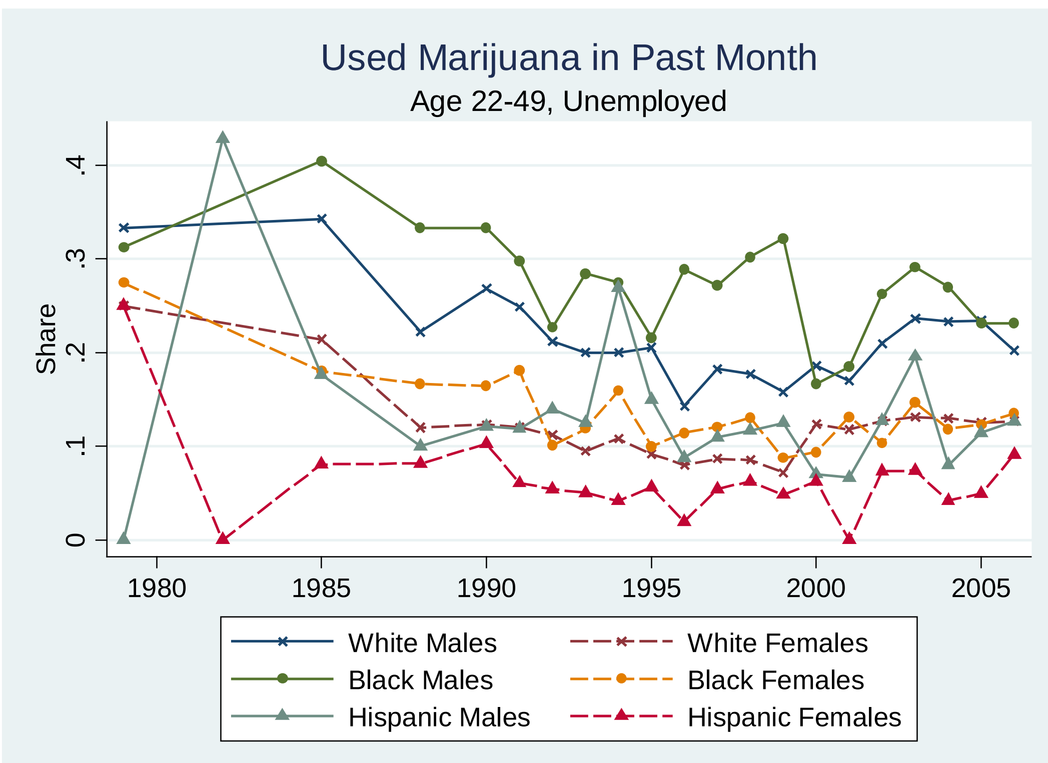


4b.

Figures 4a and 4b. Source: NSDUH. Shares of respondents reporting any past month marijuana use by education group and age.

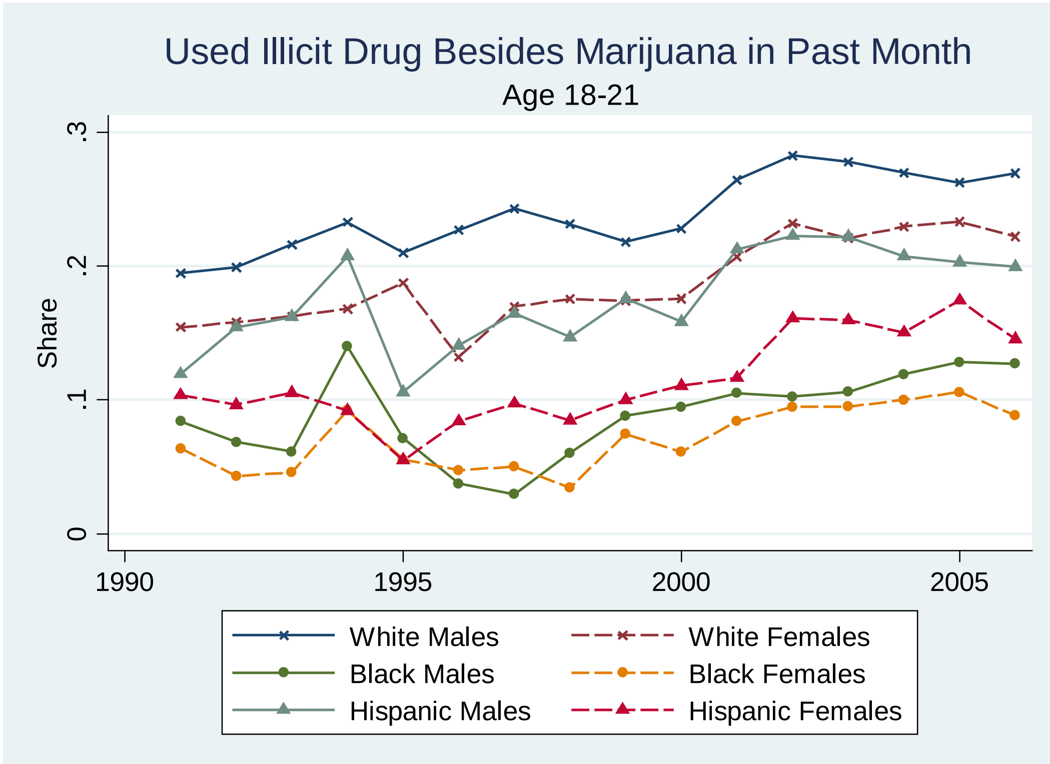


5a.

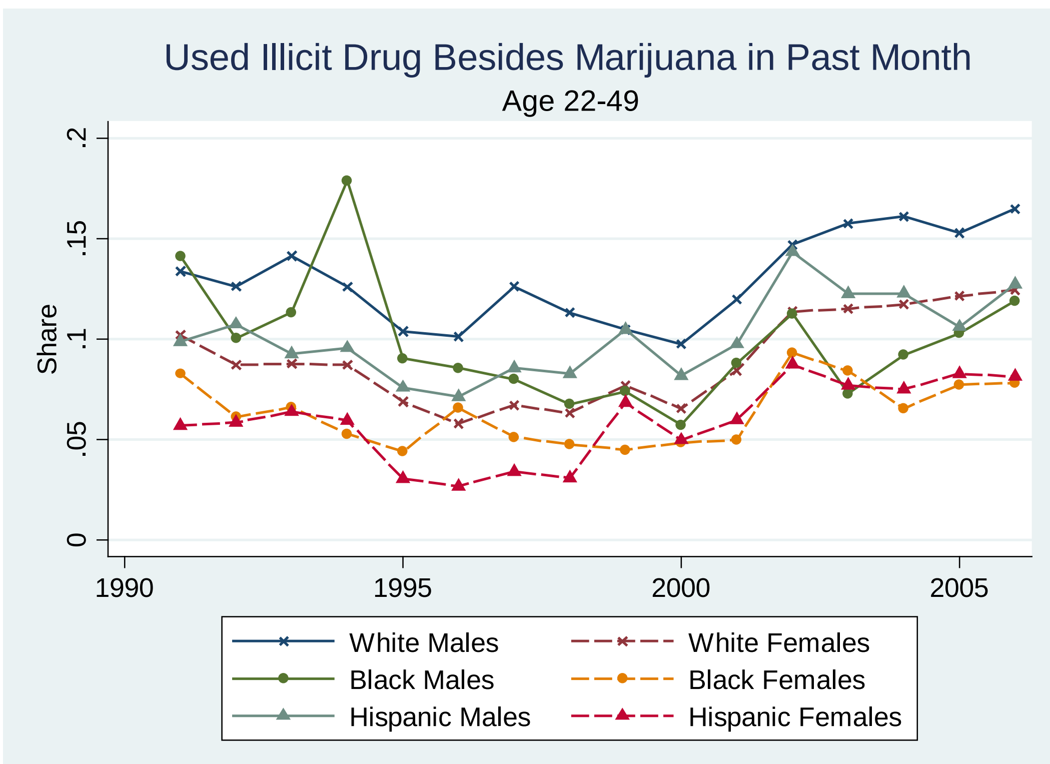


5b.

Figures 5a and 5b. Source: NSDUH. Shares of respondents, ages 22-49, reporting any past month marijuana use by race/ethnicity, gender and employment status.



6a.



6b.

Figures 6a and 6b. Source: NSDUH. Shares of respondents reporting any illicit drug use or abuse of legal narcotics in the past month by race/ethnicity, gender and age.

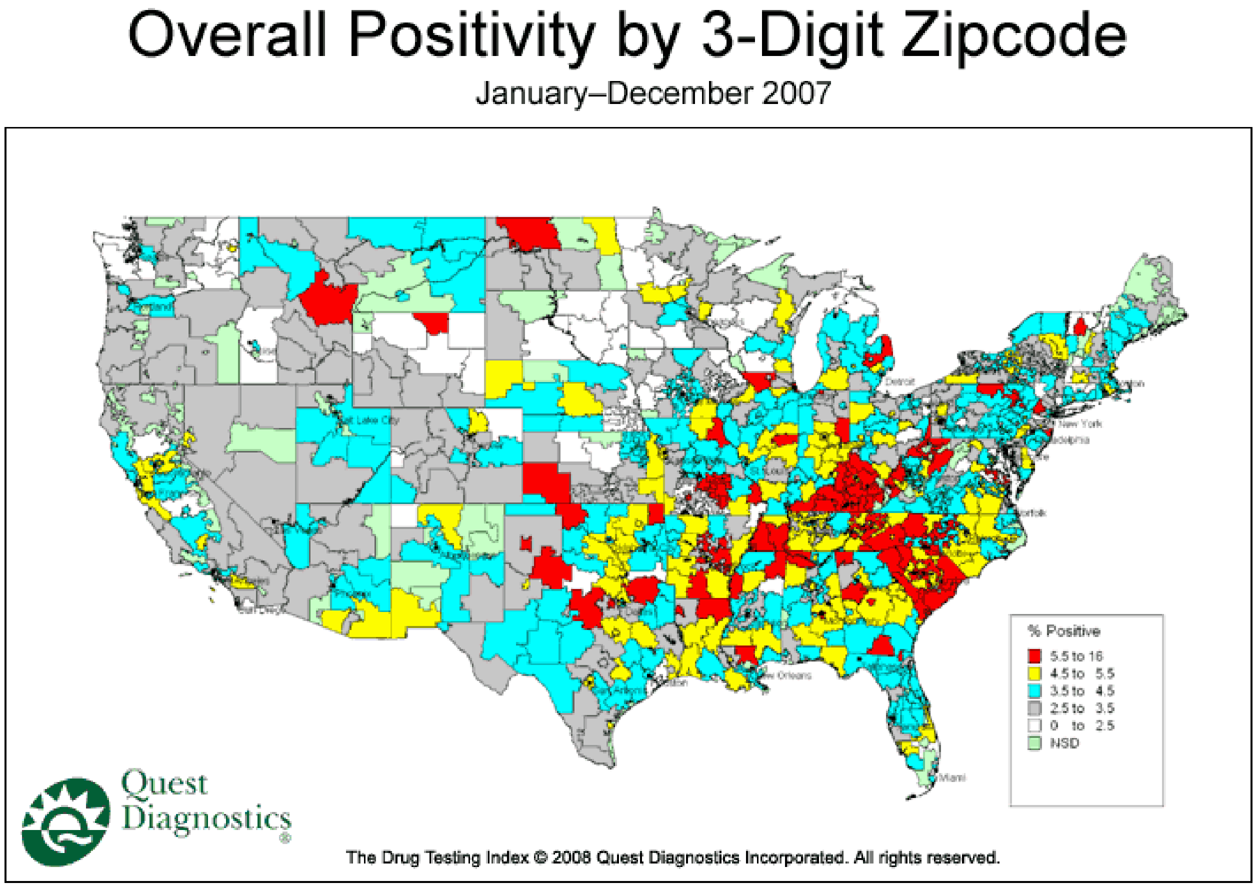
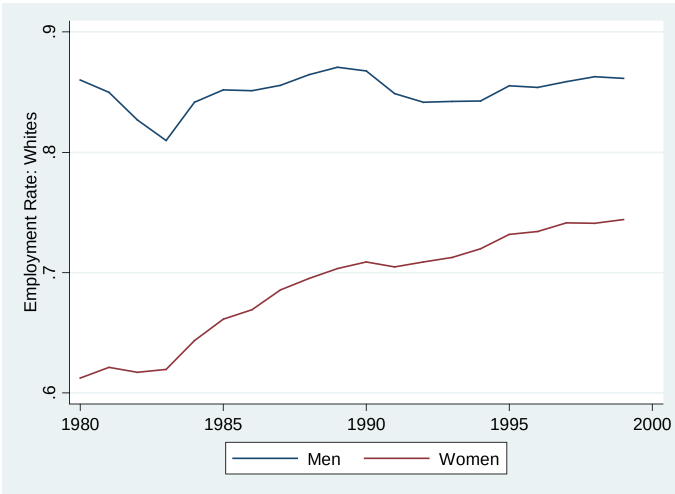


Figure 7. Source: Quest Diagnostics website. Underlying data from the Quest Diagnostics proprietary Drug Testing Index.

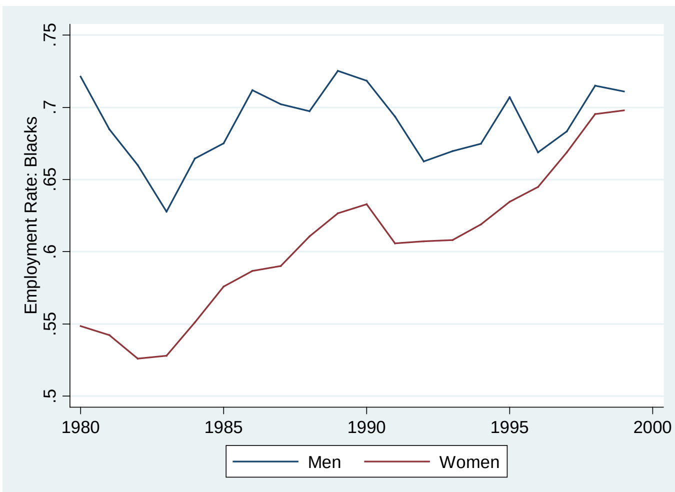
Appendix A: Robustness to the 1994 CPS Redesign

One of the main goals of the redesign was to improve labor market information collected from women. This led to a number of changes in the way women were asked about their work activities, which in turn led to discrete changes in some labor force statistics for women at the point of the redesign (Polivka, 1996). The redesign thus has the potential to cause spurious changes in relative employment trends for women. Appendix Figure A presents visual evidence that the redesign had no detectable impact on the employment series for women in my data. An additional concern is the changes in topcoding of income, from which hourly wages are constructed, that were also part of the redesign. Following Autor, Katz, and Kearney (2008), I trim the top and bottom 3% of real hourly wage earners. The hourly wage sample that I use to estimate wage equations includes only individuals with positive real hourly wages within the 3^{rd} to 97^{th} percentiles of the overall real wage distribution in the sample. As Table 3 shows, this group is about 73% of the total sample.

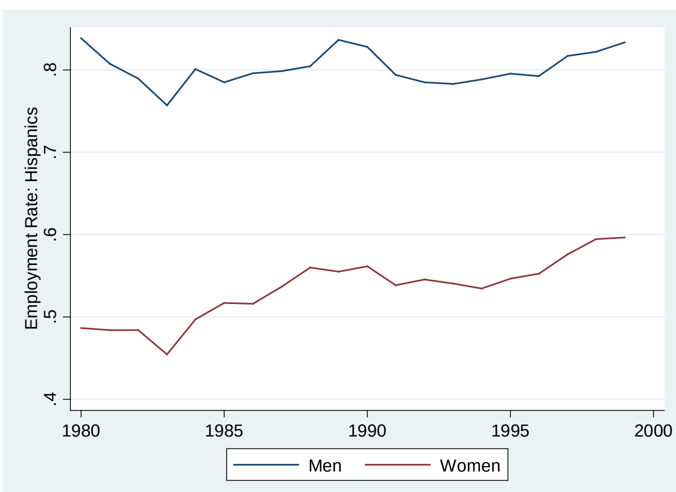
Figure A: Employment Trends in the March CPS, 1980-1999



i.

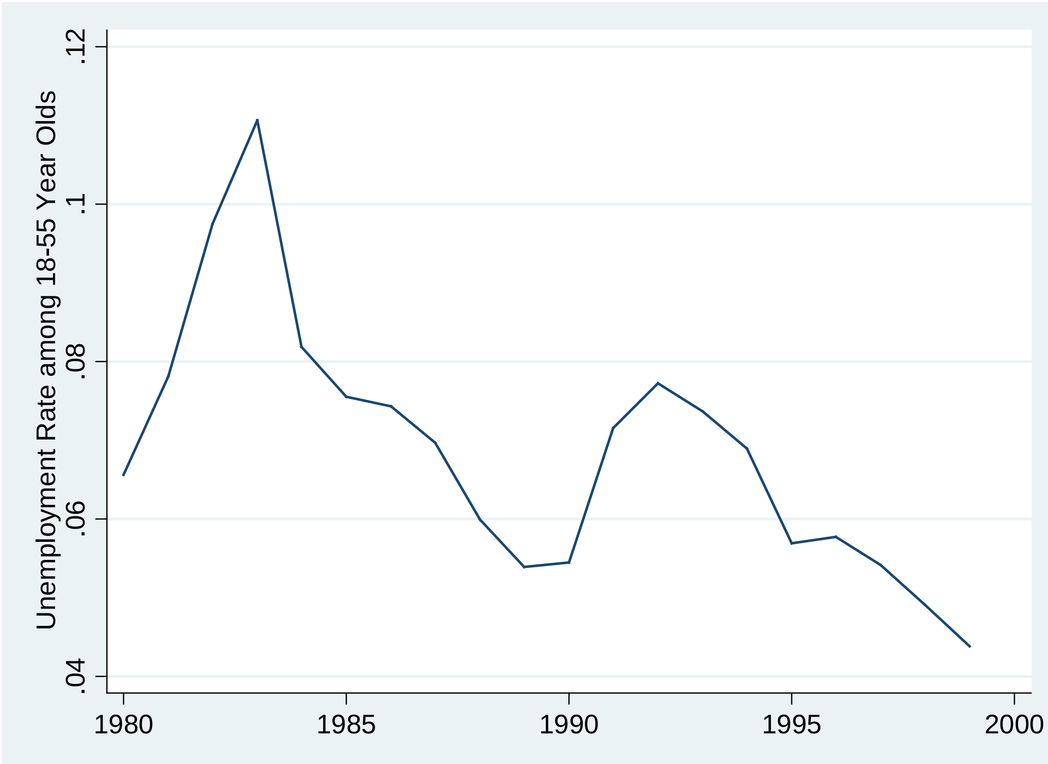


ii.



iii.

Appendix Figure B: Unemployment Rate among 18-55 Year Olds in the March CPS, 1980-1999



Notes: Source is March CPS, IPUMS version. Employment indicator constructed from *EMPSTAT* variable. CPS individual sampling weights applied.