Job Applicant Drug Screening and Employment Outcomes

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Abstract

Beginning the in late 1980s, US employers began requiring new hires to undergo drug testing before starting work, and typically rescinded a job offer if an applicant tested positive for drugs. The practice of pre-employment drug testing (PEDT) has since expanded dramatically. Results from a nationally representative survey suggest that 40-50% of employees in the US work for firms that drug screen new hires, making PEDT arguably the largest demand-side intervention in the US drug market and a widespread screening procedure.

This paper investigates the impacts of this large policy change on the US labor market. I combine several measures of PEDT intensity—including variation in the timing of PEDT laws across states— with state-level employment statistics from the Current Population Survey spanning 1979-2002. I first examine whether PEDT is associated with changing employment outcomes across demographic groups that differ in their drug usage rates. Preliminary results suggest that PEDT significantly increases employment for women relative to men but not for blacks relative to whites. These results are robust to accounting for the redesign of the CPS in the middle of the sample period. The second part of the paper examines ways in which PEDT may have affected screening and discrimination by employers. I conclude by examining how the advent of PEDT affected the costs of drug use.

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I. **Introduction**

Beginning the in late 1980s, US employers began requiring new hires to undergo drug testing before starting work. A job offer was typically rescinded if an applicant tested positive for drugs. The practice of pre-employment drug testing (PEDT) has since expanded dramatically. According to the National Survey of Drug Use and Health, a nationally representative survey conducted semi-annually by the U.S. Department of Health and Human Services, 40-50% of employees in the U.S. work for firms that drug screen new hires, making PEDT arguably the largest demand-side intervention in the US drug market.

The advent of PEDT constitutes a large scale intervention in the U.S. labor market. Moreover, there are reasons to believe that the effects of PEDT differed across segments of the labor force. Employers in all major industrial sectors practice PEDT, 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 even employment states.

In this paper, I use variation in the passage of state guidelines governing PEDT to produce reduced form estimates of PEDT's impact on employment outcomes for several demographic groups. My preferred specification uses measures of similarity in state industrial composition to weight PEDT legislation passed in other states. The resulting index uses variation in PEDT guidelines in industrially similar states to identify PEDT's effects on workers of a particular state. I merge information on PEDT intensity to micro level data from the Current Population Survey for the period 1979-2006 to examine the effects of PEDT policies on employment outcomes across demographic groups.

I find that the advent of PEDT led to a number of compositional changes within the labor market. Women gained employment relative to men, while employment among youths ages 18-25 declined. Women experienced significant wage gains relative to men, while blacks experienced significant wage decreases. Unemployment rates were largely unaffected by PEDT policies. Consistent with a process of diffusion in which state boundaries do not strictly delineate the use of PEDT policies, I find that these effects are concentrated in the early years of state PEDT guideline adoption. The effects are economically significant. My preferred estimates imply relative employment gains of 5% for women, employment declines of 4% for youth in states with higher values of the PEDT index. Relative wage gains for women in the same states were 12% and wage losses for blacks were 8%.

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

A. The Advent of Pre-Employment Drug Testing

Drug testing in the labor market takes three main forms: testing of applicants for a job; testing employees for cause—such as following an accident or on suspicions of drug use at work—or as part of a regular program of testing; and random testing of employees. Penalties for testing positive for drugs are typically severe. Current employees are often dismissed at may forfeit their access to workers' compensation if the test follows an accident in which the worker was injured. Prospective employees are typically denied employment and often barred from ever applying to the employer again, although it is unclear how thoroughly this latter penalty is enforced. Alcohol is not commonly part of drug screening, although its use it is easy to detect. The main exceptions to this are post-accident testing or testing for cause.

Drug testing entered the labor market in earnest starting in the early 1980s. The advent of testing by employers 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. Prior to 1983, the largest practitioner of employee drug testing was the military, which instituted a requirement that soldiers pass a drug test before they would be sent home from Vietnam.[1]

The legislative and policy history suggests that testing of job applicants lagged a few years behind policies testing current employees. In the early days of testing, employers were in many instances challenged in court by employees or unions who argued that such tests violated their Fourth Amendment rights to freedom from unreasonable searches or their Fifth Amendment rights to freedom from self-incrimination. Ironically, a series of court decisions—several of which were handed down by the Supreme Court—made clear that government employees had the strongest objections to the searches inherent in drug testing, since the employer in these cases was the government itself. Courts ruled repeatedly that the Fourth Amendment only protects citizens from searches by the government, thus private sector employees have no expectations of limits to search by their employers. Courts also ruled that the Fifth Amendment applies only to self incrimination in a criminal proceeding, not in other venues.[2] Once the legal basis for testing current employees was clarified, which happened roughly by the close of the 1980s, it was not difficult to extend the reasoning to pre-employment testing.

[1] Facts in this paragraph are taken from Tunnell (2004), Ch. 1.
[2] *Ibid.*

Unfortunately, representative data on the adoption of drug testing policies by employers does not exist, let alone data on the specific forms of testing. A few surveys, however, give us a picture of the increasing popularity of this practice. Harwell et al. (1996) provide an overview of these studies, and large increases in the prevalence of drug testing programs in general over the period from the late 1980s to the early 1990s. A representative BLS survey of large employers found that 32% tested their employees in 1988, a figure that rose to 45% by 1991. Hartwell and coauthors note, "Even with the methodological differences among these studies, it seems reasonable to conclude that the drug testing of job applicants and current employees has become much more common in recent years."

The most accurate information I have found to date on the use of PEDT comes from state-level legislation governing the use of PEDT. I refer to this legislation as "guidelines," rather than laws, since no state mandates pre-employment drug testing of all applicants. In most cases, testing job applicants is voluntary for employers. Fortunately, as I discuss in the next section, the variation in PEDT intensity as measured by these state level guidelines can be used in a way that captures variation in PEDT intensity that is exogenous to workers in a given state.

The language of these guidelines varies across states, but most allow employers to test job applicants if testing is in accordance with the company's written policy and applicants have already been offered the job. (This is called conditional testing.) The most frequent points of difference concern whether the guidelines require that tested positions meet any restrictions, such as safety or quality concerns, and whether PEDT is required for employers participation in the state's unemployment compensation program. As 0f 2006, PEDT was allowed in all states, although only 22 states had passed guidelines.[3] Table 1 lists the states with guidelines, along with their year of

[3] Facts in this paragraph from De Bernardo and Nieman (2006).

passage. All regions of the country are represented, and the adoption of guidelines does not appear to have a strong regional component.

B. Patterns of Drug Use

In contrast to problems with measuring the intensity of PEDT use by employers, accurate measures of drug use are available back to 1979 in the National Survey of Drug Use and Health (NSDUH).[4] The NSDUH also provides a helpful picture of the prevalence of PEDT and other forms of drug testing among employers over the last decade. Figure 7 shows that 50% of employed 22-49 year olds in the NSDUH work for employers who drug test at some point, 40% work for employers who test job applicants. These figures potentially represent undercounts of the prevalence of drug testing if employees who are never tested themselves tend to report that their employer does not test.

Figures 8 and 9 show the main patterns of drug use in the U.S. population. In some figures the data points prior to 1987 are rather noisy, as the NSDUH was conducted more infrequently and on a smaller scale in this period. I focus on marijuana use, since this is the drug most commonly detected in positive drug screens (Tunnell, 2004). Figure 8a shows that past-month marijuana use among 22-49 year olds closely tracks past-year use, and their levels differ only modestly. About 17% of this population used marijuana in the past year, and about 11% in the last month. On the other hand, about 50% of this population have tried marijuana at some point in their lives.

Figures 8b and 8c compare racial and gender 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

[4] Formerly called the National Survey of Household Drug Abuse.

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 pattern is actually reversed in the younger group. Finally, Figures 9a and 9b show that marijuana use rates among the unemployed are roughly double those of the employed. Interestingly, use among the employed declined in the 1990s but increased after 2000, following the general pattern. Rates among the unemployed were relatively stable.

C. Employer Screening

PEDT 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.[5] The most common testing kits screen for 5 to 10 different types of drugs. In some cases these include the active ingredients in prescription painkillers, for which applicants then have to provide a doctor's verification that these have indeed been prescribed. A drug test "failure" typically requires a positive screen at both the initial phase—often a stick or indicator tape used at the collection site—and a positive screen in a more sophisticated second test of the same specimen, usually conducted by a specialty lab. These second tests are highly accurate and are not considered subject to false positives.[6]

[5] 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.
[6] The Supreme Court has ruled that the procedures used in these second tests are highly accurate and admissible as evidence (Tunnell, 2004).

A bigger concern for testing firms is the rate of false negatives on these tests. While it is true that a somewhat sophisticated industry has evolved to help individuals pass drug tests, the testing industry has evolved in an equally sophisticated way to detect cheating. I have found no studies evaluating the rate of false negatives among known drug users, with or without the aid of cheating tools. The bigger threat to test validity likely comes from oversight in testing facilities. A government study found numerous lapses in testing protocol at collection sites for the federal employees drug testing program, suggesting that cheating is indeed possible if oversight at the collection site is lax (GAO, citation).

Other than the particular nature of the test, PEDT is similar to other types of employer screening.[7] Like all other screening mechanisms, employers may or may not be directly interested in the behavior they are testing. Instead, they may find it a useful correlate with harder to measure characteristics like productivity, diligence, and willingness to follow rules. Also, applicants have an incentive to lie in the screening phase. The nature of what, if anything, employers learn through drug tests and the behavioral responses of applicants are questions beyond the current scope of the paper. These will be explored in future versions.

III. Measuring Pre-Employment Drug Testing

As its history illustrates, the implementation of pre-employment drug testing differs somewhat from the typical policy change examined by economists. PEDT as a policy did not experience a zero-one change from no implementation to implementation. Instead, PEDT went from a period of very low rates of implementation in the early 1980s through a massive increase facilitated by legislative changes to current stability at high rates of implementation. The intensity of PEDT implementation also varied across states, industries, and time.

[7] Autor (2008) provides a theoretical exploration of these. Autor and Scarborough (2008) explore an example of pre-employment skills testing in detail.

I use two measures of PEDT intensity to examine the effects of increasing implementation of this policy on labor market outcomes. I present a reduced form analysis of changes in outcomes associated with the measures of PEDT intensity.

The first measure of PEDT intensity takes advantage of the creation of state laws regulating PEDT described in the previous section. The measure itself is a dummy variable for whether a state has a law governing PEDT:

(1) $postlaw_{st} = \mathrm{I}(state\ s\ has\ guidline\ law\ in\ effect\ in\ t)$

An advantage of this measure is that it allows me to develop the usual difference-in-difference estimate of the policy's effect. A disadvantage with this measure is that the D-in-D estimates likely suffer from contamination bias, particularly in the later years of my sample. This is because PEDT was not explicitly illegal prior to the passage of legal guidelines, but companies using PEDT were open to the possibility of litigation from employees who felt the tests violated their rights in some way. The threat of this legal challenge likely declined as more states passed guidelines for PEDT use, and as legal precedent increasingly established conditions under which testing was permissible. Thus, employers in the late 1990s were probably much more likely to use PEDT without explicit legal grounds from their state than were employers in the early 1980s.

To deal with this contamination bias, I create state-specific adjusted values of the index. Specifically, I weight the components of the index with measures of the similarity of a state's industrial composition to that of the state in question. The weighted index is the following:

(2) $weighted\_index_{st} = \sum_{j=1}^{50} \delta_{sjt} * postlaw_{jt}$

where δ is the Euclidian distance between the vectors of industry shares of employment in *s* and *j* in year *t*. I use data from the Bureau of Economic Analysis on state employment by industry to compute vectors of employment shares for 1979 to 2000.

The rationale for this adjusted value of the index is that employers in industry k in state j are likely to be influenced by the practices of other employers in the same industry k but a different state x. This may be because some firms span state boundaries and therefore want to implement the same policies across establishments in different states. Alternatively, this copycatting may arise through the development of professional norms within an industry, perhaps nurtured in industry publications and through industry-wide conferences. An example of this industry-wide effect is given in Tunnell (2004), who notes that when Greyhound Bus Lines instituted drug testing of their drivers in 1983, many other transportation firms felt justified in following suit.[8] Whatever the channel, it seems plausible employers in state *A* will be more likely to implement PEDT than employers in *B* if the industrial composition of employment in A is more similar to that of a third state *C* that has passed PEDT guidelines.

An important feature of this measure is that it does not use any information on whether a state has passed its own PEDT legislation. Instead, variation in the weighted index comes from changes in what other states with similar industrial compositions are doing. The differences-in-differences estimates and the weighted index estimates of PEDT's impact therefore have somewhat different properties. The identifying variation in the D-in-D estimates applies with certainty to employers in a state but potentially suffers from endogeneity if the passage of guidelines is not random as well as contamination bias if the lack of state guidelines is non-binding. In the weighted index estimates, I conjecture that the identifying variation applies to employers in a state (this is not certain), but it is less likely to suffer from endogeneity and contamination bias.

IV. Statistics from the Current Population Survey

[8] Tunnell (2004), p. 15.

I measure employment outcomes using data from the Outgoing Rotation Groups (ORG) surveys of the Current Population Survey, obtained from the NBER-produced extracts of the ORG survey microdata for 1979 to 2006.[9] Like the March CPS surveys, the ORG surveys contain information on employment outcomes that allow for calculation of hourly wages. The demographic information in the ORG surveys is more limited than in the March surveys, but the ORGs have a great advantage in that they contain respondents from every calendar month. This alleviates concerns about seasonal employment changes that differ across demographic groups. I restrict the sample to individuals in their first (of two) ORG surveys in order to avoid problems that arise through the partial overlap of individuals across calendar years in the combined set of ORG surveys.[10] This restriction means that I have a representative, random sample of U.S. households for every year of the ORG surveys.

Descriptive statistics on the final sample are given in Table 2. Keep in mind that the listed CPS variables are available in all years, but some estimates restrict the sample to the 1979-2000 period because the weighted index PEDT index cannot be calculated for a longer period without bridging industry classifications.[11] The main outcomes of interest are employment, unemployment, and real weekly wages. Employment and unemployment are measured as dummy variables. I also create dummy variables for other individual characteristics. Measuring characteristics as dummy variables allows me to interact them with the PEDT measures to examine differences in PEDT's impact across groups. Race/ethnicity is measured using indicators for black and Hispanic, where

[9] http://www.nber.org/data/cps_index.html

[10] The CPS's rotating panel design means than some individuals who are part of the sample in year x also appear in year x+1. I restrict the sample to individuals in their fourth month of CPS interviewing. Respondents are in the interview frame for four months (and given the ORG survey in their fourth month), then out for eight months, and finally in again for four months and given the ORG survey as their final CPS survey at the end of the second four months.

[11] It seems the more conservative approach to limit the dinal years of the sample rather than introduce potentially confounding variation by bridging industry classifications.

other non-white races are included in Hispanic.[12] Education is measured as a dummy variable for having some postsecondary education. I include a mean for marital status in Table 2, but I do not include marital status in any of the specifications since it is likely endogenous to education and age.

A concern with using repeated cross-sections from the CPS data over this period is the major redesign of the survey that was implemented in 1994. 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.

The preferred estimating equations I use address this concern by including group-specific year fixed effects. Any discontinuities in the level of the series of interest engendered by the redesign will be absorbed by these effects. I also present visual evidence that the redesign had no detectable impact on the series of interest in Figures 3-6. These plot employment rates, unemployment rates, and average real weekly wages for various demographic groups over the entire CPS period. A vertical line denotes 1994 in all graphs. As is clear from inspecting the figures, the redesign did not generate discontinuities in any of my series of interest.[13]

V. The Impact of PEDT on Employment Rates

A. The Estimating Equation

The specifications I estimate have the following general form:

$$(3)\ y_{ist} = \beta_0 + \beta_1 demog_i + \beta_2 policy_{st} + \beta_3 demog_i * policy_{st} + \theta_s + \theta_t + \theta_t * demog_i + \varepsilon_{ist}$$

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[12] Asian ancestry is not identified until midway through the CPS sample period.
[13] I measure employment and unemployment using the "esr," or "employment status recode" variable on the NBER files.

The *policy* variable is either the *postlaw* dummy or the *index* described in Equation 2. *Demog* is a vector of characteristics of individual *i* expressed in dummy variable form. These include gender, race (black or white), Hispanic ethnicity, and two age categories, old (age 36-55) and young (18-25).[14] The age categories were chosen with regard to patterns of drug use across age groups. The vector also includes a measure of schooling, a dummy variable indicating whether *i* has achieved any postsecondary education.

Two features of PEDT policies complicate the choice of appropriate fixed effects. First, the policy diffused nationwide over time and was not perfectly constrained within state boundaries. This means the effects of the policy may be correlated with other trends, but controlling flexibly for these trends will attenuate the policy effects I can identify. Second, the policy potentially had different effects across groups of workers, but it is not clear ex ante which groups were most affected. This makes it difficult to conclude that any coefficients in the β_3 vector are implausible. In turn, this limits the usefulness of "placebo" groups for specification checks.

I include a full set of state and year fixed effects in all specifications. These control flexibly for any long-run trends in the outcome variables that are common to all individuals, but it is worth noting that their inclusion means that I cannot identify effects of the rise in PEDT policies that are common to all workers. State and year effects alone do not control for trends that differ across demographic groups. To account for these, in include a full set of interactions between the year dummies and demographic indicators in the preferred specification. Because of the attenuation due to overcontrolling, estimates from this specification represent a lower bound on the effect of PEDT policies. For comparison, I also present estimates omitting the year dummy interactions.

[14] Hispanic includes other non-white races.

Finally, y_{ist} is one of three labor market outcomes: employment, unemployment, and log real weekly wages. In the case of the first two, I estimate Equation 3 via probit and present the results in terms of marginal effects. The wage equations are estimated via OLS.

B. Results

Results from the preferred specification are reported in Table 3. The columns of the table present separate estimations of Equation 3, changing the labor market outcome in the dependent variable as well as the manner in which age is included as a control. Columns 1, 3, and 5 present estimates from a modification of (3) that includes age and its square as controls. Columns 2, 4, and 6 include the age categories and their interactions with the PEDT index, as described above. I present both specifications to verify that results for other demographic groups are not sensitive to the way in which age is included as a control. Estimates on the other covariates change very little across specifications with the same dependent variable, so I limit discussion to results in the even numbered columns.

The first row of the table presents coefficient estimates on the PEDT index. The subsequent rows present estimates for the main effects of the demographic indicators followed immediately by their interactions with the index. The main effects all have the expected signs and are generally highly significant. Turning to the results in Column 2, the PEDT index is associated with large declines in employment for the average worker. A marginal increase in the index decreases the probability of employment by almost 6%. This effect is experienced by the average worker in the omitted categories in these specifications—these are whites, men, ages 26-35, and those without any postsecondary education.

Moving down Column 2, the interaction of the index with the female indicator shows this result is almost entirely attenuated for women. There is no difference in the impact of the index for blacks or Hispanics, or across education groups. Young workers, on the other hand, experience an additional marginal impact of the index on their probability of employment of 4%.

Estimates of the impact of the PEDT measure on unemployment probabilities are presented in Column 4. The first row shows that PEDT increased the overall probability of unemployment. In contrast to the employment results, looking down the row shows that this effect does not differ significantly across any of the demographic groups identified. Results from the wage equation are in Column 6. There were no wage effects of PEDT for the average worker, but there were large and significant changes to wages for blacks and women. Interestingly, these changes worked in opposite directions. A one unit increase in the index raised women's wages by 12%. Keep in mind that this is a large change in the index and was probably not experienced in most states.[15] Nevertheless, this represents significant changes in women's earning relative to men. PEDT policies had the opposite effect on black wages, with a unit increase in the index leading to an 8% decline in black wages. The relative earnings of other groups were unaffected by PEDT.

Table 4 presents estimates of the Equation 3 specifications omitting the interactions of the demographic variables with year effects. I again focus on the estimates in Columns 2, 4, and 6. In general, the estimated changes associated with PEDT are much larger in Table 4. This would be expected if the group specific year effects absorbed some of the relevant variation in the PEDT index. Results in column 2 again show employment declines for the average worker associated with PEDT policies, but these are much larger than those in Table 3. The employment effects for women are again entirely attenuated. In fact, combining the point estimates on *index* and *index x female* suggests that the probability of employment may have increased for women with the advent of PEDT. I again find large negative effects on youth employment but no effects on black or Hispanic employment. The column 2 results also show increases in employment of individuals with some

[15] The mean of the index over the entire CPS sample is 0.07 with a standard deviation of 0.15. The max is 2.45. Two states—the District of Columbia and Nevada—had index values of 1 or greater.

postsecondary education associated with PEDT policies. I return to the question of PEDT's impact across education groups shortly.

Like its counterpart in Table 3, column 4 shows that PEDT policies had little impact on unemployment rates. The only difference across the two tables is that estimates in Table 4 show a significant decline in female unemployment following PEDT increases. The column 6 results show more contrast with Table 3. The Table 4 wage equation estimates show large declines in wages for the average worker. These losses are entirely offset for women. Again, blacks experience significantly larger wage losses following PEDT increases. The coefficient of 0.08 on *black x index* is identical to that in Table 3. Finally, the Table 4 estimates show large wage increases for more educated (some postsecondary) and older workers.

How plausible are the results in Table 4, given what we know about the importance of background trends in labor market outcomes over this period? More to the point, how plausible is it to say that the Table 3 results represent an underestimate of the true effects of PEDT policies on the outcomes of interest? Figures 3 through 6 provide some evidence on this question by plotting trends in labor market outcomes over the CPS sample period. There are differential trends for some groups that could bias upwards the results in Table 4 if they are driven by factors other than PEDT. For example, women gained relative to men in employment over the mid- to late-1980s, as shown in Figure 3A. This gain coincided with steep increases in the PEDT index. Thus it is likely that some of the employment gains for women attributed to PEDT increases in Table 4 are actually driven by other factors.

How severe is this bias? After comparing Figures 3-6 to Tables 3 and 4, it is clear that although there are trends that coincide with PEDT, these are not always associated with finding significant impacts of PEDT on a group. For example, Figure 3b shows that blacks made employment gains relative to whites over the latter part of the 1980s—just as women gained relative to me—but the Table 4 estimates show no effect of PEDT on increasing employment among blacks. My conclusion is that the Table 4 estimates are upward biased, at least in some instances, but that the Table 3 estimates also represent a lower bound.

C. Alternative Specifications and Robustness Checks

In Table 5, I use a straightforward alternative measure of PEDT intensity: the *postlaw* dummy variable described in Equation (1). As discussed above, there are reasons to prefer the weighted index to the indicator variable as a measure of PEDT intensity, but the *postlaw* variable has an advantage in that it applies with certainty to employers in a given state at a point in time.

The specifications in Table 5 include year fixed effects only. Having found no difference across specifications using alternative age controls, I only report estimates using the age indicators as controls. In column 1, I find the same pattern of group differences in the effects of PEDT that I reported in Table 4. PEDT is associated with significant employment declines for the average individual. Women escape these employment effects, but they are magnified for youth. In contrast to Table 4, I find that blacks, Hispanics and older workers all experience small but significant declines in employment probability following the adoption of PEDT guidelines by their state.

Using this measure, I find small increases in the probability of unemployment for women, older workers, and more educated workers but small decreases for blacks. The pattern of wage changes, in column 3, is also similar to that in Table 4. Wage decreases for the average worker associated with PEDT are offset for women and more educated workers but magnified for blacks.

The striking difference between Tables 4 and 5 is in the magnitudes of the estimated effects. In most instances, the Table 5 estimates are smaller than the Table 5 estimates by an order of magnitude. Also, I find no significant effects of PEDT in a specification using the *postlaw* measure and including group specific year effects. These facts suggest that contamination bias using the *postlaw* measure may indeed be severe. A useful strategy may be to pursue additional refinements that further narrow the treatment and control groups to instances in which an increase in PEDT can be precisely measured and compared with outcomes in a valid control group.

I conduct a check on the diffusion idea that motivates the weighted index measure of PEDT intensity. If PEDT had real effects on labor market outcomes but diffused in practice such that later law changes were relatively toothless, then we would expect that early increases in measured PEDT intensity had the largest effects. I test this hypothesis by restricting the sample period to 1979-1993. I use the 1993 cutoff because it coincides with the last year of data before the CPS redesign. These estimates thus provide an additional robustness check that the redesign itself is not generating spurious results.

The results of this analysis are in Table 6. Note that the Table 6 specifications include group specific year effects. They are thus most comparable to the estimates in Table 3. Consistent with the diffusion process postulated for PEDT, the estimates in Table 6 suggest larger impacts of PEDT than those in Table 3.

VI. Conclusion

I find that the advent of PEDT led to a number of compositional changes within the labor market. Women gained employment relative to men, while employment among youths ages 18-25 declined. Women experienced significant wage gains relative to men, while blacks experienced significant wage decreases. Unemployment rates were largely unaffected by PEDT policies. Consistent with a process of diffusion in which state boundaries do not strictly delineate the use of PEDT policies, I find that these effects are concentrated in the early years of state PEDT guideline adoption. The effects are economically significant. My preferred estimates imply relative employment gains of 5% for women, employment declines of 4% for youth in states with higher values of the PEDT index. Relative wage gains for women in the same states were 12% and wage losses for blacks were 8%.

This paper has generated a number of questions. First, can more refined estimates of PEDT's impact on labor market outcomes be constructed? Second, what were the mechanisms through which PEDT had the outcomes I find? Did women make inroads into occupations and industries in which they had previously been underrepresented, raising their wages and employment? Were blacks crowded into lower paying jobs in industries where testing is less prevalent? What model of employer screening explains the observed changes following the advent of PEDT? These questions will be explored in subsequent versions of this paper. The project also raises additional questions that may provide avenues for future research. How much did PEDT contribute to the inequality trends observed over the late 1980s and early 1990s? Do employers prefer pre-employment testing to testing current employees for reasons of fairness or morale? And finally, has employee drug testing raised productivity?

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Table 1: States with PEDT Laws

Connecticut	(1987)
Iowa	(1987)
Minnesota	(1987)
Utah	(1987)
Vermont	(1987)
Florida	(1989)
Maine	(1989)
Louisiana	(1990)
Mississippi	(1991)
California	(1993)
Georgia	(1993)
Oklahoma	(1993)
Arizona	(1994)
Colorado	(1994)
Rhode Island	(1994)
Alabama	(1995)
Tennessee	(1996)
Idaho	(1997)
Montana	(1997)
Ohio	(1997)
South Carolina	(1997)
Arkansas	(1999)

Notes: Source: De Bernardo and Nieman (2006). Year of passage is in parentheses. California and Colorado are classified as having laws, although the legislation in these cases was passed at the city level, in San Francisco and Denver, respectively.

Table 2: Descriptive Statistics for the CPS Sample, 1979-2006

Variable	Mean	Observations
Age	35.56	3133825
Employed	0.76	3121298
Unemployed	0.05	3121298
Real weekly wage	444.07	2105150
Female	0.52	3133825
Black	0.10	3133825
Hispanic and Other Non-white	0.12	3133825
Any postsectondary	0.50	3128339
Married	0.59	3133825
Year PEDT law passed in state	1992.72	1322984

Table 3: Estimates Using Weighted PEDT Index, Group Specific Year Effects

Dependent Variable	1 Employed	2 Employed	3 Unemployed	4 Unemployed	5 Log Real Wage	6 Log Real Wage
Index	-0.069	-0.059	0.013	0.014	-0.044	-0.053
	(0.030)*	(0.033)+	(0.008)+	(0.008)+	(0.037)	(0.034)
Female	-0.264	-0.26	-0.003	-0.003	-0.561	-0.568
	(0.006)**	(0.006)**	(0.002)+	(0.002)+	(0.012)**	(0.013)**
x Index	0.048	0.048	0.002	0.002	0.122	0.122
	(0.025)+	(0.025)+	(0.003)	(0.003)	(0.036)**	(0.037)**
Black	-0.081	-0.082	0.052	0.052	-0.144	-0.134
	(0.010)**	(0.010)**	(0.004)**	(0.004)**	(0.020)**	(0.021)**
x Index	-0.005	-0.007	-0.004	-0.004	-0.083	-0.083
	(0.018)	(0.018)	(0.006)	(0.006)	(0.025)**	(0.026)**
Hispanic	-0.056	-0.054	0.009	0.009	-0.145	-0.151
	(0.019)**	(0.018)**	(0.002)**	(0.002)**	(0.020)**	(0.022)**
x Index	0.032	0.036	-0.007	-0.007	0.015	0.007
	(0.026)	(0.025)	(0.007)	(0.007)	(0.09)	(0.09)
Postsec. Ed.	0.062	0.063	-0.021	-0.021	0.171	0.169
	(0.005)**	(0.005)**	(0.001)**	(0.001)**	(0.009)**	(0.008)**
x Index	0.014	0.012	-0.002	-0.002	0.004	0.004
	(0.026)	(0.028)	(0.006)	(0.006)	(0.068)	(0.065)
Age	0.036		-0.004		0.133	
	(0.001)**		(0.000)**		(0.002)**	
Age squared	0.000		0.000		-0.002	
	(0.000)**		(0.000)**		(0.000)**	
Age 36-55		-0.008		-0.019		0.073
		(0.005)		(0.002)**		(0.005)**
x Index		0.006		-0.002		-0.008
		(0.017)		(0.005)		(0.025)
Age 18-25		-0.08		0.035		-0.395
		(0.005)**		(0.002)**		(0.010)**
x Index		-0.04		-0.002		0.055
		(0.018)*		(0.002)		(0.069)
Constant					3.38	6.032
					(0.035)**	(0.014)**
Observations	2464703	2464703	2464703	2464703	1651572	1651572
R-squared					0.3	0.28

Notes: Data are individuals from the CPS Outgoing Rotation Groups, 1979-2000, with month-in-sample = 4 and aged 18-55. Specifications in which the dependent variable is Employed or Unemployed are estimated by probit. Log wage equations estimated via OLS. All specifications contain a full set of state and year fixed effects plus interactions of year effects with all included demographic variables. Robust standard errors clustered on state in parentheses. ** indicates significance at the 1% level, * at the 5%, and + at the 10%.

Table 4: Estimates Using Weighted PEDT Index, Year Fixed Effects

Dependent Variable	1 Employed	2 Employed	3 Unemployed	4 Unemployed	5 Log Real Wage	6 Log Real Wage
Index	-0.173	-0.134	-0.004	-0.012	-0.341	-0.343
	(0.052)**	(0.043)**	(0.008)	(0.013)	(0.123)**	(0.123)**
Female	-0.187	-0.186	-0.012	-0.012	-0.459	-0.46
	(0.005)**	(0.005)**	(0.001)**	(0.001)**	(0.013)**	(0.013)**
x Index	0.183	0.181	0.021	0.021	0.343	0.345
	(0.064)**	(0.063)**	(0.008)*	(0.008)*	(0.086)**	(0.087)**
Black	-0.101	-0.1	0.05	0.05	-0.151	-0.147
	(0.009)**	(0.009)**	(0.002)**	(0.002)**	(0.015)**	(0.015)**
x Index	0.017	0.018	-0.011	-0.011	-0.071	-0.084
	(0.016)	(0.017)	(0.009)	(0.008)	(0.027)*	(0.027)**
Hispanic	-0.081	-0.079	0.013	0.014	-0.179	-0.18
	(0.011)**	(0.011)**	(0.002)**	(0.002)**	(0.021)**	(0.021)**
x Index	0.034	0.04	0.006	0.008	-0.059	-0.049
	(0.024)	(0.024)+	(0.009)	(0.009)	(0.056)	(0.056)
Postsec. Ed.	0.091	0.096	-0.026	-0.026	0.269	0.278
	(0.004)**	(0.004)**	(0.001)**	(0.001)**	(0.010)**	(0.010)**
x Index	0.059	0.058	0.014	0.012	0.316	0.321
	(0.024)*	(0.026)*	(0.01)	(0.009)	(0.141)*	(0.142)*
Age	0.036		-0.004		0.133	
	(0.001)**		(0.000)**		(0.002)**	
Age squared	0.000		0.000		-0.002	
	(0.000)**		(0.000)**		(0.000)**	
Age 36-55		0.003		-0.016		0.132
		(0.003)		(0.001)**		(0.005)**
x Index		-0.022		0.02		0.082
		(0.018)		(0.011)+		(0.047)+
Age 18-25		-0.105		0.025		-0.503
		(0.004)**		(0.001)**		(0.011)**
x Index		-0.11		0.000		-0.218
		(0.041)**		(0.003)		(0.132)
Constant					3.297	5.949
					(0.034)**	(0.017)**
Observations	2464703	2464703	2464703	2464703	1651572	1651572
R-squared					0.3	0.28

Notes: Data are individuals from the CPS Outgoing Rotation Groups, 1979-2000, with month-in-sample = 4 and aged 18-55. Specifications in which the dependent variable is Employed or Unemployed are estimated by probit. Log wage equations estimated via OLS. All specifications contain a full set of state and year fixed effects. Robust standard errors clustered on state in parentheses. ** indicates significance at the 1% level, * at the 5%, and + at the 10%.

Table 5: Estimates using Law Changes Only, Year Fixed Effects

Dependent Variable:	1 Employment	2 Unemployment	3 Log Weekly Wage
Postlaw	-0.025	-0.002	-0.059
	(0.007)**	(0.003)	(0.024)*
Female	-0.17	-0.011	-0.435
	(0.004)**	(0.001)**	(0.010)**
x Postlaw	0.023	0.005	0.069
	(0.006)**	(0.002)**	(0.017)**
Black	-0.098	0.048	-0.148
	(0.009)**	(0.002)**	(0.012)**
x Postlaw	0.018	-0.005	-0.036
	(0.009)*	(0.002)**	(0.013)**
Hispanic	-0.07	0.011	-0.169
	(0.007)**	(0.001)**	(0.025)**
x Postlaw	0.019	0.000	-0.033
	(0.006)**	(0.002)	(0.03)
Postsec. Ed.	0.1	-0.024	0.306
	(0.003)**	(0.001)**	(0.010)**
x Postlaw	0.006	0.003	0.047
	-0.005	(0.001)**	(0.024)+
Age 36-55	0.004	-0.015	0.141
	(0.002)+	(0.001)**	(0.003)**
x Postlaw	-0.008	0.005	0.006
	(0.003)*	(0.001)**	(0.004)
Age 18-25	-0.114	0.025	-0.519
	(0.005)**	(0.001)**	(0.011)**
x Postlaw	-0.011	-0.001	-0.027
	(0.005)*	(0.001)	(0.021)
Constant			5.934
			(0.017)**
Observations	3115813	3115813	2103264
R-squared			0.27

Notes: Data are individuals from the CPS Outgoing Rotation Groups, 1979-2006, with month-in-sample = 4 and aged 18-55. Specifications in which the dependent variable is Employed or Unemployed are estimated by probit. Log wage equations estimated via OLS. All specifications contain a full set of state and year fixed effects. Robust standard errors clustered on state in parentheses. ** indicates significance at the 1% level, * at the 5%, and + at the 10%.

Table 6: Estimates using Weighted PEDT Index, Group Specific Year Effects, Early Years 1979-1993

Dependent Variable:	1 Employment	2 Unemployment	3 Log Weekly Wage
Index	-0.135	0.037	-0.053
	(0.053)*	(0.020)+	(0.063)
Female	-0.266	-0.004	-0.568
	(0.006)**	(0.002)+	(0.013)**
x Index	0.093	-0.001	0.206
	(0.038)*	(0.006)	(0.084)*
Black	-0.085	0.058	-0.131
	(0.010)**	(0.004)**	(0.021)**
x Index	0.036	-0.022	-0.153
	(0.054)	(0.019)	(0.058)*
Hispanic	-0.061	0.012	-0.163
	(0.018)**	(0.003)**	(0.022)**
x Index	0.116	-0.03	0.05
	(0.077)	(0.027)	(0.205)
Postsec. Ed.	0.064	-0.023	0.167
	(0.005)**	(0.001)**	(0.008)**
x Index	0.029	0.001	0.052
	(0.055)	(0.01)	(0.075)
Age 36-55	-0.009	-0.021	0.073
	(0.005)	(0.002)**	(0.005)**
x Index	0.021	-0.012	-0.032
	(0.021)	(0.006)*	(0.046)
Age 18-25	-0.082	0.038	-0.395
	(0.005)**	(0.002)**	(0.010)**
x Index	-0.067	-0.011	0.132
	(0.041)	(0.008)	(0.087)
Constant			6.037
			(0.015)**
Observations	1757650	1757650	1159582
R-squared			0.28

Notes: Data are individuals from the CPS Outgoing Rotation Groups, 1979-1993, with month-in-sample = 4 and aged 18-55. Specifications in which the dependent variable is Employed or Unemployed are estimated by probit. Log wage equations estimated via OLS. All specifications contain a full set of state and year fixed effects plus interactions of year effects with all included demographic variables. Robust standard errors clustered on state in parentheses. ** indicates significance at the 1% level, * at the 5%, and + at the 10%.

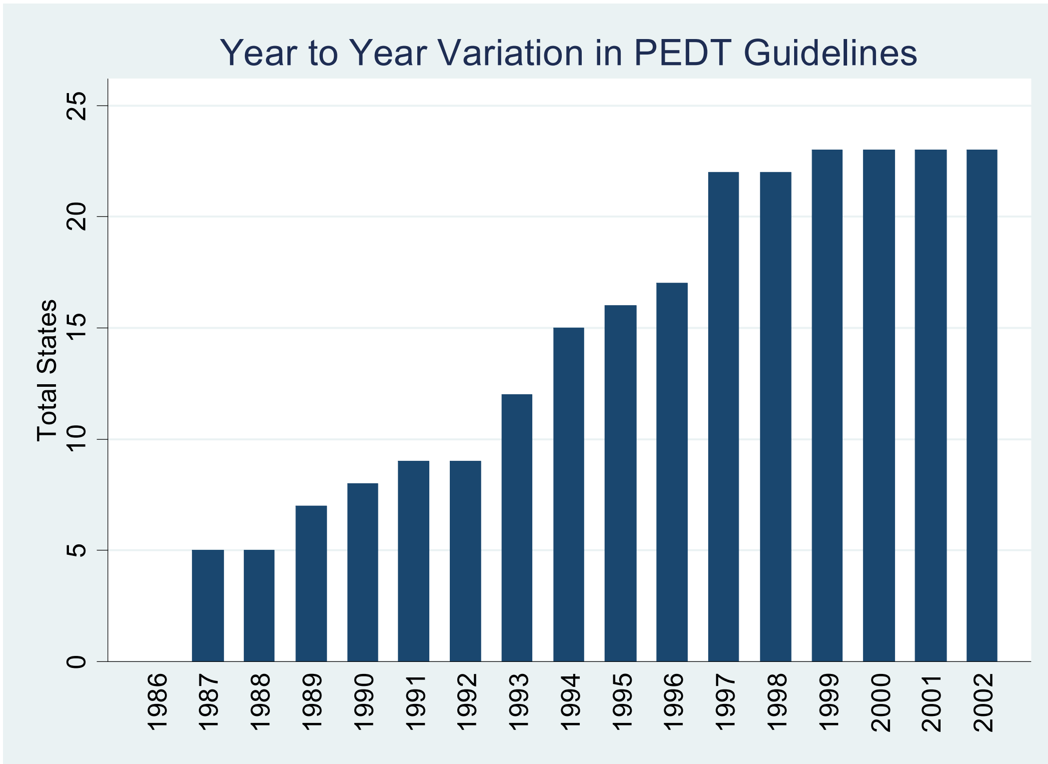

Year to Year Variation in PEDT Guidelines
Total States
0
5
10
15
20
25
1986
1987
1988
1989
1990
1991
1992
1993
1994
1995
1996
1997
1998
1999
2000
2001
2002

Figure 1.

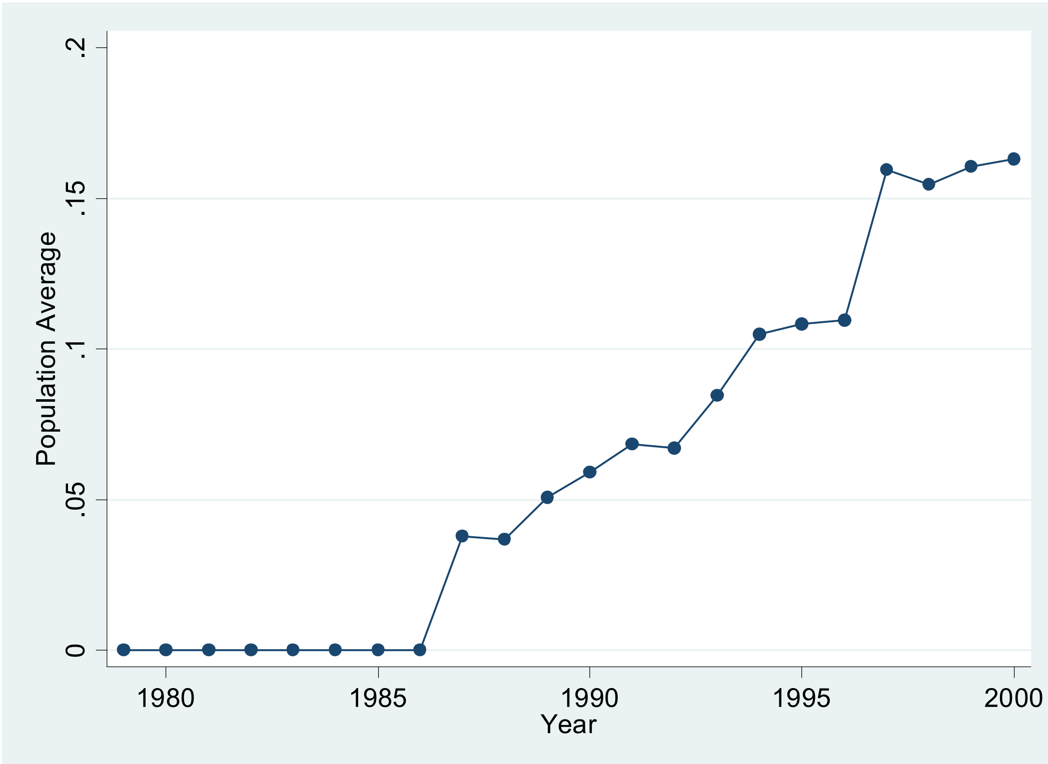
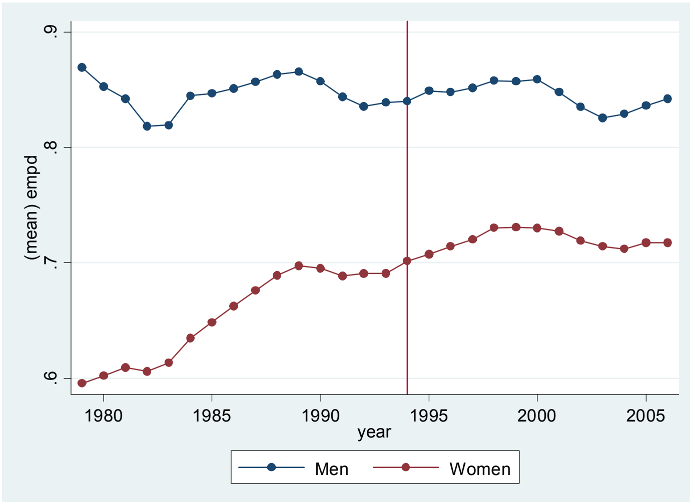


Figure 2: Population average of weighted PEDT index over time.

Figure 3: Labor Market Outcomes over Time in the CPS: Employment



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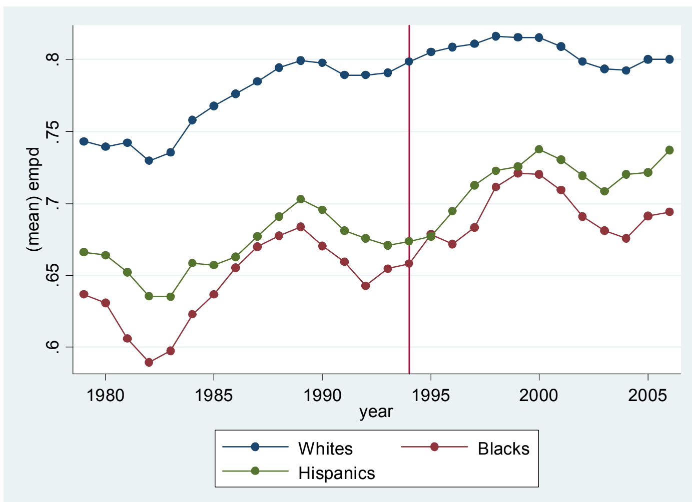

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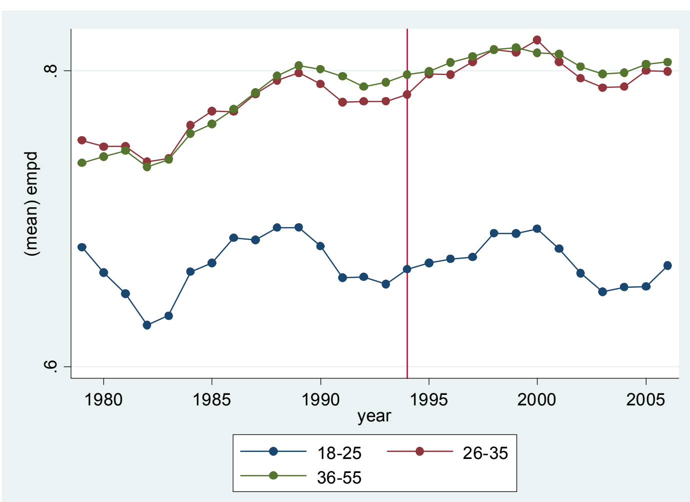
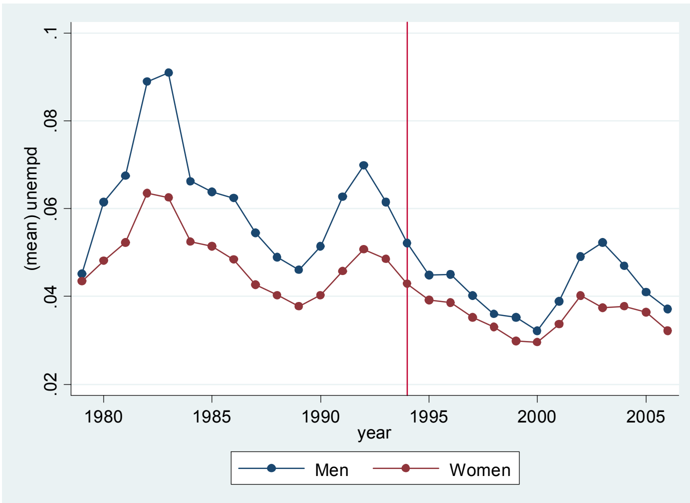


c.

Notes: Data are averages from the CPS Outgoing Rotation Groups Microdata, 1979-2006. Sample restricted to those with month-in-sample = 4 and aged 18-55. Line denotes the year of the CPS redesign, 1994.

Figure 4: Labor Market Outcomes over Time in the CPS: Unemployment Rates



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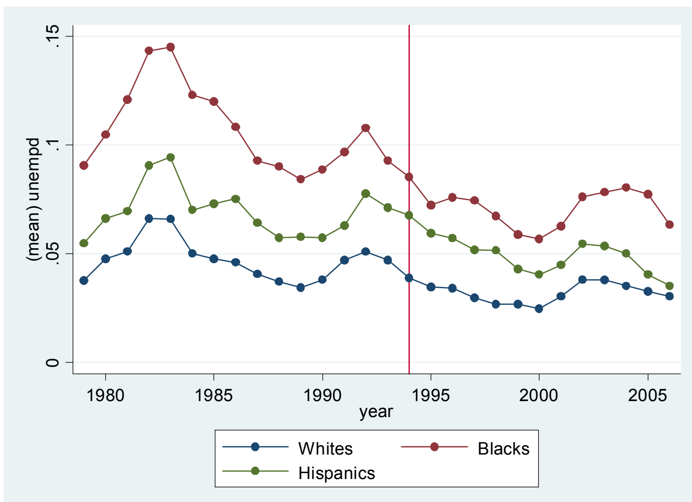

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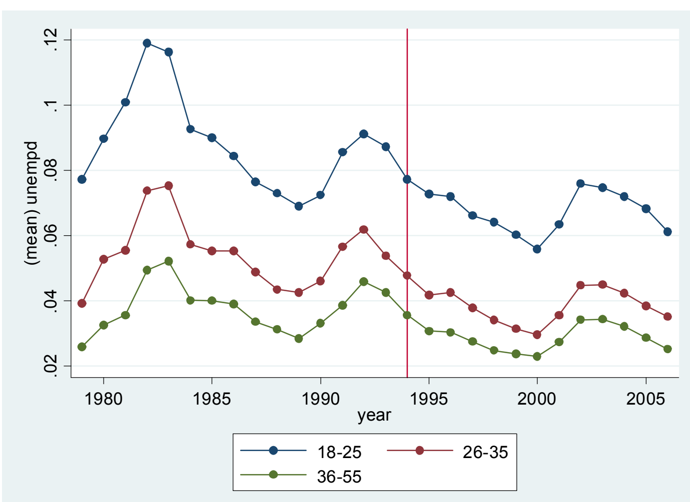

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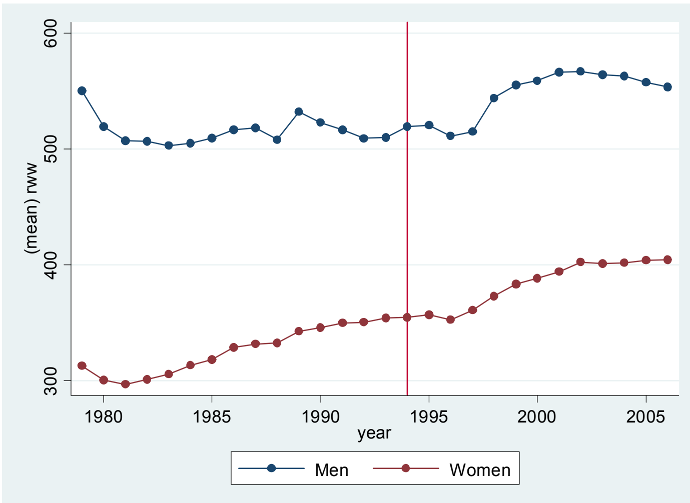
Figure 5: Labor Market Outcomes over Time in the CPS: Real Weekly Wages



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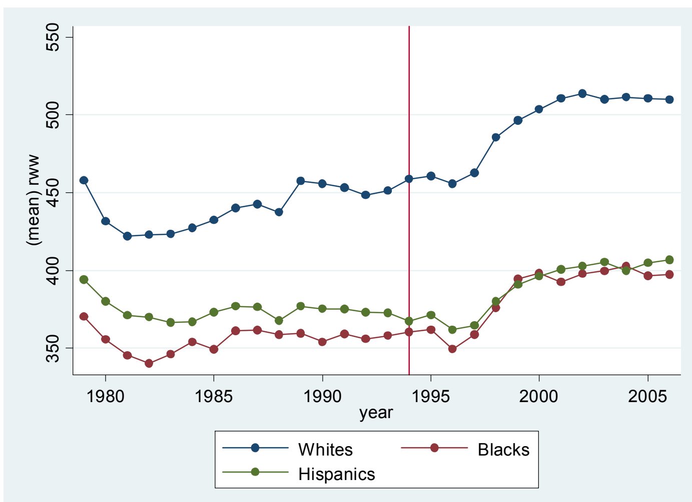

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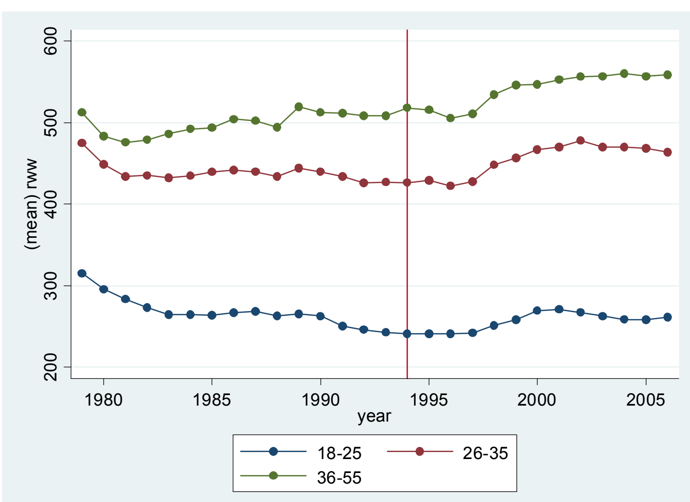

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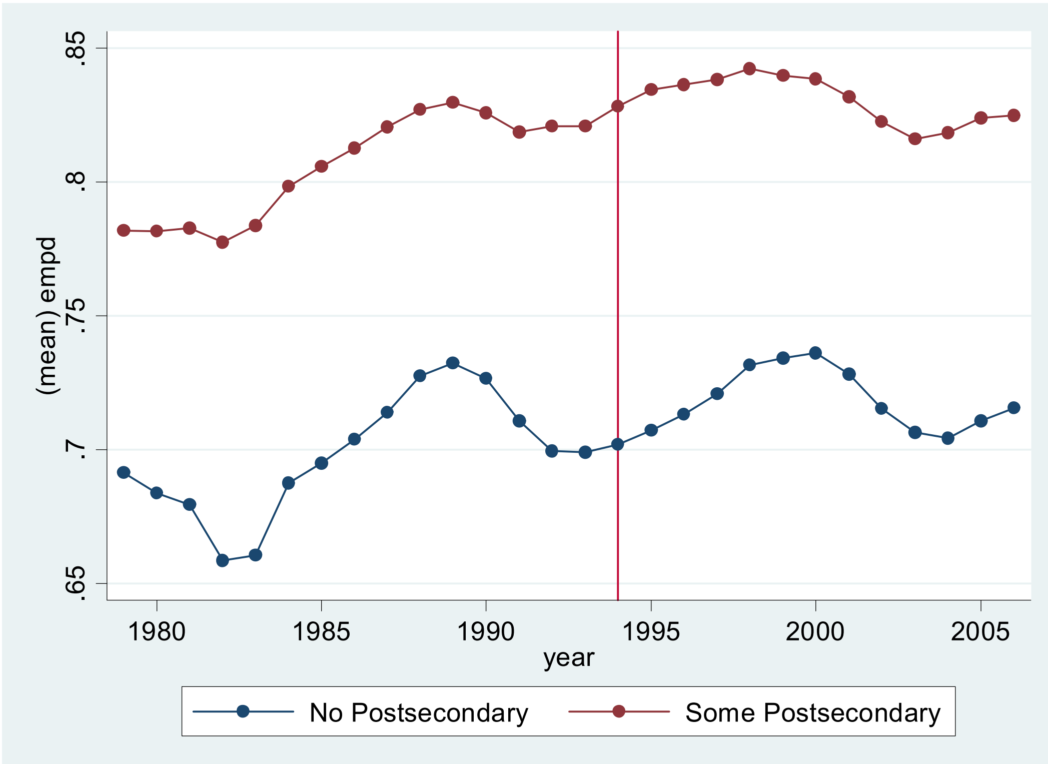


Figure 6: Employment rates across education groups.

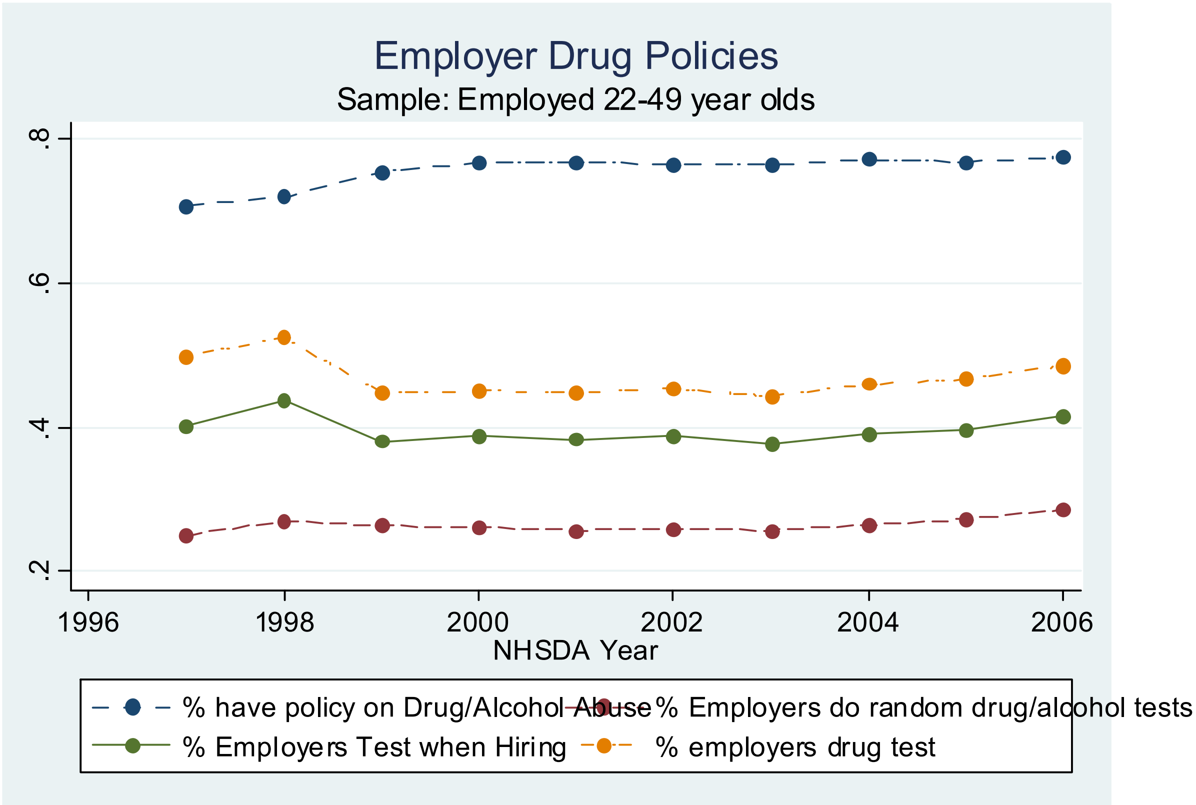
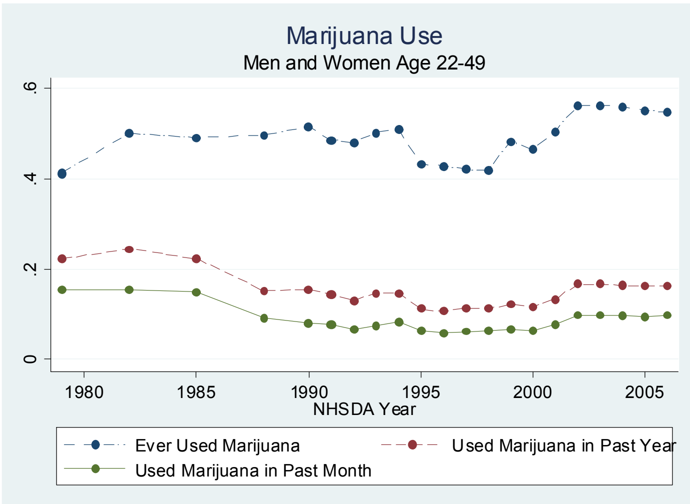


Figure 7. Employer Drug Testing in the National Survey of Drug Use and Health.

Figure 8: Marijuana Use Patterns in the NSDUH



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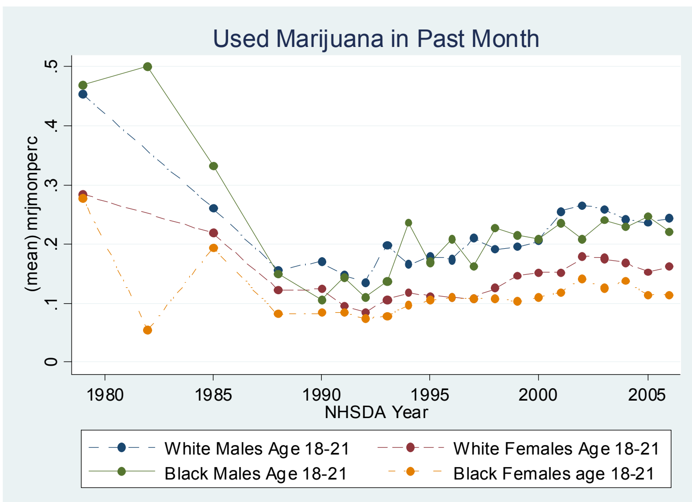

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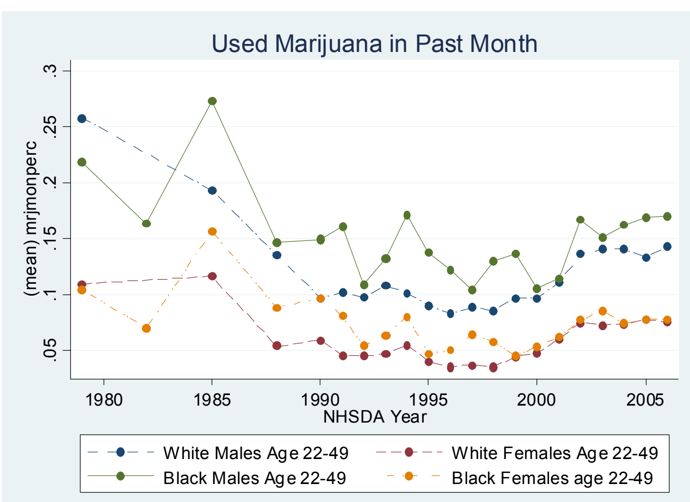

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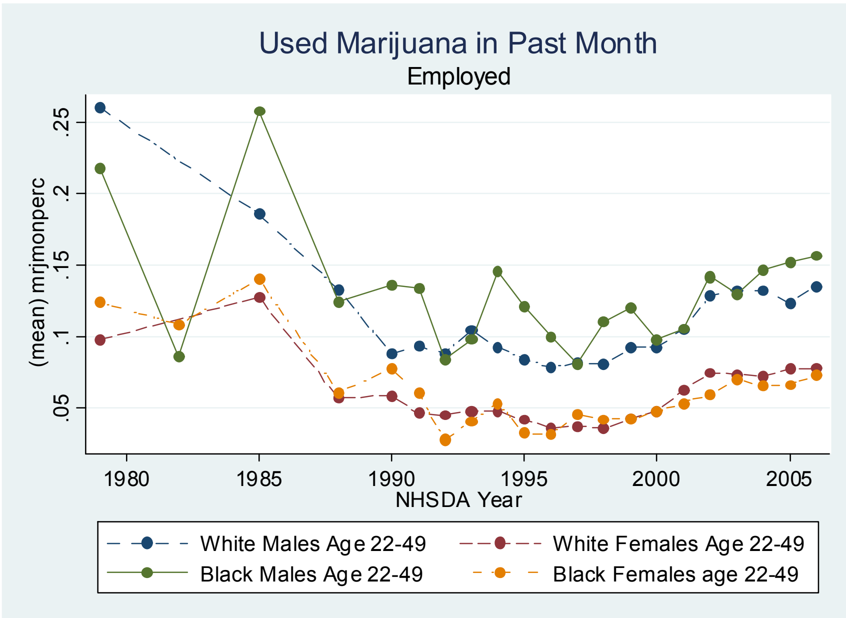
Figure 9: Marijuana Use among the Employed and Unemployed in the NSDUH



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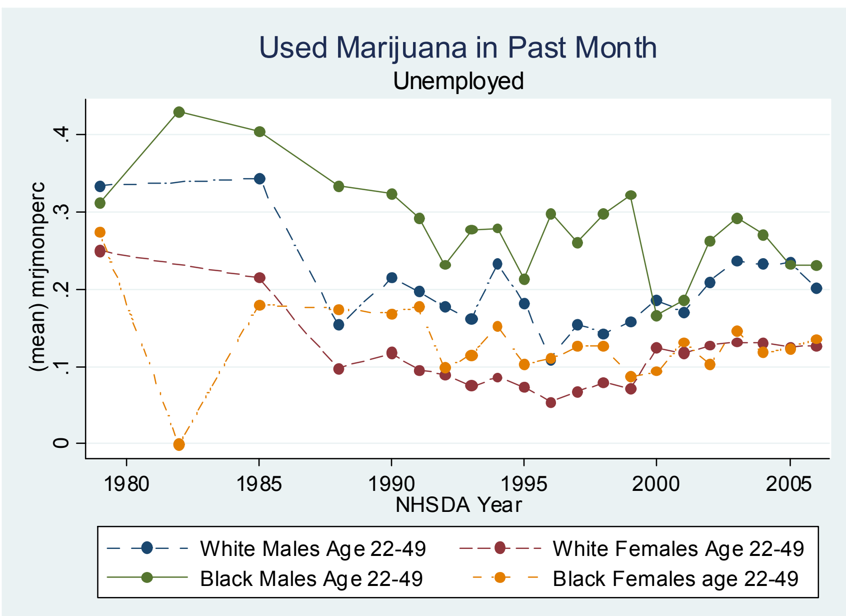

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