Effects of Drug Testing Policies on Marriage and Fertility of Black Women

Alfia Karimova

January 20, 2019

Preliminary draft

Most current version:
http://www.alfiakarimova.com/AlfiaKarimovaDrugTestingJMP.pdf

Abstract

This paper evaluates the consequences of workplace drug testing policies on marriage and fertility of black women. I exploit variation in the timing of state regulation on drug testing to evaluate the effects on the male-female earnings differential, marriage, childbearing, and children's outcomes. The results suggest that pro-testing regulation leads to a 7.3% increase in earnings for black women and a 3.1% increase in earnings for black men, decreasing the male-female wage gap. Despite economically and statistically significant changes in economic conditions, the results suggest no change in marital status. In addition, black women with at least one child have 0.1 fewer children after the policy. I also show that the policy is associated with an improvement in living circumstances of households with a black household head, suggesting that the existing narrative that the War on Drugs – and drug testing – leads exclusively to a deterioration of black families does not capture the full extent of the consequences.

1 Introduction

Almost half of U.S. workers are employed at establishments with a drug-testing program (Figure 1). The prevalence of workplace drug testing rose rapidly since the 1980s, when fewer than 5% of companies reported having a drug-testing program (U.S. Department of Labor, 1989). As part of the War on Drugs, drug testing for work may be expected to adversely affect blacks in the labour market. It is commonly suggested that the War on Drugs disproportionally affects minorities, especially blacks (Alexander, 2012; Coates, 2015; Lee, 2016; Rothwell, 2014; Tonry, 1994; Williams, 2016). Surveys indicate that respondents - including black respondents - think of a typical user as black (Burston et al., 1995; Miller-Day & Barnett, 2004). Blacks and Hispanics are more likely to be tested for illicit drugs in a healthcare setting (Kon et al., 2004; Kunins et al., 2007; Marcin et al., 2003; Moskowitz et al., 2012), where drug user status may adversely affect medical care received (Carroll, 1995; McLaughlin & Long, 1996).

Yet Wozniak (2015) shows that regulation encouraging workplace drug testing is associated with higher wages for blacks, as testing enables non-using blacks to credibly signal their non-user status.[1] This paper evaluates the effects of pro-drug testing policies on marriage and fertility of black women. Following Wozniak (2015), I exploit the variation in the timing and the incidence of state regulation on employment drug testing to further examine the differential impact on earnings of black men and black women, and the changes in marital status, childbearing, and children's health and living circumstances. Using data from the Current Population Survey (CPS) 1980-2016 and public-use Vital Statistics birth records 1989-2003, I document several results.

To begin, I find evidence that pro-testing statutes - or statutes that provide incentives for employer adoption of workplace drug testing programs - lead to larger economic gains among black women than black men. In particular, the earnings of black women increase by 7.3% after pro-testing regulation, compared to a 3.1% increase for black men, implying a reduction in the male-female earnings differential. Despite statistically and economically significant changes in relative economic conditions of black men and women, I fail to find any meaningful response in marriage.[2] The results

[1] In a related study, Shoag & Veuger (2016) find that "Ban the Box" policies which limit questions on criminal background overall benefitted black men, even though employers responded by increasing experience requirements.

[2] Kearney & Wilson (2017) also report an increase in fertility after the fracking boom, but no change in marriage rates. They contrast this finding with the effects of the Appalachian coal boom in the 1970s, suggesting that social norms are also an important determinant.

point to a decline in fertility on the intensive margin. The policy has a small, negative, and not statistically significant effect on the number of children of black women (-0.03 with s.e. of 0.04), but among women with at least one child it leads to a decline of 0.09 children (s.e. 0.04). The negative childbearing effects are driven by older married women, suggesting that the observed changes represent a decline in total fertility rather than a shift in fertility timing. I also use the CPS June Fertility Supplement to confirm that the reduction in the number of own children in the household in the March CPS sample captures a reduction in *births*, rather than an increase in the incidence of children moving out of parental home.

Studying the consequences of the policy on infant health using birth records data, I find that the share of births to teenage mothers falls by 0.8% among black women with a high school degree or less. At the same time, the share of births with a low birthweight increases by 0.5%. This set of results complements several studies suggesting that economic incentives influence teenage child-bearing (An et al., 1993; Duncan & Hoffman, 1990; Kearney & Levine, 2012, 2015; Lundberg & Plotnick, 1995), as well as Dehejia & Lleras-Muney (2004) who find that black infant health improves during economic downturns because mothers are positively selected. Because this selection effect makes analysis of later child outcomes more difficult, I focus on characteristics of households with children, rather than children's outcomes. I show that pro-testing legislation leads to a decline in use of public assistance, as measured by food stamp and public housing receipt, for black households with children.

This study adds to the small but growing body of work analyzing the relationship between gender-specific shocks to labour market conditions and marriage and fertility (Black et al., 2013; Braga et al., 2018; Dorn et al., 2018; Kearney & Wilson, 2017; Lindo, 2010; Mamun et al., 2008; Schaller, 2016; Shenhav, 2018). However, research evaluating marriage and fertility response to economic conditions among blacks is limited (Schaller (2016) and Mamun et al. (2008) are the only exceptions), and no study focuses exclusively on this population. Yet racial disparities in economic conditions and marriage and fertility are well-documented, suggesting that the black experience in the United States is unique, and the lessons from the general population may not be applicable.

At the same time, the common narrative in the media is that the War on Drugs led to disintegration of black families, largely through high incarceration rates of young black men, resulting absence of male role models and poverty, and reducing social mobility (e.g. Alexander (2012); Coates (2015); Lee (2016) and Williams (2016)). Indeed, research suggests that children of incarcerated parents are at greater risk of criminal activity, incarceration, and substance abuse (Dallaire, 2007; Murray & Farrington, 2008; Parke & Clarke-Stewart, 2003). This study offers a different point of view. My research suggests that the relationship between the anti-drug efforts and condition of black families is more nuanced, and that the increase in drug testing among the American population as part of the War on Drugs improved living circumstances of children living with black parents, as measured by the decline in use of public assistance.

2 Background

2.1 Drug testing

2.1.1 History of drug testing in the workplace[3]

The rise of workplace drug testing in the last two decades of the twentieth century is closely linked to the escalation of society's pre-occupation with illegal drugs since the 1970s. The term "War on Drugs" was first used by Nixon and arguably popularized by Ronald Reagan whose 1982 speech called for coordinated effort and additional funds to tackle the issue of drug abuse in the United States (Wisotsky, 1990). Drug testing for employment was seen as a tool to decrease demand.[4] Workplace drug screening on a mass scale was initiated by the United States military in the 1960s and 1970s, prompted by high rates of heroin use among soldiers in Vietnam (Musto, 1999; Robins et al., 1974; Tunnell, 2004; Zwerling, 1993). The earliest legislation explicitly addressing drug testing of employees by employers - the Federal Rehabilitation Act of 1973 - was somewhat liberal because it outlawed discrimination by federal agencies and recipients of federal funding against handicapped persons, which included alcohol and drug users (Karch et al., 2008; Koch, 1999; Robbins, 1991; Tunnell, 2004). However, the 1978 amendment explicitly excluded alcoholic and drug abusers from these protections (Robbins, 1991; Tunnell, 2004).

In the first half of the 1980s, public's attention was focused on several tragic incidents, especially in the railroad industry, involving alcohol and illicit drugs, as well as new statistics claiming that drug use at work was not only widespread, but cost society billions of dollars (Gilliom, 1996; Karch et al., 2008; Tunnell, 2004; Zwerling, 1993). In 1986 Drug-Free Federal Workplace Executive Order

[3] See Wozniak (2015) for another review.

[4] For example, the manual for the U.S. Federal Drug Policy states that "the workplace may be the most strategic point in society from which to combat the scourge of drugs" (Zwerling, 1993).

was signed by President Reagan requiring that federal agencies "establish a program to test for the use of illegal drugs by employees in sensitive positions" (Karch et al., 2008; Tunnell, 2004). Drug-Free Workplace Act of 1988 followed, mandating that employers with federal contracts and recipients of federal grants adopt a drug-free workplace program (Tunnell, 2004; Zwerling, 1993). Department of Transportation (DOT) implemented drug testing regulations in 1989,[5] and in 1991 Omnibus Transportation Employee Testing Act required drug and alcohol testing of employees in safety-sensitive jobs in the transportation industries, such as railroads, and mass transit (Karch et al., 2008).[6] DOT also provided regulatory oversight to drug testing in the gas and oil industry, where testing was adopted voluntarily (Karch et al., 2008).

Survey of employers on drug testing practices in 1988 shows that drug testing was concentrated in several industries. It was by far the most common in mining (including oil and extraction), transportation, and communications and public utilities (top panel panel of Figure 2), where 16-22% of workplaces have a drug testing program. 5-10% of establishments in manufacturing and wholesale trade also report having a drug testing program, while numbers for other industries are far below 5%. By late 1990s over half of the employees in these industries report working for establishments with a workplace drug testing program (Wozniak, 2015). I follow Wozniak (2015) in designating these industries as high-testing industries. Aside from industry, firm size is found to be strongly correlated with the likelihood that an establishment has a drug-testing program: only 2% of small establishments (fewer than 50 employees) compared to 43% of large establishments (1000 employees or more) (Gust et al., 1990). Figure 3 also shows that there is little relationship between drug testing and drug use by industry - fraction of respondents reporting ever using any illicit drugs or using them last month is virtually identical between testing and non-testing sectors (the difference between the share of respondents who report having used drugs last month between the high-testing and low-testing sector is 1.6 p.p.).

[5]DOT tests for marijuana, cocaine, opiates, amphetamines, and phencyclidine, and the set of drugs has not changed since the implementation of the program.

[6]In addition, in 1970s technological developments in testing produced a relatively inexpensive kit that could be used for on-site testing (Tunnell, 2004; Zwerling, 1993).

2.1.2 State policies

States began to enact legislature concerning employer drug testing in the 1990s. Some states enacted pro-testing policies that offered financial incentives to employers with a drug-testing program, in the form of discounts on worker compensation premiums, or limiting employer's legal liability, while others introduced policies that restricted or outright banned certain types of testing (e.g. random testing or periodic testing). The top panel in Figure 4 shows the distribution of years in which laws came in effect. 13 out of 16 states that passed a pro-testing law did so between 1995 and 2005. Similarly, all but 1 of anti-testing laws were implemented during this period. Map with pro-testing and anti-testing states is shown in the bottom panel of Figure 4. Although pro-testing states may appear somewhat clustered, they spread over 7 out of 9 Census divisions.

Anecdotal evidence suggests that drug testing industry pursued an aggressive marketing strategy, as well as lobbying at the state level in order to manipulate policy (Tunnell, 2004). Hoffmann-La Roche - a pharmaceutical company which had a drug-testing contract with the Pentagon - influenced introduction of state legislature through its membership in American Legislative Exchange Council, which pairs conservative lawmakers and private sector representatives to create legislature (Macdonald, 2013; Pardes, 2014). Hoffmann-La Roche even advertised its work with federal and state officials in its press release (Macdonald, 2013). Anti-testing policies arose due to case law and concerns about individual privacy.[7]

Data limitations preclude a direct study of the relationship between drug testing laws and employer drug testing,[8] though Wozniak (2015) provides several pieces of evidence suggesting a causal effect. In 2002-2003, about half of respondents in NSDUH reported testing by their employer in pro-testing states, compared to 30 percent in anti-testing states, and 40 percent in control states. Between 2002-2003 and 2007-2009, Louisiana adopted a pro-testing drug statute and a fraction of respondents reporting drug testing by employer grew by 3.4 percent, which is comparable to the

[7]Studying state characteristics associated with adoption of drug-testing policy, Lamothe (2005) finds that media coverage of drug use and having "extreme" state ideology is positively associated with adoption of any state drug-testing legislature, while share of workforce that is unionized is negatively correlated with likelihood of adoption of such policies. More liberal states are more likely to adopt anti-testing legislation, and adoption of state pro-testing policies is, counter to expectations, positively correlated with share of unionized workforce (Lamothe, 2005).should be noted that the assignment of pro-testing and anti-testing states in this study somewhat differs from treatment assignment in (Lamothe, 2005).

[8]National Survey of Drug Use and Health (NSDUH) provides state-level tabulations on drug testing by employers for 2002-2003 and 2007-2009 only.

growth rate in other states with a pro-testing statutes (4.8 percent) and higher than growth in states which did not adopt the policy (-0.4 percent) (Table A3 from Wozniak (2015)). Finally, over half of employers with drug testing programs stated that the government mandates (for certain industries) and financial incentives were the main reason for such programs (Tunnell, 2004).

2.1.3 Impact of drug testing

Setting up a standard 2-sector Roy model with a testing and non-testing sectors, where productivity in the testing sector is adversely affected by drug use, Wozniak (2015) shows that in the absence of testing, employers in the testing sector offer wages scaled down by expected productivity losses due to drug use among the population. If there are two populations of workers - with an observable population-specific characteristic, such as race - and the employer believes drug use is higher among black workers, then the employer will offer lower wages to black workers than to white workers because in expectation black workers have lower productivity. In turn, black workers will be less likely to be employed in the testing sector.

Testing enables a non-user to signal their non-user status. First, increased precision in screening decreases the probability of hiring a drug user, so that the share of non-users employed in the testing sector should increase with introduction of testing. Second, testing can differentially influence employer's beliefs about drug use for black and white workers, and therefore change the relative outcomes of black and white workers. Employers may have better information about white applicants and underestimate rate of drug use. Or employers may overestimate the likelihood of drug use among black applicants. In both cases, black workers are more like to be employed in the testing sector when testing is introduced.

This simple model captures several important features. Black-white wage penalty is well-documented.[9] What is particularly relevant is that the data indicates a larger extent of discrimination in blue-collar jobs compared to white-collar jobs. Further, evidence suggests that employers believe a low-skilled black applicant is more likely to fail a drug test compared to a low-skilled white applicant (Wozniak, 2011). Using data from the National Household Survey of Drug Use and Health for respondents 18+ years old I analyze drug use by demographic group.[10] Figure 5 shows that al-

[9]See Lang & Lehmann (2012) for a review of the literature and Neumark (2018) for a review of experimental work on discrimination, which largely confirms findings from non-experimental studies

[10]For consistency with later analysis, I define White as everyone other than Hispanic and non-Hispanic Black.

though black men are more likely to report having used illicit drugs in the last month (top panel), the difference is relatively small - only 2-3 percentage points, and the difference for women is even smaller. Notably, self-reported drug use has declined for all demographic groups since the 1980s. When I examine the differences in use for the sample with a high school degree or less (Figure 6), the difference in use between black and white men is reduced to 1-1.5 percentage points and is not statistically significant (or has a reverse sign) for the more recent time periods. Similarly focusing on women with a high school degree or less produces a much smaller race differential that is smaller than half a percentage point for the 2000-2014 time period.

Evidence supporting effectiveness workplace drug testing is somewhat mixed. Though some studies find a reduction in accidents (Jacobson, 2003; Morantz & Mas, 2008), there is also evidence that drug testing reduces the likelihood that an accident is reported. Others report no change in drug use (Yamaguchi et al., 2003; **?**). Military's drug testing policy, which includes random testing of current employees and zero tolerance, significantly reduces drug use in the military population relative to civilians (Mehay & Pacula, 1999). Similarly, a randomized control trial of mandatory random testing at schools concluded that students subject to the testing are less likely report having used drugs in the last month.(James-Burdumy et al., 2010). However, such zealous policies are rarely implemented in the private sector.

2.2 Marriage, childbearing, and the labour market

In 2016 almost three quarters of births to black women were to unmarried mothers - more than twice the share among white women. Perhaps the most prominent theory for the high rate of births out of wedlock among black women is the degradation in marriage-market value of inner-city black men brought on by decline in industrial job opportunities for low-skilled workers (Wilson, 1996). Although Wilson's hypothesis is most widely associated with the arguments he put forth in "The Truly Disadvantaged" - that joblessness among low-skilled black men is linked to the rising incidence of single motherhood among black women - his narrative is less restrictive. In his later work, he references both the low opportunity cost of childbearing for black women and weakened social norms against non-marital childbearing that allow economic conditions to play a role (Wilson, 1996). Thus far, empirical evidence on the validity of Wilson's thesis that the shrinking pool of marriageable males is responsible for the increase in the fraction of births out of wedlock among black mothers is mixed. Earliest evidence analyzing time series pointed out that marriage rates declined even among black populations for whom earnings and employment were increasing (Ellwood & Crane, 1990; **?**). Cross-sectional studies find that only a fraction of variation in marriage rates can be explained by male unemployment, while studies using individual or local area fixed effects suggest much smaller effects - that economic conditions of black men explain no more than 5% of the variation in black marriage rates (Lichter et al., 1991; Mare & Winship, 1991; Sampson, 1987; **?**). Instrumenting for men's earnings using a Bartik-type instrument produces significantly smaller (by a factor of 1/5) and less precisely measured relationships between measures of supply of marriageable men and a share of black women who are married (Wood, 1995). In contrast, Olsen & Farkas (1990) finds that a program that increased employment opportunities for youths from low-income households lead to a large increase in the likelihood of cohabitation/marriage and a sizeable reduction in fertility for black adolescents females. Mamun et al. (2008) provides the most convincing evidence to date that relies on exogenous variation in labour market opportunities. National Job Corps Study, which uses random assignment is an education and vocational training program lasting just over half a year, with participants 16-24 years old, half of whom are black. Measuring outcomes 4 years after the program, they find that accounting for endogeneity with an instrumental variables approach yields economically and statistically insignificant effects of employment on marriage for men. In contrast, increase in women's weekly earnings by $100 increases the likelihood of being married by 0.3 percentage points. Focusing on just the black sub-sample, however, produces much smaller and statistically insignificant effects (increase of $100 is associated with 0.7 p.p. increase in probability of being married), despite the fact that first stage effects are larger for black women than for the rest of the female sample.

In light of the dramatic increase in non-marital fertility observed in all demographic groups over the last several decades, Wilson's hypothesis has since been applied in the context of low-skilled men in general. But, as in the case of the black population, there is no consensus in the empirical research for the general population. Exploiting a positive income shock for low-skilled men stemming from the fracking boom and gender-specific components of trade-induced labour demand shocks, Kearney & Wilson (2017) and Dorn et al. (2018) report conflicting results, though Kearney & Wilson (2017) suggest that social norms may be an important unobservable determinant. More generally, empirical studies find a relatively relatively large and positive effects of male income on fertility (Black et al., 2013; Lindo, 2010; Schaller, 2016) and positive effect on marriage (Black et al., 2003; Blau et al., 2000). The effect of female wages on fertility are much smaller in magnitude compared to effect of men's earnings (Dorn et al., 2018; Schaller, 2016) but empirical research is scarce Shenhav (2018).

3 Data and estimation strategy

3.1 Drug testing laws

To classify states as pro- and anti- testing states, I rely on De Bernardo et al. (2008), as in (Wozniak, 2015). Pro-testing states are states where under statutory law employers with a drug testing program qualify for a discount on workers' compensation premiums or receive legal protection. I cross-reference this information with information from alternative sources[11] to identify legislative changes that occurred since 2008. I classify 2 additional states as pro-testing states: Kentucky and Wyoming, where drug-testing statutes were implemented in 2008 and 2012[12]

Anti-testing states are states where under statutory law the state places restrictions or outlaws drug testing for applicants, random testing, reasonable suspicion or for-cause testing, periodic announced testing, and post-accident testing. It is worthwhile to note that additional rules and regulations exist below the state level. For example, San Fransisco, California, places severe limitations on circumstances when drug testing can take place, and prohibits post-accident, periodic, and random testing, as does Boulder, Colorado. For the sake of consistency in definition with pro-testing states, I ignore anti-testing regulation below the state level.

3.2 Earnings, fertility and marriage from CPS

For the primary analysis, I use March Supplement of the Current Population Survey (CPS) for 1980-2016 for individuals 18-55 years old. In addition to rich information on employment and earnings, CPS also records other characteristics, such as marital status and the number of children in the household. From 3,396,955 observations, I select those living in households, and drop observations with missing values for any of the variables used in the main analysis. The final sample size consists

[11] E.g. "State-by-State Legal Status Guide" by Alere Toxicology (2016), "50 State Survey: Drug-Free Workplace Programs" by National Drug Screening (2018), "Drug and Alcohol Testing by State" by Steingold (2017), and a report by ACLU (2019).

[12] Table XX in the Appendix presents an overview of state laws.

of 3,368,474 observations.

Usual weekly hours worked, weeks worked last year, and pension plan at work are non-missing for those who are employed in the previous year (N=2,765,391). Hourly wage is calculated as last year's earnings, divided by total hours, where total hours is a product of weeks worked last year and usual hours worked per week. I exclude observations with hourly wage below 3rd percentile and above 97th percentile. Information on health insurance coverage at work is collected beginning in 1988 (N=2,694,200), and information on firm size is also available starting from 1988 for respondents who worked in the previous year (N=1,973,110). Black is defined as non-Hispanic black; white refers to everyone who is not Hispanic and not non-Hispanic black, so that "white" category includes Asians and Native Americans. The way race is collected in CPS changed over time: only "white", "black", and "other" are defined in the beginning of the sample period, while starting in 2003 respondents could select multiple races.

Table 1 shows descriptive statistics, for the full sample, and pro-testing and control states[13] before 1988, the year of the Drug-Free Workplace Act (columns 3 and 4). There are several differences between pro-testing states and other states: pro-testing states have larger black population shares - 16.3 vs. 9.9%. In contrast, in control states Hispanic population is 8.1% - more than twice the share Hispanic population in the treatment states. The numbers in this row also highlight the increase in Hispanic population share over time: 12.7% over the entire sample period, compared to a mean of only 7% in the first 9 years. Overall, the population in the treated states is worse off compared to the population in the control states: they are less educated, less likely to be employed, and earn less if employed.

In Figure 7 I present sample means for a subset of labour market outcomes by demographic group using the samples presented in columns 1, 2, and 3 in Table 1 , which highlight several important facts. Examining fraction of the sample employed, shown in the first row, indicates that over the entire sample period, black women are almost as likely to be employed as men (though this does not hold for the earlier time period). Share employed is the lowest among black men compared to white and Hispanic men. Male-female earnings differential is the lowest for blacks. In the bottom row I show fraction of the sample employed in the testing sector, conditional on being employed.[14] A

[13] Control states are states which enacted no workplace drug testing laws during the time period, or states which enacted anti-testing regulation.

[14] Testing sector is defined following Wozniak (2015). It comprises industries where at least half of employees report

much larger share of men works in the testing sector, compared to women. There is no difference in the likelihood of employment in the testing sector for blacks between treatment and control states.

CPS Fertility Supplement contains measures of other outcomes of interest but the types of questions asked varied over time. I focus on the number of births which is recorded relatively consistently over the time period of interest.[15] The supplement is chiefly administered to female respondents. I restrict the sample to observations with non-missing values for the outcomes of interest which results in a sample size of 470,424 with 22 cycles of data collected between 1980 and 2010.

Marriage and fertility outcomes from the March CPS sample and from the Fertility Supplement are shown in Tables 2 and 3, respectively. Overall, the likelihood of being married is almost twice as high among white women compared to black women, while only about 5 percentage points higher than for Hispanic women. One in five black women are single mothers, compared to fewer than one in ten for Hispanic women and only 3% of white women. Although this is partially due to higher likelihood of being separated for black women, the main factor is that they are a lot less likely to ever marry (36% of black women have never married, compared to only about 14-17% among white and Hispanic women). In Table 3 I show means for fertility outcomes for the Fertility Supplement sample. The women in the Fertility Supplement sample are younger than women in the March sample by construction - the supplement is chiefly not administered to women over 44. The number of children in the household is about 0.2 lower than for the main sample of women for each demographic group. However, the number of children conditional on having any children is very similar across the two samples. The number of births, as expected, is higher than the number of own children in the household. The latter is an imperfect measure of fertility, as it excludes children who have moved out (or no longer living). As older women are more likely to have older children, using the number of children in the household may understate fertility for the older women in the sample.

working at a testing firm: mining, manufacturing, transportation, wholesale trade, and public administration.

[15] Age at the birth of the first child has not been collected since 1995.

3.3 Vital Statistics birth records

I also employ public-use Vital Statistics data to study birth outcomes. Public-use Vital Statistics data provide state of residence of the mother until 2004, after which this information is only available in restricted-use files. Data on Hispanic origin of mother and father was recorded on birth certificates only beginning in 1989. I rely on publicly available data files for 1989-2003 and, for consistency with the analysis using CPS data, define demographic groups in a similar way: black refers to non-Hispanic black mothers, Hispanic refers to mothers of Hispanic origin, and white refers to all others not of Hispanic origin. Birth records contain important information on pregnancy outcomes, such as birthweight and gestation, allowing me to study the change in the quality of births, as measured by health of the infant. Finally, birth records also record mother's marital status, and I can infer whether a father was listed on the birth certificate from the availability of information about the father. Table 4 shows the means by race/ethnic group for the 63,123,803 birth records in the data. Black infants have the worst health outcomes at birth compared to other groups. For example, share of births which are low birthweight is 13.3% for blacks compared to 6.4% among whites and Hispanics. Almost half of birth certificates have no information on the father and fraction of teenage births is twice as high as among white births.

3.4 Estimation strategy

I use a flexible saturated specification to study the change in outcomes of interest:

$$y_{ist} = \sum_{k \in K} \Gamma^k_{ist} Pro-testing\ Law_{st} \beta^k + \sum_{k \in K} \Gamma^k_{ist} \lambda^k_s + \sum_{k \in K} \Gamma^k_{ist} \gamma^k_t + \theta_{st} + \epsilon_{ist}. \quad (1)$$

y_{ist} is a measure of a dependent variable for an individual i in state s in year t. $\sum_{k=1}^{K} \Gamma^k_{ist}$ is a set of K indicator variables for the following characteristics: black man, black woman, white man, white woman, Hispanic man, Hispanic women, high school graduate or less, young (18-25 years old).[16] I allow coefficients on indicators for demographic groups to vary by state: term λ^k_s controls for time-invariant heterogeneity in the dependent variable across group-state cells. γ^k_t absorbs group-specific changes over time. This term purges the dependent variable from variation due to time-varying

[16] When studying marriage and fertility outcomes, I estimate the models separately for men and women (or, in the case of birth records data, only for women).

heterogeneity across racial/ethnic groups. θ_{st} is a state-year fixed effect, which summarizes changes in the dependent variable in a given year that are common to all state residents. Importantly, because the policy itself varies at the state-year level, θ_{st} controls for the influence of the policy that is common to all residents of a given state. This term also addresses potential confounding relationship between state's welfare generosity and non-marital childbearing.[17]

Parameter of interest is a set of coefficients β^k, which identify group-specific effects of pro-testing legislation relative to white men, net of any policy effects common to all demographic groups. This approach is somewhat analogous to a triple-differences framework, however there is no true control group in the treated states unless we are willing to assume no effects on white men. To interpret estimates of coefficients β^k as a causal effect of the policy on group k requires the assumption that the change in the control group is a good counterfactual for the change in the treatment group in the absence of treatment, conditional on the set of covariates included in the model. I include a rich set of flexible controls which, importantly, absorb effects of the policy at the state-year level, as well as differential demographic-group specific time trends in the dependent variable. Although treatment and control states differ on observable characteristics within each demographic group, provided these differences within each demographic group do not evolve over time in a way that is not captured by state-year fixed effects, the estimates of β^k will provide a causal effect of pro-testing regulation on the outcome of interest.

The assumption required for a causal interpretation of the estimated coefficients fails if, for example, the outcome for black women in the treated states changes in a way that is correlated with treatment independent of treatment. There are several ways this assumption may fail. First, if there is a change in the unobservables that influences the dependent variable and is correlated with the timing of the policy, then I will incorrectly attribute the change in the outcome to the policy. Although this assumption is untestable, violation of this assumption requires that the change in unobservables not only occur at the same time as the pro-testing law, but also have a differential impact by demographic group since I control for state-year fixed effects, and identify policy changes within a given demographic group, net of changes that are common to all groups within a state.

[17]Theoretically, public assistance acts as a simple income transfer (with constraints) and has a non-decreasing relationship with non-marital fertility (Neal, 2004). Murray (1993) was the first to suggests that generosity of welfare benefits may be linked to incidence of out of wedlock births. Empirical research suggests a positive but modest effect of welfare benefits on fertility (Acs, 1996; Garfinkel et al., 2003; Moffitt, 1998, 2000; Wolfe et al., 2001), though there is also evidence of a positive relationship between welfare benefits and marriage (Blau et al., 2000).

Second, if a dependent variable exhibits a trend in the pre-treatment period, then estimates of treatment effect may reflect the changes in the dependent variable due to the same unobservables that drive the trend in the pre-treatment period, and not the effect of the policy. To that end, I estimate equation 1, excluding South Carolina which adopted a drug statute in 1985, and plot mean residuals for each treatment group - year - demographic group cell in Figures 8 and 9. These figures show that for black men in the pro-testing states the trend in log earnings follows roughly the trend in log earnings for black men in the control states. The same holds true for white women and black women. However, for Hispanic men there is a clear statistically significant difference between treatment and control states, suggesting that despite the flexible set of controls included in Equation 1, I do not account for the differences between treatment and control group. This implies that, conditional on covariates included in the model, Hispanics living in control states are not a good counterfactual for Hispanic men in the pro-testing states. Similarly, analyzing residuals using the number of children as the dependent variable, shows that fertility of Hispanic women in the pro-testing states is consistently lower compared to the fertility of Hispanic women in the control states (Figure 9).

An implicit assumption embedded this analysis is the homogenous composition of each demographic group over time. However, if the composition of the demographic groups changes in a way that affects the dependent variable independent of treatment, then the estimated coefficients on the treatment dummies will capture selection effects rather than policy effects. This is an especially salient issue given that the CPS does not collect data on nativity before 1994 and the broad definition of racial/ethnic groups in the CPS. In particular, the increase in the immigrant population share, especially among Hispanics, whose fertility and labour market outcomes are very different between the immigrant and native born population, is a source of concern. Immigrant Hispanic women have lower labour market attachments and worse economic opportunities compared to native Hispanic women (Blau & Kahn, 2007; Capps et al., 2007; De Jong & Madamba, 2001; Hall et al., 2010; Rivera-Batiz, 1999). Immigrant Hispanic population grew most rapidly in the South, and 4 out of 6 states with the highest Hispanic population growth are the treatment states (Georgia, Tennessee, South Carolina, and Alabama) (Hendrik F. Van den Berg, 2009). To examine the extent to which immigration influences the difference between treated and control states within a given demographic group, I use data from Census 1980, 1990, and American Community Survey (ACS) 2000-2016, which collect data on birthplace. I calculate the share of immigrants in each demographic group by treatment status and plot the difference in Figure 10. Beginning in 1980, share of immigrants among Hispanics is about 6 percent higher in the treated states than in the control states and this difference increases over time. In contrast, treated states have a lower share of immigrants compared to control states for both blacks and whites (which I use as a reference group), and over the sample period trends for both groups move in the same direction. The differential change in the composition of the Hispanic group over time in a way that is correlated with treatment suggests that parameter estimates for this group will be biased. Therefore, I exclude Hispanic group from the estimating sample.[18]

Thus far, I have related male-female differential within a given racial/ethnic group to marriage and fertility behaviour of women within the same racial/ethnic group, implicitly assuming no spill-over effects in the marriage market for other racial/ethnic groups. The consequences of violating this assumption are unlikely to be particularly extensive, especially when focusing on black women. In 2015, incidence of inter-racial marriage was 18% among black newlyweds. However, this masks substantial heterogeneity by gender – at 24% black men are twice as likely as black women to enter into an inter-racial marriage Livingston & Brown (2017). Share of interracial marriages among *all* black marriages is much lower - approximately 6% for black men and 3% for black women in 2000 (Fryer Jr, 2007). Furthermore, in the vital records data well over 90% of births to black mothers with non-missing information on the father list father's race as black.

Though I focus on economic factors as potential determinants of marriage and fertility decisions, workplace drug testing can affect marriage via other channels. First, it may reduce drug use, conditional on employment. To the extent that drug non-use is a desirable partner quality (Edin & Reed, 2005), drug testing policy may increase incidence of marriage if it reduces drug use and increases the pool of desirable partners. Second, drug testing policy may increase the ability to credibly signal their non-user status to *potential partners* if one is employed at a job with a drug testing program. Therefore, even absent any changes in drug use or shifts in employment, the policy can increase marriage rates if it increases the expected pool of desirable partners. In my discussion, I implicitly assume that change in economic conditions is the sole mechanism through which the

[18]Point estimates of the coefficients of interest are unaffected by exclusion of Hispanics. Furthermore, this strategy is consistent with Wozniak (2015) which also excluded Hispanics from most of the analyses. Table **??** in the Appendix shows the results when Hispanics are included.

policy affects marriage and fertility. Having said that, the evidence in support of reduction in drug use in response to drug testing programs is weak and I do not expect this to be an important channel.

4 Results

4.1 Labour market outcomes

First, I present results from Tables 5 which reports estimates of β^k from equation 1 for each demographic group, with white men as the omitted category, using measures of labour market outcomes as a dependent variable.[19] I estimate robust standard errors, clustering on state. Binary outcomes are multiplied by 100 so coefficients can be interpreted as change in percentage points. Logged outcomes are also multiplied by 100 so coefficients can be interpreted as percent change. I report two sets of p-values for each coefficient: an unadjusted p-value and a Bonferroni-corrected p-value, which adjusts for multiple hypothesis testing. It should be noted that the Bonferroni-correction is generally agreed to be conservative when outcomes examined are correlated with each other (Armstrong, 2014; Narum, 2006; Sedgwick, 2012).

To begin, I evaluate the effects of the policy on earnings and employment. This set of results makes it clear that black women gain by far the most after introduction of the policy. The point estimates imply that earnings of black women relative to white men increase by 7.6% in pro-testing states once pro-testing policy is introduced, net of state-specific time-invariant differences in relative log earnings of black women, net of changes in relative log earnings of black women that are common to all black women, and net of any changes in the state that are common to all residents in a given year. This point estimate remains statistically significant even after correcting for multiple testing, as indicated by the Bonferroni-corrected p-value reported at the bottom of the table. The increase in earnings of black men is 3.1% and not statistically significant even before correction. Although both black men and black women see a 2.2% increase in wages, black men are also 1.5-2 p.p. less likely to be employed or to have worked in the previous year after introduction of pro-testing policies. However, conditional on employment, black men are 1.7 p.p. more likely to be employed in the testing sector under pro-testing policy (the estimates suggest that black men were substituted for white women in the testing sector).

[19] This analysis closely follows Wozniak (2015), though I specifically focus on sex differential by racial/ethnic group.

The increase in wages and the higher likelihood of employment in the testing sector for black men are consistent with model's predictions. The lack of sorting into the testing sector by black women may at first seem to conflict with theory. However, given that a substantial fraction of employers in the low-testing sector have a drug-testing program, coupled with the fact that high-testing sector is largely composed of of male-dominated industries, these results need not be inconsistent with the model. They may simply reflect that black women sort within the low-testing sector to testing firms, while black men sort from the low-testing sector to the high-testing sector.

I examine other labour market outcomes in Tables 6. I study how the policy affects employment at a likely testing firm by using several proxies, such as firm size and employment benefits, following (Wozniak, 2015). Black men are 1-3.2 p.p. more likely work at a likely testing firm, while black women are 0.5-1.4 p.p. more likely. In the last two columns, I examine the intensive margin response via weeks worked last year and usual hours worked per week. Black women work an additional 0.54 weeks after the policy, but I find no other statistically significant changes. Applying the Bonferroni correction yields statistically insignificant estimates on all terms, with the exception of *Black men x Pro* in the regression using an indicator for employment at a large firm as a dependent variable.

As shown in Table 7, effects of the policy are concentrated among less educated individuals. The estimated coefficients for the sample with more than a high school education suggest small and statistically insignificant decline in employment, wages, and employment in the testing sector for black men and women with introduction of pro-testing statutes. This set of results is consistent with a number of previous studies, which document a larger degree of racial discrimination in low-skilled jobs compared to white-collar jobs. Therefore, I omit high-skilled individuals from the labour market analysis going forward.[20]

In Table 8 I present results from estimating equation 1 with additional terms $\sum_{k \in K} \Gamma^k_{ist} Anti-testing\ Law_{st} \beta^k$, which capture group-specific effects of anti-testing statues. Notably, most coefficient estimates on the anti-testing interactions are of the opposite sign compared to the pro-testing interaction counterpart. In particular, black men and black women have lower wages, and are less likely to be employed in the testing sector under anti-testing policies (the point estimates for black men are larger than for black women). Therefore, blacks face different labour market conditions under pro-testing and anti-testing policies, suggesting that these policies do influence workplace

[20] This stratification implicitly assumes that high- and low-skill workers are not substitutes for each other.

drug testing, as policies which encourage drug testing have opposite effects of the policies which discourage testing.

Next, in Table 9 I estimate equation 1, including industry fixed effects.[21] For black women the estimated effects of pro-testing regulation are somewhat larger when industry effects are included, implying that such regulation induces black women to switch to lower-paying industries. Comparable analysis for black men is ambiguous.

To summarize, the results indicate that black women saw an increase in earnings of 7.6%, compared to 3.1% for black men. Though both black men and women saw the same increase in log hourly wages, the regulation induced a positive effect for black women on the intensive margin (increase in weeks worked last year) and a negative extensive margin effect for black men (lower likelihood of employment), generating a difference in earnings.

4.2 Marriage and childbearing

Having documented the impact of pro-testing legislation on race-specific male-female differential in earnings, I follow by evaluating whether the laws have an impact on marriage and fertility decisions, using white women as a reference category. I find no evidence that the policy is associated with any meaningful changes in the incidence of marriage among black women, despite the changes in the male-female earnings gap documented in the earlier section. Table 10 shows no effect of the policy on marriage, cohabitation, divorce/separation, or the likelihood of being a single mother for black women.

Next, I estimate effects of pro-testing legislation on fertility, with results shown in Table 11. The coefficient estimate on *Black women x Pro* points to an overall decline in childbearing among black women, as they report 0.3 fewer children in the household. Although they are 2 p.p. more likely to have any children, and 0.9 fewer children conditional on having any children, with the latter estimate weakly significant even after adjusting for multiple testing. Taken together, these estimates suggest that there is no significant change in the fraction of black women who have children, but that there is a decline in the number of children for black women with at least one child. Studying the effects of pro- and anti-testing policies together in Table 12 shows that they have opposing effects when

[21]The estimating sample in these regression differs because it only includes respondents who have non-missing industry information, which excludes those who have not been in the labour market in the last 5 years.

analyzing fertility response on the extensive (intensive) margin. It also appears that the decline in fertility comes from women stopping early, rather than delaying childbearing, as the negative fertility effects are concentrated among older (and married) women (Table 13). These results indicate that the decline in fertility is concentrated among older women, suggesting a decline in total fertility, rather than shifting births to a later age. Given that the reduction in childbearing is concentrated among black women with at least one child, it is not surprising that rates of single motherhood are not affected. I confirm the findings from the March CPS sample, where I only observe the number of own children in the household, using the CPS Fertility Supplement, which provides data on the number of births (results shown in Table 14). The results indicate that better labour market opportunities for black women lead to a decline in fertility, indicating that negative substitution effect dominates the positive income effect.

4.3 Children's health at birth and living circumstances

I have provided evidence that increase in employer drug testing leads to an improvement in women's economic opportunities among blacks. Given the existing research on the link between female earnings, and children's outcomes (Bruins, 2017),[22] a natural extension is analysis of whether the policy has any effects for the children of the affected women.

First, in Table 15, I report results from estimating Equation 1 using Vital Statistics micro data to estimate effect of the policy on birth characteristics. Generally, evidence on the effects for black women is mixed. Two statistically significant coefficients suggest a 1 percentage point reduction in the fraction of teenage births, but also an almost half percentage point increase in the fraction of births that are below the low birthweight threshold.

Second, in Table 16, I examine whether pro-testing regulation leads to an improvement in well-being of households with children, using measures that are available consistently for the time period of interest. The estimate in the first two rows indicate that in pro-testing states households with children with a black household head have 0.5-2.5 p.p. lower likelihood of public assistance receipt. Given that the fraction of households receiving public assistance is 3-11 p.p., these point estimates represent large changes, suggesting that living circumstances of children in black families improved

[22] The mechanism through which female earnings are linked to an increase in children's welfare is both through an increase in bargaining power, and an increase in family income.

as a result of pro-testing policies. .

5 Conclusion

Drug testing for employment has grown at a rapid pace since the 1980s, with half of U.S. workforce is employed for an establishment with a drug testing program. This paper studies the consequences of policies that incentivized workplace drug testing on male-female earnings differential, marriage, childbearing, and children's outcomes among blacks. Using data from the CPS and Vital Statistics birth records, I employ a flexible saturated difference-in-differences design to examine effects of the legislation on the black population. My results indicate that pro-testing regulation increased the earnings of black men and women, but the positive effect was greater for women, reducing the male-female earnings gap. In addition, I document a decline in childbearing by 0.1 children among married black women with at least one child. Analysis of birth records also suggests that the policy reduced teenage childbearing. Finally, I find that the regulation reduced public assistance use by households with a black female household head, implying that pro-testing regulation improved living conditions of black children. The latter point is important, as it suggests that the existing narrative that the War on Drugs – and drug testing – leads exclusively to a deterioration of black families does not capture the full extent of the consequences.

6 Figures

Figure 1: Drug testing over time

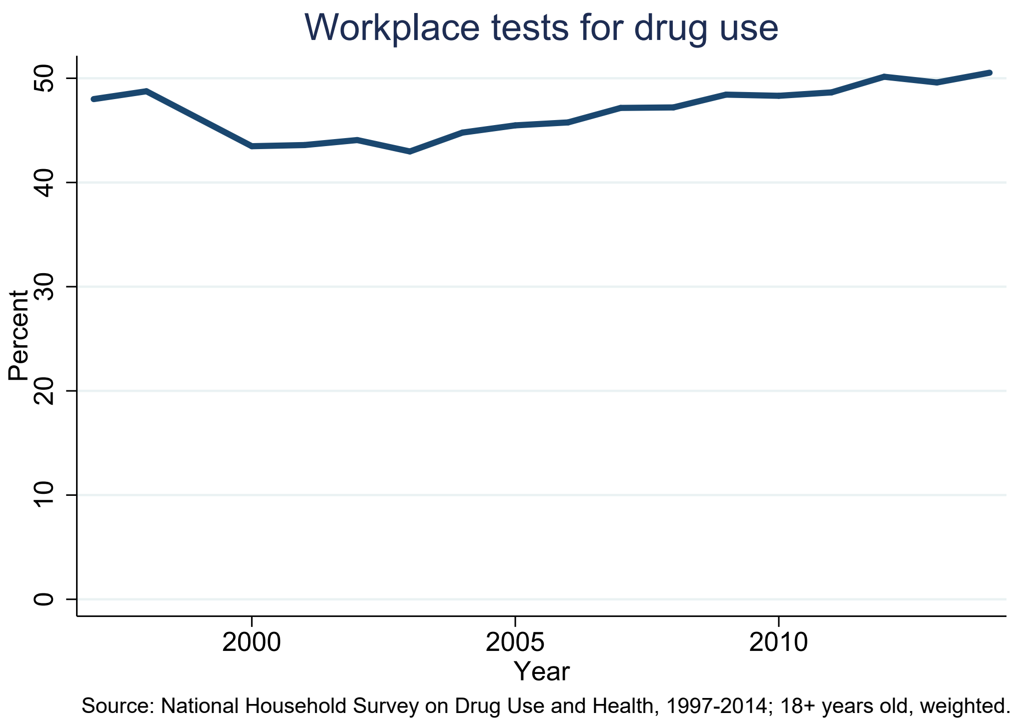


Figure 2: Drug testing by employers, by industry

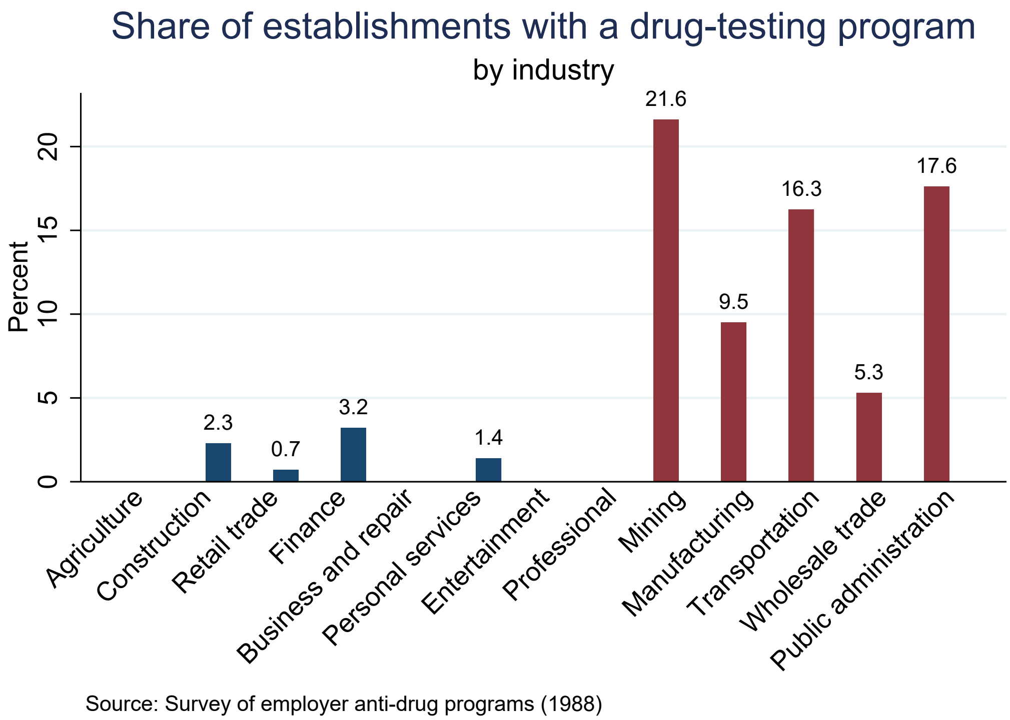
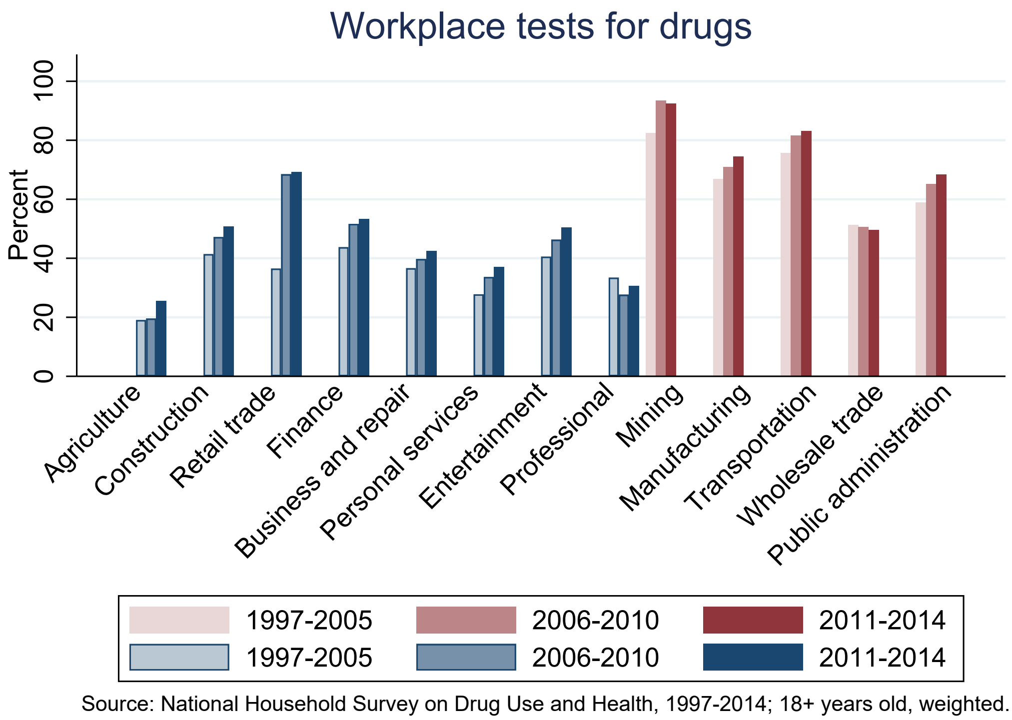


Figure 3: Drug testing and drug use by industry



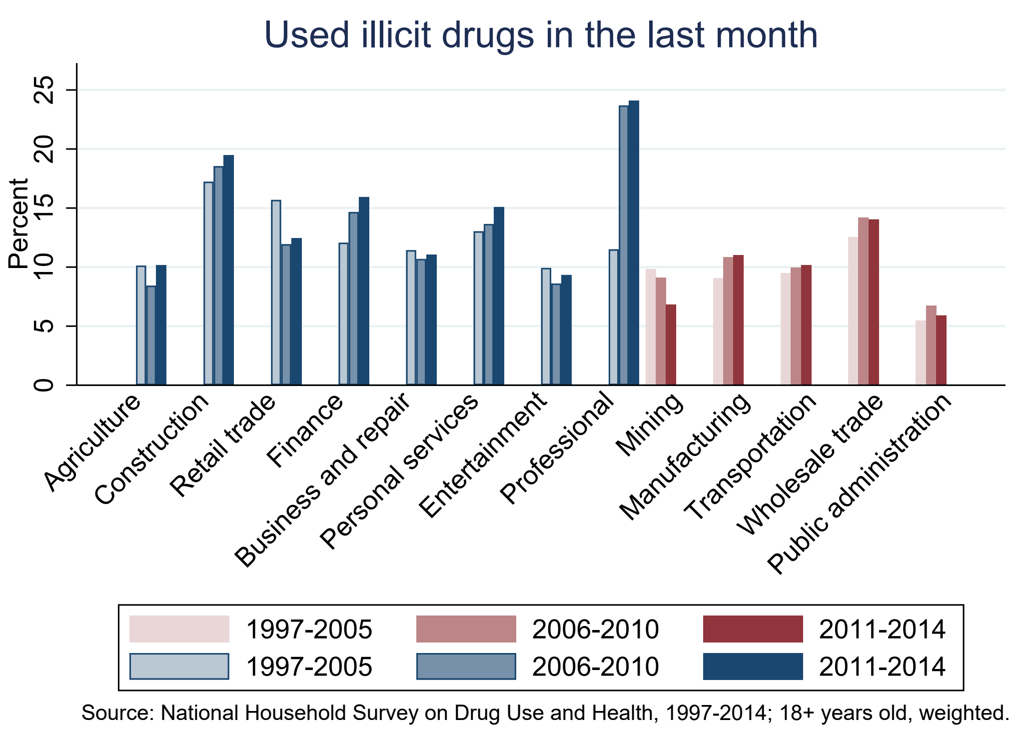
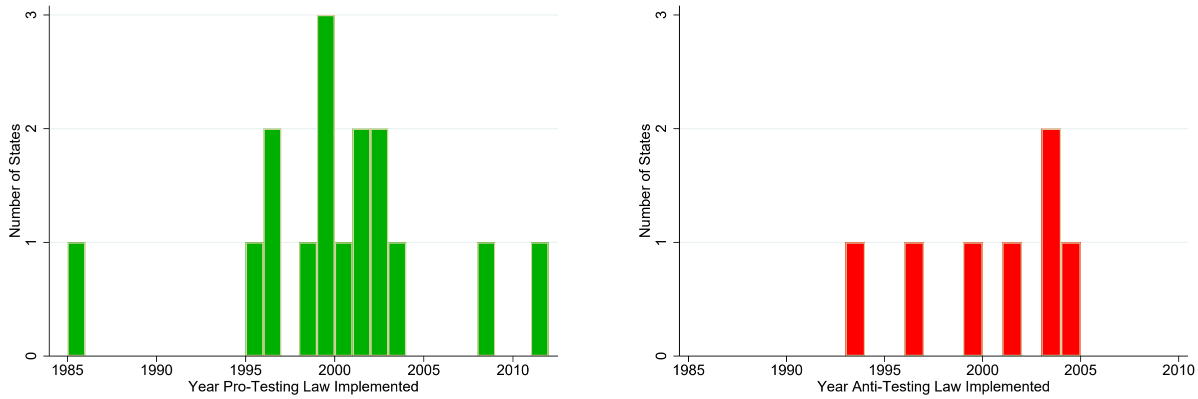
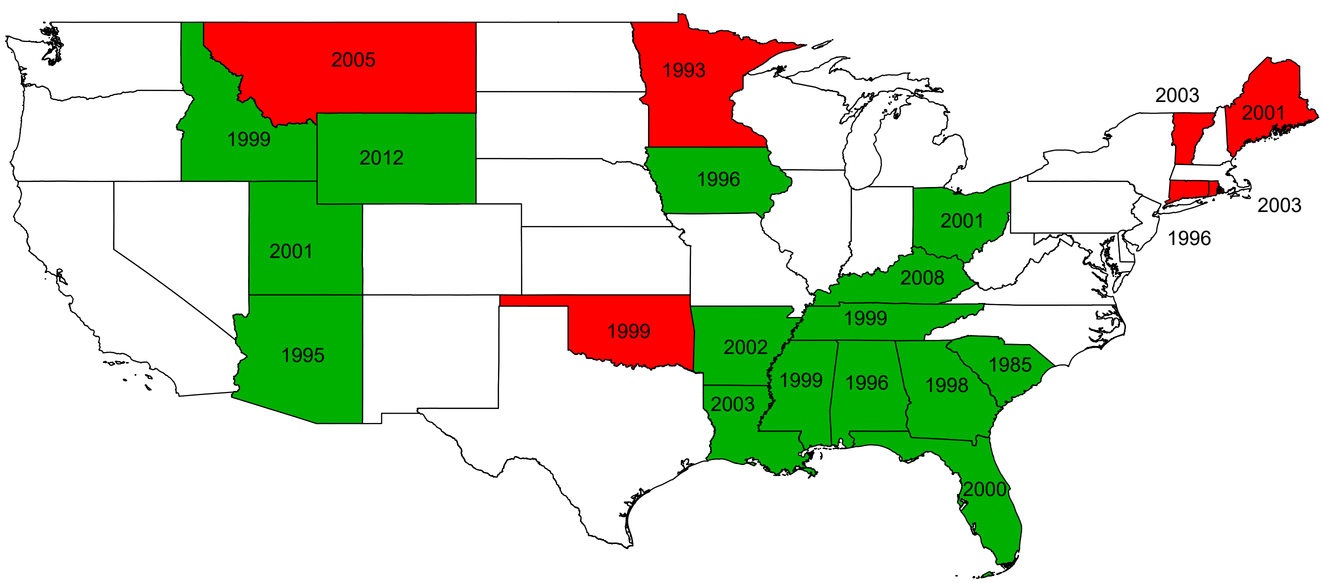


Figure 4: Drug Testing Statutes

Distribution of years in which pro- and anti-testing laws came in effect

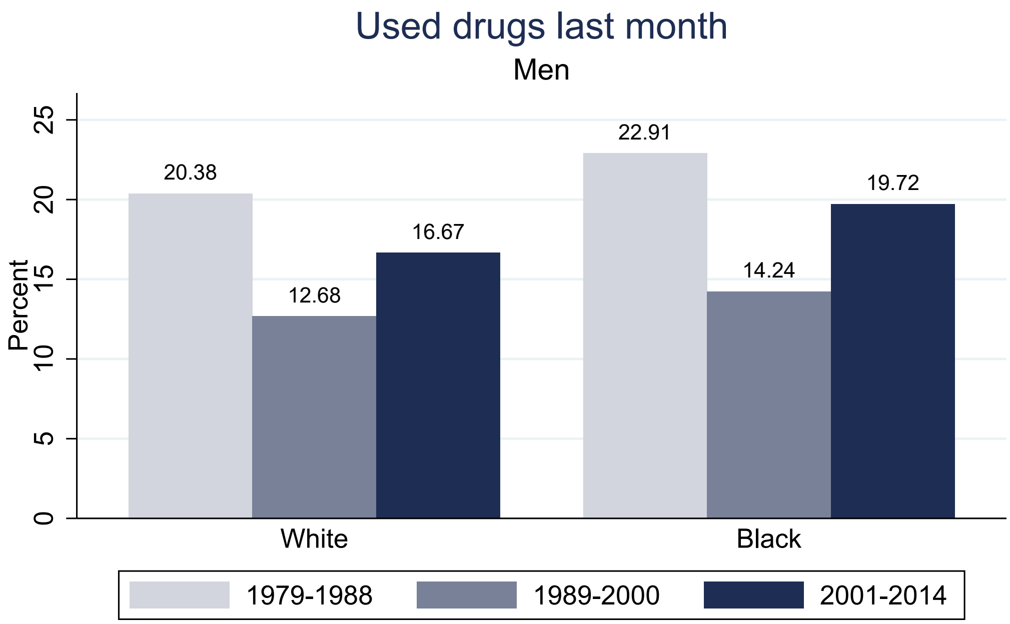


Map of pro-testing and anti-testing states

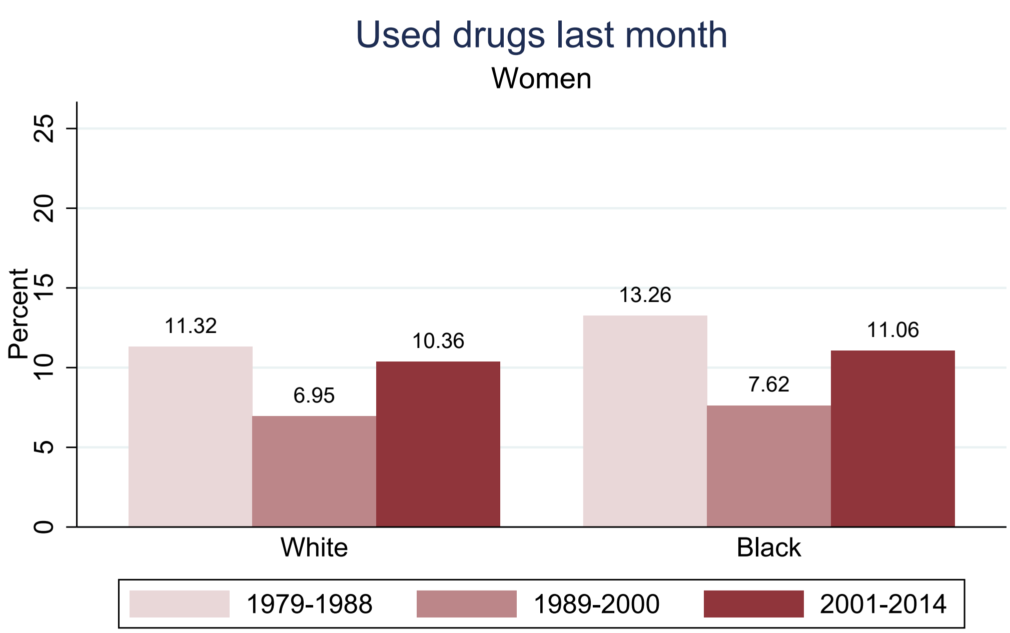


Data sources: **?**, cross-referenced with information on information available on https://www.nationaldrugscreening.com/us-state-laws.php (retrieved March 2018) to identify legislative changes that occurred since 2006. This approach classified two addition states as a pro-testing states - Kentucky and Wyoming.

Figure 5: Drug use over time by demographic group

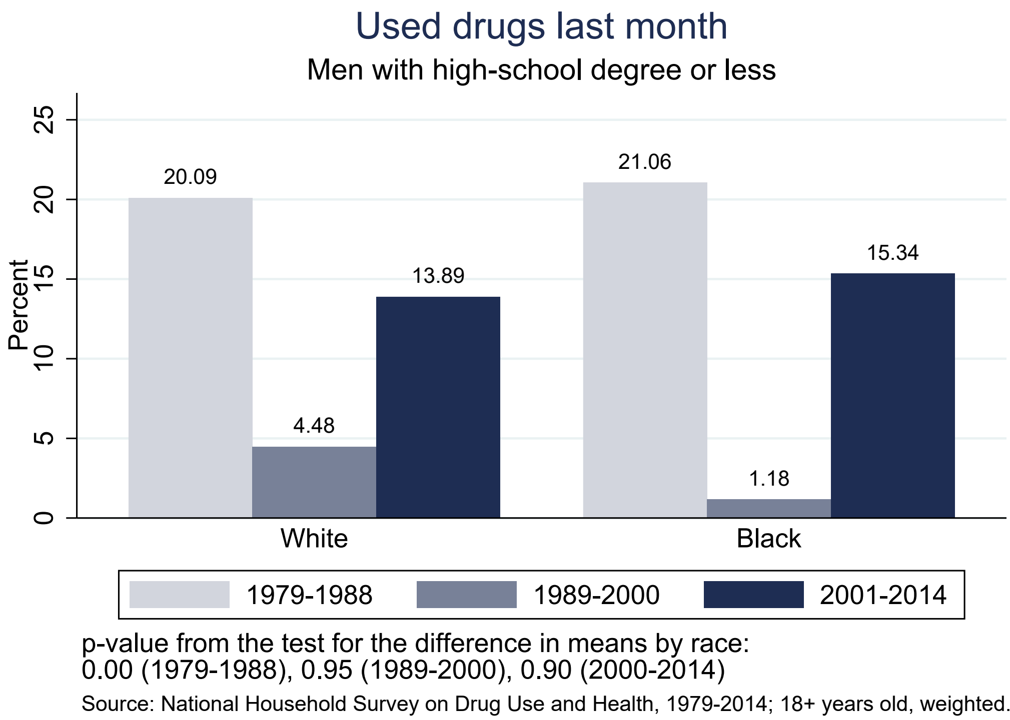


p-value from the test for the difference in means by race:
0.00 (1979-1988), 0.00 (1989-2000), 0.00 (2000-2014)
Source: National Household Survey on Drug Use and Health, 1979-2014; 18+ years old, weighted.



p-value from the test for the difference in means by race:
0.00 (1979-1988), 0.00 (1989-2000), 0.00 (2000-2014)
Source: National Household Survey on Drug Use and Health, 1979-2014; 18+ years old, weighted.

Figure 6: Drug use over time by demographic group, high school degree or less



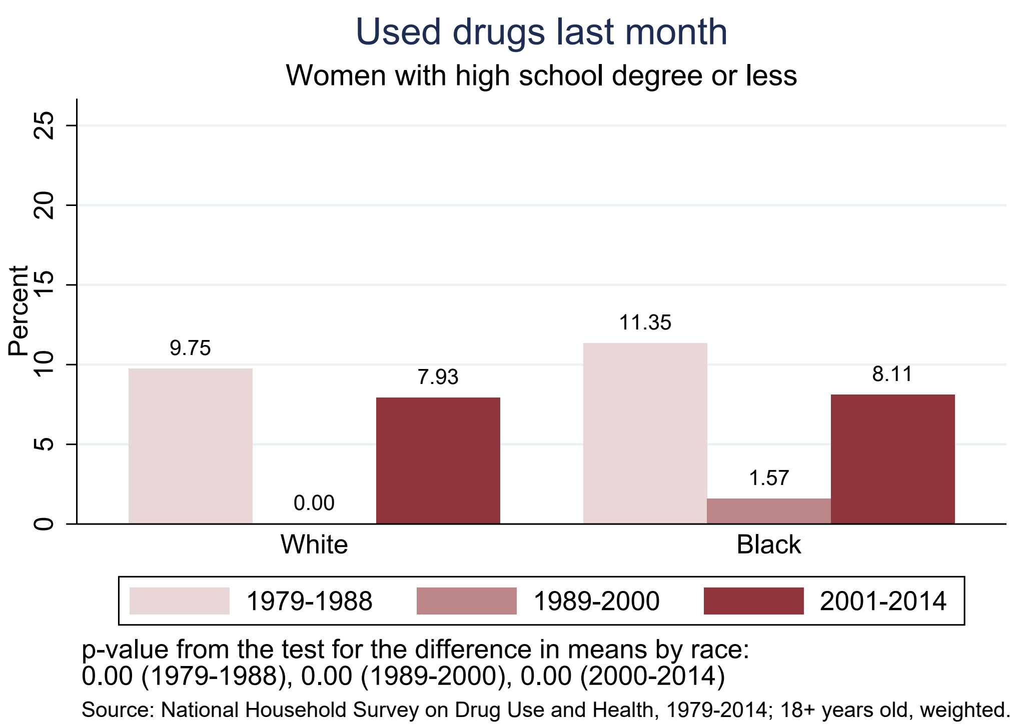
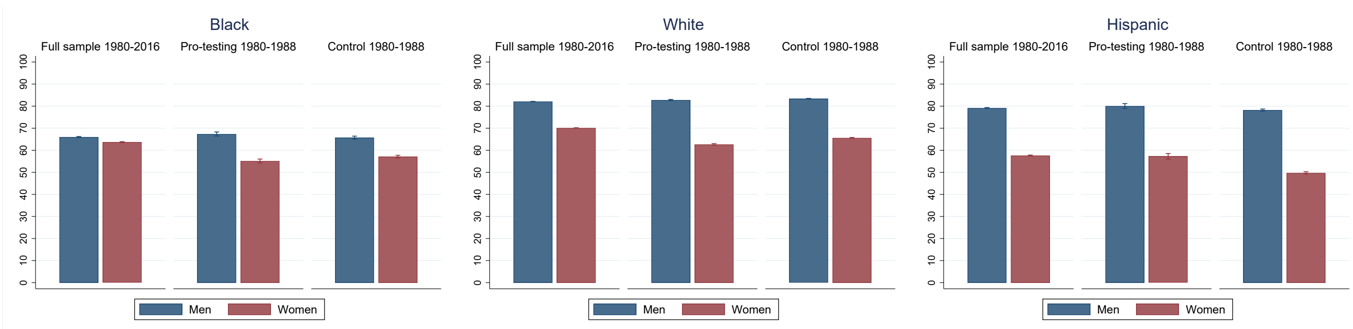
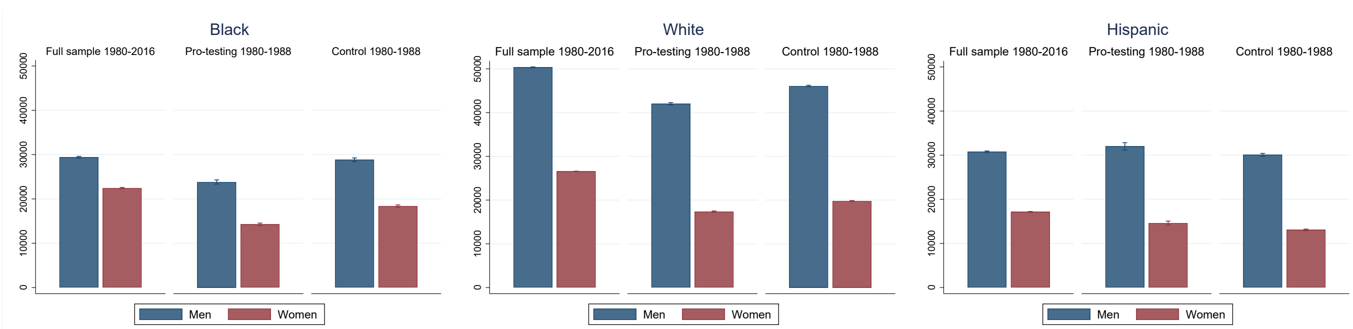


Figure 7: Labour market outcomes - group means

Share employed



Annual earnings ($2018)



Share employed in the high-testing sector

Figure 8: Pre-treatment trends in log earnings

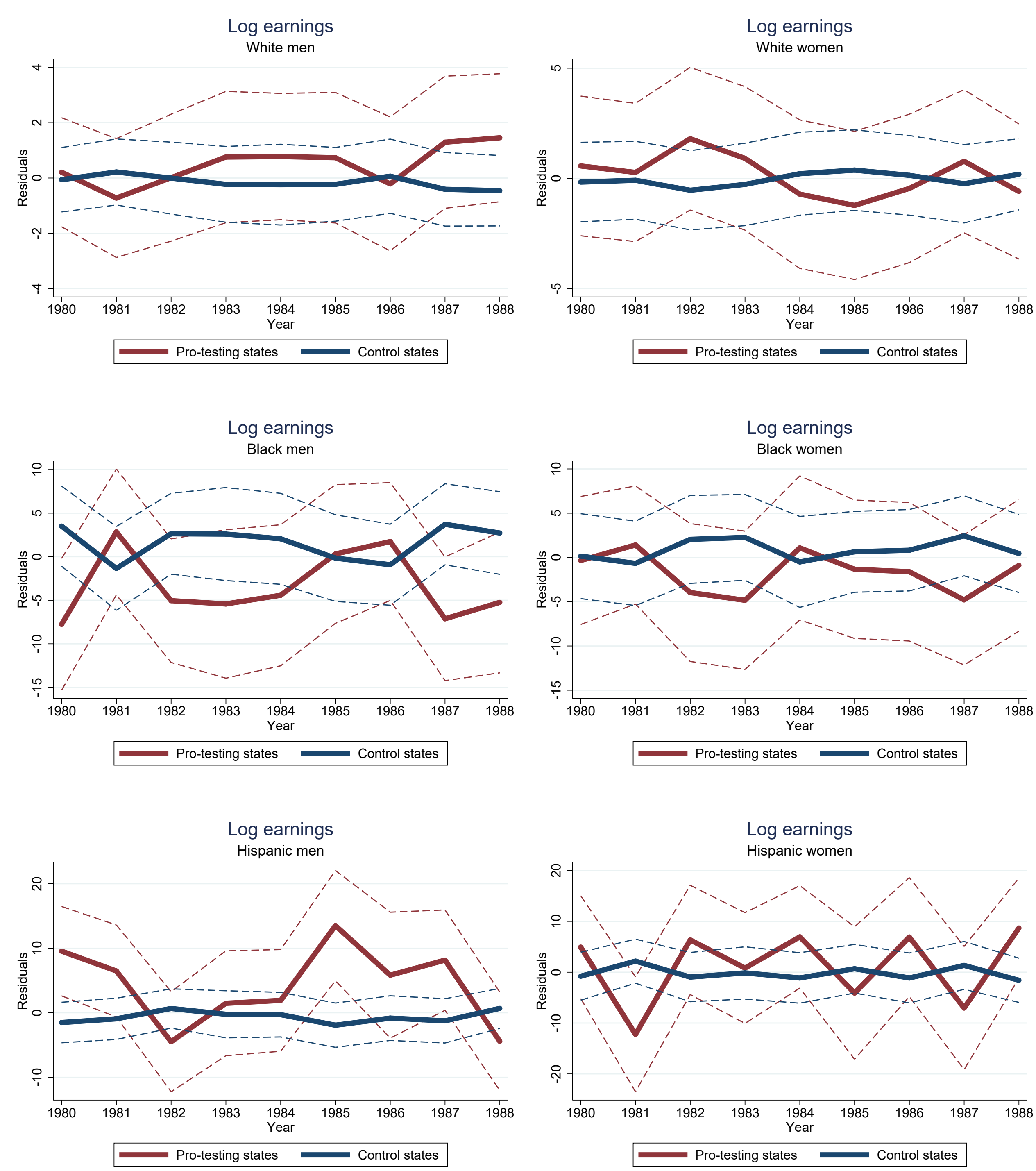


Figure 9: Pre-treatment trends in the number of children

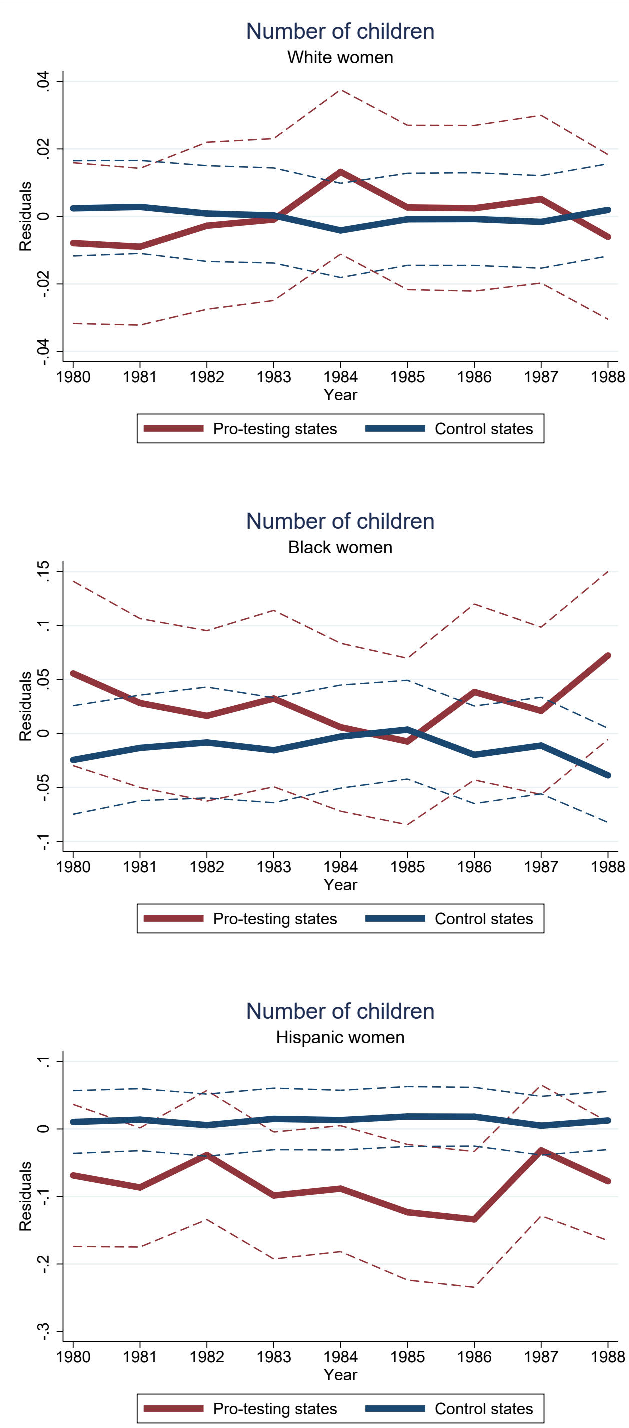
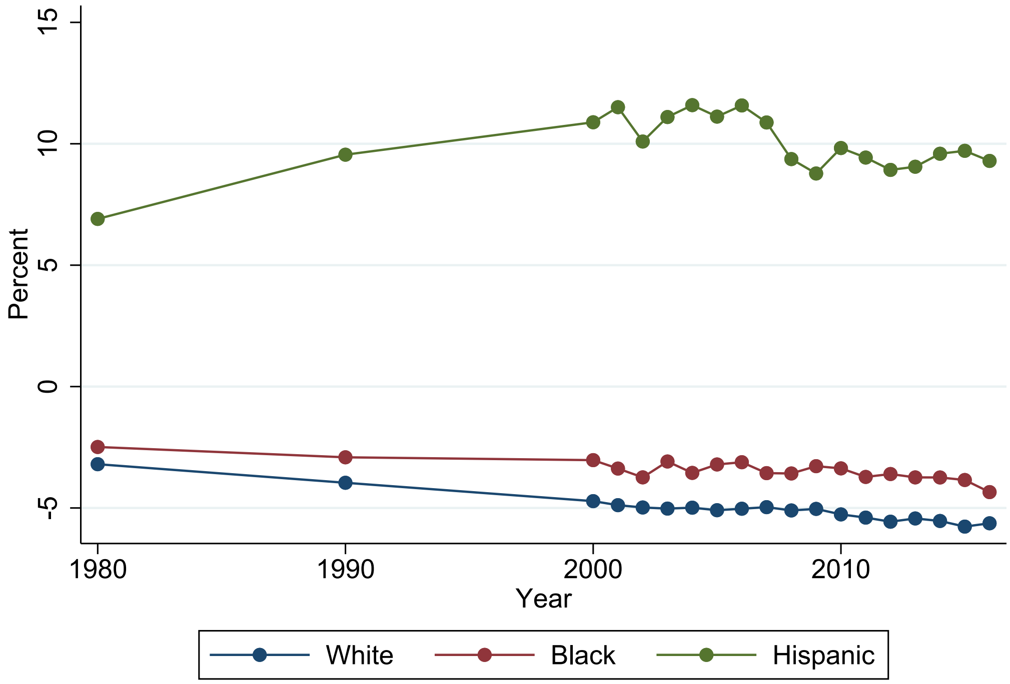
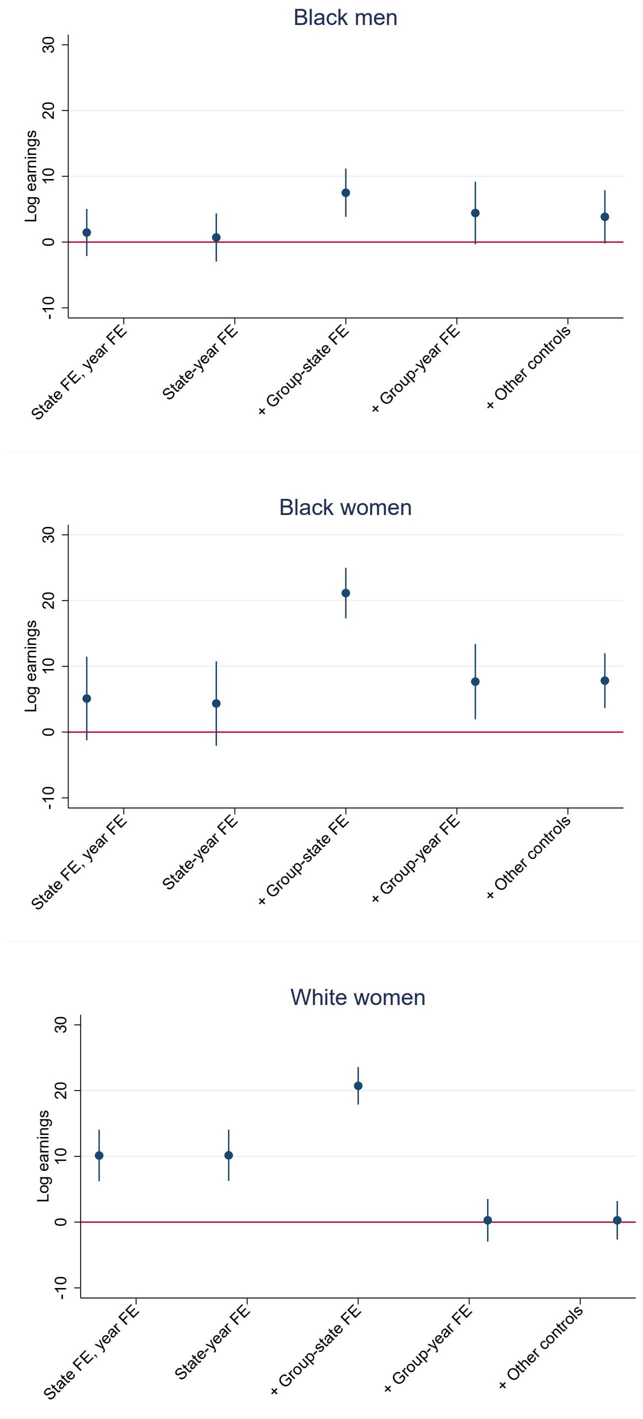


Figure 10: Difference between the share of immigrants in the treated states and the share of immigrants in the control states



Source: Census 1980, 1990, ACS 2000-2016

Figure 11: Estimate of treatment effect from the DD model, with different sets of controls



7 Tables

Table 1: Descriptive statistics for the CPS March sample

	(1) Full sample 1980-2016	(2) Pro-testing states 1980-1988	(3) Control states 1980-1988	(4) Difference	(5) Standard error
Age	35.791	34.087	34.036	0.051	(0.032)
Female	50.623	51.525	50.864	0.661	(0.152)
Black	12.15	16.218	9.96	6.259	(0.115)
Hispanic	12.71	3.721	8.174	-4.452	(0.054)
Young (ages 18-25)	21.895	26.274	26.149	0.125	(0.134)
High school or less	48.036	64.573	58.996	5.577	(0.146)
Employed	73.767	70.475	72.21	-1.735	(0.138)
Worked last year	81.591	80.851	82.05	-1.200	(0.119)
Usual weekly hours	39.356	39.121	38.717	0.404	(0.039)
Weeks worked	45.906	43.592	44.116	-0.524	(0.048)
Testing sector	30.894	37.506	37.71	-0.203	(0.174)
Large firm	45.322	44.792	43.364	1.429	(0.532)
Pension plan at work	51.918	47.883	51.068	-3.185	(0.168)
Group health insurance	63.677	66.028	68.217	-2.189	(0.434)
Hourly wage	20.546	18.385	20.265	-1.880	(0.040)
Earnings	35008	27475	30932	-3457	(97)
Observations	3368474	190618	564559		

Notes: Employment in the testing sector is defined only for those who are currently employed. Pension plan at work is defined for whose who have worked last year (N =2,765,495). Group health insurance is collected starting in 1988 (N=2,694,200) Firm size is available starting from 1988 and defined only for those who have worked last year (N=1,973,110). Usual hours worked last year and weeks worked last year are 0 for those who did not work last year. Observations for hourly wage exclude those with values below 3rd percentile or above 97th percentile.

Table 2: Summary statistics for marriage and fertility outcomes from the CPS March sample

	(1) Full sample 1980-2016	(2) Pro-testing states 1980-1988	(3) Control states 1980-1988	(4) Difference	(5) Standard error
			Black women		
Age	35.047	32.911	33.243	-0.332	(0.119)
Married	32.375	38.974	35.687	3.288	(0.554)
Separated, divorced or widowed	21.325	24.659	25.489	-0.83	(0.493)
Never married	46.299	36.367	38.824	-2.457	(0.555)
Cohabit	1.714				
Single mother (never married)	20.586	16.443	16.464	-0.021	(0.428)
Number of children	1.187	1.539	1.332	0.207	(0.017)
Any children	59.046	66.561	63.517	3.044	(0.545)
Number of children if any	2.01	2.312	2.097	0.216	(0.018)
			White women		
Age	36.321	34.461	34.373	0.089	(0.050)
Married	60.852	68.136	64.255	3.881	(0.220)
Separated, divorced or widowed	14.39	14.059	13.214	0.845	(0.162)
Never married	24.758	17.805	22.532	-4.726	(0.184)
Cohabit	2.176				
Single mother (never married)	3.045	1.019	1.175	-0.157	(0.049)
Number of children	1.023	1.137	1.107	0.030	(0.006)
Any children	53.988	59.088	56.542	2.546	(0.231)
Number of children if any	1.894	1.924	1.958	-0.034	(0.006)
			Hispanic women		
Age	34.229	33.885	32.729	1.156	(0.171)
Married	54.042	63.548	58.798	4.750	(0.798)
Separated, divorced or widowed	15.318	17.292	17.174	0.118	(0.633)
Never married	30.64	19.16	24.028	-4.868	(0.655)
Cohabit	2.687				
Single mother (never married)	9.931	2.276	5.622	-3.346	(0.266)
Number of children	1.399	1.291	1.574	-0.283	(0.022)
Any children	65.252	62.435	67.853	-5.419	(0.801)
Number of children if any	2.144	2.067	2.319	-0.252	(0.023)
Observations	1744136	99116	291673		

Notes: Cohabitation is collected beginning in 1995 (N=920,661).

Table 3: Summary statistics for marriage and fertility outcomes from CPS June Fertility Supplement sample

	(1) Full sample 1980-2016	(2) Pro-testing states 1980-1988	(3) Control states 1980-1988	(4) Difference	(5) Standard error
			Black women		
Age	30.724	27.986	28.564	-0.579	(0.198)
Married	27.865	32.199	29.151	3.048	(0.957)
Never married	57.205	59.93	62.132	-2.202	(1.013)
Cohabit	1.679	0	0	0	(.)
Single mother	19.42	5.256	5.846	-0.59	(0.456)
Number of children	1.027	0.635	0.537	0.098	(0.025)
Any children	49.794	26.25	24.561	1.688	(0.878)
Number of children (if any)	2.063	2.418	2.187	0.231	(0.058)
Number of births	1.315	0.831	0.743	0.088	(0.034)
			White women		
Age	31.32	28.594	28.793	-0.199	(0.074)
Married	51.252	48.749	43.092	5.658	(0.399)
Never married	38.033	43.453	50.168	-6.715	(0.399)
Cohabit	2.31	0	0	0	(.)
Single mother	2.884	0.388	0.467	-0.079	(0.050)
Number of children	0.823	0.421	0.38	0.041	(0.007)
Any children	42.427	21.817	19.313	2.504	(0.314)
Number of children (if any)	1.94	1.93	1.966	-0.036	(0.017)
Number of births	1.002	0.577	0.507	0.070	(0.010)
			Hispanic women		
Age	30.081	28.281	27.622	0.659	(0.358)
Married	48.345	45.334	40.933	4.402	(1.910)
Never married	40.378	47.673	53.483	-5.811	(1.933)
Cohabit	2.625	0	0	0	(.)
Single mother	8.677	1.151	2.143	-0.992	(0.400)
Number of children	1.185	0.349	0.586	-0.237	(0.032)
Any children	54.182	17.674	24.908	-7.234	(1.375)
Number of children (if any)	2.187	1.976	2.353	-0.377	(0.090)
Number of births	1.377	0.483	0.681	-0.199	(0.043)
Observations	470424	30816	98669		

Notes: Cohabitation is collected beginning in 1995 (N=241,822).

Table 4: Summary statistics for birth outcomes from the Vital Statistics records

	(1) Black women	(2) White women	(3) Hispanic women
Age	25.072	27.708	25.69
Married	31.267	78.985	58.397
Father not listed	47.177	17.35	27.497
Birthweight (grams)	3099.768	3371.907	3319.932
Gestation (weeks)	38.282	39.018	38.901
Low birthweight	13.323	6.377	6.379
Very low birthweight	3.04	1.072	1.115
Preterm birth	17.915	9.78	11.045
Teenage birth	21.803	9.217	16.808
Female child	49.225	48.685	48.996
Observations	9,505,078	42,049,449	11,569,276

Notes: Data are from publicly available Vital Statistics birth records and covers years 1989-2003. Father missing is an indicator variable flagging records where no information on the father is provided in the birth record.

Table 5: Estimates of effects of pro-testing policy on main labour market outcomes

VARIABLES	(1) Log earnings	(2) Log hourly wage	(3) Worked last year	(4) Employed in the testing sector	(5) Employed
White women x Pro	0.315	-0.181	-0.311	-1.896**	-0.195
	(1.405)	(0.663)	(0.954)	(0.833)	(0.809)
Black men x Pro	3.114	2.162	-1.729**	1.577*	-2.194**
	(2.052)	(1.383)	(0.808)	(0.939)	(1.026)
Black women x Pro	7.562***	2.231*	0.846	-0.769	0.475
	(2.027)	(1.233)	(1.453)	(1.021)	(1.313)
Observations	2,251,140	2,166,858	2,867,931	2,153,177	2,867,931
R-squared	0.257	0.197	0.072	0.075	0.075
Mean of dep. var.	1027	284.8	82.47	31.27	74.51
White women x Pro: p-value	0.824	0.786	0.746	0.0272	0.811
White women x Pro: p-value (B)	1	1	1	0.272	1
Black men x Pro: p-value	0.135	0.124	0.0372	0.0993	0.0374
Black men x Pro: p-value (B)	1	1	0.372	0.993	0.374
Black women x Pro: p-value	0.000489	0.0764	0.563	0.455	0.719
Black women x Pro: p-value (B)	0.00489	0.764	1	1	1

Notes: Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Estimates of effects of pro-testing policy on additional labour market outcomes

VARIABLES	(1) Employed at a large firm	(2) Pension plan at work	(3) Health insurance	(4) Usual weekly hours	(5) Weeks worked
White women x Pro	-1.050	-1.390*	-0.630*	-0.210	0.0895
	(0.874)	(0.697)	(0.316)	(0.194)	(0.109)
Black men x Pro	3.212***	2.379**	0.959	0.173	-0.148
	(1.027)	(0.997)	(0.991)	(0.245)	(0.200)
Black women x Pro	0.522	1.373*	0.501	0.276	0.536**
	(1.176)	(0.767)	(0.756)	(0.235)	(0.223)
Observations	1,683,189	2,385,767	2,261,389	2,385,767	2,385,767
R-squared	0.023	0.061	0.083	0.133	0.097
Mean of dep. var.	46.45	54.16	66.92	39.44	45.94
White women x Pro: p-value	0.235	0.0516	0.0519	0.284	0.415
White women x Pro: p-value (B)	1	0.516	0.519	1	1
Black men x Pro: p-value	0.00295	0.0209	0.338	0.484	0.461
Black men x Pro: p-value (B)	0.0295	0.209	1	1	1
Black women x Pro: p-value	0.659	0.0797	0.511	0.245	0.0201
Black women x Pro: p-value (B)	1	0.797	1	1	0.201

Notes: Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Estimates of effects of pro-testing policy on a subset of labour market outcomes, by education

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log hourly wage		Testing sector		Employed	
White women x Pro	0.00718	-0.414	-2.339*	-1.176**	-1.017	-0.250
	(1.092)	(0.484)	(1.304)	(0.514)	(0.715)	(0.871)
Black men x Pro	2.756	-0.210	2.363	-0.251	-2.336*	-1.390
	(1.731)	(1.683)	(1.758)	(1.965)	(1.173)	(1.186)
Black women x Pro	4.293**	-0.627	-0.562	-0.162	0.753	-1.507
	(1.689)	(1.406)	(1.437)	(1.437)	(1.386)	(1.501)
Observations	937,868	1,228,990	890,521	1,262,656	1,302,402	1,565,529
R-squared	0.145	0.163	0.079	0.064	0.067	0.050
Mean of dep. var.	265.8	299.1	35.82	28.11	67.71	80.10
White women x Pro: p-value	0.995	0.396	0.0790	0.0265	0.161	0.775
White women x Pro: p-value (B)	1	1	0.790	0.265	1	1
Black men x Pro: p-value	0.118	0.901	0.185	0.899	0.0518	0.247
Black men x Pro: p-value (B)	1	1	1	1	0.518	1
Black women x Pro: p-value	0.0142	0.658	0.697	0.911	0.589	0.320
Black women x Pro: p-value (B)	0.142	1	1	1	1	1
High school degree or less	X		X		X	
More than a high school degree		X		X		X

Notes: Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Estimates of effects of pro-testing and anti-testing policies on a subset of labour market outcomes (high school degree or less)

VARIABLES	(1) Log hourly wage	(2) Employed in the testing sector	(3) Employed
White women x Pro	0.0414	-2.298*	-1.092
	(1.114)	(1.312)	(0.723)
White women x Anti	0.462	0.564	-1.062
	(1.189)	(1.785)	(1.290)
Black men x Pro	2.346	2.152	-2.455**
	(1.720)	(1.763)	(1.193)
Black men x Anti	-14.10**	-7.227**	-3.405
	(6.205)	(3.140)	(4.466)
Black women x Pro	4.185**	-0.708	0.723
	(1.712)	(1.441)	(1.402)
Black women x Anti	-3.054	-5.353*	0.0735
	(2.704)	(3.050)	(1.693)
Observations	937,868	890,521	1,302,402
R-squared	0.145	0.079	0.067
Mean of dep. var.	265.8	35.82	67.71
White women x Pro: p-value	0.971	0.0859	0.137
White women x Pro: p-value (B)	1	0.859	1
White women x Anti: p-value	0.699	0.753	0.414
White women x Anti: p-value (B)	1	1	1
Black men x Pro: p-value	0.179	0.228	0.0448
Black men x Pro: p-value (B)	1	1	0.448
Black men x Anti: p-value	0.0274	0.0256	0.449
Black men x Anti: p-value (B)	0.274	0.256	1
Black women x Pro: p-value	0.0181	0.625	0.608
Black women x Pro: p-value (B)	0.181	1	1
Black women x Anti: p-value	0.264	0.0854	0.966
Black women x Anti: p-value (B)	1	0.854	1
High school degree or less	X	X	X

Notes: Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9: Estimates of effects of pro-testing policy on a subset of labour market outcomes with and without industry fixed effects (high school degree or less)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log hourly wage		Employed at a large firm		Pension plan at work	
White women x Pro	-0.0449	0.00718	-2.349*	-0.957	-0.184	-1.066
	(1.255)	(1.092)	(1.270)	(1.202)	(1.008)	(0.808)
Black men x Pro	2.199	2.756	3.238	2.793*	1.849	2.101
	(2.239)	(1.731)	(2.032)	(1.449)	(1.527)	(1.412)
Black women x Pro	5.480*	4.293**	3.415	1.042	2.994*	3.222***
	(2.867)	(1.689)	(2.232)	(1.425)	(1.628)	(1.144)
Observations	438,764	937,868	214,406	632,502	458,198	1,014,376
R-squared	0.246	0.145	0.061	0.022	0.170	0.042
Mean of dep. var.	274.4	265.8	49.46	40.39	53.63	45.90
White women x Pro: p-value	0.972	0.995	0.0704	0.430	0.856	0.193
White women x Pro: p-value (B)	1	1	0.704	1	1	1
Black men x Pro: p-value	0.331	0.118	0.117	0.0596	0.232	0.143
Black men x Pro: p-value (B)	1	1	1	0.596	1	1
Black women x Pro: p-value	0.0617	0.0142	0.132	0.468	0.0718	0.00692
Black women x Pro: p-value (B)	0.617	0.142	1	1	0.718	0.0691
High school degree or less	X	X	X	X	X	X
Industry FE	X		X		X	

Notes: The sample includes only respondents who are either currently employed; are looking for work and have worked previously; or not in the labour force but have worked in the last 5 years. Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Estimates of effects of pro-testing policy on marriage (March sample)

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Married	Separated, divorced or widowed	Never married	Single mother (never married)	Cohabit
Black women x Pro	1.103	-0.200	-0.903	0.869	-0.221
	(1.127)	(0.667)	(0.824)	(0.991)	(0.326)
Observations	1,488,478	1,488,478	1,488,478	1,488,478	920,659
R-squared	0.184	0.041	0.330	0.097	0.009
Mean of dep. var.	56.63	15.42	27.95	5.648	3.328
Black women x Pro: p-value	0.332	0.766	0.278	0.385	0.502
Black women x Pro: p-value(B)	1	1	1	1	1

Notes: Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 11: Estimates of effects of pro-testing policy on fertility (March sample)

VARIABLES	(1) Number of children	(2) Any children	(3) Number of children, if any
Black women x Pro	-0.0300	2.152*	-0.0929**
	(0.0427)	(1.277)	(0.0380)
Observations	1,488,478	1,488,478	879,427
R-squared	0.110	0.121	0.037
Mean of dep. var.	1.047	54.74	1.913
Black women x Pro: p-value	0.486	0.0983	0.0181
Black women x Pro: p-value(B)	1	0.590	0.108

Notes: Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** p<0.01, ** p<0.05, * p<0.1.

Table 12: Estimates of effects of pro-testing and anti-testing policies on fertility (March sample)

VARIABLES	(1) Number of children	(2) Any children	(3) Number of children, if any
Black women x Pro	-0.0306	2.086	-0.0915**
	(0.0428)	(1.281)	(0.0384)
Black women x Anti	-0.0239	-2.446*	0.0539
	(0.0281)	(1.279)	(0.0551)
Observations	1,488,478	1,488,478	879,427
R-squared	0.110	0.121	0.037
Mean of dep. var.	1.047	54.74	1.913
Black women x Pro: p-value	0.478	0.110	0.0210
Black women x Pro: p-value (B)	1	1	0.210
Black women x Anti: p-value	0.399	0.0617	0.333
Black women x Anti: p-value (B)	1	0.617	1

Notes: Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Estimates of effects of pro-testing policy on fertility, heterogeneity analysis (March sample)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Number of children, if any					
Black women x Pro	-0.0695	-0.0573*	-0.0609*	-0.157***	-0.220***	-0.0939
	(0.0477)	(0.0330)	(0.0322)	(0.0573)	(0.0675)	(0.0579)
Observations	405,100	474,327	355,899	523,528	404,166	119,362
R-squared	0.048	0.032	0.085	0.026	0.028	0.054
Mean of dep. var.	1.942	1.887	1.890	1.929	1.991	1.730
High school degree or less	X					
More than high school degree		X				
18-35 years old			X			
36+ years old				X	X	X
Married					X	
Not married						X
Black women x Pro: p-value	0.152	0.0886	0.0644	0.00835	0.00199	0.112
Black women x Pro: p-value(B)	0.909	0.532	0.386	0.0501	0.0120	0.669

Notes: Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** p<0.01, ** p<0.05, * p<0.1.

Table 14: Estimates of effects of pro-testing policy on fertility (June sample)

VARIABLES	(1) Number of births	(2) Any births	(3) Number of births, if any	(4) Number of births	(5) Any births	(6) Number of births, if any
Black women x Pro	-0.0659*	0.971	-0.144**	-0.107	-0.785	-0.0752
	(0.0381)	(1.347)	(0.0703)	(0.0689)	(0.792)	(0.0887)
Observations	189,628	189,628	103,641	146,108	146,108	107,869
R-squared	0.378	0.485	0.111	0.238	0.359	0.091
Mean of dep. var.	1.281	54.90	2.333	1.794	73.22	2.451
Black women x Pro: p-value	0.0897	0.474	0.0452	0.128	0.327	0.401
Black women x Pro: p-value(B)	0.538	1	0.271	0.767	1	1
June sample	X	X	X	X	X	X
High school degree or less	X	X	X			
36+ years old				X	X	X

Notes: Robust standard errors in parentheses (clustered at state level). p-value denotes the unadjusted p-value for the estimated coefficient, and p-value (B) denotes Bonferroni-corrected p-value. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 15: Estimates of effects of pro-testing policy on infant health (Vital Statistics birth records)

VARIABLES	(1) Married	(2) Father not listed	(3) Teenage birth	(4) Gestation (weeks)
Black women x Pro	0.00893	-0.0118	-0.0104***	0.0259
	(0.00621)	(0.0204)	(0.00322)	(0.0227)
R-squared	0.197	0.163	0.143	0.017
	Birthweight (grams)	Low birthweight	Preterm birth	Prenatal care in trim. 1
Black women x Pro	-6.473	0.00466***	-0.00196	-0.0144
	(3.980)	(0.00171)	(0.00295)	(0.00986)
R-squared	0.032	0.011	0.011	0.076
Observations	31,517,729	31,549,712	31,549,712	31,549,712

Table 16: Estimates of effects of pro-testing policy on living circumstances of households with children

VARIABLES	(1) Food stamps receipt	(2) Living in public housing
White women x Pro	1.552	-0.119
	(1.097)	(0.382)
Black men x Pro	-2.443*	-0.433
	(1.254)	(0.775)
Black women x Pro	-1.778	-1.747
	(1.703)	(2.162)
Observations	833,307	833,307
R-squared	0.184	0.104
Mean of dep. var.	11.35	3.013
White women x Pro: p-value	0.163	0.758
White women x Pro: p-value (B)	0.490	1
Black men x Pro: p-value	0.0571	0.579
Black men x Pro: p-value (B)	0.171	1
Black women x Pro: p-value	0.301	0.423
Black women x Pro: p-value (B)	0.904	1
Household heads with children	X	X

References

ACLU (2019). Drug testing, factsheet. http://www.aclu.org/FilesPDFs/testing_chart.pdf.

Acs, G. (1996). The impact of welfare on young mothers' subsequent childbearing decisions. *Journal of Human Resources*, (pp. 898–915).

Alere Toxicology (2016). State-by-state legal status guide. https://www.edrugtest.com/Messages-from-Admin/Statebystatelaw-Guide-89046.pdf.

Alexander, M. (2012). *The new Jim Crow: Mass incarceration in the age of colorblindness*. The New Press.

An, C.-B., Haveman, R., & Wolfe, B. (1993). Teen out-of-wedlock births and welfare receipt: The role of childhood events and economic circumstances. *The Review of Economics and Statistics*, (pp. 195–208).

Armstrong, R. A. (2014). When to use the b onferroni correction. *Ophthalmic and Physiological Optics*, *34*(5), 502–508.

Black, D. A., Kolesnikova, N., Sanders, S. G., & Taylor, L. J. (2013). Are children "normal"? *The review of economics and statistics*, *95*(1), 21–33.

Black, D. A., McKinnish, T. G., & Sanders, S. G. (2003). Does the availability of high-wage jobs for low-skilled men affect welfare expenditures? evidence from shocks to the steel and coal industries. *Journal of Public Economics*, *87*(9-10), 1921–1942.

Blau, F. D., & Kahn, L. M. (2007). Changes in the labor supply behavior of married women: 1980–2000. *Journal of Labor Economics*, *25*(3), 393–438.

Blau, F. D., Kahn, L. M., & Waldfogel, J. (2000). Understanding young women's marriage decisions: The role of labor and marriage market conditions. *ILR Review*, *53*(4), 624–647.

Braga, B., et al. (2018). The effects of trade exposure on marriage and fertility choices: Evidence from brazil. Tech. rep., Institute for the Study of Labor (IZA).

Bruins, M. (2017). Women's economic opportunities and the intra-household production of child human capital. *Labour Economics*, *44*, 122–132.

Burston, B. W., Jones, D., & Roberson-Saunders, P. (1995). Drug use and african americans: Myth versus reality. *Journal of Alcohol and Drug Education*.

Capps, R., Fortuny, K., & Fix, M. (2007). Trends in the low-wage immigrant labor force, 2000–2005. *Washington, DC: The Urban Institute*, *9*.

Carroll, J. (1995). The negative attitudes of some general nurses towards drug misusers. *Nursing standard (Royal College of Nursing (Great Britain): 1987)*, *9*(34), 36–38.

Coates, T.-N. (2015). The black family in the age of mass incarceration.

Dallaire, D. H. (2007). Incarcerated mothers and fathers: A comparison of risks for children and families. *Family relations*, *56*(5), 440–453.

De Bernardo, M. A., Delogu, N. N., & Mendelson, L. (2008). *Guide to State & Federal Drug-testing Laws*. Institute for a Drug-Free Workplace.

De Jong, G. F., & Madamba, A. B. (2001). A double disadvantage? minority group, immigrant status, and underemployment in the united states. *Social Science Quarterly*, *82*(1), 117–130.

Dehejia, R., & Lleras-Muney, A. (2004). Booms, busts, and babies' health. *The Quarterly Journal of Economics*, *119*(3), 1091–1130.

Dorn, D., Hanson, G. H., et al. (2018). When work disappears: Manufacturing decline and the falling marriage-market value of young men. Tech. rep., IZA Discussion Papers.

Duncan, G. J., & Hoffman, S. D. (1990). Welfare benefits, economic opportunities, and out-of-wedlock births among black teenage girls. *Demography*, *27*(4), 519–535.

Edin, K., & Reed, J. M. (2005). Why don't they just get married? barriers to marriage among the disadvantaged. *The Future of Children*, (pp. 117–137).

Ellwood, D. T., & Crane, J. (1990). Family change among black americans: What do we know? *Journal of Economic Perspectives*, *4*(4), 65–84.

Fryer Jr, R. G. (2007). Guess who's been coming to dinner? trends in interracial marriage over the 20th century. *Journal of Economic Perspectives*, *21*(2), 71–90.

Garfinkel, I., Huang, C.-C., McLanahan, S. S., & Gaylin, D. S. (2003). The roles of child support enforcement and welfare in non-marital childbearing. *Journal of Population Economics*, *16*(1), 55–70.

Gilliom, J. (1996). *Surveillance, privacy, and the law: Employee drug testing and the politics of social control*. University of Michigan Press.

Gust, S. W., Walsh, J. M., & Thomas, L. B. (1990). Drugs in the workplace: Research and evaluation data volume ii. *Research Monograph*, *100*, 20402–9328.

Hall, M., Greenman, E., & Farkas, G. (2010). Legal status and wage disparities for mexican immigrants. *Social Forces*, *89*(2), 491–513.

Hendrik F. Van den Berg, Ö. B. B. (2009). Hispanic immigration to the united states.

Jacobson, M. (2003). Drug testing in the trucking industry: the effect on highway safety. *The Journal of Law and Economics*, *46*(1), 131–156.

James-Burdumy, S., Goesling, B., Deke, J., & Einspruch, E. (2010). The effectiveness of mandatory-random student drug testing. ncee 2010-4025. *National Center for Education Evaluation and Regional Assistance*.

Karch, S. B., et al. (2008). *Workplace drug testing*. CRC Press.

Kearney, M. S., & Levine, P. B. (2012). Why is the teen birth rate in the united states so high and why does it matter? *Journal of Economic Perspectives*, *26*(2), 141–63.

Kearney, M. S., & Levine, P. B. (2015). Investigating recent trends in the us teen birth rate. *Journal of health economics*, *41*, 15–29.

Kearney, M. S., & Wilson, R. (2017). Male earnings, marriageable men, and non-marital fertility: Evidence from the fracking boom. *Review of Economics and Statistics*, (0).

Koch, D. S. (1999). Protections in federal rehabilitation legislation for persons with alcohol and other drug abuse disabilities. *Journal of Applied Rehabilitation Counseling*, *30*(3), 29.

Kon, A. A., Pretzlaff, R. K., & Marcin, J. P. (2004). The association of race and ethnicity with rates of drug and alcohol testing among us trauma patients. *Health Policy*, *69*(2), 159–167.

Kunins, H. V., Bellin, E., Chazotte, C., Du, E., & Arnsten, J. H. (2007). The effect of race on provider decisions to test for illicit drug use in the peripartum setting. *Journal of Women's Health*, *16*(2), 245–255.

Lamothe, S. (2005). State policy adoption and content: a study of drug testing in the workplace legislation. *State and Local Government Review*, *37*(1), 25–39.

Lang, K., & Lehmann, J.-Y. K. (2012). Racial discrimination in the labor market: Theory and empirics. *Journal of Economic Literature*, *50*(4), 959–1006.

Lee, T. (2016). The city: Prison's grip on the black family.

Lichter, D. T., LeClere, F. B., & McLaughlin, D. K. (1991). Local marriage markets and the marital behavior of black and white women. *American journal of Sociology*, *96*(4), 843–867.

Lindo, J. M. (2010). Are children really inferior goods? evidence from displacement-driven income shocks. *Journal of Human Resources*, *45*(2), 301–327.

Livingston, G., & Brown, A. (2017). Intermarriage in the us 50 years after loving v. virginia. Tech. rep., Pew Research Center Report.

Lundberg, S., & Plotnick, R. D. (1995). Adolescent premarital childbearing: Do economic incentives matter? *Journal of Labor Economics*, *13*(2), 177–200.

Macdonald, I. (2013). The gop's drug-testing dragnet.

Mamun, A. A., et al. (2008). *Effects of employment on marriage: Evidence from a randomized study of the Job Corps program*. Office of Planning, Research, and Evaluation, Administration for Children and Families, US Department of Health and Human Services.

Marcin, J. P., Pretzlaff, R. K., Whittaker, H. L., & Kon, A. A. (2003). Evaluation of race and ethnicity on alcohol and drug testing of adolescents admitted with trauma. *Academic emergency medicine*, *10*(11), 1253–1259.

Mare, R. D., & Winship, C. (1991). Socioeconomic change and the decline of marriage for blacks and whites. *The urban underclass*, *175*, 175.

McLaughlin, D., & Long, A. (1996). An extended literature review of health professionals' perceptions of illicit drugs and their clients who use them. *Journal of Psychiatric and Mental Health Nursing*, *3*(5), 283–288.

Mehay, S. L., & Pacula, R. L. (1999). The effectiveness of workplace drug prevention policies: does' zero tolerance'work? Tech. rep., National Bureau of Economic Research.

Miller-Day, M., & Barnett, J. M. (2004). " i'm not a druggie": Adolescents' ethnicity and (erroneous) beliefs about drug use norms. *Health Communication*, *16*(2), 209–228.

Moffitt, R. A. (1998). *The effect of welfare on marriage and fertility*. Washington, DC: National Academy Press.

Moffitt, R. A. (2000). Welfare benefits and female headship in us time series. *American Economic Review*, *90*(2), 373–377.

Morantz, A. D., & Mas, A. (2008). Does post-accident drug testing reduce injuries? evidence from a large retail chain. *American Law and Economics Review*, *10*(2), 246–302.

Moskowitz, G. B., Stone, J., & Childs, A. (2012). Implicit stereotyping and medical decisions: unconscious stereotype activation in practitioners' thoughts about african americans. *American Journal of Public Health*, *102*(5), 996–1001.

Murray, C. (1993). Welfare and the family: The us experience. *Journal of Labor Economics*, *11*(1, Part 2), S224–S262.

Murray, J., & Farrington, D. P. (2008). The effects of parental imprisonment on children. *Crime and justice*, *37*(1), 133–206.

Musto, D. F. (1999). *The American disease: Origins of narcotic control*. Oxford University Press.

Narum, S. R. (2006). Beyond bonferroni: less conservative analyses for conservation genetics. *Conservation genetics*, *7*(5), 783–787.

National Drug Screening (2018). Workplace drug testing state laws. Tech. rep., National Drug Screening.
URL `https://www.nationaldrugscreening.com/us-state-laws.php`

Neal, D. (2004). The relationship between marriage market prospects and never-married motherhood. *Journal of Human resources*, *39*(4), 938–957.

Neumark, D. (2018). Experimental research on labor market discrimination. *Journal of Economic Literature*, *56*(3), 799–866.

Olsen, R. J., & Farkas, G. (1990). The effect of economic opportunity and family background on adolescent cohabitation on adolescent cohabitation and childbearing among low-income blacks. *Journal of Labor Economics*, *8*(3), 341–362.

Pardes, A. (2014). The war on drug tests.

Parke, R. D., & Clarke-Stewart, K. A. (2003). The effects of parental incarceration on children. *Prisoners once removed: The impact of incarceration and reentry on children, families, and communities*, (pp. 189–232).

Rivera-Batiz, F. L. (1999). Undocumented workers in the labor market: An analysis of the earnings of legal and illegal mexican immigrants in the united states. *Journal of Population Economics*, *12*(1), 91–116.

Robbins, A. (1991). Employment discrimination against substance abusers: The federal response. *BCL Rev.*, *33*, 155.

Robins, L. N., Davis, D. H., & Goodwin, D. W. (1974). Drug use by us army enlisted men in vietnam: A follow-up on their return hom. *American Journal of Epidemiology*, *99*(4), 235–249.

Rothwell, J. (2014). How the war on drugs damages black social mobility. *Brookings Institution*.

Sampson, R. J. (1987). Urban black violence: The effect of male joblessness and family disruption. *American journal of Sociology*, *93*(2), 348–382.

Schaller, J. (2016). Booms, busts, and fertility testing the becker model using gender-specific labor demand. *Journal of Human Resources*, *51*(1), 1–29.

Sedgwick, P. (2012). Multiple significance tests: the bonferroni correction. *BMJ*, *344*, e509.

Shenhav, N. (2018). Bribed to wed? family formation and labor market responses to the gender wage gap.

Shoag, D., & Veuger, S. (2016). No woman no crime: Ban the box, employment, and upskilling.

Steingold, F. (2017). The employer's legal handbook: Manage your employees and workplace effectively.

Tonry, M. (1994). Race and the war on drugs. *U. Chi. Legal F.*, (p. 25).

Tunnell, K. D. (2004). *Pissing on demand: Workplace drug testing and the rise of the detox industry*. NYU Press.

U.S. Department of Labor (1989). Survey of employer anti-drug programs. Report 760, Bureau of Labor Statistics, Washington, D.C.

Williams, M. K. (2016). The war on drugs is a war on people.

Wilson, W. J. (1996). *When work disappears: The world of the new urban poor*. Vintage.

Wisotsky, S. (1990). *Beyond the war on drugs: Overcoming a failed public policy*. Prometheus Books.

Wolfe, B., Wilson, K., & Haveman, R. (2001). The role of economic incentives in teenage nonmarital childbearing choices. *Journal of Public Economics*, *81*(3), 473–511.

Wood, R. G. (1995). Marriage rates and marriageable men: A test of the wilson hypothesis. *Journal of Human Resources*, (pp. 163–193).

Wozniak, A. (2011). Field perspectives on the causes of low employment among less skilled black men. *American Journal of Economics and Sociology*, *70*(3), 811–844.

Wozniak, A. (2015). Discrimination and the effects of drug testing on black employment. *Review of Economics and Statistics*, *97*(3), 548–566.

Yamaguchi, R., Johnston, L. D., & O'Malley, P. M. (2003). Relationship between student illicit drug use and school drug-testing policies. *Journal of School Health*, *73*(4), 159–164.

Zwerling, C. (1993). Current practice and experience in drug and alcohol testing in the workplace. *Bull Narc*, *45*(2), 155–196.