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Discrimination and the Effects of Drug Testing on Black Employment

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ABSTRACT

Nearly half of U.S. employers test job applicants and workers for drugs. I use variation in the timing and nature of drug testing regulation to study discrimination against blacks related to perceived drug use. Black employment in the testing sector is suppressed in the absence of testing, consistent with ex ante discrimination on the basis of drug use perceptions. Adoption of pro-testing legislation increases black employment in the testing sector by 7–30 percent and relative wages by 1.4–13.0 percent, with the largest shifts among low skilled black men. Results suggest that employers substitute white women for blacks in the absence of testing.

JEL Classification Codes: J7, J15, K2, K3, M5

Key Words: Employment drug testing, discrimination, employment, Current Population Survey

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Discrimination and the Effects of Drug Testing on Black Employment

In 2011, the United States entered the fifth decade of its War on Drugs.[1] The drug war has been vilified both within the United States and abroad, and it is often declared a failure, as in the face of these efforts drug use has risen over the period.[2] Perhaps the most frequent charge is that the drug war has had disproportionately negative impacts on African Americans. This is certainly the case, and a large body of scholarship provides evidence affirming this claim.[3] A quiet companion to the drug war has been the increased use of drug testing within mainstream American society. U.S. employers began requiring drug tests of their employees and job applicants on a large scale in the 1980s. Drug tests are now routinely required of job applicants and employees.[4] In a comprehensive 1994 report on workplace drug testing, the National Research Council remarked that "[i]n a period of about 20 years, urine testing has moved from identifying a few individuals with major criminal or health problems to generalized programs that touch the lives of millions of citizens" (National Research Council, 1994, p. 180). According to the U.S. Department of Health and Human Services, 45 percent of employees in the United States now work for firms that conduct some form of drug testing (see Appendix Table A1), while 15–20 percent report having been tested themselves (Fendrich and Kim 2002).

Contrary to what one might expect, the rise of employer drug testing may have provided a means for African Americans to escape some of the drug war's disproportionate impacts. Even as the drug war linked blacks with drug use in the popular imagination, drug testing provided a

[1] The phrase "War on Drugs" was first used by Richard Nixon in 1971.

[2] For example, see the introductory chapters of Provine (2007) and Baum (1997). Jimmy Carter's *New York Times* Op-Ed from June 17, 2011, used the anniversary of the War on Drugs to call it a failed policy.

[3] For thorough studies, see Western (2006), Provine (2007), and Alexander (2010).

[4] Drug tests are also commonly required of military recruits and personnel, and as a pre-requisite for participation in school activities (Mehay and Pacula 1999; U.S. Supreme Court decision in Vernonia v. Acton 1995; "At One College, A Fight Over Required Drug Tests," *New York Times*, October 10, 2011. *The New York Times* also reports that three-quarters of states have recently considered a drug test requirement for some benefits. At least four have passed. ("States Adding Drug Test as Hurdle for Welfare." October 10, 2011.)

means for nonusing blacks to prove their status to employers. In this paper, I model and test for the impact of employer drug testing on labor market outcomes for blacks. I incorporate drug testing by firms and drug use by workers into a Roy model with signaling. The model allows the information in drug tests to impact hiring and reduce ex ante bias through one of two channels. The first is a standard statistical discrimination channel, in which testing provides employers with more information on blacks than whites. The second is updating of biased beliefs about use rates across the two groups. I cannot distinguish the two channels empirically, but I derive three implications for how drug testing would impact sorting into testing and nontesting sectors across race and drug use groups in the presence of either channel. I discuss facts that suggest biased beliefs cannot be ruled out in the conclusion.

To test the model's predictions, I combine data from the National Survey on Drug Use and Health—both public use and special tabulations of confidential data—and the Current Population Surveys. I also estimate a set of Mincer-style equations that allow returns to race and other characteristics to differ according to an individual's exposure to testing. Using three decades of microdata, I examine changes in outcomes within and across demographic groups and industries as drug testing prevalence increased nationally. I identify employer drug testing's impacts using state and year variation in statutes affecting the ability of employers to test both job applicants and employees. Importantly, such statutes have taken both "pro-testing" and "antitesting" forms. These contrasting statutes provide a useful check, since the estimated impacts should be different in the two groups of states when compared with nonadopting states. I also exploit differences across metropolitan areas within states in the likelihood of drug testing based on stable differences in industrial and firm size structure, both of which are strong correlates of drug testing.

Consistent with the model's predictions, I find that employment of nonusers increased in testing industries following the advent of drug testing. I find suggestive evidence that this increase was more pronounced for blacks, which is consistent with ex ante bias. Using state-level variation in the timing and nature of drug testing legislation, I find large labor market impacts for blacks, a fact that is also consistent with widespread ex ante bias. The results are largest for low-skilled black men. Specifically, pro-testing legislation increases the share of low-skilled black men working in high-testing industries by 7–10 percent relative to all states with no such law and by up to 30 percent relative to states with an antitesting law. I find similar increases in their coverage in group health and pension plans, benefits that are associated with the larger and more sophisticated firms that are also more likely to test, and in employment of low-skilled black men at large firms generally. The results are based on specifications that allow for time-varying growth in testing industries at the state level, ensuring that my results are not driven by coincident sectoral employment changes. Finally, I find that wages for low-skilled black men increase by 3–4 percent relative to states with no pro-testing law and by 12 percent relative to antitesting states. This wage increase is driven by the employment shifts into larger firms and industries with known wage premia. Results from antitesting states suggest that employers substitute white women for blacks in the absence of testing.

This paper has important implications for our understanding of labor market discrimination, and these in turn have implications for how to address it. First, I find evidence consistent with bias in hiring that is not purely taste-based. This suggests an opportunity for improving black outcomes by improving employer information about black job applicants. My interpretation of these findings is more flexible than that offered by canonical statistical discrimination models.[5] The model allows, and in the conclusion I discuss the merits of, the idea that employers operate without racial animus, conditional on their beliefs, but these beliefs may be biased. This allows a type of discrimination that is very close to the implicit discrimination described in Bertrand, Chugh, and Mullainathan (2005). This is also consistent with evidence from other social sciences and with new evidence from experimental economists. Sociologists and political scientists have long argued that certain behaviors can become "racialized"—that is, disproportionately associated with a particular group (Beckett et al. 2005 ; Gilens 1996). More recently, Albrecht et al. (2011) find that participants in a laboratory labor market experiment fail to fully update beliefs about individual productivity when this is revealed subsequent to learning that individuals belong to groups with different levels of average productivity.[6]

A second contribution is that, in contrast to most studies on discrimination, this paper evaluates a specific, current policy that policymakers can easily extend or encourage. Research on employer drug testing has so far been limited to studying specific industries or firms where testing has been implemented (Carpenter 2007; Lange et al. 1994; Jacobson 2003; Mas and Morantz (2008);; Mehay and Pacula 1999). These early studies were important for understanding effects in these industries, but they overlook the possible general equilibrium effects of such a widespread policy. Moreover, none of this earlier work examines differential impacts across racial groups.

Finally, this paper adds to the set of studies that directly examines employer responses to changes in the information they receive. These include Holzer et al (2006); Stoll and Bushway

[5] Charles and Guryan (2008, 2011) provide a useful overview of the main models of labor market discrimination and discuss challenges to testing these models. For detailed analyses of statistical discrimination models, see Aigner and Cain (1977); Altonji and Pierret (2001); Lundberg and Startz (1983); and Oettinger (1996).

[6] Fehrshtman and Gneezy (2001) also find that subjects rely on incorrect stereotypes (biased beliefs) and pay a price for it in a trust game. On the other hand, Ewens, Tomlin, and Wang (2012) find belief updating consistent with statistical discrimination when landlords receive information shocks in a housing market experiment.

(2008); Finlay (2009); and Autor and Scarborough (2008). The first three focus on the impact of criminal background information on hiring of ex-offenders and blacks. Autor and Scarborough (2008) examine the impact of a skills test on minority hiring into low skill service jobs, and find that the test increases precision of worker selection (more productive workers are hired) but that the racial composition of hiring is unchanged. They conclude that in this sector, human-based screening was unbiased relative to the skills test. This paper shows that policies that encourage employer drug testing led to economically large increases in black employment at firms that are more likely to test. This suggests that the Autor and Scarborough results may be unique to their setting and that biased screening on other dimensions may be widespread.

BACKGROUND ON DRUG USE, DRUG TESTING, AND DRUG TESTING STATUTES

The Expansion of Employer Drug Testing

The arrival of drug testing in the labor market in the early 1980s was driven by a combination of three factors: 1) a small number of somewhat sensational workplace accidents in which drugs were alleged to have played a role,2) the development of accurate and relatively inexpensive screening devices, and 3) rising public anxiety over the prevalence of drugs in society. These culminated in the creation of federal incentives for workplace drug testing.[7] The early 1980s were a period in which small numbers of employers, albeit often large ones, began requiring drug tests of their employees in an atmosphere of legal uncertainty. Litigation by tested employees was common. In 1987, an executive order by Ronald Reagan requiring that federal agencies adopt testing to establish "drug-free workplaces" went into effect. The 1988 Drug-Free

[7] Facts in this paragraph are taken from Tunnell (2004, Ch. 1); National Research Council (1994, Ch. 6); and Appendix A. A shorter review of the history of employer testing can be found in Knudsen, Roman, and Johnson (2003). See Baum (1997) for an excellent history of the drug war.

Workplace Act went further, requiring that federal contractors adopt comprehensive antidrug policies.[8] Employee and applicant drug testing was clearly in the spirit of this legislation. By the late 1980s, the grounds on which employers could require testing was well-established in the courts, notably with a major Supreme Court decision in 1989 (National Research Council, 1994, Appendix B). Thus, the late 1980s constitute a turning point after which employers begin implementing drug testing programs in increasing numbers.

Recognizing the increasing prevalence of these tests, the Bureau of Labor Statistics (BLS) conducted a survey in 1988 to gauge the extent of drug testing practices among U.S. employers (U.S. Department of Labor 1989). The findings of the report are summarized in Table 1, in the column headed "1988." A follow up to the BLS survey was conducted by outside researchers in 1993 (Hartwell et al. 1996). The findings of that report are summarized in the column headed "1993." The first point to take from Table 1 is that regularities in testing prevalence appear in both surveys. Larger employers are more likely to test than smaller employers; there is wide variation in rates of testing across industries; and there is regional variation, with larger shares of establishments testing in the South and Midwest than in the Northeast or West. Knudsen, Roman, and Johnson (2003) found similar differences across industries and firm size categories using a 1997 phone survey of employed respondents. The second point to take away from Table 1 is that the share of testing employers increased dramatically in the period between the surveys. Direct comparisons across the industry and region cells are complicated by changes in the sampled universe across the surveys.[9] However,

[8] An overview of the history and current state of the Federal Workplace Drug Testing program is provided in Bush (2008). More details on drug testing, test failure, and detection evasion is in Appendix I.

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

the share of establishments with 50 or more employees testing in 1988 was 0.16 (Hartwell et al. 1996). This rose to 0.48 by 1993, or a threefold increase for this group overall.

There has been no follow up to the 1993 survey, but comparable statistics can be computed using the annual National Survey on Drug Use and Health (NSDUH). The NSDUH questioned respondents about the drug testing policies of their employers starting in 1997. I calculated the shares of employed respondents replying that their employer practiced some form of drug testing. The final column of Table 1 reports these shares overall and by industry.[10] The NSDUH shares indicate that drug testing increased only modestly in the period following the 1993 BLS survey. The rapid expansion of employer drug testing therefore appears to have ended by the second half of the 1990s with testing stabilized at its new, higher level.[11]

State Level Drug Testing Laws

During the late 1980s, individual states also began to pass guidelines regulating the use of testing by employers (DeBernardo and Nieman 2006; National Research Council 1994). The state-level legislation grew out of the opposing forces at work behind the federal laws and legal history: the desire to punish and criminalize drug use on the one hand and concerns for privacy and civil liberties protection on the other.[12] Both sets of concerns generated legislation at the state level. Some states enacted explicitly "pro" employer testing legislation, while others enacted explicitly "anti" legislation. Broadly, pro-testing legislation provided incentives for employer testing (often through rebates on UI or worker's compensation insurance), capped

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[10] The BLS surveys omitted establishments in the agriculture and government sectors. Industry testing rates can be calculated for these in the NSDUH.

[11] The United States has been the clear leader in workplace drug testing, but it is worth noting that the practice is expanding in other developed countries as well (Verstraete 2001, 2005). Estimates suggest that about 20 percent of employers in the UK test, and the practice is not limited to countries with restrictive drug laws.[11] In fact, Finland introduced one of Europe's more expansive pieces of drug testing legislation in the early 2000's (Lamberg et al. 2008).

[12] It is unclear from the available social history whether employers as a group were in favor of drug testing.

certain liabilities for testing employers, or explicitly permitted certain types of testing. Antitesting legislation explicitly limited the types of testing employers could require.

I rely on DeBernardo and Nieman (2006) for details of the variation in state-level drug testing policies. Their *2006–2007 Guide to State and Federal Drug Testing Laws* is a resource for employment law professionals, and they categorize states as either pro- or antitesting. Twenty-one states are categorized. The remainder is considered neutral on employer drug testing. Fourteen states are classified as pro-testing; seven are antitesting. More detail on their classification is provided in Appendix Table A1. Table A1 shows that while pro-testing states are more commonly found in the South, there is still considerable variation within regions. For example, Ohio is a Northern Rust Belt state, but it is also pro-testing. Utah and Montana are both inter-mountain West states but Utah adopted pro-testing legislation while Montana adopted antitesting laws. I follow DeBernardo and Nieman in classifying states as pro, anti, or neutral on employer drug testing. They do not assign a date in which a state "became" pro- or antitesting, but they provide a complete listing of statutes related to their classification along with dates of passage. I use the year a related statute was first adopted as the "start year" for a state's employer drug testing stance.

It is difficult to demonstrate the effect of these laws empirically, since data on employer testing prevalence at the state level is nearly nonexistent. However, upon special request, the agency that conducts the NSDUH survey (The Substance Abuse and Mental Health Services Administration, or SAMHSA, within Health and Human Services) agreed to tabulate respondent answers to questions about employer drug testing at the state level for the periods 2002–2003 and 2007–2009 and provide them in a table for public circulation.[13]

[13] It is not possible to obtain comparable tabulations for earlier periods, since only very recent waves of the NSDUH were designed to be representative at the state level.

During the period 2002–2009, three states (Alaska, Arkansas, and Louisiana) adopted pro-testing laws, and three (Montana, Rhode Island, and Vermont) adopted antitesting laws. Two of these (Alaska and Arkansas) passed their first statutes in 2002, so any changes in testing prevalence over the 2002–2003 and 2007–2009 periods is arguably obscured by including post-statute data into the initial period average. However, I have fairly clean before and after information on testing prevalence for the remaining four states. In Table 2, I show levels of reported employer drug testing during the two periods for adopting states as well as two sets of comparison states.

The top panel in the table shows testing rates in Louisiana before and after it adopted its pro-testing statute. Over the period, the share of NSDUH respondents reporting some form of employer drug testing increased 3.4 percentage points from its already high level of 56.5. Other, specific forms of testing also increased noticeably. Compared to states that had similar levels of initial testing but did not pass statutes, Louisiana experienced much higher growth in employer testing.[14] This can be seen by comparing the changes in Louisiana with those in Panel B. In the nonadopting, but initially high-testing, states, the prevalence of employer testing actually decreased slightly over the period. This suggests that pro-testing statutes increase the prevalence of employer testing in a state. In fact, the growth in testing prevalence in Louisiana is closer to the growth for other initially high-testing states that already had pro-testing laws (Panel D). It is less clear from Table 2 that antitesting statutes had the effect of curtailing testing. The average change in testing prevalence in the three states adopting antitesting statutes is similar to those in initially low testing states that did not pass a law (Panel A). On the other hand, growth in testing

[14] Because there is a strong trend toward increased drug testing throughout the United States, it is important to compare adopting states with those that were initially similar in terms of drug testing prevalence. States with low levels of testing have much room for growth and may see greater growth in testing, even in the presence of anti-testing statutes, than states that begin the period at high levels of testing.

prevalence among low testing states that already had antitesting statutes (Panel C) was lower than for similar states with no statute (Panel A), providing some evidence that antitesting laws may curtail testing in some states.

The table also shows that there are large-level differences across states in the prevalence of reported testing. Respondents in pro-testing states report the highest rates of testing and those in antitesting state report the least. Respondents in pro-testing states report that their employer tests at rates 50 to 73% higher than those in antitesting states. See Appendix Table A1 for more detail on cross-state differences in testing.

Patterns and Perceptions of Drug Use

In contrast to the limited data on drug testing by employers, measures of drug use are available back to 1979 in the NSDUH. For most of the survey's history, blacks and whites have reported drug use at nearly identical rates. There is some variation in drug type, with blacks reporting more marijuana use and whites more "hard" drugs, but overall, the rate of any reported drug use in the past month is very similar for blacks and whites. Over the 1990–2006 period, 13 percent of whites and 12 percent of blacks reported some drug use in the past month in the NSDUH. This holds even within gender and skill groups, with less-skilled blacks and less skilled whites (no college education) both reporting past month use at rates of 19 percent. This is consistent with evidence in Kaestner (1999). More detail on use and reporting patterns can be found in the Appendix I.

More importantly for the purposes of this paper, there is evidence showing that *the perception* is that blacks use drugs at much higher rates than whites. In a thorough study of such perceptions and their consequences, Beckett et al. (2005) conclude that racial drug arrest disparities cannot be solely attributed to either structural differences in drug use or to policing tactics that are otherwise race-neutral. Rather, they argue that police have developed a set of perceptions around who was likely to be carrying drugs and that these perceptions led them to disproportionately target blacks. They write, “[P]opular discussions and images of the “crack epidemic” in the 1980s appear to . . . continue to shape both popular *and police* perceptions of drug users (emphasis added).” The fact that even those responsible for investigating and documenting drug crime can hold perceptions of use that differ from reality suggests that others might also hold persistent misperceptions. Several studies support this possibility. In a survey of hiring managers, Wozniak (2011) documents a belief that blacks are more likely to fail a drug test. Burston, Jones, and Roberson-Saunders (1995) cite evidence that even black youth overestimate their own drug use relative to whites. They also cite a 1989 survey in which 95 percent of respondents described “the typical drug user” as black.

A ROY MODEL OF THE EMPLOYMENT EFFECTS OF INDUSTRY DRUG TESTING

In this section, I incorporate drug use by workers and drug testing by firms into a standard, two-sector Roy model as developed in Heckman and Sedlacek (1985) and Heckman and Honore (1990). The strategy I will follow will be to solve the model in two environments: one in which drug testing is available and the other in which it is not.[15] I then derive predictions under the assumption that employers display bias toward blacks in the absence of testing. In the empirical work, I examine whether the data matches the model’s predictions conditional on the ex ante bias assumption.

Let firms be divided into the testing sector and the nontesting sector, so named because of the practices they will adopt when drug testing becomes available. Workers are endowed with a

[15] I refer to the latter as “post-drug testing” or “after the introduction of testing” to indicate that testing has been developed and become available. The model is not dynamic.

vector of sector-specific skills **s** = (s_T, s_N), denoting skills in the testing and nontesting sectors, respectively. Workers can apply for employment in either sector and move between them costlessly at any time. There are two periods, or equilibria: the pretesting period, when drug testing is not available to firms, and the posttesting period, in which all testing firms instantaneously adopt testing of all workers and job applicants.

The key modification I make to the standard Roy model is to assume that testing sector skills are negatively affected by a worker's drug use. For simplicity, I assume that drug use reduces testing sector skills to zero, so that **s** becomes the following:

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

Skills ***s*** are observable, and I assume that drug use is independent of latent skills ***s*** (i.e., skills in the absence of drug use) but obviously not of realized ***s***.[16] Testing sector firms anticipate that the total output from hiring a given set of workers—some of whom use drugs—is lower than it would be if there was no drug use. Assume for now that firms have no information about which hires are more likely to use drugs. In this case, they simply deflate offered wages by a constant probability of drug use. Thus, testing sector firms offer wages w_T equal to an applicant's expected marginal productivity given the possibility of drug use, p: $w_T = k_T(1-p)s_T$, where $k_T(1-p) = \pi_T(p)$.[17] Nontesting firms offer wages equal to expected (and realized) marginal productivity: $w_N = \pi_N s_N$, where π_N is a constant. $\pi_T(p)$ and π_N are then the sector-specific skill prices in a standard Roy model.

[16] I discuss this and the assumption that drug use sets productivity in the testing sector to zero in detail in Appendix III.

[17] This assumes that total output is a function of the sum of individual worker productivities and does not otherwise depend on their combination. k_T is a constant return to skill in the testing sector that is discounted by p to give the traditional sector-specific skill prices in the Roy model.

I assume that skills in the two sectors are log-normally distributed, with $\ln s_j \sim N(\mu_j,\ \sigma_j)$ so that $\ln s_j = \mu_j + \varepsilon_j$ for $j = T, N$.[18] Assuming workers choose their sector of employment to maximize wages, the probability of employment in the testing sector is equal to the probability that the testing sector wage exceeds the nontesting sector wage, which in turn becomes a function of the parameters of the skill distribution:

$$\begin{aligned}(2)\qquad &\Pr(T) = \Pr\left(\pi_T(p)\, s_T \geq \pi_N s_N\right) = \\ &\Pr\left(\ln k_T + \ln(1-p) + \mu_T + \varepsilon_T \geq \ln \pi_N + \mu_N + \varepsilon_N\right)\end{aligned}$$

Note that a worker's own drug use does not affect the wages he expects to receive in either sector since only population drug use is relevant for wage setting in the testing sector.

Suppose that in addition to **s** and D_i, workers possess an observable characteristic M_i which takes the values 0 and 1. Now there are two populations of workers. In principle M can be any observable characteristic, but for exposition let $M = 1$ represent blacks and $M = 0$ represent whites. The distribution of **s** does not vary across the M groups.[19]

Now consider firms' beliefs about rates of drug use in the two demographic groups. Denote these p_{M1} and p_{M0}. These may differ from true rates of use, denoted p^*_{M1} and p^*_{M0}. Without loss of generality, assume $p_{M1} > p_{M0}$. This implies that firms' productivity expectations are unequal across groups, even if firms believe the underlying skills distributions are the same, i.e., absent drug use. Firms in the testing sector will therefore offer higher wages to whites (M = 0) than they will to blacks (M = 1), conditional on s_T. Using the formula in (2), it is clear that

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

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

these differences in assumed use rates imply that $\Pr\left(T \mid s, M_i = 1\right) < \Pr\left(T \mid s, M_i = 0\right)$ in the pretesting period.[20]

Drug testing introduces a signal into this environment. Following what is known about the validity of drug tests, I assume that firms that require drug tests of their applicants receive a signal t_i of drug use with the following properties:[21]

$$
\text{(3)} \qquad \begin{aligned} & t_i = 1 \Rightarrow D_i = 1 \\ & t_i = 0 \Rightarrow E\left[D_i \mid \text{post testing}\right] = \tilde{p} \end{aligned}
$$

This type of signal potentially accomplishes two things. First, it increases the likelihood that testing sector employers select nonusers when hiring. This is because $\tilde{p} < p$, which I prove in the Theory Appendix. I refer to this effect as "increased precision" in screening. Second, the information that arrives via the signals may enable employers to revise their beliefs about use rates.

Increased precision in worker screening raises the likelihood that nonusers are employed in the testing sector. To see this, first notice that $\tilde{p} < p$ implies that $\pi(\tilde{p}) > \pi(p)$. The introduction of testing raises $\pi_T(p)$ in Equation (2) and leaves all other terms unchanged, unambiguously increasing $\Pr(T)$. In the Theory Appendix, I show that this in turn raises the probability of employment in the testing sector rising among nonusers after testing is introduced.[22]

This increase in precision need not affect blacks and whites differentially. For example, if $p_{M1} = p_{M0}$ and $\tilde{p}_{M1} = \tilde{p}_{M0}$, then testing sector employment will rise equally for blacks and

[20] It does not necessarily follow that $\Pr(T \mid M_i = 1) < \Pr(T \mid M_i = 0)$, since the relationship in the text is conditional on skills. Indeed, I will show that blacks were more likely than whites to be employed in the testing sector in the pretesting era.

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

[22] For users, the effect of testing on the probability of employment in the testing sector is actually ambiguous, as shown in the Theory Appendix.

whites after the introduction of testing. Autor and Scarborough (2008) show this more generally in a somewhat different model. As long as employer beliefs are relatively unbiased for blacks and whites, then the added precision of testing can change who is hired from each group while leaving overall group hiring rates unchanged. However, if testing affects the precision of firms' ex ante beliefs differentially, then testing may change relative outcomes across the two demographic groups.

A change in relative outcomes following the introduction of testing would be consistent with ex ante bias in employer beliefs about drug use, but the *nature* of the change in relative precision is important for the interpretation of this bias. There are two possibilities. First, employers may believe their black applicants use drugs at rates equal to the true average, $p_{M1} = p^*_{M1}$, but because of better information about white applicants, they believe use among the white applicants they consider is lower than average, $p_{M0} < p^*_{M0}$. In this case, the ex ante bias corresponds to classic statistical discrimination. Employers have more precise information that allows them to screen out some white drug users but no black users. The introduction of testing may then improve information on black applicants relative to whites. On the other hand, employers may hold biased beliefs about black drug use rates, such that $p_{M1} > p^*_{M1}$ but $p_{M0} = p^*_{M0}$. Then testing also has the potential to reduce the disparity between perceived and actual use rates for blacks. The interpretation, however, is different. In this case, the ex ante bias is driven by inaccurate employer beliefs rather than information disparities.

I cannot distinguish between the two types of bias empirically. In both cases, employer updating means that $p_{M1} - p^*_{M1} > \tilde{p}_{M1} - p^*_{M1}$. Substitution of the new employer beliefs into (2) shows that the probability of employment in the testing sector rises for blacks after testing is introduced. The revision of ex ante bias would also lead to larger changes in $\pi(p)$ for blacks than for whites, so that the probability of testing sector employment increases more for black nonusers than for white nonusers.[23]

In sum, the model generated three predictions that I will test empirically. First, the share of nonusers employed in the testing sector should increase after the advent of testing, regardless of employer bias in beliefs about drug usage. Second, if employers' beliefs about drug use are overstated for blacks relative to whites (ex ante bias of either kind), then the increase in testing sector employment should be greater among black nonusers than white nonusers. Finally, if employers are ex ante biased, testing should increase the employment of blacks in the testing sector. I discuss the two possible interpretations of ex ante bias in light of the results in the conclusion.

ASSESSING THE IMPACT OF EMPLOYER DRUG TESTING: DATA AND EMPIRICAL MODELS

Microdata Sources

I draw on microdata from two sources. The bulk of the analysis uses microdata on individuals aged 18–55 from the IPUMS versions of the March Current Population Surveys (King et al. 2010). I use this data to answer questions about differential impacts of employer drug testing on labor market outcomes *without regard to drug use*. For example, were blacks more likely to be hired into the testing sector after testing became widespread? The March CPS surveys contain the richest set of employment variables in the monthly CPS. The resulting data set includes representative, annual cross sections of prime aged individuals in the U.S. spanning 1980–2010.

[23] It is important to note that this assumes that (relative) drug use rates are unchanged across demographic groups, but the evidence in Appendix Table A2 suggests this is a reasonable assumption.

I supplement the CPS analysis with data from the National Survey on Drug Use and Health (NSDUH). The NSDUH is a nationally representative survey of individuals aged 12 and older. It is currently conducted annually although the survey was semiannual between 1987 and its inception in 1979. The sample size has increased considerably over time. The 1979 sample contained roughly 7,200 individuals and grew to include over 55,000 individuals in 2006. It is the definitive source of data on drug use in a representative U.S. population. The NSDUH contains detailed information on respondent drug use histories and, in later years, on employer drug testing practices. I use the NSDUH data to answer questions about how the sorting of drug users and nonusers changed across sectors as testing expanded. All NSDUH analysis and statistics are unweighted. Unfortunately, causal analysis of testing's impacts on labor market outcomes in the NSDUH sample are limited by two features of the survey. First, it does not include geographic identifiers below the nine census divisions. This precludes the difference-in-differences analysis I carry out in the CPS using state-year variation in drug testing legislation.[24] Second, it is not possible to construct exact hourly wages from NSDUH data as income information is only available in bins. Descriptive statistics for the NSDUH sample are available upon request.

Descriptive statistics on the CPS sample are given in Table 3. Race/ethnicity is measured using indicators for black and Hispanic.[25] Education is measured using two categories: high school dropouts and high school graduates (the low-skill group) and those with any postsecondary education (the high-skill group). Table 3 also summarizes employment outcomes of interest. Because the CPS does not ask about employer drug testing, I use three proxies for

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[24] Carpenter (2007) has carefully documented correlations between an individual's outcomes and the reported drug testing practices of her employer.

[25] Other nonwhite races are not separately identified in the CPS until the latter part of my sample period. As a result, the omitted race/ethnicity category in most specifications is properly called "whites, Asians, and Native Americans," although I refer to the group simply as "whites."

employment at a likely testing firm. The first is a dummy for employment in the high-testing sector. I define the high-testing sector as one-digit industries that achieve a testing rate of over 50 percent by the late 1990s according to Table 1. Specifically, these are mining, communications and utilities, transportation, manufacturing, and government. [26] Table 3 shows that the high-testing sector employs about 30 percent of currently employed workers. The second is the dummy variable for employment at a very large firm (> 500 employees), which is only available for 1988 and onward. As discussed above, there is a clear relationship between employer size and the likelihood of drug testing. About 40 percent of the total sample is employed in a very large firm. The final measure is a dummy indicating coverage in a group health or pension plan.[27] These benefits are likely related to employer size and sophistication—e.g., the presence of a well-developed human resources department. The benefits coverage outcome is also interesting because it reflects a broader notion of job quality than wages alone. Table 3 shows that coverage rates for both benefits are somewhat higher than 50 percent. Hourly wages are constructed by dividing wage and salary income earned last year by the product of weeks worked last year and usual weekly hours. Wages are adjusted to 1990 levels using the CPI-U.

Table 3 also breaks out various subsamples of interest. One can also compare the characteristics of CPS respondents from states that ultimately become pro- or antitesting. Since I exploit variation within states over time, identification does not require that the two groups of states look identical. Nevertheless, the two groups of states are largely balanced on the dimensions in Table 3. The main exceptions are racial composition and prevalence of employment at large firms.

[26] The universe for the industry variable is actually workers who worked at any time in the last five years. I limit this to workers who were employed at the time of the survey.

[27] The universe of the group health questions changed over time, and the question wording changed slightly. However, results are similar when pension coverage alone is the dependent variable.

Estimating Equations

I will first assess the model's prediction that the share of nonusers employed in the high-testing sector should increase after the introduction of testing. To do this, I estimate a model with employment in a high-testing industry as the dependent variable using the NSDUH data. However, since the NSDUH contains limited geographic information, I cannot exploit state-year variation in employer drug testing statutes. Instead, I identify the impact of expanded employer drug testing using time series variation in national rates of testing combined with information on regional differences in drug testing rates from Appendix Table A1. Data limitations force me to restrict the NSDUH data to the 1985–1997 waves. I divide the period into three phases: 1) the pretesting years of 1985–1988, 2) the period of rapid transition to higher testing rates of 1989 to 1994, and 3) the post-period of 1994–1997. I then divide the nine census divisions (the finest geographic information available in the public NSDUH) into low, intermediate, and high-testing based on division-level average testing rates calculated from Appendix Table A1 and noted in Table 4.

I then look for evidence of two phenomena. First, were nonusers increasingly sorted into high-testing industries over time and in higher testing regions? And second, was the shift of nonusers into testing sector employment larger for blacks? A regression with high-testing industry on the left-hand side would require examining multiple triple (drug use × time period × testing region) and quadruple interactions (the triple interaction times race) to test predictions one and two. An alternative is to examine differences in adjusted high-testing sector employment rates between users and nonusers by time period-testing region cells. I first compute the residuals from a regression of high-testing sector employment on controls for demographics (age, race, Hispanic ethnicity, sex, and educational attainment), demographic group-specific cubic time trends, group-specific division fixed effects, and all relevant main effects. I then compute the difference in means for these residuals within the nine region by time period cells, subtracting the mean residual high-testing sector employment of users from that for nonusers. This approach is more descriptive than a regression but also more transparent.[28]

I then turn to the CPS to examine the impact of state-level employer drug testing laws on relative labor market outcomes. The following equation allows the employer testing environment in an individual's state to affect the returns to her personal characteristics and generates difference-in-differences estimates of drug testing's impacts by demographic group (or difference-in-differences-in-differences [DDD] estimates):

(4) $$y_{ist} = Pro_{st}\Gamma_{ist}'\beta_1 + \tilde{\Gamma}_{ist}'\beta_2 + \beta_3 Pro_{st} + \Theta_s + \Theta_t + \Theta_s t + \varepsilon_{ist}$$

Pro_{st} is an indicator variable equal to 1 if state *s* with a pro-testing classification in DeBernardo and Nieman (2006) has enacted drug testing legislation by year *t*. β_1 and β_2 are k × 1 vectors of (demographic) group-specific coefficients. Γ is a k × 1 vector of demographic characteristics. These include indicators for black, white, and Hispanic ethnicity; gender; age less than 25; and no postsecondary education (low skill). $\tilde{\Gamma}$ is identical to Γ except that age is entered directly and age-squared is included. The specification includes a typical set of DD controls when the policy variation is at the state and year level. These are state fixed effects, Θ_s; year fixed effects, Θ_t; and state time trends. The state fixed effects absorb permanent differences across states in the outcome variable, while the year fixed effects absorb common shocks to outcomes at the national level. The state-specific time trends absorb smooth changes in labor market outcomes across states over the period of the study.

y_{ist} is one of several possible labor market outcomes. These include the three proxies for employment at a likely testing firm described above. I also examine the impact of testing

[28] Results from an equivalent regression model available upon request. A final issue with the regression approach is the need to correct standard errors for the small number of clustersin this case, at most nine.

legislation on employment in general and on log wages, although neither is represented in the model. The estimates of interest are the coefficients in the β_1 vector. These show how log wages and the four employment variables change differentially for the demographic groups in Γ after a state adopts pro-testing legislation. Therefore these are triple differenced, or DDD, estimates.

Although Equation (4) is a common specification, it is likely inadequate for studying differential impacts of time-varying, state-level policies across demographic groups. For one thing, there are likely fixed *group-specific* differences across states in the outcome variable. There are also likely important changes that are common to the U.S. labor market *for a demographic group* as a whole over this period. An example is rising wage inequality, which increased differentially for workers according to race, gender, and skill group.[29] For these and other reasons discussed below, I estimate the following as my preferred specification:

$$\begin{aligned}(5)\qquad y_{ist} &= Pro_{st}\Gamma_{ist}'\beta_1 + \tilde{\Gamma}_{ist}'\beta_2 + \beta_3 Pro_{st} + \chi_{st}'\beta_4 + \Theta_s + \Theta_s\Gamma_{ist}' + \\ &\quad t + t^2 + t^3 + t\Gamma_{ist}' + t^2\Gamma_{ist}' + t^3\Gamma_{ist}' + \Theta_s t + \Theta_s t^2 + \Theta_s t^3 + \mu_{ist}\end{aligned}$$

To arrive at (5), two main changes were made to the specification in (4). First, group-specific state effects and group-specific cubic time trends were added. These address concerns about fixed, group-specific differences across states and nonlinear, differential time trends across demographic groups noted above. As I show later, the parameter estimates of interest are unaffected by using group-specific cubic trends in place of the group-specific year effects. This speeds computation considerably. I also allow for nonlinear state trends rather than imposing linear state trends as in (4).

The second change is the addition of time varying state-level controls, X_{st}. These are the state unemployment rate, state minimum wage, state incarceration rate, and annual employment

[29] Katz and Murphy (1992). Autor, Katz, and Kearney (2008) show that the major changes in the U.S. wage structure that occurred over the 1980s and 1990s are fairly well approximated by group specific quadratics.

growth for each of the five one-digit industries that comprise the high-testing sector.[30] These controls are added to further address specific concerns about the possible endogeneity of state drug testing statutes. The first two control for variation in state labor market conditions. It is possible that employers are less opposed to legislation related to employee screening when state labor markets are slack than when they are tight. Including controls for state labor market conditions mitigates concerns that drug testing laws reflect effects of these conditions rather than the policies themselves. Similarly, the state incarceration rate is a measure of intensity of state-level efforts to curb drug trafficking. Some state legislatures may have had a general "get tough" policy on drug offenses, leading to high drug interdiction efforts at the same time that they passed pro-testing legislation. If such interdiction efforts affected drug use or perceptions of drug use, then this might lead to changes in black employment across industries independently of employer testing policies. Finally, X_{st} includes annual employment growth in the five testing sector industries. Suppose employers are concerned about drug use among blacks but sector growth means they need to hire more from this population. Testing sector employers may then push states to adopt pro-testing legislation to better enable them to screen applicants while expanding employment. The direct controls for industry growth account for this possibility. As mentioned above, my reading of the history surrounding drug testing statues suggests that these laws are driven primarily by political considerations. They are therefore likely exogenous to state labor market conditions. Consistent with this, the exclusion of X_{st} has little impact on the results I will report. I nevertheless retain X_{st} in the preferred specification for completeness.

[30] State-level unemployment rates for 1976–2010 are from the Bureau of Labor Statistics. State minimum wage data for 1969–2010 are from the Department of Labor. State prison populations for 1977–2010 are from the Bureau of Justice Statistics National Prisoner Survey series. State-level annual employment growth by one-digit industry is constructed from the Bureau of Economic Analysis CA-25 and CA-25N series.

Because the nature of drug testing legislation varied across states, I am able to expand the specification in Equation (5) to further exploit the variation in testing environments provided by states that adopted antitesting laws. In the specification below, $Anti_{st}$ is a scalar that takes the values zero or one according to timing of legislation in states classified as antitesting. The controls are the same, and the specification becomes the following (main effects for state effects and the time cubic are omitted for readability):

$$\text{(6)} \quad \begin{aligned} y_{ist} &= Pro_{st}\Gamma_{ist}{}'\beta_1 + \tilde{\Gamma}_{ist}{}'\beta_2 + \beta_3 Pro_{st} + Anti_{st}\Gamma_{ist}{}'\beta_4 + \beta_5 Anti_{st} + \chi_{st}{}'\beta_6 + \\ &\quad \Theta_s\Gamma_{ist}{}' + t\Gamma_{ist}{}' + t^2\Gamma_{ist}{}' + t^3\Gamma_{ist}{}' + \Theta_s t + \Theta_s t^2 + \Theta_s t^3 + \mu_{ist} \end{aligned}$$

Now there are two sets of DDD estimates: β_1 as before but also β_4. β_1 has a similar interpretation as in Equation (5). It estimates the differential outcomes for the demographic groups in Γ relative to the same groups in states that never pass or have not yet passed a pro-testing or antitesting law. Since the impacts of antitesting laws are now estimated in β_4, residents in those states no longer form part of the comparison group to identify β_1, after the antitesting law has passed. It is now also possible to compare impacts for the same demographic groups across different types of states, while controlling for time effects. If employer drug testing changes employment outcomes differentially across demographic groups (and if state testing laws affect employer behavior), then β_1 and β_4 should generally be of opposing signs. This additional variation allows me to test whether the content—and not just the presence—of legislation matters.

Finally, I exploit differences across local labor markets within states in the likelihood of exposure to testing. These differences arise because industrial structure and the distribution of firm sizes varies across metropolitan areas within a state, but these differences are quite stable over time. The composition of the local economy therefore creates differences in the likelihood that an individual was exposed to drug testing but does not itself respond to the adoption of testing legislation. I collected metropolitan area level information on the distribution of firm size and industrial composition and created an index of exposure to drug testing by multiplying the elements of these distributions by the national shares of reported testing by industry and firm size.[31] I normalize the index to have mean zero and standard deviation one, and incorporate it into Equation (5) by replacing the first three terms in (5) with the first seven terms in (7):

$$\begin{aligned} y_{ijst} = {} & Pro_{st} DT_{ijst} \Gamma_{ist}{}' \gamma_1 + \tilde{\Gamma}_{ist}{}' \gamma_2 + \gamma_3 Pro_{st} + \gamma_4 DT_{ijst} + DT_{ijst} \Gamma_{ist}{}' \gamma_5 + \\ & \gamma_6 Pro_{st} DT_{ijst} + Pro_{st} \Gamma_{ist}{}' \gamma_7 + remaining\ terms\ \ from\ (5) + \eta_{ijst} \end{aligned} \tag{7}$$

Here, the estimates of interest are in the vector γ_1. These show whether relative outcomes change differentially for individuals in metropolitan areas (indexed by *j*) with high drug testing exposure (DT_{ijst}) as compared to individuals in the same demographic group and state but in areas with lower exposure. These estimates provide a final check on whether differential changes in labor market outcomes after the adoption of state-level testing laws are related to the likelihood of experiencing testing.

All models are estimated using OLS, and coefficients are therefore from a linear probability model. This facilitates the calculation of total impacts across interactions and main effects. Since the means of all dependent dummy variables are well inside the unit interval, the results are very similar when estimated via probit. In all estimates using CPS data, standard errors are clustered at the state level. However, the results are robust to clustering on state and year instead of state only.

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[31] Data on MSA-level employment by firm size and industry for 1997–1999 were taken from the U.S. Census Bureau's Statistics of U.S. Businesses. I calculate the index of drug testing exposure for MSA *j* as follows:

$$\rho_j = \left(\sum_k \delta_{jk} r_k\right) + \left(\sum_m \delta_{jm} r_m\right)$$

k indicates industries and *m* indicates firm size categories. The δ terms represent the share of *j*'s employment in a particular industry or firm size category. These sum to 1 within area *j*. The *r* terms are the national level rates of employers in the various categories engaging in drug testing. These rates are taken from the sources in Table 1. Theoretically, the index can achieve a maximum value of 2, if all employers in all categories are testing, but I normalize the measure to have mean zero and standard deviation one.

RESULTS

Before moving to estimation of the empirical models, I present preliminary evidence on the impact of state-level employer drug testing policies using a simple event study analysis. I examine only one outcome—employment in a high-testing industry—for the sake of conciseness. Figure 1 shows that the prevalence of this employment declined steadily over the entire data period for both blacks and whites. Consistent with the means in Table 3, blacks are more likely than whites to work in the high-testing sector. The question for an event study, then, is not what happened to trends in testing sector employment as laws were phased in over time, but rather what happened to relative employment trends for blacks versus whites around the point at which a law was introduced?

Figures 2a and 2b answer that question. Each shows the difference between year 0 and year *t* employment rates in high-testing industries, where year zero is the year of adoption and *t* ranges from 10 years prior to passage to 10 years after. Smoothed trends in this difference are plotted separately for blacks and whites. In both panels, the trend for whites declines smoothly over time with no noticeable change in the year of passage. Consistent with Figure 1, the share of whites employed in high-testing industries is declining over time. It appears unaffected by state employer drug testing laws. For blacks, however, trends in both pro- (2a) and antitesting (2b) states show changes at year zero. In pro-testing states, the steady decline in testing sector employment among blacks stops at year zero and then reverses to tick upward slightly by several years after law passage. The change is less dramatic in antitesting states, but there is still a clear inflection point for the black trend at year zero, indicating that the decline in black testing sector employment picked up speed in the year of and immediately following passage of an antitesting law. Together, the two figures suggest that employer testing laws encouraged testing sector firms to employ blacks relative to whites in pro-testing states while antitesting laws discouraged it. To test this more formally, I turn to the empirical analysis.

The Impact of Testing on the Sorting of Users and Non-Users into Employment Sectors

Table 4 tests the first of the model's predictions: that the share of nonusers employed in the testing sector increases after the introduction of testing. Panel (i) of Table 4 shows that the probability of adjusted high testing sector employment was insignificantly different for users and nonusers in all three regions during the pretesting period. A respondent is classified as a drug user if she reports using any drug illicitly in the past month and as a nonuser otherwise.[32] During the transition period, the difference in testing sector employment widens, with non-users becoming 4 to 6 percentage points more likely to work in the high testing sector than users. For the two higher testing regions, this gap persists and retains significance into the posttesting period. However, the gap disappears in the low testing region. As shown in Appendix Table A1, many low testing states passed antitesting laws starting in the transition years. This potentially explains the roll back of the earlier effect. Panels (ii) and (iii) show that this pattern is similar for blacks and whites, with the exception that the nonuser employment advantage is only significant for blacks in the two higher testing regions in the posttesting period. This evidence affirms the model's first prediction: users sort increasingly into high-testing sector employment in times and places where testing is more common. This also confirms that drug testing provides employers with information that they use in making their hiring decisions.

To assess the model's second prediction, I consider the change in the testing sector employment gap between users and nonusers separately for blacks and whites. For both groups, the gap widens in favor of nonusers during the transition period. The gap widens further for

[32] Results are similar when users are defined as those reporting any drug use in the past year, zero otherwise.

blacks in the posttesting period but is largely stable for whites. Also, the increase in the gap over the preperiod in the highest testing region is larger for blacks than for whites. I conclude that the evidence in Table 4 is therefore suggestive that the impacts of employer drug testing were larger and more positive for nonusing blacks than nonusing whites.

The Impact of Testing on Relative Labor Market Outcomes in the CPS

The remainder of the analysis uses variation in state drug testing legislation to generate DDD estimates of the impact of testing on relative labor market outcomes. Results from the preferred specification, Equation (5), are shown in Table 5. Here the control group is comprised of individuals in all states that have not affirmatively adopted a pro-testing law. This includes states that will eventually adopt pro-testing laws in the future, states that will or have adopted antitesting laws, and all never-adopting states. The five columns report results from estimating Equation (5) with five different dependent variables.

The coefficients of interest are the interactions of demographic characteristics with the pro-testing law indicator. Blacks, Hispanics, women, and the low skilled all have consistently signed impacts of pro-testing legislation on the three measures of high-testing sector employment. For blacks and the low skilled, the impacts are positive and of similar magnitude, showing increases of about 1–3 percentage points in the dummies for high-testing industry employment, large firm employment, and benefits coverage. For blacks, the positive impacts on benefits coverage and on large firm employment are significant at the 0.1 percent and 5 percent levels, respectively. Log wages also increase for blacks following the adoption of a pro-testing law. The impacts on these measures are also positive for the low skilled but of about half the magnitude, with the exception of a statistically significant positive wage impact of 1.3 percent that is similar to the 1.4 percent increase for blacks. Impacts for the young (18–25) and Hispanics are generally very small economically and all are insignificant. There is no impact on overall employment for any group. Taken together, these results suggest that blacks experience larger and more consistent improvements in testing sector employment and wages following the adoption of a pro-testing law than any other group.

For women, on the other hand, the impacts of pro-testing legislation are uniformly negative. High-testing industry employment, large firm employment, and benefits coverage all decline for women by about 1.5 percentage points. The point estimate on log wages is also negative for women. The bottom rows of the table show that postestimation tests of equality reject that the coefficients for blacks and women are the same for all measures except the employment dummy. In other words, pro-testing legislation has significantly different impacts on blacks and women.

Before moving on from Table 5, it is worth noting that the reported additional covariates generally perform as expected. These will not be reported in subsequent tables. However, while there are no big surprises in the bottom half of the table, there is some important heterogeneity. Specifically, the significant determinants differ across the three proxies for testing sector employment. The low skilled, for example, are significantly more likely to work in a high-testing industry than the high skilled, but they are significantly less likely to work in a very large firm or receive benefits. This and the other differences across the bottom half of the columns in Table 5 suggest that the proxies for testing sector employment each capture something slightly different, which makes them useful as a set of related but not identical outcomes. To save space, coefficients on the five industry employment growth variables are not reported, but they are jointly significant.

Equation (6) incorporates the policy variation from antitesting states, and estimates from this model are presented in Table 6. The top panel shows that estimates on the *pro-testing* × *demographic group* interactions from Table 5 are robust to the addition of the antitesting interactions. In fact, the point estimates and patterns of significance are essentially unchanged between Tables 5 and 6 for the pro-testing interactions. Nevertheless, the antitesting interactions are interesting for several reasons. First, estimates for blacks are negative, economically large, and statistically significant for high testing industry employment. This suggests that the impact of pro-testing legislation on blacks is due directly to the increased adoption of testing by employers, since the passage of laws discouraging such testing leads to opposite impacts. Importantly, t-tests reject the equality of the pro- and antitesting interactions with black status for all three measures of testing sector employment and for wages, as shown in the bottom rows of the table. The negative impact of testing legislation on women appears to be confined to pro-testing states. There are no significant impacts—or even large point estimates—for antitesting laws on women in Table 5. However, t-tests reject the equality of the pro- and antitesting interactions with female status for all outcomes except general employment. Blacks and women in pro- and antitesting states therefore experience significantly different impacts of the legislation in their respective states. These impacts differ not just across blacks and women in the same states, but also across blacks (or women) in the two types of states.

Sample and population size both likely play roles in the anti-testing estimates for blacks and women. First, as is obvious from the geographic variation in Appendix Table A1, antitesting states tend to have small black populations whereas pro-testing states have larger ones. Fixed and constant-trend differences across these states are controlled in the estimates using fixed effects and state time trends, but it is still the case that state-level black populations in anti-testing states are very small. Therefore it is to be expected that point estimates for the *black antitesting* interactions will have larger standard errors than estimates for the *black* × *pro-testing* interactions. A related point is that in pro-testing states, an economically large shift in labor market outcomes for blacks may well have spillover effects to other groups, such as women, since blacks are a large share of the population in those states. This is less true in anti-testing states. Where blacks are a very small share of the population, then an economically large change for blacks may still have little impact on the labor market equilibrium as a whole. This may explain why there are strong negative impacts of employer testing on women in pro-testing states but no opposing effects in antitesting states.

The interactions with Hispanic and antitesting legislation in Table 6 are uniformly negative and economically large. However, the interactions with Hispanic are never significant, and t-tests do not reject that the interactions with Hispanic are equal across pro- and antitesting states. If these laws do have an impact on Hispanics, I am not able to precisely estimate them with the available data. Therefore I exclude Hispanics from subsequent analysis in order to focus on the impacts for blacks and women.

To examine the separate contributions of race, skill, and gender from a different angle, I break down the black and white populations into mutually exclusive demographic groups (listed in Appendix Table A2). The equations estimated in Table 7 substitute indicator variables for these eight groups for the Mincer-style controls for demographic characteristics used in Tables 5 and 6. I drop Hispanics from the sample and divide the remaining CPS respondents into categories according to race, sex, and skill. I modify Equation (6) to include indicators for the seven groups and their interactions with pro- and antitesting legislation. High-skilled white men are omitted. All other controls are modified accordingly.

The impacts of pro-testing laws in Table 7 are even larger than in earlier specifications. This is because they combine the impacts of being black, male, and low skilled, for example, that were estimated "separately" in the Mincer-style specifications. Table 7 shows that it is low skilled blacks who experience the largest positive impacts of pro-testing legislation on their labor market outcomes. All point estimates are also positive for low-skilled black women. I find that employment in the high-testing sector increases by 3.8 to 4.5 percentage points for low-skilled black men, relative to the same group in states that do not adopt a drug testing law. This is an increase of 9.7 percent for employment in a high-testing industry and roughly 7–9 percent for the other two outcomes. The magnitude is even larger when compared to low-skilled black men in antitesting states. Here the difference in testing sector employment is approximately 9–13 percentage points between blacks in pro- versus antitesting states, as shown in the bottom rows of the table. This implies a relative increase in high-testing sector employment of about 30 percent for low-skilled blacks. The results also show a statistically and economically significant wage increase of 3.3 percent for low-skilled black men in pro-testing states. The difference relative to the same group in antitesting states is even larger, at 13 percent, and also statistically significant. For low-skilled black men, I again reject that the interactions with pro- and antitesting state status are the same for all outcomes except general employment.

The pro- versus antitesting interactions are sometimes statistically unequal for women (both black and white), but for no other group are all three testing sector proxies unequal. Nevertheless, the general pattern identified in previous tables—in which impacts for white women are negative in pro-testing states and positive in antitesting states—is also apparent in Table 7. Low-skilled black men are also the only group in which the wage impacts of testing legislation are statistically different across the two groups of states, despite the significant coefficient on pro-testing legislation for low-skilled white men in the wage equation.

In unreported results, I examined whether the wage increases observed for blacks in pro-testing states in Tables 6, 7, and 8 can be explained by the shifts into testing sector employment also documented in those tables. The testing sector has larger firms and includes manufacturing and transportation industries. All three are associated with well-known wage premia. To assess the role of increased testing sector employment in raising black wages, I added the three testing sector measures to the wage equations in Tables 5, 6, and 7. The addition of these controls greatly reduced the coefficients on *pro-testing* × *black* in Tables 5 and 6. The coefficients were not statistically significant, and I could no longer reject equality of the coefficients for blacks and women (in Table 5) and blacks in pro-testing states versus antitesting states (in Tables 6 and 7). I conclude that wage increases for blacks overall are largely explained by shifts into testing sector employment.

In Table 8, I add interactions for metropolitan area drug testing exposure to the specifications in Table 7. Because larger firms and firms in industries where testing is more common are more likely to test, and because the representation of such firms differs across metropolitan areas, I expect that the impacts of state drug testing laws may differ across metro areas within a state depending on their industry and firm size structures. As described above, I develop a simple index of testing exposure at the metropolitan area level based on data from 1997–1999. At the state level, for which I have data for a longer time period, industry and firm size composition are highly stable over time. I therefore assume that MSA-level firm size and industry structure is constant and exogenous to state drug testing laws. I treat MSA-level drug testing exposure as a fixed characteristic that may alter the impact of state level drug testing laws.

I also restrict the sample to early adopting states and to observations within three years of a state's adoption of drug testing legislation. I make these restrictions for several reasons. Most importantly, the problem of workers selecting into markets based on testing is likely more severe at the metropolitan area level than at the state level. It is much easier for workers to move between MSAs than across state and regional boundaries. This is the main motivation for imposing the three year restriction. This kind of arbitrage is more likely the more time has passed since the law change. Also, changes in MSA coding after 1999 make matching the industry and firm size composition data to the CPS microdata more challenging, although not impossible.[33] This is the reason for restricting to 1999 and earlier. It is also worth noting that it is not clear we should expect MSA-level differences in industrial composition to fully explain the impacts of state-level drug testing laws across residents of different states. In other words, state drug testing policies may still have significant impacts even if MSA-level differences in industrial composition are found to contribute significantly to these impacts.

The results are shown in Table 8. Coefficients on the interactions of MSA-level drug testing exposure with exclusive demographic groups are reported in the bottom panel. The results are in the second column are striking. These show that employment in high-testing industries increased substantially more in high-testing exposure MSAs for all black groups. The coefficients indicate the impact of moving up one standard deviation in the MSA drug testing exposure index for the indicated demographic group in a pro-testing state. This is a large change in testing exposure, but the estimated changes are also large, in the range of 4.3–5.4 percentage

[33] I have experimented with using the data for 2000–2006 in this exercise. The results are largely similar to those reported but often have larger standard errors, consistent with an increase in measurement error when matching the microdata from 2000–2006 to the metropolitan area characteristics based on older MSA codes.

points. Consulting Table 3 again, these impacts for high-testing industry employment represent an increase of 13 percent or more over the mean. The pattern is less consistent for the other two measures of testing sector employment, but large firm employment and benefits coverage still show relative increases for several black groups in MSAs with higher testing exposure.

Consistent with the idea that the impact of state drug testing laws might not operate exclusively through the local composition of firm size and industry, the state level impacts in the top panel are still statistically significant for some combinations of demographic groups and outcomes. In particular, low-skilled black men are more likely to have benefits coverage in states with a pro-testing law. This does not differ across high and low testing exposure MSAs (although there is a significant boost to high skilled black men in these outcomes in MSAs with high-testing exposure).

Robustness Analysis

The potential for unobserved factors to drive policy impacts in a study of this design is always a concern. A simple way to test for the importance of these is to use a placebo dataset—in which policy changes are randomly assigned—to reestimate the main empirical models. For brevity, I focus on the specification in Column 2 of Table 7, which shows how high-testing industry employment was affected in states passing either a pro-testing law or an antitesting law. I created a placebo dataset in which states were randomly assigned law changes that match the true distribution of law changes over time and in pro-/antitesting character. For example, three states passed pro-testing laws and one passed antitesting legislation in 1999. In the placebo data, three states (from those not previously assigned in the round) will be randomly assigned a pro-testing law change and one an antitesting law change in 1999.

I drew 1,000 such sets of “placebo laws” and estimated the column 2, Table 7 specification on all of them. The results are plotted as a histogram in Figure 3. The x-axis shows the difference between the pro- and antitesting interactions with black from the estimation and therefore gives the estimated pro- minus antitesting state difference in high-testing industry employment for blacks. In other words, Figure 3 plots the effect size calculated in the bottom rows of Table 7 for each draw of the data. The placebo estimates center around zero, and the true estimate, indicated with a vertical line, is in the far right tail of the distribution. Statistical precision is high enough to distinguish the placebo estimates from the true estimate of 0.105 a great majority of the time. F-tests reject the equality of the placebo true estimates 90 percent of the time at the 5 percent level and 93.5 percent of the time at the 10 percent level. I therefore conclude that there is a strong basis for attributing causality to the policy changes in the main results. Note that the true law distribution will occasionally be drawn randomly, so it is not inconsistent with this conclusion to have some placebo estimates that are very similar to the true estimate, as happens in Figure 3.

I also examined the robustness of the main reported results to alternative control variable specifications. As discussed in Section IV, the specification in Tables 5 and higher differs from the simpler and more common specification in Equation (4). I compare estimates obtained from the preferred specification—in Equation (6)—to those from (4) by incrementally changing the control terms in (4) to match those in (6). This allows me to examine the importance of my choice of control variable specifications.

The results are shown in Appendix Table A3; more detailed discussion is provided below the table. I conclude that the inclusion of group-specific nonlinear time controls is important for the relative results I obtain, but that the form in which these are included (as group-specific year effects or as cubic time trends) is not important. I further conclude that the point estimates I obtain for blacks are robust across a variety of specifications, although the relative magnitude of these estimates is somewhat sensitive to specification choice. Finally, Column 5 shows that the estimates are not sensitive to excluding the time-varying state-level controls, so I exclude them from the preferred specification in order to retain the years 2005–2010 in the analysis.

As a final check, I restrict the data to observations from 1990 and later. This has two advantages. First, it omits the major years of the crack epidemic and associated drug wars, which may have operated differentially over time and across states in a way that affected black employment patterns but is not fully captured by the controls. Second, it aligns the data period more closely with the years of prime law passage. The cost to this change is that prelaw trends may not be well estimated for many states because of a shortened period between the start of the data and law passage. Appendix Table A4 reports the results of this exercise. For conciseness, I report only the results for the main Table 7 estimates of interest. For the most part, results from the main analysis in Table 7 are robust to this change in the data period. Overall employment for low-skilled black men is still unaffected by state employer drug testing laws. Pro-testing laws increase the share of low-skilled black men in high-testing industries and large firms relative to the same group in antitesting states, and their relative wages also increase. The p-values are above conventional levels for the wage and high-testing industry employment outcomes, but for large firm employment the difference is still statistically significant. The only result that does not hold up to the change is the positive impact on pension or health coverage. In Table A4, the difference in coverage for low-skilled black men across the two groups of states is small, negative, and statistically insignificant.

CONCLUSIONS AND DISCUSSION

This paper examined the impact of the development of widespread employer drug testing on relative employment outcomes for African Americans. I modeled the introduction of drug testing as a signal to employers in a Roy model of employment sector selection. The model showed that the impact of testing on black outcomes depends in part on employer beliefs about drug use across racial groups prior to testing. I used microdata from the National Survey on Drug Use and Health and the March Current Population Surveys to examine the impact of drug testing's expansion on black outcomes over a 30-year period.

The analysis generated several findings. First, the probability of employment in the testing sector rose markedly for nonusers as testing expanded over time. In the early 1980s, self-reported nonusers were not significantly more likely than drug users to work in high-testing industries. By the late 1990s, they were 4–8 percentage points significantly more likely to do so in regions with medium to high levels of employer drug testing. This suggests that the expansion of testing allowed employers to more reliably choose nonusers from among potential workers. Moreover, this probability increased more for nonusing blacks than for nonusing whites in regions where testing became most common. Third, employment of blacks increased at testing sector firms following the adoption of pro-testing statutes at the state level. Estimates of the increase are particularly large for low-skilled black men. Impacts for this group are economically large and equate to increases in testing sector employment of 7–10 percent for low-skilled black men in pro-testing states relative to all other states or 30 percent relative to all antitesting states. Low-skilled black men also experienced significant wage increases—of about 4 percent relative to all other states and 12 percent relative to antitesting states—following the adoption of pro-testing laws. This wage increase can be explained by increased employment in the testing sector, which has larger firms in industries with well-known wage premia. Finally, I find some evidence that employers substitute white women for blacks in the absence of drug screening.

I conclude that these results are consistent with discrimination against blacks by firms in the testing sector prior to the advent of drug testing. Because the information available via drug testing clearly impacted black hiring, the results are inconsistent with a taste-based model of discrimination. In such models, racial animus is a fixed characteristic of market participants and cannot be influenced by information. This suggests that the ex ante bias arose either because employers had information about black drug use that was correct on average but imprecise relative to that for whites, or because they held beliefs about black drug use that were inaccurate relative to their beliefs about whites, on average.

It is tempting to side with the first of these—ex ante statistical discrimination—and rule out inaccurate beliefs as unlikely to persist in equilibrium.[34] Nevertheless, three facts lead me to be more cautious. First, drug use rates rose over the 1990s for all groups, including blacks. If drug testing allowed employers to improve the precision of their employment screening for blacks relative to whites, then the costs of drug use would have increased for blacks relative to whites. This does not rule out the possibility that black drug use increased in the posttesting period, but if improved precision (reduced statistical discrimination) were important, it seems unlikely that black drug use would rise one-for-one with white drug use as the data show. Second, blacks were more likely than whites to be employed in the testing sector prior to the rise in testing. This casts some doubt on the statistical discrimination assumption that employers systematically had poor information about blacks relative to whites. Ultimately, more work is

[34] This would also be consistent with evidence of widespread statistical discrimination against blacks documented in Fryer, Pager, and Spenkuch(2011).

needed to separately identify discrimination arising from behavioral factors like racialized beliefs versus that arising from informational disparities.

An ancillary lesson for labor economists is that employers care about drug use, drug test failure, or characteristics that drug test failure proxies (or all three). This research shows that the ability to screen their workforces for drug use provided employers with additional information beyond other observable characteristics. They clearly put this information to use in their hiring and retention decisions. This is consistent with other research indicating the importance of noncognitive skills for employment outcomes.

For policymakers, this research shows that—contrary to what many might expect—drug testing by employers has *helped* African Americans make inroads into testing industries since the late 1980s. This research suggests that testing improved blacks' access to jobs in large firms, with better benefits and higher wages. It is therefore possible that drug testing is in part responsible for the fact that blacks did not fare as badly as might have been expected in the decades of rapidly rising inequality (Card and Dinardo 2002). Interestingly, Fendrich and Kim (2002) documented changes in worker attitudes toward testing that are consistent with the effects reported here. These authors collated public opinion poll data on drug testing from over 20 polls spanning 1985–1999. They found that public approval of employer drug testing has risen over time. However, this is driven by blacks, those with less than a high school education, and younger workers. Over the same period, approval declined among more educated and older workers. This suggests that these groups are aware of the benefits that testing has provided them.

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Table 1 Share of Establishments with a Drug Testing Program

<table>
<tr><th></th><th>1988</th><th>1993</th><th>1997–2006</th></tr>
<tr><td>Total</td><td>3.2</td><td>48.4</td><td>46.3</td></tr>
<tr><td>By establishment size</td><td></td><td></td><td></td></tr>
<tr><td>1–9</td><td>0.8</td><td>—</td><td>21.3</td></tr>
<tr><td>10–49</td><td>6.4</td><td>—</td><td>38.4[a]</td></tr>
<tr><td>50–99</td><td>12.4</td><td>40.2</td><td>49.3[b]</td></tr>
<tr><td>100–249</td><td>17.2</td><td>48.2</td><td rowspan="2">66.3</td></tr>
<tr><td>250–499</td><td>29.7</td><td rowspan="2">61.4</td></tr>
<tr><td>500–999</td><td>30.6</td><td rowspan="3">74.8</td></tr>
<tr><td>1000–4999</td><td>41.8</td><td rowspan="2">70.9</td></tr>
<tr><td>5000+</td><td>59.8</td></tr>
<tr><td>By industry</td><td></td><td></td><td></td></tr>
<tr><td>Mining</td><td>21.6</td><td rowspan="2">69.6</td><td>86.0</td></tr>
<tr><td>Construction</td><td>2.3</td><td>43.5</td></tr>
<tr><td>Durable Mfg.</td><td>9.9</td><td rowspan="2">60.2</td><td rowspan="2">68.6</td></tr>
<tr><td>Nondurable Mfg.</td><td>9.1</td></tr>
<tr><td>Transportation</td><td>14.9</td><td rowspan="2">72.4</td><td rowspan="2">72.4</td></tr>
<tr><td>Communic.,Utilities</td><td>17.6</td></tr>
<tr><td>Wholesale trade</td><td>5.3</td><td rowspan="2">53.7</td><td>60.1</td></tr>
<tr><td>Retail trade</td><td>0.7</td><td>42.5</td></tr>
<tr><td>FIRE</td><td>3.2</td><td>22.6</td><td>39.7</td></tr>
<tr><td>Services</td><td>1.4</td><td>27.9</td><td>36.3</td></tr>
<tr><td>Agriculture</td><td>—</td><td>—</td><td>22.3</td></tr>
<tr><td>Government</td><td>—</td><td>—</td><td>61.2</td></tr>
<tr><td>By region</td><td></td><td></td><td></td></tr>
<tr><td>Northeast</td><td>1.9</td><td>33.3</td><td>—</td></tr>
<tr><td>Midwest</td><td>3.8</td><td>50.3</td><td>—</td></tr>
<tr><td>South</td><td>3.9</td><td>56.3</td><td>—</td></tr>
<tr><td>West</td><td>2.8</td><td>46.8</td><td>—</td></tr>
</table>

NOTE: Numbers in both columns refer to the share of establishments with any kind of drug testing. Note that because the 1993 sample excludes establishments with fewer than 50 employees, some of the increase in total and industry level testing shares is due to dropping a part of the sample where testing is less prevalent. Data for 1997–2006 are average shares of 22–49-year-old employees in the NSDUH reporting that their employer conducts some form of drug testing.

[a]This number is for establishments with 10–24 employees.

[b]This number is for establishments with 25–99 employees.

SOURCE: Data for 1989 are from U.S. Department of Labor (1989), Tables 1 and 2. Data for 1993 are from Hartwell et al. (1996) Table 1.

Table 2 Changes in Reported Employer Drug Testing in the NSDUH, 2002–2003 to 2007–2009

	Levels			Percentage Point Changes		
	Any form of drug testing	**Drug testing at hiring**	**Random drug testing**	**Any form of drug testing**	**Drug testing at hiring**	**Random drug testing**
States enacting statutes in 2003 or later						
Louisiana—Pro-testing law in 2003						
2002–03	56.5	50.3	42.7			
2007–09	59.9	52.6	46.3	3.4	2.3	3.6
Rhode Island, Vermont, and Montana—Antitesting laws in 2003, 2003 & 2005, respectively						
2002–03	24.4	18.9	14.0			
2007–09	29.1	22.4	18.2	4.7	3.5	4.2
Comparison groups with no statutes enacted in 2003 or later						
A. Low testing in 2002–03 (any testing share <= 30), no statute passed						
2002–03	27.6	23.5	13.1			
2007–09	32.2	26.0	16.3	4.6	2.5	3.2
B. High testing in 2002–03 (any testing share >= 55), no statue passed						
2002–03	55.4	48.0	32.5			
2007–09	55.0	49.4	34.1	-0.4	1.4	1.6
C. Low testing in 2002–03 (any testing share <= 30) with an antitesting statute						
2002–03	25.6	20.4	17.2			
2007–09	28.7	22.2	17.1	3.1	1.8	-0.1
D. High testing in 2002-03 (any testing share >= 55) with a pro-testing statute						
2002-03	56.9	50.7	42.8			
2007-09	61.7	55.0	45.6	4.8	4.3	2.8

NOTE: Cells are cross-state averages of employed respondents answering affirmatively to a question on whether their employer conducts the listed form of testing in the 2002–2003 and 2007–2009 waves of the NSDUH. All averages weighted by 1000s of U.S. population represented by respondents to "Any drug testing" question in 2002–2003. Pro-, anti- and unclassified states are defined in Appendix Table A1.

Table 3 Descriptive Statistics for the March CPS Sample, 1980–2010

	Overall mean	All states, 1980-1988	Pro-testing states, 1980–1988	Antitesting States, 1980–1988
Age	35.7	34.2	34.2	34.1
Employed	0.75	0.72	0.72	0.75
High-testing industry	0.28	0.33	0.32	0.32
Employed in large firm (>500)	0.44	0.42	0.43	0.36
Real hourly wage ($2,000)	14.8	12.6	12.08	11.75
Log real hourly wage	2.45	2.36	2.31	2.31
In wage sample	0.73	0.72	0.71	0.74
Covered by group health	0.53	0.59	0.58	0.57
Covered by pension	0.52	0.50	0.48	0.48
Female	0.52	0.52	0.52	0.51
Black	0.10	0.09	0.13	0.03
Hispanic	0.13	0.10	0.06	0.02
Any postsecondary	0.49	0.39	0.36	0.39
Young (ages 18–25)	0.21	0.26	0.26	0.25
Pro-testing dummy	0.10	0.01	0.02	0.00
Antitesting dummy	0.04	0.00	0.00	0.00
Black subsample				
Employed	0.67	0.63	0.62	0.67
High-testing industry	0.31	0.37	0.36	0.41
Employed in large firm (>500)	0.56	0.55	0.52	0.57
Covered by group health	0.54	0.59	0.55	0.63
Covered by pension	0.54	0.52	0.45	0.55
Log real hourly wage	2.32	2.23	2.09	2.27
White subsample				
Employed	0.77	0.75	0.73	0.75
High-testing industry	0.27	0.32	0.32	0.32
Employed in large firm (>500)	0.43	0.42	0.43	0.36
Covered by group health	0.55	0.59	0.58	0.57
Covered by pension	0.55	0.50	0.48	0.48
Log real hourly wage	2.50	2.39	2.35	2.31

NOTE: Sample is restricted to those aged 18–55. Estimates are unweighted. "High-testing industry" is defined conditional on employment and is equal to one if an individual is employed in mining, transportation, communications and utilities, government or wholesale trade. One state, South Carolina, first adopted pro–drug testing legislation in 1985.
SOURCE: Data are from the IPUMS version of the annual March CPS surveys.

Table 4 Nonuser/User Difference in High-Testing Industry Employment Rates (adjusted) by Time Period and Census Division Testing Intensity

i. Whole sample

Time period	**Pre-testing 1985–1988**	**Transition 1989–1993**	**Posttesting 1994–1997**
Lowest	0.021 (0.026)	0.061 (0.012)	0.018 (0.017)
Intermediate	0.017 (0.031)	0.041 (0.010)	0.075 (0.016)
Highest	0.038 (0.026)	0.047 (0.014)	0.043 (0.020)

ii. Blacks only

Time period	**Pretesting 1985–1988**	**Transition 1989–1993**	**Posttesting 1994–1997**
Lowest	0.007 (0.061)	0.031 (0.029)	-0.020 (0.041)
Intermediate	0.078 (0.062)	0.023 (0.024)	0.101 (0.041)
Highest	0.039 (0.059)	0.032 (0.030)	0.075 (0.039)

iii. Whites only

Time period	**Pretesting 1985–1988**	**Transition 1989–1993**	**Posttesting 1994–1997**
Lowest	0.018 (0.033)	0.070 (0.015)	0.007 (0.022)
Intermediate	-0.008 (0.040)	0.051 (0.013)	0.068 (0.019)
Highest	0.047 (0.033)	0.047 (0.019)	0.030 (0.026)

NOTE: Cells show difference between mean adjusted high-testing industry employment for (monthly) nonusers and monthly users. Standard errors of the difference in parentheses. High-testing industry employment is regression adjusted using controls for demographics (age, race, Hispanic ethnicity, sex, and educational attainment), demographic-specific cubic time trends and group-specific region fixed effects, and all relevant main effects. Lowest testing divisions are New England, the mid-Atlantic, and Pacific. Intermediate testing regions are the West North Central, South Atlantic, and Mountain. Highest testing regions are the East and West South Central and East North Central.
SOURCE: Data from National Survey on Drug Use and Health, 1985–1997. Census division testing intensity tabulated from Appendix Table A4.

Table 5 Impacts of Pro-Testing Legislation by Demographic Group

Dependent variable:	Employed	Employed in hgh-test ind.	Employed in large firm	Covered by health or pension	Log real hourly wage
Black × Pro	0.00	0.016	0.02*	0.03***	0.014
	(0.006)	(0.011)	(0.01)	(0.008)	(0.008)
Hispanic × Pro	-0.007	-0.008	-0.003	-0.001	-0.02
	(0.008)	(0.01)	(0.023)	(0.027)	(0.012)
Female × Pro	0.001	-0.016	-0.014**	-0.012	-0.009
	(0.008)	(0.008)	(0.005)	(0.007)	(0.009)
Age 18–25 × Pro	0.002	-0.01	-0.003	0.009	-0.004
	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)
Low Skill × Pro	0.00	0.008	0.01	0.013*	0.013*
	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)
Pro-Testing Law	0.009	-0.005	0.006	0.001	0.026**
	(0.007)	(0.009)	(0.008)	(0.01)	(0.009)
Black	-0.156***	0.046***	0.117*	0.005	-0.108***
	(0.005)	(0.009)	(0.052)	(0.007)	(0.008)
Hispanic	-0.066***	0.12***	-0.16***	0.014	-0.082***
	(0.006)	(0.005)	(0.041)	(0.01)	(0.014)
Female	-0.225***	-0.183***	-0.017	-0.143***	-0.393***
	(0.006)	(0.005)	(0.019)	(0.004)	(0.005)
Low Skill	-0.087***	0.137***	-0.113***	-0.056***	-0.183***
	(0.003)	(0.004)	(0.027)	(0.003)	(0.005)
Age	0.04***	0.021***	-0.004***	0.039***	0.069***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age^2	-0.001***	0.00***	0.00***	0.00***	-0.001***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Minimum wage	0.004**	0.008***	0.004*	0.004	-0.007
	(0.001)	(0.002)	(0.002)	(0.002)	(0.005)
Unemployment rate	-0.012***	-0.002*	-0.002	-0.004***	-0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Incarceration rate	-0.906	1.442	1.92	-0.922	-8.119
	(1.096)	(0.968)	(2.383)	(1.109)	(4.365)
Industry level annual employment growth	Yes	Yes	Yes	Yes	Yes
N	2,723,128	2,046,460	1,703,280	2,256,956	1,994,803
Effect Size: Black - Female	-0.001	0.03	0.03	0.04	0.02
H_0: Blacks = Female (p-value)	0.99	0.05	0.01	0.002	0.11

NOTE: Sample is individuals ages 18–55. Firm size only available from 1988 onwards. Wage equation is further restricted to those with positive earnings within the 3rd and 97th percentiles of the real wage distribution in the overall sample. Specifications are estimated via OLS. All include a cubic time trend, interactions of the cubic time trend components with all demographic variables, a full set of state × demographic group dummy variables, and a full set of state × cubic time trends. Standard errors clustered on state in parentheses. *** indicates significance at the 0.1%, ** 1%, and * 5% levels.
SOURCE: Data are from March CPS 1980–2010, IPUMS version, and additional sources as described in text.

Table 6 Impacts of Pro- and Antitesting Legislation by Demographic Group

Dependent variable:	Employed	Employed in high-test ind.	Employed in large firm	Covered by health or pension	Log real hourly wage
Black × Pro	0.001	0.014	0.02*	0.029***	0.013
	(0.006)	(0.011)	(0.01)	(0.008)	(0.008)
Hispanic × Pro	-0.007	-0.009	-0.004	-0.002	-0.021
	(0.008)	(0.01)	(0.023)	(0.027)	(0.012)
Female × Pro	0.001	-0.015	-0.014**	-0.012	-0.009
	(0.008)	(0.008)	(0.005)	(0.007)	(0.009)
Young × Pro	0.003	-0.011	-0.002	0.008	-0.004
	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)
Low Skill x Pro	0.00	0.009	0.01	0.014*	0.013*
	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)
Black × Anti	0.003	-0.048*	-0.015	-0.023	-0.039
	(0.018)	(0.023)	(0.01)	(0.022)	(0.024)
Hispanic × Anti	0.002	-0.037	-0.02	-0.056	-0.043
	(0.015)	(0.031)	(0.015)	(0.029)	(0.048)
Female × Anti	0.004	0.008	0.012	0.008	0.011*
	(0.011)	(0.009)	(0.007)	(0.008)	(0.005)
Young × Anti	0.011	-0.011	0.004	-0.014*	-0.003
	(0.011)	(0.006)	(0.017)	(0.005)	(0.011)
Low Skill × Anti	-0.014	0.015	0.005	0.019**	-0.001
	(0.008)	(0.012)	(0.007)	(0.006)	(0.011)
Pro-Testing Law	0.009	-0.005	0.005	0.00	0.026**
	(0.007)	(0.009)	(0.008)	(0.01)	(0.009)
Antitesting Law	0.015**	0.00	0.002	-0.017	-0.014
	(0.005)	(0.006)	(0.013)	(0.011)	(0.026)
N	2,723,128	2,046,460	1,703,280	2,256,956	1,994,803
Effect Size: Black × Pro – Black × Anti	-0.002	0.06	0.04	0.05	0.05
H_0: Black ×x Pro = Black × Anti (p-val)	0.89	0.01	0.009	0.03	0.04
Effect Size: Female × Pro – Female × Anti	-0.003	-0.02	-0.03	-0.02	-0.02
H_0: Female × Pro= Female × Anti (p-val)	0.81	0.06	0.003	0.05	0.04

NOTE: Sample is individuals aged 18–55. Firm size only available from 1988 onward. Wage equation is further restricted to those with positive earnings within the 3rd and 97th percentiles of the real wage distribution in the overall sample. Specifications are estimated via OLS. All include all additional controls listed in Table 5, all relevant main effects, a cubic time trend, interactions of the cubic time trend components with all demographic variables, a full set of state × demographic group dummy variables, and a full set of state × cubic time trends. Standard errors clustered on state in parentheses. *** indicates significance at the 0.1%, ** 1%, and * 5% levels.
SOURCE: Data are from March CPS 1980–2010, IPUMS version and as noted in Table 5.

Table 7 Impacts by Exclusive Demographic Groups

Dependent variable:	Employed	Employed in high-test ind.	Employed in large firm	Covered by health or pension	Log real hourly wage
Pro-testing × … *(HS White Men are omitted)*					
LS Black Men	-0.006	0.038	0.045***	0.042***	0.033*
	(0.014)	(0.021)	(0.012)	(0.011)	(0.016)
HS Black Men	-0.004	-0.008	0.016	0.025	0.007
	(0.011)	(0.022)	(0.016)	(0.013)	(0.016)
LS Black Women	0.015	0.01	0.008	0.043***	0.014
	(0.015)	(0.02)	(0.013)	(0.01)	(0.013)
HS Black Women	-0.018	0.005	-0.007	-0.001	-0.008
	(0.012)	(0.015)	(0.011)	(0.011)	(0.014)
LS White Men	0.002	0.016	0.007	0.014*	0.015*
	(0.007)	(0.009)	(0.005)	(0.006)	(0.007)
LS White Women	-0.01	-0.008	-0.003	-0.004	-0.001
	(0.011)	(0.01)	(0.008)	(0.011)	(0.008)
HS White Women	-0.001	-0.009	-0.012*	-0.006	-0.005
	(0.007)	(0.008)	(0.006)	(0.006)	(0.007)
Anti-testing × … *(HS White Men are omitted)*					
LS Black Men	-0.027	-0.067	-0.082	-0.049	-0.099
	(0.039)	(0.041)	(0.047)	(0.049)	(0.065)
HS Black Men	-0.02	0.003	0.021	0.049	-0.013
	(0.014)	(0.056)	(0.06)	(0.037)	(0.019)
LS Black Women	0.016	-0.049	0.045*	-0.024	-0.03
	(0.021)	(0.042)	(0.018)	(0.037)	(0.034)
HS Black Women	0.023	-0.033	-0.021	-0.012	-0.015
	(0.016)	(0.047)	(0.047)	(0.022)	(0.015)
LS White Men	0.003	0.011	0.007	0.016	-0.01
	(0.005)	(0.01)	(0.017)	(0.014)	(0.018)
LS White Women	-0.012	0.026	0.017	0.032*	0.001
	(0.007)	(0.015)	(0.012)	(0.015)	(0.013)
HS White Women	0.009	-0.002	0.007	0.006	-0.007
	(0.009)	(0.016)	(0.01)	(0.014)	(0.012)
N	2,355,785	1,792,491	1,471,265	1,976,076	1,738,844
Effect Size: LSBM × Pro – LSBM × Anti	0.02	0.11	0.13	0.09	0.13
H_0: LSBM × Pro = LSBM × Anti (p-value)	0.59	0.02	0.01	0.07	0.05

NOTE: Sample is individuals ages 18–55. Hispanics excluded; other races defined as white. HS indicates High Skill (some postsecondary), LS Low Skill (no postsecondary). Estimation methods are the same as in Table 5. All specifications include controls for age, age^2, state-year characteristics in Table 5, a cubic time trend plus its interactions with the listed (exclusive) demographic groups, state × demographic group interactions, state-specific cubic time trends, and all relevant main effects. Standard errors clustered on state in parentheses.
*** indicates significance at the 0.1%, ** 1%, and * 5% levels.
SOURCE: Data are from March CPS 1980–2010, IPUMS version. Additional data sources described in text.

Table 8 Model with Interactions for Metro Area Drug Testing Exposure

Dependent variable:	Employed	Employed in high-test ind.	Employed in large firm	Covered by health or pension	Log real hourly wage
Pro-testing × … *(HS White Men are omitted)*					
LS Black Men	0.022	-0.011	0.008	0.061*	0.07*
	(0.025)	(0.026)	(0.018)	(0.025)	(0.033)
HS Black Men	0.019	-0.022	0.083***	-0.014	0.036
	(0.012)	(0.025)	(0.016)	(0.019)	(0.027)
LS Black Women	0.006	0.045**	-0.076**	-0.044	0.045*
	(0.014)	(0.015)	(0.025)	(0.027)	(0.021)
HS Black Women	-0.03	0.007	-0.034	0.033	0.062
	(0.019)	(0.019)	(0.018)	(0.016)	(0.045)
LS White Men	0.011*	0.026	-0.016	-0.002	0.014
	(0.005)	(0.018)	(0.012)	(0.01)	(0.015)
LS White Women	-0.017	0.01	-0.009	-0.01	-0.023
	(0.011)	(0.011)	(0.012)	(0.022)	(0.027)
HS White Women	-0.019*	-0.005	-0.008	-0.013**	-0.011
	(0.008)	(0.018)	(0.01)	(0.004)	(0.019)
Metro area drug testing exposure × pro-testing × … *(HS White Men are omitted)*					
LS Black Men	0.013	0.054**	0.01	0.005	-0.016
	(0.016)	(0.016)	(0.008)	(0.012)	(0.021)
HS Black Men	0.015**	0.043*	-0.014***	0.042**	-0.026
	(0.004)	(0.018)	(0.004)	(0.014)	(0.023)
LS Black Women	0.001	0.047***	0.058***	0.046	-0.032**
	(0.007)	(0.01)	(0.013)	(0.025)	(0.011)
HS Black Women	0.001	0.045***	0.037***	-0.002	-0.049
	(0.01)	(0.01)	(0.008)	(0.007)	(0.028)
LS White Men	0.003	0.011	0.03***	0.003	-0.009
	(0.004)	(0.012)	(0.005)	(0.007)	(0.012)
LS White Women	0.025*	0.029**	0.014*	-0.012	-0.027
	(0.01)	(0.009)	(0.006)	(0.015)	(0.021)
HS White Women	0.012	0.017	0.013	-0.015***	-0.001
	(0.007)	(0.015)	(0.009)	(0.002)	(0.016)
Observations	831,483	638,829	491,329	702,404	628,439

NOTE: Specifications include "anti' and all anti interactions, but these are not reported. Sample and data are the same as in Tables 5–7 but observations are limited to three years or less after law adoption and to years 1980–1999. Employment in large firm further restricted to 1988–1999. Standard errors clustered on state in parentheses.
*** indicates significance at the 0.1%, ** 1%, and * 5% levels.

FIGURE 1 Share of Employed Respondents Working in a High-Testing Industry, by race. Data from the CPS ASEC Supplement (March) 1980–2010

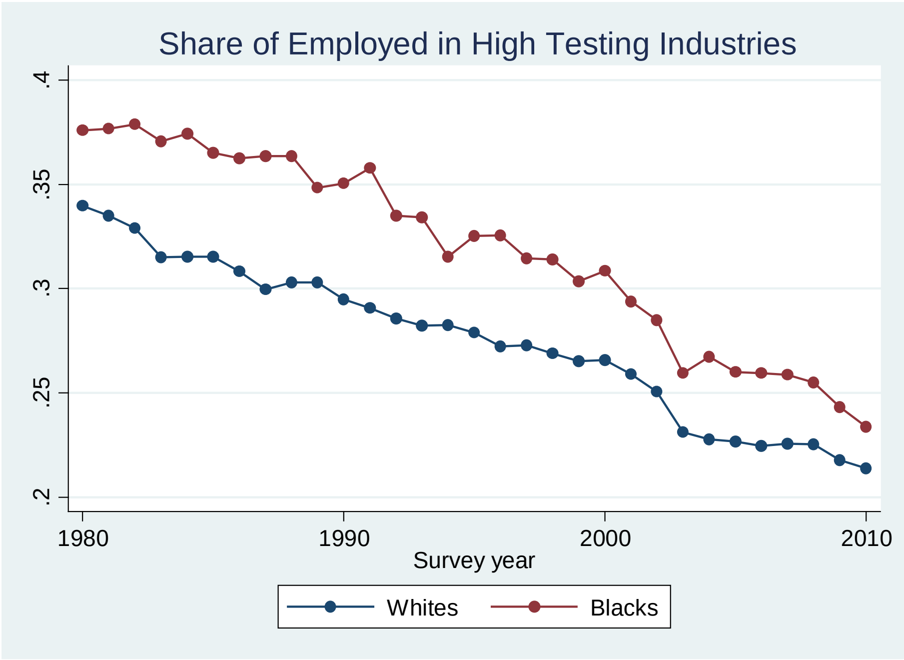
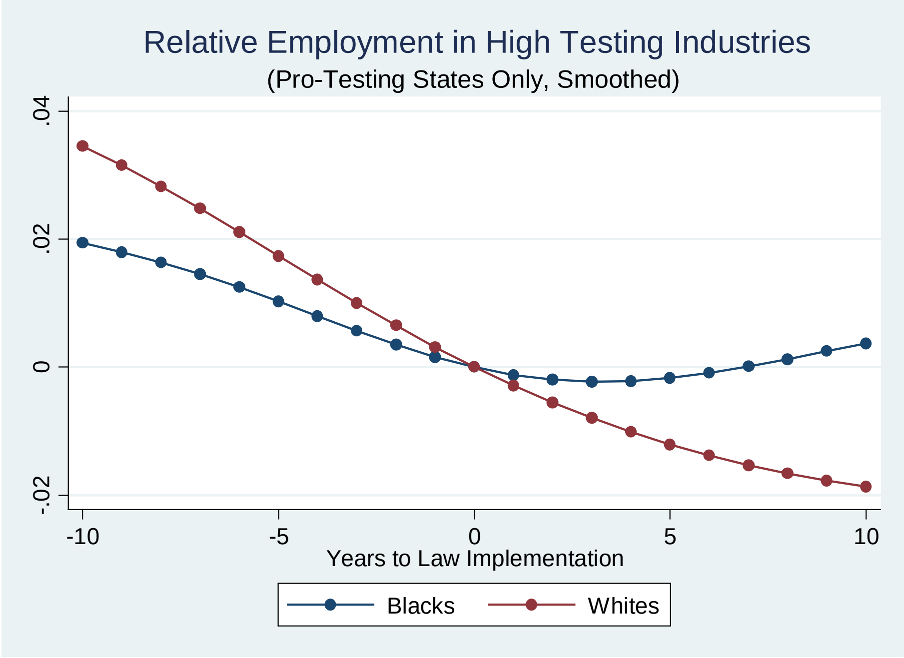
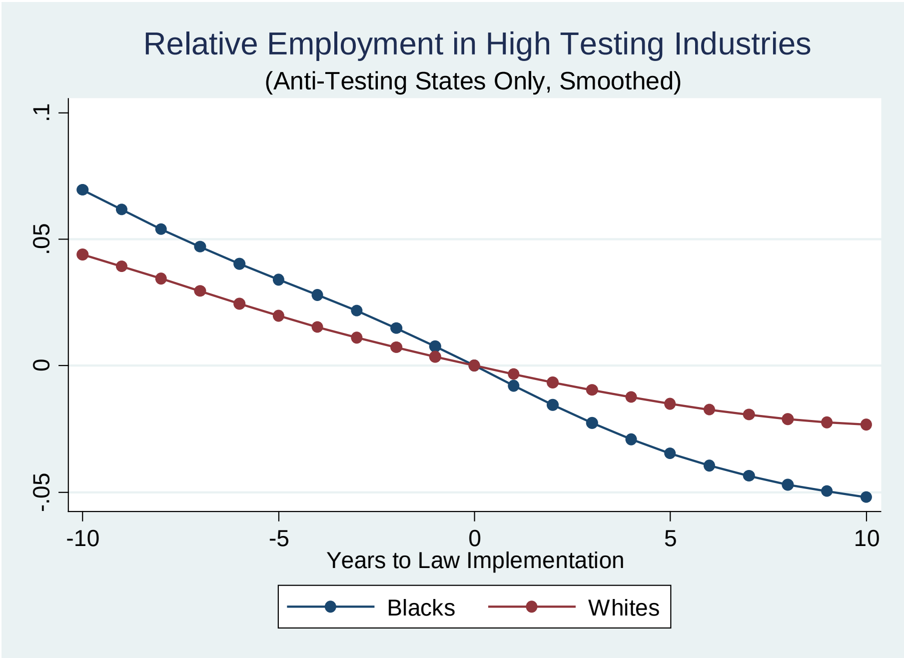


FIGURE 2a Share of Employed Respondents Working in a High-Testing Industry Relative to Year in Which a Pro-Testing Law Was Passed, by Race



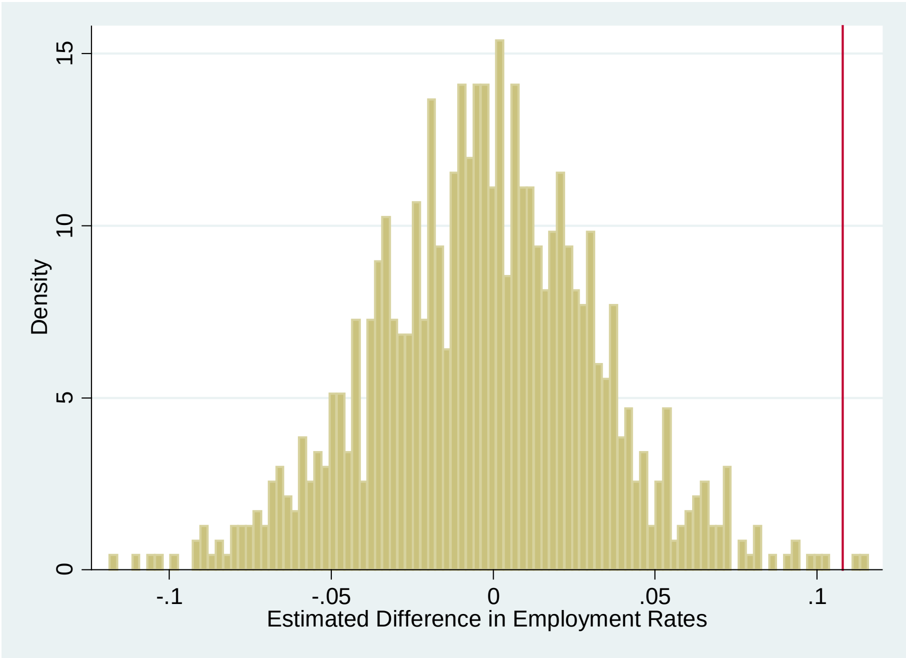
NOTE: Respondents from states adopting a pro-testing law only. Y-axis is difference between share of employed in high-testing industries in x-axis year and in year of passage.
SOURCE: Data from the March CPS 1980–2010.

FIGURE 2b Share of Employed Respondents Working in a High-Testing Industry Relative to Year in Which an Antitesting Law Was Passed, by Race



NOTE: Respondents from states adopting a pro-testing law only. Y-axis is difference between share of employed in high-testing industries in x-axis year and in year of passage.
SOURCE: Data from the March CPS 1980–2010.

FIGURE 3 Placebo Analysis



NOTE: Estimated difference in testing sector employment for low skilled black men from regressions using placebo laws. The estimated difference in the figure corresponds to the effect size in column 2 of Table 8. The figure plots differences estimated from each of 1,000 draws of a law change distribution in which states are randomly assigned to “pass” laws that match the actual law change distribution in terms of years of passage, numbers of states passing in a given year, and pro-/antitesting character of the legislation.

APPENDICES – NOT FOR PUBLICATION

APPENDIX Table A1: Employer Drug Testing Regulations and Prevalence by State

	Summary of state drug testing regulations			Share NSDUH respondents reporting testing by their employers in 2002–03		
DeBernardo and Nieman Classification (2007)	**Earliest statute**	**Legal protection/ WC benefit to employer**	**Additional mandatory, restricted, or prohibited testing**	**Any form of drug testing**	**Drug testing as part of hiring**	**Random drug testing**
U.S.				**44.6**	**38.9**	**26.4**
Pro-testing states						
Alabama	1996	Y	M	58.1	51.5	44.3
Alaska	2002	Y		44.9	34.2	31.4
Arizona	1995	Y		52.6	46.5	32.9
Arkansas	2002	Y	M	53.9	48.5	35.1
Florida	2000	Y	M	51.9	45.6	32
Georgia	1998	Y	M	50.1	43	31.4
Idaho	1999	Y		42.9	31.7	27.6
Iowa	1996	Y	P	39	33.9	23.4
Louisiana	2003	Y		56.5	50.3	42.7
Mississippi	1999	Y	R	55.5	49.8	41.2
Ohio	2001	Y	M	50.8	44.8	29.2
South Carolina	1985	Y	M	50.6	46.2	34.4
Tennessee	1999	Y	M	52.7	45.5	32.7
Utah	2001	Y		49.2	38.3	30.8
Antitesting states						
Connecticut	1996		P	42.6	38.9	22.6
Maine	2001		R	25.6	20.4	17.2
Minnesota	1993		R	39.8	33.4	19.2
Montana	2005		R	26.2	20	18.6
Oklahoma	1999		R	46.5	37.8	35.3
Rhode Island	2003		P	29.6	25.2	14.1
Vermont	2003		P	17.3	11.4	9
Unclassified states						
Omitted for space. Available upon request.						
Cross-state averages, unweighted						
All pro-testing				50.6	43.6	33.5
All antitesting				32.5	26.7	19.4
All unclassified				42.6	36.7	25.0

NOTE: M=some testing mandatory; R=some testing restricted but none prohibited; P=some testing prohibited. Cells are state averages of employed respondents answering affirmatively to a question on whether their employer conducts the listed form of testing in the 2002 and 2003 waves of the NSDUH. Respondents who indicate that their workplaces do not test for either alcohol or drug use are legitimately routed out of the questions pertaining to testing during the hiring process or random testing. For this analysis, these respondents are classified as being employed by workplaces that do not implement these practices.
SOURCE: Source for columns 2–4: DeBernardo and Nieman (2006). Source for columns 5–7: SAMHSA, Office of Applied Studies, National Survey on Drug Use and Health, 2002 and 2003. Tabulated at special request by the author.

Note on DeBernardo and Nieman classification of state policies:

A state is considered to be pro–drug testing if an employer that implements drug testing procedures in that state either receives a discount on workers' compensation premiums or receives legal protection. For example, the state of Alabama provides a 5 percent discount on workers' compensation premiums to employers which implement a drug testing program. Under this program, if an employee has caused or contributed to an on-the-job injury, then a drug test is mandatory. An employee who tests positive will be ineligible to receive any workers' compensation. An example of legal protection is provided by the state of Mississippi. Mississippi passed legislation which absolves an employer which implemented a drug testing program from civil liability. An employee cannot bring a case for defamation, libel, or slander against an employer that complied with the drug testing legislation. The following 14 states are considered pro drug testing.

A state is considered to be anti–drug testing if the state restricts or prohibits drug testing in any of the following procedures: job applicant testing, random testing, reasonable suspicion / for-cause testing, periodic announced testing, and postaccident testing. For example, the state of Montana restricts job applicant testing to jobs in hazardous work environments, fiduciary or security positions, and positions that could affect public safety. Vermont and Rhode Island prohibit random drug testing and even periodic announced drug testing. Seven states are considered anti–drug testing states. Iowa and Mississippi are two states that are considered as pro–drug testing states even though they have restrictions on drug testing. In Iowa, random drug testing must be selected by an entity independent of the employer and by using a computer procedure. Postaccident drug testing is only permitted with a serious injury or property damage in excess of $1,000. In Mississippi, job applicants must be provided with a written notice of drug testing upon application and random testing is permitted on a "neutral selection" basis.

APPENDIX I: DRUG TESTING AND DRUG USE DETAILS

Drug testing differs from other forms of employer screening and monitoring in that it requires the collection and analysis of a physical specimen. In most cases, this involves the collection of a urine specimen by a third party within a specified time frame after receiving a job offer or testing notice.[35] The most common testing kits screen for 5–10 different types of drugs, including the active ingredients in prescription painkillers. Contrary to some popular claims, drug screening has a low rate of false positives—about 2 percent or less. A bigger concern for employers is the rate of false negatives in the screening phase. While it is true that an industry has evolved to help individuals pass drug tests, the main threats to test validity are high rates of false negatives that occur even in the absence of evasion efforts by tested individuals.[36] False negative rates average 20% over the five main drug classes but are highest for marijuana—over 40% (U.S. Department of Justice 1991). A large number of false negatives are also due to generous cutoff levels established by the National Institutes on Drug Abuse rather than to technological limitations in the screening methods (National Research Council 1994, Ch. 3). A final source of false negatives is lax oversight in testing facilities, which enables cheating when testing protocols are not followed (Government Accountability Office 2007).

Drug tests are characterized by high rates of false negatives as well as timing that is frequently predictable. Nevertheless, many individuals fail them. One of the only sources of public information on drug test failure rates is Quest Diagnostics, a medical testing company that is one of the nation's largest suppliers of drug test kits and urinalysis services. Quest publishes drug test positivity rates from its labs in their annual Drug Testing Index. The index makes several points, in spite of its nonrepresentativeness. First, the number of tests performed in the United States annually is very large. Quest reported conducting 8.4 million tests in 2007, and this only represents a share of all tests performed nationally. Second, the overall failure rate in Quest labs was 3.8% in 2007, with slightly higher rates among job applicants (as opposed to testing of current employees) and in jobs where testing was not federally mandated for safety reasons (Quest Diagnostics 2008). There is also considerable geographic variation in failure rates, with the worst-performing county groups in Quest data reporting failure rates in the range of 5.5–16% in 2007. There is evidence that drug test failure typically stems from regular use. DuPont et al. (1995) estimate that a majority of those testing positive in random workplace tests are daily users while only 7% are infrequent, annual users.

Measures of drug use are available back to 1979 in the NSDUH. For most of the survey's history, blacks and whites have reported drug use at nearly identical rates. Table A2 summarizes patterns of drug use in the U.S. population aged 18–55. Use rates for all groups were stable or declining over the 1990s but increasing since 2000. This is clear in the decade-level averages in Table 2. Despite these long-run trends, there are stable differences in drug use across demographic groups and drug classes. First, marijuana use is much more common than other

[35] 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.

[36] Most efforts to substitute a urine specimen or to supply one that has been adulterated in order to conceal drug use could be easily detected by monitors at the collection site. (Bush 2008; National Research Council 1994, Ch.6.)

drug use, but group differences in marijuana use tend to be mirrored in the use of other drugs. The biggest group difference in use rates is across genders: men use drugs at nearly double the rate for women. The other major group difference is across ages. Individuals aged 18–25 are more than twice as likely as those 26–55 to have used drugs in the past month. Racial and ethnic differences in use rates are not nearly as large, with blacks and whites using at roughly equal rates and Hispanics at somewhat lower rates, especially for marijuana. Finally, drug use is also more common among the less educated, in this case, those with no postsecondary education.[37]

I also create mutually exclusive groups based on multiple demographic characteristics. Employers likely see their applicants and employees as a collection of traits, such as "black male with a high school diploma," rather than evaluating their likelihood of drug use based on their characteristics separately. Accordingly, I group NSDUH respondents into gender/race (black or white)/skill cells. Hispanics are excluded, and I did not group on age since between the ages of 18 and 65, the age distribution is fairly similar across races. The bottom panel of Table A2 shows use rates for these eight groups. Consistent with the results in the top panel, I find that within gender and skill cells, blacks and whites use drugs at similar rates. If anything, blacks use at lower rates than whites in the same categories. The exception is marijuana use in the 1980s, but this is not apparent in the larger and more extensive samples of the 1990s and 2000s.[38]

[37] Although the NSDUH is the best available source on drug use in a nationally representative population, there is considerable evidence that drug use is underreported in survey data (Mensch and Kandel 1988; Fendrich and Kim 2002 survey the literature on underreporting in household surveys; Lu, Taylor, and Riley 2001 survey the literature on underreporting among institutionalized populations). Lu, Taylor, and Riley (2001) find underreporting rates centering on 50% in a sample of arrestees. They find that underreporting differs somewhat across drug classes, with hard drugs more underreported. Some studies have found disparate rates of underreporting across races but these go in both directions (Lu, Taylor, and Riley 2001).

[38] Prior to 1987, the NSDUH was conducted at intervals of several years and sampled a much smaller number of individuals than in later years. This is reflected in the total observations reported in the table notes.

Appendix Table A2: Drug Use Rates by Group and Decade

Past month drug use:	**Any**	**Marijuana**			**Other Drugs**	
	1990–2006 Average	**1980s**	**1990s**	**2000s**	**1990s**	**2000s**
Basic demographic groups						
Whites	**0.13**	0.12	0.087	0.11	0.044	0.059
Blacks	**0.12**	0.17	0.091	0.12	0.034	0.038
Hispanic	**0.086**	0.095	0.053	0.075	0.035	0.048
Other race	**0.11**	0.16	0.065	0.10	0.035	0.05
Women	**0.091**	0.10	0.055	0.08	0.031	0.045
Men	**0.15**	0.20	0.11	0.14	0.05	0.065
Ages 18–25	**0.17**	0.24	0.12	0.16	0.055	0.077
Ages 26–55	**0.07**	0.091	0.052	0.054	0.029	0.031
No postsecondary	**0.13**	0.14	0.088	0.12	0.046	0.063
Some postsecondary	**0.10**	0.15	0.066	0.094	0.031	0.046
Selected Mutually Exclusive Groups						
LS Black Men	**0.19**	0.24	0.15	0.19	0.053	0.051
HS Black Men	**0.13**	0.24	0.095	0.13	0.03	0.034
LS Black Women	**0.10**	0.12	0.077	0.097	0.031	0.04
HS Black Women	**0.07**	0.13	0.042	0.063	0.02	0.023
LS White Men	**0.19**	0.17	0.14	0.17	0.069	0.084
HS White Men	**0.14**	0.16	0.099	0.12	0.043	0.058
LS White Women	**0.11**	0.098	0.07	0.097	0.043	0.058
HS White Women	**0.088**	0.084	0.054	0.077	0.027	0.042

NOTE: Data are from National Survey on Drug Use and Health (NSDUH), survey years 1979, 1982, 1985, 1988, 1990–2006. Total observations in 1980s (which includes 1979) equals 18,903; in 1990s equals 163,079; and in 2000s equals 246,889. Prior to 1990, non-marijuana drug use was only asked for selected drugs by name. Race categories exclude Hispanics. Sample is unweighted.

APPENDIX II: ROBUSTNESS CHECKS

APPENDIX Table A3: Comparison of pro-testing legislation impacts across models

Specification:	**[1]**	**[2]**	**[3]**	**[4]**
Black × pro	0.017 (0.013)	0.005 (0.01)	0.016 (0.01)	0.016 (0.01)
Hispanic × pro	-0.016 (0.011)	-0.032 (0.009)***	-0.01 (0.01)	-0.008 (0.01)
Female × pro	0.005 (0.013)	0.005 (0.008)	-0.018 (0.008)*	-0.016 (0.008)
Age 18–25 × pro	-0.004 (0.008)	-0.011 (0.007)	-0.009 (0.007)	-0.01 (0.007)
Low skill × pro	-0.01 (0.015)	-0.028 (0.005)***	0.01 (0.006)	0.008 (0.006)
Pro-testing law	0.00 (0.009)	0.012 (0.01)	-0.002 (0.009)	-0.008 (0.009)
State-level controls	State FE	Group × State FE	Group × State FE	Group × State FE
Year-level controls	Year FE	Year FE	Group × Year FE	Group × cubic trends
Time-varying state controls	Linear state trends	Linear state trends	Cubic state trends	Cubic state trends
R-squared	0.058	0.064	0.065	0.065
N	2,096,833	2,096,833	2,096,833	2,096,833

NOTE: Dependent variable is high-testing industry employment dummy. Data are from March CPS 1980–2010. All specifications include a full set of demographic group main effects: dummies for black, Hispanic, female, and low skill status as well as age and age squared. Coefficients on the main effects are similar across specifications. *** indicates significance at the 0.1% level.

Discussion of Table A3

Column 1 of Table A3 estimates Equation 4 (in main text). Column 2 allows the state fixed effects in Equation (4) to differ across demographic groups, and Column 3 extends this to the year fixed effects while also making the state trends more flexible. Column 4 substitutes cubic groups-specific time trends for group-specific year fixed effects. This is identical to the preferred specification in the paper, Equation (5), but omits time-varying state controls in X_{st}.

The first thing to notice in Table A3 is that the coefficients on the black × Pro interaction are largely robust across specifications. Coefficients on other interaction terms, however, are not. Comparing the estimates in Columns 1 and 2 to those in Column 3 shows that the demographic group interactions with pro-testing laws reverse sign or dramatically change magnitudes for most groups in going from the specification in [2] to that in [3]. In unreported results, I observed that this is largely due to the inclusion of group-specific year effects rather than the switch from linear to cubic state trends. Once this change is made, moving from the specification in (3) to (4) or to the preferred specification in Equation (5) of the paper has little impact on the coefficient estimates.

APPENDIX Table A4: Robustness analysis using later sample periods

Panel (i): Data restricted to 1990–2010

Dependent variable:	**Employed**	**High-test ind.**	**Employed in large firm**	**Health or pension**	**Log real hourly wage**
Effect Size: LSBM × Pro – LSBM × Anti	0.001	0.038	0.123	-0.014	0.077
H_0: LSBM × Pro = LSBM × Anti (p-val)	0.98	0.24	0.002	0.71	0.21
N	1,600,738	1,236,245	1,351,109	1,351,109	1,188,466

NOTE: Identical to Table 7 with the exception that CPS years 1980 to 1989 are excluded from the analysis.

APPENDIX III: THEORY DETAILS

Detailed Discussions

On why one sector and not the other would adopt testing:

If testing sector firms have market power while the non-testing sector is perfectly competitive, this can provide a rationale for the adoption of testing in the former. Firms with market power make some positive profits from each nonusing worker and would therefore like to screen out drug users, from whom it is assumed they make zero or negative profits. Assuming that testing sector firms have market power would not substantively alter the conclusions of the model and would be consistent with the evidence on firm size and industry mix of testing versus nontesting firms in Table 1.

On the assumption that D_i=1 leads to zero productivity:

This simplification is similar to a more general specification: $s_T(D_i) = s_T - D_i f(s_T)$ where $f' > 0, f'' > 0, \lim_{\mu \to -\infty} f = 0, and\ \lim_{\mu \to \infty} f = \infty$. In both cases the absolute productivity loss from drug use is larger for more able individuals and becomes negligible toward the very bottom of the productivity distribution. It is also similar to assuming that drug use is associated with a small probability of a large productivity loss such as that caused by a serious workplace accident or a large theft from the firm, which could be expressed $s_T = s_T + D_i * \varepsilon * loss$.

*On the assumption that D_i is independent of **s**:*

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

On the claim that the probability of drug use among hired workers is lower after testing is introduced, i.e., $\tilde{p} < p$:

Assume detection is independent of **s** within the tested population, and that a constant fraction δ of users is detected by the tests. For any population with a fixed use rate, increasing the probability of detection has an unambiguously negative effect on the probability that a hired worker is a drug user. To see this, let N_0 denote the number of nonusers in the tested population; N_1 is the number of users. Both N_0 and N_1 are fixed.

For $\delta, \delta' \epsilon$ [0,1] and $(\delta) = {(1-\delta)N_1}/{N_0 + (1-\delta)N_1}$, simple algebra shows that $p(\delta) > p(\delta') \; \forall \; \delta < \delta'$, which would imply $p > \tilde{p}$. Hence $p_{Mi} > \tilde{p}_{Mi} \forall i = 0{,}1$. Thus precision in selecting nonusers from among a given pool of workers increases under testing, as asserted in the main text.

Note that higher posttesting wages should attract more applicants to the testing sector. However, as long as the ratio of users to nonusers in the applicant pool is unchanged—which is guaranteed by the assumption that the latent skills vector is independent of drug use—the above relationship between $\tilde{p}$ and p will hold.

In principle, one could look for changes in wage structures across the two sectors following the introduction of testing in order to assess whether productivity changed in the testing sector relative to the nontesting sector. (I have done this, and the results are available upon request.) However, this exercise would not map directly into predictions of the model without further assumptions. Even with the assumption of log normality in wages, the Roy model is unable to generate unambiguous predictions the mean and variance of log wages within sectors and demographic groups. The ambiguous effect of a single-sector price change on these quantities is apparent in the formulas for them provided in Heckman and Sedlacek (1985). In the standard model, additional assumptions are required about the covariance of the disturbance terms to generate clear predictions. The ambiguity is compounded in the drug testing setting because the price change induced by testing is not equal across the using and nonusing segments of the population, and therefore even the size of the testing sector is unclear without additional assumptions about how the skill price change and detection jointly affect the sector choices of drug users.

CDF for Pr(T):

Abstracting from demographic group differences, the assumption of log normality implies that the probability of employment in the testing sector for nonusers is the following (Heckman and Sedlacek 1985):

$$\Pr(T) = \Pr\left(\ln w_T \geq \ln w_N\right) = \Phi\left(c_T\right)$$

$$\text{where } c_T = \frac{\left(\ln \frac{\pi_T\left(p\right)}{\pi_N} + \mu_T - \mu_N\right)}{\sigma^*} \text{ and } \sigma^* = \sqrt{\operatorname{var}\left(\varepsilon_T - \varepsilon_N\right)}$$