Discrimination and the Effects of Drug Testing on Black Employment

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Abstract

Half of today's US workforce is employed by firms that conduct some form of drug testing–a dramatic increase from near-zero levels of testing in the early 1980s. This paper examines the labor market impacts of this large policy change. I incorporate drug testing into a standard Roy model and derive predictions concerning sorting of drug users and demographic groups across the testing and non-testing sectors. Consistent with the model, I find increased employment of non-users in the testing sector following the advent of drug testing. The increase was larger for blacks, a group with higher perceived use rates. Using state-level variation in the timing and nature of drug testing regulation, I also find labor market impacts for blacks that are consistent with discrimination against them in the absence of reliable drug testing. The adoption of pro-testing legislation increases the share of blacks working in the testing sector by 8 to 25%, with the largest shifts among low skilled black men. It also increases blacks' benefits coverage (which is associated with firm testing) and raises low skilled black men's wages by at least 4%. Results from anti-testing states suggest that employers substitute white women for blacks in the absence of testing.

(JEL Codes: J7, J15, K2, K3, M5)

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I. Introduction

In 2011, the United States' entered the fifth decade of its War on Drugs.[1] The drug war has been widely vilified, both within the US and abroad. Critics charge that it has militarized drug production, criminalized behavior that is both minimally harmful to others and often stems from mental illness, led to unconstitutionally overcrowded prisons, and squandered resources while doing so.[2] The drug war is also often declared a failure, as in the face of these efforts drug use has risen over the period.[3] Within this mountain of criticism, 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 from across a range of disciplines.[4]

A quiet companion to the drug war has been the increased use of drug testing within mainstream American society. Drug tests are now routinely required of job applicants, employees, military recruits and personnel, and as a pre-requisite for participation in school activities. Increasingly, drug tests are being required as a condition of receiving social services and government benefits. In the 1980s, U.S. employers began requiring drug tests of their employees and job applicants on a large scale. This paper will focus on how expanded employer drug testing affected employment outcomes for African Americans, a group with high perceived drug use rates and one which has borne large impacts in other dimensions of the drug war.

Writing in its comprehensive 1994 report on workplace drug testing, the National Research Council remarks that "[i]n a period of about 20 years, urine testing has moved from identifying a few individuals with major criminal or health problems to generalized programs that touch the lives of millions of citizens. It has given rise to a[n]… industry that was unimagined just 10 years ago. Tens of millions of urine specimens are analyzed every year…" (National Research Council, 1994, p. 180). According to a nationally

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

representative survey conducted semi-annually by the U.S. Department of Health and Human Services, 50% of employees in the U.S. now work for firms that conduct some form of drug testing while 15-20% report having been tested themselves (Fendrich and Kim, 2002).

Ironically, 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 means for non-using 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 and derive implications for how the introduction of drug testing may impact the sorting of workers from different demographic groups into testing and non-testing sectors.

The model differs from those that have appeared elsewhere in the large literature on the economics of discrimination. It represents a middle ground between the two types of discrimination models: statistical and taste-based.[5] In a taste-based model, employers or other agents experience negative utility from interacting with blacks. This makes blacks more costly as workers, leading to a racial wage gap that arises from the need to compensate interacting whites in order to have blacks employed in equilibrium. In the most widely accepted statistical discrimination models, whites have no aversion to interacting with blacks per se, but the information employers have about black job applicants contains more noise than that for whites, leading to a higher likelihood of selecting a suboptimal match from a pool of black applicants. This leads to hiring blacks at wages that are closer to average quality than own quality since the greater imprecision biases an employer's estimate of a black individual's ability toward the mean. In these models, applicant average quality is either known to employers or converges to their beliefs in equilibrium.[6]

[5] Charles and Guryan (2008 and 2011) provide very useful overviews of these. Even more helpfully, their 2011 paper carefully discusses the challenges to testing these models.
[6] Statistical discrimination has long history as a potential explanation for persistent labor market disparities between blacks and whites (Aigner and Cain, 1977; Lundberg and Startz, 1983) but the practice of it has been challenging to identify empirically.

The simple model proposed here departs somewhat from these models. My model assumes employers have no aversion to hiring blacks per se, so explicit taste-based discrimination does not exist. On the other hand, I allow that the beliefs employers have about blacks relative to whites may be inaccurate, leading to lower hiring rates for blacks that are out of proportion to racial differences in average worker quality. This is similar to statistical discrimination models, in that employers are making decisions based on lower quality information on blacks. The mechanism differs, however, because employer beliefs about their applicant pool are not ultimately correct in equilibrium. Another similarity to the extant statistical discrimination models is that better information can improve hiring for blacks relative to whites. The difference here is that in addition to improving precision in worker selection, better information may modify employer beliefs and further raise levels of black hiring.

Another way to describe this mechanism is to say that employers are operating without racial animus, *conditional on their beliefs*, but these beliefs may be based in racially biased information or experiences. This feature of the model aligns closely with an important line of inquiry into racial disparities among other social scientists. Like economists, sociologists and political scientists have studied overt racist determinants of disparities (which correspond roughly to taste-based discrimination) and structural determinants, which broadly capture the ways institutions reinforce disparities with no racial bias intended. I group traditional statistical discrimination models with the latter – the institutions through which poorer information is transmitted for black applicants reinforce disparities. However, other social sciences have offered a third explanation, which is that certain behaviors become "racialized," or associated with race out of proportion to racial differences in the exercise of those behaviors.[7] In the context of this paper, this means that employers dislike drug use while having no overt animus toward blacks. However, they believe (incorrectly) that blacks are more likely than whites to use drugs and penalize them for this in the labor market.

Altonji and Pierret (2001) and Oettinger (1996) develop structural models of statistical (racial) discrimination and test them using the NLSY79. They reach different conclusions.

[7] Beckett et al (2005) provide a partial review of the literature on this in sociology. Gilens (1996) is a major example of this in political science.

I combine data from the National Survey on Drug Use and Health, the Current Population Surveys, and a variety of other sources to provide empirical tests of the model's predictions. I also estimate a set of traditional Mincer equations that allow returns to demographic characteristics (including race) to differ according to an individual's exposure to employer drug testing. Using microdata spanning two and a half decades, 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 "anti-testing" forms. These contrasting statutes provide a useful check, since the estimated impacts should be different in the two groups of states when compared with non-adopting states. I am also able to exploit differences across local markets within states in the likelihood of drug testing based on stable differences across metropolitan areas 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 non-users increased in the testing sector following the advent of drug testing and that this increase was more pronounced for blacks, a group with higher perceived use rates ex ante. Using state-level variation in the timing and nature of drug testing legislation, I also find large labor market impacts for blacks that are consistent with discrimination against them based on beliefs about their drug use prior to the advent of testing. The results are largest for low skilled black men. Specifically, the adoption of pro-testing legislation increases the share of low skilled black men working in high testing industries by approximately 8-25%. It also increases their coverage in group health and pension plans by about 25%, benefits which are associated with the larger and more sophisticated firms that are also more likely to test. Finally, I find that wages for this group increase by at least 4% in pro-testing states. Results from anti-testing states suggest that employers substitute white women for blacks in absence of testing.

This paper has important implications for our understanding of labor market discrimination. It suggests that a hybrid model of "belief based' discrimination might be useful for understanding how discrimination arises as well as how to address it. Although the model presented here is greatly simplified, it provides a more complete explanation for the empirical results than either of the competing canonical models. The paper also adds to a limited set of papers directly examining employer responses to changes in the information they receive. These include Holzer et al (2006); Stoll and Bushway (2008); Finlay (2009); and Autor and Scarborough (2008). The first three papers examine 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

This leads to the second important implication from this paper. My findings show that even at large and otherwise sophisticated firms, discrimination based on erroneous beliefs about the distribution of a valued attribute across two populations can persist. African-Americans in fact do not use drugs at higher rates than whites, but there is evidence to suggest that employers believe they do. As I will discuss in the conclusion, biased beliefs that are nevertheless influenced by information offer an explanation for the findings in this paper. This suggests that even at highly professionalized firms, the market mechanism posited by Becker (1971) can fail to eliminate this form of discrimination.

Lastly, this paper contributes to our understanding of how the implementation of drug testing as a major employment screen has impacted the labor market overall. The results therefore also offer important information to policymakers. Previous research on employer drug testing has examined outcomes in specific industries or firms where testing has been implemented (Mas and Morantz (2008); Carpenter (2007); Jacobson (2003); Mehay and Pacula (1999); Lange et al. (1994)). While these early studies were important for understanding effects in these industries, they cannot examine effects on labor market participants more broadly. Moreover, none of this earlier work examines differential impacts across racial groups.

II. Background on Drug Use, Drug Testing, and Drug Testing Statutes

A. 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: a small number of somewhat sensational workplace accidents in which drugs were alleged to have played a role; the development of accurate and relatively inexpensive screening devices; and rising public anxiety over the prevalence of drugs in society, which in turn led to the creation of federal incentives for workplace drug testing.[8] Appendix Table A1 lists major legal and policy developments involving workplace drug testing. 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. By the late 1980s, the rights of employers to conduct blanket testing of job applicants and a number of conditions under which they could require current employees to undergo testing had been established in the courts, notably with a major Supreme Court decision in 1989. Moreover, in 1987, Ronald Reagan signed an executive order requiring that federal agencies adopt testing to establish "drug free workplaces." The 1988 Drug Free Workplace Act went further, requiring that federal contractors adopt comprehensive anti-drug policies. Employee and applicant drug testing was clearly in the spirit of this

[8] Facts in this paragraph are taken from Tunnell (2004), Ch. 1; National Research Council (1994) Ch. 6 and Appendix A. Prior to the 1980s, only the military had instituted a drug testing policy for its employees. Even this was not comprehensive; rather the military required only that soldiers pass a drug test before they would be sent home from Vietnam (Tunnell, 2004). The Navy began widespread drug testing in 1982 with other branches following shortly thereafter. A shorter review of the history of employer testing can be found in Knudsen et al. (2003). See Baum (1997) for an excellent history of the drug war.

legislation. Thus, the late 1980s constitute a turning point after which employers begin implementing drug testing programs in increasing numbers.[9]

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).[10] 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 several regularities in testing prevalence appear in both surveys. Specifically, larger employers are more likely to test than smaller employers; there is wide variation in rates of establishment testing across industries; and there is variation across regions of the U.S., with larger shares of establishments testing in the South and Midwest than in the Northeast or West. Knudsen et al. (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.[11] According to Hartwell et al. (1996), however, the share of establishments with 50 or more employees testing in 1988 was 0.16. This rose to 0.48 by 1993, or a three-fold increase for this group overall.

There has been no follow up to the 1993 survey, but comparable statistics can be computed using the National Survey on Drug Use and Health (NSDUH). The NSDUH questioned respondents about the drug testing policies of their employers starting in 1997. To match the establishment data, I calculated the

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

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

[11] 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.

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.[12] In both cases, the NSDUH shares indicate that drug testing increased either not at all or only modestly in the period following the 1993 BLS survey. Inspection of the shares at an annual frequency shows that they are stable over the 1997 to 2006 period. The rapid expansion of employer drug testing therefore appears to have ended in the early 1990s with testing stabilized at its new, higher level.

B. 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.[13] 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 certain liabilities for testing employers, or explicitly permitted certain types of testing. Anti-testing legislation explicitly limited the types of testing employers could require.

I rely on DeBernardo and Nieman (2006) for the 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 anti-testing. Twenty-one states are categorized. The remainder is considered neutral on employer drug testing. Fourteen states are classified as pro-testing; seven

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

[13] It is unclear from the available social history whether employers as a whole were in favor of implementing drug testing. However, it is likely they were in favor of legal clarification of their ability to do so. A 1990 Conference Board survey of large firms found that of those that tested employees, 12% had been sued over the practice and another 24% had been required to engage in arbitration. State-level laws likely provided some of this clarification and reduced the probability of lawsuits.

are anti-testing. More detail on their classification, as well as a table listing classified states, is provided in Appendix 3. Appendix Table A3 illustrates the geographic variation in drug testing legislation. In general, pro-testing states are more commonly found in the South. However, 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 anti-testing 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 anti-testing, 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.

Appendix Table A4 provides clear evidence that these laws impacted state level employer testing policies. Upon special request, the agency that conducts the NSDUH survey (SAMHSA) agreed to tabulate respondent answers to questions about employer drug testing at the state level for the period 2002-2003 and provide them in a table for public circulation. It was not possible to obtain tabulations for earlier periods, since only very recent waves of the NSDUH were designed to be representative at the state level. Table A4 reports the share of NSDUH respondents reporting three different types of drug testing by their employer. The states are ordered according to the classification in DeBernardo and Nieman, and unweighted averages within the classifications are at the bottom of the table. These bottom rows show that employer drug testing, of all forms, is much more likely in pro-testing states than in either unclassified states or those with anti-testing laws. This difference is dramatic. For all types of testing, respondents in pro-testing states report that their employers test at rates roughly 15 percentage points above those in anti-testing states. To put this another way, respondents in pro-testing states report that their employer tests at rates 50 to 73% higher than those in anti-testing states. Respondents in unclassified states report testing rates firmly in the middle between the two legislation-adopting states.

Note that no state has fully banned testing nor has any state required drug testing of all employees in its boundaries. Therefore the state statutes interact with ongoing federal drug testing initiatives, as well as with the larger environment of common corporate practices – both of which were trending toward increased use of drug testing over the period. The results from analysis relying solely on state-level variation in drug testing policies will therefore be somewhat attenuated as drug testing becomes more common even in neutral states and as smooth controls remove some useful causal variation.

C. Drug Testing and Drug Use

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.[14] The most common testing kits screen for 5 to 10 different types of drugs, including opiates, cocaine, marijuana, PCP, and amphetamines. These also include the active ingredients in prescription painkillers, for which applicants may be required to provide a doctor's verification that these have indeed been prescribed. A drug test "failure" typically requires a positive result at both the initial screening phase and in a second, confirmatory test of the same specimen, usually conducted by a specialty lab using more sophisticated measures.[15] Contrary to some popular claims, the tests used in the initial screening phase have low rates of false positives—about 2%. The confirmatory tests are highly accurate and are not considered subject to false positives or false negatives.[16]

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

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[14] 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.
[15] Roughly 70% of employers order a confirmatory test in the event of a positive initial screen (Conference Board, 1990).
[16] The Supreme Court has ruled that the gas chromatography and mass spectrometry (GC/MS) procedures used in these second tests are highly accurate and admissible as evidence (Tunnell, 2004).

of false negatives that occur even in the absence of evasion efforts by tested individuals.[17] False negative rates average 20% over the five main drug classes but are highest for marijuana—over 40% (U.S. Department of Justice, 1991).[18] However, a large number of false negatives are due to generous cutoff levels established by the National Institutes on Drug Abuse rather than to technological limitations in the screening methods.[19] A second significant source of false negatives is lax oversight in testing facilities. A government study found numerous lapses in testing protocol at collection sites for the federally-mandated DOT drug testing program, suggesting that cheating is possible at some of these facilities (Government Accountability Office, 2007).

It is important to note that people do fail drug tests. This is in spite of fairly high rates of false negatives as well as the predictable timing of many tests. One source of public information on drug test failure rates is Quest Diagnostics, a general 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 non-representativeness. First, the number of tests performed in the U.S. 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). Figure 1 shows that there is also considerable geographic variation in failure rates, with the worst-performing areas on this measure reporting failure rates in the range of 5.5-16% in 2007. There is evidence that drug test failure typically stems from regular use. DuPont et al. (1995) estimate that a majority

[17] Most efforts to substitute a urine specimen or to supply one that has been adulterated in order to conceal drug use could be easily detected by monitors at the collection site. (National Research Council, Ch.6, 1994.)
[18] DOJ sampled over 2400 individuals held in the California criminal justice system, who presumably have little access to "masking compounds" and other evasion techniques. Using the confirmatory GC/MS procedures to establish a sample's true drug content, the DOJ researchers evaluated the accuracy of several standard screening tests. The experiment found high rates of false negatives among samples known to be drug-positive.
[19] These cutoffs are binding for federally mandated testing programs, but non-mandated private employers are not bound by them. The National Research Council report notes that detection rates are higher among firms not bound by NIDA guidelines (Ch. 3, 1994).

of those testing positive (in random workplace tests) are daily users while only 7% are infrequent, annual users.

In contrast to the limited data on drug testing by employers, measures of drug use are available back to 1979 in the NSDUH. Table 2 summarizes patterns of drug use in the U.S. population aged 18 to 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. The first pattern to notice is that marijuana use is much more common than other drug use. However, group differences in marijuana use tend to be mirrored in the use of other drugs, only at lower rates. The biggest inter-group difference in use rates is across genders: men use drugs at nearly double the rate for women. The other major demographic difference in use rates is across age groups. 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. Drug use is also more common among the less educated, in this case, those with no post-secondary education. Interestingly, this differential only emerges after 1990 but is stable thereafter, even during the period of rising use rates after 2000.[20]

In order to better understand employer perceptions of drug use, I create mutually exclusive groups based on multiple demographic characteristics. Employers likely see their applicants and employees as a collection of traits, e.g. "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

[20] 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 et al. 2001 survey the literature on underreporting among institutionalized populations). Lu et al. (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 et al. 2001).

the ages of 18 and 65, the age distribution is fairly similar across races. The bottom panel of Table 2 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.[21]

Regardless of whether blacks ever used marijuana at higher rates than whites, there a large body of evidence 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) explored the reasons for overrepresentation of blacks among arrestees for drug possession. They find that compared to their sample of users, blacks are more likely to be arrested for possession than whites, and even more so for crack in particular. They conclude that arrest disparities are therefore not a result of structural differences in drug use or that they arise from 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 as well as which drugs were "worst" and that these perceptions disproportionately targeted 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 continue to hold perceptions of use that differ from reality suggests that persistent employer misperceptions might not be unreasonable. Several studies support this possibility. Wozniak (2011) documents a belief that blacks are more likely to fail a drug test among hiring managers in a small survey. Burston et al. (1995) cite evidence that even black youth overestimate their own drug use relative to whites. They also cite a 1989 survey in which 95% of respondents described "the typical drug user" as black. Gilens (1996) finds that welfare use is an issue that has become similarly associated with blacks out of proportion to their use of it.

[21] 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.

III. 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). Let firms be divided into the testing sector and the non-testing sector, so named because of the practices they will adopt when drug testing becomes available. Workers are endowed with a vector of sector-specific skills **s** = (s_T, s_N), denoting skills in the testing and non-testing sectors, respectively. Workers can apply for employment in either sector and move between them costlessly at any time. There are two periods, or equilibria: the pre-testing period, when drug testing is not available to firms, and the post-testing 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:

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

Skills **s** are observable but drug use D_i is unobservable before the advent of testing. I also assume that drug use is independent of **s**.[22] 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. Since firms have no information about which hires are more likely to use drugs, they simply deflate offered wages by a constant probability of drug use. Thus testing sector firms offer wages 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 $k_T(1-p) = \pi_T(p)$.[23]

[22] I discuss this and the assumption that drug use sets productivity in the testing sector to zero in detail in the Theory Appendix.
[23] 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.

Non-testing firms offer wages equal to expected (and realized) marginal productivity: $w_N = \pi_N s_N$ where π_N is a constant. $\pi_T(p)$ and π_N are then the sector-specific skill prices in a standard Roy model.

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.[24] 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 non-testing sector wage, which in turn becomes a function of the parameters of the skill distribution:

$$(2)\ \Pr(T) = \Pr\left(\pi_T(p)\, s_T \geq \pi_N s_N\right) = \Pr\left(\ln k_T + \ln(1-p) + \mu_T + \varepsilon_T \geq \ln \pi_N + \mu_N + \varepsilon_N\right)$$

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.[25]

I assume that firms hold beliefs about rates of drug use in the two demographic groups, p_{M1} and p_{M0}. These may differ from actual rates of use, 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

[24] 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 $\varepsilon_T - \varepsilon_N$.
[25] 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.

(2), it is clear that these differences in assumed use rates imply that $\Pr\left(T \mid \boldsymbol{s}, M_i = 1\right) < \Pr\left(T \mid \boldsymbol{s}, M_i = 0\right)$ in the pre-testing period.

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[26]:

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

This type of signal potentially accomplishes two things. First, it increases the precision of employer beliefs about drug use among workers. This means that $\tilde{p} < p$. In the Theory Appendix, I demonstrate that this holds under certain conditions. Second, the information that arrives via the signals may enable employers to revise their beliefs about drug use rates to bring them closer to reality.

Increased precision in worker screening raises the likelihood that non-users are employed in the testing sector. To see this, first notice that $\tilde{p} < p$ implies that $\pi(\tilde{p}) > \pi(p)$. Abstracting for a moment from demographic group differences, the assumption of log normality implies that the probability of employment in the testing sector for non-users is the following (Heckman and Sedlacek, 1985):

$$\begin{aligned} & \Pr(T) = \Pr\left(\ln w_T \geq \ln w_N\right) = \Phi\left(c_T\right) \\ & \text{where } c_T = \frac{\left(\ln \frac{\pi_T\left(p\right)}{\pi_N} + \mu_T - \mu_N\right)}{\sigma^*} \text{ and } \sigma^* = \sqrt{\operatorname{var}\left(\varepsilon_T - \varepsilon_N\right)} \end{aligned} \tag{4}$$

The introduction of testing raises $\pi_T(p)$ and leaves all other terms unchanged, unambiguously increasing $\Pr(T)$.[27] Therefore the probability of employment in the testing sector rises among non-users after testing is introduced.

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

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 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 these ex ante beliefs are relatively inaccurate, 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. Employer updating of relatively biased beliefs means that $p_{M1} - p^*_{M1} > \tilde{p}_{M1} - p^*_{M1}$. (For simplicity, I abstract from the possibility that increased precision could mean that $\tilde{p} < p^*$.[28]) 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 non-users than for white non-users. It is important to note that this assumes that (relative) drug use rates are unchanged across demographic groups, but the evidence in Table 2 suggests this is a reasonable assumption.

In sum, the model generated three predictions that I will test empirically. First, the share of non-users 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), then the increase in testing sector employment should be greater among black non-users than white non-users. Finally, if employers are ex ante biased, testing should increase the employment of blacks in the testing sector overall.

IV. Methods for Assessing the Impact of Employer Drug Testing

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

[28] Given the high rates of false negatives in drug screening, it seems reasonable to ignore this case.

A. Microdata Sources

I draw on microdata from two sources. Each has features that make it a more appropriate source for some of my research questions but not for others. The bulk of the analysis uses microdata on individuals ages 18 to 55 from the IPUMS versions of the March Current Population Surveys (King et al. 2010). I use the CPS data to answer questions about differential impacts of employer drug testing on labor market outcomes without regard to drug use. For example, were blacks in general more likely to be hired into the testing sector after testing became widespread? The March CPS surveys, as is widely known, contain the richest set of employment variables in the monthly CPS. The resulting data set includes representative, annual cross sections of prime aged individuals in the U.S. population over the period 1980 to 2006. Limiting the analysis to 2006 and earlier balances the desire to allow sufficient post-legislation periods (all states but one have at least 3 years of post-legislation observations) with concerns about the 2007-2009 recession influencing trend control variables.

I supplement the analysis with data from the NSDUH. The NSDUH is a nationally representative survey of individuals aged 12 and older first conducted in 1979. It is currently conducted annually although the survey was semi-annual between 1979 and 1987. The sample size has increased considerably over the years. The 1979 sample contained roughly 7200 individuals and grew to include over 55,000 individuals in 2006. It is the definitive source of data on drug use in a representative US population. The NSDUH contains detailed information on respondent drug use histories (using primarily retrospective questions) 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 non-users changed across sectors as testing expanded. All NSDUH analysis and statistics are unweighted.

While the NSDUH contains rich information on drug use and individual exposure to employer testing, I rely on the CPS estimates of drug testing's impacts on relative labor market outcomes. This is for two reasons. First, the NSDUH does not include any geographic identifiers below the nine Census divisions.

This precludes the difference-in-differences analysis I carry out using state-year variation in drug testing legislation. Without the option of matching individuals to their likelihood of *exposure* to drug testing, I can only estimate correlations between an individual's outcomes and the reported drug testing practices of her employer.[29] 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. Other non-white races are not separately identified in the CPS until the latter part of my sample period. As a result, the omitted race/ethnicity category in most specifications is properly called "whites, Asians and Native Americans," although I will refer to the group simply as "whites." I also create a dummy variable to indicate young workers, those ages 18-25; these constitute nearly a quarter of the sample. Education is measured using two categories: high school dropouts and high school graduates (or low skill) and those with any post-secondary education (or high skill). Table 3 shows the share of the sample in the low skill group. I do not include marital status as a covariate in any of the specifications since it is likely endogenous to education and age.

Table 3 also summarizes employment outcomes of interest. Unfortunately, no large data set tracks employment by demographics and employer drug testing policies (the outcome with which the model is primarily concerned). Instead, I use three proxies for 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 all one-digit industries that achieve a testing rate of over 50% by the late 1990s according to Table 1. Specifically, these are mining; communications and utilities; transportation; manufacturing; and government.[30] The table shows that the high testing sector employs about 30% of currently employed workers when defined at the industry level.

[29] Carpenter (2007) has already carefully documented these relationships.

[30] 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.

The second is the dummy variable for employment at a very large firm (> 500 employees), which only available for 1988 onwards. As discussed above, there is a clear relationship between employer size and the likelihood of drug testing. About 40% 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.[31] These benefits are likely related to employer size but also to employer 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. It therefore also proxies for employment in the "good" sector. Table 3 shows that the coverage rates for both benefits are somewhat higher than 50%. 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 and presents means separately for them. In addition to the means for the full sample over all years of data (overall mean), the table shows means for the eight years prior to the Workplace Drug Act as well as separately for the subgroup of states that ever adopt either form of drug testing legislation. Table 3 also presents means for the ultimate dependent variables by racial subgroups for all these periods. One can therefore use Table 3 to gauge the magnitude of the estimates reported in the results section. One can also compare the characteristics of CPS respondents from states that ultimately become pro- or anti-testing. As I show below, I exploit variation within states over time, so 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 the largest firms. Since several Southern states are pro-testing, this group has a much larger share of blacks in their populations than do anti-testing states.

[31] The universe of the group health questions changed over time, and the wording of the questions was modified slightly. It is possible to adjust the coding of the group health coverage variable in the IPUMS data (INCLUGH) to account for the universe changes over time. It is not possible to correct for changes in the question wording. However, the question becomes somewhat more selective over time in terms of who is classified as having group health coverage, suggesting that trends in later periods should be toward decreasing coverage. Results are very similar when the two benefits are used a separate dependent variables.

B. Estimating Equations

I begin by assessing the model's prediction that the share of non-users 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 very 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 A4. Data limitations in this exercise force me to restrict the NSDUH data to the 1985 to 1997 waves. I divide drug testing history into three phases: the pre-testing period of 1985 to 1988, the period of rapid transition to higher testing rates of 1989 to 1994, and the post- or high-testing period of 1994 to 1997.[32] 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 A4.

To test the model's predictions, I look for evidence of two phenomena. First, were non-users increasingly sorted into high testing industries over time and in higher testing regions? And second, was the shift of non-users into testing sector employment larger for blacks? There are at least two ways to test these predictions. Since the NSDUH contains microdata on drug use, industry of employment, Census division and survey year, I could create an estimating equation with high testing industry on the right hand side and controls plus interactions of interest on the right. The first prediction requires examining a triple interaction (drug use x time period x testing region), and the second requires a quadruple interaction (the triple interaction times race). Moreover, there is more than one triple interaction of interest since interactions for

[32] This classification is based on the publicly available data on changing drug testing practices in Table 1, and the policy history in Table A1.

each testing region-time period combination must be interacted with drug use. Reading estimates from such a model becomes unwieldy.

An alternative is to simply examine differences in adjusted high testing sector employment rates between users and non-users by time period-testing region cells. I regression adjust high testing sector employment using controls for demographics (age, race, Hispanic ethnicity, sex, and educational attainment), demographic group-specific cubic time tends and group-specific division fixed effects, and all relevant main effects. I then compute the difference in means and its standard errors within the nine region-time period cells, subtracting the mean residual high testing sector employment of users from that for non-users. This approach is more descriptive but also more transparent.[33]

I then turn to the CPS to examine the impact of state-level employer drug testing laws on relative labor market outcomes. Specifically, I estimate Mincer-style equations that allow the employer testing environment in an individual's state to affect the returns to her personal characteristics. After merging the CPS microdata with information on state employer drug testing statutes, I estimate the following model:

Eqn. 1

$$y_{ist} = \beta_1 (Pro_{st}\Gamma_{ist})' + \beta_2 \tilde{\Gamma}_{ist}{}' + \beta_3 Pro_{st} + \beta_4 X_{st} + \Theta_s + \Theta_s \Gamma_{ist}{}' + \chi_t + \chi_t \Gamma_{ist}{}' + trend * \Theta_s' + \mu_{ist}$$

Here, Pro_{st} is an indicator variable equal to 1 if a state *s* with a pro-testing classification in DeBernardo and Nieman (2006) has enacted drug testing legislation by year *t*. β_1 and β_5 are 1 x k vectors of demographic group-specific coefficients. Γ is a 1 x k vector of demographic characteristics. These include indicators for black, white, and Hispanic ethnicity; gender; age less than 25; and no post-secondary education (low skill). $\tilde{\Gamma}$ controls for age directly and includes age-squared. The specification includes demographic-group specific state fixed effects (Θ_s and its interactions with Γ_{ist}), state time trends, and time-varying state-level controls. The demographic-group specific state fixed effects not only absorb fixed

[33]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 clusters – in this case, at most nine.

differences across states but also allow these fixed differences to differ across demographic groups. The demographic-group specific cubic time trends absorb changes over this period that are common to the U.S. labor market for a group as a whole. An example of this is wage inequality, which has risen over time for all workers but differentially according to race, gender, and skill group.[34] State-specific time trends absorb smooth changes in labor market outcomes across states over the period of the study. The state-year specific controls are the state unemployment rate, state minimum wage, and state incarceration rate.[35] The first two control for variation in state labor market conditions that does not follow a smooth trend. The state incarceration rate is a proxy for intensity of state-level efforts to curb drug trafficking, which may have affected drug use and perceptions of use independently of employer testing policies. Finally, the model controls for demographic group-specific non-linear (cubic) time trends, represented by χ_t.

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 legislation on employment in general and log wages, although both are outside the scope of the model.

This and all remaining models are estimated using OLS. This means that Equation 1 is a linear probability model for several outcomes. 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.[36] However, the linear probability model allows calculation of multilevel clustered standard errors, and the results are robust to clustering on state and year instead of state only.

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[34] 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. I have also estimated specifications that substitute a full set of demographic-group specific year effects for the cubic trends, and all results are robust to this change. Use of the cubic trends in estimation saves on computation time.

[35] State level unemployment rates for 1976-2009 were taken from the Bureau of Labor Statistics http://www.bls.gov/lau/home.htm. State minimum wage data for 1969-2010 was taken from the Department of Labor http://www.dol.gov/whd/state/stateMinWageHis.htm. State prison populations for 1977-2004 were taken from the Bureau of Justice Statistics http://bjs.ojp.usdoj.gov/content/data/corpop01.csv.

[36] I have also experimented with multi-level clustering on both state and year using linear probability specifications.

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. The comparison groups are the same demographic groups in the same state prior to the adoption of the legislation. Therefore these are triple differenced, or DDD, estimates.

Because the nature of drug testing legislation varied across states, I am able to expand the specification in Equation 1 to further exploit the variation in testing environments provided by states that adopted *anti-testing* 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 anti-testing. The controls are the same, and the specification becomes the following:

Eqn. 2

$$y_{ist} = \beta_1(Pro_{st}\Gamma_{ist})' + \beta_2\tilde{\Gamma}'_{ist} + \beta_3 Pro_{st} + \beta_4 X_{st} + \beta_5(Anti_{st}\Gamma_{ist})' + \beta_6 Anti_{st}$$
$$+\Theta_s + \Theta_s\Gamma_{ist}{}' + \chi_t + \chi_t\Gamma_{ist}{}' + trend * \Theta'_s + \mu_{ist}$$

Now there are two sets of DDD estimates: β_1 as before but also β_5. If employer drug testing changes relative employment outcomes differentially across demographic groups (and if state testing laws affect employer behavior), then β_1 and β_5 should generally be of opposing signs. This additional variation allows me to test whether it is not just the presence of testing legislation that matters for relative employment outcomes but whether the actual content of the legislation matters.

Finally, I am able to 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 very stable over time. The composition of the local economy therefore creates differences in the likelihood than 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 level shares of reported testing by industry and firm size.[37] I normalize the index to have mean zero and standard deviation one, and incorporate it into the Equation 1 specification:

Eqn. 3

$$y_{ist} = \gamma_1 (Pro_{st} DT_{ist} \Gamma_{ist})' + \gamma_2 \tilde{\Gamma}_{ist}{}' + \gamma_3 Pro_{st} + \gamma_4 DT_{ist} + \gamma_5 (DT_{ist} \Gamma_{ist})' + \gamma_6 Pro_{st} DT_{ist}$$
$$+ \gamma_7 (Pro_{st} \Gamma_{ist})' + \Theta_s + \Theta_s \Gamma_{ist}{}' + \chi_t + \chi_t \Gamma_{ist}{}' + \gamma_8 X_{st} + \mu_{ist}$$

Here, the main estimates of interest are in the vector γ_1. These show whether relative labor market outcomes change differentially for individuals in metropolitan areas with high drug testing exposure (DT_{ist}) 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 actually experiencing testing. However, the Equation 4 estimates have some limitations. First, migrating between MSAs within a state is much less difficult than migration across states. It is therefore more likely that migration attenuates the observed impacts of high drug testing exposure as drug users and non-users re-sort themselves across local markets within a state as employer testing increases in one market relative to another. Second, the analysis is limited to individuals in MSAs large enough to be identified in the CPS data. These may or may not be representative subsamples within states or demographic groups. Finally, the CPS is not designed to produce representative estimates at the MSA-level, particularly in the early years of the data. This puts restrictions on the models that can be estimated. Equation 3, it is important to note, retains the state-level control variables of earlier equations as the CPS design does not support estimating MSA-specific effects, particularly for

[37] 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 http://www.census.gov/econ/susb/historical_data.html. 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.

specific demographic groups. For these reasons, I present the estimates from Equation 3 as a check on the estimates from Equations 1 and 2 rather than as the preferred estimates.

V. Results

A. Using Time Variation to Identify the Impact of Testing on the Sorting of Drug Users and Non-Users into Employment Sectors

Table 4 tests the first of the model's predictions: that the share of non-users 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 non-users in all three regions during the pre-testing period. Here a respondent is classified as a drug user if she reports using any drug illicitly in the past month and as a non-user otherwise.[38] 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 post-testing period. However, the gap disappears in the low testing region. As showing in Appendix Table A3, many low testing states passed anti-testing 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 non-user employment advantage is only significant for blacks in the two higher testing regions and in the post-testing period.

The evidence so far 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, consider the change in the testing sector employment gap between users and non-users separately for blacks and whites. For both groups, the gap widens in favor of

[38] DuPont et al (1995) show that more frequent users are more likely to be detected (by random tests). Results are similar when users are defined as those reporting any drug use in the past year, zero otherwise.

non-users during the transition period. The gap widens further for blacks in the post-testing period but is largely stable for whites. Also, the increase in the gap over the pre-period in the highest testing region is larger for blacks than for whites. I conclude that the evidence in Table 4 is suggestive that the impacts of employer drug testing were larger and more positive for non-using blacks than non-using whites. This provides support for the model's second prediction.

B. The Impact of Employer Drug Testing on Relative Labor Market Outcomes in the CPS

The remainder of the analysis uses state variation in drug testing legislation to generate DDD estimates of the impact of testing on relative labor market outcomes. Results from the most basic specification, Equation 1, 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 anti-testing laws, and all never-adopting states. The five columns report results from estimating Equation 1 with five different dependent variables.

The coefficients of interest are the interactions of demographic characteristics with the time- and state-varying pro-testing law indicator. Blacks, women, the low skilled, and Hispanics 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 1 to 2.8 percentage points in the dummies for high testing industry employment, large firm employment, and benefits coverage. The positive impact on benefits coverage for blacks is significant at the 1% level. The impacts on these measures are all also positive for Hispanics but of about half the magnitude and none is significant. Log wages also increase for blacks following the adoption of a pro-testing law. The point estimate of a 1.7 percent wage increase is significant at the 10% level. There is no impact on overall employment for blacks. The wage impact for the low skilled is about half that for blacks and insignificant. Hispanics experience a significant wage decrease of 2.6%. 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. The largest negative impacts for women are on the three measures of employment in the high testing sector. High testing industry employment, large firm employment, and benefits coverage all decline for women by about 1.5 percentage points – a change that closely mirrors the improvements for blacks. The point estimate on log wages is negative for women, although it is insignificant. Post-estimation tests of equality reject that the coefficients for blacks and women are the same for all measures except the employment dummy.[39] 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. The state unemployment rate negatively affects overall employment, testing industry employment, and benefits coverage but not large firm employment. These and the other differences across the bottom half of the columns in Table 5 suggest that the three proxies for testing sector employment each capture something slightly different, which makes them useful as a set of related but not identical outcomes.

Equation 2 incorporates the policy variation from anti-testing 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 anti-testing interactions. In fact, the point estimates and patterns of significance are essentially unchanged between Tables 5 and 6 for the pro-testing interactions.

[39] Rejection is at 5% level for large firm employment, benefits coverage, and log wages; at 10% for employment in a high testing industry.

Nevertheless, the anti-testing interactions are interesting for several reasons. First, estimates for blacks are negative, economically large, and statistically significant for both high testing industry employment and pension coverage. 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 impacts on blacks that are nearly the perfect opposite. Importantly, t-tests reject the equality of the pro- and anti-testing interactions with black status for both high testing industry employment and benefits coverage. Equality of the same interactions in the wage equation is rejected at the 10% level. 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 anti-testing laws on women in Table 6. However, t-tests reject the equality of the pro- and anti-testing interactions with female status at the 10% level or better for all outcomes except general employment. Blacks and women in pro- and anti-testing 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 A3, anti-testing 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 x anti-testing* interactions will have larger standard errors than estimates for the black x pro-testing interactions. Even though the *black x anti-testing* interactions are only significant for two outcomes, the fact that they are near perfect opposites to their pro-testing counterparts is suggestive that employer testing is important for black labor market outcomes. 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 anti-testing states.

In Table 6, the interactions with Hispanic are also uniformly negative after the adoption of anti-testing legislation. The impacts on the testing sector proxies are generally positive for Hispanics, but less so than for blacks. In contrast to the case for blacks, the wage impact of pro-testing legislation is negative for Hispanics. Based on this evidence, Hispanics seem to constitute an intermediate case, experiencing some of the positive effects of employer testing but to a much smaller degree than blacks do. In order to focus on the impacts for blacks and women, I exclude Hispanics from subsequent analysis.

Table 7 probes the Table 6 estimates along two different dimensions. First, I estimate Equation 2 on subsamples of blacks only or whites only (Hispanics are excluded). Since some characteristics – skill, in particular – are correlated with race, this complicates the interpretation of the interactions demographic characteristics with testing legislation in Tables 5 and 6. For example, blacks and the low skilled are similarly affected by pro-testing legislation. It is unclear whether these impacts are largely on blacks but black is in part proxied by low skill (or vice versa) or whether the impacts are important for both blacks and the low skilled. The estimates for separate subsamples in Table 7 can disentangle these possibilities.

The second dimension I examine in Table 7 is the possibility that the impacts of testing legislation are attenuated over time either through migration as workers re-sort themselves across markets with more or less testing, or through the increasing adoption of testing by employers in states that have no explicit anti-testing laws.[40] To restrict these channels of attenuation, I limit the samples in two ways. In the upper panel of Table 7, I omit observations from states that adopt testing legislation (of either form) that are more than

[40] For example, Wal-mart drug tests all job applicants.

three years after the adoption and therefore more subject to attenuation through migration and worker re-sorting across labor markets. In the bottom panel, I omit observations from late adopting states (from 2000 and later), which are more subject to attenuation from control group contamination as employers become more likely to test in states with no specifically anti-testing legislation.[41]

Table 7 reports four sets of estimates: two subsamples estimated under two sample restrictions. The racial subsample results are broadly similar across the two sample restrictions. Estimates for both groups show that the negative impacts of pro-testing laws on women apply to both black and white women. Estimates from the black subsamples also show that low skill blacks are driving the positive impacts of pro-testing laws among blacks. Interestingly, impacts for low skill whites are all close to zero. This indicates that the positive results of pro-testing laws for blacks and the low skilled in Tables 5 and 6 are driven by the impact of testing on low skill blacks in particular. The point estimates for low skilled blacks and for women are also often larger in Table 7 than in the earlier estimates. This is consistent with the sample restrictions in the two panels of Table 7, which were applied to reduce attenuation due to control group contamination. The fact that these two crude adjustments for such contamination raises the point estimates noticeably suggests that the earlier estimates, and perhaps even those in Table 7, suffer from downward bias.

To examine the separate contributions of race, skill, and gender from a different angle, I break down the black and white populations into the mutually exclusive demographic groups from Table 2. The equations estimated in Table 8 substitute indicator variables for these eight groups for the Mincer-style controls for demographic characteristics used in earlier specifications. I again drop Hispanics from the sample and divide the remaining CPS respondents into categories according to race (black or white); sex; and skill. I modify Equation 2 to include indicators for the seven groups (high skilled white men are omitted) and their interactions with pro- and anti-testing legislation. All other controls are modified to use the exclusive groups instead of the demographic controls in earlier specifications.

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[41] Anti-testing legislation controls and interactions were included in all Table 7 specifications, but their coefficient estimates are unreported.

Consistent with Table 7, Table 8 shows that low skill blacks experience large positive impacts of pro-testing legislation on their labor market outcomes. These effects are largest and more often significant for low skilled black men, but all point estimates are also positive for low skilled black women. The impacts of pro-testing laws in Table 8 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. I find that employment in the high testing sector (by any of the three measures) increases by roughly 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 12% for employment in a high testing industry and roughly 8-9% for the other two outcomes. The magnitude is even larger when compared to low skilled black men in anti-testing states. Here the difference in testing sector employment is roughly 10 to 13 percentage points (by any of the three measures) between blacks in pro- versus anti-testing states. This implies a relative increase in high testing sector employment of approximately 25% for low skill blacks for any of the three measures. The results also show a statistically and economically significant wage increase of 4% for low skill black men in pro-testing states. Again, the difference relative to the same group in anti-testing states is even larger. For low skilled black men, I again reject that the interactions with pro- and anti-testing state status are the same for all outcomes except general employment. The pro- versus anti-testing interactions are sometimes statistically unequal for other groups, but for no other group are all three testing sector proxies unequal.

Table 8 also shows that the negative impacts of pro-testing laws for women identified in Table 7 seem to be shared by low and high skilled white women. Low skilled white men, on the other hand, experience an increase in employment overall and in testing sector employment following the adoption of pro-testing legislation while low skilled black women experience negative impacts of anti-testing legislation. However, these impacts do not differ in sign across pro- and anti-testing states and therefore do not indicate the same result found for low skilled black men: that the impacts of pro- and anti-testing laws are opposite.

Finally, I find some significant *positive* impacts of anti-testing laws for low skilled white women in Table 8. In particular, employment of low skill white women in a high testing industry increases by 3 percentage points and benefits coverage by 4 percentage points after the adoption of anti-testing legislation.

In unreported results, I examined whether the wage increases observed for blacks in pro-testing states in Tables 5, 6, 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 8. The addition of these controls greatly reduced the coefficients on *pro-testing x black* in Tables 5 and 6. The coefficients were not statistically significant, and more precisely, I could no longer reject equality of the coefficients for blacks and women (in Table 5) and blacks in pro-testing states versus anti-testing states (in Table 6). This suggests that wage increases for blacks overall are largely explained by shifts into testing sector employment. Results were different for low-skilled black men in particular. When I added the same controls to the Table 8 wage equation, the coefficients on the testing-status interactions with low skilled black male were reduced somewhat, but equality can still be rejected with a p-value of 0.07. The coefficients imply a wage gap of 11% between low skilled black men in the two groups of states. The results suggest that this group still earns significantly more in pro-testing than in anti-testing states after law passage, even conditional on sector of employment.

In Table 9, 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. These are the same restrictions that were imposed separately in Table 7. I make these restrictions for several reasons. Most importantly, the problem of contamination bias 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.[42] 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-differences in industrial composition are found to contribute significantly to these impacts.

The results are shown in Table 9. Coefficients on the MSA-level drug testing exposure interactions with exclusive demographic groups are reported in the bottom panel. The most striking results are in the second column. These show that employment in a high testing industry 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 4.4 to 6.0 percentage points. Consulting Table 3 again, these impacts for high testing industry employment

[42] 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.

represent an increase of 13% ore 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 large 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 much more likely to be covered by group health or a pension plan 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). The negative state-level impact of pro-testing laws on white women (top panel) is also largely undone in high testing exposure MSAs. This is somewhat counterintuitive, since if white women are substitutes for blacks, then we would expect the negative employment effect of pro-testing laws to be magnified in high testing exposure cities. However, it is important to recall that mobility across MSAs is substantial, and the negative impacts on white women in other MSAs may arise through current black residents of an MSA substituting for white women who might have moved in to a high testing MSA to take a job in the absence of a pro-testing law.

The possibility of general equilibrium effects of migration make it difficult to attribute intra-state differences in outcomes under state-level drug testing laws solely to the likelihood of exposure to testing in different metropolitan areas. Yet in spite of these complications, the differences in employment outcomes across blacks in the same state but different MSAs is striking. I conclude that the evidence in Table 9 is consistent with a strong positive impact of exposure to employer drug testing on the probability of black employment in high testing industries, particularly for low skilled black men. There are also likely strong positive impacts of testing on black employment in "good jobs" with benefits like group health insurance and pension coverage.

VI. 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 25 year period.

The analysis generated several findings. First, the probability of employment in the testing sector rose markedly for non-users as testing expanded over time. In the early 1980s, self-reported non-users were not significantly more likely than drug users to work in high testing industries. By the late 1990s, they were about 4-8 percentage points significantly more likely in regions with medium to high levels of employer drug testing. This suggests that the expansion of testing allowed employers to more reliably choose non-users from among potential workers. Moreover, this probability increased more for non-using blacks than for non-using whites in the highest testing regions. 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 8-25% for low skilled black men in pro-testing states. In contrast, adoption of anti-testing legislation, as happened in several states, reduced testing sector employment for this group. Low skilled black men also experienced significant wage increases – of at least 4%– following the adoption of pro-testing laws. About half of this wage increase can be explained by increased employment in the testing sector, which has larger firms in industries with well known wage premia. The remaining increase is still significant after controlling for sector of employment, implying that low skilled black men earn more after testing is introduced, even conditional on sector. Finally, I find some evidence that employers substitute white women for blacks in the absence of drug screening.

I conclude that results are consistent with widespread "belief oriented" discrimination against blacks prior to the advent of drug testing. Specifically, my interpretation of thee results is that they imply that employers hired blacks at lower rates prior to the adoption of testing because they persistently believed blacks were more likely to use drugs than whites, something that is not supported by the evidence here. The information made available by testing allowed blacks to demonstrate their non-use, and increased hiring of blacks in the testing sector. Employers may have used the information provided by tests in one of two ways: to refine their expectations about the probability that a hired black worker was a drug user, and to refine their beliefs about drug use in the black population generally. I cannot distinguish between these two possibilities.

Nevertheless, the belief based model reconciles findings that do not fit with the conventional discrimination models. On the one hand, 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. On the other, a classic statistical discrimination model predicts or supposes that employers are correct in their beliefs about group average productivities in equilibrium. Yet the evidence on drug use patterns presented here (and elsewhere) shows that relative drug use between blacks and whites is nearly identical and has not changed since the early 1980s. Therefore testing sector employers are operating in the same labor environment throughout the period under study, yet their hiring of blacks changes dramatically alongside the expansion of employer testing. A model in which employers change their beliefs about drug use among black hires can explain this shift in the face of stable drug use patterns. A classic statistical discrimination model cannot. Finally, these results are made even more surprising because, as is clear in Table 2, blacks were already employed in the high testing sector at high rates prior to testing. Therefore employers in that sector had extensive experience with this group. Yet the results show that improved information on drug use increased black employment in this sector even more and increased wages for low skilled black men within the sector.

There are two main critiques to this interpretation. First, one can ask whether it is reasonable to think that biased beliefs can persist in a market equilibrium. Earlier in the paper I documented the deeply held belief that blacks are more likely to use drugs: among employers; among police officers; and among blacks themselves. It is possible that participants simply overlook evidence contrary to those beliefs unless they view it as "definitive." Also, as Charles and Guryan (2011) explain, several models have demonstrated that taste-based discrimination can survive in a competitive market. If the profit opportunities from non-discriminating employers can be consistently passed up, it seems possible that the opportunities from refining one's deeply held beliefs can also be passed up in equilibrium. It is also difficult to imagine that blacks should respond to exaggerated perceptions of drug use by raising their use to meet expectations, as statistical discrimination models might predict. In contrast to decreasing human capital investment (which is at the center of most such models), increasing drug use is costly – financially, and potentially in terms of identity and criminal penalties. In the face of exaggerated beliefs about their drug use, it is not clear that non-using blacks would respond by engaging in drug use simply because it carries no labor market penalty. Therefore the biased beliefs continue to be biased, rather than generating self-fulfilling behavior. The second critique is that it is likely that one could write an alternative model to explain the results. This is almost certainly true. However, the fact that a simple model can explain a broad set of results that are at odds with other models suggests that models with similar features are worth exploring in more detail.

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 workforce 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 non-cognitive 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 is consistent with the analysis in Autor and Scarborough (2008), who showed that pre-employment skills testing does not necessarily negatively affect outcomes for groups that perform worse on average on the test. This is particularly true in the case of drug testing, since the available evidence on drug use suggests that blacks do not use drugs more than whites. The overall picture in this research suggests that testing improved access to so-called "good jobs" for blacks outside the service sector, with 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 be expected in the decades of rapidly rising inequality (Card and Dinardo, 2002). Interestingly, a study by 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 twenty polls spanning 1985-1999. They found that overall 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 groups with higher drug use rates 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>Non-durable Mfg.</td><td>9.1</td></tr>
<tr><td>Transportation</td><td>14.9</td><td rowspan="2">72.4</td><td rowspan="2">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>

Notes: Data for 1989 are from U.S. Department of Labor (1989), Tables 1 and 2. Data for 1993 are from Hartwell et. al. (1996) Table 1. Numbers in both columns refer to the share of establishments with any kind of drug testing. Note that because the 1993 sample excludes establishments with fewer than 50 employees, some of the increase in total and industry level testing shares is due to dropping a part of the sample where testing is less prevalent. Data for 1997-2006 are average shares of 22-49 year old employees in the NSDUH reporting that their employer conducts some form of drug testing.
a This number is for establishments with 10-24 employees.
b This number is for establishments with 25-99 employees.

Table 2: Drug Use Rates by Group and Decade

Drugs used in the past month:	**Any**	**Marijuana**			**Other Drugs**	
	1990-2006 Average	**1980s**	**1990s**	**2000s**	**1990s**	**2000s**
Basic Demographic Groups (Racial categories exclude Hispanics)						
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 Post-secondary	**0.13**	0.14	0.088	0.12	0.046	0.063
Some Post-second.	**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

Notes: 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. Sample is unweighted.

Table 3: Descriptive Statistics for the March CPS Sample, 1980-2006

	Overall Mean	All States, 1980-1988	Pro-Testing States, 1980-1988	Anti-Testing States, 1980-1988
Age	35.55	34.16	34.19	34.14
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)	0.43	0.42	0.43	0.36
Real hourly wage ($1990)	10.85	9.58	9.17	8.92
Log real hourly wage	2.16	2.08	2.03	2.03
In wage sample	0.73	0.72	0.71	0.74
Covered by group health	0.54	0.59	0.58	0.57
Covered by pension	0.53	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	0.48	0.39	0.36	0.39
Young (ages 18-25)	0.21	0.26	0.26	0.25
Pro-testing dummy	0.08	0.01	0.02	0.00
Anti-testing dummy	0.03	0.00	0.00	0.00
Black Subsample				
Employed	0.67	0.63	0.62	0.67
High testing industry	0.32	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	0.55	0.52	0.45	0.55
Log real hourly wage	2.02	1.95	1.81	1.99
White Subsample				
Employed	0.77	0.75	0.73	0.75
High testing industry	0.28	0.32	0.32	0.32
Employed in large firm (>500)	0.43	0.42	0.43	0.36
Covered by group health	0.56	0.59	0.58	0.57
Covered by pension	0.55	0.50	0.48	0.48
Log real hourly wage	2.20	2.11	2.07	2.04

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

Table 4: Non-user - 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**	**Post-Testing 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	**Pre-Testing 1985-1988**	**Transition 1989-1993**	**Post-Testing 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	**Pre-Testing 1985-1988**	**Transition 1989-1993**	**Post-Testing 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)

Notes: Data from National Survey on Drug Use and Health, 1985-1997. Census division testing intensity tabulated from Appendix Table A4. 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.

Table 5: Impact of Pro-Testing Legislation on Outcomes 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 x Pro	-0.003	0.015	0.016	0.028	0.017
	(0.007)	(0.012)	(0.01)	(0.009)**	(0.008)
Hispanic x Pro	-0.011	0.005	0.005	0.002	-0.026
	(0.009)	(0.01)	(0.02)	(0.024)	(0.009)**
Female x Pro	-0.004	-0.015	-0.013	-0.017	-0.011
	(0.01)	(0.008)	(0.005)**	(0.007)*	(0.009)
Young x Pro	0.001	-0.01	0.004	0.015	0.00
	(0.005)	(0.008)	(0.009)	(0.007)*	(0.008)
Low Skill x Pro	0.005	0.012	0.01	0.011	0.01
	(0.004)	(0.006)	(0.006)	(0.006)	(0.007)
Pro-Testing Law	0.016	-0.003	0.003	0.001	0.019
	(0.008)	(0.009)	(0.006)	(0.01)	(0.01)
Black	-0.163	0.039	0.073	0.004	-0.106
	(0.006)***	(0.009)***	(0.065)	(0.007)	(0.008)***
Hispanic	-0.07	0.132	-0.141	0.02	-0.101
	(0.004)***	(0.006)***	(0.069)*	(0.01)	(0.015)***
Female	-0.219	-0.191	-0.016	-0.142	-0.397
	(0.007)***	(0.005)***	(0.039)	(0.004)***	(0.005)***
Low Skill	-0.094	0.141	-0.187	-0.057	-0.178
	(0.003)***	(0.004)***	(0.05)***	(0.003)***	(0.006)***
Age	0.04	0.022	-0.004	0.04	0.07
	(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)***
State Min Wage	0.005	0.005	0.003	0.008	0.023
	(0.003)	(0.003)	(0.004)	(0.005)	(0.007)**
State UE Rate	-0.012	-0.003	0.00	-0.003	-0.005
	(0.00)***	(0.001)***	(0.001)	(0.001)***	(0.001)***
State Incarc Rate	-1.992	-1.092	-0.982	3.647	0.765
	(1.523)	(0.947)	(3.438)	(1.771)*	(3.709)
Observations	2134264	1603993	1218458	1777579	1571032

Notes: Data are from March CPS 1980-2006, IPUMS version. 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 x demographic group dummy variables, and a full set of state x cubic time trends. Standard errors clustered on state in parentheses. *** indicates significance at the .1% level, ** at 1%, and * at 5%.

Table 6: Impact of Pro- and Anti-Testing Legislation on Outcomes 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 x Pro	-0.003	0.013	0.016	0.027	0.016
	(0.007)	(0.012)	(0.01)	(0.009)**	(0.008)
Hispanic x Pro	-0.01	0.005	0.005	0.001	-0.027
	(0.009)	(0.011)	(0.02)	(0.024)	(0.009)**
Female x Pro	-0.004	-0.014	-0.013	-0.017	-0.01
	(0.01)	(0.008)	(0.005)**	(0.007)*	(0.009)
Young x Pro	0.001	-0.01	0.004	0.014	0.00
	(0.005)	(0.008)	(0.009)	(0.007)*	(0.008)
Low Skill x Pro	0.005	0.013	0.01	0.012	0.01
	(0.004)	(0.006)*	(0.006)	(0.006)*	(0.007)
Black x Anti	0.00	-0.055	-0.005	-0.04	-0.037
	(0.023)	(0.024)**	(0.012)	(0.017)*	(0.028)
Hispanic x Anti	-0.001	-0.031	-0.024	-0.065	-0.014
	(0.014)	(0.02)	(0.012)*	(0.027)*	(0.066)
Female x Anti	0.004	0.008	0.006	0.009	0.008
	(0.016)	(0.01)	(0.007)	(0.01)	(0.006)
Young x Anti	0.004	-0.01	0.002	-0.022	0.003
	(0.009)	(0.011)	(0.017)	(0.005)***	(0.013)
Low Skill x Anti	-0.009	0.018	0.008	0.023	-0.005
	(0.011)	(0.011)	(0.011)	(0.007)**	(0.011)
Pro-Testing Law	0.016	-0.003	0.003	0.001	0.019
	(0.008)	(0.009)	(0.006)	(0.01)	(0.01)
Anti-Testing Law	0.005	0.001	0.005	-0.026	-0.006
	(0.004)	(0.009)	(0.015)	(0.018)	(0.024)
Observations	2134264	1603993	1218458	1777579	1571032

Notes: Data are from March CPS 1980-2006, IPUMS version. 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 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 x demographic group dummy variables, and a full set of state x cubic time trends. Standard errors clustered on state in parentheses. *** indicates significance at the .1% level, ** at 1%, and * at 5%.

Table 7: Estimates from High Impact Subsamples

Dependent Variable:	Employed	Employed in High Test Ind.	Employed in Large Firm	Covered by Health or Pension	Log Real Hourly Wage
	i. Exclude observations from adopting states that are more than 3 years after law				
Whites Only					
Female x Pro	-0.01	-0.009	-0.009	-0.017	-0.015
	(0.009)	(0.007)	(0.005)	(0.008)*	(0.011)
Young x Pro	0.013	-0.005	0.006	0.013	-0.002
	(0.007)	(0.009)	(0.006)	(0.009)	(0.011)
Low Skill x Pro	0.003	0.009	0.01	0.002	-0.001
	(0.005)	(0.006)	(0.005)	(0.005)	(0.007)
Blacks Only					
Female x Pro	0.00	-0.006	-0.044	-0.018	-0.006
	(0.014)	(0.012)	(0.011)***	(0.013)	(0.013)
Young x Pro	-0.011	-0.009	0.006	0.003	-0.005
	(0.011)	(0.016)	(0.024)	(0.021)	(0.018)
Low Skill x Pro	0.023	0.027	0.018	0.028	0.034
	(0.01)*	(0.03)	(0.01)	(0.012)*	(0.016)*
	ii. Exclude observations from late adopting states				
Whites Only					
Female x Pro	-0.003	-0.029	-0.009	-0.023	-0.015
	(0.011)	(0.01)**	(0.005)	(0.009)*	(0.012)
Young x Pro	0.006	-0.013	0.011	0.021	0.006
	(0.01)	(0.007)	(0.012)	(0.013)	(0.018)
Low Skill x Pro	0.005	0.013	0.014	0.008	0.01
	(0.006)	(0.008)	(0.008)	(0.009)	(0.009)
Blacks Only					
Female x Pro	0.014	-0.012	-0.035	-0.015	-0.038
	(0.018)	(0.018)	(0.018)	(0.017)	(0.008)***
Young x Pro	-0.013	-0.032	0.025	0.007	-0.035
	(0.013)	(0.022)	(0.032)	(0.023)	(0.017)
Low Skill x Pro	0.029	0.038	0.017	0.041	0.033
	(0.012)*	(0.045)	(0.01)	(0.014)**	(0.018)

Notes: Data and basic specifications are the same as in Table 6. All equations estimated separates on black or white subsamples. Hispanics excluded; other races defined as white. Race-related variables in previous specifications excluded. Panel (i) excludes observations from states ever adopting drug testing legislation that are more than three years after the year of adoption. Panel (ii) omits all observations from states that adopt drug testing legislation after 1999. In both panels, all observations from never-adopting states are included. *** indicates significance at the .1% level, ** at 1%, and * at 5%.

Table 8: Impact of Pro- and Anti-Testing Legislation 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 x … *(HS White Men are omitted)*					
LS Black Men	0.005	0.044	0.044	0.048	0.041
	(0.013)	(0.024)	(0.013)**	(0.011)***	(0.018)*
HS Black Men	-0.012	-0.021	0.023	0.008	0.00
	(0.011)	(0.024)	(0.016)	(0.015)	(0.015)
LS Black Women	0.006	0.018	0.004	0.03	0.015
	(0.015)	(0.024)	(0.015)	(0.017)	(0.014)
HS Black Women	-0.016	0.002	-0.01	-0.002	-0.005
	(0.014)	(0.015)	(0.011)	(0.013)	(0.019)
LS White Men	0.016	0.018	0.011	0.01	0.012
	(0.004)***	(0.01)	(0.006)	(0.006)	(0.01)
LS White Women	-0.01	-0.01	0.002	-0.011	-0.008
	(0.01)	(0.01)	(0.007)	(0.012)	(0.01)
HS White Women	-0.001	-0.006	-0.007	-0.012	-0.006
	(0.007)	(0.007)	(0.007)	(0.005)*	(0.007)
Anti-Testing x … *(HS White Men are omitted)*					
LS Black Men	-0.032	-0.07	-0.085	-0.053	-0.126
	(0.046)	(0.044)	(0.051)	(0.06)	(0.074)
HS Black Men	-0.036	0.021	0.033	0.04	0.001
	(0.021)	(0.054)	(0.052)	(0.049)	(0.033)
LS Black Women	0.026	-0.066	0.063	-0.047	-0.013
	(0.021)	(0.043)	(0.015)***	(0.037)	(0.039)
HS Black Women	0.019	-0.06	-0.026	-0.041	-0.042
	(0.02)	(0.067)	(0.039)	(0.009)***	(0.011)***
LS White Men	0.007	0.009	0.009	0.017	-0.022
	(0.006)	(0.007)	(0.021)	(0.018)	(0.02)
LS White Women	-0.01	0.03	0.014	0.04	-0.004
	(0.005)	(0.016)	(0.017)	(0.019)*	(0.011)
HS White Women	0.01	-0.005	0.001	0.005	-0.016
	(0.015)	(0.017)	(0.01)	(0.017)	(0.012)
Observations	1869748	1422776	1065608	1575655	1386574

Notes: Data are from March CPS 1980-2006, IPUMS version. Sample is individuals ages 18-55. Hispanics excluded; other races defined as white. 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 x demographic group interactions, state-specific cubic time trends, and all relevant main effects. Standard errors clustered on state in parentheses. *** indicates significance at the .1% level, ** at 1%, and * at 5%.

Table 9: 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 x … *(HS White Men are omitted)*					
LS Black Men	0.022	-0.014	0.008	0.06	0.054
	(0.025)	(0.026)	(0.018)	(0.025)*	(0.032)
HS Black Men	0.017	-0.023	0.084	-0.016	0.024
	(0.012)	(0.024)	(0.016)***	(0.018)	(0.025)
LS Black Women	0.006	0.043	-0.079	-0.045	0.037
	(0.014)	(0.015)**	(0.025)**	(0.026)	(0.022)
HS Black Women	-0.031	0.007	-0.036	0.031	0.053
	(0.019)	(0.019)	(0.018)*	(0.016)	(0.044)
LS White Men	0.011	0.025	-0.016	-0.002	0.013
	(0.005)*	(0.018)	(0.012)	(0.01)	(0.014)
LS White Women	-0.017	0.01	-0.007	-0.01	-0.02
	(0.011)	(0.011)	(0.012)	(0.021)	(0.026)
HS White Women	-0.02	-0.006	-0.008	-0.014	-0.014
	(0.008)*	(0.018)	(0.01)	(0.005)**	(0.017)
Metro Area Drug Testing Exposure x Pro-Testing x … *(HS White Men are omitted)*					
LS Black Men	0.013	0.056	0.01	0.006	-0.01
	(0.017)	(0.016)**	(0.009)	(0.011)	(0.021)
HS Black Men	0.015	0.044	-0.015	0.044	-0.02
	(0.004)**	(0.019)*	(0.004)***	(0.014)**	(0.022)
LS Black Women	0.00	0.048	0.06	0.048	-0.024
	(0.007)	(0.01)***	(0.014)***	(0.025)	(0.011)*
HS Black Women	0.001	0.046	0.038	-0.001	-0.044
	(0.01)	(0.011)***	(0.008)***	(0.007)	(0.028)
LS White Men	0.003	0.011	0.03	0.003	-0.007
	(0.004)	(0.012)	(0.006)***	(0.007)	(0.012)
LS White Women	0.025	0.029	0.013	-0.012	-0.025
	(0.01)*	(0.009)**	(0.007)	(0.015)	(0.021)
HS White Women	0.013	0.017	0.013	-0.013	0.004
	(0.007)	(0.016)	(0.009)	(0.002)***	(0.015)
Observations	847823	651082	499655	715966	639429

Notes: Specifications include "anti' and all anti interactions, but these are not reported. This is 1980-1999 data and limited to 3 years or less after law adoption. Employment in large firm further restricted to 1988-1999. Standard errors clustered on state in parentheses. *** indicates significance at the .1% level, ** at 1%, and * at 5%.

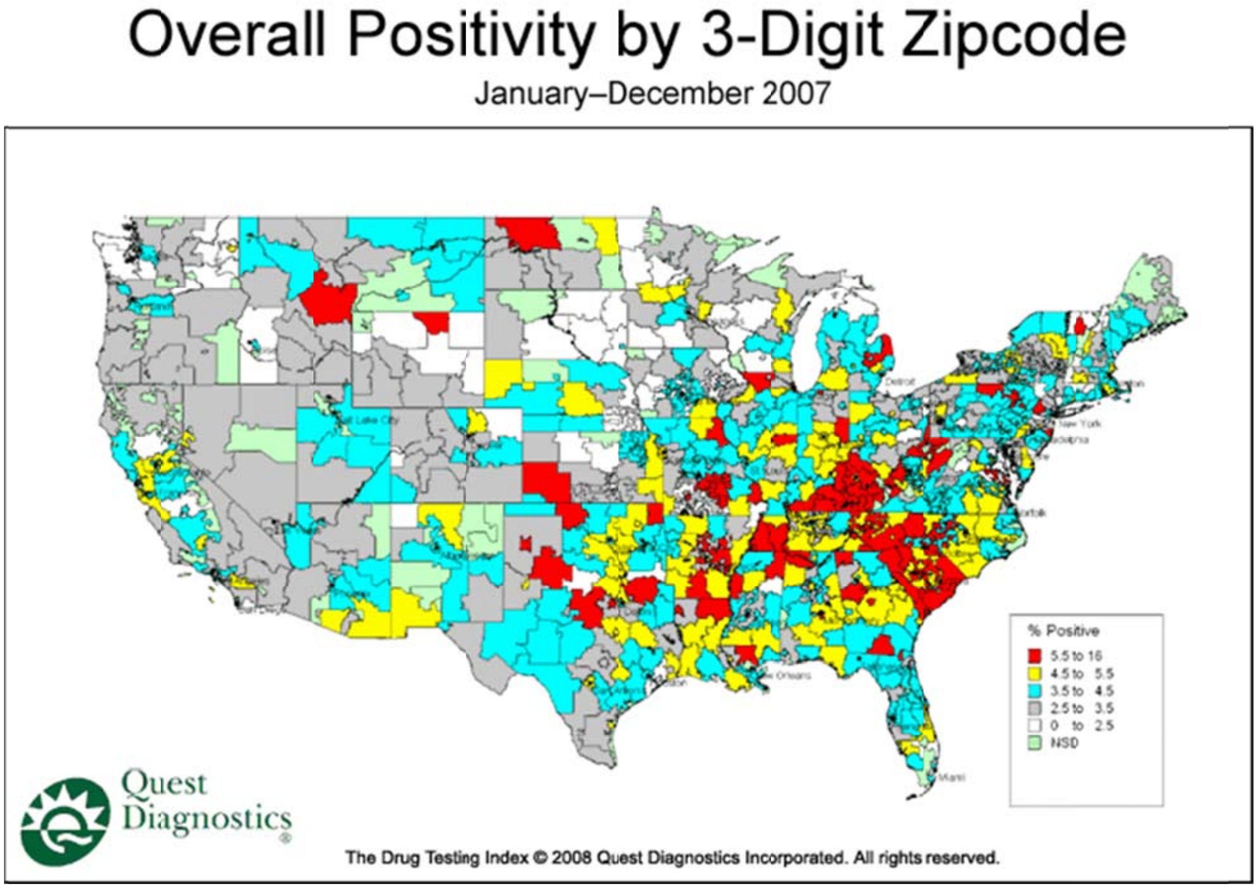

Figure 1. Source: Quest Diagnostics website. Underlying data from the Quest Diagnostics proprietary Drug Testing Index.

APPENDIX Table A1: Drug Testing Policy and Practice Timeline: Key Dates

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

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

APPENDIX Table A3: Pro- and Anti-Drug Testing States (from DeBernardo and Nieman, 2006)

State	IFDW Assessment (Earliest statute)	Legal Protection / WC Benefit to Employer	Job Applicant Testing	Random Testing	Reasonable Suspicion / For-Cause Testing	Periodic Announced Testing	Post-Accident Testing
AL	Pro (1996)	Y	M		M	M	M
AK	Pro (2002)	Y					
AZ	Pro (1995)	Y					
AR	Pro (2002)	Y	M		M	M	M
FL	Pro (2000)	Y	M	M	M	M	M
GA	Pro (1998)	Y	M	M	M	M	M
ID	Pro (1999)	Y					
IA*	Pro (1996)	Y		R	R	P	R
LA	Pro (2003)	Y					
MS*	Pro (1999)	Y	R	R		R	
OH	Pro (2001)	Y	M	M	M		M
SC	Pro (1985)	Y		M			
TN	Pro (1999)	Y	M		M	M	M
UT	Pro (2001)	Y					
CT	Anti (1996)		R	R		P	
ME	Anti (2001)		R	R	R	R	R
MN	Anti (1993)		R	R	R	R	R
MT	Anti (2005)		R	R	R	R	R
OK	Anti (1999)		R			R	R
RI	Anti (2003)		R	P	R	P	R
VT	Anti (2003)		R	P	R	P	R

Note: R=Restricted; P=Prohibited; M=Mandatory

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 five-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 fourteen states are considered pro drug testing states: Alabama, Alaska, Arizona, Arkansas, Florida, Georgia, Idaho, Iowa, Louisiana, Mississippi, Ohio, South Carolina, Tennessee, and Utah.

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 post-accident 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. The following seven states are considered anti-drug testing states: Connecticut, Maine, Minnesota, Montana, Oklahoma, Rhode Island, and Vermont. 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. Post-accident 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.

Neutral states are any states that are not classified as a pro- or anti-drug testing state. A neutral state does not receive a discount on workers' compensation premiums or receives legal protection nor does it impose restrictions on the drug testing procedures previously listed.

APPENDIX Table A4: Share of NSDUH Respondents Reporting Drug Testing by Their Employers (2002-2003 average)

Share Conducting…	Any form of drug testing	Drug testing as part of hiring	Random drug testing
US	**44.6**	**38.9**	**26.4**
Pro-Testing States			
Alabama	58.1	51.5	44.3
Alaska	44.9	34.2	31.4
Arizona	52.6	46.5	32.9
Arkansas	53.9	48.5	35.1
Florida	51.9	45.6	32
Georgia	50.1	43	31.4
Idaho	42.9	31.7	27.6
Iowa	39	33.9	23.4
Louisiana	56.5	50.3	42.7
Mississippi	55.5	49.8	41.2
Ohio	50.8	44.8	29.2
South Carolina	50.6	46.2	34.4
Tennessee	52.7	45.5	32.7
Utah	49.2	38.3	30.8
Anti-testing States			
Connecticut	42.6	38.9	22.6
Maine	25.6	20.4	17.2
Minnesota	39.8	33.4	19.2
Montana	26.2	20	18.6
Oklahoma	46.5	37.8	35.3
Rhode Island	29.6	25.2	14.1
Vermont	17.3	11.4	9
Unclassified States			
California	39.3	34.6	21.7
Colorado	43.2	34.7	24.6
Delaware	49.2	44.7	27.4
D.C.	33.3	27.9	19.7
Hawaii	37.3	32.8	23.9
Illinois	43.5	38.6	22.4
Indiana	55.4	46.8	30.1
Kansas	45.9	41	26.5
Kentucky	48.9	42.7	29.3
Maryland	41.2	35.9	24.1
Massachusetts	27.1	22.9	13.9
Michigan	45.6	40.7	21.2
Missouri	48.2	40.8	28.3
Nebraska	48.1	40.4	32.1
Nevada	54.6	50.1	31.5
New Hampshire	28.1	24.1	12.4
New Jersey	41	36.5	19.7
New Mexico	47	39.7	31.7
New York	34.1	29.4	17.6
North Carolina	55.3	49.3	35
North Dakota	37.6	25.7	27
Oregon	37	32.5	20.4

Pennsylvania	40.2	35.7	21.6
South Dakota	36.7	29.1	25
Texas	50.5	44.2	36
Virginia	40.4	35	26.8
Washington	35.4	31	20.8
West Virginia	41.8	33.9	26.5
Wisconsin	45.1	40.2	19.5
Wyoming	44.8	36.6	32.3
Cross-State Averages			
All Pro-testing	**50.6**	**43.6**	**33.5**
All Anti-testing	**32.5**	**26.7**	**19.4**
All Unclassified	**42.6**	**36.7**	**25.0**

Notes: Source: SAMHSA, Office of Applied Studies, National Survey on Drug Use and Health, 2002 and 2003. Tabulated at special request by the author. Cells are state averages of 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 workplace does 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.

THEORY APPENDIX

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 non-using 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 non-testing 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 $\boldsymbol{s}$*:*

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, Ch. 6, 1994). If drug use were closely related to underlying skills, we might expect alternative methods of detecting it to prove more useful. Also, the Conference Board study reports that supervisors are commonly advised not to try to guess at drug use among their employees but rather to look for specific changes in performance before ordering testing (Conference Board, 1990).

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 $\mathbf{s}$ 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 non-users in the population; N_1 is the number of users. For $\delta, \delta' \epsilon\ [0,1]$ and $(\delta) = (1-\delta)N_1 \big/ N_0 + (1-\delta)N_1$, simple algebra shows that $p(\delta) > p(\delta')\ \forall\ \delta < \delta'$, which would imply $p > \tilde{p}$.

Hence $p_{Mi} > \tilde{p}_{Mi} \forall i = 0,1$.[43] Thus precision in selecting non-users from among the same pool of workers increases under testing, as asserted in the main text.

[43] It is possible that changes in drug use rates or changes in the composition of the sectoral applicant pool may change the drug use rate in this population such that $p < \tilde{p}$. I discuss this possibility in the Theory Appendix.

Note that higher post-testing wages *may* attract more users to the testing sector since they still have a positive probability of evading detection. In this case, I assume firms will correctly evaluate the overall use rates in their workforce via total output even if they cannot detect individual drug use with certainty. They will therefore adjust wages downward when faced with higher shares of users in their workforce, which will in turn lower the share of users attempting to work in the sector. In this way, a new post-testing equilibrium share of users in the labor supply to the testing sector is maintained.

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 non-testing 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 non-using segments of the population, and therefore even the size of the testing sector is unclear without additional assumptions about how the skill price change and detection jointly affect the sector choices of drug users.