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DISCRIMINATION AND THE EFFECTS OF DRUG TESTING ON BLACK EMPLOYMENT

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ABSTRACT

Nearly half of U.S. employers test job applicants and workers for drugs. A common assumption is that the rise of drug testing must have had negative consequences for black employment. However, the rise of employer drug testing may have benefited African-Americans by enabling non-using blacks to prove their status to employers. I use variation in the timing and nature of drug testing regulation to identify the impacts of testing on black hiring. Black employment in the testing sector is suppressed in the absence of testing, a finding which is consistent with ex ante discrimination on the basis of drug use perceptions. Adoption of pro-testing legislation increases black employment in the testing sector by 7-30% and relative wages by 1.4-13.0%, with the largest shifts among low skilled black men. Results further suggest that employers substitute white women for blacks in the absence of testing.

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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 vilified both within the US and abroad, and it is often declared a failure, as in the face of these efforts drug use has risen over the period (Provine 2007 and Baum 1997). 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 (Western 2006, Provine 2007, and Alexander 2010). A quiet companion to the drug war has been the increased use of drug testing within mainstream American society. U.S. employers began requiring drug tests of their employees and job applicants on a large scale in the 1980s. Drug tests are now routinely required of job applicants and employees. In a comprehensive 1994 report on workplace drug testing, the National Research Council remarked that "[i]n a period of about 20 years, urine testing has moved from identifying a few individuals with major criminal or health problems to generalized programs that touch the lives of millions of citizens." (National Research Council, 1994, p. 180). According to the U.S. Department of Health and Human Services, 45% of employees in the U.S. now work for firms that conduct some form of drug testing (see Appendix Table A1), while 15-20% report having been tested themselves (Fendrich and Kim, 2002).

A common assumption is that the rise of drug testing must have had negative consequences for black employment. This is perhaps because employment screens are typically thought to disadvantage minorities. Autor and Scarborough (2008) note widespread concern that another form of screening, skills testing, would harm black applicants. The assumption might also stem from the sense that African-Americans have been disproportionately targeted by many drug policies. However, contrary to what one might expect, the rise of employer drug testing may have *benefited*

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

African-Americans. Drug testing provided a means for non-using blacks to prove their status to employers, even as the drug war linked blacks with drug use in the popular imagination.

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

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

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

The approach in this paper entails limitations. The most significant of these is that it is impossible to observe the actual testing behavior of employers. It is also difficult to determine whether testing had impacts on use rates or productivity. Nevertheless, the findings have important implications for our understanding of labor market discrimination, which in turn have implications for how to address it. These suggest we should take the paper's findings seriously but also continue to investigate the impact of drug-testing on minorities. Specifically, I find evidence consistent with bias in hiring that is not purely taste-based. This suggests an opportunity for improving black outcomes by improving employer information about black job applicants. My interpretation of these findings is more flexible than that offered by canonical statistical discrimination models.[2] The model allows that employers may operate without racial animus, conditional on their beliefs, but these beliefs may be biased. This implies a type of discrimination that is very close to the implicit discrimination described in Bertrand et al. (2005). This is also consistent with evidence from other social sciences and with new evidence from experimental economists. Sociologists and political scientists have both long argued that certain behaviors can become "racialized"– that is, disproportionately associated with a particular group. Beckett et al. (2005) and Gilens 1996 are examples. More recently, Albrecht et al. (2011) find that participants in a laboratory labor market experiment fail to fully update beliefs about individual productivity when this is revealed subsequent to learning that individuals belong to groups with different levels of average productivity.[3]

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

Finally, this paper adds to the set of studies that directly examines employer responses to changes in the information they receive. These include Holzer et al (2006); Stoll and Bushway (2008); Finlay (2009); and Autor and Scarborough (2008). The first three focus on the impact of criminal background information on hiring of ex-offenders and blacks. Autor and Scarborough

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

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

(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 impact of alternative screening technologies may not be uniform across technologies. More research is needed to understand how the impacts of a variety of new screening technologies are unfolding in the labor market.

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 inexpensive screening devices; and rising public anxiety over the prevalence of drugs in society. These culminated in the creation of federal incentives for workplace drug testing.[4] The early 1980s were a period in which small numbers of employers, albeit often large ones, began requiring drug tests of their employees in an atmosphere of legal uncertainty. Litigation by tested employees was common. In 1987, an executive order by Ronald Reagan requiring that federal agencies adopt testing to establish "drug free workplaces" went into effect. The 1988 Drug Free Workplace Act went further, requiring that federal contractors adopt comprehensive anti-drug policies.[5] Employee and applicant drug testing was clearly in the spirit of this legislation. By the late 1980s, the grounds on which employers could

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

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

require testing was well-established in the courts, notably with a major Supreme Court decision in 1989 (National Research Council, 1994, Appendix B). Thus, the late 1980s constitute a turning point after which employers begin implementing drug testing programs in increasing numbers.

Recognizing the increasing prevalence of these tests, the Bureau of Labor Statistics (BLS) conducted a survey in 1988 to gauge the extent of drug testing practices among U.S. employers (U.S. Department of Labor, 1989). The findings of the report are summarized in Table 1, in the column headed "1988." A follow up to the BLS survey was conducted by outside researchers in 1993 (Hartwell, et. al. 1996). The findings of that report are summarized in the column headed "1993." The first point to take from Table 1 is that regularities in testing prevalence appear in both surveys. Larger employers are more likely to test than smaller employers; there is wide variation in rates of testing across industries; and there is regional variation, with larger shares of establishments testing in the South and Midwest than in the Northeast or West. Knudsen 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.[6] However, the share of establishments with 50 or more employees testing in 1988 was 0.16 (Hartwell et al. 1996). This rose to 0.48 by 1993, or a 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 annual National Survey on Drug Use and Health (NSDUH). The NSDUH questioned respondents about the drug testing policies of their employers starting in 1997. I calculated the shares of employed respondents replying that their employer practiced some form of drug testing.

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

The final column of Table 1 reports these shares overall and by industry.[7] The NSDUH shares indicate that drug testing increased only modestly in the period following the 1993 BLS survey. The rapid expansion of employer drug testing therefore appears to have ended by the second half of the 1990s with testing stabilized at its new, higher level.[8]

B. State Level Drug Testing Laws

During the late 1980s, 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.[9] 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 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 are anti-testing. More detail on their classification is provided in Appendix Table A1. Table A1 shows that while pro-testing states are more commonly found in the

[7] The BLS surveys omitted establishments in the agriculture and government sectors. Industry testing rates can be calculated for these in the NSDUH.
[8] The U.S. has been the clear leader in workplace drug testing, but it is worth noting that the practice is expanding in other developed countries as well (Verstraete 2001; Verstraete 2005). Estimates suggest that about 20% of employers in the UK test, and the practice is not limited to countries with restrictive drug laws.[8] In fact, Finland introduced one of Europe's more expansive pieces of drug testing legislation in the early 2000's (Lamberg et al. 2008).
[9] It is unclear from the available social history whether employers as a group were in favor of drug testing.

South, there is still considerable variation within regions. For example, Ohio is a Northern Rust Belt state, but it is also pro-testing. Utah and Montana are both inter-mountain West states but Utah adopted pro-testing legislation while Montana adopted 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.

It is difficult to demonstrate the effect of these laws empirically, since data on employer testing prevalence at the state level is nearly non-existent. However, upon special request, the agency that conducts the NSDUH survey (The Substance Abuse and Mental Health Services Administration, or SAMHSA, within Health and Human Services) agreed to tabulate respondent answers to questions about employer drug testing at the state level for the periods 2002-2003 and 2007-2009 and provide them in a table for public circulation.[10] Within the period 2002 to 2009, one state (Louisiana) adopted a pro-testing law and three (Montana, Rhode Island, and Vermont) adopted anti-testing laws. I present evidence on the impact of these changes on reported testing rates in the NSDUH in Appendix Table A3 (Online Appendix). To summarize, I find that Louisiana experienced considerable growth in reported employer testing between 2002-2003 and 2007-2009 while states that had similar initial levels of testing but did not pass pro-testing laws actually saw testing decline somewhat. The evidence in Appendix Table A3 is mixed on whether anti-testing laws curtailed reported testing. This may be because restrictions on testing are determined in part by case law (which is not captured in the classification).[11] It is partly for this reason that I report results from specifications in which anti-testing states are allowed to have separate impacts as well as those in which anti-testing states are grouped with the controls.

[10] It is not possible to obtain comparable tabulations for earlier periods, since only very recent waves of the NSDUH were designed to be representative at the state level.
[11] This classification issue is discussed in more detail in the Online Appendix.

C. Patterns and Perceptions of Drug Use

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

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

[12] Studies on underreporting typically use arrestees as a sample because this group can be compelled to submit to specimen testing. Such studies find no pattern in underreporting that differs by race (Lu et al. 2001).

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.

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). The strategy I will follow will be to solve the model in two environments: one in which drug testing is available and the other in which it is not.[13] I then derive predictions under the assumption that employers display bias against blacks in the absence of testing. In the empirical work, I examine whether the data matches the model's predictions conditional on the ex ante bias assumption.

Let firms be divided into the testing sector and the 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 regimes: the pre-testing regime, when drug testing is not available to firms, and the post-testing regime, in which all testing firms instantaneously adopt testing of all workers and job applicants.

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

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

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

Skills $\mathbf{s}$ are observable, and I assume that drug use is independent of latent skills $\mathbf{s}$ (i.e. skills in the absence of drug use) but obviously not of realized $\mathbf{s}$.[14] Testing sector firms anticipate that the total output from hiring a given set of workers—some of whom use drugs—is lower than it would be if there was no drug use. Assume for now that firms have no information about which hires are more likely to use drugs. In this case, they simply deflate offered wages by a constant probability of drug use. Thus testing sector firms offer wages w_T equal to an applicant's expected marginal productivity given the possibility of drug use, p: $w_T = k_T(1-p)s_T$ where $k_T(1-p) = \pi_T(p)$.[15] 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.[16] 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:

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

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 $\mathbf{s}$ and D_i, workers possess an observable characteristic M_i which takes the values 0 and 1. Now there are two populations of workers. For exposition, let M=1

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

[15] 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.

[16] 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 $\varepsilon_T - \varepsilon_N$.

represent blacks and M=0 represent whites. The distribution of $\mathbf{s}$ does not vary across the M groups.[17] Now consider firms' beliefs about rates of drug use in the two demographic groups. Denote these p_{M1} and p_{M0}. These may differ from true rates of use, denoted p^*_{M1} and p^*_{M0}. Without loss of generality, assume $p_{M1} > p_{M0}$. This implies that firms' productivity expectations are unequal across groups, even if firms believe the underlying skills distributions are the same, i.e. absent drug use. Firms in the testing sector will therefore offer higher wages to whites (M=0) than they will to blacks (M=1), conditional on s_T. Using the formula in (2), it is clear that 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 regime.[18]

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[19]:

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

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

Increased precision in worker screening raises the likelihood that non-users are employed in the testing sector. To see this, first notice that $\tilde{p} < p$ implies that $\pi(\tilde{p}) > \pi(p)$. The introduction of testing raises $\pi_T(p)$ in Equation 2 and leaves all other terms unchanged, unambiguously

[17] 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.

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

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

increasing $\Pr(T)$. In the Online Appendix, I show that this in turn raises the probability of employment in the testing sector rises among non-users after testing is introduced.[20]

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 testing affects the precision of firms' ex ante beliefs differentially, then testing may change relative outcomes across the two demographic groups.

A change in relative outcomes following the introduction of testing would be consistent with ex ante bias in employer beliefs about drug use, but the nature of the change in relative precision is important for interpretation. There are two possibilities. First, employers may believe their black applicants use drugs at rates equal to the true average, $p_{M1} = p_{M1}^{*}$, but because of better information about white applicants, they believe use among the white applicants they consider is lower than average, $p_{M0} < p_{M0}^{*}$. In this case, the ex ante bias corresponds to classic statistical discrimination. Testing may then improve information on black applicants relative to whites. On the other hand, employers may hold biased beliefs about black drug use rates, such that $p_{M1} > p_{M1}^{*}$ but $p_{M0} = p_{M0}^{*}$. Then testing also has the potential to reduce the disparity between perceived and actual use rates for blacks. In this case, however, the bias is driven by inaccurate employer beliefs rather than information disparities. I cannot distinguish between the two types of bias. In both cases, the probability of employment in the testing sector rises for blacks after testing is introduced.[21]

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[20] For users, the effect of testing on the probability of employment in the testing sector is actually ambiguous, as shown in the Online Appendix.
[21] It is important to note that this assumes that (relative) drug use rates are unchanged across demographic groups, but the evidence in Appendix Table A2 suggests this is a reasonable assumption as drug use among blacks and whites follow similar time trends over the decades of greatest increase in employer testing.

In sum, the model generats 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 of either kind), 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. I discuss the two possible interpretations of ex ante bias in light of the results in the conclusion.

IV. Assessing the Impact of Employer Drug Testing: Data and Empirical Models

A. Microdata Sources

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

I supplement the CPS analysis with data from the National Survey on Drug Use and Health (NSDUH). The NSDUH is a nationally representative survey of individuals aged 12 and older. It is currently conducted annually although the survey was semi-annual between 1987 and its inception in 1979. The sample size has increased considerably over time. 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 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. Unfortunately, causal analysis of testing's impacts on labor market outcomes in the NSDUH sample are limited by two features of the survey. First, it does not include geographic identifiers below the nine Census divisions. This precludes the difference-in-differences analysis I carry out in the CPS using state-year variation in drug testing legislation.[22] 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 2. Race/ethnicity is measured using indicators for Black and Hispanic.[23] Education is measured using two categories: high school dropouts and high school graduates (the low skill group) and those with any post-secondary education (the high skill group). Table 2 also summarizes employment outcomes of interest. Because the CPS does not ask about employer drug testing, 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 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.[24] Table 2 shows that the high testing sector employs about 30% of currently employed workers. The second is the dummy variable for employment at a very large firm (> 500 employees), which is only available for 1988 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

[22] Carpenter (2007) has carefully documented correlations between an individual's outcomes and the reported drug testing practices of her employer.
[23] 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 refer to the group simply as "whites."
[24] 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.

group health or pension plan.[25] These benefits are likely related to employer size and sophistication—e.g. the presence of a well-developed human resources department. The benefits coverage outcome is also interesting because it reflects a broader notion of job quality than wages alone. Table 2 shows that 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 2 also breaks out various subsamples of interest. One can also compare the characteristics of CPS respondents from states that ultimately become pro- or anti-testing. Since I exploit variation within states over time, identification does not require that the two groups of states look identical. Nevertheless, the two groups of states are largely balanced on the dimensions in Table 2. The main exceptions are racial composition and prevalence of employment at large firms.

B. Estimating Equations

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

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

available in the public NSDUH) into low, intermediate, and high testing based on division-level average testing rates calculated from Appendix Table A1 and noted in Table 3.

I then look for evidence of two phenomena. First, were non-users increasingly sorted into high testing industries over time and in higher testing regions? Second, was the shift of non-users into testing sector employment larger for blacks? A regression with high testing industry on the left hand side would require examining multiple triple (drug use x time period x testing region) and quadruple interactions (the triple interaction times race) to test these predictions. An alternative is to examine differences in adjusted high testing sector employment rates between users and non-users by time period-testing region cells. To do this, I compute residuals from a regression of high testing sector employment on controls for demographics (age, race, Hispanic ethnicity, sex, and educational attainment), demographic group-specific cubic time trends, group-specific division fixed effects, and all main effects. I then compute the difference in means for these residuals within the nine region by time period cells, subtracting the mean residual high testing sector employment of users from that for non-users. This approach is more descriptive than a regression but also more transparent.[26]

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

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

Pro_{st} is an indicator variable equal to 1 if state *s* with a pro-testing classification in DeBernardo and Nieman (2006) has enacted drug testing legislation by year *t*. Γ is a k x 1 vector of demographic characteristics, and β_1 and β_2 are k x 1 vectors of demographic group-specific coefficients. These include indicators for black, white, and Hispanic ethnicity; gender; age less than 25; and no post-

[26] 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.

secondary education (low skill). $\tilde{\Gamma}$ is identical to Γ except that age is entered directly and age-squared is included. The specification includes a typical set of DD controls when the policy variation is at the state and year level. These are state fixed effects, Θ_s; year fixed effects, Θ_t; and state time trends. The state fixed effects absorb permanent differences across states in the outcome variable, while the year fixed effects absorb common shocks to outcomes at the national level. The state-specific time trends absorb smooth changes in labor market outcomes across states over the period of the study.

y_{ist} is one of several possible labor market outcomes. These include the three proxies for employment at a likely testing firm described above. I also examine the impact of testing legislation on employment in general and on log wages, although neither is represented in the model. The estimates of interest are the coefficients in the β_1 vector. These show how log wages and the four employment variables change differentially for the demographic groups in Γ after a state adopts pro-testing legislation. Therefore these are triple differenced, or DDD, estimates.

Although Equation 4 is a common specification, it is likely inadequate for studying differential impacts of time-varying, state-level policies across demographic groups. For one thing, there are likely fixed group-specific differences across states in the outcome variable. There are also likely important changes that are common to the U.S. labor market for a demographic group as a whole over this period. An example is rising wage inequality, which increased differentially for workers according to race, gender, and skill group.[27] For these and other reasons discussed below, I estimate the following as my preferred specification:

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

To arrive at (5), two main changes were made to the specification in (4). First, group-specific state effects and group-specific cubic time trends were added. These address concerns about fixed,

[27] 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.

group-specific differences across states and non-linear, differential time trends across demographic groups noted above. As I show later, the parameter estimates of interest are unaffected by using group-specific cubic trends in place of the group-specific year effects. This speeds computation considerably. I also allow for non-linear state trends rather than imposing linear state trends as in (4).

The second change is the addition of time varying state-level controls, X_{st}. These are the state unemployment rate, state minimum wage, state incarceration rate, and annual employment growth for each of the five one-digit industries that comprise the high-testing sector.[28] These controls are added to further address specific concerns about the possible endogeneity of state drug testing statutes. The first two control for variation in state labor market conditions. It is possible that employers are less opposed to legislation related to employee screening when state labor markets are slack than when they are tight. Including controls for state labor market conditions mitigates concerns that drug testing laws reflect effects of these conditions rather than the policies themselves. Similarly, the state incarceration rate is a measure of intensity of state-level efforts to curb drug trafficking. Some state legislatures may have had a general "get tough" policy on drug offenses, leading to high drug interdiction efforts at the same time that they passed pro-testing legislation. If such interdiction efforts affected drug use or perceptions of drug use, then this might lead to changes in black employment across industries independently of employer testing policies. Finally, X_{st} includes annual employment growth in the five testing sector industries. Suppose employers are concerned about drug use among blacks but sector growth means they need to hire more from this population. Testing sector employers may then push states to adopt pro-testing legislation to better enable them to screen applicants while expanding employment. The direct controls for industry growth account for this possibility. As mentioned above, my reading of the history surrounding drug

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

testing statues suggests that these laws are driven primarily by political considerations. They are therefore likely exogenous to state labor market conditions. Consistent with this, the exclusion of X_{st} has little impact on the results I will report. I nevertheless retain X_{st} in the preferred specification for completeness.

Because the nature of drug testing legislation varied across states, I am able to expand (5) to further exploit the variation in testing environments provided by states that adopted anti-testing laws. I create a dummy variable $Anti_{st}$ that takes the values zero or one according to timing of legislation in states classified as anti-testing. The controls are the same, and interactions of $Anti_{st}$ with Γ are added along with appropriate main effects. The interactions of $Anti_{st}$ and Γ estimate separate impacts of anti-testing laws on the groups in Γ, and residents of those states no longer form part of the comparison group to identify the impact of pro-testing laws, after the anti-testing law has passed. This additional variation allows me to test whether the content – and not just the presence – of legislation matters.

Finally, I exploit differences across local labor markets within states in the likelihood of exposure to testing. These differences arise because industrial structure and the distribution of firm sizes varies across metropolitan areas within a state, but these differences are quite stable over time. The composition of the local economy therefore creates differences in the likelihood 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 shares of reported testing by industry and firm size.[29] I

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

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

normalize the index to have mean zero and standard deviation one, and incorporate it into Equation 5 by replacing the first three terms in (5) with the first seven terms in (6):

$$y_{ijst} = Pro_{st} DT_{ijst} \Gamma_{ist}{}' \gamma_1 + \tilde{\Gamma}_{ist}{}' \gamma_2 + \gamma_3 Pro_{st} + \gamma_4 DT_{ijst} + DT_{ijst} \Gamma_{ist}{}' \gamma_5 + \gamma_6 Pro_{st} DT_{ijst} + Pro_{st} \Gamma_{ist}{}' \gamma_7 + remaining\ terms\ from\ (5) + \eta_{ijst} \tag{6}$$

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

All models are estimated using a linear probability model. This facilitates the calculation of total impacts across interactions and main effects. Since the means of all dependent dummy variables are well inside the unit interval, the results are very similar when estimated via probit. When using CPS data, standard errors are clustered at the state level.

V. Results

Before moving to estimation of the empirical models, I present preliminary evidence on the impact of state-level employer drug testing policies using a simple event study analysis. I examine only one outcome - employment in a high testing industry - for the sake of conciseness. Figure 1 shows that the prevalence of this employment declined steadily over the entire data period for both blacks and whites. Consistent with the means in Table 2, blacks are more likely than whites to work in the high testing sector. The question for an event study, then, is not what happened to trends in

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.

testing sector employment as laws were phased in over time, but rather what happened to relative employment trends for blacks versus whites around the point at which a law was introduced?

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

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

Table 4 tests the first of the model's predictions: that the share of non-users employed in the testing sector increases after the introduction of testing. Panel (i) of Table 3 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. 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.[30] During the

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

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 shown in Appendix Table A1, 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 in the post-testing period. This evidence is consistent with the model's first prediction: non-users sort increasingly into high testing sector employment in times and places where testing is more common. This also confirms that drug testing provides employers with information that they use in making their hiring decisions.

To assess the model's second prediction, I consider the change in the testing sector employment gap between users and non-users separately for blacks and whites. For both groups, the gap widens in favor of 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 3 is therefore suggestive that the impacts of employer drug testing were larger and more positive for non-using blacks than non-using whites.

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

The remainder of the analysis uses variation in state drug testing legislation to generate DDD estimates of the impact of testing on relative labor market outcomes. Results from the preferred specification, Equation 5, are shown in Table 4. Here the control group is comprised of individuals in all states that have not adopted a pro-testing law. This includes states that will adopt pro-testing laws in the future, states that will or have adopted anti-testing laws, and never-adopting states. The columns report estimates from Equation 5 using different dependent variables.

The coefficients of interest are the interactions of demographic characteristics with the pro-testing law indicator. Blacks, Hispanics, women, and the low skilled all have consistently signed impacts of pro-testing legislation on the three measures of high testing sector employment. For blacks and the low skilled, the impacts are positive and of similar magnitude, showing increases of about 1 to 3 percentage points in the dummies for high-testing industry employment, large firm employment, and benefits coverage. For blacks, the positive impacts on benefits coverage and on large firm employment are significant at the 0.1% and 5% levels respectively. Log wages also increase for blacks following the adoption of a pro-testing law. The impacts on these measures are also positive for the low skilled but of about half the magnitude, with the exception of a statistically significant positive wage impact of 1.3 percent that is similar to the 1.4 percent increase for blacks. Impacts for the young (18-25) and Hispanics are economically very small and all are insignificant.

It is important to note that there is no impact on overall employment for any group. This constitutes a first robustness check. In a design like this, there is concern that the adoption of state laws corresponds to unobserved changes in state labor markets over time. Failure to find general changes in black employment (or employment for any other group) that correlate with state level testing policy changes mitigates concerns about omitted variables driving the results observed in the high-testing sector. Taken together, the results in Table 4 suggest that blacks experience larger and more consistent improvements in testing sector employment and wages following the adoption of a pro-testing law than any other group.

For women, on the other hand, the impacts of pro-testing legislation are uniformly negative. High testing industry employment, large firm employment, and benefits coverage all decline for women by about 1.5 percentage points. The point estimate on log wages is also negative for women.

The bottom rows of the table show that post-estimation tests of equality reject that the coefficients for blacks and women are the same for all measures except the employment dummy. In other words, pro-testing legislation has significantly different impacts on blacks and women.[31]

Estimates in Table 5 incorporate the policy variation from anti-testing states. The top panel shows that estimates on the *pro-testing × demographic group* interactions from Table 4 are robust to the addition of the anti-testing interactions. In fact, the point estimates and patterns of significance are essentially unchanged between Tables 4 and 5 for the pro-testing interactions. Nevertheless, the anti-testing interactions are interesting for several reasons. First, estimates for blacks are negative, economically large, and statistically significant for high-testing industry employment. This suggests that the impact of pro-testing legislation on blacks is due directly to the increased adoption of testing by employers, since the passage of laws discouraging such testing leads to opposite impacts. Importantly, t-tests reject the equality of the pro- and anti-testing interactions with black status for all three measures of testing sector employment and for wages, as shown in the bottom rows of the table. The negative impact of testing legislation on women appears to be confined to pro-testing states. There are no significant impacts – or even large point estimates – for anti-testing laws on women in Table 5. However, t-tests reject the equality of the pro- and anti-testing interactions with female status 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 A1, anti-testing states tend to have small black populations whereas pro-testing states have larger ones. Fixed and

[31] Additional covariates generally perform as expected.

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. 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.

Interactions with Hispanic and anti-testing legislation in Table 5 are uniformly negative and economically large. However, the interactions with Hispanic are never significant, and t-tests do not reject that the interactions with Hispanic are equal across pro- and anti-testing states. If these laws do have an impact on Hispanics, I am not able to precisely estimate them with the available data. Therefore I exclude Hispanics from the subsequent analysis.

To examine the separate contributions of race, skill, and gender from a different angle, I break down the black and white populations into mutually exclusive demographic groups (listed in Appendix Table A2). The equations estimated in Table 6 substitute indicator variables for these eight groups for the Mincer-style controls for demographic characteristics used in Tables 4 and 5. I drop Hispanics from the sample and divide the remaining CPS respondents into categories according to race; sex; and skill. I modify the estimating equation to include indicators for the seven groups and their interactions with pro- and anti-testing legislation. High skilled white men are omitted. All other controls are modified accordingly.

The impacts of pro-testing laws in Table 6 are even larger than in earlier specifications. This is because they combine the impacts of being black, male, and low skilled, for example, that were estimated “separately” in the Mincer-style specifications. Table 6 shows that it is low skilled blacks who experience the largest positive impacts of pro-testing legislation on their labor market outcomes. All point estimates are also positive for low skilled black women. I find that employment in the high testing sector increases by 3.8 to 4.5 percentage points for low skilled black men, relative to the same group in states that do not adopt a drug testing law. This is an increase of 9.7% for employment in a high testing industry and roughly 7-9% 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 approximately 9 to 13 percentage points between blacks in pro- versus anti-testing states, as shown in the bottom rows of the table. This implies a relative increase in high testing sector employment of about 30% for low skill blacks. The results also show a statistically and economically significant wage increase of 3.3% for low skill black men in pro-testing states. The difference relative to the same group in anti-testing states is even larger, at 13%, and also statistically significant. 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 women (both black and white), but for no other group are all three testing sector proxies unequal. Nevertheless, the general pattern identified in previous tables – in which impacts for white women are negative in pro-testing states and positive in anti-testing states – is also apparent in Table 6. Low skilled black men are also the only group in which the wage impacts of testing legislation are statistically different across the two groups of states, despite the significant coefficient on pro-testing legislation for low skilled white men in the wage equation.

In unreported results, I examined whether the wage increases observed for blacks in pro-testing states in Tables 4, 5, and 6 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 4, 5, and 6. The addition of these controls greatly reduced the coefficients on *pro-testing x black* in Tables 4 and 5. The coefficients were not statistically significant, and I could no longer reject equality of the coefficients for blacks and women (in Table 4) and blacks in pro-testing states versus anti-testing states (in Tables 5 and 6). I conclude that wage increases for blacks overall are largely explained by shifts into testing sector employment.

In Table 7, I add interactions for metropolitan area drug testing exposure to the specifications in Table 6. Because larger firms and firms in industries where testing is more common are more likely to test, and because the representation of such firms differs across metropolitan areas, I expect that the impacts of state drug testing laws may differ across metro areas within a state depending on their industry and firm size structures. As described above, I develop a simple index of testing exposure at the metropolitan area level based on data from 1997-1999. At the state level, for which I have data for a longer time period, industry and firm size composition are highly stable over time. I therefore assume that MSA-level firm size and industry structure is constant and exogenous to state drug testing laws. I treat MSA-level drug testing exposure as a fixed characteristic that may alter the impact of state level drug testing laws.

I also restrict the sample to early adopting states and to observations within three years of a state's adoption of drug testing legislation. I make these restrictions for several reasons. Most importantly, the problem of workers selecting into markets based on testing is likely more severe at the metropolitan area level than at the state level. It is much easier for workers to move between

MSAs than across state and regional boundaries. This is the main motivation for imposing the three year restriction. This kind of arbitrage is more likely the more time has passed since the law change. Also, changes in MSA coding after 1999 make matching the industry and firm size composition data to the CPS microdata more challenging, although not impossible.[32] 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 7. Coefficients on the interactions of MSA-level drug testing exposure with exclusive demographic groups are reported in the bottom panel. The results are in the second column are striking. These show that employment in high testing industries increased substantially more in high testing exposure MSAs for all black groups. The coefficients indicate the impact of moving up one standard deviation in the MSA drug testing exposure index for the indicated demographic group in a pro-testing state. This is a large change in testing exposure, but the estimated changes are also large, in the range of 4.3 to 5.4 percentage points. Consulting Table 2 again, these impacts for high testing industry employment represent an increase of 13% or more over the mean. The pattern is less consistent for the other two measures of testing sector employment, but large firm employment and benefits coverage still show relative increases for several black groups in MSAs with higher testing exposure.

Consistent with the idea that the impact of state drug testing laws might not operate exclusively through the local composition of firm size and industry, the state level impacts in the top panel are still statistically significant for some combinations of demographic groups and outcomes. In particular, low skilled black men are more likely to have benefits coverage in states with a pro-

[32] 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.

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).

C. Robustness Analysis

The potential for unobserved factors to drive policy impacts in a study of this design is always a concern. A simple way to test for the importance of these is to use a placebo data set – in which policy changes are randomly assigned – to re-estimate the main empirical models. For brevity, I focus on the specification in Column 2 of Table 6, which shows how high-testing industry employment was affected in states passing either a pro-testing law or an anti-testing law. I created a placebo data set in which states were randomly assigned law changes that match the true distribution of law changes over time and in pro-/anti-testing character. For example, three states passed pro-testing laws and one passed anti-testing legislation in 1999. In the placebo data, three states (from those not previously assigned in the round) will be randomly assigned a pro-testing law change and one an anti-testing law change in 1999.

I drew 1000 such sets of "placebo laws" and estimated the column 2, Table 6 specification on all of them. The results are plotted as a histogram in Figure 3. The x-axis shows the difference between the pro- and anti-testing interactions with black from the estimation and therefore gives the estimated pro- minus anti-testing state difference in high-testing industry employment for blacks. In other words, Figure 3 plots the effect size calculated in the bottom rows of Table 6 for each draw of the data. The placebo estimates center around zero. The true estimate of 0.105, indicated with a vertical line, is in the far right tail of the distribution. Two-sided tests reject the equality of the absolute values of the placebo and true estimates 90% of the time at the 5% level and 93.5% of the time at the 10% level. I therefore conclude that there is a strong basis for attributing causality to the policy changes in the main results. Note that the true law distribution will occasionally be drawn randomly, so it is not inconsistent with this conclusion to have some placebo estimates that are very similar to the true estimate, as happens in Figure 3.

Given that several pro-testing laws were passed in Southern states, and that the African-American population is clustered in those states, there is concern about spatial correlation in outcomes and covariates that extends beyond the boundaries of the state (Barrios et al. 2012).[33] Following the recommendations of Barrios et al. (2012), I examine whether accounting for a geographically broader pattern of spatial correlation is likely to alter inference from the main specifications. To do this, I repeat the placebo test just described but cluster standard errors of the placebo estimates at the nine Census region level. When standard errors are thus defined, I find that two-sided tests reject equality of the placebo and true estimates 85% of the time at the 5% level and 90% of the time at the 10% level. I conclude that allowing for geographically broader spatial correlation patterns would likely increase the magnitude of standard errors in the preferred estimates but is unlikely to overturn conclusions about statistical significance, given that many estimates in the main specification are significant at the 1% level or better.[34]

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

The results are shown in Appendix Table A4; more detailed discussion is provided there as well. I conclude that the inclusion of group-specific non-linear time controls is important for the

[33] Spatial correlation within state boundaries is accounted for by clustering at the state level in the main specifications.
[34] Barrios et al. (2012) provide a means for diagnosing spatial correlation, but they do not develop a formal solution for it. Instead, they recommend clustering at higher levels of geography. This solution, however, trades the inference problems related to spatial correlation for the problems of a small number of clusters (Cameron et al. 2008). I have therefore opted to leave the standard errors of the main specifications clustered at the state level and carry out the robustness check with region-level clusters as discussed in the text.

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

As a final check, I restrict the data to observations from 1990 and later. This has two advantages. First, it omits the major years of the crack epidemic and associated drug wars, which may have operated differentially over time and across states in a way that affected black employment patterns but is not fully captured by the controls.[35] Second, it aligns the data period more closely with the years of prime law passage. The cost to this change is that pre-law trends may not be well estimated for many states, due to a shortened period between the start of the data and law passage. Appendix Table A5 reports the results of this exercise. For conciseness, I report only the results for the main Table 6 estimates of interest. For the most part, results from the main analysis in Table 6 are robust to this change in the data period. Overall employment for low skilled black men is still unaffected by state employer drug testing laws. Pro-testing laws increase the share of low skilled black men in high-testing industries and large firms relative to the same group in anti-testing states, and their relative wages also increase. The p-values are above conventional levels for the wage and high testing industry employment outcomes, but for large firm employment the difference is still statistically significant. The only result that does not hold up to the change is the positive impact on

[35] This check also largely restricts analysis to a period after the passage of the 1991 Civil Rights Act. This confounding factor is less of a concern, though, because in a very thorough study, Oyer and Schaefer (2002) find that this legislation led employers to substitute away from hiring black workers in order to reduce their exposure to lawsuits. They find that the law did not lead to additional "quota hiring" of blacks in order to prove non-discrimination. I conclude that, if anything, the impact of this concurrent factor is likely to attenuate the hiring results I find for blacks in high-testing industries under my empirical design.

pension or health coverage. In Table A5, the difference in coverage for low skilled black men across the two groups of states is small, negative, and statistically insignificant.

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 30 year period.

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

I conclude that the advent of widespread drug testing benefited black workers. I further conclude that the results are consistent with discrimination against blacks by firms in the testing sector prior to the advent of drug testing. Because the information available via drug testing 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.[36] This suggests that ex ante bias arose either because employers had information about black drug use that was correct on average but imprecise relative to that for whites, or because they held beliefs about black drug use that were inaccurate relative to those for whites on average.

It is tempting to side with the first of these – ex ante statistical discrimination – and rule out inaccurate beliefs as unlikely to persist in equilibrium.[37] However, three facts lead me to be more cautious. First, drug use rates rose over the 1990s for all groups, including blacks. If drug testing allowed employers to improve the precision of their employment screening for blacks relative to whites, then the relative costs of drug use would have increased for blacks. This does not rule out the possibility that black drug use increased in the post-testing period, but if improved precision (reduced statistical discrimination) were important, it seems unlikely that black drug use would rise one-for-one with white drug use as the data show. Second, blacks were more likely than whites to be employed in the testing sector prior to the rise in testing. This casts some doubt on the statistical discrimination assumption that employers systematically had poor information about blacks relative to whites. Ultimately, more work is needed to separately identify discrimination arising from behavioral factors like racialized beliefs versus that arising from informational disparities.

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[36] Unreported results show that drug testing legislation has no impact on the state unemployment rate. If employers practice taste-based discrimination against blacks, but pro-testing (anti-testing) rules tighten (slacken) the labor market, then testing regime could affect black hiring even in the presence of taste-based discrimination.

[37] This would also be consistent with evidence of widespread statistical discrimination against blacks documented in Fryer et al. (2011).

More work is needed to confirm the findings reported here. There is a good deal the existing data cannot tell us. For example, it is still unclear how and to what extent employers responded to state laws in their screening practices. The separate impacts of state law, federal law, and state and federal legal decisions (case law) are also not understood. More work is also needed to understand whether testing has any impact on drug use. This paper represents a "first look" at the impacts of a large scale employer screening policy, but there is much more to be done to complete the picture.

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

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Table 1: Share of Establishments with a Drug Testing Program

<table>
<tr><th></th><th>1988</th><th>1993</th><th>1997-2006</th></tr>
<tr><td>Total</td><td>3.2</td><td>48.4</td><td>46.3</td></tr>
<tr><td>By Establishment Size</td><td></td><td></td><td></td></tr>
<tr><td>1-9</td><td>0.8</td><td>-</td><td>21.3</td></tr>
<tr><td>10-49</td><td>6.4</td><td>-</td><td>38.4[a]</td></tr>
<tr><td>50-99</td><td>12.4</td><td>40.2</td><td>49.3[b]</td></tr>
<tr><td>100-249</td><td>17.2</td><td>48.2</td><td rowspan="2">66.3</td></tr>
<tr><td>250-499</td><td>29.7</td><td rowspan="2">61.4</td></tr>
<tr><td>500-999</td><td>30.6</td><td rowspan="3">74.8</td></tr>
<tr><td>1000-4999</td><td>41.8</td><td rowspan="2">70.9</td></tr>
<tr><td>5000+</td><td>59.8</td></tr>
<tr><td>By Industry</td><td></td><td></td><td></td></tr>
<tr><td>Mining</td><td>21.6</td><td rowspan="2">69.6</td><td>86.0</td></tr>
<tr><td>Construction</td><td>2.3</td><td>43.5</td></tr>
<tr><td>Durable Mfg.</td><td>9.9</td><td rowspan="2">60.2</td><td rowspan="2">68.6</td></tr>
<tr><td>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: Descriptive Statistics for the March CPS Sample, 1980-2010

	Overall Mean	All States, 1980-1988	Pro-Testing States, 1980-1988	Anti-Testing States, 1980-1988
Age	35.7	34.2	34.2	34.1
Employed	0.75	0.72	0.72	0.75
High testing industry	0.28	0.33	0.32	0.32
Employed in large firm (>500)	0.44	0.42	0.43	0.36
Real hourly wage ($2000)	14.8	12.6	12.08	11.75
Log real hourly wage	2.45	2.36	2.31	2.31
In wage sample	0.73	0.72	0.71	0.74
Covered by group health	0.53	0.59	0.58	0.57
Covered by pension	0.52	0.50	0.48	0.48
Female	0.52	0.52	0.52	0.51
Black	0.10	0.09	0.13	0.03
Hispanic	0.13	0.10	0.06	0.02
Any postsecondary	0.49	0.39	0.36	0.39
Young (ages 18-25)	0.21	0.26	0.26	0.25
Pro-testing dummy	0.10	0.01	0.02	0.00
Anti-testing dummy	0.04	0.00	0.00	0.00
Black Subsample				
Employed	0.67	0.63	0.62	0.67
High testing industry	0.31	0.37	0.36	0.41
Employed in large firm (>500)	0.56	0.55	0.52	0.57
Covered by group health	0.54	0.59	0.55	0.63
Covered by pension	0.54	0.52	0.45	0.55
Log real hourly wage	2.32	2.23	2.09	2.27
White Subsample				
Employed	0.77	0.75	0.73	0.75
High testing industry	0.27	0.32	0.32	0.32
Employed in large firm (>500)	0.43	0.42	0.43	0.36
Covered by group health	0.55	0.59	0.58	0.57
Covered by pension	0.55	0.50	0.48	0.48
Log real hourly wage	2.50	2.39	2.35	2.31

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 3: 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 A1. 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 4: Impacts of Pro-Testing Legislation by Demographic Group

Dependent Variable:	Employed	Employed in High Test Ind.	Employed in Large Firm	Covered by Health or Pension	Log Real Hourly Wage
Black x Pro	0.00	0.016	0.02*	0.03***	0.014
	(0.006)	(0.011)	(0.01)	(0.008)	(0.008)
Hispanic x Pro	-0.007	-0.008	-0.003	-0.001	-0.02
	(0.008)	(0.01)	(0.023)	(0.027)	(0.012)
Female x Pro	0.001	-0.016	-0.014**	-0.012	-0.009
	(0.008)	(0.008)	(0.005)	(0.007)	(0.009)
Age 18-25 x Pro	0.002	-0.01	-0.003	0.009	-0.004
	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)
Low Skill x Pro	0.00	0.008	0.01	0.013*	0.013*
	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)
Pro-Testing Law	0.009	-0.005	0.006	0.001	0.026**
	(0.007)	(0.009)	(0.008)	(0.01)	(0.009)
N	2723128	2046460	1703280	2256956	1994803
Effect Size: Black - Female	-0.001	0.03	0.03	0.04	0.02
H_0: Blacks = Female (p-value)	0.99	0.05	0.01	0.002	0.11

Notes: Data are from March CPS 1980-2010, IPUMS version, and additional sources as described in text. 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 5: Impacts of Pro- and Anti-Testing Legislation by Demographic Group

Dependent Variable:	Employed	Employed in High Test Ind.	Employed in Large Firm	Covered by Health or Pension	Log Real Hourly Wage
Black x Pro	0.001	0.014	0.02*	0.029***	0.013
	(0.006)	(0.011)	(0.01)	(0.008)	(0.008)
Hispanic x Pro	-0.007	-0.009	-0.004	-0.002	-0.021
	(0.008)	(0.01)	(0.023)	(0.027)	(0.012)
Female x Pro	0.001	-0.015	-0.014**	-0.012	-0.009
	(0.008)	(0.008)	(0.005)	(0.007)	(0.009)
Young x Pro	0.003	-0.011	-0.002	0.008	-0.004
	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)
Low Skill x Pro	0.00	0.009	0.01	0.014*	0.013*
	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)
Black x Anti	0.003	-0.048*	-0.015	-0.023	-0.039
	(0.018)	(0.023)	(0.01)	(0.022)	(0.024)
Hispanic x Anti	0.002	-0.037	-0.02	-0.056	-0.043
	(0.015)	(0.031)	(0.015)	(0.029)	(0.048)
Female x Anti	0.004	0.008	0.012	0.008	0.011*
	(0.011)	(0.009)	(0.007)	(0.008)	(0.005)
Young x Anti	0.011	-0.011	0.004	-0.014*	-0.003
	(0.011)	(0.006)	(0.017)	(0.005)	(0.011)
Low Skill x Anti	-0.014	0.015	0.005	0.019**	-0.001
	(0.008)	(0.012)	(0.007)	(0.006)	(0.011)
Pro-Testing Law	0.009	-0.005	0.005	0.00	0.026**
	(0.007)	(0.009)	(0.008)	(0.01)	(0.009)
Anti-Testing Law	0.015**	0.00	0.002	-0.017	-0.014
	(0.005)	(0.006)	(0.013)	(0.011)	(0.026)
N	2723128	2046460	1703280	2256956	1994803
Effect Size: Black x Pro – Black x Anti	-0.002	0.06	0.04	0.05	0.05
H_0: Black x Pro = Black x Anti (p-val)	0.89	0.01	0.009	0.03	0.04
Effect Size: Female x Pro – Female x Anti	-0.003	-0.02	-0.03	-0.02	-0.02
H_0: Female x Pro= Female x Anti (p-val)	0.81	0.06	0.003	0.05	0.04

Notes: Data are from March CPS 1980-2010, IPUMS version and as noted in Table 4. 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 4, 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 6: Impacts by Exclusive Demographic Groups

Dependent Variable:	Employed	Employed in High Test Ind.	Employed in Large Firm	Covered by Health or Pension	Log Real Hourly Wage
Pro-Testing x … *(HS White Men are omitted)*					
LS Black Men	-0.006	0.038	0.045***	0.042***	0.033*
	(0.014)	(0.021)	(0.012)	(0.011)	(0.016)
HS Black Men	-0.004	-0.008	0.016	0.025	0.007
	(0.011)	(0.022)	(0.016)	(0.013)	(0.016)
LS Black Women	0.015	0.01	0.008	0.043***	0.014
	(0.015)	(0.02)	(0.013)	(0.01)	(0.013)
HS Black Women	-0.018	0.005	-0.007	-0.001	-0.008
	(0.012)	(0.015)	(0.011)	(0.011)	(0.014)
LS White Men	0.002	0.016	0.007	0.014*	0.015*
	(0.007)	(0.009)	(0.005)	(0.006)	(0.007)
LS White Women	-0.01	-0.008	-0.003	-0.004	-0.001
	(0.011)	(0.01)	(0.008)	(0.011)	(0.008)
HS White Women	-0.001	-0.009	-0.012*	-0.006	-0.005
	(0.007)	(0.008)	(0.006)	(0.006)	(0.007)
Anti-Testing x … *(HS White Men are omitted)*					
LS Black Men	-0.027	-0.067	-0.082	-0.049	-0.099
	(0.039)	(0.041)	(0.047)	(0.049)	(0.065)
HS Black Men	-0.02	0.003	0.021	0.049	-0.013
	(0.014)	(0.056)	(0.06)	(0.037)	(0.019)
LS Black Women	0.016	-0.049	0.045*	-0.024	-0.03
	(0.021)	(0.042)	(0.018)	(0.037)	(0.034)
HS Black Women	0.023	-0.033	-0.021	-0.012	-0.015
	(0.016)	(0.047)	(0.047)	(0.022)	(0.015)
LS White Men	0.003	0.011	0.007	0.016	-0.01
	(0.005)	(0.01)	(0.017)	(0.014)	(0.018)
LS White Women	-0.012	0.026	0.017	0.032*	0.001
	(0.007)	(0.015)	(0.012)	(0.015)	(0.013)
HS White Women	0.009	-0.002	0.007	0.006	-0.007
	(0.009)	(0.016)	(0.01)	(0.014)	(0.012)
N	2355785	1792491	1471265	1976076	1738844
Effect Size: LSBM x Pro – LSBM x Anti	0.02	0.11	0.13	0.09	0.13
H_0: LSBM x Pro = LSBM x Anti (p-value)	0.59	0.02	0.01	0.07	0.05

Notes: Data are from March CPS 1980-2010, IPUMS version. Additional data sources described in text. Sample is individuals ages 18-55. Hispanics excluded; other races defined as white. HS indicates High Skill (some post-secondary), LS Low Skill (no post-secondary). Estimation methods are the same as in Table 4. All specifications include controls for age, age^2, state-year characteristics in Table 4, 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 7: 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	-0.011	0.008	0.061*	0.07*
	(0.025)	(0.026)	(0.018)	(0.025)	(0.033)
HS Black Men	0.019	-0.022	0.083***	-0.014	0.036
	(0.012)	(0.025)	(0.016)	(0.019)	(0.027)
LS Black Women	0.006	0.045**	-0.076**	-0.044	0.045*
	(0.014)	(0.015)	(0.025)	(0.027)	(0.021)
HS Black Women	-0.03	0.007	-0.034	0.033	0.062
	(0.019)	(0.019)	(0.018)	(0.016)	(0.045)
LS White Men	0.011*	0.026	-0.016	-0.002	0.014
	(0.005)	(0.018)	(0.012)	(0.01)	(0.015)
LS White Women	-0.017	0.01	-0.009	-0.01	-0.023
	(0.011)	(0.011)	(0.012)	(0.022)	(0.027)
HS White Women	-0.019*	-0.005	-0.008	-0.013**	-0.011
	(0.008)	(0.018)	(0.01)	(0.004)	(0.019)
Metro Area Drug Testing Exposure x Pro-Testing x … *(HS White Men are omitted)*					
LS Black Men	0.013	0.054**	0.01	0.005	-0.016
	(0.016)	(0.016)	(0.008)	(0.012)	(0.021)
HS Black Men	0.015**	0.043*	-0.014***	0.042**	-0.026
	(0.004)	(0.018)	(0.004)	(0.014)	(0.023)
LS Black Women	0.001	0.047***	0.058***	0.046	-0.032**
	(0.007)	(0.01)	(0.013)	(0.025)	(0.011)
HS Black Women	0.001	0.045***	0.037***	-0.002	-0.049
	(0.01)	(0.01)	(0.008)	(0.007)	(0.028)
LS White Men	0.003	0.011	0.03***	0.003	-0.009
	(0.004)	(0.012)	(0.005)	(0.007)	(0.012)
LS White Women	0.025*	0.029**	0.014*	-0.012	-0.027
	(0.01)	(0.009)	(0.006)	(0.015)	(0.021)
HS White Women	0.012	0.017	0.013	-0.015***	-0.001
	(0.007)	(0.015)	(0.009)	(0.002)	(0.016)
Observations	831483	638829	491329	702404	628439

Notes: Specifications include "anti' and all anti interactions, but these are not reported. Sample and data are the same as in Tables 4-6 but observations are limited to 3 years or less after law adoption and to years 1980-1999. Employment in large firm further restricted to 1988-1999. Standard errors clustered on state in parentheses. *** indicates significance at the .1% level, ** at 1%, and * at 5%.

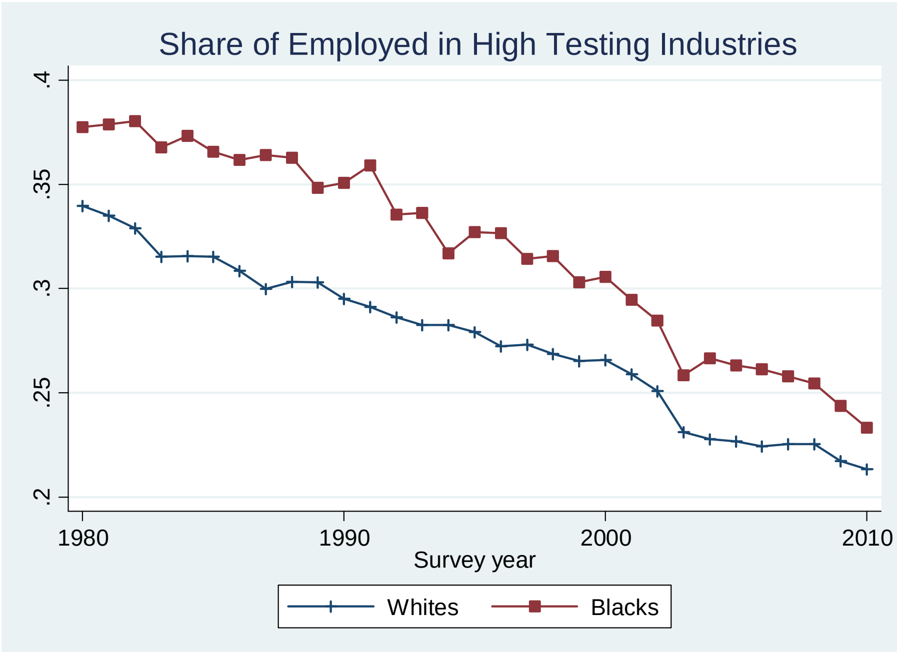

FIGURE 1. Share of employed respondents working in a high testing industry, by race. Data from the CPS ASEC Supplement (March) 1980-2010.

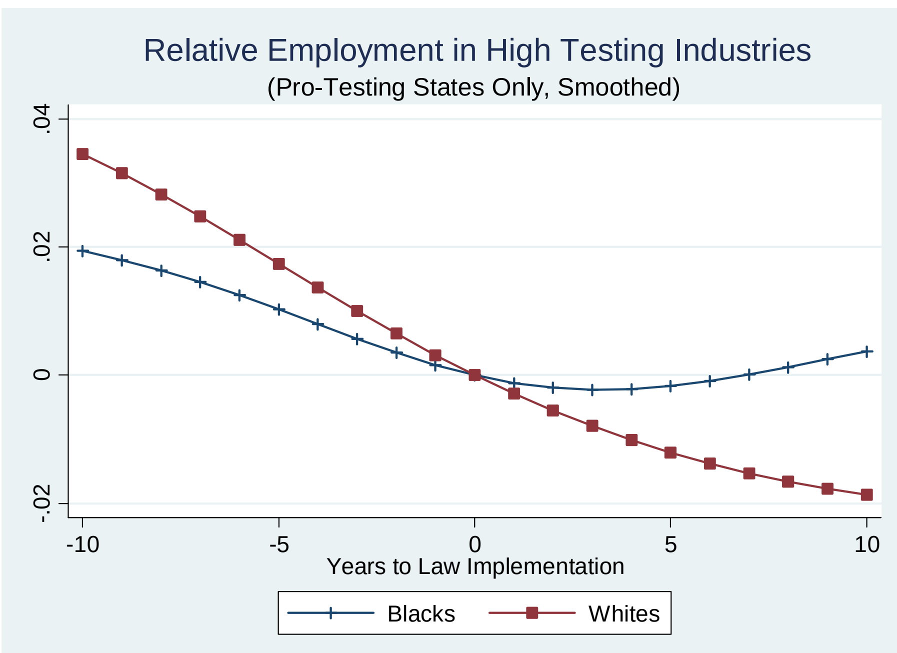

FIGURE 2a. Share of employed respondents working in a high testing industry relative to year in which a pro-testing law was passed, by race. Data from the March CPS 1980-2010. Respondents from states adopting a pro-testing law only. Y-axis is difference between share of employed in high testing industries in x-axis year and in year of passage.

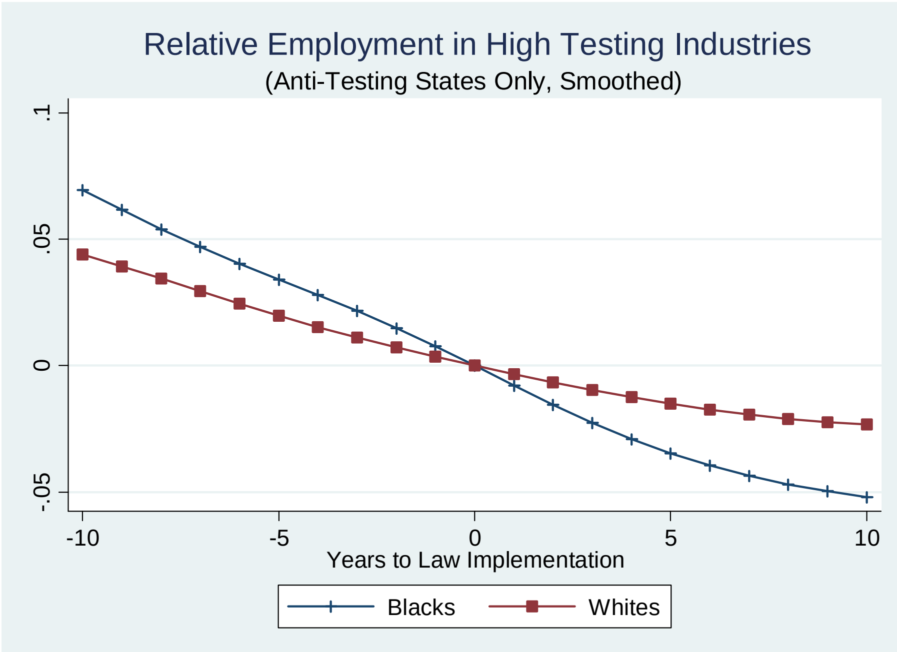

FIGURE 2b. Share of employed respondents working in a high testing industry relative to year in which an anti-testing law was passed, by race. Data from the March CPS 1980-2010. Respondents from states adopting a pro-testing law only. Y-axis is difference between share of employed in high testing industries in x-axis year and in year of passage.

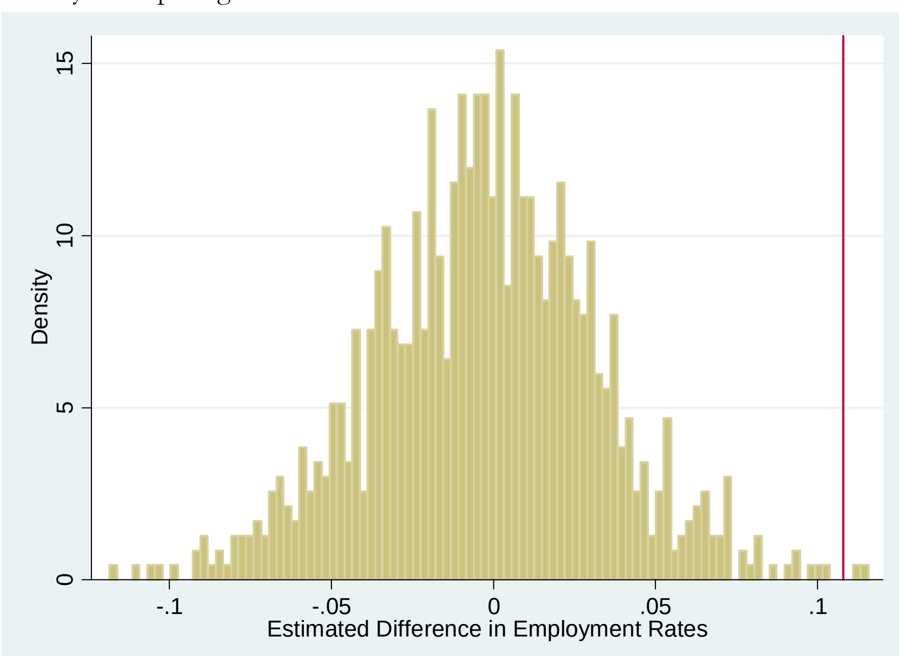

FIGURE 3: Placebo Analysis: Estimated difference in testing sector employment for low skilled black men from regressions using placebo laws. The estimated difference in the figure corresponds to the effect size in column 2 of Table 6. The figure plots differences estimated from each of 1000 draws of a law change distribution in which states are randomly assigned to "pass" laws that match the actual law change distribution in terms of years of passage, numbers of states passing in a given year, and pro-/anti-testing character of the legislation.