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IS THE CONVERGENCE IN THE RACIAL WAGE GAP ILLUSORY?

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ABSTRACT

I demonstrate that the literature on the racial wage gap has systematically overstated the gains made by African American men by ignoring their withdrawal from the labor force. Three sources of selection-bias are identified: imposing sample selection criteria based on labor supply, trimming wages on the basis of real-dollar cutoffs, and making inferences based on Current Population Survey (CPS) data whose truncated sampling design excludes the growing incarcerated population. To recover the counterfactual distribution of skill-prices for non-workers, I implement a quasi-bounds estimator that does not require the use of arbitrary exclusion restrictions for identification and find that: (1) Corrected estimates of the racial wage gap indicate a substantial role for the efficacy of the Civil Rights Act and related initiatives in affecting convergence in segregated states; ignoring selection causes estimates of convergence in the South as well as the within-cohort component of this change to be understated. (2) In contrast to the sharp convergence observed in standard wage series from 1970-90, selectivity corrected estimates indicate complete stagnation over this period with a divergence of 3.5 to 6 percentage points between 1980 and 1990. Almost half of this divergence is missed through the exclusion of the incarcerated population. The selective withdrawal hypothesis can explain 85 percent of the observed convergence between 1970 and 1990 and 40 percent of the 1960-90 convergence. (3) The disproportionate presence of highly skilled blacks in the armed forces (who are also excluded from CPS analysis) causes estimates of the racial gap to be overstated by 1 to 2 percentage points. (4) The relative increase in non-participation is a supply-side effect driven more by a massive increase in reservation wages for blacks at the bottom of the skill distribution, than by falling offer wages. (5) The significant gains made by black men during the 1960s and 1970s occured almost exclusively in the bottom offer wage decile, where significant numbers of black men were pushed out of the lowest white wage decile into higher quintiles. These gains constitute the primary location of black economic progress in the latter half of the 20th century.

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In a highly influential paper, Richard Butler and James Heckman (1977) argued that expansions in the generosity of transfer programs over the decade of the 1960s had induced lower-skilled men to withdraw from the labor force. Because African-American men were more likely to be lower-skilled, observed relative wages would increase. Therefore, a preoccupation with the wages of workers would cause social scientists to overstate the success of Title VII Legislation, or spuriously conclude that discrimination against blacks had declined. This hypothesis was used to demonstrate that Richard Freeman's landmark paper in (1973) was not consistent with the Civil Rights Act (CRA) raising the relative demand for black labor: whereas Freeman found a significant effect of Equal Employment Opportunity Commission (EEOC) expenditures on relative wages (at a time when EEOC budgets were small), Butler and Heckman argued that the selective withdrawal hypothesis could also rationalize the data.[1]

At the time of their writing, Butler and Heckman could not have anticipated the phenomenal increase in the returns to skill that would occur in the 1980s; a factor which would cause withdrawal to the extent that reservation wages were relatively fixed over this period. Nor could they have predicted the massive growth in the US prison population as a result of the "war on drugs" and the related Sentencing Reform Act of 1984 which introduced mandatory and longer sentencing guidelines for drug-related convictions. Together, these factors could generate convergence in observed wages since they disproportionately affect low-skilled blacks. Given that much of what is known on the convergence in the racial wage gap is based on a selected sample of workers, and even within that sample a group that typically meets additional criteria, it is important to understand the magnitude of possible biases that result from such sample-selection restrictions. Establishing the empirical magnitude of this hypothesized effect is the primary goal of this paper.[2] Additionally, I seek to understand the degree to which ignoring nonemployment has contaminated the measurement of factors such as schooling levels, school-quality, and discrimination in affecting the convergence. Finally, this paper decomposes the extent to which supply shifts vs. demand side forces have

[1] It is beyond the scope of this paper to review the enormous literature on the passage of the 1964 CRA and the related Voting Rights Acts of 1962 and 1965. For an introduction to this subject see the National Research Council commisioned volume *A Common Destiny: Blacks and American Society* [Jayes and Williams (1989), Chapter 6], and the rigorous reviews by Brown (1982) and Donohue and Heckman (1991). Briefly, Title VII of the CRA, which forbade discrimination in employment passed in 1964 and went into effect on July 2, 1965. Simultaneously, President Johnson's Executive Order 11246 in 1965 formed the Office of Federal Contract Complicance (OFCC), which oversaw anti-discrimination efforts in government contracts.

[2] In the spirit of this thesis, Katz and Krueger (1999) study the possibility that the 2.6 percent fall in the unemployment rate between 1985 and 1998 was a compositional effect that was driven by growing incarceration rates. Under alternative estimates of what the counterfactual labor force participation rate would be, they estimate that the true fall in the unemployment rate would have been between 2.1-2.5 percent.

contributed to the withdrawal of men from the labor-force.

This paper builds on the findings of a rich literature which has also examined this question. In one of the first tests of this hypothesis, Charles Brown (1984) adjusted aggregate Current Population Survey (CPS) data to obtain estimates of the racial wage gap that reflected nonemployment. Under the identifying restriction that *all* nonworkers earn below what the median agent earns, Brown's results attribute two-thirds of the observed convergence to the selective withdrawal of blacks from the labor force (the observed gain of 20 percent is only 7 percent when the nonemployed are accounted for). Motivated by the magnitude of Brown's results and the availability of detailed microdata, researchers have attempted to examine the empirical content of this argument in more detail. However, there is little consensus amongst the results. Of the papers that explicitly mention considering the possibility of selective withdrawal affecting the observed convergence in wages, Vroman (1986) uses the CPS-SSA matched data and finds that the selective withdrawal of blacks reduces estimates of the convergence by 25 percent; an estimate that is considerably smaller than Brown's estimate of 66 percent. The same data are used by Card and Krueger (1993) and Chay and Honore (1998) who use a sample of individuals who had earnings in multiple years and could be matched across years.[3] Under the assumption that the nonemployed earn zero dollars, Darity and Myers (1983) provide dramatically larger estimates of the role of selective withdrawal in influencing racial convergence in wages. Smith and Welch (1986) and Welch (1990) use March CPS data and do not find support for this hypothesis.[4] Blau and Beller (1992) impute wages for nonparticipants using a regression-matching estimator combined with a correction factor (to account for the fact that nonworkers differ from workers in unobservable ways), and find that the observed gains for younger blacks over the 1970s are overstated when one accounts for the nonemployed. Using a pointwise matching estimator with CPS data, Juhn (1997) finds that the selective withdrawal of blacks reduces the observed convergence by one third over 1968-88. Most recently, Chandra (2000), Johnson, Kitamura and Neal (2000), and Heckman, Lyons and Todd (2000) provide evidence that is consistent with the selective withdrawal hypothesis.

Given the enormous significance of the selective-withdrawal hypothesis for understanding

[3] Vroman (1986) and Card and Krueger (1993) reject the selective withdrawal hypothesis based on analysis using longitudinal CPS-SSA data. Vroman is criticized by Heckman (1989) for ignoring the fact that marginal black workers are not covered by social security. Vroman also demonstrates that dropouts who are transfer recipients have higher earnings than workers; however, his definition of dropouts includes those individuals who might have withdrawn because of a pure wealth effect (operating through transfers such as unemployment insurance, or Social Security payments).

[4] This is their interpretation of the results and not mine. Because of the importance of their studies, I will discuss them in more detail in the next section.

changes in the economic well-being of African Americans, as well as for the efficacy of large Federal interventions in the labor market, it is surprising to note the degree to which the existing literature does not offer a consensus estimate of the size, or even existence, of the putative effect. This paper attempts to reconcile the variance in opinions surrounding empirical studies of the selective withdrawal hypothesis. Its contributions may be summarized at five levels:

1. Nonparticipation matters: across the *entire* skill distribution, prime-age black men have withdrawn from the labor-force at rates that exceed those for comparably skilled whites. By 1990, almost 30 percent of blacks were not employed during a random reference week in the year (versus 6.1 percent for whites) and wages are not observed for 20 percent of prime age black men (7.3 percent for whites), with annual nonparticipation rates at 40 percent for certain black skill groups. Much of this withdrawal is long-term, implying that a portion of the views expressed in Smith and Welch (1986) and Welch (1990), who reject the selective-withdrawal hypothesis by focusing on workers with "marginal" attachment to the labor-force require refinement. Additionally, sample-selection criteria based on weeks worked or hours worked also generate convergence in observed wages by disproportionately excluding low-skill blacks and thereby exacerbating the bias induced by ignoring nonparticipants. For example, conditioning on Full-Time (FT) and/or a minimum level of weeks worked results in the racial wage gap "converging" by 3-5 percentage points between 1980 in 1990; not enforcing such restrictions results in estimating zero convergence over that period. Similarly, invoking miniscule "trimming" rules on the basis of real dollar cutoffs are shown to compress the racial wage gap and cause estimates of convergence over the 1960s to be understated, and of estimates over the 1980s to be overstated.

   I demonstrate the importance of not relying on inferences made on the racial wage gap from data drawn from the CPS, especially after 1980. The CPS has the advantage of producing a fairly consistent yearly time-series from 1964 onwards; however, it does not contain information on the institutionalized population. This omission overstates the convergence over time because it ignores the role of increasing criminal activity as a response to changing wage structure, as well as the degree to which tougher sentencing guidelines resulted in more men being incarcerated. Additionally, despite problems with the undercount, the Census provides a more accurate count of the Not in Labor Force (NILF) group than does the CPS. In 1990, ignoring the nonemployed will be shown to understate the racial wage gap by 11-16 percentage points; of this, 4-6 percentage points is the effect of incarceration which would be omitted by the CPS.

2. Wages for nonworkers are imputed using a technique that follows in the spirit of work by Brown (1984) and exploits later refinements by Neal and Johnson (1996) and Johnson, Kitamura and Neal (2000) in assuming that nonworkers are drawn from points on the conditional wage offer distribution that lie below that of the median respondent. This method does not rely on the presence of arbitrary exclusion restriction to identify the counterfactual distribution of wages for nonworkers. Whereas this can also

be accomplished by invoking a matching estimator and hence assuming "selection on observables," the analysis developed here combines the logic of matching estimators but retains the "selection on unobservables" flavor of traditional corrections for selection bias. Additionally, this method permits complete non-parametric identification of the standard sample-selection model and a non-parametric method to decompose the mechanisms of convergence is provided.[5]

3. Selectivity-corrected estimates of the racial wage gap indicate a substantial role for the efficacy of the CRA and related initiatives in affecting convergence, even after vintage effects generated by the retirement of older cohorts are accounted for. Ignoring non-participation in segregated states causes estimates of convergence in the 1960s to be *understated* by as much as 15 percent as a result of excluding a number of nonworking blacks in 1960 from the analysis. However, in contrast to the sharp convergence in the observed series from 1970-90, selectivity corrected estimates indicate complete stagnation over this period with a divergence of 5 percent between 1980 and 1990.

   Still pursuing the importance of samples, I find support for a theory from the Sociology literature that there is a role for the Armed Forces in compressing the racial wage gap [Mare and Winship (1984)]. The military sample is typically excluded from most analyses of labor markets because the CPS does not collect labor force data on this sample. This omission would bias empirical estimates of the racial wage gap in a manner that runs contrary to the selective withdrawal hypothesis– if the military "cream skims" the most able blacks, then including them in the analysis should raise mean and median offer wages. While the data support this view, it is not a first-order source of bias: ignoring the armed forces samples overstates the racial wage gap by 1-2 percent.

4. The recent withdrawal of black men across the skill distribution in recent years is a supply-side effect, driven by a massive relative increase in reservation wages for those in the lowest quartile of the black offer wage distribution; by 1990 blacks in the lowest quartile of the offer wage distribution had non-participation rates that were 20 percent higher than whites in the same quintile, and differences in offer wages explain 40 percent of the overall difference in participation. Over the 1960-90 period, differences in offer wages explain a declining portion of the racial gap in employment, and strengthen the empirical content of models based on blacks having higher relative returns in criminal activity, disproportionately benefiting from expansions in the disability program, or being in worse health. In 1990, wage elasticities of nonemployment imply that a 10% increase in offer wages would increase weekly participation by 2.4 percent for prime age white males who are high-school dropouts, but 3.5 percent for comparable blacks.

5. In the light of the Juhn, Murphy and Pierce (1991) thesis, that economy wide increases in wage dispersion have contributed to the slowdown in the log wage gap, I study the

[5] Donohue and Heckman (1991) and Heckman, Layne-Farrar and Todd (1996) note the first-order importance of allowing for non-linearities in the wage-schooling relationship and demonstrate that the assumption of linearity in schooling (as in the Mincerian earnings equation) contaminates inferences in Smith and Welch (1986, 1989) and Card and Krueger (1992).

extent to which blacks are positioned in white wage deciles using selectivity corrected estimates of offer wages. The results indicate that the significant gains made by black men during the 1960s and 1970s occured almost exclusively in the bottom wage decile; significant numbers of black men were pushed out of the lowest white wage decile into higher quintiles. Virtually no cross-decile convergence occured in the 1980s. Ignoring selective-withdrawal is demonstrated to paint a different economy-wide portrait of the distribution of African-American gains.

The purpose of this paper is neither to criticize earlier contributions nor to cast stones; indeed, this paper builds on the pioneering insights of the preceding literature and liberally borrows from its key findings. Throughout this paper I study outcomes for prime-age men (those aged 25-55). The study is focused on men for two reasons: first, growing incarceration and nonemployment disproportionately affects men, and second, the nature of selection mechanism determining labor force participation differs dramatically for women by race.[6] The age-restrictions were chosen to make sure that the results were not contaminated by college attendance, or at older ages, the growing phenomena of early retirement. The analysis starts in 1960 because the role of black migration from the south to the north is unimportant only after 1960 [Donohue and Heckman (1991)].

This paper is outlined as follows: Section I presents a discussion of the facts to be explained and provides evidence in favor of points (1) and (2) above. In Section II, I review the identification of the standard selection model and discuss the economic content of the commonly used pointwise matching/regression matching models that have been used to study the selective withdrawal hypothesis. I develop the bounds estimator used in this paper and demonstrate how it is nested within conventional selection models. Section III presents empirical results and Section IV offers a discussion of the potential sources of (relative) withdrawal. The Data Appendix describes standardizing assumptions that were used in order to make the census data comparable across different years, as well as the computational details of the bootstrap procedure that is utilized to recover standard-errors.

[6] Neal (2002) demonstrates that racial differences in the participation patterns of women are less likely to be motivated by differences in offer wages and posits that race differences in marriage markets and related differences in the shadow price of home production cause many white women with high offer-wages to not be at work. Including them in the analysis causes the measured wage gap for women to increase from -.18 to -.25 log points in the NLSY.

1 The Selective Withdrawal Hypothesis

1.1 Racial Differences in Incarceration and Nonemployment

Using data from the decennial census, Figure 1 describes the key facts that are central to this analysis by describing the trajectories of Employment/Population (E/P) ratios as well as relative weekly wages. All census respondents who were at work during the census reference week (including those who were self-employed or in the armed forces) are counted as being employed; those who were not in the labor force, unemployed, or institutionalized are all counted in the denominator. Relative wages are computed using respondents with incomes from wage and salary who had worked at least one week in the previous year. The first panel in Figure 1 demonstrates that not conditioning on any variables black men had weekly wages that were 48.1 percent of white men's wages in 1940. By 1990 this number had increased to 73.5 percent– a dramatic improvement of well over 50 percent over five decades, although the improvement from 1980 to 1990 was essentially nil. Figure 1 also demonstrates the phenomenal convergence in black-white earnings that occurred over the 1940s. This convergence is particularly remarkable when one notes that this period precedes the passage of *Brown vs. Board of Education* and the major Civil Rights initiatives of the 1960s.[7] In fact, there is evidence that the racial wage gap actually deteriorated slightly over the decade of the 1950s. It is apparent from this figure that the E/P ratio for prime-age blacks has fallen much faster than that for whites.

In the three other panels of Figure 1, I stratify the data by three broadly defined schooling groups. One point is immediate: inference based on aggregate time-series can be misleading; when stratified by schooling levels we see different patterns of convergence. The most "convergence" has taken place for the least skilled, as measured by those with less than a high-school degree, whereas for those with some college it has remained virtually flat since 1970.[8] Because college graduates are also the most likely to be full-time and full year work-

[7] Goldin and Margo (1992) label the 1940s as the "Great Compression," and discuss an extraordinary decade in American economic history. Their analysis identifies a number of key factors as being responsible for the convergence in wages across skill groups: period specific shifts in the structure of labor demand, wage controls imposed by the National War Labor Board, powerful unions, a rising Federal minimum wage, and a large supply of educated workers produced by the GI Bill. Margo (1995) builds on these insights in more detail in the context of the racial wage gap, and concludes that many of these factors also contributed to the closing of the racial wage gap. In addition, he suggests that Government intervention through Executive Order 8802 opened up jobs to blacks from which they were previously excluded. Margo also identifies black migration to the north and the retirement of older black cohorts as contributing factors.

[8] It is also possible to use these figures to contrast the results from computing Employment/Population ratios from the Census data to those computed from the CPS (as in the extremely important work of Juhn

ers, an analysis of the "slowdown" in convergence that focuses primarily on this group would miss the variation in behavior observed at the extensive margin of employment. These figures provide *prima facie* support for the selective withdrawal hypothesis: in the HS Dropout panel employment rates for black men are seen to have plummeted relative to those of whites. For those with more than a high-school degree, the absolute withdrawal has not been as dramatic although the relative withdrawal is sizable.[9]

In Table 1, I use Census data to document the degree to which CPS counts of the nonemployed understate the true statistics because of the sampling frame of the CPS. Table 1, Panel A displays the fraction of men in the Census reference week who were institutionalized, and Panel B adds to this fraction by also including those who were unemployed or not in the labor force (NILF) during the Census reference week.[10] Those who are not in the labor force because of being currently enrolled in school are included in Panel B, but do not contribute to the definition of being NILF (they are in the denominator but not in the numerator). The tables are stratified by 6 age x 3 schooling cells, and the columns and rows labeled 'Total' weight the individual cells by their constituent sample sizes.[11] The tables are deliberately stratified by age instead of potential-experience as with growing nonemployment over the life-cycle, the latter measure departs significantly from actual-experience. Furthermore, reliance on experience cohorts, as in Smith and Welch (1986, 1989) and Donohue and Heckman (1991), will result in pooling different birth cohorts– a combination which may be undesirable

(1992)). Between 1970 and 80, in Juhn's analysis, this ratio falls from ~0.90 to ~0.80 for black dropouts and from ~.95 to ~0.90 for white dropouts. The use of Census data suggests that this fall was from 0.80 to 0.65 for black dropouts and from 0.85 to 0.80 for white dropouts. As such, Juhn understates the strength of her central thesis.

[9] There is an important caveat to keep in mind in interpreting these figures: there have been enormous improvements in the relative quantities of black schooling. For example, the fraction of blacks (whites) with more than a HS degree grew from 11 (29) percent in 1960 to 40 (56) percent in 1990. Because of this compositional effect, blacks in 1990 with less than a high-school degree are very different from blacks in 1960 who were also high-school dropouts. Composition-adjusted estimates are computed using fixed 1975 weights and are reported in Table 7.

[10] Because of the large sample sizes available in the PUMS data the standard-errors for each of the reported means is extremely small and in the interests of conserving space I have not reported these statistics. Appendix Table 1A reports the underlying sample sizes. Standard errors for each cell will be given by $SE = \sqrt{\widehat{p}(1-\widehat{p})}/\sqrt{n}$. Using this formula, it can be noted that typical SE's ranged from 0.001-0.01.

[11] For 1990 the PUMS files of the Census do not distinguish between the incarcerated and institutionalized populations. For the purpose of making these tables consistent over time, I have combined the two categories for previous years and refer to the combined category as the incarcerated population. In 1980 the institutionalized (non-incarcerated) population was less than 0.2 percent, implying that the choice of this terminology is not a major source of bias in recent years. In 1960 and 1970 the non-incarcerated institutionalized fractions were 0.7 and 0.5 respectively. Furthermore, it can be verified that the institutionalized rates that I report in Table 1 are virtually identical to incarceration rates calculated in Western and Pettit's (2000) careful analysis (compare their Table 3 to my Table 1).

if improvements in school quality operate at the level of birth cohorts (a point which is noted in Chay and Honore (1998) and implemented in Card and Krueger (1992)). The results of these tables are particularly striking. In 1960, 4 percent of all prime age black men were incarcerated, but by 1990 that number had grown to a little over 6 percent. In examining incarceration rates for black high-school dropouts a troublesome story emerges. Between 1960 and 1990 the fraction of such men incarcerated grew by well over 200 percent. The increase in incarceration for the 'least-skilled' (those who are aged 25-35 and were also dropouts) is well over 300%, with almost a doubling of the rate over the decade of the 1980s (25% of this group were incarcerated in 1990).[12]

Almost all of the increase in incarceration rates comes from drug-related sentencing and activity. Under the Sentencing Reform Act of 1984 there were large increases in the proportion of defendants sentenced to prison time, driven largely by the more stringent sentencing guidelines as well as mandatory jail-time for drug related convictions. Using data from the *Uniform Crime Reports* I have confirmed this fact: adult drug-related arrests (involving possession, manufacture, sale, or use) rose from 322,300 in 1970 to 471,200 in 1980, and to 1.2 million in 1990. Concomitant to this trend is the fact that the number of drug related defendants in cases tried in U.S. District courts grew from 7,119 in 1980 to 20,035 in 1990. Furthermore, a Special Report from the Bureau of Justice Statistics (NCJ 171682, June 1999) states that while in 1986 Federal drug offenders could expect to serve a little under 60 percent of the prison sentence imposed (the rest being served on parole), the Sentencing Reform Act required that at least 87% of the sentence be served via prison time.

From Panel B we see that for many cells in 1990 (both white and black, but disproportionately black), over 30% of the cells were nonemployed during the census reference week. For the lowest skilled blacks, these nonemployment rates are seen to be rapidly increasing over time. By 1990, several cells had nonemployment rates in excess of 50 percent. This finding should caution researchers who want to study the racial wage gap using data from the Outgoing Rotations of the CPS, which collects data based on employment status during a very short reference period. As Panel B demonstrates, almost 30 percent of all prime-age blacks would be excluded from any analysis that solicits responses to earnings and labor sup-

[12] At the time of writing this paper, the 2000 PUMS file was still not available. However, using unpublished data from the Bureau of Justice Statistics I have compared the 1990 numbers to those for the incarcerated population on June 30th, 2000. Over this time, the overall incarceration rate for black men ages 25-29 climbed to 13.1 percent from 9.5 percent (an increase of almost 40 percent). The increase for whites is negligible (for 25-29 year olds from 1.3 percent in 1990 to 1.7 percent in 2000). As such, the bias from using the CPS has continued to grow over time.

ply questions over a random reference week. The corresponding exclusion for whites would be 11.5 percent. It is interesting to note two features of the data that are obvious from these tables. First, much like the well understood age-earnings profiles, there are pronounced age-incarceration and age-nonemployment profiles with the first being far more well-defined than the second. Second, the largest increases in nonemployment occurred between 1970 and 1980. However, the rapid growth in incarceration was a phenomenon that occurred over the 1980s.

The results of Table 1 should not be interpreted to mean that the reported fractions of men who are incarcerated or not at work also represent the fraction without legitimate wage and salary observations. Those results are reported in Appendix Table A2: Panel A. The distinction arises because weekly wages in the Census data are computed by dividing annual earnings *last year* by weeks worked *last year*, whereas the results of Table 1 refer to activity during the reference week of the census *this year*. Therefore, any respondent who worked at least one week last year for pay will have a legitimate skill price. However, the extent to which blacks are missing skill prices (as a consequence of not working even one week in the previous year) is dramatically higher than for comparable whites. In 1980 and 1990, skill prices were missing for almost 20 percent of prime-age blacks, but for approximately 7 percent of whites. The probability of annual participation is seen to be an increasing function of observable skill, suggesting that if a similarly monotone relationship describes the within-cell relationship between the probability of working and unobservable skill, the assumption of "selection on observables" would be entirely inappropriate.

1.2 The Role of the Armed Forces

In 1941, President Roosevelt issued Executive Order 8802 outlawing discrimination in defense related industries. This Order also established the Fair Employment Practices Commission (FEPC), which did not have the authority to prosecute new cases, but relied more on persuasion and the threat of presidential intervention. This initiative was followed by President Truman's Executive Order 9981 in 1948 which made the Armed Forces institute a policy of equal opportunity and treatment. Consistent with this liberal view of the Armed Forces as a nondiscriminatory employer, Table 2 reports participation in the Armed Forces from 1960-90 by education level, age and race (the fraction in the Armed Forces with less than a high-school degree is not reported as this group is non-existent and any positive counts tend to be driven by reporting error). The columns and rows labeled 'Total' therefore represent the fraction of Census respondents who reported having at least 12 years of schooling and who were also in the Armed Forces. There is a noticeable age-enlistment profile, with men of younger ages being most likely to be in the Armed Forces. A non-trivial number of blacks are in the armed forces– with typical rates for younger blacks in the order of 5 percent. Before the passage of the CRA, a significant fraction of educated blacks were in the Armed Forces and there is some evidence to suggest that they withdrew from the armed forces after the passage of the legislation: almost 10 percent of black men ages 25-29 were in the Armed Forces in 1960, but the number fell to 5.5 percent by 1970. Mare and Winship (1984) demonstrate that there was a large increase in the fraction of men of both races aged 20-23 who were enlisted between 1966-72 as a consequence of the Vietnam War. For this group both races had virtually identical enlistment rates. There was a much smaller increase for men aged 24-29; in this group black men were more likely be enlisted.

Table 2 also demonstrates that the overall share of prime-age and educated blacks in the Armed Forces has been declining over time, but is still almost twice the rate for comparably experienced whites. In the sociology literature, Mare and Winship (1984) suggest that "creaming" such blacks from the civilian labor force may be a contributory factor to observed employment disparities in the civilian labor market. To the extent that more educated men also earn more, the decline in the fraction of highly educated blacks after 1960 raises the possibility that the literature has *understated* the economic well-being of blacks at least for the pre-1965 period; a possibility that would cause one to overstate the magnitude of the effect of Title VII Legislation. The degree to which this bias matters is an empirical question and will be studied in detail in Section III.

1.3 Revisiting Smith and Welch

This section continues the focus on potential selection bias that is driven by the disproportionate exclusion of black respondents. In a series of pioneering papers, Smith and Welch (1986) and Welch (1990) argue that the selective withdrawal hypothesis is not of first-order empirical significance. Both papers match respondents to the March CPS in adjacent years and compare the earnings of workers who worked one year and not the next, or vice-versa, and do not find support for the hypotheses that these marginal workers (exiters) received lower wages than respondents who worked both years.[13] This approach, while ingenious,

[13] Smith and Welch (1986) pursue this approach but do not present detailed results. It should be noted that Welch's own results (Table 11, p S45) are consistent with the selective withdrawal hypotheses– with the exception of very young black men and those aged 55-61, both black and white exiters are found to earn less than stayers. For black men aged 25-34, exiters earned 65 percent of what stayers earned. For those aged 35-54, exiters earned approximately 56 percent of the wage of stayers. However, in interpreting his results

biases their results because of the sample inclusion criterion that the respondent be successfully matched across years: the sample omits persons who were out of the labor force in both years, moved, or those who worked one year and were incarcerated in another. By construction, these analyses identify the "marginal" worker and inferences made on such workers are biased towards zero since there is a growing fraction of men who have not worked in a long time and are therefore not close to the margin of working. Such men will be excluded from the analysis as they would have missing wage observations in both years of the match.

In Table 3, Panels A (all ages) and B (age 25-35), I perform a more direct analysis using a question on the Census which asks the currently nonemployed about when they last worked. This question was asked starting with the 1960 Census and I have standardized the responses to this question. I have also excluded individuals who were currently in school during the Census reference week in order to present a more meaningful picture of the extent of nonemployment. Notice that *at best* Welch's analysis can only capture those respondents in the top row (those who worked either this year or last) of any panel. This is an upper bound on the quality of the match since I am ignoring the possibility of not being able to match respondents who worked one year, but were incarcerated in the next year.[14] The other rows where nonparticipants last worked several years ago are excluded from their analysis by design. Such selection can be particularly problematic when the number of long-term nonemployed has been growing steadily over time. In 1980 and 1990, Welch's analysis would have excluded over 50% percent of black nonworkers and 40% of white nonworkers.

The underlying trends are troublesome: in 1960 only 3.4 percent of prime aged blacks who were currently not working, had never worked; by the 1980s and 1990s that percentage had grown threefold to 10 percent. In addition, a growing fraction of black men are classified as being long-term nonparticipants. For example, in 1990 almost 34 percent of black men had last worked 6 or more years ago. Panel B of Table 3 reports the same tabulation as Panel A

Welch concludes "...relative wages of those who leave the labor force are high enough that the changes in composition of the remaining workforce cannot conceivably be an important cause of observed increases in the relative wages of black men (p.55)." Therefore, he does not rule out the selective withdrawal hypothesis but believes that it is only an issue of "finetuning (p. S44)" the observed convergence. I am grateful to Derek Neal for suggesting the inclusion of this clarification.

[14] At Jim Smith's suggestion, I have explored the quality of matching respondents across contingent 'March' surveys of the CPS to shed further light on this approach during the decade of the 1990s. The March 1994 and 1995 could not be matched because of confidentiality induced revisions to household identifiers. For other years, a simple match based on HHID, HHNUM and LINENO yielded an average match rate of 71 percent (of those who were at risk of being matched), but only 57 percent for respondents aged 25-29. When one conditions further on having a wage observation in one of the years, the fraction falls to 39 percent. This finding confirms the extent to which selection-bias could influence the Smith and Welch findings.

but conditions on individuals aged 25-35. Over the 1970s and 1980s young men dramatically reduced their attachment to the labor force and significant fractions became part of the long-term unemployed; a lesson that reinforces the central message in Juhn (1992). These results on the growth of the long-term nonemployed will be used later to justify the estimator used to impute wages for nonparticipants.

1.4 Measuring the Racial Wage Gap: The Sensitivity to Samples

While much intellectual energy has been focused on the role of demand shifts, schooling levels, and school quality in reducing the racial wage gap, quantifying the relative importance of these factors depends critically on the overall convergence that is being explained. In Table 4, I report estimates of the sensitivity of the unadjusted *observed* racial wage gap (meaasured as the difference of log wages) to sample selection criertia based on labor supply, but still ignoring the nonemployed. The purpose of the table is to demonstrate that these criteria can affect estimates of the gap, its trajectory, and consequently, the facts to be explained. In the first column, I include all workers who worked between 1-52 weeks and note that the racial wage gap shrinks by 10 percentage points between 1960 and 1970 and again between 1970 and 1980, with no change from 1980 onwards. However, selecting those workers who worked fulltime (column 2: similar to the work of O'Neil (1990) who requires respondents to work at least 12 weeks and primarily full-time), or those who worked at least 27 weeks (column 3: as in Smith and Welch (1989)), results in a measured improvement of 3 percentage points over the 1980s. Selecting on having worked last year as well as being at work during the census reference week yields comparable results (column 5).[15] The Juhn, Murphy and Pierce (1992) sample (column 4: individuals who worked fulltime as well as at least 39 weeks in the previous year) result in estimating a convergence of almost 5 percentage points over the 1990s. Sample restrictions also affect the estimated *levels* of the racial wage gap– in comparing the levels of the gap in 1990 across columns there are differences of 3-6 percentage points in the measured gap.[16] Table 4 also reports the effects of sample restrictions on estimated medians.

[15] This restriction was implemented to simulate published tables of earnings by race, such as the U.S. Census Bureau's *Current Population Reports*, Series P-60 which are based on a sample of workers who worked last year and also during the reference week in March. Similarly, Card and Krueger (1993) require that respondents be successfully matched across four years of data and have earnings in all four years.

[16] Heckman, Lyons and Todd (2000) also note that samples matter for the study of the racial wage gap. In particular they note that the results in Smith and Welch (1986) and Card and Krueger (1992) are sensitive to the choice of samples and that different samples lead to discordance in even estimating the direction of convergence in the 1980-90 perriod. HLT also use a sample of workers who worked 1-52 weeks but do not provide a justification for this choice. Bollinger and Chandra (2001) use monte carlo simulations to

Trimming the data on the basis of real dollar cutoffs also affects estimates of the racial wage gap and this point is studied in columns 6 and 7 of Table 4. Fixed real dollar cutoffs are used in the work of Card and Krueger (1992) who restrict their sample to those respondents with weekly wages between \$35 and \$2,591 in 1979 dollars. Similarly, Donohue and Heckman (1991) require respondents to earn at least \$500 per year and between \$20-\$4000 per week and \$1-\$500 per hour. Over time, the offer wage distribution shifts to the right, implying that the imposition of a 1980 lower bound (be it a dollar cutoff or percentile) will delete a disproportionate number of African Americans in earlier years. Similarly, the upper bound will truncate high-wage whites in later years. Together, the two effects will cause estimates of the racial wage gap to be understated by causing artificial convergence in wage levels. For the purpose of illustration, I have reported the results of trimming the data at the 1st and 99th percentiles of the 1970 and 1980 wage distribution (these cutoffs translate into bounds of \$77 and \$2369 in 1970, and \$58 and \$2642 in 1980– both reported in 1997 dollars). Estimates of the racial wage gap are sensitive to both forms of trimming. Comparing the estimates of the gap from column 1 to those in columns 6 and 7 demonstrates the bounds result in reducing the estimated gap by 5-6 percentage points. Furthermore, they also reduce the amount of total wage convergence that occured over the 1960s by 25% (using 1970 bounds) and by 45% (using 1980 bounds).

The above section documents the sensitivity of the measured racial wage gap to specific sample-selection restrictions. When invoked together, the bias can be especially severe. The recent work of Couch and Daly (2000) on the convergence in the racial wage gap (which received significant news coverage in *Business Week*, November 29, 1999) can be explained entirely by sample restrictions, and exemplifies the pitfalls associated with ignoring nonparticipation and simultaneously enforcing drastic sample selection criteria. In this paper, the authors conclude that the racial wage gap "converged at a rate of 0.59 percentage points per year between 1990 and 1998. The rate of convergence for younger workers was more rapid at 1.40 percentage points per year." These conclusions are an exclusive function of the sample restrictions imposed in the paper: the authors use CPS data and retain a sample of respondents who usually worked fulltime and worked at least 39 weeks. In addition, the earnings data are trimmed at the top and bottom percentiles. Unsurprisingly, each of these restrictions will disproportionately delete young and low-skilled African Amerian men, and

demonstrate that commonly invoked trimming rules do not "clean" data and in general tend to attenuate regression coefficients for known measurement error processes in earnings data. Their analytical results demonstrate that a lot of information is required to justify the use of a trimming procedure.

ironically, lead the authors to conclude that the relative improvement in wellbeing for this group was the most significant.

In the light of this analysis, the message is that innocuous sample restrictions designed for the most part to help the researcher obtain tighter standard-errors or circumvent measurement error bias, also have the unintended consequence of generating first-order discrepancies in the magnitude and even direction of the racial wage gap. While not described in this paper, the common practice of discarding workers with imputed values for earnings or labor supply also has this effect since it deletes almost 30% of whites earning over $50,000, as well as a number of institutionalized respondents (who are disproportionately likely to be black relative to their population share). Many researchers discard observations for respondents on the grounds that measurement error in weeks worked or earnings may result in wages that are too high for such workers. This point is discussed in more detail in the Data Appendix to this paper and little support is found for the conjecture that respondents with loose attachment to the labor force have higher wages than observationally equivalent workers. For example, Appendix Table A3: Panel A demonstrates that a significant fraction of respondents had loose attachment to the labor force (as measured by the fraction working less than 14 weeks in the year). Discarding them would delete a disproportionate share of low-skill and African American respondents and therefore cause illusory convergence. However, Table A3: Panel B demonstrates that there is virtually no economically significant difference in the average weekly wages of the full sample and those who worked more than 14 weeks. This finding implies that those who worked less than 14 weeks did indeed earn more than observationally equivalent respondents and even though measurement error may still compromise the quality of resulting estimates, the bias from sample-selection will be the more dominant source of contamination.

This section has identified four potential sources of bias that have contaminated the measurement of the racial wage gap: systematically ignoring the nonemployed, ignoring the Armed Forces samples, implicitly assuming that the short-term nonemployed are comparable to the long-term nonemployed, and using sample-selection rules that delete large numbers of low-skilled African American men. The next section develops a framework which allows the effect of each potential source of bias to be quantified.

2 Econometric Statement of the Problem

To place the selective-withdrawal hypothesis in an econometrically tractable framework, I rely on the role of the distribution of equilibrium offer wages as important in measuring the racial wage gap. Begin by considering the unconditional distribution of offer wages and assume that an agent works in the formal sector if his (o)ffer wage in that sector exceeds his (r)eservation wage ($z = 1$ iff $w^o > w^r$; $z = 0$ otherwise, and $w^o = \ln(\text{offer wage})$). Using Smith and Welch's (1986) insight, one can invoke the law of total probability, and express the pointwise expectation of latent offer wages $E[w^o_{it}|X]$ for agents from race i in year t and with covariates X as:

$$E[w^o_{it}|X] = E[w^o_{it}|X, z = 1]\Pr(z = 1|X) + E[w^o_{it}|X, z = 0]\Pr(z = 0|X) \tag{1}$$

Here, $E(w^o_{it}|X, z = 1)$ is the (pointwise) mean of observed wages, and $E(w^o_{it}|X, z = 0)$ is the mean of offer wages to the nonemployed. In other words, it is the average wage offer that they would be offered if they sought employment.[17] $\Pr(z = 1|X)$ is the proportion of workers in the economy. Under the assumption that the relative demand curve is not perfectly elastic, an analysis of the racial wage gap that ignores the selective-withdrawal thesis suffers from two sources of bias: (a) underestimating $\Pr(z = 0|X)$, as would be the case if CPS data were used instead of Census data, or if trimming rules that disproportionately discard nonworkers were adopted. Or, (b) underestimating $E[w^o_{it}|X, z = 0]$, by assuming selection on observables when it is inappropriate. With census data, the only quantity not identified by the data is $E[w^o_{it}|X, z = 0]$ in (1). Therefore, the social scientist must make assumptions about the data generating process which determines this parameter and it is instructive to review the alternative approaches taken in the literature to recover this quantity:

1. The parametric selection model which allows for "selection on unobservables" is utilized in Hoffman and Link (1984). The authors use March 1980 CPS data with experience, education indicators, veteran status, region indicators, marital status, and a public/private sector indicator in the wage equation, but substitute age instead of experience in the participation probit along with omitting employment sector. They find no evidence of the selective withdrawal hypothesis for males aged 21-34, but do so

[17] The "experiment" here is to ask what is the offer wage that each nonemployed agent would get if he chose to work. Therefore, I am ignoring general equlibrium effects and not asking what the offer wage distribution would be if all nonemployed agents chose to get wage offers simultaneously. The latter experiment would shift the entire distribution of wages for workers and nonworkers in complex ways that depend on unknown elasticities of substitution. Estimating the magnitude of these general equilibrium effects is an important avenue for future research.

for those aged 35-55. [18]

2. Heckman, Lyons and Todd (2000) (henceforth HLT) use a Taylor-series expansion of the participation probit to flexibly estimate the control-function in the second stage. They use of the number of persons under the age of 18 in the household, unearned income (if available), a home ownership indicator, the interval value of that home, and state level unemployment and welfare participation rates as exclusion restrictions. It is unclear how some these variables would be constructed for the incarcerated sample since the value of one's home, or family structure, would not be defined for those in institutionalized group-quarters.[19] Furthermore, the difficulty of justifying legitimate exclusion restrictions for prime-age men cautioned me from pursuing this approach.

3. Matching is operationalized in the work of Juhn (1992, 1997), who imputes wages for non-workers in the CPS (ignoring the incarcerated sample) by conditioning on race, schooling (four categories) and experience (six five-year categories) and then assigning the wages of similar workers to those non-workers. Note that Juhn does not impose a specific functional form on the relationship between wages, experience, and schooling– her approach is entirely nonparametric. This is a great virtue of the matching approach and will be retained in the estimator proposed in this paper. The degree to which the researcher conditions on X improves the quality of the match, and it is probably the case that conditioning on experience and schooling does not adequately address the degree to which "selection on unobservables" is circumvented.[20] Unlike Juhn's analysis, identification in studies such as Blau and Beller (1992) is implicitly achieved through the use of regression matching. The assumption of linearity is more restrictive than allowing a nonparametric relationship between the offer wages and the observable

[18] Using PUMS data from 1980 and 1990 I was not able to reconstruct this result, and note that the magnitude of second-stage coefficients is extremely sensitive to the specifications used for the participation probit as well as the wage equation. In particular, the decision to use a quadratic function of age or potential experience generated very different parameter estimates. Furthermore, allowing for nonlinearities in the wage-schooling relationship as per the results in Heckman, Layne-Farrar and Todd (1996) prevented the model from converging. These results are available from the author upon request.

[19] I note that the HLT exclusion restrictions generate peculiar wage predictions for nonworkers if the incarcerated samples are used, and indicator variables are constructed to flag "value of home-missing" or "persons under 18– missing." I am grateful to Petra Todd for alerting me to the fact that the constant term in the second-stage needs to be recovered in such models. I have done so by using the notion of "identification at infinity" as developed by Heckman (1990). I have also experimented with estimating a semi-parametric model (via a Fourier Flexible Form or semiparametric estimation of the participation equation) using the HLT exclusion restrictions but my estimates were *extremely* sensitive to the choice of exclusion restrictions as well as the choice of semi-parametric correction. Specifically, small perturbations to the set of HLT restrictions generated very different results.

[20] In Juhn's model nonworkers are matched to workers by an ingenious matching algorithm: Pointwise in the above covariates each worker is reweighted to stand in for himself and a fraction of nonworkers. This is accomplished by redefining a new weight for group j: $\Psi_j = (N_j^{0-13} + N_j^{14-26})/N_j^{14-26}$. Part year workers in group j who worked 14-26 weeks now proxy for themselves, and workers who worked less than 14 weeks, as well as all nonworkers in group j. In Appendix Table A3 I demonstrate that treating workers who worked 1-13 weeks as nonworkers results in significantly reducing the sample of observed wage offers and hence, will overstating the case for the selective withdrawal hypothesis. Furthermore, from Table A3 it is also not apparent that the wages of this group are any different from other workers in their skill group; in the absense of more information, measurement error does not appear to be a first-order concern for this group.

characteristics. To allow for the possibility that nonworkers differ from workers along unobservable dimensions, Blau and Beller experiment with a deflation factor, κ such as 0.6 and 0.8. Note that the amount by which non-workers earn less than comparably skilled workers is (a) fixed over time, (b) fixed over the entire skill distribution, and (c) not estimated by the data.

Despite their intuitive appeal and independence from the use arbitrary instruments, matching estimators should be viewed with caution: recent theoretical and empirical work by Heckman, Ichimura, Smith and Todd (1998) (henceforth HIST) finds that matching estimators perform best when a rich set of conditioning variables are used. In their analysis, which utilizes experimental data from the JTPA evaluation, matching on crude demographic variables results in estimates that are severely biased. This finding has enormous implications for the willingness of social-scientists to embrace matching as a general solution to solving the selection bias problem. The cure however, is more difficult to find: most research in empirical social-science is performed on datasets such as the CPS or Decennial Census where the only covariates available to the researcher are age, years of schooling, census region of residence and race. The use of NLSY data improves matters by giving the economist access to crude measures of achievement, as measured by AFQT scores. However, important variables such as motivation, effort, ambition and tenacity which are 'observable' to a potential employer's Human Resources Department are unobservable to the econometrician.

2.1 Accounting for Nonemployment using Median Regression

The estimator developed in this paper explicitly recognizes the limitations of the kinds of data that are presently available for social-science research and is a (pointwise) nonparametric version of the approach discussed in Neal and Johnson (1996) and Johnson, Kitamura and Neal (2000). In order to control for the unobserved variables a simple identifying assumption is made. First, similar to pointwise matching estimators, I place workers and nonworkers in different cells by matching them on the basis of crude observables such as race, cohort, region and schooling. I then assume that nonworkers will earn less than the median person in that cell. This assumption is similar in spirit to that used by Brown (1984) but weaker along one dimension. Brown assumes that non-workers of a given race earn wages that are less than the median agent in that group's aggregate wage distribution. In contrast, I assume that nonworkers of a given race earn less than the median agent conditional on age and schooling.[21]

Note that I do not need an arbitrary exclusion restriction or reliance on functional form

[21] To clarify, consider the following examples: the econometrician must impute wages for (A) a nonworking 30 year old black male with a college degree, and (B) a nonworking 55 year old black male who is a high school dropout. In Brown's analysis, both persons are assumed to earn less than the median black worker.

to achieve identification. I do however, have to appeal to the *a priori* assumption that nonworkers have lower unobservables characteristics (operating through lower unobserved skill, motivation, effort, ambition and tenacity) that cause them to have lower wages than the median earner in their (pointwise) cell. The assumption underlying this estimator may also be justified by recalling the lessons learned in Lynch (1989). Lynch demonstrates that the longer one is out of work the less likely one is to find another job. This effect is shown to be much stronger for minorities than it is for whites. Given the demonstrated relationship between experience and earnings it is also safe to conclude that were the long-term unemployed men to seek employment, their offer wages would be extremely low.

2.1.1 Specifics

Johnson, Kitamura and Neal use a Mincerian wage equation in their analysis. I will not impose this restriction and will allow the returns to age and schooling to have discontinuities at all points in the skill distribution and vary by race. Therefore, for any given skill cell (which is defined by discrete year x race x education x age categories) we can illustrate the joint (latent) distribution of reservation wages and offer wages by Figure 2. In this framework, which is identical to that used in the standard parametric framework, offer wages are observed for all respondents below the 45 degree line. As drawn, I have assumed that the upper support of the pointwise distribution of offer-wages is observed. Support for this assumption is found by exploiting the pattern of between cell variation in offer wages and participation; Appendix Table A1 notes that in cells with highly skilled workers, annual participation rates approach 100 percent. Therefore, if the same pattern also persists within cells (and high wage workers also more likely to work), the upper supports of the offer wage distribution will be defined. By assuming that log offer wages for nonworkers would lie below the median cell wage, the median can be recovered. Figure 2 also demonstrates the difference between wages for workers and those for nonworkers: $E[w_{it}^o|X, z = 1]$ considerably overstates $E[w_{it}^o|X, z = 0]$ and assuming selection on observables in the absence of high-quality data will introduce considerable bias in selectivity corrected estimates.

Heckman (2001) discusses evidence confirming that it is appropriate to assume the normality of the *latent* log wage distribution implying therefore that log offer wages are normal. For the purpose of my implemention, I do not require that log-normality necessarily hold– but I do require that log offer wages are asymmetric and log normality is a sufficient condition for symmetry. Define the true (pointwise) median of the latent log offer-wage distribution

In contrast, I assume that if A were to work, he would earn less than the median person in the distribution of wages for 30 year old black males with a college degree. Similarly, B is assumed to have an offer wage that is less than the median earner in the distribution of wages for all 55 year olds who are high-school dropouts.

as:

$$\Upsilon_{50,X} = F_X^{-1}(0.50) = \inf\{x : F_X(w) \geq 0.50\} \tag{2}$$

where the X subscripts explicitly refer to the fact that we are conditioning on available covariates.[22] The pointwise racial wage gap is measured as $GAP_X = E(ln\ w^o|X, Black = 1) - E(ln\ w^o|X, Black = 0)$ and the aggregate gap requires integration over the supports of the $X's$. Under log-normality of the offer wage distribution, we are assured that the mean and median are equivalent, implying that $GAP_X = Med(ln\ w^o|X, Black = 1) - Med(\ln\ w^o|X, Black = 0)$ or equivalently, $GAP_X = ln(Med\ w^o|X, Black = 1) - \ln(Med\ w^o|X, Black = 0)$.[23]

As Figure 2 demonstrates, the method of assigning all nonworkers to below the median can result in a "classification error" where some non-workers who would actually earn more than the median are incorrectly assigned to have offer wages below the median. Of course, there will be no classification error if the distribution of reservation wages is a deterministic or constant function of the skill covariates (an assumption that is often made in search models)– in this case, all nonworkers are correctly predicted to earn less then the pointwise median respondent. Johnson, Kitamura and Neal (2000) explore this issue in more detail using NLSY data. In particular, for current nonworkers they search the data forward and backward for a wage observation to see if it is above or below the predicted median. Their results (see their Figure 1: Panel B) may be summarized as follows: in 1992 wage data were missing for 8 percent of their sample (in contrast to 8.8 percent for in my data: see Appendix Table A2). For the men for whom wages were imputed, 15% had wages reported

[22] The corresponding sample quantity is analogously defined by using $\widehat{\Upsilon}_{50,n} = F_n^{-1}(0.50) = \inf\{x : F_n(w) \geq 0.50\}$, where the empirical distribution function is defined by $F_n = n^{-1}\sum_{i-1}^{n} I_{\{W_i \leqslant w\}}$. This definition guarantees that the sample percentiles are well defined under discontinuities and nonmonotonicity of F_n.

[23] It is possible to nest this framework within the classical models of "index-sufficiency." In the HIST framework, the latent distribution of log offer wages is given by: $w_k^o = \Gamma_1(X) + \epsilon_k$. Define $I^* = \Gamma_2(R) + v_k$,where $I^* = w_k^o - w_k^r$ and v_k is independent of $\Gamma_2(R)$ and R is $[X : E]$, where R includes variables that comprise an exclusion restriction (E). Therefore, we observe offer wages if $I^* > 0$ and do not otherwise. Hence, $\Pr(z = 1|R) = F_v(\Gamma_2(R))$, implying that $\Gamma_2(R) = F_v^{-1}(\Pr(z = 1|X))$. In this class of models index sufficiency states that: $E[\epsilon|X, \Gamma_2(R), z = 1] - E[\epsilon|X, \Gamma_2(R), z = 0] = 0$. If index sufficiency holds, we can recover the wages for workers and nonworkers by using:

$$\begin{aligned} E[w|X, z &= 1] = \Gamma_1(X) + E[\epsilon|v > -\Gamma_2(R)] \\ E[w|X, z &= 0] = \Gamma_1(X) + E[\epsilon|v < -\Gamma_2(R)] \end{aligned}$$

Under log-normality of the offer wage distribution $\Gamma_1(X) = \Upsilon_{50,X}$. Therefore, all the parameters of the above equation can be identified (while allowing the workers to differ from nonworkers in unobservable ways without the use of an exclusion restriction). It permits quasi-nonparametric identification in that the functional relationship between offer wages and observable characteristics is not specified and the joint density between offer wages and reservation wages is not fully parametrized although the covariance is restricted to be between 0 and 1.

in another year that always exceeded the predicted wage; the 15% error rate falls to 10% when one only considers the non-disabled group but this is a self-reported measure of health. As such, the error in prediction is small. Note that the 15% error rate is observed for current workers who were not at work in 1991 but worked in either 1990-91 or 1993-94; Therefore, it is possible to reduce the magnitude of classification errors by assuming that the logic of assigning nonworkers to earn less than the pointwise median *only* applies if one has been out of the labor force for three or more years. To understand the magnitude of the classification error problem in the more realistic case where both offer wages and reservation wages have a bivariate distribution, I experiment with three alternative assignment rules:

1. *Median-O:* Assume that respondents who do not have a wage observation (those who worked 0 weeks last year) have equal probabilities of having offer wages above and below the pointwise median. This method assumes "selection on observables" in that non-workers have a distribution of offer wages that is identical to that of workers once observable dimensions of skill are controlled for. This estimator will be labeled *Median-O*, where the 'O' refers to the fact that it is identical to recovering the pointwise observed median.

2. *Median-NJ:* Assume that all respondents who do not have a wage observation (those who worked 0 weeks last year) have a zero probability of having offer wages above the pointwise median. This implementation follows in the spirit of the Neal-Johnson estimator and will be labeled *Median-NJ.* It differs slightly in that all respondents who are NILF but in school are assumed to have offer wages above the median. Appendix Table A2: Panel A reports the exact fraction of men in each cell for whom this assumption was made. In 1990 for example, 20% of black men did not have offer wages– all these men were placed below their respective pointwise medians.

3. *Median*: Assume that respondents who do not have a wage observation (those who worked 0 weeks last year) and who have not worked in three years have a zero probability of having offer wages above the pointwise median. Therefore, if a respondent did not work last year but worked this year, or if they last worked even two years ago they would be assumed to have equal probabilities of having offer wages above and below the pointwise median. Respondents who are NILF but in school are assumed to have offer wages above the median. This method is less stringent than *Median-NJ* and is labeled *Median.* Appendix Table A2: Panel B reports the exact fraction of men in each cell for whom this assumption was made. In 1990 for example, even though wages are missing for 20% of black men, only 12.7 percent of them were assigned wages using the median imputation rule.

By construction, *Median-O* >*Median* >*Median-NJ.* However, the "true" estimates of the racial wage gap will lie between those estimated by *Median-NJ* which assigns all nonworkers without wages to lie below the median and those estimated by *Median* since *Median* may be considered to be a realistic upper bound for a variety of reasons. First, only the very long term nonemployed are assigned to generate offer wages below the pointwise median with this estimator. Second, recall that Johnson, Kitamura and Neal detected prediction violations for only 15% of the sample of nonworkers who had loose attachment to the labor force. I am being especially conservative by placing 50% of workers with loose labor market attachment over the pointwise median and by using an expanded definition of what constitutes "loose attachment."[24] Finally, Welch (1990) demonstrates that the short-term unemployed (those who worked in the census year or preceeding year) are closer to their employed or enrolled in-school counterparts, than they are to the long-term unemployed or NILF group in terms of their propensities to be married–spouse present, living with a parent or other relative, or living alone. They are however much more likely to be unmarried or married-spouse absent, or be living with their parents than those who are employed (even within narrowly defined age categories). Despite these facts the *Median* estimator treats these short-term unemployed respondents, as well as those who worked even two years ago, as being identical to those who worked. For these reasons, results from the *Median* estimator may be thought of as representing conservative estimates of the selective withdrawal hypothesis.

3 Results

3.1 Selectivity Corrected Estimates

Table 5 presents the key empirical results. Following the results of Table 4, anyone who worked at least one week last year for wage and salary is treated as a worker. I present observed means (column 1) as well as observed medians (column 2). These were computed by taking the pointwise mean (or median) of the 4 year x 2 race x 3 education x 6 age category = 144 cells that saturate the data, and then integrating over the supports of these cells. Several features of this table are noteworthy: First, as Panel A demonstrates, the observed mean and median tell a similar story in terms of wage convergence– by 1990 the racial wage gap was -.35 log points. To the extent that differences in log offer wages approximate percentage differences, both matching (column 3) and median-O (column 4) lower that estimate by a little less than 2 percentage points (since matching computes means, the corrected results from matching should be compared to the observed means in column 1, whereas the results from median based corrections should be compared to the observed median in column 2). These results are unsurprising– both corrections simply reweight the data over the distribu-

[24] Several readers of this paper have recommended experimenting with placing only 15% of nonworkers above the median as per the results of Kitamura, Neal and Johnson (2000). The logic of randomly assigning 15% of respondents without wages to lie above the median does *not* follow directly from the results in Kitamura, Neal and Johnson (2000). More information is needed to perform such an an assignment since the rate of violations may vary across cells and assuming that it is orthogonal to measured skill is an assumption that goes beyond their results.

tion of the covariates using the full-sample instead of being restricted to the distribution of covariates for workers. They do not allow non-workers to differ from workers in unobservable ways. In columns (5) and (6) I implement median estimators that allow for selection on unobservables of the type discussed in the preceding section. The estimated wage gaps from using the *Median-NJ* estimator are larger than those obtained using *Median;* this is to be expected since *Median-NJ* places all non-workers below the pointwise median whereas *Median* only does it for the long-term unemployed. Given that the true answer lies in between these two estimators, it is reassuring that their difference is small for most of the sample period. The difference of 6 percentage points in 1990 reflects growth in the number of workers with transitional attachment to the labor force over the the1980s. The results obtained from *Median* convey a very different picture of black economic progress than those obtained by only looking at the observed series. Whereas the observed median (computed using all respondents who worked at least one week in the previous year) reports a convergence of 8.5 percentage points between 1960 and 1970, a further convergence of 8 percentage points during the 1970s and stagnation during the 1980s, accounting for the nonemployed results in a convergence of 10 percentage points over the 1960s, 4 percentage points over the 1970s and a divergence of over 3 percentage points in the 1980s. Note that these are conservative estimates for what the "observed data" report– in the light of typically invoked sample inclusion criteria reported in Table 4, most observed series would have reported convergence over the decade of the 1980s. The estimates from *Median* demonstrate that selection accounts for 38 percent of the 1960-90 convergence (the measured convergence of 0.16 log points is reduced to 0.10 log points when selection is accounted for) and 86 percent of the 1970-90 convergence.[25]

Table 5 also reports estimates of the racial wage gap by excluding two groups: in Panel B the institutionalized sample during the census reference week is excluded, and in Panel C all respondents in the armed forces during the census reference week are deleted. These exclusions were imposed to give other researchers a sense of the bias that results if CPS data are used to study the racial wage gap. The exclusion of the institutionalized sample causes the level of the racial wage gap to be understated by 4-5 percentage points in 1990

[25] I have also replicated the analysis by saturating the data with five education categories ($<$ 9 yrs of schooling, 9-11 yrs, 12, 13-15 yrs, and 16+ yrs) instead of three education categories. The results are virtually identical to those obtained in above. For example, the Median corrected estimates reported in Panel A: Column 6 changed to -0.562 in 1960, -0.460 in 1970, -0.421 in 1980 and -0.464 in 1990. I have refrained from pursuing this classification in the paper because it unclear whether reported schooling of less than 9 yrs in 1980 and 1990 is correct or dominated by measurement error. Furthermore, if the data are saturated further (for example, by allowing for respondents with 0-4 years of schooling to be in their own cell), annual nonparticipation rates exceed 50% and nullify the applicability of the Median based corrections for selection.

and the bias grows over time (comparing results in Panel A columns 5 and 6 to those in the same columns in Panel B). It can also be seen that the divergence reported in columns 5 and 6 of Panel A over the decade of the 1990s is approximately halved if the incarcerated are excluded (more precisely *Median-NJ* removes 60% and *Median* removes 40% when this sample is excluded). Panel C reports that the role of the Armed Forces in compressing the racial wage is small– the exclusion of respondents in the armed-forces causes the level of the racial wage gap to rise by about 1.5-2.0 percentage points in 1980 and 1990. The rate of convergence is not estimated to be statistically different from that obtained from Panel A for the 1960s and 1990s, although ignoring the Armed Forces does cause estimates of convergence in the 1970s to be *understated* by 20-35 percent. This understatement of the convergence is driven by relatively large numbers of young black men who enlisted in the Armed Forces during the Vietnam War. Overall however, while there is certainly support for the Mare and Winship contention that omitting the Armed Forces sample causes estimates of the gap to be overstated, the bias is not of a first-order nature.

In a series of important papers, Heckman and Paynor (1989) and Donohue and Heckman (1991) demonstrate that the thrust of Federal intervention occurred in the South, a thesis that suggests that we should see larger convergence in southern states during the decade of the 1960s. Panel D restricts the analysis to southern states. It can be seen that the 1960 gap in the South was 0.13 log points higher than that in the whole country. In comparing column (6) to column (2) we see that the observed gap shrank by 9.5 percentage points over the 1960s. However, accounting for the non-employed *raises* the convergence to 11 percentage points over this period– a result that is identically estimated by *Median* and *Median-NJ.* This happens because incorporating the nonemployed into the analysis magnifies estimates of the log wage gap in both 1960 and 1970, but by relatively more in 1960, thereby causing an understatement of overall convergence. This finding runs contrary to the original Butler-Heckman thesis: the data are consistent with a view that ignoring the nonemployment causes the analyst to overstate convergence in 1970, but this bias is greater in 1960– therefore causing the actual convergence to be understated by 14 percent (1.5 percentage points of 11 percentage points) in the South. Adjusting for selection also reduces estimates of wage convergence during the 1970s in previously segregated states: the observed convergence of 0.155 log points is reduced to 0.10-0.12 points in the selectivity corrected estimated. By 1980 most of the North-South difference in the race gap had been removed; by 1990 the levels of the gap across regions were virtually identical.

3.2 How Reasonable are these Estimates?

In Table 6, I report the underlying point estimates to better understand the strength of the identification assumptions used in this paper and to determine whether the underlying 'true' wages as determined by the alternative estimators are plausible. To focus the discussion I limit the analysis to 1980 and 1990 because it is in those years that nonparticipation becomes a central issue. For each year and by each race x education x age cell, I report three statistics for hourly wages: the observed mean, the reported median and the corrected median (as computed by the *Median* estimator). Hourly wages were computed by dividing weekly wages by the average number of hours worked by workers in that cell. I have focused on hourly wages because counterfactual hourly wages can be readily compared to known bounds for hourly wages such as the minimum wage. The point estimates reported by the Median estimator are within sensible bounds. For example, even in the cell with the highest nonparticipation rate (blacks aged 25-24 with less than a HS degree in 1990), wages are estimated to be \$4.9/ hour (in 1997 dollars). In 1989 the value of the minimum wage was \$3.35 (in current dollars) and \$4.30 in 1997 dollars. Therefore, these estimates do meet basic logical tests for consistency– they are considerably above the minimum wage. For some cells with young workers with more than a HS degree, the selectivity-corrected estimates are greater than the observed median. This occurs because of the presence of large numbers of respondents who do not have skill prices but are currently enrolled in school. Since this group is predicted to earn over the median respondents wage, in cells where there are more respondents enrolled in school than the long-term nonemployed, the corrected estimates will be larger than the observed series. As Table 6 demonstrates, such corrections are observed for young blacks aged 25-35 with more than a high-school degree in 1980 and 1990, and for comparably skilled whites in 1980 and 1990 (although the difference is only economically and statistically significant for whites in 1990).

Another feature of the data to note from Table 6 is that the distribution of observed log wages is negatively skewed– the median exceeds the mean. Ideally, in the absence of selection, the log wage (offer) distribution would have equivalent mean and median but the fact that the observed median exceeds the observed mean implies that mass has been removed from the left tail of the distribution, thereby giving it a negative skew.[26] In a model where latent

[26] Mean wages are probably overstated for cells with low participation rates and cells where large numbers of respondents worked few weeks (this is because earnings will be divided by a very small number of weeks). Furthermore Bollinger (1998), who studies the nature of measurement error in the CPS, demonstrates that low-earning workers tend to overstate their earnings. If this is true, the gap between reported mean and median wages should expand as one moves up the skill distribution, and Table 9 confirms that this is indeed true.

wages are log-normally distributed this will only happen if the selection is coming from the left tail of the pointwise distribution of offer wages. This empirical observation supports the logic of assigning nonworkers a wage below that of the median agent.

3.3 Schooling, School Quality and Discrimination

The results reported in the above section do not adjust for the fact that, at a point in time, as well as over time, whites and blacks have different levels of schooling and experience. Observers of the racial wage gap might want to decompose the sources of black economic progress into their constituent components– schooling levels, school quality and the role of discrimination. In particular, the results of Table 5 are driven by differences between whites and blacks in observable skill ("between skill differences"), and differences in unobserved skill as well as the possible contribution of discrimination ("within skill differences"). To study the contribution of these factors it is instructive to consider a non-parametric decomposition method. For those respondents with wages, we may write the observed racial (log) wage gap between blacks and whites as $\Gamma_{Obs} = E(w_{bt}|z=1) - E(w_{wt}|z=1)$ in year t. This gap may in turn be expressed as:

$$\begin{aligned}\Gamma_{Obs} &= \int E(w_{bt}|X, z=1) f(X_{bt}|z=1) dX + \int E(w_{wt}|X, z=1) f(X_{wt}|z=1) dX \\ &= \underbrace{\int E(w_{bt}|X, z=1)[f(X_{bt}|z=1) - f(X_{wt}|z=1] dX +}_{\text{Explained Difference}} \\ &\quad \underbrace{\int E(w_{bt} - w_{wt}|X, z=1) f(X_{wt}|z=1) dX}_{\text{Unexplained "Residual" Difference}} \end{aligned} \tag{3}$$

This is a non-parametric version of the familiar Blinder-Oaxaca decomposition. Its virtue is that there is *ipso facto* no problem with overlapping supports since the skill cells are constructed separately by race but using the same age x schooling combinations. Therefore, unlike the conventional decomposition where "counterfactual" wages of blacks are typically estimated by extrapolating into a region of no support, the nonparametric method does not suffer from this limitation. In the above equation, the "within" difference (or residual difference) is weighted by the white distribution of skill for workers but since this weighting is arbitrary Table 6 reports estimates with contemporaneous white and black weights separately.

When non-workers are accounted for, the above decomposition changes on two counts. First, the observed within-skill gap is now measured by the difference in the average *offer* wages for blacks and whites. Second, this gap will now be weighted by the white (or black) distribution of skill for all workers $f(X_{it})$, instead of $f(X_{it}|z=1)$.The selectivity corrected gap Γ_{Sel} with contemporaneous white weights may be expressed as:

$$\Gamma_{Sel} = \underbrace{\int E(w_{bt}|X)[f(X_{bt}) - f(X_{wt})]dX}_{\text{Explained Difference}} + \underbrace{\int E(w_{bt} - w_{wt}|X)f(X_{wt})dX}_{\text{Unexplained ``Residual'' Difference}} \qquad (4)$$

Results from the above decompositions are reported in Table 6, which reports the "residual" difference in the racial wage gap. The observed series are weighted using equation (3) whereas the corrected series reflect equation (4). The table therefore answers the following question: if whites (blacks) had the same observable characteristics as blacks (whites) what would the trajectory of the racial wage gap be? To the extent that policy makers are interested in answering questions like "what would the size of the racial wage gap be if blacks had white characteristics?" interest should be focused on tables with white weights. Estimates of the residual gap obtained using the $Median$ and *Median-NJ* estimators indicate that the residual gap comprised almost 80 percent of the observed gap in 1960. By 1970 this component of the gap had fallen to 73 percent and has marginally declined to 70 percent since then. This finding is consistent with an interpretation where decreases in discrimination or improvements in school-quality caused an improvement in black economic well being over the 1960s, but where improvements in either of these factors essentially ceased after 1970. Ignoring selection results in the residual gap comprising a much larger portion of the measured gap (75 percent in 1970 and later). When black weights are used the explained portion declines. This finding implies that the residual gap is larger in skill-cells in which blacks are concentrated (those with relatively lower education and experience), and therefore that the returns to schooling are actually higher for blacks than whites, and that the residual gap is a decreasing function of schooling levels.

The above decomposition is useful in explaining the magnitude of the racial wage gap at a point in time. To understand the trajectory of the residual gap over time, consider *changes* in the decomposition reported above: if ΔX_t represents the difference in $X's$ at time t, the total change $\Gamma_t - \Gamma_{t'}$ may be written as:

$$\begin{aligned}\Gamma_t - \Gamma_{t'} &= \underbrace{\int E(w_{bt}|X)[\Delta X_{t'} - \Delta X_t]dX + \int [E(w_{bt'} - w_{bt}|X)]\Delta X_{t'}dX}_{\text{Change in Explained Difference}} \\ &= \underbrace{\int [E(w_{bt'} - w_{wt'}|X) - E(w_{bt} - w_{wt}|X)]f(X_{wt'})dX}_{\text{Change in Unexplained Difference because of Changes in Relative Returns}} + \\ &\quad \underbrace{\int [f(X_{wt'}) - f(X_{wt})]E(w_{bt'} - w_{wt'}|X)]dX}_{\text{Change in Unexplained Difference because of Changes in White Covariates}} \qquad (5)\end{aligned}$$

The first two terms of the above decomposition define the portion of convergence that is attributable to improvements in the observable characteristics for both whites and blacks as well as within-cell improvements for whites, $E(w_{bt'} - w_{bt}|X)$. The last two terms measure the contribution of changes in the residual gap and may be isolated by examining the panels of Table 7 that are weighted by contemporaneous weights. If the effects of school-quality manifest themselves by changing the returns to schooling then we must isolate the middle term in the above decomposition (this is the mechanism by which school-quality affects wage improvements in Smith and Welch (1986) and Card and Krueger (1992)).[27] Therefore, it is necessary to compute the above decomposition with fixed-weights (a fixed distribution for $f(X_{it'})$ over time would set $\Delta X_t = 0$). In this paper I have used two sets of fixed-weights: the white distribution of skill in 1975 (computed by averaging the 1970 and 1980 distribution of skill) and the analogous black distribution of skill from 1975. The use of these weights will primarily affect point estimates of the racial wage gap in 1960 and 1990 since they are furthest away from the reference year. Results from fixed-weight analysis are also reported in Table 6. Estimates of convergence using fixed-weights are virtually identical to those obtained from the imposition of contemporaneous weights, implying that improvements in the relative levels of skill to changes in the residual gap (the last term in the decomposition above) do not contribute substantially to convergence in the racial wage gap.

The central insights of the numerous estimates reported in Tables 5 and 6 are graphically summarized in Figure 3 and the figure provides separate panels for all states and southern states. The figure demonstrates the difference between the observed and corrected (as computed by *Median* and *Median-NJ*) series. To keep the graph tractable, I have graphed the contribution of changes in relative returns (the race-year interaction term from the above

[27] This term is the non-parametric analog to the "race-year interaction" in the work of Smith and Welch (1986, Tables A.3-A.6) and Donohue and Heckman (1991, p.1620). Its nonparametric structure also allows for improvements in school-quality to operate through the intercepts of a conventional wage equation as in Heckman, Layne-Farrar and Todd (1996).

decomposition), but only as computed by the *Median* estimator. It is immediate from this figure that during the 1960-70 period the full amount of convergence from the *Median* estimator can be explained by the race-interaction term implying that convergence in the levels of skill does not contribute to convergence over this period– across all states "within cell" improvements account for 93% of the true convergence and 98% of convergence in the South. If this finding is interpreted in the light of Donohue and Heckman's contention that the timing of the school quality hypothesis is not sufficiently aligned to explain the 1960-70 convergence, the reduction in the within-skill gap may be interpreted as a decline in discrimination over this period. This interpretation will be studied in more detail in the next section through a within cohort analysis. During the 1970-80 period the race year term explains 28% of the convergence in all states, implying that over 70% is attributable to convergence in relative skill levels and aggregate changes in the economy. However, in the South the contribution of this term continues to be substantial– almost 80% of the convergence over the 1970s is attributable to this term. Whereas this is less true in southern states where the share of the race-year interaction explains over 75% of the selectivity corrected estimates from *Median,* it is clear that there was also convergence in skill levels that contributed to the overall convergence. Over the 1980-90 period the observed series shows a divergence in the racial wage gap– a trend that is considerably magnified in the corrected series. Once again, the race-year interaction explains over 90% of the decline, although it can be seen that convergence in relative skill levels continued. Had this convergence not occured, the divergence in the racial wage gap in the 1980s would have been greater by 11 percent. As such, these results reinforce the central message of Juhn, Murphy and Pierce (1991).

3.4 Cohort Level Analysis

To disentangle the role of improving school quality from that of declining discrimination in affecting wage convergence, Table 7 reports both within and between cohort estimates of the racial wage gap, and reports results for observed medians, corrected medians and corrected medians using contemporaneous white weights. I define a cohort as a group of individuals born in a three year window centered on the year labeled 'Birth Year.' In each year of the Census the racial wage gap may be computed for a number of cohorts. Because the focus on black economic progress during the 1960s centers on the South, I first emphasize the results in Table 7: Panel B. As can be clearly seen from the columns labeled "Corrected-Median" a substantial portion of the progress between 1960 and 1970 as well as from 1970 to 1980 is indeed driven by vintage effects (the replacement of retiring older cohorts with newer ones)– it can be clearly seen that the wage gap for older cohorts is much greater than the corresponding measure for entering cohorts. However, it is also apparent that substantial within cohort compression in the racial wage gap occured for almost all cohorts during the 1960s; a feature that cannot be generated by improvements in school-quality (which would tend to benefit only younger cohorts), but is more readily explained by reductions in discrimination. Table 7 also reports the portion of convergence between year t and year $t-1$ that may be attributed to within cohort improvements in the racial wage gap. This is the portion of the convergence in percentage points that can be explained using changes for continuing cohorts using the decomposition in Card and Krueger (1992) wherein within cohort improvements are weighted by base year weights.[28] The results demonstrate that ignoring nonparticipation results in *understating* the role of within cohort convergence over the decade of major civil rights initiatives. For example, the total observed convergence between 1960 and 1970 in southern states was 8 percent, of which 2.4 percentage points is attributable to within cohort improvements (implying that 30 percent of the measured convergence in within cohort). In the selectivity corrected estimates, the corrected convergence is 0.11 log points and the within cohort component comprises over 50 percent (5.8/11) of this improvement. Negative values for the within cohort component occur because of the disproportionate location of blacks in the lower portion of the skill distribution– a group whose wages in real terms between 1980 and 1990 [Katz and Autor (2000)].

This analysis may also be used to shed light on the racial wage across cohorts. Returning to Panel A of Table 7, we see that ignoring nonparticipation causes a dramatic understatement of the racial wage gap for younger cohorts. In 1990 for example, cohorts who had just turned 25 saw a gap of a little less than 0.40 log points; the observed series on the other hand estimates the gap to be just under 0.30 log for this group. Blacks born in 1965 were the first cohorts to be born in fully integrated hospitals (as a result of Title VI legislation) and constitute the leading edge of cohorts that attended desegregated schools in the south.[29] Interestingly, even though correcting for selection dramatically raises estimates of the racial wage gap by almost 30 percent for this cohort, it is also clear that the offer wage gap for this cohort is almost 10 percentage points smaller in magnitude than that of the preceeding

[28] In Card and Krueger's (1992) framework, the overall gap in a given year can be expressed as the weighted average of gaps for all j cohorts $1, 2, ..., J$ in that year: $GAP_t = \sum \alpha_t^j GAP_t^j$ where α_t^j is the relative weight for cohort j in year t. The "within cohort" component of the change in the gap would therefore be $GAP_{t+1} - GAP_t = \sum \alpha_t^j (GAP_{t+1}^j - GAP_t^j)$. This is the within cohort number that is reported in Table 8.

[29] It was only after the Supreme Court's passage of *Green v. Board of Education of New Kent County and Alexander v. Holmes County* in 1968 and 1969 respectively that southern states began the process of desegregation. On the eve of the passage of these rulings, only 22 percent of blacks were in desegregated schools. However, the increase from almost 0 percent to 22 percent occured between 1963 and 1967 implying that cohorts born at that time would have been amongst the first to be educated in fully integrated schools.

cohort. This improvement persists even when observable dimensions of skill between whites and blacks are accounted for; a point that can be seen in the last panel of Table 7 where contemporaneous white weights are imposed on the data. While this analysis does not establish causality, it does provide suggestive evidence that cohorts born at the time of the Civil Rights initiatives appear to be much better off than their predecessors. Pursuing this line of inquiry with longer panels would likely be a fruitful area for new research. Table 7 also demonstrates that black cohorts born in Southern states between 1945 and 1955 realized significant gains in relative wages relative to earlier cohorts. For example, in Southern states, the relative wage gap for entering cohorts in 1970 is dramatically lower than that for entering cohorts in 1960. Similarly, in 1980, entering cohorts (who were born in 1950-55) had a relative wage gap of 0.31 log points vis-a-vis a gap of approximately 0.47 log points for cohorts that entered in 1970. Discrete improvements in relative wages of this nature are consistent with school quality improvements in the South, and are consistent with the findings of Donohue, Heckman and Todd (2002) who demonstrate that between the late 1930s to 1960, black schooling quality improved relative to whites. As such, for cohorts born between 1935-1960 should see convergence in relative wages.

4 Discussion: What explains the Withdrawal?

4.1 Differences in Offer Wages

One class of explanations attempts to reconcile the differences in participation by appealing to demand side factors such as declining offer wages for the least skilled. Stated differently, how much of the employment differential may be explained by differences in offer wages between blacks and whites? To answer this question, I follow the pioneering framework of Juhn (1992) and denote the aggregate participation rate for race $i \in \{black, white\}$as $P_{it}(W_t^i)$. This function measures the probability of a respondent working during the census reference week as a function of his offer wage. The difference in participation between whites and blacks may be expressed as:

$$P_t^w - P_t^b = [P_t^w(W_t^w) - P_t^w(W_t^b)] + [P_t^w(W_t^b) - P_t^b(W_t^b)] \tag{6}$$

The first term measures the predicted component of differences in participation that are a function of measurable differences in offer wages; actual white participation rates at each wage $P_t^w(W_t^w)$ are subtracted from the counterfactual white participation rate (computed by assigning the black distribution of offer wages to whites). The second term is the residual component and measures the racial gap in participation that is attributable to race differences in participation *at the same wage* (here, the black wage). This term assesses the role of the entire participation schedule shifting, and is evaluated at a common offer wage distribution. Therefore, racial differences in willingness to participate in the disability program, the returns to crime, or other "tastes" driven by changes in social-pathologies such disenfranchisement from the formal labor market would all shift the participation function differentially by race. I can estimate $p_{it}(w)$ with $\widehat{p}_{it}(w)$ by examining participation rates at different points on the support of the offer wage distribution $f_{it}(w)$. In other words, by using the *estimated* offer wage distribution, we can compute the empirical counterpart to $p_{it}(w)$ by calculating the cumulative participation rate at each value of the offer wage.

Juhn uses the results from her matching estimator to estimate $f_{it}(w)$. Even if selection on observables is appropriate, this method only yields $\widehat{w} = X\beta$, not the distribution of $X\beta + \epsilon$; the variance of $f_{it}(w)$ is still unknown. However, it is possible to overcome this problem by exploiting the information contained in the assumption that the latent distribution of offer wages is log normal. To see this, note that it is always true that: $var(w|X) = E(w^2|X) - [E(w|X)]^2$,where $var(w|X)$ refers to the variance of conditional offer wages. Corrections for sample-selection bias yield consistent estimates of $E(w|X)$. To obtain an estimate of the first term I note that $E(w^2|X) = E(w^2|X, w > \overline{w}) \Pr(w > \overline{w}|X) + E(w^2|X, w < \overline{w}) \Pr(w < \overline{w}|X)$. If it is indeed the case that log offer wages are normally distributed (and therefore symmetric), then it is also the case that $E(w^2|X, w > \overline{w}) = E(w^2|X, w < \overline{w})$. We may compute the variance by calculating $E(w^2|X, w > \overline{w})$ for those observations with (observed) offer wages above the corrected median. Once $var(w|X)$ is estimated, we can assign each non-worker an arbitrary "error" with a draw from a normal distribution with mean $E(w|X)$ and variance $var(w|X)$. Note that this method allows for the variance of residual skill to vary by race and skill, and more importantly over time.

Figure 4 describes the results of this analysis. For each year of the data I compute an estimate of the black offer wage distribution (using the *Median* estimator and adjusting for the variance using the method described above) and compute black and white participation rates at each decile of the black offer wage distribution. The figure graphs the racial difference in participation at each decile of this distribution. Therefore, over time, the black offer wage distribution is allowed to change, but white participation is always evaluated at black offer wages. Figure 4 demonstrates a massive increase in reservation wages for blacks in the bottom deciles of the black offer wage distribution between 1970 and 1980; in the bottom two deciles the racial gap in participation averages 20 percent.[30] During the 1960s, this gap

[30] I was concerned that even within deciles blacks may be earning less that whites. While there is some evidence for this claim, it is not substantive. This can be seen in the first panel of Table 9 where within decile the racial gap in wages is essentially zero. The larger discrepency in the top decile of the black distribution is to be expected– almost all college-educated white men would be assigned to that black decile. Using the data

was around 5 percent and black participation actually exceeded white partcipation rates in the middle of the black offer wage distribution. Over the median black offer wage, differences in participation rates have remained relatively stable since 1960. Table 9 presents the actual data by wage percentile of the black offer wage distribution and reports the actual racial gap in wages at each percentile of the black distribution (ideally, this difference would be zero or close to it), as well as participation rates at each decile by race. At the bottom of Table 9 I report the results of the decomposition discussed above. In 1960, over 80 percent of the racial difference in participation was attributable to differences in offer wages, whereas by 1990 that fraction had fallen to 40 percent. The declining explanatory power of the offer-wage model demonstrates the importance of searching for supply-side explanations in explaining the decline in black labor force participation in the last quarter of the 20th century.[31]

4.2 Black Economic Progress in an Era of Increasing Wage Dispersion

This analysis can also be used to shed light on the precise location of economic gains made by African-American men in the economy wide distribution of skill. In the light of the Juhn, Murphy and Pierce (1991) thesis, that economy wide increases in wage dispersion have contributed to the slowdown in the log wage gap, I study the extent to which blacks are positioned in white wage deciles using selectivity corrected estimates of offer wages. Figure 5 reports the fraction of African-American men in each decile of the white offer wage distribution. If blacks and whites had the same offer wage distribution, there would be 10 percent of blacks in each white offer wage decile. It is important to note that Figure 5 differs from Figure 4 in that the white distribution of offer wages is plotted on the x-axis of Figure 5. The advantage of this approach is that it allows me to abstract from changes in industrial stucture, and having to predict industry and occupation for nonworkers. Its limitation is that it treats offer wages as a sufficient statistic for tracking changes in labor market well-being. The results indicate that the significant gains made by black men during the 1960s and 1970s occured almost exclusively in the bottom wage decile; significant numbers of black men were pushed out of the lowest white wage decile into higher quintiles. Concurrently, there was a substantial increase in the proportion of black men in the fourth and fifth deciles of the white

reported in Table 9 it is also possible to recompute Figure 4 by assigning blacks in decile i their participation rate in decile $i+1$, an assignment that *ipso facto* gurantees that black participation is being evaluated at an offer wage higher than that for whites. The results of this recalculation yield results that are qualitatively similar to those in Figure 4.

[31] Aaronson (2002) notes that models which allow the current work decision to be a function of forward-looking predictions of the returns to experience have considerably more predictive power than those which are "myopic". To the extent that the returns to experience have fallen more at the bottom of the offer wage distribution and, furthermore, more for blacks than whites within deciles, her findings (even though they abstract from race) provide an extremely important angle for future research to investigate.

offer wage distribution during the 1960s, and an increase in the fraction of the sixth through ninth deciles during the 1970s. These findings support an interpretation of the effects of the CRA and related initiatives whereby the economic prospects of the least-skilled blacks were most affected, with much smaller effects on the economic well-being of more skilled workers. Since the gains during the 1960s and 1970s are not uniformly distributed, this finding also emphasizes the need to appreciate the heterogenous impacts of Federal interventions in labor markets. In the 1980s, cross-decile movement essentially ceased; there was a slight decrease in the fraction of black men the second decile, and a corresponding minor increase in the third through fifth deciles. When the same figure is generated using wages for only workers, the fraction of black men in the bottom two deciles is understated by almost 5 percentage points with an accompanying overstatement of the fraction in higher wage deciles. Furthermore, the striking reduction in the fraction of black men occupying the bottom wage decile of the white offer wage distribution is severely understated.

4.3 The Disability Program

The leading supply-side explanation comes from the role of the Disability Insurance Program, and constitutes the primary suspect in the original Butler-Heckman paper. As this section demonstrates, the DI program cannot explain the withdrawal during the 1960s, but it may be the most attractive explanation for recent years. For the purpose of historical accuracy it should be noted that Federal Government labor economists first noted the connection between disability benefits and labor force withdrawal, but failed to suggest a connection with this withdrawal and the putative success of Title VII Legislation. As early as 1972, Gastwirth (1972) attributed more than 90 percent of the decline in the labor force participation of prime aged men (aged 25-55) between 1956 and 1968 to three factors: (a) the expansion of disability benefits to men under the age of 50 (50 percent of the 90 percent), (b) increases in the number of full-time graduate students (10 percent) and (c) changes in the 1967 CPS definition of employed and unemployed (30 percent). Building on this work, Siskind (1975) notes that the last inference does not appear to be entirely correct. Siskind in turn provides detailed disability takeup rates by age and race which support the disability-benefits induced explanation for declining labor force participation. Figure 6 studies the relationship between Labor Force Participation (LFP) and disability benefits takeup rates. The data used to produce this graph are from the *1974 Manpower Report of the President*, as presented in Siskind (1975). The first panel of graphs report LFP rates by race and age. The second panel reports the corresponding percentage of men who were receiving disability benefits at the end of the year. Because the CRA took effect on July 2, 1965, I have highlighted that year to emphasize any before-after treatment effects, or the presence of "run-up" effects. Several features of these trends are worth noting. First, for all three age groups under consideration, there were declining trends in LFP that had begun well before the passage of the CRA. These declines began before 1955; before anyone could have anticipated the passage of the Great Society's programs. Second, the relative declines are greatest for those men aged 45-54 and do not appear for younger men. Furthermore, an examination of the second panel shows that the largest increases in the percent of men on disability have occurred for those aged 45-54.While there were large expansions in the fraction of younger men receiving disability benefits, the increase in recipiency is not large enough to explain the corresponding declines in LFP. Therefore, while there is certainly evidence to support the central contention of the Butler-Heckman hypothesis, and that of Heckman (1989), that the LFP rates of black men declined faster than that of white men, there does not appear to be any *prima facie* evidence that supports the theory that the growth in the disability program caused these relative declines through 1975.

In contrast to the above results, Autor and Duggan (2001) demonstrate that the relationship between DI and participation is much stronger in the 1980s and 1990s: in 1984, 30 percent of high school dropout males who were nonparticipants were receiving DI or SSI. By 1999, the fraction had risen to 47%. Amongst those aged 25-64 the fraction of nonparticipants on disability grew from 45 percent to 57 percent. This growth is a function of both falling skill prices (which raises benefit replacement levels and affects the incentives to enter unemployment), as well as of changes in the generosity of the disability program. Their research, though it abstracts from race, is in the spirit of the original Butler-Heckman hypothesis and suggests that the DI program based link that Butler-Heckman posited for the 1960s may actually have been more empirically relevant for the 1980s. Amongst the least-skilled, the growing generosity of the DI program appears to hold significant explanatory power. Researching the possible connection between race, health, and the DI program remains an exciting avenue for future research.

5 Conclusions

Ever since Myrdal published his monumental treatise, *An American Dilemma,* in 1944, considerable intellectual energy has been devoted to studying the causes and dynamics of the racial wage gap. However, much of the literature constituting this debate has relied on inferences made on CPS data and therefore has ignored the growing nonparticipation problem amongst blacks that is driven by increases in incarceration rates and labor supply responses to falling skill-prices. The purpose of this paper has been to revisit a thesis first propounded by

Butler and Heckman almost 25 years ago, and to evaluate whether a significant portion of the observed convergence in black-white earnings may be explained by the selective withdrawal of low-skilled blacks from the labor force. Despite the importance of this topic and the amount of intellectual energy devoted to studying black economic progress, the US. Commission on Civil Rights has remained suspicious of the possible magnitude of the selective withdrawal hypothesis:

> "Empirical research suggests, that this potential bias, under most plausible assumptions, would not account for a large share of the growth in the relative earnings of black males." [United States Commission on Civil Rights (1986)]

This paper aggressively challenges the above view, and demonstrates that the selective withdrawal hypothesis can explain 85 percent of the observed convergence between 1970 and 1990, and 40 percent of the 1960-90 convergence. Interestingly, it refutes the original Butler-Heckman contention that Freeman (1973) had overstated the case for the CRA and related initiatives. In southern states I find evidence for having *understated* the convergence over the 1960s however the massive increase in nonparticipation and incarceration since 1980 have caused observed series to be dramatically overstated. These corrections have two messages for researchers– first, that the simultaneous modelling of wages and participation is central to any analysis of the racial wage gap, and second, innocuous sample selection criteria can be the cause of enormous bias– even the direction of convergence will be incorrectly estimated through the inclusion of only a select sample of workers.

One explanation for the decline in partcipation is that anti-discrimination efforts weakened significantly over the 1980s. This is the view espoused in Bound and Freeman (1992, p.229) who argue that "firms no longer facing an affirmative action gun" were under no compulsion to maintain the gains achieved in the late 1960s. This appealing argument is not entirely consistent with the historical record for it is not the case that the efficacy of the CRA was correlated with measured anti-discriminatory budgets. The persuasive evidence presented in Brown (1982) and Donohue and Heckman (1991) demonstrates that the greatest gains in the racial wage gap were achieved during a period of weak EEOC budgets. It is however, important to note one interesting fact: Bound and Freeman cite evidence showing that federal contractors who were covered by mandatory affirmative action plans did not reduce the share of black males that they employed. By itself, this fact is not supportive of their thesis but remains an extremely important avenue for future research: in a general-equilibrium model of labor markets with multiple sectors, successful enforcement in one sector will depress relative wages in another by diverting white labor from the covered to the uncovered sector. However in the light of Figures 4 and 5, to the extent that blacks in the lowest deciles of the offer wage distribution are disproportionately hurt by reduced enforcement efforts, the Bound-Freeman finding deserves more attention, as does the broader topic of heterogenous treatment-effects from Civil Rights enforcement efforts on the economy-wide distribution of offer wages.

The corrected trends documented in this paper offer bleak predictions for future trends in the racial wage gap, especially amongst younger and lesser-skilled groups. By 1990, almost 30 percent of blacks were not employed during a random reference week in the year (versus 6.1 percent for whites) and wages are not observed for 20 percent of prime age black men (7.3 percent for whites), with annual nonparticipation rates at 40 percent for certain black skill groups. Much of this withdrawal is long-term. One source of "progres" that may generate the illusion of convergence in the coming years is the legalization of abortion following *Row vs. Wade* in 1973. Gruber, Levine and Staiger (1999) demonstrate that the marginal child affected by this ruling would have had a 40-60 percent greater chance of living in a single-parent family, dying as an infant, or growing up in poverty and welfare. Cohorts affected by the legalization of abortion would be entering the labor market at the time of the 2000 census. To the extent that black children are disproportionately more likely to be the marginal child, the legalization of abortion provides avenues by which 'convergence' could manifest itself. In the spirit of Donohue and Levitt (2000) who demonstrate that legalized abortion accounts for almost 50 percent of the drop in crime in the 1990s, incarceration rates should start to fall for younger black cohorts in a manner that mirrors the declines in crime. The magnitude of this effect is unknown but will serve to reduce the bias associated with the use of CPS data to study the racial wage gap. Therefore, it is possible that rapid convergence in wages and employment may be observed in skill-cells where the marginal child is most likely to have been located. These effects should give social-scientists and policy-makers little reason to be sanguine, for the convergence would have remained inherently illusory.

6 Data Appendix

The data used in this paper are derived from the PUMS files of the US. Decennial Census 1960-90. For 1960 there is only one public use file. In 1970 I use the State 15% sample, and in 1980 and 1990 I use the entire "B" sample. In 1960 and 1970, the Census did not ask respondents for the number of hours worked, or the number of weeks worked. Instead, respondents were asked to report their answers to a bracketed version of the question. Buchinsky (1994) provides a method to convert bracketed "weeks worked last year" responses to a continuous measure and I follow his algorithm. As an alternative, I have also experimented with assigning the mean and the median value of the bracketed interval as the true value of the variable. Based on a validation study that I conducted using the 1980 and 1990 Census, Buchinsky's method was preferred in terms of generating estimates that were closer to the reported values for these years.

Because of the well-known problems with the Census "hot-deck" allocation procedures I was cautious about the use of respondents with imputed data. In unpublished work I have dropped all records with imputed values for either age, gender, race, schooling, hours worked, weeks worked last year, or wage and salary income. However, in this analysis I have retained respondents with imputed data for two reasons: First, the number and quality of imputation flags changes over time and second, large numbers of African American men are deleted if the imputed samples are deleted. Throughout this paper I define "black" and "white" as respondents who identified themselves as being black or white, but were not of Hispanic ancestry. Wage and salary data are deflated to constant 1997 dollars using the chain-weighted Implicit GDP Price Deflator.

In 1980 and 1990 those individuals who claimed to have fewer than 8 years of schooling and were younger than 35 years of age have been combined with other high-school dropouts.[32] In examining the characteristics of this group of individuals I noted that they had high rates of being NILF and incarcerated (in fact over 50 percent of blacks aged 25-30 in 1990 with fewer than nine years of schooling had no weekly wages). For those with weekly wages, these were lower than those of all other skill groups (but had larger variance). My results are impervious to dropping this group completely from the sample or simply combining them with other high-school dropouts. However, "within-cell" estimates for dropouts aged less than 35 are sensitive to this restriction. Appendix Table 1A describes the final sample sizes used for analysis in this paper. In 1960, it is not possible to directly estimate the fraction of respondents who last worked two years ago (that is, in 1958). To recover this quantity, I divided the fraction of respondents who last worked between 1955 and 1958 by four.

6.1 Measuring Skill Prices

The Census asks questions on total income from wage and salary last year, weeks worked last year, and hours worked last week. I exclude those workers with self-employed income from the construction of skill prices, because observed skill prices for the self-employed also reflect a return to capital. I have replicated the analysis presented in Table 5 with the self-employed samples and find that the results are always within the 95 percent confidence-interval of the full-sample estimates reported in Table 5: Panel A. To construct a measure of skill price I

[32] I am grateful to Derek Neal for this suggestion. However, I alone am responsible for any errors in adopting this approach.

use two alternative measures. In the first, weekly wages are defined by total wage and salary income divided by weeks worked last year. By ignoring the number of hours worked last week the social-scientist is implicitly assuming that conditional on working a certain number of weeks there is no variation across workers in the number of hours worked. My second measure divides weekly wages by hours worked last week (or a particular reference week in the 1940 Census). This measure loosely corresponds to "skill-price" in conventional models of labor demand. The obvious problem with this measure is that the product of weeks worked last year and hours worked last week is only a proxy for total hours worked last year. In 1980 and 1990, the Census also asked respondents for "usual hours worked last year." In these years, the correlation coefficient between the two measures of hours worked was 0.65. Whereas many labor economists would prefer the use of the latter measure of hours worked, it is not necessarily superior to the former. Conditional on only knowing the total number of weeks worked last year, and not the joint distribution of weeks worked and hours worked in each week, what is needed is an estimate of average hours worked. This may or may not correspond to the usual hours worked question. First, respondents may not recall the average number of hours worked last year and may incorrectly report it. Secondly, they may interpret the question literally and report the modal number of hours worked across all weeks worked last year. Therefore, it is possible that the response to hours worked last week is actually a superior measure of hours worked than usual hours worked last year. Weekly wages are the object of interest in Card and Krueger (1992), Juhn, Murphy, and Pierce (1991) and for much of the analysis in Katz and Autor (2000). Despite its theoretical limitations, the use of weekly wages provides the cleanest proxy for skill prices.

In order to discard observations that are considered to be "gross errors," I depart from the literature and do not trim my samples based on being above or below an arbitrary cutoff as specified by an upper and lower bound on real skill prices. This approach, while popular, ignores the fact that over time economic growth will shift the distribution of wages to the right. Therefore, deleting observations that make over $100 an hour (in 1997 dollars), or less than one half of the 1982 value of the minimum wage, over the entire 1940-90 period will result in dropping very different groups of people over time. I winsorize the data at 1-percent and 99-percent. These bounds are allowed to vary by year. This procedure was pursued in the light of analytical and simulation results in Bollinger and Chandra(2001).[33]

6.2 Imputation of Weekly Wages for Armed Forces Samples

In using the Armed Forces sample, a possible source of bias may be introduced if the analyst uses their weekly earnings as an estimate of their skill-price in the conventional wage and salary market. The source of this potential bias is the fact that a large portion of the compensation for members of the Armed Forces may come in the form of "in kind" transfers such as food and housing allowances. In 1991 for example, these allowances totaled 19 percent of cash compensation. To examine this possibility I compared (pointwise) differences in average weekly wage with and without the armed forces sample to see if the latter group

[33] Bollinger and Chandra demonstrate that trimming the data is a desirable procedure only for very special measurement-error processes which are *not* found for the data generating process describing wages or earnings. They demonstrate that attenuation bias is introduced if the analyst trims the data when in fact a conventional measurement error process is at work. They demonstrate that the process of winsorizing or doing nothing appears to be most desirable strategy to adopt in working with wage data.

were earning significantly less than observationally equivalent wage and salary workers. There is considerable evidence for this theory, although the results do vary over time and by skill category. Whites currently in the Armed Forces tend to earn 12-15% less than comparably skilled whites; for blacks, the opposite result generally holds true. Since joining the Armed Forces may be a response to the civilian labor market that civilians face, I did not want to impute wages based on the effect of veteran status on earnings. Instead, I use the work of Asch and Hosek (1999) and noted that conditional on age and education, total military compensation is at the 78th percentile of civilian pay for junior enlisted personnel (E4s at YOS 4) and at the 70th percentile of civilian pay for mid-career officers (O4 at YOS 12). I have therefore assigned all military personnel to lie above their pointwise median. For matching estimators, the wages of the Armed Forces sample are recoded upwards to the pointwise mean if they are less than that mean.

6.3 Standard Errors

Throughout this paper, standard-errors were computed using bootstrap replications for all estimates. The details are as follows: First the data were resampled 200 times (with replacement) by race and year. This resulted in 4 (year) x 2 (race) x 200 = 1600 datasets. Next, the first dataset for blacks in 1960 was merged with the first dataset for whites in 1960, and then merged with the first dataset for blacks in 1970 and then whites in 1970, until there were a total of 200 datasets each with sample sizes given in Appendix Table A1. To ensure replicability, point estimates reported in the paper are the actual results from the PUMS data and not the sample average across the 200 samples. However, to obtain standard-errors for the reported estimates the programs used in this paper were estimated on the 200 datasets and the standard-deviation of the answers was saved to a separate file. This method, while tedious, allows the standard-errors to account for the sampling distribution of the covariates in addition to the sampling distribution of the point estimates.

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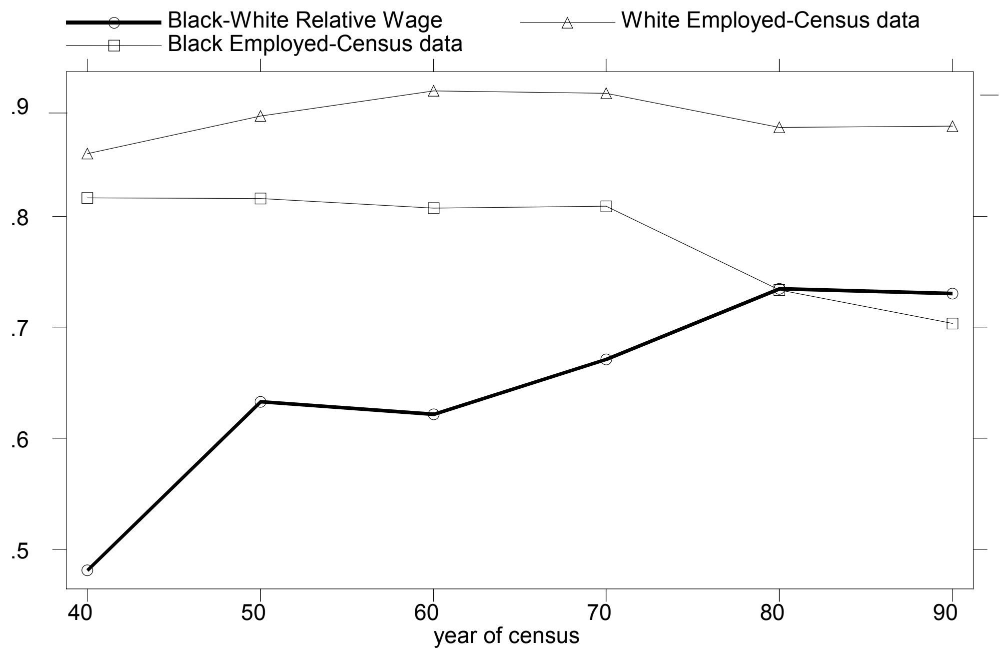
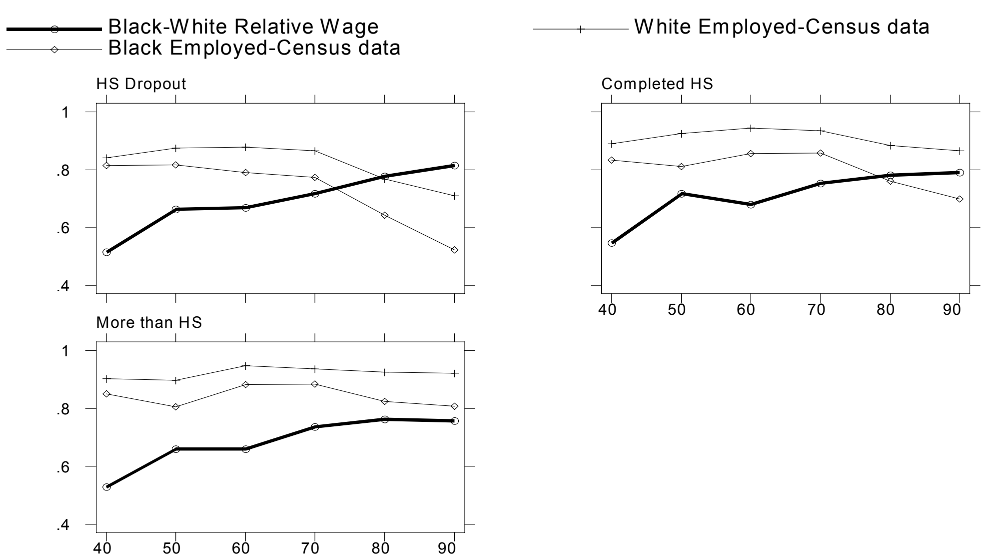
Figure 1: Black-White Relative Wages and Employment Population Ratios, for Men aged 25-55

Panel A: All Schooling Groups



Panel B: By Schooling Group



Author's calculations from the PUMS data. No sample restrictions have been placed on the data for the construction of employment/population ratios. Relative wages were computed by using weekly wages for wage and salary workers who worked at least one week in the previous year.

Figure 2: Median Regression with Sample Selection

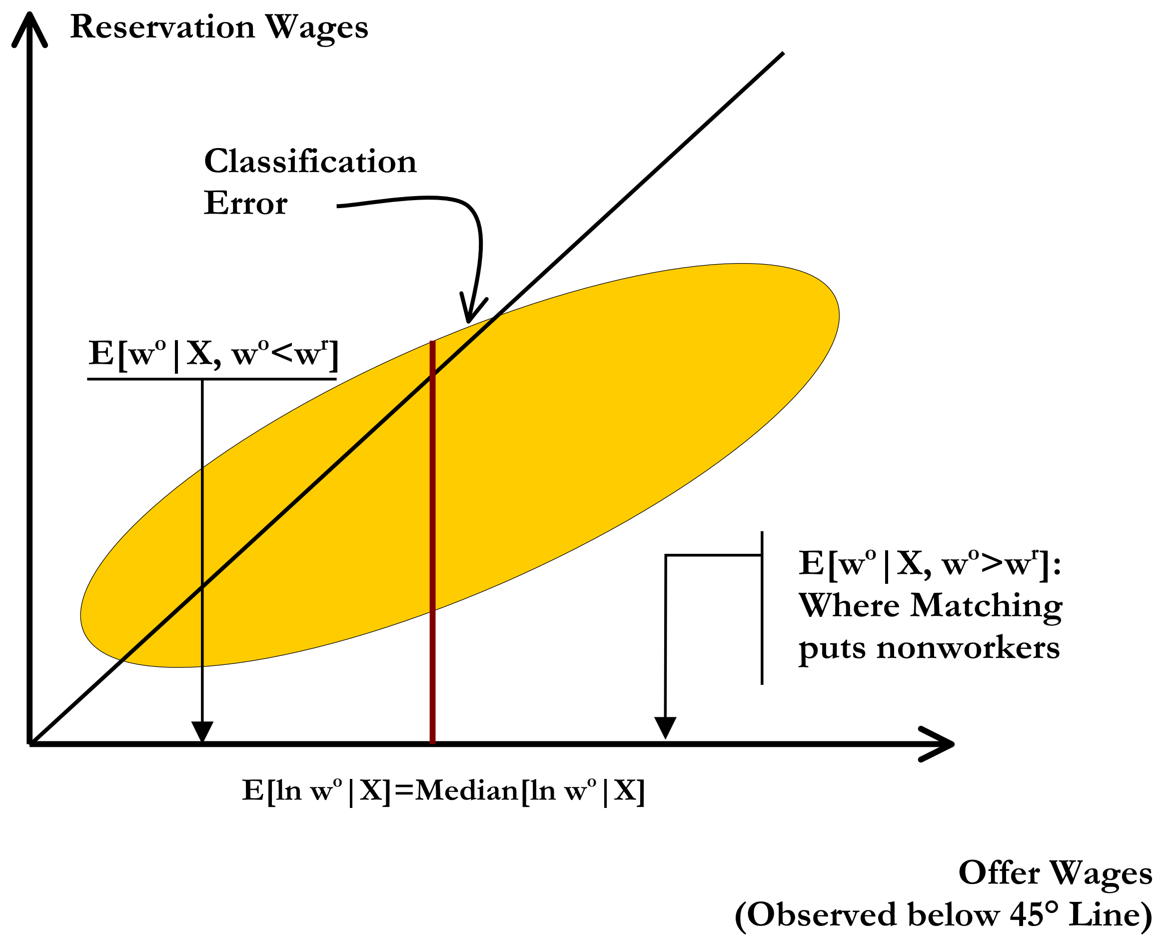
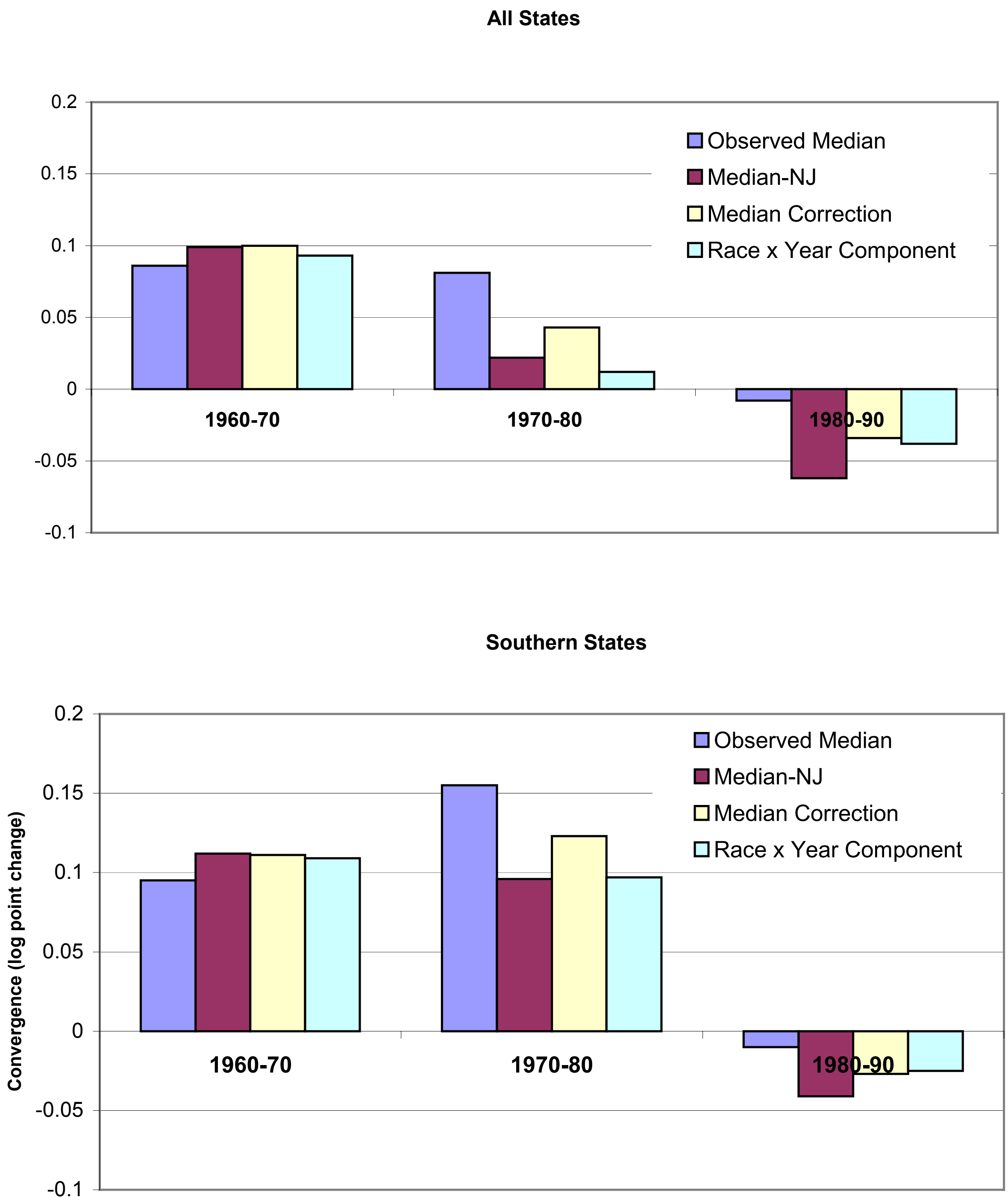


Figure 3: Observed and Corrected Estimates of the Racial Wage Gap with Fraction Explained by Race-Year Component



Observed series refer to observed medians, corrections refer to those obtained from the *Median* estimator (see Section 2.1 of text for details). Race x Year Interaction is the component of the change that can be explained by changes in the within-skill race gap.

Figure 4: Racial Difference in Weekly Participation Rates by Black Wage Decile

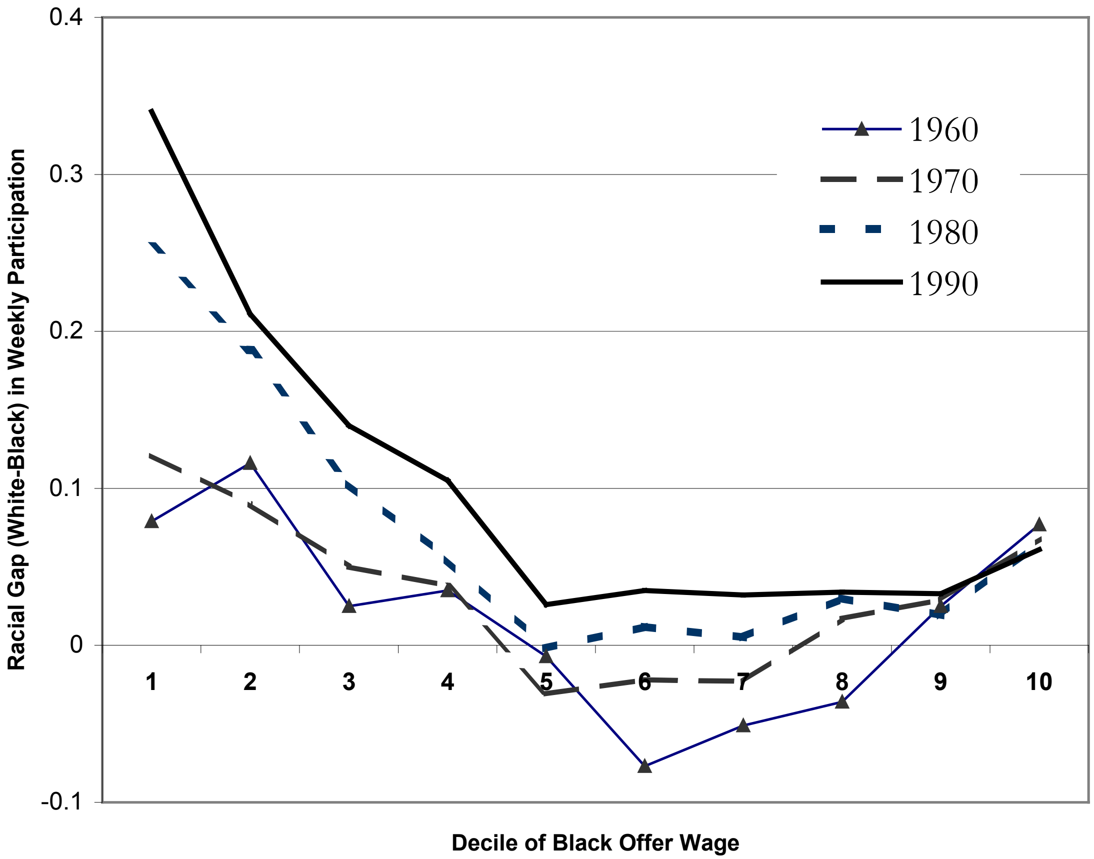
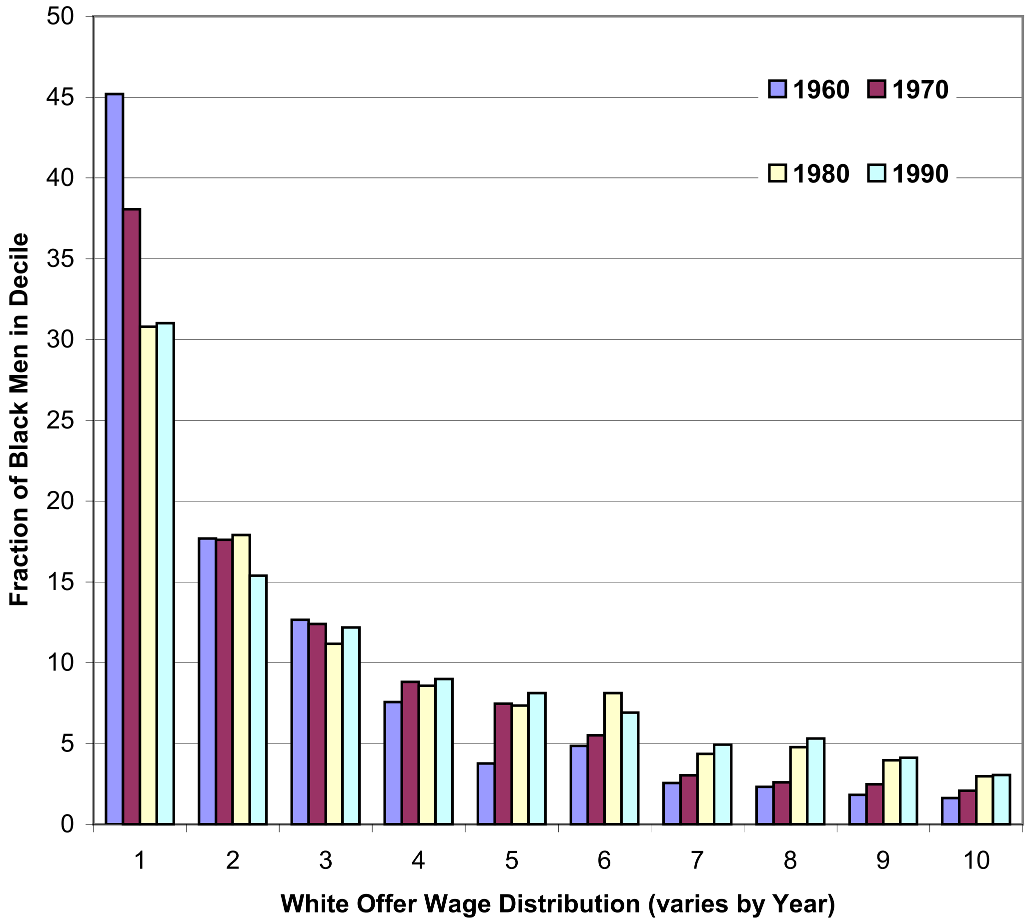


Figure reports White-Black difference in participation rates during the census reference week at each decile of the black offer wage distribution. The black offer wage distribution was computed using the *Median* estimator described in the text and is allowed to change over time.

Figure 5: Fraction of Black Men by Decile of White Offer Wage Distribution



The white offer wage distribution was computed using the *Median* estimator described in the text and is allowed to change over time.

Figure 6: Labor Force Participation Rates and Percent Receiving Disability Benefits, CPS Data from Siskind (1975)

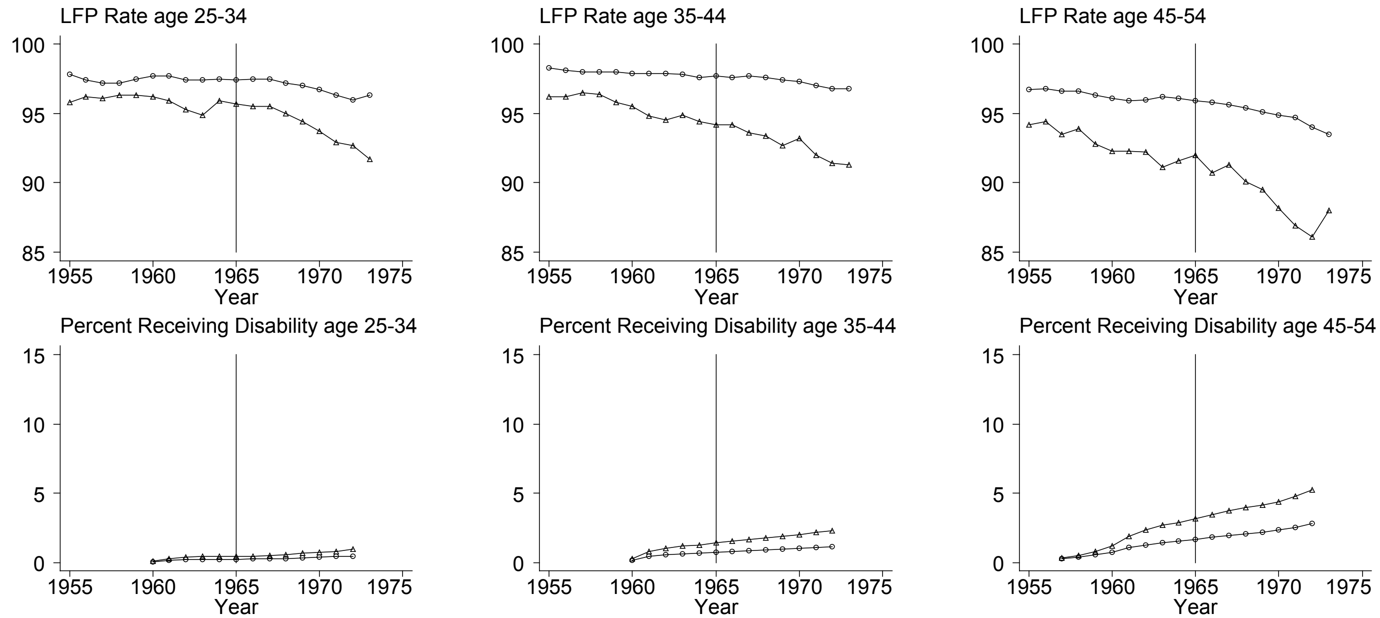


Table 1: Fraction of Prime Age Men who are Institutionalized, or Unemployed, NILF and Institutionalized during the Census Reference Week

	Panel A: Fraction Institutionalized								Panel B: Fraction Institutionalized, Unemployed, NILF							
	Whites				Blacks				Whites				Blacks			
	< HS	HS	HS+	Total	< HS	HS	HS+	Total	< HS	HS	HS+	Total	< HS	HS	HS+	Total
1960																
25-29	0.021	0.005	0.002	0.010	0.068	0.025	0.016	0.053	0.118	0.054	0.039	0.072	0.219	0.138	0.083	0.187
30-34	0.017	0.004	0.003	0.009	0.058	0.036	0.025	0.050	0.103	0.045	0.030	0.065	0.208	0.154	0.093	0.186
35-39	0.017	0.005	0.004	0.010	0.050	0.036	0.027	0.045	0.110	0.046	0.031	0.069	0.204	0.132	0.103	0.183
40-44	0.016	0.005	0.005	0.010	0.038	0.028	0.015	0.035	0.112	0.051	0.037	0.078	0.197	0.147	0.098	0.183
45-49	0.014	0.008	0.006	0.011	0.027	0.021	0.033	0.026	0.118	0.062	0.050	0.092	0.200	0.155	0.110	0.190
50-54	0.015	0.008	0.008	0.013	0.028	0.017	0.014	0.026	0.150	0.090	0.074	0.126	0.225	0.140	0.120	0.213
Total	0.016	0.006	0.004	0.011	0.044	0.029	0.022	0.040	0.121	0.055	0.041	0.083	0.209	0.143	0.098	0.190
1970																
25-29	0.023	0.006	0.002	0.008	0.079	0.028	0.014	0.048	0.141	0.066	0.050	0.076	0.253	0.142	0.105	0.185
30-34	0.019	0.006	0.002	0.008	0.051	0.028	0.007	0.037	0.121	0.055	0.039	0.067	0.203	0.136	0.086	0.164
35-39	0.014	0.004	0.002	0.006	0.044	0.021	0.008	0.033	0.116	0.052	0.040	0.068	0.195	0.123	0.086	0.160
40-44	0.013	0.005	0.003	0.007	0.035	0.016	0.010	0.028	0.113	0.058	0.048	0.076	0.210	0.137	0.088	0.180
45-49	0.013	0.005	0.003	0.008	0.026	0.021	0.026	0.025	0.133	0.071	0.057	0.092	0.215	0.160	0.105	0.195
50-54	0.012	0.006	0.003	0.008	0.020	0.026	0.010	0.020	0.160	0.091	0.069	0.118	0.252	0.163	0.110	0.229
Total	0.015	0.005	0.002	0.008	0.040	0.024	0.012	0.032	0.133	0.066	0.050	0.084	0.222	0.141	0.096	0.185
1980																
25-29	0.035	0.007	0.003	0.008	0.101	0.039	0.026	0.050	0.264	0.128	0.067	0.112	0.452	0.265	0.161	0.278
30-34	0.027	0.006	0.003	0.007	0.071	0.035	0.022	0.040	0.236	0.110	0.056	0.095	0.345	0.234	0.138	0.230
35-39	0.019	0.005	0.003	0.006	0.041	0.026	0.017	0.029	0.210	0.094	0.048	0.092	0.326	0.215	0.135	0.232
40-44	0.013	0.004	0.003	0.005	0.025	0.016	0.010	0.018	0.206	0.093	0.052	0.100	0.286	0.202	0.129	0.222
45-49	0.009	0.002	0.002	0.004	0.016	0.004	0.007	0.011	0.210	0.107	0.060	0.117	0.333	0.201	0.138	0.257
50-54	0.008	0.003	0.002	0.004	0.014	0.011	0.013	0.014	0.249	0.142	0.087	0.161	0.356	0.251	0.180	0.306
Total	0.016	0.005	0.003	0.006	0.043	0.027	0.019	0.031	0.231	0.113	0.061	0.112	0.352	0.234	0.147	0.255
1990																
25-29	0.042	0.015	0.007	0.013	0.231	0.077	0.052	0.095	0.288	0.130	0.062	0.108	0.619	0.318	0.182	0.320
30-34	0.044	0.013	0.006	0.012	0.162	0.065	0.043	0.072	0.283	0.122	0.057	0.100	0.506	0.320	0.174	0.290
35-39	0.035	0.012	0.006	0.010	0.110	0.049	0.044	0.058	0.286	0.129	0.061	0.099	0.490	0.286	0.177	0.274
40-44	0.031	0.009	0.004	0.008	0.070	0.043	0.036	0.046	0.282	0.134	0.068	0.104	0.391	0.275	0.178	0.257
45-49	0.021	0.007	0.005	0.007	0.067	0.026	0.027	0.039	0.275	0.129	0.076	0.118	0.403	0.250	0.176	0.270
50-54	0.013	0.005	0.004	0.006	0.034	0.024	0.016	0.025	0.299	0.159	0.104	0.157	0.374	0.266	0.163	0.276
Total	0.030	0.011	0.005	0.010	0.114	0.054	0.040	0.061	0.286	0.133	0.068	0.112	0.466	0.294	0.176	0.285

Source: Authors tabulations from the PUMS data for 1960-1990 (1990 data have been weighted using person weights). No sample restrictions have been placed on the data. See Data Appendix for details of PUMS sample. NILF stands for Not in the Labor Force, but does not include respondents who were enrolled in school.

Table 2: Fraction of Prime Age Men in the Armed Forces during the Census Reference Week

	Whites			Blacks		
	HS	HS+	Total	HS	HS+	Total
1960						
25-29	0.067	0.050	0.059	0.103	0.087	0.098
30-34	0.049	0.026	0.038	0.059	0.046	0.054
35-39	0.041	0.032	0.037	0.041	0.025	0.035
40-44	0.028	0.040	0.033	0.012	0.030	0.019
45-49	0.012	0.018	0.014	0.013	0.015	0.014
50-54	0.006	0.009	0.007	0.010	0.005	0.008
Total	0.037	0.031	0.034	0.052	0.041	0.048
1970						
25-29	0.037	0.052	0.044	0.057	0.052	0.055
30-34	0.043	0.031	0.037	0.059	0.049	0.056
35-39	0.047	0.033	0.040	0.074	0.046	0.064
40-44	0.019	0.019	0.019	0.037	0.031	0.035
45-49	0.008	0.015	0.011	0.018	0.012	0.016
50-54	0.004	0.009	0.006	0.009	0.005	0.008
Total	0.027	0.029	0.028	0.049	0.038	0.045
1980						
25-29	0.033	0.023	0.027	0.059	0.044	0.052
30-34	0.029	0.024	0.026	0.041	0.030	0.035
35-39	0.022	0.025	0.024	0.038	0.049	0.042
40-44	0.012	0.021	0.017	0.025	0.033	0.028
45-49	0.004	0.013	0.009	0.009	0.013	0.011
50-54	0.002	0.004	0.003	0.006	0.003	0.005
Total	0.019	0.020	0.020	0.037	0.034	0.036
1990						
25-29	0.028	0.037	0.033	0.046	0.061	0.053
30-34	0.015	0.027	0.023	0.028	0.054	0.041
35-39	0.012	0.022	0.019	0.019	0.039	0.030
40-44	0.006	0.016	0.013	0.004	0.022	0.014
45-49	0.003	0.008	0.006	0.003	0.013	0.008
50-54	0.001	0.003	0.002	0.001	0.005	0.003
Total	0.013	0.021	0.018	0.022	0.039	0.031

Source: Authors tabulations from the PUMS data 1990 (1990 data have been weighted using person weights). See Data Appendix for details of sample.

Table 3: Last Year Worked for Currently Non-Employed Prime-Age Men

Panel A: Last Worked- Age 25-54

	1960	1970	1980	1990
Whites				
This yr/Last yr	69.0	67.1	64.1	60.6
2-5 yrs ago	12.4	15.3	14.3	16.2
6+ yrs ago	14.2	12.4	16.3	17.6
Never Worked	4.4	5.2	5.3	5.6
Total	100.0	100.0	100.0	100.0
Blacks				
This yr/Last yr	63.1	58.4	49.3	48.0
2-5 yrs ago	15.6	17.2	15.4	18.4
6+ yrs ago	17.9	17.2	26.5	23.8
Never Worked	3.4	7.2	8.8	9.8
Total	100.0	100.0	100.0	100.0

Panel B: Last Worked- Age 25-34

	1960	1970	1980	1990
Whites				
This yr/Last yr	77.6	77.2	76.6	70.9
2-5 yrs ago	9.5	10.1	9.3	13.0
6+ yrs ago	6.4	5.0	7.4	8.7
Never Worked	6.5	7.7	6.7	7.4
Total	100.0	100.0	100.0	100.0
Blacks				
This yr/Last yr	68.3	66.0	56.2	53.9
2-5 yrs ago	14.9	15.7	14.5	17.5
6+ yrs ago	11.5	8.6	17.9	15.8
Never Worked	5.3	9.7	11.4	12.8
Total	100.0	100.0	100.0	100.0

Authors tabulations from the PUMS data. Starting in 1960 the Census asks respondents who were not working during the reference week for when they last worked. Responses have been standardized to permit comparability across years. No sample restrictions have been placed on the data except of omitting those currently in school.

Table 4: Sensitivity of the Measured Racial Wage Gap to Sample-Selection Restrictions

	(1) Worked 1-52 Weeks	(2) Worked Full-Time	(3) Worked at least 27 weeks	(4) Worked at least 39 weeks and Full-Time	(5) Worked 1-52 Weeks and Currently Working	(6) Worked 1-52 Weeks and Trim at 1970 1 and 99 percentiles	(7) Worked 1-52 Weeks and Trim at 1980 1 and 99 percentiles
Means							
1960	-0.550	-0.526	-0.551	-0.535	-0.545	-0.486	-0.504
1970	-0.450	-0.443	-0.457	-0.455	-0.448	-0.412	-0.450
1980	-0.356	-0.338	-0.353	-0.341	-0.343	-0.305	-0.318
1990	-0.353	-0.303	-0.321	-0.293	-0.314	-0.313	-0.347
Medians							
1960	-0.493	-0.456	-0.465	-0.479	-0.465	-0.454	-0.465
1970	-0.403	-0.390	-0.403	-0.403	-0.382	-0.379	-0.403
1980	-0.330	-0.293	-0.342	-0.305	-0.322	-0.328	-0.327
1990	-0.373	-0.308	-0.356	-0.266	-0.325	-0.332	-0.372

Weeks worked refer to weeks worked last year, and full-time hours are defined as 35 or more hours computed from "usual hours worked last year" in 1980-90 and hours worked last week in 1960-70. Samples are restricted to respondents who were not self-employed during the census reference week and who had valid wage observations from last year. Means report: $E(\ln w_b) - E(\ln w_w)$ and medians report $Med(\ln w_b) - Med(\ln w_w)$. Non-workers are excluded from the estimation sample, and wages for those currently in the armed forces are imputed using the details described in the Data Appendix. Bootstrapped standard-errors were always less than (0.010) for means and (0.015) for medians.

Table 5: Selectivity Corrected Estimates of the Racial Wage Gap

	Observed		Corrected			
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Median	Matching	Median-O	Median-NJ	Median
Panel A: All States, all ages						
1960	-0.550	-0.509	-0.552	-0.510	-0.575	-0.566
1970	-0.450	-0.423	-0.453	-0.426	-0.476	-0.465
1980	-0.356	-0.342	-0.365	-0.351	-0.454	-0.422
1990	-0.353	-0.350	-0.370	-0.368	-0.516	-0.456
Panel B: All States, Excluding Incarcerated Sample						
1960	-0.549	-0.507	-0.550	-0.509	-0.555	-0.546
1970	-0.448	-0.419	-0.450	-0.421	-0.465	-0.444
1980	-0.351	-0.338	-0.359	-0.346	-0.428	-0.397
1990	-0.337	-0.338	-0.350	-0.351	-0.453	-0.419
Panel C: All States, Excluding Armed Forces Sample						
1960	-0.552	-0.507	-0.553	-0.509	-0.578	-0.570
1970	-0.453	-0.424	-0.456	-0.427	-0.480	-0.471
1980	-0.358	-0.335	-0.368	-0.345	-0.466	-0.436
1990	-0.356	-0.350	-0.373	-0.368	-0.530	-0.474
Panel D: Southern States						
1960	-0.654	-0.644	-0.652	-0.642	-0.690	-0.688
1970	-0.551	-0.549	-0.550	-0.548	-0.578	-0.577
1980	-0.405	-0.394	-0.410	-0.400	-0.482	-0.454
1990	-0.393	-0.404	-0.404	-0.417	-0.523	-0.481

Table reports difference of log offer-wages for blacks and whites. Observed mean and median are computed over the observed distribution of wages and covariates. Matching assigns all non-workers in each (year x race x age x education) cell the mean ln weekly wage, and Median-O assigns each nonworker the observed median. Median-NJ assigns all non-workers to below the pointwise median. Median assumes that only long-term nonworkers earn less than the cell median. See Section 2.1 of text for details. Bootstrapped standard-errors are always less than (0.010) for means and (0.012) for medians.

Table 6: Observed and Estimated Hourly Offer Wages (in 1997 dollars)

		Whites				Blacks			
		<HS	HS	HS +	Total	<HS	HS	HS +	Total
1980									
25-34	Observed Mean	$10.3	$12.7	$13.8	$13.0	$8.7	$10.3	$11.7	$10.4
	Observed Median	11.3	13.5	14.9	14.1	9.2	11.3	13.0	11.4
	Corrected Median	10.9	13.8	15.0	14.2	7.7	11.0	13.1	10.8
35-44	Observed Mean	12.5	15.5	19.5	16.5	10.0	12.3	15.3	12.1
	Observed Median	13.8	16.6	20.5	17.4	10.6	13.9	16.8	13.5
	Corrected Median	13.1	16.7	20.7	17.4	9.5	13.1	17.0	12.2
45-54	Observed Mean	13.6	16.4	21.2	17.0	10.3	12.9	15.6	11.8
	Observed Median	15.0	17.9	21.9	17.9	11.5	14.8	17.7	13.4
	Corrected Median	13.6	17.2	21.9	17.6	8.8	13.5	16.8	11.4
Total	Observed Mean	12.3	14.4	16.6	14.9	9.6	11.3	13.2	11.2
	Observed Median	13.6	15.7	17.5	16.0	10.4	12.2	14.5	12.3
	Corrected Median	12.7	15.8	17.5	15.9	8.6	12.2	14.8	11.3
1990									
25-34	Observed Mean	$9.0	$11.1	$13.5	$12.2	$7.1	$8.6	$10.9	$9.3
	Observed Median	9.6	11.7	14.2	12.9	7.3	9.0	11.2	9.9
	Corrected Median	8.8	11.7	14.7	13.1	4.9	8.3	11.7	9.1
35-44	Observed Mean	10.8	13.6	17.6	15.8	8.8	11.0	14.1	12.0
	Observed Median	11.5	14.4	18.5	16.9	8.9	11.8	15.6	12.9
	Corrected Median	10.0	14.2	18.6	16.4	6.8	10.7	15.4	11.7
45-54	Observed Mean	12.3	15.2	20.5	17.3	10.2	13.2	16.6	13.2
	Observed Median	13.3	16.2	21.9	18.7	10.8	14.4	17.8	14.3
	Corrected Median	11.1	15.9	21.2	17.6	8.1	12.4	17.1	12.0
Total	Observed Mean	10.5	12.8	16.4	14.5	8.6	10.1	12.9	10.9
	Observed Median	11.2	13.4	17.3	15.4	8.9	10.8	13.8	11.4
	Corrected Median	9.9	13.4	17.4	15.4	6.2	9.8	13.8	10.5

Hourly wages were computed by dividing weekly wages (observed or predicted) with the average number of hours worked in a week by workers in the relevant *year x race x education x age* cell. Self-employed workers are excluded from the analysis. Median assumes that long-term nonworkers earn less than the cell median. See Section 2.1 of text for details.

Table 7: Estimates of the Residual Racial Wage Gap

Panel A: All States

	Observed	Corrected			Observed	Corrected	
		Median-NJ	Median			Median-NJ	Median
With Contemporaneous White Weights				With Contemporaneous Black Weights			
1960	-0.416	-0.458	-0.451	**1960**	-0.439	-0.499	-0.490
1970	-0.319	-0.347	-0.339	**1970**	-0.338	-0.384	-0.375
1980	-0.258	-0.322	-0.298	**1980**	-0.271	-0.360	-0.333
1990	-0.269	-0.360	-0.319	**1990**	-0.278	-0.407	-0.355
With Fixed 1975 White Weights				With Fixed 1975 Black Weights			
1960	-0.396	-0.422	-0.418	**1960**	-0.409	-0.452	-0.445
1970	-0.310	-0.332	-0.325	**1970**	-0.323	-0.359	-0.351
1980	-0.265	-0.338	-0.313	**1980**	-0.280	-0.379	-0.353
1990	-0.275	-0.401	-0.351	**1990**	-0.290	-0.469	-0.399

Panel B: Southern States

	Observed	Corrected			Observed	Corrected	
		Median-NJ	Median			Median-NJ	Median
With Contemporaneous White Weights				With Contemporaneous Black Weights			
1960	-0.519	-0.553	-0.554	**1960**	-0.534	-0.570	-0.571
1970	-0.417	-0.438	-0.438	**1970**	-0.434	-0.449	-0.453
1980	-0.290	-0.346	-0.323	**1980**	-0.294	-0.358	-0.335
1990	-0.304	-0.371	-0.341	**1990**	-0.309	-0.391	-0.356
With Fixed 1975 White Weights				With Fixed 1975 Black Weights			
1960	-0.504	-0.535	-0.536	**1960**	-0.52	-0.555	-0.556
1970	-0.407	-0.43	-0.427	**1970**	-0.416	-0.434	-0.436
1980	-0.296	-0.353	-0.330	**1980**	-0.303	-0.368	-0.344
1990	-0.311	-0.386	-0.355	**1990**	-0.323	-0.412	-0.374

Table reports difference of log offer-wages for blacks and whites. All columns report estimates of the residual racial wage gap for both the observed and selection corrected data. Observed series refer to the observed pointwise median integrated over the skill distribution for workers. Median-NJ assigns all non-workers to below the pointwise median. Median assumes that only long-term nonworkers earn less than the cell median. See Section 2.1 of text for details. Bootstrapped standard-errors are always less than (0.014).

Table 8: Estimates of the Racial Wage Gap, by Birth Cohort

Panel A: All States

Observed Median

Birth Year	1960	1970	1980	1990
1905	-0.526			
1910	-0.547			
1915	-0.517	-0.508		
1920	-0.522	-0.508		
1925	-0.500	-0.462	-0.388	
1930	-0.469	-0.433	-0.379	
1935	-0.452	-0.421	-0.420	-0.341
1940		-0.380	-0.370	-0.329
1945		-0.297	-0.321	-0.304
1950			-0.283	-0.340
1955			-0.254	-0.343
1960				-0.370
1965				-0.294
All Cohorts	-0.504	-0.427	-0.333	-0.335
Within Cohort %		0.020	0.019	-0.009

Corrected-Median

Birth Year	1960	1970	1980	1990
1905	-0.642			
1910	-0.617			
1915	-0.575	-0.569		
1920	-0.575	-0.534		
1925	-0.556	-0.506	-0.546	
1930	-0.481	-0.480	-0.496	
1935	-0.520	-0.439	-0.495	-0.492
1940		-0.376	-0.446	-0.455
1945		-0.346	-0.378	-0.434
1950			-0.337	-0.442
1955			-0.333	-0.450
1960				-0.494
1965				-0.388
All Cohorts	-0.561	-0.460	-0.415	-0.451
Within Cohort %		0.026	-0.033	-0.051

Corrected with Concurrent White Weights

Birth Year	1960	1970	1980	1990
1905	-0.535			
1910	-0.524			
1915	-0.473	-0.419		
1920	-0.446	-0.379		
1925	-0.432	-0.381	-0.376	
1930	-0.363	-0.351	-0.351	
1935	-0.410	-0.310	-0.357	-0.311
1940		-0.249	-0.330	-0.320
1945		-0.243	-0.260	-0.289
1950			-0.248	-0.301
1955			-0.254	-0.342
1960				-0.387
1965				-0.275
All Cohorts	-0.448	-0.330	-0.299	-0.325
Within Cohort %		0.0203	0.019	-0.009

Panel B: Southern States

Observed Median

Birth Year	1960	1970	1980	1990
1905	-0.606			
1910	-0.648			
1915	-0.675	-0.630		
1920	-0.658	-0.643		
1925	-0.654	-0.599	-0.498	
1930	-0.577	-0.580	-0.460	
1935	-0.584	-0.541	-0.494	-0.439
1940		-0.521	-0.451	-0.449
1945		-0.371	-0.376	-0.433
1950			-0.297	-0.43
1955			-0.264	-0.385
1960				-0.367
1965				-0.261
All Cohorts	-0.631	-0.550	-0.383	-0.388
Within Cohort %		0.024	0.053	-0.052

Corrected-Median

Birth Year	1960	1970	1980	1990
1905	-0.651			
1910	-0.673			
1915	-0.725	-0.683		
1920	-0.740	-0.613		
1925	-0.706	-0.639	-0.568	
1930	-0.638	-0.631	-0.587	
1935	-0.646	-0.555	-0.545	-0.516
1940		-0.541	-0.505	-0.550
1945		-0.401	-0.425	-0.498
1950			-0.325	-0.507
1955			-0.305	-0.461
1960				-0.473
1965				-0.394
All Cohorts	-0.687	-0.576	-0.441	-0.480
Within Cohort %		0.051	0.022	-0.079

Corrected with Concurrent White Weights

Birth Year	1960	1970	1980	1990
1905	-0.531			
1910	-0.558			
1915	-0.612	-0.509		
1920	-0.593	-0.491		
1925	-0.555	-0.495	-0.366	
1930	-0.502	-0.475	-0.417	
1935	-0.526	-0.381	-0.390	-0.299
1940		-0.380	-0.367	-0.376
1945		-0.310	-0.296	-0.333
1950			-0.238	-0.359
1955			-0.241	-0.348
1960				-0.374
1965				-0.303
All Cohorts	-0.556	-0.430	-0.317	-0.347
Within Cohort %		0.065	0.031	-0.039

Table reports difference of log offer-wages for blacks and whites. Each birth cohort includes all persons born in the three-year interval centered on the reported birth year. Median assumes that long-term nonworkers earn less than the cell median. Within cohort percentage is the portion of the convergence in percentage points that can be explained using changes for continuing cohorts using the decomposition in Card and Krueger (1992). See text for precise details. Bootstrapped standard-errors are always less than (0.021).

Table 9: Racial Difference in Participation: Evidence from Differences in Offer Wages

Black Offer Wage Decile	Simulated Racial Gap (W-B) in Offer Wages				Observed Black Participation				Simulated White Participation			
	1960	**1970**	**1980**	**1990**	**1960**	**1970**	**1980**	**1990**	**1960**	**1970**	**1980**	**1990**
1	-0.021	-0.061	-0.069	0.014	0.654	0.651	0.500	0.332	0.733	0.772	0.754	0.672
2	0.004	0.001	0.007	0.013	0.592	0.636	0.472	0.448	0.708	0.726	0.660	0.659
3	0.004	0.003	0.006	0.009	0.727	0.716	0.596	0.588	0.752	0.766	0.699	0.728
4	0.003	0.010	0.006	0.003	0.719	0.767	0.663	0.607	0.754	0.805	0.718	0.712
5	0.003	0.006	0.004	0.000	0.815	0.826	0.801	0.760	0.808	0.795	0.799	0.786
6	0.007	0.003	0.003	0.005	0.876	0.888	0.817	0.807	0.799	0.866	0.829	0.842
7	0.000	0.004	0.004	0.005	0.903	0.905	0.871	0.853	0.852	0.882	0.876	0.885
8	0.009	0.004	0.004	0.002	0.911	0.912	0.875	0.873	0.875	0.929	0.905	0.907
9	0.008	0.008	0.006	0.004	0.921	0.918	0.911	0.898	0.946	0.947	0.930	0.931
10	0.054	0.050	0.033	0.054	0.877	0.879	0.874	0.892	0.954	0.947	0.940	0.953
Total	0.007	0.003	0.000	0.011	0.799	0.808	0.736	0.706	0.903	0.905	0.872	0.874

	1960	**1970**	**1980**	**1990**
1. Black Observed Participation Rate	0.799	0.808	0.736	0.706
2. White Observed Participation Rate	0.903	0.905	0.872	0.874
3. Difference	0.104	0.097	0.136	0.168
4. White Simulated Participation Rate	0.818	0.843	0.811	0.808
5. Predicted Difference: (2)-(1)	0.085	0.061	0.061	0.067
6. Explained Component: (5)/(3)	82%	63%	45%	40%

Offer wage distribution was computed with the Median estimator which assigns all long-term nonworkers to lie below the pointwise median respondent. Each non-worker was also assigned a random draw from a N(0,V) distribution whose variance was estimated pointwise. See text for precise details. Racial gap in offer wages is the difference of log offer-wages for blacks and whites. Computing white participation rates at relevant deciles of the black offer wage distribution generated simulated white participation. Predicted difference in the lower panel refers to that portion of the racial difference in participation that can be explained using differences in offer wages.

Appendix Table A1: Sample Sizes by Age x Schooling Cells

	Whites				Blacks			
	< than HS	HS	HS+	Total	< than HS	HS	HS+	Total
1960								
25-29	14,933	14,879	12,853	42,665	3,223	1,066	503	4,792
30-34	19,891	14,426	13,549	47,866	3,555	892	567	5,014
35-39	21,210	16,190	13,104	50,504	3,707	851	445	5,003
40-44	22,883	14,280	10,174	47,337	3,587	580	336	4,503
45-49	25,488	10,793	8,273	44,554	3,542	375	272	4,189
50-56	30,255	8,438	8,314	47,007	3,628	300	216	4,144
Total	134,660	79,006	66,267	279,933	21,242	4,064	2,339	27,645
1970								
25-29	11,175	20,708	20,117	52,000	2,604	2,215	1,021	5,840
30-34	11,608	17,520	15,302	44,430	2,597	1,657	694	4,948
35-39	13,457	15,617	14,557	43,631	2,890	1,306	710	4,906
40-44	17,985	14,885	14,107	46,977	3,237	1,048	581	4,866
45-49	19,204	15,726	13,226	48,156	3,439	907	430	4,776
50-56	24,493	16,602	11,944	53,039	3,828	664	381	4,873
Total	97,922	101,058	89,253	288,233	18,595	7,797	3,817	30,209
1980								
25-29	8,918	28,330	38,957	76,205	2,690	4,310	3,526	10,526
30-34	8,393	22,747	39,170	70,310	2,348	3,295	3,069	8,712
35-39	9,388	20,278	26,448	56,114	2,242	2,436	1,720	6,398
40-44	9,990	17,591	19,067	46,648	2,426	1,865	1,287	5,578
45-49	11,583	16,057	16,746	44,386	2,579	1,383	993	4,955
50-56	19,919	18,409	18,865	57,193	3,661	1,235	939	5,835
Total	68,191	123,412	159,253	350,856	15,946	14,524	11,534	42,004
1990								
25-29	7,751	28,090	39,081	74,922	1,748	4,436	3,579	9,763
30-34	7,563	29,134	43,977	80,674	1,716	4,082	3,970	9,768
35-39	6,195	23,457	46,601	76,253	1,632	3,324	3,680	8,636
40-44	6,216	19,435	43,366	69,017	1,552	2,564	3,040	7,156
45-49	7,518	18,154	30,202	55,874	1,622	1,858	1,772	5,252
50-56	10,080	19,145	25,218	54,443	2,075	1,805	1,480	5,360
Total	45,323	137,415	228,445	411,183	10,345	18,069	17,521	45,935

Authors tabulations from the PUMS data. No sample restrictions have been placed on the data. See Data Appendix for details of sample.

Appendix Table A2: Fraction of Prime Age Men without Skill Prices, and Fraction who are Long-Term Nonemployed

	Panel A: Fraction without Wage Observation From Last Year								Panel B: Fraction without Wage Observation From Last Year, currently not at work or school, and last worked three or more years ago							
	Whites				Blacks				Whites				Blacks			
	< HS	HS	HS+	Total	< HS	HS	HS+	Total	< HS	HS	HS+	Total	< HS	HS	HS+	Total
1960																
25-34	0.069	0.033	0.039	0.049	0.134	0.093	0.084	0.120	0.035	0.011	0.006	0.019	0.084	0.054	0.030	0.072
35-44	0.076	0.040	0.042	0.057	0.130	0.095	0.055	0.119	0.044	0.015	0.011	0.027	0.089	0.055	0.033	0.079
45-54	0.109	0.071	0.076	0.095	0.156	0.105	0.094	0.148	0.065	0.032	0.028	0.052	0.110	0.067	0.056	0.104
Total	0.087	0.045	0.049	0.066	0.140	0.095	0.076	0.128	0.050	0.017	0.013	0.032	0.094	0.056	0.036	0.084
1970																
25-34	0.068	0.024	0.030	0.037	0.132	0.071	0.060	0.099	0.042	0.012	0.006	0.017	0.086	0.042	0.018	0.059
35-44	0.066	0.025	0.020	0.038	0.125	0.068	0.041	0.101	0.043	0.014	0.009	0.023	0.089	0.044	0.021	0.069
45-54	0.100	0.046	0.035	0.067	0.155	0.091	0.072	0.138	0.069	0.029	0.020	0.044	0.117	0.061	0.056	0.103
Total	0.082	0.031	0.028	0.048	0.139	0.074	0.056	0.112	0.054	0.018	0.011	0.028	0.099	0.047	0.027	0.076
1980																
25-34	0.131	0.043	0.036	0.050	0.281	0.146	0.104	0.167	0.083	0.019	0.009	0.021	0.189	0.082	0.039	0.096
35-44	0.128	0.047	0.029	0.055	0.214	0.135	0.091	0.155	0.083	0.025	0.012	0.031	0.150	0.087	0.047	0.102
45-54	0.179	0.085	0.060	0.107	0.288	0.174	0.126	0.232	0.131	0.057	0.034	0.073	0.218	0.119	0.081	0.170
Total	0.152	0.056	0.039	0.067	0.264	0.148	0.104	0.180	0.105	0.031	0.015	0.038	0.189	0.090	0.048	0.116
1990																
25-34	0.178	0.061	0.035	0.058	0.392	0.209	0.101	0.198	0.112	0.030	0.010	0.026	0.269	0.121	0.044	0.116
35-44	0.222	0.080	0.041	0.067	0.339	0.200	0.110	0.188	0.160	0.047	0.018	0.038	0.238	0.129	0.058	0.119
45-54	0.243	0.104	0.061	0.104	0.322	0.208	0.122	0.219	0.184	0.069	0.036	0.070	0.253	0.152	0.076	0.161
Total	0.214	0.078	0.043	0.073	0.352	0.206	0.108	0.199	0.151	0.045	0.019	0.041	0.254	0.130	0.055	0.127

Source: Authors tabulations from the PUMS data for 1960-1990 (1990 data have been weighted). Self-employed are excluded from the table.

Appendix Table A3: Sensitivity of Weekly Wages to Sample Selection Criteria

Panel A: Fraction with Wage Observation who worked 1-13 Weeks Last Year

	Whites				Blacks			
	< HS	HS	HS+	Total	< HS	HS	HS+	Total
1960								
25-34	0.025	0.011	0.028	0.021	0.048	0.027	0.045	0.043
35-44	0.022	0.008	0.007	0.014	0.039	0.020	0.033	0.035
45-54	0.026	0.012	0.010	0.020	0.044	0.024	0.031	0.041
Total	0.024	0.010	0.017	0.018	0.043	0.024	0.038	0.040
1970								
25-34	0.020	0.008	0.025	0.017	0.034	0.022	0.033	0.029
35-44	0.015	0.007	0.006	0.010	0.023	0.021	0.013	0.021
45-54	0.017	0.009	0.009	0.013	0.026	0.018	0.008	0.024
Total	0.017	0.008	0.015	0.013	0.027	0.021	0.021	0.025
1980								
25-34	0.037	0.017	0.020	0.021	0.059	0.042	0.037	0.045
35-44	0.026	0.012	0.010	0.014	0.032	0.026	0.023	0.027
45-54	0.020	0.014	0.010	0.015	0.022	0.018	0.022	0.021
Total	0.026	0.015	0.015	0.017	0.037	0.033	0.031	0.034
1990								
25-34	0.054	0.024	0.020	0.024	0.084	0.064	0.045	0.060
35-44	0.037	0.021	0.013	0.017	0.060	0.039	0.028	0.038
45-54	0.030	0.016	0.013	0.017	0.034	0.027	0.024	0.029
Total	0.040	0.021	0.016	0.020	0.060	0.049	0.035	0.046

Panel B: Difference of E(ln w|weeks>13) and E(ln w|all weeks)

	Whites				Blacks			
	< HS	HS	HS+	Total	< HS	HS	HS+	Total
1960								
25-34	**-0.010**	-0.001	-0.009	-0.007	-0.009	-0.002	-0.014	-0.008
35-44	-0.009	-0.004	-0.002	-0.006	0.000	0.001	0.009	0.001
45-54	**-0.012**	-0.002	-0.005	-0.009	-0.003	-0.008	-0.020	-0.004
Total	-0.010	-0.002	-0.006	-0.007	-0.004	-0.002	-0.008	-0.004
1970								
25-34	-0.003	-0.001	-0.007	-0.004	0.002	0.008	0.007	0.005
35-44	-0.003	-0.002	-0.003	-0.002	0.008	0.001	0.001	0.005
45-54	-0.004	-0.003	-0.003	-0.004	0.003	-0.003	-0.004	0.001
Total	-0.003	-0.002	-0.005	-0.003	0.004	0.004	0.003	0.004
1980								
25-34	**-0.012**	-0.004	-0.007	-0.007	0.004	-0.010	**-0.011**	-0.007
35-44	-0.004	-0.002	-0.002	-0.003	0.000	-0.003	-0.004	-0.002
45-54	-0.003	-0.002	-0.004	-0.003	-0.004	-0.005	-0.010	-0.005
Total	-0.006	-0.003	-0.005	-0.005	0.000	-0.007	-0.009	-0.005
1990								
25-34	**-0.028**	-0.009	**-0.010**	**-0.011**	**-0.012**	**-0.032**	**-0.019**	**-0.023**
35-44	**-0.015**	-0.009	-0.008	-0.009	-0.006	**-0.018**	**-0.019**	**-0.016**
45-54	**-0.013**	-0.007	-0.009	-0.009	**-0.015**	**-0.013**	**-0.012**	**-0.014**
Total	**-0.019**	-0.008	-0.009	**-0.010**	**-0.011**	**-0.024**	**-0.018**	**-0.019**

Armed Forces sample has been excluded from the analysis. Bootstrapped standard-errors based on 100 replications (within year cluster) were computed for each cell. In Panel B bold type indicates that the difference is statistically significant at the 5 percent significance level.