# Is the Convergence in the Racial Wage Gap Illusory? New Estimates of the Butler-Heckman Hypothesis*

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**Abstract**

In the spirit of Butler and Heckman (1977), I demonstrate that the literature on the racial wage gap has systematically overstated the economic gains of African American men by ignoring their withdrawal from the labor force. I demonstrate the existence of an important source of bias which has contaminated studies that have made inferences based on Current Population Survey (CPS) data. These data have a truncated sampling design since they explicitly exclude sampling the growing incarcerated population. To recover the counterfactual distribution of wages for non-workers, I generalize the implementation of a non-parametric bounds estimator that does not require the use of arbitrary exclusion restrictions or functional form assumptions for identification. Using data from the US Decennial Census I find dramatically larger results for the empirical content of the Butler-Heckman hypothesis. In contrast to the sharp convergence in the observed gap between 1970 and 1990 for low-skilled workers, I find that the true gap actually diverged for these groups. Between 1960 and 1990, 60 percent of the convergence is driven by the selective-withdrawal hypothesis. Similar results are obtained for a cohort-based analysis. Despite these findings, I find that ignoring the relatively large incarcerated sample in 1960 caused previous analyses to understate the efficacy of the Civil Rights Act.

Keywords: Civil Rights Act, Incarceration, Racial Wage Gap
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# 1 Introduction

In a highly influential paper Richard Butler and James Heckman (1977) cautioned social scientists to look beyond the dynamics of the racial wage gap for workers, and argued that because of expansions in the generosity of transfer programs, the least-skilled blacks were systematically withdrawing from the labor force causing observed relative wages to increase. Therefore, a preoccupation with the wages of full-time workers may cause social scientists to overstate the success of Title VII Legislation, or spuriously conclude that discrimination against blacks has declined. The original Butler-Heckman paper and the subsequent article by Heckman (1989) forcefully emphasize the role of expanding transfer programs in reducing black participation rates. This hypothesis was used to demonstrate that Richard Freeman's pioneering paper in (1973) was not consistent with the EEOC's raising the relative demand for black labor: whereas Freeman found a significant effect on EEOC expenditures on relative wages, Butler and Heckman argued that Freeman's analysis was not consistent with the relative demand curve shifting to the right; an occurrence which would have raised both relative wages and employment. On the contrary, they found that transfer programs whose expansion coincided with the passage of the Civil Rights Act (CRA) had caused a substantial withdrawal of the lowest skill blacks. Butler and Heckman do find some evidence that Title VII Legislation improved the wages of younger blacks, however, they claim that there is little evidence to suggest that the intervention raised the relative wages of all blacks.

At the time of writing their paper, Butler and Heckman could not have anticipated the phenomenal increase in the returns to skill that would occur in the 1980s. Nor could they have predicted the massive growth in the US prison population. Therefore, in addition to expanding transfer programs, it is also possible that falling skill prices for the least skilled have reinforced the incentives to withdraw from the formal labor market, and increased the incentives to participate in criminal activity. In this paper, I shall refer to both reasons for withdrawing from the labor force as constituting the Butler-Heckman hypothesis since both forms of withdrawal reflect the more general idea that the least-skilled blacks are relatively less likely to be at work. This possibility will in turn manifest itself as convergence in observed wages.[1] It should be stressed however, that the original Butler-Heckman paper is only concerned with the role of expanding transfer programs (primarily the generosity of the disability program) in reducing the attachment of black males to the labor force.

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 by race. He found that even though published median earnings ratios converged from 0.59 in 1953 to 0.71 in 1978, the corrected ratios moved from 0.57 to only 0.61 over the same period. Under the identifying restriction that nonworkers earn

[1] 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, driven by growing incarceration rates have had a compositional effect. Under alternative estimates of what the counterfactual labor force participation rate for those incarcerated would be, they estimate that the true fall in the unemployment rate would have been between 2.1-2.5 percent.

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). Despite the magnitude of Brown's finding, the US. Commission on Civil Rights, remains 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)]."

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, Welch (1990) uses March CPS data and does not find support for this hypothesis.[2] 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 re used by Card and Krueger (1993) and Chay and Honore (1998).[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. Blau and Beller (1992) and Western and Pettit (1999) 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 considerably overstated when one accounts for the nonemployed. Using a pointwise matching estimator with CPS data, Juhn (1997) finds that the selective withdrawal of

[2] This is Welch's interpretation of his results and not mine. Because of the importance of Welch's study, I will discuss it in more detail in Section 2. Welch matches respondents to the March CPS in adjacent years. Conditional on a successful match he compares the earnings of workers who worked one year and not the next, or vice-versa, and does not find support for the hypotheses that these marginal workers received lower wages. This approach, while ingenious, biases his results because of the sample inclusion criteria 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. 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. However, in interpreting these results Professor Welch writes "...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.

[3] Vroman (1986) and Card and Krueger (1993) reject the selective withdrawal hypothesis based on analysis using longitudinal CPS-SSA data. Vroman is severely 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). Heckman's criticism of Vroman's sample may be construed to apply to Card and Krueger's (1993) analysis.

blacks reduces the observed convergence by one third over 1968-88. Most recently, Chandra (2000a,b) and Heckman, Lyons and Todd (2000) all find evidence that is consistent with the selective withdrawal hypothesis. Given the enormous significance of the selective-withdrawal hypothesis for understanding changes in the economic well-being of African Americans, as well 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 four levels:

1. An important source of contention comes from the datasets being analyzed. I demonstrate the importance of not relying on inferences made on the racial wage gap from data drawn from the CPS. 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.[4] This omission overstates the convergence over time because it ignores the role of increasing criminal activity as a response to changing wage structure. As Section II demonstrates, the explosion in the degree to which lower-skilled black men are incarcerated plays a pivotal role in contributing to the convergence in observed wages. With the exception of Chandra (2000a,b) the other papers use datasets that exclude the incarcerated population. The use of Census data allows me to capture the increasing fraction of incarcerated men; a group omitted from the CPS by sample design. Additionally, the Census data allows the analysis to go back to 1960– prior to the enactment of Title VII legislation– a feature that is not possible with CPS microdata.

2. I examine a previously unstudied aspect of the racial wage gap by focusing on the significant role of employment in the armed forces in compressing the racial wage gap. 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. In the sociology literature, Mare and Winship (1984) provide evidence that some of the most able blacks are in the military. The military sample is typically excluded from most analyses of labor markets because the CPS does not collect earnings data on this sample. Even if this data were available, it is difficult to make comparisons across the wage and salary and military samples since a large proportion of compensation for the armed forces is "in kind." This omission biases empirical estimates of the racial wage gap in a manner that runs contrary to the selective withdrawal hypothesis– if the most skilled blacks

[4] Published tables of earnings by race, such as the U.S. Census Bureau's *Current Population Reports*, Series P-60, add an additional restriction as they are based on a sample of workers who worked last year and also during the reference week in March.

are in the military, then including them in the analysis should raise mean and median offer wages.[5]

3. This paper develops an easily operationalizable approach to studying the size of the selective withdrawal hypotheses. The method follows in the spirit of work by Brown (1984), and builds on 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 either an exclusion restriction (a variable that affects labor force participation but does not affect wages), or functional form assumptions 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. Therefore, contrary to matching estimators, it allows nonworkers to earn less than workers even after controlling for their observable characteristics.

4. The results from the first three sections are also used to study whether the selective withdrawal hypothesis also holds for a within-cohort analysis. A within-cohort analysis has the advantage of controlling for unobserved factors such as differences in the quality of schools attended by blacks and whites, and allows for observing behavior over the life cycle, factors that Chay and Honore (1998) argue are of importance because across-cohort analysis may be contaminated by across-cohort vintage effects in participation.

Throughout this paper I study outcomes for prime-age men (those aged 25-55). These age-restrictions were chosen to make sure that the results for young blacks were not contaminated by attendance in college, or, at older ages the growing phenomena of early retirement. This paper is outlined as follows. Given that much of the debate on the empirical content of the selective withdrawal hypothesis centers as much on the data being used, as on the use of alternative methods it is imperative to fully understand both in detail. The next section presents a discussion of the facts to be explained and provides evidence in favor of points (1) and (2) above. In Section III, 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 hypotheses. I develop the bounds estimator used in this paper and demonstrate how it is nested within conventional selection models. Section IV presents empirical results and Section V offers concluding comments. The Data Appendix describes standardizing assumptions that were used in order to make the census data comparable across different years.

[5] Brown (2000) is an exception to the common practice in the economics literature of ignoring the military sample. In his paper, Brown examines the extent to which the relatively equal and integrated environment in the military affects outcomes for the children of military families.

## 2 Revisiting the Butler-Heckman Hypothesis

The original Butler-Heckman (henceforth BH) paper and the subsequent article by Heckman (1989) emphasize the role of expanding transfer programs in affecting black participation rates. The essence of the BH thesis is best summarized in their own words:

> "Macro relative wage and income data are quite sensitive to the relative number of blacks in the labor force and to the composition of the black workforce. As the relative number of blacks in the workforce declines, and as low wage blacks are siphoned out of the labor force by transfer programs, measured relative wages of blacks tend to rise. Such growth in relative black status has nothing to do with a lessening of discrimination against blacks [Butler and Heckman (1977)]."

This hypothesis was used to demonstrate that the results in Freeman (1973) which were based on macro-data were not consistent with the EEOC raising the relative demand for black labor: whereas Freeman's pioneering paper found a significant effect of EEOC expenditures on relative wages, Butler and Heckman argued that such expenditures should have also raised relative employment in addition to black wages. On the contrary, they found that transfer programs whose expansion coincided with the passage of the CRA had caused a substantial withdrawal of the lowest skill blacks.[6] This section reviews the empirical evidence in favor of various dimensions of this hypothesis. Its primary goal is to demonstrate the extreme sample-selection bias that results if the wrong sample is used to evaluate black economic progress.

### 2.1 Racial Differences in Wages and Employment

Figure 1 demonstrates that not conditioning on any variables, in 1940 black men's weekly wages were 48.1 percent of white men's wages. By 1990 this number had increased to

[6] It is beyond the scope of this paper to review the entire literature on on the efficacy of Title VII of the CRA which passed in 1964 and went into effect on July 2, 1965, or that of President Johnson's Executive Order 11246 in 1965 which formed the Office of Federal Contral Complicance (OFCCP). For an introduction to this subject see Chapter 6 of the National Research Council commisioned volume *A Common Destiny: Blacks and American Society* [Jayes and Williams (1989)], and the rigorous reviews by Brown (1982) and Donohue and Heckman (1991). In an imporant contribution to this literature, Chay (1998) provides an excellent review of the historical facts on this subject and demonstrates that the 1972 amendment to the CRA which in part extended coverage to firms with 15-24 employees was extremely successful.The central debate in this literature focuses on the appropriateness of studying the racial gap across covered and uncovered sectors: if markets are competitive one would *ex ante* expect no difference; a result which is also consistent with the civil-rights legislation having no effect. On the other hand, if firms hire in a common labor market, OFCCP induced hiring could simply result in greater employment in the covered sector with accompanying losses in the uncovered sector. By focusing on one industry (textiles in South Carolina) that had a history of excluding Blacks, Heckman and Paynor (1989) circumvent many of these identification problems and provide clear evidence that the EEOC and OFCCP provided a major impetus to improving black relative employment. Chandra (2001) attempts to reconcile the puzzles described in Brown (1982) and studies the post-CRA process by which southern employers were able to successfully hire black workers despite the fact that EEOC budgets were low, enforcement was weak, and public opposition to the legislation was high.

73.5 percent– a dramatic improvement of well over 50 percent over five decades, although the improvement from 1980 to 1990 was essentially zero. Figure 1 also demonstrates the phenomenal convergence in black-white earnings that occurred over the decade of 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. Figure 1 also plots employment/population ratios for prime age men by race. 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. It is clearly apparent from this figure that the emp/pop ratio for prime-age blacks has fallen much faster than that for whites.

In Figure 2, I separate the analysis by three broadly defined schooling groups. One point is immediate: as the quote from the original Butler-Heckman paper notes, 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. Because college graduates are also the most likely to be full-time and full year workers, 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. For example, Juhn, Murphy and Pierce (1991) use CPS data and limit their sample to respondents who usually worked full-time and participated in the labor force for at least thirty-nine weeks. This sample restriction will have the likely consequence of limiting their analysis to studying the figure for college graduates. It should be noted that Figure 2 is broadly supportive of the selective withdrawal hypothesis: in the first panel employment rates for black men are seen to have plummeted relative to whites. Even though the same general pattern is observed for higher schooled groups the withdrawal has not been as dramatic.[8]

In both Figures 1 and 2 the timing of large increases in nonemployment appears to be correclated with increases in measured convergence.

[7] Goldin and Margo (1992) label the 1940s as the "Great Compression," and provide a fascinating inquiry into 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 the previously discussed Executive Order 8802 opened up jobs to blacks that they were previously excluded from. Margo also identifies black migration to the north and the retirement of older black cohorts as being contributing factors.

[8] There is an important caveat to keep in mind in interpreting the results of Figure 2: there have been enormous improvements in the relative quantities of black schooling. For example, Chandra (2000b) shows that 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. A within-cohort analysis, as pursued in Section 4, circumvents these problems.

## 2.2 Racial Differences in Incarceration and Nonemployment

In Tables 1 and 2, 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 displays the fraction of men in the Census reference week who were institutionalized and Table 2 adds to this fraction by also including those who were unemployed or not in the labor force.[9] The tables are separated by 6 age x 3 schooling cells, and the rows and columns labeled 'Total' report the relevant marginal distributions. In Table 1, I have also used unpublished data for 2000 from the Bureau of Prisons to demonstrate the growing trend in the incarceration rate of young blacks.[10] 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. However, 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%. The increase for the 'least-skilled,' those who were the youngest (aged 25-35) and were also dropouts, is well over 300%. Comparing the 2000 data to that for 1990 we see the incarceration rate for black men ages 25-29 has climbed to 13.1 percent from 9.5 percent (an increase of almost 40 percent!).Whereas there are substantial increases for whites in the same age and schooling cells, the increase is not as dramatic.

From Table 2 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 rate in excess of 50 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 phenomena that occurred over the 1980s. The results of Tables 1 and Table 2 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 4A. The distinction

[9] 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. The reported statistics are estimated very precisely.

[10] 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 0.50. Furthermore, it can be verified that the 'incarceration' rates that I report in Table 1 are virtually identical to those obtained in Western and Pettit's (2000) careful analysis (compare their Table 3 to my Table 1).

arises because weekly wages in the Census data are computed by dividing annual earnings last year by weeks worked last year. Tables 1 and 2 report nonemployment rates during Census reference week; many of those currently nonemployment may have worked during the previous year and will therefore have legitimate observations for weekly wages.

Whereas the PUMS data are at this point only available through the 1990 census, incarceration rates have continued to grow throughout the 1990s. In Figure 3 I use data from the US Bureau of Prisons to document the continuing increase in the incarceration rate. The data used in Figure 3 represent race-specific incarceration rates for all men over the age of 18 and not those between the ages of 25 and 55. Therefore, the reported incarceration rate will be slightly lower than that reported in Table 1.[11] Together, Figure 3 and Table 1 demonstrate the continued (and growing) selection bias that the incarcerated sample poses for inferences made on CPS based data.

When we combine these results with those from the returns-to-skill literature it becomes obvious that previous analysis has overstated the convergence for at least two reasons. First, consider the immense literature on the returns to skill that is rigorously reviewed by Katz and Autor (2000). It should be noted that if the least skilled do not work, then much of the literature that Katz and Autor discuss has actually understated both the degree to which the US wage inequality grew in the 1980s and 1990s. For example, Katz and Autor (2000) demonstrate that skill prices as proxied by weekly earnings actually fell in real terms for low-skilled men over the period 1963-95, with almost all of the real decrease taking place over the 1980s and 1990s (see Figures 1 and 2 in Katz and Autor (2000, p.1471)). Katz and Autor utilize a conventional sample of full-time (at least 35 hours) and full year workers (at least 40 weeks). However, as Table 2 illustrates, a substantial portion of the low-skilled population would be eliminated in such a sample. This fact would suggest that the real wage declines that Katz and Autor report may have far more severe than previously thought. As such, the selective withdrawal hypothesis has implications for the literature on skill-biased technological change. Second, for the same reasons discussed above, the true increase in the return to skill would have been more than previously thought. This is because the returns to skill, $r$, are typically measured as some variant of $r = ln(\overline{w}|Schooling = S) - ln(\overline{w}|Schooling = S-1)$. Omitting the offer wages of nonworkers biases the second quantity upwards and $r$ downwards.

## 2.3 The Role of Disability Benefits

For the purpose of historical accuracy it should be noted that labor economists at the U.S. Federal Government were actually the first to note the connection between disability benefits

[11] It is of critical importance to compute the incarceration rate by including all persons who are in Federal and State prisons, as well as those housed in local jails. This last group has been growing over time as a Federal and State prisons are operating at capacity and divert some of their caseload to the local jails. This is the approach taken in Western and Pettit (2000), but is not the sample reported in the commonly referenced Bureau of Prisons annual publication *Prisoners in 2000* (various years are available). This publication reports (race x gender x age) specific incarceration rates only for the prison population and excludes the jail population.

and labor force withdrawal but failed to suggest a connection with this withdrawal and the putative success of Title VII Legislation. Their contributions have so far not been acknowledged in the literature: 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) definitional 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 4 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 the trends are worth noting. First, for all three age groups under consideration, there were declining trends in LFP that had begun much before the passage of the CRA. These declines began well before 1955; before any one could have anticipated the passage of the Great Society's programs. Second, the relative declines are greatest for those men aged 45-54 and not younger men. Furthermore (by examining the second panel), the largest increases in the percent of men on disability have occurred for those aged 45-54. For younger age groups while there were large expansions in the fraction of men receiving disability benefits, the increase in recipiency not large enough to explain the corresponding declines in LFP. Therefore, while there is certainly evidence to support the central contention of the BH 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.[12]

## 2.4 The Role of the Armed Forces

Table 3 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).[13]

[12] Autor and Duggan (2001) effectively demonstrate that the growing generosity of the disability system over the 1980s has lowered the observed unemployment rate by two-thirds of a percent primarily by inducing high-school dropouts to exit the labor force directly instead of entering unemployment. Their research, even though it abstracts from race, is in the spirit of the original Butler-Heckman hypothesis and suggests that the link that BH posited for the 1960s may actually have been more empirically relevant for the 1980s.

[13] In using the Armed Forces sample for regression analysis 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

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. Similar to Tables 1 and 2 there is a noticeable age-enlistment profile, with men of younger ages being most likely to be in the Armed Forces. 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 falls to 5.5 percent by 1970. To the extent that higher 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 Title VII Legislation. The degree to which this bias matters is an empirical question and will be studied in detail in Section 4. Table 4 also demonstrates 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.[14]

### 2.5 Reexamining Welch (1990)

This section studies the role of potential sample selection bias in affecting the results of Welch (1990). As previously discussed, Welch matched respondents over adjacent years of the CPS. Conditional on a successful match, Welch concludes that there is little support for the hypothesis that those who exited the labor force (or those who entered) earned less than those who stayed in the labor force. By construction, Welch is identifying the "marginal" worker and basing his inferences on the marginal worker's earnings. There are however, a growing fraction of men who have not worked in a long time; discarding the selective withdrawal hypothesis on the basis of the marginal worker may not the appropriate test of the theory. These men will be excluded from Welch's analysis as they would have missing wage observations in both years of the match (this approach is also used in Smith and Welch (1986), but they do not publish their results).

In Table 4, 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. Notice that *at best* the Welch analysis can only capture those respondents in the two row of any panel. The rest are excluded from Welch's analysis by design. Such selection can be particularly problematic

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. 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 were earned significantly less than observationally equivalent wage and salary workers. As Appendix 2A demonstrates these differences are never statistically significant. I have also experimented with assigning the armed forces sample wages above the pointwise median but my results are insensitive to this adjustment.

[14] Mare and Winship (1984) demonstrate that there was a massive 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 idential enlistment rates. There was a much smaller increase for men aged 24-29; in this group black men were more likely be enlisted.

when the number of long-term nonemployed has been growing steadily over time. In 1990, Welch's analysis would have excluded well over 50% percent of black nonworkers. In 1980, he would have excluded about 50%. The fraction of whites excluded by Welch's matching method is high (35% in 1980 and 40 % in 1990) but not as high as the fraction of blacks excluded. The trends described in Table 5 are troublesome: in 1960 only 4 percent of prime aged blacks had never worked. In the 1980s and 1990s that percentage had grown to 10%. Over time there has been a steady increase in the fraction of blacks who last worked many years ago. When this fact is combined with the lessons learned in Lynch (1989) the sanguine picture that measured progress in the racial wage presents turn sour. Lynch demonstrates that the longer one is out of work the less likely they are 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.[15]

## 3 Econometric Statement of the Problem

To place the BH hypothesis in an econometrically tractable framework, I rely on the role of the distribution of offer wages as important in measuring the racial wage gap. Offer wages in the formal sector of the economy are of interest in comparing racial differences in economic progress because they represent the skill price received by economic agents. In competitive labor markets the skill price received by a price-taking agent reflects the quality and quantity of human capital (both observable and unobservable) that an agent possesses. Convergence in the offer wages of similar blacks and whites therefore represents convergence in their endowments of skill (acquired or inherited) and may also reflect a reduction in employer prejudices.

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). Using Smith and Welch's (1986) insight, I can invoke the law of total probability, and express the latent density of unconditional offer wages $f(w^o_{it})$ for agents from race $i$ in year $t$ as

$$f(w^o_{it}) = f(w^o_{it}|z = 1)\Pr(z = 1) + f(w^o_{it}|z = 0)\Pr(z = 0) \tag{1}$$

Here, $f(w^o_{it}|z = 1)$ is the observed distribution of wages, and $f(w^o_{it}|z = 0)$ is the unobserved distribution of offer wages to the nonemployed. In other words, it is the distribution of wage offers that they would be offered if they sought employment.[16] $\Pr(z = 1)$is the

[15] I have also stratified the results of Table 4 by education and experience. As one would expect, it is the least-skilled who are most likely to constitute the long-term unemployed and never-worked population. For example, in 1990 only 16 percent of black men with less than a high-school degree had worked (down from 33 percent in 1960). The fraction of this group that has never worked, or worked more than 10 years ago has grown from 17 percent in 1960 to 28 percent in 1990. These results are available from the author on request.

[16] The "experiment" here is to ask what is the offer wage that each nonemployed agent would get if he chose

proportion of workers in the economy. Instead of focusing on the entire distribution, I focus on the mean wage offer in this paper:

$$E[w_{it}^o] = E[w_{it}^o|z=1]\Pr(z=1) + E[w_{it}^o|z=0]\Pr(z=0) \tag{2}$$

An analysis of the racial wage gap, that ignores the Butler-Heckman thesis, focuses on the trajectory of racial differences in the earnings distributions or ratios for workers (for example, by studying the trajectory of $E[w_{bt}|z=1]/E[w_{wt}|z=1]$. A test of the Butler-Heckman thesis consists of determining whether comparing $f(w_{bt}^o)$ to $f(w_{wt}^o)$ yields different results from comparing $f(w_{bt}^o|z=1)$ to $f(w_{wt}^o|z=0)$. The only quantity not identified by the data is $f(w_{it}^o|z=0)$ in (1) or $E[w_{it}^o|z=0]$ in (2). Therefore, the social scientist must make assumptions about the data generating process which determines this distribution or parameter. It is important to note that the BH hypothesis is empirically relevant only if the relative demand curve for black labor is not perfectly elastic. If it is, then even though $f(w_{bt}^o|z=1)$ may differ from $f(w_{wt}^o|z=0)$ there will be no price adjustment in the labor market. In this section, I discuss alternative identifying restrictions that have been invoked in the literature to recover this quantity, and simultaneously discuss the economic content of these approaches.

### 3.1 Control Function Estimation

In a series of pioneering papers, Heckman (1976, 1979) developed this class of models in parametric framework. The power of these models is that the role of unobservables is explicitly formalized in a rationalizable economic model in which purposeful economic agents make work decisions that are optimal for them. In subsequent work Heckman and Robb (1985, 1986) develop the analytics of selection problem in its most general form. Most recently, Heckman, Ichimura, Smith, and Todd (1998) (henceforth HIST) use data from an experimental control group to study the empirical content of alternative characterizations of selection bias and find support for this characterization of selection bias.

In the HIST framework, assume that the latent distribution of log offer wages is given by:

$$w_k^o = \Gamma_1(X) + \epsilon_k \tag{3}$$

Recall that we only observe wages for the $k$th individual if $w_k^o > w_k^r$. 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

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. Identifying the role of general equilibruim effects is critical for the development of labor market interventions and public policies designed to increase the labor supply of targeted groups, and this is an important avenue for future research.A related extention that has been overlooked in the literature is to apply the insights of Heckman and Sedlacek (1985, 1990) to studying the selective withdrawal hypothesis. Doing so would be an important first step towards estimating the general-equilibrium model.

variables that comprise an exclusion restriction ($E$). Therefore, we observe offer wages if $I^* > 0$ and do not otherwise.[17] 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:

$$
\begin{aligned}
E[w|X, z &= 1] = \Gamma_1(X) + E[\epsilon|v > -\Gamma_2(R)] \\
E[w|X, z &= 0] = \Gamma_1(X) + E[\epsilon|v < -\Gamma_2(R)]
\end{aligned} \tag{4}
$$

More generally, what is required is an expression for $E[\epsilon|v, \Gamma_2(R)]$, termed a control function by Heckman and Robb (1985, 1986), $K(\Gamma_2(R))$, which will depend on $\Pr(z = 1|R)$ and the joint density $h(\epsilon_k, v_k)$. Knowing the control function allows the economist to recover $\Gamma_1(X)$ uniquely.[18] When the form of the joint density $h(\epsilon_k, v_k)$ is unknown we can use a control function to approximate the unknown function. For example, by following the logic in Andrews (1991) in the class of global approximations for $h(\epsilon_k, v_k)$, two candidates are a polynomial expansion of $\Gamma_2(R)$, where the number of approximating terms increases with sample size, or a Fourier series expansion of the index $\Gamma_2(R)$. It is trivial to show that an exclusion restriction is necessary if the control functions are estimated using nonparametric or semiparametric regression.[19]

[17] In reality, it may not be correct to treat all nonworkers as having similar offer wages. For example, Flinn and Heckman (1983) demonstrate that unemployment and being out of the labor force are two distinct states. In this paper, the presence of institutionalized and incarcerated workers adds to this complication. To accomodate the possibility of $e$ different employment states, and $n$ different nonemployment states (1) can be modified and rewritten as $E[w_{it}] = \sum_e [P_{et}] E[w_{et}, z = 1] + \sum_n [P_{nt}] E[w_{nt}, z = 0]$.This is the approach taken in Chandra (2000b) who uses the method of simulated moments to estimate a multinomial probit model. The instability of Chandra's estimates to ad hoc normalization restrictions on the covariance matrix (not required in theory, but in reality required for computational purpuses) cautioned me against pursuing this approach.

[18] Performing the analytics for the general case, the control-functions have the following forms:

$$
\begin{aligned}
E[\epsilon|v &> -\Gamma_2(R)] = \frac{\int_{-\infty}^{+\infty} \epsilon \int_{-\Gamma_2(R)}^{+\infty} h(\epsilon, v) dv d\epsilon}{\int_{-\Gamma_2(R)}^{+\infty} h(v) dv} \\
E[\epsilon|v &< -\Gamma_2(R)] = \frac{\int_{-\infty}^{+\infty} \epsilon \int_{-\infty}^{-\Gamma_2(R)} h(\epsilon, v) dv d\epsilon}{\int_{-\infty}^{-\Gamma_2(R)} h(v) dv}
\end{aligned} \tag{5}
$$

If the form of the joint density $h(\epsilon_k, v_k)$ is known, substituting estimates of $\Gamma_2(R) = F_v^{-1}(\Pr(z = 1|X))$ into the above yields an explicit functional form for the unknown expectation.In conventional applications, the empirical literature typically assumes that $\Gamma_1(X)$ and $\Gamma_2(R)$ are linear functions of the parameters, restricted further through a Mincerian specification, that $\epsilon_k$and $v_k$are iid and distributed as $N(0, \Sigma)$, and that $F_v(.)$ is the gaussian cumulative. In this framework, the participation decision is correlated with earnings only through $\epsilon_k$. The parametric model yields an explicit functional form: $E[\epsilon|v > -\Gamma_2(R)] = \frac{\sigma_{\varepsilon\nu}}{\sigma_v^2}\left(\frac{\phi(R\gamma)}{\Phi(R\gamma)}\right)$. For identification it is also necessary to assume that $\sigma_v^2 = 1$, implying that a test of selectivity is the regression coefficient on the inverse mills ratio term, which measures $\sigma_{\varepsilon\nu}$.

[19] An additional step in the estimation of the control functions through semi-parametric estimation is to

The parametric selection model 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, and use 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 for those aged 35-55. Using PUMS data from 1980 and 1990 I was not able to reconstruct this result, and note that the maginitude of second-stage coefficients is extremely sensitive to the specifications used for the participation probit as well as the wage equation. Heckman, Lyons and Todd (2000) (henceforth HLT) recommend the 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 home, and state level unemployment and welfare participation rates as exclusion restrictions. It is unclear how some these variables are constructed for the incarcerated sample. Using a semi-parametric binary choice model, Chandra (2000b) notes that the HLT exclusion restrictions wreck havoc in the wage predictions for nonworkers in 1950. Note however, that HLT only use these exclusion restrictions for 1960 onwards. The difficulty of justifying legitimate exclusion restrictions for prime-age men cautioned me from pursuing this approach.

### 3.2 Pointwise Matching

Models of index sufficiency nest matching models under the restriction that conditional on a set of observable characteristics, mean offer wages are the same for the employed and nonemployed. Formally:

$$E[w_{it}^o|X, z=1] = E[w_{it}^o|X, z=0] \Longrightarrow E[w_{it}^o|X, z] = E[w_{it}^o|X] \tag{6}$$

This assumption defines a pointwise (nonparametric) matching estimator and conditional on the observables assumes ignorable selection because $w_{it} \perp z|X$, where $\perp$ denotes the notation for conditional independence. If this assumption is invoked along with equation (4) we get $E[\epsilon|v > -\Gamma_2(R)] = E[\epsilon|v < -\Gamma_2(R)] = 0$. This assumption implies that (pointwise) workers and nonworkers have identical mean offer wages. From an economists perspective it is also crucial to note that Heckman and Vytlacil (2001) demonstrate that the identifying assumptions behind this class of models are not consistent with a Roy model of selection.[20]

Matching is operationalized in the work of Juhn (1992, 1997), who imputes wages for nonworkers 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 the relationship between wages, experience

recover the constant term in the control function, a step which is sensitive to bandwidth selection rules. Heckman (1990) provides a simple method to do so, by appealing to the notion of identification at infinity.

[20] Angrist (1998) provides an example of the type of data needed to invoke the use of a matching estimator. Angrist is interested in controlling for differences between verterans and nonveterans, all of whom applied to join the volunteer forces between 1979-83. Because the military is known to screen applicants on the basis of age, schooling, and test scores, Angrist is able to match on these variables and hence justify the use of (6).

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.[21]

It is apparent from (6) that the degree to which the researcher conditions on $X$ improves the quality of the match.[22] 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 as well as the nonworkers in group $j$. However, as the results of Section 2 have demonstrated, reliance on CPS data substantially biases the count of $N_j^{0-13}$downwards. This is because the actual number of nonemployed agents in group $j$ includes the incarcerated population which is excluded as part of the CPS sampling frame.

An alternative to this approach would be the use of the "Cell Minimum" estimator which is motivated by Manski (1995) and used in Chandra (2000a). Here all non-earners in a cell are assigned the wages of the lowest earner in each cell to all non workers in that cell. Under the critical assumption that the offer wage distribution for the nonemployed shares the same supports as that for workers, it is possible to bound $E[w_{it}|X]$. More specifically, assume that the density $f(w_{it}|X)$ is defined on the interval $[\underline{w}, \overline{w}]$ . Then the unknown quantity $E[w_{it}|X, z = 0]$ must lie between $[\underline{w}, \overline{w}]$ . If we assume that the supports of the wage distribution for workers provide us with an estimate of $[\underline{w}, \overline{w}]$ ,we may bound $E[w_{it}]$ as $\beta_{it}^U \leq E[w_{it}] \leq \beta_{it}^L$ where, $\beta_{it}^L = \Pr(z = 1)E[w_{it}|z = 1] + \Pr(z = 0)\underline{w}$ and $\beta_{it}^U = \Pr(z = 1)E[w_{it}|z = 1] + \Pr(z = 0)\overline{w}$. To be absolutely clear, note that the supports of the conditional offer wage distribution $[\underline{w}, \overline{w}]$ will change depending on the group under consideration. It should be noted that there might be considerable sampling variation within each of these cells, and that the use of the cell "max" or "min" is extremely sensitive to the sample under study.

[21] It is possible to simplify the complexity of the method of matching by reducing the dimensionality of the problem by comparing people who are similar in terms of their propensity to work, or propensity score. Define $P(X) = Pr(z = 1|R)$. This approach collapses the complexity of the matching problem from a multidimensional problem to a scalar problem. If (6) is satisfied then we may also write $f(w_{it}) \perp z|P(X)$. This in turn implies that $E[w_{it}|P(X), z = 1] - E[w_{it}|P(X), z = 0] = 0$. The advantage of propensity score matching is that it provides a mechanism to match workers to nonworkers when some or all of the $X$'s are continuous and the functional form between the the dependent variable and $X's$ is known. It is of paramount importance to note that the propensity score estimator shares the same limitations as the pointwise matching estimator: the social-scientist must be able to assert that the $X$'s adequetly control for the participation decision.

[22] To see this better, notice that the most simple approach of approximating $f(w_{it}|X, z = 0)$ with $f(w_{it}|X, z = 1)$ will result in imposing the restriction that workers do not differ from non-workers in terms of their unobservables Essentially, what is required (behaviorally) is that the decision to work is determined by a draw from a Bernoulli distribution. To operationalize this requirement econometrically one would need to condition on an extremely rich set of covariates. Using experimental data, HIST (1998) demonstrate that this is only possible with a rich-set of very high quality covariates.

### 3.3 Regression Matching

Unlike Juhn's analysis, identification in studies such as Blau and Beller (1992) is implicitly achieved through the use of regression matching, an assumption which imposes the additional restriction of linearity.[23] Regression matching is equivalent to pointwise nonparametric matching if the regression functional form is the correct form. In the Blau and Beller model:

$$\begin{aligned} E[w_{it}^o|X, z &= 1] = X\beta \\ E[w_{it}^o|X, z &= 0] = E[w_{it}^o|X, z = 1]\kappa \end{aligned} \tag{7}$$

This approach has the advantage of allowing for nonworkers to be different from workers, even when the observable characteristics are accounted for– through the use of a correction factor $\kappa \in [0, 1]$. Hence, this class of models does allow for a highly parameterized form of "selection on unobservables" where every nonworker earns a fixed amount less than an observationally equivalent worker– note that $\kappa$ is independent of $X$.

Blau and Beller experiment with *a priori* values of $\kappa$ such as 0.6 and 0.8. In contrast to this approach, models of "index-sufficiency" with an exclusion restriction allow for unobservable characteristics to affect different workers differently. This is because the control function in (4) varies even within $X$ because of variation obtained through $R$. In the absence of an exclusion restriction, models of index-sufficiency collapse to a version of (7) where the parametric control function is determined by the data. Therefore, regardless of whether an exclusion restriction exists, models of index sufficiency allow for a richer characterization of selection bias than that obtained by invoking (7).

Regression matching is also operationalized in the work of Western and Pettit (1999). In this paper the authors supplement data from the Outgoing Rotations of the CPS from 1982-1996 with data from the Survey of Inmates of Federal Correctional Facilities and the Survey of Inmates of State Correctional Facilities. They first estimate a wage hedonic for workers (using age, education, marital status, and region indicator variables). Using the estimated parameters and means of these variables for nonworkers they compute predicted earnings for the incarcerated sample. The authors use a $\kappa$ of 40 percent. This is also the approach taken in Smith and Welch (1986) and Welch (1990) who instead of using an *ad hoc* correction factor, compare the wages of respondents who could be matched across two adjacent years of the March CPS in order to estimate $\kappa$.

[23] Blau and Beller first estimate race-gender-experience wage regressions for workers and then predict wages for non-workers (using the nonworker's sample means). Because of their reliance on CPS data Blau and Beller do not sample the incarcerated populations. The assumption of linearity is more restrictive than allowing a nonparametric relationship between the offer wages and the observables characteristics. To allow for the possibility that nonworkers differ from workers along unobservables, they experiment with reducing the predicted wages by a correction factor of 0.8-0.6. Empirically, the choice of this adjustment factor is not found to dramatically alter their estimates.

### 3.4 Matching with Selection On Unobservables: Intuition

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) 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. Conditioning on these variables produces matches that are extremely crude relative to those that would have been obtained by using the set of covariates available to most Human Resource departments. 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 program administrator or potential employer are unobservable to the econometrician.

The estimator developed in this paper explicitly recognizes the limitations of the kinds of data that are presently available for social-science research. In order to control for the unobservable 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 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. 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. In contrast, I assume that if A were to work, he would earn less than the median person in the distribution of wages for 30yr 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 55yr old who are high-school dropouts. Note that I do not need an arbitrary exclusion restriction or reliance on functional form to achieve identification. As such my estimator is a (pointwise) nonparametric version of the approach discussed in Neal and Johnson (196) and Johnson, Kitamura and Neal (2000). 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 causes them to have lower wages than the median earner in their (pointwise) cell. It it possible to understand the strength of this assumption by using the work of Manski (1990) and considering a bounds approach to this restriction.

### 3.4.1 Derivation and Bounds

Assume latent offer wages are log-normal and therefore that ln offer wages are normal. If so the transformed pointwise offer wage distribution, $f_X(w)$, is symmetric.[24] This implies that the median and mean are equivalent statistics, although with different sampling distributions. Define the true (pointwise) median of the latent log offer-wage distribution as:

$$\Upsilon_{50,X} = F_X^{-1}(0.50) = \inf\{x : F_X(w) \geq 0.50\} \tag{8}$$

where the $X$ subscripts explicitly refers to the fact that we are conditioning on available covariates.[25] Typically the racial wage gap is measured as $GAP = E(ln\ w^o|X, Black = 1) - E(ln\ w^o|X, Black = 0)$. Under log-normality of the offer wage distribution, we are assured that $\Upsilon_{50,X} = \Gamma_1(X)$, implying that $GAP = Med(ln\ w^o|X, Black = 1) - Med(\ln\ w^o|X, Black = 0)$. Noting that the percentiles of a distribution are preserved under monotonic transformations, $GAP = ln(Med\ w^o|X, Black = 1) - \ln(Med\ w^o|X, Black = 0)$.

Since $f_X(w)$ is unknown I follow Manski (1994) by noting that the d.f. of a variable may be bounded. First note that the d.f. may be expressed as:

$$\Pr(w \leq t|X) = \Pr(w \leq t|X, z = 1) \Pr(z = 1|X) + \Pr(z = 0|X) \Pr(w \leq t|X, z = 0) \tag{9}$$

All terms are known except for $\Pr(w \leq t|X, z = 0) \in [0, 1]$. Taking these limits we can bound the d.f. as:

$$\begin{aligned} \Pr(w &\leq t|X, z = 1) \Pr(z = 1|X) \\ &\leq \Pr(w \leq t|X) \leq \\ \Pr(w &\leq t|X, z = 1) \Pr(z = 1|X) + \Pr(z = 0|X) \end{aligned} \tag{10}$$

What this allows me to do is to generate two values for the median (and under symmetry, the mean) which represent the largest and smallest values that the median can take.[26]

[24] Heckman (2001) discusses evidence confirming that it is appropriate to assume the normality of the *latent* log wage distribution.

[25] The corresponding sample quantity is analogusly 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 gurantees that the sample percentiles are well defined under discontinuities and nonmonotonicity of $F_n$.

[26] Manski (1994) offers an alternative derivation that is outlined here: if $\Pr(w \leq t|X, z = 1) \Pr(z = 1|X) \geq \alpha$ then $\Pr(w \leq t|X, z = 1) \geq \alpha / \Pr(z = 1|X)$. Note that the upper bound is informative only if $\Pr(z = 1|X) > \alpha$. Similarly, to obtain the smallest value of the lower bound we need the infimum of all possible values for $\Pr(w \leq t|X, z = 1)$. Noting that $\Pr(z = 0|X)$ is equal to $1 - \Pr(z = 1|X)$, and that if $\Pr(w \leq t|X, z = 1) \Pr(z = 1|X) + \Pr(z = 0, X) \leq \alpha$ then $\Pr(w \leq t|X, z = 1) \leq 1 - (1 - \alpha)/\Pr(z = 1|X)$. The lower bound is informative only if $\Pr(z = 1|X) > 1 - \alpha$. More generally, we can define the value of the conditional $\alpha \in [0, 1]$ quantile of $f(w^o|X)$ as: $q(\alpha|X) = \inf w : f(w^o \leqslant w|X) \geqslant \alpha$.

Using this logic we can bound this arbitrary quantile as $r(\alpha|X) \leqslant q(\alpha|X) \leqslant s(\alpha|X)$ where:

#### 3.4.2 Derivation of Lower Bound

The derivation of the lower bound $\underline{t}$ proceeds in a similar manner. Generate a new random variable $\underline{w}$ with wages for nonworkers set to some number incrementally greater than zero, and take the median of this new random variable. Behaviorally, this bound corresponds to a model where pointwise all respondents have the same reservation wage, implying that censored wages are necessarily below observed wages. This bound will constitute the principle estimator used in the paper.

$$\begin{aligned} \underline{t}_X &= F^{-1}_{\underline{w}|X}(0.5) \\ &= \min\ \underline{t} : Pr(\underline{w} < \underline{t}|X) = 0.5 \end{aligned} \tag{11}$$

#### 3.4.3 Derivation of Upper Bound

To derive the upper bound $\bar{t}$ for the median (largest possible value for $t$), we need to minimize $\Pr(w \leq t|X)$ and therefore focus on the first inequality in (10). This case corresponds to a situation where all the nonworkers are drawn from above the pointwise median, hence $\Pr(w \leq t|X, z = 0) = 0$. One way to derive this bound is to generate a new random variable $\overline{w}$ with wages for nonworkers set to infinity, and take the median of this new random variable. Pointwise, for each cell $X$:

$$\begin{aligned} \bar{t}_X &= F^{-1}_{\overline{w}|X}(0.5) \\ &= \min\ \bar{t} : Pr(\overline{w} < \bar{t}|X) = 0.5 \end{aligned} \tag{12}$$

This upper-bound while statistically operationalizable has little economic content: the possibility that all non-workers are drawn from above the median is not an interesting case to focus on. However with further assumptions it is possible to tighten the upper bound obtained above and produce a more realistic upper-bound. Reconsider equation (12) and assume the monotonicity of offer wages. Specifically, it may be reasonable assume that nonworkers have a higher probability of drawing wage offers from below the median:

$$\Pr(w \leq t|X, z = 1) \leq \Pr(w \leq t|X, z = 0) \tag{13}$$

This is weaker than assuming than the earlier assumption (for the upper bound) that their offer wages are necessarily greater than the median. Under this restriction we can use

$$\begin{aligned} r(\alpha|X) &= [1 - (1-\alpha)/\Pr(z = 1|X)] \text{ quantile of } f(w|X, z = 1) \\ s(\alpha|X) &= \alpha/\Pr(z = 1|X)] \text{ quantile of } f(w|X, z = 1) \end{aligned}$$

For example, the quantity $r(\alpha|X)$ can be estimated by examining Appendix Table 4A. Let $p$ be the fraction of respondents for whom skill-prices are observed in cell $X$. An estimate of $r(\alpha|X)$ will be given by computing the $50 - p$ percentile of the distribution of measured wages in cell $X$.

a number greater than 0 for the $\Pr(w \leq t|X, z = 0)$ in LHS of (10). In fact by giving 51 percent of nonworkers a sufficiently low wage, we can assure that (13) will be satisfied. This is trivially satisfied by reporting the measured median.

### 3.4.4 Properties

1. The estimator used by Neal and Johnson (1996) (henceforth NJ) and by Johnson, Kitamura and Neal (2000) corresponds to the lower bound estimator invoked under the additional restriction of linearity. For the purpose of this analysis however, I ignore the assumption of linearity and focus on its general application. Brown (1984) also invokes this bound but does not condition of the X's. To show that the (B)rown's bound is a special case of the (pointwise) NJ bound and generates the largest possible effect for the Butler-Heckman hypothesis, we need to show that $\underline{t}_B \leq \underline{t}_{NJ.}$ Here, $\underline{t}_{NJ.} = \int \underline{t}_X dF(X)$

Proof: A condition for the equivalence of the two estimators is that $F^{-1}_{\underline{w}|X}(0.5) = F^{-1}_{\underline{w}}(0.5)$ $\forall X$; that the median of the distribution of offer wage distributions is independent of the X's. However, if $\underline{t}_X > \underline{t}_B$ for any $X$ then $\underline{t}_B < \underline{t}_{NJ.}$ This will happen pointwise if for any cell: $\int^{\underline{t}_B} \underline{w}_X dF(\underline{w}_X) < 0.5$ In words, if there is sufficient mass (more than 50 percent) in a pointwise distribution that is greater than $\underline{t}_B$, then $\underline{t}_X > \underline{t}_B$,yielding in turn the result that $\underline{t}_B \leq \underline{t}_{NJ.}$

2. Rules for combining cells. The NJ estimator is impervious to combining two cells ($X$ and $Y$ into $XY$) under the following restrictions: Define the new cell median as $\underline{t}_{XY} = \min t : Pr(\underline{w} < \underline{t}|XY) = 0.5$. Then if $\underline{t}_X \leq \underline{t}_Y$ it must be the case that $\underline{t}_X \leq \underline{t}_{XY} \leq \underline{t}_Y$. Therefore as long as the nonearners in cell $Y$ earn less than $\underline{t}_{XY}$ the bound is robust to the combination. This property is empiricially useful: under the new coding scheme adopted in the 1990 Census, schooling is reported in bracketed intervals (e.g. 0-4, 5-8 years.) The analyst can combine these cells as long as it can be assumed that nonworkers with 5-8 years of schooling earn less than the median person in the combined 0-8 years of schooling cell. It also cautions us against combining cells for workers with 12 yrs of schooling with those of workers with a college degree.

3. The use of bounds analysis can put results from matching estimators into perspective. The maximum evidence *against* the Butler-Heckman hypothesis is generated by the upper bound estimator which assumes that all nonworkers are drawn from the top of the wage distribution. Therefore, if the results from matching estimators are close to those obtained from the upper bound, we can conclude that matching estimators work by assuming that nonworkers are drawn from above the median.[27]

4. The bounds as well as the pointwise medians may be estimated by using their sample analogues.[28]

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[27] Technically, the maximum evidence against the BH hypothesis is generated by assuming that all non-working white men earn below their median, and all non-working black men earn more than their (cell) median. I am grateful to Charles Brown for this clarification.

[28] It is important to note one caveat: to rule out results that are driven by finite sample discontinuties it is

5. Because the ordering of percentiles is robust to monotone transformations, this method does not require post-estimation retransformations of the data if predictions in the original metric are of interest (for example, offer wages for nonworkers). One method to do this is the use of the smearing estimate that is developed in Duan (1983). Alternatively, a biased but consistent approximation is given by $\widehat{y} = \exp(\widehat{\sigma}^2/2)\exp(\widehat{\log y})$.

6. Conventional estimates of the racial wage gap are reported as $GAP = E(ln\ y_b|X) - E(ln\ w_w|X)$. If wages are log-normally distributed, then we also have $Med\ (ln\ y) = E(ln\ y)$. The racial wage gap can therefore be estimated as $ln(Med\ y_b|X) - ln(Med\ w_w|X)$.

### 3.5 Comparison with Other Estimators

The central problems with using either matching or matching with a correction-factor are graphically demonstrated in the four panels of Figure 5. Figure 5a represents the implicit assumptions behind all selection models. The joint-distribution of offer-wages and reservation has been drawn under the assumption of positive covariance between the two variables. Offer wages are only observed if they exceed reservation wages (below the 45 degree line). Figure 5a can be interpreted as representing the conditional or unconditional distribution of offer and reservation wages. Recall that the parameter of interest is $\Gamma_1(X)$. This can easily recovered if the analyst has an estimate of $E[w_{it}^o|X, z = 0]$. It can be seen that the practice of estimating $E[w_{it}^o|X, z = 0]$ with $E[w_{it}^o|X, z = 1]$ can lead to substantial bias as the two quantities are very different. It is also possible to see that attempting to bound $E[w_{it}^o|X, z = 0]$ by the wages of the lowest worker $K^{L-W}$ may still not be an appropriate proxy for the lowest wage for non-workers which is $K^{L-NW}$. Panel 5a also makes explicit the importance of the "common-support" criterion of conventional models of index-sufficiency. Even if a legitimate exclusion restriction, $E$, exists, this class of models can only recover offer wages correctly if the supports of the distribution of offer wages for workers is identical to that for non-workers.This is because the existence of $E$ only guarantees that $Pr(z = 1|X, E = e) - Pr(z = 1|X, E = e') \neq 0$, for two different values of $E$.

In contrast to making these assumptions $\Gamma_1(X)$ may be recovered under the assumption that all nonworkers earn less than $\Gamma_1(X)$ and censoring rates do not exceed 50 percent. As the figure demonstrates there is the possibility of misclassifying a number of nonearners under this assumption. However, by assigning nonearners to lie above $\Gamma_1(X)$ it is possible to study

possible to follow the asymptotic results proved in Manski (1994): For any $\delta > 0$, and sample size $N$, Manski demonstrates that for an arbitrary $s$-quantile: $s(\alpha|X)_N \in [s(\alpha - \delta|X), s(\alpha + \delta|X)]$. Manski notes that the Glivenko-Cantelli theorem may be generalized to the estimation of the empirical distribution function, proving that the nonparametric Nadaraya Watson estimates converge (uniformly) to their population counterparts. As a consequence of the Manski/Stute result it may be tempting to use the above result to estimate $s(\alpha|X)$ by using an average of $s(\alpha - \delta|X)$ and $s(\alpha - \delta|X)$. However, doing do yields a consistent estimate with variance that is four times larger than the variance of the sample analogue to $s(\alpha|X)$! To see this fact note that the joint distribution of two percentiles ($p_1$ and $p_2$) given by $\xi_{p_1}$ and $\xi_{p2}$ of $f(w)$ is:

$\sqrt{n}\binom{\widehat{\xi}_{p1} - \xi_{p1}}{\widehat{\xi}_{p_2} - \xi_{p_2}} \underrightarrow{D} N(0, \Sigma)$, where $\sigma_{ii} = p_i(1 - p_i)/f^2(\xi_{pi})$ and $\sigma_{12} = p_1(1 - p_2)/(f(\xi_{p1})f(\xi_{p2}))$. The variance of the sample median is simply $0.5/f^2(0.5)$. Under symmetry, $f(\xi_{p1}) = f(\xi_{p2})$ if $p1$ and $p2$ represent the $p1$and $(1 - p1)$ quantiles of $f(w)$. It is immediate from this fact that the ratio of the asymptotic variances is 4, rendering this method undesirable. A formal proof is available from the author on request.

the strength of this assumption. Note however, that in a behavioral model where (pointwise) reservation wages are the same for everyone in the cell, there will be no classification error. Graphically, this is represented in Figure 5b. Here, the 'cloud' is a horizontal line. Such a model would be generated by a decision rule where observationally equivalent agents pick a certain percentile of the offer wage distribution as their reservation wage. Also shown in Figure 5b is a 'cloud' for another cell. It can be seen that even though the clouds are horizontal lines for each cell, when aggregated across cells the joint-distribution of offer and reservation wages will resemble more of a traditional bivariate plot.

Panel 5c demonstrates an increase in reservation wages. Three features of the increase are immediate: i) $E[w_{it}^o|X, z = 1]$ has increased because a number of previously low-wage workers are not working, ii) $E[w_{it}^o|X, z = 0]$ has fallen because low-wage respondents in time $t$ are now non-earners in time $t+1$ and iii) there is no change in the median $\Gamma_1(X)$. Since $dE[w_{it}^o|X, z = 1]/d\Pr(z = 0|X) > 0$ the bias from using a matching estimator increases as the degree of non-participation increases. This point is clearly evident in the figure: proxying $E[w_{it+1}^o|X, z = 0]$ with $E[w_{it+1}^o|X, z = 1]$ results in a much bigger bias than using $E[w_{it}^o|X, z = 1]$ for $E[w_{it}^o|X, z = 0]$. This applies to the cell-minimum case as well; $K^{L-NW}$ does not change, but its proxy $K^{L-W}$ increases.

Finally, Figure 5d illustrates the very special case in which matching estimators work. Here in period $t$, $E[w_{it}^o|X, z = 0] = E[w_{it}^o|X, z = 1]$ or more generally $E[w_{it}^o|X, z = 0] = k.E[w_{it}^o|X, z = 1]$ if a correction factor is used. A separate cloud must describe the relationship between offer and reservation wages for workers and non-workers. However, in period $t + 1$ if the increase in reservation wages is sufficiently high, then it is no longer the case that $E[w_{it+1}^o|X, z = 0] = E[w_{it+1}^o|X, z = 1]$.This is because $E[w_{it}^o|X, z = 0]$ falls with the increase in reservation wages, whereas $E[w_{it}^o|X, z = 1]$ increases. Therefore, the imposition of a fixed constant of proportionality would only be valid if as reservation wages increased, offer wages *simultaneously increased* for the nonemployed in a manner such that the relationship $E[w_{it}^o|X, z = 0] = k.E[w_{it}^o|X, z = 1]$ was preserved. Another case in which matching estimators work is where the distribution of reservation wages and offer wages is described by a vertical line.

# 4 Results

## 4.1 Evidence from the Aggregate Distribution of Weekly Wages

In Table 5 I duplicate the approach of Brown's seminal study. Reported in the table are observed mean weekly wages (to provide a reference point) and estimates from the Median-L and Median-M estimators. Median-(L)ower puts all non-workers below the aggregate median and Matching-(M)onotinicity assigns half of non-workers to lie above the median, the other half below. A more intuitive way to think about Median-M is that it represents the observed median wage. Table 5 should be interpreted as follows: in 1960 observed median wages for blacks (whites) were $319 ($519). By 1990, these had grown to $487 ($684). The observed median ratio therefore moved from 0.615 to .699, an improvement of approximately

12.8 percent. However, the corrected ratios (from Median-L) moved from 0.56 to 0.611 over this period; a smaller increase of 7.9 percent. In each year it can be seen that the corrected ratios are significantly different from measured ratios. It is also important to note that the increase between 1960 and 1970 is larger for the Median-L estimator than that for the observed ratio. This finding arises because CPS based analysis ignores the fact that there were significant declines in the incarceration rate between 1960 and 1970 for blacks, (with statistically insignificant declines for whites). These declines represent a previously undocumented feature of the impact of the CRA on labor markets: by improving labor market outcomes for blacks, a smaller share may have engaged in criminal activity. Falling incarceration rates skewed in favor of blacks represent an extraordinary exception to the historical trend in these data and imply that previous research which has focused on the LFP rate (as computed off of CPS) may have understated an important dimension along which the CRA may have acted.

Table 6 utilizes the insights of the previous section to tighten the bounds. Here, the dynamics of the racial wage gap are reported by broad schooling level as well as for four age categories. For each skill group, *changes* (as measured by the difference of logs) in the results from five estimators are reported:

$$\begin{aligned} Observed &: \quad E(ln\ w^o|X, z = 1) \\ Matching &: \quad E(ln\ w^o|X, z = 0) = E(ln\ w^o|X, z = 1) \\ Median\ L &: \quad E(ln\ w^o|X) = Med(ln\ \underline{w}^o|X) \\ Median\ M &: \quad E(ln\ w^o|X) = Med(ln\ w^o|X, z = 1) \\ Cell\ Min &: \quad E(ln\ w^o|X, z = 0) = \min(ln\ w^o|X, z = 1) \end{aligned} \tag{14}$$

Conditioning on $X$ implies that the data were saturated by 240 (2 race x 4 year x 6 age x 5 education) cells and the relevant cell statistics were used in the above selection corrections.

The results of Table 6 are particularly striking. First, there is evidence that previous analysis has actually *understated* the convergence between 1960 and 1970. This is particularly true for those with HS degrees and those aged 25-35. Second, large portions of the observed convergence in wages are shown to be completely illusory: amongst those who were high school dropouts, the observed convergence of 0.21 log points is re-estimated to be a divergence of -0.078 log points. Similarly, for young men aged 25-29 measured convergence has been an impressive 16 percent between 1960 and 1990. However, accounting for non-participants reduces the convergence to less than 1-percent. Most of the correction is driven in the 1980s and 1990s in a manner that is consistent with the results of Tables 1 and 2. In comparing the results obtained from a matching estimator to those obtained from the lower-bound Median-L estimator, it can be seen that while the matching estimator reduces the observed convergence, *ipso facto* it cannot generate a substantial correction. The largest corrections for the matching estimator are seen in the columns labeled 'All Groups.' This is to be expected: matching is only reweighting the data over the full supports of the $X's$; within cell the correction is zero by construction. Across all schooling and experience groups we see that the gap closed by approximately 19 percent between 1960 and 1990. However, the corrections suggest that the true convergence was only 6 percent. Therefore, between 1960 and 1990, 60 percent of the convergence is driven by the selective-withdrawal hypothesis.

In Table 7 I replicate the analysis of Table 6 but only focus on Southern states. As carefully documented in Heckman and Paynor (1989) and Donohue and Heckman (1991), the South was where the CRA was found to be most effective. Focused Federal attention on an unwilling South is what caused an entire pattern of discrimination to be shattered. Aggregate improvements in the South are found to be the same as in all states. However, for those workers in the South with a HS degree and those aged 25-29 improvements in the south were greater that the US average. It is interesting to note that amongst high-school dropouts the divergence in the racial wage gap has been half of what it has been nationwide. One possible reason for this is that white HS dropouts in the south are closer to their black counterparts than in the north.

### 4.2 Evidence at the cohort level

In their analysis of civil rights policies in affecting the racial wage gap, Chay and Honore (1998) make an important point: in discussing the results of Juhn (1997) they note that her analysis could be potentially confounded by cross-cohort effects. If their contention is correct it would be necessary to perform the analysis at the level of individual cohorts. In Table 8 I attempt to explore their criticism of Juhn's results in more detail. The decennial nature of the US Census makes this a difficult task. Using the PUMS data, I have placed all individuals into 5 cohorts which can be tracked over different years of the census data. Each cohort includes all men born in the specified year as well as one year before and one year after. By exploiting this quasi-panel feature of the census data it is possible to see how the racial wage gap has changed within cohort.

Across all schooling levels we see that there were large effects of the CRA on the cohort born in 1935 (men who would have entered the labor market in 1960). The effect is not seen for earlier cohorts. In fact, there is some evidence to suggest that the economic well-being of these older cohorts is overstated if one simply focuses on observed wages. The largest gains from the CRA accrued to those born in the 1935 cohort with less than a HS degree as well as those with a HS degree. This suggests that the opening up of relatively unskilled jobs to previously excluded blacks constitutes the primary area where gains were made.

### 4.3 Are these Estimates Reasonable?

In Table 9, 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 used are plausible. To focus the discussion I limit the analysis to the two groups where selection is likely to play an important role, those with less than a HS degree and those aged 25-35. For each of these groups I report the point estimates as determined by the alternative approaches discussed in (14).

Note that the distribution of wages is positively skewed– in each year the actual mean of weekly earnings is significantly above the reported median. However, this is not true of the ln wage distribution. Here the median exceeds the mean. In a model where latent wages are ln-normally distributed this will only happen if the selection is coming from the left tail. Ideally, the ln-wage distribution would have equivalent mean and medians but the fact that the median exceeds the mean implies that some mass has been removed from the left tail of the distribution, thereby giving it a negative skew. This empirical observation provides evidence that supports the assumption of assigning nonworkers a wage below that of the median agent.

The point estimates reported by the Median-L estimator are within known sensible bounds. For example, true median weekly wages are estimated to be $215 in 1990 for black HS dropouts. This may seem like a very small estimate of weekly wages, especially when compared to median wages of $345. However, one should note that the average HS dropout in 1990 worked 40 hours a week, suggesting an hourly wage of $5.37 (in 1997 dollars). However, in 1989 the value of the minimum wage was $3.35 (in current dollars) and $4.30 in 1997 dollars. Therefore, these estimates while significantly lower than observed skill-prices, do meet basic logical tests for consistency.

### 4.4 How Important is Incarceration?

In order to understand the implications of studying the racial wage gap with datasets that do not sample the incarcerated population it is important to perform the analysis with and without the incarcerated samples. This is the approach taken in Table 10 where I have excluded respondents who are currently incarcerated from the analysis. In comparing Table 10 with Table 6 we see that excluding the incarcerated sample results in the divergence of 12 log points between 1980 and 90 for HS dropouts falling to a divergence of 4.6 log points. For this group, two-thirds of the divergence is a function of the counting the incarcerated population. Similarly, amongst those aged 25-29 ignoring the incarcerated population reduces the divergence from -11.2 to -0.7. For more skilled groups the correction induced by the incarceration sample falls in a manner that is consistent with the skill-incarceration profiles described in Table 1.

This result does not bode well for researchers who use CPS or NLSY data to study the racial wage gap. Unless the object of interest is to analyze the gap or changes in the gap for relatively skilled workers, the results of Table 10 demonstrate that excluding the incarcerated sample can result in significant bias. In addition, to the degree that incarceration rates have increased over time, the bias from excluding this group will cause the analyst to significantly overstate changes in the racial wage gap.

## 5 Conclusions

Ever since Myrdal published his monumental treatise *An American Dilemma* in 1944, considerable intellectual energy has been devoted towards studying the causes and dynamics of the racial wage gap. However, much of the literature that constitutes this debate stems has relied on inferences on CPS or even more selectively on CPS-SSA data and therefore has ignored the growing nonparticipation problem amongst blacks that is driven primarily by increases in incarceration rates. The purpose of this paper has been to revisit a thesis first propounded by Butler and Heckman almost 25 years ago, and evaluate whether an 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. By identifying the distribution of offer wages to blacks and whites as the distribution of interest in assessing black economic progress, I discuss the economic content of alternative identifying assumptions used to recover this latent distribution. The approach considered in this paper nests the pioneering work of Heckman (1976, 1979) with an informative framework developed by Manski (1994, 1995) in that unobservable characteristics are allowed to affect the offer wages of workers. By redefining the parameter of interest to be the median of the distribution of log offer wages and by assuming that nonworkers earn less than the (pointwise) median respondent in each cell, it is possible to recover this quantity. As such, the paper builds on important contributions by Brown (1984) and Kitamura, Neal and Johnson (2000) but is more general in its approach. The use of bounds analysis is demonstrated to be informative and suited for the study of this particular problem. In contrast to more structural approaches which rely on the analyst correctly specifying the joint distribution of offer and reservation wages, or asserting the existence of legitimate exclusion restrictions, the assumptions used in this paper appear to be weaker. It is however important to note that this approach may not be suited for assessing the role of the Civil Rights Legislation on black women or the dynamics of the racial wage gap for women– an important but often ignored area of research.[29]

Using US. Decennial Census data, I demonstrate that studies which have made inferences based on the CPS have excluded the institutionalized and incarcerated populations and thereby dramatically understated the extent of black nonemployment in recent years. I find support for a modified version of the original Butler-Heckman hypothesis where the growth in the disability program does not appear to have contaminated Freeman's assessment of the efficacy of the Civil Rights Act. In fact, I find that declining incarceration rates between 1960 and 1970 may have caused prior research to have understated the true effect of Title VII Legislation. However, since the passage of the Civil Rights Act there is considerable evidence of divergence in the racial wage gap for high-school dropouts and high-school graduates. Amongst college graduates, there is little evidence of the selective withdrawal hypothesis. Across all skill groups the trajectory of convergence remains absolutely flat since 1970. The cohort level analysis reveals that there was virtually no improvement in the relative wages of older cohorts (those born prior to 1930) as a result of the CRA and that the benefits of the Federal intervention accrued almost exclusively to those cohorts born in 1935 and well as to

[29] Neal (2001) demonstrates that racial differences in the participation patterns of women are less like to be motivated by differences in offer wages. Neal convincingly argues that differences in the marriage markets facing black and white women and related differences in the shadow price of home production cause many women with high offer-wages to not be at work. In studying the offer wages of women however, it is much easier to find and justify the existence of legitimate exclusion restrictions; a task that is exceedingly difficult for men.

younger cohorts who entered the labor market after the passage of the Act.

The causes behind the withdrawal are difficult to isolate and I discuss four possible explanations. One explanation for the declines is that anti-discrimination efforts weakened significantly over the 1980s. This is the view espoused by 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 argument, while appealing, is not entirely consistent with the historical record for it 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 are covered by mandatory affirmative action plans did not reduce the share of black males employed by them. By itself this is not supportive of their thesis: in a general-equilibrium model of labor markets with multiple sectors, successful enforcement in one sector will simply depress relative wages in another by diverting white labor from the covered to the uncovered sector. This finding deserves more attention as does the empirical content of Bound and Freeman's more general thesis.

As demonstrated in this paper, in the 1960s there appears to be little connection between the growth of the disability program and labor force withdrawal for men. However, Autor and Duggan (2001) demonstrate that the relationship 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 go into unemployment) as well as changes in the generosity of the disability program. Amongst the least-skilled the growing generosity of the DI program appears to be an explanation with significant explanatory power.

A portion of the Autor and Duggan's explanation comes from real declines in the wages of the lowest skilled. In the presence of skill biased technological change which "stretches" the skill distribution, it is possible that the relative position of black men on the aggregate wage distribution has not changed, even though the convergence in observed or corrected mean weekly earnings has stagnated or even deteriorated. This point constitutes the central thesis of Chinhui Juhn, Kevin Murphy and Brooks Pierce (1991) and remains the single most dominant explantion for the divergence in the racial wage gap. Finally, there is the bi-directional relationship between crime and wages. As offer wages fall, the incentives to engage in criminal activity increase. Simultaneously, the introduction of crack-coccaine in the 1980s may have further increased these incentives to withdraw. The deleterious effects of incarceration have disastrous life-cycle consequences.[30] Given the high rates of recidivism in the population, many low-skilled men in my sample who are not incarcerated but are currently not at work may have had prior convictions. This in turn would significantly

[30] Western and Kling (2000) provide a fascinating review of this literature and discuss the known evidence in favor of disentangling the causal relationship between incarceration and employment.

reduce their future re-employment probabilities; a point which would reinforce the use of my identifying assumption that nonwokers earn less than the pointwise median person.

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. One source of 'progress' 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 legalization would have had a 40-60 percent greater chance of living in a single-parent family, die as an infant, or grow up in poverty and welfare. Cohorts that were affected by the legalization of abortion would be entering the labor market at the time of the 2000 census. To the extent that black babies 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, incarceration rates should start to fall for younger blacks 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" samples. 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 simple method to convert bracketed weeks worked last year responses to a continuous measure. In this paper, I follow his algorithm with a modification to assure internal consistency. 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 Chandra (2000a) 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 the purpose of this paper this sample restriction does not matter: the results of Chandra (2000a) are virtually identical to those obtained in this paper. I restrict my analysis to prime-age men aged 25-55 so that my results are not contaminated by the increasing prevalence of early retirement amongst men. Because a substantial fraction of younger cohorts in the 20-25 age group are enrolled in college, this group was excluded from the analysis to avoid incorrectly classifying this group as being out of the labor force. 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.

## 6.1 Measuring Skill Prices

The Census data have 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. Ideally, a worker's skill price is defined by wage income divided by the total hours worked in order to earn that income. Unfortunately, in most labor market data the quantity in the denominator is not directly observed and must be estimated. To construct a measure of skill price I use three alternative measures. The first, weekly wages is 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. Weekly wages are the object of interest in Juhn, Murphy, and Pierce (1991, 1993) 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.

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.[31]

### 6.2 Measurement Error in Reported Wages

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 the 1982 value of the minimum wage, over the entire 1940-90 period will result in dropping very different groups of people over time. For example, sample exclusions are based on real wages are standard in most of the literature of the returns to skill (see for example, Bound and Freeman (1992) who exclude workers whose hourly earnings exceeded $100 or were below $1 per hour in 1983). The extent to which such exclusion criteria affect estimates of the racial wage gap is exacerbated by the length of time under study. The deletion of workers with high skill prices results in primarily dropping highly skilled white workers, thereby overstating the convergence in black-white wages.

[31] A third measure allows me to take a more agnostic approach toward the problem of imputing skill prices. I assume that both measures of skill price discussed above are noisy reports of "true" skill prices. In order to recover the true price I appeal to the literature on optimal signal extraction and nonparametrically regress the hourly measure of skill price on the weekly measure and then recover the fitted values. The advantage of the nonparametric approach (such as LOWESS) as opposed to a parametric approach with a polynomial smoother is that the nonparametric regression is fit using a locally weighted smoother. Polynomial smoothing methods impose are global in nature. Therefore, what happens on one of end of the distribution will affect the fitting of the polynomial on the other end. I constructed this fitted skill price for 1980 and 1990 data using the extremely intensive LOWESS procedure with a tri-cube weighting function. In both years, results from this exercise gave results that were very similar to those obtained from using weekly wages as a measure of skill. This result seems to suggest that the skill price using hourly wages appears to have a smaller signal to noise ratio than that obtained from using weekly wages. More detailed results are available from the author on request.

Instead of trimming the data at a set real dollar cutoff, I winsorize the data at 1-percent and 99-percent. This procedure was pursued in the light of analytical and simulation results in Bollinger and Chandra(2001). 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 massive attenuation bias is introduced if the analyst trims the data when in fact a conventional measurement error process is at work. They go on to demonstrate that the process of winsorizing or doing nothing appears to be most desirable strategy to adopt in working with wage data. After winsorizing the data I created two samples, one with the winsorized data and the other with the weekly wages of those who worked 1-13 weeks set to missing. This is the approach taken in Juhn (2000) who treats workers with weekly wages but who worked 1-13 weeks as non-workers. Juhn's motivation for this approach is that these respondents tend to have very high skill prices largely because they worked very few weeks. In Appendix Table 3A I present the results of this comparison: the cells where workers who worked 1-13 weeks have high skill prices are primarily those of young, unskilled blacks. Because there are a very few cells in where the two samples give statistically different results, I chose not to impose Juhn's restriction on the data. This decision will bias my results against finding evidence in favor of the selective withdrawal hypothesis.

In 1980 and 1990 those individuals who claimed to have less than 8 years of schooling and were less 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 less than nine years of schooling had no weekly wages). For those with weekly wages, they were lower than that 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.

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

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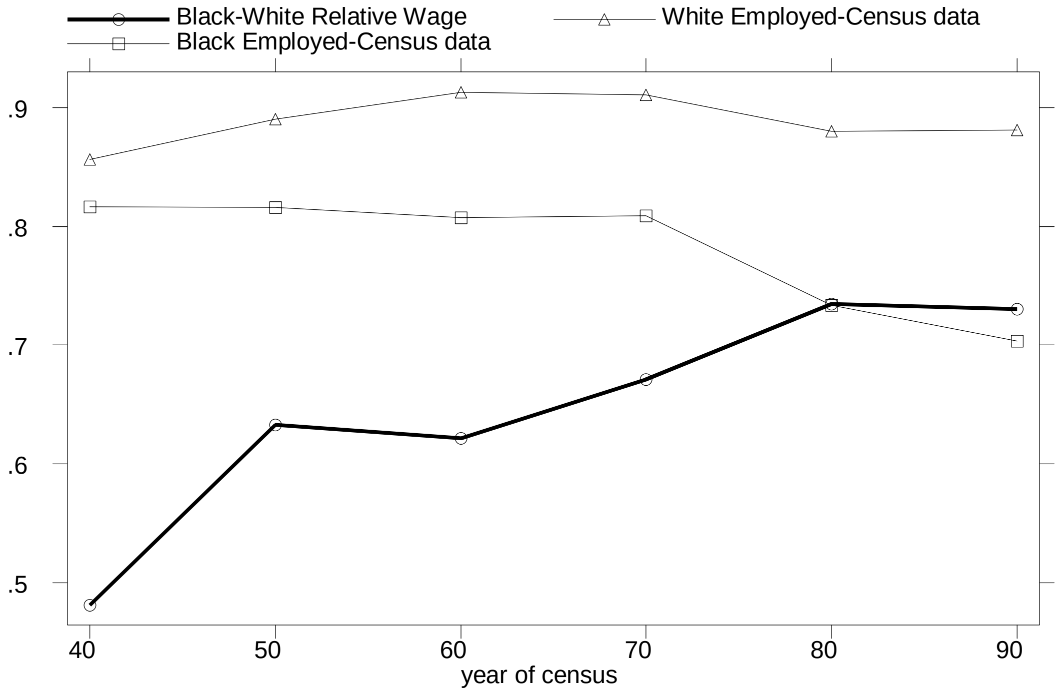
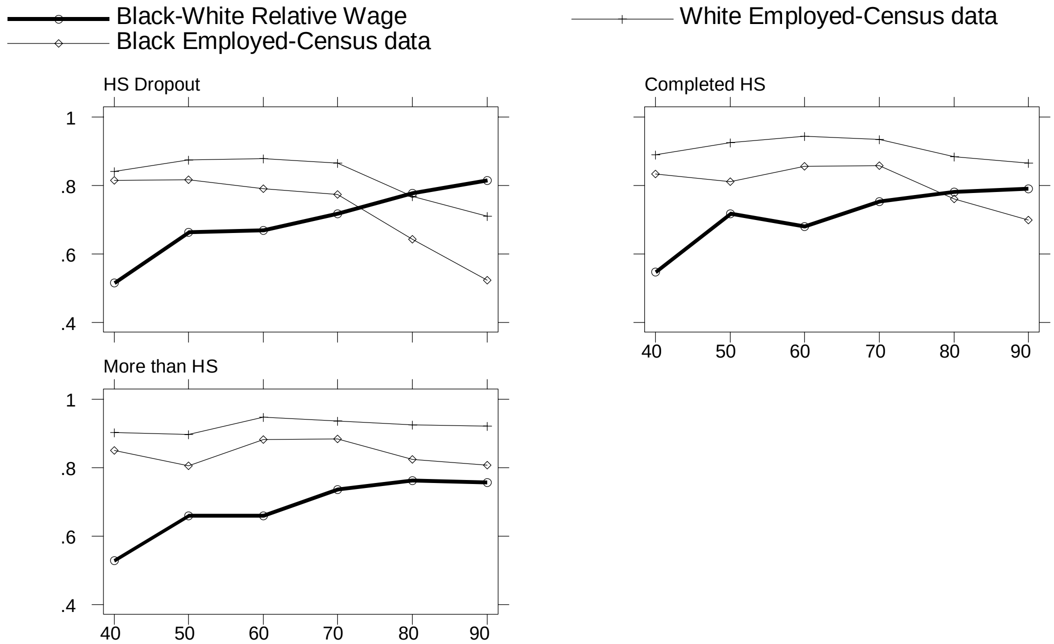
**Figure 1: Black-White Relative Wages and Employment Population Ratios, for Men aged 25-55**



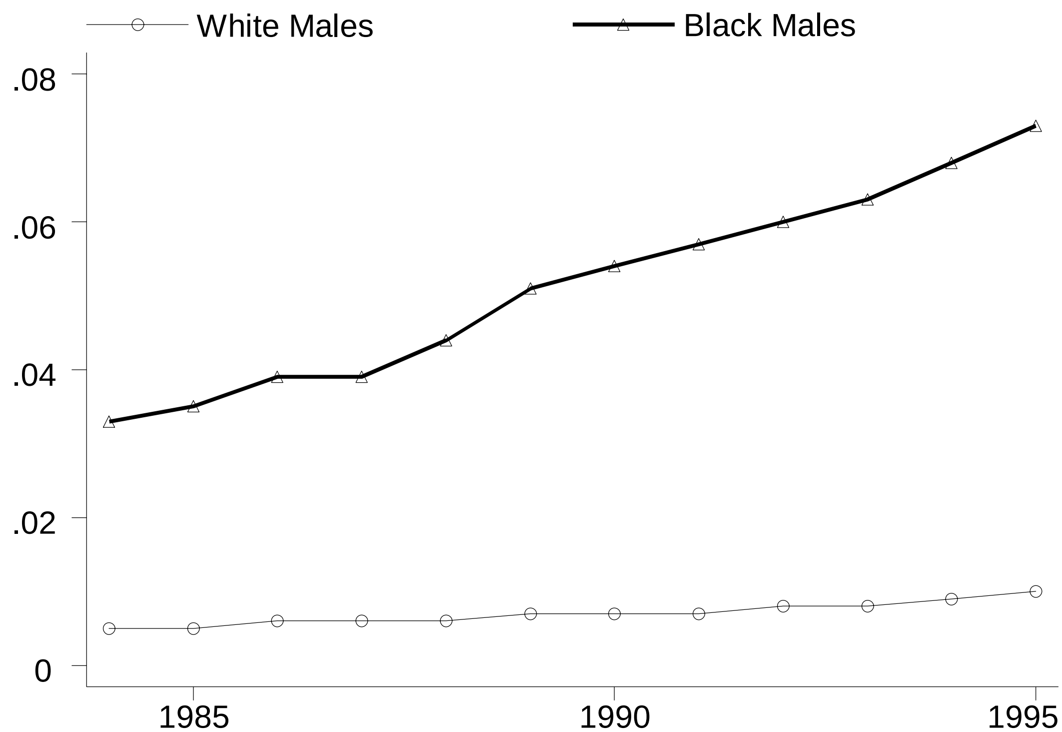

Author's calculations from the PUMS data. No sample restrictions have been placed on the data for the construction of employment/population ratios. Relative wages were computed by using weekly wages for wage and salary workers who worked at least one week in the previous year.

**Figure 2: Black-White Relative Wages and Employment Population Ratios, for Men aged 25-55 by Schooling Level**



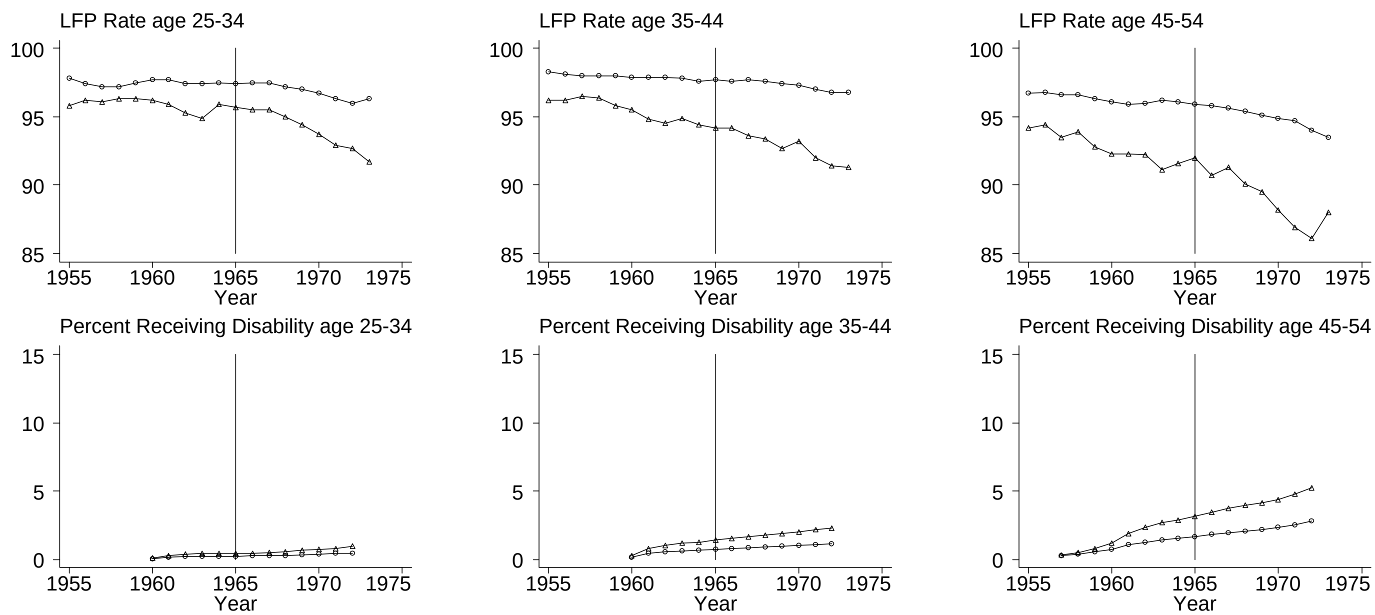

Author's calculations from the PUMS data. No sample restrictions have been placed on the data for the construction of employment/population ratios. Relative wages were computed by using weekly wages for wage and salary workers who worked at least one week in the previous year.

**Figure 3: Fraction of all Men by Race, Incarcerated in Federal, State Prisons or Local Jails at Midyear, men aged 18 and older**




Source: Data from Bureau of Justice Statistics (various years). See text for details.

**Figure 4: Labor Force Participation Rates and Percent Receiving Disability Benefits, by Age and Race using CPS Data**




Source: Figures generated from data published in Siskind (1975). See Section 2.3 of text for details.

**Figure 5a: Bounds, Matching and Index Sufficiency**

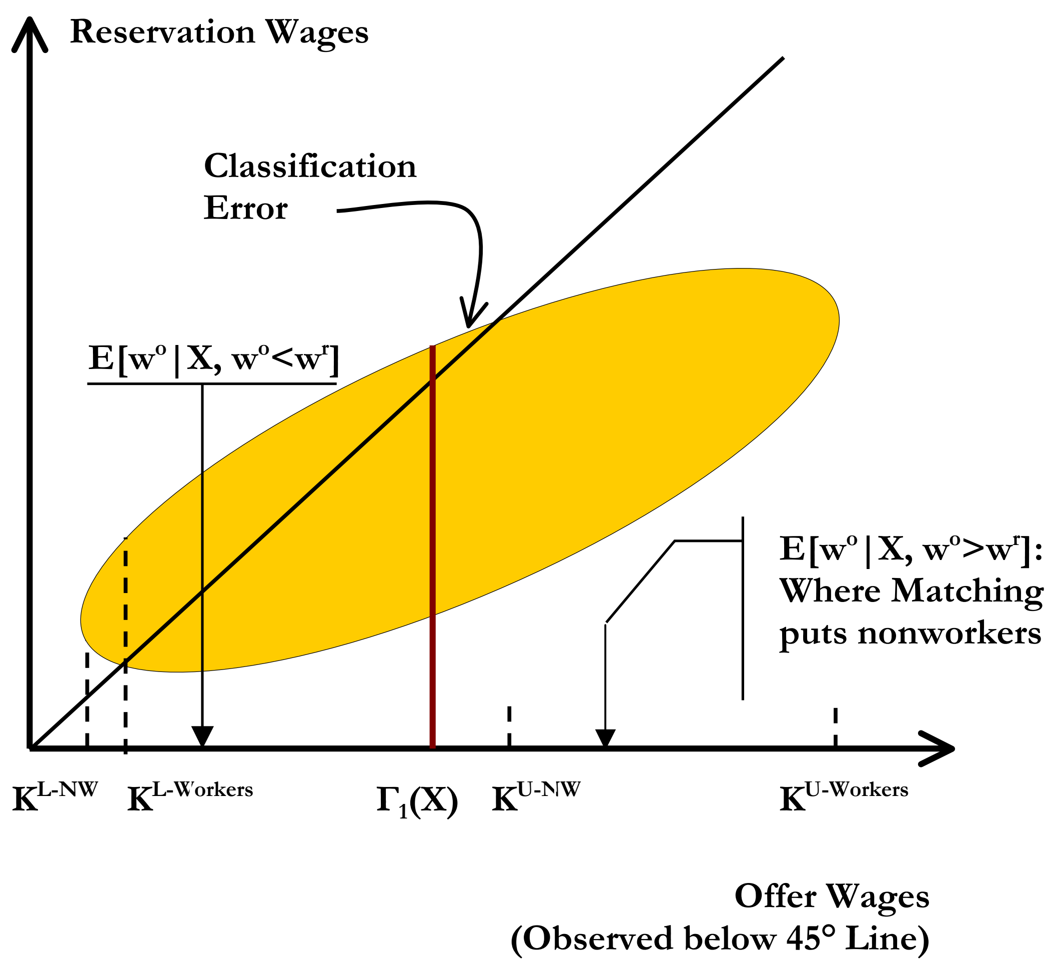


Figure 5b: When is there no Classification Error?

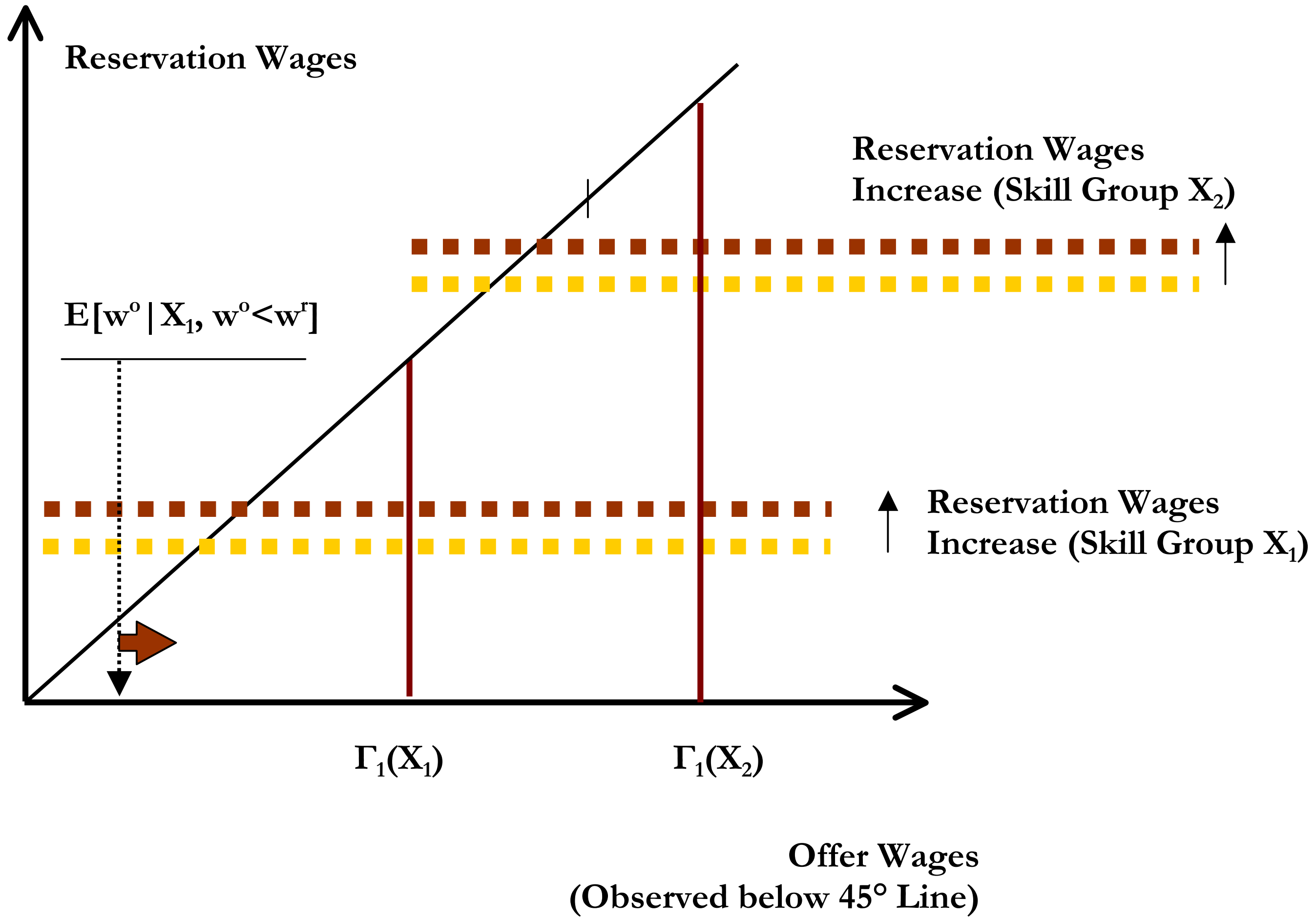


Figure 5c: Modeling an Increase in Reservation Wages

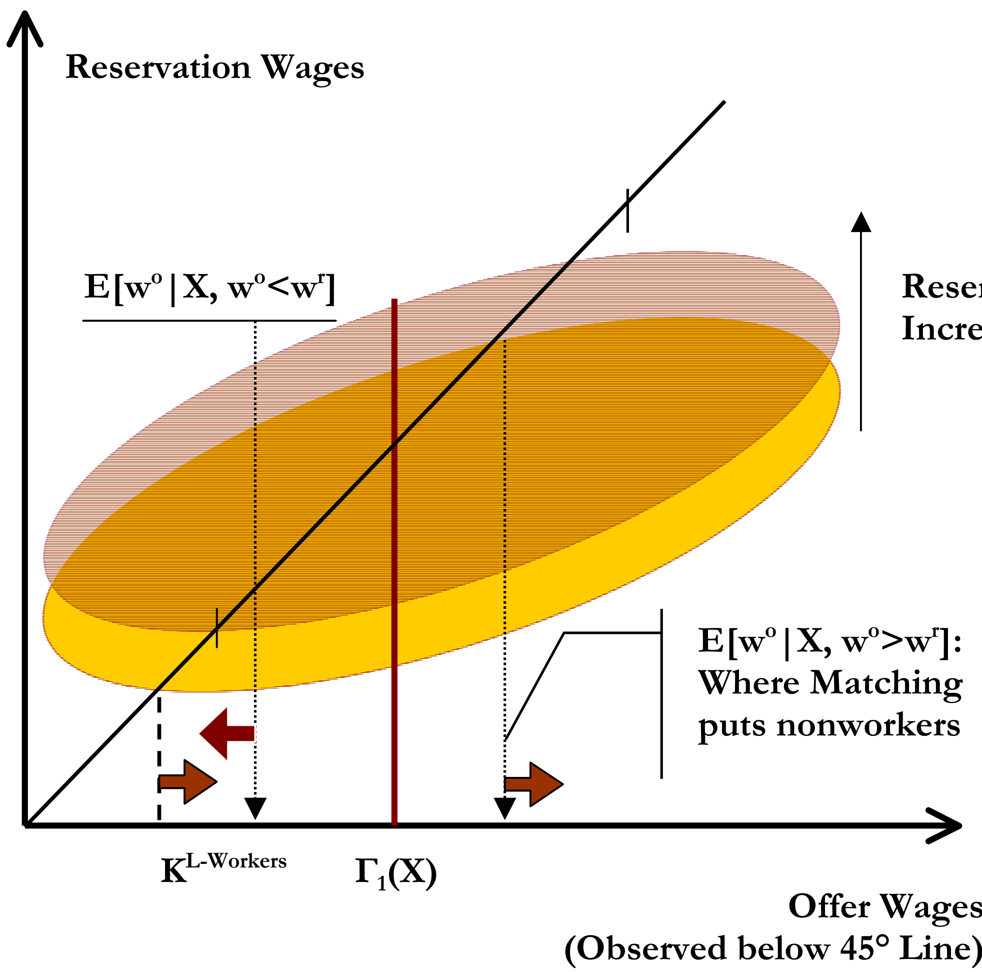
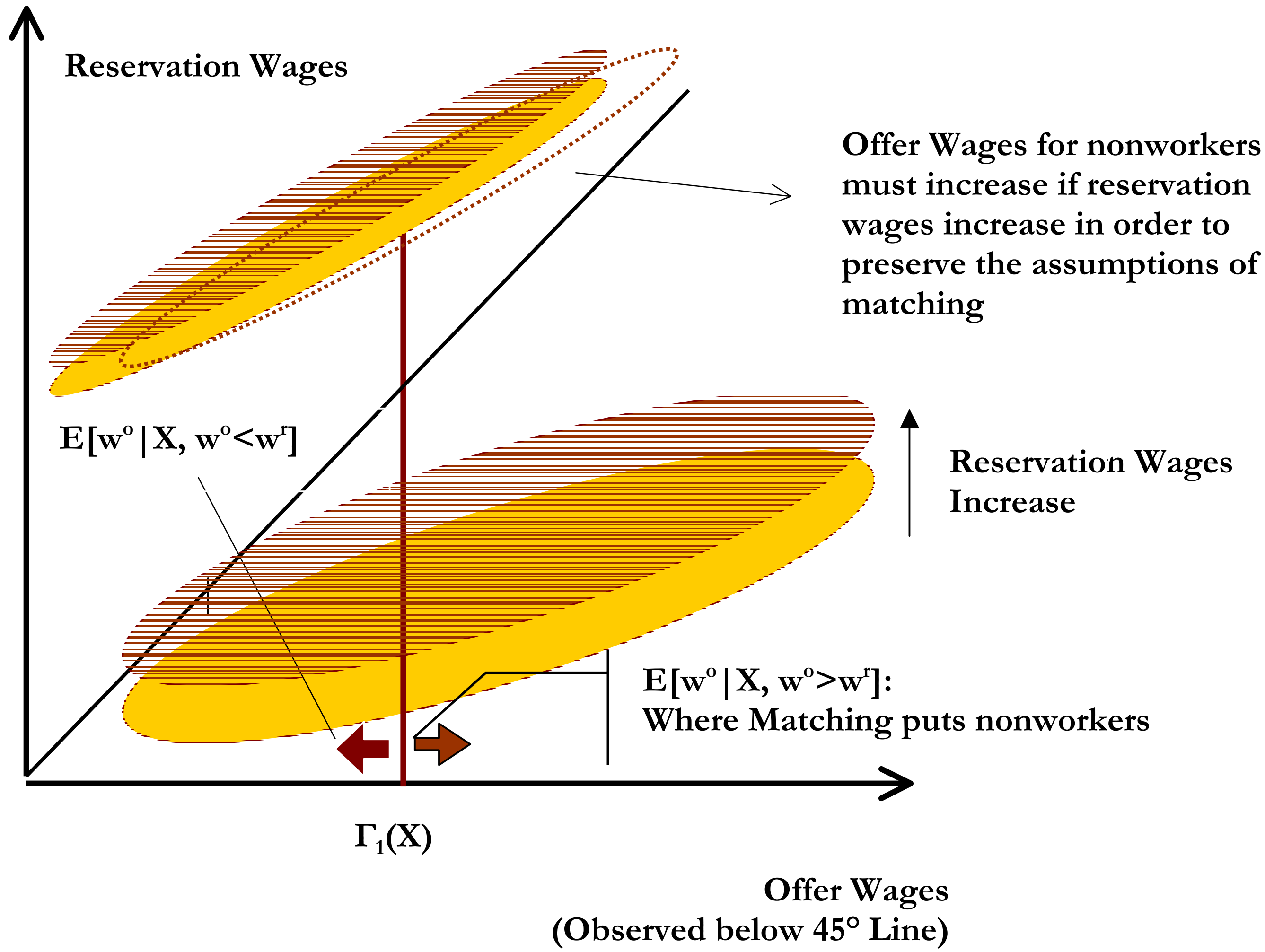


Figure 5d: Behavioral Model for the Use of Matching Estimations



## Table 1: Fraction of Prime Age Men who are Institutionalized during the Census Reference Week

| | Whites | | | | Blacks | | | |
|---|---|---|---|---|---|---|---|---|
| | < than HS | HS | HS+ | Total | < than HS | HS | HS+ | Total |
| **1960** | | | | | | | | |
| **25-29** | 0.021 | 0.005 | 0.002 | 0.010 | 0.068 | 0.025 | 0.016 | 0.053 |
| **30-34** | 0.017 | 0.004 | 0.003 | 0.009 | 0.058 | 0.036 | 0.025 | 0.050 |
| **35-39** | 0.017 | 0.005 | 0.004 | 0.010 | 0.050 | 0.036 | 0.027 | 0.045 |
| **40-44** | 0.016 | 0.005 | 0.005 | 0.010 | 0.038 | 0.028 | 0.015 | 0.035 |
| **45-49** | 0.014 | 0.008 | 0.006 | 0.011 | 0.027 | 0.021 | 0.033 | 0.026 |
| **50-54** | 0.015 | 0.008 | 0.008 | 0.013 | 0.028 | 0.017 | 0.014 | 0.026 |
| **Total** | 0.016 | 0.006 | 0.004 | 0.011 | 0.044 | 0.029 | 0.022 | 0.040 |
| **1970** | | | | | | | | |
| **25-29** | 0.023 | 0.006 | 0.002 | 0.008 | 0.079 | 0.028 | 0.014 | 0.048 |
| **30-34** | 0.019 | 0.006 | 0.002 | 0.008 | 0.051 | 0.028 | 0.007 | 0.037 |
| **35-39** | 0.014 | 0.004 | 0.002 | 0.006 | 0.044 | 0.021 | 0.008 | 0.033 |
| **40-44** | 0.013 | 0.005 | 0.003 | 0.007 | 0.035 | 0.016 | 0.010 | 0.028 |
| **45-49** | 0.013 | 0.005 | 0.003 | 0.008 | 0.026 | 0.021 | 0.026 | 0.025 |
| **50-54** | 0.012 | 0.006 | 0.003 | 0.008 | 0.020 | 0.026 | 0.010 | 0.020 |
| **Total** | 0.015 | 0.005 | 0.002 | 0.008 | 0.040 | 0.024 | 0.012 | 0.032 |
| **1980** | | | | | | | | |
| **25-29** | 0.035 | 0.007 | 0.003 | 0.008 | 0.101 | 0.039 | 0.026 | 0.050 |
| **30-34** | 0.027 | 0.006 | 0.003 | 0.007 | 0.071 | 0.035 | 0.022 | 0.040 |
| **35-39** | 0.019 | 0.005 | 0.003 | 0.006 | 0.041 | 0.026 | 0.017 | 0.029 |
| **40-44** | 0.013 | 0.004 | 0.003 | 0.005 | 0.025 | 0.016 | 0.010 | 0.018 |
| **45-49** | 0.009 | 0.002 | 0.002 | 0.004 | 0.016 | 0.004 | 0.007 | 0.011 |
| **50-54** | 0.008 | 0.003 | 0.002 | 0.004 | 0.014 | 0.011 | 0.013 | 0.014 |
| **Total** | 0.016 | 0.005 | 0.003 | 0.006 | 0.043 | 0.027 | 0.019 | 0.031 |
| **1990** | | | | | | | | |
| **25-29** | 0.042 | 0.015 | 0.007 | 0.013 | 0.231 | 0.077 | 0.052 | 0.095 |
| **30-34** | 0.044 | 0.013 | 0.006 | 0.012 | 0.162 | 0.065 | 0.043 | 0.072 |
| **35-39** | 0.035 | 0.012 | 0.006 | 0.010 | 0.110 | 0.049 | 0.044 | 0.058 |
| **40-44** | 0.031 | 0.009 | 0.004 | 0.008 | 0.070 | 0.043 | 0.036 | 0.046 |
| **45-49** | 0.021 | 0.007 | 0.005 | 0.007 | 0.067 | 0.026 | 0.027 | 0.039 |
| **50-54** | 0.013 | 0.005 | 0.004 | 0.006 | 0.034 | 0.024 | 0.016 | 0.025 |
| **Total** | 0.030 | 0.011 | 0.005 | 0.010 | 0.114 | 0.054 | 0.040 | 0.061 |
| **2000** | | | | | | | | |
| **25-29** | | | | 0.017 | | | | 0.131 |
| **30-34** | | | | 0.019 | | | | 0.119 |
| **35-39** | | | | 0.015 | | | | 0.101 |
| **40-44** | | | | 0.010 | | | | 0.064 |
| **45-55** | | | | 0.006 | | | | 0.034 |

Authors tabulations from the PUMS data for 1960-1990. No sample restrictions have been placed on the data. See Data Appendix for details of PUMS sample. For 2000 institutionalized rates refer to incarceration rates obtained from unpublished data obtained from the Bureau of Justice Statistics (BJS) and represent the incarcerated population on June 30th, 2000. Population (denominator) figures for 2000 were adjusted for the 1990 Census undercount by BJS. See Section 2.2 of text for details.

## Table 2: Fraction of Prime Age Men who are either Unemployed, NILF or Institutionalized during the Census Reference Week

| | Whites | | | | Blacks | | | |
|---|---|---|---|---|---|---|---|---|
| | < than HS | HS | HS+ | Total | < than HS | HS | HS+ | Total |
| **1960** | | | | | | | | |
| **25-29** | 0.127 | 0.057 | 0.091 | 0.091 | 0.226 | 0.141 | 0.143 | 0.198 |
| **30-34** | 0.104 | 0.045 | 0.039 | 0.068 | 0.211 | 0.154 | 0.123 | 0.191 |
| **35-39** | 0.110 | 0.046 | 0.031 | 0.069 | 0.204 | 0.132 | 0.103 | 0.183 |
| **40-44** | 0.112 | 0.051 | 0.037 | 0.078 | 0.197 | 0.147 | 0.098 | 0.183 |
| **45-49** | 0.118 | 0.062 | 0.050 | 0.092 | 0.200 | 0.155 | 0.110 | 0.190 |
| **50-56** | 0.150 | 0.090 | 0.074 | 0.126 | 0.225 | 0.140 | 0.120 | 0.213 |
| **Total** | 0.122 | 0.056 | 0.053 | 0.087 | 0.210 | 0.144 | 0.118 | 0.193 |
| **1970** | | | | | | | | |
| **25-29** | 0.147 | 0.069 | 0.097 | 0.096 | 0.258 | 0.147 | 0.157 | 0.198 |
| **30-34** | 0.123 | 0.056 | 0.051 | 0.071 | 0.210 | 0.139 | 0.102 | 0.171 |
| **35-39** | 0.118 | 0.053 | 0.045 | 0.070 | 0.199 | 0.123 | 0.101 | 0.165 |
| **40-44** | 0.114 | 0.059 | 0.051 | 0.078 | 0.212 | 0.137 | 0.093 | 0.181 |
| **45-49** | 0.134 | 0.071 | 0.059 | 0.093 | 0.217 | 0.160 | 0.109 | 0.197 |
| **50-56** | 0.161 | 0.092 | 0.071 | 0.119 | 0.256 | 0.163 | 0.110 | 0.232 |
| **Total** | 0.135 | 0.067 | 0.064 | 0.089 | 0.226 | 0.143 | 0.117 | 0.191 |
| **1980** | | | | | | | | |
| **25-29** | 0.269 | 0.133 | 0.106 | 0.135 | 0.464 | 0.275 | 0.216 | 0.304 |
| **30-34** | 0.237 | 0.111 | 0.067 | 0.101 | 0.351 | 0.236 | 0.163 | 0.241 |
| **35-39** | 0.212 | 0.095 | 0.053 | 0.095 | 0.328 | 0.218 | 0.152 | 0.239 |
| **40-44** | 0.207 | 0.094 | 0.056 | 0.103 | 0.287 | 0.204 | 0.142 | 0.226 |
| **45-49** | 0.212 | 0.108 | 0.064 | 0.119 | 0.338 | 0.204 | 0.146 | 0.262 |
| **50-56** | 0.249 | 0.143 | 0.091 | 0.163 | 0.359 | 0.255 | 0.192 | 0.310 |
| **Total** | 0.233 | 0.116 | 0.075 | 0.120 | 0.357 | 0.239 | 0.176 | 0.267 |
| **1990** | | | | | | | | |
| **25-29** | 0.298 | 0.134 | 0.096 | 0.129 | 0.639 | 0.330 | 0.215 | 0.341 |
| **30-34** | 0.287 | 0.124 | 0.069 | 0.108 | 0.515 | 0.328 | 0.191 | 0.301 |
| **35-39** | 0.289 | 0.131 | 0.068 | 0.104 | 0.502 | 0.291 | 0.191 | 0.284 |
| **40-44** | 0.285 | 0.137 | 0.073 | 0.108 | 0.399 | 0.280 | 0.189 | 0.265 |
| **45-49** | 0.277 | 0.131 | 0.080 | 0.121 | 0.414 | 0.254 | 0.186 | 0.278 |
| **50-56** | 0.301 | 0.160 | 0.107 | 0.160 | 0.379 | 0.271 | 0.167 | 0.281 |
| **Total** | 0.290 | 0.135 | 0.080 | 0.120 | 0.477 | 0.301 | 0.193 | 0.296 |

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

## Table 3: Fraction of Prime Age Men who are Currently in the Armed Forces

| | 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 |
| **50-56** | 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 |
| **50-56** | 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 |
| **50-56** | 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 |
| **50-56** | 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 |

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

## Table 4: Last Year Worked for Currently Non-Employed Prime-Age Men

| **Panel A: Whites** | **1960** | **1970** | **1980** | **1990** |
|---|---|---|---|---|
| **Worked this year** | 39.5 | 41.8 | 33.9 | 29.8 |
| **Worked Last year** | 29.5 | 25.2 | 30.1 | 30.6 |
| **Worked 2-5 years ago** | 12.5 | 15.7 | 14.8 | 16.9 |
| **Worked 6-10 years ago** | 4.2 | 3.7 | 10.1 | 9.2 |
| **Worked More than 10 years ago** | 9.6 | 8.1 | 5.6 | 7.7 |
| **Never worked** | 4.7 | 5.5 | 5.5 | 5.8 |
| **Total** | 100.0 | 100.0 | 100.0 | 100.0 |

| **Panel B: Blacks** | 1960 | 1970 | 1980 | 1990 |
|---|---|---|---|---|
| **Worked this year** | 32.9 | 31.8 | 24.0 | 21.6 |
| **Worked Last year** | 30.0 | 26.4 | 24.8 | 26.4 |
| **Worked 2-5 years ago** | 15.5 | 17.2 | 15.7 | 19.0 |
| **Worked 6-10 years ago** | 5.4 | 4.0 | 17.1 | 13.2 |
| **Worked More than 10 years ago** | 12.3 | 12.8 | 9.1 | 9.9 |
| **Never worked** | 3.9 | 7.9 | 9.2 | 9.8 |
| **Total** | 100.0 | 100.0 | 100.0 | 100.0 |

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

## Table 5: Estimates of the Selective Withdrawal Hypothesis, All States

| | | 1960 | 1970 | 1980 | 1990 |
|---|---|---|---|---|---|
| **White Men** | **Observed-Mean** | $569 | $740 | $768 | $768 |
| | **Median-L** | 505 | 664 | 678 | 645 |
| | **Median-M** | 519 | 671 | 699 | 684 |
| **Black Men** | **Observed-Mean** | $349 | $491 | $562 | $560 |
| | **Median-L** | 285 | 411 | 424 | 394 |
| | **Median-M** | 319 | 449 | 505 | 478 |
| **Ratios (SE)** | **Median-L** | 0.564 | 0.619 | 0.625 | 0.611 |
| | | (0.005) | (0.007) | (0.001) | (0.002) |
| | **Median-M** | 0.615 | 0.669 | 0.722 | 0.699 |
| | | (0.001) | (0.009) | (0.005) | (0.009) |
| **Changes (SE)** | | **1960-70** | **1970-80** | **1980-90** | **1960-90** |
| | **Median-L** | 0.092 | 0.010 | -0.023 | 0.079 |
| | | (0.002) | (0.003 | (0.003) | (0.009) |
| | **Median-M** | 0.085 | 0.077 | -0.033 | 0.128 |
| | | (0.002) | (0.003) | (0.001) | (0.001) |

Point estimates reported are observed sample values of the statistics rounded to the nearest dollar (deflated to 1997 dollars). Bootstrapped standard-errors based on 100 replications (within year clusters) are reported in parenthesis. Median-L assumes that all nonworkers earn less than the aggregate median, Median-M assumes that only half of all non-workers earn less than the aggregate median. Change is computed as the difference in the log of the ratios. See Section 4.1 of text for exact definitions.

## Table 6: Changes in the Racial Wage Gap by Schooling and Age, All States

| Schooling | Less than HS | | | | HS | | | | HS+ | | | | All Groups | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 |
| **Observed** | 0.093 | 0.078 | 0.041 | 0.212 | 0.099 | 0.013 | 0.004 | 0.116 | 0.104 | 0.044 | 0.010 | 0.158 | 0.096 | 0.091 | 0.008 | 0.195 |
| **Matching** | 0.092 | 0.075 | 0.033 | 0.200 | 0.100 | 0.012 | 0.003 | 0.115 | 0.106 | 0.043 | 0.009 | 0.158 | 0.096 | 0.084 | 0.000 | 0.180 |
| **Median-L** | 0.099 | -0.058 | -0.119 | -0.078 | 0.123 | -0.082 | -0.049 | -0.008 | 0.096 | 0.014 | -0.015 | 0.095 | 0.106 | -0.001 | -0.048 | 0.057 |
| **Median-M** | 0.081 | 0.050 | -0.003 | 0.128 | 0.088 | -0.016 | -0.012 | 0.060 | 0.065 | 0.063 | -0.017 | 0.111 | 0.083 | 0.075 | -0.021 | 0.137 |
| **Cell Min** | 0.079 | -0.043 | 0.076 | 0.112 | 0.110 | -0.106 | 0.004 | 0.008 | 0.086 | -0.054 | 0.050 | 0.082 | 0.081 | -0.047 | 0.041 | 0.075 |

| Age | 25-29 | | | | 30-34 | | | | 40-44 | | | | 50-54 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 |
| **Observed** | 0.133 | 0.069 | -0.038 | 0.164 | 0.129 | 0.089 | -0.055 | 0.163 | 0.074 | 0.097 | 0.044 | 0.215 | 0.076 | 0.114 | 0.085 | 0.275 |
| **Matching** | 0.130 | 0.065 | -0.048 | 0.147 | 0.126 | 0.086 | -0.063 | 0.149 | 0.074 | 0.098 | 0.035 | 0.207 | 0.079 | 0.109 | 0.079 | 0.267 |
| **Median-L** | 0.153 | -0.036 | -0.112 | 0.005 | 0.155 | 0.013 | -0.131 | 0.037 | 0.065 | 0.053 | -0.021 | 0.097 | 0.085 | -0.025 | 0.090 | 0.150 |
| **Median-M** | 0.109 | 0.058 | -0.047 | 0.120 | 0.104 | 0.074 | -0.085 | 0.093 | 0.074 | 0.075 | 0.020 | 0.169 | 0.066 | 0.118 | 0.038 | 0.222 |
| **Cell Min** | 0.131 | -0.081 | 0.004 | 0.054 | 0.146 | -0.040 | -0.038 | 0.068 | 0.060 | -0.010 | 0.056 | 0.106 | 0.020 | -0.058 | 0.179 | 0.141 |

Point estimates reported are observed sample values of the statistics rounded to the nearest dollar. Bootstrapped standard-errors based on 100 replications (within year clusters) ranged from .0001 to .009. Matching assigns all non-workers in each (6 age x 5 Education) cell the mean ln weekly wage. Median-L assumes that all nonworkers earn less than the cell median, Median-M assumes that only half of all non-workers earn less than the median. Cell-Min assigns all nonworkers the wages of the 1-percentile in the cell. Change is computed as the difference in the log of the ratios. See Section 4.1 of text for exact definitions.

## Table 7: Changes in the Racial Wage Gap by Schooling and Age, Southern States

| Schooling | Less than HS | | | | HS | | | | HS+ | | | | All Groups | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 |
| **Observed** | 0.082 | 0.122 | 0.055 | 0.259 | 0.131 | 0.071 | 0.012 | 0.214 | 0.059 | 0.086 | 0.000 | 0.145 | 0.095 | 0.140 | 0.011 | 0.246 |
| **Matching** | 0.082 | 0.118 | 0.049 | 0.249 | 0.131 | 0.071 | 0.011 | 0.213 | 0.066 | 0.087 | 0.000 | 0.153 | 0.094 | 0.135 | 0.004 | 0.233 |
| **Median-L** | 0.099 | 0.042 | -0.055 | 0.086 | 0.144 | 0.028 | -0.033 | 0.139 | 0.063 | 0.076 | -0.028 | 0.111 | 0.107 | 0.074 | -0.045 | 0.136 |
| **Median-M** | 0.083 | 0.117 | 0.010 | 0.210 | 0.133 | 0.065 | 0.009 | 0.207 | 0.055 | 0.101 | -0.034 | 0.122 | 0.092 | 0.140 | -0.019 | 0.213 |
| **Cell Min** | 0.066 | 0.026 | 0.059 | 0.151 | 0.119 | -0.010 | 0.007 | 0.116 | 0.044 | -0.001 | 0.050 | 0.093 | 0.062 | 0.016 | 0.025 | 0.103 |

| Age | 25-29 | | | | 30-34 | | | | 40-44 | | | | 50-54 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 |
| **Observed** | 0.152 | 0.122 | -0.014 | 0.260 | 0.124 | 0.156 | -0.042 | 0.238 | 0.068 | 0.162 | 0.025 | 0.255 | 0.059 | 0.126 | 0.059 | 0.244 |
| **Matching** | 0.147 | 0.118 | -0.020 | 0.245 | 0.119 | 0.152 | -0.046 | 0.225 | 0.067 | 0.167 | 0.011 | 0.245 | 0.063 | 0.124 | 0.056 | 0.243 |
| **Median-L** | 0.191 | 0.049 | -0.121 | 0.119 | 0.157 | 0.103 | -0.095 | 0.165 | 0.086 | 0.112 | -0.014 | 0.184 | 0.019 | 0.010 | 0.067 | 0.096 |
| **Median-M** | 0.152 | 0.124 | -0.024 | 0.252 | 0.130 | 0.165 | -0.089 | 0.206 | 0.081 | 0.122 | 0.045 | 0.248 | 0.048 | 0.134 | 0.022 | 0.204 |
| **Cell Min** | 0.135 | -0.010 | 0.005 | 0.130 | 0.113 | 0.037 | -0.032 | 0.118 | 0.043 | 0.054 | 0.024 | 0.121 | -0.004 | -0.035 | 0.105 | 0.066 |

Point estimates reported are observed sample values of the statistics rounded to the nearest dollar. Bootstrapped standard-errors based on 100 replications (within year clusters) ranged from .0001 to .009. Matching assigns all non-workers in each (6 age x 5 Education) cell the mean ln weekly wage. Median-L assumes that all nonworkers earn less than the cell median, Median-M assumes that only half of all non-workers earn less than the median. Cell-Min assigns all nonworkers the wages of the 1-percentile in the cell. Change is computed as the difference in the log of the ratios. See Section 4.1 of text for exact definitions.

## Table 8: Within Cohort Changes in the Racial Wage Gap, by Schooling

| | | Less than HS | | | HS | | | HS+ | | | All Groups | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 1960-70 | 1970-80 | 1980-90 | 1960-70 | 1970-80 | 1980-90 | 1960-70 | 1970-80 | 1980-90 | 1960-70 | 1970-80 | 1980-90 |
| **Born 1915** | **Observed** | 0.079 | | | 0.101 | | | 0.116 | | | 0.062 | | |
| | **Matching** | 0.077 | | | 0.101 | | | 0.121 | | | 0.063 | | |
| | **Median-L** | -0.024 | | | 0.078 | | | 0.064 | | | -0.041 | | |
| | **Median-M** | 0.037 | | | 0.120 | | | 0.072 | | | 0.033 | | |
| **Born 1925** | **Observed** | 0.070 | 0.066 | | -0.008 | 0.130 | | 0.041 | -0.026 | | 0.034 | 0.066 | |
| | **Matching** | 0.067 | 0.068 | | -0.008 | 0.130 | | 0.043 | -0.025 | | 0.035 | 0.069 | |
| | **Median-L** | 0.037 | -0.092 | | 0.090 | -0.060 | | 0.001 | 0.056 | | 0.026 | -0.087 | |
| | **Median-M** | 0.057 | 0.046 | | 0.096 | 0.065 | | -0.019 | 0.106 | | 0.036 | 0.072 | |
| **Born 1935** | **Observed** | 0.051 | 0.018 | 0.082 | 0.067 | 0.000 | 0.089 | -0.003 | -0.022 | 0.161 | 0.004 | 0.002 | 0.102 |
| | **Matching** | 0.050 | 0.019 | 0.085 | 0.067 | 0.000 | 0.089 | -0.003 | -0.021 | 0.163 | 0.005 | -0.002 | 0.099 |
| | **Median-L** | 0.112 | -0.065 | 0.080 | 0.167 | -0.085 | 0.041 | 0.051 | 0.019 | 0.101 | 0.084 | -0.068 | 0.039 |
| | **Median-M** | 0.023 | 0.034 | 0.115 | 0.099 | -0.037 | 0.064 | 0.078 | 0.003 | 0.138 | 0.028 | 0.004 | 0.095 |
| **Born 1945** | **Observed** | | 0.025 | 0.088 | | -0.045 | 0.022 | | -0.090 | -0.002 | | -0.057 | 0.012 |
| | **Matching** | | 0.020 | 0.093 | | -0.045 | 0.022 | | -0.091 | -0.006 | | -0.060 | 0.005 |
| | **Median-L** | | -0.052 | -0.010 | | -0.087 | 0.010 | | 0.009 | -0.045 | | -0.056 | -0.056 |
| | **Median-M** | | -0.027 | 0.086 | | -0.030 | 0.028 | | -0.053 | -0.063 | | -0.042 | -0.013 |
| **Born 1955** | **Observed** | | | -0.042 | | | -0.006 | | | -0.146 | | | -0.094 |
| | **Matching** | | | -0.041 | | | -0.006 | | | -0.146 | | | -0.102 |
| | **Median-L** | | | -0.031 | | | -0.071 | | | -0.101 | | | -0.096 |
| | **Median-M** | | | -0.133 | | | -0.039 | | | -0.101 | | | -0.107 |
| **Cross Section** | **Observed** | 0.101 | 0.060 | 0.074 | 0.108 | 0.004 | 0.012 | 0.116 | 0.038 | 0.007 | 0.109 | 0.071 | 0.020 |
| | **Matching** | 0.102 | 0.052 | 0.063 | 0.108 | 0.002 | 0.011 | 0.120 | 0.034 | 0.009 | 0.109 | 0.063 | 0.013 |
| | **Median-L** | 0.089 | -0.066 | -0.114 | 0.130 | -0.103 | -0.044 | 0.083 | 0.048 | -0.023 | 0.101 | -0.007 | -0.048 |
| | **Median-M** | 0.072 | 0.037 | 0.035 | 0.119 | -0.037 | -0.002 | 0.116 | 0.047 | -0.017 | 0.093 | 0.059 | -0.005 |

Point estimates reported are observed sample values of the statistics rounded to the nearest dollar. Bootstrapped standard-errors based on 100 replications (within year clusters) ranged from .0001 to .009. Matching assigns all non-workers in each (6 age x 5 Education) cell the mean ln weekly wage. Median-L assumes that all nonworkers earn less than the cell median, Median-M assumes that only half of all non-workers earn less than the median. Change is computed as the difference in the log of the ratios. See Section 4.1 of text for exact definitions.

## Table 9: How Reasonable are these Estimates?
## Point Estimates of Average Weekly Wages for HS Dropouts and Men Aged 25-29

| E(ln w\|X) | Less than HS | | | | Aged 25-29 | | | |
|---|---|---|---|---|---|---|---|---|
| | 1960 | 1970 | 1980 | 1990 | 1960 | 1970 | 1980 | 1990 |
| **Whites** | | | | | | | | |
| **Observed** | 6.086 | 6.297 | 6.268 | 6.109 | 6.058 | 6.327 | 6.234 | 6.179 |
| **Matching** | 6.084 | 6.295 | 6.267 | 6.109 | 6.056 | 6.325 | 6.231 | 6.174 |
| **Median-L** | 6.124 | 6.323 | 6.246 | 5.987 | 6.112 | 6.373 | 6.278 | 6.186 |
| **Median-M** | 6.152 | 6.365 | 6.366 | 6.180 | 6.135 | 6.404 | 6.313 | 6.240 |
| **Cell Min** | 5.890 | 6.131 | 5.907 | 5.720 | 5.943 | 6.243 | 6.108 | 6.067 |
| **Blacks** | | | | | | | | |
| **Observed** | 5.604 | 5.908 | 5.957 | 5.839 | 5.594 | 5.996 | 5.972 | 5.879 |
| **Matching** | 5.602 | 5.905 | 5.952 | 5.827 | 5.590 | 5.989 | 5.960 | 5.855 |
| **Median-L** | 5.584 | 5.882 | 5.747 | 5.369 | 5.572 | 5.986 | 5.855 | 5.651 |
| **Median-M** | 5.688 | 5.982 | 6.033 | 5.844 | 5.677 | 6.055 | 6.022 | 5.902 |
| **Cell Min** | 5.358 | 5.678 | 5.411 | 5.300 | 5.377 | 5.808 | 5.592 | 5.555 |
| **Whites** | | | | | | | | |
| **Observed** | $494 | $613 | $629 | $540 | $479 | $626 | $590 | $561 |
| **Matching*** | 439 | 542 | 527 | 450 | 427 | 558 | 508 | 480 |
| **Median-L*** | 457 | 557 | 516 | 398 | 451 | 586 | 533 | 486 |
| **Median-M*** | 470 | 581 | 582 | 483 | 462 | 604 | 552 | 513 |
| **Cell Min*** | 361 | 460 | 368 | 305 | 381 | 514 | 449 | 431 |
| **Blacks** | | | | | | | | |
| **Observed** | $328 | $437 | $486 | $430 | $320 | $475 | $483 | $434 |
| **Matching*** | 271 | 367 | 385 | 339 | 268 | 399 | 388 | 349 |
| **Median-L*** | 266 | 359 | 313 | 215 | 263 | 398 | 349 | 285 |
| **Median-M*** | 295 | 396 | 417 | 345 | 292 | 426 | 412 | 366 |
| **Cell Min*** | 212 | 292 | 224 | 200 | 216 | 333 | 268 | 259 |

Point estimates reported are observed sample values of the statistics rounded to the nearest dollar. Bootstrapped standard-errors based on 100 replications (within year clusters) ranged from $1 to $6 for weekly wages. Matching assigns all non-workers in each (6 age x 5 Education) cell the mean ln weekly wage. Median-L assumes that all nonworkers earn less than the cell median, Median-M assumes that only half of all non-workers earn less than the median. See Section 4.1 of text for exact definitions. Statistics marked with a (*) report exp(E(ln w|X)).

**Table 10: How Important is Incarceration?**
**Changes in the Racial Wage Gap by Schooling and Age, All States without the Incarcerated Sample**

| Schooling | Less than HS | | | | HS | | | | HS+ | | | | All Groups | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 |
| **Observed** | 0.092 | 0.081 | 0.051 | 0.224 | 0.102 | 0.017 | 0.007 | 0.126 | 0.106 | 0.047 | 0.012 | 0.165 | 0.098 | 0.093 | 0.014 | 0.205 |
| **Matching** | 0.092 | 0.077 | 0.045 | 0.214 | 0.101 | 0.018 | 0.006 | 0.125 | 0.108 | 0.046 | 0.012 | 0.166 | 0.095 | 0.089 | 0.009 | 0.193 |
| **Median-L** | 0.075 | -0.029 | -0.046 | 0.000 | 0.119 | -0.066 | -0.048 | 0.005 | 0.083 | 0.031 | -0.014 | 0.100 | 0.090 | 0.021 | -0.028 | 0.083 |
| **Median-M** | 0.083 | 0.048 | 0.029 | 0.160 | 0.091 | -0.019 | 0.000 | 0.072 | 0.068 | 0.065 | -0.016 | 0.117 | 0.084 | 0.076 | -0.008 | 0.152 |

| Age | 25-29 | | | | 30-34 | | | | 40-44 | | | | 50-54 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 | 1960-70 | 1970-80 | 1980-90 | 1960-90 |
| **Observed** | 0.136 | 0.074 | -0.035 | 0.175 | 0.128 | 0.091 | -0.047 | 0.172 | 0.073 | 0.100 | 0.048 | 0.221 | 0.080 | 0.116 | 0.083 | 0.279 |
| **Matching** | 0.134 | 0.070 | -0.043 | 0.161 | 0.125 | 0.088 | -0.054 | 0.159 | 0.071 | 0.101 | 0.039 | 0.211 | 0.082 | 0.111 | 0.078 | 0.271 |
| **Median-L** | 0.144 | -0.006 | -0.070 | 0.068 | 0.103 | 0.044 | -0.121 | 0.026 | 0.065 | 0.055 | 0.009 | 0.129 | 0.097 | -0.001 | 0.085 | 0.181 |
| **Median-M** | 0.098 | 0.060 | -0.034 | 0.124 | 0.100 | 0.078 | -0.067 | 0.111 | 0.078 | 0.080 | 0.015 | 0.173 | 0.078 | 0.112 | 0.043 | 0.233 |

Point estimates reported are observed sample values of the statistics rounded to the nearest dollar. Bootstrapped standard-errors based on 100 replications (within year clusters) ranged from .0001 to .009.. Matching assigns all non-workers in each (6 age x 5 Education) cell the mean ln weekly wage. Median-L assumes that all nonworkers earn less than the cell median, Median-M assumes that only half of all non-workers earn less than the median. Cell-Min assigns all nonworkers the wages of the 1-percentile in the cell. Change is computed as the difference in the log of the ratios. See Section 4.1 of text for exact definitions.

**Appendix Table 1A:**
**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 2A:**
**Log Differences Between Average Weekly Wages With and Without the Armed Forces Sample**

| | | Whites | | | Blacks | | |
|---|---|---|---|---|---|---|---|
| | | **HS** | **HS+** | **Total** | **HS** | **HS+** | **Total** |
| | **1960** | | | | | | |
| **25-29** | | -0.027 | -0.007 | -0.017 | -0.028 | -0.010 | -0.023 |
| **30-34** | | -0.018 | -0.003 | -0.011 | -0.019 | -0.009 | -0.015 |
| **35-39** | | -0.009 | 0.001 | -0.005 | -0.008 | -0.003 | -0.006 |
| **40-44** | | -0.006 | 0.000 | -0.004 | -0.004 | 0.005 | -0.001 |
| **45-49** | | -0.004 | -0.002 | -0.003 | -0.004 | -0.005 | -0.004 |
| **50-56** | | -0.002 | -0.001 | -0.002 | -0.002 | 0.007 | 0.001 |
| **Total** | | -0.013 | -0.002 | -0.008 | -0.015 | -0.004 | -0.011 |
| | **1970** | | | | | | |
| **25-29** | | -0.013 | -0.015 | -0.014 | -0.012 | -0.014 | -0.013 |
| **30-34** | | -0.014 | -0.006 | -0.010 | -0.010 | -0.011 | -0.010 |
| **35-39** | | -0.014 | -0.006 | -0.010 | -0.015 | -0.006 | -0.012 |
| **40-44** | | -0.005 | -0.002 | -0.004 | -0.003 | -0.001 | -0.002 |
| **45-49** | | -0.001 | 0.001 | 0.000 | -0.003 | -0.001 | -0.002 |
| **50-56** | | 0.000 | 0.000 | 0.000 | -0.002 | 0.003 | 0.000 |
| **Total** | | -0.008 | -0.006 | -0.007 | -0.009 | -0.007 | -0.008 |
| | **1980** | | | | | | |
| **25-29** | | -0.012 | -0.005 | -0.008 | -0.013 | -0.011 | -0.012 |
| **30-34** | | -0.012 | -0.005 | -0.008 | -0.012 | -0.005 | -0.009 |
| **35-39** | | -0.009 | -0.006 | -0.007 | -0.008 | -0.010 | -0.009 |
| **40-44** | | -0.003 | -0.003 | -0.003 | 0.003 | -0.004 | 0.000 |
| **45-49** | | -0.001 | 0.000 | 0.000 | 0.001 | 0.001 | 0.001 |
| **50-56** | | 0.000 | 0.000 | 0.000 | 0.001 | 0.001 | 0.001 |
| **Total** | | -0.007 | -0.004 | -0.005 | -0.008 | -0.007 | -0.007 |
| | **1990** | | | | | | |
| **25-29** | | -0.006 | -0.008 | -0.007 | 0.003 | -0.015 | -0.005 |
| **30-34** | | -0.003 | -0.007 | -0.005 | 0.001 | -0.009 | -0.004 |
| **35-39** | | -0.002 | -0.005 | -0.004 | -0.002 | -0.004 | -0.003 |
| **40-44** | | -0.001 | -0.003 | -0.002 | 0.000 | -0.002 | -0.001 |
| **45-49** | | 0.000 | 0.000 | 0.000 | 0.000 | -0.002 | -0.001 |
| **50-56** | | 0.000 | 0.000 | 0.000 | 0.000 | 0.002 | 0.001 |
| **Total** | | -0.003 | -0.004 | -0.004 | 0.001 | -0.006 | -0.003 |

Bootstrapped standard-errors based on 100 replications (within year clusters) were computed for each cell. Asterix (*) indicates that the difference is statistically significant at the 5 percent significance level.

**Appendix Table 3A:**
**Log Difference between Average Weekly Wages of all Respondents and those who Worked at Least 13 Weeks Last Year.**

| | Whites | | | | Blacks | | | |
|---|---|---|---|---|---|---|---|---|
| | < than HS | HS | HS+ | Total | < than HS | HS | HS+ | Total |
| **1960** | | | | | | | | |
| **25-29** | 0.003 | 0.007 | 0.009 | 0.006 | 0.026* | 0.006 | 0.011 | 0.020 |
| **30-34** | 0.004 | 0.003 | -0.002 | 0.002 | 0.002 | 0.024 | 0.006 | 0.007 |
| **35-39** | 0.002 | 0.002 | -0.001 | 0.001 | 0.012 | 0.017 | 0.016 | 0.013 |
| **40-44** | 0.001 | -0.001 | 0.003 | 0.001 | 0.031* | 0.005 | 0.038 | 0.028 |
| **45-49** | 0.000 | 0.005 | -0.001 | 0.001 | 0.020 | -0.007 | 0.015 | 0.017 |
| **50-56** | 0.002 | 0.003 | -0.002 | 0.001 | 0.022 | 0.006 | -0.027 | 0.018 |
| **Total** | 0.002 | 0.003 | 0.001 | 0.002 | 0.019 | 0.011 | 0.012 | 0.017 |
| **1970** | | | | | | | | |
| **25-29** | 0.010 | 0.004 | 0.005 | 0.005 | 0.017 | 0.034 | 0.036 | 0.027 |
| **30-34** | 0.008 | 0.002 | 0.001 | 0.003 | 0.027 | 0.014 | 0.004 | 0.019 |
| **35-39** | 0.007 | 0.001 | -0.001 | 0.002 | 0.022 | 0.021 | 0.003 | 0.019 |
| **40-44** | 0.004 | 0.001 | 0.000 | 0.002 | 0.028 | 0.010 | 0.012 | 0.023 |
| **45-49** | 0.003 | 0.001 | -0.001 | 0.001 | 0.022 | 0.005 | -0.002 | 0.016 |
| **50-56** | 0.008 | 0.003 | 0.003 | 0.005 | 0.019 | 0.012 | 0.004 | 0.017 |
| **Total** | 0.006 | 0.002 | 0.001 | 0.003 | 0.022 | 0.019 | 0.014 | 0.021 |
| **1980** | | | | | | | | |
| **25-29** | 0.004 | 0.004 | -0.001 | 0.001 | 0.056* | 0.017 | 0.009 | 0.024 |
| **30-34** | 0.002 | 0.001 | 0.001 | 0.001 | 0.032* | 0.015 | 0.003 | 0.015 |
| **35-39** | 0.006 | 0.002 | 0.001 | 0.002 | 0.011 | 0.008 | 0.003 | 0.008 |
| **40-44** | 0.010 | 0.004 | 0.001 | 0.004 | 0.018 | 0.009 | 0.010 | 0.013 |
| **45-49** | 0.005 | 0.003 | 0.000 | 0.003 | 0.003 | 0.012 | 0.005 | 0.006 |
| **50-56** | 0.009 | 0.005 | 0.001 | 0.005 | 0.010 | 0.003 | -0.006 | 0.006 |
| **Total** | 0.006 | 0.003 | 0.000 | 0.003 | 0.021 | 0.012 | 0.005 | 0.014 |
| **1990** | | | | | | | | |
| **25-29** | -0.007 | 0.001 | -0.002 | -0.001 | 0.037* | 0.008 | 0.011 | 0.014 |
| **30-34** | -0.006 | 0.001 | -0.003 | -0.002 | 0.026* | 0.006 | -0.009 | 0.003 |
| **35-39** | 0.003 | -0.001 | -0.002 | -0.001 | 0.017 | 0.007 | -0.007 | 0.003 |
| **40-44** | -0.007 | -0.001 | -0.003 | -0.003 | 0.042* | -0.008 | -0.007 | 0.003 |
| **45-49** | -0.001 | -0.001 | -0.003 | -0.002 | 0.021 | 0.008 | -0.003 | 0.009 |
| **50-56** | 0.003 | 0.001 | -0.003 | -0.001 | -0.008 | -0.002 | -0.007 | -0.005 |
| **Total** | -0.002 | 0.000 | -0.003 | -0.002 | 0.021 | 0.004 | -0.003 | 0.005 |

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

## Appendix Table 4A:
## Fraction of men with Weekly Wage Observations

| | Whites | | | | Blacks | | | |
|---|---|---|---|---|---|---|---|---|
| | < than HS | HS | HS+ | Total | < than HS | HS | HS+ | Total |
| **1960** | | | | | | | | |
| **25-29** | 0.925 | 0.966 | 0.954 | 0.948 | 0.858 | 0.912 | 0.919 | 0.876 |
| **30-34** | 0.936 | 0.968 | 0.967 | 0.954 | 0.874 | 0.901 | 0.914 | 0.883 |
| **35-39** | 0.928 | 0.963 | 0.962 | 0.948 | 0.875 | 0.910 | 0.935 | 0.886 |
| **40-44** | 0.919 | 0.956 | 0.952 | 0.937 | 0.864 | 0.898 | 0.958 | 0.876 |
| **45-49** | 0.907 | 0.943 | 0.935 | 0.921 | 0.863 | 0.887 | 0.903 | 0.868 |
| **50-56** | 0.878 | 0.911 | 0.914 | 0.890 | 0.825 | 0.906 | 0.909 | 0.835 |
| **Total** | 0.913 | 0.955 | 0.951 | 0.934 | 0.860 | 0.905 | 0.924 | 0.872 |
| **1970** | | | | | | | | |
| **25-29** | 0.927 | 0.975 | 0.962 | 0.960 | 0.852 | 0.923 | 0.925 | 0.892 |
| **30-34** | 0.937 | 0.977 | 0.981 | 0.968 | 0.883 | 0.937 | 0.963 | 0.912 |
| **35-39** | 0.933 | 0.978 | 0.984 | 0.966 | 0.877 | 0.931 | 0.960 | 0.903 |
| **40-44** | 0.934 | 0.971 | 0.976 | 0.958 | 0.872 | 0.933 | 0.957 | 0.895 |
| **45-49** | 0.916 | 0.963 | 0.972 | 0.946 | 0.860 | 0.920 | 0.939 | 0.878 |
| **50-56** | 0.887 | 0.945 | 0.957 | 0.920 | 0.831 | 0.894 | 0.915 | 0.846 |
| **Total** | 0.918 | 0.969 | 0.972 | 0.952 | 0.861 | 0.926 | 0.944 | 0.888 |
| **1980** | | | | | | | | |
| **25-29** | 0.871 | 0.956 | 0.957 | 0.947 | 0.686 | 0.847 | 0.884 | 0.818 |
| **30-34** | 0.867 | 0.958 | 0.971 | 0.954 | 0.757 | 0.863 | 0.910 | 0.851 |
| **35-39** | 0.879 | 0.953 | 0.972 | 0.949 | 0.776 | 0.866 | 0.909 | 0.846 |
| **40-44** | 0.866 | 0.953 | 0.968 | 0.940 | 0.796 | 0.863 | 0.909 | 0.844 |
| **45-49** | 0.844 | 0.931 | 0.954 | 0.916 | 0.737 | 0.844 | 0.902 | 0.799 |
| **50-56** | 0.807 | 0.901 | 0.928 | 0.876 | 0.694 | 0.806 | 0.844 | 0.741 |
| **Total** | 0.848 | 0.944 | 0.961 | 0.933 | 0.736 | 0.852 | 0.896 | 0.820 |
| **1990** | | | | | | | | |
| **25-29** | 0.831 | 0.940 | 0.961 | 0.941 | 0.592 | 0.792 | 0.892 | 0.791 |
| **30-34** | 0.813 | 0.939 | 0.968 | 0.944 | 0.647 | 0.790 | 0.904 | 0.814 |
| **35-39** | 0.791 | 0.925 | 0.964 | 0.938 | 0.629 | 0.800 | 0.894 | 0.811 |
| **40-44** | 0.764 | 0.914 | 0.954 | 0.926 | 0.694 | 0.800 | 0.886 | 0.814 |
| **45-49** | 0.773 | 0.912 | 0.949 | 0.914 | 0.667 | 0.809 | 0.875 | 0.790 |
| **50-56** | 0.744 | 0.880 | 0.926 | 0.877 | 0.687 | 0.774 | 0.882 | 0.773 |
| **Total** | 0.786 | 0.922 | 0.957 | 0.927 | 0.658 | 0.794 | 0.892 | 0.801 |

Sample includes all respondents who had wage and salary or military earnings last year and worked at least one week last year. Self-Employed workers are excluded from the analysis.