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THREE ESSAYS ON CAUSES OF SKILL, RACIAL AND ETHNIC LABOR MARKET DIFFERENCES

By

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ABSTRACT

THREE ESSAYS ON CAUSES OF SKILL, RACIAL AND ETHNIC LABOR MARKET DIFFERENCES

By

Linda Andrea Bailey

Labor market differences by skill, race, and ethnicity persist in the U.S. This dissertation investigates possible influences on labor markets of less-skilled workers and racial and ethnic labor market gaps. First, it asks how major immigration reform affected relative labor markets. Second, it asks how labor markets of blacks are affected by relatively higher arrest rates. Finally, it asks how immigrant status affected displaced workers' labor market re-entry.

Chapter 1 examines the 1986 Immigration Reform and Control Act (IRCA) impacted less-skilled native workers' relative labor markets. I allow the Act's effect to vary by race. The Act allowed approximately three million illegal immigrants to apply for legal residence in the U.S. Previous work examining the Act's effects has not allowed varying racial or skill impact. I find that IRCA had differential effects by skill level and race. These effects were either short-lived or inconsequentially small in magnitude.

Chapter 2 examines the relationship between higher relative arrest rates for blacks and their relative labor market outcomes. I find that cities in which blacks have higher relative arrest rates are those in which they have worse relative hourly wages. After controlling for city-specific effects, there is no evidence that higher arrests lead to worse

relative labor markets for black males. After controlling for city specific effects, blacks are still assigned to lower status occupations the higher their relative arrest rate.

Chapter 3 examines the effect of immigrant status on labor market re-entry for displaced workers. Immigrant status positively affects the chances of being re-employed and lowers earnings losses among workers who make a full-time to full-time job transition. The lower earnings losses among immigrants appear to be due to their concentration in the low end of the earnings distribution. Possible explanations for the higher employment probabilities of immigrants are examined. Immigrants are more like to switch occupations when re-employed. I do not find support for the hypothesis that differential access to government income support or non-labor income induced immigrants to search more intensely for work. Additional differences among immigrant groups are examined.

To my parents, Francis and Pauline, and my siblings, Bernie and Stephen, who have loved and supported me every step of the way to the fulfillment of this dream.

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CHAPTER 1

DID THE IMMIGRATION REFORM AND CONTROL ACT AFFECT RELATIVE LABOR MARKET OUTCOMES?

1.1 Introduction

The Immigration Reform and Control Act (IRCA), passed in October 1986, aimed to reduce the number of undocumented immigrants in the U.S. in two ways. First, IRCA enacted penalties for employers who hired illegal aliens or who failed to check legal work status before hiring, in order to deter prospective illegal immigration by removing the lure of jobs. Second, IRCA provided amnesty for qualified illegal aliens and the opportunity for them to obtain legal immigrant status. Amnesty was granted in recognition of the commitment of these immigrants to residence in the U.S. and for some their attachment to the U.S. labor market.

The general immigration literature has recognized that differences in skill level and race matter when measuring the impact of immigration on domestic labor markets.[1] The skill level of the immigrant group may have differential impact on the U.S. labor market depending on the skill distribution of the natives in the market. Previous literature exploring the IRCA's impact has not allowed the effect to vary by skill, race and legal status while using a measure directly related to IRCA. This paper adds to the current literature by exploring a question which cannot be answered from previous work: has the IRCA—which largely increased the legal labor supply with Hispanics of relatively low education levels—worsened the relative position of disadvantaged American?

[1] See Card (2001) and Jaeger (1996).

While many previous studies (see for example Crane et al. 1990; Juffras 1991; Phillips and Massey 1999) have investigated the impact of IRCA on various aspects of the U.S. labor market, exploration of IRCA's impact on native groups by skill and race is incomplete. Sorensen and Bean (1994) and Bucci and Tenorio (1997) examine the effects on native workers by looking at changes in wages before and after IRCA's passage. Sorensen and Bean look at the log of real wages of males with Mexican ancestry, as well as native and immigrant white males. They find that there were no significant changes in the wages of white male natives or white male immigrants after the IRCA. Bucci and Tenorio investigate changes in the immigrant-native wage gap potentially caused by IRCA's employer sanctions provisions. Discrimination against immigrants is hypothesized to occur if employers view expected IRCA fines as a tax on immigrant labor, but not on native labor. They find that the increase in the native-immigrant wage gap is primarily due to increased nepotism towards natives rather than increased discrimination against immigrants. Neither paper uses a direct measure of either the amnesty or sanctions. Analyses such as these that rely solely on the passage of time for their identification strategy are subject to the criticism that concurrent changes offer alternative explanations for the changes occurring in the labor markets under study.

Therefore, the empirical strategy I propose here uses both wage changes and a direct measure of the potential impact of the IRCA amnesty. Cobb-Clark, Shiells and Lowell (1995) (hereafter CSL), and Fry, Lowell and Haghihat (1995) (hereafter FLH), examine IRCA's impact on average wages at the metropolitan area-industry level. Both papers look at the impact of the employer sanctions and amnesty components of IRCA separately, using regional measures of average monetary fines and the ratio of annual

amnesty applications to metropolitan area population, respectively. Both papers find that higher relative amnesty populations are associated with higher wages.[2] The industries were chosen for their high immigrant participation rates and the metropolitan-level wage data used averages across workers of different skill level, race and legal status.

Though the IRCA was passed in 1986, a full understanding of its impact remains relevant in light of on-going support for further such amnesties by the business community and labor unions, which both benefit from immigration.[3] Though the IRCA amnesty was supposed to be a one-time occurrence, an additional 6 million people have taken advantage of several amnesties after 1986 and more are likely (Briggs, Jr., 2003).[4] Most recently, a policy similar in magnitude to the IRCA amnesty has been proposed to regularize the estimated 5 million plus undocumented immigrants currently residing in the U.S. [5,6]

1.2 Potential Impact on the Native Labor Market

Along with the introduction of penalties for employers of illegal labor, IRCA introduced two programs under which illegal aliens could apply to regularize their status. First, the legally authorized worker (LAW) program, also called the general amnesty, allowed applications from illegal aliens who had resided in the U.S. since January 1982, thereby showing a commitment to long-term residence. Second, the special agricultural

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[2] Both papers also instrument for the amnesty populations. The stated result is robust.

[3] The AFL-CIO openly states its support for further amnesty of illegal workers (see http://www.aflcio.org/aboutaflcio/ecouncil/ec0731a2001.cfm, site checked January 11, 2003).

[4] In fact, Chau (2001) models the decision of whether or not a country should grant an amnesty at a point in time and finds, under costly internal enforcement of immigration laws, intermittent amnesties are optimal.

[5] October 1996 estimates of the illegal population is between was between 4.6 and 5.4 million persons and growing at a rate of over ¼ million persons per year (Source: "Illegal Alien Resident Population," USCIS accessible http://uscis.gov/graphics/shared/aboutus/statistics/illegalalien/).

[6] The current President has proposed a legalized work visitor program reminiscent of the Bracero program.

worker (SAW) program allowed illegal aliens who had worked in agriculture in seasons prior to the IRCA to apply for amnesty.

The employer sanction and amnesty provisions targeted the undocumented population. Nevertheless, natives could also be affected. The employer sanctions should have reduced the demand for undocumented workers. The downward shift of the demand curve for undocumented labor from D_u to D_u' is shown in Figure 1. I follow CSL and view fines as a tax on unauthorized labor. Thus, D_u' represents the demand curve net of expected fines. Figure 2 shows the corresponding upward shift of the documented labor demand curve for low-skilled labor, D_a to D_a'. As hypothesized by CSL, if the SAW program induces new undocumented migrants, this would shift the supply curve for unauthorized workers further to the right, S_u to S_u'.[7]

Based on this model, effective employer sanctions should increase demand for less-educated authorized workers to fill new vacancies that would otherwise be filled by illegal immigrants.[8] In the extreme case that undocumented labor only occupied jobs that natives did not want, sanctions should not affect native outcomes. Papademetriou et al. (1991 p.42) use data from a survey of employers and note that firms that report regularly hiring unauthorized workers were "9.5% more likely to increase wages to attract authorized workers" than other firms. Hiring fines are imposed on firms that "knowingly" hire illegal workers. FLH find significant positive wage effects related to larger hiring fines in a metropolitan area. This casts doubt on the idea that illegal and

[7] New illegal aliens, who otherwise may have remained in the home country, are thought to have entered the U.S. in order to fraudulently apply for amnesty (see Designations of Temporary Protected Status and Fraud in Prior Amnesty Programs: Hearing before the subcommittee on Immigration and Claims http://commdocs.house.gov/committees/judiciary/hju59871.000/hju59871_0f.htm).

[8] IRCA's employer sanctions did not affect existing illegal workers who stayed with the same employer. Only new hires were affected by the law.

legal workers are in completely separate markets. Notably, FLH find that the lower wages resulting from fines levied on employers for not checking work authorization offset the effects of hiring fines.

The amnesty provision increased the supply of authorized workers by giving legal status to millions of illegal aliens. However, undocumented workers may still confine themselves to the lower segment of the labor market solely because of their legal status. Thus, changes in legal status may spell increased competition for authorized workers as amnesty recipients move out of marginalized jobs.[9] In Figure 2, S_a shifts to S_a' after the amnesty. As these persons were initially unauthorized, there should be an initial reduction in the supply of undocumented workers, S_u' shifts to S_u''.

Depending on the relative size of the increased competition and demand shifts, declining labor market outcomes for natives may or may not be evident. Since there are opposing effects on wages, the direction of IRCA's impact is an empirical question.

In addition to the aforementioned, IRCA could affect labor market outcomes of native blacks particularly strongly. Since non-Hispanic blacks and Hispanics have greater occupational similarity than non-Hispanic whites and Hispanics, increased competition for low-skilled jobs from the newly-legalized residents could have a greater negative impact on blacks than whites.[10] Keeping in mind, however, blacks may have competed with undocumented workers prior to the amnesty; employers who now comply with IRCA would hire only authorized workers, perhaps improving the labor market opportunities of these legal minorities. Most likely, this would be apparent in the market for less-educated labor as undocumented workers generally fall in this category.

[9] Kossoudji and Cobb-Clark (2000) find evidence of upward occupational mobility of the newly legalized.
[10] See Bansak and Raphael (2001) and Altonji and Card (1991).

Similarly, if Hispanics and non-Hispanic blacks are competing for the same jobs and documented Hispanics experience IRCA-induced discrimination, blacks may experience better labor market outcomes. Thus, the effect on blacks relative to other groups is ambiguous and is a major focus of this study.

1.3 DATA and EMPIRICAL STRATEGY

1.3.1 Data

To convincingly examine the effects of the amnesty, a direct measure of its potential impact to a locality provides, arguably, the best tool. The 1990 Legalization Summary Tapes, created by the Immigration and Naturalization Service (INS), provide one.[11] These data contain the IRCA amnesty applicants' county of intended residence. Due to confidentiality requirements, data on the actual number of amnesty recipients in an MSA is unavailable. Nevertheless, over 90% of applications were ultimately approved.[12] Hence, the Summary Tapes give a reasonable measure of the actual size of the legalized population. I divide the number of MSA applicants under the general amnesty program by the 1982 population in the corresponding MSA.[13] The resulting variable becomes the proportion of a city's 1982 population that applied for the amnesty (hereafter PROP).

For data on native labor market outcomes and demographic features, I use the 1986, 1987, 1991, and 1992 Current Population Survey (CPS) March files for the main

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[11] I am extremely grateful to Deborah Cobb-Clark for providing this data.

[12] See Chart 1.4 for denial rates. Although, standard processing of LAW applications was done regionally, there is no reason to believe that denials systematically varied by MSA. Juffras (1991 pp. 58-60) discusses the legalization process, including denial rates. He notes that initially restrictive document requirements were challenged in court and overturned. Thus, the later processing allowed for liberal documentation resulting in a denial rate of 1 in 10,000 applications in the second phase of the general legalization program.

[13] The 1982 population figures are based on interpolation between the 1980 and 1983 MSA populations from the 1985 Statistical Abstract of the United States.

analysis. These files provide labor market information by skill and race groups from the previous year.[14] 1985 and 1986 data identify the relative pre-amnesty position of different skill and racial groups in cities with higher amnesty populations.[15] The latter years provide post-amnesty labor market data.[16] Applications to take advantage of the amnesty under the general legalization program were accepted from May 5, 1987 to May 4, 1988. The SAW program had an official deadline of November 30, 1988.[17] Thus, prior to 1987 there should be no legalization effects. Applicants obtained temporary work status, which was revocable if amnesty was denied. Thus, they did not have the same legal work status as permanent residents until later.

Chart 1.5 presents the inflow of legal immigrants from 1900-1996. The large increase in legally admitted immigrants in 1990 and 1991 relative to trends in prior years was due to the legalization applicants. Approximately 74% of LAW applicants and 5% of SAW applicants had achieved permanent residency status by the end of the fiscal year 1990. By the end of the fiscal year 1991, over 2.4 million people had gained permanent residence under both programs. This comprised approximately 90% of those who would eventually be granted permanent residence.[18] Hence, at this point, much of the amnesty population had the same legal labor market standing as a native.

Ideally, this policy analysis should take into account the effect on all authorized workers, but legal status is not available in the CPS data used here. As a result, I choose to examine a subset of authorized workers, natives. Unfortunately, in the years of data

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[14] These 'before' years are chosen as number of MSAs identified increased dramatically from the 1986 March files onward. In addition, some MSA definitions changed at the same time as the increase. Thus, using data from earlier files would make comparisons with data from after the IRCA problematic.
[15] These data are in the 1986 and 1987 CPS March files.
[16] CPS Outgoing Rotations did not identify as many cities as the CPS March files until 1989.
[17] Late filings and court challenges to restrictions on those who qualified for the amnesty allowed for further applications.
[18] See 1991 INS Statistical Yearbook pp. 70-1.

used for this analysis, native status is also not available.[19] I, therefore, use a proxy for nativity, non-Hispanic black and white status. Thus, the sample excludes anyone declaring Spanish ethnicity and retains those reporting race as black or white. This is reasonable as tabulations from the 1990 PUMS 1% sample[20] indicate that, nationally, about 90% of non-Hispanic whites and non-Hispanic blacks report that they were born in the U.S. Around 50% of those with Spanish ethnicity and approximately 18% of non-Pacific Islander Asians report being U.S. born. This holds even in high immigrant cities. In the New York and Los Angeles MSAs, approximately 90% of non-Hispanic whites still report that they are U.S. born, while 81% of non-Hispanic blacks in New York and 88% of those in Los Angeles report that they are U.S. born. For those claiming Spanish ancestry, those figures fall to 45% for New York and 30% for Los Angeles. The figures for non-Pacific Islander Asians are below 20%.

Hispanics would confound the effect of the IRCA because they could experience discrimination or penalties for illegal status after 1986 or if they are amnesty recipients, they would earn a return to legal status.[21] By limiting the sample to non-Hispanics, I essentially avoid the problems associated with expected differences in the IRCA's impact by legal status. In addition to ethnicity and race restrictions, due to the interest in labor market outcomes, my sample is limited to the working population, aged 18 to 64 years. Only residents of MSAs identified in the CPS March files are included. I exclude states

[19] CPS Supplements in 1983, 1986, 1988 and 1989 used by Sorensen and Bean (1994) contain immigrant status, but there are fewer MSAs identified in these supplements than in the March files until 1989.
[20] This data set contains demographic information on a 1% sample from the 1990 U.S. Census.
[21] Newly legalized workers have been documented to experience high wage growth, while those remaining illegal have experienced wage penalties. See Kossoudji and Cobb-Clark (2000, 2002) and Phillips and Massey (1999).

in New England.[22] As New England counties are not strictly contained in one MSA, the county level data in the Summary Tapes cannot be reliably allocated to a particular city.

The hourly wage provides one assessment of the labor market impact of the IRCA. However, examination of changes to hourly wages does not adequately address whether the native population is better or worse off. If, for instance, employers shift from full time workers, who have expensive benefits, to more highly paid part-time workers without such benefits, we may optimistically misinterpret the labor market conditions after IRCA. We may see average hourly wages increase, but lower annual incomes, as a result of the reduced hours. This work therefore utilizes the CPS data to examine a broader range of outcomes.

The CPS March files provide data for the previous year on annual wage income, usual hours worked per week, weeks worked per year, employment status, and family poverty level. These outcomes are investigated here. With the exception of log hourly wages, sample sizes utilize all those with information whose hourly wages do not exceed $100 (1982-1984 dollars). Only positive log hourly wages are used for the wage regressions. Hourly wages are calculated by dividing annual labor income by annual weeks times usual weekly hours. In order to calculate log annual labor income, 0.00001 replaces zeros for those with no wage income reported. The U.S. Consumer Price Index for all Urban Consumers (CPI-U) adjusts wage and income data to 2003 dollars. Of note, some MSAs had no applicants; the Los Angeles-Long Beach metropolitan area had the highest PROP with approximately 8 amnesty applicants out of every 100 residents in 1982.

[22] New England states are Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island and Vermont. City boundaries/groups rather than county groups define New England MSAs. There are 23 MSAs, which would constitute 8.6% of the sample, that are excluded because of this restriction.

Table 1.1 gives mean outcomes, for males and females, before and after IRCA for the three education groups examined: high school dropouts (HDOs), high school graduates without a four-year college degree (HGs) and college graduates (CGs). The means are disaggregated by a dichotomous version of the variable used to identify the IRCA's effect: MSAs with relative low amnesty population shares (low-PROP MSAs) and those with relatively high shares. Low-PROP areas are those with amnesty shares at or below the median MSA share of 0.00046.

Comparing hourly wages before and after IRCA for low-PROP MSAs for the male sample, we can see that all education groups had lower real wages after 1987. The same is true for high-PROP MSAs. If we examine wages before and after IRCA to determine its effect, we could be erroneously attributing negative changes to the policy. On the other hand, a comparison of wages in low-PROP and high-PROP MSAs after 1987 reveals that the latter have higher average wages for all education groups. Examination of pre-1987 figures, however, shows that high-PROP areas have historically high wages. Thus, a comparison of low-PROP and high-PROP MSAs only might give the impression that the policy increased hourly wages. To calculate the IRCA's effect, we should control for initial conditions. We can do this by comparing wage changes between high and low-PROP MSAs before and after 1987.

However, if changes over time in low-PROP cities differed systematically from those in high-PROP cities, we could once again make erroneous conclusions about the IRCA's effect. In addition, amnesty recipients belong mainly to the lowest education category, HDOs. There could be competitive effects from IRCA on HDOs, who are the most similar in human capital terms, while complementary effects could be exerted on

higher education groups. For these reasons, a third level of differencing to capture differences in MSA time trends is desired.

1.3.2 Empirical Strategy

My approach is similar to CSL and FLH's in that the identifying variable is at the MSA level. The variable differs slightly as they use annual LAW applications divided by the MSA population in the year of application, whereas PROP, the variable used here is the total stock of applications under the program divided by the 1982 population. PROP has no time dimension, varying only across cities. In order to examine the effects in the years when most LAW applicants became permanent residents I cannot use their measure. Data on admissions of amnesty recipients as legal permanent residents by year and metropolitan area is not available. Additionally, the use of the later population figures in the denominator also biases the results towards positive findings when compared to the original measure. Cities with better labor markets may have growing populations, decreasing PROP in areas with better labor markets.

Using a 'difference-in-differences' approach, I examine the aforementioned labor market outcomes and identify the desired effect by interactions between lower education group indicators for HDOs and HGs, PROP and a post-IRCA indicator. Along with metropolitan area fixed effects, the interaction of PROP with the lower education groups control for the relative position of HDOs and HGs in cities with larger amnesty populations, prior to IRCA. These terms interacted with the post-IRCA indicator identify the change in the relative position of less-skilled workers due to the IRCA. PROP interacted with a post-IRCA indicator captures any spurious correlation between changes in cities with higher amnesty populations and labor market outcomes.

The following regression estimates the effect of the newly legalized on the change in the relative labor market outcomes of the less-educated (hereafter Model 1):[23]

$$Y_{ijt} = X_{ijt}\beta_1 + AA_t\beta_2 + HDO_{ijt}\beta_3 + HG_{ijt}\beta_4 + HDO_{ijt}*PROP_j\beta_5 + HG_{ijt}*PROP_j\beta_6 + HDO_{ijt}*AA_t\beta_7 + HG_{ijt}*AA_t\beta_8 + PROP_j*AA_t\beta_9 + HDO_{ijt}*PROP_j*AA_t\beta_{10} + HG_{ijt}*PROP_j*AA_t\beta_{11} + BLACK_{ijt}\beta_{12} + BLACK_{ijt}*AA_t\beta_{13} + MSA_j + \varepsilon_{ijt} \quad (1.1)$$

where, Y_{ijt} denotes various labor market outcomes (log hourly wage, log annual income, hours worked per week, annual weeks, probability of non-employment, probability of living in a poor family). X_{ijt} includes indicators for marital status, age, age-squared; AA_t denotes the after the amnesty indicator; HDO_{ijt} high school dropout indicator; HG_{ijt} high school graduate-incomplete college indicator; $PROP_j$ proportion of MSA amnesty Applicants to 1982 MSA Population; $BLACK_{ijt}$ black indicator; MSA_j metropolitan area fixed effects; ε_{ijt} – random error with an expected value of zero and independent of the above controls.

Metropolitan area fixed effects control for permanent differences in localities that may be correlated with the size of the amnesty population, e.g., the historic attractiveness of the city to immigrants.[24] In terms of the regression, this requires omitting PROP in level form, as it is perfectly collinear with the metropolitan area fixed effects.

The coefficient β_9 captures local labor market changes that may be unrelated to the amnesty, but which occurred in higher PROP cities after IRCA. β_{10} and β_{11} give the relative effects of the legalized population on the change in the gaps between HDOs and HGs and their more educated counterparts.[25]

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[23] Standard errors are robust to variation across metropolitan area pre- and post-IRCA observations.
[24] The MSA fixed effects are estimated by a dummy variable regression.
[25] An alternate method would be to have fixed effects interacted with low education and post-Amnesty indicators, followed by an examination of changes in the return to education based on different sizes of the

The estimated IRCA effects are not subject to the argument that the amnesty population located in areas with better labor markets for such workers. Intuitively, the general amnesty was directed at immigrants that were considered to have long-term commitments in terms of residency, having lived in the U.S since 1982. So it is reasonable to assume these applicants were already settled by the period of the labor market data used here. Moreover, unlike SAW participants, they were not noted for highly mobile behavior in response to job opportunities.[26]

The hypothesis being explored here is that the relative position of less-skilled workers may be affected by the legalization of low-skilled illegal aliens. CGs would pick up general changes in local labor market demand conditions across cities that may confound the effect of IRCA.[27] Additionally, CGs would capture any scale effects for the additional paper work costs associated with IRCA sanctions. Although CGs are unlikely to be illegal, IRCA requires all employers to have INS I-9 forms on record for staff.[28] Thus, unless employers do not fill out I-9 forms for CGs, the new IRCA paperwork costs are expected to affect all education groups similarly. CGs do not constitute a control group as they, too, are expected to be affected by labor demand or supply shifts of less-educated groups. Our primary interest here, however, is whether the approximately 3 million persons legalized by the IRCA contributed to the widening wage gap between skilled and unskilled workers over the 1980s.[29]

Amnesty population. This would be similar to method used by Reimers (1998) to study the effects of immigration. The drawback to this method is that small sample sizes for some education and/or race groups severely limit the number of cities that could be used.

[26] SAW applicants moved from state to state depending on seasonal work (INS 1991 p.71).

[27] Although, Topel (1994) finds "there is no evidence that different regional evolutions of wages are demand-driven; the whole story is on the supply side."

[28] I-9 forms require proof of work authorization.

[29] See Katz and Murphy (1992).

If areas with higher amnesty populations experienced negative demand shocks, relative to low-PROP cities, then studying only less-educated workers could yield misleading results. So the third-level of differences better addresses the question of how IRCA changed the relative labor outcomes of less-educated workers compared to CGs.

To address potential additional labor market competition for blacks, I further interact the PROP terms with race-education indicators, allowing the effect of IRCA to vary across race (hereafter called Model 2). Thus, the post-IRCA changes of each race-education group relative to the post-IRCA changes of white CGs are estimated.

In addition to the main analysis, I explore CSL's hypothesis of increased competition as the legalized population acquires additional human capital. Using the 1993 and 1994 CPS March files, I re-estimate Models 1 and 2.[30] Finally, separate regressions are run for males and females.

1.3.3 Limitations of Data and Methodology

I do not control for sanctions effects. However, as mentioned earlier, FLH find that the effects of sanctions are small and offsetting. Furthermore, in the most recent literature on the effects of IRCA, there appears to be a consensus that fraudulent documents, combined with inadequate enforcement, render employer sanctions ineffective regarding its main intent—deterring the employment of illegal aliens.[31]

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[30] Besides allowing the investigation of later effects, this allows me to address a potential problem with 1991 and 1992 data. Recall that the March files provide data on the previous year's labor market outcomes. Since the period from July 1990 to March 1991 was an economic recession, (see Business Cycle Dating Committee of NBER at http://www.nber.org/cycles.html) the relative labor market outcomes of less-skilled workers could be negatively affected independent of the newly legalized. In contrast, March 1991 through March 2001 is recorded as a period of expansion. Thus for the later years of data, we can expect increasingly tight labor markets, which are not expected to negatively affect low-skilled workers compared to others.

[31] See Legomsky (1998) for a detailed discussion of employer sanctions. Passel (1998 p. 198) also discusses the lack of effectiveness of sanctions in achieving their deterrence aim.

While the metropolitan area fixed effects included in the regression control for the overall effect of immigration, they may not account for the impact of general immigration on the relative position of less-educated workers. As the deviation in the legal immigration trend is almost entirely due to the amnesty, there is a basis for not accounting for other immigration in an examination of the amnesty effects.[32]

This work uses only applications under the general amnesty program. Of the approximately 3 million IRCA amnesty applications, the SAW program had over 1.2 million, with over 1 million approved for temporary residency. Migration by SAW applicants from agricultural to non-agriculture jobs should imply greater negative effects than LAW applicants, most of who already worked and lived in cities. Arguably, any analysis of competitive effects should account for their impact. Their inclusion is problematic as SAWs may bias estimates towards more positive findings.[33] Prevailing labor market conditions may be important in the location decision for this group.

1.4 IRCA's Effect on Labor Market Gaps

To address the question of what effects the IRCA had on less-educated workers relative to college graduates, Tables 1.2 through 1.5 give the estimated coefficients from equation (1.1) for variables measuring IRCA's effect. Coefficients for less-educated groups show IRCA-induced changes relative to CG changes. The coefficient on the PROP-post-IRCA (AA) interaction term controls for general post-IRCA changes in higher PROP MSAs. Negative coefficients on the interaction between the post-IRCA indicator, the education group indicators (HDO and HG), and PROP would support the hypothesis that the effect of the increased labor supply of low-skilled workers dominated

[32] See again Chart 1.5.

[33] The more detailed version of this paper includes results accounting for SAW applications. Those results show less evidence of a negative impact as expected.

the increased demand due to employer sanctions. A positive coefficient would indicate that higher demand for less-educated native workers outweighed the increase in competition. In other words, it may reflect success in reducing employer demand for illegal workers or discrimination against Hispanics. Significant effects are discussed below. Long-term effects examined by using later post-IRCA data than the main analysis are also discussed. See Appendix A Table A1.1 for calculations of implied effects for selected outcomes in representative low-PROP, middle-PROP and high-PROP MSAs.

1.4.1 Wages

First, I explore the estimated effects with the log hourly wage as the dependent variable by asking the question, "Do the earnings of less-skilled workers exhibit a relative decline in areas with larger newly legalized populations?" Table 1.2 columns (1) and (2) show the estimated effects of IRCA on the log hourly wage gap between less-educated workers and CGs under Model 1 assumptions. Under Model 1, the effect is constant across race.

For both sexes, the post-IRCA effect of higher legalized populations on the relative wages of HGs is positive and significant at the 10% level. These estimates imply that if an additional person out of every 100 members of the city's population changed legal status, an increase of 0.91% in the average hourly wages of male HGs relative to male CGs is expected. For female high school graduates, the increase is about .85% per 1/100 increase in the population share that changes status. Practically, these estimates imply very small changes for two reasons. First, a 1/100 increase in the amnesty share is

quite large and therefore unlikely.[34] Second, an increase of 1% in the average wage of female HGs would only come to about 13 cents per hour for this sample.

Tables 1.2 columns (3) and (4) examine the hypothesis of CSL that acquisition of skills by the newly legalized population may exert competitive effects in years subsequent to their analysis[35]--the years immediately after IRCA passed. This analysis with data from later post-IRCA years shows a significant decline in the relative log hourly wage of male HDOs compared to male CGs in cities with higher legalized populations. In addition, the later data shows that higher legalization rates are associated with lower hourly wages in the MSA. This contrasts with the findings of the previous literature which uses earlier data.

1.4.2 Annual Income, Usual Weekly Hours, Annual Weeks, Probability of Non-Employment and Living in a Poor Family

As mentioned before, the hourly wage—while a good measure of the labor market position of a group—may not give a full picture of IRCA's impact. In Table 1.3, log annual income, usual hours worked per week, annual weeks worked, and the probability of non-employment are examined for both sexes. Again, IRCA's effect is restricted to be the same across race within education categories. The sample for these regressions uses all persons satisfying the race, ethnicity and residence restrictions and not just those with positive log hourly wages. IRCA did not significantly affect males, but it did significantly lower weekly hours worked for female HGs relative to female CGs.

Long-term effects in Table 1.4, based on the later post-IRCA years, reveal general declines for all less-educated males relative to CGs, with significant relative declines in

[34] Only 35 of 224 MSAs used in this analysis had amnesty population shares in excess of 0.01.
[35] CSL use data up to 1989. These years immediately follow the IRCA, but do not encompass the years in which most of amnesty recipients became permanent residents.

usual weekly hours and annual weeks worked of male HGs in the post-IRCA period in cities with higher legalized populations. For males, both HGs and HDOs see a significant increase relative to CGs in the probability of non-employment in cities with higher legalized populations after IRCA. In these later years, less-educated females do not experience any adverse effects on their relative position after IRCA.

Table 1.5 columns (1) and (2) show that less-educated members of both sexes are more likely than CGs to live in poor families, but the estimated effects are insignificant. In the later post-IRCA years, Table 1.5 columns (3) and (4) show that IRCA significantly increased the likelihood of living in a poor family for less-educated persons relative to CGs. Again, these data are a couple of years after most of the amnesty applicants had received permanent residency status. If CSL's hypothesis is true, the competitive effects should be stronger at this later stage.

It is of note that Tables 1.2 through 1.5 show significant negative effects of larger legalized population shares on CGs. Again, this implies that cities with relatively higher amnesty populations experienced general declines in their labor markets. These results suggest that aggregation in the previous literature where higher legalized population shares were associated with higher wages masked differing effects. Possibly, as demand for native-appearing HGs increased in cities with large illegal populations, substitution effects forced employers to reduce demand for CGs.

1.5 IRCA's Effect by Race

To address the question of whether the IRCA effect differed by race-education group, Tables 1.6 through 1.8 give the estimated coefficients from Model 2 which allows the IRCA's effect on the relative position of each race-education group relative to white

CGs to vary. Coefficients for all race-education groups give IRCA changes relative to CGs' changes. Significant effects of the IRCA on black-white labor market gaps for similarly educated workers are discussed along with notable findings on changes to the within-race return to education.[36]

1.5.1 Wages

Model 2 allows black education groups to be differently affected than whites. Table 1.6 shows that IRCA induced a narrowing of the wage gap between white HGs and white CGs. Columns (1) and (3) show no changes in the status of black males relative to white male CGs. So the earlier estimate indicating that IRCA led to a narrowing of the gap between HGs and CGs is driven by improvements in the position of white male HGs relative to white CGs. Similarly, while estimated effects for HDOs of both races are negative, it is only white male HDOs who see a significant decline in their position relative to white male CGs. In later years, IRCA significantly reduces the wage gap between female HGs and white female CGs. White female HDOs also saw their relative wages increase compared to white CGs.

In their investigation of possible discrimination against Hispanics due to IRCA, Bansak and Raphael (2001) use blacks and whites as comparison groups. Though they use earlier years of data for their analysis, they found post-IRCA declines in Hispanic wages relative to black wages, but no declines relative to white wages. Their sample is limited to southwestern states, which had high numbers of amnesty recipients. The post-

[36] Tests for significant changes in the within black return to education or in return to education across race are not shown.

IRCA changes in the black-white hourly wage gap (not shown) related to the size of the amnesty population offers a possible explanation for that result.[37]

1.5.2 Annual Income, Usual Weekly Hours, Annual Weeks, Probability of Non-Employment and Living in a Poor Family

Table 1.7, which again allows for IRCA's effect to vary by race, shows that the annual incomes of white male HDOs increase relative to white male CGs in cities with higher amnesty populations. Black male HDOs experience declines in their annual income when compared to white male CGs, but the change is insignificant. However, as the gap between white CGs and HDOs narrowed, while the gap between white CGs and black HDOs widened, the annual income gap between white HDOs and black HDOs increased significantly due to higher legalized populations, post-IRCA. The decline is significant at the 1% level. An increase in the amnesty applicant population share of a city of 1/1000 reduced average black male HDOs' annual income, relative to average white HDOs, by about $1077.[38]

While there are significant positive effects of IRCA on black male CGs' position relative to white male CGs in weekly hours, annual weeks and the probability of employment, these do not translate into significant declines in black-white log annual income gaps. Possibly, black male CGs and white male CGs are equally qualified, but blacks have lower reservation wages in cities with high amnesty populations. If there were general labor market downturns in cities with higher amnesty populations after 1987, cost-conscious employers may have substituted more expensive white CGs with

[37] Model 2 with black-PROP interaction, but no education-PROP interactions show an insignificant narrowing of the black-white wage gap in cities with higher legalized populations.

[38] I estimated wage effects based on an increase in the amnesty population share of 1/100 as the concern about immigration is about high levels of immigration. An increase of 1/1000 may be more germane as out of 224 MSAs in the sample on 35 had a PROP value greater than 0.01.

black CGs. Another possibility is that the IRCA increased demand for natives at lower levels of the occupation structure, causing increases in relative wages of HGs. This may have had an indirect effect on firm demand for high-skilled workers. Unexpectedly, black male CGs are the only group significantly more likely than white male CGs to live in poor families, post-IRCA, in cities with higher legalized populations. Possibly, the distribution of the gains to blacks was highly skewed.

Table 1.7 shows that, for the female sample, the significant decline in relative hours worked by HGs is driven by a decline in weekly hours of white HGs relative to white CGs in higher PROP cities. Black CGs show relative as well as absolute improvements in annual income and weeks worked in cities with higher legalized populations when compared to white CGs in such cities. These improvements led to widening annual income and weeks worked gaps in cities with higher legalized populations between black CGs and black HGs. Less-educated black females were significantly less likely to live in poor families before 1987 than their white counterparts. The only group that became significantly more likely than white CGs to live in poor families in cities with higher legalized populations is black female HGs.

Despite generally negative effects on annual income, IRCA lowered the likelihood of living in poor families for less-educated workers. As the poverty index is based on total family income and not just labor income, the results may reflect differences in the importance of non-labor income.

From Table 1.8, using later post-IRCA years, all significant results indicate a negative impact on labor market outcomes of less-skilled males relative to male CGs due to IRCA. These changes do not lead to significant declines in log annual income for less-

educated workers when compared to CGs in cities with higher amnestied populations. For females, the gap between the annual incomes of black HGs and white CGs narrowed in cities with higher legalized populations.

1.6 Conclusion

The 1986 Immigration Reform and Control Act was the culmination of years of efforts to address the problem of illegal immigration. It was a two-pronged approach aimed at reducing both the incentive to migrate for potential immigrants and figuratively "wiping the slate clean" for persons already entrenched in the U.S. This study examines a hitherto unasked question of how less-educated workers—those most similar in human capital characteristics to the amnesty recipients (mainly high school dropouts)—fared after IRCA relative to more highly-educated workers in cities with higher legalized populations. It also investigates whether blacks are differently affected given the greater similarity of their occupational distribution to that of Hispanics, who constitute the majority of amnesty applicants.

In sum, I find positive post-IRCA effects of higher legalized populations on wages of high school graduates relative to college graduates of both sexes during the years when the greatest number of LAW applicants gained permanent residence. When the IRCA's effect is allowed to vary across race, white male and black female high school graduates were the only less-educated group that had significant wage gains on college graduates due to the amnesty in later post-IRCA years. Black female high school graduates narrowed their annual income gap with white female college graduates (though white female high school graduates did not).

Black male high school dropouts' annual incomes were negatively affected relative to white high school dropouts in the years that most amnesty recipients obtained permanent resident status. In later years, there are significant negative effects on the hourly wages of male high school dropouts and a general worsening of outcomes for black male high school graduates relative to white male high school graduates. This did not translate into lower annual income effects though. Negative post-IRCA effects of higher legalized populations on the relative position of less-educated blacks compared to black college graduates were also found. Thus, concern about the impact of further amnesties on low-skilled blacks is warranted.

There are positive IRCA effects of higher legalized populations on relative labor market outcomes of high school graduates, who comprise a large part of the labor force. These findings suggest that—when legalizing undocumented persons through amnesty—there are deleterious effects on black males and male high school dropouts generally. In the absence of a credible effort to enforce employer sanctions, further amnesties may increase competition for existing authorized workers without any of the benefits, such as increased demand for low-skilled workers, that came with the 1986 IRCA.

Figure 1.1 IRCA's Impact on the Unauthorized Labor Market

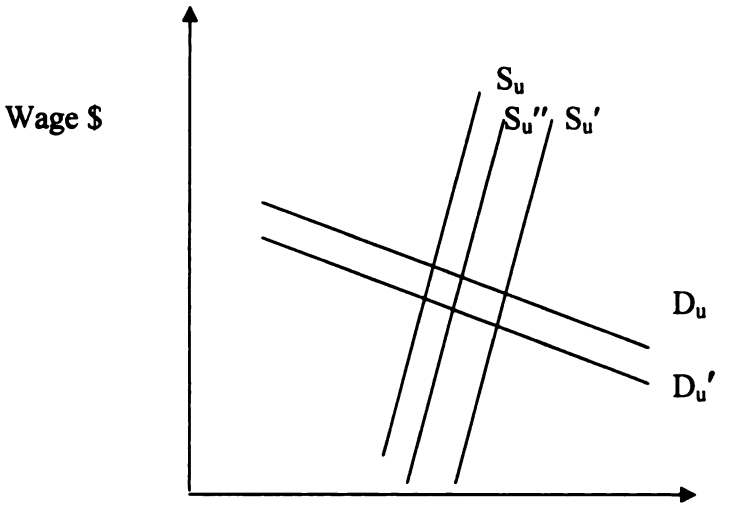


Figure 1.2 IRCA's Impact on the Authorized Labor Market

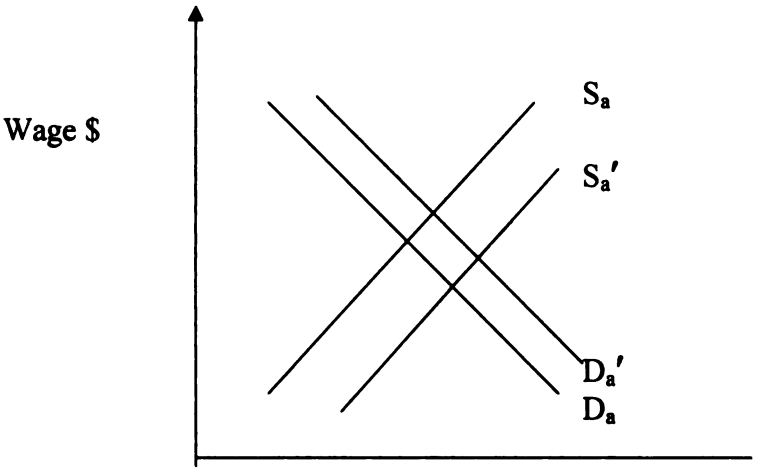


Figure 1.3 Distribution for Sample of Men in 1991 and 1992 CPS

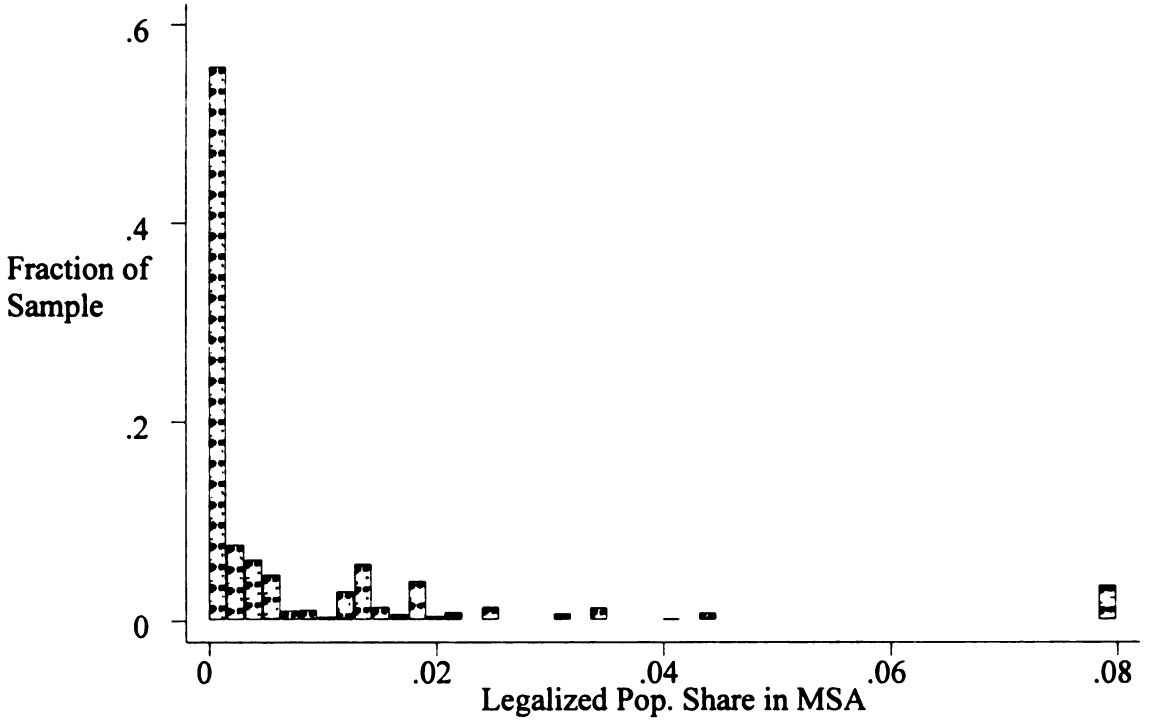
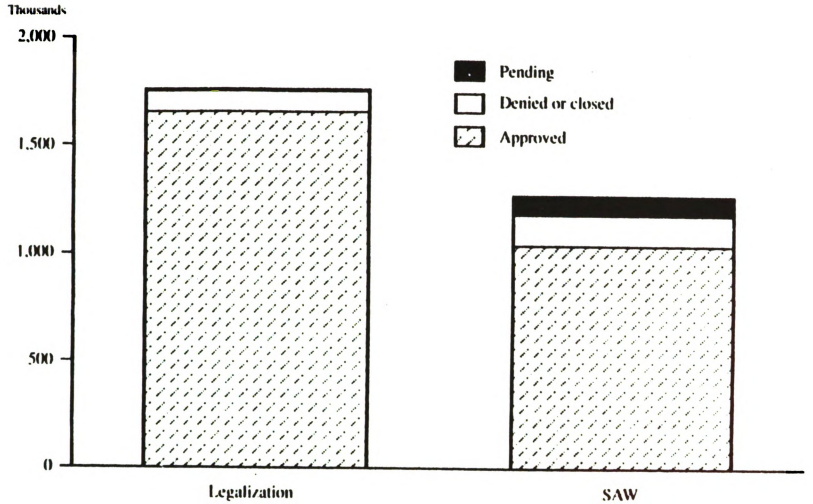
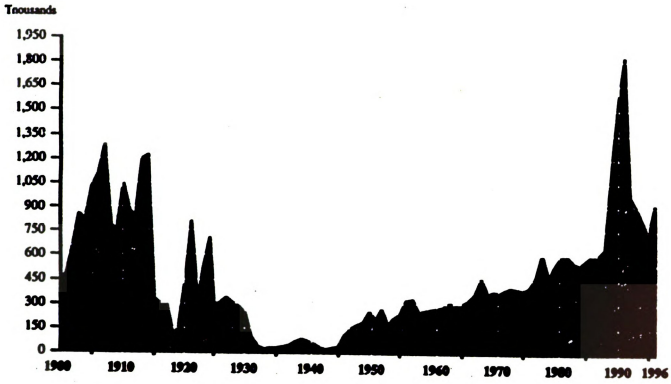


Chart 1.4 Status of LAW and SAW Amnesty Program Applicants.



Source: 1991 INS Statistical Yearbook

Chart 1.5 Spike over 1989 through 1993 Reflecting Increased Legal Immigration due to IRCA.



Source: 1996 INS Statistical yearbook.

Table 1.1a. PROP and Dependent Variable Means by Time Period-Education-PROP Group

	Male Sample							
Education-PROP Group	Hourly Wages[1] ($)	Annual Income ($)	Weekly Hours	Annual Weeks	Proportion not Employed	Proportion living in a Poor Family	PROP	Sample Size
Pre-1987 Low-PROP								
HDO	14.16	17,960	29.9	32.2	0.24	0.18	0.0002	3,338
HG	17.49	30,264	37.6	41.5	0.08	0.06	0.0002	10,805
CG	26.01	52,178	42.3	47.0	0.04	0.03	0.0002	3,879
Post-1987 Low-PROP								
HDO	12.84	15,390	29.1	31.7	0.26	0.19	0.0002	2,621
HG	16.66	29,426	38.4	43.2	0.08	0.06	0.0002	10,954
CG	25.01	49,711	42.9	47.0	0.05	0.03	0.0002	3,903
Pre-1987 High PROP								
HDO	14.98	19,101	30.8	33.2	0.22	0.14	0.0109	4,068
HG	18.74	31,825	37.2	41.6	0.08	0.05	0.0124	17,449
CG	28.45	55,647	41.8	46.4	0.04	0.02	0.0133	8,768
Post-1987 High PROP								
HDO	13.86	17,203	29.6	32.8	0.25	0.16	0.0115	3,265
HG	17.86	30,192	38.1	42.2	0.10	0.06	0.0119	16,861
CG	27.88	53,754	42.8	47.0	0.05	0.02	0.0128	8,656

[1] This is a smaller sample of those with hourly wages over $1. Dollars are adjusted to 2003 by the CPI-U.

Table 1.1b. PROP and Dependent Variable Means by Time Period-Education-PROP Group

	Female Sample							
Education-PROP Group	Hourly Wages[1] ($)	Annual Income ($)	Weekly Hours	Annual Weeks	Proportion not Employed	Proportion living in a Poor Family	PROP	Sample Size
Pre-1987 Low-PROP								
HDO	8.83	5,661	16.3	19.0	0.51	0.32	0.0002	3,362
HG	11.67	13,180	25.8	32.0	0.26	0.11	0.0002	13,252
CG	16.88	24,062	31.5	38.5	0.15	0.03	0.0002	2,995
Post-1987 Low-PROP								
HDO	9.11	5,881	16.6	19.5	0.52	0.31	0.0002	2,616
HG	11.84	14,575	27.6	34.3	0.23	0.1	0.0002	12,977
CG	18.26	28,738	33.6	40.7	0.13	0.03	0.0002	3,446
Pre-1987 High PROP								
HDO	9.80	6,861	17.8	20.6	0.48	0.27	0.0117	4,208
HG	13.06	15,301	26.6	32.5	0.25	0.09	0.0127	21,512
CG	19.57	28,469	32.1	39.1	0.15	0.03	0.0127	6,949
Post-1987 High PROP								
HDO	9.62	7,107	17.7	21.1	0.49	0.31	0.0111	3,540
HG	13.20	16,184	27.6	34.1	0.24	0.09	0.0116	19,928
CG	20.69	30,747	33.2	39.6	0.15	0.03	0.0124	7,549

[1] This is a smaller sample of those with hourly wages over $1. Dollars are adjusted to 2003 by the CPI-U.

Table 1.2. IRCA's Effect on Log Hourly Wages: Coefficients Estimates from Equation (1.1).

	(1) Male	(2) Female	(3) Male	(4) Female
CG	-0.50	-0.78	-0.78	-1.43
	(0.46)	(0.56)	(0.42)*	(0.49)***
Gap between CG and HDO	0.04	0.78	-2.37	1.43
	(0.72)	(1.38)	(1.15)**	(1.30)
Gap between CG and HG	0.91	0.85	0.65	1.22
	(0.41)**	(0.50)*	(0.45)	(0.47)***
Observations	78,424	72,323	75,955	70,768
R-squared	0.36	0.23	0.36	0.23

Standard errors robust to differences across MSAs in the pre- and post-1987 periods are in parentheses.
Columns (1) and (2) use data from 1990 and 1991, while (3) and (4) use from 1992 and 1993.
*Significant at the 10% level; ** 5%; ***1%.

Table 1.3. IRCA Effect on Outcomes: Coefficients Estimates from Equation (1.1).

	Male Sample			
	Log Annual Income	Usual Weekly Hours	Annual weeks	Probability of non-employment
CG	-14.13	-14.18	-22.21	0.06
	(3.72)***	(7.76)*	(6.62)***	(0.12)
Gap between CG and HDO	14.5	21	22.62	0.59
	(10.34)	(20.78)	(25.70)	(0.45)
Gap between CG and HG	5.66	-7.92	-11.09	0.23
	(4.00)	(10.04)	(8.16)	(0.16)
Observations	94,567	94,567	94,567	94,567
R-squared	0.1	0.17	0.18	0.13
	Female Sample			
	Log Annual Income	Usual Weekly Hours	Annual weeks	Probability of non-employment
CG	-10.98	-5.88	-32.91	0.38
	(4.36)**	(10.70)	(11.33)***	(0.20)*
Gap between CG and HDO	1.65	-27.41	23.69	0.23
	(14.75)	(27.25)	(28.42)	(0.66)
Gap between CG and HG	-3.37	-24.53	-11.61	0.31
	(5.40)	(11.94)**	(12.89)	(0.25)
Observations	102,334	102,334	102,334	102,334
R-squared	0.12	0.13	0.12	0.12

Standard errors robust to differences across MSAs in the pre- and post-1987 periods are in parentheses.
*Significant at the 10% level; ** 5%; ***1%.

Table 1.4. IRCA Effect on Outcomes: Coefficients Estimates from Equation (1.1) Using Later Post-IRCA data.

	Male Sample			
	Log Annual Income	Usual weekly hours	Annual weeks	Probability of non-employment
CG	-15.01	-24.74	-39.9	0.32
	(6.07)**	(7.63)***	(7.17)***	(0.17)*
Gap between CG and HDO	-16.55	-29.73	-5.69	1.13
	(12.78)	(21.10)	(21.19)	(0.48)**
Gap between CG and HG	-6.71	-24.54	-39.88	0.65
	(5.77)	(9.75)**	(8.44)***	(0.22)***
Observations	92,415	92,415	92,415	92,415
R-squared	0.1	0.17	0.18	0.13
	Female Sample			
	Log Annual Income	Usual weekly hours	Annual weeks	Probability of non-employment
CG	-17.55	-18.71	-31.41	0.43
	(6.56)***	(11.18)*	(13.47)**	(0.23)*
Gap between CG and HDO	-5.7	-25.31	-18.31	0.18
	(15.85)	(28.19)	(38.40)	(0.68)
Gap between CG and HG	3.6	-7.87	-8.71	0.17
	(6.28)	(11.66)	(13.99)	(0.26)
Observations	100,128	100,128	100,128	100,128
R-squared	0.12	0.13	0.12	0.11

Standard errors robust to differences across MSAs in the pre- and post-1987 periods are in parentheses.
*Significant at the 10% level; ** 5%; ***1%.

Table 1.5. IRCA's Effect on Probability of Living in a Poor Family: Estimates from Equation (1.1).

	(1) Male	(2) Female	(3) Male	(4) Female
CG	0.29	0.23	0.26	-0.01
	(0.10)***	(0.12)**	(0.08)***	(0.11)
Gap between CG and HDO	0.25	0.74	0.94	1.33
	(0.41)	(0.62)	(0.57)*	(0.48)***
Gap between CG and HG	0.06	0.12	0.32	0.38
	(0.15)	(0.17)	(0.13)**	(0.13)***
Observations	94,567	102,334	92,415	100,128
R-squared	0.07	0.17	0.07	0.18

Standard errors robust to differences across MSAs in the pre- and post-1987 periods are in parentheses.
Columns (1) and (2) use data from 1990 and 1991, while (3) and (4) use from 1992 and 1993.
*Significant at the 10% level; ** 5%; ***1%.

Table 1.6. IRCA's Effect on Log Hourly Wages by Race: Estimates from Model (2).

	(1)	(2)	(3)	(4)
	Impact on Whites			
	Male	Female	Male	Female
CG	-0.67	-0.61	-0.81	-1.64
	(0.48)	(0.61)	(0.45)*	(0.53)***
Gap between CG and HDO	0.21	1.15	-2.03	2.01
	(0.84)	(1.29)	(1.20)*	(1.13)*
Gap between CG and HG	1.07	0.52	0.85	1.36
	(0.41)***	(0.67)	(0.46)*	(0.57)**
	Impact on Blacks relative to White CGs			
	Male	Female	Male	Female
CG	3.38	-1.93	0.99	2.15
	(1.32)**	(1.26)	(1.30)	(1.26)*
HDO	0.08	-1.51	-4.1	-0.09
	(2.80)	(2.75)	(3.58)	(3.89)
HG	0.99	1.52	-0.37	1.95
	(1.07)	(1.06)	(1.16)	(1.12)*
Observations	78,424	72,323	75,955	70,768
R-squared	0.36	0.23	0.36	0.23

Standard errors robust to differences across MSAs in the pre- and post-1987 periods are in parentheses.
Columns (1) and (2) use data from 1990 and 1991, while (3) and (4) use from 1992 and 1993.
*Significant at the 10% level; ** 5%; ***1%.

Table 1.7. IRCA Effect on Outcomes by Race: Estimates from Model (2).

	Male Sample				
	Impact on Whites				
	Log Annual Income	Usual Weekly Hours	Annual weeks	Probability of non employment	Probability of living in a Poor Family
CG	-13.74	-19.65	-26.67	0.13	0.24
	(3.68)***	(8.46)**	(6.87)***	(0.14)	(0.10)**
Gap between CG and HDO	28.74	36.01	42.07	0.26	0.02
	(11.91)**	(25.69)	(28.69)	(0.56)	(0.41)
Gap between CG and HG	4.81	-0.01	-2.53	0.2	0.01
	(4.31)	(12.32)	(8.76)	(0.19)	(0.13)
	Impact on Blacks relative to White CGs				
CG	-8.25	100.42	79.06	-1.21	0.92
	(14.78)	(32.59)***	(27.55)***	(0.58)**	(0.50)*
HDO	-35.91	1.19	-12.54	1.25	1.02
	(22.96)	(52.82)	(62.14)	(1.13)	(1.37)
HG	7.59	-19.57	-33.85	-0.02	0.67
	(9.15)	(21.73)	(26.84)	(0.45)	(0.46)
Observations	94,567	94,567	94,567	94,567	94,567
R-squared	0.1	0.17	0.18	0.13	0.07

Standard errors robust to differences across MSAs in the pre- and post-1987 periods are in parentheses.
*Significant at the 10% level; ** 5%; ***1%.

Table 1.7 (contd). IRCA Effect on Outcomes by Race: Estimates from Model (2).

	Female Sample				
	Impact on Whites				
	Log Annual Income	Usual Weekly Hours	Annual weeks	Probability of non employment	Probability of living in a Poor Family
CG	-15.4	-8.46	-41.91	0.54	0.29
	(4.38)***	(12.47)	(11.52)***	(0.19)***	(0.11)**
Gap between CG and HDO	4.4	-30.81	38.65	0.19	0.15
	(12.22)	(24.97)	(24.58)	(0.58)	(0.38)
Gap between CG and HG	0.69	-23.33	-2.81	0.19	-0.09
	(5.26)	(12.82)*	(12.69)	(0.23)	(0.18)
	Impact on Blacks relative to White CGs				
CG	39.7	8.38	77.11	-1.36	0.11
	(14.38)***	(43.27)	(43.52)*	(0.53)**	(0.49)
HDO	13.57	35.18	-12.49	-0.77	1.9
	(28.43)	(48.19)	(70.03)	(1.43)	(1.60)
HG	2.2	8.69	0.61	-0.23	0.94
	(10.48)	(17.67)	(25.10)	(0.52)	(0.46)**
Observations	102,334	102,334	102,334	102,334	102,334
R-squared	0.12	0.14	0.12	0.12	0.18

Standard errors robust to differences across MSAs in the pre- and post-1987 periods are in parentheses.
*Significant at the 10% level; ** 5%; ***1%.

Table 1.8. IRCA Effect on Outcomes by Race: Estimates from Model (2) Using Later Post-IRCA Data

	Male Sample				
	Impact on Whites				
	Log Annual Income	Usual Weekly Hours	Annual weeks	Probability of non-employment	Probability of living in a Poor Family
CG	-16.17	-26.05	-41.88	0.37	0.24
	(5.94)***	(8.44)***	(7.26)***	(0.17)**	(0.10)**
Gap between CG and HDO	-13.19	-24.65	-3.57	1.2	1.36
	(18.00)	(27.44)	(22.60)	(0.55)**	(0.73)*
Gap between CG and HG	-5.84	-19.38	-33.81	0.56	0.34
	(5.73)	(10.71)*	(8.70)***	(0.22)**	(0.14)**
	Impact on Blacks relative to White CGs				
CG	16.04	24.26	30.65	-0.79	0.69
	(14.40)	(40.25)	(31.53)	(0.66)	(0.51)
HDO	-24	-42.72	-1.36	0.49	-0.71
	(30.36)	(58.84)	(72.92)	(1.59)	(1.63)
HG	-4.32	-46.61	-62.3	0.81	0.33
	(8.44)	(16.94)***	(20.93)***	(0.37)**	(0.32)
Observations	92,415	92,415	92,415	92,415	92,415
R-squared	0.11	0.17	0.18	0.13	0.07

Standard errors robust to differences across MSAs in the pre- and post-1987 periods are in parentheses.
*Significant at the 10% level; ** 5%; ***1%.

Table 1.8 (contd.). IRCA Effect on Outcomes by Race: Estimates from Model (2) Using Later Post-IRCA Data

	Female Sample				
	Impact on Whites				
	Log Annual Income	Usual Weekly Hours	Annual weeks	Probability of non-employment	Probability of living in a Poor Family
CG	-18.05	-18.99	-35.81	0.49	0.04
	(7.55)**	(12.92)	(16.21)**	(0.27)*	(0.11)
Gap between CG and HDO	-3.56	-21.52	0.21	-0.02	0.73
	(22.52)	(39.95)	(50.55)	(0.96)	(0.66)
Gap between CG and HG	1.13	-13.55	-10.23	0.26	0.31
	(6.56)	(12.71)	(15.33)	(0.25)	(0.11)***
	Impact on Blacks relative to White CGs				
CG	1.1	-9.39	37.5	-0.43	-0.04
	(18.84)	(44.91)	(51.02)	(0.88)	(0.40)
HDO	5.79	6.93	-33.8	0.04	1.77
	(32.28)	(63.77)	(77.67)	(1.54)	(1.69)
HG	17.67	36.19	36.26	-0.94	0.21
	(9.20)*	(17.85)**	(22.81)	(0.46)**	(0.43)
Observations	100,128	100,128	100,128	100,128	100,128
R-squared	0.12	0.13	0.12	0.11	0.18

Standard errors robust to differences across MSAs in the pre- and post-1987 periods are in parentheses.
*Significant at the 10% level; ** 5%; ***1%.

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Appendix A: Practical Estimates of IRCA's Impact.

Table A1.1 gives estimated changes in labor market gaps due to IRCA in 2003 dollars, usual weekly hours, annual weeks worked and employment likelihoods and probabilities of living in a poor family for three cities. Jackson, MS is representative of low-PROP cities. Baltimore has a PROP close to the median PROP for a city. Atlanta is near the 75th percentile city.

Table A1.1 IRCA's Estimated Impact for Selected MSA's and Outcomes.

MSA	Jackson, MS	Baltimore, MD	Atlanta, GA
Legalized Population Share	0.00020	0.00042	0.00241
1990 and 1991 Labor Makets			
Annual Income of Black Male HDOs Relative to White Male HDOs*	-$205.79	-$405.04	-$2,719.77
1992 and 1993 Labor Markets			
Hourly Wages of Male HDOs Relative to Male CGs	$0.00	-$0.01	-$0.07
Hourly Wages of Female HGs Relative to CGs	$0.002	$0.01	$0.04
Usual Weekly Hours Male HGs Relative to CGs	-11 minutes	-23 minutes	-2.3 hours
Annual Weeks Worked of Male HGs Relative to CGs	-2.2 days	-4.8 days	-4.2 weeks
Probability of Non-Employment of Male HGs Relative to CGs	0.00001	0.00002	0.00011
Probability of Living in a Poor Family for Female HDOs Relative to CGs	0.00017	0.00015	0.00069

* Adjusted to 2003 dollars by the CPI-U

Chapter 2

Criminal Justice Outcomes and Relative Labor Markets of Blacks

2.1 Introduction

Previous work has documented empirical differences in labor market outcomes across race; blacks on average perform less favorably than whites.[39] Also, arrest rates for blacks are high relative to those for whites.[40] Employers form beliefs about the reliability of prospective hires based on past experience with employees, especially in regards to the stereotypical views they have of workers along race and gender lines.[41] Of particular concern in this work is the perception of employers about the returns to transactions with blacks relative to whites.

Lundberg and Startz (2002) model how uncertainty about the returns to transactions can lead to and perpetuate discrimination against minorities. In particular, minority status signals greater uncertainty about returns even if the average return is the same for minorities and non-minorities. However, Lundberg and Startz note that if the expected return from trade with a minority is lower, the disparity may not be discriminatory if average wage equals average productivity across groups. Their

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[39] See, for example, Moss and Tilly (1991), Neumark (1999), Mason (2000), Chandra (2000), and Heckman, Lyons and Todd (2000) for discussions of the black-white wage gap changes over the later part of the twentieth century.

[40] This statement is based on examination of national data on incarceration by race in various years of the Statistical Abstract of the United States over the period 1978 to 1992. The author's own calculations of total arrested persons divided by population according to race at the city and metropolitan area level using data from the Uniform Crime Reporting Program and the National Incident Based Reporting System support the conclusion that the number of black arrests relative to their population in an area is generally, though not always, higher than that of whites.

[41] The Multi-City Study of Urban Inequality (MCSUI) surveys employers as to their perceptions of employees by race, ethnicity and gender. For a discussion of the findings see Moss and Tilly (2001).

statistical discrimination model postulates that black stereotypes may lead to lower weighting of individual productivity indicators such as education or experience. Statistical discrimination models generate bias against (favoritism towards) a particular group(s) because of a feature of the group(s) other than race. This differs from taste discrimination which implies a dislike for (nepotism towards) the group without apparent reason.

The Multi-City Study of Urban Inequality (MCSUI) finds negative perceptions among employers of black males hired for low-skilled jobs.[42] The concern of this paper is that factors exogenous to individuals may affect the general perception of black males. In particular, if employers have observed higher absenteeism among blacks in general, employers may believe that black candidates are more likely to miss work compared to whites.[43] Corcoran and Duncan (1979) include absenteeism as one of a host of productivity factors that may affect labor market gaps by race. They note that white males have lower absenteeism rates than black males.

This study focuses on the effect of one factor exogenous to the individual that may affect absenteeism by race – relative arrest rates by race. Absenteeism and arrests are directly related. Hindelag et al. (1981) note that an arrest implies not just immediate detention (perhaps overnight or more), but may also require future court appearances and thus is expected to infringe on time on the job. They also report that arrestees may not

[42] See again Moss and Tilly (2001).

[43] The literature on absenteeism has been used as an explanation for gender wage gaps in labor market outcomes (Gronau 1988; Ruhm 1998). These works hypothesize that children factor into the career path of women not only in terms of planned pregnancies, which legally entitles them to paid-leaves of some duration, but also in terms of more frequent spontaneous absences. Vistnes (1997) finds that the presence of young children increases the probability of female missing work. She also finds that among females missing work the presence of young children increased the number of absences, while among men who missed work it increased the days lost from work. She notes the men were likely to be single or married with a working wife.

truthfully explain arrest-related absences to their employers. Grogger (1995) suggests that employers may fire workers who report their arrests due to the costs associated with employee absenteeism.

As blacks have higher arrest rates than whites, *ceteris paribus*, they will have higher unexplained absences. Moreover, comparison of survey data on a sample of arrestees (Hindelag et al., 1981) with actual arrest histories suggests that, when asked, blacks are less likely than whites to admit past criminal involvement.[44] If blacks are more likely to underreport arrests to survey workers, blacks may be even less likely to report arrests to employers. Thus, in the absence of information on relative arrest probabilities and arrests' subsequent effect on work attendance, employers may be more likely to associate black absenteeism with a poor work ethic rather than an exogenous factor such as an arrest. This, in turn, may reduce future black hires.[45] In short, all else equal, employers may statistically discriminate against blacks to avoid employees who do not show up for work.[46]

Higher relative arrest rates for blacks may reflect higher levels of participation in crime by blacks. Relatively greater participation in crime by blacks may reduce productivity on the job even absenteeism between blacks and whites were the same. Perhaps, blacks who are working in crime may exert less effort while on the legal job; motivation and tiredness are potential reasons for less effort. Previous literature has documented from microdata that past arrests of an individual implies relatively lower

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[44] Supportive of this Grogger (1998) discusses evidence that actual arrest data imply higher relative arrest rates for blacks when compared to whites but self-reports imply similar ones.

[45] Moss and Tilly (2001) note that in a survey soliciting impressions of workers by race, "blacks are less reliable" was a frequent response.

[46] While it must be recognized that many other factors may affect the perceptions of employers (including actual work ethic of each race), this work only asks whether the relative arrest rate is a contributing factor to black-white labor market gaps. The arrest rate is the hypothesized exogenous channel through which absenteeism can be affected, impacting the relative absenteeism rate and the wider black population.

future employment probabilities for the individual (Freeman 1992; Grogger 1992). It should be noted that Grogger (1995) finds only modest and short-lived effects of arrests on future outcomes after controlling for individual fixed effects.[47] This suggests that poor labor market outcomes of those with arrest histories are not due to actual arrests; rather that criminals with arrest histories are low-productivity workers to begin with.

Employers may practice statistical discrimination because they believe that blacks are more likely to have criminal backgrounds because of higher proportions of the black populations that are involved in crime. Holzer, Raphael and Stoll (2002) suggest that such stereotypical views may be responsible for relatively less hiring of blacks by small firms compared to large firms.[48] Further, support for employer dislike of criminals is found in Pager (2003), which uses audits to test whether a criminal background reduces employment prospects. She finds significant reductions in opportunities for both whites and blacks due to admission of criminal backgrounds.

The present study hypothesizes that higher arrest rates will lead to more absenteeism for blacks leading to lower productivity of blacks as a group, potentially causing statistical discrimination against individual blacks. This work tests whether there is a relationship between relatively worse labor market outcomes for blacks and their higher arrest rates. Specifically, it examines whether labor market outcomes for blacks are relatively worse in areas where blacks are more likely to be arrested. I include city-specific fixed effects to partially control for the possibility that cities where blacks face higher discrimination by the criminal justice system are also cities in which blacks face

[47] Findings by Kaestner (1994) support this. He finds that illicit drug use negatively affects labor supply of young adults when fixed effects are not controlled for. Once individual specific effects are included in the model, the effect of illicit drugs goes away.

[48] They find evidence consistent with statistical discrimination against blacks. However, they do not control for employer tastes and so are unable to rule out taste discrimination.

greater taste-based employer discrimination and other effects which may cause relative labor market outcomes and arrest rates to be spuriously related.

2.2 Theoretical Framework

Let a one-period economy consist of the government, N^A type A workers, N^B type B workers, and N identical employers with two jobs each (job 1 and job 2).[49] Assume $N< N^B < N^A$ so that there will be some unemployment and type B is a minority group. Let n^t_j represent the number of type t persons that are hired in job j.

Both types consist of criminals and non-criminals. Assume that type B has a higher proportion of criminals than type A. Assume that the government arrests types with a probability that is a monotonically increasing function of the rate at which types commit crimes. So type B non-criminals have a higher probability of arrest than a type A non-criminals due to statistical discrimination by the police, but type B non-criminals have a lower probability of arrest than a type B criminal.[50] Let i^t denote the average probability that the government arrests a type t worker. Thus, $i^t= \alpha^t i^{tC} + (1-\alpha^t)i^{tN}$ (where α^t denotes the proportion of type t persons who commit crimes, i^{tC} denotes the probability that a criminal of type t is arrested and i^{tN} denotes the probability that a non-criminal of type t is arrested).

Let p^t denote the average probability that a type t worker will be absent from the job in a given period. Let *a* denote the exogenous probability of absenteeism by all workers regardless of type. Given the above assumptions, $p^t = a + i^t$. Since a greater number of type B workers commit crimes, there will be relatively more arrests of type B

[49] Type and race are used interchangeably.
[50] Knowles, Persico and Todd (1999) examine whether police discriminated against blacks in motor vehicle searches for drugs. They test whether this is due to racial prejudice or statistical discrimination and cannot reject the statistical discrimination model.

workers.[51] Thus, $p^B > p^A$. Let MRP^t_j denote the marginal revenue product generated by a type t worker in job j, w^t_j denote the wage paid to type t in job j and C_j denote the extra cost an employer incurs when a worker does not show up for the job j. If workers do not show up for the job, assuming an optimal initial allocation of inputs by the firm, a more costly production method is utilized so that output remains unchanged.[52] Thus $C \geq 0$ is assumed. There are no other costs of production.[53] Employers are unable to observe whether a given individual is a criminal or not and employees who are arrested do not inform employers of the arrest. Given the evidence that a significant number of employers would rather not hire persons with a criminal background (Holzer, Raphael and Stoll 2002), this is a reasonable assumption. Employers will, however, form estimates over time of the average rates of absenteeism for each race. Based on the expectations of absenteeism by types, the optimal allocation of workers within jobs, therefore, must satisfy the following:

$$\frac{MRP^B_j}{w^B_j + p^B C_j} = \frac{MRP^A_j}{w^A_j + p^A C_j} \qquad (2.1)$$

Based on the higher average probability of arrest in the black population, and the necessary ensuing absenteeism, employers overestimate the probability of absenteeism for non-criminal blacks and of course for blacks who are never arrested. They therefore incorrectly underestimate the return to hire or job placement of non-criminal and never arrested blacks. Equation (2.1) implies:

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[51] The model ignores other links between criminality and productivity besides absenteeism.

[52] For example, if a fast food restaurant worker does not show up for their particular job at lunch time the manager may be forced to help out or staff the position in such a way that other workers will work overtime. Assuming diminishing marginal productivity either a lower quality service or quantity work will be produced.

[53] This is a simplification under the assumption that other costs are not impacted by the race assignments of employees to these jobs.

(i) Since $p^B > p^A$, on average, if workers are paid the same wage regardless of type for job j, then an employer will always prefer to hire a type A worker provided workers are equally productive when they are on the job;

(ii) assuming equal productivity on the job and equal wages across types for the same job, if the government requires hire of both types, employers will always prefer to allocate type B employees to jobs with lower C_j;[54]

(iii) if we assume $C_2 > C_1$ and that the government requires hire of both types in both jobs, then $n^A_2 > n^B_2$ or $(n^A_2 / N^A) / (n^B_2 / N^B) > 1$ (where n^t_j is the number of type t workers in job j).[55] Thus, a larger share of population of type A relative to type B get hired for the job where absenteeism is more costly to employers;

(iv) if types are equally productive on the job and employers can pay different wages within the same job category then $w^B_j < w^A_j$.

While there are productivity differences on average by criminal and non-criminal type, type B non-criminals will pay an unfair share of the costs of absenteeism in their type. They are paid lower than their productivity warrants and will be disproportionately denied better (higher C_j) jobs. Non-criminals of type B who ultimately never get arrested pay the greatest share of the cost on average, even though they are more productive than type A criminals and type A non-criminals who get arrested.

This project looks at the effect of relative arrest rates on relative labor market outcomes for blacks compared to whites in cities across the U.S. In the data to be used here, I cannot test whether a black who never gets arrested is paid less than an observably

[54] See Bulows and Summers (1986) for another explanation of placement of blacks into "bad" jobs in dual labor markets.

[55] See Holzer and Neumark (2000) for a survey of the literature on Affirmative Action, a policy designed to increase the employment opportunities of minorities and women.

similar white who never gets arrested. I test the more limited hypothesis – which the above model implies should be observable in the data – that groups of workers with higher arrest rates will be, on average, less productive and will thus have worse labor market outcomes on average.

2.3 Data

Data on the number of blacks and whites arrested in a city are obtained from the Uniform Crime Reporting Program (UCR): Arrests by Age, Sex, and Race for Police Agencies in Metropolitan Statistical Areas for 1979, 1989 and 1999.[56] The data contains arrests by race for each reporting city. Police agencies report their total arrests by age, sex, race and ethnicity on a voluntary basis. The sample is restricted to police agencies that report arrests by race. The geographic unit used for this analysis is the city, so only reporting agencies located in the core city of a metropolitan statistical area are included.

The paper uses the relative arrest rates of blacks to whites in a city to proxy for relative absenteeism caused by arrests.[57] Arrest rates for blacks are calculated by dividing the number of black arrests by the black population in each city.[58] The same variable is calculated for whites. The relative arrest rate is the ratio of per capita black arrests to per capita white arrests.

Labor market data on annual wage and salary earnings, usual weekly hours, employment and weeks worked are obtained from the 1980, 1990 and 2000 Public Use

[56] If available, for cities not reporting in the stated years, but which reported in the previous year, I calculate the relative arrest rate for the previous year and substitute it for the current year's value. As will be discussed later on in the paper, relative arrest rates are highly serially correlated.

[57] Again, though this is not the only factor affecting absenteeism, (Markam and McKee 1991; Wilson and Peel 1991) nor is absenteeism the only reason for correlations between arrests and productivity, it is the only channel explored in this paper.

[58] Population estimates by race for cities are obtained from the 1980 Census of the Population: Characteristics of the Population Series, Decennial Lookup (1990 estimates) tool and American Factfinder (2000) tool on the U.S. Census Bureau's web page (www.census.gov). These figures are used to calculate population growth rates on which I base estimates of populations in the year prior to each Census.

Microdata Sample (PUMS) 5% State Samples.[59] This is a survey of 5% of the U.S. Population taken at the time of the decennial censuses. The PUMS provide information on the earnings and labor force status in the year prior to the Census along with current demographic information and city of residence. This information is used to construct average labor market outcomes by race in cities that are identified in the UCR data.

I restrict the sample to males reporting either black or white race, no college degree and aged 16 to 45 years inclusive.[60] The sample also excludes those in the military, institutional group quarters and those working for government agencies. City level averages of black and white characteristics – age, education, marital status indicators, income, employment likelihoods, earnings– are obtained by using weighted averages of variables across samples of blacks and whites living in identifiable cities.[61,62] Only cities with at least 30 persons of each race in the PUMS data in each year are included.[63] Thus, each city contains a sample of at least 30 white males and 30 black males. These men all report having fewer than four years of college and have jobs outside the military or government. Note: the UCR data contains actual arrests in a city. Though figures are likely to be underreported due to omission of certain types of crimes or less than 12 months of reporting, it is not expected to bias the relative arrests. Arrest figures are used even if there are very few arrests reported of men of either race as the

[59] These data sets are freely available at www.ipums.org.
[60] Based on author calculations in the UCR data for the years and cities used here the 16-44 year old share of the arrestees range from a minimum of .53 to a maximum of .93 with the average being 0.83. With the exception of 2 city-year observations the share exceeds 70%. A significant part of the denominator for this calculation includes under-16 juvenile arrestees who are considered outside of the labor market not people over 45 years old.
[61] Averages are weighted by the population frequency weights in the census data.
[62] Grouped years of education recodes are used so this is not interpretable in the usual terms of years of education. However, higher values indicate more years of education.
[63] See Appendix B Table A2.2 for a list of cities used and their relative arrest rates by year.

data are not a random sample of arrestees from which we are calculating proportions. It is important to recall that the figures are total reported arrestees by race.

In order to investigate whether employers assign jobs to new hires differently depending on a perception of absenteeism, some proxy for the cost of absenteeism in various occupations is needed. I use an updated version of the Duncan Socio-Economic Index for 1980 and 1990 census occupation codes for this purpose.[64] The ranking of costs is determined by matching occupation codes to the index of socio-economic occupation status for 13 occupation categories.[65] While occupation prestige rank is not a perfect measure of the cost of absenteeism on the job, it does provide some measure of the relative value society places on particular occupations. While jobs with higher annual incomes are usually those with greater prestige (and here a proxy for higher C), it is important to note they do not correlate perfectly.[66] Further, Stoll, Raphael and Holzer (2001) and Giuliano (2003) find that race of the employer/hiring manager is an important determinant of the employment likelihoods of blacks. This adds further motivation for examining occupational placement outcomes in the context of the impact of discrimination. In formulating policies to address gaps in labor market outcomes by race, investigating factors which determine who obtains positions with hiring authority may be useful. This is the case if indeed having more minority managers/employers increases the probability of employment of minority workers.

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[64] See Hauser and Warren (1996).

[65] See Appendix A Table A2.1 for index and occupation categories. While a detailed occupational index is available it is not used for two reasons. First, the 2000 PUMS codes are more detailed than those of 1990 and 1980 and would require omission of many occupations that could not be identically matched with their earlier code. Second, up to a point, coarser index information sets may be a better proxy for the absenteeism costs than the detailed occupation figures.

[66] Saxenian (2001) states there is little evidence of earnings discrimination against Chinese and Indians in managerial and professional occupations. It also discusses the initial concentration of these groups into professional rather than managerial positions based on investor preferences for non-Asian managers.

An additional control variable employed is the relative institutionalization rate, which is calculated from the 1980, 1990 and 2000 PUMS.[67] I limit the sample used for this calculation to 16-45 year old males with less than a college degree to match the general arrestee population.[68] The institutionalization rate for each race is the weighted population in institutional group quarters divided by the total estimated state population of that race. While the intent is to control for relative depletion of the low-skilled or low-productivity employees from a city, it is not possible to ascertain the origin city of inmates/institutionalized persons.[69] Thus, the state level variable is a second best measure of the depletion of the low-skilled population. Omission of this variable may bias upwards the estimated effect of relative arrests since relative incarceration rates are positively correlated with relative arrests. For example, suppose the distribution of abilities is equal across races and cities. If high school dropouts have low levels of employment and black high school dropouts are incarcerated at higher rates than white dropouts in a particular city, ceteris paribus, the city will have relatively higher employment rates for blacks than whites. This might mistakenly be interpreted as a city with a higher distribution of productive blacks relative to whites.

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[67] 1990 and 2000 PUMS files do not report group quarters, the variable indicating residence in a correctional institution, with the detail available in 1980. Thus, persons in mental institutions, old age homes, homes for the handicapped, hospitals, homes for the mentally handicapped or any other non-institutional group housing are also included in this variable. Arguably, it still holds value in capturing the depletion by race of low productivity persons from the labor force. In the 1980 PUMS, for men aged 16-45 years, 65% of those institutionalized were in correctional institutions, 13% were in mental institutions, 5% were in homes for the elderly and 18% were in other institutions (such as mentally handicapped homes). These totals may include institutionalized persons as well as resident employees.

[68] I assume that those with 4 or more years of college are college graduates.

[69] Butler and Heckman (1977) discuss the importance in accounting for the withdrawal of low-skilled workers from the labor force in the context of evaluating Equal Employment Opportunity Act's (EEO) effectiveness. They discuss the possible overestimation of the EEO's effect in narrowing black white labor market gaps. They argue that concurrent increases in welfare generosity induced selective labor force withdrawal from low productivity blacks.

The institutionalization rate also captures the actual ratio of criminal participation by race.[70] In addition, the period of absence from the job for an arrestee should be short-lived. If, however, arrests lead to incarceration, the cost to an employer is higher than the cost of an arrest because recruiting a new employee is necessary in addition to the usual cost of absenteeism. Then employers would consider both the relative incarceration rates and the relative arrest rates before hiring.

Data on institutionalized persons aged 16 to 45 years indicates that the population consists mainly of persons with 12 years of education or less.[71] Unlike the available data on the prison population, the arrests data do not contain education, so I am unable to estimate arrest rates of those without a college degree. However, I assume that the probability of arrest for college graduates is low enough so as not to be an important consideration to prospective employers.[72] Another undesirable inclusion in the arrest data that must be noted is the female arrestees. The proportion of women arrested varies from city to city. The UCR data from the cities and years used here shows that between 10% and 34% of total reported arrestees in a city are female.[73] Unfortunately, I cannot accurately remove females from the total black arrests or the total white arrests. However, using data from the National Incident Based Reporting System (NIBRS) available for a few cities in this data, I compare the difference between the ideal variable, relative arrest rates for 16-45 year old black males relative to white males, and the one

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[70] Although, if blacks are sentenced at higher rates than whites (and for longer periods) this may overestimate the criminal participation rates of blacks relative to whites. There should nevertheless be a significant positive correlation between true localized criminality and incarceration rates.

[71] Based on author calculations with the 1980, 1990 and 2000 PUMS, approximately 3% of black and white males aged 16 to 45 years correctional institutions had 4 or more years of college. Approximately 43% of those in correctional attended 12th grade and/or had some incomplete college education.

[72] This is based on the work done for the previous footnote. Arrests lead to incarcerations. Given the low percentage of college graduates incarcerated, it seems fair to assume the number of arrestees is also minor.

[73] Figures calculated from UCR data from 1978, 1979, 1988, 1989, 1998, and 1999. This also shows that average female share of the arrestee population in the sample of cities and years used is roughly 19%.

which I use in this paper relative arrest rates for blacks versus whites. Appendix C contains a discussion of the findings of this comparison.

A summary of relative arrest rates and labor market outcomes by year are given in Table 2.1. It is worth repeating that the sample used to calculate labor market outcomes by city is restricted to those without a 4-year college degree. Thus, in the average city used here, the relative education level of blacks without 4 or more years of college education is higher than that of whites without 4 years or more of college. This is possibly caused by the inclusion of Hispanics in the white average as they constitute a disproportionately large portion of the high school dropout population.

2.4 Empirical Methodology

Using a linear regression model and city-year level observations, I estimate the effect of these relatively higher arrest rates for blacks on their relative labor market outcomes. As average relative characteristics of the black and white populations are also expected to affect relative labor market outcomes, these are used as additional controls in the linear regression model.

The following regression model is estimated:

$$RelOutcome_{jt} = \beta_0 + \beta_1 RelArrest_{-1,jt} + \beta_2 RelInstitution_{jt} + \beta_3 RelEd_{jt} + \beta_4 RelAge_{jt} + \beta_5 RelAgesq_{jt} + \beta_6 RelMar_{jt} + \beta_7 RelSing_{jt} + \beta_8 Y90_t + \beta_9 Y00_t + city_j + \varepsilon_{jt} \quad (2.2)$$

The prefix Rel indicates averages for blacks in city j in year t divided by averages for whites in city j in year t for the following variables: Outcome - annual wage and salary income, employment rate, hourly wage or occupational rank; $Arrest_{-1}$ – city arrest rate for blacks relative to whites in the calendar year prior to the Census; Institution – state institutionalized rate in census year; Ed – average Education Level; Age – average

age; Agesq – average of age-squared; Mar – proportion married; Sing – proportion single; Y90 – census year is 1990; Y00 – census year is 2000; city – city fixed effect; and ε - randomly distributed error term.

Equation (2.2) is estimated by pooled ordinary least squares (OLS) both without and with city-specific effects. The estimated effects of relative arrest rates have both positive and negative biases and so the direction of the bias cannot be ascertained. The city fixed effects control for time invariant factors that are correlated with relative arrest rates and relative outcomes and should account for much of this bias. For example, the level of taste-based discrimination in a city might be common to both police and employers. Thus, a city with relatively higher arrest rates because of taste-based discrimination within the criminal justice system may also be a place in which employers are less willing to hire blacks. As employers would learn about relative absenteeism of workers over time, past arrest rates may also be a factor. However, the correlation between relative arrest rates in the two years prior to each census year exceeds .90. Thus, the relative arrest rate in the year prior to the census is expected to capture information contained in the most recent arrest rates. Employers may be unaware of the cause of absenteeism differences and attribute it differences in work ethic.

2.4.1 Related Issues

Calculations of relative arrest rates include cities that report less than 12 months out of the year. Since both black and white arrest reports will be affected, unless there is seasonality in arrests that differ by race, I do not expect this to bias the estimate of the relative arrest rate by race. In the 2000 PUMS, it is apparent that Hispanics are reporting a race other than black or white in greater proportions than in either the 1980 or 1990

Censuses. This leads to an undercount of the white population in comparison to the measure used in prior years and an inflation of their relative arrest rates. This is problematic as arrest data includes a large number of Hispanics who have been reported as white.[74] Thus Hispanic-whites are overwhelmingly included in the arrest figure, but not as numerously in the 2000 Census population counts. An examination of the relative arrest rates by year reveals that with the overestimate of the white arrest rate in 2000, average relative arrest rates of blacks compared to whites decreased slightly. This trend holds if the arrest rates are calculated using black and non-black to make the relative calculations. So the movement is not an artifact of the change in race-reporting by Hispanics. Another complication is that Hispanics have relatively higher arrest rates than non-Hispanic whites, but also have relatively lower labor market achievement. Thus, exclusion of Hispanics would provide a better test of this paper's hypothesis than the current data and is an important avenue for future work.[75]

Importantly, though the model predicts a ceteris paribus negative relationship, it is not possible in a real world setting to obtain data where ability, perceptions and all other factors are the same for blacks as well as whites. In particular, the unobserved factor of future arrest is not observable in these data. As a second best, the estimation procedure controls for a variety of factors including city-specific fixed effects and then asks the question of whether or not the arrest rates matter to city-level averages of the relative labor market outcomes.

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[74] The National Incident Based Reporting System (NIBRS) contains person level data on arrestees, including race and Hispanic ethnicity. In the 1999 data, less than 2% of those reported as having Hispanic ethnicity also had race reported as black.

[75] NIBRS provides person level data on arrestees, but currently many of the largest U.S. cities do not report using this system. This data set may be useful for future work, to provide a cleaner test, when more cities are reporting.

The city fixed effects control for any features constant in all three census periods but correlated with arrest rates. However, a bias remains if changes in both relative arrest rates and relative outcomes are due to changes in underlying prejudices. Additionally, in cities where blacks face more discrimination when seeking legal employment, they may be more likely to turn to crime as a means of economic support over time.[76] In other words, relative arrest rates may be endogenous. Ideally, an instrumental variables procedure would be implemented, but this study is a first step to assess the importance of impact of arrest rates on general labor market outcomes of blacks relative to whites.

It is important to compare the results without the city-fixed effects to those which control for the fact that blacks may be worse (better) off in general in particular cities. If we find that relative arrest rates are related to lower labor market outcomes for blacks when compared to whites, it would be interesting to see if city-specific effects can account for all of such effects. It is also possible that, even without controls for city fixed effects, other factors not accounted for in this analysis are more important than relative arrest rates for determining relative outcomes. So examination of the basic relationship without controlling for city fixed effects is an important piece of the analysis.

2.5 Results

Table 2.2 gives the results of the OLS estimates of equation (2.2) without controls for city specific fixed effects. Relative arrest rates only affect the relative hourly wage for blacks compared to white. The effect of relative arrest rates on annual income and the occupation index is negative, but insignificant.

[76] See Ehrlich (1974) for early work on this avenue of thought.

Table 2.2 also shows that relative institutionalization rates are positively related to city averages of relative labor market outcomes. This supports the hypothesis that low productivity blacks are being removed from the relevant civilian labor force leaving behind a relatively more productive group of blacks. Another more causal interpretation is that institutionalizing relatively more blacks will improve the labor market for the remaining blacks if labor markets are segmented along racial lines. This would occur as the labor supply curve for black jobs shifts to the left to a greater degree than that for whites due to institutionalization.

Table 2.3 gives the results controlling for city-specific fixed effects. The effect of higher relative arrest rates on the relative labor market outcomes of blacks is no longer significant. This implies that cities in which blacks are more likely to be arrested are also those in which blacks have lower hourly wages, independent of the arrest rates. However, the occupation index is significantly negatively correlated with arrest rates even after controlling for city-specific effects.[77] This finding is significant as the impact of occupational placement is not limited to individuals who do not move up the occupation ladder. The findings of Stoll, Raphael and Holzer (2001) and Giuliano (2003) suggest that assignment of blacks into lower occupation groups may echo in the form of lowering hiring rates of black males. Secondary effects may operate with a lag. As mentioned before, due to the high correlation between the relative arrest rates over time, inclusion of current and lagged arrest rates to capture these effects is beyond the scope of this paper.

[77] While the negative effect is robust, the significance is affected by the measure of absenteeism cost used. In particular, use of a 6-category index rather than the 13-category index renders the effect insignificant.

2.6 Conclusion

This paper investigates the possibility that absenteeism as caused by relatively higher arrest rates impacts relative labor market outcomes of blacks. Effect of relative arrests on earnings, employment and occupational rank are examined. Hourly wages are found to be negatively impacted by higher relative arrest rates if one does not control for city-specific effects. However, the type of occupation assigned to blacks is significantly negatively related to arrest rates even after city fixed effects are controlled for. While this paper is unable to explore the repercussions of relatively higher arrest rates as it operates against the placement of black males in higher status occupations, it does suggest that further research needs to be done to explore this particular cause of black-white achievement gaps.

Table 2.1 Mean of Relative Independent and Dependent variables.

Year	1979	1989	1999
Relative Age	1.01	1.00	0.99
Relative Proportion Married	0.81	0.68	0.77
Relative Proportion Single	1.19	1.32	1.22
Relative Education Level	0.99	1.02	1.02
Relative Wage and Salary Income (Annual)	0.75	0.69	0.71
Relative Hourly Wage	1.02	0.95	0.96
Relative Employment Rate	0.87	0.85	0.86
Relative Socio-Economic Index (13 categories)	0.90	0.92	0.93
Relative Arrest Rates (Black over White)	2.31	3.33	2.93
Relative Arrest Rates (Black over Non-Black)	2.49	3.73	3.62
Relative Incarceration Rate (State Level)	4.20	5.55	5.98
N	77	67	74

Table 2.2 OLS Estimates of the Effect of Relative Arrest Rates on Relative Labor Market Outcomes of Blacks Compared With Whites

	Annual Wage and Salary Income	Probability of Employment in the Previous Year	Hourly Wage	Occupation Index
Relative Arrest Rate	-0.005	0.005	-0.02	-0.003
	(0.006)	(0.004)	(0.012)*	(0.002)
Relative Institutionalized Rate	0.013	0.001	0.02	0.011
	(0.005)**	(0.003)	(0.011)*	(0.002)***
Relative Education	0.779	0.223	0.343	0.511
	(0.096)***	(0.059)***	(0.194)*	(0.041)***
Relative Age	3.525	3.309	3.669	0.47
	(2.190)	(1.340)**	(4.440)	(0.932)
Relative Age Squared	-1.334	-1.594	-1.932	-0.175
	(1.127)	(0.690)**	(2.286)	(0.480)
Relative Proportion Married	0.171	0.207	0.177	-0.069
	(0.065)***	(0.040)***	(0.133)	(0.028)**
Relative Proportion Single	-0.073	0.039	0.069	-0.057
	(0.047)	(0.029)	(0.096)	(0.020)***
1990 Census Indicator	-0.046	-0.005	-0.073	-0.012
	(0.019)**	(0.012)	(0.038)*	(0.008)
2000 Census Indicator	-0.053	-0.014	-0.087	0
	(0.019)***	(0.011)	(0.038)**	(0.008)
Constant	-2.317	-1.289	-1.319	0.185
	(1.068)**	(0.654)**	(2.166)	(0.454)
Observations	218	218	218	218
R-squared	0.5	0.28	0.09	0.58

Standard errors in parentheses

* Significant at 10%; ** at 5%; *** at 1%

Table 2.3 Fixed Effect Estimates of the Effect of Relative Arrest Rates on Relative Labor Market Outcomes of Blacks Compared With Whites

	Annual Wage and Salary Income	Probability of Employment in the Previous Year	Hourly Wage	Occupation Index
Relative Arrest Rate	-0.007	-0.002	0.001	-0.007
	(0.010)	(0.006)	(0.026)	(0.004)*
Relative Institutionalized Rate	0.002	-0.003	0.014	0.002
	(0.009)	(0.005)	(0.023)	(0.004)
Relative Education	1.066	0.313	1.007	0.356
	(0.246)***	(0.143)**	(0.633)	(0.100)***
Relative Age	1.406	0.985	10.862	-0.257
	(2.718)	(1.580)	(6.994)	(1.101)
Relative Age Squared	-0.437	-0.49	-5.811	0.307
	(1.389)	(0.807)	(3.573)	(0.562)
Relative Proportion Married	0.001	0.07	0.48	-0.005
	(0.098)	(0.057)	(0.251)*	(0.040)
Relative Proportion Single	-0.161	-0.021	0.426	-0.012
	(0.082)*	(0.048)	(0.211)**	(0.033)
1990 Census Indicator	-0.067	-0.009	-0.115	0.01
	(0.023)***	(0.013)	(0.059)*	(0.009)
2000 Census Indicator	-0.067	-0.02	-0.09	0.03
	(0.024)***	(0.014)	(0.062)	(0.010)***
Constant	-1.079	0.059	-5.971	0.523
	(1.359)	(0.790)	(3.497)*	(0.551)
Observations	218	218	218	218
R-squared	0.41	0.19	0.13	0.35
Number of Cities	95	95	95	95

Standard errors are in parentheses

* Significant at 10%; ** 5%; *** 1%

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Appendix A: Socio-Economic Index for 13 Occupation Categories

Table A2.1

Socio-Economic Index for 13 Occupation Categories	
Professional Specialty	57.84
Executive Administrative and Managerial Occupations	50.64
Technicians and Related Support Occupations	44.35
Sales Occupations	35.96
Protective Service Occupations	35.85
Administrative Support Occupations, including Clerks	34.22
Precision Production, Craft and Repair Occupation	30.91
Transportation and Machine Moving Occupations	25.49
Machine Operators Assemblers and Inspectors	24.03
Service Occupations, Except Protective Household	20.43
Handlers, Equipment Cleaners, Helpers and Laborers	19.84
Farming, Forestry and Fishing Occupations	19.52
Private Household Occupations	17.69

Appendix B: Relative Arrest Rates by Year and City

Table A2.2

Relative Arrest Rates of Blacks to Whites in Each Year

City		1979	1989	1999
Albany	NY	2.66	4.84	
Allentown	PA	3.26	4.65	3.32
Anaheim	CA	2.40	1.95	2.25
Ann Arbor	MI	4.05	4.84	4.91
Atlanta	GA	1.16		
Austin	TX		2.04	
Bakersfield	CA	1.41	2.94	2.52
Baltimore	MD	1.85	2.32	2.64
Baton Rouge	LA	3.10	2.60	3.06
Beaumont	TX	1.36	2.45	2.09
Birmingham	AL	0.87		
Boise	ID			3.55
Boston	MA	2.54	4.27	2.86
Bridgeport	CT	2.02	1.89	1.20
Brownsville	TX			0.48
Buffalo	NY	3.26	3.64	3.07
Charlotte	NC	2.70		4.02
Chicago	IL	3.54	3.11	2.85
Cleveland	OH	1.59	3.03	
Colorado Springs	CO		3.19	
Columbia	SC	2.03		2.98

Columbus	GA			2.57
Corpus Christi	TX			1.36
Dayton	OH	1.57		2.21
Denver	CO	1.66	2.91	
Des Moines	IA	2.34	4.78	4.19
Detroit	MI	1.98	1.62	1.19
Erie	PA	3.60	4.54	4.24
Eugene	OR	2.71	3.96	
Evansville	IN			4.33
Fayetteville	NC			3.08
Flint	MI	2.69	2.96	2.21
Fort Wayne	IN	2.05	4.19	4.70
Fort Worth	TX	1.26	2.34	
Fresno	CA	1.33	1.90	1.74
Garden Grove	CA	3.97	1.99	1.22
Gary	IN	1.48	1.71	0.95
Grand Rapids	MI	2.45	5.58	4.24
Greensboro	NC	2.14	3.45	3.45
Hampton	VA	1.62	2.18	2.34
Jackson	MS		2.31	
Houston	TX	1.28		
Huntsville	AL			3.02
Jackson	MS			2.00
Jersey City	NJ	2.25	3.46	2.54
Kansas City	MO	2.28		
Knoxville	TN	0.96	1.85	2.45
Lansing	MI	2.60	4.99	3.11
Las Vegas	NV	2.18	2.03	
Little Rock	AR	2.07		3.29
Long Beach	CA	2.39	2.67	2.16
Los Angeles	CA	2.23	2.02	1.88
Louisville	KY	1.44	1.96	
McAllen	TX			0.56
Minneapolis	MN	5.60	6.92	7.89
Mobile	AL	1.51	2.70	2.99
Modesto	CA	2.60	4.31	2.20
New Haven	CT	2.40	2.31	1.84
New Orleans	LA	1.18	2.27	1.40
New York	NY	2.64	2.36	1.81
Newark	NJ	1.60	2.11	1.66
Newport News	VA		3.73	3.18
Norfolk	VA	1.40	3.16	3.01
Oakland	CA	2.13		

Oklahoma City	OK	1.91		2.92
Ontario	CA		1.57	0.96
Oxnard	CA	0.82		1.02
Paterson	NJ	1.81	1.62	1.17
Philadelphia	PA	2.56	2.94	1.72
Phoenix	AZ	2.95	3.56	
Pittsburgh	PA		4.27	3.96
Portland	OR	3.10	4.95	
Portsmouth	VA		2.04	
Providence	RI	4.67	5.05	2.76
Provo	UT			2.29
Raleigh	NC	2.74		4.10
Reno	NV		2.65	4.68
Richmond	VA	1.44	2.17	3.54
Riverside	CA	2.73	3.36	2.40
Rochester	NY	3.14	4.19	2.75
Sacramento	CA	1.52	3.95	2.13
Saint Louis	MO	2.62	4.38	4.70
Saint Paul	MN	5.35	8.85	8.34
Salem	OR		2.29	
Salinas	CA		1.71	1.06
Salt Lake City	UT	5.02		2.64
San Antonio	TX	1.36	1.69	
San Bernadino	CA	1.74		1.63
San Diego	CA	2.64	3.51	
San Francisco	CA	2.70	4.65	4.75
San Jose	CA	2.06	2.80	
Santa Ana	CA	1.42	1.93	1.78
Santa Rosa	CA		4.54	
Shreveport	LA		3.90	
South Bend	IN	3.30	3.32	4.60
Spokane	WA	4.32	4.65	6.29
Springfield	MA	2.59	2.43	1.68
Stamford	CT	3.91	7.78	4.67
Stockton	CA	2.05		2.12
Syracuse	NY	3.88	3.56	4.87
Tacoma	WA	2.67		3.05
Toledo	OH	2.42		3.46
Tucson	AZ	2.65		
Tulsa	OK	2.02		2.33
Virginia Beach	VA	1.40	1.79	2.15
Waterbury	CT	3.01	4.03	2.51
Worcester	MA	3.99	6.71	2.85

Appendix C: Comparison of arrest rates for blacks relative to whites with those for 16-45 year old black males compared to 16-45 year old white males.

NIBRS data from 13 cities used in this paper were used to construct the ideal measure of total arrests of 16 to 45 year old black males. Using the 2000 PUMS, I estimated the 16 to 45 year old black male population in each city. The black arrest rate is thus the number of 16 to 45 year old blacks arrested in a city divided by their estimate of the 16 to 45 year old black population in the city. Ideally, we would use only arrests of residents of the city, but resident data has a high rate of "unknown" responses.

The corresponding variable is also calculated for white males aged 16 to 45. This is used to calculate the preferred relative arrest rate for this paper (which cannot be calculated using UCR data). The actual measure used in this paper is also calculated using NIBRS data. The actual measure takes total arrests of all ages and sexes and for each race and divides by the population of that race. The following gives the mean relative arrest rates for these 13 cities and the correlation between the actual and ideal measures. Though relative arrests are higher on average for the restricted group the correlation between them is quite high at 0.88.

Table A2.3 Mean of Relative Arrest Rates for Blacks Compared to Whites

Both Sexes and All Ages	2.95
16 to 45 year-old Males	3.33
Correlation of the above measures:	0.88

CHAPTER 3

THE IMPACT OF JOB DISPLACEMENT ON IMMIGRANTS

3.1 Introduction

The U.S. economy grew continuously from the first quarter of 1991 through the first quarter of 2001.[78] In periods such as these when the economy is growing, workers who lose their jobs may not find it difficult to obtain new employment. However, as in recent years, when the economy slows, unemployment rises and labor market re-entry becomes increasingly difficult, increased attention turns to the labor market prospects for unemployed workers. In times of such slow job growth, the flow of workers into unemployment is not as likely to be cushioned by job creation elsewhere.[79]

Current debate centers around the effect of outsourcing jobs from the U.S. and the continuing ability of the U.S. economy to absorb the people who lose their jobs.[80] This paper joins the growing literature[81] on the group of workers who lose their jobs due to slack economic conditions or plant, shift or company closings, by examining the role of immigrant status for "displaced workers." Hereafter, in contrast to persons who lose their jobs due to incompetence or other personal reasons, I use the term "displaced workers" to refer solely to persons losing jobs for the aforementioned economic reasons.

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[78] Source: National Bureau of Economic Research Business Cycle Dating Committee accessed at http://www.nber.org/cycles/cyclesmain.html on February 15, 2004. Though employment lags the measure used by the Dating Committee, i.e., output growth, unemployment also fell over much of this period with the exception of 1991-1992 and 2000-2001 (Bureau of Labor Statistics).

[79] McMenamin, Krantz and Krolik (2002) discuss the 2002 job market and note that while some sectors increased the number employed, there were widespread sectoral job losses that outweighed them.

[80] Several recent articles in the New York Times discuss job loss. See, for example, Stephen Lohr (2004). See also The Economic Report of the President 2004 and Kletzer (1998) for discussions of the effects of general outsourcing both inside and outside the U.S.

[81] For surveys of this literature see Hamermesh (1989) and Fallick (1996). For more recent work see also Farber, Haltiwanger and Abraham (1997), Kletzer (1998) and Chan and Huff Stevens (2001).

Recent concerns about displaced workers come from the new constituency of high-skilled workers who are being laid off. In the later part of the 20th century, the effects of globalization were concentrated on tradable manufactured goods, which led to displacement in the relatively low-skilled job market in the U.S.[82] Displacement occurring ostensibly due to NAFTA, advances in free trade and improvements in technology weakened labor market power of low-skilled workers.[83] More recently, communications and technological advances are claimed to have led to an exodus of white-collar service jobs from the U.S. mainland to Asian countries. These advances also lead to outsourcing of traditional in-house services to outside contractors.[84] Farber (2003) finds that there has been an increase in displacement rates of workers with 16 and more years of education over the early and mid-90s and mentions that this is consistent with reports of "substantial" white-collar job layoffs over the period.[85] As a result, there is heightened interest in the effects of technology, immigration and trade in services on all workers in the U.S. job market.[86]

Previous literature regarding displaced workers has not focused separately on the impact of displacement on immigrants. An exception is Chi and McCall (2004). They note that rates of displacement are influenced by skill, industry and occupation. Chi and

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[82] See the Economic Report of the President 2004 pp. 53-82. In particular, see chart 2-9, which shows the increasing share of imports that make up the total share of goods purchased in the U.S.

[83] Employment Policy Institute briefing paper #147: Robert Scott "The High Price of Free Trade: NAFTA's failure has cost jobs across the nation." November 17, 2003. See also Katz and Murphy (1991) for an investigation of causes of declines in relative wages of less-skilled workers.

[84] The Economic Report of the President 2004 discusses the "prominent" issue of the new impetus to outsource professional service jobs in addition to concerns about losses of manufacturing jobs and "offshore outsourcing" (p.229). See also Bartel, Lach and Sicherman (2004), who discuss increasing trends to outsourcing from of former in-house jobs to outside service firms.

[85] While the rates of displacement are still lower than that of the less-skilled workers, this absolute increase in rates of displacement may be the result of increased outsourcing of jobs to other countries and outside firms.

[86] Kletzer (1998). In addition, Hamermesh (1989) finds that over time there has been a "secular" increase in rates of displacement beyond the business cycle fluctuations.

McCall test whether immigrant status is a factor by which displacement rates differ. Their paper also tests for differences in immigrant take-up rates of unemployment insurance benefits. They find that newly arrived and non-citizen immigrants are less likely to be displaced than natives. In addition, among qualified displaced workers, newly arrived non-citizen immigrants are less likely to claim benefits than other immigrants and natives.

The idea that immigrants may have different labor market experiences than those of natives is not a novel one. There is a large literature concerned with the assimilation of immigrants into host country labor markets (see, for example, Daneshvary, Herzog, Hofler and Schlottmann 1982; Borjas 1985; Schoeni 1987; Schoeni 1988; Butcher and Piehl 1998; Friedberg 2000; Hu 2000; Butcher and DiNardo 2002). Much of this previous literature is concerned with the observed achievement gap between immigrants and natives. Immigrants, on average, have worse labor market outcomes than similar natives. These differences might even have an effect on second generation immigrant performance (Hatton 1997).

Research on displaced workers continues to be relevant for policy formulation regarding unemployed workers displaced by the recent recession and jobless growth period,[87] immigrant use of government income support programs (Borjas and Hilton 1996; Borjas 1999) and the relatively worse labor market performance of immigrants. This paper adds to the literature by examining whether displaced immigrants fare worse than displaced natives or whether they are better able to adjust to job loss. Given the recent efforts to restrict immigrant use of government welfare programs (Zimmerman and Tublin 1999; Ku and Freilich 2001) and the expectation and trends towards greater rates

[87] McMenamin, Krantz and Krolik (2003).

of displacement (Kletzer 1998; Farber 2003; Chi and McCall 2004), added information on the experiences of these workers is important. Following Farber (2003), I examine the probability of survey-date employment after displacement in the prior three years and the change in weekly earnings for workers re-employed full-time after losing full-time jobs.

3.2 Conceptual Framework

Following Farber (2003), I estimate a linear regression model controlling for sex, non-white race, education, age and tenure on the lost job. To address the question of how immigrants differ from native, an immigrant dummy is included.[88] To address potential discrimination against people reporting Spanish ancestry, who largely report white as their race and comprise roughly half the weighted sample of immigrants, an indicator of Hispanic origin is also included.[89]

The following regression is estimated:

$$Y = \beta_0 + \beta_1 Imm + \beta_2 Hisp + \beta_3 Female + \beta_4 Nonwhite + \beta_5 Ed0\_11 + \beta_6 Ed13\_15 + \beta_7 Ed16\_ + \beta_8 Age25\_34 + \beta_9 Age35\_44 + \beta_{10} Age45\_54 + \beta_{11} Age55\_64 + \beta_{12} Ten2\_3 + \beta_{13} Ten4\_10 + \beta_{14} Ten11\_20 + \beta_{15} Ten20\_ + \beta_{16} YSD2 + \beta_{17} YSD3 + \beta_{18} Unem + \varepsilon \quad (3.1)$$

The following indicators are used as controls: Imm – non-US country of birth; Hisp – Hispanic; Female – gender is female; Nonwhite – race is not white; Ed0_11 – 11th grade or less; Ed13_15 – some college, no degree; Ed 16_ - Bachelor's or higher degree;

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[88] Reitz and Sklar (1997) found differences in ethnic assimilation pressures by race. Specifically, European-origin immigrants face cultural assimilation pressures and economic costs due to cultural differences, but racial minority immigrants experience discrimination on the basis of their skin color rather than ethnic and cultural differences with the majority. As 71% of the sample of immigrants is white, the effect on this group is expected to dominate. Thus, racial interactions with immigrant status are not included.

[89] Additionally, based on penalties to illegal status relative to legal immigrants found in Phillips and Massey (1999) and Kossoudji and Cobb-Clark (2002), the Hispanic variable is needed to absorb some of the effect of illegal status that may be captured by the immigrant status variable. Of the estimated 5 million illegal immigrant residents in the U.S., over 70% are believed to be from Hispanic countries. See Estimates of the Undocumented Immigrant Population Residing in the U.S: October 1996 accessed at http://uscis.gov/graphics/shared/aboutus/statistics/illegalalien/illegal.pdf on May 28, 2004.

Age25_34 – age is between 25 and 34 years inclusive; Age35_44 – age is between 35 and 44 years inclusive; Age45_54 – age is between 45 and 54 years inclusive; Age55_64 – age is between 55 and 64 years inclusive; Ten1_3 – tenure on lost job of 1 to 3 years; Ten4_10 – tenure on lost job of 4 to 10 years; Ten11_20 – tenure on lost job: 11 to 20 years; Ten20_ - tenure on lost job: 20 or more years; YSD2 – 2 years since job loss; YSD3 – 3 years since job loss; Unem – MSA unemployment rate in the year prior to the survey; ε - random error; Y is the outcome variable, which is either a survey date employment indicator or change in log real earnings between current and old job.[90]

The motivating question is: do immigrants who are displaced suffer greater economic losses than natives? In periods of recession, when search queues for jobs are longer, immigrants have been the target of political rhetoric which claims immigrants take jobs away from native-born Americans with the argument that immigrants have lower reservation wages.[91] Have immigrants experienced a backlash in the form of discrimination in hiring in light of NAFTA, other trade agreements and weakened labor markets of less-skilled workers? Do immigrants really take jobs away from U.S. born Americans due to lower reservation wages? In the latter case, we would expect to see relatively higher probabilities of employment for displaced immigrants compared to natives, combined with relatively lower earnings conditional on employment.

New immigrants tend to settle in areas where there are already large populations of established immigrants (Bartel 1989). To the extent that areas with high concentrations of immigrants have better (worse) labor markets for displaced workers relative to those with higher proportions of natives, displaced immigrants may have

[90] The CPI-U is used to adjust earnings to 2003 dollars.
[91] McMenamin, Krantz and Krolik (2002) note that with weak employment growth late in 2002, duration of unemployment increased.

easier (harder) labor market re-entry experiences than natives. In truth, the distribution of immigrants across the U.S. may account for any differences found by immigrant status, when immigrants have the same experience as natives in the same region.

To address this distribution issue, in an alternative specification, I include metropolitan statistical area (MSA) fixed effects, in addition to the variables used in equation (3.1), to estimate the effect of immigrant status. Also, accounting for MSA fixed effects helps to address the possible endogeneity of the initial location of immigrants within the U.S. on the basis of maximizing expected income.[92] In other words, new immigrants may go where labor market re-entry is easier due to better job opportunities or lower resistance to immigrant hires.[93]

3.3 Data

The data come from Current Population Survey (CPS) Displaced Worker Supplements (DWS) from 1996, 1998, 2000 and 2002.[94] These data ask workers 20 years and older whether they have lost a job in the 3 years prior to the survey year. The sample is restricted to those who have lost jobs due to plant/company closing, insufficient work or position/shift abolished, rather than all those who lost a job. This is done to exclude low productivity workers who may induce their job loss and have subsequent difficulty obtaining employment.

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[92] For example, Borjas (1999) finds that immigrant welfare recipients are more likely than other immigrants or natives to live in states with higher benefits.

[93] One example of this would be immigrants who are hired on the basis of labor shortages in an area for certain types of jobs. For example, Massey et al. (1994) p. 705 note that farm workers, laborers and craft workers were over represented in Puerto Rican migrants to the U.S. partially due to existing job offers in the U.S.

[94] While immigrant status is available from the 1994 DWS onwards, MSA identifiers are not available in 1994.

Table 3.1 gives weighted sample means and counts for selected variables used in this paper by immigrant status.[95] Immigrants are more likely to be nonwhite and are almost ten times more likely than natives to be Hispanic. The proportion of immigrants re-employed at the survey date is about 4 percent lower than the rate for natives. However, immigrants who lost full-time jobs and manage to obtain full-time re-employment have lower earnings losses than natives. While these immigrants are re-employed at earnings about 1.4% below their earnings on the lost jobs, natives earn about 10% less on average in the current jobs. For the sample of all workers whose weekly earnings on both jobs are reported, displaced immigrants surprisingly see weekly earning growth on average.[96] Unsurprisingly, based on the immigrant assimilation literature, immigrants earned less than natives both before and after displacement.

3.4 Results

To address the issue regarding the effect of immigrant status at different points in the business cycle, I will briefly discuss some results not shown here. I estimate equation (3.1) without MSA fixed effects in each of the survey years. As MSA specific effects are not included in these annual regressions, I include the 1994 CPS DWS in this exercise. As a check on the methodology, I closely replicate Farber's results with the estimation of equation (3.1) without the immigrant status and Hispanic indicators. I find that the effect of immigrant status does not significantly affect re-employment likelihoods in most of the survey years (mainly boom periods). However, data from the 1994 survey suggests that immigrants were significantly less likely than natives to be re-employed in early 1994.

[95] Sampling weights from the CPS Outgoing Rotation Files are used.
[96] Estimates of weekly earnings growth among immigrants and natives from the CPS March files show that immigrants had faster weekly earnings growth overall from 1994-2002. See Appendix A for details on these calculations.

These workers were displaced over the 1991-1993 period when the economy was just starting to recover from the previous recession. Generally, immigrant status positively affects the change in weekly earnings, so that immigrants have lower earnings losses than natives. Workers displaced in 1991 through 1993 have a negative (though insignificant) estimated effect of immigrant status. This period led up to the 1992 Presidential elections, when the economy was emerging from a recession and included heated debate about the labor market effects of immigration and trade. Hence, it is possible that immigrants face greater difficulty when jobs are hard to come by, but are no worse of in other years when the economy was generally experiencing growth

Table 3.2 column (1) gives coefficients from a regression pooling the 1996-2002 DWS surveys to estimate equation (3.1). It examines the likelihood of survey-date employment of workers displaced from 1993-2001. Results show that immigrants were no different from natives in their propensity to re-enter the employed part of the labor force, while Hispanics are significantly less likely to obtain re-employment by the survey date. Roughly half of immigrant workers are Hispanic; with the omission of the Hispanic indicator the immigrant coefficient is significantly negative.

Table 3.2 column (3) examines the effect of immigrant status on the change in log weekly earnings. Immigrants employed full-time, who had lost full-time jobs, had earnings losses 8.5% lower than those for similar natives. Hispanics did not have different earnings losses conditional on full-time to full-time employment transitions.

Table 3.2 columns (2) and (4) re-estimate the effect of immigrant status including MSA specific effects to control for pre-existing conditions such as growing labor markets which may have historically attracted immigrants. Column (2) results suggest that

immigrants were actually concentrated in cities with lower probabilities of re-employment. From Table 3.1 recall, immigrants were less likely to be employed at the survey date. From the results in column (2), however, if an immigrant and native reside in the same MSA, the immigrant is about four percentage points more likely to be employed at the survey date; this difference is significant. It should be noted these results include annual MSA unemployment rates. Column (4) shows that the result that immigrant status implies lower earnings losses is robust to the addition of MSA effects, though the point estimate is slightly lower. Hispanics are less likely to be re-employed, but are no different from non-Hispanics in their loss of earnings from the old job to the current job.

3.5 Why Might Immigrants Be Hurt Less by Displacement? Exploring the Results

As displaced immigrants appear to suffer somewhat less than natives, an investigation of potential reasons for this result is in order. I explore three possibilities for the more positive results for immigrant workers. First, immigrants are concentrated in jobs with low earnings and as such cannot experience as much earnings loss as higher paid natives when moving from one full time job to another. For example, a person who loses a full time minimum wage job and then is reemployed full time is unlikely to experience any wage loss at all. In addition, if immigrants accept lower wages than similar natives, immigrants may be more likely to be re-employed. The hypothesis that immigrants exhibit greater flexibility in the labor market after displacement is explored by examining whether immigrants are more likely than natives to turn to self-employment or switch occupations. Finally in this section, I explore the possibility that immigrants search more intensely for jobs following displacement because they may

have different access to government income support programs that ease the burden of being unemployed than natives (Chi and McCall 2003; Borjas 2003) or they have less non-labor income.

3.5.1 Earnings on the Old Job

Recall from Table 3.1 that immigrants have lower weekly earnings than natives (among those formerly full-time workers who are re-employed full-time) in both the current and pre-displacement jobs. I test the hypothesis that lower earnings losses of immigrants are due to their concentration at the lower end of the weekly earnings distribution limiting their potential loss conditional on full-time re-employment. I calculate quintiles for real weekly earnings on the lost job in each of the survey years. Indicators of the highest four earnings quintiles on the lost job are included as additional variables in the specification that adds MSA fixed effects to equation (3.1). Results for the survey-date and earnings loss regressions are shown in Table 3.3 columns (1) and (2).

Clearly, the higher the earnings quintile on the old job the greater is the probability of re-employed at the survey date, holding constant other observable wage determining factors like tenure and education. However, adding the pre-displacement earnings quintiles has almost no effect on the coefficient of the immigrant dummy, which remains positive and significant in the re-employment equation. Adding the earnings quintile variables affects the earnings loss regression, making the coefficient of the immigrant dummy statistically insignificant. It appears that the apparently smaller percentage job losses for immigrants are due to the fact that, even after controlling for observable wage determining characteristics, they tend to be concentrated in lower earning jobs, and thus have less to lose.

3.5.2 Labor Market Flexibility of Immigrants

In this section, I investigate whether displaced immigrants are willing to do more to be gainfully employed than similar natives. Are immigrants more likely to go into self-employment rather than remain unemployed? Are immigrants more willing to switch occupations? To the extent that workers are displaced from declining occupations, they will be re-employed faster if they are willing to learn a new trade. Controls from the preceding regression (Table 3.3 columns 1 and 2) are used in regressions of indicators of self-employed or switched occupations; results are reported in Table 3.3 columns (3) and (4) respectively.

In this sample, immigrants are not different from natives in their propensity to enter self-employment after displacement. Immigrants are, however, significantly more likely to report that they are currently in a different occupation from the occupation on the job they lost. This is consistent with the idea that immigrants are more willing to lose occupation-specific human capital and try something new in order to become re-employed.

3.5.3 Government Income Support, Non-Labor Income and Search Intensity

Borjas (2003) finds evidence that after welfare reforms enacted in 1996 curtailed the eligibility of non-citizen immigrants to participate in various income support programs – Aid to Families with Dependent Children (AFDC)/ Temporary Assistance to Needy Families (TANF), Medicaid, Food Assistance or Supplemental Security Income (SSI) – immigrant labor supply increased in response. In this section, I test whether greater success of immigrants in finding re-employment results from greater search intensity, as their non-work alternatives (government income support) will appear less

attractive. Lower welfare eligibility (or even perceptions of lower eligibility) for non-citizen immigrants (almost 57 % of the sample of immigrants) may have led to lower participation rates in government income support programs among non-citizen immigrants.

Borjas (1999) shows although there were substantial declines in the participation rates after the 1996 reforms in a several welfare programs, immigrant participation rates remained above native rates. He also notes that in response to restrictions on welfare use by non-citizens many qualified non-citizens would apply to become naturalized citizens, nullifying the restrictions on welfare use.[97] However, there are several reasons to believe that immigrants may be less likely than natives to use welfare programs in the period used here. First, as mentioned earlier there were welfare reforms in 1996 which restricted non-citizen immigrant use of public welfare (Zimmerman and Tublin 1999; Ku and Freilich 2001). Though welfare reforms mainly restricted use by immigrants arriving after 1996, if immigrants mistakenly believe they are ineligible participation rates will decline.[98,99] Second, Borjas (1999) notes immigrants stay longer on welfare. Time limits for welfare use, which were part of the 1996 welfare reforms, may reduce the eligibility of immigrants to a greater degree than that of natives. Finally, Haider, Schoeni, Bao and Danielson (forthcoming) note that much of the decline in welfare participation found in Borjas (1999) and similar studies can be explained by macroeconomic improvements

[97] In this analysis I do not distinguish between citizen and non-citizen immigrants.
[98] Eligibility may not be an issue in this sample as over 90% of the immigrants arrived prior to 1996.
[99] Borjas (1999) discusses the possibility that immigrants will reduce their participation in welfare even if their eligibility does not decline. He also discusses the fact that even though the language of the legislation restricted use of non-citizen immigrants who were in the U.S. prior to the legislation, this was not fully enforced.

over the period. If immigrants had greater opportunities over this period than earlier ones, their relative welfare participation may fall.

Borjas' findings may not be representative of government assistance use by immigrant status in the current work for a few reasons. First, the age group of the displaced worker sample in the current work is restricted to 20 through 64 years, whilst Borjas' study of immigrant welfare use is at the household level and there are no restrictions on the age of the household head. Second, Borjas' work shows participation rates only up to 1998, which includes only 2 of the 4 years used in this paper.[100] In addition, I define immigrants solely as foreign-born persons. In Borjas' work, second generation immigrants' welfare use is classified as immigrant use if the household head is an immigrant. Another issue is that displaced workers are atypical. Hamermesh (1989) notes displaced workers are considered to have strong labor force attachment. This cannot be said of the typical welfare recipient.

In order to obtain representative government assistance participation rates for the relevant groups used in this section of the analysis, I first calculate take-up rates of unemployment insurance benefits for the current sample. Appendix B Table A3.2 shows the take-up rate of unemployment compensation by sex, employment status and immigrant status in the current sample. Calculations use CPS sampling weights. Not surprisingly, currently unemployed displaced workers have a higher take-up rate of unemployment benefits than do currently employed displaced workers. Surprisingly, considering findings of higher welfare participation rate of immigrants (Borjas 1999; Borjas and Hilton 1996), displaced male immigrants are 8 percentage points less likely to

[100] Recall, after the passage of time, time limit use on welfare should further reduce immigrant participation even relative to native declines for this reason.

receive unemployment benefits than displaced male natives from the group employed at the survey date. Unemployed displaced male immigrants are 4 percentage points less likely than unemployed displaced natives to receive unemployment benefits. Even female immigrants are equally or only slightly more likely to use unemployment compensation. While I am unable to restrict the sample by eligibility[101] it seems unlikely that difference between the participation rates of male natives and immigrants is due entirely to eligibility or eligibility perceptions, since the take-up rates for female immigrants is so similar to that of natives. From the results for currently unemployed workers in Table A3.2, male natives have the greatest use of unemployment benefits, while female immigrants, male immigrants and female natives follow (in that order). The figures do not, however, differ markedly from one another.

Since women are the main beneficiaries of public welfare, the social safety net for displaced women may be greater than that for men. Borjas and Hilton look at several welfare programs whose main beneficiaries are women (Currie and Grogger 2000; Kaestner and Kashaul 1997) and find that on average immigrants have much greater participation rates in these programs than natives. Thus, male immigrants may have the lowest income support from these programs, while female immigrants the greatest.

The difficulty in ascertaining which group has the greatest motivation to search for a new job is apparent. I have chosen to calculate the relative degree to which each group will be motivated to search using the CPS Annual Demographic (March) files for 1996, 1998, 2000 and 2002. These data contain information on income received from various government income support programs. In addition, they contain income of other

[101] Recent immigrants may not have the required duration of employment to qualify. However, fewer than 10% of immigrants in the sample arrived within the three years prior to the survey. Thus, controlling for tenure on the job, this form of income support should be open to most of men and women in the sample.

household members. First, I examine participation in various government income support programs (public assistance, supplemental security and unemployment compensation) in the previous year using a linear regression model with a variety of demographic, geographic and time controls. I control for weeks unemployed in the previous year and non-labor income available from the household. Annual non-labor income available to an individual is calculated as household income minus own wage and salary income. Second, I examine differences in access to non-labor income between immigrant-gender groups. Further details of the procedure are in Appendix B.

To test the hypothesis that differences in government income support participation influences search intensity, I replace the immigrant dummy by three indicators (male immigrant, female immigrant and female native) in the survey-date regression including MSA fixed effects and quintile dummies. Under the first hypothesis that government income support differences drive search intensity I expect that female immigrants should be re-employed in the highest proportion as controlling for other factors they are the least likely to receive part of their income from the government (see Appendix B Table A3.3 column 4). Male immigrants and female natives should be re-employed in the next highest proportions in that order. As overall government income support use by female and male immigrants are not significantly different, all immigrants may have higher search intensity than natives. The lowest re-employment rate is expected for the male natives. Under the hypothesis that non-labor income represents the cushion that individuals can fall back on in the event of displacement, the following order of re-employment is expected: male immigrants should search the hardest followed by male natives, female immigrants then female natives (Appendix B Table A3.3 column 5).

Table 3.3 column 5 shows that female immigrants are 4.8 percentage points less likely to be re-employed than male immigrants, who are the most likely to be re-employed. Female immigrants are about 2.5 percentage points more likely than female natives to be re-employed, but the difference is insignificant. Female natives are 1.2 percentage points more likely than male natives to be re-employed at the survey date. So the hypothesis of search intensity (greatest to least: female immigrants, male immigrants, female natives, male natives) is contradicted by the finding of re-employment order here (highest to lowest: male immigrants, female immigrants, female natives, male natives).

Similarly, the findings presented in Table 3.3 columns 5 through 7 are contrary to what the non-labor income motivation hypothesis predicts (greatest search motivation to least: male immigrants, male natives, female immigrants, and female natives).[102] Results in Table 3.3 column 6 control for marital status. The regression results in column 7 control for marital status interacted with female indicators. None of the results suggests that either the government income support or non-labor income fully determine the degree to which each group returns to work. The ranking of this last hypothesis holds with the exception of male native re-employment rates. Quite likely, access to assets can explain the lower than expected re-employment rates of male natives. I cannot account for asset holdings with the CPS March files.

3.6 Heterogeneity among Immigrants

Immigrants are a rather heterogeneous group. This section examines differences in re-employment outcomes between immigrants. As discussed earlier, non-citizen immigrant access to welfare was limited by welfare reforms in 1996. In addition, citizens

[102] See Appendix A for details on the calculation of the ranking of search intensity.

qualify for a number of federal jobs that are not available to non-citizen immigrants. I estimate the regressions by citizenship status of immigrants to see if there are any differences in their re-employment outcomes. Borjas (1985) suggests that source country of immigrants matters for immigrant quality and assimilation in the U.S. To address concerns that immigrants' source country may capture differences in human capital (or value of such in the U.S. labor market), I break immigrants into groups by their region of origin. Hispanics, however, are grouped together due to language and cultural similarities. Finally, I ask whether the blow to immigrants created by displacement might be cushioned if they live in areas with greater populations from their own country.

3.6.1 Citizenship

Table 3.4 columns (1) and (6) are estimates of regression (1) with MSA fixed and quintiles. They include only immigrants who report that they are citizens. Columns (2) and (7) are similar but include only non-citizen immigrants. Besides the fact the non-citizen immigrants have higher point estimates than naturalized immigrants, the earlier results that immigrants are more likely to be re-employed and have similar earnings losses, when compared to natives, holds.

3.6.2 Region of Origin and Immigrant Type

As in section 3.5.3, I split the original immigrant indicator into several indicators. Firstly, all immigrants reporting Hispanic ancestry are assigned to one group regardless of region of origin. Indicators are also created separately for Asian, European and West Indian immigrants. I group Middle Eastern and African immigrants into one indicator due to small sample sizes from these areas. Immigrants from other part of the world are

assigned to one of the aforementioned groups.[103] Hispanics are the only immigrant group more likely than natives to be re-employed. Most other immigrant groups (Asians are the exception) are less likely to be re-employed than natives (Table 3.4 column 3). Table 3.4 column (8) shows that while European and Asian immigrants have lower re-employment likelihoods, they have lower earnings losses. This suggests that these groups may have higher reservation wages than other groups.

3.6.3 Ethnic Networks

The literature on the impact of ethnic networks provides mixed evidence on the extent to which networks improve labor market outcomes of co-ethnics. Here, the term co-ethnic refers to immigrants from the same country. Reitz and Sklar (1997) note traditional theories view ethnic network use as limiting opportunities. Similarly, Nee, Sanders and Sernau (1994), who interview Asian immigrants, finds that many prefer jobs outside the ethnic enclaves because of higher wages available to them in the outside labor market. Tienda and Lii (1987) test whether enclaves improve outcomes for minorities by protecting against discrimination and raising self-esteem, but find no support for this argument.[104] In contrast, Wilson and Portes (1980) find that Cuban immigrants working within an enclave earn similar returns to Cuban immigrants in the general labor market. Potentially, therefore, larger populations from one's own country may improve the outcome of immigrants but it remains an empirical question.

This section tests whether larger ethnic networks mitigated effects of discrimination and/or possibly provided access to information and opportunities that may

[103] Immigrants from Oceania are grouped with Asians. Immigrants from Canada, New Zealand, Australia and the former Soviet Union are grouped with Europeans.

[104] They also discuss previous literature suggesting that initial limitations give way to greater political power as the minority population increases, leading to economic access on a previously unattainable scale.

not exist without networks to assist job search. To measure ethnic networks, population counts by country of birth of immigrants were obtained at the MSA level from the 2000 Public-Use Microdata Sample (PUMS) from the U.S. Census. The sample is restricted to residents in MSAs identified in both the CPS and PUMS files. Each immigrant's network is measured as the number of people from the immigrant's country residing in his or her MSA in 2000, divided by the MSA population.[105] The network measure for non-immigrants is set to zero.[106] Figure 1 provides the weighted sample distribution of values of the ethnic population share variable. The largest population shares are for Mexicans and Cubans. Most other immigrant shares were below 15% of the MSA population.

Table 3.4 column (4) shows that after controlling for network strength, immigrant status still increases the probability of re-employment. However, larger networks do positively affect immigrants' survey-date employment likelihoods. Since as expected, having a non-zero ethnic network exerts a positive effect on the probability of survey-date employment, immigrant groups will have different experiences based on the size of the population from their own country in an area. For example, displaced Mexicans in New York, where approximately 1.2% of the population is Mexican, have a higher probability of being re-employed than natives of 5.9 percentage points, while displaced Mexican immigrants in Fort Lauderdale, where the Mexican population share is only 0.6%, experience only 4.8 percentage points increase in their relative probability of

[105] It must be noted that this calculated measure will generally understate the population it targets as several continent level answers are allowed (e.g. Other Africa). Thus, country of birth cannot be assigned for some immigrants.

[106] I assume that natives have the same network. Under this assumption, the effect of the general preferences for natives over and above immigrants (since they are a majority) is captured by the immigrant dummy.

being re-employed. Meanwhile, displaced Jamaicans in New York, where they constitute 1.4% of the population, have an increased probability of finding re-employment of 6.1 percentage points when compared to natives. In Fort Lauderdale, where Jamaicans make up 3.9% of the population, Jamaicans should have an even greater chance of re-employment (8.8 percentage points more) when compared to natives in Fort Lauderdale. It should be noted, however, that the immigrant dummy is still positive and significant – immigrants in cities in which no co-ethnics reside still have higher re-employment probabilities than comparable natives.

On the other hand, larger ethnic networks imply significantly larger earnings losses for immigrants displaced from full time jobs and re-employed in full time jobs. As mentioned earlier, the earnings loss result is not completely surprising given the findings of Nee, Saunders and Sernau (1994) for Asian immigrants. Even the results of Wilson and Portes (1980) – Cuban immigrants in enclaves earn returns similar to Cubans earnings in the primary market[107] without culturally assimilating (e.g., acquiring English proficiency) – can be consistent with these findings. Lazear (1999) argues that larger ethnic communities lower pressure to assimilate. Thus, workers displaced from an enclave job may be disproportionately poor English speakers. It is well documented that English language proficiency affects wages in the general labor market (see McManus 1990 and McManus, Gould and Welch 1983). While there may be no penalties for poor English skills on the old job, poorer English-speaking immigrants (who are disproportionately in cities with large enclaves) will experience lower wages, if re-employment is not found within enclaves.

[107] The term primary market comes from theories of a dual labor market. The primary market contains good jobs while the secondary market contains less desirable or bad jobs.

I allow for differences by region of source country and include a set of interactions between immigrant type and the network variable (Table 3.4 columns 5 and 10), I find that Hispanics drive the positive results for immigrants' re-employment. No other group is significantly more likely to be re-employed than natives. Even after accounting for earnings quintile on the old job, Hispanics immigrants have larger earnings losses than natives, while Asian and European immigrants have lower earnings losses. This is consistent with the hypothesis that Hispanic immigrants, at least, have lower reservations wages and this is assisting them to obtain re-employment.

To summarize, the fact that even after accounting for the ethnic network size, immigrants are more likely to be re-employed than natives implies that networks do not fully explain the success of immigrants. Overall immigrants have better re-employment outcomes than non-immigrants even with no ethnic network. Nevertheless, the ethnic network analysis provides some interesting insight into the factors affect the success of displaced immigrants.

3.6 Conclusion

This work examines whether displaced immigrants face more difficult re-entry into the labor markets than natives. I find that immigrant status increases the likelihood of re-employment after exogenous displacement. In addition, immigrant status mitigates the earnings losses for those displaced from full-time jobs, and re-employed full-time. Taken together, the employment and earnings results are consistent with the hypothesis that immigrants who are displaced face a higher demand for their services than do observably similar natives. However, the lower earnings losses of immigrants can be explained by their concentration in the low end of the earnings distribution on the lost

job. Immigrants exhibit greater flexibility in their re-entry to the labor market as they are more likely to switch occupations to gain re-employment. Immigrants are not different in their propensity to become self-employed. Results of analyses suggesting that welfare reforms affecting immigrant participation in government income support programs or differences in non-labor income caused differences in search intensity for re-employment yielded inconsistent results.

Larger ethnic networks positively affect re-employment probabilities for immigrants, but this effect alone cannot explain the higher re-employment probabilities of immigrants relative to natives. On the negative side, larger ethnic networks increase earnings losses among currently full-time immigrant workers who were previously displaced from full-time jobs. Further exploration disaggregating immigrants by source region revealed results consistent with the idea that Hispanic immigrants have lower reservation wages than natives and are thus more likely to obtain re-employment, while Asian and European immigrants have higher reservation and are less likely to obtain re-employment by the survey date.

The immigrant population share is increasing. As such, knowledge of their assimilation and labor market experiences is important as the debate continues on whether immigrants are net contributors in their use of government income supports programs. This work documents a surprising and robust finding that immigrants displaced are more likely to be re-employed than natives. Further investigation of this feature may reveal policy prescriptions to help cushion the blow to increasing populations of displaced workers as technology allows for greater trade in goods and services and

perhaps international wage convergence. The findings suggest that if natives lower their reservation wages their re-employment prospects will increase after displacement.

Figure 3.1 Ethnic Population Share Distribution

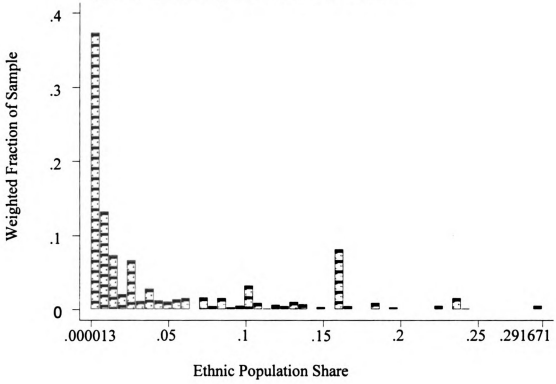

Table 3.1. Mean of Selected Variables by Immigrant Status.*

	Natives	Immigrants
N	**12490**	**1641**
Survey Date Employment	0.72	0.68
Weekly Earnings Old Job	646.35	541.36
Weekly Earnings Current Job	600.18	557.73
Change in Earnings (%)	-7.14%	3.02%
Hispanic	0.06	0.50
Female	0.45	0.42
Nonwhite	0.14	0.27
Education (median years)	13-15	12
Tenure on the lost job (years)	4.7	3.6
Age (years)	38	39
N**	**7587**	**935**
Weekly Earnings Old Job***	$671	$573
Weekly Earnings Current Job	$620	$565
Change in Log Earnings (%)	-10.2%	-1.4%

Table 3.2. Effect of Immigrant Status for Workers Displaced from 1993-2001.

	(1)	(2)	(3)	(4)
	Survey Date Employment		Change in Log Weekly Earnings	
Constant	0.705	0.716	0.054	0.018
	(0.020)***	(0.023)***	(0.034)	(0.044)
Immigrant	0.019	0.042	0.085	0.05
	(0.015)	(0.014)***	(0.034)**	(0.025)**
Hispanic	-0.039	-0.052	0.042	0.046
	(0.017)**	(0.015)***	(0.028)	(0.028)
Female	-0.066	-0.058	0.012	0.026
	(0.008)***	(0.007)***	(0.014)	(0.014)*
Non-White	-0.096	-0.109	-0.03	-0.028
	(0.013)***	(0.011)***	(0.021)	(0.022)
Less than High School Diploma	-0.084	-0.085	-0.038	-0.022
	(0.016)***	(0.013)***	(0.026)	(0.027)
Some College No Degree	0.042	0.049	-0.015	-0.008
	(0.010)***	(0.009)***	(0.017)	(0.016)
College Degree	0.079	0.086	-0.002	0.013
	(0.011)***	(0.010)***	(0.020)	(0.018)

* Indicates significance at 10%; ** 5%; *** 1%

Table 3.2 (contd.). Effect of Immigrant Status for Workers Displaced from 1993-2001.

	(1)	(2)	(3)	(4)
	Survey Date Employment		Change in Log Weekly Earnings	
Age 25 to 34 years	0.017	0.022	-0.02	-0.027
	(0.015)	(0.013)*	(0.026)	(0.026)
Age 35 to 44 years	-0.016	-0.01	-0.057	-0.057
	(0.015)	(0.013)	(0.027)**	(0.027)**
Age 45 to 54 years	-0.025	-0.027	-0.084	-0.086
	(0.016)	(0.014)*	(0.027)***	(0.028)***
Age 55 to 64 years	-0.189	-0.181	-0.099	-0.087
	(0.020)***	(0.017)***	(0.039)**	(0.035)**
Tenure 1 to 3 years	0.051	0.059	-0.003	0.013
	(0.011)***	(0.009)***	(0.022)	(0.018)
Tenure 4 to 10 years	0.057	0.057	-0.054	-0.046
	(0.012)***	(0.011)***	(0.023)**	(0.020)**
Tenure 11 to 20 years	0.066	0.06	-0.149	-0.146
	(0.016)***	(0.015)***	(0.037)***	(0.027)***
Tenure 20 + years	-0.039	-0.026	-0.214	-0.19
	(0.024)	(0.020)	(0.048)***	(0.039)***
2 years since Job Loss	0.182	0.175	0.035	0.033
	(0.009)***	(0.009)***	(0.017)**	(0.016)**
3 years since Job Loss	0.194	0.192	0.047	0.049
	(0.010)***	(0.009)***	(0.018)***	(0.016)***
Annual Unemployment Rate	-0.017	-0.021	-0.01	-0.011
	(0.003)***	(0.004)***	(0.003)***	(0.007)
Observations	14131	14131	6444	6444
R-squared	0.1	0.11	0.03	0.05
MSA fixed effects	No	Yes	No	Yes

* Indicates significance at 10%; ** 5%; *** 1%

Table 3.3. Coefficients from estimations investigating better immigrant outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Survey-Date Employment	Change in Log Earnings	Self-Employed	Changed Occupation	Survey-Date Employment		
Constant	0.549	0.268	0.013	0.206	0.545	0.546	0.564
	(0.021)***	(0.050)***	(0.019)	(0.034)***	(0.021)***	(0.022)***	(0.023)***
Quintile 2	0.527	-0.328	-0.044	-0.023	0.526	0.526	0.525
	(0.011)***	(0.026)***	(0.009)***	(0.016)	(0.011)***	(0.011)***	(0.011)***
Quintile 3	0.521	-0.495	-0.045	0.029	0.521	0.521	0.518
	(0.012)***	(0.027)***	(0.009)***	(0.017)*	(0.012)***	(0.012)***	(0.012)***
Quintile 4	0.539	-0.588	-0.043	0.099	0.54	0.54	0.536
	(0.012)***	(0.028)***	(0.009)***	(0.017)***	(0.012)***	(0.012)***	(0.012)***
Quintile 5	0.546	-0.815	0	0.106	0.547	0.547	0.542
	(0.012)***	(0.030)***	(0.010)	(0.018)***	(0.012)***	(0.012)***	(0.012)***
Immigrant	0.058	-0.03	-0.007	0.036			
	(0.012)***	(0.029)	(0.011)	(0.020)*			
Male Immigrant					0.085	0.086	0.083
					(0.016)***	(0.016)***	(0.016)***
Female Immigrant					0.037	0.036	0.007
					(0.017)**	(0.017)**	(0.018)
Female Native					0.012	0.011	-0.021
					(0.007)*	(0.007)	(0.010)**
Divorced, Widowed, Separated						0.011	-0.036
						(0.009)	(0.014)***
Never Married						-0.001	-0.027
						(0.009)	(0.012)**
Female * Divorced, Widowed, Separated							0.087
							(0.018)***
Female * Never Married							0.058
							(0.016)***
Observations	12488	4984	8416	8327	12488	12488	12488
R-squared	0.39	0.19	0.05	0.06	0.39	0.39	0.39

Standard Errors are in parentheses; * represents significance at 10%; ** 5%; *** 1%.

Table 3.4. Coefficients of Regressions Estimating Immigrant Heterogeneity.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Survey-Date Employment					Change in Log Earnings				
Immigrant	0.036	0.086		0.047		0.024	-0.048		0.003	
	(0.016)**	(0.016)***		(0.014)***		(0.035)	(0.035)		(0.032)	
Hispanic Immigrant			0.081		0.046			-0.171		-0.094
			(0.018)***		(0.022)**			(0.118)		(0.047)**
Asian Immigrant			-0.065		0.042			0.268		0.222
			(0.043)		(0.051)			(0.121)**		(0.110)**
Europe/Australia/			-0.165		-0.094			0.305		0.281
Canada Immigrant			(0.043)***		(0.051)*			(0.123)**		(0.112)**
Middle Eastern/			-0.131		-0.054			0.212		0.154
African Immigrant			(0.071)*		(0.084)			(0.173)		(0.175)
			-0.121		-0.068			0.015		-0.018
West Indian Immigrant			(0.039)***		(0.047)			(0.100)		(0.110)
Country Network				0.415					-1.221	
				(0.198)**					(0.432)***	
Hispanic Network					0.661					-0.537
					(0.224)***					(0.462)
Asian Network					-4.44					0.468
					(1.301)***					(2.938)
European/Australina/Ca					-1.732					-3.657
nadian Network					(3.348)					(8.290)
Middle Eastern/African					-3.376					13.419
Network					(21.204)					(55.971)
West Indian Network					-0.859					0.373
					(0.536)					(1.199)
Observations	11664	11829	12312	12312	12312	4730	4788	4984	4984	4984
R-squared	0.4	0.39	0.39	0.39	0.39	0.2	0.19	0.19	0.19	0.19

Standard Errors are in parentheses; * represents significance at 10%; ** 5%; *** 1%.

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Appendix A: Weekly Earnings Change by Immigrant Status: 1994-2002.

Table A3.1 Weekly Earnings Growth by Immigrant Status: 1994-2002.

Year	Natives		Immigrants	
1994	$495.40	-	$388.61	-
1995	$609.71	18.75%	$532.42	27.01%
1996	$668.11	8.74%	$589.56	9.69%
1997	$672.93	0.72%	$558.65	-5.53%
1998	$682.00	1.33%	$584.55	4.43%
1999	$701.55	2.79%	$611.96	4.48%
2000	$696.16	-0.77%	$599.65	-2.05%
2001	$732.53	4.97%	$638.17	6.03%
2002	$758.63	3.44%	$646.10	1.23%
Average Wage Growth		4.99%		5.66%
Cumulative Growth: 1994-2002		53.14%		66.26%

Using a sample limited to men and women ages 20 to 64, who are not in school or retired and report a desire to work, I calculate average weekly earnings from the CPS March files. Persons who are unemployed have weekly earnings of $0 in these calculations. Average annual wage growth is slightly higher for immigrants using this sample, but is not significantly different from natives. Average weekly earnings grew 66% from 1994 to 2000 for immigrants. While natives had a lower rate of growth, 53%, I must note that this is mainly due to changes in the unemployment rate. If I restrict the sample to exclude people who worked at least part of the year, immigrants have slightly lower weekly earnings growth. Nevertheless, the figure which this calculation is intended to be compared with includes displaced workers who are not re-employed at the survey date. Thus, excluding unemployed persons would be inappropriate.

Appendix B: Participation Rates in Government Income Support Programs and Access to Non-Labor Income

Table A3.2 Proportion of Weighted Sample Receiving Unemployment Insurance Benefits By Immigrant Status and Sex

	Survey Date Employment Status	
	Male	
	Employed	Not Employed
Natives	0.38	0.53
N	4,958	1,762
Immigrants	0.30	0.49
N	641	254
	Female	
	Employed	Not Employed
Natives	0.38	0.48
N	3,895	1,723
Immigrants	0.40	0.50
N	450	273

*Sample excludes non-responses

Table A3.2 uses the displaced worker sample in this paper and calculates usage rates of Unemployment Insurance benefit by immigrant status, gender and whether or not the worker was employed at the survey date. In addition to the above calculations, I utilize data in the CPS March files (1996, 1998, 2000, 2002) on income from public welfare, supplemental security income and unemployment insurance benefits to calculate participation rates of 20 to 64 year olds by immigrant status and sex. In a linear regression, controlling for age, sex, education, race, year of survey, weeks worked in the previous year, non-labor income in household and MSA fixed effects, I calculate relative usage rates of male immigrants, female immigrants and female natives compared to male natives in the top ½ of the Table. Table A3.3 columns (1) to (3) give coefficients indicating usage rates of Supplement Security Income, public welfare assistance and

unemployment compensation relative to male natives. Column (4) shows relative participation in at least one of the three programs. Column (5) uses non-labor income (an independent variable in the first four regressions) as the dependent variable. The second ½ of Table A3.3 gives the expected search intensity based on the value of relative income support available for each column. Program participation (or non-labor income) is expected to be negatively related to search intensity. So that in Panel A column (1) where female natives use public assistance in the greatest proportion, they are expected to be re-employed at the lowest rates. The sample is restricted to those who want to work and are not in school.

Table A3.3. Coefficients from Estimates of Government Income Support Use and Access to Non-Labor Income and Implied Search Intensity Rank.

	Panel A				
	(1)	(2)	(3)	(4)	(5)
	Public Assistance	SSI	UI Benefits	All	Non-Labor Income
Male Immigrant	-0.007	-0.002	-0.016	-0.026	-4,169.55
Female Native	0.014	0.001	-0.023	-0.009	11,694.16
Female Immigrant	0.008	-0.003	-0.032	-0.027	7,210.33
N	281,168	281,168	281,168	281,168	281,168
	0.07	0.01	0.04	0.08	0.14
	Panel B				
	Implied Search Intensity Rank Based on above Use Rates				
Male Native	2	3	4	4	2
Male Immigrant	1	2	3	2	1
Female Native	4	4	2	3	4
Female Immigrant	3	1	1	1	3

Notes: All estimates are significantly different from male natives and in most cases each other. The exception is that immigrants do not differ by sex in their SSI usage rates. This drives the overall usage of Government Income Supports so that there is no difference between immigrants by gender.