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War on Welfare: the Victors, the Victims and a New Welfare Regime

Economic Consequences of Welfare Reform

by

Neeraj Kaushal

**A dissertation submitted to the Graduate Faculty in Economics in partial fulfillment
of the requirements for the degree of Doctor of Philosophy,
The City University of New York**

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This manuscript has been read and accepted by the Graduate Faculty in Economics in satisfaction of the dissertation requirement for the Degree of Philosophy.

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Abstract

War on Welfare: the Victors, the Victims and a New Welfare Regime Economic Consequences of Welfare Reform

**by
Neeraj Kaushal**

Adviser: Professor Robert Kaestner

This dissertation consists of three papers. In the first paper, I investigate the effect of welfare reform on the employment, marriage rate and fertility of low-educated unmarried women. I find that welfare reform has induced low-educated unmarried women to move from welfare to work in significant numbers. I estimate that the imposition of time limits and other administrative reforms correlated with it increased the employment of unmarried women with 12 or fewer years of education by 484,239. This represents approximately 32% of the decline in welfare caseloads for this group since 1994. I also find that women who left welfare for employment worked approximately 30 hours per week, which even at low wages may significantly improve their financial status relative to public assistance. I find little evidence that welfare reform affected the fertility of low-educated unmarried women.

The second paper investigates the effect of the 1996 welfare reform on immigrants. Federal law banned legal non-citizens from receiving any federally financed means-tested benefits in their first five years in the US. However, a number of states restored some of these benefits. I study the effect of the state level variation in policies on the employment, hours worked and marriage rate of low educated women. The results suggest that welfare reform induced native-born citizens and foreign-born non-citizens to increase their employment. It appears to have had a

larger effect on the least educated native-born women and among foreign-born, a larger effect on more recent immigrants.

In the final paper, I study the effect of safety-net programs on the locational choices of new immigrants. I use a conditional logit model to obtain estimates of the effect of the policies of interest. I control for unmeasured, time-invariant characteristics of states that may be correlated with locational choice and benefit availability. To control for time-varying, unmeasured state characteristics, I use a difference-in-differences procedure. I find that safety-net programs do not affect the locational choices of new low-skilled women immigrants. This result is robust to a variety of specifications including those limited to the target group; difference-in-differences models with two different control groups; and models that take account of the number of programs available to immigrants and generosity of benefits.

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Chapter 1

Introduction

WAR ON WELFARE

Thirty-two years after Lyndon Johnson declared war on poverty, the US government declared another war, this time on welfare to “end welfare as we know it.” A 61-year-old welfare regime under Aid to Families with Dependent Children (AFDC) was abolished. A new program, Temporary Aid to Needy Families (TANF), replaced AFDC, and as the name suggests made welfare a temporary assistance, limited to a lifetime benefit of five years.¹ Welfare is no longer a lifelong entitlement, nor free. Under the new welfare regime, most adults who receive welfare have to work in return. Among other provisions, the 1996 Federal law gives strong financial incentives to low-income families to leave welfare for work. It also provides child and health care support to ease the transition.

Johnson’s war was to “empower” the poor by giving them cash and non-cash assistance. War on welfare was designed to reduce welfare use and promote self-sufficiency. Its objectives were to inculcate values of work and personal responsibility. War on poverty led to expansion in social welfare programs for the poor. The Federal government enacted several new programs such as Medicare, Medicaid, Food Stamps and Supplemental Security Income. By 1995, there were 300 social programs for the poor, and the state and federal spending on these programs had increased from around \$40 billion to \$360 billion.² War on welfare, on the other hand, has sought to integrate the indigent with the mainstream of the society by giving them incentives to work, making work more remunerative and discouraging long-term public assistance. Its first

¹ Federal Law exempts each state up to 20 percent of its average monthly caseload from time limit on assistance.

² Working Toward Independence. (www.whitehouse.gov/news/releases/2002/02/welfare-reform-announcement-book.html)

significant victory is an impressive decline in the number of families on welfare. Between March 1994 to September 2001, welfare caseload fell by 60 percent.

THE VICTORS

War on welfare has been an instant political success. Politicians from both sides of the aisle have been trying to claim credit for welfare reform. Democrats consider welfare reform to be one of the biggest achievements of the Clinton Presidency. The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) was introduced by a Republican-majority Congress, and Republicans assert that the political credit for welfare reform is rightfully theirs.

The impressive political success of welfare reform underscores the failures of the old welfare regime. In its six decade long existence, welfare under AFDC clearly failed to alleviate poverty. In fact, child poverty rose between the 1970s and early 1990s.³ Far from reducing poverty, if the growing public opinion on the issue is to be believed, welfare became its cause. Prior to welfare reform, the American public had come to believe that welfare discouraged poor parents to work, led to a decline in the number of children reared in two parent families and an increase in out-of-wedlock birth (Weaver 2000, Swahill and Haskins, 2002). War on welfare sought to change all this by introducing policies that were expected to inculcate a work ethic and transform behaviors that cause dependency such as non-marital birth. Among other initiatives, federal law strengthened rules to establish paternity, reformed child support enforcement program,

³ It is possible to argue that in the absence of AFDC, child poverty may have increased even further.

made it mandatory on teen-mothers to attend school and live with their parents in order to receive welfare benefits.

Enacted and implemented in the midst of a decade long economic boom, war on welfare exhibited initial signs of success. Between March 1994 and September 2001, three million families have moved out of welfare. That most of them did so even before the federal five-year limit forced them to leave welfare is evidence that welfare reform successfully induced former recipients to give up a life of dependency on state assistance. A large number of welfare leavers have started working. Working Towards Independence, the TANF reauthorization proposal of the Bush administration reviews state-level evaluation studies from more than 40 states. According to the review, on average 60 percent of mothers who leave welfare are employed at any given moment, and over a period of time more than 80 percent are employed. The income of bottom 40 percent of single mothers (total income under \$21,000) rose by over 25 percent between 1993 and 2000, according to the review.

There was widespread fear that welfare reform would make the poor poorer and increase child poverty. Instead, according to the US census bureau, in 2000, poverty rate dropped to 11.3 percent, almost equal to the record low level of 11.1 percent set in 1973. The decline in child poverty was even more impressive – it fell from 22.7 percent in 1993 to 16.2 percent in 2002.

Three factors explain the economic achievements of the poor during the nineties. First, the ten-year long economic boom, which opened economic opportunities for everyone including the poor. Second, welfare reform that made welfare an inferior option in comparison to work. And third, a plethora of work support programs such as Earned

Income Tax Credit, minimum wage legislation, state income supplement programs that made work more remunerative than welfare (Swahill and Haskins, 2002). The last two factors are an integral part of the strategy to win war on welfare.

THE VICTIMS

For most families the transition from welfare to work is unlikely to be smooth. The fragile families that are making an attempt to become economically self-sufficient are likely to be hit more severely by a recession than those on a higher income ladder. Firstly, if they lose their jobs, unlike the rich, the poor do not have a cushion of savings to fall upon. Secondly, many of those who have made the transition from welfare to work have not worked long enough to become eligible for unemployment insurance. Statistics from the Administration for Children and Families show that the 2001 recession hit the poor adversely. The decline in caseloads that began in March 1994 was arrested in July 2001. In the 2001 recession, the number of families on TANF increased by 19,020 during July to September 2001, the latest month for which the data are available.

Worrisome though the recent increase in caseloads is, the 2001 recession has not wiped out the impressive progress made by former welfare recipients in the labor market since the passage of PRWORA in August 1996. Employment data released by the Bureau of Labor Statistics show that over 1.2 million single mothers who maintain families started working between August 1996 and December 2001. Single mothers, who make up less than six percent of the labor force, grabbed over 20 percent of the six million new jobs created during this five-and-a-half year period. It should be added that the 2001

recession was relatively mild. A more severe recession could hit the indigent more severely.

The real victims of welfare reform, however, are those who have failed to make a transition from welfare to work. As of September 2001, there were 2.1 million families still on welfare. Soon many of them will be hit by federal time limits. These are the people who have not benefited from welfare reform or the several Federal and state work-support programs. Additional effort would be required to improve their labor market skills. State governments will have to decide whether they should use state funds to provide welfare to those in dire need beyond the five-year limit, or should these poor families be pushed off the welfare rolls. Expert opinion differs widely on this issue. In Haskins and Blank (2002), Haskins strongly supports time limits “as a way to send the message that public assistance is not a long-term entitlement for anybody.” (page 28). Blank, on the other hand, recommends abolition of the Federal time limits. Her second best policy prescription is to “stop the federal time-limit clock when families are working 25 or more hours per week.” (page 28).

The second group at risk of being adversely affected by welfare reform is the children of unmarried mothers who have begun working. As mentioned earlier, census data show that child poverty has fallen by 29 percent between 1993 and 2000. We still need to ask the question: are the children of single mothers better off if their mothers work? Who is taking care of the children? Working mothers report that they have problems finding satisfactory childcare (Jencks and Swingle, 2000). Paxson and Waldfogel (2001) find that strict lifetime welfare limits and tougher sanctions for noncompliance have resulted in higher levels of child maltreatment. To enable single

parent families achieve long-term economic independence, state governments should strengthen childcare provisions.

The third group that has been adversely hit by PRWORA consists of legal non-citizens, especially those who arrived in the US after August 1996. PRWORA restricted non-citizens' eligibility to public assistance programs such as cash benefits, Food Stamps, Supplemental Security Income (SSI) and Medicaid. Backlash against the initial law forced the government to restore Food Stamps and SSI benefits for children, elderly and disabled immigrants. PRWORA also gave state governments the freedom to formulate state level welfare policies, and many states have restored benefits to new immigrants. As a result, the present welfare policy towards immigrants is a mellowed down version of the initial policy (Zimmerman and Tumlin 1999). President Bush has also proposed restoration of food stamps to legal immigrants who have lived in the country for five years. Despite these concessions, however, unless low educated non-citizen mothers make a transition from welfare to work or become citizens, welfare reform would affect them adversely.

GOALS UNACCOMPLISHED

Welfare reform had two primary goals: reduce welfare use by inducing welfare recipients to work, and discourage behaviors that lead to state dependency such as non-marital births. It has successfully helped a large number of former welfare recipients to move from welfare to work (CEA 1999, Kaushal and Kaestner 2001, O'Neill and Hill 2001). However, there is no consistent evidence that welfare reform has been able to reduce non-marital births or increase marriage (Kaushal and Kaestner 2001, Joyce,

Kaestner and Korenman, 2002). The failure of welfare reform to attain this objective is an issue of heated debate among conservatives and liberals. When Congress re-authorizes TANF, conservatives are likely to push state governments to impose stricter restrictions to address these issues. Democrats, on the other hand, may oppose such restrictions.

Lack of clear evidence on the effect of welfare reform on non-marital births raises several important questions. Is welfare, as commonly believed, a cause of out-of-wedlock fertility? If the answer is no, welfare reform cannot be expected to reduce out-of-wedlock fertility. The society will have to try other means to attain this objective. If, on the other hand, welfare is one of the underlying causes of high non-marital births, the questions that need to be asked are: one, is it the job of the government to promote marriage and reduce non-marital births? Two, did welfare reform address the issue of growing non-marital pregnancy and non-marital births adequately? Both questions raise contentious issues and it would be difficult to reach a consensus on either.

Chapter 2

From Welfare to Work: Has Welfare Reform Worked?

Main Findings

In this paper, I obtain estimates of the effect of welfare reform, as measured by the imposition of time limits, family cap provisions, work requirements and sanctions for non-compliance, on the employment, marriage rate and fertility of low-educated unmarried women. My analysis shows that welfare reform has induced low-educated unmarried women to move from welfare to work in significant numbers. I estimate that the imposition of time limits and other administrative reforms correlated with it have increased the employment of unmarried women with 12 or fewer years of education by 484,239. This represents approximately 32% of the decline in welfare caseloads for this group since 1994. I also present evidence that women who have left welfare for employment worked approximately 30 hours per week, which even at low wages may significantly improve their financial status relative to public assistance. The analysis further shows that welfare reform increased the marriage rate of women who did not have a high school degree. Finally, I find little evidence that the imposition of various aspects of welfare reform affected the fertility of low-educated unmarried women.

INTRODUCTION

Recent welfare reform has been a unique social experiment intended to bring the poor into the mainstream of the economy. In the past, most anti-poverty policies involved putting more funds into programs meant for the poor. Welfare reform, on the other hand, has sought to reduce the role of cash assistance. Its aim is to eliminate the need for direct cash assistance by reducing economic dependence on the state through increased employment and changes in behaviors relating to marriage and out-of-wedlock childbirth that may cause or increase dependency.

Statistics indicate that welfare reform has achieved considerable success in attaining its objective of reducing state dependency. Between March 1994 and September 2001, welfare caseloads fell by 61 percent. Although federal law to “end welfare as we knew it” was passed in August 1996 (Personal Responsibility and Work Opportunities Reconciliation Act-PRWORA), a number of states began reforming welfare in the early nineties. Welfare caseloads started to decline soon thereafter, at a modest pace in the beginning, but as more states adopted reform, welfare rolls fell faster. After the federal legislation was passed, the welfare caseload declined even more precipitously. Between August 1996 and September 2001, 2.3 million families left the welfare rolls, almost four times the number that moved out of welfare between January 1994 and August 1996. In 1994, 5.5 percent of the US population was dependent on welfare. Seven years, and a series of state and federal welfare reforms later, the proportion has shrunk to 2.1 percent.

The shrinking tally of welfare recipients has raised concerns about the plight of indigent single mothers and their dependent children, the main beneficiaries of welfare or what is now called Temporary Aid to Needy Families (TANF). Social scientists are

asking the following questions. Has welfare reform helped or hurt the poor? Are welfare beneficiaries leaving welfare for work or are they joining the unemployed? What has motivated the exit from welfare: the strong economy or the administrative policies that encourage work and other behavioral changes? The last question is particularly important because its answer foretells what will happen during less hardy economic times like the present one. If the economy is responsible for the decline in caseloads and growth in employment, any macroeconomic setback will leave many former welfare recipients in financial hardship, particularly those who have exhausted their lifetime benefits. On the other hand, if welfare reform has caused former welfare recipients and potential welfare recipients to invest in employment and employment related skills and to alter behaviors that create economic dependency, a recession will not have as adverse a consequence as predicted by opponents of welfare reform.

Former welfare recipients, the fragile families that are making an attempt to become economically self-sufficient, are likely to be hit more severely by a recession than those on a higher income ladder. Firstly, if they lose their jobs during a recession, unlike the rich, the poor do not have a cushion of savings to fall upon. Secondly, many of those who have made a transition from welfare to work have not worked long enough to become eligible for unemployment insurance. Statistics from the Administration for Children and Families show that the present recession has hit the poor adversely. The decline in caseloads that began in March 1994 was arrested in July 2001. In the 2001 recession, the number of families on TANF increased by 19,020 during July to September 2001, the latest month for which the data are available.

Worrisome though the recent increase in caseloads is, the 2001 recession has not wiped out the impressive progress made by former welfare recipients in the labor market since the passage of PRWORA in August 1996. Employment data released by the Bureau of Labor Statistics show that over 1.2 million single mothers who maintain families started working between August 1996 and December 2001. Single mothers, who make up less than six percent of the labor force, grabbed over 20 percent of the six million new jobs created during this five-and-a-half year period. Evidence from studies of those who exit welfare, or what have come to be known as "leaver" studies, is consistent with the increase in employment among single mothers. In their review of such studies, Brauner and Loprest (1999) report that between 53 and 70 percent of women who recently left welfare were employed and that their employment tended to be continuous (70 to 80 percent of the time) and substantial in terms of hours worked per week (over 30 hours per week). Moreover, Cancian et al. (1999) find that after adjusting for inflation both earnings and family income of welfare leavers increase over time.

As noted, one key question is whether the movement from welfare to work is largely the result of a booming economy, or a result of the welfare reform law? Statistics describing the growth of employment among single mothers are unable to distinguish between the two causes and recent research has not reached a consensus on this issue. An early paper by the Council of Economic Advisers (CEA , 1997) attributes somewhat less than a third of the decline in caseloads between 1993 and 1996 to state welfare reform and slightly more than a third to economic growth. In contrast, Ziliak et al. (2000) attributes less than a sixth of the decline during this period to state welfare reforms. A more recent paper by the CEA (1999) reports that a third of the post 1996

decline in welfare caseloads is due to welfare reform and only 8 to 10 percent is due to economic expansion and associated improvement in employment conditions. Schoeni and Blank (2000), however, conclude that the federal reform law passed in 1996 had little effect on work behavior and that most of the employment growth was due to the buoyancy in the economy.

As this brief review has indicated, the evidence is mixed as to the relative importance of welfare reform vis-à-vis economic expansion in reducing welfare caseloads. More importantly, there are few papers that examine the effect of welfare reform on employment and the contribution of welfare reform induced employment growth to changes in the caseload. It is important to differentiate between studies that examine changes in the caseload and studies that examine changes in employment. While the two outcomes are linked, changes in employment are not the converse of changes in caseload. It is interesting from a policy point of view to investigate how much of the change in the caseload can be explained by changes in employment. Leaver studies address this issue directly, but these studies cannot provide direct estimates of the effects of welfare reform since some women who exited welfare would have done so even in the absence of reform. Furthermore, studies of women who exit welfare do not provide information on changes in entry into the welfare system.

In this study, I focus on the question of how welfare reform has affected employment and my analysis captures both entry and exit effects. Thus, this study differs from leaver studies and from several recent analyses that have focused only on changes in the caseload.¹ In addition, I examine how welfare reform (waivers under Aid to Families

¹ See for example Grogger (2000), Wallace and Blank (1999), Figlio and Ziliak (1999), Ziliak et al. (2000), and the Council of Economic Advisors (1997, 1999).

with Dependent Children (AFDC) and TANF) has affected fertility and marriage rates, a question that few studies have investigated, even though fertility and marriage are other potentially important source of changes in the caseload.

Finally, I investigate a number of other issues raised in the literature such as whether welfare reform affected mothers with younger children more than those who have older children and whether women with a high school degree are affected more than are women without a high school degree. I also investigate whether the employment effects of welfare reforms are different for women living in central city areas as compared to those living in outside central city areas, as the two groups face different labor market conditions.

Another difference between this analysis and previous studies relates to methodology. Most previous studies use econometric specifications that rely on time trends, and contemporaneous and lagged measures of economic variables (e.g., unemployment rate) to account for the influence of non-policy related variables on caseload and employment changes. This strategy is problematic as evidenced by the controversy associated with the first CEA (1997) study on the effect of welfare waivers (see Ziliak et al. 2000, Figlio and Ziliak 1999 and Moffitt 1999). Moreover, Wallace and Blank (1999) note that the conventional econometric specifications do a poor job of explaining changes in welfare caseloads in the early 1990s, providing yet more evidence of the sensitivity of estimates to specification issues. In this analysis, I adopt a somewhat different empirical strategy. Similar to Meyer and Rosenbaum (1999), I use a difference-in-differences methodology to identify the effect of welfare reform on employment.² The

² Grogger (2000) also uses a difference-in-differences approach using families with older children as controls for families with younger children. Schoeni and Blank (2000) combine what I have referred to as

difference-in-differences procedure provides an alternative, and arguably preferable, way of controlling for the influence of non-policy related changes in employment, marriage rate and fertility, the outcomes I study in this paper.

WELFARE POLICY

I focus on four key aspects of welfare reform under AFDC (welfare prior to August 1996) and TANF: time limits on cash benefits, family cap provisions, sanctions on cash benefits if failed to comply with work requirements, and exemptions from work requirements if the recipient has a child under a certain age. Federal law imposes a maximum lifetime benefit limit of five years for each family, but 19 states have lifetime limits shorter than five years.³ Also, all but two states (Michigan and Vermont use state funds after five years to replace federal money) have had a time limit since the implementation of TANF, but many imposed these limits before the federal law was passed. Federal law does not require the imposition of a cap on welfare benefits if a child is born while a family is on welfare, but nineteen states do not provide for additional benefits in such circumstances, and many of them imposed family caps prior to the 1996 federal reform. Similarly several states started imposing sanctions for non-compliance with work requirements under AFDC. The severity of these sanctions differs across states. Some resort to partial denial of benefits on non-compliance, others deny all benefits. Some impose partial denial on first time offenders and full sanctions on those who do not comply with work requirements repeatedly. A few states always impose

an econometric approach with a difference-in-differences approach. I comment on the merits of this approach below.

³ The Federal Law exempts each state up to 20 percent of its average monthly caseload from time limit on assistance.

partial sanction, and others deny all benefits even to first time offenders. To capture the severity of sanctions, in my analysis, I define a variable “sanctions” equal to 1 if a state imposes at least partial sanctions on first offense and full sanctions on repeated offenders. For states that do not impose any sanctions or always impose partial sanctions, the variable is equal to 0.

Policies regarding exemptions from work requirements also differ across states. Five states give no such exemptions. Others give exemptions if the age of the child is below a certain minimum, which again differs across states. In this analysis, I define a variable called “low exemptions” if a state allows exemptions from work only if the youngest child is less than six months of age or if the state does not allow any exemptions.

A key difference between the 1996 federal welfare reform and most state level waivers implemented prior to 1996 was that the former transformed a lifetime guarantee for cash benefits to a time limited benefit. I consider time limits to be the most potent tool of recent welfare reform. Therefore, I want to study the effect of time limits on all the outcomes of interest in this paper: employment, hours worked, fertility and marriage rate. To do so, I construct two policy variables: time limits and other reforms. “Time limits” is equal to 1 if the state has a time limit, otherwise 0. “Other reforms” is equal to 1 if the state has any of the other three policies – family cap, sanctions and low exemptions, but no time limits, otherwise 0. This analysis, therefore, investigates the effect of time limits plus the other correlated reforms, and the effects of other reforms in the absence of time limits.

As stated most states (with the exemption of Michigan and Vermont) now provide cash benefits on a time limited basis. However, few welfare recipients were hit by the time limits during the period of my investigation. Despite that the pressure on welfare recipients to reduce their dependency on welfare is likely to grow as the welfare clock starts ticking and as time limits get nearer. To see if the actual limit has compelled low skilled mothers to make the transition, I also study the effect of years since time limits imposed, or what I call welfare clock, on all the outcomes of interest

The outcome of welfare reform largely depends on how rigorously the law is implemented. Since TANF allows states considerable discretion, the intensity of implementation of welfare reform has not been uniform across states. To address this issue, I develop an index of the intensity of reform. I create five dummy variables: no reform, low intensity reform, only time limits, medium intensity reform and high intensity reform. No reform is a situation in which a state has none of the four policies studied here. Low intensity reform is defined as a situation in which a state does not have any time limits, but has one or more of the other three policies – low exemptions, sanctions and family cap. As the name suggests, only time limits is equal to 1 for states that impose only time limits but none of the above three policies. Medium intensity reform is a situation in which a state has one or two of the above three policies (but not all three) along with time limits. High intensity reform is a situation in which a state has all the four policies.

There are other aspects of welfare reform. I choose the above mentioned four policies as they are easy to define. To the extent the other aspects of welfare reform, such as earnings disregard, are correlated with these four policy variables, we need to be

cautious in our interpretation of the results. All policy variables are measured as of the date they became effective.⁴

DATA AND ANALYSIS SAMPLES

I use the Census Bureau's March series of Current Population Surveys (CPS) that is based on a nationwide sample of approximately 62,500 housing units interviewed each year, for the period 1992-2000. This is a period that covers all the recent federal and state welfare policy changes. The March CPS contains information on important individual characteristics such as age, education level, ethnicity, race, personal earnings, state of residence; family characteristics such as family size, number of children under age six, number of children under age 18, family income and its composition. I control for these factors to distil the effect of welfare reform on the employment, hours worked, marriage rate and fertility of low skilled women. This data set also gives information whether a woman had a child in the last one year, the outcome of interest in the fertility analysis.

Employment Sample

I first investigate the labor market experience of less-educated unmarried women with children, potential candidates for welfare benefits. As I describe in more detail below, I use a difference-in-differences (DD) methodology to obtain estimates of the effect of welfare reform on employment. The DD procedure compares the changes in employment before and after welfare reform of two groups of women, those most and

⁴ I used a variety of data sources to define the policy variables including the 1999 CEA report, information from the National Governors Association and summaries of legislation maintained by the Agency for Children and Families of the Department of Health and Human Services.

least likely to be affected by the welfare reform law. Therefore, I need to select two groups of women who are similar so the comparison is meaningful, but who differ in the way welfare reform affects them. Accordingly, I limit the sample to women between the ages of 18 and 44 since approximately 80 percent of all women on welfare fall into this age group. Second, I limit the sample to women with 12 or fewer years of education; again because approximately 80 percent of welfare recipients meet this criterion. I define a target group, the group most likely affected by welfare reform, consisting of unmarried women with children in this broader group, which is appropriate given that more than 80 percent of welfare recipients are unmarried mothers. I use two comparison groups: married women with children and unmarried women without children. Both groups are unlikely to be affected by welfare reform, although some married women will be eligible for welfare. I use two different comparison groups to test whether the estimates are sensitive to this choice.

The selection of target and comparison groups using marital status and presence of children is not innocuous. The advantage of this approach is that I identify a target group in which a large majority of women are at risk of public assistance receipt and a comparison group that has similar education level and child rearing responsibilities, two very important correlates of labor market behavior. The disadvantage is that welfare reform may affect fertility and marriage, and thus we could confound estimates of the effect of welfare reform with changes in sample composition. For example, if welfare reform decreases fertility and increases marriage, then the before and after difference we focus on is the sum of three effects: welfare reform, other time-varying factors that affect outcomes, and changes in sample composition. I try to account for the effect of other

factors by subtracting out the before and after difference of the comparison group. Thus, the estimate of the effect of welfare reform may be biased, assuming the validity of the comparison group, by changes in sample composition.

I partly address this concern by modifying the choice of target and comparison groups. Specifically, I use the same sample of low-educated women, but use only marital status to define target and comparison groups: unmarried women are the target group and married women are the comparison group. This strategy eliminates the potential bias from changes in composition due to welfare reform induced changes in fertility and allows me to assess the importance of this issue.

An alternative to the above strategy is to follow Schoeni and Blank (2000), who use low-educated women as the target group and high-educated women as a comparison group. I do not believe this to be an effective strategy for two reasons. First, the target and comparison groups are highly contaminated. Most of the women in the target group are not in fact at risk of welfare receipt and are therefore unaffected by welfare reform (e.g., most of the married women) and a significant number of women in the comparison group are at risk of welfare receipt (e.g., unmarried women with children) and are therefore affected by welfare reform. This problem can lead to severely downward biased estimates of the effect of welfare reform.⁵ Second, I do not believe that high-educated women are a good comparison group for less-educated women—i.e. the

⁵ The magnitude of the bias is $(\alpha - \beta)$, where α is the proportion of women in the target group at risk of being affected by welfare reform, and β is the similar proportion in the comparison group. If, for example, $(\alpha - \beta)$ is 0.15 (0.25-0.1), which is not unlikely for the target and comparison groups used by Schoeni and Blank (2000), the resulting estimate will be only 15 percent as big as the true estimate. The downward bias also implies that it is necessary to use a very large sample since the effect of interest is quite small.

unaccounted for variation over time in the employment of less educated women is not similar to that of women with higher education.⁶

I am not merely interested in the effect of welfare reform on the employment of the treatment group, but also whether the women in this group are economically better off after the policy changes. For that I also investigate the hours worked of women who started working as a result of welfare reform. Given their relatively poor job market skills, it is important to ask whether women in the treatment group can compete in the job market. One way to answer this question is to see if former welfare recipients are moving from state subsidy to state-supported employment. To answer this question, I study the effect of welfare reform policies on the employment of women in the public and the private sector.

Labor market conditions in central city areas differ markedly from the labor market conditions in areas outside the central city. Although job opportunities outside of the central city areas are aplenty, low-educated unmarried women with children encounter other obstacles such as lack of public transportation and proper childcare facilities. Therefore, I am interested in studying the effect of welfare policies on less-educated mothers living in central city and non-central city areas. For the analysis on central city areas, I restrict the treatment and comparison groups to women living in central city areas, and similarly for the analysis on non-central city areas, I restrict the sample to women living outside the central city areas. The rest of the analysis in this

⁶ Schoeni and Blank (2000) include linear time trends in their model to control for state-specific and group-specific (by education category) trends. However, their data cover nearly twenty years, during which there were considerable policy changes (e.g., Earned Income Tax Credit) and macroeconomic effects that make it unlikely that a linear trend will be sufficient.

section is similar to the employment analysis that covers both central city and non-central city areas.

A caveat of the analyses on central city and non-central city areas is that if welfare policies induced potential welfare recipients to migrate from central city to non-central city areas (or vice versa), the estimated coefficients of my analysis would be biased. For instance, if welfare reform increases the flow of the treatment group from central city to non-central city areas, the before and after change in employment would be the sum of three effects: welfare reform, other time-varying factors that affect employment and changes in sample composition due to migration. Assuming that the comparison group is valid, the estimate of the employment effects of welfare reforms in non-central city areas (as well as in central city areas) would be biased by the changes in sample composition. Other research (Kaestner et. al. 2002) shows that welfare reform induced less-educated unmarried mothers to move from central city to non-central city areas, and a large degree of this migration was employment induced. In the light of this research, my estimates of the effects of welfare reform on the employment of women in central city and non-central city areas should be interpreted with caution.

Marriage Sample

One of the objectives of welfare reform has been to increase the existence of two parent families. Has it been able to achieve this objective? To investigate this issue the next outcome of interest is the marriage rate of low-educated women. Obviously, marital status cannot be used to define the treatment and comparison groups in this analysis. For this analysis, the treatment group is all women with high school or less education, and I

use all women with 13-15 years of education as the control group. Again, both the treatment and the control groups are restricted to 18-44 years age group. I am also interested in studying whether welfare policies have induced low-educated mothers to marry. To investigate that I restrict the treatment and comparison groups to women with at least a child.

Fertility Sample

The third outcome of interest is fertility. There is extensive economic literature on the impact of AFDC on the fertility of unmarried women (Moffitt 1992, 1998). In his review, Moffitt (1998) concludes that the evidence provided by previous research generally indicates that government cash assistance affects fertility, but that the magnitude of that effect is unknown. He notes, however, that this conclusion reflects more recent research and is subject to significant qualification due to a number of studies that found no such effect. Given the lack of consensus on this issue, I consider it appropriate to study the impact of welfare reforms on the fertility of unmarried mothers.

For this analysis, I focus on the fertility of all unmarried women with 12 or fewer years of education. The target group for this analysis is expanded to include unmarried women without children because many of these women are at risk of having a child out of wedlock and going on welfare, and welfare reform may influence their fertility decisions. The comparison group is married women with 12 or fewer years of education. To test the sensitivity of the results to choice of comparison group, I also use unmarried women with more education (13 to 15 years) as a comparison group. Obviously, the same remarks made with reference to the target and comparison groups used in the

employment analysis apply in this case. There is a tradeoff between choosing target and comparison groups that are less contaminated and the possible bias related to compositional changes.

RESEARCH STRATEGY

I first analyze the impact of various aspects of welfare reform on the employment and hours of work of unmarried women with children. As noted, I use a quasi-experimental research design commonly referred to as a difference-in-differences (DD) analysis. The DD procedure compares the change over time in the employment of a group affected by welfare reform, the target group, to the change over time in the employment of a similar group that is unaffected by welfare reform, the comparison group.

A critical assumption underlying any ‘difference-in-differences’ analysis is that unmeasured, time-varying factors influence the target and comparison groups equally. This may not be a valid assumption and therefore I test the sensitivity of my results by using two different comparison groups. My first comparison group consists of married women with children who have 12 or fewer years of education. While this appears to be a reasonable comparison group, my estimates may have a bias because a small proportion of women in the comparison group may be receiving or at risk of receiving welfare benefits because of the AFDC-UP program. The distribution of TANF caseloads by marital status shows that among those whose marital status is known, approximately 18 percent are married couples. The other problem associated with using married women with at least one child as the comparison group is the necessity to control for family (non-

earned) income, which according to economic theory is jointly determined with a woman's labor force participation, and therefore, potentially endogenous.⁷ My second comparison group, unmarried women without children, does not suffer from these two problems. This group falls outside the scope of TANF, and therefore, does not receive any treatment. However, this group may have different labor market experiences because of the absence of children in the household. I report the results obtained using both comparison groups.

The model specification used in the analysis is as follows:

(1)

$$LFP_{ijkt} = X_{ijkt}\beta_x + Z_{jt}\beta_z + \beta_t Y_t + \alpha_1 \text{Treat}_k + \alpha_2 \text{Timelimit}_{jt} + \alpha_3 [\text{Treat}_k \times \text{Timelimit}_{jt}] + \alpha_4 \text{Other Reform}_{jt} + \alpha_5 [\text{Treat}_k \times \text{Other Reform}_{jt}] + e_{ijkt}$$

where LFP_{ijkt} is a measure of labor force attachment for woman 'i' in state 'j' with marital status 'k', in year 't.' I use two measures of labor force attachment: employment in the week prior to the March CPS survey and hours of work in the week prior to the March CPS survey. The independent variables in equation (1) include a vector of personal characteristics, X_{ijkt} , such as woman's age, race, number of children below age six, number of older children, and non-earned family income (including husband's earnings if married)⁸. I specify age using five age dummy variables: 18-22 years, 23-27 years, 28-32 years, 33-38 years and 39-44 years.

⁷ The same unobservable factors that affect the LFP of a woman – for instance, whether her husband is working or not-- also influence her 'other income.'

⁸ I define other family income in two ways: Other Income I = Family Income – Individual's Earnings and Other Income II = Family Earnings – Individual's Earnings + Net Rent + Dividend + Interest Income. "Other Income I" assumes all family income other than the individual's earnings is exogenous, including earnings-conditioned transfer payments (which is definitely endogenous). "Other Income II" assumes others' earnings are exogenous to the woman's employment, that is, not jointly determined with hers. This is not unreasonable if the family members' labor supply decisions are viewed as sequential, with the woman conditioning her labor supply decision on others' predetermined earnings. The regression results,

The variable $Treat_k$ in equation (1) denotes membership in our target group and therefore equals one if woman 'i' is a single mother with 12 or fewer years of education, and zero otherwise. $Timelimit_{jt}$ denotes that state j imposed a time limit on welfare benefits in year t, and zero otherwise. $Other Reform_{jt}$ equals one if state j has any of the following policies - family cap, sanctions or low exemptions, as defined earlier, but no time limits in year t, and zero otherwise. Also included in equation (1) are Y_t , a vector of year dummy variables, and Z_{jt} , which is a vector of state characteristics such as state dummy variables and state-specific unemployment rates.⁹ I include contemporaneous and two lags of the aggregate state unemployment rate and interact these variables with the variable $Treat_k$.

In equation (1), coefficient α_2 measures the effect of time limits on the comparison group (when $Treat = 0$). Since the comparison group is assumed to be unaffected by welfare reform, α_2 controls for unmeasured determinants of employment that are correlated with the policy variable and that may confound estimates of its effect. Coefficients α_3 and α_5 measure the effect of time limits (α_3) and other reform (α_5) on the employment of the target group holding constant unmeasured time-varying factors correlated with welfare reform.

A strength of the difference-in-differences analysis is that it controls for time variation in outcomes that are unrelated to welfare reform. This of course is true only if the comparison group is appropriate. This suggests that it is not as crucial in a DD analysis to control for macroeconomic activity as it is in other approaches. To be

however, are unaffected by the definition of 'other income' I use. Hence, I report results with Other Income II.

⁹ I use the unemployment rate of men and women combined throughout our analysis.

cautious, however, my regression model includes controls for macroeconomic activity, specifically the state unemployment rate in the month of the survey, and one- and two-year lags of the average annual unemployment rate. I also interact the unemployment rates with the dummy variable indicating membership in the target group. Interestingly, the estimates associated with the unemployment variables are rarely significant, either individually or as a group. This result provides some evidence that the comparison groups are well chosen.

Another way to test the validity of the comparison group is to obtain difference-in-differences estimates for a period during which there was no policy change. Under these circumstances the difference-in-differences estimates of the effect of (pseudo) policy should be zero. I chose the period between 1988 and 1992 to implement such a test because it predates welfare reform and, importantly, expansion of the earned income tax credit. I estimated a model similar to equation (1) using two years of data, 1988 and 1992, and treating 1992 as the reform year ($\text{Reform}=1$).¹⁰ I estimated this model twice, using the two comparison groups noted above. In both cases, the results indicated that there was no statistically significant difference in the change in employment between the treatment and comparison groups. Nor was there an economically important difference as the coefficients were very close to zero. Note that this was a period in which econometric models did a poor job of explaining caseload behavior and a period when employment rose and fell over the business cycle (Wallace and Blank 1999). While the critical assumption underlying the difference-in-differences analysis cannot be tested definitively for the period of interest, this does provide some additional evidence that my comparison groups are appropriate.

I estimate several variants of equation (1) to test specific hypotheses described in the introduction. To study whether unmarried mothers with a high school degree are affected by reform differently from unmarried mothers with less than high school education, I estimate equation (1) separately by education level. There are a variety of reasons why welfare reform may have a different effect by education level. For example, unmarried mothers without a high school degree are likely to earn less than unmarried mothers with a high school degree. Consequently, some women who have less than high school education may find the opportunity cost of work (paying for baby-sitting, transport cost, etc) too high to take a job, and they may like to use their entire lifetime benefits of five years before deciding to work. Indeed, I find this distinction important, as an F- test rejects the specification that does not distinguish between the two groups: those with a high school degree and those without it.

Similarly, unmarried mothers with young children may react to welfare reform differently than women with relatively older children. First, the opportunity cost of work is higher for women with younger children than for women with older kids. Second, under AFDC single mothers with younger kids were likely to get benefits for a much longer period, (till their kids turned 18). But TANF has limited their benefits to a maximum of five years. Given the loss, they may be more likely to join work after reform than are women with older children. To study whether welfare reform influenced unmarried women with younger children more or less than unmarried women with older kids, I divide the sample into two groups: those with at least one child younger than 6 and those with children between 6-18 (no child less than 6 years). An F-test rejects the

¹⁰ I also used the years 1988 and 1991 and found similar results as those reported in the text.

specification that does not distinguish between the two groups of unmarried mothers when the comparison group is married women.¹¹

Finally, I estimate equation (1) using measures of the intensity of welfare reform in place of time limits and other reform. As mentioned earlier I create five categories of reform: no reform (the omitted category), and low-, only time limit, medium- and high-intensity reform. I also estimate equation (1) with the policy variable “years since time limits were imposed.”

I do the entire analysis by restricting the sample to central city and non-central city areas. I also repeat this analysis for the following two outcomes: probability of getting a job in the private sector and probability of getting a job in the public sector.

An alternative to the difference-in-differences approach is to limit the sample to members of the target group, and to control for unmeasured time variation using a purely statistical procedure. The advantage of this approach is that I do not need to identify an appropriate comparison group, which may be difficult in practice. Instead, I include controls for state-specific time trends in the model. The regression model specification for this alternative is:

$$(2) \text{LFP}_{ijt} = \alpha_j + \delta(\beta_j \times \text{Trend}_t) + X_{ijt} \Gamma + Z_{jt} \lambda + \alpha_1 \text{Reform}_{jt} + e_{ijt}.$$

There are two disadvantages with this approach. First, there may be non-linear time effects that will remain unaccounted for by the linear trend specified in equation (2). Second, there will be a significant amount of collinearity between the state-specific trends and the indicators of welfare reform. This collinearity can make it difficult to

¹¹ The F-test for the above specification is not rejected for unmarried women without a high school degree

identify the effect of the policy, and also prohibit the inclusion in the regression model of controls for non-linear time effects, since this will only exacerbate the collinearity problem. For instance, in this case, a regression of an indicator of welfare policy (time-limited benefits) on controls for state and state-specific trends yields an R-square statistic of 0.74, which indicates there is high degree of collinearity between state-specific trends and the policy variable. Nonetheless, I test this methodology for the employment analysis, and present the results.

A difference-in-differences approach is also used to examine the effect of welfare reform on fertility and marriage rate. As discussed earlier, in both the analyses, the target and comparison groups differ. In both cases, I use two comparison groups to test the sensitivity of my findings to the choice of comparison group. The empirical model for these analyses is similar to equation (1) except for the following differences. First, in the marriage analysis, the dependent variable is an indicator of whether a woman is married or not. Second, the dependent variable in the fertility analysis is an indicator of whether the woman had a child within the last year.¹² Third, in the fertility analysis, I modify the independent variables measuring the number of children so that they measure the number of children at time t-1, or as of the previous March interview. This eliminates the mechanical relationship between the dependent variable and these explanatory variables. Finally, in the fertility analysis, the policy variables are measured as of year t-2 since a woman who has a child in year t-1 made her decision to get pregnant in year t-2.

when the comparison group is unmarried women without children.

¹² Specifically, the dependent variable equals 1 if there is a own child under age 1 in the family.

RESULTS

LABOR MARKET EXPERIENCE

Labor Force Participation

Table 1 gives a brief description of the employment sample. The most important point to note about Table 1 is the similarity of the means for the target and comparison groups. For example, during the period under study, the employment rate of unmarried women with children who have less than a high school degree is 0.374; for married women with children who have less than a high school degree, the employment rate is 0.423. Similarly, the employment rate of unmarried women with children who have a high school degree is 0.646; for married women with children and a high school degree is 0.662. The similarity of means between the target and comparison groups is important because it provides initial evidence that the two groups have similar labor market behavior and is consistent with the assumption underlying the DD analysis that variation over time in employment outcomes is also similar between the two groups.

Estimates of equation (1) are presented in Table 2. Columns 1 and 2 in each row are a separate regression. The estimated coefficient in Column 3 is the result of a separate regression. And Columns 4-7 in each row is the result of a separate regression.¹³ Standard errors have been corrected for heteroscedasticity using the method suggested by White (1980). The comparison groups underlying the analyses of Table 2 are married women or married women with children. All married women are used as the comparison group for the target group consisting of all unmarried women, and married women with children are used as a comparison group for the target group of unmarried women with children.

¹³ Unless stated otherwise, all the Tables presented in this paper give results in the pattern described here. Tables 1, 7, 12 and 17 do not follow this pattern.

The estimates in Table 2 show that the recent increase in the employment of unmarried mothers is not entirely due to economic expansion. Welfare reform has played a significant role in bringing unmarried mothers into the labor force. The ordinary least squares estimates of the effect of time limits are positive and statistically significant in most cases.

Estimates in column 1 in Table 2 show that the imposition of time limits has increased the employment of unmarried women by 3.3 to 4.6 percentage points. It is important to remember that what I have referred to as contamination of the target and comparison groups results in a downward biased estimate of the effect of welfare reform on this group of women. Among unmarried women who have less than a high school degree, 56% do not have children and many of these women are not likely to be affected by welfare reform. Correspondingly, some portion of the comparison group of married women with less than a high school degree will be affected by welfare reform since nearly 83% of them have children and given their education are expected to have relatively low family incomes.

Limiting the sample to women with children results in large increases in estimated effects. This is expected given the more appropriate target and comparison groups and the smaller level of contamination in these groups. Among unmarried women with children who have less than a high school education, time limits increase employment between 9.3 and 12.6 percentage points depending on the age of the children and whether the woman was ever married. The largest effect is for unmarried women with no young children in the household. Estimates of the effect of time limits pertaining to unmarried women with children and a high school degree indicate effects of between 1.4 and 10.4

percentage points. For this group, the largest effect is for women with small children in the household.

The estimates in Table 2 provide evidence that can be used to evaluate whether compositional change related to welfare reform's effect on fertility is an important source of bias in the analysis that uses the presence of children to define target and comparison groups. Assume 50 percent of all unmarried women with less than a high school degree are at risk of welfare receipt and affected by welfare reform, and 20 percent of all married women with less than a high school degree are similarly at risk. The DD estimates obtained using target and comparison groups defined solely on the basis of marital status will be 30 percent of the true estimate. So the 4.6 percentage point estimate of the effect of welfare reform on the employment of unmarried women with less than a high school degree may mask a true effect of 15.3 ($4.6 \div 0.3$) percentage points. I have argued that using the presence of children to define target and comparison groups reduces the contamination and reduces the downward bias associated with this problem. The estimate of the effect of time limits that uses a target group of unmarried women with children who have less than a high school degree is 10.7 percentage points. Even the 10.7 percentage point estimate associated with unmarried women with children is downward biased. If we assume that 90 percent of unmarried women with children and less than a high school degree are potentially affected by welfare reform and that 30 percent of married women with children of similar education are also affected, then the 10.7 percentage point estimate is only 60 percent of the true estimate of 17.8 ($10.7 \div 0.6$) percentage points.¹⁴ The upshot is that estimates of the effect of welfare reform on

¹⁴ Similar calculations for unmarried women with a high school degree reveal smaller differences between the estimates obtained with and without using the presence of children to define target and comparison

employment using various definitions of target and comparison groups are yielding similarly sized estimates of the true underlying effect of welfare reform on the employment of women at risk. These results suggest that the bias due to compositional change induced by welfare reform's effect on fertility is not large and not the reason for the positive and significant estimates in Table 2.

The estimated effects of reforms other than time limits are also presented in Table 2 (Column 2). Other reforms in the absence of time limits have not influenced the employment of unmarried women with children who have less than a high school degree. This indicates that the most potent tool of welfare reform was the imposition of time limits, which induced a large number of low-educated unmarried mothers to move from welfare to work. Among unmarried women with a high school degree, other reforms without time limits have a positive effect on employment only in the case of unmarried woman with at least a child less than six years of age. For this group, however, other reforms were as effective as time limits in inducing women to move from welfare to work.

What makes the effect of time limits even more impressive is that few recipients during this period were hit by time limits. The large-scale shift from welfare to work reflects that recipients realize that it is not in their interest to stay on welfare. For one, they lose precious labor market experience if they stay on welfare. This raises an issue: did time limits have a one time impact or are more low educated women likely to opt for work as their welfare clock moves forward? Analysis in Column 3 of Table 2 explores this issue, and shows that more people opt for work as their welfare clock starts ticking.

groups. This smaller difference is consistent with the fact the presence of children is less of a risk factor of welfare receipt among this more educated group.

A one- year increase in years since the time limit was imposed raises the employment of unmarried women without a high school degree by 2.1 percentage points, and the employment of unmarried women with a high school degree by 1.1 percentage points. Again, as in the analysis in Columns 1 and 2 of Table 2, the effect of the welfare clock increases as I use presence of children to define the target and comparison groups. As the clock advances by one year, between 4.2 to 4.9 percentage points of unmarried mothers without a high school degree start working depending on whether they ever married and the age of their youngest child. Assuming a 5-year lifetime limit for all states, such an increase implies that time limits would induce around a quarter of the unmarried mothers without a high school degree to start work. The impact of the welfare clock on unmarried mothers with a high school degree is statistically significant, but is less than half the impact on those without a high school degree. Employment of unmarried mothers who have a high school degree increased by 1.4 to 2.7 percentage points depending on whether they ever married and the age of their youngest child. For this group, the increase of the above order means that over a 5-year lifetime limit employment would increase by 7 to 13.5 per cent due to the imposition of time limits. We should not be alarmed by the difference in the increase in the employment of the two groups. As Table 1 indicates the labor market attachment of unmarried mothers with a high school degree for my sample is 64.6 percent, 73 percent higher than the labor force attachment of unmarried women without a high school degree, which is 37.4 per cent. Therefore, a smaller proportion of women in the former category are likely to be affected by welfare reform.

Columns 4 to 7 of Table 2 present the results for an alternative specification of the welfare reform variables used in equation (1). The goal of this analysis is to measure the effect of the intensity of welfare reform. For these models I grouped states by whether they had no reform (omitted category), some type of reform but no time limit (low intensity), only time limits, time limits with at least one but not more than two of the other three welfare policies considered in this paper (medium intensity reform), and time limits with all the other three policies (high intensity reform).

The estimates reveal that what I refer to as high intensity reform has large and statistically significant effects on the employment of unmarried women. There is a dose-response effect with high intensity reform having the largest effect, followed by the effect of medium intensity reform, which is in turn greater than the effect of only time limits. For women without a high school degree, the effect of high intensity reform is between 24 to 80 percent higher than the effect of medium intensity reform depending on the age of the youngest child, and whether they ever married. The effect of medium intensity reform is in turn almost two to three times the effect of “only time limits” on employment. The effect of low intensity reform is never statistically different from zero for women without a high school degree. As low intensity reforms (reforms without any time limits) are mostly associated with AFDC waivers, these results indicate that state-level welfare reform prior to August 1996 did not have much effect on the employment of unmarried mothers who did not have a high school degree.

The largest effects of high intensity reform among unmarried women with less than a high school degree are for those with no young children. For this group, high intensity reform is associated with a 15.8 percentage point increase in the employment

rate. This is a large effect, more than a third of the mean employment of this group. High intensity reform increases the employment of never-married mothers without a high school degree by 15 percent, which is almost half of the mean employment of this group (an even larger effect).

The effect of high intensity reform on unmarried mothers with a high school degree is smaller than on those who do not have a high school degree. With respect to demographic characteristics, for unmarried women with a high school degree, high intensity reform has the highest effect on those with a young child. Low intensity reforms also have a significantly large effect on the employment of this group, and is greater than the effect of time limits without any other reform. In contrast, unmarried mothers with no young child are unaffected by any kind of reform, except for “only time limits.” The effect of the intensity of reform on never married mothers with a high school degree differs from the effect on other demographic groups. For this group, as expected, the effect of high intensity reform is higher than that of the lower intensity reforms. But there is not much difference between the effect of low intensity reform and only time limits on the employment of this group, and the effect of medium intensity reform is marginally less than the other two reforms. This shows that never married women with children seem to have reacted in a manner which is quite different from other groups of single women with children (that is, divorced, separated and widowed women). AFDC waivers (low intensity reform) appear to be as effective as time limits and reforms correlated with them to induce never married mothers to join work.

The efficacy of the difference-in-differences methodology depends critically on the appropriateness of the comparison group. Thus, it is prudent to test the sensitivity of

the estimates to choice of comparison group. Accordingly, in Table 3, I present estimates using an alternative comparison group. The presentation of Table 3 mirrors that of Table 2, except that the comparison group underlying the analysis in Table 3 consists of low-educated, unmarried women without children. For unmarried women with children who have a high school degree, the estimates in Table 3 are very similar to those presented in Table 2. Among unmarried women with children who do not have a high school degree, however, the estimates in Table 3 are sometimes smaller than those in Table 2.

Specifically, estimates of the effect of time limits and medium or high intensity reform on employment in Table 3 tend to be smaller than in Table 2, particularly for never married women. This finding provides some evidence that the choice of comparison group does make a difference, at least for unmarried mothers without a high school degree. There is no definitive way to choose which set of estimates is better. However, I prefer the estimates in Table 2 because I believe that married women with children is a better comparison group for unmarried women with children than is unmarried women without children because children are a more important determinant of labor market experiences than marital status. The means in Table 1 illustrate this point. The largest differences in employment rates are between unmarried women with no children and unmarried women with children, whereas the difference between married women with children and unmarried women with children is quite modest.

An alternative to the difference-in-differences analysis is to restrict the model to the target group and control the impact of time varying factors by introducing state-trends. The econometric specification of this model is given in equation (2). The results of this analysis are presented in Table 4. As Table 4 shows that the effect of almost all the

elements of welfare reform appear to be statistically insignificant in this model, except for the effect of high intensity reform on the employment of unmarried with no young child and without a high school degree (the effect is positive and large), and the model that estimates the effect of the welfare clock on the employment of unmarried low skilled women. In the latter case, the effect is quite close to the effect measured in the other two models. The statistically insignificant results could be due to the very high collinearity between the state-specific trends and the indicators of welfare reform. As mentioned earlier, a regression of an indicator of welfare policy (time-limited benefits) on controls for state and state-specific trends yielded an R-square statistic of 0.74, indicating a high degree of collinearity between state-specific trends and the policy variable.

Of the three models analyzed here, I believe the results presented in Table 2 are the most trust-worthy. I use these estimates to obtain a rough estimate of the contribution that welfare reform induced employment made to the decline in caseloads. I focus only on the effect of time limited benefits since almost all states implemented time limits during the period of this research. To calculate this number, I first multiply the change in the employment rate of unmarried women associated with time limits by the number of unmarried women. For this, I use the number of unmarried women as estimated by the 1994 CPS and 0.04 as the change in the employment rate of unmarried women due to time limits. The 0.04 figure comes from estimates in Table 2, which indicate an increase of between 3.3 and 4.6 percentage points in the employment of unmarried women associated with time limits. I choose 1994 as the base year for two reasons. Firstly, all but one state, Iowa, implemented time limits after 1994. Iowa implemented time limits in October 1993, which for the purpose of this analysis would show an effect only in the

March 1994 survey. So effectively, 1994 is the correct base year for observing the effect of time limits. Two, the level of caseloads reached a peak in 1994, and has been falling since then. The multiplication of number of unmarried women with the change in employment rate yields the number of unmarried women who are working as a result of time limited benefits. I can compare this number with the decline in the number of unmarried women on the caseload between 1994 and 2000 to obtain the proportion of the decline in caseload that is attributable to increased employment resulting from welfare reform.

In 1994, there were 12,105,694 unmarried women with 12 or fewer years of education. The imposition of time-limited benefits resulted in a 0.04 percent, or 484,239 increase in the number of low-educated, unmarried women employed. According to CPS data, between 1994 and 2000, the number of unmarried women with 12 or fewer years of education who were receiving public assistance declined by 1,512,465. Thus, I estimate that approximately 32% of the decline in the caseload among this group is attributable to increased employment related to the imposition of time limited benefits. This figure is much higher than that reported by Schoeni and Blank (2000) who find that welfare reform associated with the federal law had little effect on work behavior, and is similar to the CEA (1999) estimate that suggests a third of the caseload decline in this period was due to the administrative reform. Note, however, that CEA (1999) estimate refers to changes in the caseload due to all behavioral changes including, but not limited to, employment. My estimates related to employment explain nearly all of the change in the caseload reported by CEA. We should also note that this estimate does not take into account additional employment related declines in the caseload associated with other

types of reform. For example, estimates in Table 2 suggest that other reforms without time limits also had employment effects in the case of high school educated single mothers with young children. The same holds for never married mothers with a high school degree. Thus, the contribution of welfare reform-induced employment may be even larger than I estimate. On the other hand, if I allow for the fact that some women who are working are also on welfare, I would obtain a somewhat smaller estimate of the employment related decline in caseloads.

Hours of Work

I am interested in investigating not only the effect of welfare reform on the employment of unmarried mothers, but also the effect of reform on their economic plight. Thus, the next outcome I examine is hours of work of unmarried mothers¹⁵. The empirical specification for this analysis is also given by equation (1) and the results are presented in Table 5 (comparison group low-educated married woman) and Table 6 (comparison group low-educated unmarried woman without children). As in Table 2, in Table 5 too, column 1 and 2 in each row is a separate regression. The estimated coefficient in column 3 is the result of a separate regression. And column 4-7 in each row is the result of a separate regression.

Estimates in Table 5 show that welfare reform, specifically the imposition of time limits, increase the hours worked per week of unmarried mothers. I first give the results for women without a high school degree. Imposition of time limits increases the average hours worked per week of unmarried mothers without a high school degree by 3.199 to

¹⁵ These estimates include all women, including those who do not work, that is, those who work zero hours.

3.798, depending on the age of their youngest child and whether they ever married. The increase among women without a young child is marginally higher than among those with a young child. Never-married mothers are induced to work marginally more than divorced or separated mothers as a result of the imposition of time limits or other reforms correlated with time limits.

Among unmarried mothers with a high school degree, time limits induce an increase of -0.009 to 3.534 hours per week. In this education group, those with young children increase their working hours by the maximum amount (3.534 hours). The working hours of women with no young child are unaffected by the imposition of time limits. This is in confirmation with the employment analysis which shows that the employment of unmarried mothers with no young children and a high school degree is unaffected by the imposition of time limits. Never married mothers with a high school degree are working a greater number of hours, as compared to divorced or separated mothers who are induced to join the work force by welfare reform.

In general, the other estimates in Table 5 are similar to those related to employment. The impact of reforms other than time limits on unmarried women's hours of work is statistically insignificant in most cases except for unmarried women with a high school degree and young children, and for never married mothers. Again this confirms the results in the employment analysis.

Column 3 in Table 5 gives the impact of years since time limit (or welfare clock) on the hours worked of unmarried low educated women. Unmarried mothers without a high school degree increased their work by approximately 1.5 hours a week (irrespective of the age of their youngest child or whether they were ever married) as the time limit

grew closer by a year. Unmarried mothers with a high school degree increase their work by a third of an hour to an hour per week as their welfare clock advances by a year, depending on their demographic characteristics.

Estimates in Table 5 also show that as welfare reform becomes more severe, working hours of unmarried mothers without a high school degree increase. High intensity reform increases the working hours of unmarried women with children who have less than a high school degree by 4.321 to 5.227 hours a week. For unmarried women with children and a high school degree, high intensity reform is associated with a change in average hours worked per week of between -0.713 and 4.198 .

Table 6 is a mirror image of Table 5 except that it uses unmarried women without children as the control group. As in the employment analysis, in the working hours analysis too, the effects of various aspects of welfare reform are the same for unmarried mothers with high school degree irrespective of the control group I use. For unmarried mothers without a high school degree, the impact of time limits and policies correlated with time limits is lower when unmarried women without children are used as the control group. Again it is not possible to judge the efficacy of the control groups. As argued earlier, I think married women with children are a better control group as children are a better determinant of employment than marital status.

The small, but significant increases in average hours worked per week in Tables 5 and 6 mask a relatively large change in labor force attachment among unmarried mothers who entered the labor market because of welfare reform. In Table 7, I calculate the average hours of work per week among those who were induced to work by welfare

reform.¹⁶ In general, the figures in Table 7 indicate that among unmarried women with less than a high school degree, new welfare reform-induced entrants to the labor market worked about the same number of hours as the existing members of the labor force. Moreover, their commitment to the labor force in terms of hours worked per week was substantial, about 31 hours on average. This figure is very close to that reported by Brauner and Loprest (1999), in their summary of the experiences of women who recently left welfare. Among unmarried women with a high school degree, estimates in Table 5 indicate that new entrants worked approximately 29 hours per week, which is less than the working hours of existing labor force participants.

Gladden and Taber (2000) report that women with less than a high school degree had an average hourly wage of \$7.55 in 1997. If I use a somewhat lower figure of \$7.00 as an estimate of the hourly wage of women who entered the labor market because of welfare reform, estimates in Table 7 indicate that low-educated, unmarried mothers who started working due to welfare reform will earn approximately \$217 per week on average. If these women work 40 weeks per year they will earn approximately \$8,680. These women will also be eligible for the earned income tax credit that will provide up to 40 percent additional earnings, which raises average earnings to \$12,152 per year. These families will remain eligible for food stamps and Medicaid will cover their children's health care. Transitional benefits for the mother include childcare and Medicaid for one

¹⁶ The calculations in Table 5 are as follows. Let H_0 be the average work hours of all unmarried mothers before reform, 'a' be the proportion of unmarried mothers that work and H_w the average hours of work for those who worked before reform. Therefore, $H_0 = a H_w$. Assume that time limits do not affect the work hours of unmarried mothers who worked before reform. Let H_1 be the average work hours of unmarried women after reform, 'b' be the proportion of unmarried mothers who started working after reform, and H_n be the average hours of unmarried women who began working after reforms. Thus, $H_1 = a H_w + b H_n$ and $H_1 - H_0 = b H_n$. Solving for H_n yields $H_n = [H_1 - H_0]/b$, where $H_1 - H_0$ is coefficient a_3 from equation (1) when hours worked is the dependent variable and b is coefficient a_3 from equation (1) when labor force participation is the dependent variable.

year after leaving welfare. Thus, the financial picture of women who have left welfare for work improves significantly given that the cash assistance associated with TANF is quite low, with the national median being approximately \$5000 per year for a mother and two children. Moreover, evidence suggests that these women can expect wage growth over time and therefore a better economic future (Cancian et al. 1999, Gladden and Taber 2000).

Employment in public versus private sector

The evidence in the employment and hours worked analysis shows that welfare reform has induced a large number of unmarried woman with children to move from welfare to work, and on average, they are economically better off by the transition. Naturally, there is considerable concern about the relatively poor job market skills of less-educated unmarried mothers. No one expects the transition from welfare to work to be smooth. One of the questions that needs to be asked is: are low-skilled women doing as well in getting private sector jobs as they are in getting public sector jobs? Or are the former welfare recipients moving from state subsidy to state supported employment? To answer this question, I study the effect of welfare reform policies on the employment of women in the public and private sector.

The description of the employment sample in Table 1 shows that a very large proportion of the people in our treatment group are employed in the private sector. Around 6 percent of the working unmarried mothers without a high school degree work in the public sector; and around 12 percent of those with a high school degree work in the public sector.

As in the earlier analysis, I use a D-D methodology to investigate this issue, and the empirical specification for this analysis is given by equation (1). Tables 8-11 give the result of this analysis using two different comparison groups. Table 8 estimates the effect of various aspects of welfare reform on the employment of low-skilled unmarried mothers in the private sector with similarly skilled married mothers as the control group. Table 9 gives the same for low-skilled unmarried mothers with unmarried low-skilled women without children as the control group.

There are two main things to notice from Tables 8 and 9. One, a large number of unmarried low skilled women started working in the private sector, irrespective of the control group we use. Two, the estimates in Table 8 are quite similar to the estimates in Table 2 that give the effect of welfare policies on the overall employment of the treatment group. (Similarly the estimates in Table 9 are quite similar to the estimates in Table 3.) This indicates that an overwhelmingly large number of unmarried low-skilled women who were induced to work by welfare reform actually started working in the private sector. Given the similarity of these results with the results in Tables 2 and 3, a detailed discussion of these results would amount to repeating the earlier analysis. Hence, I choose not to discuss these results any further.

Tables 10 and 11 present the estimates of the effect of welfare policies on the employment of unmarried women in the public sector (with the two different control groups). These estimates further strengthen the conclusion drawn in the earlier analysis that most of the unmarried mothers are being absorbed in private sector jobs and not in public sector jobs. Given that there are not too many public sector jobs, it is expected that more low-skilled women would be absorbed in the private sector. Therefore, this analysis

merely points out that welfare reform has not moved low-skilled mothers from welfare to public sector jobs, that most of them have been absorbed by the private sector.

When married woman are the comparison group, estimates in Table 10 show that time limits increase the public sector employment of unmarried women without a high school degree by 0.008 to 0.02 percentage points depending on the age of their youngest child and whether they ever married. The largest effect is on women with no child less than 6 years, but as a comparison with Table 8 shows, even this group is around 4.6 times ($0.093 \div 0.020$) more likely to have joined the private sector than the public sector. In general, (compare Tables 8 and 10) the effects of time limits on the private sector employment of unmarried mothers are at least four times and as much as 10 times larger than the effect of time limits on their public sector employment. Except for the case of never married women, the advancement of the welfare clock has not induced unmarried mothers without a high school degree to join the public sector. Columns 4-7 in Table 10 show that high intensity reform did induce unmarried mothers without a high school degree to join public sector, but (in comparison with the coefficients in Table 8) the estimates of the increase in the private sector employment of this group are at least 4 to 10 times larger.

For unmarried mothers with a high school degree, the coefficients indicating the effect of time limits on their public sector employment are never statistically significant. Similarly, this group is unaffected by the welfare clock or the intensity of reforms.

Table 11 presents the effect of welfare policies on the employment of low skilled mothers in the public sector with unmarried women without children as the comparison

group. The estimates are rarely significant, and confirm the results of the earlier analysis in Table 10 with married women as the comparison group.

Employment in central city versus non-central city areas:

I am also curious to know whether welfare reforms have been equally effective in inducing single mothers living in central city and non-central areas to join work. The two groups face very different labor market conditions. As argued earlier, while it is easier for low skilled mothers to find jobs outside the central city areas, they face certain other obstacles in the form of poor facilities for childcare, poor public transportation. Are these obstacles formidable? The following analyses explore this issue. These analyses are similar to the earlier ones except that for the analysis on central city areas, the sample of observations is limited to women living in these areas and for the analysis on non-central city areas, I restrict the sample to women living outside the central-city areas.

Table 12 gives the means of the samples for this analysis. As in the earlier analyses, the sample means of the treatment and control groups are quite similar, justifying my choice of treatment and comparison groups. The second thing to notice from Table 12 is that the probability of being employed for both the treatment and the control groups is higher in non-central city areas than in central city areas. Thus, despite the obstacles, the employment rate of unskilled unmarried mothers is higher in non-central city areas.

I first study the effect of welfare reform on the employment of women living in central city areas. Tables 13 and 14 give the results of this analysis with the two comparison groups. A striking feature of the estimates in both the tables is that the

employment effects of welfare reform are considerably subdued for unmarried low-skilled mothers living in central city areas, in comparison with employment effects for the combined sample of women living in central and non-central city areas. (Tables 2 and 3).

Table 13 gives the estimates of welfare reform on unmarried women with married women as the comparison group. Column 1 of this table shows that time limits have a significant effect on the employment of single mothers without a high school degree only if they have a young child. For those with a high school degree, time limits have a positive and significant effect if she has a young child. Other reforms in the absence of time limits do not have a significant effect on the employment of unmarried mothers living in central city areas. Years since time limits were imposed has a statistically significant effect on the employment of unmarried women without a high school degree, but not on the employment of unmarried mothers women with a high school degree. Columns 4 to 7 of Table 13 capture the effect of the intensity of welfare reform. Again, high intensity has a positive and significant effect in inducing unmarried mothers without a high school degree to work. For those with a high school degree, the effect is statistically insignificant except when they have young kids or they have never been married. All the other levels of reform – low intensity, only time limits and medium intensity reform have no effect on the employment of unmarried low skilled mothers, irrespective of their demographic group or education level.

Table 14 gives the employment effects of welfare reform on the treatment group with unmarried women without children as the comparison group. As mentioned earlier, the estimated coefficients in this Table are smaller than the estimated coefficients in

Table 3, which gives the employment effects of welfare reform on the combined sample of unmarried mothers living in central city and non-central city areas, using the same control group as Table 14. Also, the estimated coefficients in Table 14 differ from the estimated coefficients in Table 13 indicating the significance of the comparison group. More specifically, Table 14 shows that unmarried mothers who live in central city areas are induced to join work in large numbers. The largest employment effect of time limits is on unmarried women with young children who have a high school degree. Time limits increase the employment of this group by 8.1 percentage points. Interestingly, reforms other than time limits have a larger effect (17.3 percentage points) than time limits on the employment of women in this group. This indicates AFDC waivers (all but two states have time limits under TANF) had a much greater effect than TANF in influencing high school educated unmarried mothers with young children to move from welfare to work. That also explains why low intensity reforms (defined as reforms other than time limits) had a much larger effect than the higher intensity reforms in inducing this group to move from welfare to work, when unmarried women without children are the comparison group. As argued earlier, it is difficult to decide which comparison group is better. I prefer married women with children as a comparison group as children are a more important determinant for work than marital status.

I now discuss the effect of welfare reform on the employment of unmarried mothers who live outside the central-city areas using the two comparison groups. Tables 15 and 16 present the results of this analysis. These results echo the results in Tables 2 and 3 (overall employment effects). In many cases the estimated coefficients are up to 40 percent higher than the estimated coefficients in Tables 2 and 3. These results point out

that welfare reform has induced a large number of unmarried low-skilled mothers move from welfare to work in non-central city areas. To escape repetition, I do not discuss these results in detail here.

My estimates of the effect of welfare reform on the employment of low-skilled unmarried mothers in central city and non-central city areas may be biased if welfare reform also induced low skilled women to move out of central city areas to non-central city areas in search of jobs or for some other reason. In that case, at least a part of the difference in low-skilled women's reaction to welfare reform that I observe between the estimates for the central city and non-central city areas is due to the change in the size of treatment group in the two areas caused by migration which is in turn an outcome of welfare reform. In which case we need to interpret the above results with caution.

Kaestner et.al. (2002) finds that welfare reform has induced women from central city areas to migrate to non-central city areas, and a significant degree of this movement was employment related. Therefore, at least a part of the difference in the two results is due to changes in sample sizes.

MARRIAGE

The next outcome I study is the marriage rate of low skilled women. The analysis is similar to the earlier analysis, except that I cannot use marital status to define my treatment and comparison groups. In this analysis, the treatment groups are all women in age group 18-44 years with and without a high school degree. The comparison group for both the treatment groups is all women in the above age group with 13-15 years of

education. I do estimate another model for women who previously had a child to see if this group is affected by welfare reform differently from the larger group.

Table 17 gives the mean marriage rates for the treatment and comparison groups in this analysis, which are quite similar except for one case. In the case of women without a high school degree and at least a child the average marriage rate is 0.599, much lower than the average marriage rate for the comparison group used for this group, women with 13-15 years of education and at least a child (mean = 0.726).

Table 18 gives the results of the effects of welfare reform on the marriage rate of the treatment groups. It shows that time limits have a positive and statistically significant effect on the marriage rate of women without a high school degree. Other reforms have a negative but statistically insignificant effect on this group. The welfare clock also has a positive effect on the marriage rate of this group. And the marriage rate of this group increases with the intensity of reform. When we restrict the treatment group to women with at least a child and without a high school degree, the effect of various aspects of welfare reform on marriage increases. Welfare reform does not appear to have any effect on the marriage rate of women with a high school degree, irrespective of whether they have a child or not. The effect is often negative, but never statistically insignificant.

The above analyses show that women without a high school degree have responded to welfare reform by joining work and marriage. But women with a high school degree, indicating better job market skills have overwhelmingly joined work, and have not responded by marriage. This analysis indicates that a part of the increase in the employment of unmarried unskilled women without a high school degree (Table 2) could be due to the change in the treatment and control group samples due to welfare reform's

impact on marriage. The results in Table 3 may also be affected by the change in the sample size of the treatment and comparison groups, but by a smaller degree. This also explains, at least to some extent, the difference in the estimated coefficients in Tables 2 and 3 for unmarried mothers without a high school degree. As noted earlier, the estimated coefficients in Tables 2 and 3 for unmarried mothers with a high school degree were quite similar. One way to take care of the change in sample sizes due to welfare reforms effect on marriage is to use, like Schoeni and Blank (2000), education, and not marriage, to define the treatment and the comparison groups. However, as argued earlier, I think education is a much more important determinant of employment than marriage. A comparison group with higher education than the treatment group is not good for controlling over-time changes in employment.

FERTILITY

The last outcome I examine is fertility, as measured by whether an unmarried woman had a birth in the last year. Table 17 gives a brief description of the samples used in the analysis. The sample selection criteria remain nearly the same: women age 18 to 44 with 12 or fewer years of education, although for some analyses I use unmarried women with 13 to 15 years of education as the comparison group. The target group for this analysis is expanded to include all low-educated unmarried women because unmarried women without children are at risk of having a child out of wedlock and going on welfare, and welfare reform may influence their fertility decisions. I use two comparison groups: married women with 12 or fewer years of education and unmarried women with 13 to 15 years of education. Note that the mean incidence of a recent birth differs

substantially between the target and comparison groups. This raises questions as to the adequacy of the comparison groups, and suggests a cautious interpretation of the findings.

For this analysis, I do not divide the sample by education status because of sample size considerations. Given the relatively infrequent occurrence of a birth in the last year (mean incidence of between 0.044 and 0.062) and the expectedly small effect of welfare reform, large sample sizes are required in order to have sufficient power to detect reliably an effect of welfare reform. Therefore, I chose to use the combined sample of low-educated women. I do, however, estimate separate models for women who previously had a child, because certain aspects of welfare reforms, specifically family cap, are designed to affect the fertility of women with children.

The estimates of the effect of welfare reform on fertility are presented in Table 19. Row 1 of Table 19 shows the estimates associated with the comparison group consisting of married women with 12 or fewer years of education. Row 2 shows the same with married mothers with 12 or fewer years of education as the comparison group. Row 3 shows the estimates associated with a comparison group consisting of unmarried women with 13 to 15 years of education, and Row 4 shows the same with unmarried mothers with 13-15 years of education as the comparison group.

Results in Row 2 shows that time limits have a positive and significant effect on the fertility of unmarried low-skilled mothers. Years since time limits also have a positive effect on the fertility of this group. This result is counter to almost all theory. All other estimates presented in Table 19 are statistically insignificant. From this I conclude that welfare reform has no effect on the fertility of low-educated women.

CONCLUSIONS

Welfare reform has made a vast change in the lives of low skilled unmarried mothers. My analysis shows that the transformation of welfare from entitlement to a temporary relief has induced low-educated unmarried mothers to move from welfare to work in significant numbers. I estimate that the imposition of time limits and other administrative reforms correlated with it have increased the employment of unmarried mothers with 12 or fewer years of education by 484,239. This represents approximately 32% of the decline in welfare caseloads for this group since 1994. This is a significant impact for a policy whose full effect has yet to be felt because relatively few women in the sample have actually run up against their state's time limit.

Moreover, estimates obtained indicate that the new entrants to the labor market are working approximately 31 hours per week. Even at low wages (e.g., \$7.00/hr.) and with intermittent periods of unemployment (12 weeks per year), this type of labor force attachment can significantly improve the financial position of former welfare recipients. These new entrants to the labor market gain access to the earned income tax credit and transitional childcare and medical benefits, and they continue to have access to food stamps and Medicaid for their children. Thus, low-wage work is a viable alternative to welfare and many women may have been induced to recognize this when welfare was made a temporary benefit. More importantly, these women begin to accumulate on-the-job experience that results in continued wage growth and an improving economic position. I also find that less-educated unmarried mothers are not moving from welfare to state-supported employment, that they are successfully competing for jobs in the private sector. And that women in non-central city areas have experienced much larger

employment effects, although this result has an upward bias due to welfare induced migration from central city to non-central city areas.

I also examine the effect of welfare reform on marriage and fertility, as one of the objectives of welfare reform was to change behaviors that cause or increase dependency. I find that welfare reform has increased the marriage rate of women without a high school degree, but not of high school-educated women. This better educated group indicating superior job market skills has overwhelmingly joined work, and has not responded to welfare reform by marriage. I find little evidence that welfare reform affected the fertility of low-educated women. This finding is not that unexpected from an empirical perspective. Past attempts to link welfare policies to fertility have yielded inconsistent evidence. However, recent welfare reform represents a much larger policy change than that used in previous studies and therefore provides a better setting to test whether cash assistance and welfare policies affect fertility. The absence of a significant effect may indicate that cash assistance is not an important determinant of fertility and thus recent reform had no effect on fertility. Alternatively, it may be too soon to estimate the effect. In my analysis, the policy variables were lagged two years and the data ended in 2000, leaving basically two years (1998) of post federal reform data. Perhaps women's fertility behavior responds with a lag and the effect of reform will grow over time.

Table 1
Description of the Employment Sample: CPS, 1992-2000

Description of the Employment Sample: CPS, 1972-2000					
Description of Target and Comparison Groups	Size of the Group	Probability of Being Employed	Average Work Hours	Probability of Being Employed	
				Private Sector	Public Sector
Women 18-44, Education < High School					
Comparison Groups: Married Women	17134	0.439	14.891	0.384	0.026
Married Women with Children	14164	0.423	14.171	0.369	0.025
Unmarried Women with No Children	11393	0.469	13.244	0.436	0.021
Target Groups: Unmarried Women	20880	0.426	12.805	0.391	0.022
Unmarried Women with Children	9487	0.374	12.277	0.338	0.024
Never Married Women with Children	4952	0.314	10.013	0.285	0.020
Women 18-44, Education = High School					
Comparison Groups: Married Women	48939	0.684	23.397	0.544	0.086
Married Women with Children	38417	0.662	22.135	0.517	0.089
Unmarried Women with No Children	21105	0.729	26.065	0.652	0.060
Target Groups: Unmarried Women	36452	0.694	24.715	0.608	0.068
Unmarried Women with Children	15347	0.646	22.859	0.547	0.078
Never Married Women with Children	6468	0.568	19.369	0.493	0.064

Table 2
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women, 1992-2000
(Based on Difference-in-Differences Analysis using Married Women as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School Unmarried Women	0.046*** (0.013)	0.020 (0.023)	0.021*** (0.005)	0.023 (0.023)	0.014 (0.020)	0.033 (0.020)	0.070*** (0.016)
Unmarried Women with Children¹	0.107*** (0.017)	0.005 (0.029)	0.044*** (0.006)	0.011 (0.029)	0.055** (0.025)	0.096*** (0.027)	0.144*** (0.021)
At least a Child < 6 years old²	0.093*** (0.022)	-0.012 (0.037)	0.046*** (0.008)	- 0.012 (0.037)	0.035 (0.034)	0.073** (0.035)	0.132*** (0.027)
No Child < 6 years old³	0.126*** (0.027)	0.025 (0.047)	0.042*** (0.009)	0.041 (0.048)	0.084*** (0.039)	0.126*** (0.041)	0.158*** (0.032)
Never Married Women with Children¹	0.108*** (0.021)	-0.014 (0.034)	0.049*** (0.007)	-0.008 (0.034)	0.028 (0.031)	0.121*** (0.033)	0.150*** (0.025)
Education = High school Unmarried Women	0.033*** (0.008)	0.019 (0.012)	0.011*** (0.003)	0.016 (0.012)	0.030** (0.013)	0.030*** (0.011)	0.035** (0.010)
Unmarried Women with Children¹	0.057*** (0.011)	0.039** (0.017)	0.019*** (0.004)	0.035** (0.017)	0.054*** (0.018)	0.046*** (0.015)	0.063*** (0.014)
At least a Child < 6 years old²	0.104*** (0.017)	0.104*** (0.025)	0.027*** (0.006)	0.103*** (0.025)	0.068** (0.027)	0.111*** (0.024)	0.117*** (0.021)
No Child < 6 years old³	0.014 (0.015)	-0.027 (0.023)	0.014*** (0.005)	-0.035 (0.024)	0.044* (0.024)	-0.008 (0.019)	0.013 (0.019)
Never Married Women with Children¹	0.089*** (0.017)	0.087*** (0.024)	0.029*** (0.005)	0.086*** (0.024)	0.081*** (0.026)	0.077*** (0.022)	0.100*** (0.021)

Notes: Each row in column 1 and 2 is a separate regression. Each row in column 3 is a separate regression and each row in column 4-7 is also a separate regression. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 or more, year effects, state effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. ¹Married Women with at least a child as the comparison group. ²Married Women with at least a child less than six as the comparison group. ³Married Women with no child less than six years (but at least a child between 6-18) as the comparison group. Heteroscedasticity adjusted standard errors in parentheses. * .05< p< .1, ** .01< p< .05, *** p< .01.

Table 3
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women, 1992-2000
(Based on Difference-in-Differences Analysis using Unmarried Women without Children as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School							
Unmarried Women with Children	0.085*** (0.018)	-0.024 (0.028)	0.032*** (0.006)	-0.017 (0.028)	0.051** (0.026)	0.081*** (0.028)	0.113*** (0.022)
At least a Child < 6 years old ¹	0.104*** (0.020)	-0.028 (0.031)	0.041*** (0.007)	-0.025 (0.031)	0.068** (0.030)	0.114*** (0.031)	0.123*** (0.025)
No Child < 6 years old ¹	0.065*** (0.022)	-0.017 (0.041)	0.021*** (0.007)	-0.004 (0.041)	0.036 (0.033)	0.042 (0.035)	0.100*** (0.027)
Never Married Women with Children	0.086*** (0.022)	-0.048 (0.033)	0.036*** (0.008)	-0.043 (0.033)	0.034 (0.031)	0.106*** (0.033)	0.111*** (0.026)
Education = High school							
Unmarried Women with Children	0.051*** (0.012)	0.046** (0.019)	0.017*** (0.004)	0.044** (0.019)	0.051*** (0.019)	0.051*** (0.017)	0.050*** (0.015)
At least a Child < 6 years old ¹	0.103*** (0.015)	0.107*** (0.024)	0.031*** (0.005)	0.107*** (0.024)	0.078*** (0.025)	0.113*** (0.021)	0.108*** (0.019)
No Child < 6 years old ¹	0.002 (0.014)	-0.016 (0.023)	0.005 (0.004)	-0.020 (0.023)	0.025 (0.023)	-0.004 (0.019)	-0.006 (0.018)
Never Married Women with Children	0.081*** (0.017)	0.089*** (0.025)	0.026*** (0.006)	0.091*** (0.025)	0.079*** (0.027)	0.084*** (0.023)	0.080*** (0.021)

Notes: For each row column 1 and 2 is a separate regression, column 3 is a separate regression and column 4-7 a separate regression. ¹The regression involved using two dummy variables for the target group: unmarried mothers with a child less than age 6 and unmarried mothers with no child less than age 6. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 years or more, year fixed effects, state fixed effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. Heteroscedasticity adjusted standard errors in parentheses. * .05<p<.1, ** .01<p<.05, *** p<.01.

Table 4
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women, 1992-2000

Sample Description	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School							
Unmarried Women with Children	0.012 (0.022)	-0.030 (0.033)	0.020** (0.009)	-0.020 (0.034)	0.017 (0.036)	-0.028 (0.036)	0.041 (0.032)
At least a Child < 6 years old	0.005 (0.027)	-0.018 (0.042)	0.034*** (0.012)	-0.030 (0.043)	0.045 (0.044)	-0.029 (0.046)	-0.001 (0.040)
No Child < 6 years old	0.024 (0.036)	-0.036 (0.053)	0.003 (0.015)	0.005 (0.056)	-0.018 (0.058)	-0.015 (0.058)	0.100* (0.053)
Never Married Women with Children	-0.043 (0.059)	-0.121 (0.084)	0.016 (0.025)	-0.079 (0.088)	-0.113 (0.097)	0.001 (0.094)	-0.001 (0.088)
Education = High school							
Unmarried Women with Children	0.026 (0.017)	0.006 (0.022)	0.016** (0.007)	-0.003 (0.024)	0.044 (0.031)	0.022 (0.025)	0.014 (0.025)
At least a Child < 6 years old	0.035 (0.025)	0.017 (0.032)	0.006 (0.010)	0.014 (0.034)	0.040 (0.043)	0.038 (0.038)	0.026 (0.036)
No Child < 6 years old	0.016 (0.023)	-0.012 (0.030)	0.028*** (0.009)	-0.029 (0.032)	0.046 (0.043)	0.011 (0.032)	-0.004 (0.034)
Never Married Women with Children	0.016 (0.033)	0.032 (0.042)	0.004 (0.013)	0.008 (0.068)	-0.008 (0.093)	0.029 (0.067)	0.057 (0.074)

Note: For each row column 1 and 2 is a separate regression, column 3 is a separate regression and column 4-7 a separate regression. Each regression controls for state effects and state-trend effects. All the regressions also control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 years or more, year fixed effects and unemployment rates- current, with one and two year lags. Heteroscedasticity adjusted standard errors are in parentheses.

* .05<p<.1, ** .01<p<.05, *** p<.01.

Table 5
Estimates of Effect of Time Limits and Other Restrictions on the Working Hours of Unmarried Women, 1992-2000
(Based on Difference-in-Differences Analysis using Married Women as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School Unmarried Women	1.726*** (0.505)	-0.295 (0.867)	0.912*** (0.173)	-0.300 (0.871)	1.165 (0.744)	0.841 (0.740)	2.432*** (0.599)
Unmarried Women with Children ¹	3.376*** (0.650)	-0.707 (1.085)	1.485*** (0.229)	-0.494 (1.090)	2.364** (0.957)	2.518*** (0.977)	4.393*** (0.779)
At least a Child < 6 years old ²	3.199*** (0.812)	-0.799 (1.345)	1.508*** (0.289)	-0.886 (1.352)	2.350* (1.211)	1.516 (1.236)	4.321*** (0.984)
No Child < 6 years old ³	3.670*** (1.067)	-0.718 (1.820)	1.516*** (0.368)	-0.140 (1.827)	2.530* (1.531)	3.712** (1.582)	4.545*** (1.267)
Never Married Women with Children ¹	3.798*** (0.775)	-0.493 (1.215)	1.590*** (0.271)	-0.248 (1.223)	1.669 (1.125)	3.479*** (1.177)	5.227*** (0.932)
Education = High school Unmarried Women	0.930*** (0.344)	0.687 (0.503)	0.367*** (0.114)	0.540 (0.506)	0.615 (0.554)	1.174*** (0.458)	0.826 (0.423)
Unmarried Women with Children ¹	1.656*** (0.478)	1.425** (0.699)	0.598*** (0.157)	1.220* (0.704)	1.306* (0.749)	1.687*** (0.630)	1.655*** (0.591)
At least a Child < 6 years old ²	3.534*** (0.678)	3.627*** (0.974)	0.924*** (0.228)	3.606*** (0.981)	1.534 (1.061)	4.002*** (0.926)	4.198*** (0.833)
No Child < 6 years old ³	-0.009 (0.672)	-0.817 (1.006)	0.346 (0.217)	-1.218 (1.012)	1.263 (1.059)	-0.261 (0.859)	-0.713 (0.837)
Never Married Women with Children ¹	2.963*** (0.665)	3.381*** (0.938)	1.134*** (0.219)	3.361*** (0.943)	2.200** (1.048)	2.857*** (0.872)	3.389*** (0.819)

Notes: Each row in column 1 and 2 is a separate regression. Each row in column 3 is a separate regression and each row in column 4-7 is also a separate regression. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 or more, year effects, state effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. ¹Married Women with at least a child as the comparison group. ²Married Women with at least a child less than six as the comparison group. ³Married Women with no child less than six years (but at least a child between 6-18) as the comparison group.

Heteroscedasticity adjusted standard errors in parentheses.

* .05< p<.1, ** .01< p<.05, *** p<.01.

Table 6
Estimates of Effect of Time Limits and Other Restrictions on the Working Hours of Unmarried Women, 1992-2000
(Based on Difference-in-Differences Analysis using Unmarried Women without Children as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School							
Unmarried Women with Children	1.869*** (0.644)	-1.379 (0.986)	0.700*** (0.225)	-1.048 (0.990)	1.056 (0.943)	1.779* (0.927)	2.581*** (0.793)
At least a Child < 6 years old ¹	2.380*** (0.705)	-1.193 (1.057)	0.855*** (0.248)	-0.989 (1.062)	1.579 (1.085)	2.882*** (1.035)	2.751*** (0.885)
No Child < 6 years old ¹	1.323 (0.821)	-1.607 (1.504)	0.517* (0.287)	-1.093 (1.514)	0.555 (1.247)	0.442 (1.283)	2.405 (1.025)
Never Married Women with Children	2.208*** (0.763)	-1.506 (1.119)	0.743*** (0.265)	-1.205 (1.127)	0.683 (1.102)	2.679** (1.127)	3.072*** (0.938)
Education = High school							
Unmarried Women with Children	1.624*** (0.526)	1.597** (0.778)	0.538*** (0.173)	1.506* (0.782)	1.438* (0.819)	1.639** (0.699)	1.606** (0.646)
At least a Child < 6 years old ¹	3.549*** (0.619)	3.637*** (0.947)	1.067*** (0.207)	3.647*** (0.953)	2.452** (1.006)	3.748*** (0.870)	3.895*** (0.770)
No Child < 6 years old ¹	-0.186 (0.615)	-0.446 (0.985)	0.060 (0.199)	-0.639 (0.992)	0.526 (1.006)	-0.280 (0.825)	-0.587 (0.771)
Never Married Women with Children	2.721*** (0.696)	3.339*** (0.998)	1.016*** (0.229)	3.435*** (1.003)	2.301** (1.086)	2.671*** (0.919)	2.990** (0.857)

Notes: For each row column 1 and 2 is a separate regression, column 3 is a separate regression and column 4-7 a separate regression. ¹The regression involved using two dummy variables for the target group: unmarried mothers with a child less than age 6 and unmarried mothers with no child less than age 6. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 years or more, year fixed effects, state fixed effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. Heteroscedasticity adjusted standard errors in parentheses. * .05<p<.1, ** .01<p<.05, *** p<.01.

Table 7
Working Hours of Unmarried Mothers, 1992-2000

Treatment Group	Working Hours of Unmarried Mothers		
	Worked Before Reform	Began Working Due to Time Limit	Began Working Due to High Intensity Reform
Education<High School			
Unmarried Women with at least a Child	32.83	31.55	30.51
With at least a Child < 6 years old	31.41	34.40	32.73
With no Child less than Age 6	33.93	29.13	28.77
Never Married Mothers, Education<High School	31.89	35.17	34.85
Education = High School			
Unmarried Women with at least a Child	35.38	29.05	26.27
With at least a Child < 6 years old	33.65	33.98	35.88
With no Child less than Age 6	36.71	-0.64	-54.85
Never Married Mothers, High School Educated	34.10	33.29	29.25

¹ Based on the difference-in-differences analysis with married women with at least a child as the comparison group.

Table 8
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women in the Private Sector, 1992-2000
(Based on Difference-in-Differences Analysis using Married Women as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School							
Unmarried Women	0.026* (0.013)	0.020 (0.023)	0.016*** (0.004)	0.025 (0.023)	- 0.006 (0.020)	0.018 (0.020)	0.048*** (0.016)
Unmarried Women with Children ¹	0.084*** (0.017)	0.012 (0.029)	0.036*** (0.006)	0.018 (0.029)	0.035 (0.025)	0.074*** (0.026)	0.117*** (0.020)
At least a Child < 6 years old ²	0.077*** (0.022)	-0.016 (0.036)	0.041*** (0.008)	- 0.016 (0.036)	0.032 (0.033)	0.050 (0.034)	0.113** (0.026)
No Child < 6 years old ³	0.093*** (0.027)	0.049 (0.047)	0.032*** (0.009)	0.063 (0.047)	0.046 (0.039)	0.107*** (0.041)	0.121*** (0.032)
Never Married Women with Children ¹	0.082*** (0.021)	0.001 (0.033)	0.037*** (0.007)	0.006 (0.033)	0.001 (0.030)	0.095*** (0.032)	0.124*** (0.025)
Education = High school							
Unmarried Women	0.015* (0.009)	0.004 (0.013)	0.005* (0.003)	0.004 (0.013)	0.021 (0.014)	0.016 (0.012)	0.012 (0.011)
Unmarried Women with Children ¹	0.035*** (0.012)	0.015 (0.018)	0.012*** (0.004)	0.014 (0.018)	0.032 (0.019)	0.031* (0.016)	0.039** (0.015)
At least a Child < 6 years old ²	0.071*** (0.018)	0.075*** (0.026)	0.017*** (0.006)	0.077*** (0.026)	0.025 (0.027)	0.088*** (0.024)	0.084*** (0.022)
No Child < 6 years old ³	0.001 (0.017)	-0.047* (0.026)	0.008 (0.006)	-0.050 (0.026)	0.039 (0.028)	-0.018 (0.022)	-0.005 (0.021)
Never Married Women with Children ¹	0.069*** (0.017)	0.054** (0.025)	0.023*** (0.006)	0.058** (0.025)	0.058** (0.027)	0.060*** (0.023)	0.084*** (0.021)

Notes: Each row in column 1 and 2 is a separate regression. Each row in column 3 is a separate regression and each row in column 4-7 is also a separate regression. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 or more, year effects, state effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. ¹Married Women with at least a child as the comparison group. ²Married Women with at least a child less than six as the comparison group. ³Married Women with no child less than six years (but at least a child between 6-18) as the comparison group.

Heteroscedasticity adjusted standard errors in parentheses.

* .05<p<.1, ** .01<p<.05, *** p<.01.

Table 9
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women in the Private Sector, 1992-2000
(Based on Difference-in-Differences Analysis using Unmarried Women without Children as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School							
Unmarried Women with Children	0.078*** (0.018)	-0.019 (0.028)	0.031*** (0.006)	-0.15 (0.028)	0.047* (0.026)	0.081*** (0.026)	0.098*** (0.022)
At least a Child < 6 years old ¹	0.092*** (0.020)	-0.028 (0.031)	0.039*** (0.007)	-0.028 (0.031)	0.066** (0.030)	0.103*** (0.030)	0.104*** (0.025)
No Child < 6 years old ¹	0.064*** (0.022)	-0.004 (0.040)	0.023 (0.007)	0.006 (0.041)	0.027 (0.033)	0.056 (0.035)	0.092*** (0.027)
Never Married Women with Children	0.077*** (0.021)	-0.035 (0.032)	0.031*** (0.007)	-0.033 (0.032)	0.025 (0.030)	0.103*** (0.032)	0.098** (0.026)
Education = High school							
Unmarried Women with Children	0.047*** (0.013)	0.030 (0.020)	0.016*** (0.004)	0.030 (0.020)	0.028 (0.021)	0.057*** (0.018)	0.050*** (0.016)
At least a Child < 6 years old ¹	0.097*** (0.016)	0.092*** (0.024)	0.030*** (0.005)	0.094*** (0.025)	0.047* (0.025)	0.115*** (0.022)	0.109*** (0.020)
No Child < 6 years old ¹	0.000 (0.015)	-0.033 (0.025)	0.004 (0.005)	-0.035 (0.025)	0.009 (0.026)	0.004 (0.021)	-0.007 (0.019)
Never Married Women with Children	0.074*** (0.018)	0.063** (0.026)	0.025*** (0.006)	0.068*** (0.026)	0.049* (0.028)	0.081*** (0.024)	0.085*** (0.022)

Notes: For each row column 1 and 2 is a separate regression, column 3 is a separate regression and column 4-7 a separate regression. ¹The regression involved using two dummy variables for the target group: unmarried mothers with a child less than age 6 and unmarried mothers with no child less than age 6. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 years or more, year fixed effects, state fixed effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. Heteroscedasticity adjusted standard errors in parentheses. * .05< p<.1, ** .01< p<.05, *** p<.01.

Table 10
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women in the Public Sector, 1992-2000
(Based on Difference-in-Differences Analysis using Married Women as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School Unmarried Women	0.009** (0.004)	0.001 (0.008)	0.002 (0.001)	0.001 (0.008)	0.002 (0.007)	0.006 (0.006)	0.014*** (0.005)
Unmarried Women with Children ¹	0.013** (0.006)	0.000 (0.010)	0.003 (0.002)	0.000 (0.010)	0.005 (0.009)	0.011 (0.009)	0.018*** (0.006)
At least a Child < 6 years old ²	0.008 (0.006)	0.003 (0.010)	0.002 (0.002)	0.004 (0.010)	-0.011 (0.009)	0.005 (0.010)	0.019*** (0.007)
No Child < 6 years old ³	0.020* (0.011)	-0.002 (0.020)	0.004 (0.003)	-0.003 (0.020)	0.024 (0.017)	0.017 (0.016)	0.018 (0.012)
Never Married Women with Children ¹	0.018*** (0.006)	-0.006 (0.010)	0.006** (0.002)	-0.006 (0.010)	0.010 (0.011)	0.017 (0.007)	0.021*** (0.007)
Education = High school Unmarried Women	0.004 (0.005)	-0.001 (0.007)	0.003* (0.002)	-0.003 (0.007)	0.001 (0.008)	0.004 (0.006)	0.005 (0.006)
Unmarried Women with Children ¹	0.004 (0.007)	0.002 (0.010)	0.002 (0.002)	-0.001 (0.010)	0.010 (0.12)	0.002 (0.009)	0.002 (0.008)
At least a Child < 6 years old ²	0.012 (0.009)	0.008 (0.012)	0.002 (0.003)	0.007 (0.012)	0.028 (0.015)	0.008 (0.011)	0.005 (0.010)
No Child < 6 years old ³	-0.001 (0.011)	-0.003 (0.016)	0.004 (0.003)	-0.009 (0.016)	-0.004 (0.018)	-0.002 (0.014)	-0.001 (0.013)
Never Married Women with Children ¹	0.005 (0.009)	0.018 (0.013)	0.001 (0.003)	0.013 (0.013)	0.009 (0.016)	0.004 (0.011)	0.001 (0.011)

Notes: Each row in column 1 and 2 is a separate regression. Each row in column 3 is a separate regression and each row in column 4-7 is also a separate regression. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 or more, year effects, state effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. ¹Married Women with at least a child as the comparison group. ²Married Women with at least a child less than six as the comparison group. ³Married Women with no child less than six years (but at least a child between 6-18) as the comparison group.

Heteroscedasticity adjusted standard errors in parentheses.

* .05< p<.1, ** .01< p<.05, *** p<.01.

Table 11
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women in the Public Sector, 1992-2000
(Based on Difference-in-Differences Analysis using Unmarried Women without Children as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School							
Unmarried Women with Children	0.005 (0.006)	-0.000 (0.009)	-0.000 (0.002)	0.000 (0.009)	0.005 (0.009)	0.005 (0.008)	0.006 (0.007)
At least a Child < 6 years old ¹	0.010 (0.006)	0.002 (0.009)	0.002 (0.002)	0.002 (0.009)	-0.001 (0.008)	0.010 (0.009)	0.017** (0.007)
No Child < 6 years old ¹	-0.001 (0.008)	-0.004 (0.015)	-0.004 (0.002)	-0.003 (0.015)	0.013 (0.014)	-0.002 (0.012)	-0.008 (0.009)
Never Married Women with Children	0.009 (0.006)	-0.006 (0.009)	0.003 (0.002)	-0.005 (0.009)	0.008 (0.010)	0.010 (0.010)	0.009 (0.007)
Education = High school							
Unmarried Women with Children	0.002 (0.007)	0.008 (0.010)	0.001 (0.002)	0.005 (0.010)	0.024** (0.012)	-0.004 (0.009)	-0.006 (0.009)
At least a Child < 6 years old ¹	0.004 (0.008)	0.011 (0.011)	0.000 (0.002)	0.009 (0.011)	0.034** (0.014)	-0.003 (0.010)	-0.007 (0.009)
No Child < 6 years old ¹	0.000 (0.009)	0.007 (0.014)	0.001 (0.003)	0.002 (0.014)	0.015 (0.016)	-0.006 (0.012)	-0.005 (0.011)
Never Married Women with Children	0.006 (0.009)	0.026** (0.013)	-0.000 (0.003)	0.022* (0.013)	0.026* (0.016)	0.000 (0.012)	-0.004 (0.011)

Notes: For each row column 1 and 2 is separate regression, column 3 is a separate regression and column 4-7 a separate regression. ¹The regression involved using two dummy variables for the target group: unmarried mothers with a child less than age 6 and unmarried mothers with no child less than age 6. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 years or more, year fixed effects, state fixed effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. Heteroscedasticity adjusted standard errors in parentheses. * .05<p<.1, ** .01<p<.05, *** p<.01.

Table 12
Description of the Employment Sample: Central City and Non-central city Areas, CPS, 1992-2000

Description of Target and Comparison Groups	Central City		Non-central City	
	Size of the Group	Probability of Being Employed	Size of the Group	Probability of Being Employed
Women 18-44, Education < High School				
Comparison Groups: Married Women	5513	0.410	11621	0.454
Married Women with Children	4632	0.390	9532	0.439
Unmarried Women with No Children	3865	0.430	7528	0.489
Target Groups: Unmarried Women	8181	0.382	12699	0.455
Unmarried Women with Children	4316	0.338	5171	0.405
Never Married Women with Children	2539	0.287	2413	0.342
Women 18-44, Education = High School				
Comparison Groups: Married Women	8671	0.637	40268	0.694
Married Women with Children	6654	0.611	31763	0.673
Unmarried Women with No Children	5950	0.692	15155	0.744
Target Groups: Unmarried Women	10773	0.637	25679	0.718
Unmarried Women with Children	4823	0.568	10524	0.681
Never Married Women with Children	2577	0.509	3891	0.607

Table 13
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women in Central City Areas, 1992-2000
(Based on Difference-in-Differences Analysis using Married Women as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School Unmarried Women	0.035 (0.024)	-0.039 (0.049)	0.024*** (0.008)	-0.021 (0.050)	-0.005 (0.032)	-0.018 (0.042)	0.081*** (0.028)
Unmarried Women with Children ¹	0.068** (0.029)	-0.081 (0.057)	0.042*** (0.010)	-0.064 (0.057)	0.015 (0.038)	0.037 (0.050)	0.119*** (0.034)
At least a Child < 6 years old ²	0.071* (0.036)	-0.067 (0.071)	0.045*** (0.013)	-0.056 (0.071)	0.035 (0.048)	-0.012 (0.063)	0.122*** (0.043)
No Child < 6 years old ³	0.052 (0.048)	-0.114 (0.096)	0.034** (0.017)	-0.085 (0.097)	-0.015 (0.061)	0.085 (0.082)	0.105* (0.056)
Never Married Women with Children ¹	0.042 (0.033)	-0.099 (0.060)	0.041*** (0.012)	-0.077 (0.061)	-0.033 (0.043)	0.056 (0.057)	0.103*** (0.039)
Education = High school Unmarried Women	0.008 (0.018)	0.005 (0.033)	0.002 (0.006)	-0.003 (0.033)	0.039 (0.025)	-0.021 (0.025)	-0.003 (0.022)
Unmarried Women with Children ¹	0.030 (0.024)	0.013 (0.041)	0.007 (0.008)	0.006 (0.042)	0.043 (0.033)	-0.005 (0.034)	0.037 (0.031)
At least a Child < 6 years old ²	0.064* (0.035)	0.068 (0.058)	0.002 (0.012)	0.060 (0.058)	0.069 (0.046)	0.035 (0.052)	0.074* (0.044)
No Child < 6 years old ³	-0.005 (0.034)	-0.052 (0.059)	0.015 (0.011)	-0.058 (0.059)	0.010 (0.048)	-0.032 (0.045)	-0.004 (0.044)
Never Married Women with Children ¹	0.058* (0.031)	0.047 (0.048)	0.015 (0.010)	0.048 (0.049)	0.052 (0.041)	0.007 (0.043)	0.097** (0.039)

Notes: Each row in column 1 and 2 is a separate regression. Each row in column 3 is a separate regression and each row in column 4-7 is also a separate regression. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 or more, year effects, state effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. ¹Married Women with at least a child as the comparison group. ²Married Women with at least a child less than six as the comparison group. ³Married Women with no child less than six years (but at least a child between 6-18) as the comparison group.

Heteroscedasticity adjusted standard errors in parentheses.

* .05< p<.1, ** .01< p<.05, *** p<.01.

Table 14
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women in the Central City Areas, 1992-2000
(Based on Difference-in-Differences Analysis using Unmarried Women without Children as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School							
Unmarried Women with Children	0.055* (0.030)	-0.039 (0.051)	0.033*** (0.010)	-0.030 (0.051)	0.030 (0.038)	0.114** (0.049)	0.064* (0.036)
At least a Child < 6 years old ¹	0.061* (0.032)	-0.030 (0.054)	0.036*** (0.011)	-0.026 (0.054)	0.060 (0.042)	0.100* (0.053)	0.059 (0.040)
No Child < 6 years old ¹	0.053 (0.035)	-0.054 (0.072)	0.030** (0.012)	-0.037 (0.073)	-0.003 (0.047)	0.149** (0.066)	0.071 (0.043)
Never Married Women with Children	0.033 (0.033)	-0.061 (0.054)	0.032*** (0.012)	-0.048 (0.055)	-0.010 (0.042)	0.140*** (0.055)	0.043 (0.041)
Education = High school							
Unmarried Women with Children	0.047* (0.024)	0.094** (0.042)	0.013* (0.008)	0.088** (0.042)	0.020 (0.032)	0.066* (0.034)	0.055* (0.030)
At least a Child < 6 years old ¹	0.081*** (0.028)	0.173*** (0.049)	0.021** (0.009)	0.166*** (0.050)	0.045 (0.039)	0.100** (0.043)	0.096*** (0.036)
No Child < 6 years old ¹	0.009 (0.028)	0.007 (0.052)	0.006 (0.009)	0.002 (0.051)	-0.007 (0.040)	0.033 (0.039)	0.008 (0.037)
Never Married Women with Children	0.070** (0.030)	0.119** (0.049)	0.021** (0.010)	0.121** (0.049)	0.031 (0.040)	0.079** (0.043)	0.104*** (0.038)

Notes: For each row column 1 and 2 is a separate regression, column 3 is a separate regression and column 4-7 a separate regression. ¹The regression involved using two dummy variables for the target group: unmarried mothers with a child less than age 6 and unmarried mothers with no child less than age 6. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 years or more, year fixed effects, state fixed effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. Heteroscedasticity adjusted standard errors in parentheses. * .05<p<.1, ** .01<p<.05, *** p<.01.

Table 15
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women in non-Central city Areas, 1992-2000
(Based on Difference-in-Differences Analysis using Married Women as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School Unmarried Women	0.049*** (0.017)	0.035 (0.026)	0.021*** (0.006)	0.033 (0.026)	0.025 (0.027)	0.046** (0.023)	0.060*** (0.020)
Unmarried Women with Children ¹	0.127*** (0.022)	0.023 (0.035)	0.044*** (0.008)	0.025 (0.035)	0.077** (0.036)	0.113*** (0.032)	0.156*** (0.026)
At least a Child < 6 years old ²	0.107*** (0.029)	0.006 (0.045)	0.047*** (0.010)	0.000 (0.045)	0.016 (0.050)	0.107** (0.043)	0.140 (0.035)
No Child < 6 years old ³	0.151*** (0.034)	0.037 (0.056)	0.044*** (0.012)	0.046 (0.056)	0.148*** (0.053)	0.121** (0.048)	0.173*** (0.040)
Never Married Women with Children ¹	0.153*** (0.029)	0.009 (0.043)	0.053*** (0.010)	0.005 (0.044)	0.084* (0.048)	0.140*** (0.042)	0.186*** (0.034)
Education = High school Unmarried Women	0.040*** (0.009)	0.020 (0.013)	0.014*** (0.003)	0.018 (0.013)	0.027* (0.016)	0.039*** (0.012)	0.045*** (0.011)
Unmarried Women with Children ¹	0.067*** (0.013)	0.038** (0.019)	0.023*** (0.004)	0.035* (0.020)	0.069*** (0.021)	0.056*** (0.017)	0.074*** (0.016)
At least a Child < 6 years old ²	0.123*** (0.020)	0.098*** (0.029)	0.036*** (0.007)	0.099*** (0.029)	0.078** (0.033)	0.131*** (0.027)	0.134*** (0.024)
No Child < 6 years old ³	0.019 (0.017)	-0.021 (0.026)	0.014** (0.005)	-0.029 (0.026)	0.063 (0.028)	-0.006 (0.022)	0.019 (0.021)
Never Married Women with Children ¹	0.103*** (0.021)	0.094*** (0.029)	0.035*** (0.007)	0.095*** (0.029)	0.126*** (0.035)	0.095*** (0.026)	0.102*** (0.025)

Notes: Each row in column 1 and 2 is a separate regression. Each row in column 3 is a separate regression and each row in column 4-7 is also a separate regression. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 or more, year effects, state effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. ¹Married Women with at least a child as the comparison group. ²Married Women with at least a child less than six as the comparison group. ³Married Women with no child less than six years (but at least a child between 6-18) as the comparison group.

Heteroscedasticity adjusted standard errors in parentheses.

* .05< p< .1, ** .01< p< .05, *** p< .01.

Table 16
Estimates of Effect of Time Limits and Other Restrictions on the Employment of Unmarried Women in the Non-central City Areas, 1992-2000
(Based on Difference-in-Differences Analysis using Unmarried Women without Children as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School							
Unmarried Women with Children	0.099*** (0.023)	-0.017 (0.035)	0.029*** (0.008)	-0.010 (0.035)	0.062 (0.038)	0.074** (0.032)	0.134*** (0.028)
At least a Child < 6 years old ¹	0.130*** (0.027)	-0.026 (0.039)	0.043*** (0.009)	-0.023 (0.040)	0.057 (0.047)	0.134*** (0.038)	0.158*** (0.032)
No Child < 6 years old ¹	0.064** (0.029)	-0.001 (0.050)	0.013 (0.010)	0.012 (0.050)	0.066 (0.047)	0.003 (0.041)	0.105*** (0.035)
Never Married Women with Children	0.123*** (0.030)	-0.033 (0.042)	0.037*** (0.010)	-0.031 (0.043)	0.074 (0.049)	0.102** (0.042)	0.155*** (0.035)
Education = High school							
Unmarried Women with Children	0.057*** (0.015)	0.034 (0.021)	0.019*** (0.005)	0.034 (0.021)	0.081*** (0.025)	0.051*** (0.019)	0.052*** (0.018)
At least a Child < 6 years old ¹	0.115*** (0.018)	0.086*** (0.028)	0.035*** (0.006)	0.090*** (0.028)	0.107*** (0.032)	0.121*** (0.025)	0.115*** (0.022)
No Child < 6 years old ¹	0.004 (0.016)	-0.017 (0.026)	0.005 (0.005)	-0.020 (0.026)	0.056** (0.028)	-0.012 (0.022)	-0.005 (0.020)
Never Married Women with Children	0.091*** (0.022)	0.086*** (0.030)	0.029** (0.007)	0.089*** (0.030)	0.131*** (0.037)	0.091*** (0.027)	0.075*** (0.026)

Notes: For each row column 1 and 2 is a separate regression, column 3 is a separate regression and column 4-7 a separate regression. ¹The regression involved using two dummy variables for the target group: unmarried mothers with a child less than age 6 and unmarried mothers with no child less than age 6. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 years or more, year fixed effects, state fixed effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. Heteroscedasticity adjusted standard errors in parentheses. * .05<p<.1, ** .01<p<.05, *** p<.01.

Table 17
Description of the Sample for Fertility and Marriage Analysis, CPS 1992-2000

Description of Target and Comparison Groups	Size of the Group	Probability of Being Married
Marriage Analysis, Women 18-44		
Comparison Groups: Education = 13-15 years	82808	0.497
Education = 13-15 years with Children	43497	0.726
Target Groups: Education < High School	38014	0.451
Education < High School with Children	23651	0.599
Education = High School	85391	0.573
Education = High School with Children	53764	0.714
Fertility Analysis, Women 18-44	Size of the Group	Probability of Having a Child Last year
Comparison Groups: Education = < High School, Married	66073	0.082
Education = 13-15 years, Unmarried	41687	0.020
Education = < High School, Married with Children	50882	0.073
Education = 13-15 years, Unmarried with Children	11487	0.034
Target Groups: Education = < High School, Unmarried	57332	0.044
Education = < High School, Unmarried with Children	23799	0.062

Table 18
Estimates of Effect of Time Limits and Other Restrictions on the Marriage Rate of Low Educated Women, 1992-2000
(Based on Difference-in-Differences Analysis using Better Educated Women as the Comparison Group)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education<High School							
All Women ¹	0.039*** (0.007)	-0.001 (0.011)	0.011*** (0.002)	0.000 (0.011)	0.030*** (0.010)	0.031*** (0.010)	0.048*** (0.008)
All Women with at least a Child ²	0.062*** (0.009)	-0.005 (0.014)	0.018** (0.003)	-0.005 (0.014)	0.040*** (0.013)	0.048*** (0.013)	0.081*** (0.010)
Education = High School							
All Women ¹	-0.003 (0.005)	-0.003 (0.007)	-0.001 (0.002)	-0.003 (0.008)	0.001 (0.008)	-0.005 (0.007)	-0.004 (0.006)
All Women with at least a Child ²	-0.001 (0.006)	-0.015 (0.009)	-0.001 (0.002)	-0.014 (0.009)	0.005 (0.010)	-0.010 (0.009)	0.002 (0.008)

Notes: ¹Women with 13-15 years of education as the control group. ² Women who have 13-15 years of education and at least a child as the control group. For each row column 1 and 2 is a separate regression, column 3 is a separate regression and column 4-7 a separate regression. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 years or more, year fixed effects, state fixed effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. Heteroscedasticity adjusted standard errors in parentheses. * .05<p<.1, ** .01<p<.05, *** p<.01.

Table 19
Estimates of Effect of Time Limits and Other Restrictions on the Fertility of Low Educated Women, 1992-2000
(Based on a Difference-in-Differences Analysis)

Treatment Group	Time Limit	Other Reforms	Years Since Time Limit	Low Intensity Reform	Only Time Limit	Medium Intensity Reform	High Intensity Reform
Education = < High School ¹	0.004 (0.004)	-0.000 (0.005)	0.002 (0.001)	-0.001 (0.006)	0.003 (0.007)	0.010 (0.006)	0.000 (0.005)
Education = < High School with Children ²	0.010* (0.006)	0.002 (0.008)	0.003* (0.002)	0.002 (0.008)	0.005 (0.010)	0.013 (0.008)	0.010 (0.008)
Education = < High School ³	0.001 (0.003)	-0.006 (0.004)	0.001 (0.001)	-0.006 (0.004)	-0.001 (0.005)	0.001 (0.005)	0.001 (0.004)
Education = < High School with Children ⁴	0.005 (0.007)	-0.007 (0.010)	0.002 (0.002)	-0.009 (0.010)	-0.002 (0.011)	0.002 (0.010)	0.011 (0.009)

Note: ¹Married women with high school or lower education as the comparison group; ² Married women with high school or low education with children as the comparison group. ³ Unmarried women with 13-15 years of education as the comparison group; ⁴ Unmarried mothers with 13-15 years of education as the comparison group. For each row column 1 and 2 is a separate regression, column 3 is a separate regression and column 4-7 a separate regression. All the regressions control for age, race, other family income, number of kids younger than 6 years, number of kids age 6 years or more, year fixed effects, state fixed effects, unemployment rate- current, one and two year lags and interactions of unemployment rates with the treatment group. Heteroscedasticity adjusted standard errors in parentheses. * .05< p<.1, ** .01< p<.05, *** p<.01.

Chapter 3

Immigrant and Native Responses to Welfare Reform

Main Findings

In this paper, I investigate the effect of federal welfare reform on the employment, hours of work and marriage rates of four groups of low-educated women: foreign-born citizens, foreign-born non-citizens, all foreign-born women- citizens as well as non-citizens, and native-born citizens. Among foreign-born women, I investigate whether the behavioral response to welfare reform differed by recency of immigration. Finally, because some states created programs to insure that all legal immigrants remained eligible for benefits under the Temporary Assistance to Needy Families (TANF) program and others did not, I compare the response of foreign-born between these states to investigate whether the immigrant provisions of federal welfare reform legislation had a “chilling” effect.

The results suggest that welfare reform induced native-born citizens and foreign-born non-citizens to increase their employment and attachment to the labor market. TANF appears to have had a larger effect on the least educated native-born women and among foreign-born, a larger effect on more recent arrivals. The “chilling” hypothesis that has received so much attention in the popular press is not supported by my results. Finally, my estimates indicate that TANF had no effect on native- and foreign-born women’s marriage decisions.

INTRODUCTION

When Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) comes for reauthorization before the US Congress this year, one of its most controversial feature – treatment of immigrants - is likely to be an issue of heated discussion. PRWORA dramatically altered the nation's immigrant policy—government programs aimed at promoting the social and economic integration of immigrants. Federal welfare reform legislation barred future legal immigrants (those arriving after the passage of the law) from receiving cash assistance under the Temporary Assistance to Needy Families (TANF) program, as well as most other federal means-tested benefits (e.g., food stamps), for five years. The Federal government left it up to states' discretion whether current legal immigrants would be eligible for such assistance. Notably, every state but Alabama maintained the TANF eligibility of current legal immigrants and 19 states created state programs to provide TANF-like assistance to future legal immigrants during the five-year period for which they were barred from receiving federal benefits (Zimmerman and Tumlin 1999). The heterogeneous state responses to PRWORA, as well as subsequent federal legislation restoring some lost benefits to immigrants, makes it clear that the nation does not share a common vision related to the treatment of immigrants.

Why were immigrants singled out for special treatment? One possible explanation is that the number of immigrants to the country had been growing rapidly prior to the passage of the law, as was the foreign born share of the population. Naturally, policy makers were concerned about the effect of this large influx of immigrants. For example, immigrants may have contributed to the stagnating wages of

less skilled workers and worsened wage inequality, which was also growing during this period. The increase in immigration prior to PRWORA was truly large. According to the Immigration and Naturalization Service (INS), approximately 13.5 million legal immigrants came to the United States between 1981 and 1996 and perhaps as many as 5 million undocumented immigrants also entered the country during this period. The only other period in which such a large number of immigrants entered the country was between 1900 and 1920. In sum, the growth in immigration and the potentially adverse effects of that immigration may have created some anti-immigrant sentiment (a new nativism) that became manifest in the federal welfare reform law.

A second potential explanation for why immigrants were treated differently was that it was seen as a way to address two problems associated with immigration that were directly related to the Aid to Families with Dependent Children (AFDC) program. Not only was there a large number of immigrants entering the U.S. prior to PRWORA, but the immigrants that were entering were less educated (relative to natives) than previous immigrants reflecting the increase in the number of immigrants from Latin America and Asia (INS 1996). Moreover, recent immigrants were more likely to use AFDC and other welfare programs than were earlier immigrants and immigrants had higher rates of participation in welfare programs than natives (Borjas 1995, Borjas and Hilton 1996).¹ This increasingly greater use of public funds erodes the fiscal benefits associated with immigration and led to concerns that immigrants, particularly newer immigrants, were a fiscal drag—i.e., consuming more than they were producing (Borjas and Hilton 1996,

¹ It remains true, however, that conditional on observable characteristics, immigrants were less likely to use AFDC than non-immigrants (Butcher and Hu 2000, Borjas and Hilton 1996). However, immigrant use of all social welfare programs was greater than that of natives even after adjusting for observable characteristics (Borjas and Hilton 1996).

Smith and Edmonston 1997). There was also a debate over whether immigrants were attracted to states such as California and New York that have generous cash assistance benefits (Borjas 1999). Therefore, barring immigrants from cash assistance and other federal safety net programs was seen as a way to save money and increase the fiscal benefits of immigration and to reduce “adverse” immigration—i.e, immigration by future welfare recipients.

The immigrant provisions of PRWORA were not universally supported and future debates over this issue will benefit from evidence of the effect of welfare reform on immigrants. In this paper, I provide such evidence by investigating the effect of PRWORA on the employment, hours of work and marriage rates of four groups of low-educated women: foreign-born citizens, foreign-born non-citizens, all foreign-born women- citizens as well non-citizens - and native-born citizens. The objectives of welfare reform were to reduce dependency on public assistance by encouraging women to work and to change behaviors that create dependency (e.g., non-marital births). Therefore, my analyses of employment and marriage are central to the evaluation of the success of welfare reform. In addition, the immigrant provisions of welfare reform grew out of perceptions about immigrant use of cash assistance and were intended to save money by barring some immigrants from receiving benefits. My investigation of the effect of welfare reform on foreign-born women, both citizens and non-citizens, will contribute to and expand knowledge about differences between foreign- and native-born women’s use of public assistance and their response to welfare reform. Importantly, I investigate whether the behavioral response to welfare reform differed by recency of immigration. And my analysis of the effect of welfare reform on foreign-born women, a

group which consists partly of newly barred legal immigrants, will provide evidence of the potential savings associated with the immigrant provisions of welfare reform.

Finally, because some states created programs to insure that all legal immigrants remained eligible for TANF and others did not, I can compare the response of foreign-born non-citizens between these states to investigate whether there was a “chilling” effect of the immigrant provisions of federal welfare reform legislation.

PRWORA’S INCENTIVES: IMMIGRANTS AND NATIVES

A primary goal of welfare reform was to promote economic self-sufficiency for native-born Americans as well as immigrants. To accomplish this goal, the federal government made receipt of cash assistance temporary and required welfare recipients to work, thereby eliminating the guarantee in the Aid to Families with Dependent Children (AFDC) program of long-term income support for unmarried mothers with dependent children. One would expect that under the new policies current and potential welfare participants would begin to make life choices that would improve their chances for self-support.

The federal welfare reform law does not distinguish among citizens by nativity and the time limited benefits and other provisions of PRWORA apply to all citizens. However, the behavioral response (e.g., changes in employment and marital decisions) to welfare reform may differ between foreign- and native-born citizens. For example, Borjas and Hilton (1996) report that foreign-born women have longer average spells of AFDC receipt than do native-born women. As a result, welfare reform may have a larger effect on foreign-born women than it does on native-born citizens. The reason for this is that the reduction in lifetime benefits associated with PRWORA’s time limits represents a

larger change in policy for foreign-born women who expect to receive benefits for a longer period. Other considerations, however, suggest that foreign-born women may have a smaller behavioral response. For example, foreign-born women may have inferior labor market opportunities compared to native-born women, say because of language barriers, discrimination and local demand conditions. In this case, provisions of PRWORA such as the work requirements will not be as utility reducing for foreign-born women relative to native-born women and foreign-born women will be less likely to exit welfare in response to this aspect of welfare reform (Besley and Coate 1992, 1995). In general, the behavioral response to welfare reform will differ depending on the underlying cause of welfare participation, which may differ by women's nativity status. I examine this issue explicitly in the empirical analysis and in doing so investigate whether there is heterogeneity in the behavioral response of immigrants by citizenship and recency of immigration.

An important aspect of PRWORA is the distinction it makes among foreign-born non-citizens. Specifically, it creates two eligibility classes among non-citizen legal permanent residents. Legal immigrants who arrived in the U.S. prior to August 22, 1996 were eligible for AFDC and remained eligible for TANF in every state but Alabama. Legal immigrants arriving in the U.S. after August 22, 1996 are ineligible for TANF for five years unless they live in one of the 19 states that made state funds available to maintain the eligibility of this group. Thus, all legal immigrants are affected by welfare reform, but some newly arrived immigrants face the more draconian policy of being denied access to government cash assistance and other benefits such as Medicaid and

food stamps. Thus, the expectation is that newly arrived legal immigrants would have a larger behavioral response to TANF than legal immigrants who arrived earlier.

Finally, since some states maintained eligibility of newly arrived immigrants, I can compare the behavioral response of new immigrants in these states to that of new immigrants in states that did not maintain eligibility to test whether there was a “chilling” effect of PRWORA. There have been many reports that the immigrant provisions of PRWORA have discouraged participation among eligible immigrants who are confused or frightened by the immigrant provisions of the law (Fix and Passel 1999, Zimmerman and Fix 1998). No difference in the behavioral responses of foreign-born non-citizens in these two groups of states would be evidence consistent with a “chilling” effect since actual eligibility differences did not matter as much as passage of the law.

PREVIOUS RESEARCH

To my knowledge, there have been only two previous studies of the effect of federal welfare reform on immigrants’ use of cash assistance. Fix and Passell (1999) compared changes in welfare participation between 1994 and 1997 among citizens and non-citizens. They found that non-citizen participation in welfare, defined as participation in AFDC/TANF, SSI and General Assistance, declined significantly more than did citizen participation. These authors concluded that most of the decline (relative to citizens) is due to a “chilling” effect since very few of the non-citizens were ineligible for benefits during the period studied. Borjas (2000) studied the same issue and found that the decline in welfare participation observed by Fix and Passell (1999) was largely

driven by the trend in California, which predated implementation of PRWORA in California.

The major weakness with these studies is their simple before and after design. Changes in welfare participation rates may be driven by a variety of factors, for example macroeconomic changes, and these studies do not control for these factors. Specifically, citizens and non-citizens may face different economic conditions or may have different responses to equivalent economic changes. Indeed, the spatial concentration of non-citizens strongly suggests that citizens and non-citizens face different labor market conditions. Therefore, it is not clear whether welfare reform is the cause of the relative (to citizens) decline in welfare participation. A second weakness of these studies is that the data extend only through 1997 or 1998, and federal welfare reform had not been widely implemented by that time.

In contrast to these studies, my analysis controls for several factors that may affect decisions about work and welfare participation. Most importantly, I include explicit and implicit controls for macroeconomic conditions and policy changes during the period of my study. I also use data that extend through 1999 by which time federal welfare reform had been completed. Finally, I do not examine welfare participation, but rather the determinants of welfare participation: employment and marital status. While the outcomes are linked, changes in the caseload are not the converse of changes in employment or marital status. It is interesting from a policy point of view to investigate how much of the change in the caseload can possibly be explained by changes in employment or marriage.

RESEARCH DESIGN

To estimate the effect of welfare reform on employment, hours of work and marriage, I use a quasi-experimental research design commonly referred to as a difference-in-differences (DD) analysis. The DD procedure compares the change over time in outcomes of a group affected by welfare reform, the target group, to the change over time in outcomes of a similar group that is unaffected by welfare reform, the comparison group. The underlying logic of the DD methodology is illustrated in Table 1, which refers to employment, but a similar logic is applied to the analysis of other outcomes.

Table A
Mean Employment Before and After Welfare Reform

Period / Group	Before Welfare Reform (e.g., 1995)	After Welfare Reform (e.g., 1999)	After-Before
Target Group (Affected by Reform)	A	B	B-A
Comparison Group (Unaffected by Reform)	C	D	D-C
Difference-in-Differences	(B-A)-(D-C)		

In Table A, the difference B-A measures the change in employment of the target group, the group affected by welfare reform, before and after welfare reform. This difference may be due to the effect of welfare reform and other factors that change over time. The difference D-C measures the change in employment before and after welfare reform of the comparison group, a group similar to the target group, but a group unaffected by welfare reform. Changes in the employment of the comparison group are due to other factors since this group is unaffected by welfare reform. Thus, the

difference-in-differences, (B-A)-(D-C), measures effect of welfare reform on the target group. The effect of other factors is eliminated by subtracting the before and after change of the comparison group from the before and after change of the target group. Obviously, a critical assumption of the DD procedure is that changes in employment caused by other factors are the same for the target and comparison groups.

The DD analysis can also be cast in a regression framework. For example, the regression specification that corresponds to Table A is as follows:

$$(1) \text{Emp}_{ikt} = \alpha_0 + \alpha_1 \text{Treat}_k + \alpha_2 \text{Reform}_t + \alpha_3 (\text{Treat}_k \times \text{Reform}_t) + e_{ikt}$$

where Emp_{ikt} is an indicator of whether woman 'i' in group 'k' in year 't' is employed. The variable Treat_k in equation (1) denotes membership in my target group. Reform_t is an indicator equal to one if it is an observation taken after welfare reform. The key parameter in equation (1) is α_3 , which is the DD estimate corresponding to (B-A)-(D-C).

As written, the DD estimate obtained from a regression using the equation (1) specification would equal the estimate obtained by the subtractions in Table A. This simple specification of the regression model generates no advantage over the differences in means in Table A. A more complex specification of the regression model that includes controls for personal characteristics, unmeasured state effects and unmeasured time effects does have some advantage. Notably, DD estimates obtained from such a model will be more precise and allow for a more refined specification of time effects vis-a-vis policy effects. In practice, I estimate a model similar to equation (2) below.

$$(2) \text{Emp}_{ijt} = \beta_j + (\beta_{jk} \times \text{Treat}_k) + \delta_t + X_{ijt} \Gamma + Z_{jt} \lambda + \alpha_1 \text{Treat}_k + \alpha_2 \text{Waiver}_{jt} + \alpha_3 \text{TANF}_{jt} + \alpha_4 (\text{Treat}_k \times \text{Waiver}_{jt}) + \alpha_5 (\text{Treat}_k \times \text{TANF}_{jt}) + e_{ijt}$$

Equation (2) includes controls for state effects (β_j and β_{jk}), which I allow to differ by target and comparison group, year effects (δ_t), personal characteristics (X_{ijkt}) such as age, race and recency of immigration (for foreign born), and state level variables (Z_{jt}) such as the unemployment rate and state Earned Income Tax Credit (EITC). Equation (2) also reflects a more general specification of welfare reform by differentiating between AFDC waivers and TANF. This is particularly important because of the special immigrant provisions of TANF. Coefficients α_4 and α_5 measure the effect of AFDC waivers (α_4) and TANF (α_5) on the employment of the target group holding constant unmeasured time-varying factors correlated with welfare reform.

A strength of the difference-in-differences analysis is that it controls, in a parsimonious way, for time variation in outcomes that is unrelated to welfare reform, for example, due to macroeconomic changes.² Clearly, this statement is correct only if I have chosen the “right” comparison group. Assuming this to be the case, it implies that it is not as crucial in a DD analysis of the effect of welfare reform to control for macroeconomic activity as it is in other approaches (see Ziliak et al. 2000, Figlio and Ziliak 1999). To be cautious, however, my regression model includes a control for macroeconomic activity, specifically the state unemployment rate in the month of the survey. I also interact the unemployment rates with the dummy variable indicating membership in the target group. Similarly, I include controls for state EITC policies and

² An alternative to the difference-in-differences procedure is to restrict the sample to the target group and include state-specific trends. However, there is not enough time variation in TANF implementation to make this a feasible strategy. A regression of the TANF policy variable on just state and year dummies yields an R-square of 0.84.

interact the state-EITC variable with the dummy variable indicating the membership of my target group.³

STATISTICAL ISSUES

Validity of Comparison Groups

As noted, a crucial aspect of the DD analysis is the validity of the comparison group. For the employment analysis, I define the target group to be unmarried women with 12 or fewer years of education. These women are a reasonable target group since many of them are at risk of welfare receipt. Education and marital status are strong correlates of welfare participation and a large portion of the AFDC/TANF caseload consists of low-educated, unmarried women. Indeed, approximately 80 percent of the caseload in the early 1990s consisted of women with 12 or fewer years of education and an equally large proportion were unmarried (Kaushal and Kaestner 2000). The comparison group corresponding to this target group is married women with 12 or fewer years of education. Eligibility for AFDC/TANF is largely determined by family composition and there is a large difference in welfare participation rates between low-educated married women and low-educated unmarried women. While some low-educated married women are surely at risk of welfare participation, the majority is not.⁴ The inclusion of women at risk of welfare receipt in the comparison group will bias my estimates toward zero with the size of the bias depending on the difference in the

³ None of the estimates associated with the interaction term between state-EITC and the target group is statistically significant. Nor is the estimate associated with the interaction term between unemployment rate and the treatment group statistically significant. These results lend some credibility to the validity of my comparison groups.

⁴ Figures calculated from the CPS from 1994 shows that approximately 20 percent of the welfare caseload is married.

proportion of at-risk women in the target and comparison groups. For example, if 60 percent of the women in the target group are at risk of welfare receipt and 20 percent of the women in the comparison group are at risk of welfare receipt, the DD estimate will be 40 percent of the true estimate.

The downward bias caused by misclassification of target and comparison group members also applies to my analysis of foreign-born women. A portion of non-citizens in the target group will be undocumented immigrants. These women were never eligible for cash assistance, and therefore, welfare reform had no effect on their eligibility.

In order to gauge the sensitivity of estimates to my choice of comparison group, I obtain estimates of all models using three comparison groups. My second comparison group is unmarried women with 13 to 15 years of education.⁵ The problem with using this group is that a significant fraction of it, for example those with children, is at risk of welfare receipt. Thus, the DD estimates using this group are likely to be downward biased. My third comparison group is married women with 13-15 years of education. This group is less likely to be at risk of receiving welfare than my other two comparison groups, but this group differs from the target group both in terms of marital status and education. The employment experience of this group, therefore, may deviate from the employment experiences of my target group. In general, there is no perfect comparison group, and selecting a comparison groups usually involves a tradeoff between a group that is very similar to the target group, but which may be affected by the policy of interest, and a group that is clearly not affected by the policy, but which may be dissimilar to the target group.

While the key assumption underlying the DD methodology is that the trend in employment (that is unrelated to policy changes) is the same for the target and comparison group, similar levels of employment are also important. For example, it is not always obvious whether the appropriate “differences” in the difference-in-differences procedure should be measured in absolute or relative (i.e., percentage) terms. The importance of this distinction is obviated when both groups have the same level of employment. The similarity of the mean level of employment between the target and comparison groups also provides initial evidence that the two groups have similar labor market experiences and is consistent with the fundamental identifying assumption underlying the DD analysis.

In order to provide some evidence of the validity of my comparison groups, I present the mean employment rates for the different groups in Figures 1 and 2. The data underlying Figures 1 and 2 were drawn from the March Current Population Surveys of 1979 to 2000, and refer to employment in the week of the survey. Figure 1 presents mean rates of employment for the target group—unmarried women with less than a high school degree—and the three comparison groups. It is clear that the level and trend in employment of unmarried (target group) and married (comparison group) women with less than a high school degree are quite similar over the period. Notably, there is a spike in the employment of unmarried women that occurs around 1997, or about the same time TANF was being implemented. It is also true that the trend in employment of women (married and unmarried) with 13 to 15 years of education is similar to that of the other two groups, but there is a perceptible increase in employment of unmarried women with

⁵ I do not include unmarried women with 16 or more years of education in the alternative comparison group because it is unlikely that their labor market experiences will be similar to that of women in the target

13 to 15 years of education in 1997 that suggests that this group may have also been affected by TANF. Unlike unmarried women with 13-15 years of education, married women with similar education do not experience any spike since 1997, consistent with my conjecture that the employment of married women with some college is not likely to be affected by welfare reform. Also, the level of employment is much higher for the more educated women. Overall, Figure 1 provides some justification for my choice of target and comparison groups and evidence consistent with my preference for the comparison group consisting of married women with similar education.

The evidence in Figure 2 is a bit more problematic in this regard. There appears to be significant differences in both the trend and level of employment of unmarried and married women with a high school degree in the period prior to 1994. In the period between 1994 and 1997, however, the level and trend in employment of the two groups are similar. Again there is a spike in the employment of unmarried women around 1997. Although not as clear, I believe that the married women with high school comparison group is superior, primarily because during the pre period of my analysis, roughly between 1994 and 1997, the level and trend in employment of this group is the same as that of the target group. Moreover, this is a group unlikely to be affected by welfare reform. But as noted above, I present estimates using all the three comparison groups for the reader's benefit since the superiority of my preferred comparison group cannot be established with certainty.

One confounding influence on employment that may have a differential effect on the target and comparison groups is the federal EITC, which expanded between 1994 and 1996. The EITC will likely affect the target group more than the comparison group and

group.

could confound estimates of the effect of AFDC waivers and TANF. However, this problem should bias my estimates of the effect of welfare reform (TANF) toward zero since TANF was implemented in 1997 and the EITC would have raised employment prior to 1997. As noted, I include a variable measuring state EITC policy.

A difference-in-differences approach is also used to examine the effect of welfare reform on marital status. For this analysis, I define the target group to be all women with 12 or fewer years of education and the comparison group to be all women with 13 to 15 years of education. Obviously, since the outcome is marital status, I cannot stratify on the basis of this variable. Therefore, I am relegated to using education to classify women into those most and least likely to be affected by welfare reform. This will undoubtedly increase the bias of the DD estimate, particularly when the target group is women with 12 years of education, but there are few feasible alternatives.

Selection

As noted, I study the effect of TANF and AFDC waivers on the employment of three groups of low-educated, unmarried women: native-born, foreign-born citizens and foreign-born non-citizens. Welfare reform may have affected citizenship, as some immigrants may have become naturalized citizens in order to be eligible for benefits. This compositional change would bias my estimates of the behavioral effect of welfare reform on these two groups of foreign-born women. However, it is worth noting that from a policy perspective, it is still interesting to identify the “total” effect of a policy, including any selection effects that are caused by the policy.

There is evidence of increased naturalization through out the 1990s, but data on naturalizations indicate a large increase in naturalizations before PRWORA was passed.

According to Fix and Haskins (2002), the increase in naturalizations during this period was mainly the result of increased immigration in the early 1990s and the 1986 Immigration Reform and Control Act, which granted permanent legal status to approximately 2.7 million immigrants. However, it is not possible to rule out that some legal immigrants actually naturalized in response to welfare reform and its differential treatment to non-citizens.

To address this issue, I combine the foreign-born citizens and non-citizens samples. This approach eliminates the selection issue relating to naturalization and allows me to identify the behavioral effect of welfare reform. The disadvantage of this approach is that the two groups received different treatments (i.e., they faced different policies). A portion of foreign-born non-citizens (the newly arrived) received a stronger treatment since they are denied benefits. Thus, I still expect the effect to be larger for the foreign-born group than native-born, but it may be more difficult from a statistical point of view to identify this expected effect. However, I identify new immigrants on the basis of their arrival year in the US, and a legal resident is allowed to apply for naturalization only after being in that status for five years. According to INS, the median number of years of residence in the US before naturalization is nine years. This suggests that the increased naturalization noticed during the nineties has not affected the status of immigrants who came after August 1996. It is reasonable to assume that all those who arrived after the passage of the law are non-citizens, and except for those who live in the 19 states that provide TANF to new immigrants the remaining are denied any benefits under the new law. Thus, the data allow us to identify new immigrants (i.e., post 1996)

and I can test whether new immigrants had a larger behavioral response consistent with the more draconian policy change that they faced.

A second type of selection relates to the decision by states to provide state-financed benefits as a substitute for federal benefits. This problem is sometimes referred to as policy endogeneity; for example, states with a large number of immigrants may have extended benefits to newly arrived immigrants. A review of state policy does not suggest that this is a problem. Of the top six immigrant states, which have approximately 75 percent of the total foreign-born population, only California provides state funds for the TANF needs of the legal immigrants who arrived in the US after August 1996. The other five top immigrant states - New York, Florida, Texas, Illinois and New Jersey- do not. Thus the states that provide TANF to new immigrants are not necessarily the ones that attract immigrants in large numbers. Nor do all large immigrant states deny new immigrants TANF benefits. According to estimates based on CPS data, in 1996, 48 percent of the foreign born population lived in states that had provided for state level funds for new immigrants. Contrary to the expectations of welfare magnet theorists, in 1998, this proportion fell to 46 percent.⁶ Moreover, Kaushal (2002) has extensively analyzed the location choices of new immigrants and she finds no evidence that state welfare (or other safety net) policies had an effect on the locational choices of this group.

DATA

The data used in the study is the Current Population Survey – Outgoing Rotation Group File for the years 1994 to 1999. The Outgoing Rotation Group File is a 25 percent

sample from each monthly Current Population Survey (CPS) file and contains information on nativity status, citizen status, and recency of immigration, as well as information on important welfare related outcomes such as employment, hours of work and marital status. Importantly, the Outgoing Rotation Group File (ORG) does not include information on fertility or welfare receipt and so I do not examine the effect of welfare reform on these outcomes. The ORG does, however, have information on other individual characteristics such as age, education level, ethnicity, race, and place of residence. I can use these in the regression analysis to increase the precision of the DD estimates. I limit the time period to the years 1994 to 1999 because information about nativity, citizenship and recency of immigration is unavailable prior to 1994.

One of the most important characteristics of the ORG data for the purposes of this study is the relatively large number of observations in these data. The March CPS file contains more comprehensive information (e.g., welfare receipt) about respondents than the ORG, but it does not have sufficient number of observations to carry out analyses separately by nativity and citizenship status. This stratification is essential to the goals of this study.

I focus on three samples of women: native-born citizens, foreign-born citizens and foreign-born non-citizens. As noted, I also combine the sample of foreign-born women, citizens as well as non-citizens to address potential selection problems related to the effect of welfare reform on naturalization.

In all cases, I limit the sample to women between 18 and 44 years of age because very few women over age 44 are at risk of welfare receipt. I exclude younger women because

⁶ These estimates are three-year averages of the foreign born population living in each state. The estimates are corrected for new weights for the years 1994 and 1995 computed by Jeffery Passel of the Urban

I use education to stratify the sample and there is little variation in education among women below age 18. I also exclude from the analysis women with more than 15 years of education. These women are not likely to be at risk of welfare receipt and their labor market and marital experiences are not likely to be comparable to those of the low-educated women in my target groups. For native-born citizens, I select a 25 percent random sample from the ORG to reduce the computational burden associated with the large number of observations for this demographic group. The 25 percent sample provides sufficient sample sizes.

Information about state level policies related to welfare reform is merged to the individual level data. Policy variables are measured as of the date they became effective. I use a variety of data sources to define the policy variables including the 1999 CEA report, information reported in Schoeni and Blank (2000), data maintained by the Urban Institute and data collected by the National Governors Association. I use two broad categories of reform: AFDC waivers and TANF.

Table 1 provides descriptive information about the employment rates of the samples and the sample sizes of my target and comparison groups for the U.S. as a whole. Among the foreign born groups, the sample sizes are not large, particularly for foreign-born citizens, but are sufficient to detect reasonable sized effects. For example, an estimate of the effect of welfare reform on employment is simply the difference in mean employment before and after welfare reform, or

$$(3) \pi = \overline{EMP}_{before} - \overline{EMP}_{after} .$$

Institute. We thank Jeffery Passel for sharing the corrected weights.

The variance of the effect is simply the variance of the difference in mean employment before and after welfare reform:

$$(4) \text{Var}(\pi) = 2\sigma^2 / N.$$

In equation (4), σ^2 is the variance of the binomial outcome of employment, which I assume is equal to 0.24 (implying a mean employment of 0.60). Equation (4) also assumes that there are an equal number of observations before and after welfare reform and that the variance of employment is equal in each period. To detect a significant effect, the estimate has to be 1.96 times larger than its standard error, or

$$\begin{aligned} \pi / \sqrt{\text{Var}(\pi)} &> 1.96 \\ (5) \pi / \sqrt{2\sigma^2 / N} &> 1.96 \\ N &> 2(1.96)^2 \sigma^2 / \pi^2 = 2(1.96)^2 (0.24 / \pi^2) \end{aligned}$$

The necessary sample size depends on the size of the true effect. So, to detect a true effect of 0.03, or a three percentage point change in the employment rate, the required sample size is 4098 ($2N$ in equation 5). The preceding calculation is meant to be more illustrative than definitive, for example this sample size calculation ignores type II errors and is based on a two-tailed (1.96) test, but it demonstrates that the sample sizes in Table 1 are sufficient.

The second point to note about Table 1 is the relatively similar employment rates of my target and comparison groups, particularly when marital status is used to define these groups. For example, among the native-born group, the employment rate of unmarried women without a high school degree is 0.437; the similar figure for married women without a high school degree is 0.505. Table 1 also indicates that education is a stronger correlate of employment than is marriage. For example, the employment rate for

unmarried women with 13 to 15 years of education is 0.753, which is 12.7 percentage points higher than the employment rate for unmarried women with 12 or fewer years of education.

RESULTS

Effects of Welfare Reform on Employment

Table 2 presents the DD estimates of the effect of AFDC waivers and TANF on the employment of unmarried women. Analyses were done separately for four groups: native-born citizens, foreign-born citizens, foreign-born non-citizens, and all foreign born women citizens as well as non-citizens. F-tests reject a model that pools native and foreign-born women. The top panel of Table 2 presents estimates obtained using married women with similar education levels as the comparison group. The middle panel presents estimates obtained using unmarried women with some college (13 to 15 years of education) as the comparison group. And the bottom panel presents estimates obtained with married women with some college as the comparison group. Each row (within column) of Table 2 presents estimates from a separate regression. So columns 1 and 2 in row 1 present DD estimates of the effect of AFDC waivers and TANF on unmarried women with 12 or fewer years of education, and columns 1 and 2 in row 2 present similar estimates for unmarried women with less than 12 years of education (i.e., without high school degree).

Note that I do not obtain estimates of the effect of AFDC waivers on the employment of foreign-born women (columns 3 to 5). The reason is the lack of within-state variation in state-level welfare policy (i.e., AFDC waivers) in the states where the

vast majority of foreign-born women live. Over 80 percent of foreign-born women lived in states that had no within-state variation in pre-TANF welfare policy and none of the top six “immigrant” states (CA, FL, IL, NJ, NY, TX) implemented a waiver during the period of my study.⁷ Three of these states – California, New Jersey and Illinois had major waivers in place during the period of the study, but these were introduced in 1992 or 1993 prior to 1994, the base year of my study. Therefore, there is little within state variation in AFDC waivers during the period of my study. I also do not present separate estimates by education level for foreign-born citizens because of small sample sizes. All estimates are obtained by ordinary least squares (OLS) regression using White’s (1980) correction for heteroscedasticity.

Estimates in the top panel of Table 2 indicate that TANF increased the employment of low-educated unmarried native-born women in the U.S. by approximately 2.7 percentage points, which represents a relative effect of about 4.3 percent. The effect of TANF was significantly larger for women without a high school degree than it was for women with a high school degree. Among those without a high school degree, TANF is associated with a 5.8 percentage point, or 13 percent, increase in employment. The similar effect for those with a high school degree is 1.5 percentage points or two percent, but the estimate is not statistically significant. The smaller effect of TANF on women with a high school degree is expected since fewer of these women are truly at risk of welfare receipt. In contrast to TANF, AFDC waivers had no statistically significant effect on the employment of native-born low-educated unmarried women. Similar estimates using other data sources and methodologies have been reported by Kaushal and Kaestner (2001) and the Council of Economic Advisers (1999).

⁷ We do include an AFDC waiver dummy variable in these regression models.

Estimates in the middle and bottom panels of Table 2 are for the most part similar to those in the top panel. The estimates associated with TANF policy tend to be somewhat smaller in magnitude in the bottom two panels, and the estimates associated with AFDC waivers are somewhat larger in the bottom panel than the top panel. In fact, estimates in the bottom panel suggest that AFDC waivers decreased employment of unmarried women with a high school degree. This is a counter intuitive finding. Overall, estimates in Table 2 indicate that TANF was associated with an increase in employment among unmarried, native-born women with less than a high school degree and AFDC waivers had no consistent effect.

While the TANF effect may seem too small to be consistent with the large declines in AFDC/TANF caseloads, it should be recognized that this is a downward biased estimate of the effect of TANF on those who are actually at risk of welfare receipt. Only a portion of the target group is truly at risk of welfare receipt and some portion of the comparison group is also at risk. Thus, the 2.7 percentage point increase in the employment rate associated with TANF may mask a much larger increase in the employment rate of women truly at risk. However, 2.7 percent of all low-educated unmarried native-born women represents approximately 326,854 women and suggests that employment increases related to TANF may have accounted for a relatively large part of the decline in the welfare caseload. According to the Department of Health and Human Services (www.acf.dhhs.gov/news/tables.html), between August 1996 and June 2000, the number of families on welfare declined by 2,207,000. And as reported by Fix and Haskins (2002), between 1994 and 1999, the number of foreign-born families on welfare fell by 310,000. This implies that about 1,897,000 native born families got off

the welfare rolls during the period of my study. If I assume that approximately 60 percent of this group of families were headed by unmarried women with 12 or fewer years of education, these figures suggest that the number of native-born unmarried women with 12 or fewer years of education who were receiving public assistance declined by 1,138,200. Thus, I estimate that approximately 29 percent of the decline in the caseload during this period among this group was due to increased employment related to TANF.

Obviously, these calculations provide only a rough estimate of the TANF-induced employment related decline in welfare caseloads. I have argued earlier that due to the contamination in my target and comparison groups, the estimated coefficients contain a downward bias. If, on the other hand, I allow for the fact that some women who are working are also on welfare, I would obtain a somewhat smaller estimate of the employment related decline in caseloads. The above calculations, however, make the point that the relatively small estimates in Table 2 can account for a significant portion of the change in the welfare caseload. My estimates are also similar to those reported in previous studies. Estimates from the CEA (1999) study suggest that a third of the caseload decline in this period was due to the administrative reform, only 8 to 10 percent due to economic expansion and associated improvement in employment conditions. The current estimate is also similar to Kaushal and Kaestner (2001) who conclude that 29 percent of the decline in caseload among unmarried low-educated women was due to increased employment related to TANF policies such as time limited benefits. Finally, O'Neill and Hill (2001) conclude that TANF policy explains 60 percent of the rise in employment among single mothers. The difference in O'Neill and Hill (2001) and my

results could partly be explained by the difference in our treatment groups: single mothers versus low-educated single women. My estimate is larger than that reported by Schoeni and Blank (2000), however, who conclude that welfare reform associated with the federal law had little effect on work behavior, and that most of the employment growth was due to the buoyancy in the economy.

Estimates in the top and bottom panels of Table 2 indicate that TANF had no statistically significant effect on the employment of foreign-born women, although most estimates are positive (columns 3, 4, 5). Among foreign-born women (whether citizens, non-citizens or the two groups combined), TANF was associated with a non-trivial increase in employment of one to two percentage points (three to four percent), but these effects were not significant. It is surprising that estimates in the middle panel of Table 2 pertaining to foreign-born women are larger than estimates in the other panels. I have argued that DD estimates obtained using unmarried women with 13 to 15 years of education as the comparison group are likely to have a larger bias (toward zero) than those obtained using low-educated married women because more of the former are at risk of welfare receipt and therefore potentially affected by the law. Thus, I am somewhat skeptical of the estimates in the middle panel of Table 2, particularly because of the similarity of the estimates in the top and bottom panels, but I can't rule out the possibility that TANF may have had a significant effect on the employment of foreign-born non-citizens. Although there is no definitive way to resolve the issue about which comparison group is superior, I believe that the estimates in the top panel are more credible, as I argued above when I discussed the evidence in Figures 1 and 2.

Estimates in Table 2 suggest that TANF had a larger effect on the employment of native-born than foreign-born women, although the differences are not statistically significant.⁸ At first glance, this may be surprising since a portion of foreign-born women faced a more draconian policy—denial of access not only to cash assistance, but also Medicaid and food stamps. However, this group of newly arrived immigrants makes up a relatively small portion of all immigrants. Later in the analysis I compare the response of newly arrived immigrants to that of immigrants who came to the US earlier.

It is perhaps inappropriate to compare estimates across the four samples defined by nativity and citizenship because the “quality” of the target and comparison groups may be sample specific. For example, a significant portion of the foreign-born sample is undocumented and ineligible for AFDC or TANF benefits. Therefore, this group is unaffected by welfare reform and thus the target group in this sample may consist of fewer women at risk of welfare receipt. This would lead to a DD estimate that is more biased toward zero and is consistent with most of the estimates in Table 2. In general, since the “quality” of the target and comparison groups may differ by demographic groups, differences in estimates may reflect differences in behavioral responses and/or differences in the magnitude of the downward bias caused by measurement error (comparison group members in target group) that differs by demographic groups.

In order to obtain some idea of the relative quality of the target and comparison groups in each sample, I used data from the March CPS in 1994 to compare rates of AFDC receipt across the three samples. Among native-born women, 15 percent of the target group (unmarried women with 12 or fewer years of education) and 2 percent of the

⁸ We calculate the standard errors of the differences under the assumption that the covariance between estimates is zero.

comparison group (married women with 12 or fewer years of education) received AFDC in the past year (i.e., 1993). Among foreign-born non-citizens, 18 percent of the target group and 5 percent of the comparison group received AFDC in the past year. Finally, among foreign-born citizens, 13 percent of the target group and 1 percent of the comparison group received AFDC in the past year. While not definitive, these figures suggest that the quality of the target and comparison groups is roughly similar if I use the difference in average AFDC receipt in 1994 between the target and comparison groups to evaluate “quality.” For all three groups of women, however, a greater proportion of unmarried women with 13 to 15 years of education participated in AFDC in 1994 than did married women with 12 or fewer years of education. This suggests that the DD estimates presented in the middle panel of Table 2 would be smaller than those presented in the top panel. This is clearly not the case for the foreign-born non-citizen sample and this fact motivates my skepticism related to the significant estimates of the effect of TANF on foreign-born non-citizens in the middle panel of Table 2. Assuming that the “quality” of the experiment is roughly the same for native- and foreign-born women, the top and bottom panels in Table 2 imply that foreign-born women were less affected by TANF than native-born women. Again, a caveat is necessary because estimates in the middle panel of Table 2 suggest an opposite conclusion.

An important issue in the immigration literature is assimilation, or how fast immigrants start acting like native-born persons. To address this issue I allow the effect of welfare reform among foreign-born women (citizens and non-citizens) to differ by the recency of immigration. Table 3 presents the estimates of this analysis.⁹ I perform the

⁹ An F-test rejects the model that restricts the effect of welfare reform to be the same across the three groups depending on their recency of arrival.

same analysis for foreign-born non-citizens, and find that the estimated coefficients are very similar to those obtained in Table 3. This is not surprising since foreign-born non-citizens are a large portion of all foreign-born women. Moreover, the similarity of the estimates suggests that the estimated effect represents predominantly a behavioral response and that there is relatively little selection due to naturalization, which is consistent with the evidence presented above. To avoid repetition, I present only the estimates for the combined sample.¹⁰

Table 3 presents the estimates from this analysis. Estimates in the top and bottom panels of Table 3 indicate that the effect of TANF was larger for more recent immigrants as compared to immigrants who arrived earlier. According to my preferred set of estimates (top panel), TANF increased the employment of foreign-born non-citizens who immigrated in the past five years by 4.1 percentage points. An important point to note here is that all immigrants who are subject to the more severe provisions of PRWORA, that is, legal non-citizens who arrived in the US after August 1996, belong to the group that immigrated less than five years ago. Therefore, I expect this group to be affected by TANF more than the other two groups. Among those who immigrated 10 or more years earlier, TANF had no statistically significant effect on employment. Importantly, these models hold constant the effect of recency of immigration on the employment level so my estimates are not confounded by the effect of unmeasured characteristics associated with recency of immigration. As discussed above, differences in the behavioral response to welfare reform stem from different underlying causes of program participation. Estimates in Table 3 suggest that recency of immigration signals something about the reasons why low-educated immigrant women require cash assistance. Identifying those

¹⁰ A similar analysis could not be performed for foreign-born citizens because of small sample sizes.

reasons is beyond the scope of this analysis because of data limitations, but future research will want to address this issue. On the other hand, recent immigrants faced a more draconian policy change since many were completely denied cash assistance benefits and this may be the underlying explanation of the larger estimates for this group. Finally, estimates in the middle panel of Table 3 indicate that TANF was associated with an increase in the employment for all groups of foreign-born women with relatively little difference by recency of immigration. As in the earlier analysis, the results in the bottom panel, using married women with 13-15 years of education as control, are more or less similar to the results in the top panel that uses married women with high school or less as the control group.

Another issue related to immigrants and welfare reform that has received much attention is the “chilling” hypothesis. As noted, the “chilling” hypothesis suggests that the anti-immigrant provisions of PRWORA have caused even eligible immigrants to forgo benefits because the anti-immigrant sentiment of the law created confusion and fear among all immigrants. To test this hypothesis, I exploit the variation in state policies that offset the PRWORA immigrant regulations. Specifically, 19 states continued to offer TANF-like benefits to legal immigrants arriving after 1996 and I test whether the effect of TANF differed in states that denied benefits as compared to states that continued benefits to legal immigrants. If the “chilling” hypothesis is correct, the effect of TANF on new immigrants should be approximately equal in both types of states since what is important is the fear and stigma created by the law and not actual eligibility. Again, it is important to note that recency of immigration is being held constant in this analysis. Thus, I am comparing the employment of recently arrived immigrants after TANF to the

employment of recently arrived immigrants before TANF. I allow that effect to differ by whether new immigrants were eligible for TANF. Again, I conduct the same analysis by restricting the sample to foreign-born non-citizens, and results (not presented) are very similar to the results obtained with the sample of all foreigners, citizens and well as non-citizens.

Table 4 presents the estimates of this analysis and they are striking. In states that made welfare benefits available to new immigrants, TANF had no effect on these new immigrants' employment. In contrast, TANF had a large effect on the employment of new immigrants in states that denied TANF benefits. Similar differences in the effect of TANF between the two types of states were not observed among immigrants who arrived prior to 1996. I tested whether the effect of TANF for earlier arriving immigrants differed between the two types of states and I could not reject the null hypothesis of no difference. Therefore, the difference in the effect of TANF among new immigrants is not due to some unmeasured difference between states that extended eligibility and those that did not. Note that even in the sample of all foreign-born women, those who arrived after 1995 are non-citizens, that is, they are denied benefits in the first five years of their stay in the US unless they live in one of the 19 states that provide state-level funds for new immigrants. As mentioned earlier, immigrants are allowed to apply for naturalization after they have spent five years in the US. Therefore, none of the women in my sample who arrived in the US after 1995 became eligible for citizenship during the period of my analysis.

The estimates in Table 4 are inconsistent with the "chilling" hypothesis, which would suggest effects that are not as different as those reported in Table 4. The

“chilling” hypothesis suggests that the anti-immigrant sentiments associated with TANF would cause new immigrants in both types of states to respond more or less equally since the important aspect of the law was the debate and controversy over the immigrant provisions that may have frightened eligible immigrants from obtaining benefits. The chilling hypothesis suggests that differences in actual eligibility should not matter that much, but the estimates in Table 4 strongly suggest that eligibility does matter. Given that the relative decline in welfare caseload of foreign-born is more than that of natives, it would be appropriate to add that the “chilling” effect of TANF was greater in states that do not provide welfare benefits to new immigrants than in those that do.

Effects of Welfare Reform on Hours of Work

In addition to employment, I examined the effect of welfare reform on usual hours of work in the week prior to the survey for my three groups of women.¹¹ This analysis allows me to evaluate the extent of the labor market commitment of women who have been induced to work by welfare reform. The empirical analysis of hours of work is identical to that of employment. Table 5 presents the simple descriptive statistics for hours of work by demographic characteristics and Table 6 presents the DD estimates of the effect of AFDC waivers and TANF on hours of work per week.

Estimates in Table 6 are consistent with those in Table 2 related to employment. TANF increased the average hours worked per week of native-born women and foreign-born women. The effects of TANF were greatest among native-born women who had less than a high school degree. Estimates indicate that TANF increased average hours

¹¹ It makes little difference whether we use usual hours worked per week or actual hours worked per week as the dependent variable.

worked per week by approximately half-hour to one hour per week for native- and foreign-born women. The small, but significant increases in average hours worked per week in Table 6 mask a relatively large change in labor force attachment among unmarried women who entered the labor market because of welfare reform. I can calculate the average hours of work per week among those who were induced to work by TANF as follows. First, I assume that TANF did not affect the hours worked per week of unmarried women who worked before TANF. Thus, the change in average hours worked per week in Table 6 is assumed to be due solely to the change in hours worked among women induced to work by TANF. Thus, I can divide the change in average hours worked per week associated with TANF by the change in the proportion of women who worked to obtain an estimate of the change in hours worked per week among those induced to work by TANF. For example, estimates in Table 2 indicate that TANF increased the proportion of native born women with 12 or fewer years of education who work by 0.027 and estimates in Table 6 indicate that TANF increased the average hours worked per week for this group by 0.840 hours. Thus, I can divide the 0.840 by 0.027 to obtain an estimate the average hours worked per week of native-born women induced to work by TANF. This calculation turns out to be 31.1 hours. Similar calculations for foreign-born women (from Column 6 of Tables 2 and 5) yield an estimate of 51 hours per week, which is implausibly large. If I use estimates from the middle panel of Tables 2 and 6 for foreign-born non-citizens, a more reasonable estimate of 23.3 hours is obtained. These are rough estimates and probably too large given my assumptions, but they illustrate the point that new “welfare-reform” entrants to the labor market worked a significant number of hours per week.

Gladden and Taber (2000) reported that women with less than a high school degree had an average hourly wage of \$7.55 in 1997. If I use a somewhat more conservative figure of \$7.00 as an estimate of the hourly wage of unmarried women who entered the labor market because of welfare reform, estimates in Tables 2 and 6 indicate that low-educated, unmarried women who started working due to welfare reform will earn between \$ 163 (foreign-born non-citizen) and \$218 (native-born citizen) per week on average. If these women work 40 weeks per year they will earn between \$6520 (foreign-born) and \$8,720 (native-born citizens). These women will also be eligible for the earned income tax credit that will provide up to 40 percent additional earnings, which raises average earnings to between \$9128 and \$12,208 per year. These families will remain eligible for food stamps and Medicaid will cover their children's health care expenses. Transitional benefits for the mother include childcare and Medicaid for one-year after leaving welfare. Thus, the financial picture of women who have left welfare for work improves significantly given that the cash assistance associated with TANF is quite low, with the national median being approximately \$5000 per year for a mother and two children. Moreover, evidence suggests that these women can expect significant wage growth over time and therefore an improving financial future (Cancian et al. 1999, Gladden and Taber 2000).

Effects of Welfare Reform on Marital Status

One of the primary objectives of welfare reform was to reduce dependency on government assistance by encouraging women to change behaviors that are the underlying cause of dependency. Marital choice is one such behavior. In this section I

present estimates of the effect of welfare reform on marital choices. One problem associated with this analysis is the “quality” of the target and comparison groups. I have argued that the use of education alone to define target and comparison groups is less than ideal and in the best case results in downward biased estimates, but may even lead to wrongly signed estimates if the marital experiences of more educated women are dissimilar to those of less educated women. Table 7 presents some evidence related to this point. The mean marriage rates of the target and comparison groups are similar only for the native-born women. For foreign-born women, there are significant differences, both statistically and practically, between the marriage rates of low- and high-educated women. These considerations imply a cautious interpretation of the estimates presented below.

Table 8 presents estimates of the effect of welfare reform on marriage. There are no statistically significant estimates in Table 8. TANF appears to have no statistically significant effect on the marriage rates of native- and foreign-born women. One positive implication of these findings is that they support my use of marital status to define target and comparison groups in the employment analysis. If I had found a significant effect of TANF on marriage, then part of the effects of welfare reform estimates for employment outcomes may have been due to compositional changes because I stratified the sample by marital status.

CONCLUSIONS

In this paper, I have investigated the effect of welfare reform on employment, hours of work and marital status of four groups of low-educated unmarried women:

native born citizens, foreign-born citizens, foreign-born non-citizens and all foreign born women, citizens as well as non-citizens. The results of this analysis suggest that welfare reform, particularly TANF, induced native-born citizens and foreign-born women, non-citizens in particular, to increase their employment and attachment to the labor market. TANF appears to have had a larger effect on the least educated native-born women and a larger effect on more recent arrivals among foreign-born women. Estimates did not yield a clear answer to the question of whether TANF had a larger impact on native-born women as compared to foreign-born women. My preferred estimates, those obtained using low-educated married women as a comparison group, suggest that in general native-born women had the larger behavioral response, but estimates obtained using an alternative comparison group of more educated unmarried women suggest the opposite. Estimates obtained using a third comparison group—married women with 13 to 15 years of education—were similar to those obtained with my preferred comparison group.

What wasn't ambiguous was the result that recent immigrants, those arriving in the last five years, were the group of foreign-born women most affected by TANF. This is consistent with the more draconian policy that newly arrived immigrants faced. New immigrants were denied cash assistance and all other means-tested federal benefits including Medicaid and food stamps. Importantly, the estimated effect is not affected by selection due to naturalization since all newly arrived immigrants will be non-citizens because of immigration law that requires five-year residency before naturalization. Moreover, Kaushal (2002) has shown that locational choices of new immigrants were not affected by access to safety net benefits, so there is little reason to expect that the observed effect is due to a compositional change in the "new" immigrant pool.

The “chilling” hypothesis that has received so much attention in the popular press and professional literature is not supported by my results. I found that actual eligibility for benefits is an important determinant of the behavioral response to welfare reform. This is inconsistent with the “chilling” hypothesis, which suggests that the debate and controversy surrounding passage of TANF was the primary cause of immigrant behavior. My results strongly suggest otherwise. Finally, my estimates indicate that TANF and AFDC waivers had no effect on native- and foreign-born citizens’ marriage decisions.

Table I
Description of the Employment Sample: Women in Age Group 18-44 Years, CPS 1994-1999

Nativity/	US Born		Foreign-born Citizens		Foreign-born Non-Citizens		Foreign Born (Citizens + Non-citizens)	
	No. of Observations	Average Employment	No. of Observations	Average Employment	No. of Observations	Average Employment	No. of Observations	Average Employment
Comparison Groups:								
Married Women with High School Degree or less	22498	0.678	3615	0.629	13696	0.434	17311	0.475
Married Women with Less Than High School Degree	3517	0.505	1183	0.559	8011	0.386	9194	0.408
Married Women with High School Degree	18981	0.710	2432	0.663	5685	0.502	8117	0.550
Married Women with Some College	17410	0.756	2240	0.724	3487	0.558	5727	0.623
Unmarried Women with Some College	17130	0.753	1717	0.709	3434	0.600	5151	0.636
Target Groups:								
Unmarried Women with High School Degree or Less	21464	0.626	1721	0.644	8420	0.529	10141	0.549
Unmarried Women with Less Than High School Degree	6364	0.437	705	0.540	4840	0.472	5545	0.481
Unmarried Women with High School Degree	15100	0.705	1016	0.715	3580	0.606	4596	0.630

Table 2
Difference in Differences Estimates of the Effect of TANF and AFDC Waivers on the
Employment of Unmarried Women in Age Group 18-44 Years, CPS 1994 -1999

Nativity/ Target Group	US Born		Foreign-born Citizens	Foreign-Born Non- citizens	Foreign Born (Citizens + Non-citizens)
	TANF	Waiver	TANF	TANF	TANF
Unmarried Women¹					
With High School Degree or Less	0.027* (0.014)	-0.023 (0.016)	0.006 (0.022)	0.017 (0.020)	0.012 (0.018)
With Less than High School Degree	0.058* (0.033)	-0.037 (0.039)	--	0.018 (0.027)	0.015 (0.025)
With High School Degree	0.015 (0.015)	-0.025 (0.018)	--	0.026 (0.032)	0.014 (0.027)
Unmarried Women²					
With High School Degree or Less	0.011 (0.014)	-0.007 (0.017)	0.037 (0.046)	0.077*** (0.029)	0.057** (0.024)
With Less than High School Degree	0.044** (0.022)	0.022 (0.026)	--	0.087*** (0.032)	0.072*** (0.027)
With High School Degree	-0.001 (0.016)	-0.018 (0.018)	--	0.068** (0.034)	0.045 (0.028)
Unmarried Women³					
With High School Degree or Less	0.017 (0.014)	-0.039** (0.016)	0.026 (0.044)	0.004 (0.030)	0.013 (0.024)
With Less than High School Degree	0.045** (0.022)	-0.014 (0.026)	--	0.009 (0.033)	0.026 (0.027)
With High School Degree	0.005 (0.015)	-0.049*** (0.018)	--	-0.000 (0.035)	0.002 (0.028)

Note: ¹ Married women with the same level of education as the comparison group. ² Unmarried women with some college as the comparison group.

³ Married women with some college as the comparison group. *0.05<p<0.10, ** 0.01<p<0.05, *** p<0.01.

Table 3
Difference in Differences Estimates of the Effect of TANF on the Employment of
Unmarried Foreign-born Women in Age Group 18-44 Years, Depending on their Arrival in the US, CPS 1994 -1999

Target Group	Immigrated less than Five years ago	Immigrated between 5-10 years ago	Immigrated more than 10 years ago
	TANF	TANF	TANF
Unmarried Women¹			
With High School Degree or Less	0.041* (0.023)	0.021 (0.023)	-0.009 (0.020)
With Less than High School Degree	0.067** (0.032)	0.012 (0.031)	-0.010 (0.027)
With High School Degree	0.024 (0.034)	0.042 (0.035)	-0.003 (0.029)
Unmarried Women²			
With High School Degree or Less	0.074*** (0.028)	0.053* (0.029)	0.054** (0.025)
With Less than High School Degree	0.145*** (0.034)	0.050 (0.034)	0.050* (0.030)
With High School Degree	0.038 (0.035)	0.069* (0.037)	0.042 (0.030)
Unmarried Women³			
With High School Degree or Less	0.043 (0.028)	0.020 (0.028)	-0.006 (0.025)
With Less than High School Degree	0.113*** (0.034)	0.017 (0.033)	-0.015 (0.030)
With High School Degree	0.018 (0.035)	0.039 (0.036)	-0.024 (0.030)

Note: ¹ Married women with the same level of education as the comparison group. ² Unmarried women with some college as the comparison group.

³ Married women with some college as the comparison group. *0.05<p<0.10, ** 0.01<p<0.05, *** p<0.01.

Table 4
Difference in Differences Estimates of the Effect of TANF on the Employment of
Unmarried Foreign-born Women in Age Group 18-44 Years, Depending on their Arrival in the US, CPS 1994 -1999

Target Group	Immigrated after 1995		Immigrated Before 1996
	TANF –New Immigrants Eligible	TANF –New Immigrants Ineligible	TANF
Unmarried Women¹			
With High School Degree or Less	-0.006 (0.037)	0.052* (0.029)	0.008 (0.018)
With Less than High School Degree	0.005 (0.051)	0.090** (0.040)	0.006 (0.025)
With High School Degree	-0.009 (0.054)	0.014 (0.043)	0.016 (0.027)
Unmarried Women²			
With High School Degree or Less	0.024 (0.040)	0.084** (0.033)	0.056** (0.024)
With Less than High School Degree	0.077 (0.052)	0.162*** (0.042)	0.059** (0.028)
With High School Degree	0.002 (0.055)	0.026 (0.044)	0.050* (0.029)
Unmarried Women³			
With High School Degree or Less	-0.009 (0.040)	0.054 (0.033)	0.009 (0.024)
With Less than High School Degree	0.045 (0.052)	0.131*** (0.042)	0.010 (0.028)
With High School Degree	-0.019 (0.055)	0.009 (0.044)	0.003 (0.029)

Note: ¹ Married women with the same level of education as the comparison group. ² Unmarried women with some college as the comparison group.

³ Married women with some college as the comparison group. *0.05<p<0.10, ** 0.01<p<0.05, *** p<0.01.

Table 5
Description of the Hours Worked (Usual) Sample: Women in Age Group 18-44 Years, CPS 1994-1999

Nativity/	US Born		Foreign-born Citizens		Foreign-born Non-Citizens		Foreign Born (Citizens + Non-citizens)	
	No. of Observations	Average Employment	No. of Observations	Average Employment	No. of Observations	Average Employment	No. of Observations	Average Employment
Comparison Groups:								
Married Women with High School Degree or less	22498	22.586	3615	22.591	13696	14.866	17311	16.479
Married Women with Less Than High School Degree	3517	16.194	1183	19.798	8011	13.210	9194	14.058
Married Women with High School Degree	18981	23.770	2432	23.949	5685	17.198	8117	19.221
Married Women with Some College	17410	24.959	2240	25.705	3487	18.760	5727	21.476
Unmarried Women with Some College	17130	24.372	1717	23.392	3434	18.704	5151	20.267
Target Groups:								
Unmarried Women with High School Degree or Less	21464	20.656	1721	22.139	8420	17.836	10141	18.566
Unmarried Women with Less Than High School Degree	6364	12.435	705	18.315	4840	15.560	5545	15.910
Unmarried Women with High School Degree	15100	24.121	1016	24.793	3580	20.913	4596	21.770

Table 6
Difference in Differences Estimates of the Effect of TANF and AFDC Waivers on the
Working Hours (Usual) of Unmarried Women in Age Group 18-44 Years, CPS 1994 -1999

Nativity/ Target Group	US Born		Foreign-born Citizens	Foreign-Born Non- citizens	Foreign Born (Citizens + Non-citizens)
	TANF	Waiver	TANF	TANF	TANF
Unmarried Women¹					
With High School Degree or Less	0.840 (0.555)	-0.807 (0.651)	-0.679 (1.710)	1.034 (0.796)	0.608 (0.721)
With Less than High School Degree	1.105 (1.206)	-1.154 (1.444)	--	0.497 (1.056)	0.404 (0.957)
With High School Degree	0.659 (0.644)	-0.865 (0.746)	--	2.117* (1.270)	1.174 (1.094)
Unmarried Women²					
With High School Degree or Less	0.498 (0.579)	0.492 (0.672)	0.066 (1.872)	2.159** (1.101)	1.261 (0.933)
With Less than High School Degree	1.262 (0.795)	1.472 (0.960)	--	1.767 (1.228)	1.267 (1.065)
With High School Degree	0.224 (0.642)	0.127 (0.739)	--	2.714** (1.338)	1.420 (1.134)
Unmarried Women³					
With High School Degree or Less	0.628 (0.589)	-0.927 (0.685)	-1.327 (1.829)	0.753 (1.173)	0.031 (0.954)
With Less than High School Degree	1.148 (0.800)	-0.097 (0.964)	--	0.328 (1.295)	0.024 (1.086)
With High School Degree	0.393 (0.651)	-1.219 (0.751)	--	1.465 (1.399)	0.207 (1.151)

Note: ¹ Married women with the same level of education as the comparison group. ² Unmarried women with some college as the comparison group.

³ Married women with some college as the comparison group. *0.05<p<0.10, ** 0.01<p<0.05, *** p<0.01.

Table 7
Description of the Marriage Sample: Women in Age Group 18-44 Years, CPS 1994-1999

Nativity/	US Born		Foreign Born	
	No. of Observations	Probability of Being Married	No. of Observations	Probability of Being Married
Comparison Group: Women with Some College	34540	0.504	10878	0.526
Target Group: Women with High School Degree or Less	43962	0.512	27452	0.631

Table 8
Difference in Differences Estimates of the Impact of TANF and AFDC Waivers on the
Probability of Being Married: Women in Age Group 18-44 Years, CPS 1994 -1999.¹

Nativity/ Target Group	US Born		Foreign Born
	TANF	Waivers	TANF
Women With High School Degree or Less	-0.015 (0.010)	-0.010 (0.012)	-0.017 (0.015)
With Less than High School Degree	-0.009 (0.016)	0.022 (0.019)	-0.011 (0.017)
With High School Degree	-0.016 (0.011)	-0.019 (0.012)	-0.021 (0.017)

Note: ¹ Foreign-born women with some college as the comparison group.

* 0.05<p=<0.10, ** 0.01<p=<0.05, *** p=<0.01.

Figure1
Mean Employment of Women
Age Group 18-44 Years

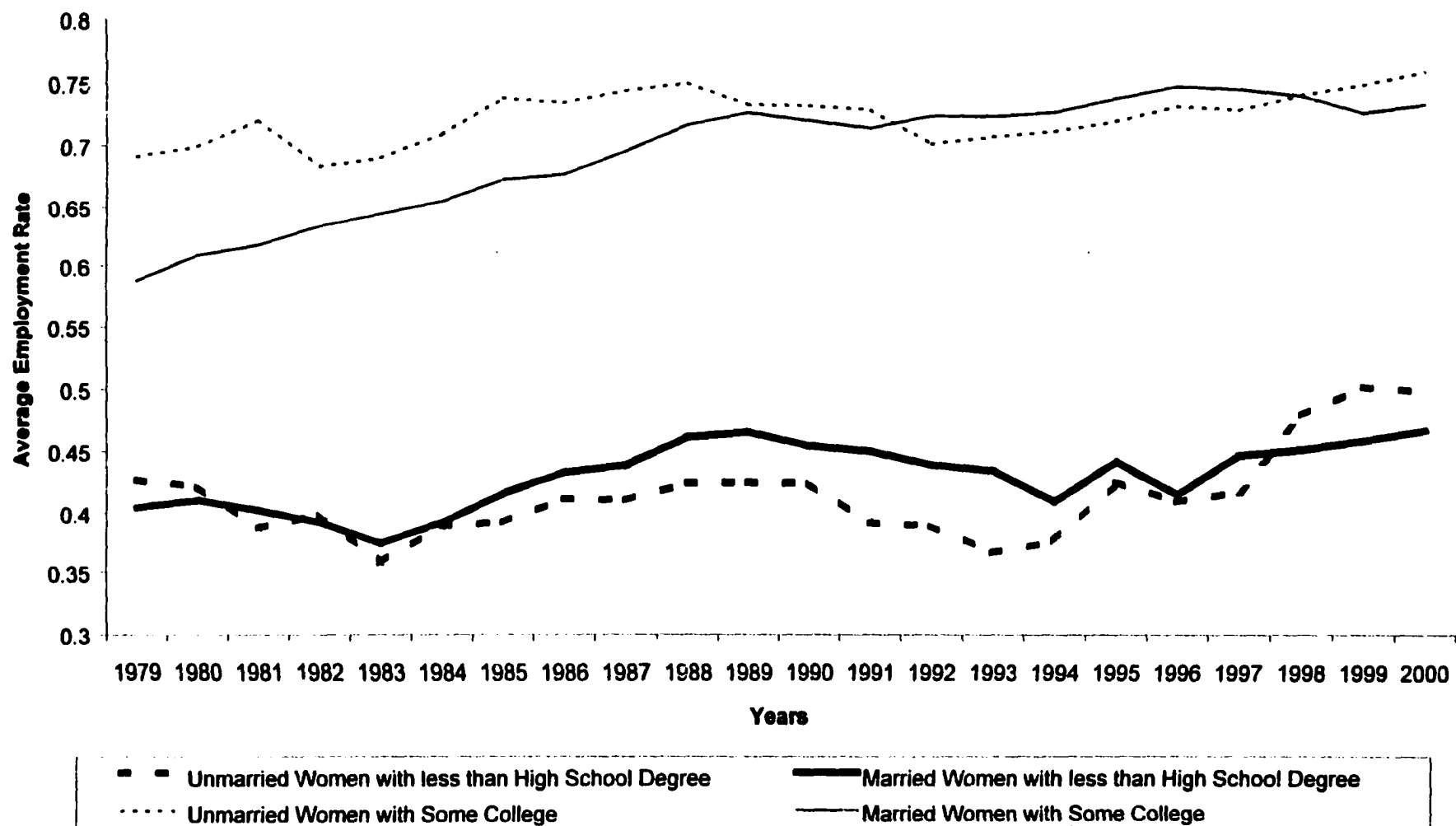
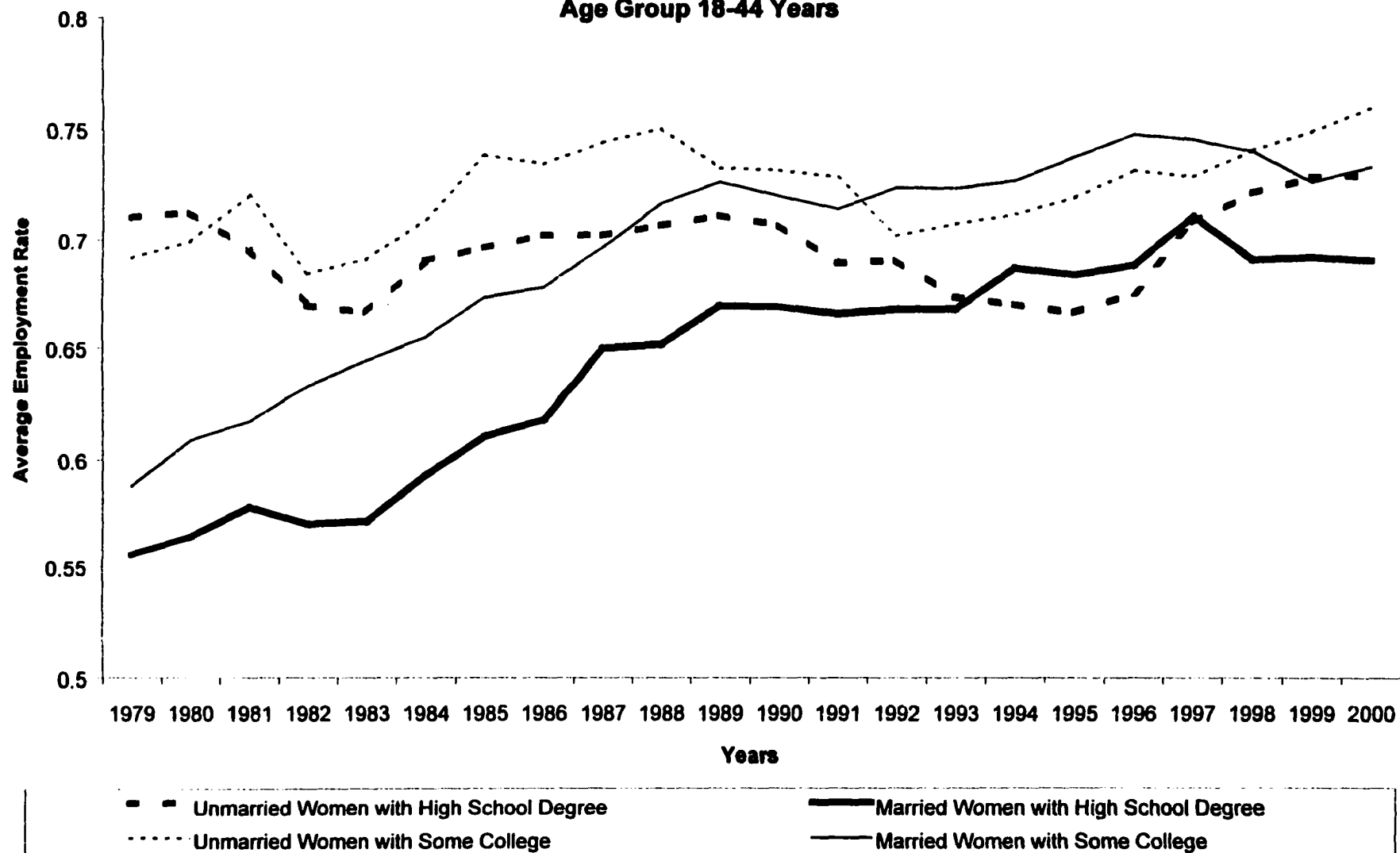


Figure 2
Mean Employment of Women
Age Group 18-44 Years



Chapter 4

New Immigrants' Location Choices: Magnets without Welfare

Main Findings

In this paper, I study the effect of safety-net programs on the locational choices of new immigrants. The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) banned legal non-citizens, who arrived in the US after August 1996, from receiving any federally financed means-tested benefits in their first five years in the US. However, a number of states restored some of these benefits using state-level funds. I use this state-level variation in policy to study whether or not new immigrants make their location decisions on the basis of benefit availability and generosity. I use a conditional logit model to obtain estimates of the effect of the policies of interest. I control for unmeasured, time-invariant characteristics of states that may be correlated with locational choice and benefit availability. To control for time-varying, unmeasured state characteristics, I use a difference-in-differences procedure that compares the effect of the policy on the location choices of unmarried low-skilled immigrant women, the group most likely to be affected by policy changes, to the effect of the policy on the location choices of married low-skilled and unmarried skilled women immigrants, two groups that are less likely to be affected by the policy changes.

My study shows that safety-net programs do not affect the locational choices of new low-skilled women immigrants. This result is robust to a variety of specifications including those limited to the target group; difference-in-differences models with each of the control groups, and models that take account of the number of programs available to immigrants.

INTRODUCTION

The United States is experiencing the largest influx of immigrants in its recorded history. Close to 11 million immigrants arrived in the country during the nineties, more than in any other decade in the past two centuries.¹ Not only is the volume of inflow unprecedented, it is accompanied by an impressive geographic dispersion of immigrants. The magnetic appeal of traditional immigrant states like California, New York, New Jersey and Illinois appears to be waning. Passel and Zimmermann (2001) find that the proportion of immigrants in the top six immigrant states fell during 1995 to 1999.² California, abode to almost a third of the immigrant population experienced the largest decline: its share in foreign-born population dropped from 35 percent in 1995 to 30 percent by 1999.³ New York, New Jersey and Illinois experienced the same trend. Only two traditional immigrant states – Texas and Florida experienced a higher than nationwide growth in foreign-born population during this period. As many as 19 states with traditionally low immigrant populations received a large number of immigrants.

Large-scale immigration as has been noticed in last couple of decades, has economic, social and political repercussions for natives. The diversity of skills and culture immigrants bring make a positive net contribution to the US economy (Smith and Edmonston, 1997). Benefits from immigration, however, are not equally shared by all.

¹ Passel and Zimmerman (2001).

² The INS data on new immigrants granted permanent resident status exhibit a similar, though less pronounced, trend of geographical dispersion. According to INS, in 1990, 27 percent of the new immigrants expressed California to be their intended state of residence. This proportion fell to 25 percent in 1998. During this period, the proportion of new immigrants expressing New York as their choice of residence fell from 22 to 18 percent. While New Jersey, Florida and Texas more or less maintained their share of new immigrants, the share of Illinois fell by one percent. (The top six immigrant states are: California, New York, Florida, Texas, Illinois and New Jersey.)

³ In this period of immigrant deluge, the number of immigrants living in California remained constant.

Immigration has been linked with growing inequalities in the US. It is also associated with a third of the decline in the relative wage of less educated native workers.⁴ Although the costs and benefits of immigration permeate through the entire country, they are more acutely felt at the epicenters, the states where immigrants live. Hence, from a public policy perspective, it is important to understand the factors that determine the locational decisions of immigrants.

One factor that may affect the location decisions of immigrants is their access to public programs and the generosity of such programs. During the early nineties, there was considerable concern in the public policy arena about the link between immigration flows and the generosity of means-tested programs. The concern was partly due to the fact that the new waves of immigrants that arrived during the seventies and the eighties were relatively less educated and poor as compared to natives (Smith and Edmonston 1997, Camarota 1998).⁵ This was also a period during which immigrants' dependence on cash and non-cash government assistance grew disproportionately large (Borjas 1994, Borjas and Hilton 1996). These two factors led to the fear that the US welfare state was attracting low skilled immigrants, of which many either arrived illegally or over-stayed their legally permitted stay, and that these immigrants were a fiscal as well as economic drag.⁶ As expected, the fear was far more intense in immigrant states like California, Texas and Florida that had to bear the added burden of providing public services

⁴ Borjas (1996) and Camarota (1998).

⁵ Jasso, Rosenzweig and Smith (2000) find that the quality of male legal immigrants, determined by their skills, fell during 1972-1985. The trend got reversed during 1986-1994, with the quality of male legal immigrants improving steadily.

⁶ A study for the National Research Council finds that the overall fiscal burden imposed by immigrants – the cost of state and local services received and state and local taxes paid – was \$ 166 to \$ 226 per native household in 1989-90 (adjusted to 1996 dollars).

including schooling and emergency medical care to illegal immigrants. A number of states even sued the federal government for forcing them to pay for services for illegal immigrants. In California, voters even passed Proposition 187 denying all medical and social services to undocumented immigrants.⁷

During the early nineties, much of the fury at the state level was against illegal immigrants. Ironically, it was unleashed on new legal immigrants. In August 1996, Congress passed the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) denying newly arrived non-citizens in their first five years in the US access to means tested federal benefits (e.g. cash benefits, Medicaid, Food Stamps, Supplemental Security Income (SSI)). Two main factors explain this reaction. Firstly, banning non-citizen immigrants from means-tested benefits was seen as a major source of savings.⁸ That they lacked the political power to oppose such legislation was an added incentive. And secondly, the government wanted to discourage immigration of low-skilled people, who were at risk of becoming a state liability.

The immigrant provisions under PRWORA did not receive full political support from all quarters. Backlash against the original law forced the federal government to restore food stamps and SSI benefits for children, elderly and disabled immigrants. State level reaction to the federal law made it clear that not all states share the same vision on this issue as the Federal government. Many states restored public programs for new immigrants by using state funds -- 10 states have set up substitute food stamps programs, 14 have substitute Medicaid programs and 19 have provided funds for the welfare needs

⁷ The constitutionality of the proposition was challenged in the court, which prevented the state government of California to implement it. See Singer (2001), Viglucci (1994), news reports from The New York Times.

of recent immigrants (Zimmermann and Tumlin, 1999). Interestingly, of the six key immigrant destination states listed above, only California has decided to provide funds for all the three programs mentioned here. Illinois is the only other state in the group that has a substitute Medicaid program for new immigrants.

One of the objectives of PRWORA, as mentioned above, was to demagnetize welfare and other social programs. This objective was based on the belief that immigrants take such benefits into consideration when deciding whether or not to come to the U.S. and where to live in the US. The purpose of this research is to investigate whether there is any basis for this belief and answer the following questions: Do low-skilled immigrants need greater incentives to transfer from welfare to work as compared to low-skilled natives? Has PRWORA and subsequent policy changes in recent immigrants' access to means-tested programs influenced the location decisions of less-skilled immigrant women, potential recipients of these benefits? And finally, is the recent geographic dispersion of immigrants a result of the above-mentioned policy changes?

To investigate these issues, I study the impact of various elements of PRWORA on the location choices of unmarried low-skilled immigrant women, who arrived in the US in 1996, a year prior to the policy change, and in 1998, a year after the change. I use a conditional logit model of location choice with fixed state effects. To control for time-varying, unmeasured state characteristics, I use a difference-in-differences procedure that compares the effect of the policy on the location choices of unmarried low-skilled immigrant women, the group most likely to be affected by policy changes, to the effect of

⁸ Denying benefits to legal permanent residents resulted in 44 percent of the total net savings from PRWORA.

the policy on the location choices of married low-skilled and unmarried skilled women immigrants, two groups that are less likely to be affected by the policy changes.

PREVIOUS RESEARCH

Past research on the impact of social programs for the poor on the location choices of immigrants is inconclusive. While Bartel (1989), Buckley (1996), Borjas (1999) and Dodson III (2001) find that immigrants flock to states with generous welfare policies, Zavodny (1997, 1998) do not find any evidence to this effect. These papers suffer from a number of empirical, theoretical and data related problems, which this paper attempts to address.

According to the welfare magnet hypothesis, low-skilled immigrants get attracted to states that offer generous welfare benefits. A large number of immigrants live in states that offered generous benefits under Aid to Families with Dependent Children (AFDC). California and New York that absorb around 50 percent of the immigrants arriving in the US in a year were among the states with the most generous AFDC benefits. It would be incorrect, however, to conclude from this evidence that immigrants choose to live in these states because of their generous welfare policies. It is possible that there is no causal link between state welfare generosity and concentration of immigrants in a few states. For instance, states that exhibit a greater tolerance towards immigrants, hence attracting a larger number of them, may also be liberal in their policies towards the poor, including low-skilled immigrants. That is, a third factor may be influencing both immigrant location choices and state generosity towards the poor. In econometrics, this is referred to as statistical endogeneity.

It is also possible that the flow of low-skilled immigrants to a state may force the state government to change the generosity level. For instance, states that experience a large flow of unskilled immigrants may like to reduce the level of benefits to discourage future flows of low-skilled immigrants. If so, state welfare policies may not be the cause, but the effect of the settlement patterns of immigrants. In econometrics parlance, this phenomenon is referred to as reverse causality or structural endogeneity. All the papers mentioned above treat the variable welfare generosity (benefit level) as exogenous, while it exhibits traits of both statistical and structural endogeneity. Recent changes in social policies towards immigrants offer an opportunity to address this issue. Heterogenous reactions by state governments towards immigrant provisions in PRWORA suggest that state policies towards immigrants could not have been easily predicted.⁹ I use the changes in state policies towards immigrant's eligibility for means-tested programs to test the welfare magnet hypothesis.

Another issue confronting past research on location choices of immigrants is the need to differentiate between immigrants' sensitivity to welfare benefits and the welfare magnet hypothesis. According to economic theory, generous benefits increase welfare dependency among the poor and the unskilled; higher benefits induce more people into opting for welfare instead of work. Butcher and Hu (2000) show that a disproportionately large number of immigrants are low skilled. Their research shows that every fifth person without a high school degree is an immigrant, which is twice the share of immigrants in the total population. Given their social, educational and economic background, immigrants are more likely to be on welfare in states that give generous welfare benefits

⁹ For instance, for a state like California, that had passed a legislation to ban all social services to illegal immigrants, to provide funds for the Medicaid, TANF and food stamps needs of new immigrants was quite

than natives. Therefore, Borjas (1999) finding that immigrant welfare recipients are more heavily concentrated in states that give higher welfare benefits as compared to immigrants who are not on welfare, or natives whether on welfare or not, is not a test of the welfare magnet hypothesis. It merely indicates that immigrants are more sensitive to welfare benefits than natives, which is expected given the socio-economic status of immigrants. A more appropriate methodology to test the welfare magnet hypothesis is to study the impact of social policies on the location choices of immigrants, instead of their sensitivity to welfare generosity.

Past research on the welfare magnet hypothesis is also criticized for the data used in empirical work. Both Bartel (1989) and Borjas (1999) use census data. This data set does not distinguish between legal and illegal immigrants. Welfare law does. All legal immigrants were allowed to receive welfare benefits under AFDC, but illegal immigrants were not. TANF too makes a clear distinction between the two groups. Studies that use census data suffer from the problem of defining their target groups imprecisely. In their elegant critique of studies on recent immigrants that use census or CPS data, Jasso, Rosenzweig and Smith (2000) argue,

“the samples (CPS data) on which conclusions are drawn may be neither recent nor immigrant. As is well known, it is not clear whether respondents, in answering the standard questions about time since immigration, are referring to their first, last or most salient time of entry to the United States..... Since non-immigrants and illegal immigrants have relatively short stay in the United States, they are especially numerous among ‘recent immigrants’ (page 189).”

unpredictable.

In this paper, I avoid this problem by using INS data on new immigrants, which provides information on foreigners granted permanent legal status in a certain year.¹⁰ The data set also gives information on the immigrant's year and category of arrival in the US that allow me to restrict the study to new non-refugee immigrants.

Most of the earlier studies on immigrants' location choices have confronted a number of other data problems. One of them is related to how best to control for characteristics of destinations. A number of them control for destination characteristics such as state population, mean wage, share of people from immigrant's country of birth (Jaeger 2000), unemployment rate, taxes, share of manufacturing and agriculture employment (Dodson III 2001). However, there are several unobserved destination features that remain uncontrolled. Like Zavodny (1997, 1998), I introduce state fixed effects to control these unobserved destination characteristics.

A second issue concerns costs of immigration. A number of studies use the distance between the capital of immigrant's country of birth and the population weighted center of the metropolitan area in the US where they finally decide to live as a proxy for cost of immigration (Bartel 1989, Jaeger 2000). Zavodny (1997) uses the distance between the most populated city in the source country and the most populated city in the state of intended residence. However, there could be several country of origin and state of destination specific characteristics, which may determine the costs and benefits of immigration. In this paper, I control for these factors by dividing new immigrants into 10 categories based on their countries/ regions of birth. Separate analysis for each

¹⁰ Although not as widely used as CPS, the INS data set has been used in a number of other studies on location choice such as Buckley 1996, Zavodny 1997, 1998, Jaeger, 2000 and Dodson III 2001.

country/region automatically controls for any country of origin specific factors, including cost that may influence the location choices of immigrants.

State fixed effects combined with a separate analysis for each of the country-group allows me to control for factors that are hard to measure. That is, factors (that cannot be measured) that not only make California and New York to be the most favorite states for all immigrants, and Montana to be one of the least favorite states, but also factors that make states in the East Coast to be the favorite residential choice for the people from the Caribbean and South Americans and the West Coast to be the favorite choice of residence for Central Americans and Filipinos.

Yet another data issue that most previous studies have encountered is how to estimate the number of immigrants living in a state or a region. This is an important determinant in the location choice of immigrants because immigrants like to live in states with a high foreign born population and a high population of immigrants from their country of origin. Estimates of foreign-born population living in small states computed from CPS data contain significant margins of error. Besides, CPS did not provide this data before the year 1994. Most previous studies have, therefore, used the census to estimate the population of immigrants in a state/ county. As a result most of the previous studies on immigrants' location choices have clustered around the census data collection years 1980 or 1990 (Bartel, 1989, Borjas 1998, Jaeger 2000).

This has created a gap in the immigrant location choice literature. There is no study on the location choices of immigrants between 1970 and 1980. To my knowledge, there is only one paper that investigates impact of welfare benefits on the location choice of immigrants during the late eighties and early nineties. Using INS data, Zavodny (1998)

investigates the location choices of immigrants during 1989 and 1994. She makes a linear interpolation of the number of foreign-born living in a state in 1989, 1991, 1992 and 1993 from the number of foreign born population living in a state in 1980, 1990 and 1994. It can be argued that this is not a perfect substitute for the actual number of foreign-born people living in a state. Fortunately, this data limitation can now be overcome as the CPS has started providing data on immigrants since 1994. I use a three-year moving average to compute the number of foreign-born people living in a state to overcome the deficiencies in the CPS data. I also use INS data to control for recent immigrants by country of birth.

IMMIGRANTS' ACCESS TO SOCIAL PROGRAMS

There has been a distinct change in welfare policies towards immigrants who arrived in the US after August 1996, when PRWORA was passed. The objective of federal law was to restrict immigrants' use of public welfare programs. All foreign-born US citizens were treated as natives, subject to the general policies of welfare reform that restricted benefits to a lifetime maximum of five years, imposed work restrictions and sanctions on those who did not comply with those provisions. The law restricted non-citizens' eligibility to public assistance programs such as Temporary Assistance to Needy Families (TANF), Food Stamps, Supplemental Security Income (SSI) and Medicaid. Political backlash against the initial law forced the government to restore Food Stamps and SSI benefits for children, elderly and disabled immigrants. PRWORA also gave state governments the freedom to formulate state level welfare policies. As a result, the present welfare policy towards immigrants is a mixture of state and federal policies. As

Zimmermann and Tumlin (1999) show, these policies make a distinction between legal citizens who arrived in the US before and those who arrived after the passage of the law.

Prior to PRWORA all illegal immigrants were barred from public assistance. Legal immigrants were eligible for AFDC, but those who came under family sponsored categories were subject to a restriction. The restriction was that in the first three years of their arrival immigrants were the responsibility of their sponsor and the income of the sponsor was assumed to be available to them while ascertaining their eligibility to means-tested programs.

PRWORA continues to bar illegal immigrants from any public assistance. While the federal government does not allocate any funds for newly arrived non-citizens, PRWORA gives states the option to make legal non-citizens who arrived before the law was passed eligible for these programs. All states, except Alabama, have used the option and as a result, legal non-citizens who arrived in the US prior to August 1996 are eligible for TANF in all states (except Alabama).

Under federal law non-citizens who arrived in the US after August 1996 are barred from receiving TANF benefits in their first five years in the US. But 19 states have provided funds for the welfare needs of these immigrants for the period they are barred from TANF by federal law. New York, New Mexico and Alaska provide General Assistance, but no TANF, to new immigrants. (General Assistance involves a significantly lower cash benefit than TANF.)

Most working age non-citizens are not eligible for food stamps under the federal law. But 17 state governments have substitute programs for pre-enactment non-citizens and 10 have such programs for post-enactment non-citizens. All states, except Wyoming,

have state-funded Medicaid programs for pre-enactment non-citizens and 14 states have substitute Medicaid programs for post-enactment non-citizens.

Studies show that welfare reform has made a deep impact on the lives of low-educated unmarried women. It has encouraged a large number of women to leave welfare for work (CEA 1997, CEA, 1999 and Kaushal and Kaestner 2001). Kaestner and Kaushal (2001) find that welfare reform induced a greater proportion of non-citizen low-educated women who arrived in the US in the last five years to work as compared to the earlier cohorts of immigrants. Kaestner et al (2001) finds evidence that welfare had distorted the location choices of unskilled unmarried women, and welfare reform has led to increased migration of unmarried low-educated women, and a great degree of this migration is employment induced.

One of the initial objectives of PRWORA was to reduce adverse immigration (inflow of low skilled immigrants). If the welfare magnet hypothesis is true, the new law should have created new magnets for low-educated new women immigrants, and new immigrants should have stopped locating in traditional immigrant states and started locating in the new magnet states. The purpose of this paper, as mentioned earlier, is to test this hypothesis by exploiting the variation in public policy brought about by PRWORA.

THEORETICAL ISSUES

Immigrants leave their country of birth in search of a new abode where they can get better work opportunities, higher living standards, and greater political freedom. They

search for ethnic enclaves in their acquired homeland to maximize the opportunities and to minimize the psychological and economic costs of relocation.

Mincer (1978) argues that family ties have an important bearing on people's migration decisions. In the case of natives, he argues, family ties deter migration. For new immigrants, family ties, however distant, ease the process of relocation by providing access to, or information about, the labor market. In these ethnic enclaves, new immigrants also find cultural and linguistic affinities, which ease the process of relocation, but may slow down the assimilation process.

One factor that may explain the clustering of immigrants is immigration law that gives overwhelming preference to family based immigration. The Immigration and Naturalization Service gives permanent resident status to foreigners under two categories: numerically unlimited visas for immediate relatives of US citizens (spouses, minor children and parents), refugees and asylees, and numerically limited visas. Under the numerically limited category, there are three sub-categories: family sponsored preference, employment-based preference and immigration under the diversity program. Immigration under the employment-based category was limited to 20 percent of the worldwide limits till October 1991, when the Immigration Act of 1990 became effective. This law more than doubled the quota for employment sponsored immigrants, and reduced the maximum number of visas available to unskilled immigrants and their family under the employment sponsored category. Despite this change, during the nineties, around 60 percent of the legal immigrants to the US fell under the immediate relative of US citizen or family preference category (INS). It should not be a surprise, therefore, that these

immigrants, who primarily come to the US to unite with their family members choose to live in the same cities as the latter.

Beside the location pattern of previously arrived immigrants, there are several other factors that may explain the location choices of new immigrants. These factors can be grouped in three broad categories: personal characteristics of immigrants, the locational characteristics of the states in which they locate and the locational characteristics of the countries from which they come. Personal characteristics of immigrants include their age, sex, marital status and skills including knowledge of the English language.

As argued earlier, an important locational characteristic is the distance between immigrant's country of origin and her residence in the US, which determines the cost of relocation. Shwartz (1973) argues that distance affects immigration flows through diminished information and increased travel and psychic costs. Controlling for other factors, immigrants may like to minimize the travel and psychic costs of relocation by minimizing this distance.

Other locational characteristics of states include observed characteristics such as state population, number of foreign born people living in the state, the number of foreign born people from the immigrants' country of birth, state unemployment rate with one and two years lag and real wage of workers. There are also several locational characteristics that are not observed by researchers such the political and cultural acceptance of immigrants in general, or immigrants of a certain ethnicity, in particular. Immigrants may be taking these unobserved factors into consideration while making their location decisions in the US.

Finally, state level social security net provided by programs like TANF, Medicaid and SSI, and new immigrants' access to them may be a factor in determining their residential choices. Access to these programs ensures that in the worse case scenario, if immigrants do not get a job, they are better off living in states that provide them access to social programs than in states that do not.¹¹ Borjas (1999) has argued that new immigrants are more likely to make their location choices on the basis of these factors than immigrants already living in the country or natives. New immigrants factor in travel and relocation expenses when they make the decision to come to the US. Having incurred these expenses, it is in their best interest to live in states that provide them access to means-tested programs. In contrast to new immigrants, residents have to incur additional relocation and travel expenses if they want to migrate to states that offer generous benefits. This paper is an attempt to investigate whether there is any empirical evidence behind this argument.

ECONOMETRIC MODEL

Consider an individual i from country k who has taken the decision to move to the United States and has been granted the legal permanent residence status by the INS. Individual i has the option to live in one of the 51 states in the US, and she derives a certain level of utility U_{ikt} if she chooses state s as her residence in year t . The probability that i will move to state s is given by

$$P_{ikt} = \Pr(U_{ikt} \geq U_{ijt} \forall j \neq s)$$

¹¹ An extreme interpretation of welfare magnet hypothesis could be that a large number of immigrants get attracted to the US due to the social welfare programs. If the programs are withdrawn, these immigrants would either go back to their countries of birth or would stop coming to the US.

Since utility is an ordinal concept, assume that U_{iskt} is a linear function of a number of individual and location specific factors that can be measured and a random term e_{iskt} that captures the influence of factors that cannot be measured.

$$U_{iskt} = \alpha_0 L_{st-1} + \alpha_1 X_i + \alpha_2 C_{skt-1} + \alpha_3 T_{sk} + \alpha_4 Pol_{skt-1} + e_{iskt}$$

As discussed earlier, the utility that i gets from choosing state s as her residence depends on L_{st-1} -- a vector of location characteristics that vary across locations and over time such as -- the unemployment rate in state s in years $t-1$ and $t-2$, the median real wage of workers of similar skills in state s in year $t-1$, the population of state s in year $t-1$ and number of foreign born people living in state s in year $t-1$.¹² The probability that person i will choose to live in state s in the US also depends on C_{skt-1} , the number of recent immigrants from country k living in state s in year $t-1$. An individual's choice of residence also depends on X_i , a vector of individual i 's personal characteristics such as her skills, age and marital status.¹³

Other factors that may influence the location choice of an immigrant include: Pol_{skt-1} , state s 's policy towards providing social programs to new immigrants in year $t-1$ and T_{sk} , a vector of state dummies, that captures the impact of all observed and unobserved state characteristics that do not vary over time. As the empirical analysis is done separately for the ten countries/regions of origin, the state dummies also control for geographical factors such as climate difference between immigrant's country of birth and

¹² The explanatory variables are lagged by a year on the assumption that immigrants make their decision to move to state s in year t based on their knowledge of the macroeconomic conditions in the state in year $t-1$. Moreover, studies show that inflow of low-skilled workers reduces wages of low-skilled native workers. To avoid this endogeneity, therefore, in the above model, I use wage in year $t-1$ as an explanatory variable, which is exogenous for immigrants who arrive in the host country in year t .

¹³ As I use a conditional logit model, X_i s drop out of the regressions unless they are interacted with each of the 51 options. In my model, I use personal characteristics to define each sample. I also use a model in which age is interacted with the 51 state options. The results using this model are similar to the results from

her new state of residence, and the distance between the state and immigrant's country of origin, and other unobservable time-invariant state-country of birth specific locational characteristics.¹⁴

$$\text{Let } Z_{iskt} = [L_{st-1} \quad X_i \quad C_{skt-1} \quad T_{sk} \quad Pol_{st-1}]$$

and

$$\text{let } \delta = [\alpha_0 \quad \alpha_1 \quad \alpha_2 \quad \alpha_3 \quad \alpha_4].$$

$$\therefore U_{iskt} = Z_{iskt} \delta + e_{iskt}$$

The probability function can be written as

$$\begin{aligned} P_{iskt} &= \Pr(Z_{iskt} \delta + e_{iskt} \geq Z_{ijkt} \delta + e_{ijkt} \quad \forall j \neq s) \\ &= \Pr(e_{ijkt} \leq Z_{iskt} \delta - Z_{ijkt} \delta + e_{iskt} \quad \forall j \neq s) \end{aligned}$$

Assuming $e_{iskt} \sim \text{iid}$, with Weibull distribution, the probability that person i will choose state s is given by

$$P_{iskt} = \frac{e^{Z_{iskt} \delta + e_{iskt}}}{\sum_{s=1}^{51} e^{Z_{iskt} \delta + e_{iskt}}}$$

with the condition that $\sum P_{iskt} = 1$

This is the standard form of the conditional logit estimation. The marginal effect of a change in a locational characteristic on the probability of the immigrant residing in a state can be obtained by differentiating P_{iskt} with respect to the characteristic.

$$\frac{\partial P_{iskt}}{\partial Z_{iskt}} = P_{iskt} (1 - P_{iskt}) \delta$$

a model that does not use age-state interactions. The results reported here are of the models without any interactions.

¹⁴ State dummies also control for demographic factors mentioned above that do not vary with time. In the empirical analysis that follow, however, I control for all the factors mentioned above.

Under the condition of independence from irrelevant alternatives, McFadden (1974) showed that conditional logit estimates of the coefficients in the model are consistent. The assumption of independence from irrelevant alternatives (IIA), as elucidated in Ben-Akiva and Lerman (1985), implies “for a specific individual the ratio of choice probabilities of any two alternatives is entirely unaffected by the systematic utilities of any other alternative.” (page 108).

Algebraically, the IIA assumption can be explained as follows. Let C_n be the choice set available to immigrant i with subscript n being the number of states in each choice set. The IIA assumption implies:

$$\Pr(s|C_{51}) / \Pr(j|C_{51}) = \Pr(s|C_{50}) / \Pr(j|C_{50})$$

The above condition means that the ratio of the probability that i chooses to live in California (state s) to the probability that she chooses to live in New York (state j) is unaffected by whether she has the option to live in Alaska (or New Jersey) or not. In more general terms, the above condition takes the following form:

$$\Pr(s|C_n) / \Pr(j|C_n) = \Pr(s|C_{n-r}) / \Pr(j|C_{n-r})$$

This is a restrictive assumption. It limits the cross elasticities of the probabilities of choosing various states with respect to the location characteristic of another state to be the same. As Hausman and McFadden (1984) argue the IIA condition does not allow for different degrees of substitutability and complementarity among choices. I use a test proposed by Hausman and McFadden (1984) to test the validity of the IIA in this model. I randomly restrict the number of choices open to an immigrant to 35 states, and test the null hypothesis whether the estimated coefficients for the policy variables from the restricted choice set are the same as the estimated coefficients with the full choice set of

51 states.¹⁵ A stronger version of the test of the IIA assumption would be to conduct this test for all the various possible sub-sets of choices. However, I think that restricting the model to randomly selected 35 choices (less than 70 per cent of the choices in the full set) would give a reasonable idea of the validity of my assumption.

It is possible that some unobserved time varying factors that are correlated with the policy variables may bias the estimated coefficients in this analysis. To control for these factors, I use a variation of the commonly used difference-in-differences (DD) methodology. In the DD procedure, change over time in the location choice of a group affected by social policy changes, the treatment group, is compared to the change over time in the location choice of a similar group, also called the comparison group, that is unaffected by the policy changes. As the comparison group is assumed to be unaffected by the policy change, the change overtime in the location choice of this group captures the influence of factors other than public policies, that influence both the treatment and the comparison groups equally. To isolate the influence of public policy changes on the location choice of the treatment group from these other factors, the changes in the location choices of the comparison group are subtracted from the changes in the location choices of the treatment group. The underlying assumption in this analysis is that time varying factors other than public policies with regard to welfare benefits, Medicaid and food stamps have the same effect on the location choices of the treatment and the comparison groups.

¹⁵ Let δ_r be a vector of the estimated coefficient from the restricted set of choices and V_r be the corresponding covariance matrix, and δ be the estimated coefficient for the full set of choices and V be the corresponding covariance matrix. Hausman and McFadden develop a test for the null hypothesis that $\delta_r = \delta$. The test statistic for the test : $(\delta_r - \delta)'(V_r - V)^{-1}(\delta_r - \delta)$ is asymptotically χ^2 with n degrees of freedom, where n is the number of elements in the sub-vector of coefficients that is identifiable from the restricted choice set model.

In the commonly used DD analyses, the effects of all other explanatory variables (except the policy variables) are often constrained to be the same for both the treatment and the comparison groups. Computational problems associated with conditional logit models and large samples restrict me from doing the same in this paper. I conduct the analysis separately for the treatment and the comparison groups, and then compare the impact of policy variables on them. In other words, I don't constrain the impact of observable factors other than policy variables to be the same for both the groups, but I assume that time varying unobserved factors affect the two groups equally. This is a preferred estimation strategy as it is less restrictive. Also, it allows me to control for time varying unobservable factors other than the policy changes that may be contaminating the estimated coefficients.

The treatment group in this analysis is unmarried unskilled women immigrants. A critical aspect of the DD analysis is the choice of the comparison group. To test the validity of my results, I use two comparison groups: married unskilled women immigrants and unmarried skilled women immigrants. Both the comparison groups have some weaknesses. Although a large number of women receiving TANF, Medicaid and food stamps are unmarried, poor unskilled married women are also eligible for these benefits. Hence, the location choice of low-skilled married women may also be affected by social policy changes to a small extent. Besides, the location choices of married women are not independent of the location choices of their husbands.

Skilled unmarried women, the other comparison group, are not likely to be affected by social policy changes as they are ineligible for these programs. But it can be argued that time varying factors, such as employment opportunities, may affect the

location choices of this group in a manner different from the location choices of unskilled unmarried women immigrants. For this reason, I control for the state real wage rate of workers with at least some college education in the analysis for skilled unmarried women, and I control for the real wage of workers with education high school or less in the analysis for unskilled women.

DATA AND SAMPLE ANALYSIS

I use INS data on immigrants who arrived in the US during October 1995 to September 1996 (henceforth referred to as 1996) and during October 1997 to September 1998 (henceforth referred to as 1998). The first year captures the location choice of new immigrants in the period just before the policy change. In most states, PRWORA changes related to immigrants came into effect during 1997. So the second year of study is chosen to capture the variation in location choice of new immigrants after the new policy is implemented. There are certain advantages about using this data set. As mentioned above, the INS data contains information on legal immigrants only, which is crucial for this study as illegal immigrants are ineligible to participate in the three social programs discussed here. Another advantage to using this data set is that unlike the CPS, which gives information on only a sample of immigrants, the INS provides information on all the immigrants who arrived in the US in a particular year. It also provides detailed information on the nationality, country of birth and country of last residence of immigrants.

The INS data set also gives information on the year of arrival of the people who are given permanent resident status in a year. As I am interested in the location choices of

new immigrants, I choose immigrants who arrived in the US during the two years under study.

The data set sheds light on a number of personal characteristics of immigrants such as their age, sex and marital status, which are useful to define the treatment and comparison groups. Almost 80 percent of TANF beneficiaries are 18 to 45 years old women; a similar proportion are unmarried women. An equally significant number of Medicaid and food stamps recipients are also low-skilled unmarried women in the above age bracket. So I define low-skilled unmarried women in the above age bracket as the treatment group. To see the effect of changes in policy regarding these three programs, I investigate the location choices of the treatment group and compare them with the location choices of low-skilled married women immigrants and skilled unmarried women immigrants, the two comparison groups who are unlikely to be affected by the above policy changes. The comparison groups are also restricted to the above age group.

The INS data set has a few limitations. For instance, it only gives the state of intended residence of new immigrants, which in some cases, may differ from their actual state of residence. Besides, the data set is not very rich in personal information on immigrants. For instance, it does not give any information on the education level of immigrants. The INS data set specifies the present occupation of immigrants granted visa under the employment preference category; for others, it gives the occupation category in the country of last residence. Jasso, Rosenzweig and Smith (2000) have used occupation categories of immigrants to impute their education level. Imitating their technique, Jaeger (2000) finds that the imputed educational attainment is correct in 65 percent of the cases. I am interested in the location choice of low-skilled women, potential candidates for

welfare, food stamps and Medicaid. Instead of imputing education, I use their occupation category to determine their skill level. I select women with the following low-skilled occupational categories: unemployed, homemakers, laborers, service and unknown employment, and conduct the analysis separately for married and unmarried women from these occupational groups. I also conduct the same analysis on a group of skilled unmarried women. This group includes professional women such as doctors, nurses, executives, engineers, teachers or artists.

As mentioned earlier, distance affects immigration flows through diminished information and increased travel and psychic costs. Besides factors like climate in the home country may also be a factor in the location choice. To control for these factors, I further stratify new legal women immigrants on the basis of their country of birth. I do this analysis for new immigrant women from five countries: Mexico, the Philippines, China, Vietnam and the Dominican Republic; and five regions: Central America (outside of Mexico); three countries in South Asia - Bangladesh, India and Pakistan; four countries from East Europe – Russia, Ukraine, Romania and Poland; three countries from South America -- Ecuador, Columbia and Peru, and the Caribbean excluding Dominican Republic, Haiti and Cuba. I do not include immigrants from Haiti and Cuba as immigrants from these two countries are treated as refugees so far as state assistance is concerned (Dodson III, 2001). These 10 countries/ regions cover over 65 percent of all immigrants in the above age groups who arrived in the US in the two years under study.

For the purpose of this study, I want to control for the foreign born population living in a state. CPS estimates of state level immigrant population are not very precise. To improve the precision, I compute a three-year moving average of foreign-born population

in each state for 1995 and 1997 from March CPS, and merge this variable with the INS data set.¹⁶ I use the INS data to determine the state of residence of recent immigrants who arrived in the US in the last five years by the country/region of birth. These two variables are affected by the flow of arrivals in year t . To avoid regressing the dependent variable on itself, I lag both variables by one year. Along with these variables, state unemployment rates for years $t-1$ and $t-2$ are merged with the individual level INS data.

Census bureau estimate of state level population for 1995 and 1997 are also merged with the original data set. State median wage of workers with 12 or fewer years of education in year $t-1$ is computed from the CPS outgoing rotation files. It is deflated by the national CPI, and merged with the INS data. As mentioned earlier, in the analysis for skilled unmarried women, the state real median wage of workers with at least some college is merged with the INS data. The usual problems associated with measurement of the control variables are greatly reduced by the use of state-fixed effects since few of these variables vary significantly over the period of study.

I study the impact of three policy variables: state policies towards the eligibility of recent immigrants for Medicaid, TANF and food stamps. Information reported in Zimmermann and Tumlin (1999) is used to construct the policy variables, and merged with the above data set. Like Borjas (2001), I compute an index of welfare benefits, based on whether a state has one or more of the above three programs for post-reform immigrants, and study whether new immigrants are going to states that are providing them access to a larger number of programs.

¹⁶ These estimates are corrected for new weights for the years 1994 and 1995 computed by Jeffrey Passel of the Urban Institute. I thank Jeffrey Passel for sharing the corrected weights.

The generosity of benefits under the three programs studied here differs across states. For instance, in 1996, in Alaska, the most generous state under AFDC, the maximum cash benefit for a family of three was \$923 per month, seven and a half times the cash benefit awarded by the least generous state Mississippi (\$120 a month). In their cost-benefit analysis, new immigrants may not merely look at their eligibility for means-tested programs, but also the differences in generosity between states. A complete specification of the cost-benefit analysis would require constructing a variable on the basis of both generosity and eligibility. Therefore, I also construct the three policy variables according to their benefit level. The maximum cash benefit for a family of three is taken from the Green Book 1996 and 1998. Maximum food stamps voucher for a family of three is also taken from the Green Book. Average Medicaid expenditure per eligible person is computed from Health Care Financing Administration HCFA-64. All the three amounts are deflated by the national consumer price index. All the three variables are computed according to immigrant eligibility and the amount of the benefit. So if new immigrants are not eligible for TANF in a state, the policy variable on TANF is 0. If they are eligible, the variable takes the value of the CPI deflated benefit level.

RESULTS

Tables 1 and 2 give the intended state of residence of immigrants who arrived in the US in 1996 by the country of their birth, and depict the large scale clustering of immigrants in a few states. In eight out of the ten countries/regions, at least 30 per cent of new immigrants chose a single state to be their intended state of residence. More than 60 percent of new immigrants from the Caribbean (excluding Haiti and Cuba) expressed

New York to be their intended state of residence. As many as 48 percent of the Filipinos and 44 percent of the Mexicans said they intended to settle in California.

The second thing to note from these tables is that the geographic dispersion of new immigrants varies depending on their country of birth. In general new immigrants from Latin America are far less dispersed than immigrants from Europe or Asia. New immigrants from the Caribbean are the least dispersed group: around 85 percent go to three states: New York, New Jersey and Florida. Around 83 per cent of new Mexicans intend to live in four states: California, Texas, Illinois and Arizona. New immigrants from the rest of Central America are geographically much less concentrated.

In comparison with new immigrants from Latin America, new immigrants from Asia and Eastern Europe are more evenly dispersed (Table 2). Among the immigrants from Asia considered here, the Filipinos are the most clustered group followed by the Chinese, with immigrants from Bangladesh, India and Pakistan being relatively evenly dispersed. Similarly, new immigrants from Poland, Romania, Russia and Ukraine are also relatively evenly dispersed. In short, Tables 1 and 2 make it clear that although most new immigrants live in a few states, the degree of their dispersion (and the states they choose to live) differ by their country of birth. This justifies my strategy of conducting separate analyses for groups of immigrants on the basis of their country/region of birth.

Effect of Eligibility for Means-tested Programs:

Tables 3-4 give the conditional logit estimates of the locational choice model for unmarried low-skilled women in age group 18 to 45 years, potential candidates for social programs for the poor. These tables give the estimates of the coefficients (with standard

errors in parenthesis), which differ from the marginal effects of explanatory variables. As argued earlier, the marginal effect of the policy change on the probability of residing in a state differs across states depending on the mean probability that the immigrant will live in that state. However, as will become clear in the later part of this analysis, I am more interested in the sign and the statistical significance of the coefficients for policy variables, than their size.

I now discuss the results from Tables 3 and 4. Each column in Tables 3 and 4 represents a separate regression. The first column for each country group does not control for state fixed effects, the second column does. I first discuss the results of the analysis without state fixed effects. In this section, the three policy variables are defined in terms of new immigrants' eligibility for a means-tested program. In 1996, all the three variables – TANF, Medicaid and Food Stamps - are equal to one for every state. In 1998, these variables are equal to 1 if the state provides immigrants access to that program, otherwise 0.

Without state controls, the impact of TANF on the location choices of immigrants from all country/regional groups is negative, and is statistically significant in five cases.¹⁷ This is counter-intuitive, as according to economic theory, low-skilled women immigrants are more likely to live in states that provide them access to welfare benefits.

Without controlling for state effects, Medicaid has a positive impact on the location choice of low-skilled women from eight out of the ten country / region groups, of which six are statistically significant. It has a negative, but statistically insignificant, impact in two cases. Food Stamps have a positive and statistically significant impact in

¹⁷ I also conduct all the regressions in this analysis, by using TANF + general assistance as a policy variable instead of just TANF. The overall results remain the same. Hence, I do not report them separately.

two of the 10 cases. In the remaining eight cases, food stamps do not have any significant impact on the location choices of low-skilled women.

The signs of the impact of demographic characteristics on location choices of immigrants are the same for all the 10 country/regional groups in the analysis with no state fixed effects, and similar to what has been found in previous studies. Population has a positive and statistically significant impact on the location choices of immigrants from the 10 country/regional groups. Number of recent immigrants from the same country also has a positive impact on the location choices of immigrants. However, new immigrants from all the 10 groups are discouraged from settling in states with a high foreign-born population. This gives us an interesting insight into the location choices of immigrants who arrived in 1996 and 1998. They did not want to go to states with a high foreign-born population, but they were more inclined to move to states with a high recently arrived population from their country of birth. That is, after controlling for the number of people from their country of birth, immigrants prefer to live in states that have a more homogeneous population. There are two possible explanations for this behavior. First, new immigrants want to settle in states where they are one of the largest, and therefore, politically prominent, ethnic minorities. Second, new immigrants often compete for jobs with immigrants already living in the US. Therefore, they may like to go to states where there are fewer immigrants with whom they may have to compete.

The impact of economic characteristics of states is not as consistent across country/region groups, which is quite in line with some of the earlier studies. State real wage of workers with 12 or fewer years of education has a positive and significant impact on the location choice of eight out of 10 groups; it is negative and significant in the case

of Mexicans and negative but not statistically significant in the case of immigrants from Vietnam. State unemployment rate in the year prior to the immigrants' arrival has a negative impact in seven of the 10 groups, and is statistically significant in four of them. The coefficient is positive in three cases and statistically significant in two of them. The state unemployment rate two years prior to the arrival of the immigrant is positive and statistically significant in four cases, and negative and statistically significant in one case.

When I control for state effects the impact of most of the explanatory variables disappears. This is expected in the case of the economic and demographic factors as they are highly correlated with state fixed effects, with the correlation coefficient between each of these factors and state effects to be between 0.9 and 1. This indicates that the overtime variation in demographic and economic factors is very small. But the time variation in the policy variables continues to be high. The correlation coefficient between state effects and the policy variables is less than 0.66. This is because, as explained earlier, each of the policy variables is 1 in 1996. In 1998, it is equal to 1 for states that have a substitute program for new immigrants and 0 for those that do not.

A comparison of the standard errors of the coefficients further illustrates this point. The standard errors for the estimated coefficients of the state demographic and economic characteristics increase by three to 50 times when I introduce state fixed effects, which indicates severe collinearity. The increase in the standard errors for the coefficients measuring the impact of policy variables is much less. For instance, the estimates for Mexico show that when state effects are introduced the standard error of the estimated coefficient for number of foreign born people in the state increases by 13 times, the standard error for the coefficient for population increases by 10 times, the standard

errors for the coefficients for state unemployment rates double and for the real wage coefficient the standard error increases at least six times. In contrast, the increase in the standard error for the coefficient for TANF is only 20 percent, for the coefficient for food stamps just 30 percent, and for the coefficient for Medicaid a mere 40 percent. This indicates that state controls do not wipe out the effect of the policy changes, but make them less biased by purging out the impact of time invariant state specific factors that influence the location decisions of immigrants.

Tables 3 and 4 show that when state effects are controlled, TANF does not have a statistically significant effect on the location choice of immigrants in any country/region group. Medicaid does not have any statistically significant impact on the location choices of immigrants in the case of eight groups. It has a positive and significant effect on the location choices Filipinos, but a negative impact on the location choice of immigrants from the Caribbean (outside of the Dominican Republic). Lastly, Food Stamps do not have significant impact on the location choice of eight out of ten country/region groups. It has a positive and significant impact in the case of new immigrants from other Caribbean countries and a negative and significant impact in the case of immigrants from the rest of Central America. This shows that the three policy variables do not have any statistically significant impact of the location choices of most immigrant groups; in the few cases when the analysis does show a statistically significant impact, it is not consistent across country/ regional groups.

The last row of both Tables 3 and 4 gives the chi-square statistic for the test of significance of IIA for the three policy variables. In all cases, except for the Dominican Republic, the test shows that I cannot reject the hypothesis that the coefficients for policy

variables are the same whether the number of choices open to an immigrant are 51 states, or are randomly restricted to 35.¹⁸ This shows that the IIA assumption is not restrictive in this model.

It is possible that the above results are contaminated by time variant changes in unobserved location characteristics correlated with the policy changes. To purge the analysis of these effects, I compare the effect of social programs on the location decisions of low-skilled unmarried women immigrants, the treatment group, with the location decisions of married low-skilled and unmarried skilled immigrant women, the two comparison groups. The results of the impact of policy changes on the location choices of the three groups are in Tables 5 and 6.

Column I of Tables 5 and 6 gives the impact of policy changes on the location choices of the treatment group (repeated from Tables 3 and 4 to simplify comparisons.) Column II of Tables 5 and 6 gives the impact of policy changes on the location choices of the first comparison group, married low-skilled women. Column III gives the same for the second comparison group, unmarried skilled women. Column IV gives the difference between column I and II, or what I call the difference-in-differences estimate of the impact of the policy changes on location choices of unmarried unskilled women with married unskilled women as the comparison group.¹⁹ Column V (column I - column III)

¹⁸ The test statistic for the Dominican Republic is negative. Hausman and McFadden (1984) argue that this may happen due to lack of semi-definiteness in finite sample applications.

¹⁹ McFadden (1973) showed that conditional logit estimate of δ is consistent and $\sqrt{N(\delta - \delta^*)}$ converges to a normal random vector with zero mean and covariance matrix $p \lim_{n \rightarrow \infty} (-\partial^2 L(\delta^*) / \partial \delta \partial \delta)^{-1}$. (Here δ^* is the parameter vector and $L(\delta^*)$ is the maximum likelihood function.) Therefore, if δ_T is the maximum likelihood estimator for the treatment group and δ_C is the maximum likelihood estimator for the comparison group, the DD estimator, $\delta_T - \delta_C$ is also consistent with the mean $\delta_T^* - \delta_C^*$ and standard

gives the same with unmarried skilled women as the comparison group. Standard errors for the DD estimates are given in parenthesis.

Column II shows that TANF has not had any impact on the location choices of married low-skilled women in nine out of 10 country/regional groups. In the case of married unskilled Filipino women the effect of TANF is negative and statistically significant. Similarly, Medicaid has no impact on the location choices of women from nine out of 10 cases. Only in the case of Mexican low-skilled married women, Medicaid has a positive and statistically significant effect. Food Stamps do not have any impact on the location choices of married women from any of the 10 country/regional groups.

Column IV gives the difference between the impact of the three policy changes on the location choices of unskilled unmarried and married women, and further strengthens the earlier results that policy changes associated with PRWORA have had no impact on the location choices of unskilled unmarried immigrant women. The impact of public programs is statistically significant in only two cases: food stamps have a negative and significant impact on the location choices of the treatment group from the rest of Central America and Medicaid has a positive impact on the location choices of the treatment group from Filipino. In the remaining cases, none of the policies considered here have any impact.

I want to test the DD approach using another comparison group - unmarried skilled women. Column III in Tables 5 and 6 gives the impact of policy changes on the second comparison group. It shows that TANF has a positive and statistically significant

error $\sqrt{e_T^2 + e_C^2}$ where e_T is the standard error for the treatment group and e_C is the standard error for the comparison group.

impact on the location choices of two country/regional groups and no impact in the remaining eight cases. Medicaid has a negative and significant impact on the location choices of two groups and no impact in the remaining eight cases, and food stamps have a negative and significant impact on the location choices of Chinese skilled women immigrants and no impact in the remaining cases. Column V gives the DD estimate the impact of the three policy changes on the location choices of unmarried unskilled women, with skilled unmarried women as the comparison group. It shows that TANF has a negative and significant impact in the location choices of two groups – China and Bangladesh, India and Pakistan; Medicaid has a positive and significant impact in the case of three groups – rest of Central America and Bangladesh, India and Pakistan and Peru, Columbia and Ecuador. Food stamps have a negative and significant impact in the location choices of two groups – rest of Central America and Peru, Columbia and Ecuador. The impact of TANF and Food Stamps in the above cases is counter-intuitive. More important, of the 30 cases, the coefficient is statistically insignificant for 23, and has a negative and statistically significant sign for four cases. In only three cases, the coefficient is positive and statistically significant.

Thus, the analysis using an alternate comparison group yields the same answer that the location decisions of immigrant unskilled unmarried women have been largely unaffected by the recent policy changes in their access to means-tested programs.

Effect of Intensity of Eligibility for Means-tested Programs:

As mentioned earlier, I am also interested in investigating the impact of the intensity of social programs on the location decisions of immigrants. I construct five

dummy variables. Variable ‘All Three’ is equal to one for states that have all three programs (TANF, Medicaid and food stamps) for recent immigrants, otherwise 0.

Variable ‘TANF and Medicaid’ is equal to one for states that have TANF and Medicaid, but no Food Stamps; otherwise 0. As the title suggests, variable ‘TANF only’ is equal to one for states that provide for the TANF needs of recent immigrants, but do not provide any of the other two programs. The variable is 0 in all other cases. And variable ‘other’ is 1 for all other cases when a state has one or more of the above three programs, except for the above three categories. Variable ‘no social program’ is equal to one in states that do not have any social program for recent immigrants; it is the omitted category in all the regressions.

The analysis for the Dominican Republic has slightly different variables, as not many women from the Dominican Republic went to states that provided for only TANF and Medicaid in 1996, rendering the above classification meaningless. The intensity of public programs in this case is defined as ‘all three’ which is equal to one in states that provided new immigrants access to all the three policies, and zero otherwise. ‘Only two’ is equal to one for states that provide new immigrants access to two of the programs, zero otherwise. Similarly, ‘Only one’ is equal to one for states that provide new immigrants access to one program only, and zero otherwise. Variable “no social program” is the omitted category in this analysis too.

Tables 7 and 8 give the impact of the intensity of reform on the target group and the two comparison groups with state fixed effects. These tables echo the results obtained in the analysis earlier. The intensity of public programs does not have an impact on the location choices of unmarried unskilled women (column I in Tables 7 and 8). Intuitively,

if public programs affected the location decisions of new immigrants, the impact should increase with the intensity of the programs. The impact of 'All Three' should be the highest followed by the impact of 'TANF and Medicaid', 'Only TANF' and 'Other' in that order. The results in column I do not indicate that. Moreover, all the coefficients are statistically insignificant, except for 'TANF and Medicaid' for Filipinos immigrants, which is positive and significant. Again, the coefficient for "All Three" is statistically insignificant, and is almost a third of the coefficient for "TANF and Medicaid."

Column II in Tables 7 and 8 gives the impact of the index of policy reform on the location choices of married low skilled women. The impact of "All Three" is positive and significant only in the case of Mexican married low-skilled women (column II). It is negative and significant in the case of Filipino married women. In all other cases, the impact of "All Three" is statistically insignificant. The impact of "TANF and Medicaid" is positive and significant for married low skilled women from Peru, Columbia and Ecuador and Bangladesh, India and Pakistan, and statistically insignificant in all other cases.

Column III in Tables 7 and 8 gives the impact of the index of policy reform on the location choices of unmarried skilled women. The impact of "All Three" is positive in the case skilled women from the rest of the Caribbean. In all the other cases, none of the policies have any impact on the location choices of immigrants (except for "Only TANF" in the case of skilled immigrants from Vietnam). I do not report the results for unmarried skilled women for three groups of unmarried skilled women, as not many women went to either the states that provided for both 'TANF and Medicaid' or the states that provided 'only TANF', rendering this classification meaningless.

The difference-in-differences estimates of the effect of the index of policy reform is shown in columns IV (with unskilled married immigrant women as the control group) and V (with skilled unmarried women as the control) of Tables 7 and 8. These estimates show that low skilled unmarried women immigrants from nine of the ten country/region groups are unaffected by any of the above policy changes independent of the comparison group I use. The only exception is Filipino low skilled new immigrant women. When married low skilled women immigrants are used as the comparison group, “All Three” and “TANF and Medicaid” have a positive effect on the location choices of the Filipino low skilled women immigrants. But the coefficient for “TANF and Medicaid” is almost double the coefficient for “All Three”, which is counter-intuitive. When unmarried women are the control group, the DD estimate shows that “All Three” have a positive impact on the location choices of unmarried unskilled Filipinos.

To conclude, this analysis further strengthens the earlier results that recent policy changes in new immigrants’ access to means-tested programs have not affected in their location decisions.

Effects of Eligibility and Generosity of Means-tested Programs

So far in the analysis, I compared the changes in new immigrants’ entry to states that have cutoff their eligibility to means-tested programs to changes in immigrants’ entry to states that maintained eligibility. As mentioned earlier, a complete specification of the cost-benefit analysis would require constructing a variable on the basis of both eligibility and generosity of benefits. I construct all the three policy variables according to immigrant eligibility and the benefit amount in a state. So if new immigrants are not

eligible for a program, the policy variable is 0. If they are eligible, as described earlier, the variable takes the CPI deflated value of the benefit.²⁰ Tables 9 and 10 give the results of this analysis.

Column I in Tables 9 and 10 gives the conditional logit estimates of the effects of the generosity of the three programs on the locational choice of low-skilled unmarried women immigrants. It shows that generosity of the three programs have no statistically significant impact on the locational choice of this group except for two cases. The level of food stamps voucher has a positive and significant impact in the case of new immigrants from rest of the Caribbean; TANF generosity has a negative and significant impact on the location choice of new immigrants from Bangladesh, India and Pakistan.

Column II in Tables 9 and 10 gives the conditional logit estimates of the effects of the generosity of the three programs on the locational choice on the first comparison group, low-skilled married women immigrants. It shows that generosity of Food Stamps vouchers have no statistically significant impact on the locational choice of this group. The generosity of TANF benefits also do not have a statistically significant impact except for the case for Filipino women, who are less likely to go to states that provide more generous benefits. Medicaid expenditure also does not have a significant effect, except for Chinese women, who are more likely to go to states with high Medicaid expenditure.

The analysis using the second comparison group, unmarried skilled women, are in column III of Tables 9 and 10. It shows that TANF generosity has a positive and significant impact on the location choices of immigrants from China and the rest of the

²⁰ I also construct a variable that combines all the three benefits, and repeat the above analysis. The results of the analysis do not change. Therefore, I do not report the results. As New York, New Mexico and Alaska, give general assistance and no TANF, I use the amount of benefit under general assistance instead of TANF benefit for 1998 for these three states.

Caribbean. In the remaining eight cases, TANF generosity has no impact on the location choice of this group. Average Medicaid expenditure per eligible person has statistically insignificant effect in all the 10 cases. The level of Food Stamps voucher has a negative and significant impact on the residential choice of Chinese women and no impact in the other nine cases.

Columns IV and V give the DD estimates using the two comparison groups. It shows that for seven of the 10 country/regional groups, TANF generosity has no effect on the locational choice of the treatment group irrespective of the comparison group I use. TANF generosity has a negative and significant impact on the locational choice of the treatment group from Bangladesh, India and Pakistan when either of the comparison groups is used. The impact is negative for Chinese low-skilled unmarried immigrants also, and it is statistically significant when the comparison group is unmarried skilled Chinese women. This is counter-intuitive as immigrants should be attracted to states that are more generous in giving cash benefits. I observe the reverse phenomenon here. In only one case, that of Filipino women, I find that welfare generosity has a positive and statistically significant impact when married women are the comparison group.

In seven of the 10 cases, average state Medicaid expenditure has statistically insignificant impact on the location choices of the treatment group irrespective of the comparison group I use. When unmarried skilled women are the comparison group, state Medicaid expenditure has a positive and significant impact on the location choice of the treatment group from the rest of Central America, Peru, Columbia and Ecuador and Rest of the Caribbean. But when married low skilled women are the comparison group, the

impact of Medicaid becomes statistically insignificant in two cases, and turns negative in the case of women from the rest of the Caribbean.

The effect of food stamp voucher is also statistically insignificant in most of the cases irrespective of the comparison groups used; except for the women from the rest of Central America and Peru, Columbia and Ecuador for whom the impact is negative.

When married low-skilled women are the comparison group, the effect of food stamp vouchers is positive and significant for women from Rest of the Caribbean.

In conclusion, the generosity analysis echoes the results of the earlier two analyses. The generosity of means-tested programs does not appear to affect the location choices in most cases. In the few cases where the generosity of the programs has an effect, it is often counter-intuitive. This shows that there are no welfare magnets as far as new immigrants are concerned.

CONCLUSION

The above analysis shows that changes in immigrants' access to means-tested programs – TANF, Medicaid and food stamps - have no impact on the location choices of new unmarried women immigrants who are low-skilled. The difference-in-differences estimates of the impact of PRWORA on these immigrants using two comparison groups – married-unskilled and unmarried-skilled recent women immigrants -- are also statistically insignificant in most cases. The few cases when the impact is statistically significant, the sign is not consistent across groups. The analysis using the generosity of means-tested programs yields the same result.

From this I conclude that as far as new immigrants are concerned there are no welfare magnets. My analysis differs from previous studies that found positive evidence in support of the welfare magnet hypothesis on at least three accounts. Firstly, I study the effect of changes in policies that denied new immigrants' access to means-tested programs in a number of states. Earlier papers tested inter-state and overtime variations in welfare generosity. The results of the earlier studies may be contaminated by the endogeneity of the policy variables. From the pattern of recent state level policy changes, it is reasonable to argue that these could not have been predicted. Secondly, I control for state effects that most other studies do not. However, even in the analysis that does not control for state effects, I do not get consistent results in favor of the welfare magnet hypothesis. Thirdly, I also do the analysis of immigrants' location choices by their countries/ regions of birth.

I also find that immigrants' location choices are unaffected whether a state offers all the three or fewer programs to immigrants, which further strengthens my results that there are no welfare magnets for new immigrants. To test the model specification, I test the IIA assumption by restricting the choice set to 35 states and compare with the results with the unrestricted model. The test validates the IIA assumption in this analysis.

My analysis shows that accessibility to means-tested programs (or their generosity) is not an issue in their location decisions when immigrants first arrive in the US. Therefore, it is unlikely that PRWORA will significantly alter the composition of immigrants, or their location pattern within the US. Therefore, states should not erroneously believe that they can export their immigrant problem by denying immigrants benefits. Current immigration law, which gives preference to family members of US

citizens and legal immigrants, is likely to continue attracting immigrants from Latin America and Asia with relatively low education and skill levels in large numbers. These immigrants will continue to locate in ethnic enclaves in large urban areas. Given their low socio-economic background, newer waves of immigrants are likely to be relatively poor, and consistent with their poverty, they will require public assistance more than those with higher incomes. As previous research has shown immigrants will not require more public assistance than similarly situated natives (Butcher and Hu, 2000, Kaestner and Kaushal 2001). Therefore, denial of benefits to new immigrants cannot be justified on the premise that new immigrants are a particularly poorly motivated group who are unlikely to achieve economic self-sufficiency. As argued earlier, it can be defended only on the grounds of increased fiscal savings that it generated.

Table 1
Location Choice of New Immigrants, by Country of Birth: 1996

Mexico		Rest of Central America ¹		Peru, Columbia and Ecuador		Dominican Republic		Other Caribbean Countries ²	
Intended States of Residence	New Immigrants (%)	Intended States of Residence	New Immigrants (%)	Intended States of Residence	New Immigrants (%)	Intended States of Residence	New Immigrants (%)	Intended States of Residence	New Immigrants (%)
California	43.63	California	35.13	New York	35.10	New York	68.33	New York	55.76
Texas	29.47	New York	12.62	New jersey	20.46	New jersey	14.16	Florida	15.67
Illinois	6.21	Texas	10.24	Florida	15.05	Massachusetts	5.95	New Jersey	8.27
Arizona	4.07	Florida	9.32	California	8.10	Florida	5.64	Maryland	3.64
New Mexico	2.17	New Jersey	5.13	Connecticut	2.59	Rhode Island	1.67	Connecticut	3.59
Florida	1.73	Virginia	4.31	Texas	2.49	Pennsylvania	0.76	Massachusetts	2.38
Colorado	1.45	Maryland	4.26	Illinois	2.44	Connecticut	0.75	Pennsylvania	1.87
Washington	1.10	Illinois	2.34	Virginia	2.02	Maryland	0.53	California	1.39
Georgia	1.09	Massachusetts	1.83	Massachusetts	1.67	DC	0.34	Texas	1.05
Nevada	0.93	Louisiana	1.48	Maryland	1.52	Texas	0.28	Georgia	1.01
Oregon	0.83	DC	1.36	Pennsylvania	0.88	Illinois	0.23	Illinois	0.94
New York	0.78	Georgia	1.13	Rhode Island	0.84	California	0.21	DC	0.54
Pennsylvania	0.59	Nevada	0.91	Georgia	0.74	Michigan	0.13	Ohio	0.54
Kansas	0.48	North Carolina	0.76	Minnesota	0.48	North Carolina	0.12	Virginia	0.44
Idaho	0.48	Connecticut	0.73	Utah	0.45	Virginia	0.12	Minnesota	0.43
Total	95.01	Total	91.55	Total	94.83	Total	99.22	Total	97.52
# of New Immigrants	52929	# of New Immigrants	17879	# of New Immigrants	17158	# of New Immigrants	25783	# of New Immigrants	24039
% new immigrants in the top 6 states	87.28	% new immigrants in the top 6 states	77.25	% new immigrants in the top 6 states	83.79	% new immigrants in the top 6 states	96.51	% new immigrants in the top 6 states	89.31

Note: ¹ Excludes immigrants from Mexico.

² Excludes immigrants from the Dominican Republic, Haiti and Cuba.

Table 2
Location Choice of New Immigrants, by Country of Birth: 1996

China		Bangladesh, India and Pakistan		Vietnam		Philippines		Poland, Romania, Russia and Ukraine	
Intended States of Residence	New Immigrants (%)	Intended States of Residence	New Immigrants (%)	Intended States of Residence	New Immigrants (%)	Intended States of Residence	New Immigrants (%)	Intended States of Residence	New Immigrants (%)
NY	31.92	California	18.44	California	37.81	California	48.45	Illinois	23.88
California	30.3	New York	13.73	Texas	10.98	Hawaii	12.95	New York	21.47
Massachusetts	3.75	New jersey	13.58	Virginia	3.61	New York	4.34	New Jersey	9.42
New Jersey	3.35	Illinois	10.9	Washington	3.61	New jersey	4.01	California	7.91
Illinois	2.64	Texas	7.36	Pennsylvania	3.09	Illinois	3.96	Michigan	4.18
Texas	2.32	Pennsylvania	3.91	Florida	2.93	Washington	3.77	Pennsylvania	3.85
Pennsylvania	2.26	Maryland	3.08	New York	2.88	Florida	2.70	Connecticut	3.28
Washington	1.91	Florida	2.85	Georgia	2.87	Virginia	2.61	Massachusetts	2.81
Maryland	1.90	Michigan	2.61	Massachusetts	2.24	Texas	2.43	Florida	2.38
Hawaii	1.51	Georgia	2.51	Minnesota	1.93	Nevada	1.60	Ohio	2.29
Ohio	1.47	Ohio	2.25	Oregon	1.80	Maryland	1.56	Texas	2.29
Florida	1.39	Virginia	2.25	Kansas	1.65	Alaska	0.95	Washington	1.61
Oregon	1.34	Massachusetts	2.06	New Jersey	1.64	Pennsylvania	0.86	Virginia	1.53
Virginia	1.34	North Carolina	1.64	Illinois	1.63	Oregon	0.79	Maryland	1.47
Michigan	1.04	Connecticut	1.44	Maryland	1.60	Michigan	0.74	Wisconsin	1.08
Total	88.44	Total	88.61	Total	80.27	Total	91.12	Total	89.45
# of New Immigrants	26561	# of New Immigrants	30051	# of New Immigrants	10014	# of New Immigrants	37405	# of New Immigrants	20860
% new immigrants in the top 6 states	74.28	% new immigrants in the top 6 states	67.92	% new immigrants in the top 6 states	62.03	% new immigrants in the top 6 states	77.48	% new immigrants in the top 6 states	70.71

Table 3

Conditional Logit Estimates of the Effects of New Immigrants' Eligibility to Means-tested Programs on the Location Choice of Low-skilled Unmarried Women Immigrants

Country of birth/ Explanatory Variables	Mexico		Rest of Central America ¹		Peru, Columbia and Ecuador		Dominican Republic		Other Caribbean Countries ²	
	No Fixed State Effects	State Fixed Effects	No Fixed State Effects	State Fixed Effects	No Fixed State Effects	State Fixed Effects	No Fixed State Effects	State Fixed Effects	No Fixed State Effects	State Fixed Effects
TANF	-1.0565** (0.1540)	-0.0686 (0.1884)	-1.6870** (0.2276)	0.9036 (0.5431)	-0.3808 (0.2182)	-0.0642 (0.3986)	-0.8255** (0.2838)	-0.7957 (0.8793)	-0.6970** (0.1651)	0.2801 (0.4167)
Medicaid	0.9208** (0.0932)	0.2278 (0.1371)	0.6069** (0.1541)	0.1755 (0.2812)	0.3464 (0.1797)	0.1369 (0.3094)	-0.3793 (0.2449)	0.4975 (0.8414)	-0.2703 (0.1489)	-0.6940* (0.3170)
Food Stamp	0.2387 (0.1551)	0.0143 (0.2025)	1.1307** (0.2306)	-1.2122* (0.5307)	-0.3373 (0.2482)	-0.5761 (0.4232)	0.4931 (0.2690)	0.5434 (0.5509)	0.7692** (0.1655)	0.7294* (0.3443)
State Unemployment Rate Last Year	-0.6809** (0.0712)	-0.1387 (0.1957)	0.5989** (0.1027)	-0.0132 (0.4731)	-0.5870** (0.0851)	0.0457 (0.4451)	-0.0750 (0.0929)	0.8854 (0.5738)	-0.1243 (0.0656)	-0.1585 (0.3637)
State Unemployment Rate 2 Years Ago	0.6074** (0.0620)	-0.1641 (0.1213)	-0.3492** (0.1084)	-0.0128 (0.2437)	0.6848** (0.0854)	0.1412 (0.2902)	0.0550 (0.1018)	-0.2106 (0.3748)	0.2075** (0.0669)	0.1070 (0.2016)
State Real Wage Rate of Women with 12 or Fewer Years of Education	-0.4751** (0.0624)	0.2996 (0.4117)	0.6793** (0.0786)	0.2158 (0.7545)	0.8132** (0.0875)	0.8316 (0.8011)	2.2422** (0.1161)	-0.5719 (1.1354)	1.0154** (0.0614)	0.4383 (0.7253)
Population	0.0002** (0.0000)	-0.0005** (0.0002)	0.0002** (0.0000)	-0.0001 (0.0005)	0.0001** (0.0000)	-0.0004 (0.0006)	0.0001** (0.0000)	0.0003 (0.0013)	0.0002** (0.0000)	0.0003 (0.0005)
Number of recent immigrants from the same country/ region	0.0228** (0.0011)	-0.0086 (0.0142)	0.0326** (0.0068)	0.0242 (0.0185)	0.0920** (0.0032)	-0.1211 (0.0910)	0.0307** (0.0008)	0.0417 (0.0773)	0.0410** (0.0008)	0.0222 (0.0382)
Number of Foreign Born People in the State	-0.0006** (0.0000)	0.0004 (0.0013)	-0.0006** (0.0001)	-0.0009 (0.0020)	-0.0002** (0.0001)	0.0033 (0.0023)	-0.0001 (0.0001)	-0.0002 (0.0052)	-0.0003** (0.0000)	0.0014 (0.0024)
Chi-Square Statistic	--	3.93	--	3.57	--	0.80	--	-389.43	--	0.82

Note: Excludes immigrants from Mexico. ¹ Excludes immigrants from the Dominican Republic, Haiti and Cuba. * .01<p<.05, ** p<.01

Table 4

Conditional Logit Estimates of the Effect of New Immigrants' Eligibility to Means-tested Programs on the Location Choice of Low-skilled Unmarried Women Immigrants										
Country of birth/ Explanatory Variables	China		Bangladesh, India and Pakistan		Vietnam		The Philippines		Poland, Romania, Russia and Ukraine	
	No Fixed State Effects	State Fixed Effects	No Fixed State Effects	State Fixed Effects	No Fixed State Effects	State Fixed Effects	No Fixed State Effects	State Fixed Effects	No Fixed State Effects	State Fixed Effects
TANF	-0.3578 (0.2274)	-0.4590 (0.4733)	-1.2183** (0.2100)	-0.5527 (0.3322)	-0.0368 (0.2452)	-0.1244 (0.3917)	-0.0136 (0.1330)	0.2901 (0.2561)	-0.4932 (0.3083)	0.6356 (0.5123)
Medicaid	0.6733** (0.2130)	0.4190 (0.3646)	0.6593** (0.1296)	0.3289 (0.2493)	0.5341* (0.2226)	0.2364 (0.3524)	1.5795** (0.1266)	0.4066* (0.1834)	0.3895 (0.2083)	0.0012 (0.4673)
Food Stamp	0.2055 (0.2231)	-0.4518 (0.4169)	0.1084 (0.2132)	0.2813 (0.3563)	0.4286 (0.2673)	-0.1579 (0.4270)	0.0217 (0.1312)	-0.2480 (0.2900)	0.3606 (0.3333)	-0.2785 (0.5510)
State Unemployment Rate Last Year	0.3633** (0.1238)	0.7084 (0.3903)	-0.2695** (0.1018)	0.3226 (0.3340)	-0.0788 (0.1492)	0.0506 (0.3884)	0.1316 (0.0697)	-0.1503 (0.2390)	-0.6492** (0.1727)	-0.7353 (0.5193)
State Unemployment Rate 2 Years Ago	-0.0566 (0.1263)	0.0430 (0.2701)	0.1694 (0.1067)	-0.1309 (0.2502)	0.0545 (0.1420)	0.1321 (0.2415)	0.0807 (0.0684)	0.1821 (0.2095)	0.5393** (0.1725)	0.1090 (0.3752)
State Real Wage Rate of Women with 12 or Fewer Years of Education	0.7630** (0.1200)	-1.5067* (0.6848)	1.2075** (0.1050)	-0.1235 (0.4761)	-0.0708 (0.1185)	0.3751 (0.6355)	0.5562** (0.0567)	0.4508 (0.4607)	1.5191** (0.1689)	1.2565 (0.9955)
Population	0.0002** (0.0000)	0.0013* (0.0006)	0.0002** (0.0000)	0.0002 (0.0004)	0.0002** (0.0000)	-0.0005 (0.0006)	0.0001** (0.0000)	0.0007 (0.0005)	0.0002** (0.0000)	0.0004 (0.0007)
Number of recent immigrants from the same country/ region	0.0932** (0.0037)	0.0724 (0.0501)	0.0425** (0.0033)	0.2395 (0.1383)	0.0195** (0.0044)	-0.0233 (0.0152)	0.0762** (0.0042)	0.0275 (0.0207)	0.0477** (0.0031)	0.0545 (0.0576)
Number of Foreign Born People in the State	-0.0007** (0.0001)	-0.0025 (0.0021)	-0.0004** (0.0001)	-0.0024 (0.0022)	-0.0004** (0.0001)	-0.0008 (0.0020)	-0.0008** (0.0001)	0.0010 (0.0012)	-0.0005** (0.0001)	0.0025 (0.0025)
Chi-Square Statistic	--	3.17	--	0.19	--	0.76	--	5.16	--	4.37

* .01<p<.05, ** p<.01

Table 5
Conditional Logit Estimates of the Effect of New Immigrants' Eligibility to Means-tested Policies on the
Location Choices of Low-skilled Unmarried, Low-skilled Married and Skilled Unmarried Women
Immigrants

	Unmarried Low-skilled Women	Married Low- skilled Women	Unmarried Skilled Women	Difference-in-differences Estimate	
	I	II	III	I-II	I-III
Mexico					
TANF	-0.07 (0.19)	0.04 (0.09)	0.76 (0.73)	-0.11 (0.21)	-0.83 (0.75)
Medicaid	0.23 (0.14)	0.19** (0.07)	-0.19 (0.92)	0.04 (0.16)	0.42 (0.93)
Food Stamps	0.01 (0.20)	0.03 (0.10)	-0.65 (0.99)	-0.02 (0.22)	0.66 (1.01)
Rest of Central America¹					
TANF	0.90 (0.54)	0.03 (0.17)	-0.23 (0.65)	0.87 (0.57)	1.13 (0.84)
Medicaid	0.17 (0.28)	0.08 (0.12)	-0.93** (0.44)	0.09 (0.30)	1.1* (0.52)
Food Stamps	-1.21** (0.53)	0.03 (0.18)	0.61 (0.63)	-1.24* (0.56)	-1.82* (0.82)
Peru, Columbia and Ecuador					
TANF	-0.06 (0.40)	0.14 (0.21)	-0.03 (0.71)	-0.20 (0.45)	-0.03 (0.81)
Medicaid	0.14 (0.31)	0.20 (0.15)	-1.27** (0.46)	-0.06 (0.34)	1.41** (0.55)
Food Stamps	-0.58 (0.42)	-0.28 (0.21)	1.05 (0.59)	-0.3 (0.47)	-1.63* (0.72)
Dominican Republic					
TANF	-0.80 (0.88)	-0.29 (0.42)	-3.05 (3.62)	-0.51 (0.97)	2.25 (3.72)
Medicaid	0.50 (0.84)	0.12 (0.38)	-0.88 (2.36)	0.38 (0.92)	1.38 (2.50)
Food Stamps	0.54 (0.55)	-0.03 (0.30)	-0.15 (3.05)	0.57 (0.63)	0.69 (3.10)
Rest of the Caribbean²					
TANF	0.28 (0.42)	0.40 (0.36)	1.26* (0.56)	-0.12 (0.55)	-0.98 (0.70)
Medicaid	-0.69** (0.32)	-0.09 (0.27)	-0.87 (0.46)	-0.60 (0.42)	0.18 (0.56)
Food Stamps	0.73** (0.34)	-0.16 (0.30)	0.51 (0.40)	0.89 (0.45)	0.22 (0.61)

Note: ¹ Excludes immigrants from Mexico. ² Excludes immigrants from the Dominican Republic, Haiti and Cuba. Coefficients for each country/region in columns I to III represent a separate regression. Each regression controls for state fixed effects, state unemployment rates with one and two years lags, state population last year, number of recent foreign born people living in the state last year and number of recent immigrants from that region living in the state last year. Columns I and II control for last year's real median wage of women with 12 or fewer years of education. Column III controls for last year's median wage of women with at least some college. * .01 < p < .05, ** p < .01

Table 6
Conditional Logit Estimates of the Effect of New Immigrants' Eligibility to Means-tested Policies on the Location Choices of Low-skilled Unmarried, Low-skilled Married and Skilled Unmarried Women

	Unmarried Low-skilled Women	Married Low- skilled Women	Unmarried Skilled Women	Difference-in-differences Estimate	
	I	II	III	I-II	I-III
China					
TANF	-0.46 (0.47)	-0.14 (0.17)	1.00* (0.41)	-0.32 (0.50)	-1.46** (0.62)
Medicaid	0.42 (0.36)	0.26 (0.14)	0.17 (0.36)	0.16 (0.39)	0.25 (0.51)
Food Stamps	-0.45 (0.42)	-0.10 (0.16)	-0.84* (0.42)	-0.35 (0.45)	0.39 (0.59)
Bangladesh, India and Pakistan					
TANF	-0.55 (0.33)	0.02 (0.08)	1.27 (0.71)	-0.57 (0.34)	-1.82** (0.78)
Medicaid	0.33 (0.25)	0.11 (0.06)	-0.94 (0.59)	0.22 (0.26)	1.27* (0.64)
Food Stamps	0.28 (0.36)	-0.09 (0.09)	-0.16 (0.73)	0.37 (0.37)	0.44 (0.81)
Vietnam					
TANF	-0.12 (0.39)	-0.11 (0.17)	0.31 (0.37)	-0.01 (0.42)	-0.43 (0.54)
Medicaid	0.24 (0.35)	-0.04 (0.15)	-0.08 (0.32)	0.28 (0.38)	0.32 (0.47)
Food Stamps	-0.16 (0.43)	0.15 (0.18)	-0.14 (0.38)	-0.31 (0.47)	-0.02 (0.57)
Philippines					
TANF	0.29 (0.26)	-0.22* (0.11)	-0.27 (0.35)	0.51 (0.28)	0.56 (0.44)
Medicaid	0.41* (0.18)	0.01 (0.09)	-0.08 (0.26)	0.40* (0.20)	0.49 (0.32)
Food Stamps	-0.25 (0.29)	-0.03 (0.13)	0.03 (0.40)	-0.22 (0.32)	-0.28 (0.49)
Poland, Romania, Russia and Ukraine					
TANF	0.64 (0.51)	0.15 (0.17)	0.01 (0.34)	0.49 (0.54)	0.63 (0.61)
Medicaid	0.00 (0.47)	-0.04 (0.16)	-0.20 (0.34)	0.04 (0.50)	0.20 (0.58)
Food Stamps	-0.28 (0.55)	-0.22 (0.19)	0.27 (0.39)	-0.06 (0.58)	-0.55 (0.67)

Note: Coefficients for each country/region in columns I to III represent a separate regression. Each regression controls for state fixed effects, state unemployment rates with one and two years lags, state population last year, number of recent foreign born people living in the state last year and number of recent immigrants from that region living in the state last year. Columns I and II control for last year's real median wage of women with 12 or fewer years of education. Column III controls for last year's median wage of women with at least some college. * .01<p<.05, ** p<.01

Table 7
Conditional Logit Estimates of the Impact of the Intensity of Public Policies on the Location Choices
of Low-skilled Unmarried, Low-skilled Married and Skilled Unmarried Women Immigrants

	Unmarried Low-skilled Women	Married Low- skilled Women	Unmarried Skilled Women	Difference-in-differences Estimate	
	I	II	III	I-II	I-III
Mexico					
All Three	0.50 (0.30)	0.33* (0.13)	--	0.17 (0.33)	--
TANF, Medicaid	-0.47 (0.43)	-0.21 (0.20)	--	-0.26 (0.47)	--
Only TANF	-0.11 (0.24)	0.24* (0.12)	--	-0.35 (0.27)	--
Other	0.24 (0.14)	0.25** (0.07)	--	-0.01 (0.16)	--
Rest of Central America¹					
All Three	-0.17 (0.36)	0.14 (0.15)	--	-0.31 (0.39)	--
TANF, Medicaid	0.27 (0.96)	0.12 (0.29)	--	0.15 (1.00)	--
Only TANF	0.77 (0.69)	0.16 (0.22)	--	0.61 (0.72)	--
Other	0.07 (0.30)	0.14 (0.14)	--	-0.07 (0.33)	--
Peru, Columbia and Ecuador					
All Three	-0.52 (0.39)	-0.09 (0.18)	0.14 (0.69)	-0.43 (0.43)	-0.66 (0.79)
TANF, Medicaid	0.09 (0.54)	0.58* (0.29)	-0.82 (0.88)	-0.49 (0.61)	0.91 (1.03)
Only TANF	-0.03 (0.62)	-0.31 (0.30)	0.46 (1.23)	0.28 (0.69)	-0.49 (1.38)
Other	0.15 (0.37)	-0.06 (0.18)	-0.83 (0.55)	0.21 (0.41)	0.98 (0.66)
Dominican Republic					
All Three	0.25 (0.68)	-0.21 (0.35)	--	0.46 (0.76)	--
Only Two	-0.06 (0.64)	-0.17 (0.35)	--	0.11 (0.73)	--
Only One	-1.85 (1.25)	-0.28 (0.46)	--	-1.57 (1.33)	--
Rest of the Caribbean²					
All Three	0.56 (0.35)	0.20 (0.28)	1.04* (0.44)	0.36 (0.45)	-0.48 (0.56)
TANF, Medicaid	-0.70 (0.54)	0.42 (0.48)	0.60 (0.55)	-1.12 (0.72)	-1.30 (0.77)
Only TANF	1.07 (0.64)	0.44 (0.49)	1.63* (0.77)	0.63 (0.81)	-0.56 (1.00)
Other	-0.26 (0.42)	0.07 (0.36)	-0.26 (0.65)	-0.33 (0.55)	0.00 (0.77)

Note:¹Excludes immigrants from Mexico. ²Excludes immigrants from the Dominican Republic, Haiti and Cuba. Coefficients for each country/region in columns I to III represent a separate regression. Each regression controls for state fixed effects, state unemployment rates with one and two years lags, state population last year, number of recent foreign born people living in the state last year and number of recent immigrants from that region living in the state last year. Columns I and II control for last year's real median wage of women with 12 or fewer years of education. Column III controls for last year's median wage of women with at least some college. * .01<p<.05, ** p<.01

Table 8
Conditional Logit Estimates of the Impact of the Intensity of Public Policies on the Location Choices of
Low-skilled Unmarried, Low-skilled Married and Skilled Unmarried Women Immigrants

	Unmarried Low-skilled Women	Married Low- skilled Women	Unmarried Skilled Women	Difference-in-differences Estimate	
	I	II	III	I-II	I-III
China					
All Three	-0.52 (0.42)	-0.00 (0.15)	0.38 (0.46)	-0.52 (0.46)	-0.90 (0.62)
TANF, Medicaid	0.33 (0.51)	0.12 (0.20)	0.88 (0.53)	0.21 (0.55)	-0.55 (0.73)
Only TANF	-0.23 (0.70)	-0.14 (0.24)	1.12 (0.60)	-0.09 (0.74)	-1.35 (0.92)
Other	0.71 (0.48)	0.22 (0.16)	0.15 (0.47)	0.49 (0.51)	0.56 (0.67)
Bangladesh, India and Pakistan					
All Three	0.11 (0.30)	0.00 (0.07)	0.25 (0.45)	0.11 (0.31)	-0.14 (0.59)
TANF, Medicaid	-0.07 (0.42)	0.24* (0.11)	-0.02 (0.81)	-0.31 (0.43)	-0.05 (0.91)
Only TANF	-0.07 (0.56)	-0.05 (0.12)	1.32 (1.09)	-0.02 (0.57)	-1.39 (1.22)
Other	0.57 (0.28)	0.10 (0.08)	-0.94 (0.68)	0.47 (0.29)	1.51* (0.73)
Vietnam					
All Three	0.07 (0.44)	-0.01 (0.19)	0.17 (0.41)	0.08 (0.48)	-0.10 (0.60)
TANF, Medicaid	-0.27 (0.53)	-0.08 (0.23)	-0.06 (0.54)	-0.19 (0.58)	-0.21 (0.76)
Only TANF	0.02 (0.56)	-0.10 (0.24)	1.02* (0.47)	0.12 (0.61)	-1.00 (0.73)
Other	0.11 (0.43)	0.04 (0.18)	0.51 (0.39)	0.07 (0.47)	-0.40 (0.58)
Philippines					
All Three	0.34 (0.24)	-0.26* (0.12)	-0.56 (0.35)	0.6* (0.27)	0.9* (0.42)
TANF, Medicaid	0.94** (0.32)	-0.20 (0.12)	-0.07 (0.46)	1.14** (0.34)	1.01 (0.56)
Only TANF	-0.16 (0.44)	-0.21 (0.15)	-1.13 (0.60)	0.05 (0.46)	0.97 (0.74)
Other	0.32 (0.21)	0.02 (0.12)	-0.28 (0.31)	0.30 (0.24)	0.60 (0.37)
Poland, Romania, Russia and Ukraine					
All Three	-0.02 (0.48)	-0.06 (0.18)	0.17 (0.31)	0.04 (0.51)	-0.19 (0.57)
TANF, Medicaid	1.14 (0.71)	-0.16 (0.26)	-0.54 (0.49)	1.30 (0.76)	1.68 (0.86)
Only TANF	-0.70 (1.08)	0.15 (0.25)	-0.22 (0.55)	-0.85 (1.11)	-0.48 (1.21)
Other	-0.30 (0.50)	-0.11 (0.18)	-0.33 (0.43)	-0.19 (0.53)	0.03 (0.66)

Note: Coefficients for each country/region in columns I to III represent a separate regression. Each regression controls for state fixed effects, state unemployment rates with one and two years lags, state population last year, number of recent foreign born people living in the state last year and number of recent immigrants from that region living in the state last year. Columns I and II control for last year's real median wage of women with 12 or fewer years of education. Column III controls for last year's median wage of women with at least some college. *.01<p<.05, ** p<.01

Table 9
Conditional Logit Estimates of the Effects of Generosity of Means-tested Programs on the Location Choices of Low-skilled Unmarried, Low-skilled Married and Skilled Unmarried Women Immigrants

	Unmarried Low-skilled Women	Married Low-Skilled Women	Unmarried Skilled Women	Difference-in-differences Estimate	
	I	II	III	I-II	I-III
Mexico					
TANF	-0.0013 (0.0008)	-0.0002 (0.0004)	0.0009 (0.0030)	-0.0011 (0.0009)	-0.0022 (0.0031)
Medicaid	0.0000 (0.0000)	0.0000 (0.0000)	-0.0001 (0.0003)	0.0000 (0.0000)	0.0001 (0.0003)
Food Stamps	0.0005 (0.0010)	0.0004 (0.0005)	0.0002 (0.0039)	0.0001 (0.0011)	0.0003 (0.0040)
Rest of Central America¹					
TANF	0.0025 (0.0022)	-0.0003 (0.0007)	-0.0040 (0.0026)	0.0028 (0.0023)	0.0065 (0.0034)
Medicaid	0.0001 (0.0001)	0.0000 (0.0000)	-0.0002 (0.0001)	0.0001 (0.0001)	0.0003** (0.0001)
Food Stamps	-0.0046 (0.0024)	0.0004 (0.0008)	0.0045 (0.0031)	-0.0050* (0.0025)	-0.0091** (0.0039)
Peru, Columbia and Ecuador					
TANF	-0.0010 (0.0015)	0.0007 (0.0008)	-0.0001 (0.0024)	-0.0017 (0.0017)	-0.0009 (0.0028)
Medicaid	0.0001 (0.0001)	0.0001 (0.0001)	-0.0004 (0.0001)	0.0000 (0.0001)	0.0004* (0.0002)
Food Stamps	-0.0025 (0.0020)	-0.0014 (0.0010)	0.0044 (0.0027)	-0.0011 (0.0022)	-0.0069* (0.0034)
Dominican Republic					
TANF	-0.0048 (0.0033)	0.0002 (0.0018)	-0.0070 (0.0101)	-0.0050 (0.0038)	0.0022 (0.0106)
Medicaid	0.0003 (0.0003)	-0.0001 (0.0001)	-0.0003 (0.0007)	0.0004 (0.0003)	0.0006 (0.0008)
Food Stamps	0.0039 (0.0029)	-0.0001 (0.0014)	0.0007 (0.0136)	0.0040 (0.0032)	0.0032 (0.0139)
Rest of the Caribbean²					
TANF	0.0002 (0.0018)	0.0012 (0.0014)	0.0054** (0.0024)	-0.0010 (0.0023)	-0.0052 (0.0030)
Medicaid	-0.0001 (0.0001)	0.0001 (0.0001)	-0.0003 (0.0001)	-0.0002* (0.0001)	0.0002* (0.0001)
Food Stamps	0.0032* (0.0016)	-0.0013 (0.0014)	0.0019 (0.0018)	0.0045* (0.0021)	0.0013 (0.0025)

Note: ¹ Excludes immigrants from Mexico. ² Excludes immigrants from the Dominican Republic, Haiti and Cuba. Coefficients for each country/region in columns I to III represent a separate regression. Each regression controls for state fixed effects, state unemployment rates with one and two years lags, state population last year, number of recent foreign born people living in the state last year and number of recent immigrants from that region living in the state last year. Columns I and II control for last year's real median wage of women with 12 or fewer years of education. Column III controls for last year's median wage of women with at least some college. * .01<p<=.05, ** p<=.01

Table 10
Conditional Logit Estimates of the Effects of Generosity of Means-tested Programs on the Location Choices of Low-skilled Unmarried, Low-skilled Married and Skilled Unmarried Women Immigrants

	Unmarried Low-skilled Women	Married Low- skilled Women	Unmarried Skilled Women	Difference-in-differences Estimate	
	I	II	III	I-II	I-III
China					
TANF	-0.0019 (0.0019)	-0.0003 (0.0007)	0.0044** (0.0017)	-0.0016 (0.0020)	-0.0063* (0.0025)
Medicaid	0.0001 (0.0001)	0.0001* (0.000)	0.0001 (0.0001)	0.0000 (0.0001)	0.0000 (0.0001)
Food Stamps	-0.0025 (0.0020)	-0.0009 (0.0008)	-0.0043* (0.0020)	-0.0016 (0.0021)	0.0018 (0.0028)
Bangladesh, India and Pakistan					
TANF	-0.0038** (0.0013)	0.0001 (0.0003)	0.0036 (0.0026)	-0.0039** (0.0013)	-0.0074** (0.0029)
Medicaid	0.0001 (0.0001)	0.0000 (0.0000)	-0.0002 (0.0002)	0.0001 (0.0001)	0.0003 (0.0002)
Food Stamps	0.0027 (0.0016)	-0.0002 (0.0004)	-0.0007 (0.0027)	0.0029 (0.0016)	0.0034 (0.0031)
Vietnam					
TANF	0.0004 (0.0017)	-0.0003 (0.0007)	0.0002 (0.0016)	0.0007 (0.0018)	0.0002 (0.0023)
Medicaid	0.0000 (0.0001)	-0.0000 (0.0000)	0.0000 (0.0001)	0.0000 (0.001)	0.0000 (0.0001)
Food Stamps	-0.0007 (0.0020)	0.0007 (0.0009)	-0.0005 (0.0018)	-0.0014 (0.0022)	-0.0002 (0.0027)
Philippines					
TANF	0.0013 (0.0009)	-0.0009* (0.0004)	-0.0013 (0.0013)	0.0022* (0.0010)	0.0026 (0.0016)
Medicaid	0.0000 (0.0001)	-0.0000 (0.0000)	-0.0001 (0.0001)	0.0000 (0.0001)	0.0001 (0.0001)
Food Stamps	-0.0018 (0.0013)	0.0002 (0.0006)	0.0003 (0.0018)	-0.0020 (0.0014)	-0.0021 (0.0022)
Poland, Romania, Russia and Ukraine					
TANF	0.0026 (0.0020)	0.0004 (0.0007)	0.0002 (0.0013)	0.0022 (0.0021)	0.0024 (0.0024)
Medicaid	-0.0001 (0.0001)	0.0000 (0.0000)	-0.0000 (0.0001)	-0.0001 (0.0001)	-0.0001 (0.0001)
Food Stamps	-0.0008 (0.0025)	-0.0010 (0.0009)	0.0010 (0.0019)	0.0002 (0.0027)	-0.0018 (0.0031)

Note: Coefficients for each country/region in columns I to III represent a separate regression. Each regression controls for state fixed effects, state unemployment rates with one and two years lags, state population last year, number of recent foreign born people living in the state last year and number of recent immigrants from that region living in the state last year. Columns I and II control for last year's real median wage of women with 12 or fewer years of education. Column III controls for last year's median wage of women with at least some college. * .01<p<.05, ** p<.01

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