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IMMIGRANT AND NATIVE RESPONSES TO WELFARE REFORM

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### ABSTRACT

In this paper, we investigate the effect of federal welfare reform on the employment, hours of work and marriage rates of three groups of low-educated women: foreign-born citizens, foreign-born non-citizens and native-born citizens. Among non-citizens, we 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, we compare the response of foreign-born non-citizens 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 non-citizens, a larger effect on more recent arrivals. The “chilling” hypothesis that has received so much attention in the popular press is not supported by our results. Finally, our estimates indicate that TANF had no effect on native- and foreign-born citizens’ marriage decisions. TANF was associated with a decrease in the marriage rates of foreign-born non-citizens.

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## **Introduction**

It was rumored that on the day President Clinton signed the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, the Statue of Liberty looked slightly greener than usual. Someone even said that Lady Liberty threw up. While the hyperbole associated with this story is obvious, so is the moral; the immigrant provisions of PRWORA represented a backlash against immigrants that is at odds with the fabled inscription of the Statue of Liberty and the perception of the United States as a place hospitable to immigrants. Federal welfare reform legislation barred future legal immigrants (those arriving after 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, and 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 our nation does not share a common vision related to the treatment of immigrants.

Why were immigrants singled out for special treatment? One reason was that the number of immigrants to the country had been growing rapidly prior to 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 that became manifest in the federal welfare reform law.

A second reason to treat immigrants 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).<sup>1</sup> 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, 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 program was seen as a way to save money and increase the fiscal benefits of immigration and to reduce “adverse” immigration, that is 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, we provide such evidence by investigating the effect of PRWORA on the employment, hours of work and marriage rates of three groups of low-educated women: foreign-born citizens, foreign-born 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-

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<sup>1</sup> 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).

marital births). Therefore, our 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. Our 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, we investigate whether the behavioral response to welfare reform differed by recency of immigration. And our analysis of the effect of welfare reform on foreign-born non-citizens, a group which consists partly of newly barred legal immigrants, will provide evidence of the potential cost 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, we 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**

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. We 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.

A unique aspect of PRWORA is the distinction it makes among foreign-born non-citizens. Specifically, it creates two eligibility classes among foreign-born non-citizens who entered the country legally. 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.

In our empirical analysis, we examine the effect of welfare reform on foreign-born non-citizens. This group consists of documented and undocumented immigrants, although the data do not allow us to distinguish between the two. Undocumented immigrants were never eligible for cash assistance and welfare reform had no effect on their behavior. Thus, the behavioral response that we are attempting to measure in this analysis is that associated with legal immigrants. The data allow us to identify new immigrants (i.e., post 1996) and therefore we can test whether new immigrants had a larger behavioral response consistent with the more draconian policy change that they faced. Finally, since some states maintained eligibility of newly arrived immigrants, we 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 our knowledge, there has been only one previous study of the effect of federal welfare reform on immigrants’ use of cash assistance. Fix and Passell (1999) compare 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.

The major weakness with this study is its simple before and after design. Changes in welfare participation rates may be driven by a variety of factors, for example macroeconomic changes, and this study did 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 the study is that the data extend only through 1997 and federal welfare reform had not been widely implemented by that time.

In contrast to this study, our analysis controls for several factors that may affect decisions about work and welfare participation. Most importantly, we include explicit and implicit controls for macroeconomic changes in the economy. We also use data that extend through 1999 by which time federal welfare reform had been completed. Finally, we 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 Strategy

To estimate the effect of welfare reform on employment, hours of work and marriage, we 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 1**  
**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 1, 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 1 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  $\text{Emp}_{ikt}$  is an indicator of whether woman ‘i’ in group ‘k’ in year ‘t’ is employed. The variable  $\text{Treat}_k$  in equation (1) denotes membership in our target group.  $\text{Reform}_t$  is an indicator equal to one if it is an observation taken after welfare reform. The key parameter in equation (1) is  $\alpha_3$ , 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 1. This simple specification of the regression model generates no advantage over the differences in means in Table 1. 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, we estimate a model similar to equation (2) below.

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

Equation (2) includes controls for state effects ( $\beta_j$ ), which we allow to differ by target and comparison group, year effects ( $\delta_t$ ), 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. 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  $\alpha_4$  and  $\alpha_5$

measure the effect of AFDC waivers ( $\alpha_4$ ) and TANF ( $\alpha_5$ ) 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.<sup>2</sup> Clearly, this statement is correct only if we 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, our regression model includes a control for macroeconomic activity, specifically the state unemployment rate in the month of the survey. We also interact the unemployment rates with the dummy variable indicating membership in the target group.

As noted, a crucial aspect of the DD analysis is the validity of the comparison group. For the employment analysis, we 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.<sup>3</sup> The inclusion of women at risk of welfare receipt in the comparison group will bias our estimates toward zero with the size of the bias depending on the difference in the proportion of at-risk women in the target and comparison groups.

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<sup>2</sup> 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.

<sup>3</sup> CPS data from 1994 show that approximately 20 percent of the welfare caseload is married.

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.

In order to gauge the sensitivity of estimates to our choice of comparison group, we estimate equation (2) using an alternative comparison group. Our second comparison group is unmarried women with 13 to 15 years of education.<sup>4</sup> The problem with using this group is that a significant proportion of it, for example those with children, are at risk of welfare receipt. Thus, the DD estimates using this group are likely to be severely downward biased, particularly when the target group is unmarried women with a high school degree. It is also the case that marital status is a more important determinant of the level of employment than education (see Table 1 and discussion below).

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 our comparison groups, we 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 pertains to all women with less than a high school degree. It is clear that the level and trend in employment of unmarried and married 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 unmarried women with 13 to 15 years of education is similar to that of the other two groups, but there is a perceptible increase in employment for this group in 1997 that suggests that this group may have also been affected by TANF. However, the level of employment is much higher for the more educated unmarried women. Overall, Figure 1 provides some justification for our choice of target and comparison groups and evidence consistent with our 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. By 1994, however, and in the period between 1994 and 1997, 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, we believe that the married women comparison group is superior, primarily because it is the group most unaffected by welfare reform and because during the pre period of our analysis, roughly between 1994 and 1997, the level and trend in employment is the same for the two groups. But as we noted above, we present estimates using the alternative group for the reader's benefit since the superiority of our preferred comparison cannot be established with certainty.

A difference-in-differences approach is also used to examine the effect of welfare reform on marital status. For this analysis, we 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, we cannot stratify on the basis of this variable. Therefore, we are 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.

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<sup>4</sup> We do not include unmarried women with 16 (BA) 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 with 12 or fewer years of education.

## 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 we do not examine the effect of welfare reform on these outcomes. This ORG does, however, have information on other individual characteristics such as age, education level, ethnicity, race, and place of residence. We can use these in the regression analysis to increase the precision of the DD estimates. We limit the time period to the years 1994 to 1999 because information about 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 citizen status. This stratification is essential to the goals of this study.

We focus on three samples of women: native-born citizens, foreign-born citizens and foreign-born non-citizens. In all cases, we limit the sample to women between the ages of 18 and 44 years of age because very few women over age 44 are at risk of welfare receipt. We exclude younger women because we use education to stratify the sample and there is little variation in education among women below age 18. We 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 our target groups. For native-born citizens, we 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. We 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. We 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 our 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

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

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

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

In equation (4),  $\sigma^2$  is the variance of the binomial outcome of employment, which we 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

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

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 our 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 three groups: native-born citizens, foreign-born citizens and foreign-born non-citizens. The top panel of Table 2 presents estimates obtained using married women with similar education levels as the comparison group. The bottom panel presents estimates obtained using unmarried women with more education as the comparison group. Each row (within column) of Table 1 presents estimates from a separate regression. So row 1 presents DD estimates of the effect of AFDC waivers and TANF on unmarried women with 12 or fewer years of education, and row 2 presents similar estimates for unmarried women with 12 years of education (i.e., high school degree). Note that we do not obtain estimates of the effect of AFDC waivers on the employment of foreign-born women. The reason for this is the lack of within-state variation in state-level welfare policy (i.e., AFDC waivers) in the states where 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 our study.<sup>5</sup> We also do not present separate estimates by education level for foreign-born citizens because of small sample sizes. All estimates were 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 2.6 percentage points, which represents a relative effect of about four 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 1.4 percentage points or two percent. 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 (2000) and the Council of Economic Advisers (1999). Finally, estimates in the bottom panel of Table 2 are qualitatively consistent with those in the top, but smaller in magnitude. This result is exactly what one would expect since a larger fraction of the comparison group used to obtain these estimates is affected by welfare reform than was the case in the top panel.

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 2.6 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, 2.6 percent of all low-educated unmarried native-born women represents approximately 325,000 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

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<sup>5</sup> We do include an AFDC waiver dummy variable in these regression models.



the Department of Health and Human Services ([www.acf.dhhs.gov/news/tables.html](http://www.acf.dhhs.gov/news/tables.html)), between August 1996 and June 2000, the number of families on welfare declined by 2,207,000. If we 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 unmarried women with 12 or fewer years of education who were receiving public assistance declined by 1,324,200. Thus, we estimate that approximately 25 percent of the decline in the caseload during this period among this group was due to increased employment related to TANF. Obviously this is a rough estimate of the TANF-induced employment related decline in welfare caseloads, but it makes the point that the relatively small estimates in Table 2 can account for a significant portion of the change in the welfare caseload.

Estimates in the top panel of Table 2 indicate that TANF had no statistically significant effect on the employment of foreign-born women. Among non-citizens, 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 bottom panel of Table 2 pertaining to foreign-born women are larger than estimates in the top panel. We 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, we are somewhat skeptical of the estimates in the bottom panel of Table 2, but we can't rule out the possibility that TANF may have had a significant effect on the employment of foreign-born non-citizens. We believe the estimates in the top panel, however, are more credible and prefer to emphasize these estimates in our discussion. Because there is no definitive way to resolve this issue, we present both sets of estimates throughout the paper, but we discuss only those obtained using married women with similar education as the comparison group.

It is perhaps inappropriate to compare estimates across the three 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 non-citizen 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 order to obtain some idea of the relative quality of the target and comparison groups in each sample, we 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 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 we 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 bottom 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 our skepticism related to the significant estimates of the effect of TANF on foreign-born non-citizens in the bottom panel of Table 2. Assuming that the “quality” of the experiment is roughly the same for native- and foreign-born women, our preferred estimates 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 bottom 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 we allowed the effect of welfare reform among foreign-born non-citizens to differ by the recency of immigration. A similar analysis could not be performed for foreign-born citizens because of small sample sizes. Table 3 presents the estimates from

this analysis. Estimates in Table 3 indicate that the effect of TANF was larger for more recent immigrants as compared to immigrants who arrived earlier. According to our preferred set of estimates, TANF increased the employment of foreign-born non-citizens who immigrated in the past five years by 4.1 percentage points. 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 our estimates are not confounded by the effect of unmeasured characteristics associated with recency of immigration. However, the immigrant provisions of TANF may have altered the composition of new immigrants and the estimates in Table 3 related to the newest immigrants may be a combination of a behavioral response and a selection effect. Both effects are due to TANF, but it is a factor to consider when comparing behavioral responses across groups in Table 3.<sup>6</sup> As discussed above, differences in the behavioral response to welfare reform stem from different underlying causes of program participation. Estimates in Table 3, if we assume that they represent different behavioral responses, suggest that recency of immigration signals something about the reasons why low-educated immigrant women require cash assistance. Identifying those 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 bottom panel of Table 3 indicate that TANF was associated with an increase in the employment for all groups of foreign-born non-citizens with relatively little difference by recency of immigration.

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, we exploit the

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<sup>6</sup> This point reinforces the earlier one that it is difficult to know whether the larger estimated effects are due to a larger behavioral response or because of differences in the quality of the target and comparison groups.

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 we 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, we are comparing the employment of recently arrived immigrants after TANF to the employment of recently arrived immigrants before TANF. We allow that effect to differ by whether new immigrants were eligible for TANF.

Table 4 presents the estimates of this analysis and they are striking. In states that made TANF 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. We tested whether the effect of TANF for earlier arriving immigrants differed between the two types of states and we 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.

The estimates in Table 4 are inconsistent with the “chilling” hypothesis, which would suggest more equal sized effects. The “chilling” hypothesis suggests that the anti-immigrant sentiments associated with the 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. Differences in actual eligibility should not matter that much, but the estimates in Table 4 strongly suggest that eligibility does matter.

## **Results - Effects of Welfare Reform on Hours of Work**

In addition to employment, we examined the effect of welfare reform on usual hours of work in the week prior to the survey for our three groups of women.<sup>7</sup> This analysis allows us 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 non-citizen 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 worked per week by approximately one hour per week for native born women and between one half and two hours per week for foreign-born non-citizen 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. We can calculate the average hours of work per week among those who were induced to work by TANF as follows. First, we 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 due solely to the change in hours worked among women induced to work by TANF. Thus, we 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 0.026 and estimates in Table 6 indicate that TANF increased the average hours worked per week for this group by 0.811 hours. Thus, we can divide the 0.811 by 0.026 to obtain an estimate the average hours worked per

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<sup>7</sup> It makes little difference whether we use usual hours worked per week or actual hours worked per week as the dependent variable.

week of native-born women induced to work by TANF. This calculation turns out to be 31.2 hours. Similar calculations for foreign-born non-citizen women yield an estimate of 64.9 hours per week, which is implausibly large. If we use estimates from the bottom panels of Tables 2 and 6 for foreign-born non-citizens, a more reasonable estimate of 28.6 is obtained. In any case, these figures indicate that new “welfare-reform” entrants to the labor market worked a significant number of hours and about the same number of hours as the existing members of the labor force with similar characteristics.

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 we 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 \$200 (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 \$8,008 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 \$11,211 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).

### **Results - Effects of Welfare Reform on Marital Status in U.S.**

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 choices are one of the most important of these behaviors. In this section we 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. We 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 few statistically significant estimates in Table 8. TANF appears to have no statistically significant effect on the marriage rates of native- and foreign-born citizens. One positive implication of these findings is that they support our use of marital status to define target and comparison groups. If we had found a significant effect of TANF on marriage, then part of the effects of welfare reform that we found for employment outcomes may have been due to compositional changes because we stratified the sample by marital status. Estimates of the effect of TANF on marriage rates of foreign-born non-citizens, however, are negative and significant. These estimates are unexpected since the incentives in TANF the legislation were intended to increase marriage rates in the post-TANF period.

## **Conclusions**

In this paper, we have investigated the effect of welfare reform on employment, hours of work and marital status of three groups of low-educated unmarried women: native born citizens, foreign-born citizens and foreign-born non-citizens. The results of this analysis suggest that welfare reform, particularly TANF, 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 a larger effect on more recent arrivals among foreign-born non-citizens.

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. Our 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 the alternative comparison group of more educated unmarried women suggest the opposite. What wasn't ambiguous was the result that recent immigrants, those arriving in the last five years, were the most affected by TANF. This may reflect a larger behavioral response, but also may be due to a compositional change in the "new" immigrant pool. Understanding the reasons for different behavioral responses is beyond the scope of this paper, but provides a stylized fact that future research should investigate.

The "chilling" hypothesis that has received so much attention in the popular press and professional literature is not supported by our results. We 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. Our results strongly suggest otherwise.

Finally, our estimates indicate that TANF and AFDC waivers had no effect on native- and foreign-born citizens' marriage decisions. TANF was associated with a decrease in the marriage rates of foreign-born non-citizens. This latter result is counterintuitive and not readily explained.



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**Table 1**  
**Description of the Employment Sample: Women Age 18-44, CPS 1994-1999**

Nativity/	US Born		Foreign Born US Citizen		Foreign Born Non-Citizen	
	No. of Observations	Average Employment	No. of Observations	Average Employment	No. of Observations	Average Employment
<b>A. Comparison Groups:</b>						
Married Women with Education High School or less	22498	0.678	3615	0.629	13696	0.434
Married Women without High School	3517	0.505	1183	0.559	8011	0.386
Married Women with a High School Degree	18981	0.710	2432	0.663	5685	0.502
Unmarried Women with 13-15 Years of Schooling	17130	0.753	1717	0.709	3434	0.600
<b>B. Target Group</b>						
Unmarried Women with Education High School or less	21464	0.626	1721	0.644	8420	0.529
Unmarried Women without High School	6364	0.437	705	0.540	4840	0.472
Unmarried Women with a High School Degree	15100	0.705	1016	0.715	3580	0.606

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

Nativity/ Target Group	US Born		Waiver	Foreign Born US Citizen		Foreign Born Non-Citizen	
	TANF			TANF		TANF	
Unmarried Women <sup>1</sup>							
With high school degree or less	0.026* (0.014)		-0.023 (0.016)		0.008 (0.041)		0.016 (0.020)
Without high school	0.058* (0.033)		-0.037 (0.039)		---		0.017 (0.026)
With high school	0.014 (0.015)		-0.026 (0.018)		---		0.025 (0.032)
Unmarried Women <sup>2</sup>							
With high school degree or less	0.013 (0.011)		0.008 (0.013)		0.030 (0.046)		0.075*** (0.029)
Without high school	0.038** (0.017)		0.023 (0.019)		---		0.086*** (0.032)
With high school	0.002 (0.012)		0.002 (0.012)		---		0.067* (0.034)

Note: <sup>1</sup> Married women with the same level of education as the comparison group.  
<sup>2</sup> Unmarried women with 13 to 15 years of schooling 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 and AFDC Waivers on the**  
**Employment of Unmarried Foreign Born Non-citizens, Age 18-44, Depending on their Arrival in the US, 1994 -1999.**

Target Group	Immigrated less than Five years ago	Immigrated between 5-10 years ago	Immigrated between more than 10 years ago
	TANF	TANF	TANF
Unmarried Women <sup>1</sup>			
With high school degree or less	0.041* (0.024)	0.020 (0.025)	-0.005 (0.023)
Without high school	0.072** (0.033)	0.006 (0.032)	-0.011 (0.030)
With high school	0.023 (0.037)	0.049 (0.039)	0.011 (0.036)
Unmarried Women <sup>2</sup>			
With high school degree or less	0.084*** (0.032)	0.073** (0.033)	0.071** (0.031)
Without high school	0.146*** (0.038)	0.061 (0.038)	0.061* (0.036)
With high school	0.052 (0.040)	0.097** (0.042)	0.062 (0.038)

Note: <sup>1</sup> Married women with the same level of education as the comparison group.

<sup>2</sup> Unmarried women with 13 to 15 years of schooling 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 Non-citizens, Age 18-44, Depending on their Arrival in the US, 1994 -1999.**

Target Group	Immigrated after 1995		Immigrated Before 1996
	TANF –New Immigrants Eligible	TANF –New Immigrants Ineligible	
Unmarried Women <sup>1</sup>			
With high school degree or less	-0.011 (0.039)	0.051* (0.031)	0.013 (0.021)
Without high school	0.007 (0.053)	0.097** (0.041)	0.006 (0.027)
With high school	-0.021 (0.059)	0.010 (0.047)	0.032 (0.032)
Unmarried Women <sup>2</sup>			
With high school degree or less	0.031 (0.044)	0.101*** (0.037)	0.075** (0.029)
Without high school	0.084 (0.056)	0.171*** (0.045)	0.072** (0.033)
With high school	0.002 (0.060)	0.046 (0.049)	0.076** (0.035)

Note: <sup>1</sup> Married women with the same level of education as the comparison group.  
<sup>2</sup> Unmarried women with 13 to 15 years of schooling 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 Age 18-44, CPS 1994-1999**

Nativity/	US Born		Foreign Born US Citizen		Foreign Born Non-Citizen	
	No. of Observations	Average Hours Worked	No. of Observations	Average Hours Worked	No. of Observations	Average Hours Worked
<b>A. Comparison Groups:</b>						
Married Women with Education High School or less	22498	22.586	3615	22.591	13696	14.866
Married Women without High School	3517	16.194	1183	19.798	8011	13.210
Married Women with a High School Degree	18981	23.770	2432	23.949	5685	17.198
Unmarried Women with 13-15 Years of Schooling	17130	24.372	1717	23.392	3434	18.704
<b>B. Target Group</b>						
Unmarried Women with Education High School or less	21464	20.656	1721	22.139	8420	17.836
Unmarried Women without High School	6364	12.435	705	18.315	4840	15.560
Unmarried Women with a High School Degree	15100	24.121	1016	24.793	3580	20.913

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

Nativity/ Target Group	US Born		Foreign Born US Citizen		Foreign Born Non-Citizen	
	TANF	Waiver	TANF	TANF	TANF	TANF
Unmarried Women <sup>1</sup>						
With high school degree or less	0.811 (0.555)	-0.823 (0.651)	-0.637 (1.710)		1.039 (0.795)	
Without high school	1.146 (1.205)	-1.135 (1.443)	---		0.495 (1.026)	
With high school	0.621 (0.644)	-0.887 (0.746)	---		2.122 (1.267)	
Unmarried Women <sup>2</sup>						
With high school degree or less	0.434 (0.452)	0.505 (0.504)	-0.146 (1.867)		2.146* (1.102)	
Without high school	1.231** (0.617)	1.020 (0.700)	---		1.781 (1.228)	
With high school	0.086 (0.501)	0.276 (0.559)	---		2.688** (1.339)	

Note: <sup>1</sup> Married women with the same level of education as the comparison group.

<sup>2</sup> Unmarried women with 13 to 15 years of schooling 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 Age 18-44, CPS 1994-1999**

Nativity/	US Born		Foreign Born US Citizen		Foreign Born Non-Citizen	
	No. of Observations	Probability of Being Married	No. of Observations	Probability of Being Married	No. of Observations	Probability of Being Married
A. Comparison Group: Women with 13-15 years of Schooling	34540	0.504	3957	0.566	6921	0.504
B. Target Group: Women with 12 or Fewer Years of Schooling	43962	0.512	5336	0.677	22116	0.619

**Table 8**  
**Difference in Differences Estimates of the Impact of TANF and AFDC Waivers on the**  
**Probability of Being Married: Women Age 18-44, CPS 1994-1999<sup>1</sup>**

Nativity/ Target Group	US Born		Foreign Born US Citizen	Foreign Born Non-Citizen
	TANF	Waivers		
Women With High School Degree or Less	-0.015 (0.010)	-0.010 (0.012)	0.043 (0.028)	-0.043 (0.019)
Without high school	-0.008 (0.016)	0.022 (0.019)	---	-0.033 (0.020)
With high school	-0.016 (0.011)	-0.019 (0.012)	---	-0.053** (0.021)

Note: <sup>1</sup> Women with 13 to 15 years of schooling as the comparison group.

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