Welfare Bans, Non-Citizen Wages, and Search: Evidence from PRWORA

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May 5th, 2021

Abstract

Search theory generally predicts that a reduction in welfare payments to the unemployed decreases wages. After building a search model that captures this relationship, I empirically test it by exploiting the variation in welfare eligibility rules caused by the 1996 U.S. welfare reform, also known as PRWORA, to estimate the effect of welfare bans on non-citizen wages. Using a triple-differences approach with a rich set of demographic and socioeconomic controls, I find no evidence that PRWORA decreased the wages of non-citizens. These results suggest that my search model cannot explain the effect of PRWORA on non-citizen wages.

*I thank Professor Cumby and Professor Ludema for their helpful guidance and support. I also thank Elizabeth Ash for her useful feedback.

1 Introduction

Unemployment benefits are meant to help job searchers by giving them time to obtain a job that suits their skills and preferences. Due to political reasons, however, not everyone is guaranteed access to these benefits. In 1996, a Congressional law known as the Personal Responsibility and Work Opportunity Act (PRWORA) caused many non-citizens to lose eligibility for welfare programs, many of which provided sources of income to the unemployed. This paper asks the following question: What is the effect of this welfare ban on non-citizen wages?

This paper has two motivations. The first motivation is that welfare is a heavily debated issue in Congress and within the government more generally, so knowing how welfare bans affect workers can help policymakers make more informed decisions. The second motivation is to test the prediction from search theory that a reduction in welfare payments to the unemployed decreases wages. The 1996 U.S. welfare reform law provides me an opportunity to see if this well-established theoretical relationship between welfare bans and wages holds in my empirical work.

Beginning with Borjas's seminal paper studying the effects of 1996 U.S. welfare reform on the health insurance coverage rate and labor supply of the immigrant population (Borjas 2003), many economists have studied this policy. To my knowledge, however, there are no papers other than mine at this time that explore its effects on non-citizen wages. While a large literature explores how large structural forces such as skill-biased technological change affect the U.S. wage structure (Autor et al. 2008), these papers overlook how the enactment of specific policies may drive differences in wages. As such, my paper presents a unique contribution to the intersection of the literature on immigration, wage inequality, and search.

This paper estimates the effect of PRWORA on the wages of the non-citizen population. I analyze microdata from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS) using the IPUMS website (Flood et al. 2021). My data provides detailed information on wages and an extensive collection of demographic and socioeconomic variables. I begin my empirical analysis by using a differences-in-differences (DID) estimator to capture the effect of PRWORA on the differences in wages between citizens and non-citizens. However, one potential concern with using DID is that it fails to account for the heterogeneity in the state-level responses to the welfare reform. When PRWORA went into effect, states had the choice to continue providing welfare programs for non-citizens or to discontinue them. The relative generosity of state-level welfare programs for non-citizens varied substantially even among states that continued providing welfare for non-citizens. I improve upon the differences-in-differences estimator by adopting a triple-differences (DIDID) model that appropriately takes into account the differences in welfare eligibility rules across states. The triple-differences estimator better captures the two groups at comparison, non-citizens who were affected by the 1996 U.S. welfare reform and everyone else in the sample who was not. With this methodological improvement, I more cleanly estimate the effect of PRWORA on the wages of non-citizens who lived in states that banned welfare for them.

To structure my empirical findings, I build a simple model of a labor market with search frictions. I consider a search equilibrium that includes workers and firms. Using a Nash bargaining approach, I pin down the determined wage and find that a reduction in welfare payments to the unemployed decreases wages. I extend my simple search model by allowing for heterogeneity in the citizen and non-citizen submarkets for labor. This extension captures the same underlying intuition in predicting that a welfare ban on non-citizens would cause their wages to decrease.

To test my theoretical predictions, I present estimates of the effect of PRWORA on the wages of non-citizens. Using the differences-in-differences approach with a variety of controls, I find no evidence that PRWORA decreased the wages of non-citizens. My triple-differences model similarly indicates that PRWORA did not decrease the wages of non-citizens in states that restricted benefits. When I test for heterogeneity in the effect of PRWORA by splitting the sample by gender, poverty status, and employment status in the last year, I continue to find no evidence that PRWORA decreased non-citizen wages. To support the validity of my results, I run a variety of regressions to test if my regressions are sensitive to alternative specifications and find that they are generally robust.

Since employers compensate employees through a variety of channels other than wages, I extend my analysis to better understand how PRWORA affected non-citizen compensation. In particular, I investigate whether or not PRWORA caused higher participation rates in employer-sponsored health insurance. Using a linear probability model and its probit equivalent, I find evidence that PRWORA increased the employer-sponsored health insurance coverage rate for non-citizens. Furthermore, I find evidence that the effects of PRWORA on employer-sponsored healthcare coverage were particularly salient for the male population. Overall, these empirical findings support the broader finding that welfare bans did not decrease forms of non-citizen compensation that may not be reported in the paycheck.

I also consider how welfare bans may incentivize non-citizens to move to states that provide welfare for them. I test the effect of PRWORA on immigration by running a linear probability model where the dependent variable is a dummy that indicates whether or not the individual migrated in the last year. I find that PRWORA had no effect on the immigration patterns of non-citizens. Hence, I rule out immigration as a mechanism that may have determined the effect of PRWORA on non-citizen wages.

In conclusion, contrary to the findings of my search model, I find that non-citizen wages did not change in response to the welfare ban induced by PRWORA. This result implies that my search model cannot fully explain the effect of PRWORA on non-citizen wages.

In Section 2, I discuss the details of the 1996 U.S. welfare reform and connect my work to literature on immigration and wages. Section 3 presents the stylized search model that structures the empirical work. I discuss and characterize the data in section 4. Then, I outline my empirical strategy in section 5. Section 6 describes my results. Finally, section 7 presents a conclusion of the findings. My appendices are located after my bibliography. Tables and figures are at the end.

2 Literature Review

The 1996 U.S. welfare reform generally imposed harsher welfare eligibility rules on non-citizens[1]. While PRWORA drastically decreased the funding of federal welfare programs for non-citizens, it gave states the autonomy to decide whether or not to use state-level funding to support these welfare programs for non-citizens. For instance, states could use state-funding to provide SSI, food stamps, and Medicaid for non-citizens that would have otherwise lost eligibility for welfare. In general, there is substantial variation in how states revised their welfare eligibility rules for non-citizens[2].

The rules of the 1996 U.S. welfare reform differentiated non-citizens from naturalized citizens[3]. In contrast to non-citizens, naturalized citizens retained access to a wider set of federal welfare programs, such as Supplemental Security Income (SSI) and food stamps (Fix and Passel 2002). In fact, Fix and Passel (2002) finds that naturalized citizens and native citizens had roughly the same access to federal welfare programs.

Fix and Passel (1999) documents the trends in welfare program participation in the immigrant population before and after the enactment of PRWORA. While welfare use among citizen households fell by 14 percent from 1994 to 1997, welfare use among non-citizen households dropped at a much larger degree in the same timeframe at 35 percent. Furthermore, the decline in welfare use in the immigrant community was particularly driven by low-income households. On the other hand, no drastic changes in welfare program participation seemed to have occur for naturalized citizens. Although these trends do not conclusively prove that PRWORA had a casual effect on welfare program participation for non-citizens, the data

[1]Undocumented immigrants, however, were unaffected by the policy since they did not qualify for welfare before the enactment of the policy. This paper focuses specifically on the effects of PRWORA on documented non-citizens. For a detailed summary of how the provisions of the policy affected immigrants, see Vialet and Eig (1998).

[2]Zimmermann and Tumlin (1999) and Tumlin et al. (1999) review the choices made available to the states after the passage of PRWORA.

[3]The welfare reform policy originally made every immigrant ineligible for SSI, food stamps, and Medicaid after January 1st, 1997. Right after PRWORA was enacted, political opposition led to additional welfare reforms that reverted certain stipulations of the original policy. In particular, the Balanced Budget Act of 1997 resulted in a much smaller portion of the pre-enactment immigrant population losing eligibility to welfare.

shows that non-citizens relied significantly less on welfare compared to naturalized citizens and native-born citizens after the enactment of PRWORA.

As one of the first seminal papers that studies the effects of the policy on the immigrant population, Borjas (2003) exploits the quasi-experimental nature of the 1996 U.S. welfare reform and finds evidence that the policy encouraged immigrants to increase their labor supply to qualify for employer-sponsored insurance. Borjas (2004) follows this work and finds that PRWORA caused an increase in the percentage of immigrant households that experience food insecurity. Drawing from the empirical strategy in these papers, I use the classification of states with restrictive welfare policies for immigrants in Borjas (2003) to construct my triple-differences model. My paper differs from Borjas (2003) in that the dependent variable in my regression analysis, wages, explores a different avenue in which PRWORA affects the behavior of immigrants. The theoretical underpinning of my paper also differs from Borjas (2003) in that it emphasizes the role that search frictions has on immigrant decision-making.

Since Borjas (2003), many economists have explored the effects of PRWORA. Amuedo-Dorantes et al. (2016) finds evidence that the 1996 U.S. welfare reform led to a reduction of immigrant fertility rates, which they suspect is a result of PRWORA causing more immigrant families to experience financial pressures that would dissuade them from having children. Different aspects of the legislation have been studied in wide variety of areas other than immigration, including crime and disability[4]. For example, Graefe and Lichter (2008) use PRWORA as a quasi-experiment and find that there was no effect of welfare bans on marriage. These results showed that the policy likely failed its goal of promoting marriage. The empirical findings in this paper have been revisited by Low et al. (2018), who find that women are disadvantaged by the policy to a greater extent by men. While the focus of my paper is on the effect of PRWORA on non-citizens, these findings emphasize how the native population was also affected by PRWORA.

[4]For more examples of economic studies involving PRWORA, see Kaestner and Kaushal (2005), Kim and Joo (2011), Deshpande (2016), Yang (2017), and Tuttle (2019).

Beyond contributing to the research on PRWORA, this paper engages the immigration literature. Borjas (1999) finds evidence that immigrants are incentivized to immigrate to states that offer welfare. This empirical finding has an important implication for the results of my paper. Recall that some states choose to discontinue welfare for immigrants while some continued it. Therefore, it may be possible that PRWORA created incentives for immigrants in states that restricted welfare to leave their residence and migrate to another state or country. This incentive may contribute to the differences in wages between those affected by the policy and those unaffected. My work closely acknowledges the potential effects of immigration on the differences in wages induced by PRWORA.

A large literature in immigration has studied the nature of immigrant wages. Looking at the relationship between immigration and inequality, Card (2009) finds that the effect of immigration on wages is small while its effect on the immigrant wage gap is substantial. Other papers have looked for different channels that affect the immigrant wage gap. Bartolucci (2014) examines establishment-level data and finds that discrimination explains a portion of the immigrant wage gap in Germany. I contribute to this literature on the wage gap between natives and immigrants by introducing a novel channel that may influence it, the 1996 U.S. welfare reform policy.

My findings also contribute to the growing research in income inequality. Piketty et al. (2018) analyze national accounts to show that U.S. income inequality has been increasingly rising since 1980, a time period that includes the passage of PRWORA. A seminal paper by Autor et al. (2008) suggests skill-biased technological change may play a significant role in the U.S. income distribution. Derenoncourt and Montialoux (2021) show how policies that expanded minimum wage to a broader range of industries in the latter half of the 20th century reduced wage inequality, particularly for black workers[5]. My research similarly explores how PRWORA can influence wage inequality by affecting the labor market outcomes of immigrant communities.

[5]Bailey et al. (2016) finds similar results.

The literature on search theory provides a rich discussion on the relationship between welfare and wages. Several classic studies in particular note that unemployment insurance causes people to change their search behavior (Mortensen 1977, Baily 1978). In general, most search models show that a worker's wage falls with a reduction in unemployment benefits (Rogerson et al. 2005). More recent work by Marinescu and Skandalis (2020) empirically tests the implications of search theory using data from French workers and finds that the monthly wages of workers slightly decreases after unemployment benefits run out, an observation that is consistent with their search model. In a similar vein, Schmieder et al. (2016) uses an exogenous shock in welfare benefits in Germany to find evidence that extending the duration of unemployment insurance led a slight decrease in reemployment wages. It is clear from these studies that welfare affects wages. However, both of these studies differ substantially with each other and from my paper in that we all investigate vastly different welfare policies in different countries. My paper offers a new dimension to investigate the predictions of search theory on the relationship between welfare and wages.

3 Theory

To structure the empirical approach of this paper, I use a search model to demonstrate the potential consequences that welfare bans might have on wages. The basis of this model is from the exposition of Petrosky-Nadeau and Wasmer (2017) on the workhouse search and matching model for the labor market as presented in Mortensen and Pissarides (1994) and Pissarides (2000). Consider the search equilibrium that results when workers and firms interact in the labor market. To simplify the analysis, suppose that each firm would like to hire at most one worker. Suppose time is continuous. Workers can be either employed or unemployed, while firms can either be vacant or filled. In this environment, unemployed job seekers are looking to be matched with firms with vacancies and vice versa. When workers and firms are matched, an output is produced. Assume that the profit streams of a match between a worker and a firm ends at rate s.

Let $w > 0$ represent the wages that the job seeker receives if she is employed and $b > 0$ is the income that she receives if she is unemployed. For the purposes of the model, I suppose b represents the kinds of welfare benefits for the unemployed that would be restricted from non-citizens as a result of the 1996 U.S. welfare reform. Let $\mathcal{V}$, $\mathcal{U}$, and $\mathcal{M}$ represent the number of vacancies, jobs, and matches respectively in each time period t. Then define

$$\theta = \frac{\mathcal{V}}{\mathcal{U}}$$

as the labor market tightness, that is, the relative availability of jobs in the labor market. Let

$$f(\theta) = \frac{\mathcal{M}}{\mathcal{U}}$$

represent the transition rate for unemployed workers. Define γ and x as the cost of keeping a job posted and the output of a match respectively for every time period t. Finally, define W_u, W_n, J_π, and J_v as the value associated with an unemployed worker, an employed worker, a filled position, and a vacancy. Specifically, we have the asset values of unemployment and employment being

$$rW_u = b + f(\theta)(W_n - W_u) + \frac{\partial W_u}{\partial t}$$

$$rW_n = w + s(W_u - W_n) + \frac{\partial W_n}{\partial t}$$

where r represents the discount rate.

Consider a Nash bargaining game where firms with vacancies and job seekers negotiate for a wage in every period t. The solution to this game provides the following sharing identities:

$$\alpha_L = \frac{W_n - W_u}{\sum}$$

$$1 - \alpha_L = \frac{J_\pi - J_v}{\sum}$$

where $\sum$ represents the total surplus of a match and α_L and $1 - \alpha_L$ represents the relative bargaining power of workers and firms respectively. Using these identities in conjunction with the asset values of unemployment and employment gives the Nash wage

$$w = (1 - \alpha_L)b + \alpha_L(x + \gamma\theta)$$

It follows that the agreed upon wage between workers and firms is directly proportional to unemployment benefits. In other words, a reduction in welfare payments to the unemployed decreases wages. Hence, my search equilibrium model predicts that PRWORA would decrease non-citizen wages.

One concern about using this simple search model to understand the effect of PRWORA of non-citizens is that it does not model the differences between citizens and non-citizens in the work force. To address this concern, I extend my search model to allow for heterogeneous groups of workers using the setup from Cahuc et al. (2008) as summarized by Petrosky-Nadeau and Wasmer (2017). Beyond the advantages in modelling differences in groups in the workforce, I also relax my strong assumption of only single worker and firm matches by allowing for firms to hire multiple workers. The aggregate productivity of a match between workers and a firm is then defined by the function F.

Let there be two distinct types of workers, citizens, denoted by the subscript C, and non-citizens, denoted by the subscript I. Let $N = (\mathcal{N}_{\mathcal{C}}, \mathcal{N}_{\mathcal{I}})$, $V = (\mathcal{V}_{\mathcal{C}}, \mathcal{V}_{\mathcal{I}})$, and $\Theta = (\theta_{\mathcal{C}}, \theta_{\mathcal{I}})$ represent employment, the number of vacancies, and the labor market tightness respectively for the citizen and non-citizen submarkets for labor. I also allow unemployment benefits and search costs to differ in the citizen and non-citizen submarkets. The value of a worker to a firm depends on her type. Formally, the value of a marginal worker of type k to a firm is

$$J_{\pi k} = \frac{\frac{\partial F(N)}{\partial \mathcal{N}_k} - w_k(N) - \mathcal{N}_C \frac{\partial w_C(N)}{\partial \mathcal{N}_k} - \mathcal{N}_I \frac{\partial w_I(N)}{\partial \mathcal{N}_k}}{r + s}$$

In other words, the net marginal product of the marginal worker in addition to her effect on the firm's wage costs constitute her value to the firm. Consider the Nash bargaining game that occurs in each submarket. I assume that the bargaining weight α_L is the same for citizens and non-citizens. Then I get the following surplus sharing rule:

$$\alpha_L J_{\pi k} = (1 - \alpha_L)(W_{nk} - W_{uk})$$

Substitution gives the following system of partial differential equations:

$$w_k(N) = (1 - \alpha_L) r W_{uk} + \alpha_L \left(\frac{\partial F(N)}{\partial \mathcal{N}_k} - \mathcal{N}_C \frac{\partial w_C(N)}{\partial \mathcal{N}_k} - \mathcal{N}_I \frac{\partial w_I(N)}{\partial \mathcal{N}_k} \right)$$

It follows that $w_k(N)$ is directly proportional to W_{uk}. Recall that W_{uk} is directly proportional to b_k, the unemployment benefits to a worker of type k. Therefore, it follows that a reduction in unemployment benefits for the non-citizen population, as induced by a policy like PRWORA, would cause non-citizen wages to decrease. Hence, the original prediction of my simple search model stays true when allowing for differences in groups in the labor market.

4 Data

To empirically test the predictions of my search model, I analyze microdata from the Current Population Survey (CPS) using the IPUMS website (Flood et al. 2021). The CPS is a monthly survey of a sample of the U.S. population conducted by the Census Bureau. It includes basic demographic information in addition to other topics that can be found in supplemental surveys. I also make use of the Annual Social and Economic Supplement (ASEC) of the CPS. The ASEC is conducted every March and includes more detailed income data on survey participants.

The statistics of certain underrepresented demographic groups in the U.S. may have large standard errors due to the lack of people with those backgrounds in the sample. As such, the

CPS features a complicated sampling design that is done to generate accurate statistics for certain demographic groups that are historically under-sampled in the survey. This approach generates probability weights that may be applied to the data to account for this sampling design. For the purposes of this paper, the summary statistics and regression analysis does not incorporate probability weights. Solon et al. (2013) argues that there are reasons for and against weighting data depending on the goal of the researcher. The primary goal of this paper is to understand the relationship between welfare bans and immigrant wages. Since my regression analysis already controls for socioeconomic and demographic factors, using weights is not a primary concern of this paper.

In this paper, my sample is pooled cross-sectional data consisting of survey participants in the March 1994 and March 1998 CPS. Before I conduct my empirical analysis, I do some cleaning on my data. By construction, the CPS includes individuals who are 15 years or older. While reductions in welfare payments influence entire households, the effects of PRWORA on wages are the most relevant for members of the labor force. Therefore, I exclude people in the sample who are younger than 18 and older than 65 as they are not as likely to be in the labor force. After dropping these observations from the data, my entire sample consists of 169372 survey participants. Additionally, I express my data on wages in real terms. In particular, I deflate my data on yearly earnings in 1998 to get the real income using 1994 as the base year.

To better understand my sample, I generate summary statistics from the March 1994 and March 1998 CPS in Table 1. Overall, immigrants make up 13 percent of the sample. 4 percent of the sample is naturalized citizens and 9 percent of the sample is non-citizens. On average, participants in the sample make 18782 dollars. The average age of the sample is 39 years. Furthermore, 48 percent of the population is male, 85 percent of the population is white, 16 percent have not graduated high school, 33 percent have a high school diploma without a college degree, and 30 percent have a college degree. Over time, average yearly earnings increased from 17609 dollars in 1994 to 20113 dollars in 1998.

Table 2 and Table 3 examine the summary statistics of citizens and different immigrant groups within March 1994 and March 1998 CPS. Immigrants tend to have lower levels of education than natives, and a lower percentage of immigrants tends to be white compared to natives. I also find that immigrants make consistently less in wages than natives in 1994 and 1998. This immigrant-native wage gap is around 4000 dollars in the sample.

The summary statistics of the immigrant population hides several differences between non-citizens and naturalized citizens. While non-citizens make lower wages than natives, I also find that naturalized citizens make higher wages than natives. A higher percentage of naturalized citizens have completed college in comparison to natives and non-citizens. Furthermore, a larger percentage of naturalized citizens are Asian or Pacific Islander compared to natives and non-citizens. The average age of naturalized citizens is also the highest out of these three groups at approximately 43 years. These differences suggest that these socioeconomic and demographic characteristics vary substantially across these groups.

I note that the yearly earnings of natives, non-citizens, and naturalized citizens all increased by roughly 2000 dollars from 1994 to 1998. Nevertheless, these aggregate trends do not provide evidence that PRWORA had no effect on the earnings of non-citizens relative to natives as there may be a variety of other factors that may explain why immigrants experienced less wage growth compared to natives.

5 Empirical Strategy

To identify the effect of PRWORA, I first implement the following differences-in-differences regression:

$$\log Y_i = \alpha + \beta_1 G_i + \beta_2 P_i + \beta_3 (G_i \times P_i) + \gamma X_i + u_i$$

where $\log Y_i$ represent log yearly earnings[6] for person i[7], P_i is a dummy variable that equals unity if the person is surveyed in 1998, G_i is a dummy variable that equals unity if person i is a non-citizen, and X_i represents a vector of demographic and socioeconomic controls. Specifically, I control for race, gender, age, education, marriage status, and number of children. Furthermore, I include fixed effects for states and state-citizenship groups.

I also include the interaction term $G_i \times P_i$. The effect of PRWORA on the wages of non-citizens is measured by the coefficient on this interaction term, β_3, since

$$\begin{aligned}\beta_3 = &(E(\log Y|G=1,P=1) - E(\log Y|G=0,P=1))\\ &-(E(\log Y|G=1,P=0) - E(\log Y|G=0,P=0))\end{aligned}$$

Due to the nature of differences-in-differences, I note that β_3 specifically estimates the effect of PRWORA on non-citizen wages relative to citizens. In other words, β_3 is the differential effect of PRWORA on non-citizens compared to the rest of the sample. As the literature firmly established, citizens also experienced important changes in their access to welfare. Although I may not explicitly mention that the DID estimates identify the differential effect of PRWORA in every instance that I mention it in the paper, this nuance is implicit throughout my empirical analysis.

Since I limit the timeframe of my analysis from 1994 to 1998, the DID coefficient is likely capture the immediate effect of PRWORA. Removing the years 1995, 1996, and 1997 from my sample also helps to distinguish the effect of PRWORA on non-citizens wages after the enactment of the policy on August 22nd, 1996. One necessary assumption for the differences-in-differences approach is the parallel trends assumption (Angrist and Pischke 2009). In other words, the trends in wage growth must be the same for the groups in comparison. To justify the validity of this assumption, I construct a time series graph in Figure 1 to show the

[6]The fact that wages are nonnegative values and tend to be right-skewed makes transforming my wage data by log scale more appropriate. Hence, my coefficients should be interpreted as percentages rather than dollar values.

[7]I index my observations by i to emphasize the fact that I am using cross-sectional data.

differences in average yearly earnings for citizens and non-citizens. The graph shows that the growth in yearly earnings remain roughly the same for citizens and non-citizens from 1994 to 1998. This graph provide evidence that the parallel trends assumption is satisfied.

While differences-in-differences is a promising first step in identifying the causal effect of PRWORA on non-citizen wages, it does not account for the choices of some states to continue providing welfare for non-citizens. These state-driven choices mean that the treatment group of the policy is not the entire non-citizen population, but rather the group of non-citizens who live in states that experienced a loss in welfare benefits. As mentioned in Berck and Villas-Boas (2016), triple-differences may improve upon the differences-in-differences approach by accounting for the heterogeneous effects of the treatment. In order to more cleanly estimate the effect of PRWORA, I estimate the following triple-differences (DIDID) model:

$$\begin{aligned}\log Y_i = \alpha + \beta_1 G_i + \beta_2 S_i + \beta_3 P_i + \beta_4 (G_i \times S_i) + \beta_5 (G_i \times P_i) + \beta_6 (S_i \times P_i) \\ + \beta_7 (G_i \times S_i \times P_i) + \gamma X_i + u_i\end{aligned}$$

where S_i represents the states that adopted restrictive welfare eligibility rules for non-citizens after the enactment of the 1996 U.S. welfare reform. I use the same controls and fixed effects that I specified for my DID model. Notice that

$$\begin{aligned}\beta_7 = ((E(\log Y|G=1, P=1, S=1) - E(\log Y|G=0, P=1, S=1)) \\ -(E(\log Y|G=1, P=1, S=0) - E(\log Y|G=0, P=1, S=0))) \\ -((E(\log Y|G=1, P=0, S=1) - E(\log Y|G=0, P=0, S=1)) \\ -(E(\log Y|G=1, P=0, S=0) - E(\log Y|G=0, P=0, S=0))),\end{aligned}$$

so the coefficient β_7 captures the casual effect of the policy on the wages of those who were affected by PRWORA, non-citizens living in states that banned welfare for them. I carefully note that coefficient β_7 represents a differential effect of PRWORA, that is, the effect of

PRWORA on the wages of non-citizens living in states that banned welfare for them relative to the rest of the population. As with the differences-in-differences estimator, this nuance is implied whenever I refer to the triple-differences estimates of the effect of PRWORA on non-citizen wages.

For the purposes of this paper, I adopt the classification of states with restrictive welfare eligibility rules for non-citizens from Borjas (2003). Using this classification, I denote a state as restrictive if they decided to ban food assistance or SSI to pre-enactment non-citizens and TANF, Medicaid, food assistance, or SSI to post-enactment non-citizens[8]. These states are Alabama, Alaska, Arizona, Arkansas, District of Columbia, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Montana, Nevada, New Mexico, North Carolina, North Dakota, Oklahoma, South Carolina, South Dakota, and West Virginia[9].

Wooldridge (2010) shows that a key assumption needed for triple-differences is that the trend in the differences in wages between non-citizens and citizens must be the same in states before the enactment of the 1996 U.S. welfare reform[10]. In other words, the triple-differences model requires an assumption that is similar to the parallel trends assumption in DID models. To justify this assumption for my triple-differences model, I illustrate the yearly earnings of non-citizens and citizens in states with and without restrictive welfare policies from 1994 to 1998 in Figure 2. The graph shows that the differences in wages between non-citizens and citizens in states that banned welfare and those that did not are roughly parallel before the enactment of PRWORA. I take this graph as evidence that the assumptions for my triple-differences model are satisfied. Similar to the result in Figure 1, the trends in the post-enactment period do not seem to have significantly changed from the pre-enactment period.

I cluster my standard errors by state and citizenship status for my DID and DIDID models. Borjas (2003) takes a similar approach in clustering standard errors. The intuition

[8] I also classify the District of Columbia as a state.

[9] See Borjas (2003) for more information on this classification scheme.

[10] See page 151 of Wooldridge (2010) for a detailed discussion of the triple-differences approach.

for my choice to use these standard errors comes from Abadie et al. (2017). The authors of this paper argue that standard errors should be clustered when the assignment of the treatment is clustered. More formally, consider the dummy variable W that indicates the treatment of the policy. In the context of my paper, W refers to the interaction term $G \times S \times P$. When $G \times S \times P$ equals unity, it means that the sampled individual has received the treatment, that is, they have lost access to welfare. Clearly, this treatment was not randomly assigned to the population. This treatment was given to non-citizens living in states with restrictive welfare policies. Hence, there is correlated assignment within clusters grouped by state and citizenship status. According to Abadie et al. (2017), it follows that standard errors should be clustered by state and citizenship status.

6 Results

6.1 Non-Citizen Wages

I use a differences-in-differences approach to estimate the causal effects of PRWORA on immigrant wages in Table 4. The differences-in-differences coefficient in my first regression measures the effect of PRWORA on the wages of non-citizens. I find that the DID coefficient is +.269. In other words, my results imply that non-citizens experienced a 26.9 percent increase in yearly earnings compared to citizens from 1994 to 1998. These results are statistically significant at the 1 percent level. In my second regression of Table 4, I include fixed effects for occupations. Using these fixed effects, I find that the DID coefficient drastically decreases in size to +.0416 with a large p-value of .463. The differences between these two regressions indicate that my significance of my first DID regression stems largely for the occupational choices of non-citizens.

I run my triple-differences model in Table 5. In the first regression, I find that the DIDID coefficient is +.125. This coefficient implies that PRWORA caused the yearly earnings of non-citizens to increase by 12.5 percent. However, the p-value of this coefficient is .608. In my second regression of Table 5, I include fixed effects for occupations. I find that the DIDID coefficient on this regression is +.228 with a p-value of .317. Therefore, this regression does not provide evidence that non-citizens experienced a decrease in yearly earnings due to the 1996 U.S welfare reform.

A noticeable feature of my triple-differences estimates is that the magnitude and the significance of the DIDID coefficients are different compared to my DID coefficients. These changes illustrate how the triple-differences approach generates differences in the estimation of the effect of PRWORA and suggests that the differences-in-differences approach may not capture the true effect of the welfare reform. Due to the fact that triple-differences better accounts for the heterogeneity in state-driven choices to discontinue welfare for non-citizens, I consider to the triple-differences approach of evaluating the effect of PRWORA as more valid compared to the differences-in-differences approach.

To test for heterogeneous treatment effects based on employment status, I split the sample by those who were unemployed for at least a week in the last year and those who were not. The reason why I conduct this check is to see if unemployed people are taking jobs with higher or lower wages. Using the sample of people who were unemployed for more than a week, I find in Table 6 that my triple-differences regression reports a DIDID coefficient of -.139. Like many of the other DIDID estimates, it is statistically insignificant at the 10 percent level with a p-value of .633. The negative sign of the DIDID coefficient on the sample of unemployed people in the last year implies that PRWORA may have caused a decrease in wages particularly for unemployed non-citizen job seekers, but the large p-value associated with this estimate means that there is no statistically significant evidence of this relationship at the 10 percent level. On the sample of people who were employed the entire past year, I find that the DIDID coefficient is +.238 with a p-value of .265. This estimate is similar to previously reported DIDID coefficients.

I conduct sensitivity analysis in Appendix A and find that my results are robust to an alternative welfare specification, a variety of standard errors, fixed effects for state-time pairs, controls for hours and weeks worked, and winsorizing my wage data at the 5% and 10% level. In Appendix B, I test for sources of heterogeneity in poverty status and gender in the effect of PRWORA. In summary, there is no evidence that PRWORA had statistically significant effects on these groups at the ten percent level.

6.2 Health Insurance

Up until this point of the paper, I have not considered the fact that employers may be compensating their employees through channels other than just wages alone. For instance, many U.S. employers provide their employees with health insurance. To examine if sources of income other than wages for non-citizens changed due to PRWORA, I estimate the effect of PRWORA on employer-sponsored healthcare coverage with the following DIDID approach using the linear probability model (LPM):

$$\begin{aligned} Z_i = \alpha + \beta_1 G_i + \beta_2 S_i + \beta_3 P_i + \beta_4 (G_i \times S_i) + \beta_5 (G_i \times P_i) + \beta_6 (S_i \times P_i) \\ + \beta_7 (G_i \times S_i \times P_i) + \gamma X_i + u_i \end{aligned}$$

where Z_i is a dummy that equals unity if the individual is enrolled in an employer-sponsored health insurance program. The controls are identical to my differences-in-differences and triple-differences model. I include fixed effects for states, state-citizenship groups, and occupations.

In Figure 3, I examine trends in the employer-sponsored health insurance coverage rate for citizens and non-citizens living in states that kept and banned welfare for them. I find that the trends in employer-sponsored health care before 1996 remain largely the same for non-citizens in states with and without welfare. This conclusion applies for citizens as well. However, after 1996 the percentage of non-citizens covered by employer-sponsored health insurance in states that banned welfare rapidly escalates, increasing more than 5 percent from 1996 to 1997, in contrast to the other groups in the figure who experienced much smaller growth. These visual results provide evidence that pre-trends in the control and treatment group of my DIDID model are parallel. Furthermore, the large spike in non-citizen enrollment in employer healthcare after the passage of PRWORA suggests that non-citizens may have been incentivized to get healthcare covered by their employer during this time period.

I corroborate the visual evidence in Figure 3 that PRWORA increased the non-citizen employer-sponsored healthcare coverage rate with my regression results in Table 7. In my first regression, I run my LPM and find that the non-citizens living in states that banned welfare are 5.24 percent more likely to get employer-sponsored healthcare coverage due to PRWORA. Furthermore, this estimate is statistically significant at the 10 percent level with a p-value of .077. To support the robustness of these results, I also run this regression using the probit equivalent of my linear probability model. In other words, I estimate the following equation:

$$\begin{aligned} P(Z_i|\cdot) = \Phi(\beta_1 G_i + \beta_2 S_i + \beta_3 P_i + \beta_4 (G_i \times S_i) + \beta_5 (G_i \times P_i) + \beta_6 (S_i \times P_i) \\ + \beta_7 (G_i \times S_i \times P_i) + \gamma X_i) \end{aligned}$$

The advantage of using probit is that it may be a better fit of the data since the dependent variable can only take values 0 and 1. I run my probit model in the second regression of Table 7, I find that PRWORA caused more non-citizens to get employer-sponsored welfare. This result is associated with a p-value of .074 and is statistically significant at the 10 percent level. These results fall in line with Borjas (2003), who finds that non-citizens were more likely to get employer-sponsored healthcare coverage due to PRWORA.

To expand upon the analysis done in Borjas (2003), I test to see if PRWORA had a heterogeneous impact on the private healthcare coverage of non-citizens depending on their gender. I use the linear probability model that I previously specified, but now I stratify the sample into a female sample and a male sample. To the best of my knowledge, this dynamic has not been studied before with my empirical strategy. In Table 8, I find that the DID coefficient is +.0228 on the female sample. This coefficient, however, has a p-value of .433, indicating that the effect is not statistically significant at the ten percent level. On the other hand, I find that the DIDID coefficient on the male sample is +.072 with a p-value of .043. In other words, I find statistically significant evidence at the 5 percent level that PRWORA caused non-citizen males to be 5 percent more likely to enroll in employer-sponsored healthcare. These results imply that the effects of PRWORA on health insurance were primarily driven by non-citizens males.

6.3 Migration

While my theoretical model focuses on how of search frictions determine the effect of PRWORA on the wages of non-citizens, there is also a concern that immigration patterns might drive differences in wages. I address this concern by estimating the effect of PRWORA on migration with a linear probability model and probit model. My dependent variable is a dummy that is unity if the person migrated in the past year. The controls and fixed effects in this model are identical to my previous regressions on health insurance.

In Table 9, my LPM suggests that the probability that a non-citizen affected by PRWORA migrated in the last year slightly decreased. However, this estimate is associated with a p-value of .818 and is not statistically significant at the 10 percent level. Furthermore, the R-squared of this regression is .026, which is a sign that my regression poorly fits the data. Given the relatively small size of the coefficients along with these other observations, I find no evidence in the data that PRWORA influenced migration patterns among non-citizens affected by PRWORA. To test the robustness of this finding, I run a probit model in place of the linear probability model. Interestingly, the estimated effect of PRWORA on migration switches sign, but with a p-value at .518, it is far from statistically significant at the ten percent level. Based on these results, it seems that the PRWORA had a minimal effect on immigration. As such, I have evidence that my estimates of the effects of PRWORA on non-citizens are not driven by immigration patterns.

6.4 Discussion

While my search model predicts that PRWORA would cause non-citizens to accept lower wages, the empirical evidence shows that there is no evidence that non-citizens had lower wages due to PRWORA. Therefore, the predictions of my search model are not represented in my data. There a few possible explanations for this. First, the equilibrium wage only holds in the long-run when the worker and firm have had sufficient time to engage in the bargaining process. As such, my triple-differences estimates of the effect of PRWORA, which are only feasible to compute in the timeframe right before and after the enactment of the policy, may be unable to capture this long-run relationship. Secondly, there is a chance that my search model does not capture relevant features of PRWORA and its effects on the citizen and non-citizen populations. For instance, I assume bargaining weights between citizens and non-citizens are equal, when this may actually not be the case in practice. Furthermore, my search model does not consider how other factors such as on-the-job search may also determine wages in the labor market. The disparity between my empirical and theoretical conclusions provides a puzzle to explore in the literature.

7 Conclusion

This paper studies the effect of the 1996 U.S. welfare reform on non-citizen wages. Using a rich set of demographic and socioeconomic controls with a triple-differences model, I find no evidence that PRWORA decreased the yearly earnings of non-citizens in states that banned welfare but do find evidence that PRWORA increased non-citizen participation in employer-sponsored healthcare coverage. Furthermore, I find no evidence that PRWORA caused non-citizens to change their migration patterns. These results imply that the predictions of my search model cannot explain the effects of PRWORA on non-citizen wages.

My findings provide a variety of directions that can be extended for future research. Researchers could extend my reduced-form estimates by more closely integrating search theory with econometric work through calibration or structural techniques. Also, understanding the dynamics between welfare bans and wage inequality in the immigrant population can be further explored in other similarly structured policies.

Although the stipulations of PRWORA have been greatly revised since the initial enactment of the policy, its impact has continued to be felt even in the modern era of the COVID-19 pandemic. For instance, legal issues surrounding the nuances of PRWORA arose during debates over expanding welfare benefits to non-citizens during the pandemic (Harrington 2020). The ramifications of the pandemic may provide researchers opportunities to extend the findings of this paper.

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Appendix A Robustness Checks

Having established the effects of PRWORA on the wages of immigrant population, I run additional regressions to test whether or not my results are robust to a variety of alternative specifications. In particular, I test the robustness of the triple-differences regression presented in the second regression of Table 5.

A natural robustness check to conduct is to test whether or not my results are sensitive to how I categorized states with restrictive welfare policies for non-citizens. To use a slightly different measure than the ones proposed in Borjas (2003), I choose a specification where I denote a state as having restrictive welfare eligibility rules for non-citizens if they were classified as least available in terms of welfare using the Zimmerman-Tumlin categorization of state welfare policies in their policy report on the U.S. welfare reform (Zimmerman and Tumlin 1999). According to this classification, the states that had restrictive welfare policies for non-citizens are Alabama, Arkansas, Idaho, Indiana, Louisiana, Mississippi, Ohio, Oklahoma, South Carolina, South Dakota, Texas, and West Virginia. I incorporate this definition of what constitutes a state with restrictive welfare policies for non-citizens into my baseline triple-differences model to estimate the effects of PRWORA in the first regression of Table 10. I find the DIDID coefficient is +.226 with a p-value of .322. This result is similar to my original estimates. Hence, I find evidence that my results are robust to how the states were classified in terms of their generosity with providing welfare to non-citizens.

Another robustness check that I conduct is to consider including additional controls. In the second regression of Table 10, I add a set of controls by interacting a dummy variable for every state with a dummy variable indicating if the individual was surveyed before or after the enactment of PRWORA. This set of variables allow me to control for any state-specific factors that change over time. I find that DIDID coefficient is +.255. While the p-value drops to .206, it remains far from statistically significant at the ten percent level. Hence, this regression provides evidence that my results are robust to my additional controls. I also include controls for hours and weeks worked. Yearly earnings depend on how many hours of work that an individual does during the year. People who work longer hours will make more in wages compared to those who are paid at the same hourly rate. With the addition of this control, the DIDID coefficient is .186 with a p-value of .256. These results remain largely similar to the baseline DIDID specification.

The last set of adaptions I make to the baseline triple-differences regression is by winsorizing the data on wages. Given that the data is collected from surveys, there may be a concern that our results are driven by extreme outliers in the data. In regression 3, I winsorize the data on wages at the 5 percent level. I find that the DIDID coefficient is +.223. This estimate has a p-value of .324 and is statistically insignificant at the 10 percent level. The fourth regression winsorizes the data at the 10 percent level and produces similar results. Hence, I find evidence that my results are robust to winsorizing the data on wages.

Table 11 concludes my robustness tests of my baseline triple-differences model by using alternative specifications of standard errors. In my first regression, I use robust standard errors that do not account for clustering. I obtain a p-value of .149 on the DIDID coefficient. My second regression in Table 8 uses standard errors clustered by state and obtains a DIDID coefficient with a p-value of .298. These standard errors produce similar estimates of the standard error compared to my original baseline model that uses standard errors clustered by state and immigration status. Finally, my third regression uses standard errors clustered by occupation. The DIDID coefficient on this regression has a p-value of .109. Based on these results, my results are robust to different standard error specifications. Regardless, Abadie et al. (2013) provides a good case for clustering my standard errors based on state and immigration status and that doing otherwise would inaccurately portray the certainty of my results.

Appendix B Heterogeneity

I run regressions on stratified samples to test for heterogeneity in the effect of PRWORA on non-citizen wages in Tables 12 and 13. As with my robustness checks, I use the second regression of Table 6 as the baseline triple-differences model for these regressions. My first regression estimates the baseline triple-differences model on the male people in the sample. I find that the DIDID coefficient is +.0390 for females. The magnitude of the coefficient is much smaller, but with a p-value of .867, it is not statistically significant at the 10 percent level. In my second regression, I reduce my sample to the male participants of the survey data and find a much larger DIDID coefficient at +.402. However, with a p-value of .241, this effect is also not statistically significant at the 10 percent level. Based on these results, I find that PRWORA may have affected women differently in that the estimated size of the coefficients change, but the large p-values means that there is no evidence that PRWORA affected these two groups.

In my next test of heterogeneity, I split my sample depending on their poverty status. The CPS includes a variable that classifies individuals as falling above or below the poverty line. I use this variable in my first regression to run my baseline triple-differences model on the sample of people who are above the poverty line. I find that the DIDID coefficient is +.152, similar to the baseline results, but the p-value is larger at .523. I then run a DIDID regression on the sample of people who fall below the poverty line. I find that the DIDID coefficient is +.168 with a p-value of .608. Even after splitting the sample by poverty status, I find no statistically significant estimated effects of PRWORA. In conclusion, my heterogeneity tests suggest that there is no evidence that PRWORA affected the wages of non-citizens, even after stratifying the sample by identifying groups that potentially may have been affected differently than the whole non-citizen sample.

Table 1: Summary Statistics, March CPS 1994 and 1998

	Full sample	1994	1998
Yearly earnings (dollars)	18781.81	17608.99	20113.00
	(24767.7)	(19945.6)	(29237.7)
Age	38.90	38.59	39.26
	(12.39)	(12.38)	(12.39)
Male	0.48	0.48	0.48
	(0.500)	(0.500)	(0.500)
Female	0.52	0.52	0.52
	(0.500)	(0.500)	(0.500)
Immigrant:			
Naturalized citizen	0.04	0.04	0.05
	(0.199)	(0.184)	(0.215)
Not a citizen	0.09	0.09	0.09
	(0.284)	(0.280)	(0.289)
Race:			
White	0.85	0.85	0.85
	(0.355)	(0.358)	(0.353)
Black	0.10	0.09	0.10
	(0.293)	(0.293)	(0.293)
Asian or Pacific Islander	0.04	0.03	0.04
	(0.185)	(0.179)	(0.190)
Education:			
No high school diploma	0.16	0.16	0.16
	(0.366)	(0.368)	(0.364)
High school diploma	0.33	0.34	0.33
	(0.471)	(0.473)	(0.470)
College degree	0.30	0.29	0.31
	(0.458)	(0.454)	(0.463)
N	169372	90042	79330

mean coefficients; sd in parentheses

Table 2: Summary Statistics by Citizenship and Immigration Status, March CPS 1994

	Natives	Naturalized citizens	Non-citizens	Immigrants
Yearly earnings (dollars)	18132.88	20146.39	11200.13	13801.62
	(20096.0)	(22143.1)	(15868.1)	(18374.6)
Age	38.71	42.56	35.71	37.70
	(12.44)	(11.61)	(11.49)	(11.94)
Male	0.48	0.46	0.49	0.48
	(0.500)	(0.499)	(0.500)	(0.500)
Female	0.52	0.54	0.51	0.52
	(0.500)	(0.499)	(0.500)	(0.500)
Race:				
White	0.87	0.64	0.70	0.68
	(0.334)	(0.480)	(0.457)	(0.465)
Black	0.10	0.05	0.07	0.06
	(0.299)	(0.218)	(0.249)	(0.241)
Asian or Pacific Islander	0.01	0.27	0.17	0.20
	(0.100)	(0.444)	(0.379)	(0.401)
Education:				
No high school diploma	0.13	0.19	0.43	0.36
	(0.341)	(0.396)	(0.495)	(0.480)
High school diploma	0.35	0.27	0.24	0.25
	(0.477)	(0.442)	(0.426)	(0.431)
Bachelor's degree or more	0.29	0.37	0.21	0.26
	(0.456)	(0.482)	(0.410)	(0.438)
N	79151	3167	7724	10891

mean coefficients; sd in parentheses

Table 3: Summary Statistics by Citizenship and Immigration Status, March CPS 1998

	Natives	Naturalized citizens	Non-citizens	Immigrants
Yearly earnings (dollars)	20760.03	21871.70	13113.54	16140.39
	(29466.2)	(32422.7)	(23893.5)	(27462.2)
Age	39.43	42.54	35.91	38.20
	(12.48)	(11.76)	(11.16)	(11.80)
Male	0.48	0.48	0.49	0.49
	(0.500)	(0.500)	(0.500)	(0.500)
Female	0.52	0.52	0.51	0.51
	(0.500)	(0.500)	(0.500)	(0.500)
Race:				
White	0.87	0.66	0.77	0.73
	(0.331)	(0.475)	(0.421)	(0.443)
Black	0.10	0.08	0.06	0.07
	(0.299)	(0.267)	(0.245)	(0.253)
Asian or Pacific Islander	0.01	0.26	0.16	0.19
	(0.109)	(0.438)	(0.366)	(0.395)
Education:				
No high school diploma	0.12	0.21	0.46	0.37
	(0.328)	(0.404)	(0.498)	(0.483)
High school diploma	0.34	0.27	0.23	0.25
	(0.474)	(0.443)	(0.424)	(0.431)
Bachelor's degree or more	0.32	0.37	0.20	0.26
	(0.466)	(0.482)	(0.402)	(0.438)
N	68219	3840	7271	11111

mean coefficients; sd in parentheses

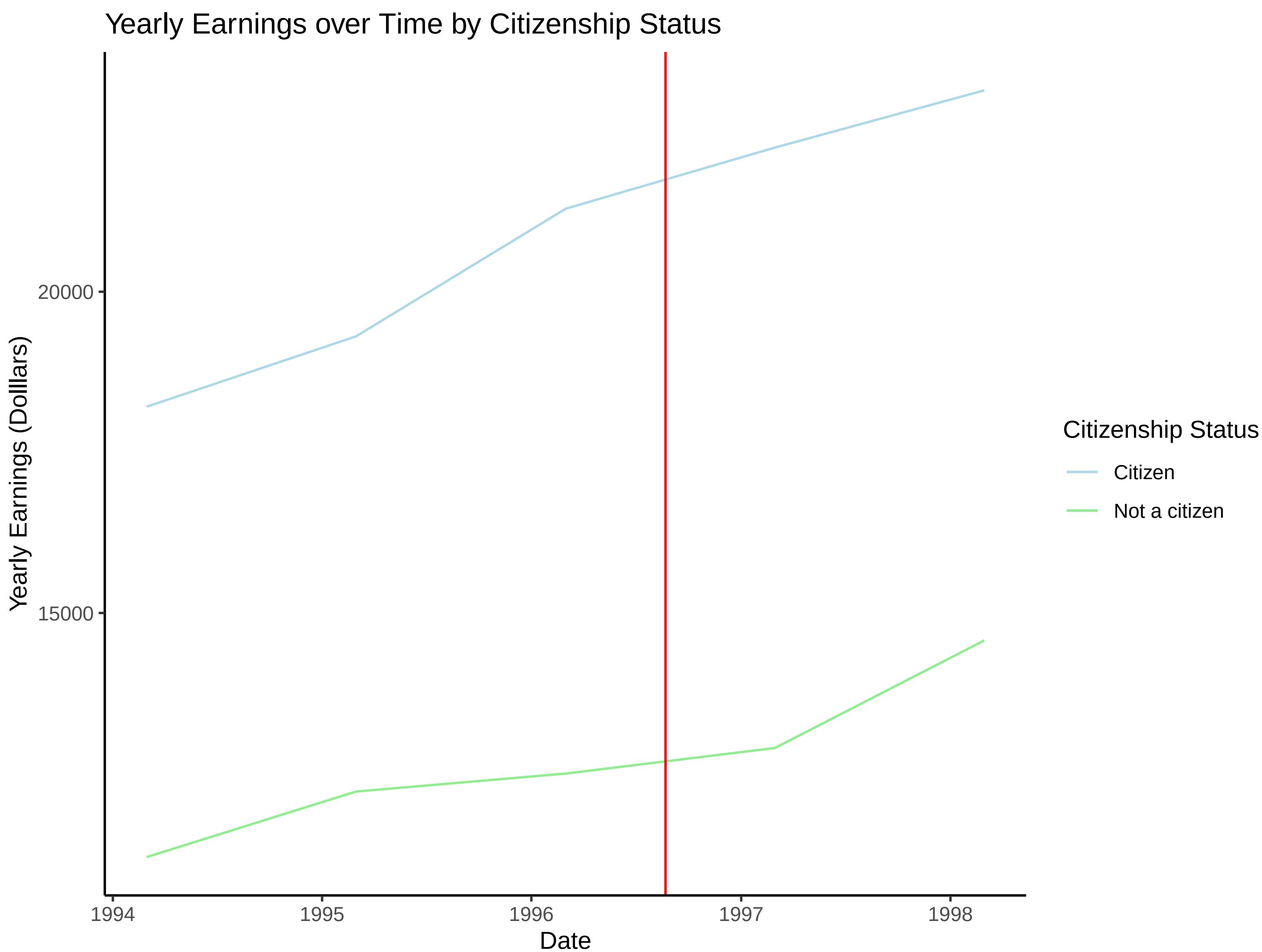

Figure 1: Yearly Earnings over Time by Citizenship Status, 1994-1998

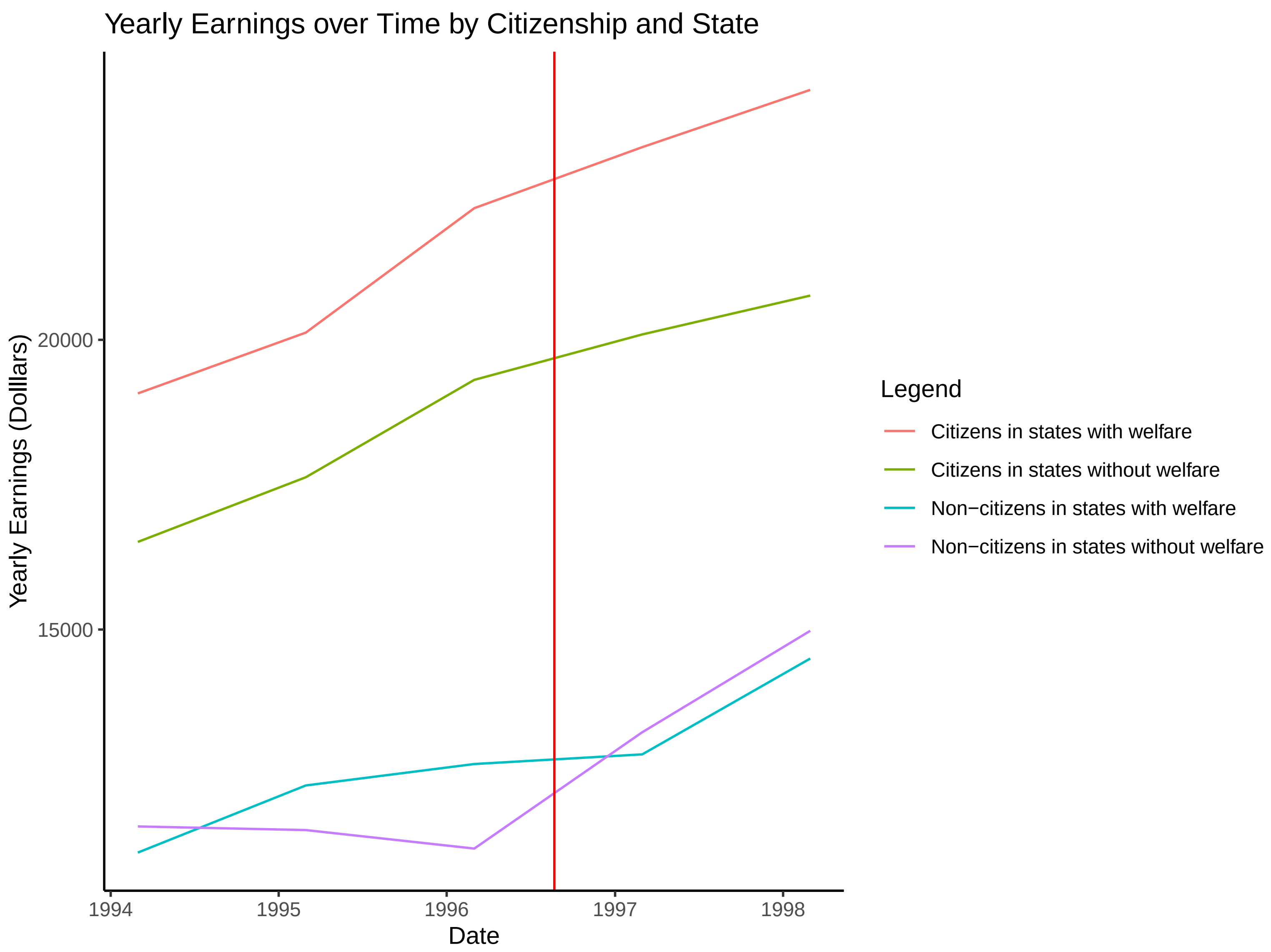

Figure 2: Yearly Earnings over Time by Citizenship and State, 1994-1998

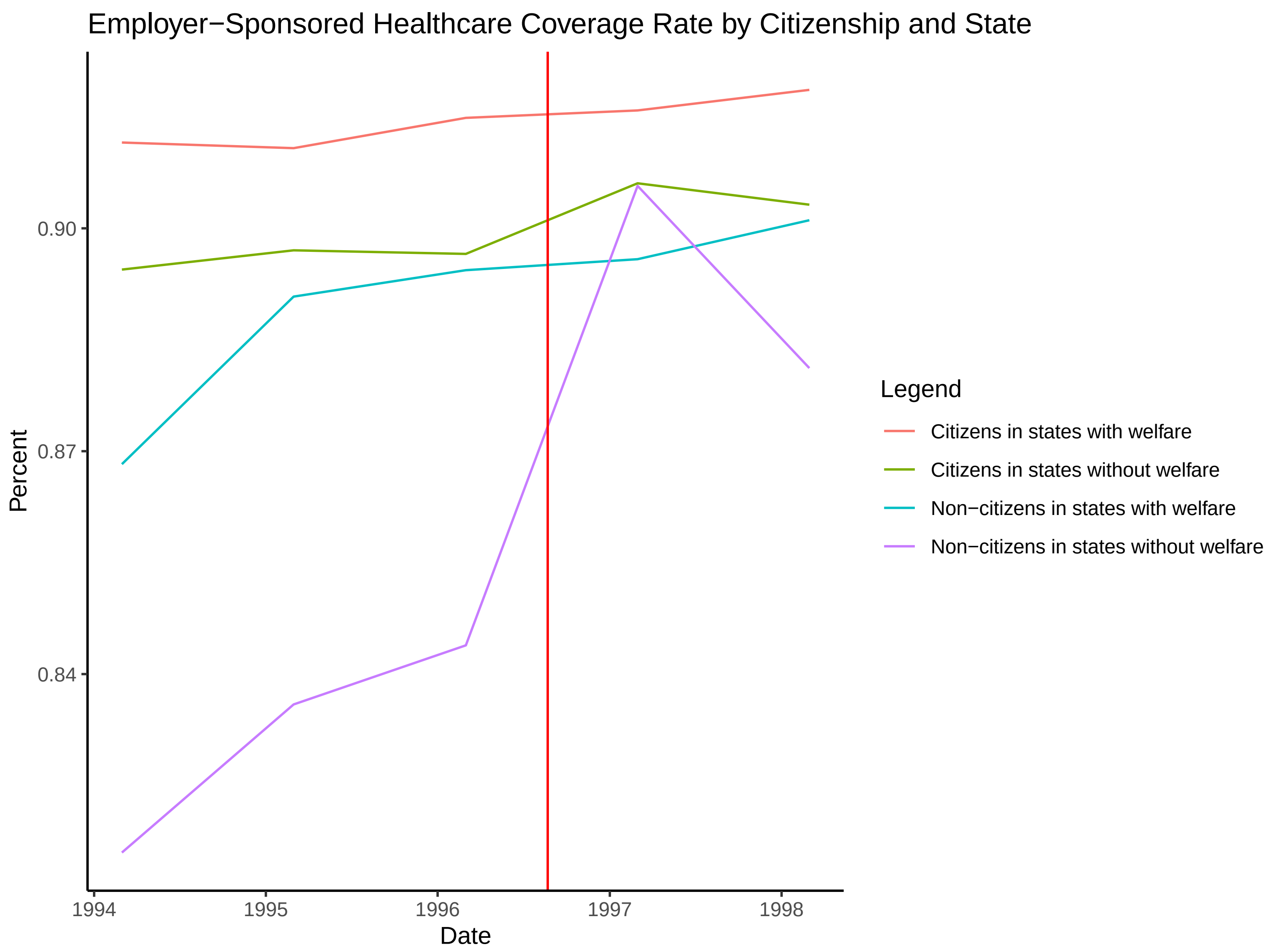

Figure 3: Employer-Sponsored Healthcare Coverage Rate by Citizenship and State, 1994-1998

Table 4: DID Effects of PRWORA on Wages

	(1) DID	(2) DID
Year*Non-citizen	0.269***	0.0416
	(0.000)	(0.463)
State (with interaction) FEs	Yes	Yes
Occupation FEs	No	Yes
N	169372	169372
R^2	0.081	0.454

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. The dependent variable is log yearly earnings. I control for time, citizenship status, race, gender, age, education, marital status, and number of children. Standard errors are clustered by state and citizenship status.

Table 5: DIDID Effects of PRWORA on Wages

	(1) DIDID	(2) DIDID
Time*Welfare Ban*Non-citizen	0.125	0.228
	(0.608)	(0.317)
State (with interaction) FEs	Yes	Yes
Occupation FEs	No	Yes
N	169372	169372
R^2	0.081	0.454

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. The dependent variable is log yearly earnings. I control for time, citizenship status, race, gender, age, education, marital status, and number of children. Standard errors are clustered by state and citizenship status.

Table 6: Heterogeneity in Employment Status for Effects of PRWORA on Wages

	(1) DIDID	(2) DIDID
Time*Welfare Ban*Non-citizen	-0.139	0.238
	(0.633)	(0.265)
Unemployed last year	Yes	No
Employed last year	No	Yes
N	14443	154929
R^2	0.051	0.470

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. The dependent variable is log yearly earnings. I control for time, citizenship status, race, gender, age, education, marital status, and number of children. Standard errors are clustered by state and citizenship status.

Table 7: Effects of PRWORA on Employer-Sponsored Healthcare

	(1) LPM	(2) Probit
Time*Welfare Ban*Non-citizen	0.0524*	0.180*
	(0.077)	(0.074)
N	169372	169372
R^2	0.216	

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. The dependent variable is a dummy variable that equals unity if the person has employer-sponsored health insurance. I control for time, citizenship status, race, gender, age, education, marital status, and number of children. Standard errors are clustered by state and citizenship status.

Table 8: Heterogeneity in Gender for Effects of PRWORA on Employer-Sponsored Healthcare

	(1) LPM	(2) LPM
Time*Welfare Ban*Non-citizen	0.0228	0.0742^{**}
	(0.433)	(0.043)
Female sample	Yes	No
Male sample	No	Yes
N	87920	81452
R^2	0.225	0.195

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. The dependent variable is a dummy variable that equals unity if the person has employer-sponsored health insurance. I control for time, citizenship status, race, age, education, marital status, and number of children. Standard errors are clustered by state and citizenship status.

Table 9: Effects of PRWORA on Migration

	(1) LPM	(2) Probit
Time*Welfare Ban*Non-citizen	-0.00595	0.0957
	(0.818)	(0.518)
N	169372	169372
R^2	0.026	

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. The dependent variable is a dummy variable that equals unity if the person migrated in the past year across states or countries. I control for time, citizenship status, race, gender, age, education, marital status, and number of children. Standard errors are clustered by state and citizenship status.

Table 10: Robustness Checks for Effects of PRWORA on Wages

	(1) DIDID	(2) DIDID	(3) DIDID	(4) DIDID	(5) DIDID
Time*Welfare Ban*Non-citizen	0.226	0.255	0.186	0.223	0.219
	(0.322)	(0.206)	(0.256)	(0.324)	(0.331)
Alternative welfare specification	Yes	No	No	No	No
Time*State FEs	No	Yes	No	No	No
Controls for hours and weeks worked	No	No	Yes	No	No
Winsorize 5%	No	No	No	Yes	No
Winsorize 10%	No	No	No	No	Yes
N	169372	169372	169372	169372	169372
R^2	0.454	0.454	0.719	0.454	0.454

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. The dependent variable is log yearly earnings. I control for time, citizenship status, race, gender, age, education, marital status, and number of children. Standard errors are clustered by state and citizenship status.

Table 11: SE Specifications for Effects of PROWORA on Wages

	(1) DIDID	(2) DIDID	(3) DIDID
Time*Welfare Ban*Non-citizen	0.228	0.228	0.228
	(0.149)	(0.298)	(0.109)
Robust SE	Yes	No	No
SE clustered by state	No	Yes	No
SE clustered by occupation	No	No	Yes
N	169372	169372	169372
R^2	0.454	0.454	0.454

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. The dependent variable is log yearly earnings. I control for time, citizenship status, race, gender, age, education, marital status, and number of children.

Table 12: Heterogeneity in Gender for Effects of PRWORA on Wages

	(1) DIDID	(2) DIDID
Time*Welfare Ban*Non-citizen	0.0390	0.402
	(0.867)	(0.241)
Female sample	Yes	No
Male sample	No	Yes
N	87920	81452
R^2	0.540	0.324

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. I control for time, citizenship status, race, age, education, marital status, and number of children. Standard errors are clustered by state and citizenship status.

Table 13: Heterogeneity in Poverty Status for Effects of PRWORA on Wages

	(1) DIDID	(2) DIDID
Time*Welfare Ban*Non-citizen	0.152	0.168
	(0.523)	(0.608)
Above poverty line	Yes	No
Below poverty line	No	Yes
N	149214	20158
R^2	0.424	0.412

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: The sample is people ages 18-65 in the March 1994 and 1998 CPS. The dependent variable is log yearly earnings. I control for time, citizenship status, race, gender, age, education, marital status, and number of children. Standard errors are clustered by state and citizenship status.