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# The Effects of DACAmentation: The Impact of Deferred Action for Childhood Arrivals on Unauthorized Immigrants☆

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ABSTRACT

As the largest immigration policy in 25 years, Deferred Action for Childhood Arrivals (DACA) made deportation relief and work authorization available to 1.7 million unauthorized immigrants. This paper looks at how DACA affects DACA-eligible immigrants' labor market outcomes. I use a difference-in-differences design for unauthorized immigrants near the criteria cutoffs for DACA eligibility. I find DACA increases the likelihood of working by increasing labor force participation and decreasing the unemployment rate for DACA-eligible immigrants. I also find DACA increases the income of unauthorized immigrants in the bottom of the income distribution. I find little evidence that DACA affects the likelihood of attending school. Using these estimates, DACA moved 50,000 to 75,000 unauthorized immigrants into employment. If the effects of Deferred Action for Parents of Americans and Lawful Permanent Residents (DAPA) are similar to DACA, then DAPA could potentially move over 250,000 unauthorized immigrants into employment.



## 1. Introduction

The United States has the largest immigrant population of any nation in the world. With 40.7 million people,[1] the United States has four times as many foreign-born residents than any other country.[2] However, in the United States, 11.4 million of these individuals, or 3.6% of the entire US population, are unauthorized immigrants and have no legal status (Baker and Rytina, 2013). These unauthorized immigrants face a unique set of challenges to their economic well-being compared to citizens and authorized immigrants. Some of these challenges include the threat of deportation, lack of legal work authorization, and insufficient documentation for banking, loans, and driver's licenses. These challenges likely contribute to unauthorized immigrants' below-average levels of income, educational attainment, and above-average levels of unemployment (Fortuny et al., 2007; Rivera-Batiz, 1999; and Smith, 2006).

Due to the unique challenges unauthorized immigrants face, extensive political debate has occurred over what immigration policies should be implemented to help improve unauthorized immigrants' economic well-being without incentivizing additional illegal immigration. On June 15, 2012, President Obama used his prosecutorial discretion and announced Deferred Action for Childhood Arrivals (DACA). This announcement directed the Department of Homeland Security to accept applications for DACA from unauthorized immigrants who had arrived in the United States as children (under the age of 16) and were under the age of 31 as of June 15, 2012. Individuals whose applications are accepted receive two years of deportation relief and work authorization. Continued DACA approval is conditional on renewal every two years. With 1.7 million unauthorized immigrants potentially eligible (Passel and Lopez, 2012), DACA has provided relief from deportation and work authorization to more unauthorized immigrants than any other immigration policy since the 1986 Immigration Reform and Control Act (Baker, 2014).

Without work authorization, documentation for loans and driver's licenses, and with the possibility of deportation, unauthorized immigrants have additional labor market frictions than do authorized immigrants and citizens. DACA-eligible unauthorized immigrants could potentially reduce these labor market frictions and improve their labor market outcomes by applying for and obtaining DACA.

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[1] Census Bureau.
[2] United Nations, Department of Economic and Social Affairs, Population Division

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0047-2727/

Particularly, I use three different measurements of employment to look at how the reduction of labor market frictions through DACA has affected unauthorized immigrants likelihood of working. I estimate whether these changes in the likelihood of working stem from changes in labor force participation or unemployment. Importantly, I estimate how these changes in working affect the income of unauthorized immigrants throughout the income distribution. Lastly, I look at whether DACA affects schooling decisions through its substitutability with working.

In this paper, I look at how DACA affects DACA-eligible immigrants' labor market outcomes through the reduction of labor market frictions. To do so, I use American Community Survey (ACS) data on over 400,000 immigrants and 5 million citizens ages 18–35 from 2005 to 2014. I estimate the effect of DACA by using a difference-in-differences empirical design. To enhance validity, I estimate the effect of DACA by performing the difference-in-differences estimation for samples of unauthorized immigrants who are just above and below DACA eligibility cutoffs. Specifically, I look at unauthorized immigrants who were just above and below the age of 16 when they entered the United States and those who were just above and below the age of 30 on June 15, 2012. In addition, I test for selection into the ACS sample of unauthorized immigrants and for differential pre-trends that may bias the results.

I find DACA has had large effects on DACA-eligible individuals' labor market outcomes, and find suggestive evidence for some schooling decisions. For DACA-eligible individuals, DACA has increased the likelihood of working by 3.7–4.8 percentage points and the number of hours worked per week by 0.9–1.7 hours. The increase in the likelihood of working and in the number of hours worked per week comes from both an increase in labor force participation and a decrease in unemployment. These estimates provide a lower bound on the intent-to-treat effect of DACA which may be as much as 1.6 times larger. In addition, the increased likelihood of working has increased the income for those in the bottom of the income distribution. Despite the increased employment, I find little evidence that DACA has influenced the likelihood of an individual being self-employed. Within two years of implementation, DACA moved 50,000–75,000 unauthorized immigrants into employment.

Since one of the requirements for obtaining DACA is to have a high school diploma or a General Educational Development (GED) certificate, I also test whether DACA had affected unauthorized immigrants' educational attainment. I find suggestive evidence that DACA pushed over 25,000 DACA-eligible individuals into obtaining their GED certificate in order to be eligible for DACA. Although working and attending school are likely substitutes, and DACA has had a positive effect on the likelihood of working, I find little evidence that DACA has affected the likelihood of attending school.

The difference-in-differences results directly answer the policy question of how DACA has affected its target population. The results also inform future immigration policies on how a reduction in labor market frictions through deferred action and work authorization might affect the larger unauthorized immigrant population. Particularly, the findings shed light on how the Deferred Action for Parents of Americans and Lawful Permanent Residents (DAPA) policy, which expands DACA and gives deferred action and work authorizations to most unauthorized immigrants who have children that are citizens, might affect the 3.7 million eligible unauthorized immigrants.[3] If the effects of DAPA are similar to the effects of DACA, then DAPA could move over 250,000 unauthorized immigrants into employment. However, due to the demographic differences between the DAPA-eligible and DACA-eligible populations, the effects of the two policies may not be similar. The results demonstrate that illegal status hurts young immigrants' ability to work, by keeping them out of the labor force and unemployed. Even in a short two-year time span, deferred action and work authorization helped young unauthorized immigrants find employment.

Due to the recency of DACA and data limitations, little work has looked at how DACA affects unauthorized immigrants. A few studies using small sets of survey data have provided suggestive evidence of an increase in job changes, employment, and decreases in school attendance (Gonzales et al., 2014; and Kosnac et al., 2014). However, these studies only have a few DACA-eligible individuals in their sample and are mostly descriptive studies that lack causal identification. In concurrent work, Amuedo-Dorantes and Antman (2016) use monthly Current Population Survey data along with a difference-in-differences strategy to look at the effect of DACA. Although they effectively use their difference-in-differences strategy, their analysis is limited by a small sample size of 11,526 non-citizens of which only a small fraction are eligible for DACA after its availability (400–450 individuals). They find DACA reduces school enrollment for these 450 DACA-eligible individuals, and provide some evidence of an increase in the likelihood of working for men. The limited sample size prevents them from looking at labor market outcomes with enough precision to detect sizable changes. This paper uses over 400,000 non-citizens and over 5 million citizens to estimate the effect of DACA on labor market and schooling outcomes. Similar to Amuedo-Dorantes and Antman (2016), I find positive effects of DACA on employment for men. I also find positive effects of DACA on employment for women. In addition, I find beneficial effects of DACA on labor force participation, unemployment, and number of hours worked per week. I also find increases in income for those in the bottom of the income distribution. However, in contrast to their results, I find no evidence of an effect of DACA on school attendance. This difference in the effect on school enrollment may be due to sampling error from their small sample size or because of strong differential pre-trends in school attendance that are observed. In addition, this paper provides a detailed analysis of the effect of DACA by income quantile, uses citizens as an additional control group, and provides tests for sample selection that may potentially bias the results.

This paper is also closely related to work done on the 1986 Immigration Reform and Control Act (IRCA). The IRCA granted amnesty and a pathway to citizenship to approximately 2.8 million unauthorized immigrants (Baker, 2014). Most studies have found the IRCA increased unauthorized immigrants' incomes (Bratsberg et al., 2002; Kossoudji and Cobb-Clark, 2002, Orrenius and Zavodny, 2012; and Rivera-Batiz, 1999), decreased crime rates (Baker, 2014), increased educational attainment (Cortes, 2013), and had little effect on long-term patterns of undocumented immigration (Orrenius and Zavodny, 2003). However, Amuedo-Dorantes and Bansak (2011) and Amuedo–Dorantes et al. (2007) have also found that unauthorized immigrants' labor force participation decreased and unemployment rates rose. In addition to the IRCA, Kaushal (2006) found that the 1997 amnesty program, the Nicaraguan Adjustment and Central American Relief Act (NACARA), increased the real wages of undocumented foreign-born men from affected countries by 3%.

Although both are major immigration policies, the IRCA and DACA differ in many ways that may cause them to affect unauthorized immigrants differently. The largest difference is that the IRCA gave amnesty and a pathway to citizenship, whereas DACA gives only two years of deportation relief and work authorization. The IRCA was also implemented when fewer legal barriers to employing unauthorized immigrants existed. Lastly, the two policies are more than 25 years apart with different labor markets. NACARA was implemented on a much smaller scale than either the IRCA or DACA and was implemented over 15 years ago.

The rest of the paper will proceed as follows. Section 2 describes the timing, benefits, and eligibility criteria of DACA. Section 3

[3] http://migrationpolicy.org/news/mpi-many-37-million-unauthorized-immigrants-could-get-relief-deportation-under-anticipated-new

describes the ACS data. Section 4 develops a conceptual framework for interpreting the results. Section 5 describes the difference-in-differences methodology and the samples used for the analysis. Section 6 reports the results of how DACA affects eligible unauthorized immigrants. Section 7 discusses how the results can help inform current and future immigration policy. Section 8 concludes.

## 2. Deferred Action for Childhood Arrivals

On June 15, 2012, from the Rose Garden, President Obama used his prosecutorial discretion and announced Deferred Action for Childhood Arrivals (DACA). This announcement directed the Department of Homeland Security to accept applications for DACA from qualified unauthorized immigrants. Individuals whose applications are accepted receive deferred action, which gives them two years of relief from deportation and work authorization. Continued DACA eligibility is conditional on renewal every two years.

After the announcement of DACA in June 2012, the Department of Homeland Security's Citizenship and Immigration Services (USCIS) started accepting applications for DACA on August 15, 2012. To apply for DACA, individuals have to fill out three forms, pay a processing fee of 465 dollars, and provide documentation that they meet the eligibility criteria. Although many forms and documentation are required, over 90% of processed applications are approved. The USCIS estimated applications would take 4–6 months to be processed. By the end of 2012, over 100,000 unauthorized immigrants' DACA applications had been approved. By the end of 2013 and 2014, over 500,000 and 600,000 DACA applications had been approved, respectively. Fig. 1 uses data reported by the USCIS[4] and shows the number of DACA applications approved over time. The black line represents the number of DACA applications approved in each quarter of the year. The gray bars represent the cumulative number of DACA applications approved. As Fig. 1 shows, very few DACA applications were approved until the last quarter of 2012, and the bulk of DACA applications were approved over the span of a year, from October, 2012 to September, 2013.

To qualify for DACA, unauthorized immigrants have to meet six criteria[5]: (1) applicants had no lawful status as of June 15, 2012 (i.e., an unauthorized immigrant as of June 15, 2012); (2) applicants came to the United States before the age of 16; (3) applicants must have been under the age of 31 as of June 15, 2012; (4) applicants must also have continuously resided in the United States since June 15, 2007; (5) applicants must be currently in school, have graduated or obtained a certificate of completion from high school, have obtained a General Education Development (GED) certificate, or be an honorably discharged veteran of the Coast Guard or Armed Forces of the United States; (6) applicants cannot have been convicted of a felony, significant misdemeanor, or three or more other misdemeanors. In addition to these DACA qualification criteria, an individual must be 15 years or older to submit the DACA application. To prove they meet these requirements, individuals must submit documentation from a list of approved sources given by the USCIS. For example, passports or birth certificates from an individual's country of origin are required to prove an individual's age, and school or medical records are used to prove an individual came to the United States before the age of 16.

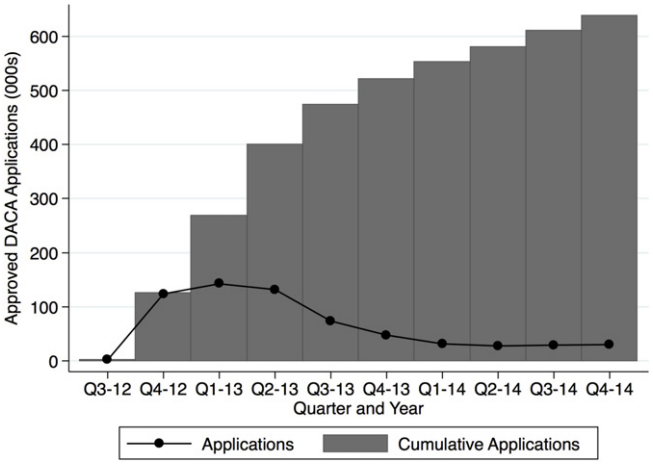


**Fig. 1.** Number of approved DACA applications by quarter. Note: The black line represents the number of DACA applications approved in a given quarter. The bars represent the cumulative number of DACA applications approved by a given quarter. The y-axis shows the number of approved applications in thousands. The x-axis shows the quarter and year.

To better understand these criteria, a breakdown of the United States' unauthorized immigrant population is helpful. As of January 2012, the Department of Homeland Security estimated 11.4 million unauthorized immigrants were living in the United States (Baker and Rytina, 2013). Of these, 80% were from Central and South America and 59% were from Mexico. Approximately 4.4 million of the 11.4 million unauthorized immigrants were under the age of 31 as of June 15, 2012. Of these 4.4 million, approximately 950,000 were immediately eligible for DACA (Passel and Lopez, 2012). In addition, approximately 770,000 were potentially eligible in the future. Of these 770,000 individuals, 450,000 met all the qualification criteria but were currently under the age of 15. The other approximately 320,000 individuals met all the qualification criteria but had no high school diploma or GED certificate. Although individuals have to pay money, they may be wary of future deportation from applying, and must obtain substantial documentation, by the end of 2014, 67% of the 950,000 individuals immediately eligible for DACA had been approved.[6] The composition of DACA-approved individuals' nationality was somewhat similar to that of the unauthorized immigrant population as a whole, with 92% from Central and South America and 78% from Mexico.

Clearly, the reason for so many individuals willing to take the time and money to apply for DACA is the perceived benefits from DACA approval. The two most obvious, and likely the largest, benefits of DACA approval are relief from deportation and work authorization. Individuals with DACA receive deferred action in which all removal actions are deferred and individuals are authorized to be present in the United States. Along with this deferred action, DACA recipients are legally allowed to work in the United States. Many smaller benefits accompany these two main benefits. DACA recipients receive a Social Security Number, which allows them to legally open a bank account and build a credit history. In all states, except Arizona and Nebraska, DACA recipients can legally obtain a driver's license.[7] However, DACA recipients are not eligible for federal welfare or federal student aid.

[4] http://www.uscis.gov/tools/reports-studies/immigration-forms-data/data-set-deferred-action-childhood-arrivals

[5] http://www.uscis.gov/humanitarian/consideration-deferred-action-childhood-arrivals-daca

[6] http://www.uscis.gov/tools/reports-studies/immigration-forms-data/data-set-deferred-action-childhood-arrivals

[7] http://www.nilc.org/dacadriverslicenses2.html

## 3. Data

The main data used to look at the effects of DACA are individual-level data from the American Community Survey (ACS). I use ACS data from 2005 to 2014. I start with the 2005 ACS sample because it is the first year with a full one-percent sample of the United States. The 2014 ACS sample is the most recent sample available. The ACS data provide eight years of data prior to DACA and two years after. The collection of ACS data in each year is evenly distributed between each month of the year. The ACS provides many outcomes of interest including if individuals are working, in the labor force, unemployed, self-employed, their income, number of hours worked per week, whether they obtained a GED, and whether they are in school. It also provides a rich set of demographic information on individuals to be used as controls.

The ACS includes questions that allow me to focus on the unauthorized immigrant population and determine if individuals are DACA eligible. The most difficult DACA qualification criteria to identify in the ACS is whether the individual is an unauthorized immigrant. The ACS asks each individual if they are a US citizen. No additional information on legal status is available if the individual is a non-citizen. The Census Bureau and the Department of Homeland Security estimate that nearly 40% of these non-citizens are authorized immigrants (Acosta et al., 2014, Baker and Rytina, 2013). Although the variable non-citizen includes all unauthorized immigrants, it also includes many authorized immigrants. In the Empirical method section, I will discuss how this inclusion of authorized immigrants causes the analysis to underestimate the intent-to-treat effect of DACA.

These ACS data also include questions that allow me to identify individuals who meet the other DACA qualification criteria. The ACS question on quarter of birth allows me to determine the age of each individual as of June 30, 2012, and whether they are under the age of 31. Using the question on how long the individual has resided in the United States, along with their age, I determine the age at which each individual entered the United States. This question also allows me to identify if the individual has been in the United States for at least five years. Using the ACS question on education, I can determine individuals' educational obtainment. To limit the sample to only individuals who meet DACA's education requirement, I restrict my sample to only individuals who have a high school degree. Lastly, whether an individual has committed a felony or significant misdemeanor is not observed. I create the variable "eligible" for whether an individual meets all of the DACA qualifications as of June 15, in the year prior to the individual's ACS sample year (except for having committed a crime or not).

To better understand how unauthorized immigrants are included in the ACS data, the sampling process for the ACS is as follows. First, the Census Bureau uses its Master Address File, which is an inventory of all known housing units and group quarters, as the sample frame from which the Census Bureau draws its sample for the ACS. The Census Bureau estimates that from 2005 to 2014, the Master Address File covers the housing for 92.5–94.0% of the entire US population. Each month, a systematic sample of addresses is drawn from the Master Address File to represent each US county. The ACS survey is then mailed to the selected sample at the beginning of the month. Non-respondents are then contacted by telephone one month later for a computer-assisted telephone interview. One third of the nonrespondents to the mail or telephone survey are then contacted in person to complete the ACS survey one month following the telephone survey attempt. The Census Bureau reports that from 2005 to 2014, 65.5–68.7% of the addresses selected for the sample completed the survey. Of those contacted in person, 96.7–98.0% completed the survey.

In addition to the details of the ACS sampling procedure, understanding how the sampling and interview process relate to being an unauthorized immigrant is important. In regards to the ACS and unauthorized immigrants, the Census Bureau states, "The ACS interviews the resident population without regard to legal status or citizenship."[8] The fact that the ACS conducts interviews without regard to legal status can be more easily seen as the sampling and interview process is broken down. First, because the sample frame is created by using the near universe of US addresses, unauthorized immigrants are no more or less likely to be selected into the sample frame than are authorized immigrants or citizens. Second, because a systematic sample of address are drawn from the sample frame, unauthorized immigrants are no more or less likely to be selected to be sent the ACS survey. Therefore, the ACS sampling does not select a specific type of unauthorized immigrant to be included in the ACS, but is representative of the unauthorized immigrant population in the United States. The ACS sampling procedure supports the assertion that the estimates from this paper are informative about DACA-eligible unauthorized immigrants as a whole. Also, because the sampling procedure did not change between 2005 and 2014, and unauthorized immigrants were sampled in the same way before and after DACA became available, the selection of unauthorized immigrants into the sample will not affect the results. Although the ACS did not sample a specific type of unauthorized immigrant or change its sampling procedure in such a way to detrimentally affect the results, potential concerns arise regarding how the survey and item response rates of unauthorized immigrants may affect the results. I discuss these concerns in the Results section along with tests to determine their potential influence on the results.

Using the ACS data, I analyze four main types of labor market and schooling outcomes. The first outcome is the likelihood of an individual to be working. The ACS provided three survey questions that help measure this outcome. They are a binary variable for whether an individual worked in the last week and in the last year, and a continuous variable for the usual number of hours worked each week. All three of these outcome variables provide insight into whether a person is working. The second type of outcome comprises three outcome variables that help describe the underlying reason for why a person is working or not. These three outcome variables are a binary variable for whether an individual is in the labor force or not, whether unemployed or not, and whether self-employed or not. These three outcome variables help break down how DACA is affecting the likelihood of working. The third type of outcome is an individual's income. The ACS income variable measures the total amount of income an individual receives from all sources in the last 12 months. This outcome variable is used to help determine if DACA improves recipients' economic well-being and stability. The last type of outcome comprises two variables that help describe the schooling choices of individuals. The outcome variables used are whether an individual is attending school and whether the individual has obtained a GED. I use the binary outcome variable of whether an individual is attending school, because working and attending school are likely substitutes for each other and DACA may have unattended effects on an individual's likelihood of attending school. I use the binary outcome variable of having obtained a GED, because a requirement for obtaining DACA is to have a high school diploma or a GED certificate, and DACA may therefore incentivize some unauthorized immigrants to obtain their GED. The exact wording from the ACS survey for each outcome and control variable is shown in the web appendix.

Table 1 shows the summary statistics for the sample of non-citizens ages 18–35 with at least a high school degree from 2005 to 2014. The first two columns show the summary statistics for the DACA-eligible and DACA-ineligible individuals, respectively. The third column shows the difference between the two groups' means

[8] https://www.census.gov/history/pdf/acsdesign-methodology2014.pdf

**Table 1**
Summary statistics.

| Variable | Mean | | Difference | *t*-Statistic |
|---|---|---|---|---|
| | DACA Eligible | DACA Ineligible | | |
| Working | 65.3 | 66.5 | −1.2 | −3.3 |
| In labor force | 73.9 | 71.9 | 2.0 | 6.5 |
| Unemployed | 11.7 | 7.5 | 4.1 | 16.2 |
| Income | 15,787 | 24,358 | −8,571 | −31.8 |
| Hours worked per week | 27.1 | 28.6 | −1.5 | −9.1 |
| Worked in past year | 75.1 | 73.9 | 1.2 | 3.5 |
| Self-employed | 4.7 | 6.6 | −1.9 | −14.0 |
| Attending school | 32.0 | 21.6 | 10.4 | 28.1 |
| GED | 3.9 | 2.5 | 1.4 | 11.1 |
| Years in US | 15.5 | 6.4 | 9.1 | 144.4 |
| Age entered US | 8.4 | 22.3 | −13.8 | −287.3 |
| Male | 52.6 | 51.9 | 0.7 | 3.1 |
| White | 75.1 | 59.0 | 16.1 | 16.3 |
| Black | 9.3 | 9.0 | 0.2 | 0.4 |
| Asian | 14.5 | 30.7 | −16.2 | −27.5 |
| Hispanic ethnicity | 65.4 | 42.1 | 23.2 | 20.8 |
| Home language of Spanish | 63.6 | 41.3 | 22.3 | 20.3 |
| Born in Latin America | 72.4 | 47.4 | 24.9 | 30.9 |
| Age | 23.9 | 28.6 | −4.7 | −148.7 |
| Married | 24.0 | 51.4 | −27.4 | −88.6 |
| Live in a metro area | 92.4 | 92.9 | −0.4 | −1.9 |
| High school degree | 49.8 | 37.2 | 12.6 | 30.3 |
| Some college | 40.2 | 25.4 | 14.7 | 43.2 |
| College degree | 10.0 | 37.3 | −27.3 | −65.4 |
| Observations | 99,844 | 338,866 | | |

Note: The sample for the summary statistics includes non-citizens who are ages 18–35 and have at least a high school degree and corresponds to the sample in Panel C of Table 2. All binary variables are represented in percent terms.

and the fourth columns shows the *t*-statistic when testing the difference between the two means. The clearest differences between the two groups are that the DACA-eligible group tends to have entered the United States at a younger age and to be younger. In addition to the difference between the DACA-eligible and DACA-ineligible groups that can be seen in Table 1, both groups are more likely to be Hispanic, speak Spanish at home, live in a metro area, and have only a high school degree, compared to citizens of the same age (see Table A.3). In addition, both groups are about 6 percentage points less likely than citizens to be in the labor force or to be working. Although DACA-eligible individuals' incomes are much lower than citizens, DACA-ineligible individuals' incomes are similar to citizens.

## 4. Conceptual framework

In this section, I will look at the reasons why obtaining DACA may potentially affect the labor market and schooling outcomes of unauthorized immigrants and the potential consequences of these effects. First, I look at why obtaining DACA potentially affects unauthorized immigrants' labor market outcomes. Initially, DACA itself did not change the labor demand or the labor supply curves. DACA did not change employers' desire to hire a worker at a given wage. In addition, DACA did not change individuals' (citizens, authorized immigrants, and unauthorized immigrants) willingness to work at a given wage. However, DACA did reduce the frictions for DACA-eligible unauthorized immigrants to find employment, by providing work authorization, legal documentation for banking and driver's licenses, and removing potential deportation if discovered working illegally. These attenuations in frictions mainly arose from that fact that unauthorized immigrants who obtained DACA could now obtain employment from all potential employers instead of just employers who were willing to overlook individuals' legal work status. These attenuations in frictions allowed those who obtained DACA to have fewer barriers to working and to have more employment options. As such, one would expect DACA to increase individuals' likelihood of working. This increase in the likelihood of working could arise from both discouraged workers entering the labor force and unemployed unauthorized immigrants finding employment. This increase in working should in turn increase DACA-eligible unauthorized immigrants' income. This increase in income should be particularly pronounced for those in the bottom of the income distribution due to not being able to previously find steady employment. The results of this paper will test if these reductions in labor market frictions for DACA-eligible unauthorized immigrants allow them to improved their labor market outcomes by being more likely to work, less likely to be unemployed, and by increasing their income.

Note that although DACA itself does not change the labor supply curve and instead attenuates labor-market frictions for DACA-eligible individuals, if these frictions are attenuated and those who obtain DACA increase their likelihood of working, the supply of labor will in turn increase. This increase in the supply of labor could potentially have a negative effect on overall wages. The results indicate DACA moved approximately 50,000–75,000 unauthorized immigrants into employment. This change in the supply of labor accounts for only 0.94–1.41% of the 5.33 million individuals who gained employment in 2013 and 2014 (Bureau of Labor Statistics). This finding implies that the effect of DACA on overall wages would likely be very small and would be unable to be detected in these data. However, with the much larger population that would be affected by DAPA or a large-scale amnesty program, the increase in the supply of labor may be a larger concern.

These attenuations of labor market frictions and the subsequent increase in the supply of labor have potential welfare implications. By reducing labor market frictions for unauthorized immigrants and therefore increasing their employment options, DACA clearly increases the welfare of DACA-eligible individuals. However, the increase in labor supply and the potential decrease in overall wages, may lead to a decrease in the welfare of citizens and authorized immigrants. However, the welfare increases and decreases are not likely to be symmetric. Because DACA relieves large frictions for DACA-eligible unauthorized immigrants, the resulting increase in employment is likely for individuals who are not at the margin of being willing to work, but rather are well within the margin of being willing to work. Conversely, if the increased supply of labor from DACA-eligible individuals displaces workers, these displaced workers are likely to be just at the margin of being willing to work. This would imply that the overall welfare effect is not a pure transfer to DACA-eligible individuals, but would likely enhance efficiency, although it would not be Pareto efficient.

In addition to labor market outcomes, I look at why obtaining DACA may potentially affect individuals' schooling outcomes. First, I look at the potential effect of DACA on attending school, and then the potential effect on obtaining a GED. The additional options from obtaining DACA may have a direct positive effect on the likelihood of attending schooling through legal documentation that gives immigrants access to loans to pay for tuition, the ability to obtain a driver's license so they can attend school while still living with their parents, or the ability to work while attending school to cover their tuition and living expenses. In addition, by obtaining assurance through DACA of being able to legally work in the future, DACA-eligible individuals may be more willing to invest in their human capital. However, besides these potential positive effects of DACA, work authorization has an indirect negative effect on school attendance. Working and attending school (particularly attending full time) are likely substitutes for each other. Once DACA-eligible individuals obtain DACA and can more easily find employment, they may substitute their time away from attending school and toward working. Second, because one of the requirements for obtaining DACA is to have a high school diploma or a GED certificate, DACA may directly incentivize unauthorized immigrants who do not have a high school

diploma or GED, but are otherwise eligible for DACA, to obtain a GED so they can reap the potential benefits of DACA. The results look at whether DACA affects both the likelihood of attending school and of obtaining a GED.

## 5. Empirical method

To measure the effect of DACA, I use a difference-in-differences (DID) approach. By comparing DACA-eligible individuals with DACA-ineligible individuals before and after the implementation of DACA, I can measure its effect. The simplest approach to test if DACA has an effect on DACA-eligible individuals is by comparing the outcome means of individuals eligible for DACA with those ineligible both before and after DACA became available. Figs. 2 through 5 show these simple mean comparisons between non-citizens ages 18–35 with at least a high school degree from 2005 to 2014. With DACA only being available at the end of 2012, I should only observe its effect for the years 2013 and 2014, with possibly a small effect in 2012. I discuss the results shown in these figures in detail in the Results section.

As mentioned earlier, one of the limitations with the ACS data is the inability to distinguish between unauthorized and authorized non-citizens. According to the Census Bureau (Acosta et al., 2014), the ACS estimates that there were 8.3 million non-citizens in the US between the ages of 18 and 35 in 2012. The Department of Homeland Security (Baker and Rytina, 2013) estimates that of these 8.3 million non-citizens, 38.9% were authorized immigrants and 61.1% were unauthorized immigrants. If the sample were restricted to just unauthorized immigrants, the DID estimates would be the intent-to-treat effect. However, due to this contamination of authorized immigrants in the non-citizen sample, the DID estimates are not be the intent-to-treat effect. Instead, the DID estimates will be systematically biased toward zero and will underestimate the intent-to-treat effect. With nearly 40% of the non-citizen sample being authorized immigrants, the intent-to-treat effect of DACA will be approximately 1.6 times larger than the estimates from the DID estimation. When the estimation is performed on subsamples of the data that tend to have a higher percentage of unauthorized immigrants, such as low-income and low-education subsamples (Passel and Cohn, 2009), the DID estimates are larger. However, these larger estimates may also be because unauthorized immigrants in these subsamples benefit more from obtaining DACA. Similarly, sampling error that incorrectly specifies the DACA-eligible variable would also bias the estimates toward zero. The DID estimates will provide a lower bound for the intent-to-treat effects of DACA. In addition, because only 67% of DACA-eligible individuals obtained approval, the treatment on the treated effects could potentially be as much as 1.5 times larger than the intent-to-treat effects. However, any treatment on the treated effect derived from the DID estimates could be biased by selection into who applies for DACA.

The main analysis for this paper simultaneously uses a DID approach along with some regression discontinuity design elements. I will use the DID approach on samples with individuals just above and below different DACA qualification criteria. The main model is as follows:

$$Y_{it} = \beta_0 + \beta_1 Eligible_{it} * After_{it} + \beta_2 Eligible_{it} + \beta_3 After_{it} + \beta_4 X_{it} + \beta_5 W_{it} + \theta_t + \gamma_s + \gamma_s t + \varepsilon_{it} \quad (1)$$

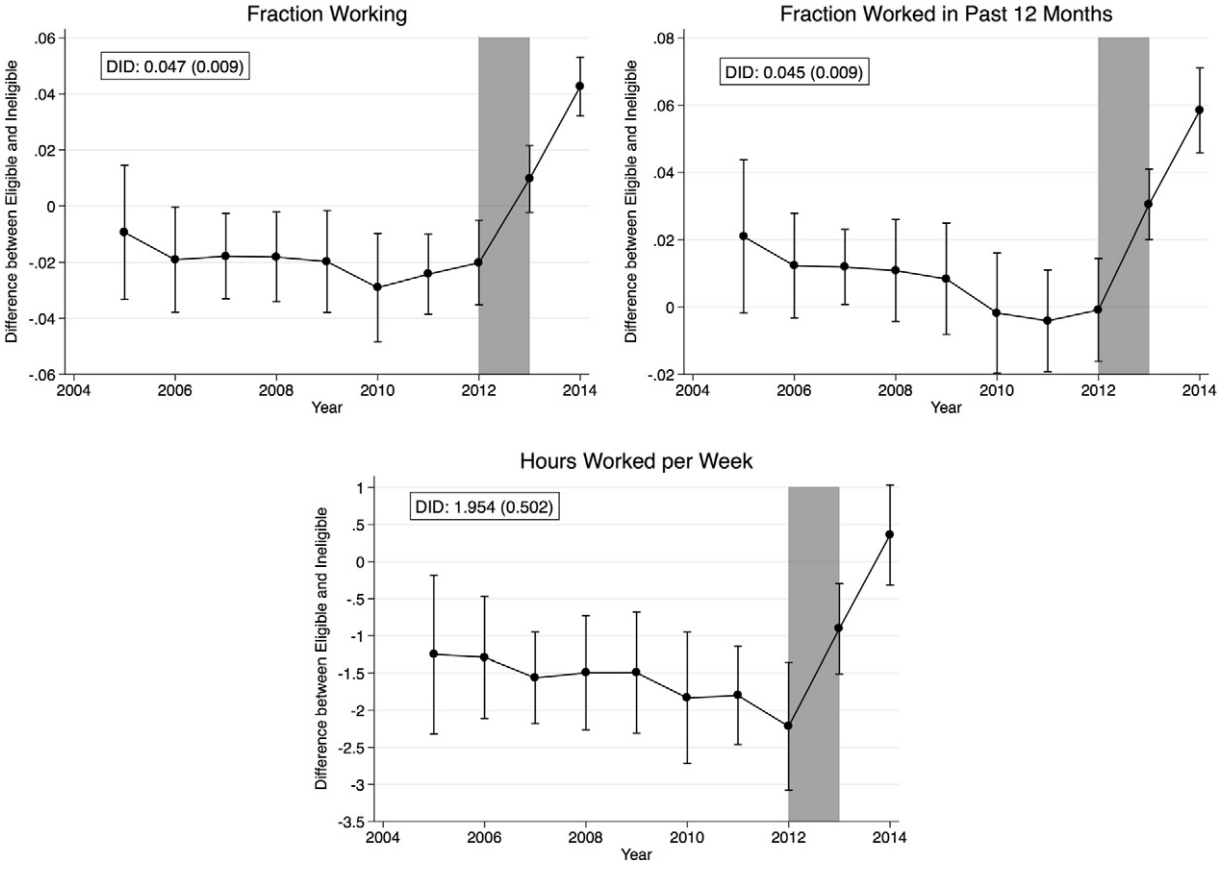


**Fig. 2.** Difference in working by DACA eligibility. Note: Each figure shows the mean difference of the given variable between DACA-eligible and DACA-ineligible individuals for each year from 2005 to 2014. The sample is the same as Panel C of Table 2 and includes all non-citizens with at least a high school degree and who are between the ages of 18 and 35. DID estimates without controls that account for pre-trends are shown in the box. The shaded area between 2012 and 2013 represents when DACA became available.

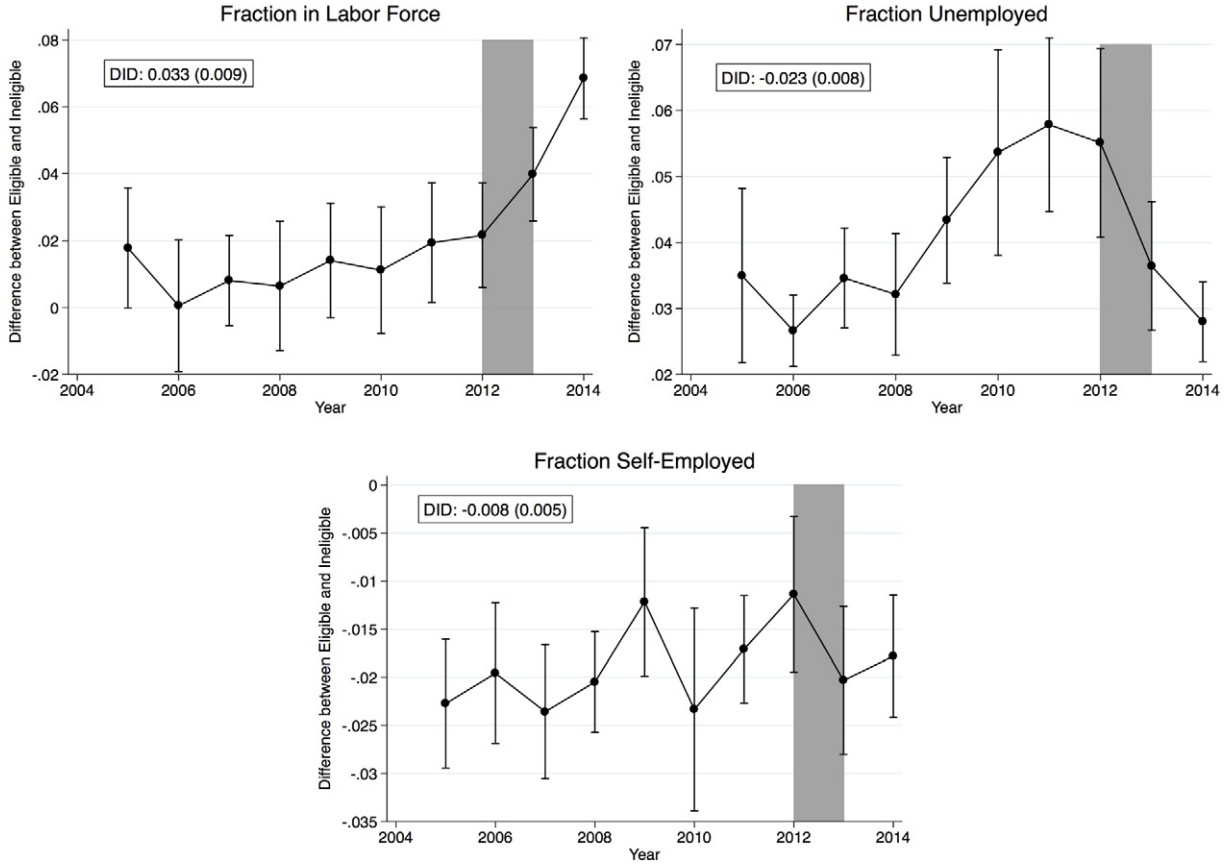


**Fig. 3.** Difference in employment status by DACA eligibility. Note: Each figure shows the mean difference of the given variable between DACA-eligible and DACA-ineligible individuals for each year from 2005 to 2014. The sample is the same as Panel C of Table 2 and includes all non-citizens with at least a high school degree and who are between the ages of 18 and 35. DID estimates without controls that account for pre-trends are shown in the box. The shaded area between 2012 and 2013 represents when DACA became available.

where $Y_{it}$ is the outcome variable of interest (e.g., working, unemployed, in school, etc.) for individual $i$ in year $t$. The variable $Eligible_{it}$ is a binary variable equal to one if individual $i$ is eligible for DACA, and zero if ineligible. The creation of this variable was described in the Data section. The variable $After_{it}$ is a binary variable equal to one if it is after DACA became available, and equal to zero if before. Since the ACS only reports the year in which the interview is performed, I use the cutoff between 2012 and 2013 as the threshold for when DACA became available. Therefore, $After_{it}$ is equal to one if the year is 2013 or 2014 and zero if the year is from 2005 to 2012. The parameter of interest, $\beta_1$, is the coefficient on the interaction term between $Eligible_{it}$ and $After_{it}$. The vector $X_{it}$ contains demographic controls including years of education, sex, race, ethnicity, marital status, and state-level unemployment rates. The vector $W_{it}$ non-parametrically controls for the eligibility criteria by including fixed effects for individual $i$'s age and age when arrived in the United States. The vectors $\theta_t$ and $\gamma_s$ allow for time and state fixed effects, respectively. Lastly, $\gamma_s t$ allows for state-specific time trends. When estimating Eq. (1), standard errors are clustered at the state-year level.

I estimate this DID model on four different samples. The first sample includes all non-citizens ages 18–30 with at least a high school degree who entered the United States between the ages of 12 and 19. The cutoff for DACA eligibility was entering the United States before the age of 16. This sample allows a DID estimate to be obtained for individuals near the DACA age cutoff for entering the United States (four years above and below the cutoff) and therefore uses the variation in eligibility due to when an individual entered the United States. The second sample includes all non-citizens ages 27–34 with at least a high school degree who entered the United States before the age of 16. Because the cutoff for DACA eligibility was being under the age of 31, this sample captures the variation in eligibility due to the age criterion. Third, the DID model is estimated without any regression discontinuity element. This last sample includes all non-citizens ages 18–35 with at least a high school degree and therefore uses all sources of variation in DACA eligibility. This sample is more akin to a typical DID estimation that relies heavily on the pre-trends of the two groups being similar, although the two groups may not be similar. The last sample the DID model is estimated on includes all citizens and non-citizens ages 18–35 with at least a high school degree.

The parameter of interest in all specifications is the coefficient on the interaction term between $Eligible_{it}$ and $After_{it}$. This coefficient estimates the change in the outcome variable for individuals eligible for DACA after DACA became available compared to those ineligible for DACA. The main assumption that must hold in order for the estimates to be unbiased is that the DACA-eligible and -ineligible groups have parallel trends, and the parallel trends would have continued in the absence of DACA. To support the assumption of parallel trends, I test for pre-existing trends. In addition to the test for pre-existing trends, the trends can be seen in Figs. 2 through 5. Lastly, using the two samples of individuals just above and below the DACA criteria cutoffs, near the age of 30 and near the age of 16 when entering the United States, increases the likelihood that the eligible and ineligible groups are similar and have parallel trends.

One potential concern for the interpretation of the DID estimates is that some unauthorized immigrants may not have the required

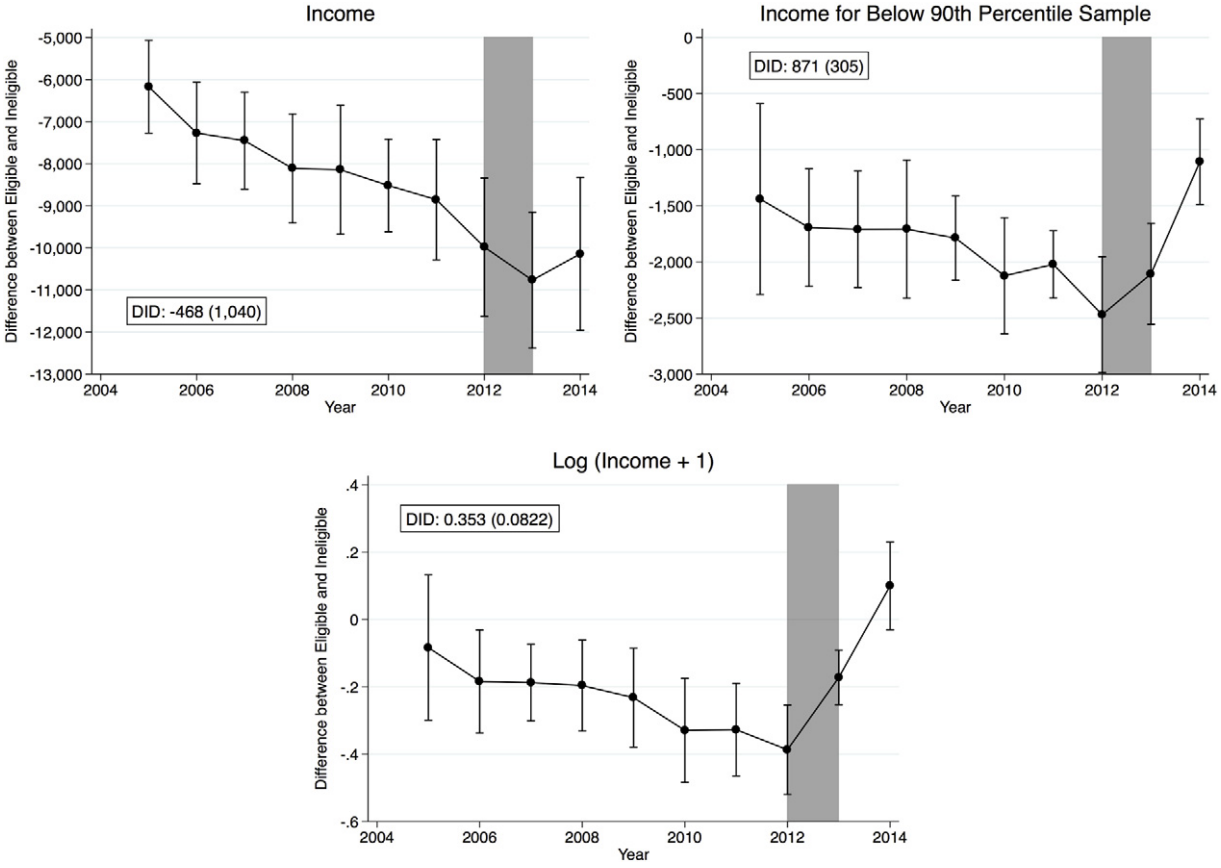


**Fig. 4.** Difference in income by DACA eligibility. Note: Each figure shows the mean difference of the given variable between DACA-eligible and DACA-ineligible individuals for each year from 2005 to 2014. The sample is the same as Panel C of Table 2 and includes all non-citizens with at least a high school degree and who are between the ages of 18 and 35. The first figure includes all individuals in the sample. The second figure restricts the sample to individuals with income below the 90th percentile. The third figure uses the log of income plus one as the outcome variable instead of linear income. DID estimates without controls that account for pre-trends are shown in the box. The shaded area between 2012 and 2013 represents when DACA became available.

money or may worry that obtaining DACA will increase the future likelihood of being deported, and therefore unauthorized immigrants who obtain DACA may be a specifically selected type of unauthorized immigrant. Because a large portion (67%) of DACA-eligible individuals applied and obtained DACA, this concern is somewhat limited; however, the two thirds who obtain DACA may still be substantially different from the one third who did not. This concern will not affect the main policy implications of the results, because this concern does not affect the DID estimates. The DID estimates will still estimate the effect of how DACA affected DACA-eligible unauthorized immigrants regardless of whether they obtained DACA, and still be a lower bound on the intent-to-treat effect. However, this concern could potentially bias any treatment on the treated effects derived from these DID estimates. If a program with permanent deportation relief and

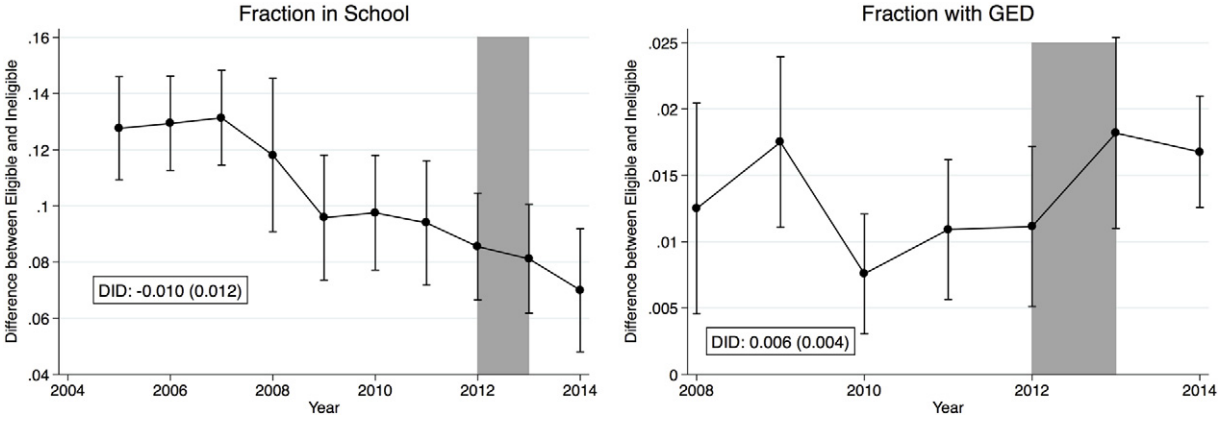


**Fig. 5.** Difference in education outcomes by DACA eligibility. Note: Each figure shows the mean difference of the given variable between DACA-eligible and DACA-ineligible individuals for each year from 2005 to 2014. The sample is the same as Panel C of Table 2 and includes all non-citizens with at least a high school degree and who are between the ages of 18 and 35. DID estimates without controls that account for pre-trends are shown in the box. The shaded area between 2012 and 2013 represents when DACA became available.

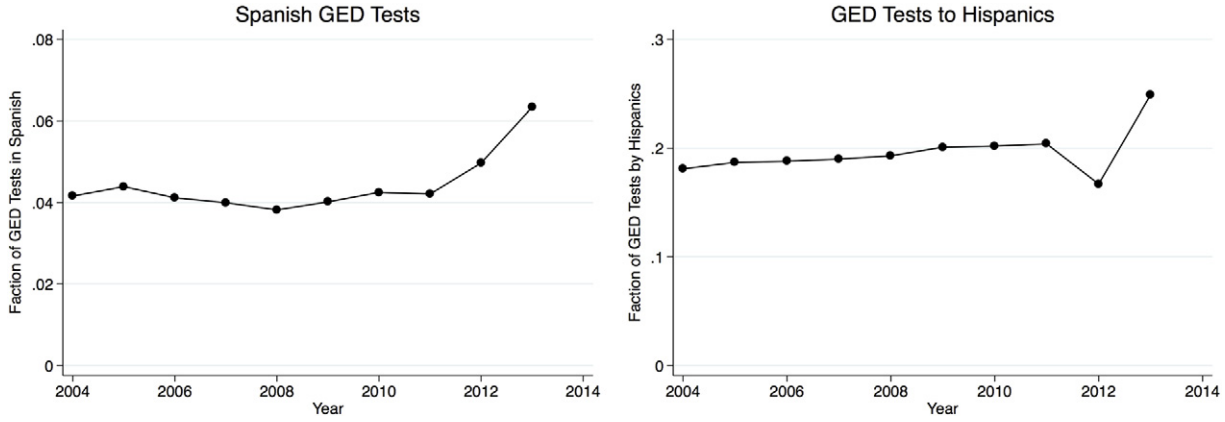


**Fig. 6.** Fraction of GED tests administered in Spanish and to Hispanics. Note: The first graph shows the fraction of GED tests administered in Spanish for each year. The second graph shows the fraction of GED tests administered to Hispanics for each year.

work authorization were implemented such that immigrants were not concerned about future deportation and a higher percentage of eligible unauthorized immigrants who applied for and obtained deportation relief, then the treatment on the treated effects from such a program may be larger or smaller.

## 6. Results

### 6.1. Graphical results

In this section, I compare the outcome means of individuals eligible for DACA with those ineligible both before and after DACA became available. Each point in Figs. 2 through 5 shows the difference in the mean for individuals eligible for DACA and those ineligible. The simple DID estimates without controls that account for differential pre-trends are reported in each graph. The sample includes all non-citizens ages 18–35 with at least a high school degree (for alternative samples, see Figs. A.1–A.12). Because DACA became available at the end of 2012, its effects should be observed in the years 2013 and 2014. The effects are likely to be larger in 2014 than 2013 because a large portion of individuals would not have received approval before being surveyed in 2013.

The first graph in Fig. 2 shows the difference in means for the fraction of individuals working. This graphs shows similar pre-trends from 2005 to 2012, with the difference in means remaining relatively stable. However, once DACA became available, this difference increased by 6.3 percentage points from 2012 to 2014. The results for the fraction who worked in the past 12 months and the usual number of hours worked per week are similar and show substantial increases in employment for those eligible for DACA compared to those ineligible once DACA became available.

This effect of DACA on employment can come from either changes in labor force participation or unemployment. The results for the fraction in the labor force in Fig. 3 are similar to the results from the fraction working with staple pre-trends followed by increases in labor force participation in 2013 and 2014. The pre-trends in being unemployed are less similar, but there are still significant drops in the difference in unemployment in 2013 and 2014, respectively. The fraction of individuals self-employed shows little evidence that DACA had an impact on the likelihood of being self-employed.

The first graph in Fig. 4 shows the difference in income between DACA-eligible and -ineligible individuals. A strong differential pre-trend in income appears for the two groups prior to the availability of DACA. The difference in the mean income declines from 2005 to 2013, followed by a small uptick in 2014 that is not statistically significant. Part of this slow response for income may occur because the ACS measures income by asking individuals their income over the past 12 months. Because DACA recipients in 2013 could only have had their DACA approval for at most a year (and likely much less) and because many DACA recipients in 2013 would have been interviewed before obtaining DACA approval, this income measure may underestimate the effect of DACA on income. However, the strong differential pre-trends appear to be driven by the top 10% of the income distribution. The second graph restricts the sample to the bottom 90% of the income distribution, and the pre-trends for the two groups are much more stable. For the bottom 90% in the income distribution, there is a 1,364 dollar increase in income for DACA-eligible individuals compared to those ineligible between 2012 and 2014. Also, although an imperfect measure, the third graph shows the difference in the log of income plus one and finds large increases in log income once DACA became available.

In Fig. 5, the pre-trends for the fraction in school are different for the two groups. There is also no clear change in the mean difference when DACA becomes available. The fraction with a GED shows the mean difference in the fraction of individuals with a GED. The ACS first started asking this question in 2008, so the graph only covers the years 2008–2014. There is no clear change in the mean difference in GED obtainment once DACA became available.

To better look at the effect of DACA on GED attainment, I also use annual data from the GED Testing Service[9] on the fraction of GED tests that were taken in Spanish and by Hispanics each year (Fig. 6). Data for the GED end in 2013 because the 2002 Series GED Test expired at the end of 2013. From 2004 to 2011, the fraction of GED tests administered in Spanish ranged from 3.8% to 4.4%. However, from 2011 to 2013, the fraction of GED tests administered in Spanish increased 2.1 percentage points. This increase implies that over 13,000 more individuals took the GED test in Spanish in 2013 than in 2011. In Panel B, the fraction of GED tests that were administered to Hispanics from 2004 to 2011 monotonically increases from 18.1 to 20.4%. However, contrary to what would be predicted, there is a substantial drop in 2012 to 16.7%. Then, as predicted, there is a large increase in 2013 to 24.9%. The 4.5-percentage-point increase from 2011 to 2013 is the equivalent to an additional 27,000 Hispanics taking the GED test in 2013 as compared to 2011. However, this result should be interpreted cautiously because of the dip that occurs in 2012 and the null effect in the ACS data.

[9] http://www.gedtestingservice.com/educators/historical-testing-data

### 6.2. Difference-in-differences results

Moving from the simple approach to a more sophisticated approach, I estimate Eq. (1). I perform this estimation separately for the four different samples described in the Empirical method section. Table 2 reports the estimates from Eq. (1) for each of the four samples. Panel A of Table 2 reports the estimates from the sample that includes all non-citizens ages 18–30 with at least a high school degree who entered the United States between the ages of 12 and 19. This sample performs a DID estimation on individuals near the DACA-criteria cutoff for the age at which individuals entered the United States. The column headers indicate the outcome variables of interest that were described in the Data section. The first row reports the coefficient on the interaction term between $Eligible_{it}$ and $After_{it}$. The second row reports the coefficient on $Eligible_{it}$.

Column 1 indicates that non-citizens eligible for DACA are 4.8 percentage points more likely to be working than non-citizens ineligible for DACA after DACA became available. In other words, DACA increases the likelihood of working by 4.8 percentage points for non-citizens who meet the DACA requirements. With a base of 65% of DACA-eligible individuals working, the estimate implies DACA increases the likelihood of a DACA-eligible individual working by 7.3%. Both of these estimates are lower bounds on the intent-to-treat effect. Because approximately 40% of the non-citizen sample are authorized immigrants, the intent-to-treat effects are likely 1.6 times larger than DID estimates. Therefore the intent-to-treat effect of DACA on the likelihood of working could be as large as 7.7 percentage points, or 11.7%.

The increase in the likelihood of working can come from two different sources. The first source is individuals entering the labor force. The second is individuals moving from unemployment to employment. Columns 2 and 3 look at these two different sources separately. Column 2 shows DACA increases the likelihood of a DACA-eligible individual being in the labor force by 3.7 percentage points. Column 3 shows that for DACA-eligible individuals, DACA decreases the likelihood of being unemployed by 1.9 percentage points. From these estimates, DACA appears to move eligible individuals into the labor force and move them from unemployment to employment.

Column 4 shows that although DACA increases the likelihood of working, there is little evidence that it had an effect on DACA-eligible individuals' income for the sample as a whole. However, as was seen in Fig. 4, DACA appears to have an effect on income for individuals in the bottom of the income distribution or when less weight is placed on the upper tail of the income distribution. As such, I estimate the DID estimates using quantile regressions over the income distribution. Fig. 7 shows the results of this quantile regression for both income and the log of income plus one. Because just over 25% of individuals have zero income, estimates are not available for the lower quarter of the income distribution. As can be seen, DACA appears to have increased the income of those between the 30th and 60th percentile by 400–800 dollars or about 5–20%. DACA appears to have had little effect on those in the top of income distribution.

Columns 5 and 6 use two different measures for working. Column 5 indicates DACA-eligible individuals work 1.7 hours more per week than DACA-ineligible individuals after DACA became available. This increase can also be thought of as one additional full-time job per 23 DACA-eligible individuals. Column 6 indicates that DACA-eligible individuals are 3.9 percentage points more likely to have worked in the past 12 months. Column 7 tests whether DACA approval moves individuals from self-employment to the formal labor market. I find no statistically significant affect of DACA on self-employment.

Columns 8 and 9 look at academic attainment. Column 8 finds DACA decreases the likelihood of attending school for DACA-eligible individuals by 2.1 percentage points. This effect is statistically significant; however, the effect is likely biased due to the differential pre-trends in schooling between the DACA-eligible and -ineligible groups that can be seen in Fig. 2. Once I test for pre-trends in Table 3, the effect on schooling is indistinguishable from zero. Column 9 looks at the effect of DACA on the likelihood of having attained a GED certificate. DACA does not appear to have an effect on the number of individuals that have attained their GED.

Panels B, C, and D of Table 2 show analogous results for three additional samples. Panel B shows the results for non-citizens with at least a high school degree who are ages 27–34 and entered the United States before the age of 16. This sample includes individuals who are just above and below the age cutoff for DACA eligibility (must be under 31) but meet all other DACA eligibility requirements. The results for the individuals near the age cutoff are quite similar to the results found in Panel A. The notable difference in Panel B is that there is no statistically significant effect on schooling. Panel C includes all non-citizens with at least a high school degree who are ages 18–35. Instead of using variation in DACA eligibility that comes from being just above or below a DACA criteria cutoff along with the DID methodology as does Panels A and B, this sample only uses the DID methodology. Panel D includes all citizens and non-citizens with at least a high school degree. The results from this sample are qualitatively the same; however, the estimates tend to be smaller. These smaller estimates are likely due to the stronger differential pre-trends for this sample. Once these differential pre-trends are accounted for, the estimates for this sample are similar in magnitude to those of the first three samples (see Table A.6).

### 6.3. Potential concerns

A major concern about the empirical method used is the possibility of differential trends in the outcome variables for DACA-eligible and -ineligible individuals. Fig. 2 looks at this assumption graphically, but further analysis is performed in Table 3. Table 3 estimates Eq. (1) with the variable $Eligible_{it}$ interacted with a binary variable for each year. The interaction with the 2012 binary variable is the omitted interaction. If differential trends are a problem, the coefficients on the interaction terms leading up to 2013 and 2014 should be statistically significant and in the same direction as the coefficients on the 2013 and 2014 interaction terms. When testing for pre-trends, statistically significant effects remain for working, labor force, unemployment, hours worked per week, and worked in the past year. These point estimates vary in magnitude compared to the estimates in Table 2 but tend to be qualitatively similar. School attendance is no longer affected, likely due to the clear pre-trends for school attendance. However, there does not appear to be clear pre-trends for the other outcomes. Analogous to Table 3, Tables A.4, A.5, and A.6 show the pre-trends results for the samples in Panels B, C, and D of Table 2, respectively. Because 2012 was an election year in which immigration policy was a major topic of conversation, unauthorized immigrants may have been concerned about possible future immigration policies and therefore underinvested in work and education in 2012. If this were true, then the effects could be biased when they are compared to the omitted 2012 interaction. Tables A.18–A.21 show the pre-trends when the 2011 interaction is the omitted interaction. The effects are very similar regardless of which year is used as the omitted interaction.

Another potential concern for the identification strategy is the possibility that individuals change how they respond to the ACS after they receive DACA. Unauthorized immigrants may be more willing to answer (or truthfully answer) the citizenship question once they have obtained DACA. Also, because working without proper documentation is illegal, undocumented workers may be hesitant to respond to questions about employment. Once they receive legal status and work authorization through DACA, they may change their survey-response behavior and be more likely to respond to citizenship and employment questions. Thus, instead of DACA actually increasing recipients' likelihood of working, it may just increase

**Table 2**
The effect of DACA on labor market and education outcomes.

| Variables | Working | Labor force | Unemployed | Income | Hours per week | Worked in past year | Self-employed | School | GED |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: entered US between ages 12 and 19* | | | | | | | | | |
| Eligible*After | 0.048*** | 0.037*** | −0.019*** | −1 | 1.715*** | 0.039*** | −0.005 | −0.021** | 0.001 |
| | [0.010] | [0.011] | [0.007] | [466] | [0.420] | [0.011] | [0.005] | [0.009] | [0.004 ] |
| Eligible | −0.027** | −0.042*** | −0.014 | −447 | −0.378 | −0.033*** | 0.001 | −0.046*** | 0.007* |
| | [0.011] | [0.012] | [0.011] | [279] | [0.374] | [0.011] | [0.006] | [0.010] | [0.004] |
| Observations | 102,765 | 102,765 | 68,831 | 102,765 | 102,765 | 102,765 | 79,454 | 102,765 | 72,117 |
| R-squared | 0.145 | 0.150 | 0.032 | 0.199 | 0.224 | 0.137 | 0.017 | 0.413 | 0.042 |
| *Panel B: ages 27 to 34 in June 2012 and entered US before age 16* | | | | | | | | | |
| Eligible*After | 0.044*** | 0.028** | −0.022*** | 1,397 | 1.184** | 0.027** | 0.017* | 0.002 | 0.009 |
| | [ 0.013] | [0.012] | [0.008] | [904] | [0.486] | [0.011] | [0.010] | [0.010] | [0.010] |
| Eligible | 0.008 | 0.007 | −0.003 | 214 | 0.366 | 0.014 | −0.007 | −0.015 | 0.010 |
| | [0.018] | [0.016] | [0.011 ] | [1,047] | [0.620] | [0.015] | [0.012] | [0.010] | [0.011 ] |
| Observations | 33,236 | 33,236 | 26,796 | 33,236 | 33,236 | 33,236 | 29,819 | 33,236 | 23,939 |
| R-squared | 0.059 | 0.067 | 0.027 | 0.143 | 0.110 | 0.073 | 0.018 | 0.079 | 0.078 |
| *Panel C: all non-citizens ages 18 to 35 with at least a high school degree* | | | | | | | | | |
| Eligible*After | 0.037*** | 0.033*** | −0.010** | −1,045 | 0.931*** | 0.030*** | −0.002 | −0.005 | 0.003 |
| | [ 0.006 ] | [0.005] | [0.004] | [672] | [0.317] | [0.006] | [0.003] | [0.006] | [0.002] |
| Eligible | 0.069*** | 0.065*** | −0.013*** | 5,801*** | 2.414*** | 0.060*** | 0.006* | −0.028*** | −0.004 |
| | [0.006] | [0.005] | [0.004] | [437] | [0.228] | [0.005] | [0.004] | [0.005] | [0.003] |
| Observations | 438,710 | 438,710 | 308,368 | 438,710 | 438,710 | 438,710 | 355,205 | 438,710 | 306,442 |
| R-squared | 0.130 | 0.131 | 0.032 | 0.220 | 0.187 | 0.127 | 0.022 | 0.297 | 0.052 |
| *Panel D: all citizens and non-citizens ages 18 to 35 with at least a high school degree* | | | | | | | | | |
| Eligible*After | 0.021*** | 0.017*** | −0.009*** | −529 | 0.600** | 0.025*** | 0.002 | 0.017*** | 0.007*** |
| | [0.005] | [0.004] | [0.004] | [490] | [0.256] | [0.006] | [0.003] | [0.005] | [0.002] |
| Eligible | −0.006** | −0.012*** | -0.006*** | 163 | −0.344*** | −0.030*** | 0.013*** | −0.023*** | −0.022*** |
| | [0.003] | [0.003] | [0.002] | [159] | [0.110] | [0.003] | [0.002] | [0.002] | [0.001] |
| Observations | 5,636,126 | 5,636,126 | 4,411,763 | 5,636,126 | 5,636,126 | 5,636,126 | 5,111,496 | 5,636,126 | 4,048,401 |
| R-squared | 0.084 | 0.069 | 0.045 | 0.248 | 0.161 | 0.059 | 0.015 | 0.322 | 0.116 |

Note: Table 2 reports the estimates from Eq. (1) for four separate samples. All four samples are restricted to individuals with at least a high school degree. Panel A uses the sample of non-citizens who entered the United States between the ages of 12 and 19. Panel B uses the sample of non-citizens between the ages of 27 and 34 who arrived in the United States before the age of 16. Panel C uses the sample of non-citizens between the ages of 18 and 35. Panel D uses the sample of citizens and non-citizens between the ages of 18 and 35. Each column indicates the outcome variable of interest. The first row of each panel reports the estimated coefficient on the interaction term. The second row reports the estimated coefficient on the DACA-eligibility variable. Standard errors are clustered at the state-year level. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

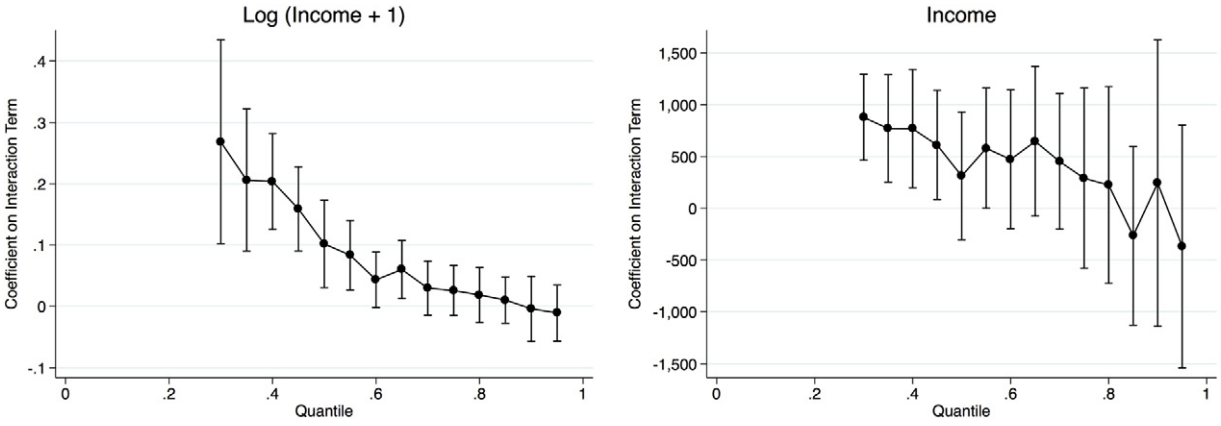


**Fig. 7.** Effect of DACA on income by income quantile. Note: Each point represents the coefficient on the interaction term between DACA eligibility and the binary variable for after DACA became available when a quantile regression is estimated using the specification from Eq. (1). No estimates are available prior to the 0.30 quantile because 25 to 30% of individuals in the sample have an income of zero. The sample is the same as Panel A of Table 2. The outcome variable for the first graph is log(*Income* + 1). The outcome variable for the second graph is income.

recipients' likelihood of reporting on the ACS survey that they worked. Using the quality flags in the ACS, I am able to test if the availability of DACA changes the likelihood of DACA-eligible individuals responding to particular questions. To do so, I use the DID estimation shown in Eq. (1) with the outcome variable, $Y_{it}$, as an indicator variable equal to one if the survey question for the outcome variable of interest was not answered and the outcome variable of interest was imputed by the ACS. The indicator variable is equal to zero otherwise. Table 4 reports the coefficients on the interaction term and on the DACA-eligible variable for the estimation performed. If the availability of DACA increases the likelihood of DACA-eligible immigrants responding to an ACS survey question, the coefficient on the interaction term should be negative and statistically significant. As Table 4 shows, for all of the outcome survey question, the coefficient on the interaction term is close to zero and none are statistically significant. This finding would indicate that DACA-eligible immigrants did not change their survey response behavior very much after DACA became available and is likely not driving the positive effects on employment. Analogous to Table 4, Tables A.8, A.9, and A.10 show the results for the samples in Panels B, C, and D of Table 2, respectively.

One potential concern is that although all unauthorized immigrants are equally likely to be sampled, unauthorized immigrants that complete the ACS may be different from those who do not complete the ACS, and DACA affects the type of unauthorized immigrants who complete the ACS differently than those who do not complete the ACS. Although the ACS does not ask about an individual's legal status, some unauthorized immigrants might be more wary of completing a government survey. Alternatively, because completing the ACS is required by law, some unauthorized immigrants may be more inclined to fill out the ACS. If the group that is more inclined to answer the survey is also affected differently by DACA, the generalizability of the results may be affected. This concern will not affect the internal validity of the results, but may limit how much the results can be generalized to the DACA-eligible population as a whole. This is a possible concern because from 2005 to 2014, 65.5–68.7% of the

**Table 3**
Pre-trends.

| Variables | Working | Labor force | Unemployed | Income | Hours per week | Worked in past year | Self-employed | School | GED |
|---|---|---|---|---|---|---|---|---|---|
| Eligible*2014 | 0.056*** | 0.041** | −0.021* | 281 | 2.378*** | 0.041** | 0.005 | 0.005 | 0.006 |
| | [0.018] | [0.017] | [0.011] | [902] | [0.715] | [0.019] | [0.009] | [0.013] | [0.007] |
| Eligible*2013 | 0.033** | 0.002 | −0.040*** | −318 | 0.964 | 0.020 | −0.008 | 0.012 | 0.003 |
| | [0.016] | [0.018] | [0.012] | [838] | [0.757] | [0.017] | [0.009] | [0.015] | [0.006] |
| Eligible*2011 | −0.011 | −0.008 | 0.008 | −439 | −0.655 | −0.022 | 0.000 | 0.026** | 0.001 |
| | [0.015] | [0.015] | [0.012] | [757] | [0.626] | [0.014] | [0.008] | [0.012] | [0.006] |
| Eligible*2010 | 0.002 | −0.011 | −0.014 | −49 | 0.333 | 0.003 | 0.004 | 0.029 | 0.001 |
| | [0.015] | [0.017] | [0.015] | [711] | [0.650] | [0.014] | [0.008] | [0.018] | [0.006] |
| Eligible*2009 | 0.011 | 0.005 | −0.006 | 478 | 0.949 | 0.006 | 0.012 | 0.024* | 0.010 |
| | [0.017] | [0.016] | [0.012] | [833] | [0.675] | [0.017] | [0.009] | [0.013] | [0.006] |
| Eligible*2008 | 0.014 | −0.014 | −0.030*** | 392 | 0.588 | 0.002 | 0.000 | 0.020 | 0.006 |
| | [0.013] | [0.013] | [0.011] | [793] | [0.633] | [0.014] | [0.008] | [0.014] | [0.007] |
| Eligible*2007 | −0.002 | −0.015 | −0.010 | −101 | −0.713 | −0.015 | −0.005 | 0.040*** | – |
| | [0.014] | [0.014] | [0.012] | [730] | [0.601] | [0.014] | [0.009] | [0.014] | – |
| Eligible*2006 | −0.027 | −0.054*** | −0.022** | −981 | −0.744 | −0.032* | 0.005 | 0.051*** | – |
| | [0.016] | [0.016] | [0.010] | [805] | [0.671] | [0.016] | [0.008] | [0.012] | – |
| Eligible*2005 | −0.015 | −0.035** | −0.016 | 675 | −0.077 | −0.006 | 0.011 | 0.043** | – |
| | [0.018] | [0.016] | [0.013] | [730] | [0.688] | [0.018] | [0.009] | [0.019] | – |
| Observations | 102,765 | 102,765 | 68,831 | 102,765 | 102,765 | 102,765 | 79,454 | 102,765 | 72,117 |
| R-squared | 0.145 | 0.151 | 0.033 | 0.200 | 0.224 | 0.138 | 0.017 | 0.414 | 0.042 |

Note: Table 3 reports the estimates from Eq. (1) with *Eligible* interacted with each year. The 2012 interaction is the omitted interaction. The estimation uses the sample from Panel A of Table 2. Each column indicates the outcome variable of interest. Each row reports the estimated coefficient on the given interaction term. Standard errors are clustered at the state-year level. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

**Table 4**
The effect of DACA on survey-item response rates.

| Variables | Citizen | Working | Labor force | Unemployed | Income | Hours per week | Worked in past year | Self-employed | School | GED |
|---|---|---|---|---|---|---|---|---|---|---|
| Eligible*After | −0.002 | −0.002 | 0.004 | −0.007 | 0.005 | 0.007 | −0.002 | 0.004 | 0.001 | 0.002 |
| | [0.005] | [0.006] | [0.006] | [0.006] | [0.009] | [0.006] | [0.005] | [0.005] | [0.005] | [0.008] |
| Eligible | 0.014** | 0.010* | 0.007 | 0.023*** | 0.006 | 0.005 | 0.010* | 0.009 | 0.002 | 0.002 |
| | [0.005] | [0.005] | [0.007] | [0.006] | [0.007] | [0.006] | [0.006] | [0.006] | [0.004] | [0.009] |
| Observations | 102,765 | 102,765 | 102,765 | 68,831 | 102,765 | 102,765 | 102,765 | 102,765 | 102,765 | 72,117 |
| R-squared | 0.014 | 0.018 | 0.012 | 0.017 | 0.613 | 0.013 | 0.016 | 0.010 | 0.013 | 0.039 |

Note: Table 4 reports the estimates from Eq. (1) with the outcome variable, $Y_{it}$, being an indicator variable equal to one if the survey question for the outcome variable of interest was not answered and the outcome variable of interest was imputed by the ACS. The indicator variable is equal to zero otherwise. The estimation uses the sample from Panel A of Table 2. Each column indicates the outcome variable of interest. The first row of each panel reports the estimated coefficient on the interaction term and the second row reports the estimated coefficient on the DACA-eligibility variable. Standard errors are clustered at the state-year level. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

addresses that were sent the ACS survey completed it. However, for the one third of the nonrespondents that were randomly assigned to be contacted in person, 96.7–98.0% completed the survey. Because nearly all households complete the ACS survey if selected to be contacted in person, little to no selectivity of individuals into this subsample will take place based on their willingness to fill out the ACS survey. I estimate the main results using this subsample and report the results in Table A.7. The results from this subsample, in which selection on the willingness to complete the ACS is minimal to nonexistent, are very similar to the main results. This finding implies that either no selection occurs in the willingness of unauthorized immigrants to complete the ACS, or DACA does not differentially affect this type of unauthorized immigrants. Either way, this finding suggests that the results from the ACS are likely generalizable to the population of DACA-eligible unauthorized immigrants as a whole.

An additional potential concern for the identification strategy is the possibility that individuals' likelihood of completing the ACS changes after they receive DACA. If unauthorized immigrants' willingness to complete the ACS changes once they received DACA, the composition of individuals in the DACA-eligible group might change and bias the results. Similar to the previous concern, the results from Table A.7 show that when the sample is restricted to a subsample for which the survey-completion rate is over 95% and therefore there is little room for a compositional change to those included in the DACA-eligible group, the estimates for the effect of DACA are very similar. This finding implies that little change occurs in the composition of the DACA-eligible group and little to no bias to the results. In addition, I test whether the observable characteristics for the DACA-eligible group change once DACA became available in 2013. Due to strong pre-trends in observable characteristics, I use the same specification as the pre-trend tables without controls to test whether 15 different observable characteristics changed between 2012 and 2013. Table A.11 reports the coefficient on the interaction term between 2013 and being DACA eligible, with the 2012 interaction omitted. Of the 60 different coefficients estimated for the 15 observable characteristics and the 4 different samples, only 7 of the 60 coefficients are significant at the 90% confidence level. This test indicates that there is little evidence of a change in the observable characteristics of the DACA-eligible group after DACA became available. This test implies that receiving DACA did not change individuals' willingness to complete the ACS or the composition of the DACA-eligible group. Therefore, this concern would likely have little to no effect on the results.

Lastly, a potential concern is that DACA recipients are switching from informal to formal jobs and the estimates are measuring this switching and not actual labor market effects. One benefit of the ACS data is that the main question used to determine whether someone is working includes work that was formal or informal. The wording of the question is as follows: "LAST WEEK, did this person do ANY work for pay." This wording allows both formal and informal work to be included, and therefore the effects of DACA can be seen on all types of work. The questions about whether an individual worked last year and the hours worked per week also ask about all work, whether formal or informal. In addition, the ACS also determines whether a person is self-employed. This question allows me to look at the effect of DACA on any potential movement from self-employment (more likely an informal job) to working for someone else (more likely a formal job). The results show little evidence of an effect on changes in self-employment. The ACS also asks for the occupation of individuals who have worked in the past five years. Using this variable, I estimate the main results for more formal occupations (e.g., teacher, software developer, retail clerk, etc.) and more informal occupations (e.g., cook, waitress, landscaper, child care worker, etc.). Due to the omission of individuals who may start working for the first time in five years because of obtaining work authorization through DACA, the estimates from this heterogeneity test will likely be biased. However, if the estimates for individuals with a formal occupation are similar to those with an informal occupation, individuals just switching from informal to formal jobs are unlikely to be driving the main estimates. Tables A.22–A.25 report this heterogeneity test for each of the samples. The results show little evidence of a difference between the estimates for formal and informal occupations. The results of this test imply that the main results are likely not being driven by individuals just switching from informal to formal jobs.

### 6.4. Subsample and robustness results

Tables 5–8 use the sample and specification from Panel A of Table 2 to look at how the results differ for different subsamples of income, ethnicity, gender, and education. These results look to see if particular subsamples are more or less affected by DACA, and look at how sensitive the results are to the exclusion of particular groups of individuals. Tables A.12, A.13, and A.14 show the subsample results for the samples in Panels B, C, and D of Table 2, respectively.

Table 5 shows the results for individuals below the median income, above the median income, and below the 90th percentile of the sample. Two notable differences exist between the subsamples. First, for individuals below the median and below the 90th percentile, the beneficial effects of DACA are larger. The main effects on working, labor force participation, and unemployment are about twice as large as those for the above-the-median-income subsample. Second, for individuals below the median income, DACA increased the income of DACA-eligible individuals by a statistically significant 339 dollars.

**Table 5**
The effect of DACA by income level.

| Variables | Working | Labor force | Unemployed | Income | Hours per week | Worked in past year | Self-employed | School | GED |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: below median income* | | | | | | | | | |
| Eligible*After | 0.063*** | 0.050*** | −0.049** | 339*** | 1.891*** | 0.049*** | −0.022** | −0.036*** | 0.000 |
| | [0.015] | [0.017] | [0.020] | [111] | [0.471] | [0.017] | [0.009] | [0.013] | [0.005] |
| Eligible | −0.024* | −0.031** | 0.001 | −275*** | −0.190 | −0.036*** | 0.006 | −0.067*** | 0.010** |
| | [0.013] | [0.014] | [0.019] | [76] | [0.403] | [0.014] | [0.009] | [0.012] | [0.004] |
| Observations | 54,417 | 54,417 | 23,315 | 54,417 | 54,417 | 54,417 | 31,892 | 54,417 | 39,413 |
| R-squared | 0.044 | 0.061 | 0.035 | 0.077 | 0.079 | 0.058 | 0.035 | 0.468 | 0.047 |
| *Panel B: above median income* | | | | | | | | | |
| Eligible*After | 0.024*** | 0.017** | −0.008 | −1,086 | 1.065** | 0.020*** | 0.004 | −0.005 | 0.002 |
| | [0.009] | [0.007] | [0.005] | [850] | [0.471] | [0.007] | [0.007] | [0.010] | [0.005] |
| Eligible | −0.055*** | −0.076*** | −0.018* | −784 | −2.006*** | −0.054*** | −0.005 | 0.006 | 0.002 |
| | [0.016] | [0.013] | [0.011] | [654] | [0.581] | [0.012] | [0.009] | [0.016] | [0.014] |
| Observations | 48,348 | 48,348 | 45,516 | 48,348 | 48,348 | 48,348 | 47,562 | 48,348 | 32,704 |
| R-squared | 0.041 | 0.060 | 0.010 | 0.160 | 0.093 | 0.074 | 0.018 | 0.233 | 0.043 |
| *Panel C: below 90th percentile income* | | | | | | | | | |
| Eligible*After | 0.050*** | 0.038*** | −0.021*** | 343 | 1.777*** | 0.041*** | −0.006 | −0.025*** | −0.001 |
| | [0.011] | [0.011] | [0.007] | [260] | [0.426] | [0.012] | [0.005] | [0.010] | [0.004] |
| Eligible | −0.024** | −0.039*** | −0.015 | 84 | −0.266 | −0.031*** | 0.001 | −0.050*** | 0.008** |
| | [0.011] | [0.012] | [0.011] | [185] | [0.371] | [0.011] | [0.006] | [0.010] | [0.004] |
| Observations | 96,111 | 96,111 | 62,453 | 96,111 | 96,111 | 96,111 | 72,898 | 96,111 | 67,419 |
| R-squared | 0.137 | 0.145 | 0.031 | 0.235 | 0.214 | 0.131 | 0.016 | 0.428 | 0.041 |

Note: Table 5 reports the estimates from Eq. (1) for individuals below the median income level (Panel A), above the median income level (Panel B), and below the 90th percentile in income (Panel C). The estimation uses the sample from Panel A of Table 2. Each column indicates the outcome variable of interest. The first row of each panel reports the estimated coefficient on the interaction term and the second row reports the estimated coefficient on the DACA-eligibility variable. Standard errors are clustered at the state-year level. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table 6 shows the results for the subsample of individuals who identify as Hispanic and for the subsample of Mexicans. The results are fairly similar in both magnitude and sign to the main results in Table 2. This similarity is important, because approximately 78% of DACA recipients were Mexican. They indicate a positive effect of DACA on working, labor force, hours worked per week, and worked in the last year for both Hispanics and Mexicans. The results also show a negative effect on unemployment.

Table 7 shows the results separately by gender. I find no statistically significant differences in the coefficients between men and women. The coefficients are both of similar magnitude and sign to the main results in Table 2. Table 8 shows the results separately by education level. The results in Panel A are for individuals with a high school degree or some college, and are similar to the main results found in Table 2. The results in Panel B are for individuals with a college degree or more, and are similar in sign yet smaller in magnitude. Due to a much smaller sample size and much larger standard errors, these results are at most marginally significant.

In addition to the sensitivity analysis performed by looking at different subsamples, Table 9 looks at the robustness of the results to different choices of specification. Each row represents a different specification or sample and each cell in the row is the coefficient on the interaction term along with its standard error. Panel A of Table 9 estimates Eq. (1) with varying levels of controls. Row 1 contains no controls and only includes an indicator for DACA eligibility, an indicator for if the year is after DACA was available, and the interaction of the two. For most of the estimates, the magnitudes are approximately 50% larger than the baseline results from Table 2. However, for schooling, the magnitude is four times larger. Row

**Table 6**
The effect of DACA by ethnicity.

| Variables | Working | Labor force | Unemployed | Income | Hours per week | Worked in past year | Self-employed | School | GED |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Hispanic* | | | | | | | | | |
| Eligible*After | 0.044*** | 0.032*** | −0.020** | 82 | 1.939*** | 0.039*** | −0.008 | −0.023** | −0.002 |
| | [0.012] | [0.010] | [0.009] | [528] | [0.455] | [0.010] | [0.007] | [0.009] | [0.005] |
| Eligible | −0.010 | −0.015 | −0.005 | −109 | −0.113 | −0.020 | 0.002 | −0.062*** | 0.006 |
| | [0.017] | [0.016] | [0.015] | [326] | [0.550] | [0.015] | [0.008] | [0.015] | [0.007] |
| Observations | 52,285 | 52,285 | 39,065 | 52,285 | 52,285 | 52,285 | 43,294 | 52,285 | 35,296 |
| R-squared | 0.145 | 0.149 | 0.030 | 0.190 | 0.209 | 0.158 | 0.014 | 0.271 | 0.033 |
| *Panel B: Mexican* | | | | | | | | | |
| Eligible*After | 0.046*** | 0.040*** | −0.012 | 0 | 1.785*** | 0.041*** | −0.016* | −0.022* | −0.001 |
| | [0.014] | [0.012] | [0.009] | [501] | [0.588] | [0.012] | [0.009] | [0.013] | [0.007] |
| Eligible | 0.011 | 0.010 | −0.005 | −162 | 0.676 | −0.002 | 0.001 | −0.065*** | 0.007 |
| | [0.021] | [0.019] | [0.018] | [476] | [0.745] | [0.019] | [0.010] | [0.018] | [0.010] |
| Observations | 34,072 | 34,072 | 25,374 | 34,072 | 34,072 | 34,072 | 28,174 | 34,072 | 22,375 |
| R-squared | 0.187 | 0.198 | 0.033 | 0.206 | 0.250 | 0.208 | 0.015 | 0.215 | 0.033 |

Note: Table 6 reports the estimates from Eq. (1) for Hispanics (Panel A) and Mexicans (Panel B). The estimation uses the sample from Panel A of Table 2. Each column indicates the outcome variable of interest. The first row of each panel reports the estimated coefficient on the interaction term and the second row reports the estimated coefficient on the DACA-eligibility variable. Standard errors are clustered at the state-year level. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

**Table 7**
The effect of DACA by gender.

| Variables | Working | Labor force | Unemployed | Income | Hours per week | Worked in past year | Self-employed | School | GED |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: women* | | | | | | | | | |
| Eligible*After | 0.041*** | 0.037*** | −0.013 | 198 | 1.487*** | 0.038*** | −0.003 | −0.012 | 0.000 |
| | [0.013] | [0.013] | [0.012] | [504] | [0.429] | [0.012] | [0.007] | [0.015] | [0.005] |
| Eligible | −0.011 | −0.034** | −0.028 | −47 | 0.715 | −0.014 | −0.001 | −0.041*** | 0.008 |
| | [0.016] | [0.017] | [0.017] | [345] | [0.470] | [0.016] | [0.010] | [0.015] | [0.005] |
| Observations | 48,153 | 48,153 | 28,107 | 48,153 | 48,153 | 48,153 | 34,302 | 48,153 | 33,831 |
| R-squared | 0.087 | 0.090 | 0.031 | 0.182 | 0.119 | 0.085 | 0.020 | 0.405 | 0.045 |
| *Panel B: men* | | | | | | | | | |
| Eligible*After | 0.049*** | 0.034** | −0.022*** | −299 | 1.716*** | 0.035** | −0.006 | −0.028*** | 0.001 |
| | [0.014] | [0.014] | [0.008] | [737] | [0.634] | [0.015] | [0.006] | [0.010] | [0.005] |
| Eligible | −0.040*** | −0.049*** | −0.004 | −756* | −1.260*** | −0.049*** | 0.003 | −0.052*** | 0.008 |
| | [0.013] | [0.014] | [0.014] | [416] | [0.486] | [0.013] | [0.007] | [0.013] | [0.007] |
| Observations | 54,612 | 54,612 | 40,724 | 54,612 | 54,612 | 54,612 | 45,152 | 54,612 | 38,286 |
| R-squared | 0.207 | 0.222 | 0.040 | 0.194 | 0.283 | 0.205 | 0.019 | 0.421 | 0.044 |

Note: Table 7 reports the estimates from Eq. (1) for women (Panel A) and men (Panel B). The estimation uses the sample from Panel A of Table 2. Each column indicates the outcome variable of interest. The first row of each panel reports the estimated coefficient on the interaction term and the second row reports the estimated coefficient on the DACA-eligibility variable. Standard errors are clustered at the state-year level. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

2 adds demographic controls including education level, sex, race, ethnicity, marital status, state level unemployment rates, and fixed effects for individuals' age and age entered the United States. Once these controls are included, the estimates are extremely similar to the baseline estimates. Row 3 includes year and state fixed effects, and little change occurs in the estimates. Row 4 includes state time trends and are the same as the baseline estimates found in Panel A of Table 2.

Panel B shows the estimates for different sample choices. Row 1 restricts the sample to those individuals who are only two years above or below the age cutoff for entering the United States, instead of the four years used in the baseline estimation. The magnitude of these results are approximately 20% larger than the baseline results. The sample for row 2 includes those who are six years above or below the cutoff, and finds similar to slightly smaller results than the baseline results. Row 3 includes all education levels instead of only a high school degree or more. With this sample, many of the individuals (35%) will have less than a high school degree, causing them to be ineligible for DACA and therefore biasing the estimates toward zero. Rows 4 and 5 are for the subsample of individuals who are married and single. DACA appears to have a larger effect on single than married individuals; however, most of these differences are not statistically significant.

## 7. Policy implications

The results help inform two main policy questions. First, how did the implementation of DACA affect its target population and what might happen if DACA were rescinded? Second, what do the results imply for potential future immigration policies such as a temporary or permanent amnesty program or Deferred Action for Parents of Americans and Lawful Permanent Residents (DAPA). Answering these two questions looks at the efficacy of current immigration policy and informs the debate on future immigration policy.

The results speak directly to how the implementation of DACA affected its target population – young unauthorized immigrants. The DID results are a lower bound of the intent-to-treat estimates so can be interpreted as a lower bound on the average effect of DACA on the DACA-eligible population. The results indicate that the implementation of DACA has increased the target population's likelihood of working by approximately 4 percentage points. This increase comes from an increase in labor force participation and a decrease in the unemployment rate of the target population. DACA has increased the income of those in the bottom of the income distribution. These effects imply that in its first two years, DACA moved 50,000–75,000 unauthorized immigrants into employment. Also, note that over two-thirds of DACA-eligible individuals had applied for DACA as

**Table 8**
The effect of DACA by education level.

| Variables | Working | Labor force | Unemployed | Income | Hours per week | Worked in past year | Self-employed | School | GED |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: less educated (high school degree or some college)* | | | | | | | | | |
| Eligible*After | 0.050*** | 0.039*** | −0.020*** | 341 | 2.039*** | 0.045*** | −0.005 | – | – |
| | [0.011] | [0.011] | [0.007] | [386] | [0.435] | [0.012] | [0.005] | – | – |
| Eligible | −0.024** | −0.041*** | −0.016 | −130 | −0.270 | −0.031*** | 0.001 | – | – |
| | [0.012] | [0.012] | [0.011] | [260] | [0.378] | [0.011] | [0.006] | – | – |
| Observations | 89,308 | 89,308 | 58,314 | 89,308 | 89,308 | 89,308 | 67,549 | – | – |
| R-squared | 0.151 | 0.159 | 0.033 | 0.178 | 0.236 | 0.144 | 0.017 | – | – |
| *Panel B: highly educated (college degree or more)* | | | | | | | | | |
| Eligible*After | 0.032 | 0.025 | −0.010 | −3,024* | −0.643 | −0.003 | −0.003 | – | – |
| | [0.023] | [0.021] | [0.015] | [1,818] | [0.972] | [0.019] | [0.013] | – | – |
| Eligible | 0.013 | 0.055 | 0.063 | 1,052 | 1.702 | 0.016 | −0.018 | – | – |
| | [0.082] | [0.076] | [0.063] | [2,917] | [2.604] | [0.077] | [0.040] | – | – |
| Observations | 13,457 | 13,457 | 10,517 | 13,457 | 13,457 | 13,457 | 11,905 | – | – |
| R-squared | 0.084 | 0.079 | 0.036 | 0.148 | 0.117 | 0.072 | 0.028 | – | – |

Note: Table 8 reports the estimates from Eq. (1) for individuals with a high school degree or some college (Panel A) and for college graduates (Panel B). The estimation uses the sample from Panel A of Table 2. Each column indicates the outcome variable of interest. The first row of each panel reports the estimated coefficient on the interaction term and the second row reports the estimated coefficient on the DACA-eligibility variable. Standard errors are clustered at the state-year level. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

**Table 9**
Robustness checks.

| Variables | Working | Labor force | Unemployed | Income | Hours per week | Worked in past year | Self-employed | School | GED |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: different sets of control variables* | | | | | | | | | |
| No controls | 0.083*** | 0.072*** | −0.025*** | 781 | 3.411*** | 0.068*** | −0.002 | −0.092*** | 0.005 |
| | [0.012] | [0.013] | [0.007] | [515] | [0.535] | [0.013] | [0.005] | [0.015] | [0.004] |
| Demographic controls | 0.050*** | 0.039*** | −0.020*** | −43 | 1.774*** | 0.040*** | −0.005 | −0.021** | 0.001 |
| | [0.010] | [0.011] | [0.007] | [472] | [0.420] | [0.011] | [0.005] | [0.010] | [0.004] |
| Year and state FE | 0.049*** | 0.038*** | −0.020*** | −28 | 1.739*** | 0.040*** | −0.004 | −0.021** | 0.001 |
| | [0.010] | [0.010] | [0.007] | [469] | [0.416] | [0.011] | [0.005] | [0.009] | [0.004] |
| State time trends (baseline) | 0.048*** | 0.037*** | −0.019*** | −1 | 1.715*** | 0.039*** | −0.005 | −0.021** | 0.001 |
| | [0.010] | [0.011] | [0.007] | [466] | [0.420] | [0.011] | [0.005] | [0.009] | [0.004] |
| *Panel B: different samples* | | | | | | | | | |
| Enter US age: 14 to 17 | 0.059*** | 0.041*** | −0.031*** | 843 | 2.287*** | 0.050*** | −0.003 | −0.014 | 0.002 |
| | [0.015] | [0.015] | [0.010] | [629] | [0.596] | [0.015] | [0.007] | [0.011] | [0.005] |
| Enter US age: 10 to 21 | 0.041*** | 0.036*** | −0.011** | −111 | 1.403*** | 0.035*** | −0.005 | −0.019** | 0.004 |
| | [0.009] | [0.009] | [0.005] | [439] | [0.350] | [0.008] | [0.004] | [0.008] | [0.003] |
| All education levels | 0.039*** | 0.036*** | −0.008 | 125 | 1.415*** | 0.034*** | −0.009 | −0.016* | 0.003 |
| | [0.009] | [0.008] | [0.006] | [367] | [0.350] | [0.009] | [0.005] | [0.009] | [0.003] |
| Married | 0.034* | 0.006 | −0.036*** | 862 | 1.188 | 0.020 | −0.004 | −0.008 | 0.000 |
| | [0.020] | [0.019] | [0.012] | [1,014] | [0.765] | [0.019] | [0.012] | [0.014] | [0.008] |
| Single | 0.049*** | 0.044*** | −0.013 | −322 | 1.771*** | 0.042*** | −0.005 | −0.023** | 0.000 |
| | [0.011] | [0.011] | [0.008] | [453] | [0.440] | [0.011] | [0.005] | [0.011] | [0.003] |

Note: Table 9 reports the estimates from Eq. (1) with varying levels of controls (Panel A) and for different samples (Panel B). The estimation uses the sample from Panel A of Table 2. Each column indicates the outcome variable of interest. Each row reports the estimated coefficient on the interaction term. Standard errors are clustered at the state-year level. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

of 2014, which implies these applicants anticipated the benefits of DACA approval to be greater than the time costs of applying and the 465 dollar fee. Because DACA was done through prosecutorial discretion, it is more susceptible to being rescinded than a law. The results of this paper predict that the elimination of DACA would have similar effect sizes, but in the opposite direction.

Note that the estimates from the DID analysis are partial equilibrium effects. Therefore, DACA creates potential general equilibrium effects arising from the increase in the supply of workers. The results suggest DACA moved approximately 50,000–75,000 unauthorized immigrants into employment in 2013 and 2014. This increase in the supply of workers only accounts for 0.94–1.41% of the 5.33 million individuals who gained employment in 2013 and 2014 (Bureau of Labor Statistics). This finding suggests that the general equilibrium effects of DACA on wages is likely to be small. However, the general equilibrium effects may be more problematic for a future immigration policy that targets a larger population. Also, as discussed in the conceptual framework section, due to breaking large labor market frictions for unauthorized immigrants and the minimal general equilibrium effect on wages, the overall welfare effect of DACA is likely not just a pure transfer to DACA-eligible individuals, but would likely enhance efficiency, although it would not be Pareto efficient.

The results also help inform future immigration policy. Approximately 11.4 million unauthorized immigrants live in the United States (Baker and Rytina, 2013). However, only 5.4% of unauthorized immigrants have obtained deferred action and work authorization through DACA. From the results above, DACA improved the labor market outcomes for this small portion of unauthorized immigrants. However, the majority of unauthorized immigrants are excluded from DACA and do not receive these benefits. A temporary or permanent amnesty program could expand deferred action and work authorization to a larger population of unauthorized immigrants. However, because such an expansion to a larger population would include a different type of unauthorized population, whether the benefits would be similar to those found for DACA is unclear. The results from Panels A and B of Table 2 come from samples that are close to the DACA cutoff criteria. Therefore, these estimates would be particularly policy relevant if DACA were expanded by increasing the age requirements. For individuals near these age cutoffs, the benefits are likely to be very similar.

However, if future temporary or permanent amnesty programs are expanded to include older unauthorized immigrants who likely entered the United States at older ages and may have restricted English proficiency, the effects of deferred action and work authorization may be quite different from those found for DACA. In addition, the effects could vary depending on whether the amnesty program was temporary or permanent. The intent-to-treat effects for a temporary program are likely smaller than for a permanent program, due to lower application rates from unauthorized immigrants wary of future deportation. Smaller effects for a temporary program may also come from less human capital investment without the assurance of the ability to work in the future.

The biggest potential difference between DACA and a temporary or permanent amnesty program for a larger unauthorized immigrant population are the potential general equilibrium effects. Because DACA has moved only a small number of individuals into employment, the downward pressure on wages from the increased supply of workers is likely very small. However, a larger temporary or permanent amnesty program could move many more workers into employment and could cause larger deceases in wages, particularly for low-skilled jobs. These potential general equilibrium effects make the welfare consequences unclear. Unauthorized immigrants will clearly be better off, but if the downward pressure on wages is large enough, they could offset these benefits.

Of particular importance is what these results imply for DAPA. DAPA was announced by President Obama on November 20th, 2014, and would expand eligibility for deferred action and work authorization to an additional 3.7 million unauthorized immigrants.[10] This

[10] http://migrationpolicy.org/news/mpi-many-37-million-unauthorized-immigrants-could-get-relief-deportation-under-anticipated-new

new policy would expand DACA by eliminating the under-31 age requirement and would require individuals to only have been in the United States since 2010 instead of 2007. Because this paper estimates the effect of DACA on individuals near the cutoffs of these requirements, the effects of DAPA on these unauthorized immigrants should be very similar to the effects of DACA. However, DAPA would also make all parents of a US citizen who have been in the United States for at least five years eligible for deferred action and work authorization. The effects of DAPA on this population are much less clear. This population of parents would be older, already have children, and may already be well established in a job. Therefore, the labor market effects of DAPA on this population may be smaller than the effects of DACA. If the effects of DAPA were the same as DACA, DAPA would move approximately 250,000 unauthorized immigrants into employment.

## 8. Conclusion

Because the United States has the most unauthorized immigrants of any country in the world, immigration policies affect millions of people. Deferred Action for Childhood Arrivals (DACA), one of the largest immigration policies in the last 25 years, has reduced the labor market frictions for young unauthorized immigrants by giving them deportation relief and work authorization. This paper looks at how DACA has affected unauthorized immigrants eligible for DACA. I find that those eligible for DACA are more likely to work. This increase in the likelihood of working comes from both a movement into the labor force and a decrease in unemployment. Those in the bottom of the income distribution have seen increases in their income. I also find some evidence of DACA increasing unauthorized immigrants GED attainment, although this evidence is at most suggestive.

The results of this paper shed light on how the lack of legal status in the United States depresses individuals' labor market outcomes. The results speak directly to how deportation relief and work authorization affect young unauthorized immigrants. Studying the effects of DACA gives insights into how future immigration policies, such as an amnesty program or DAPA, would affect their target populations. As immigration policies are studied and refined, they will be able to better benefit the large population of unauthorized immigrants in the United States.

## Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2016.08.014.

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