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ABSTRACT

Does Access to Citizenship Confer Socio-Economic Returns? Evidence from a Randomized Control Design*

Based on observational studies, conventional wisdom suggests that citizenship carries economic benefits. We leverage a randomized experiment from New York where low-income registrants who wanted to become citizens entered a lottery to receive fee vouchers to naturalize. Voucher recipients were about 36 p.p. more likely to naturalize. Yet, we find no discernible effects of access to citizenship on several economic outcomes, including income, credit scores, access to credit, financial distress, and employment. Leveraging a multi-dimensional immigrant integration index, we similarly find no measurable effects on non-economic integration. However, we do find that citizenship reduces fears of deportation. Explaining our divergence from past studies, our results also reveal evidence of positive selection into citizenship, suggesting that observational studies of citizenship are susceptible to selection bias.

JEL Classification: G51, J15, J31

Keywords: citizenship, naturalization, immigrant integration

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1 Introduction

Effective integration of immigrants is a long-standing policy challenge for governments throughout the world (OECD and IMF, 2016; Abramitzky and Boustan, 2017). Immigrants often face systematic disadvantages in host country labor markets due to discrimination, lack of recognition of foreign credentials, limited language proficiency, and other barriers (Chiswick, 1978; McManus et al., 1983; Cain, 1986; Borjas, 1995; Friedberg, 2000; Riach and Rich, 2002; Oreopoulos, 2011). Recognizing these disadvantages, policy makers and researchers have proposed easing the route to citizenship as a potential catalyst for economic integration. In their seminal study Bratsberg et al. (2002) argued that "naturalization is not an insignificant event that occurs during the assimilation process [...]. To the contrary, naturalization accelerates the process of labor market assimilation." A review from the Institute for the Study of Labor asserted that, "Evidence suggests the benefits of naturalization for first-generation immigrants are significant [...]. Liberalizing access to citizenship could thus be a key policy tool for improving the rate of economic and social integration of immigrants in their host country" (Gathmann and Monscheuer, 2020). Similarly, a comprehensive expert report by the National Academies of Sciences concluded that "U.S. citizenship [...] improves employment outcomes, wage growth, and access to better jobs." (National Academies of Sciences, Engineering, and Medicine, 2015, p. 165).

There have been dozens of observational studies reporting significant economic returns to citizenship on earnings, including a 6-14% citizenship premium for the United States (e.g., Bratsberg et al., 2002; Miller and Chiswick, 1992; Mazzolari, 2009) and mostly positive effects across many immigrant-accepting countries with magnitudes ranging up to 28% in France (Govind, 2020).[1] Yet, research in this area has been hampered by the difficulty of controlling for unobserved differences — such as motivation and perseverance — between naturalized and non-naturalized immigrants that are likely correlated with economic outcomes. To the best of our knowledge, random assignment of citizenship has not been utilized to date, limiting the ability to infer causal relationships.

[1] To examine the conventional wisdom in the literature, we accumulated a systematic sample of studies estimating the citizenship premium. We first chose the most cited papers in Google Scholar that cited one of the two most-cited papers on citizenship effects (Bratsberg et al., 2002; Miller and Chiswick, 1992). Researchers of 76% of these papers inferred from their data that there was a significant earnings premium for at least one country under study and/or a major immigrant group. A snowball sample of articles based on keywords and another sample that focused primarily on U.S. data showed similar support for a citizenship premium. The two leading literature reviews found on average positive returns to acquiring citizenship. A full accounting of these studies is in Section A1.6 and Table A14 in the Appendix.

Between 2016 and 2018 New York State implemented a program to help low-income immigrants to naturalize. Eligible lawful permanent residents could sign up for a lottery that randomly selected some registrants to receive a voucher to pay for the naturalization application fee. This lottery provides a unique opportunity to study the impacts of access to citizenship using a randomized control design. It allows us to identify the returns to citizenship in a population that the literature has identified as the most likely to gain – immigrants that are (1) eligible and motivated to naturalize and (2) in the lower part of the income distribution.[2]

We focus on four research questions. First, what are the causal effects of access to citizenship on immigrants' financial outcomes, including income, credit scores, financial distress, and access to credit? Second, if there are any economic returns, are they driven by improvements in employment, labor force participation, human capital investments, or occupational upgrading? Third, what are the impacts of access to citizenship on non-economic outcomes including the political, social, psychological, linguistic, and navigational integration of immigrants and their fears of deportation? Fourth, is there selection bias that impedes the identification of a causal effect of citizenship?

Our analysis combines individual level data from the program registration, follow-up surveys of registrants, and matched administrative records from a credit bureau containing financial information in the years prior to and for up to five years following the voucher lottery. We compare the outcomes of the treatment group of registrants who were selected to receive the naturalization fee voucher to those of the control group who were not selected for the voucher to estimate intention-to-treat (ITT) effects. We also estimate the local average treatment effects (LATE) of citizenship, using the voucher lottery as an instrument for self-reported naturalization. Unless indicated otherwise, all our analyses follow a pre-analysis plan that we registered before linking the outcomes with the treatment indicators.

Our study yields several findings. First, consistent with previous findings (Hainmueller et al., 2018), we find that being selected to receive a fee voucher in the lottery resulted in a large increase in naturalization on the order of 36 percentage points (first-stage partial F-statistic > 200). Yet, this did not translate into discernible improvements in the financial outcomes as measured in the matched credit records. In particular, the estimates suggest that for up to five years after the

[2]While our estimates focus on a population that is of considerable policy interest, future experimental research is needed to examine the potential impacts of access to citizenship in other contexts or time periods.

lottery, average income among registrants in the treatment group was similar to that in the control group. We also find no discernible effects on credit scores, various measures of financial distress, and measures of access to credit. These null findings are stable across subgroups and across quantiles of the outcome distributions. Moreover, we similarly find no discernible effects when we replicate the models using survey-based measures of income and financial distress. In addition, we examine whether the impacts grow over time but find no discernible evidence of a change in slopes between the treatment and control group.

Our null findings are meaningfully precise. Based on the panel models, the point estimate for the ITT effect on income is 0.2% with a 95% confidence interval that rules out impacts below -1.7% and above 2.2%. Given an average annual household income of around $51,123, this amounts to an average gain of $102 with a range between a $870 decrease and a $1,124 increase. The point estimate for the LATE effect on income is 1.8% with a 95% confidence interval that rules out impacts below -1.8% and above 5.3%. The point estimate for the ITT effect on the credit score is minus 6.8 points, with a 95% confidence interval that rules out effects below minus 17 points and above 3.1 points (compared to a mean score of 638 points and a standard deviation of 150). For financial distress, the 95% confidence interval rules out effects below -0.05 points and above 0.09 points for our index of distress measures (compared to a mean index score of 1.08 and a standard deviation of 1.26). For our index of credit access, the 95% confidence interval rules out effects below -0.03 points and above 0.01 points (compared to a mean index score of 0.84 and a standard deviation of 0.26).

Second, consistent with these null findings, we also find no support for envisioned mechanisms that explain how citizenship can improve economic integration (e.g., Bratsberg et al., 2002; Liebig, 2011). In particular, using data on educational loans and self-reported educational enrollment, we find no discernible evidence that access to citizenship encourages immigrants to invest in host country specific human capital. Similarly, we find no measurable effects on self-reported employment, labor force participation, and occupational upgrading.

Third, the null findings on economic returns raise the question of whether citizenship may improve other (non-economic) dimensions of integration, such as immigrants' English skills, political engagement, their social ties with natives, or psychological attachments to the US. To examine this question, we leverage a survey-based multi-dimensional measure of immigrant integration developed by Harder et al. (2018). As with the economic returns, we find no support for the idea that access to citizenship resulted in greater political, social, psychological, navigational or linguistic integration. However, we do find that citizenship reduced fears of deportation.

Fourth, our study also reveals the presence of positive selection effects. When we use the non-experimental variation and compare naturalizers with non-naturalizers, we find that citizenship is associated with higher income, fewer third-party debt collections, and a higher probability of having an open credit line. This finding suggests that absent random assignment, the selection into citizenship is an obstacle to causal identification.

This study contributes to the literatures on economic integration of immigrants and the design of citizenship policies. Econometric tests of the citizenship premium have been conducted in at least eleven OECD countries.[3] These studies have given rise to a conventional wisdom that the citizenship premium is significant, especially for immigrants from poorer countries and those that are more disadvantaged in the labor market. Yet, prior tests have relied exclusively on observational studies and our study provides—to the best of our knowledge—the first experimental estimates of the effects of citizenship. Our findings run counter to the literature and temper optimism about the promise of citizenship as an important policy lever to improve the socio-economic integration of immigrants.

The rest of this paper is organized as follows. Section 2 describes the experimental design. Section 3 presents the data sources, research design checks, and summary statistics. Section 4 lays out the empirical approaches we use and Section 5 presents our results. Lastly, Section 6 concludes the study.

[3]These studies include the following: the U.S. (Bratsberg et al., 1992; Sumption and Flamm, 2012; Chiswick, 1978; Catron, 2019; Mazzolari, 2006, 2009; Pastor and Scoggins, 2012; Picot and Hou, 2011; Shierholz, 2010; Zhou and Lee, 2013; Enchautegui and Giannarelli, 2015); Canada (Pivnenko and DeVoretz, 2003; DeVoretz and Pivnenko, 2005; Miller and Chiswick, 1992; Pendakur and Bevelander, 2014; Picot and Hou, 2011; Mata, 1999), Germany (Steinhardt, 2012; Constant et al., 2007; Euwals et al., 2010; Gathmann and Keller, 2018; Gathmann and Monscheuer, 2020; Sajons, 2019); Sweden (Scott, 2008; Engdahl, 2011; Bevelander, 2000; Kogan, 2010; Helgertz et al., 2014; Pendakur and Bevelander, 2014); Norway (Bratsberg and Raaum, 2011; Hayfron, 2008); Switzerland (Hainmueller et al., 2019; Steinhardt, 2011, 2012); Netherlands (Peters et al., 2017, 2019; Euwals et al., 2010; Bevelander and Veenman, 2006); France (Fougère and Safi, 2009; Govind, 2020; Jarreau, 2020), Austria (Kogan, 2010); Denmark (Helgertz et al., 2014); Belgium (Corluy et al., 2011); and other countries as well in a variety of cross-national comparisons (Mazzolari, 2009; Gathmann and Monscheuer, 2020; Corluy et al., 2011; OECD, 2011).

2 Experimental Design

Our study leverages a statewide public-private program in New York State that promoted naturalization among low-income legal permanent residents who were eligible to naturalize. Figure 1 illustrates the experimental design. The program operated in 2016, 2017, and 2018. Each year, there was a statewide publicity campaign in the spring and summer that encouraged eligible immigrants to register for the program. This included press conferences, public service announcements on English and Spanish television programs, and advertisements in subways and newspapers as well as on radio and social media platforms. Interested immigrants could register by completing a registration form that was used to assess their eligibility. Registration was available online, by phone, or in person at specific non-profit immigrant service provider locations throughout the state. The registration was offered in several languages, including English, Spanish, Chinese, Italian, Russian, Korean, and Haitian Creole.

To be eligible for the program, immigrants had to be 18 years or older, reside in New York State, have a household income that falls at or below 300% of the Federal Poverty Guidelines (FPG), and meet the eligibility requirements for naturalization. The income requirements were designed to promote naturalization among low-income immigrants who faced financial barriers to apply. Typically, the naturalization application requires a fee. The fee was $680 in 2016 and increased to $725 in 2017 and 2018.[4] For low-income immigrants, these fees can serve as a barrier for naturalization (Hainmueller et al., 2018). In addition, very low-income immigrants whose household income falls at or below 150% of the FPG or who receive means-tested benefits from the government were eligible for a federal fee waiver that eliminates the application fee (Yasenov et al., 2019). Research has shown that informing low-income immigrants about their eligibility for the fee waiver increases naturalization rates (Hotard et al., 2019).

The program sorted all eligible registrants into two groups after they had completed the registration. First, if a registrant's household income was below 150% of the FPG or they received means-tested benefits, then the registration system informed them that they potentially qualified for a fee waiver from the federal government. Second, and the focus for this study, are those registrants with a household income between 150% and 300% of the FPG and who did not receive

[4] A reduced fee option was also introduced in 2017 that allowed some immigrant to pay $405 depending on their household income.

means-tested benefits.[5] For this group, the registration system entered them into a lottery for a chance to receive a voucher that paid the fee for their naturalization application.

The voucher lottery was run separately in 2016, 2017, and 2018. Each year after the registration period closed, all registrants who were eligible for the voucher lottery were randomly assigned to one of two groups. If a registrant was assigned to the treatment group, she received a voucher that paid the full cost of applying for citizenship. The voucher was processed by a specific non-profit immigrant service provider. It was directly paid to the United States Citizenship and Immigration Services (USCIS) by the non-profit and could not be used for any other purpose than to pay for the naturalization application. Registrants assigned to the control group did not receive a fee voucher. Table A1 in the Appendix presents a timeline of the lottery and data collections.[6]

Each year, the voucher lottery was conducted within geographic blocks. In particular, eligible registrants were assigned to a geographic block based on their geocoded street address provided during registration and their expected naturalization fee (full or reduced price). The geographic blocking was conducted to minimize the distance that lottery winners would have to travel to get their vouchers processed at the state-designated immigrant service providers. A lottery was conducted in all blocks where the number of eligible registrants exceeded the number of available vouchers. Only New York City and Long Island had blocks that met this criterion and the study was therefore limited to registrants from those blocks. Table A2 in the Appendix shows the number of registrants randomly assigned into the treatment and control groups for each block and registration year. This research was approved by the Stanford University IRB protocol #34554.

[5] In 2017, for a three person household this range corresponded to between \$30,630 and \$61,260 annual income respectively.

[6] In 2016, the registration window was from July 11, 2016, to September 23, 2016. The voucher lottery draw took place after the closing of the registration in the following week. Voucher winners were notified the week of October 23, 2016. In 2017, the registration window was from September 24, 2016, to July 28, 2017. The voucher lottery draw took place after the closing of the registration in the following week. Voucher winners were notified the week of August 7, 2017. In 2018, the registration window was from April 4, 2018, to July 3, 2018. The voucher lottery draw took place after the closing of the registration in the following week. Voucher winners were notified the week of July 10, 2018.

3 Data

3.1 Sources

We rely on three data sets. The first is the registration data from the program, which includes contact information and demographic characteristics of eligible registrants, including age, marital status, gender, country of origin, education, year of green card receipt, language, income, and employment. Note that these variables were measured prior to the lottery, so we use them as pre-treatment covariates. The registration data also contains information on the registration year, randomization block, and whether each registrant was selected for the naturalization fee voucher in the lottery.

The second data set is from follow-up surveys that were conducted with each cohort of registrants. The follow-up surveys measured the first stage effect of the intervention – whether receiving the voucher increased naturalization rates. During the surveys registrants were asked whether they had submitted their naturalization application and whether they had attained citizenship. The follow-up surveys also included a wide range of integration outcomes (detailed below).

Each cohort received annual follow-up surveys beginning approximately one year after the voucher winners were selected. Table A1 in the Appendix provides more information about the timeline of the surveys.

Given this timeline, we have data on outcomes measured for up to three years after the lottery for all three registration cohorts. For the cohorts that registered in 2016, 2017, and 2018, the three-year outcomes were measured in the survey waves that were conducted in 2019, 2020, and 2021, respectively. These three-year outcomes are the main focus of the analyses, but we also consider analyses of two-year outcomes (available for all three cohorts), and analyses using all available survey years. For the 2016 (2017) cohort we also have five-year (four-year) outcomes.

The third data set is from a credit bureau and contains information on the financial situation of the registrants. Similar data from credit bureaus has been previously used in other studies (e.g., Finkelstein et al. (2012); Meier and Sprenger (2010); Brown et al. (2016)). To match the registrants to the credit records the credit bureau was provided with a dataset that contained the matching variables (reported name, address, and date of birth), the voucher assignment, citizenship indicators, the geographic randomization block, and two covariates from the registration data.[7] The credit bureau then matched the registrants to their credit records using a proprietary algorithm.[8] Overall, the match rate was 93.3% (see more below). The credit bureau then returned the data files but removed all personally identifiable information, such that the data is de-identified and we are unable to link the records to specific study registrants. Due to the anonymized nature of the matching, only a limited set of two covariates from the registration data could be included – indicators for above/below median household (HH) income and whether the registration was conducted in English or another language.

The credit data includes six yearly snapshots of the financial situation for each registrant for the summers of 2016 through 2021. Thus, the snapshots cover one month before and approximately one, two, and three years after the voucher lottery assignment for all registrants. In addition, the total number of pre- and post-treatment periods vary by lottery registration cohort. For the cohort that registered for the lottery in 2016, we observe the financial situation for one pre-intervention period, about one month before the lottery (in 2016), as well as in the five years following the intervention (in 2017, 2018, 2019, 2020, and 2021). Next, for the cohort that registered for the lottery in 2017, we observe the financial situation for two pre-intervention periods, about one year and about one month before the lottery (in 2016 and 2017, respectively), as well as in four years following the intervention (in 2018, 2019, 2020, and 2021). Lastly, for the cohort that registered for the lottery in 2018, we observe the financial situation for three pre-intervention periods, about two years, one year, and one month before the lottery (in 2016, 2017, and 2018, respectively), as well as in the three years following the intervention (in 2019, 2020, and 2021). This data structure allows us to examine both balance on pre-treatment outcomes as well as analyze short- and medium-term effects.

3.2 Sample

In the credit bureau data, not all registrants are matched to a credit file in every year during 2016-2021. However, once a registrant is first matched, she is also matched in all subsequent years.

[7]Registrants consented to participate in the research study and to have their information linked with administrative data for research purposes.

[8]Credit data pull and match for an analytical purpose does not have an impact on consumer's scores or credit history.

For the majority of results in the analyses below we focus on the set of registrants for whom we have outcome data in the first three years following the lottery. We sometimes refer to this as the ("balanced") four-year panel. When estimating panel models (explained below) we use the sample of registrants who are matched by the credit bureau data for the entire 2016-2021 period (i.e., in the "full" six-year panel). Similarly, in the survey data, we focus on registrants who have answered the three-year survey questions. When estimating panel models we are additionally restricted to outcomes which were also measured at baseline.

3.3 Preregistration

This study was pre-registered at the AEA RCT Registry under protocols AEARCTR-0006790 and AEARCTR-0007148. This pre-analysis plan described the sample, statistical models, outcomes, and control variables and was registered before linking the outcomes and the treatment indicators. Unless indicated otherwise, all analyses follow this pre-analysis plan. There are three deviations that we explain below.

First, in the pre-analysis plan we specified outcomes measured two years following the lottery, but below we present three-year outcomes. After we had filed the pre-analysis plan, we received an extension of funding to collect one more follow-up survey wave in 2021 and obtained one additional year of credit bureau data. For transparency reasons we present the results that leverage all available outcome data—including the additional year of follow-up data—so that we have three-year outcomes for all three registration cohorts. For full disclosure, we also present in the Appendix the results using the two-year outcomes and they are similar to the three-year outcomes.

Second, motivated by a study from Asad (2020) we decided to add in our additional survey wave a question to measure fears of deportation. In contrast to the other outcomes, this question was not included in the pre-analysis plan and is not available for the other survey waves.

Third, in the pre-analysis plan we registered to use the Vantage score as the main credit score outcome, and also a second credit score as an additional measure. However, after the data agreement was signed, the credit bureau informed us that the secondary credit score cannot be included in the analysis due to contractual restrictions.

3.4 Outcomes

We consider pre-specified outcomes that come from two sources – credit bureau data and the follow-up surveys. Outcomes from the former capture the financial situation of the registrants and have the advantage that they are based on administrative data and are not affected by self-reporting biases or attrition. The advantage of the latter source – the survey – is that it measures a broader set of outcomes, including additional economic variables such as employment and occupation, as well as non-economic integration.

3.4.1 Credit Bureau Data

In building our set of outcomes from the credit bureau data we followed Finkelstein et al. (2012). We analyze ten main outcome variables grouped into four broad domains.

Main Outcomes in the Credit Bureau Data:

- *Income.* This is an estimate of household income provided by the credit bureau. We use a logarithmic transformation. The credit bureau uses a large set of financial inputs to estimate the joint gross adjusted income (line 37 of the 1040 federal tax form). The inputs include multiple sources of income and debt service parameters (such as monthly spending data, and up to 30 months of account history including credit lines, length of credit history, historical credit card balances, and recent credit card transactions). Boatman et al. (2020) validates this income measure against self-reported income data and finds that the two match well.

- *Credit score.* The credit bureau provides the VantageScore (version 3.0), a widely used credit score index representing risk of loan default.

- *Financial distress.* This category includes several variables that capture adverse financial events, such as delinquency (number of credit transactions 30+ days past due) and collection (number of third party collections). The delinquency variable is top-coded at five or more (i.e., 0-5 integer scale), while the collection variable is top-coded at three or more (0-3 scale). We examine these events separately as well as take their average, which we call "Index (Distress)." We do not analyze bankruptcies or liens because they are extremely rare in our sample.

- *Access to credit.* First, we use several variables that measure whether the person has access to credit – indicators for having a credit score, having at least one open line of credit, and having a "thick" file (three or more open lines of credit). We use these variables individually and also take their average which we refer to as "Index (Access)." Second, for those registrants who have access to credit, and using a logarithmic transformation, we compute the amount of credit given using the total credit line of all open revolving transactions.

In addition to these main outcomes, we use one more variable to gauge educational investment.

Mechanisms in the Credit Bureau Data:

- *Educational Investment.* We use an indicator whether the respondent has had any student loan trade as a measure of investing in education.

3.4.2 Survey Data

The follow-up surveys allow us to (i) replicate some of the credit bureau analyses using self-reported measures of income, (ii) analyze a wider range of economic and non-economic outcomes, and (iii) more deeply examine mechanisms that might drive the potential economic returns of access to citizenship. The main outcome variables in the survey cover two broad domains.

Main Economic Outcomes in the Survey Data:

- *Income.* We ask respondents for their household and personal incomes in the past year. Additionally, we create an equivalized household income per capita using information on their household sizes. We use a log transformation of all three income variables.

- *Financial Distress.* We ask whether the respondents can afford unexpected expenses of $500, $1,000 and $10,000 respectively. We then use three different indicators for responding positively to each question.

Mechanisms in the Survey Data:

- *Educational Investment.*

  We use an indicator for the respondent being enrolled in school at the time of the survey.

- *Labor Market Outcomes.* We use indicators for whether the registrant was currently employed and whether they were in the labor force at the time of the survey.

- *Occupational Upgrading.* Each respondent was asked to input manually their current occupation. We then link each informative response to a five-digit Standard Occupation Classification (SOC) code and use the 2018 American Community Survey (Ruggles et al., 2019) to obtain more information about each occupation. Specifically, we calculate the average (log) wage and years of education among all people employed in each occupation in 2018 as well as the wage and education ranks. The Appendix provides more details on these variables and the procedure we use to link the manually entered occupations to IPUMS SOC codes.

Additional Outcomes in the Survey Data:

- *Non-economic Integration* To measure non-economic integration, we deploy the IPL-12 multidimensional integration index developed and validated by Harder et al. (2018). The IPL-12 index has been used and further validated in a variety of settings to measure immigrant integration (e.g., Aksoy et al., 2020; Kunwar, 2020; Wasem, 2020). It defines successful integration as having the knowledge and the capacity to succeed in the host country. The index contains two questions on each of six dimensions of integration – economic, political, social, psychological, navigational and linguistic. For example, navigational integration is measured as the difficulty for immigrants to see a doctor or search for a job in the host country. Similarly, social integration is a function of the number of native-born with whom the immigrant has had a dinner or a phone conversation in the past twelve months. All variables are measured on the 0-1 scale with higher values indicating higher degrees of integration. We consider the following eight outcome variables - overall IPL-12 index which combines all six dimensions, IPL-12 index excluding the economic dimension, and indexes for each of the six dimensions separately.

- *Fears of Deportation* Motivated by a recent study from Asad (2020), we examine whether naturalization may have affected registrants' perceived security of status and their fears of deportation. To this end we asked registrants how much, if at all, they worry that they could be deported. The answer options were on a four-point Likert-type scale and included worry a lot, some, not much, or not at all. We use the full four point scale and a dichotomized version of this measure in the analyses.

3.5 Balance, Match Rates, and Survey Attrition

We conduct various checks to examine the validity of the experimental design. The results are presented in the Appendix. First, we run balance checks to examine whether the treatment group of registrants who won the lottery and were offered the naturalization fee voucher are similar to the control group of registrants who did not win the lottery. We regress the treatment indicator on the full set of pre-specified pre-treatment covariates measured in the registration data. The results are presented in Table A3 in the Appendix. Consistent with the random lottery assignment, we find that the covariates are well-balanced; the p-value against the null that they jointly do not predict the treatment assignment is equal to 0.50.

Second, the estimates based on the credit bureau data may be affected by differential rates of matching into the credit bureau data between the treatment and the control groups. The bureau returned a match if it deemed that there was a high probability that there was a successful linkage. A record may not match if the information provided by the registrant during registration was incorrect or if the registrant did not have a credit file. Overall, the match rate to at least one year of data is 93.3%. As shown in Table A4 in the Appendix, the regression results confirm that the match rate is statistically indistinguishable between the treatment and the control group. This holds for matching into the file in at least one year (i.e., ever), for being matched into the four-year panel, and into the six-year panel.

Third, the estimates based on the follow-up surveys may be affected by differential attrition between the treatment and the control group. To check for this, we regress an indicator for whether the registrant responded to the three-year follow-up survey (i.e., whether the respondent answered the income question on that survey) on the treatment indicator, the full set of pre-treatment covariates from the registration data, and the interactions between the treatment indicator and pre-treatment covariates. The results are shown in Table A5 in the Appendix. Overall, the response rate for the three-year survey was about 47.0%. Voucher recipients and registrants who were college graduates and those who had more recently obtained their green card were more likely to respond in general. However, the interaction terms between the treatment indicator and the pre-treatment covariates are jointly insignificant (p-value equal to 0.60). While this test does not rule out the possibility of differential non-response based on unobserved confounders, it does suggest that voucher recipients who responded were similar to non-voucher recipients who responded in terms of the pre-treatment covariates. In the robustness tests we also replicate the impact estimates that are based on the survey data while correcting for potential differential non-response using inverse probability weighting and the findings are very similar.

3.6 Summary Statistics

Tables 1 and 2 present summary statistics for the covariates and pre-treatment outcomes for the entire sample (columns 1-2), the treatment (column 3) and control groups (column 4). We also show the p-values (column 5) for the test of difference in means between treatment and control registrants and the number of observations (column 6). All variables are measured prior to the lottery. For the credit bureau data, the sample is restricted to registrants in the balanced four-year panel – i.e., those for whom we have credit bureau data in the year of the lottery as well as each of the first three years following the lottery (balanced four-year panel). We have information on 1,225 (1,153) individuals who were assigned to the treatment (control) group in this sample.

Consistent with the randomized lottery assignment, all variables' means are similar between the treatment and control groups. The average age of registrants in our sample is 42.1 years, the sample has slightly more women (54.4%) than men, and the most common origin country is the Dominican Republic (27.3%). Most registrants completed the registration in the English (63%), followed by those who used Spanish (34%). Most (88.0%) of our registrants have an open credit line and 65.6% had a "thick file" Virtually everyone (96.7%) has a VantageScore with an average VantageScore of 629. Nearly four in five registrants (79.0%) are in the labor force and the vast majority of those (93.2%) are employed.

4 Empirical Specification

We use canonical, pre-specified methods for analysis of randomized encouragement designs to measure the impacts of the intervention (Holland, 1988; Imbens and Angrist, 1994; Duflo et al., 2007; Bloom, 2008).

4.1 Three-Year Outcomes Model

For our baseline models, we estimate the ITT effects using the following equation:

$$\mathrm{y}_i = \alpha_0 + \alpha_1 \mathrm{LOTTERY}_i + \theta X_i + B_k + \epsilon_i, \tag{1}$$

where y_i is an outcome variable defined above; $LOTTERY_i$ is a dummy variable for whether or not registrant i won in the lottery and was offered a voucher; the term X_i is a vector of pre-specified, pre-randomization control variables; B_k is a vector of dummy variables that indicate the geographic lottery randomization blocks, and ϵ_i is the error term. When using the credit bureau data X_i includes indicators for above/below median HH income and English language ability and the pre-treatment outcome. When using the survey data X_i includes income, age, female, origin dummies (Dominican Republic, Ecuador and Colombia), year of green card, education dummies, language dummies (English and Spanish), and time between registration and voucher lottery. We use robust standard errors and employ block-level inverse probability weights to account for the unequal probability of treatment assignment within each block. We do not cluster the standard errors because the randomization occurred at the individual level (Abadie et al., 2017).

The coefficient of interest is α_1 which identifies the ITT effect of winning the voucher in the lottery. It can be interpreted as the impact of obtaining access to citizenship by overcoming the financial hurdle of paying for the naturalization fee. The ITT effect is causally identified by virtue of the randomized lottery.

For our baseline specification we pool the data from all three cohorts and estimate the models using the outcomes that are measured three years after the intervention for each cohort.[9] For models using the credit bureau data, we also focus on registrants in the balanced four-year panel. In the Appendix we report the results from various robustness checks of our main specification.

Next, to obtain the LATE of citizenship for compliers we estimate the following equation using two stage least squares (2SLS):

$$\mathrm{y}_i = \beta_0 + \beta_1 \mathrm{CITIZENSHIP}_i + \phi X_i + B_k + \epsilon_i, \tag{2}$$

[9] The appendix also presents the results for two-year outcomes.

where $CITIZENSHIP_i$ is a binary treatment variable for whether or not a registrant reported that he or she applied for citizenship. In this equation, $CITIZENSHIP_i$ is instrumented by $LOTTERY_i$ to accommodate non-compliance. All other aspects of the estimation remain the same as in the ITT model. Our measure for the $CITIZENSHIP_i$ indicator is whether or not the registrant reported having submitted their citizenship application during the first check-in survey.[10]

The coefficient β_1 identifies the LATE of citizenship for compliers, i.e., registrants who would naturalize if given the fee voucher and would not otherwise.[11] The causal interpretation of this parameter requires the additional identifying assumption that winning the voucher in the lottery has no effect, on average, on the outcomes of interest that does not operate via the impact of the voucher on naturalization.[12] Given that the voucher was sent directly to USCIS and could only be used to pay for the naturalization application fee, we believe that this assumption is reasonable. Yet, the exclusion restriction may not strictly hold. One possible violation is that winning the lottery could, in theory, have a positive psychological effect, but it is not likely that this would have a lasting impact on the outcomes we study. Another possible violation is that for always-takers (i.e., registrants who would always naturalize regardless of winning the voucher), the voucher could act as a one-time substitute for cash. Given that the average registrant had a pre-lottery household income of about \$46,630, this represents a one-time 1.56% increase. It is theoretically possible that this could have resulted in a short-term positive indirect effect on some of the outcomes unrelated to naturalization. However, it seems unlikely that this effect would persist for multiple years after the lottery. This scenario might introduce a small upward bias in our LATE estimates.

Whether one prefers the ITT or the LATE is largely a matter of analytical interest and methodological considerations; both estimands are used in the literature on the effects of citizenship. The ITT captures the impact of access to citizenship and is arguably more relevant from a policy perspective given that governments can change eligibility criteria or design policy encouragements for immigrants to apply for naturalization, but typically not force them to naturalize. The ITT has

[10] We also use a secondary version of this variable that measures whether or not the registrants reported having submitted their citizenship application during any survey. We prefer the former because it is measured at roughly the same time interval for all cohorts. For the secondary measure we have more surveys for the earlier cohorts, as those registrants have had more time to complete their citizenship application. Both measures yield similar results.

[11] Note that we report robust standard errors for the LATE. We do not use the tF adjustment Lee et al. (2022) or Anderson-Rubin confidence intervals since the first-stage F statistics are > 200.

[12] It also requires the monotonicity assumption which rules out the presence of defiers, i.e., registrants who would only naturalize if they do not win the voucher. We consider this assumption to be plausible given that the voucher enables registrants to pay for the naturalization fee, but cannot be used otherwise.

therefore been the focus in many recent studies of citizenship (e.g. Mazzolari, 2009; Hainmueller et al., 2015, 2017a; Gathmann and Keller, 2018; Hainmueller et al., 2019; Felfe et al., 2020). The LATE captures the effect of citizenship per se for compliers, but requires the exclusion restriction. In the appendix we use kappa weighting (Abadie, 2003) to estimate the mean characteristics of compliers and find that they are similar to the overall sample across the pre-treatemt covariates. This speaks to the external validity of the LATE estimates (see Table A13).

Note that some of the earlier studies on citizenship simply regressed outcomes on a citizenship indicator plus controls without an explicit identification strategy. These models require the selection on observables assumption that states that citizenship is as good as randomly assigned conditional on the controls. This assumption is doubtful given that immigrants self-select into citizenship based on unobserved confounders— such as motivation and perseverance—and the potential for selection bias has been recognized in the literature (e.g. Liebig, 2011; Engdahl, 2014; Hainmueller et al., 2015; Bratsberg et al., 2002) (also see Section 5.4).

4.2 Panel Models and Dynamic Effects

Here we analyze models that leverage the full six-year panel structure of the credit bureau data across the three registration cohorts. When using the survey data, we also employ this model in a six-year panel. In particular, we estimate the following panel equation:

$$y_{it} = \delta_i + \sigma_t + \gamma LOTTERY_{it} + \epsilon_{it}, \quad (3)$$

where the terms δ_i and σ_t represent individual and year fixed effects, and $LOTTERY_{it}$ is the randomized treatment indicator coded as one for observations from years when a registrant was assigned to the voucher and zero otherwise. The coefficient of interest is γ which identifies the ITT effect pooling together the short- and medium-term effects of the intervention. For the LATE analysis we estimate the equivalent 2SLS model where $CITIZENSHIP_{it}$ is instrumented by $LOTTERY_{it}$. When using the credit bureau data, we focus on a sample of individuals which are successfully matched by the credit bureau in each year during 2016-2021 (i.e., balanced six-year panel). When using the survey data, these models are limited to outcome variables which were measured prior to the randomization. Standard errors are clustered at the individual level. Again, we weight each observation by the block-level inverse probability of being randomized into the treatment group.

Following the analysis in Bratsberg et al. (2002), we also estimate panel models where we allow the effects of access to citizenship to change over time. In particular, we estimate the following equation:

$$\begin{aligned} y_{it} = \delta_i + \sigma_t + \theta_1 LOTTERY_{it} + \\ + \theta_2(LOTTERY_{it} \times YEARS\_SINCE_{it}) + \theta_3 YEARS\_SINCE_{it} + \epsilon_{it}, \end{aligned} \tag{4}$$

where the term θ_1 identifies the effect of the intervention in the first year and θ_2 measures how the effect changes for each additional year following the intervention (as measured by the variable $YEARS\_SINCE_{it}$). In other words, θ_2 identifies the linear change in the outcome trends between treated and control registrants in the years following the intervention. All other aspects of the model remain as specified above. In the appendix we also show dynamic treatment effect estimates from a similar panel model where we allow the effect of the treatment to vary in each time period relative to the treatment assignment.

4.3 Distributional Effects

We estimate quantile regressions at various points of the distributions of the continuous outcomes including (log) income and the VantageScore. As with the baseline model, we use three-year outcomes and a sample of registrants in the balanced four-year panel. Additionally, in the appendix we replicate our main model on subgroups based on the pre-treatment covariates that we have in the credit bureau data—indicators for above/below median HH income and English language ability—as well as pre-specified subgroups for the survey data.

5 Results

5.1 First Stage

We begin by examining whether the random assignment of the vouchers via the lottery increased the probability of naturalization. Table 3 presents the first stage estimates from regressing the citizenship indicator on the voucher indicator and the indicators for the geographic randomization blocks. The first two columns show the effect of voucher assignment for registrants who are matched in the four-year panel of credit bureau data. The last two columns show the effect for respondents in the survey data. We find that the voucher assignment increased the likelihood of submitting a citizenship application by about 35-37 percentage points, on average.[13] As expected, given the random lottery assignment, the estimate of the effect of the voucher on the increase in citizenship is nearly identical when we control for the baseline covariates in columns 2 and 4. The first stage partial F-statistics are above 200, indicating that the voucher instrument is strong. Overall, these results suggest that the application fees constitute a financial barrier to citizenship for low-income immigrants.

5.2 Treatment Effect Estimates: Credit Bureau Data

5.2.1 Three-Year Outcomes

Before turning to the formal impact estimates from the regression models, we provide graphical evidence that illustrates the main findings. We construct "event study"-type plots of the mean outcome values for the treatment and control groups from two years prior up to five years after the voucher assignment. Year zero refers to the year of voucher lottery. Recall that the three cohorts registered for the lotteries that occurred in 2016, 2017, and 2018, respectively, but the credit bureau data captures the years 2016-2021. Therefore, for event years 0, 1, 2, and 3 the sample includes all three registration cohorts, but for event years -2, -1, 4, and 5 the sample composition changes.[14]

The results are presented in Figure 2. We focus on the four main outcomes – (log) income (top left panel), the VantageScore (top right), the index that combines the measures of financial distress (bottom left), and the index that combines the access to credit measures (bottom right).[15] The shaded regions correspond to 95% confidence intervals. Consistent with the random assignment of the vouchers, the treatment and control group have similar trends prior to the intervention across

[13]As reported in a previous study that only used the 2016 cohort and only looked at the effects on uptake of citizenship, the voucher increased the likelihood of submitting a citizenship application by approximately 40% (Hainmueller et al., 2018).

[14]For event year -1 we only have outcomes for the 2017 and 2018 registration cohorts, and for event year -2 only for the 2018 registration cohort. In addition, for event year 4 we have outcomes only for the 2016 and 2017 registration cohorts, and for event year 5 only for the 2016 registration cohort.

[15]Figure A1 displays the results for the rest of the outcomes.

all four outcomes. Moreover, both groups also have very similar trends for up to five years following the intervention, demonstrating that naturalization vouchers had no discernible effect on any of the four outcomes.

Next, we turn to the formal effect estimates from the regression models. The top panel in Table 4 presents the ITT estimates from the baseline specification. Consistent with the graphical evidence, we find that receipt of the naturalization voucher had no discernible positive impact on the financial outcomes of registrants. For income, the point estimate is 0.0% with a 95% confidence interval that rules out effects below -2.4% and above 2.4%. Moreover, we find no discernible effects on the measures of financial distress or access to credit. If anything, we find a small drop in VantageScore scores and the probability of having an open credit line, but these effects are not robust across specifications and we therefore refrain from interpreting these findings as negative effects.

Our estimates are meaningfully precise in substantive terms. The average registrant had an annual household income of around $54,122 three years after the lottery, which implies that the point estimate of the income effect amounts to a change of $0 and the 95% confidence interval ranges from a decrease of about $1,271 to an increase of about $1,271. For the effect on the VantageScore, the point estimate is -12 points while the 95% confidence interval ranges between -22 and -2 points which is rather small compared to a mean score of 655 points and a standard deviation of 142. For the effect on our index of distress, the point estimate is 0.07 with a 95% confidence interval that ranges between -0.02 points and above 0.15 points compared to a mean index score of 1.1 and a standard deviation of 1.3. For the effect on our index of access to credit the point estimate is -.01 with a 95% confidence interval ranges from -0.03 points to 0.01 points compared to a mean index score of 0.9 and a standard deviation of 0.2. The bottom panel in Table 4 presents the LATE estimates from the baseline specification. Again, we find no discernible evidence that citizenship improved the financial outcomes of immigrants. As expected, the LATE estimates are about three times larger than the ITT estimates.

Taken together, these results show that the voucher intervention considerably increased the uptake of citizenship but this did not lead to discernible improvements in financial outcomes as measured three years after the lottery. In Appendix Table A6 we show that the results are similar when using randomization inference. In Appendix Table A7 we present the *p*-values associated with each of the coefficients after correcting for multiple hypothesis testing and the smallest adjusted p-value controling for the family-wise error rate is 0.13.

5.2.2 Panel Models and Dynamic Effects

To leverage the full credit bureau panel data for the three registration cohorts over the entire 2016-2021 period, we now present estimates from a panel regression. Table 5 presents the ITT and LATE estimates following equation (3). The results mirror the null findings from the earlier models, but are slightly more precise. For income, the ITT estimate is 0.2% with a 95% confidence interval that rules out effects below -1.7% and above 2.2%. The corresponding LATE estimate is 1.8% (95% CI = -1.8%, 5.3%). The null findings are similar for the other financial outcomes including the measures of financial distress and access to credit. Again, the point estimate of the ITT effects on VantageScore is negative (-6.8 points decrease), but the 95% confidence interval is consistent with no effect (95% CI = -16.7, 3.1). The same is true for the effect on the probability of having an open credit line.

Next, we investigate whether the effects grow over time. We follow Bratsberg et al. (2002) and add an interaction term that allows for a change in the slope such that the gap between the treated and control registrants may change each year following the intervention (see equation (4)). The results are shown in Table 6. In contrast to Bratsberg et al. (2002), we find no evidence that the effects grow over time. In fact, for income, the immediate effect (θ_1) is again close to zero with a point estimate of 0.3% (95% CI = -2.0%, 2.7%) and the interaction term (θ_2) is also close to zero, with a slightly negative point estimate at -0.1% (95% CI = -1.2%, 0.9%). When combining the coefficients, the model implies that the effect on income two years after the intervention is 0.0% with a 95% confidence interval that rules out effects below -1.9% and above 2.0%. The implied effect four years after the intervention is a 0.2% drop, with a 95% confidence interval that rules out effects below 3.7% and above 3.2%. The results for the other outcomes variables are similarly small and indistinguishable from zero. For the VantageScore the dynamic effect is, if anything, sightly negative. In the Appendix Table A8 and Figure A2 we also show dynamic treatment effect estimates from a panel model with person and year fixed effects where we allow the effect of the treatment to vary in each time period relative to the treatment assignment (Sun and Abraham, 2021) and the results are substantively similar.

5.2.3 Distributional Effects

To examine distributional effects, Figure 3 presents the results from quantile regressions applied to the main continuous outcomes including (log) income (left panel) and the VantageScore (right). The estimates suggest that the null effects are stable across the entire distributions. For income, the point estimates are consistently close to zero, including at the bottom and the top of the distribution. For the VantageScore, the point estimates are negative across all quantiles but with small magnitudes and mostly statistically insignificant at conventional levels.

5.3 Survey Data

5.3.1 Three-Year Outcomes

We now turn to the results using the data from the follow-up surveys. As explained above, these surveys included self-reported measures of income and financial distress and therefore allow us to replicate the previous models. Table 7 presents the regression coefficients for our main outcomes of interest. We find that the results from the survey data outcomes are consistent with those from the credit bureau data. We find no evidence that access to citizenship is associated with a discernible impact on household income, equivalized household income, personal income, or financial distress. In fact, the null results are similar than those from the credit bureau data reported above. Table A9 in the Appendix shows that the same conclusion holds when adding a correction for survey non-response using inverse probability weighting. Table A11 in the Appendix shows that the same conclusion also holds when replicating the full panel models using the survey outcomes.

5.3.2 Mechanisms

Prior work has proposed several mechanisms through which citizenship can improve the economic integration of immigrants (e.g. Bratsberg et al., 2002; Liebig, 2011). First, citizenship can signal a higher commitment to stay and invest in one's future in the host country. It, therefore, may encourage immigrants to invest in host country specific human capital because they expect higher returns to these investments after naturalization. Second, it is also possible that citizenship unlocks new employment opportunities, and is therefore associated with improved labor market outcomes such as higher likelihood of being employed or occupational upgrading.

To test for the first mechanism we leverage information on educational loans from the credit files. In particular, we use the probability of having at least one student loan as an outcome variable.[16] If naturalization encourages investments in host country specific human capital, we expect that registrants in the treatment group take on more student loans than those in the control. Overall, about 18% of registrants have taken at least one student loan in the three years after the voucher lottery. We also use an indicator for the registrants being enrolled in school which was the case for 7% of respondents. To test for the second group of mechanisms, we utilize information from the follow-up surveys. Specifically, we analyze the registrants' employment status, labor force participation status as well as information on their occupations.

The results are displayed in Table 8. Again, all outcomes are measured three years after the voucher lottery. The results indicate meaningfully precise null effects for all mechanisms. The 95% confidence interval of the ITT effect on the the probability of having a student loan ranges from a decrease of 2.7 percentage points to an increase of 1.1 percentage points. For the impact on being employed, the 95% confidence interval ranges from a decrease of 6.0 percentage points to an increase of 1.9 percentage points. Similarly, we find no discernible impacts on occupational wages or occupational upgrading.

5.3.3 Non-Economic Integration

The previous null findings on economic returns beg the question of whether access to citizenship may have resulted in gains in terms of other dimensions of immigrant integration. Previous observational research has identified positive impacts of access to citizenship on a variety of non-economic integration outcomes, such as educational strategies of parents (Felfe et al., 2020); social integration (Hainmueller et al., 2017b; Gathmann and Monscheuer, 2020); or political integration (Hainmueller et al., 2015; Street, 2017). To examine this question, we leverage the multi-dimensional integration measure developed by Harder et al. (2018). The findings for the impacts on non-economic integration are presented in Table 9. All outcomes are measured three years after the voucher lottery. Column 1 shows the results for the overall IPL 12 integration index that aggregates all six dimensions into a single metric including economic, political, social, psychological, navigational, and linguistic integration. Column 2 shows the results when we omit the economic dimension from

[16] This analysis was not pre-registered.

the IPL 12 and aggregate the remaining five dimensions. The rest of the columns show the results for each integration dimension separately.

We find no discernible evidence that access to citizenship improves any dimension of immigrant integration. The point estimates are close to zero and are not statistically significant at conventional levels. For example, the point estimate of the ITT effect on the overall integration index is a 0.0004 decrease with a 95% confidence interval that rules out decreases larger than -.015 and increases larger than .014. Given that the mean value of the integration index is 0.65 with a standard deviation of 0.15, these estimates of the null effects are meaningfully precise.

5.3.4 Fears of Deportation

Previous work by Asad (2020) suggests that a high proportion of Latinos report deportation fears, even among lawful permanent residents, and that these reported fears are consistently high since the mid 2000s. This raises the question whether naturalization leads to a reduction in fears of deportation, given that citizenship provides immigrants the strongest legal protection against potential deportation. To examine this we use a survey question embedded in the 2021 follow up survey that asked registrants how much, if at all, they worry that they could be deported.[17]

The results are shown in Table 10. We find that citizenship consistently reduced fears of deportation. For the pooled sample, the ITT estimates show a .15 point reduction in fears of deportation (95% confidence interval from -.24 to -.06) measured on the four-point Likert scale that ranged from "worry a lot" (4) to "not at all" (1). Using the dichotomized measure there was a 7.4 percentage point drop (95% confidence interval from -.11 to -.04) in the probability of having a high fear of deportation (answer options "worry a lot" or "some"). The LATE estimates suggest that citizenship reduced fears by .42 on the four point scale (95% confidence interval from -.66 to -.18), and by 20.1 percentage points (95% confidence interval from -.30 to -.10) for the binary measure, respectively. The other models in the Table 10 show that this reduction in fear of deportation is stable across the three registration cohorts, so regardless of whether the outcome was measured three, four, or five years after the lottery.

[17] This outcome was not pre-specified.

5.4 Selection

How may we reconcile the null findings on economic outcomes from our randomized control design with the observational literature on citizenship that has tended to find positive economic effects of access to citizenship? Some studies have raised concerns about selection bias (Engdahl, 2014; Hainmueller et al., 2019), but others have argued that it can be overcome (Bratsberg et al., 2002; Liebig, 2011; National Academies of Sciences, Engineering, and Medicine, 2015). Our experiment offers a unique opportunity to compare the effect estimates from a standard observational regression versus those from the randomized experiment. A key concern for the non-experimental regression is that immigrants self-select into citizenship based on unobserved confounders such as ability and motivation. In other words, those who choose to apply for citizenship might have better outcomes even in absence of naturalization.

We proceed by ignoring the experimental design and estimating models using the control group sample where the three-year outcomes are regressed on the citizenship indicator and the baseline covariates. Table 11 presents the results. Those that select into citizenship have better three-year financial outcomes. While the effects for income are not significant at conventional levels, the effects are statistically significant at conventional levels for almost all of the other outcomes.[18] In particular, we find that citizenship is associated with a significant 26 points gain in the VantageScore (p-value=0.01), a .31 reduction in delinquencies (p=0.032), a.16 drop in collections (p=0.008), a 0.24 units decrease in the financial distress index (p=0.011), a 5.6% increase in having an open line (p=0.004), a 9.5% increase in having a thick file (p= 0.004), and a 0.05 units increase in the access to credit index (p=0.001).

If we had access only to these observational data, we might conclude that citizenship significantly improves the financial outcomes of immigrants. In addition, the sample consists of only eligible immigrants who were motivated enough to register for a naturalization program and, therefore, motivation is also controlled for by design. Yet, none of these effects are present when we leverage

[18] Note that these tests for selection are likely conservative because we reduce power by only using the control group. In fact, we do find a statistically significant effect on income when using the entire sample instead of just the control group (p=0.05). We also find similar significant effects on income when using the panel model specification only with the control group (p=0.06) or with the full sample (p=0.004). This suggests that the selection problems are present for income as well. In fact, when we run the dynamic version of the panel regression ignoring the experimental design we re-produce the results in Bratsberg et al. (2002) that the effect of citizenship on income grows over time; the interaction term between citizenship and years since citizenship has a p-value of 0.066 in the control group only and 0.061 in the full sample. According to these models citizenship would lead to a 7% income gain within four years.

the experimental variation in citizenship.

6 Discussion

The results from this experimental study of the effects of citizenship provide a somewhat sobering picture of the impact of access to naturalization on the economic prospects of immigrants. While the voucher intervention resulted in a large increase in citizenship uptake, it did not translate into discernible improvements in short to medium term financial outcomes. These results are in contrast with the most highly cited research on the economic impact of citizenship in the United States which found considerable gains from citizenship (Bratsberg et al., 2002; Miller and Chiswick, 1992; Mazzolari, 2009). The results are also in contrast with the conventional wisdom in the literature that has documented positive effects of citizenship in various other countries (Liebig, 2011) and summarized in the Appendix. Similarly, we found no discernible effects on related outcomes such as financial distress, access to credit, and credit scores. There is also no support for the idea that access to citizenship led to higher educational investment, employment, labor force participation, or occupational upgrading. Moreover, we found no discernible effects on non-economic integration using an index that captures social, political, psychological, navigational, and linguistic integration. Lastly, we found that citizenship had a lasting impact on reducing fears of deportation.

How to reconcile these differences in the estimated effects? One possible interpretation is that the previous observational estimates were driven by positive selection. Indeed, when we ignore the experimental variation and compare naturalized with non-naturalized immigrants, we document the presence of positive selection effects with a significant citizenship premium. These effects are present despite the fact that our sample was limited to only motivated immigrants who registered for the program. It stands to reason that these selection effects would be even stronger in observational designs that cannot control for motivation.

Taken together, our results cast some doubt on the prominent claim in the literature that facilitating access to citizenship provides an effective policy lever to accelerate the economic integration of immigrants (Bratsberg et al., 2002; Miller and Chiswick, 1992; Gathmann and Keller, 2018; Hainmueller et al., 2019). At least within the context of this first randomized experiment on access to citizenship, we do not find discernible evidence of an economic citizenship premium in the short to medium term.

How should we judge the external validity of our experimental results? One the one hand, one could argue that our experiment has external validity for a variety of reasons. It took place in New York, a major urban immigrant labor market that shares many similarities with other large U.S. metropolitan areas with a significant share of immigrants, such as Los Angeles, Chicago, Houston, or Atlanta. In fact, currently around 85% of all immigrants in the U.S. live in the top 100 metro areas. In addition, our experiment was embedded in the context of a typical program aiming to promote citizenship among low income immigrants who are interested in naturalization. Moreover, our sample involved low-income immigrants, precisely the group for which most existing research (e.g, Bratsberg et al., 2002; Liebig, 2011) has found the largest returns to citizenship.

On the other hand, our results do not rule out the possibility of an economic citizenship premium in other contexts. Potential moderating factors include labor market flexibility, and the intensity of labor market discrimination based on citizenship status. Indeed, with the courts acceding greater devolution of immigration rights to the states in the 1990s, those states with the largest immigrant populations have been more accommodating to immigrant rights (Schuck, 1997). These factors may well have played a role in helping legal permanent residents in New York City. Another moderating factor is national, and concerns the difficultly of obtaining citizenship. For example, Hainmueller et al. (2019) document positive long-term economic effects of citizenship in Switzerland, a country with more demanding requirements for naturalization compared to the U.S. It is possible that, in a setting where naturalization requires a more costly investment, the signaling value of citizenship is higher, giving rise to a premium. Another factor may be historical time; in earlier eras, for example during America's Great Migration, a quasi-experimental study (Catron, 2019) revealed a substantial citizenship premium. More experimental research from other contexts is needed to better evaluate the extent to which citizenship can act as a catalyst for integration or have an effect on other outcomes. Similarly, our results are limited to short to medium term outcomes, and follow-up work on our sample (or other experimental samples) is needed to consider the impacts on longer term outcomes.

Lastly, recall that our results concern the impact of access to citizenship among immigrants who have already attained permanent residency in the United States. These results therefore do not speak to whether there may be significant economic returns to obtaining other immigrant statuses such as permanent residency (Phillips and Massey, 1999; Cascio and Lewis, 2019) or other forms of legal protection such as deferred action (Hainmueller et al., 2017c; Pope, 2016). Moreover, the results do not show that citizenship has no value for immigrants. In fact, naturalization puts immigrants on a virtually equal legal footing with natives and opens the door to benefits such as the ability to travel and return on a U.S. passport, access to restricted jobs, the right to vote, protection from deportation, and the opportunity to sponsor family members for visas. Consistent with this we did find psychological benefits in terms of reduced fears of deportation.

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7 Figures and Tables

Figure 1: Experimental Design

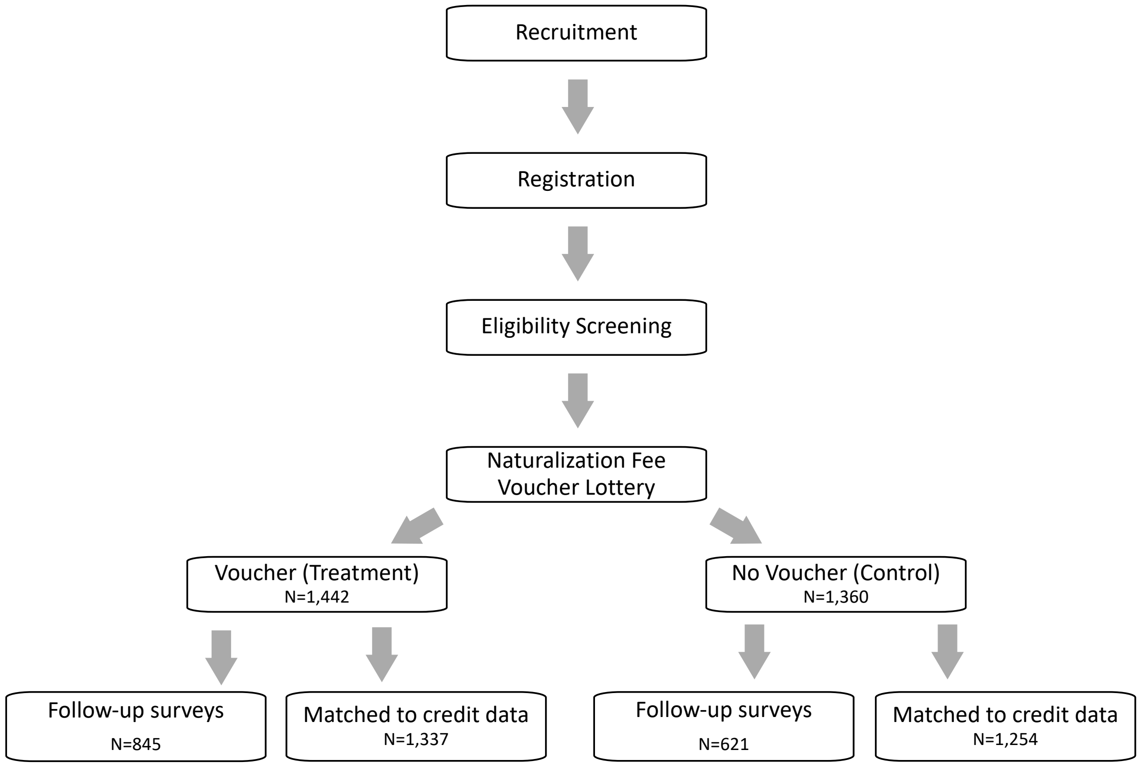


Table 1: Summary Statistics at Baseline (Covariates)

	Overall Mean	Overall SD	Treatment Mean	Control Mean	p-value	N
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Credit Bureau Data			
Registered in English	0.63	0.48	0.64	0.62	0.43	2,329
Above Median HH Income	0.49	0.50	0.48	0.51	0.15	2,329
			Panel B: Registration Data			
Age	42.08	14.03	42.40	41.74	0.22	2,802
Female	0.54	0.50	0.54	0.55	0.72	2,802
High School Graduate	0.27	0.44	0.26	0.27	0.55	2,802
Some College	0.23	0.42	0.23	0.23	0.88	2,802
College Graduate	0.29	0.46	0.29	0.30	0.83	2,802
Registered in English	0.63	0.48	0.63	0.62	0.71	2,802
Registered in Spanish	0.34	0.47	0.33	0.34	0.41	2,802
Green Card Year	2004.50	9.93	2004.46	2004.55	0.80	2,802
Dominican Republic	0.27	0.45	0.25	0.30	0.01	2,802
Ecuador	0.08	0.27	0.08	0.09	0.35	2,802
Colombia	0.06	0.24	0.06	0.06	0.71	2,802
Married	0.40	0.49	0.40	0.41	0.46	2,802
Single	0.33	0.47	0.34	0.32	0.42	2,802

Notes: All variables are measured prior to the voucher assignment. In the credit bureau data the sample is restricted to registrants who are matched to the credit bureau data for the year of the lottery and the three years following the lottery (balanced four-year panel). Column five displays the p-value for the test of difference in means between the two groups.

Table 2: Summary Statistics at Baseline (Pre-Treatment Outcomes)

	Overall Mean (1)	Overall SD (2)	Treatment Mean (3)	Control Mean (4)	p-value (5)	N (6)
	Panel A: Credit Bureau Data					
Income						
Log Income	10.75	0.34	10.76	10.74	0.37	2,271
Credit Score						
VantageScore	629.12	148.41	630.64	627.51	0.61	2,378
Financial Distress						
Delinquency	1.64	1.92	1.66	1.61	0.51	2,378
Collections	0.41	0.84	0.41	0.41	0.80	2,378
Index (Distress)	1.02	1.25	1.04	1.01	0.56	2,378
Access to Credit						
Open Line	0.88	0.32	0.88	0.88	0.93	2,378
Thick File	0.66	0.47	0.66	0.65	0.81	2,378
Has VantageScore	0.97	0.18	0.97	0.97	0.62	2,378
Credit Limit	8.85	1.38	8.88	8.82	0.31	1,971
Index (Access)	0.84	0.26	0.84	0.83	0.82	2,378
Educational Investment						
Student Loans	0.18	0.39	0.19	0.17	0.19	2,378
	Panel B: Registration Data					
Income						
Log Income	10.44	0.35	10.44	10.43	0.40	2,802
Log Equivalized Income	10.13	0.20	10.13	10.13	0.96	2,802
Labor Market Outcomes						
Employed	0.93	0.26	0.94	0.92	0.12	2,202
In Labor Force	0.79	0.41	0.79	0.79	0.98	2,802
Educational Investment						
In School	0.07	0.25	0.07	0.06	0.84	2,802

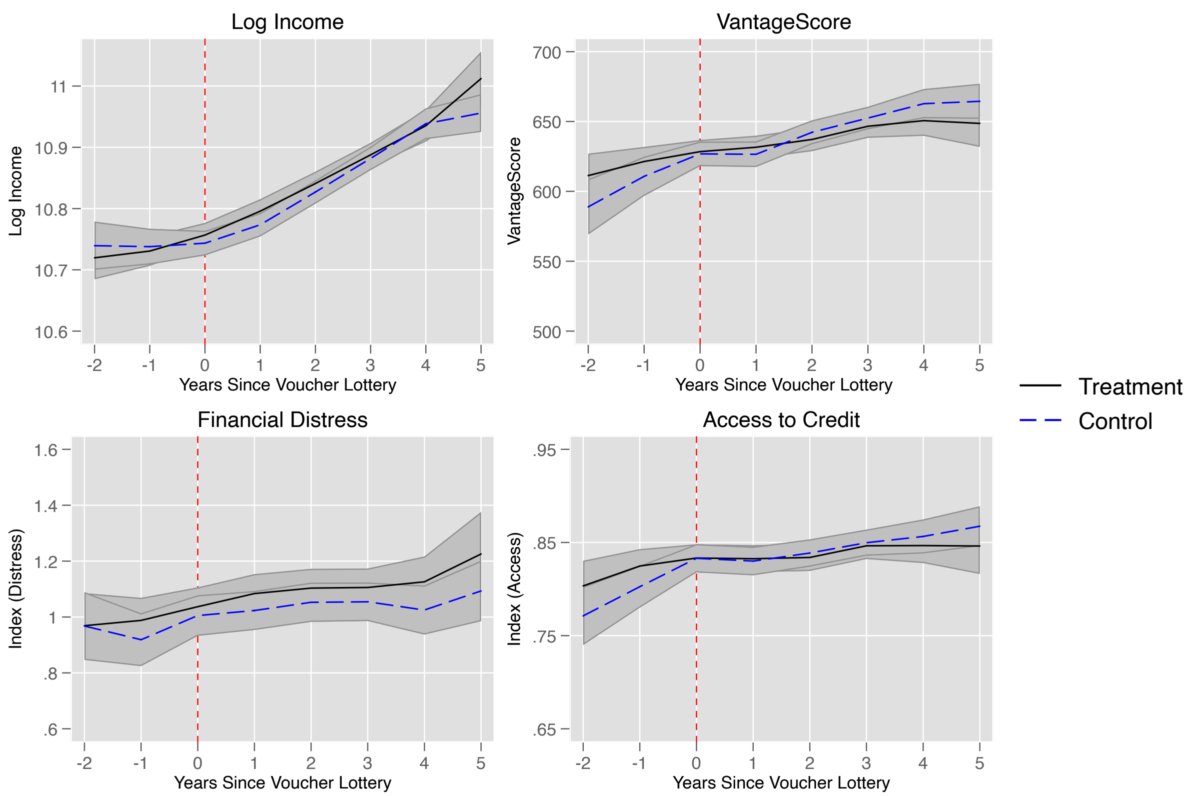
Notes: All variables are measured prior to the voucher assignment. In the credit bureau data the sample is restricted to registrants who are matched to the credit bureau data for the year of the lottery and the three years following the lottery (balanced four-year panel). The last column displays the *p*-value for the test of difference in means between the two groups.

Table 3: First Stage Results

	Credit Bureau Data		Survey Data	
	Citizenship	Citizenship	Citizenship	Citizenship
Voucher	0.346^{***}	0.354^{***}	0.366^{***}	0.369^{***}
	(0.024)	(0.024)	(0.021)	(0.021)
Partial F-statistic	209.29	224.61	293.81	306.41
N	1564	1541	1984	1984
$\bar{Y}$	0.62	0.63	0.62	0.62
Controls	No	Yes	No	Yes

Notes: The outcome is self-reported submission of the naturalization application N-400 at the first year check-in survey and the independent variable is voucher assignment. All regressions control for randomization block dummies and are weighted by the block-level inverse probability of treatment assignment. In the credit bureau data the sample is restricted to registrants who are matched to the credit bureau data for the year of the lottery and the three years following the lottery (balanced four-year panel). Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure 2: Treatment Effects on Financial Outcomes: Graphical Evidence (Credit Bureau Data)



Notes: Outcome means for registrants in the treatment (black solid lines) and control (blue dashed) groups by year relative to the voucher lottery. The outcome variable is denoted in the title of each panel. Note that the sample is restricted to registrants who are matched into the credit data for all six years (i.e. balanced six year panel). N = 2,081. Outcomes are available for years 2018-2021, but the three cohorts registered for the voucher lotteries in 2016, 2017, and 2018, respectively. Therefore the sample includes all three registration cohorts for event years 0, 1, 2, and 3; event year -1 includes the 2017 and 2018 registration cohort; event year -2 includes the 2018 registration cohort; event year 4 includes the 2016 and 2015 registration cohort; and event year 5 includes the 2016 registration cohort. Shaded regions correspond to 95% confidence intervals.

Table 4: Treatment Effects: Three-Year Financial Outcomes (Credit Bureau Data)

Panel A: Intent To Treat Effects (ITT)

	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
Voucher	-0.000	-12.154*	0.104	0.025	0.065
	(0.012)	(5.019)	(0.063)	(0.031)	(0.041)
N	2192	2329	2329	2329	2329
$\bar{Y}$	10.909	655.640	1.781	0.397	1.089

	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
Voucher	-0.029*	0.006	-0.009	-0.012	-0.023
	(0.012)	(0.017)	(0.006)	(0.009)	(0.051)
N	2329	2329	2329	2329	1798
$\bar{Y}$	0.895	0.711	0.974	0.860	9.352

Panel B: Local Average Treatment Effects (LATE)

	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
Citizenship	0.025	-37.492*	0.139	0.099	0.122
	(0.041)	(17.472)	(0.216)	(0.101)	(0.138)
N	1454	1541	1541	1541	1541
$\bar{Y}$	10.896	657.625	1.750	0.352	1.051

	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
Citizenship	-0.063	0.028	-0.025	-0.024	0.016
	(0.040)	(0.058)	(0.022)	(0.030)	(0.163)
N	1541	1541	1541	1541	1217
$\bar{Y}$	0.904	0.741	0.974	0.873	9.406

Notes: The outcome is denoted in the column header and is measured three years after voucher assignment. Panel A presents the ITT effects and Panel B shows the LATE effects. All regressions control for randomization block dummies, HH income, English language as well as the outcome value at baseline. The sample is restricted to registrants who are matched in the balanced four-year panel. All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 5: Treatment Effects: Panel Estimates for Financial Outcomes (Credit Bureau Data)

	Panel A: Intent To Treat Effects (ITT)				
	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
Voucher	0.002	-6.754	0.009	0.022	0.016
	(0.010)	(5.049)	(0.054)	(0.028)	(0.035)
N	11949	12486	12486	12486	12486
$\bar{Y}$	10.842	638.841	1.732	0.420	1.076
	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
Voucher	-0.010	-0.015	-0.010	-0.012	-0.057
	(0.011)	(0.016)	(0.007)	(0.009)	(0.045)
N	12486	12486	12486	12486	10324
$\bar{Y}$	0.877	0.674	0.967	0.839	9.072
	Panel B: Local Average Treatment Effects (LATE)				
	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
Citizenship	0.018	-11.929	-0.046	0.059	0.006
	(0.018)	(9.571)	(0.102)	(0.053)	(0.067)
N	7819	8184	8184	8184	8184
$\bar{Y}$	10.828	641.762	1.684	0.404	1.044
	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
Citizenship	0.008	-0.004	-0.019	-0.005	-0.045
	(0.021)	(0.031)	(0.014)	(0.016)	(0.080)
N	8184	8184	8184	8184	6813
$\bar{Y}$	0.884	0.690	0.967	0.847	9.132

Notes: Each entry is an estimated coefficient from a separate regression of the full panel model. Panel A presents the ITT effects and Panel B shows the LATE effects. All regressions control for individual and year fixed effects. The sample is restricted to individuals who are matched in the balanced six-year panel. All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Standard errors are clustered at the individual level and shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

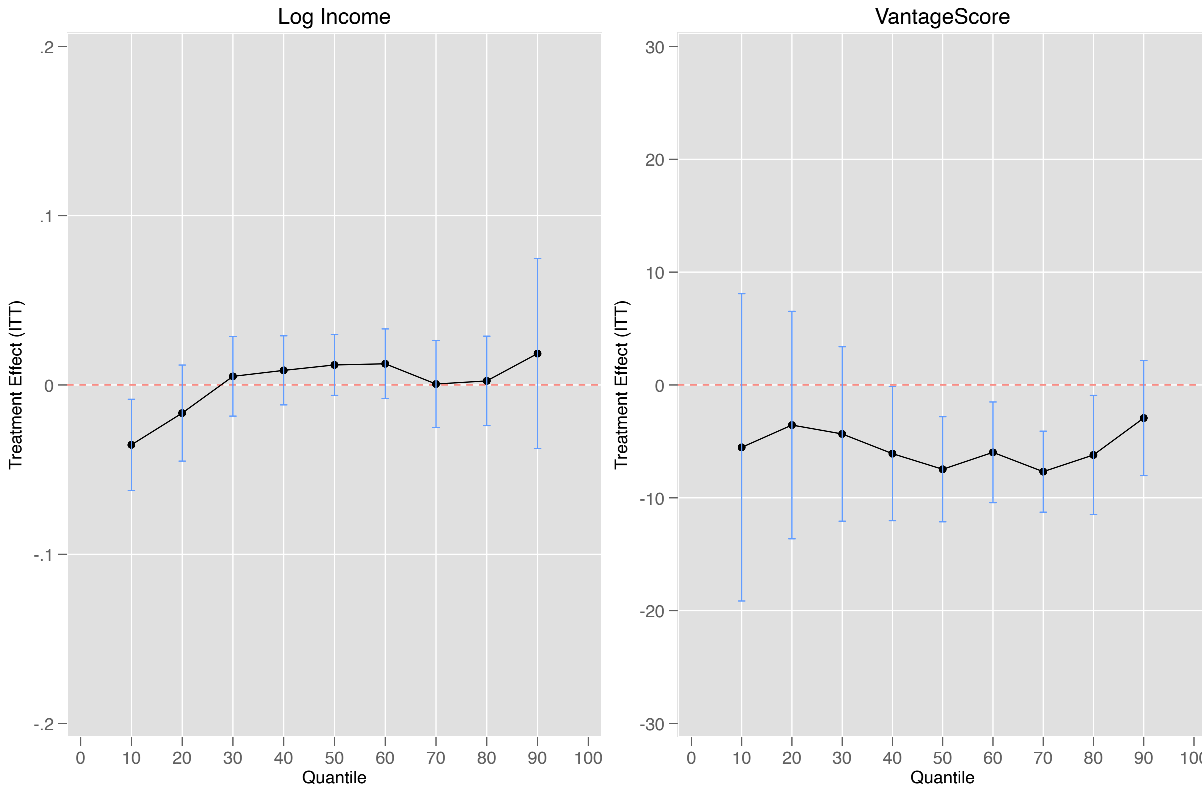
Table 6: Treatment Effects: Dynamic Panel Estimates for Financial Outcomes (Credit Bureau Data)

	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
Voucher	0.003	-0.148	0.023	0.018	0.020
	(0.012)	(5.485)	(0.062)	(0.035)	(0.042)
Voucher × Years Since	-0.001	-3.252	-0.006	0.002	-0.002
	(0.006)	(2.112)	(0.028)	(0.016)	(0.019)
N	11949	12486	12486	12486	12486
$\bar{Y}$	10.842	638.841	1.732	0.420	1.076
Effect After 2 Yrs	0.000	-6.652	0.011	0.022	0.016
Lower 95% CI	-0.019	-16.777	-0.100	-0.033	-0.055
Upper 95% CI	0.020	3.472	0.121	0.078	0.088

	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
Voucher	0.004	-0.013	-0.001	-0.003	-0.038
	(0.014)	(0.019)	(0.008)	(0.010)	(0.053)
Voucher × Years Since	-0.007	-0.000	-0.004	-0.004	-0.008
	(0.006)	(0.008)	(0.003)	(0.004)	(0.023)
N	12486	12486	12486	12486	10324
$\bar{Y}$	0.877	0.674	0.967	0.839	9.072
Effect After 2 Yrs	-0.010	-0.014	-0.010	-0.011	-0.054
Lower 95% CI	-0.032	-0.046	-0.024	-0.028	-0.145
Upper 95% CI	0.013	0.019	0.004	0.006	0.038

Notes: As in Table 5 except that the regressions follow the dynamic panel model. Additionally, the last three rows present the predicted effects two years after the voucher lottery along with the corresponding 95% confidence intervals. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure 3: Quantile Regressions: Three-Year Financial Outcomes (Credit Bureau Data)



Notes: Each dot represents an estimated coefficient from a quantile regression of the outcome on voucher assignment at a specific quantile in the distribution. The outcome is denoted in the title of each panel and is measured three years after voucher assignment. All regressions control for randomization block dummies, HH income, English language as well as the outcome value at baseline. The sample is restricted to registrants who are matched in the four-year panel. All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Vertical lines correspond to 95% confidence intervals obtained using robust standard errors.

Table 7: Treatment Effects: Three-Year Financial Outcomes (Survey Data)

	Panel A: Intent To Treat Effects (ITT)					
	Income			Financial Distress		
	Log Income	Log Equivalized Income	Log Personal Income	Can Afford a \$500 Expense	Can Afford a \$1,000 Expense	Can Afford a \$10,000 Expense
Voucher	-0.003	0.008	0.045	0.009	0.008	0.009
	(0.034)	(0.036)	(0.038)	(0.027)	(0.027)	(0.018)
N	1310	1294	1242	1415	1415	1415
$\bar{Y}$	10.600	10.214	10.392	0.592	0.384	0.124
	Panel B: Local Average Treatment Effects (LATE)					
	Income			Financial Distress		
	Log Income	Log Equivalized Income	Log Personal Income	Can Afford a \$500 Expense	Can Afford a \$1,000 Expense	Can Afford a \$10,000 Expense
Citizenship	-0.028	0.013	0.102	0.065	-0.015	0.012
	(0.093)	(0.099)	(0.104)	(0.075)	(0.074)	(0.050)
N	1045	1032	990	1119	1119	1119
$\bar{Y}$	10.613	10.221	10.412	0.594	0.394	0.124

Notes: The outcome is denoted in the column header and is measured three years after voucher assignment. Panel A presents the ITT effects and Panel B shows the LATE effects. All regressions control for randomization block dummies, a set of baseline covariates as well as the outcome value at registration (when available). All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 8: Treatment Effects on Mechanisms: Three-Year Outcomes (Survey Data)

	Panel A: Intent To Treat Effects (ITT)							
	Educational Investment		Labor Market Outcomes		Occupational Upgrading			
	Student Loans	In School	Employed	In Labor Force	Average Wage	Wage Rank	Average Education	Education Rank
Voucher	-0.008	-0.014	-0.020	-0.014	0.044	6.793	0.115	5.126
	(0.010)	(0.011)	(0.020)	(0.022)	(0.040)	(7.072)	(0.095)	(6.434)
N	2329	1415	890	1415	757	757	757	757
$\bar{Y}$	0.221	0.035	0.902	0.785	10.069	136.564	13.335	166.215
	Panel B: Local Average Treatment Effects (LATE)							
	Educational Investment		Labor Market Outcomes		Occupational Upgrading			
	Student Loans	In School	Employed	In Labor Force	Average Wage	Wage Rank	Average Education	Education Rank
Citizenship	-0.020	-0.075*	-0.052	0.001	0.091	7.449	0.177	6.837
	(0.032)	(0.033)	(0.052)	(0.061)	(0.110)	(19.537)	(0.266)	(17.976)
N	1541	1119	704	1119	604	604	604	604
$\bar{Y}$	0.237	0.040	0.907	0.783	10.080	138.843	13.407	171.327

Notes: The outcome is denoted in the column header and is measured three years after voucher assignment. Panel A presents the ITT effects and Panel B shows the LATE effects. All regressions control for randomization block dummies, a set of baseline covariates as well as the outcome value at registration (when available). All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

† The data source for "Student Loans" is the credit bureau while all other outcomes come from the survey data.

Table 9: Treatment Effects on Non-Economic Integration: Three-Year Outcomes (Survey Data)

	Panel A: Intent To Treat Effects (ITT)							
	Overall Index	Overall Index*	Political Integration	Economic Integration	Linguistic Integration	Navigational Integration	Psychological Integration	Social Integration
Voucher	-0.000	-0.001	0.003	0.003	0.002	-0.012	0.010	-0.010
	(0.007)	(0.008)	(0.013)	(0.014)	(0.010)	(0.015)	(0.010)	(0.015)
N	1184	1307	1358	1217	1363	1337	1364	1341
$\bar{Y}$	0.652	0.660	0.622	0.619	0.769	0.649	0.768	0.471
	Panel B: Local Average Treatment Effects (LATE)							
	Overall Index	Overall Index*	Political Integration	Economic Integration	Linguistic Integration	Navigational Integration	Psychologic Integration	Social Integration
Citizenship	0.003	0.007	0.017	-0.005	0.016	-0.023	0.038	-0.003
	(0.020)	(0.020)	(0.034)	(0.038)	(0.028)	(0.041)	(0.029)	(0.040)
N	948	1044	1080	970	1085	1063	1086	1068
$\bar{Y}$	0.656	0.663	0.627	0.622	0.781	0.650	0.765	0.473

Notes: The outcome is denoted in the column header and is measured three years after voucher assignment. "Overall Index*" refers to an integration index excluding the economic dimension. Panel A presents the ITT effects and Panel B shows the LATE effects. All regressions control for randomization block dummies, a set of baseline covariates as well as the outcome value at registration (when available). All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 10: Treatment Effects: Fear of Deportation (Survey Data)

	Panel A: Intent To Treat Effects (ITT)							
	Pooled Outcomes		3-year Outcomes		4-year Outcomes		5-year Outcomes	
	Fear	High Fear	Fear	High Fear	Fear	High Fear	Fear	High Fear
Voucher	-0.148***	-0.074***	-0.129*	-0.053*	-0.029	-0.077**	-0.284***	-0.104***
	(0.045)	(0.019)	(0.069)	(0.028)	(0.090)	(0.038)	(0.086)	(0.035)
N	1290	1290	587	587	346	346	357	357
$\bar{Y}$	1.372	0.106	1.401	0.114	1.344	0.101	1.353	0.100

	Panel B: Local Average Treatment Effects (LATE)							
	Pooled Outcomes		3-year Outcomes		4-year Outcomes		5-year Outcomes	
	Fear	Low Fear	Fear	Low Fear	Fear	Low Fear	Fear	Low Fear
Citizenship	-0.419***	-0.201***	-0.404*	-0.193**	-0.278	-0.277**	-0.549***	-0.197***
	(0.123)	(0.051)	(0.225)	(0.091)	(0.286)	(0.132)	(0.166)	(0.067)
N	1045	1045	467	467	256	256	322	322
$\bar{Y}$	1.391	0.109	1.410	0.113	1.380	0.110	1.372	0.103

Notes: The outcome is denoted in the column header. The "Fear" outcome captures responses to a question that asked registrants how much, if at all, they worry that they could be deported. The answer options are coded on a four-point scale including "worry a lot" (4), "some" (3), "not much" (2), or "not at all" (1). The "High Fear" outcome is coded as a binary indicator for answers "worry a lot" (4) and "some" (3) versus answers "not much" (2), or "not at all" (1). Panel A presents the ITT effects and Panel B shows the LATE effects. All regressions control for randomization block dummies, and a set of baseline covariates. All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 11: Non-Experimental Estimates: Three-Year Outcomes (Credit Bureau Data)

	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
Citizenship	0.029	26.185**	-0.314*	-0.158**	-0.236*
	(0.024)	(10.072)	(0.147)	(0.060)	(0.093)
N	722	744	744	744	744
$\bar{Y}$	10.871	659.500	1.745	0.353	1.049

	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
Citizenship	0.056**	0.095**	0.012	0.054**	0.176
	(0.019)	(0.033)	(0.011)	(0.017)	(0.110)
N	744	744	744	744	653
$\bar{Y}$	0.913	0.728	0.976	0.872	9.237

Notes: The outcome is denoted in the column header and is measured three years after voucher assignment. The independent variable of interest is the self-reported submission of the naturalization application (N-400 form). The sample is restricted to registrants who are matched in the four-year panel and did not receive a voucher. All regressions control for HH income and English language. Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Appendix A: For Online Publication

A1.1 Experiment Timeline and Sample Size

Table A1 presents the timeline of the experiment for each of the three registration cohorts. The symbol "#" denotes survey waves with outcomes data. For instance, the September 2019 survey contains one-year outcomes for the cohort that registered for the lottery in 2018, two-year outcomes for the 2017 registration cohort, and three-year outcomes for the 2016 registration cohort.

Next, Table A2 shows the sample sizes by treatment and control groups for each year and randomization block. In total, 1,442 out of 2,802 registrants were randomly chosen to receive a voucher for the naturalization application, while 1,360 people were selected to be in the control group.

A1.2 Balance Tests

A1.2.1 Survey Data

Table A3 presents balance tests using the survey data. The outcome variable is voucher assignment and it is regressed on the covariates measured at baseline. In Column 1 we use the full sample and in Column 2 we restrict the sample to registrants who have responded to the income question at the three-year survey. In the bottom of the table we display the F-statistic for the test of joint significance of all variables along with the associated p-value. We use robust standard errors. The F-statistics are 0.5 and 0.9 in Columns 1 and 2 respectively. Overall, we find that the two groups are well balanced on the covariates as expected given the random assignment in the lottery.

A1.2.2 Credit Bureau Matching Rate

We now turn to testing for balance in the credit bureau match rate between the treatment and control groups. As mentioned in the main text, we provided the credit bureau with a spreadsheet containing each registrant's name, address and date of birth, which they used to match the records to their credit data. This resulted in 93.3% of the registrants being matched by the bureau's proprietary algorithm in at least one year. A non-match may result from either incorrect demographic information or a lack of a credit file. The credit bureau returned to us data files with credit information for each matched registrant in each year during the 2016-2021 period. However, not every registrant had a credit file in each of these six years. Hence, all registrant could be split into three groups – those who were not matched in any of the five years (group I), those who were matched in only some years (group II), and those who were matched in all years (group III). If a registrant was matched in a given year, this implied they were matched in all subsequent years (i.e., there is no attrition). For instance, a registrant might first appear in our data in 2018 which means we have their information for 2018, 2019, 2020, and 2021, but not for 2016 or 2017. This means that some registrants in group II could be matched in every year in the three years following voucher assignment, while others could not. The group of registrants that were matched in the four years starting with the year of the lottery (i.e., are in the balanced four-year panel) is the sample of registrants for which we estimate most of our results on the impact of citizenship.

We conduct tests of whether the match rate was similar in the treatment and control group. In Columns 1-2 of Table A4 we regress an indicator for being in group I (relative to groups II and III) on voucher assignment. Note that the total number of observations (2,802) equals the total number of people registered in the experiment as shown in Table A2. Also note that, as mentioned above, this match rate was 93.3% (shown in the bottom).

In Columns 3-4 we regress an indicator for being matched in every year during the 2016-2021 period. As expected, this match rate was the lowest – about 74.3%. Similarly, in Columns 5-6 we present the same results for being matched in the first three years after voucher assignment. The match rate to the four-year panel was around 85.0%. We use robust standard errors. None of the coefficients are statistically significant, indicating a similar match rate in the treatment relative to the control group.

A1.3 Survey Attrition Test

Table A5 displays the results from the survey attrition tests. The outcome variable is an indicator for the registrant having responded to the income question at the three-year check-in survey. In Column 1 we regress it on an indicator for voucher assignment. In Column 2 we add a set of demographic control variables and in the last column we interacted these variables with the treatment indicator. For conciseness, we show only the regression coefficients and omit the standard errors. In the bottom of the table we present F-statistics for joint significance of all covariates along with the associated p-values.

Registrants in the treatment group were about 16.1% more likely to have responded to the three-year survey. This also depends on their individual characteristics – registrants who were college graduates and those who had more recently obtained their green card were more likely to respond in general. The F-statistics in all three columns are large (p-values $< .000$), confirming this pattern. However, attrition would bias our estimates only if this pattern were different by treatment and control groups, e.g. if college graduates from the treatment group were more likely to respond than college graduates in the control group. To test for this, in Column 3 we interact all covariates with the treatment indicator. None of 15 coefficients are significant and the F-statistic for the joint significance of these interactions is equal to .87 with p-value of 0.60. Overall, while we see that demographics played a role in attrition, we find no strong evidence that this pattern differed by treatment assignment. Consistent with this we find that the main results are similar when correcting for attrition using inverse probability weighting (see Table A9).

A1.4 Robustness Checks and Additional Results

In this subsection we present several robustness checks of our main results.

- *Event Study Plots for Other Outcomes:* Figure A1 follows Figure 2 and shows the event study plot results for the remaining outcome variables. None of these plots presents evidence of a discernible positive impact of citizenship on financial outcomes.

- *Correcting For Attrition:* Table A9 replicates Table 4 while multiplying the weights by the inverse probability of responding to the income question in the three-year survey. This is a common method for correcting for differential attrition between the treatment and control groups. The results are similar to those without the correction. None of the coefficients are positive and significant, thus indicating no discernible effect of citizenship.

- *Panel Results with Survey Outcomes:* In Table A11 we present the full panel results for the survey data. Note that here we are limited in the number of outcome variables that we can use as only some of the outcome questions were asked at baseline. Again, we find no evidence that access to citizenship affects income or some of the mechanisms that might drive improvement in financial standing such as labor market improvements and educational investment.

- *Randomization Inference:* Table A6 presents the results from randomization inference on our ITT estimates presented in Panel A of Table 4. Each entry is a p-value obtained from 1,000 random permutations of the treatment indicator and estimating our three-year outcomes ITT model. It corresponds to the probability that these permutations produce more extreme ITT estimates than the ones from Panel A of Table 4. None of the p-values are smaller than conventional testing levels, indicating that we cannot reject the sharp null of no effect of access to citizenship on financial outcomes.

- *Quantile Regression results with Survey Outcomes:* Quantile regression results from the survey data are displayed in Figure A3. Each dot represents an estimated coefficient of the impact of access to citizenship on the outcome variable – income (left panel) or overall integration index (right). The vertical bars correspond to 95% confidence intervals. None of the coefficients are statistically significant.

- *Multiple Hypothesis Testing Correction:* Throughout the paper we use various likely correlated outcome variables which can raise concerns about multiple hypothesis testing issues. To correct for this, we applied the Holland-Copenhaver (Holland and Copenhaver, 1987) and the Benjamini-Hochberg (Benjamini and Hochberg, 1995) procedures which control the family-wise error rate (FWER) and the false discovery rate (FDR) respectively and calculated "adjusted" p-values. The results are shown in Tables A7 (credit bureau data outcomes) and A10 (survey data outcomes). In Columns 1 and 2 we display the (unadjusted) p-values that correspond to the estimated coefficients presented in Tables 4, 7, 8, and 9 associated with the two-sided null hypotheses that their true values are zero. In Columns 2 and 5 we show the adjusted q-values controlling for the FWER and in Columns 3 and 6 we control for the FDR. None of the adjusted p-values in either of the two tables falls below 0.05 (or 0.1) indicating a lack of statistically significant coefficients once we correct for multiple hypothesis testing.

- *Dynamic Treatment Effects:* Table A8 and Figure A2 show dynamic treatment effect estimates from a panel model. Here we replicate the models show in Table 5 with person and year fixed effects and use a set of leads and lags of the treatment indicator that indicates a specified period relative to the timing of the treatment assignment (see, for example, Sun and Abraham (2021)). The estimating equation is $y_{it} = \delta_i + \sigma_t + \sum_l \gamma_l 1\{t - E_i = l\} + \epsilon_{it}$ where the terms δ_i and σ_t represent individual and year fixed effects, and E_i indicates the year t when registrant i received the voucher treatment. The reference category is $l = -1$, i.e., the year before the voucher assignment. The results are substantively similar to the other panel models.

- *Results with Delta of Three-Year Outcomes:* Following our pre-analysis plan, Table A12 replicates the main models for the Credit Bureau data using as the outcome the change between the three-year outcome and the pre-treatment outcome. Note that the baseline outcome is removed from the set of controls. The results are similar to those presented in Table 4 suggesting that the results are robust to specifying the model in changes from baseline rather than levels.

- *Characteristics of Compliers* Here we conduct analyses to characterize the sub-poplation of compliers for the LATEs. Recall that the compliers in our analysis are those registrants who naturalize when they receive the voucher but do not naturalize when they do not receive the voucher in the lottery. Since we do not observe directly who is a complier, we use the Abadie (2003) kappa weighting approach to estimate mean pre-treatment covariate values for compliers and compare them to the mean pre-treatment covariate values in the overall sample of registrants. The results are displayed in Table A13. We find that the compliers are very similar to the overall sample across the covariates in both the registration and the credit bureau data. This supports the external validity of the LATE estimation.

- *Results with Two-Year Outcomes:* Tables B1-B4 in Appendix B show the results for two-year outcomes which we specified in the pre-analysis plan. The results are similar to the three-year outcomes.

- *Results for Sub-Groups:* Tables B5-B38 show in Appendix B show the show estimates that replicate the main models for samples restricted to specific sub-groups of registrants defined by specific pre-treatment measures. Note that these estimates are based on smaller sample sizes and therefore should be interpreted with caution. The null effects are largely consistent across the subsets, but less precisely estimated than the models that leverage the full samples. A small number of effects are significant for some outcomes in some groups, but this is to be expected even if there are no effects given the testing across multiple subgroups. There is no sub-group that shows any consistently significant effects across multiple related outcomes.

- *Results by Registration Cohort:* Tables B39-B50 in Appendix B show estimates that replicate the main models for samples restricted to each specific registration cohort, i.e. those who registered for the 2016, 2017, or 2018 lottery, respectively. Note that these estimates are based on smaller sample sizes and therefore should be interpreted with caution. Just as with the other subgroups, the null effects are largely consistent across all three cohorts, but less precisely estimated than the models that leverage the full samples. A small number of effects are significant for some outcomes in some cohorts, but this is to be expected even if there are no effects given the testing across multiple subgroups. There is no cohort that shows any consistently significant effects across multiple related outcomes.

A1.5 Occupation Codes Classification

In follow-up surveys, we asked registrants to indicate their occupations. This was an open-ended question, so respondents could write what they thought best described their occupation. This required us to translate answers into standardized codes that would allow us to estimate the expected income of each subject given his/her job classification. Below we describe the procedure for translating the job description of subjects into an estimate of expected wage and education level using the the 2018 IPUMS American Community Survey (ACS).

1. Relying on Codes for Occupation SOC (OCCSOC), we had three coders translate the 1349 unique answers on the survey to the categories in OCCSOC. One of the coders covered all rows; two of the coders split the sample in half.

2. Although SOC codes had a specificity based on 6 digits, we determined that we could make reasonable judgments at the 5-digit level. Note that there are about 400 unique occupations at the 5-digit level.

3. Coders were able to classify 1080 rows (80%). In these 1080 rows, coders classified 210 unique 5-digit occupation categories. Of these inter-coder reliability was about 70%.

4. For those cases where the codings differed, we followed two procedures.

   4a. First, we scraped the O*NET Code Connector web page which contains the description of each occupation. We then used a fuzzy matching algorithm to have a third set of codes for each unique occupation from the survey. Where the fuzzy match agreed with one of the two manual codes, we automatically chose that coding.

4b. Where the fuzzy match did not help distinguish the two manual codes, we reviewed the ACS wage data for each of the two manually listed occupation codes. If the census wage data for each occupation code was within 0.25 standard deviation (SD) of the mean for all occupations, one of the two manually entered occupation codes was chosen at random. However, if the income data for each of the two occupation codes was above 0.25 SD, that case was marked as a missing value and not included in estimations on the return to citizenship for occupational status.

5. For each unique 5-digit occupation we obtained the average (log) yearly wage, average years of education, as well as the yearly wage rank and the education rank (on the 1-399 scale) using the 2018 ACS. We then merged this information back to the survey responses.

A1.6 Literature Review

Table A14 contains a list of academic papers studying the link between citizenship and financial outcomes along with some information on the underlying analyses they perform.

The intuitive idea that citizenship may constitute a powerful policy instrument to speed up immigrants' economic integration was initially put to an econometric test in Chiswick (1978) where no significant effect was found. But fourteen years later, a seminal paper of Bratsberg et al. (1992) revealed a "citizenship premium" in the United State for those immigrants that receive it. Bratsberg et al's seminal paper, using cross-sectional data from the Current Population Survey (CPS) and a panel of foreign-born respondents from the National Longitudinal Survey of Youth (NLSY), estimated about a six to twelve percent citizenship premium in earnings and faster wage growth immediately after naturalization. They concluded that "naturalization accelerates the process of labor market assimilation" by removing employment barriers and encouraging investment in U.S.-specific human capital. In that same year, Miller and Chiswick (1992) similarly reported a significant citizenship premium in the U.S. and Canada. On the basis of these studies and a few less well-identified papers, an expert panel from the National Academy of Sciences (NAS) concluded that "a meaningful citizenship premium remains" (National Academies of Sciences, Engineering, and Medicine, 2015, p. 13). To be sure, NAS experts and the research community recognized that the Bratsberg et al. paper had low power due to few observations. Less obvious was that it did not fully address the selection issue. Even in a panel set-up, immigrants who have private information on the returns they could realize should they apply for citizenship, may have biased upwards the estimate on the citizenship premium.

From these studies a conventional wisdom has emerged that the citizenship premium is significant in many OECD countries. While the magnitude of the citizenship premium in wages varies from around 5% in Miller and Chiswick (1992) to 28% in Govind (2020), the large majority of studies find positive and significant effects at least for some groups or countries. To provide an unbiased summary of findings supporting this conventional wisdom, we chose the most cited papers in Google Scholar that themselves cited either Chiswick's or Bratsberg et al.'s seminal articles in which the abstract advertised a statistical test of the returns to citizenship. Of the seventeen papers with reported results, thirteen (76%) inferred from their data that there was a significant earnings premium for at least one country under study and/or a major immigrant group. We then conducted a snowball sample of relevance based on references from studies in leading journals. Here we analyzed twenty articles, with fourteen (70%) inferring a significant citizenship premium. In the combined set of papers (seminal, cited and snowball), twelve focused primarily on U.S. data;

of these, ten (83%) reported a positive naturalization coefficient. The two most prominent summaries of this literature (Liebig, 2011; National Academies of Sciences, Engineering, and Medicine, 2015) report a positive and significant coefficient on the average returns to citizenship. Another pattern that emerged from this literature is that there are heterogeneous effects for the citizenship premium. Many studies find that the citizenship premium is concentrated among immigrants from poorer countries and those that are more disadvantaged in the labor market. This gives support to the claim in our study that the sampled population was the one most likely to enjoy a citizenship premium.

A2 Additional Figures and Tables

Table A1: Timeline

Cohort 2016	
Registration ends	September 2016
Survey #1	March 2017
Survey #2	November 2017#
Survey #3	July 2018#
Survey #4	September 2019#
Survey #5	November 2020#
Survey #6	December 2021#
Cohort 2017	
Registration ends	August 2017
Survey #1	July 2018#
Survey #2	May 2019
Survey #3	September 2019#
Survey #4	November 2020#
Survey #5	December 2021#
Cohort 2018	
Registration ends	July 2018
Survey #1	May 2019
Survey #2	September 2019#
Survey #3	November 2020#
Survey #4	December 2021#

The symbol "#" denotes survey waves with outcome data. For example, the September 2019 survey contains 1-year outcomes for the cohort that registrants for the lottery in 2018, 2-year outcomes for the 2017 registration cohort, and 3-year outcomes for the 2016 registration cohort.

Table A2: Sample Size by Registration Cohort and Randomization Block

Cohort	Block ID	Treatment	Control
2016	1	23	6
2016	2	18	11
2016	3	20	8
2016	4	248	502
2017	5	168	93
2017	6	235	145
2017	7	38	8
2017	8	49	10
2018	9	244	217
2018	10	305	295
2018	11	41	20
2018	12	53	45
Total	2,802	1,442	1,360

Notes: Number of registrants in each geographic lottery block by treatment and control assignment. The treatment group are registrants who were selected to receive the voucher in the lottery and control group are registrants who were not selected.

Table A3: Balance Test for Lottery Assignment (Registration Data)

	Voucher (1)	Voucher (2)
Log Income	0.036	0.017
	(0.034)	(0.048)
Green Card Year	0.000	-0.004*
	(0.001)	(0.002)
Age	0.001	0.001
	(0.001)	(0.001)
Female	-0.002	-0.031
	(0.020)	(0.029)
Dominican Republic	-0.039	-0.010
	(0.025)	(0.038)
Ecuador	-0.030	-0.035
	(0.039)	(0.058)
Colombia	-0.006	0.081
	(0.043)	(0.057)
Married	0.024	0.033
	(0.027)	(0.041)
Single	0.066*	0.063
	(0.028)	(0.042)
High School Graduate	-0.009	-0.034
	(0.030)	(0.048)
Some College	0.008	0.017
	(0.032)	(0.049)
College Graduate	-0.005	-0.018
	(0.032)	(0.047)
Registered in English	-0.018	0.052
	(0.054)	(0.075)
Registered in Spanish	-0.021	0.035
	(0.058)	(0.080)
N	2802	1310
$\bar{Y}$	0.500	0.586
F-statistic	0.494	0.861
p-value	0.616	0.393

Notes: The outcome is the treatment indicator (i.e., being selected for the voucher in the lottery). In Column 2 the sample is restricted to registrants who have responded to the income question in the survey three years after voucher assignment. The F-statistics and p-values shown in the bottom rows are computed from an omnibus test against the joint null that all regression coefficients are equal to zero. All regressions are weighted by the block-level inverse probability of assignment into the treatment group and control for randomization block dummies. Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4: Balance Test for Matching to Credit Bureau Data

	Matched Ever		In Balanced 6-Year Panel		In Balanced 4-Year Panel	
	(1)	(2)	(3)	(4)	(5)	(6)
Voucher	-0.003	0.000	-0.014	0.005	0.002	0.000
	(0.009)	(0.010)	(0.017)	(0.017)	(0.014)	(0.014)
N	2802	2738	2802	2738	2802	2738
$\bar{Y}$	0.933	0.934	0.743	0.744	0.849	0.851
Controls	No	Yes	No	Yes	No	Yes

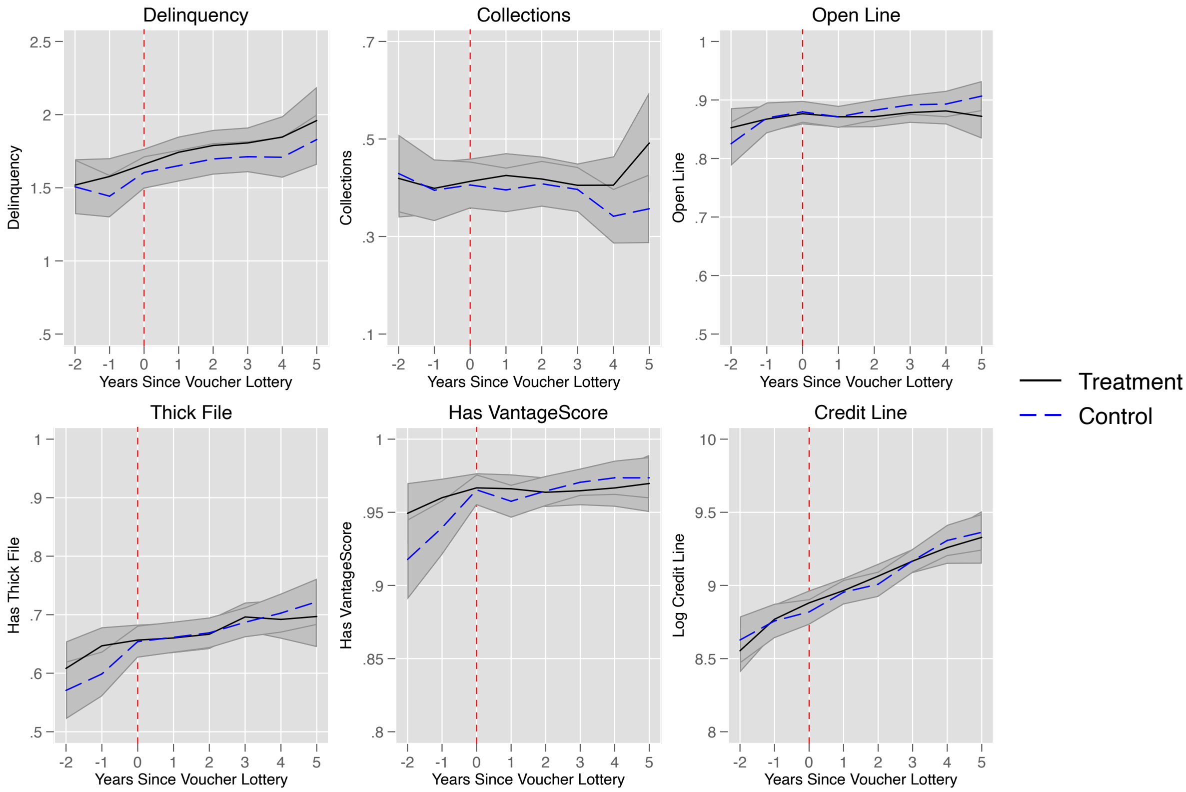
Notes: The outcome in Columns 1-2 is an indicator for being matched by the credit bureau for at least one year between 2016 and 2021 while in Columns 3-4 it is an indicator for being matched in all six years of the credit bureau data. In Columns 5-6 it is an indicator for being matched into the balanced 4-year panel, i.e. the four years starting with the year of the lottery and the three following years. The independent variable of interest is voucher assignment. Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A5: Attrition Test for Participation in Follow-up Surveys

	Responded Three-Year Survey (1) b	Responded Three-Year Survey (2) b	Responded Three-Year Survey (3) b
Voucher	0.161***	0.158***	4.582
Log Income		0.059	0.064
Green Card Year		0.005***	0.006***
Age		-0.002	-0.001
Female		0.034	0.051
Dominican Republic		-0.045	-0.068*
Ecuador		-0.041	-0.039
Colombia		-0.015	-0.088
Married		0.063*	0.048
Single		0.059*	0.048
High School Graduate		0.058*	0.058
Some College		0.084**	0.050
College Graduate		0.135***	0.118**
Registered in English		0.000	-0.100
Registered in Spanish		-0.002	-0.078
Voucher X Log Income			-0.012
Voucher X Green Card Year			-0.002
Voucher X Age			-0.001
Voucher X Female			-0.035
Voucher X Dominican Republic			0.042
Voucher X Ecuador			-0.004
Voucher X Colombia			0.144
Voucher X Married			0.034
Voucher X Single			0.026
Voucher X High School Graduate			-0.006
Voucher X Some College			0.058
Voucher X College Graduate			0.024
Voucher X Registered in English			0.180
Voucher X Registered in Spanish			0.133
Voucher X Time b/w Reg. & Randomiz.			-0.000
N	2802	2802	2802
$\bar{Y}$	0.471	0.471	0.471
F-stat (all)	9.239	10.847	7.471
p-value (all)	0.000	0.000	0.000
F-stat (interactions)			0.865
p-value (interactions)			0.604

Notes: The Table shows coefficient estimates from an OLS regression model. The outcome is an indicator for the individual having responded to the income question in the survey three years after voucher assignment. The F-statistics and p-values labeled 'all' in the bottom rows are computed from an omnibus test against the joint null that all regression coefficients are equal to zero; the F-statistics and p-values labeled 'interactions' in the bottom rows are computed from an omnibus test against the joint null that all the regression coefficients for the interactions are equal to zero. All regressions are weighted by the block-level inverse probability of assignment into the treatment group and control for randomization block dummies. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure A1: Treatment Effects: Additional Graphical Evidence (Credit Bureau Data)



Notes: Outcome means for registrants in the treatment (black solid lines) and control (blue dashed) groups by year relative to the voucher lottery. The outcome variable is denoted in the title of each panel. Note that the sample of registrants changes with the event year. Shaded regions correspond to 95% confidence intervals.

Table A6: Treatment Effects: Randomization Inference (Credit Bureau Data)

	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
p-value	0.88	0.02	0.07	0.44	0.10

	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
p-value	0.01	0.71	0.20	0.21	0.57

Notes: Each entry is a p-value obtained from 1,000 permutation tests by randomly rearranging the treatment indicator and running our three-year outcomes ITT model. The p-values correspond to the probability that these permutations produce more extreme ITT effects (i.e., two-sided tests). The underlying regressions are unweighted. All other aspects of the estimation are as in Panel A of Table 4.

Table A7: Multiple Testing Correction of Three-Year Results (Credit Bureau Data)

Outcome	ITT			LATE		
	Unadjusted p-value (1)	Adjusted FWER (2)	Adjusted FDR (3)	Unadjusted p-value (4)	Adjusted FWER (5)	Adjusted FDR (6)
Income						
Log Income	1.00	1.00	1.00	0.55	0.95	0.69
Credit Score						
VantageScore	0.02	0.13	0.08	0.03	0.28	0.32
Financial Distress						
Delinquency	0.10	0.58	0.29	0.52	0.95	0.69
Collections	0.42	0.89	0.60	0.33	0.94	0.69
Index (Distress)	0.12	0.58	0.29	0.38	0.94	0.69
Access to Credit						
Open Line	0.01	0.13	0.08	0.12	0.68	0.60
Thick File	0.71	0.96	0.79	0.63	0.95	0.70
Has VantageScore	0.16	0.66	0.33	0.25	0.90	0.69
Credit Limit	0.65	0.96	0.79	0.92	0.95	0.92
Index (Access)	0.20	0.67	0.33	0.43	0.94	0.69

Notes: Columns 1 and 4 present the p-values of the coefficients displayed in Panels A and B of Table 4, respectively. Columns 2 and 5 show the q-values after the Holland-Copenhaver adjustment for multiple hypothesis testing controlling the Family-wise Error Rate (FWER). Columns 3 and 6 display the q-values after the Benjamini-Hochberg correction controlling the False Discovery Rate (FDR).

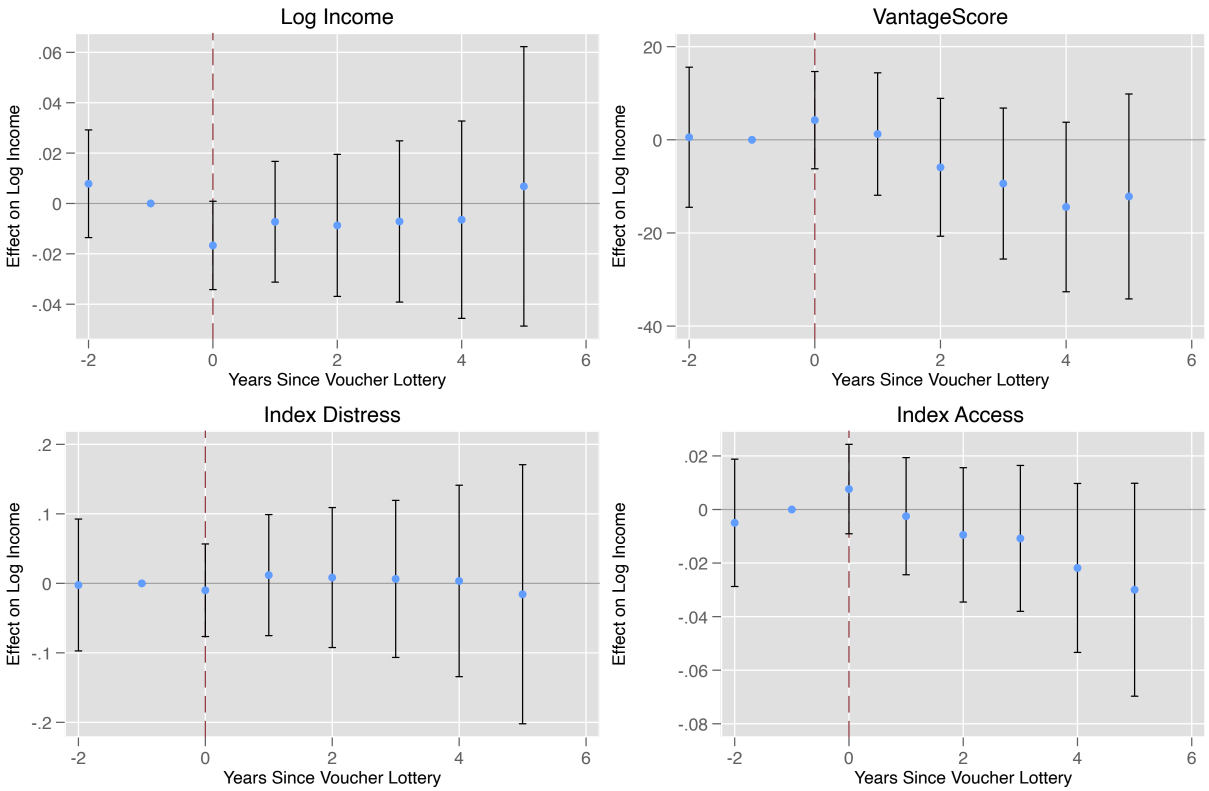
Table A8: Dynamic Treatment Effects: Panel Estimates (Credit Bureau Data)

	Panel A: Intent To Treat Effects (ITT)				
	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
Years Since Lottery: -2	0.008	0.544	-0.020	0.015	-0.002
	(0.011)	(7.670)	(0.074)	(0.036)	(0.048)
Years Since Lottery: 0	-0.017	4.215	-0.008	-0.012	-0.010
	(0.009)	(5.324)	(0.051)	(0.028)	(0.034)
Years Since Lottery: 1	-0.007	1.240	0.004	0.019	0.012
	(0.012)	(6.702)	(0.068)	(0.035)	(0.044)
Years Since Lottery: 2	-0.009	-5.905	0.001	0.016	0.008
	(0.014)	(7.548)	(0.078)	(0.041)	(0.051)
Years Since Lottery: 3	-0.007	-9.398	0.003	0.010	0.006
	(0.016)	(8.264)	(0.088)	(0.045)	(0.058)
Years Since Lottery: 4	-0.006	-14.430	-0.013	0.020	0.003
	(0.020)	(9.280)	(0.107)	(0.055)	(0.070)
Years Since Lottery: 5	0.007	-12.164	-0.068	0.036	-0.016
	(0.028)	(11.218)	(0.139)	(0.079)	(0.095)
N	11949	12486	12486	12486	12486
$\bar{Y}$	10.842	638.841	1.732	0.420	1.076

	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
Years Since Lottery: -2	0.007	-0.025	0.003	-0.005	-0.133*
	(0.015)	(0.024)	(0.011)	(0.012)	(0.063)
Years Since Lottery: 0	0.016	0.002	0.005	0.008	0.031
	(0.012)	(0.016)	(0.007)	(0.009)	(0.041)
Years Since Lottery: 1	0.006	-0.014	0.000	-0.003	-0.037
	(0.015)	(0.021)	(0.009)	(0.011)	(0.056)
Years Since Lottery: 2	0.001	-0.021	-0.008	-0.009	-0.050
	(0.017)	(0.023)	(0.010)	(0.013)	(0.066)
Years Since Lottery: 3	-0.006	-0.010	-0.017	-0.011	-0.060
	(0.018)	(0.025)	(0.012)	(0.014)	(0.072)
Years Since Lottery: 4	-0.014	-0.032	-0.020	-0.022	-0.081
	(0.021)	(0.030)	(0.013)	(0.016)	(0.086)
Years Since Lottery: 5	-0.036	-0.034	-0.020	-0.030	-0.104
	(0.027)	(0.039)	(0.015)	(0.020)	(0.113)
N	12486	12486	12486	12486	10324
$\bar{Y}$	0.877	0.674	0.967	0.839	9.072

Notes: Each entry is a set of estimated coefficients from a separate regression of a full dynamic panel model. All regressions control for individual and year fixed effects. The treatment variables are coded using indicators variables for being in a specified period relative to the timing of the treatment assignment. The reference category is $t-1$, the year before for the lottery assignment. The sample is restricted to individuals who are matched in the six-year panel. All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Standard errors are clustered at the individual level and shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure A2: Dynamic Treatment Effects: Panel Estimates (Credit Bureau Data)



Notes: Each dot represents an estimated treatment effect coefficient from the full dynamic panel models show in Table A8. The outcome is denoted in the title of each panel. Vertical lines correspond to 95% confidence intervals obtained using robust standard errors.

Table A9: Treatment Effects: Three-Year Outcomes and Correcting for Attrition (Survey Data)

	Panel A: Intent To Treat Effects (ITT)					
	Income			Financial Distress		
	Log Income	Log Equivalized Income	Log Personal Income	Can Afford a \$500 Expense	Can Afford a \$1,000 Expense	Can Afford a \$10,000 Expense
Voucher	0.014	0.022	0.042	0.013	0.011	0.006
	(0.036)	(0.038)	(0.038)	(0.028)	(0.028)	(0.019)
N	1310	1294	1242	1415	1415	1415
$\bar{Y}$	10.551	10.171	10.360	0.577	0.368	0.124
	Panel B: Local Average Treatment Effects (LATE)					
	Income			Financial Distress		
	Log Income	Log Equivalized Income	Log Personal Income	Can Afford a \$500 Expense	Can Afford a \$1,000 Expense	Can Afford a \$10,000 Expense
Citizenship	0.007	0.044	0.088	0.080	-0.003	0.003
	(0.098)	(0.106)	(0.103)	(0.080)	(0.078)	(0.053)
N	1045	1032	990	1119	1119	1119
$\bar{Y}$	10.562	10.175	10.379	0.580	0.375	0.122

Notes: As in Table 7 except that we multiply the weights by the inverse probability of responding to the income question in the two-year survey. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A10: Multiple Testing Correction of Three-Year Results (Survey Data)

Outcome	ITT			LATE		
	Unadjusted p-value (1)	Adjusted FWER (2)	Adjusted FDR (3)	Unadjusted p-value (4)	Adjusted FWER (5)	Adjusted FDR (6)
	Panel A: Main Outcomes					
Income						
Log Income	0.93	1.00	0.93	0.77	1.00	0.90
Log Equivalized Income	0.83	1.00	0.93	0.90	1.00	0.90
Log Personal Income	0.24	0.81	0.93	0.33	0.91	0.90
Financial Distress						
Can Afford a $500 Expense	0.74	1.00	0.93	0.39	0.91	0.90
Can Afford a $1,000 Expense	0.76	1.00	0.93	0.84	1.00	0.90
Can Afford a $10,000 Expense	0.63	0.99	0.93	0.80	1.00	0.90
	Panel B: Economic Mechanisms					
Educational Investment						
In School	0.19	0.77	0.47	0.02	0.15	0.16
Employed	0.31	0.80	0.47	0.32	0.90	0.82
In Labor Force	0.54	0.80	0.54	0.98	0.98	0.98
Occupational Upgrading						
Average Wage	0.27	0.80	0.47	0.41	0.93	0.82
Wage Rank	0.34	0.80	0.47	0.70	0.97	0.82
Average Education	0.23	0.79	0.47	0.51	0.94	0.82
Education Rank	0.43	0.80	0.50	0.70	0.97	0.82
	Panel C: Non-Economic Integration					
Overall Index	0.96	1.00	0.96	0.89	1.00	0.93
Overall Index*	0.88	1.00	0.96	0.72	1.00	0.93
Political	0.79	1.00	0.96	0.62	1.00	0.93
Economic	0.85	1.00	0.96	0.90	1.00	0.93
Linguistic	0.87	1.00	0.96	0.57	1.00	0.93
Navigational	0.43	0.98	0.96	0.58	1.00	0.93
Psychological	0.35	0.97	0.96	0.19	0.82	0.93
Social	0.49	0.98	0.96	0.93	1.00	0.93

Notes: Columns 1 and 4 of Panel A present the p-values of the coefficients displayed in Panels A and B of Table 4, respectively. Columns 1 and 4 of Panel B do the same for the estimates shown in Tables 7 - 9. Columns 2 and 5 show the q-values after the Holland-Copenhaver adjustment for multiple hypothesis testing controlling the Family-wise Error Rate (FWER). Columns 3 and 6 display the q-values after the Benjamini-Hochberg correction controlling the False Discovery Rate (FDR). The adjustments are made within each Panel.

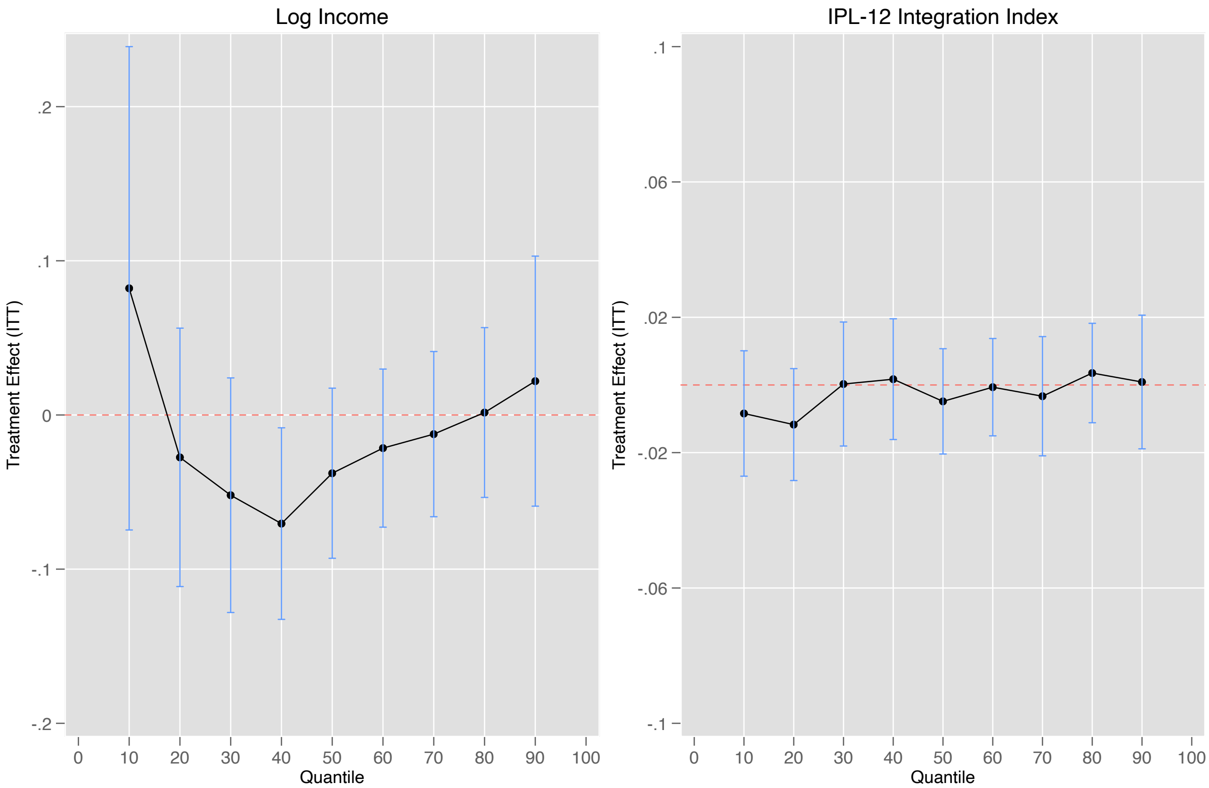
Table A11: Treatment Effects: Full Panel Estimates (Survey Data)

	Panel A: Intent To Treat Effects (ITT)				
	Income		Labor Market Outcomes		Educational Investment
	Log Income	Log Equivalized Income	Employed	In Labor Force	In School
Voucher	-0.009	-0.009	-0.019	0.022	-0.006
	(0.034)	(0.034)	(0.025)	(0.027)	(0.014)
N	7409	7335	6010	8373	8373
$\bar{Y}$	10.522	10.166	0.892	0.725	0.048

	Panel B: Local Average Treatment Effects (LATE)				
	Income		Labor Market Outcomes		Educational Investment
	Log Income	Log Equivalized Income	Employed	In Labor Force	In School
Citizenship	-0.057	-0.047	-0.048	0.052	-0.063
	(0.079)	(0.078)	(0.052)	(0.065)	(0.035)
N	5289	5220	4084	6317	6317
$\bar{Y}$	10.547	10.175	0.897	0.708	0.049

Notes: Each entry is an estimated coefficient from a separate regression of the full panel model. Panel A presents the ITT effects and Panel B shows the LATE effects. All regressions control for individual and year fixed effects. All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Standard errors are clustered at the individual level and shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure A3: Quantile Regressions: Three-Year Outcomes (Survey Data)



Notes: Each dot represents an estimated coefficient from a quantile regression of the outcome on voucher assignment at a specific point in the distribution. The outcome is denoted in the title of each panel and is measured three years after voucher assignment. All regressions control for randomization block dummies and a set of baseline covariates. All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Vertical lines correspond to 95% confidence intervals obtained using robust standard errors.

Table A12: Treatment Effects: Change in Outcomes Between Three-Year Outcome and Baseline (Credit Bureau Data)

	Panel A: Intent To Treat Effects (ITT)				
	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
Voucher	-0.000	-14.686*	0.088	0.026	0.057
	(0.013)	(5.924)	(0.069)	(0.037)	(0.045)
N	2192	2329	2329	2329	2329
$\bar{Y}$	0.160	26.513	0.149	-0.011	0.069

	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
Voucher	-0.032*	-0.002	-0.014	-0.016	-0.072
	(0.014)	(0.021)	(0.008)	(0.010)	(0.059)
N	2329	2329	2329	2329	1798
$\bar{Y}$	0.012	0.058	0.006	0.025	0.433

	Panel B: Local Average Treatment Effects (LATE)				
	Income	Credit Score	Financial Distress		
	Log Income	VantageScore	Delinquency	Collection	Index (Distress)
Citizenship	0.021	-47.120*	0.087	0.107	0.097
	(0.046)	(20.663)	(0.234)	(0.118)	(0.151)
N	1454	1541	1541	1541	1541
$\bar{Y}$	0.170	24.751	0.194	-0.029	0.082

	Access to Credit				
	Open Line	Thick File	Has VantageScore	Index (Access)	Credit Line
Citizenship	-0.071	0.022	-0.056	-0.035	-0.044
	(0.048)	(0.070)	(0.030)	(0.035)	(0.184)
N	1541	1541	1541	1541	1217
$\bar{Y}$	0.017	0.074	0.007	0.033	0.443

Notes: The outcome is denoted in the column header and is measured as the difference between the outcome three years after voucher assignment and the outcome at baseline in the month before the voucher assignment. Panel A presents the ITT effects and Panel B shows the LATE effects. All regressions control for randomization block dummies, HH income and English language. The sample is restricted to registrants who are matched in the four-year panel. All regressions are weighted by the block-level inverse probability of assignment into the treatment group. Robust standard errors are shown in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A13: Baseline Covariates for Compliers and the Overall Sample of Registrants

	Overall Mean	Compliers Mean
	Panel A: Credit Bureau Data	
Income		
Log Income	10.73	10.76
Credit Score		
VantageScore	669.06	668.24
Financial Distress		
Delinquency	1.40	1.58
Collections	0.26	0.32
Index (Distress)	0.83	0.95
Access to Credit		
Open Line	1.00	1.00
Thick File	0.78	0.74
Has VantageScore	1.00	1.00
Credit Limit	8.90	8.90
Index (Access)	0.93	0.91
Educational Investment		
Student Loans	0.20	0.14
	Panel B: Registration Data	
Income		
Log Income	10.45	10.45
Log Equivalized Income	10.13	10.12
Labor Market Outcomes		
Employed	0.93	0.93
In Labor Force	0.78	0.79
Educational Investment		
In School	0.07	0.05
Age	41.43	42.35
Female	0.55	0.66
High School Graduate	0.26	0.22
Some College	0.23	0.25
College Graduate	0.32	0.32
Registered in English	0.62	0.56
Registered in Spanish	0.34	0.41
Green Card Year	2004.93	2004.93
Dominican Republic	0.27	0.29
Ecuador	0.08	0.06
Colombia	0.06	0.08
Married	0.41	0.45
Single	0.34	0.32

Notes: All variables are measured prior to the voucher assignment. In the credit bureau data the sample is restricted to registrants who are matched to the credit bureau data for the year of the lottery and the three years following the lottery (balanced four-year panel). The covariate means for the compliers are computed using kappa weighting (Abadie (2003)). Note that for both data sets the sample is restricted to registrants for whom the full covariate data is available to compute the kappa weights.

Table A14: List of Studies Analyzing the Naturalization Premium

Citation	Sample	Geo region	Method	Outcomes	Estimated Premium	Premium > 0?
Seminal Papers						
Bratsberg, Bernt, James F. Ragan, Jr., and Zafar M. Nasir. 2002. "The Effect of Naturalization on Wage Growth: A Panel Study of Young Male Immigrants." Journal of Labor Economics, 20(3): 568–597.	Cross-sectional data, including the 1990 census and 1994–98 current population surveys, and a longitudinal sample of foreign-born youths drawn from the National Longitudinal Survey of Youth, a longitudinal survey of 12,686 youths aged 14–22 when first interviewed in 1979. The sample has 683 foreign born respondents, and the present study tracks these individuals through the first 13 waves of the NLSY (1979–91).	United States	The longitudinal data allow estimates of precise timing of naturalization, with individual fixed effects, to estimate short and long-term wage and employment effects, and to see if the effects are present previous to the naturalization event.	Income; job status (from blue to white collar)	In the panel study, faster wage growth of immigrants who naturalize between 5.4% and 11.8%. These gains from naturalization occur only after citizenship is attained, and are greater for immigrants from less-developed countries and persist with controls for unobserved productivity.	Yes
Chiswick, Barry R. 1978. "The Effect of Americanization on the Earnings of Foreign-born Men." Journal of Political Economy, 86(5): 897–921.	White men 25-64 from 5% sample of the 1970 US census	United States	Cross sectional data, using multiple regression analysis with a basic human capital earnings function with demographic controls for country of origin, years in the US, and citizenship, comparing immigrants and natives	Income	Earnings are insignificantly lower (about 5%), once controlling for years in US and years squared, for foreign born who are not U.S. citizens.	No
Miller, Paul, and B.R. Chiswick. 1992. "Language in the Immigrant Labour Market." In Immigration, Language Ethnicity: Canada and the United States. , ed. Barry R. Chiswick, 229–296. American Enterprise Institute.	US (micro data from 1980 census) and Canada (micro data from 1981 Census)	Canada and US	Cross sectional data.	Dominant language fluency; income	Positive coefficient on income, by 5% in US and 7% in Canada; but authors acknowledge they cannot control for motivation to become citizens.	Yes
The Most Cited Papers that Reference either Chiswick (1978) or Bratberg et al. (2002)						
Euwals, Rob, Jaco Dagevos, Merove Gijsberts, and Hans Roodenburg. 2010. "Citizenship and Labor Market Position: Turkish Immigrants in Germany and the Netherlands." International Migration Review, 44(3): 513–538.	Survey data of Turkish immigrants in both countries; the German Socio-Economic Panel 2002 and the Dutch Social Position and Use of Provisions Survey 2002.	Netherlands and Germany	Cross-sectional analysis, with no attempt to separate out selection vs. direct effects of citizenship. Assumption is that if positive, at least some of the variance is likely to be a direct effect.	Employment rates, tenured job rates, and job prestige scores.	Labor market position improves for citizens in Netherlands, but inconsistent results for Germany	Mixed
Kogan, Irena. 2003. "Ex-Yugoslavs in the Austrian and Swedish Labour Markets: The Significance of the Period of Migration and the Effect of Citizenship Acquisition." Journal of Ethnic and Migration Studies, 29(4): 595–622.	Austrian 1996 micro-census and the Swedish 1997 labour force survey.	Sweden and Austria	Track immigrant cohorts over time	Labour force participation, unemployment, economic sector concentration and occupational status	Yugoslavs in Austria get a significant advantage with naturalization; no effect of naturalization of Yugoslavs in Sweden	Mixed
Helgertz, Jonas, Pieter Bevelander, and Anna Tegunimataka. 2014. "Naturalization and Earnings: A Denmark-Sweden Comparison." European Journal of Population, 30: 337–359.	Longitudinal register data from 1986	Denmark and Sweden	Individual fixed-effect regression analysis	Income	A consistent naturalization premium is detected for immigrants of Asian and African descent, but not for any other immigrant group.	Mixed

Corluy, Vincent, Ive Marx, and Gerlinde Verbist. 2011. "Employment Chances and Changes of Immigrants in Belgium: The Impact of Citizenship." International Journal of Comparative Sociology, 52(4): 350–368.	Labour Force Survey data for 2008	Belgium	Probit regressions	Employment probabilities and unemployment risks	Citizenship acquisition is associated with better labour market outcomes for non-Western immigrants in general, and remains after controlling for years of residence since migration.	Yes
Steinhardt, Max Friedrich, and Jan Wedemeier. 2012. "The Labor Market Performance of Naturalized Immigrants in Switzerland—New Findings from the Swiss Labor Force Survey." Journal of International Migration and Integration, 13: 223–242.	First generation Swiss immigrants, w/ and w/out citizenship from 2008 wave of the Swiss Labor Force survey.	Switzerland	Blinder-Oaxaca decomposition technique reveals results are largely caused by positive selection.	Wages	Higher status jobs for naturalized not fully explained by selection.	Yes
Constant, Amelie, Liliya Gataullina, and Klaus F. Zimmermann. 2009. "Naturalization Proclivities, Ethnicity and Integration." International Journal of Manpower, 30(1-2): 70-82.	387 immigrant household heads from the 2005 wave of the German Socio-Economic Panel (GSOEP) which reports on ethnic groups of immigrants who arrived in Germany as "guest workers" and includes post guest-worker immigrants who arrived after 1973. Sample only contains foreign-born immigrants who came from Turkey or from the former Yugoslavia and who have lived in Germany at least 8 years.	Germany	Comparison of immigrants who have not naturalized, but would like to do it in the future with those who already have citizenship.	Acquiring or wanting to acquire German citizenship	No estimation on returns to acquiring, though the data are there to see if labor force outcomes of those wanting to acquire and those that have acquired are different; this would permit a causal inference on the direct effects of citizenship.	Inconclusive
Zhou, Huiquan, and Sungkyu Lee. 2013. "Effects of US Citizenship on Wages of Asian Immigrant Women." International Journal of Social Welfare, 22(4): 420–430.	26,132 foreign born Asian women who were currently employed from the 2007 ACS	United States	OLS regression analyses to examine the moderating effects of US citizenship status on the relationship to wages, controlling for socio-demographic characteristics including age, ethnicity, marital status, educational level, number of children under age of 5, family income, and geographic location.	Natural log of total monthly income	US citizenship positively moderated the relationship between sample characteristics and wages. However, after controlling for socio-demographic, occupational, and immigration-related characteristics, no wage difference was found between US citizens and noncitizens.	No
Bevelander, Pieter, and Ravi Pendakur. 2012. "Citizenship, Co-ethnic Populations and Employment Probabilities of Immigrants in Sweden." Journal of International Migration and Integration, 13: 203–222.	Register data which includes demographic, socio-economic, and immigrant specific information on the whole population of Sweden held by Statistics Sweden (STATIV) for the year 2006; full sample of foreign-born and working age when they become eligible for work permits.	Sweden	Instrumental variable regressions in which citizenship is instrumented by the time of citizenship eligibility.	Employment	Citizenship acquisition has a positive impact for a number of immigrant groups. This is particularly the case for non-EU/non-North American immigrants.	Yes
Catron, Peter. 2019. "The Citizenship Advantage: Immigrant Socioeconomic Attainment in the Age of Mass Migration." American Journal of Sociology, 124(4): 999–1042.	The complete-count 1920 census, a new panel data set linking European immigrants between the 1910 and 1920 complete-count censuses, and the 1% 1920 census sample from the Integrated Public Use Microdata Series (IPUMS)	United States	The difference between intending citizens and noncitizens estimates selection, and the difference between intending citizens and citizens estimates the value of citizenship.	Occupation income score is calculated by IPUMS	Noncitizens had a lower occupation-based income compared with intending citizen counterparts, suggesting positive selection into citizenship. But also evidence for a citizenship advantage in occupational income with a positive and significant coefficient comparing citizens with those who declared intent but hadn't acquired citizenship.	Yes

Mazzolari, Francesca. 2009. "Dual Citizenship Rights: Do They Make More and Richer Citizens?" Demography, 46(1): 169–191.	Microdata from the 1990 and 2000 U.S. censuses, specifically the 5% and 1% Integrated Public Use Microdata Series (IPUMS) files; working-age, foreign-born individuals from Latin American countries who were at least 18 when they arrived in the United States and who have been living in the United States for at least five years (three years if married to a U.S. citizen)	United States	In the 1990s, Colombia, the Dominican Republic, Ecuador, Costa Rica, and Brazil passed dual citizenship laws granting their expatriates the right to naturalize in the receiving country without losing their nationality of origin. Estimate the impact of this change on wages of immigrants from these countries	Employment, earnings; welfare use	Immigrants from the countries that granted dual citizenship during the 1990s (an exogenous treatment inducing citizenship applications in the US) experience a statistically insignificant 1% earnings gain, but a 3.6 percentage point increase in the probability of full-time work relative to other Latin American immigrant groups, a 1.5 percentage point relative drop in the probability of receiving income from public assistance programs, and if on welfare, a 17% relative drop in payments.	Yes
DeVoretz, Don, and Sergiy Pivnenko. 2005. "The Economic Causes and Consequences of Canadian Citizenship." International Migration and Integration, 6: 435–468.	Canadian 1996 census	Canada	Binder-Oaxaca decomposition analysis of income data	Wages	Sizeable economic benefits of citizenship attributed almost entirely to self-selection	No
Gathmann, Christina, and Nicolas Keller. 2018. "Access to Citizenship and the Economic Assimilation of Immigrants." The Economic Journal, 128(616): 3141–3181.	Microcensus, an annual survey of 1% of the German population; supplemented for robustness with the SOEP, an annual panel interviewing more than 20,000 individuals.	Germany	Residency reforms generating exogenous variation in the reduced waiting time for citizenship	Employment rates, working hours and job stability	Faster access to citizenship improves the economic situation of immigrant women (but not men), especially their labour market attachment with higher employment rates, longer working hours and more stable jobs. Immigrants also invest more in host country-specific skills like language and vocational training.	Yes
Peters, Floris, Maarten Vink, and Hans Schmeets. 2017. "Anticipating the Citizenship Premium: Before and After Effects of Immigrant Naturalisation on Employment." Journal of Ethnic and Migration Studies, 44: 1051–1080.	Micro-level register data from Statistics Netherlands from 1999 through 2011 (N=94,320)	Netherlands	Track individuals every 6 months starting from moment of arrival until they emigrate or reach end of study's observation period. Exclude students, retirees, youth, Suriname migrants (who already have citizenship).	Employment probability	One-time boost in probability of having employment after naturalisation (positive signaling), but employment probability develops faster in years leading up to naturalisation (human capital investment in anticipation).	No
Steinhardt, Max Friedrich. 2012. "Does Citizenship Matter? The Economic Impact of Naturalizations in Germany." Labour Economics, 19(6): 813 –823.	Registry data from Institute of Employment Research covering 80% of labor force in Germany of employees who have a foreign nationality throughout the observation period, and foreign employees who naturalize at a certain point of time. All individuals are legally employed non-German immigrants from non-German speaking countries of origin.	Germany	Panel data; pooled OLS specification	Wages	Fixed effects estimates for males show an increased wage growth in years post-naturalization (a 9.2% difference including selection; less by individual fixed effects with a slow post-naturalization growth in annual earnings). This is especially the case among outside EU immigrants; results for women are entirely the result of selection due to observable characteristics.	Yes
Fougère, Denis, and Mirna Safi. 2009. "Naturalization and Employment of Immigrants in France (1968-1999)." International Journal of Manpower, 30(1): 83–96.	Permanent Demographic Sample (EDP) longitudinally tracks a panel dataset including citizenship and employment of almost 1 percent of the French population. Individuals are pooled across several censuses. Sample (of 4 birth dates in the year) from census data from information contained in five French censuses.	France	Bivariate probit model to separate out employment and naturalization; then prediction of naturalization from size of the foreigner population (which slows down the administering of citizenship) and the number of immigrants from the same home country (which fosters naturalization) used as an instrumental variable.	Employment	Naturalization increases the probability of employment by nearly 23%.	Yes
Enchautegui, Maria E. and Linda Giannarelli. 2015. "The Economic Impact of Naturalization on Immigrants and Cities." Urban Institute.	Twenty-one US Cities	United States	Data from the combined 2011–13 American Community Survey (ACS), relying on propensity score matching (PSM).	Earnings, employment, and home ownership for eligible immigrants; tax revenues at all levels.	With naturalization, individual annual earnings increase by an average of 8.9 percent, or $3,200; employment rate rises 2.2%; and home ownership increases 6.3%. Naturalization of those eligible also increases tax revenues. In New York City, naturalization causes a decrease in the overall cost of six public benefits.	Yes

Pendakur, Ravi, and Pieter Bevelander. 2014. "Citizenship, Enclaves and Earnings: Comparing Two Cool Countries." Citizenship Studies, 18(3-4): 384–407.	Canadian 2006 census and the Swedish 2006 register data	Canada and Sweden	Instrumental variable regression; citizenship acquisition rules and the years since first eligibility for citizenship as an instrument for citizenship.	Earnings	Heterogeneous effects due to home country, but on average a significant positive effect, ranging from .41 to .75 log points for different sub-groups.	Yes
Engdahl, Mattias. 2014. "Naturalizations and the Economic and Social Integration of Immigrants." Working Paper 2014:11.	Panel of population-wide data of non-OECD immigrants covering the years 1990 to 2009	Sweden	Panel regressions following pre- and post-naturalization trends	Earnings	Positive trends in income for naturalized immigrants commence before naturalization	No
Long, James E. 1980. "The Effect of Americanization on Earnings: Some Evidence for Women." Journal of Political Economy, 88(3): 620–629.						Not tested
Literature Review						
National Academies of Sciences, Engineering, and Medicine. 2015. The Integration of Immigrants into American Society. Washington, DC: The National Academies Press.						Yes
Liebig, Thomas. 2011. "Main Findings of the Joint EC/OECD Seminar on Naturalisation and the Socio-Economic Integration of Immigrants and their Children." In Naturalisation: A Passport for the Better Integration of Immigrants?. , ed. OECD, 15–20. OECD Publishing.						Yes
Snowball Sample from References in Top Journal Publications						
Scott, Kirk. 2008. "The Economics of Citizenship: Is There a Naturalization Effect?" In The Economics of Citizenship. , ed. Pieter Bevelander and Don DeVoretz, 107–126. Malmo University Press.	Swedish census 1990, and the Swedish Longitudinal Immigrant database (SLI) which is a merging of a number of registers kept by Statistics Sweden from 1980 to 2001.	Sweden	Recoded SLI database into a series of repeated cross-sections; followed by longitudinal study with a random effects logit on data from the SLI. Only examines those that eventually become citizens; though this violates a prohibition of estimating models conditioned on future events, it helps isolate some of the effects of selection into citizenship.	Economic integration: employment and income	Naturalization premium for immigrants largely caused by selection and not related to legal status (citizenship). The bump in cross-section for E.Europeans goes away in a panel.	No

Engdahl, Mattias. 2011. "The Impact of Naturalisation on Labour Market Outcomes in Sweden." In Naturalisation: A Passport for the Better Integration of Immigrants?. 99–130. OECD Publishing.	LINDA database from Statistics Sweden, a 1982-2005 panel covering about 20% of foreign born who arrived in Sweden as working age with annual cross-sections representative of the entire immigrant population.	Sweden	Growth in income in years prior to naturalization in pooled data suggest selection is the principal explanation for any citizenship/immigration correlation.	Earnings and employment	Hypothesized relationship of the larger impact of naturalization for immigrants from low-income countries is not confirmed. Overall, considerable variation in outcomes for many sub-groups (gender; home region)	No
Picot, Garnett, and Feng Hou. 2011. "Citizenship Acquisition in Canada and the United States: Determinants and Economic Benefit." In Naturalisation: A Passport for the Better Integration of Immigrants?. 154–183. OECD Publishing.	2006 census data for Canada, and pooled data from three rounds of the American Community Survey (ACS) for the United States	Canada and US	Cross-sectional census data adjusted for individual and group controls associated with labor market success; acknowledges that self-selection may explain their results but no attempt to measure this effect.	Employment, occupational status, wages	In North America economic outcomes are superior among immigrants who are citizens, as compared to those who are not. This result holds even after accounting for differences between the two groups in observed personal and job characteristics. Employment rates are higher, unemployment rates lower, a larger percentage are in higher status occupations, and their weekly wages are higher by 5% to 15%, depending upon the group and data source.	Yes
Steinhardt, Max Friedrich. 2011. "The Impact of Naturalisation on Immigrant Labour Market Integration in Germany and Switzerland." In Naturalisation: A Passport for the Better Integration of Immigrants?. 146–153. OECD Publishing.	Germany: register data from the employment sample of the Institute for Employment Research (IAB). The data set is a 2% random sample of all employees covered by social security during the period 1975 to 2001, restricted to full-time employed males who have a foreign nationality throughout the observation period or who change from a foreign nationality to German citizenship at a certain point of time. Switzerland: Swiss Labor Force Survey focusing on male first-generation immigrants with and without Swiss citizenship who are fully employed, who have entered Switzerland before 1993	Germany; Switzerland	Blinder-Oaxaca decomposition of longitudinal data follows employees over time through their employment history and separate out selection effects.	Wages	Germany: wage growth is accelerated in the years after the naturalisation event. Switzerland: immigrants with Swiss citizenship earn higher wages than foreign employees.	Yes
Bratsberg, Bernt, and Oddbjørn. 2011. "The Labor Market Outcomes of Naturalised Citizens in Norway." In Naturalisation: A Passport for the Better Integration of Immigrants?. 184–205. OECD Publishing.	Longitudinal data records describing the immigration history and labor market outcomes of individuals covering a 16-year period.	Norway	In longitudinal model, with individual fixed effects, naturalisation effects are identified by differential change in outcomes around the time of acquisition of citizenship.	Employment, occupational status, wages	For certain immigrant groups in Norway, longitudinal data reveal small, but statistically significant, negative effects on employment and earnings. For other groups, no effect of citizenship on labour market status.	No
Peters, Floris, Hans Schmeets, Maarten Vink. 2019. "Naturalisation and Immigrant Earnings: Why and to Whom Citizenship Matters." European Journal of Population, 36(3): 511–545.	All 74,531 foreign born individuals from register data from Statistics Netherlands 1999-2011	Netherlands	Panel data	Income	Naturalisation confers a premium for migrants from economically less developed countries and unemployed migrants; but earnings develop faster leading up to naturalisation than afterwards, consistent with the notion of anticipation, though it continues for a one-shot boost post naturalization.	Yes
Sajons, Cristoph. 2019. "Birthright Citizenship and Parental Labor Market Integration." Labour Economics, 57: 1–22.	Pooled data from the German Microcensus covering the years 2001 to 2008; migrant parents with children born within three years around the enactment of the reform, i.e., between 1997 and 2002; to control for possible age or composition effects, migrant parents in "mixed" families are an additional control group.	Germany	Introduction of birthright citizenship for the children of immigrants in Germany at the beginning of 2000 as exogenous source of variation in the citizenship status of immigrant children.	Current employment and the number of hours worked in the week before the interview	A difference in differences estimation shows that getting a child with the citizenship of the host country does not affect the employment rate of the fathers, but reduces the labor market attachment of the (low skilled) mothers (when their children are at their youngest); any positive effect is completely due to selection.	No

Sumption, Madeleine, and Sarah Flamm. 2012. "The Economic Value of Citizenship for Immigrants in the United States." Migration Policy Institute, Washington, DC.	Unadjusted ACS and CPS (Current Populations Survey) data, made available by IPUMS	United States	Regressions on individual data with controls; and discounted based on the findings in other papers for selection effects.	Earnings	Naturalized citizens have weathered the effects of the economic crisis more successfully, experiencing a decline in median annual earnings of 5 percent from 2006 to 2010, compared to 19 percent for noncitizens and 8 percent for the US born. As a result, the earnings gap between naturalized and noncitizen immigrants increased from 46 percent to 67 percent over the same period. This holds up to about 5% once controlling for observables.	Yes
Pastor, Manuel, and Justin Scoggins. 2012. "Citizen Gain: The Economic Benefits of Naturalization for Immigrants and the Economy." Equity Research Institute.	Individual-level data from the Census Bureau's 2010 Public Use Microdata Sample with data on the number of legal permanent residents eligible to naturalize from the U.S. Office of Immigration Statistics (OIS)	United States	Individual level data with standard controls that predict wages.	Individual earnings and GDP.	Controlling for characteristics that predict individual wages, the "earnings premium" associated with naturalization can rise by around 8 to 11 percent, with large effects on GDP if Legal Permanent Residents were to more rapidly apply for citizenship.	Yes
Govind, Yajna. 2020. "Is Naturalization a Passport for Better Labor Market Integration? Evidence From a Quasi-Experimental Setting." Institut National en Etudes Demographiques, Forthcoming.	French administrative panel data (Echantillon Demographique Permінent (EDP)) which includes the civil registries of marriage, the population census, and panel data on employees for 1 to 4% of the French population.	France	Exploited a change in the law of naturalization through marriage in France in 2006, amending the eligibility criteria of applicants by increasing the required number of years of marital life from 2 to 4, providing a quasi-experimental setting.	Annual earnings	Among those working, citizenship leads to an increase in annual earnings by 28%.	Yes
Gathmann, Christina, and Ole Monscheuer. 2020. "Naturalization and Citizenship: Who Benefits?" IZA World of Labor, 125.	Various empirical studies in Europe	Germany	2000 reform in Germany on years of residence before eligibility for citizenship	Wages, investment in skills, age of marriage and fertility	Residualized monthly wages rise in the year immigrants become eligible for citizenship with significant results for women and for all genders for immigrants from poorer countries	Yes
Bevelander, Pieter, and Justus Veenman. 2006. "Naturalization and Employment Integration of Turkish and Moroccan Immigrants in the Netherlands." Journal of International Migration and Integration, 7: 327–349.	From the Dutch survey 'Social Position and Use of Public Utilities by Migrants' for the years 2002 and 2003, random samples in thirteen cities of refugees from Afghanistan, Iran, Iraq, Somalia and former Yugoslavia, and immigrants from Turkey and Morocco.	Netherlands	Multivariate regressions, controlling for observables that explain decision to naturalize	Employment; wages	With controls, 1.5 (odds ratio) average positive returns for citizenship on employment; no significant return on wages.	Yes
Pivnenko, Sergiy, and Don DeVoretz. 2004. "The Recent Economic Performance of Ukrainian Immigrants in Canada and the U.S." IZA Working Paper No. 913	1996 Canadian census; from Central and Western Canada, individuals aged 18 to 65 who reported wage and salary incomes.	Canada (the US component does not assess citizenship).	Multivariate regressions, controlling for observables that explain wages.	Wages	Ukrainians in Canada get a wage benefit with citizenship: holding other relevant factors constant, the value of Canadian citizenship to Ukrainian immigrants accounts for 23 per cent of expected increase in wage earnings, whereas for non-Ukrainian immigrants about 16 per cent.	Yes
Shierholz, Heidi. 2010. "The Effects of Citizenship on Family Income and Poverty." Economic Policy Institute.	Current Population Survey of US Census (March supplements) covering the years 1993 to 2007	United States	Regressions on individual data with controls	Wages and Poverty Rates	Controlling for observable demographic characteristics, the average income of adult citizen immigrants is 14.6% higher, and the poverty rate is 3.0 percentage points lower, than that of adult non-citizen immigrants.	Yes
Akbari, Ather H. 2008. "Immigrant Naturalization and its Impacts on Immigrant Labour Market Performance and Treasury." In The Economics of Citizenship. , ed. Pieter Bevelander and Don DeVoretz, 127–154. Malmo University Press.	Cross-sectional data for the year 2000	United States	Cross sectional regressions	Tax payments	Citizens pay more taxes	Yes

Hayfron, John E. 2008. "The Economics of Norwegian Citizenship." In The Economics of Citizenship, ed. Pieter Bevelander and Don DeVoretz, 89–104. Malmo University.	From registry data, six-wave panel dataset from the Norwegian Database "FD-Tygd Panel". The Database contains a sample of 10 percent of the Norwegian population between 16 and 66 years of age. The data follow the individual over a period of eight years, 1992-2000. For each individual, periods with benefits from the National Insurance System are registered.	Norway	Longitudinal regressions	Earnings	When age, age squared, years prior to naturalization, years since naturalization and years since naturalization squared are added to the wage equations, the coefficient on the citizenship dummy variable becomes statistically insignificant.	No
Hainmueller, Jens and Hangartner, Dominik and Ward, Dalston, 2019, The effect of citizenship on the long-term earnings of marginalized immigrants: Quasi-experimental evidence from Switzerland, Science Advances, 5(12)	Applicants for Swiss Citizenship	Switzerland	Exploit the quasi-random assignment of citizenship in Swiss municipalities that held referendums to decide the outcome of individual naturalization applications.	Income	Winning Swiss citizenship in the referendum increased annual earnings by an average of approximately 5000 U.S. dollars over the subsequent 15 years. This effect is concentrated among more marginalized immigrants.	Yes
Mata, F. 1999. Patterns of Acquiring Citizenship. In S. S. Halli and L. Driedger (eds.), Immigrant Canada: Demographic, Economic and Social Challenges. Toronto: University of Toronto Press, pp. 163-182	1991 census Public Use Microdata File on Individuals (PUMF); 424,367 workforce members aged 15-64 who had not attended school in the nine months prior to the census day: 81 per cent were Canadian citizens by birth, 13 per cent were naturalized immigrants, and the remaining 6 per cent were non-citizens.	Canada	Principal components as the multivariate analysis technique in order to identify the major socio-demographic and economic dimensions of citizenship status. Cluster analysis was used to validate the results.	Social economic status	Acquisition of citizenship by immigrant groups does not seem to matter much in explaining positions in an occupational and economic hierarchy such as the Canadian one, which is heavily structured along ethnic and nationality lines.	No
Jarreau, Joachim. Naturalization policy and the economic integration of immigrants: new evidence for France.	Panel data for foreign residents in France (2004-2017) from sub-sample of the French census, the Echantillon demographique permanent.	France	Exploiting a reform in 2010 that gave local administrative discretion created spatial differences in the conditional probabilities of naturalization; this allows for estimates of the effect of naturalization on labor market outcomes.	Earnings; wages	Positive effects on earnings, for men, due mostly to more hours worked. Citizenship tends to reduce the incidence of part-time work, possibly also increasing work stability. There is no evidence of causal effects on wage rates or employment.	Yes