



**HAL**  
open science

## To become or not to become French: Conscription, naturalization, and labor market integration

Yajna Govind, Louis Sirugue

► **To cite this version:**

Yajna Govind, Louis Sirugue. To become or not to become French: Conscription, naturalization, and labor market integration. 2023. halshs-04423916

**HAL Id: halshs-04423916**

**<https://shs.hal.science/halshs-04423916>**

Preprint submitted on 29 Jan 2024

**HAL** is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.

**TO BECOME OR NOT  
TO BECOME FRENCH:  
CONSCRIPTION, NATURALIZATION,  
AND LABOR MARKET INTEGRATION**

**YAJNA GOVIND  
LOUIS SIRUGUE**

**WORKING PAPER N°2023/29**

**DECEMBER 2023**

**WORLD** .....  
**INEQUALITY**  
..... **LAB**

# To become or not to become French: Conscription, naturalization, and labor market integration\*

Yajna Govind<sup>†</sup>      Louis Sirugue<sup>‡</sup>  
JOB MARKET PAPER

Version: December 1, 2023

Click [here](#) for the latest version.

## Abstract

We examine how changing the costs of acquiring citizenship translates into naturalization decisions for second-generation immigrants, and the effect of naturalization on their labor market outcomes. We exploit the abolition of mandatory military service in France as an exogenous reduction in the cost of citizenship for men. We find that the reform induced a jump in male naturalization rates, entirely driven by European Union citizens. Using a Synthetic Difference-in-Differences, we show that the probability of employment for EU males consequently increased by 2 percentage points, mainly through a reduction in inactivity rather than unemployment. We provide suggestive evidence that this effect is mainly driven by an increase in public-sector employment and a reduction in self-employment, and is associated with an enhanced sense of belonging.

*JEL classification:* J1, J21, J24, J61

*Keywords:* Naturalization, military service, human capital, labor market

---

\*The authors would like to thank Pascale Champalaune, Paul Dutronc-Postel, Laurent Gobillon, Eric Maurin, Thomas Piketty, Paolo Santini, Patrick Simon, Liam Wren-Lewis, for invaluable comments, as well as participants at the 18<sup>th</sup> Doctorissimes, 1<sup>st</sup> Welfare & Policy Conference, the 71<sup>st</sup> AFSE annual congress, the 2023 EALE conference, the 22<sup>nd</sup> Journées Louis-André Gérard-Varet, the 4<sup>th</sup> Bristol Economic Policy and Behaviour Workshop, the Migration and Politics Workshop, the 27<sup>th</sup> Meeting of Young Economists, and seminar participants at Paris School of Economics and Copenhagen Business School. This work has been funded by public grants overseen by the French National Research Agency as part of the “Investissements d’avenir” program (references ANR-17-EURE-001 and ANR-10-EQPX-17 - Centre d’accès sécurisé aux données - CASD), and the French Collaborative Institute on Migration coordinated by the CNRS (reference ANR-17-CONV-0001).

<sup>†</sup>Copenhagen Business School, World Inequality Lab, CI-Migration

<sup>‡</sup>Paris School of Economics, EHESS, INED, CI-Migration

# 1 Introduction

The challenges posed by the significant migration inflows in host countries in the last decades have ignited heated debates on the most adapted migration policies to implement. Migrant-receiving countries are increasingly tightening regulations governing the acquisition of various forms of residence and employment permits, including asylum applications, work permits, family reunification permits, permanent residence, and citizenship. Naturalization, which represents the final legal step in the integration process for migrants, has consistently been a focal point of this debate. On the one hand, naturalization is viewed as a tool for fostering integration, while on the other hand, it is perceived as a reward for the successful integration of foreign nationals. Those who advocate for the latter also push for more stringent conditions in order to screen the best-integrated migrants. However, there is limited empirical evidence on how migrants respond to changes in the cost of naturalization, and on the impact of naturalization on their economic integration.

We rely on two aspects of the French context to study these matters. First, individuals born in France to foreign parents automatically acquire French citizenship at the age of 18, with the possibility to officially renounce it if they do not wish to acquire it. Second, during the 1990s, compulsory conscription for male citizens made naturalization a costly choice for foreign men. Altogether, these two features induced a salient trade-off for second-generation men at the age of 18 between doing military service and renouncing French citizenship. In this paper, we leverage the abolition of compulsory conscription in 1997 for men born after 1978 as an exogenous shock in the costs of acquiring French citizenship for children of immigrants using various administrative and survey data.

Our results show that the abolition of compulsory conscription induced a jump in naturalization rates for males relative to females. This effect is entirely driven by European Union citizens, the group of second-generation immigrants for whom acquiring French citizenship should matter the least. Yet, we find that this surge in naturalization induced a 2 percentage-point significant increase in employment for this group. We show that these results can be explained by an expanded access to public sector jobs and a departure from self-employment. We also provide suggestive evidence that naturalization is associated with an increased sense of belonging. These results extend the existing literature by providing quasi-experimental evidence on how changes in the costs of naturalization translate into naturalization decisions, and on the causal effect of naturalization on the labor market outcomes of second-generation immigrants.

To rationalize the naturalization decision with respect to the abolition of compulsory conscription, we introduce a simple theoretical framework. The decision to naturalize depends on the expected benefits and costs involved. On the one hand, the benefits include the right to reside and work in France, as well as social, economic, and political integration (Hainmueller et al., 2015, 2017; Govind, 2021). On the other hand, military

service constitutes a significant cost of naturalization. Other costs for those born in France are negligible given the automatic citizenship acquisition rule. If the cost of military service was high enough to induce refusals of French citizenship, its abolition should generate a jump in naturalization rates.

Our model incorporates two likely dimensions of heterogeneity in the naturalization decision process: education level and birth nationality group. We hypothesize that the cost of conscription decreases with education. Indeed, even though the opportunity cost of lower-educated individuals is lower in terms of wage, they were typically assigned more strenuous positions within the military service (Maurin and Xenogiani, 2007) and are more likely to face liquidity constraints. We thus expect that they would refuse French citizenship more frequently under compulsory conscription, and that they would react to its abolition to a greater extent. We also expect birth nationality groups to matter, as European Union (EU) citizens can freely work and reside in France, unlike individuals from other birth nationalities. The cost of military service is thus more likely to be binding for EU citizens since their benefits of naturalization are lower, particularly when compared to individuals from nationalities that are typically discriminated against on the labor market.

To test these hypotheses empirically, we exploit the fact that women were exempt from compulsory conscription and therefore were unaffected by the abolition of military service. Using a Difference-in-Differences approach, we compare the naturalization rate of foreign men born in France before and after December 31<sup>st</sup>, 1978 to that of their female counterparts. We find that at the abolition of compulsory conscription, the naturalization rate of males increased from 67% to 79%, while the rate for females remained stable at around 85%. This suggests that a quarter of citizenship refusals by young males were due to compulsory conscription and that its abolition halved the gap with women.

Consistent with our theoretical framework, results show that this effect is entirely driven by European Union citizens at birth, for whom the benefits of acquiring French citizenship are lower. Within this group, the abolition of compulsory conscription increased male naturalization rates by 12 percentage points. No significant effect is observed for other birth nationalities, for which the cost of military service is therefore not binding. Among EU citizens, we find that the increase in naturalization rates is 50% larger for low-educated individuals compared to high-educated individuals, supporting the hypothesis that the cost of military service is lower for the latter.

We then take advantage of the fact that only EU males experienced a jump in naturalization to study its effect on labor market outcomes using a Synthetic Difference-in-Differences approach. This method, introduced by Arkhangelsky et al. (2021), allows us to exploit the rich set of potential control groups to best capture how the outcomes of EU males would have evolved absent the abolition of compulsory conscription. The synthetic

control group closely mirrors the trend in the employment rate of male European-Union citizens, until the reform, when the employment rate for the treated group diverged from its path with a 2 percentage-point increase. Given that this effect is driven by 12% of EU males, it constitutes a 16 percentage-point increase among compliers. We show that this increase in employment is primarily attributable to a decrease in inactivity rather than in unemployment.

We provide evidence for two potential mechanisms explaining these results. First, we observe a significant increase in the probability of being employed in the public sector and a decrease in self-employment for EU males relative to the control groups. This is in line with the idea that citizenship acquisition expands the set of labor market opportunities for naturalized individuals. Second, we find that the sense of belonging, which is a potential driver of socio-economic integration, increased among EU males in comparison to groups whose naturalization rate did not react to the reform.

We conduct sensitivity checks showing that our results are robust to the set of control groups considered in our Synthetic Difference-in-Differences setting, to anticipation effects, and differential attrition. We also provide suggestive evidence that the abolition of compulsory conscription did not affect employment among second-generation immigrants other than through naturalization. Indeed, while the difference in employment between males and females jumps at the moment of the reform for EU citizens, it remains stable among the other birth nationality groups for whom military service did not affect naturalization decisions. In addition, [Mouganie \(2020\)](#) documents that in France overall, military service had either no effect or positive effects on labor market outcomes. Thus, if anything, our study may underestimate the actual labor market impact of naturalization.<sup>1</sup> Moreover, our results do not support the hypothesis that military service had an impact on second-generation immigrants' education level.

This paper relates and contributes to three different strands of the literature. First, it sheds light on the effects of acquiring permanent residence or citizenship on economic integration. This literature has largely established a positive correlation between naturalization and labor market outcomes, starting with the work of [Chiswick \(1978\)](#). A more recent strand of this literature has explored the causal link between the two, for first-generation immigrants ([Gathmann and Keller, 2018](#); [Hainmueller et al., 2019](#); [Govind, 2021](#)). We add to this literature by studying the labor market integration of second-generation immigrants, touching upon the literature on birthright citizenship which has so far focused on educational outcomes ([Felfe et al., 2020, 2021](#); [Dahl et al., 2022](#)). We provide evidence on the causal effect of naturalization for the offspring of immigrants

---

<sup>1</sup>[Mouganie \(2020\)](#) shows that men from low socio-economic backgrounds do not adjust on the educational margin, and conscription has no labor market effects for that group. In the case of individuals from high socio-economic backgrounds, the two plausible scenarios resulting from their conceptual framework are that conscription has either no or positive direct effects on employment and earnings.

using a novel methodology, the Synthetic Difference-in-Differences approach proposed by [Arkhangelsky et al. \(2021\)](#), which found immediate empirical applications as it limits the risk of violation of the parallel trends assumption ([Barwick et al., 2019](#); [Brown et al., 2021](#); [Campbell, 2023](#); [Lang et al., 2023](#)). Our findings demonstrate that even populations who might have less to gain from naturalization, here second-generation EU citizens, experience improved economic integration from naturalization.

Second, this paper contributes to the literature examining the naturalization decisions of foreigners. The main determinants of naturalization identified so far include the political conditions and urban concentration of the host country ([Yang, 1994](#)), educational attainment and age at migration ([Chiswick and Miller, 2009](#)), and gender.<sup>2</sup> In addition, costs such as civic knowledge requirements, naturalization fees, and dual citizenship rules, have been shown to directly affect take-up, especially that of low-educated or EU citizens ([Vink et al., 2021](#); [Yasenov et al., 2019](#); [Peters and Vink, 2021](#)). The French context offers an ideal case study since individuals born to foreign parents can automatically naturalize at the age of 18, hence minimizing almost any other cost generally associated with the naturalization decision.

Third, we contribute to the literature on the effects of military service. Existing research has mainly focused on the impact of conscription on citizens' education, earnings, political behavior, and criminal activity (e.g., [Angrist 1990](#); [Card and Cardoso 2012](#); [Hjalmarsson and Lindquist 2019](#); [Bingley et al. 2020](#), and more specifically on France: [Maurin and Xenogiani 2007](#); [Fize and Louis-Sidois 2020](#); [Mouganie 2020](#)). In general, conscription is shown to have either no impact or a positive impact on educational outcomes, in line with draft avoidance behaviors ([Maurin and Xenogiani, 2007](#); [Mouganie, 2020](#); [Savcic et al., 2023](#)). On the labor market side, peacetime conscription is shown to have a negative effect on earnings in the Netherlands and in Denmark ([Hubers and Webbink, 2015](#); [Bingley et al., 2020](#)), and either no effect or positive effects in Germany, Portugal, and France ([Bauer et al., 2012](#); [Card and Cardoso, 2012](#); [Mouganie, 2020](#)). To the best of our knowledge, we are the first to investigate the effects of military service on non-citizens, and specifically its implications for their naturalization decisions.

The remainder of this paper is structured as follows. Section 2 provides background information on military service in France, the 1997 reform, and the relevant naturalization process. Section 3 presents the data sources. Section 4 outlines our theoretical and empirical framework for studying naturalization take-up, and presents the results of the effect of the military service reform on naturalization rates. Section 5 describes the empirical approach used to study the impact of naturalization on labor market outcomes, presents the results, and explores potential underlying mechanisms. Section 6 provides various robustness analyses, and Section 7 concludes.

---

<sup>2</sup>See [Gathmann and Garbers \(2023\)](#) for a detailed review of this literature.

## 2 Context of the reform

### 2.1 Military service

France has had an organized military service system since the end of the 18<sup>th</sup> century. All French men were obliged to enroll in military service at the age of majority, with a possibility to postpone conscription up to the age of 22. As from the 1970s, women could also participate in military service on a voluntary basis. In the case of dual citizenship, the country in which a person was required to fulfill his military service obligation could vary depending on the existence or absence of bilateral agreements between France and the country of origin. Thirteen European countries signed a convention on May 6<sup>th</sup>, 1963 stating that individuals with dual nationalities from these countries were required to fulfill their military obligations in the country of usual residence.<sup>3</sup> Other special conventions were also ratified bilaterally with Israel, Switzerland, Algeria, and Tunisia, among others. For instance, the Franco-Algerian convention allowed bi-nationals to choose the country in which they would like to complete their military service, irrespective of their place of residence.

With a decline in military needs, the length of conscription has frequently been shortened over time and lasted 10 months in the 1990s. In February 1996, Jacques Chirac, the president of France at the time, declared his intention to abolish mandatory military service in order to professionalize it. The French government began discussing the reform of the military service in November 1996, and the law was voted and passed in October 1997. It aimed at progressively suspending compulsory military service, seeking to end it completely by 2003. During this transition period, the former regulations on military duty continued to apply to men born before 1979, even if they postponed their conscription, while those born after December 31<sup>st</sup>, 1978 were exempt from compulsory conscription. A decree published in June 2001 officially marked the end of mandatory military service for all in France.

### 2.2 Naturalization

Unlike the United States, most migrant-receiving countries have not adopted outright birthright citizenship, or the *jus soli* principle (right of soil). In France, children born on the French territory to two foreign parents can obtain French citizenship prior to

---

<sup>3</sup>These 13 countries are Germany, Austria, Belgium, Denmark, Spain, France, Ireland, Italy, Luxembourg, Netherlands, Norway, the United Kingdom, and Sweden.



reaching the age of 18 upon request, or automatically at the age of 18 years old.<sup>4,5</sup> Those who do not wish to acquire French citizenship at 18 years old can refuse the automatic naturalization through the “voluntary loss of nationality”. To do so, individuals must complete specific administrative procedures by submitting the required documents to the Court of Justice between the ages of 17 and a half, and 19. An estimated 10 to 15% of young adults refused citizenship in the 1980s (Massot, 1985). If they chose to renounce their French citizenship, they could not be reinstated due to the unfulfillment of military obligations (Spire and Thave, 1999) prior to its abolition. In all cases, they would be subject to the more stringent requirements faced by other foreigners if they subsequently wished to acquire French citizenship. These made the refusal at 18 an almost definite choice.

### 2.3 Naturalization and Conscription

Historically, naturalization policies have been tightly related to military needs. Indeed, the law of the 26<sup>th</sup> of June 1889 that established the automatic attribution of French citizenship to children of foreign parents born in France can be traced back to demographic and military concerns (Massot, 1985). Among other reasons, it was put in place to ensure that second-generation immigrant men would be liable to serve in the French military service (Spire, 2005).

Conscription thus became an obligation associated with obtaining the nationality. The timing of the choice to (refuse to) naturalize coincided with the enrollment for military service, making the latter salient in the naturalization decision. In fact, it is estimated that around 90% of the citizenship refusal among second-generation in 1990s were men (Weil, 2002), indicating that military service was considered a cost of naturalization. The abolition of military service in 1997 thus made naturalization less costly for second-generation men.

---

<sup>4</sup>The majority of children obtain citizenship at 18 years but they can obtain it as early as 13 years of age if the child is born and resided in France since the age of 8 and the parents requested it. Above the age of 16, children can apply for French citizenship if they have resided in France for at least 5 years since the age of 11.

<sup>5</sup>The rules were changed between the period 1993 and 1998. The Pasqua Law of 1993 required children born in France to foreign parents to explicitly express their wish to acquire French citizenship. This reform should not affect our setting for two reasons. First, upon the reversal of the reform in 1998 with the Guigou law, the affected cohorts (those born between 1975 to 1980) were ex-post automatically naturalized. Second, this reform should not affect men and women differently. We show that there are no apparent effects on women born before and after 1975.

## 3 Data

### 3.1 French Population Census

We conduct the main analysis on the French Population Census (Recensement de la Population - RP), collected by the French National Institute for Statistics and Economic Studies (INSEE). Until 1999, it was exhaustive and collected every 6 to 9 years. Since 2004, it is exhaustive for municipalities under 10,000 inhabitants and covers 40% of dwellings in municipalities that exceed this threshold. We thus weight all computations using the survey weights provided by INSEE. Under this new data collection procedure, 20% of individuals are surveyed each year such that each period of 5 consecutive yearly census surveys (Enquêtes Annuelles de Recensement - EAR) constitutes a complete survey wave.

We specifically rely on the 2014 survey wave, which gathers forms collected from 2012 to 2016, such that individuals born around January 1<sup>st</sup> 1979 are surveyed at age 35 on average. French citizenship acquisition is thus observed relatively early in adulthood, and labor market outcomes are observed sufficiently late for individuals in the post-treatment cohorts to have completed their education and joined the labor market. Note that the collection process of a complete survey wave over 5 years allows to control for age variation within birth cohort.

The census notably documents individuals' age, gender, educational and labor market outcomes, marital and nationality status. Both the current nationality and the birth nationality are observed for French citizens, which allows to distinguish individuals who acquired the French citizenship from those who were born French. For non-French citizens, their current nationality is observed.<sup>6</sup> Also, the census does not contain information on dual nationalities.<sup>7</sup>

More complex information such as family links between censused individuals are only available for a 40% subset of the population. Hereinafter, we refer to this subsample as the “complementary census sample”, while we refer to the full sample as the “main census sample”.

Our sample of interest consists in second-generation immigrants, i.e., individuals born in France without French citizenship. They represent 1.3% of the population among individuals born  $\pm 10$  years around the threshold, i.e., from 1969 to 1988. Table 1 shows the descriptive statistics of this population using the main census sample.

---

<sup>6</sup>For these individuals, we must assume their birth nationality to be identical to their current nationality. We test this assumption using the Permanent Demographic Sample, described in Section 3.2, which follows a sample of the population across census waves. In this sub-population, 96% of the foreigners born  $\pm 10$  years around the cutoff, i.e., from 1969 to 1998, and observed in both the 2014 census and in the 1990 census, have the same nationality in the two census waves.

<sup>7</sup>As long as an individual was born French, no information on other potential nationalities is available. For those who acquired French citizenship, a single birth nationality can be reported, and for those who are not French at the time of the census, a single current nationality can be reported regardless of their number of nationalities.

Table 1: Descriptive statistics of the main sample

	Male	Female	All
Age	35.05	34.59	34.81
Citizenship acquisition	72.18%	84.22%	78.37%
High school	55.12%	68.96%	62.24%
Higher education	35.34%	46.6%	41.13%
Birth nationality:			
European Union	50.73%	46.15%	48.37%
Other Europe	12.37%	12.58%	12.48%
Africa	31.00%	35.09%	33.11%
Other	5.90%	6.18%	6.04%
Observations	65,347	67,722	133,069

*Notes:* This table provides the average demographic characteristics of our sample of interest: individuals born in France without French citizenship from 1969 to 1988. Source: French Population Census, wave 2014, INSEE.

Our sample of interest comprises about 7,000 individuals per birth cohort, with about as many males as females. Individuals are observed at age 35 on average, and 78% of them acquired French citizenship. Females tend to be more educated than males, and they refuse French citizenship less often. Almost half of the sample has a birth nationality that pertains to the European Union, and African birth nationalities represent a third of the sample. The shares of the most represented birth nationalities in our sample are presented in Appendix Table B.1.

## 3.2 Additional datasets

### 3.2.1 Permanent Demographic Sample - EDP

INSEE produces a panelized version of the census data, the Permanent Demographic Sample (Échantillon Démographique Permanent - EDP), for a subset of individuals born on specific dates during the year. The EDP initially comprised individuals born on one of the first 4 days of October. The sample selection criteria then progressively widened to include individuals born during the first 4 days of April and July, and from the 2<sup>nd</sup> to the 5<sup>th</sup> of January. The most recent EDP information thus covers about 4% of the population.

This longitudinal dataset has the advantage to provide some panel dimension, but it also has limitations. Individuals are observed at variable ages and calendar years due to the rotating yearly collection procedure of the French population census. Also, the sample

size is rather scant as it comprises only 5,535 individuals born in France without French citizenship  $\pm 10$  years around the threshold, compared to 133,069 individuals in our main dataset.

### 3.2.2 Trajectories and Origins - TeO

Besides its absence of the panel dimension, the French population census does not contain much information related to individuals' ethnic origins in general. The Trajectories and Origins survey (Trajectoires et Origines - TeO) jointly conducted by the INSEE and the French Institute for Demographic Studies (INED) was precisely created to palliate this problem. Its large number of questions about individuals' origins and its over-representation of minority groups allows us to study in more depth ethnicity-related issues.

This additional dataset is thus of particular interest to provide complementary insights on this matter but is also rather limited in its sample size. We exploit two waves of TeO that were conducted in 2008 and 2019, as repeated cross-sectional data. With around 1,450 individuals born in France without French citizenship  $\pm 10$  years around the threshold, it represents 1% of our main sample. Nevertheless, it allows us to explore mechanisms related to perceived discrimination and sense of belonging.

## 4 Compulsory conscription and naturalization

### 4.1 Theoretical framework

As described in Section 2, children born in France to foreign parents are automatically granted French citizenship at the age of 18 unless they opt out. In the 1980s, 10 to 15% of these latent acquisitions were actually declined each year (Massot, 1985). Indeed, even though naturalization is automatic, the associated duties such as military service can incentivize the offspring of immigrants to refuse French citizenship. We model this decision process with Equation 1.

$$\text{Refusal}_i = \mathbb{1} \left\{ \underset{(+)}{B_1 + B_2 (\mathbb{1}\{\text{Birth nat}_i \notin \text{European Union}\})} + \underset{(+)}{\eta_i} < \underset{(-)}{C_1 + C_2 (\text{Edu}_i)} \right\} \quad (1)$$

For individual  $i$ , the binary variable  $\text{Refusal}_i$  takes the value 1 if the total cost of acquiring French citizenship exceeds the associated benefits.

We denote  $B_1$  the homogeneous benefits of acquiring French citizenship, including electoral rights, access to public sector jobs restricted to the French population, and a

signal potentially valued by employers on the labor market.

Yet, part of the benefits of acquiring French citizenship may not be homogeneous, especially with respect to individuals' birth nationality. In particular, we let the benefits depend on whether birth nationality pertains to the European Union (EU). We make this distinction because since 1958, the Treaty of Rome allows EU citizens to freely reside and work in any country of the European Union regardless of their specific nationality. Individuals with a birth nationality that does not pertain to the European Union would thus have higher benefits to obtaining French citizenship in terms of residence permits and labor market access. On top of that, individuals whose birth nationality is typically discriminated against in the labor market, such as African nationalities (Duguet et al., 2010; Adida et al., 2016; Primon and Simon, 2018), would benefit even more from French citizenship as a signal to employers. These mechanisms are captured by the term  $B_2$ .

We also include exogenous and normally distributed individual preferences for acquiring French citizenship,  $\eta_i$ , to allow for individual variations.

On the costs side,  $C_1$  denotes the common administrative costs associated with naturalization. As it only consists in sending supporting documents, it is assumed to be small and identical for all individuals.

However, the cost of military service, represented by  $C_2$ , is likely to be relatively large and to vary with education. Indeed, less strenuous positions were typically assigned to more educated conscripts (Maurin and Xenogiani, 2007). In addition, the lower-educated may be more liquidity constrained as they likely come from lower socio-economic backgrounds. Those who did not complete high school can even already be on the labor market by age 18 and may have important disruptive effects. We thus expect the cost of military service to be decreasing in the level of education.<sup>8</sup> Military service may also have beneficial aspects, such as exposition to social diversity, or the possibility to obtain a driver's license for free (Avrillier et al., 2010). Because these potential benefits are likely to be homogeneous, we include them implicitly in  $C_2$  that must be interpreted as the *net* cost of military service.

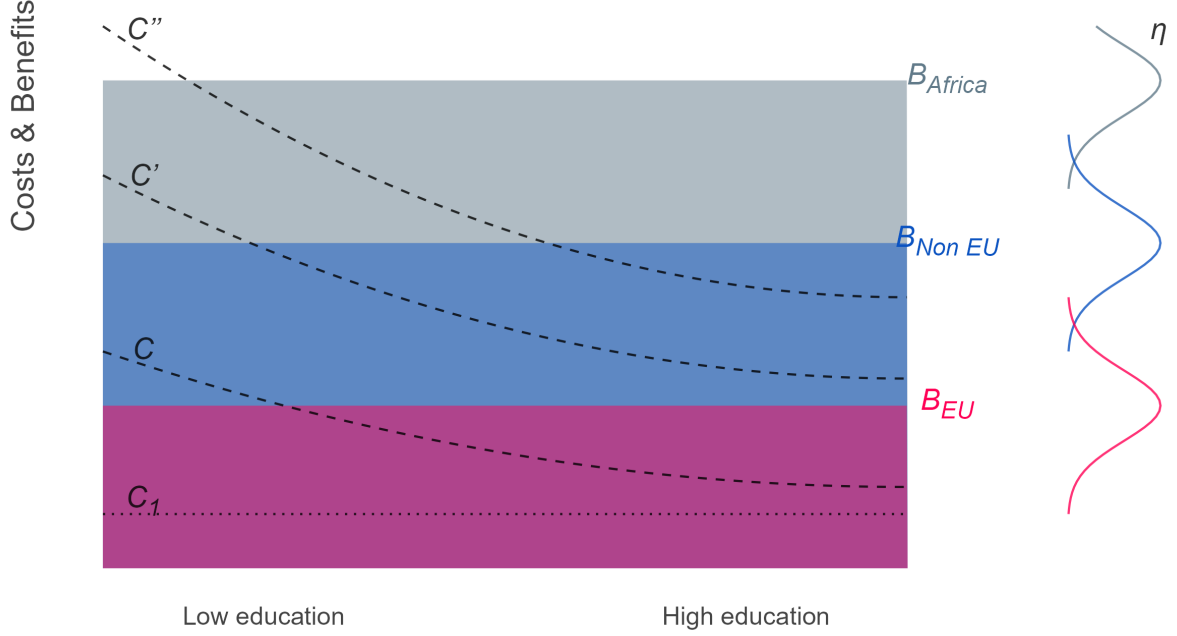
Note that the cost of military service  $C_2$  only applies to males born before the abolition of compulsory conscription. The total cost for females amounts to  $C_1$  irrespective of the period. For males, it drops from  $C_1+C_2$  in the pre-period to  $C_1$  in the post-period. This drop in the costs should thus entail a drop in the share of refusals of French citizenship for males. However depending on the relative size of the different costs and benefits, military service would not necessarily constitute a binding cost to citizenship acquisition for all groups.

Figure 1 proposes a graphical representation of this theoretical framework. The over-

---

<sup>8</sup>The fact that higher-educated individuals would typically have higher forgone expected earnings increases the relative cost of military service at the top of the education distribution, but we do not expect this effect to be strong enough to reverse the sign of the relationship.

Figure 1: Costs and benefits of citizenship acquisition



*Notes:* This figure proposes a schematic depiction of the costs and benefits associated with acquiring French citizenship for individuals born in France without French citizenship. The underlying theoretical framework is formalized in Equation 1. The amount of benefits derived from the acquisition of French citizenship varies between European-Union citizens at birth (red), individuals born with a European nationality that does not pertain to the EU (blue), and those born with an African nationality (gray). The average benefits within each group are represented by the upper border of the stacked areas. Benefits vary individually within each group according to the corresponding  $\eta$  distributions. On the costs side, the horizontal dotted line  $C_1$  represents the small administrative cost associated with the automatic acquisition process. The dashed curves  $C$ ,  $C'$ , and  $C''$  represent the total cost associated with citizenship acquisition for a low, a medium, and a high cost of military service respectively. The cost of military service is given by the difference between the corresponding dashed curve and  $C_1$ . It is assumed to be decreasing with education along the  $x$ -axis, and applies only to males born before the abolition of compulsory conscription. For females and for males born after the reforms, the total cost of acquiring French citizenship amounts to  $C_1$ .

layed areas  $B_{EU}$ ,  $B_{Non EU}$  and  $B_{Africa}$  represent the average level of total benefits from citizenship acquisition for the three groups of birth nationality identified in the theoretical framework. Within birth-nationality group, total benefits vary individually with individual preferences  $\eta_i$  whose distribution is represented on the right panel, but they do not depend on education.

The horizontal line  $C_1$  represents the small administrative cost associated with the automatic acquisition process. It constitutes the total cost for females irrespective of the period and for males who were not subject to compulsory conscription. It is assumed to be below each birth-nationality group's average total benefit.

For males subject to compulsory conscription, the total cost is assumed to decrease with education through  $C_2$ , as motivated above. The larger the cost of military service  $C_2$ , the higher the share of males for whom it would be binding and who would consequently

refuse the acquisition of French citizenship. We illustrate this mechanism with three potential  $C_2$  levels, which added to  $C_1$  give the total costs  $C$ ,  $C'$ , and  $C''$ .

With a relatively low cost of military service such as in total cost  $C$ , only males for whom the cost is the highest, i.e., the lower-educated, and for whom the total benefit is the lowest, i.e, those born citizens of the European Union, would refuse French citizenship.

With an intermediate value of  $C_2$  such as in  $C'$ , the cost of the military service could be binding all along the education distribution for the nationality group with the lowest benefits, i.e., European Union citizens at birth, and for the lowest educated among the next nationality group in terms of benefits.

With a relatively high cost of military service such as in  $C''$ , even some individuals from the nationality group that benefits the most from French citizenship would refuse it, especially the lowest educated for whom the cost of the military service is particularly high.

After the abolition of compulsory conscription, the cost of citizenship acquisition falls down to  $C_1$ , which is expected not to be binding for any group on average, individual preferences aside. Note that a given magnitude of  $\eta_i$  can still result in a negative net benefit, implying that the share of citizenship acquisition would not necessarily reach 1 for males after the abolition of compulsory conscription, nor for females in both periods.

We can draw three main hypotheses from this theoretical framework. First, for individuals born in France to foreign parents, with a sufficiently large cost of military service, the share of males who became French by acquisition should increase with its abolition, while that of females should remain constant.

Second, the increase in the share of acquisition of French citizenship should be higher for less educated males. Indeed, for these individuals the cost of the military service is expected to be larger, and is thus more likely to be binding.

Third, the increase in the share of acquisition of French citizenship should be higher for males who were born citizens of the European Union than for males born with other nationalities. Indeed, for them, the benefits of acquiring French citizenship are lower as they are already entitled to reside and work on the French territory.<sup>9</sup> The cost of military service is thus more likely to be binding for them as well, especially relative to individuals born with nationalities that are typically discriminated against and for whom the French citizenship could constitute a particularly valuable signal on the labor market.

## 4.2 Empirical framework

We expect the abolition of compulsory conscription to reduce the incentive for males to refuse the automatic acquisition of French citizenship at age 18. To test this hypothesis

---

<sup>9</sup>Note that since the law n° 91-715 of July 26, 1991, 80% of public sector employment are accessible to EU citizens.

we exploit the fact that women were not subject to compulsory conscription. Specifically, we compare the difference in male naturalization rates after and before the reform to the difference in female naturalization rates in a Difference-in-Differences setting. Equation 2 displays the corresponding specification.

$$\text{Naturalization}_i = \alpha + \beta \text{Male}_i + \delta \text{Post}_i + \gamma(\text{Male}_i \times \text{Post}_i) + \varepsilon_i \quad (2)$$

$\text{Naturalization}_i$  is a dummy variable taking the value 1 if the individual acquired French citizenship and 0 otherwise,  $\text{Male}_i$  is a dummy variable taking the value 1 for males and 0 for females, and  $\text{Post}_i$  is a dummy variable taking the value 1 if the individual was born after 1978 and 0 otherwise since only males born until 1978 were subject to compulsory conscription.

Our main coefficient of interest is  $\gamma$ , which quantifies the difference in the naturalization rates before and after the reform for men, net of the difference in the naturalization rates of women. For  $\gamma$  to capture the causal effect of the abolition of compulsory conscription on naturalization rates, the difference in female naturalization rates between the two periods must be equal to the difference that would have been observed for males absent the abolition of compulsory conscription. The parallel trend assumption must hold, and female naturalization rates must not be affected by the reform directly, nor through their spouse. We provide evidence supporting these two assumptions in Section 4.3. According to the theoretical framework developed in Section 1, we expect  $\gamma$  to be positive.

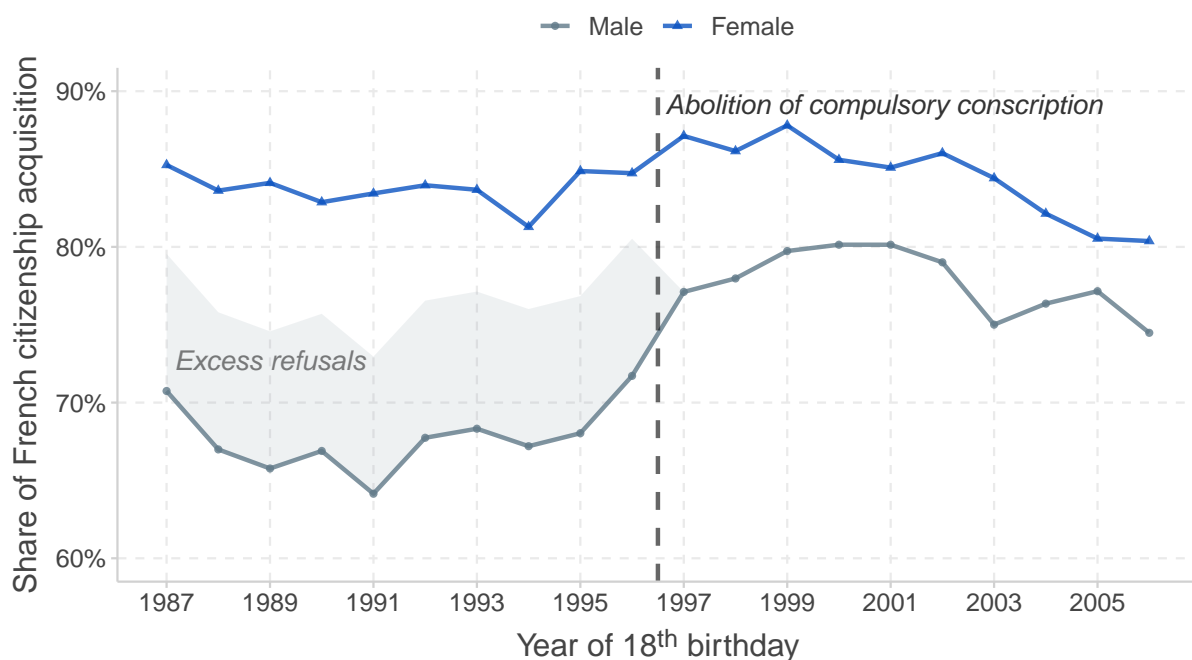
### 4.3 Results

Figure 2 shows the naturalization rates of individuals born in France without French citizenship by birth cohort and separately for females and males. Given that for males born before 1979, the trade-off between doing the military service and refusing the automatic acquisition of French citizenship is faced at age 18, birth cohorts are labeled according to the year of 18<sup>th</sup> birthday on the  $x$ -axis. The vertical dashed line represents the moment of the abolition of compulsory conscription. The corresponding difference-in-differences regression results for a window of  $\pm 5$  years around the threshold are displayed in Appendix Table B.2.

The naturalization rate of females remains stable at around 85% over the period. The fact that it never reaches 100% even though females were not subject to compulsory conscription could be explained by exclusive dual nationalities, identities and sense of belonging, individual preferences, or the intention not to stay in France. The naturalization rate of males however does increase markedly at the threshold. This is in line with the fact that the automatic acquisition of French citizenship was suddenly no longer tied to the cost of doing military service, which induced fewer refusals.



Figure 2: Naturalization rates of second-generation immigrants



*Notes:* This figure represents the share of acquisition of French citizenship among individuals born in France without French citizenship, separately for males (gray) and females (blue) and for birth cohorts from 1969 to 1988. The  $x$ -axis is labeled according to the year of 18<sup>th</sup> birthday, which is when males born before 1979 must decide whether to do military service or to refuse French citizenship. The vertical dashed line represents the abolition of compulsory conscription. For males, after this point, citizenship acquisition is not tied to doing the military service anymore. The shaded area represents the fraction of refusals of French citizenship by young males that were induced by compulsory conscription. The height of the area is obtained from a Difference-in-Differences regression between males and females born 5 years before and after 1979, as specified in Equation 2. The corresponding regression results are shown in Appendix Table B.2. Source: French Population Census, wave 2014, INSEE.

The overall share of compliers is given by the difference between male naturalization rates after and before the reform, net of the difference observed for females. We estimate this Difference-in-Differences following Equation 2. It amounts to 8.8% and is represented graphically by the shaded area. Corresponding results are reported in Appendix Table B.2. Thus, the abolition of compulsory conscription virtually halved the initial gap of 17.1% in naturalization rates between males and females. As the refusal rate for males born before 1979 amounts to about one-third, results suggest that compulsory conscription was responsible for about a quarter of refusals of French citizenship by young males before its abolition. In absolute terms, around 5,000 young males born in France refused French citizenship because of compulsory conscription during the decade leading to its abolition.

Yet, this result can only be interpreted as causal if the trend in the naturalization rate of females is a valid counterfactual for that of males. The fact that the two trends are parallel in the pre-period supports the validity of this assumption. The  $p$ -value associated with the 5-year pre-trend amounts to .39.

In addition, the naturalization rate of females must not have been affected by the

reform. Even though females are not directly concerned by the abolition of compulsory conscription, those who are married to someone affected by the reform could in turn be affected indirectly through the acquisition of French citizenship by marriage. We test this hypothesis by plotting the naturalization rate of females against the birth cohort of their husbands instead of their own. Because these computations require identifying the family links between censused individuals, we rely on the complementary sample of the population census instead of the main sample. Results are displayed in Appendix Figure A.2. We distinguish women whose husband was born French from women whose husband was born foreigner. In both cases, no discontinuity or kink is observed around the threshold. This suggests that in addition to not being affected directly by the reform, females are not affected indirectly through their spouses either.

We then investigate the heterogeneity of this effect according to the two major dimensions of the incentive scheme drawn in Section 4.1: education and group of birth nationality. We distinguish individuals who studied up to high school from those who pursued higher education. We also distinguish individuals whose birth nationality pertains to the European Union from other European nationalities, and from African nationalities.<sup>10</sup> The remaining nationalities are very heterogeneous and would form a too small group for their naturalization rates to be reliably computed by gender and birth cohort.

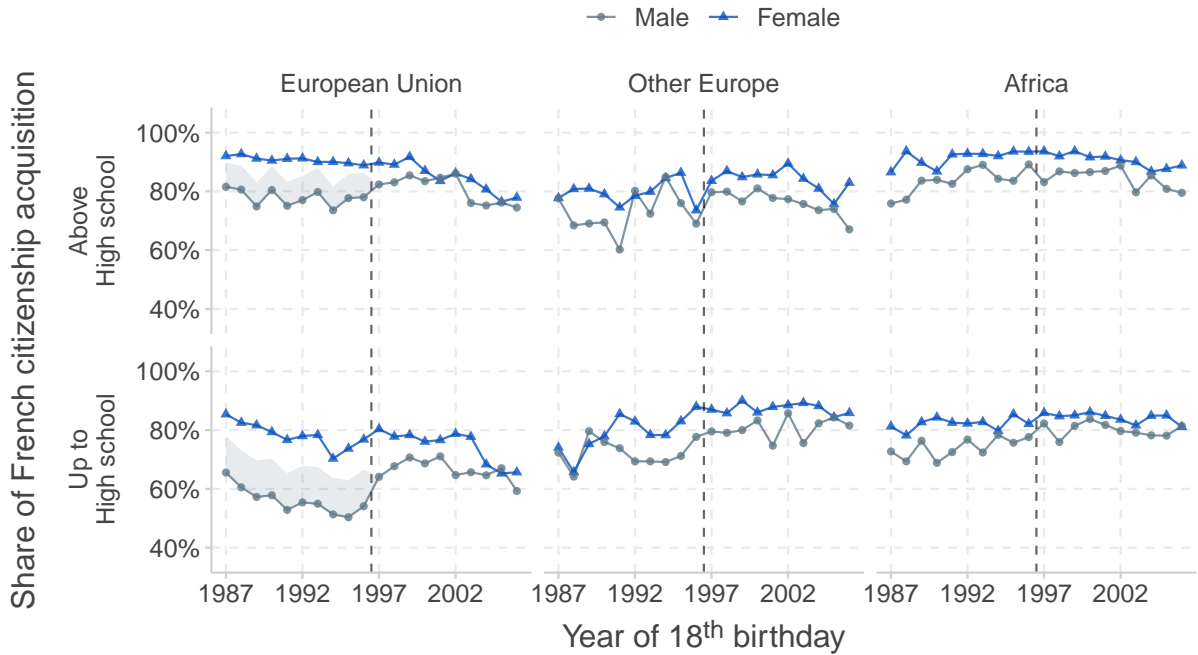
Figure 3 shows the evolution of naturalization rates by birth cohort for each of the 6 resulting groups, for males and females separately. Corresponding regression results are shown in Appendix Table B.3. Female naturalization rates are relatively stable over the period irrespective of the education level or group of birth nationality. For males, however, the picture varies with respect to the group of birth nationality. Males born with an African nationality, or a European nationality outside the EU, have lower naturalization rates than females but follow a very similar trend. For individuals born EU citizens, however, there is a sudden increase in naturalization rates at the moment of the reform. The impact of the abolition of compulsory conscription thus appears to entirely come from EU citizens. This is in line with the incentive scheme described in Section 4.1, as this group is the one for whom the benefits of acquiring French citizenship are expected to be the lowest. The cost of military service is not binding for men from the two other nationality groups, irrespective of their education levels, as their benefits of acquiring French citizenship are too high.

Overall, naturalization rates tend to be higher for the most educated on average. Also, within European Union citizens at birth, we observe that the effect is 50% larger for the lower educated (12pp., against 8pp. for the higher educated), those for whom the cost of

---

<sup>10</sup>We consider European Union as in 1996, i.e., Austria, Belgium, Denmark, Finland, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, and Sweden. Other European countries are grouped into the non-EU category. Because Austria, Finland, and Sweden entered the EU only two years prior to the reform, we provide robustness checks without these three countries in Appendix Figure A.1 and Appendix Table B.4. Results remain virtually unaffected by the exclusion of these countries.

Figure 3: Naturalization rates by education and birth nationality groups



*Notes:* This figure represents the share of acquisition of French citizenship among individuals born in France without French citizenship for 6 subgroups defined by birth nationality (European Union, Other Europe, and Africa) and education (up to high school and above high school). In each of the 6 corresponding panels, shares of citizenship acquisition are represented separately for males (gray) and females (blue) and for birth cohorts from 1969 to 1988. The  $x$ -axis is labeled according to the year of 18<sup>th</sup> birthday, which is when males born before 1979 must decide whether to do military service or to refuse French citizenship. Shaded areas represent the fraction of refusals of French citizenship by young males that were induced by compulsory conscription. The height of the area is obtained from a Difference-in-Differences regression between males and females born 5 years before and after 1979, estimated on the corresponding subgroup as specified in Equation 2. The corresponding regression results are shown in Appendix Table B.3. A significant effect is found only for individuals born citizens of the European Union. Source: French Population Census, wave 2014, INSEE.

military service is expected to be larger, and thus more likely binding.

Rates of acquisition of French citizenship tend to be higher among individuals whose birth nationality is an African one. This is coherent with the fact that this group is the most likely to be discriminated against on the labor market, and thus the one that would benefit the most from French citizenship as a signal to employers.

## 5 Naturalization and labor market outcomes

### 5.1 Empirical framework

We exploit the sudden increase in naturalization among males born in France without French citizenship to estimate the causal impact of naturalization on labor market outcomes. As shown above, the only group to react to the abolition of compulsory conscription was male born EU citizens, while both males and females among other groups of birth

nationalities remained unaffected by the reform. Thus, several potential control groups can be considered. Females born citizens of the European Union have the advantage of being of the same birth nationalities as the treated group. Males born with a nationality outside the EU, either other European nationalities or African nationalities, have the same gender.

Thus, these different potential control groups all have common characteristics with the treated group that are likely to induce co-movement in the outcomes of interest. This makes our empirical setting suited to the use of a Synthetic Difference-in-Differences strategy, recently proposed by [Arkhangelsky et al. \(2021\)](#). This approach extends the Synthetic Control approach introduced by [Abadie and Gardeazabal \(2003\)](#) in two ways.

First, the inclusion of a group fixed effect allows us to weight control groups based on their demeaned trend instead of their absolute trend such that a control group that is more distant from the treated group in absolute terms but whose time variations are similar can be attributed a higher weight.

Second, the Synthetic Difference-in-Differences approach introduces time weights for pre-treatment years in addition to group weights.<sup>11</sup> Time weights are computed to give more importance to time units for which the outcome levels of control groups are closer to their average in the post-period. This typically favors the fit for recent time units to prevent potential discrepancies right before the event from contaminating the estimated treatment effect.

The estimated model is as follows.

$$(\hat{\tau}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \arg \min_{\tau, \mu, \alpha, \beta} \left\{ \sum_{i=1}^N \sum_{t=1}^T (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i \hat{\lambda}_t \right\}, \quad (3)$$

where the  $N$  groups are indexed by  $i$ , and the  $T$  birth cohorts are indexed by  $t$ .  $Y_{it}$  denotes the average of a given labor market outcome among individuals from group  $i$  and birth cohort  $t$ .  $\mu$  is a constant term,  $\alpha_i$  a group fixed effect, and  $\beta_t$  is a time fixed effect.  $W_{it}$  is a binary variable taking the value 1 for the treated group in years post-reform, and the value 0 otherwise.  $\hat{\omega}_i$  and  $\hat{\lambda}_t$  are the vectors of group and time weights, computed as described in [Arkhangelsky et al. \(2021\)](#). Each of these weight vectors sums to 1 and can only take positive values. Note that with a single control group, omitting these two vectors of weights gives the standard Difference-in-Differences specification. The standard Synthetic Control specification can be retrieved by discarding the group fixed effect  $\alpha_i$  and the vector of time weights  $\hat{\lambda}_t$ .

Our set of control groups includes all female birth nationality groups, i.e., females born EU citizens, females born with a non-EU European nationality, and females born with an African nationality. It also includes male birth nationality groups for whom the cost of

---

<sup>11</sup>Note that group weights and time weights are computed independently from one another.

the military service was not binding, i.e., males born with a non-EU European nationality, and males born with an African nationality. In addition, we also consider both females and males born abroad with a nationality outside the EU and who arrived in France early during childhood.<sup>12</sup> In particular, for comparability with individuals born in France from foreign parents, we consider children who arrived in France by age 6 to ensure they were schooled in France from the beginning of primary school. We also exclude EU citizens from the group of individuals born abroad because even though they are not subject to the automatic acquisition of citizenship, the military service cost may enter their decision to apply for citizenship.

The identification of the parameter of interest  $\tau$  with Synthetic Difference-in-Differences relies on the parallel trend assumption. Absent the shock in naturalization rates induced by the abolition of compulsory conscription, the trend that the treated group would have followed must be the same as the trend followed by the synthetic control group, i.e., by the weighted average of the different control groups considered. This implies (i) that the abolition of compulsory conscription must not have impacted the outcome of the treated group other than through the jump in naturalization it induced, (ii) that no concomitant event has impacted differently the treated and the control groups, and (iii) that none of the individual control groups were impacted by the reform, neither directly nor indirectly. We provide support for the validity of these assumptions in Section 5.3.

Under these assumptions, the parameter  $\tau$  identifies an intention-to-treat (ITT) impact. Indeed, it captures the average effect on all EU males regardless of compliance status. The average treatment effect on the compliers population, i.e., the local average treatment effect (LATE), can then be obtained by dividing the ITT  $\tau$  by the share of compliers. The share of compliers corresponds to the baseline Difference-in-Differences coefficient  $\gamma$  in Equation 2, estimated on EU citizens at birth. Regression results for cohorts  $\pm 5$  years around the reform are presented with and without controls for age and education in Appendix Table B.5. The share of compliers amounts to 12%. Yearly Difference-in-Differences coefficients are shown in Figure A.3, and the first stage estimated with a Synthetic Difference-in-Difference is presented in Figure A.4.

## 5.2 Results

We show in Figure 4 the effect of the jump in naturalization rates observed for EU males on their probability of employment, using the Synthetic Difference-in-Differences approach specified in Equation 3.

The top panel displays the average share of employed individuals by birth cohort

---

<sup>12</sup>In practice, low sample size prevents us from breaking down these two groups even further by groups of birth nationalities. We do not consider individuals born French as potential control groups because of the direct effect of compulsory conscription on education identified by [Maurin and Xenogiani \(2007\)](#). This issue is discussed in more detail in Section 5.3.1.

separately for each group. The solid blue line corresponds to the evolution of the outcome of EU males, those who experienced a jump in their naturalization rates at the moment of the reform.

The individual trends underlying that of the synthetic control group are reported in gray for each control group separately. Over the whole period, males born citizens of the European Union are the ones with the most favorable employment outcomes. Females born citizens of the European Union do also have good employment outcomes relative to other groups. This highlights the importance of relying on the Synthetic Difference-in-Differences approach instead of the standard Synthetic Control. Indeed, while the latter would give virtually all the weight to EU females despite slightly converging trends, the former abstracts from the proximity in levels and it weights groups based on their demeaned trend. The resulting synthetic trend, computed following [Arkhangelsky et al. \(2021\)](#)'s methodology and represented with a solid red line, closely follows that of the treated group, with more stability than any other individual trend. This makes Synthetic Difference-in-Differences also more suited than standard Difference-in-Differences in our setting.

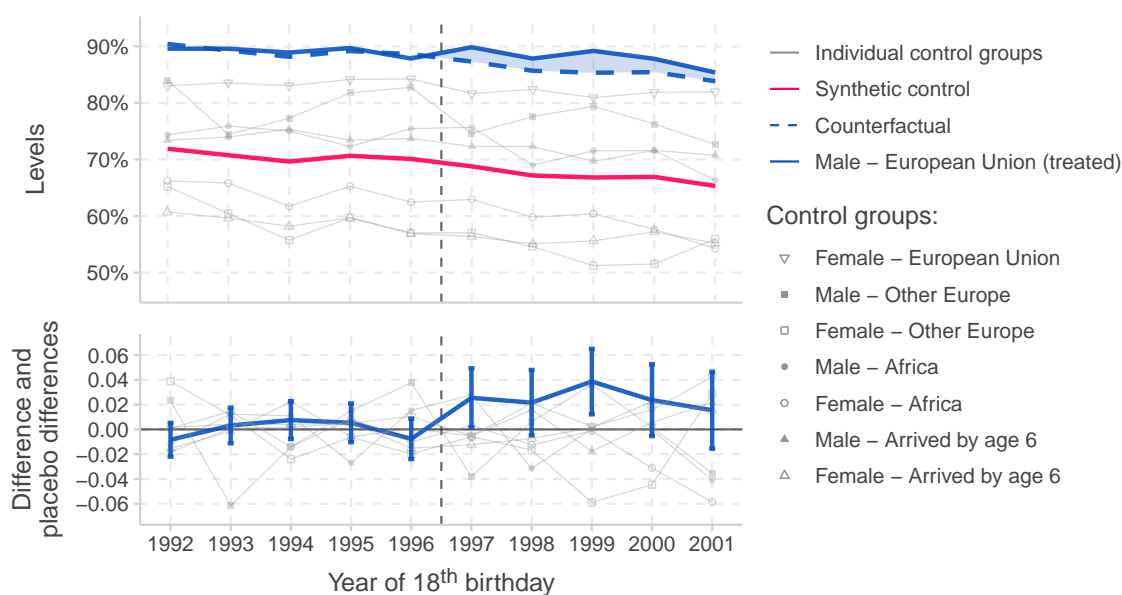
The dashed blue line represents the counterfactual evolution of the treated group absent the reform. It is obtained by shifting the synthetic trend by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The vertical dashed line represents the implementation of the reform. Before the reform, the synthetic control group and the treated group share the same slightly decreasing but steady trend in employment. While the synthetic control group remains on the same path after the reform, a jump in employment is observed for the treated group.

The bottom panel displays the difference between the treated group and the synthetic control group with a solid blue line, centered at the pre-treatment average difference. Vertical lines shows the corresponding bootstrapped 95% confidence intervals. The gray lines are placebo effects, obtained from the same calculations except that the treatment status is fictitiously attributed to the corresponding control group. While all placebo differences in trends remain relatively gathered around 0, the actual difference in trends is flat until the reform and then shifts upwards, above placebo differences.

Baseline Synthetic Difference-in-Differences results on employment, controlling for age and education, are gathered in Table 2 column (1). Columns (2) and (3) report the same results for unemployment and inactivity. Corresponding graphical representations are presented in Appendix Figures A.6 and A.5.

The first row of Table 2 shows the effect  $\hat{\tau}$  of the reform on employment, unemployment, and inactivity, for those who experienced a jump in naturalization. It corresponds to intent to treat (ITT) effects, estimated with the Synthetic Difference-in-differences ap-

Figure 4: Synthetic Difference-in-Differences - Employment



*Notes:* The upper panel of this figure shows the share of individuals in employment by birth cohort within each of the groups used in our Synthetic Difference-in-Differences identification strategy. That of the treated group, males born European Union citizens in France, is represented as a solid blue line. That of each of the individual control groups is represented with gray lines, whose marker indicate the corresponding group. The red line shows the trend of the synthetic control group. The dashed blue line represents the counterfactual trend of the treated group. It corresponds to the trend of the synthetic control group shifted by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The bottom panel displays the difference between the treated group and the synthetic control group with a solid blue line, centered at the pre-treatment average difference. Vertical lines shows the corresponding bootstrapped 95% confidence intervals. The gray lines are placebo effects, obtained from the same calculations except that the treatment status is fictitiously attributed to the corresponding control group. Source: French Population Census, wave 2014, INSEE.

proach as specified in Equation 3, and controlling for age and education. The second row documents the corresponding local average treatment effect, computed as the ITT divided by the share of compliers. Bootstrapped standard errors are reported in parentheses.

Columns (1) indicates a significantly positive effect of naturalization on the probability of employment. The magnitude of the ITT amounts to 2 percentage points, which is substantive given that it is driven by the 12% of the treated group who actually acquired citizenship in response to the abolition of compulsory conscription. Indeed, the LATE indicates that the effect on compliers reaches 16.3 percentage points. Given that the probability of employment in the treatment group is around 90%, as can be seen from Figure 4, this suggests that among EU males, compliers have lower employment rates than average. This is in line with the fact that the lower educated are those who reacted the most to the reform, as observed in Figure 3.

Columns (2) and (3) suggest that this increase in the probability of employment is

Table 2: Effect of naturalization on labor market outcomes

	Employment	Unemployment	Inactivity
ITT ( $\hat{\tau}$ )	0.020** (0.009)	-0.004 (0.008)	-0.012* (0.006)
LATE ( $\hat{\tau}/\hat{\gamma}$ )	0.163* (0.084)	-0.035 (0.069)	-0.098* (0.057)
Group weights ( $\hat{\omega}_i$ )			
Female - European Union	0.160	0.172	0.169
Male - Other Europe	0.085	0.092	0.151
Female - Other Europe	0.113	0.100	0.058
Male - Africa	0.153	0.152	0.170
Female - Africa	0.151	0.174	0.147
Male - Arrived by age 6	0.175	0.150	0.171
Female - Arrived by age 6	0.163	0.160	0.134
Time weights ( $\hat{\lambda}_t$ )			
1992	0	0	0
1993	0	0	0
1994	0.594	0.301	0.777
1995	0.303	0.699	0
1996	0.103	0	0.200
Education	X	X	X
Age	X	X	X
First-stage F-stat.	629***		
Share of compliers ( $\hat{\gamma}$ )	0.121*** (0.009)		

*Notes:* This table reports the results of a Synthetic Difference-in-Differences estimating the effect of the increase in the naturalization rates of males born European Union citizens in France induced by the abolition of compulsory conscription on their employment status. Three employment statuses used as binary outcome variables are considered: Employment, Unemployment, and Inactivity. The first row display the Intention-to-treat effect, and the second row shows the Local Average Effect. Bootstrapped standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. The group and time weights computed following Arkhangelsky et al. (2021) are reported for each specification. Education and age are controlled for in each specification. The F-statistic of the first stage and the share of compliers estimated in Appendix Table B.5 are reported at the bottom of the table. Source: 2014 French Population Census.

mainly related to a reduction in inactivity than in unemployment. Indeed, while the reduction in inactivity reaches almost 10 percentage points for compliers, a non-significant 3.5 percentage point decrease is observed in unemployment.<sup>13</sup>

<sup>13</sup>Note that while in a standard Difference-in-Differences approach, the coefficients associated with the



### 5.3 Assumptions validity

For the estimated coefficients to capture the causal effect of naturalization, the parallel trend assumption must hold between the treated group and the synthetic control group. In our setting, its validity relies on three core hypotheses. First, military service must not have a direct impact on male labor market outcomes other than through naturalization. Second, female labor market outcomes must not be affected by the reform through the outcomes of their husbands. Third, military service must not be binding in the naturalization decision of immigrants arrived by age 6 included in our set of control groups. Evidence provided in this section suggest that none of these threats to identification is at play in our setting, supporting the validity of the parallel trend assumption.

#### 5.3.1 No direct effect of military service

To use males whose birth nationality does not pertain to the EU as control groups, we must ensure that there was no direct effect of military service on labor market outcomes for second-generation immigrants. This is also crucial to attribute the effect of the reform on labor market outcomes entirely to the shock in naturalization rates it induced.

To the best of our knowledge, no consensus has emerged on the direct effect of military service on labor market outcomes in the literature so far. Part of the heterogeneity in the evidence put forward is probably due to the large differences in the modalities of military services across countries. In the French context, [Maurin and Xenogiani \(2007\)](#) show that compulsory conscription incentivized young males to pursue higher education in order to postpone military service. Their results suggest that the abolition of compulsory conscription had a direct negative effect on males' education relative to females' education due to draft avoidance behaviors. However, the incentive scheme faced by individuals born in France without French citizenship is not equivalent to that faced by individuals born French. Indeed, most French citizens must do military service and do not have a second nationality to rely on.

To test whether the effect identified by [Maurin and Xenogiani \(2007, Figure 2\)](#) applies to our setting, we reproduce their result as closely as possible with our data. They use the French Labor Force Surveys 1991-2002 to show the changes in the proportion of men and women aged 17 to 23 still in school across birth cohorts. We cannot use the exact same variable definition because of differences in data collection periodicity. Alternatively, we consider two variables: the proportion of individuals who completed high school, and the proportion of individuals who pursued higher education. In Appendix Figure A.7, we follow [Maurin and Xenogiani \(2007, Figure 2\)](#) and plot the difference between the proportion in a given birth cohort and the proportion in the 1974 birth cohort, within

---

variables Employed, Unemployed, and Inactive would sum to 1, they would not generally do so with the Synthetic Difference-in-Differences approach.

gender. We perform these computations separately for individuals born French and for the three groups of birth nationality of second-generation immigrants.

Similarly to what has been shown by [Maurin and Xenogiani \(2007\)](#), we observe for natives that the divergence in the growth of education rates between males and females is more marked from 1978 onward than for the previous birth cohort.<sup>14</sup> However, we do not observe such an acceleration of the divergence in the growth of male and female education rates for either of the 3 birth nationality groups of second-generation immigrants. This suggests that while compulsory conscription did incentivize males born French to pursue higher education, second-generation immigrants did not adopt this behavior. This is probably because for them naturalization considerations enter the decision process at age 18.

In addition, we test whether military service had a direct effect on labor market outcomes comparing the difference in employment trends by gender separately for second-generation immigrants born EU citizens and those born with other birth nationalities. Indeed, while EU males experienced both the abolition of military service and a jump in naturalization rates, non EU males only experienced the abolition of military service. Appendix Figure A.8 shows the difference in employment rates between males and females per birth cohort, separately for those born EU citizens and for those born with other European or African nationalities. While no jump is observed between non EU males and females, a clear deviation from the pre-trend is observed for EU citizens, with a magnitude comparable to that obtained with the Synthetic Difference-in-Differences.

This suggests that military service did not have a direct effect on labor market outcomes for second-generation immigrants who did not experienced a jump in naturalization. As there is no reason for EU citizenship to affect this relationship, it is likely that this absence of effect holds for EU citizens as well. We cannot test this assumption directly, but [Mouganie \(2020\)](#) documents that in France overall, military service had either no effect or positive effects on labor market outcomes. Thus, if anything, our study may underestimate the actual labor market impact of naturalization.

### 5.3.2 No spillover effect on females

Females were not subject to compulsory conscription, so they were not directly affected by the reform. Still, females whose spouse pertains to the treatment group could be subject to spillover effects. This could arise under assortative mating based on citizenship or immigration status, and if in addition, females adapt their employment status to

---

<sup>14</sup>In our case, however, results indicate that the divergence did not begin in 1978 but started slightly and progressively from the beginning of the study period. This difference with what is put forward in [Maurin and Xenogiani \(2007\)](#) may be due to the difference in the variable definition or sampling variation between the two data sources. Notably, much more stable trends are obtained from the population census than from the labor force survey, probably because of its much larger sample size.

their husbands'. To test this hypothesis, we compare the evolution of females' outcomes according to the birth cohort of their husbands. We do so for females whose husband was born EU citizen and for females whose husband was born with another nationality. We use the latter as a control group because only EU citizens at birth reacted to the reform. Thus, for women whose husband is not an EU citizen, no change in labor market outcomes is expected with respect to whether their husband was born before or after the reform. For spouses of males in the treated group, however, discontinuity in their labor market outcomes at the threshold could indicate spillover effects.

Because these computations require identifying the family links between censused individuals, we perform this exercise on the complementary sample of the population census. Results are presented in Appendix Figure A.12. Around the threshold, no differential trend is observed between females whose husband was born with an EU nationality and females whose husband was not. Even though the difference in trends is relatively noisy due to the relatively low sample size, it remains quite stable over the whole period for all the outcomes. This is suggestive evidence that women were not affected by the reform, even those whose husband was.

### 5.3.3 No effect on immigrants arrived by age 6

In Section 5.1 we show that no group of second-generation immigrants except EU male citizens experienced a jump in naturalization rates at the moment of the reform. For non-EU citizens born abroad and arrived in France by age 6 to be a suitable control group, their naturalization decision must not be affected either by the abolition of compulsory conscription. Appendix Figure A.9 shows the naturalization rates in this group by birth cohort for males and females, separately for those born EU citizens and for those born with another nationality. We observe the same pattern as for second-generation immigrants. While for EU citizens at birth we do observe a jump in naturalization rates among males, the trend remains flat around the threshold for those born with other nationalities. This suggests that first-generation immigrants do account for the cost of doing military service when applying for French citizenship as well, and that it was not binding for non EU citizens at birth in this group either. Thus, non-EU males and females born abroad and arrived in France by age 6 constitute potential control groups for EU male second-generation immigrants.

Yet, because French military service was less intensive for older men, we must ensure that the abolition of compulsory conscription did not entail a drop in the age of naturalization for this group. Indeed, as first-generation immigrants, they choose when to apply for citizenship instead of deciding whether or not to acquire automatically citizenship at age 18. Because the year of naturalization is not available in the data, we exploit the electoral information of the Permanent Demographic Sample and use the age at registration

in electoral lists as a proxy for the age of naturalization. As shown in Appendix Figure A.10, the trends in registration rates in the electoral lists reproduce closely the trends in naturalization rates depicted in Figure 2. We show the evolution of age at registration in electoral lists by birth nationality groups and generation of immigration in Figure A.11. The average age at registration of first-generation immigrants arrived by age 6 follows a stable trend over the period, with no jump observed around the reform.

## 5.4 Mechanisms

In this section, we explore the potential mechanisms that could help explain the positive effect of naturalization for second-generation EU males.

### 5.4.1 Expanded access to the labor market

Given that part of the public sector is conditional on having French nationality, naturalization expands the labor market access of foreigners, providing more outside options. We investigate whether this played a role in the positive employment effect of naturalization. To do so, we estimate a Synthetic Difference-in-Differences as described in Section 5.1 on the share of individuals working in the public sector. Results are represented graphically in Figure 5. The corresponding regression results are documented in Appendix Table B.6.

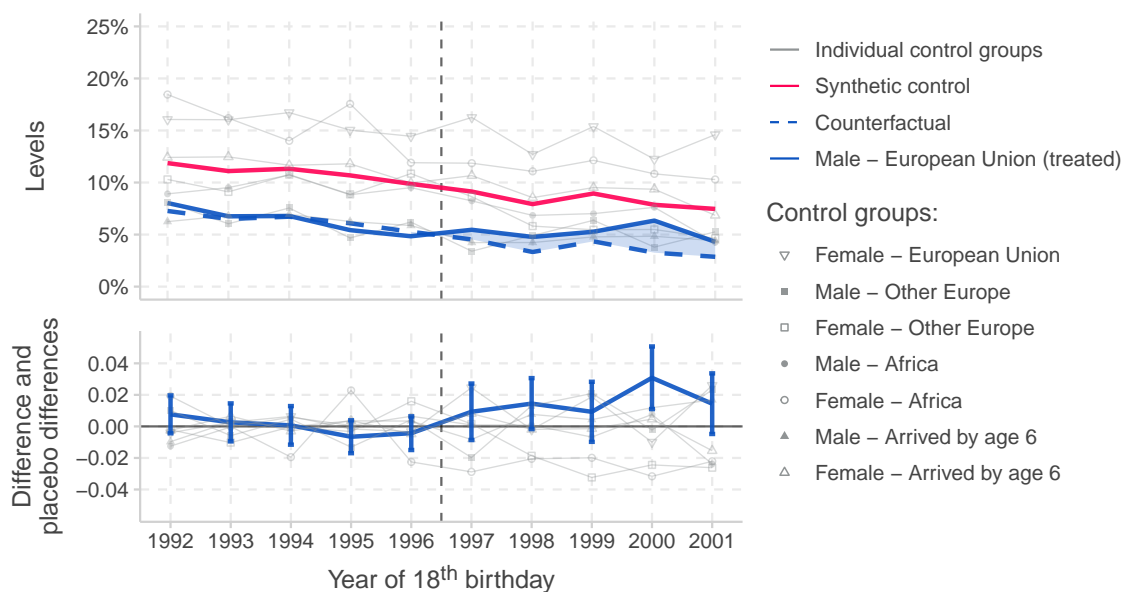
Results suggest that the 12 percentage-point increase in the naturalization rates of EU citizens induced a 1.4 percentage-point increase in their probability of being employed in the public sector, in line with the fact that part of the positions in the public sector require French citizenship. This corresponds to an 11.9 percentage-point increase for the group of compliers.

Yet, the decline in the share of EU males in the public sector is particularly steep relative to that of the other control groups in the pre-period. This results in a slightly decreasing pre-trend, which possibly biases our estimate downwards. This suggests that access to public sector jobs accounts for at least half of the employment effect of naturalization.

Given that naturalization removes barriers to part of the labor market, it might provide individuals with more and better labor market opportunities away from precarious and unstable jobs such as self-employment. To test this hypothesis, we estimate a Synthetic Difference-in-Differences on the share of self-employed individuals. Results are represented graphically in Figure 6. The corresponding regression results are documented in Appendix Table B.7.

Results show a decreasing trend in self-employment for the treated group, accentuated after the reform. Even though barely significant, this pattern is in line with the idea that citizenship acquisition could indeed constitute a stepping stone to depart from self-

Figure 5: Synthetic Difference-in-Differences - Public job



*Notes:* The upper panel of this figure shows the share of public-sector employees by birth cohort within each of the groups used in our Synthetic Difference-in-Differences identification strategy. That of the treated group, males born European Union citizens in France, is represented as a solid blue line. That of each of the individual control groups is represented with gray lines, whose marker indicate the corresponding group. The red line shows the trend of the synthetic control group. The dashed blue line represents the counterfactual trend of the treated group. It corresponds to the trend of the synthetic control group shifted by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The bottom panel displays the difference between the treated group and the synthetic control group with a solid blue line, centered at the pre-treatment average difference. Vertical lines shows the corresponding bootstrapped 95% confidence intervals. The gray lines are placebo effects, obtained from the same calculations except that the treatment status is fictitiously attributed to the corresponding control group. Source: French Population Census, wave 2014, INSEE.

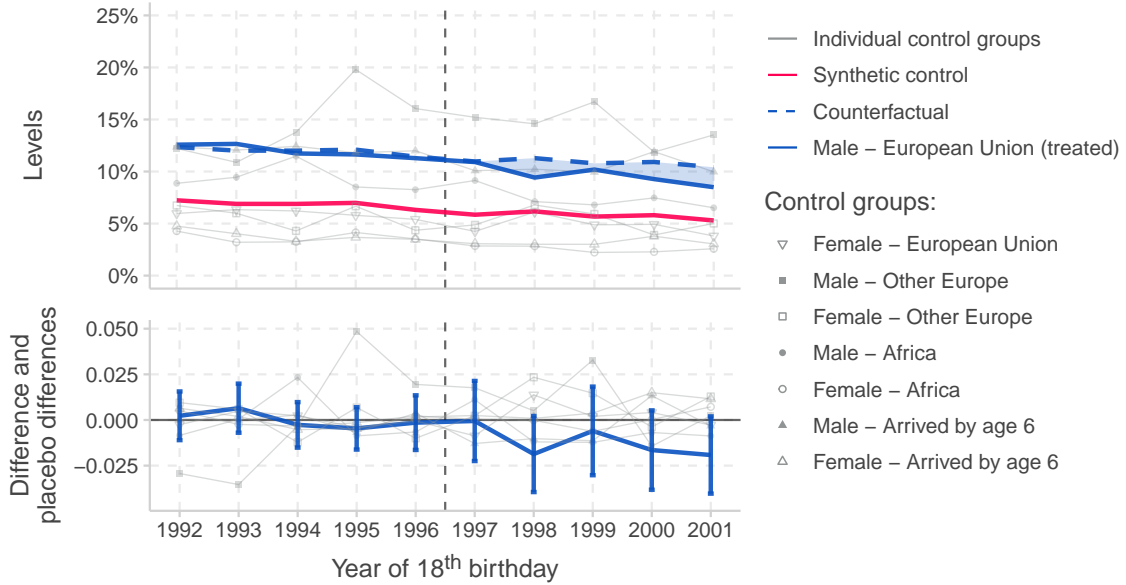
employment towards other types of occupations that may offer more stability on the labor market.

#### 5.4.2 Sense of belonging

Naturalization can promote a sense of belonging to the host country. This is not only important for social cohesion and identity formation, but also for human capital investments of naturalized citizens, and hence, for their labor market integration. In addition, the increased public sector employment could also be driven by an enhanced identification with the host country.

In order to test whether naturalization has impacted the sense of belonging of naturalized individuals, we exploit the Trajectory and Origins survey. Because the sample size of this dataset does not allow us to compute time trends for each potential control group separately, we rely on a standard Two-stage Least-squares (TSLS) setting. To maximize

Figure 6: Synthetic Difference-in-Differences - Self-employed



*Notes:* The upper panel of this figure shows the share of self-employment by birth cohort within each of the groups used in our Synthetic Difference-in-Differences identification strategy. That of the treated group, males born European Union citizens in France, is represented as a solid blue line. Each of the individual control groups is represented with gray lines, whose markers indicate the corresponding group. The red line shows the trend of the synthetic control group. The dashed blue line represents the counterfactual trend of the treated group. It corresponds to the trend of the synthetic control group shifted by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The bottom panel displays the difference between the treated group and the synthetic control group with a solid blue line, centered at the pre-treatment average difference. Vertical lines show the corresponding bootstrapped 95% confidence intervals. The gray lines are placebo effects, obtained from the same calculations except that the treatment status is fictitiously attributed to the corresponding control group. Source: French Population Census, wave 2014, INSEE.

the sample size, we gather in a single comprehensive control group individuals from all the individual control groups considered in the Synthetic Difference-in-differences framework. In addition, we extend the window birth cohorts to  $\pm 7$  years. We estimate the following TSLS model:

$$Y_i = \alpha_2 + \beta_2 \text{Treated}_i + \delta_2 \text{Post}_i + \gamma_2 \widehat{\text{Naturalization}}_i + \lambda_2 X_i + \epsilon_i, \quad (4)$$

where  $\widehat{\text{Naturalization}}_i$  refers to binary variable taking the value 1 if the individual acquired French citizenship and 0 otherwise, instrumented by the interaction between being born after the reform and being in the treated group, i.e., EU citizens born in France.  $X$  is a set of controls including age, education, gender, EU citizenship, being born in France, and a survey wave fixed effect.

We estimate Equation 4 using two dependent variables, one taking the value 1 if individuals agree with the statements “I feel French”, and the other with the statement

“I feel at home in France”. Results are displayed in Appendix Table B.9.

Results suggest that naturalization led to the feeling of being French for more than half of the compliers. A positive effect is also observed on the feeling of being at home in France, even though lower and not statistically significant. We cannot distinguish whether this sense of belonging results from the effect of naturalization on employment or fosters it, but this is suggestive evidence that the sense of belonging may enter the relationship between naturalization and employment.

### 5.4.3 Discrimination

In addition to the above potential mechanisms, the literature has also pushed forward the idea that naturalization helps foreigners’ labor market integration through a reduction in discrimination. However, among immigrants, second-generation EU citizens seem to be the least discriminated against in France (Primon and Simon, 2018). Given this fact, we might expect this channel of naturalization not to play an important role in this case.

To test this hypothesis, we rely on questions about perceived discrimination available in the two waves of the Trajectories and Origins survey. Specifically, we estimate Equation 4 on two binary outcomes: self-reported discrimination experienced during the past 5 years, and more specifically self-reported discrimination about skin tone, origins, nationality, or accent. Results are displayed in Appendix Table B.8.

The results suggest that there is no, or if anything, a decrease in self-reported discrimination induced by the reform. Indeed, the estimated effect is negative for both outcomes, but is also small and not statistically significant. This may either be due to the lack of statistical power in the TeO survey, or evidence that this channel is not as important for second-generation EU citizens as for other groups that experience more discrimination.

## 6 Robustness

### 6.1 Birth nationality substitutions

We show in Section 4 that only individuals born citizens of the European Union react to the abolition of compulsory conscription. This is in line with the fact that their benefits of acquiring French citizenship are lower than those of other nationality groups, for which the cost of military service was not binding under compulsory conscription.

Yet, this heterogeneity could also arise from variations in the cost of military service across birth nationality groups. Indeed, for an individual whose country of birth nationality has a military service that is longer or harder than the French one, the French military service would not constitute a comparatively large cost.

We thus distinguish individuals according to whether military service in their country

of birth nationality in 1996 was longer or shorter than the French military service, within the 3 groups of birth nationality. Appendix Figure A.13 shows the trend in male and female naturalization rates separately for each of the six resulting groups.

Rates of citizenship acquisition tend to be lower for individuals whose birth nationality country has a shorter military service, especially among individuals born with a European nationality that does not pertain to the European Union. This suggests that the cost of the French military service is not constant across birth nationalities, and does vary with respect to the duration of the military service in the country of birth nationality.

Still, the effect of the abolition of compulsory conscription does not appear to vary within nationality groups depending on the relative length of the military service. Only European Union citizens at birth do react to the reform, regardless of the relative length of the French military service.

## 6.2 Anticipation of the reform

Even though the reform did not exempt men born before the 1st of January 1979 from doing military service, it became easier to avoid doing military service after the announcement of the reform.<sup>15</sup> Many of the French males who were born in the last cohorts subject to compulsory conscription and who postponed their military service to pursue higher education actually did not do it in the end.

In our case, anticipation could lead individuals born before 1979, who would have refused French citizenship absent the announcement, to actually accept French citizenship because they anticipate that they will be able to avoid it. This type of behavior would bias our results because these individuals would be considered treated while they pertain to the control group.

To investigate this issue we show the naturalization rates of males and females born EU citizens by month of birth instead of year of birth in Appendix Figure A.15. Due to the amount of noise in the monthly trend, it is difficult to confidently identify from which specific birth month individuals react. Still, there is a clear anticipation phenomenon of two to four months. Anticipation thus starts relatively late given that presidential announcements were made seven and ten months before the effective abolition of compulsory conscription. However, individuals born in France without French citizenship can refuse French citizenship from six months before their eighteenth birthday. Thus some individuals could have refused citizenship early, and have then turned 18 between the announcement of the reform and its effective implementation.

Anticipation is of particular importance in the case of a Synthetic Difference-in-differences identification strategy, because the weighting of the time units tends to give

---

<sup>15</sup>[Fize and Louis-Sidois \(2020\)](#) document this issue in more detail using data from the archives of the French Ministry of Defense.



more importance to more recent periods, which are the ones potentially subject to anticipation. We thus test the robustness of our result to the exclusion of the 1978 birth cohort, with and without time weights. The resulting ITT coefficients are presented in Appendix Table B.10.

ITT effects tend to be slightly smaller when using time weights, which is usually the case because they tend to fit more closely on recent values of the pre-trend, and thus avoid the propagation of slight wedges at the end of the pre-period to the estimated difference in the post-period. But both with and without time weights, results are robust to the exclusion of the 1978 cohort, both in terms of magnitude and statistical significance. The same pattern is observed for LATE coefficients, presented in Appendix Table B.12.

### 6.3 Differential attrition

The theoretical framework presented in Section 4.1 implicitly assumes that second-generation immigrants would stay in France in adulthood in the same proportions among cohorts turning 18 before and after the abolition of compulsory conscription. If the cost of the French military service and the naturalization refusals influence individuals' migration decisions, differential attrition around the threshold could induce a selection bias.

To investigate this issue we make use of the Permanent Demographic Sample, which follows a subset of the main population census across census waves. Appendix Figure A.14 shows the probability for individuals born in France without French citizenship to be observed in the 2014 census wave, i.e., in our main sample, conditional on being observed in the 1990 (top panel) or 1999 (bottom panel) census by gender and birth cohort. We separate individuals who were born EU citizens from other birth nationalities. Missing information at the end of the period for EU individuals from the 1999 census wave is due to insufficient sample size.

Results reveal no differential attrition around the threshold. Males' and females' probabilities to be observed in the 2014 census wave conditional on being observed in either the 1990 or the 1999 census wave follow a very stable trend over the period. This holds both for individuals whose birth nationality pertains to the EU and for other second-generation immigrants.

### 6.4 General equilibrium effects

In Section 5.3, we find suggestive evidence that compulsory conscription did not incentivize second-generation immigrants to pursue longer studies in order to postpone military service. Still, the fact that [Maurin and Xenogiani \(2007\)](#) identify a negative effect of the abolition of compulsory conscription on education for the overall male population raises an issue of general equilibrium effect. Indeed, if individuals born after the reform in the

majority group become less educated due to the abolition of compulsory conscription, this could constitute a shock on the labor market for other males in these birth cohorts. As the rates of high school graduation and higher education remained quite stable for second-generation male immigrants, their relative education level increased.

To test this hypothesis, we control for the share of natives in individuals' local labor market, defined at the employment zone level.<sup>16</sup> Results are presented in Appendix Table B.11. Controlling for the share of natives in the employment zone leaves our estimates virtually unchanged. This absence of a general equilibrium effect could be due to non-substitutability between the two groups, resulting from sectoral labor segregation, to labor market frictions in the matching process, or to any other departure from neo-classical settings.

## 6.5 Control group combinations

We provide in Appendix Figures A.16 to A.20 comprehensive assessments of the role that the set of control groups used to generate the synthetic control group has on our results.

Each row of the tile plot on the left panel corresponds to a given estimation. A blue tile indicates that the control group of the corresponding column was included in the computation of the synthetic control group, while a gray tile indicates that it has been excluded. There are 120 rows, one for each of the possible combinations of control groups formed by sets of 2 to 7 control groups. The specification which includes the whole set of 7 control groups is our baseline specification.

Specifications are sorted by the value of the corresponding ITT coefficient reported on the scatter plot on the right panel. The color of each dot indicates the p-value of the pre-trend. To obtain this value, we compute the difference between the average outcome in the treated group and that in the synthetic control group, and regress it on the birth cohort for the years before the reform. The p-value is then computed from a standard two-sided t-test on the slope coefficient. The vertical solid line represents our baseline coefficient, and a vertical dashed line is placed at 0. The kernel density of this distribution of 120 coefficients is shown on the top panel.

There are two main takeaways from this figure. First, the pre-trend is quite well captured as long as there are enough control groups in the set. Second, as long as the set of control groups allows the synthetic control group to capture the pre-trend, resulting estimates remain relatively close to our baseline.

---

<sup>16</sup>The employment zone is a geographical unit whose borders are based on the share of individuals who both work and live in the zone. It is computed with the algorithm `LabourMarketAreas`, available in `OpenAccess`.

## 7 Conclusion

This paper aims to shed light on the effect of policies that change naturalization costs on second-generation immigrants' naturalization decisions, and on the causal effect of naturalization on labor market outcomes. We rely on the fact that citizenship is automatically granted to individuals born in France to foreign parents and that the obligation for citizens to undergo military service makes naturalization a costly choice. We exploit the abolition of compulsory military service in France as an exogenous shock that reduced the cost of naturalization for second-generation men while not impacting women.

We show that, as anticipated theoretically in a simple rational choice model, the decrease in the cost of naturalization led to a jump in naturalization rates. This increase is entirely driven by individuals with the lowest expected benefits: individuals born European Union citizens who can already work and reside in France. They refuse citizenship more often when there are military service obligations, while this cost is not binding for other birth nationality groups. Additionally, we show that there is a skill gradient in the cost of military service, with the low-educated reacting more to the reform. We exploit the variety of unaffected gender and birth nationality groups in a Synthetic Difference-in-Differences approach to study the causal impact of this jump in naturalization rates on labor market outcomes. Our results show positive labor market effects, with an increase in employment and a reduction in inactivity for EU citizens. We find that this employment effect is accompanied by an increase in public sector employment and in the sense of belonging, and by a decrease in self-employment.

Altogether, these results provide valuable insights both on the citizenship take-up decision of children of immigrants, and on the gains from naturalization for those who would have refused French citizenship had it entailed higher costs. In particular, our findings illustrate how policies that affect naturalization costs heterogeneously across immigrants can unintentionally divest specific groups of improved labor market prospects.

## References

- Abadie, Alberto and Javier Gardeazabal**, “The economic costs of conflict: A case study of the Basque Country,” *American Economic Review*, 2003, *93* (1), 113–132.
- Adida, Claire L, David D Laitin, and Marie-Anne Valfort**, ““One Muslim is enough!” Evidence from a field experiment in France,” *Annals of Economics and Statistics/Annales d’Économie et de Statistique*, 2016, (121/122), 121–160.
- Angrist, Joshua D**, “Lifetime earnings and the Vietnam era draft lottery: evidence from social security administrative records,” *The American Economic Review*, 1990, pp. 313–336.
- Arkhangelsky, Dmitry, Susan Athey, David A Hirshberg, Guido W Imbens, and Stefan Wager**, “Synthetic difference-in-differences,” *American Economic Review*, 2021, *111* (12), 4088–4118.
- Avrillier, Paul, Laurent Hivert, and Francis Kramarz**, “Driven out of employment? The impact of the abolition of national service on driving schools and aspiring drivers,” *British Journal of Industrial Relations*, 2010, *48* (4), 784–807.
- Barwick, Panle Jia, Shanjun Li, Liguo Lin, and Eric Zou**, “From fog to smog: The value of pollution information,” Technical Report, National Bureau of Economic Research 2019.
- Bauer, Thomas K, Stefan Bender, Alfredo R Paloyo, and Christoph M Schmidt**, “Evaluating the labor-market effects of compulsory military service,” *European Economic Review*, 2012, *56* (4), 814–829.
- Bingley, Paul, Petter Lundborg, and Stéphanie Vincent Lyk-Jensen**, “The opportunity costs of mandatory military service: evidence from a draft lottery,” *Journal of Labor Economics*, 2020, *38* (1), 39–66.
- Brown, Zach Y, Eduardo Montero, Carlos Schmidt-Padilla, and Maria Micaela Sviatschi**, “Market structure and extortion: Evidence from 50,000 extortion payments,” Technical Report, National Bureau of Economic Research 2021.
- Campbell, Travis**, “Black Lives Matter’s effect on police lethal use of force,” *Journal of Urban Economics*, 2023, p. 103587.
- Card, David and Ana Rute Cardoso**, “Can compulsory military service raise civilian wages? Evidence from the peacetime draft in Portugal,” *American Economic Journal: Applied Economics*, 2012, *4* (4), 57–93.

- Chiswick, Barry R**, “The effect of Americanization on the earnings of foreign-born men,” *Journal of Political Economy*, 1978, *86*, 897–921.
- **and Paul W Miller**, “Citizenship in the United States: The roles of immigrant characteristics and country of origin,” in “Ethnicity and labor market outcomes,” Vol. 29, Emerald Group Publishing Limited, 2009, pp. 91–130.
- Dahl, Gordon B, Christina Felfe, Paul Frijters, and Helmut Rainer**, “Caught between cultures: Unintended consequences of improving opportunity for immigrant girls,” *The Review of Economic Studies*, 2022, *89* (5), 2491–2528.
- Duguet, Emmanuel, Noam Leandri, Yannick L’horty, and Pascale Petit**, “Are young French jobseekers of ethnic immigrant origin discriminated against? A controlled experiment in the Paris area,” *Annals of Economics and Statistics/Annales d’Économie et de Statistique*, 2010, pp. 187–215.
- Felfe, Christina, Helmut Rainer, and Judith Saurer**, “Why birthright citizenship matters for immigrant children: Short-and long-run impacts on educational integration,” *Journal of Labor Economics*, 2020, *38* (1), 143–182.
- , **Martin G Kocher, Helmut Rainer, Judith Saurer, and Thomas Siedler**, “More opportunity, more cooperation? The behavioral effects of birthright citizenship on immigrant youth,” *Journal of Public Economics*, 2021, *200*, 104448.
- Fize, Etienne and Charles Louis-Sidois**, “Military service and political behavior: Evidence from France,” *European Economic Review*, 2020, *122*, 103364.
- Gathmann, Christina and Julio Garbers**, “Citizenship and integration,” *Labour Economics*, 2023, *82*, 102343.
- **and Nicolas Keller**, “Access to citizenship and the economic assimilation of immigrants,” *The Economic Journal*, 2018, *128* (616), 3141–3181.
- Govind, Yajna**, “Is naturalization a passport for better labor market integration? Evidence from a quasi-experimental setting,” *halshs-03265055*, 2021.
- Hainmueller, Jens, Dominik Hangartner, and Dalston Ward**, “The effect of citizenship on the long-term earnings of marginalized immigrants: Quasi-experimental evidence from Switzerland,” *Science advances*, 2019, *5* (12), eaay1610.
- , – , **and Giuseppe Pietrantuono**, “Naturalization fosters the long-term political integration of immigrants,” *Proceedings of the National Academy of Sciences*, 2015, *112* (41), 12651–12656.

- , – , and – , “Catalyst or crown: Does naturalization promote the long-term social integration of immigrants?,” *American Political Science Review*, 2017, 111 (2), 256–276.
- Hjalmarsson, Randi and Matthew J Lindquist**, “The causal effect of military conscription on crime,” *The Economic Journal*, 2019, 129 (622), 2522–2562.
- Hubers, Frank and Dinand Webbink**, “The long-term effects of military conscription on educational attainment and wages,” *IZA Journal of Labor Economics*, 2015, 4 (1), 1–16.
- Lang, Valentin, David Mihalyi, and Andrea F Presbitero**, “Borrowing costs after sovereign debt relief,” *American Economic Journal: Economic Policy*, 2023, 15 (2), 331–358.
- Massot, Jean**, “Français par le sang, Français par la loi, Français par le choix,” *Revue européenne des migrations internationales*, 1985, 1 (2), 9–19.
- Maurin, Eric and Theodora Xenogiani**, “Demand for education and labor market outcomes lessons from the abolition of compulsory conscription in france,” *Journal of Human resources*, 2007, 42 (4), 795–819.
- Mouganie, Pierre**, “Conscription and the returns to education: Evidence from a regression discontinuity,” *The Scandinavian Journal of Economics*, 2020, 122 (3), 1112–1139.
- Peters, Floris and Maarten Vink**, “Heterogeneous Naturalization Effects of Migrant Destination Dual Citizenship Reform: Quasi-Experimental Evidence from Europe,” *OSF Preprints*, 2021.
- Primon, Jean Luc and Patrick Simon**, “Mesurer le racisme? L’apport des enquêtes quantitatives à la sociologie du racisme,” *Sociologie et sociétés*, 2018, 50 (2), 175–202.
- Savcic, Ruzica, Nikolaos Theodoropoulos, and Dimitrios Xefteris**, “Conscription and educational outcomes,” *Journal of Population Economics*, 2023, pp. 1–26.
- Spire, Alexis**, *Étrangers à la carte*, Grasset, 2005.
- and **Suzanne Thave**, “Les acquisitions de nationalité depuis 1945,” 1999.
- Vink, Maarten, Anna Tegunimataka, Floris Peters, and Pieter Bevelander**, “Long-term heterogeneity in immigrant naturalization: The conditional relevance of civic integration and dual citizenship,” *European Sociological Review*, 2021, 37 (5), 751–765.
- Weil, Patrick**, *Qu’est-ce qu’un français?*, Grasset, 2002.

**Yang, Philip Q**, “Explaining immigrant naturalization,” *International migration review*, 1994, 28 (3), 449–477.

**Yasenov, Vasil, Michael Hotard, Duncan Lawrence, Jens Hainmueller, and David D Laitin**, “Standardizing the fee-waiver application increased naturalization rates of low-income immigrants,” *Proceedings of the National Academy of Sciences*, 2019, 116 (34), 16768–16772.

# Appendix

## A Additional figure

Figure A.1: Naturalization rates by education and birth nationality groups - Excluding Austrian, Finnish, and Swedish birth nationalities

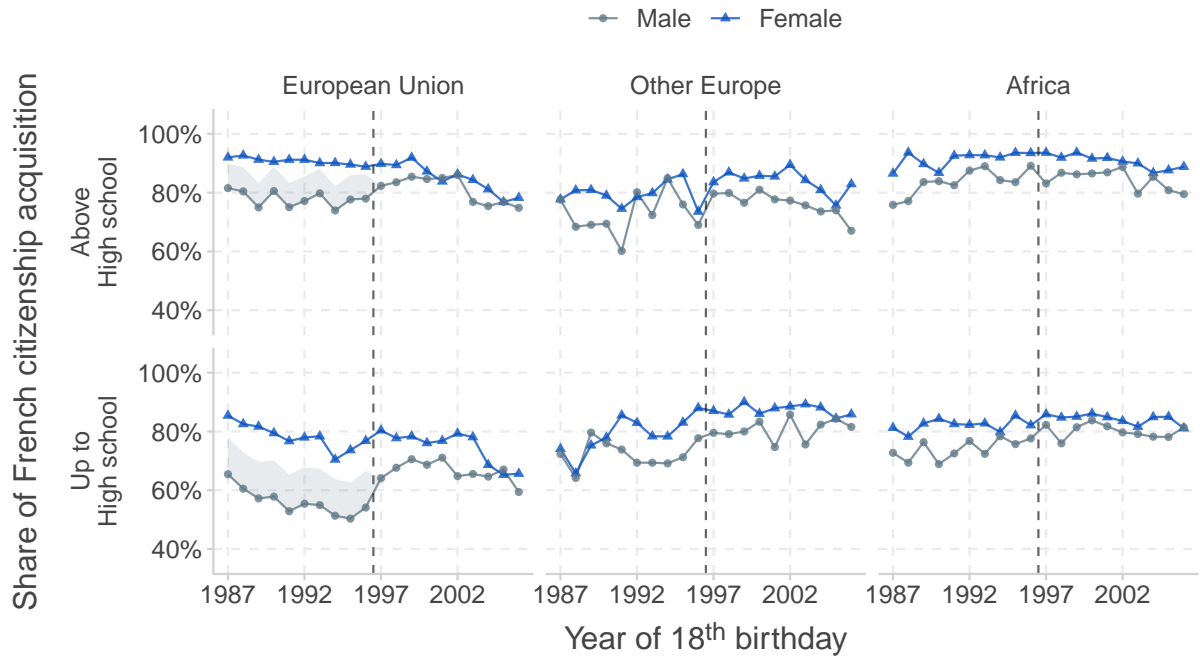


Figure A.2: Female naturalization rates by their husband's birth cohort

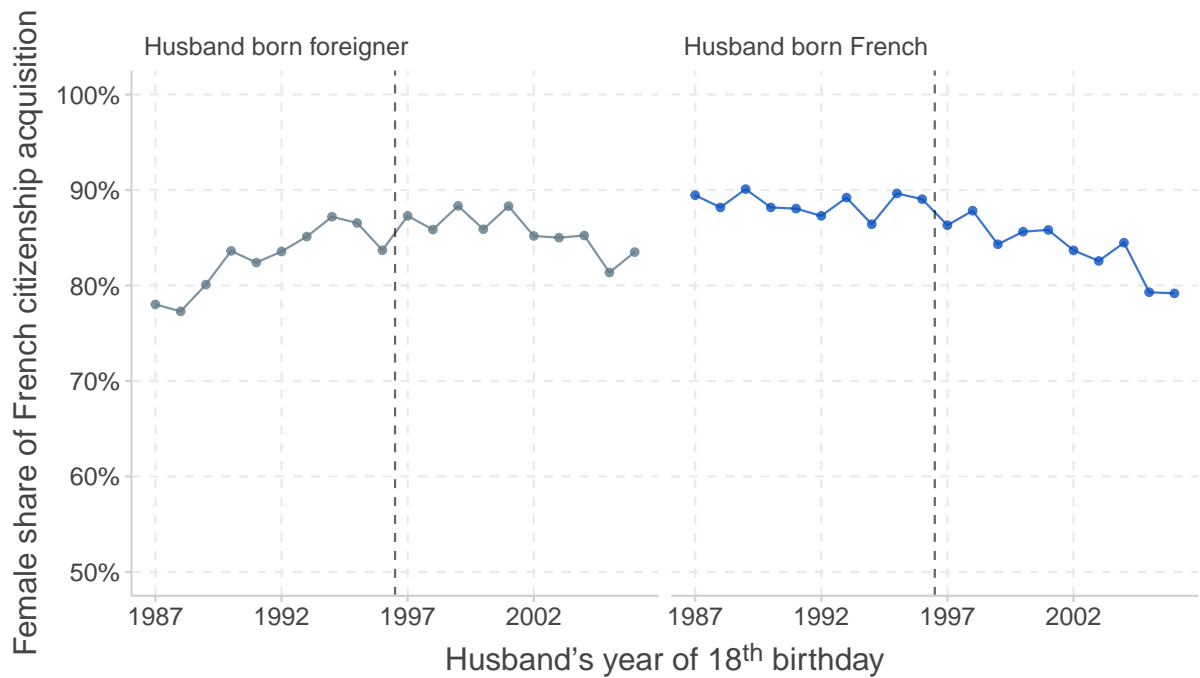




Figure A.3: Naturalization rates of EU second-generation immigrants

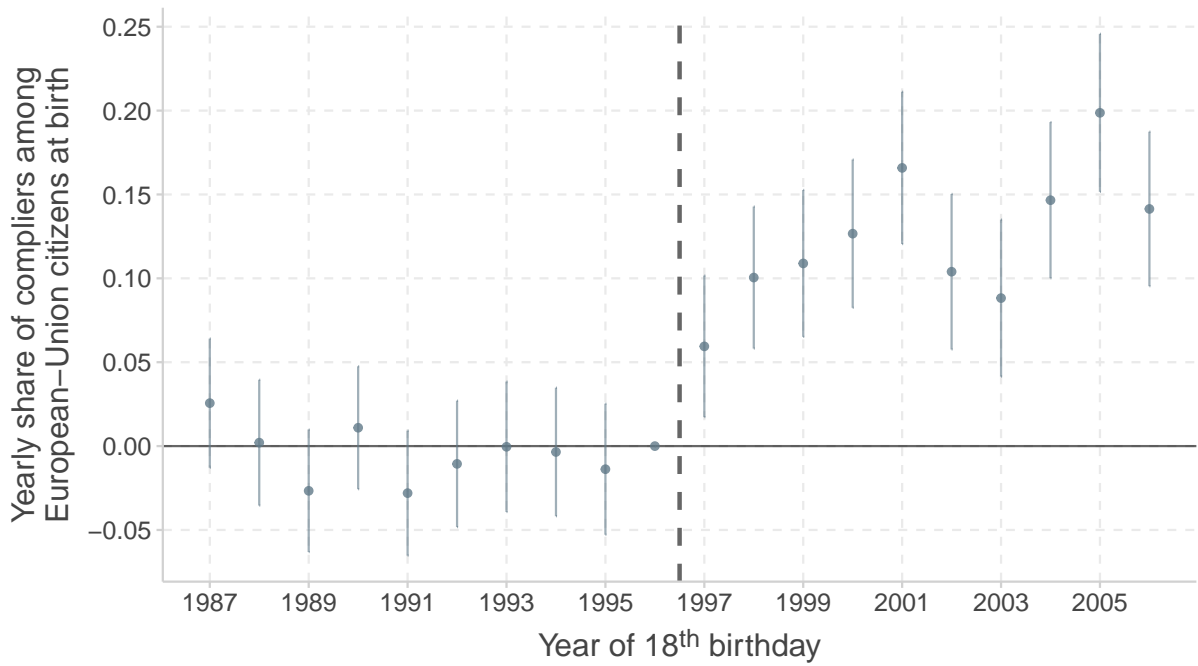


Figure A.4: Synthetic Difference-in-Differences - Naturalization

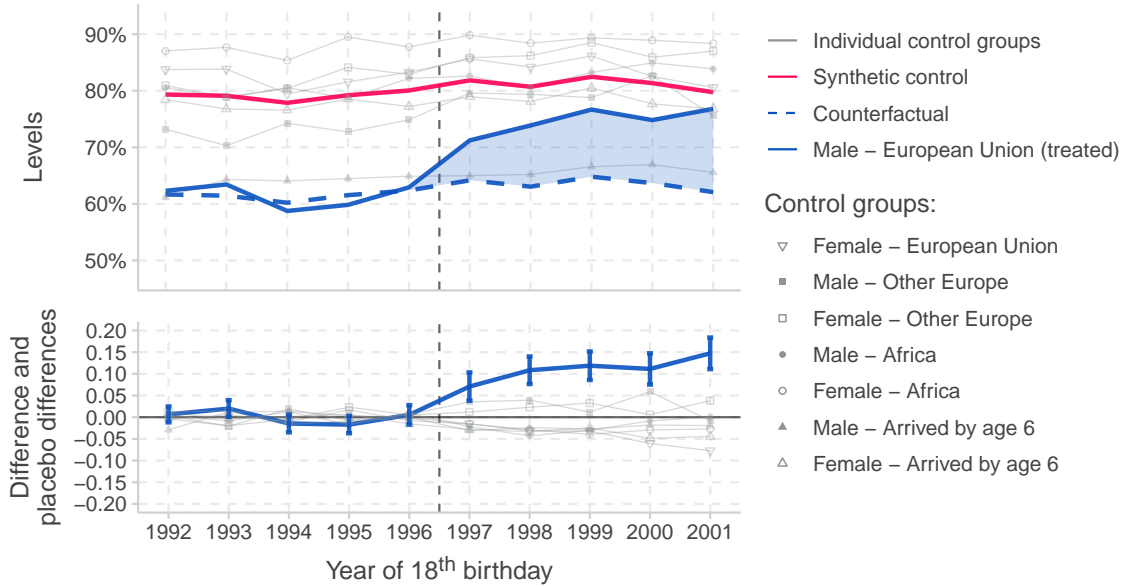


Figure A.5: Synthetic Difference-in-Differences - Inactive

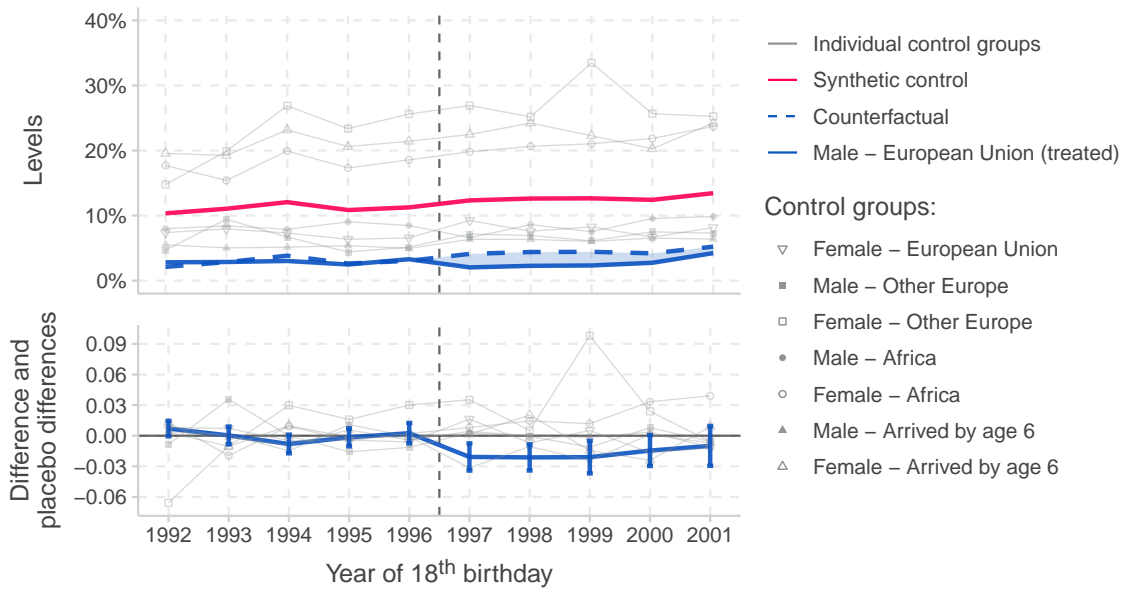


Figure A.6: Synthetic Difference-in-Differences - Unemployed

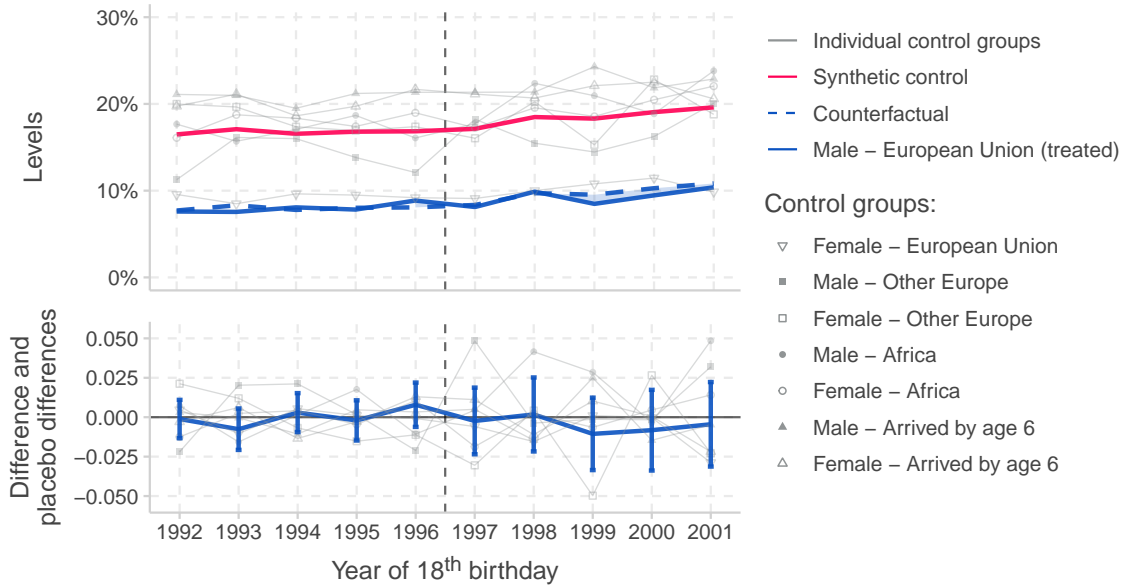


Figure A.7: Education rates relative to the 1974 birth cohort

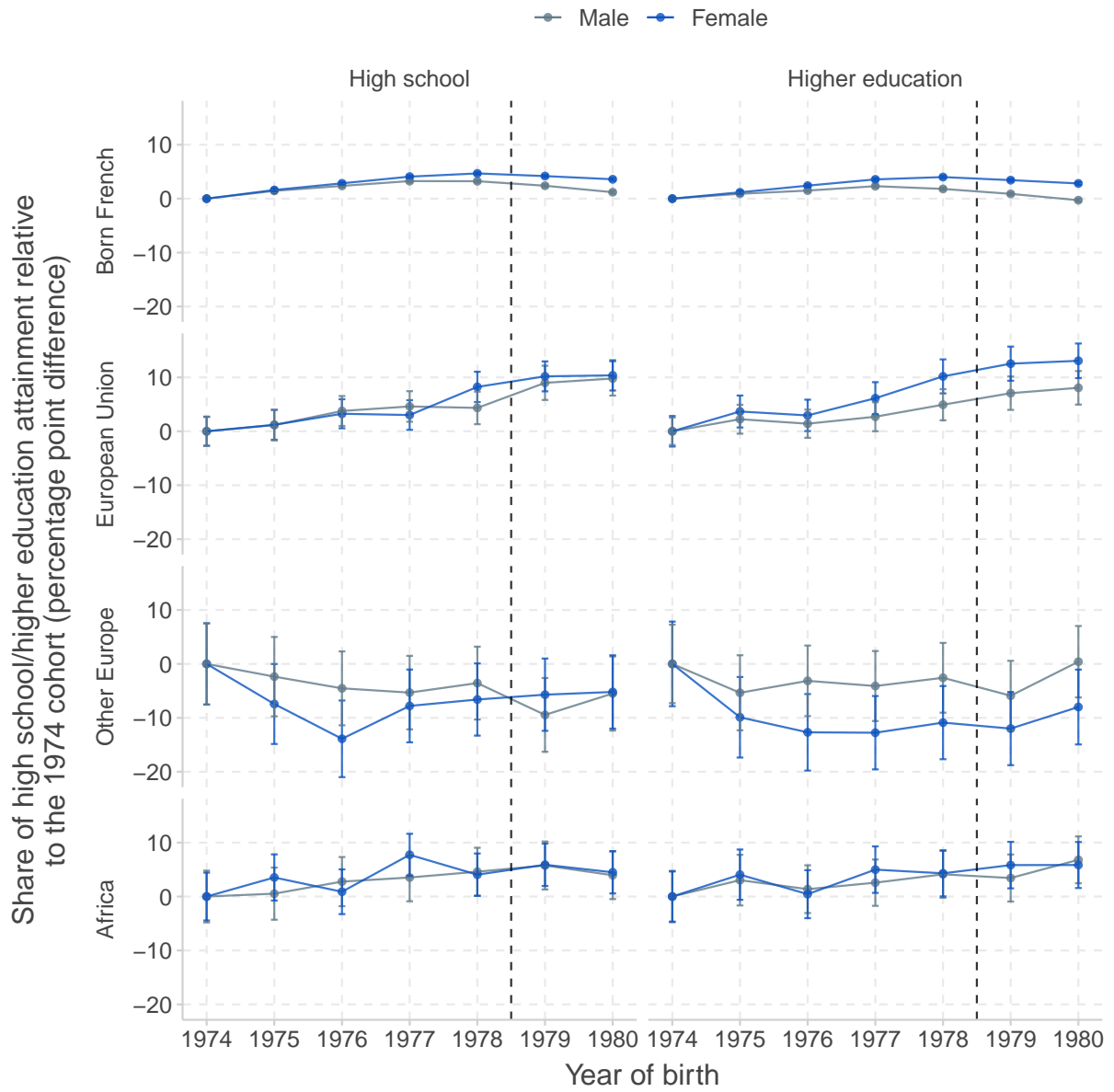


Figure A.8: Detrended difference in employment trends between males females - EU vs. Non-EU citizens at birth

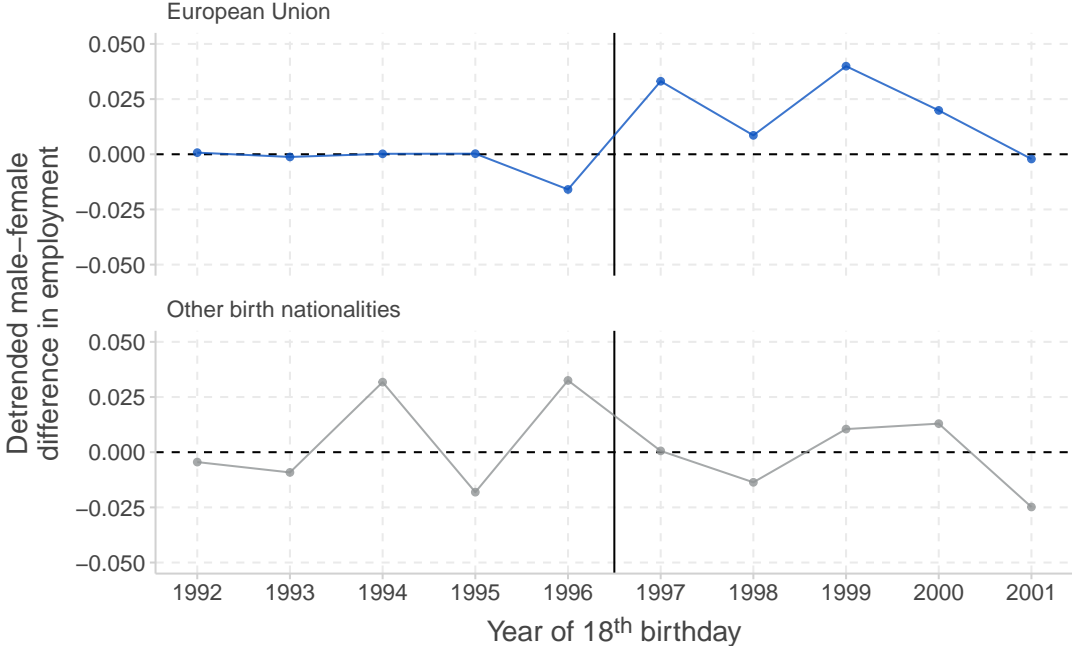


Figure A.9: Naturalization rates of first-generation immigrants arrived by age 6

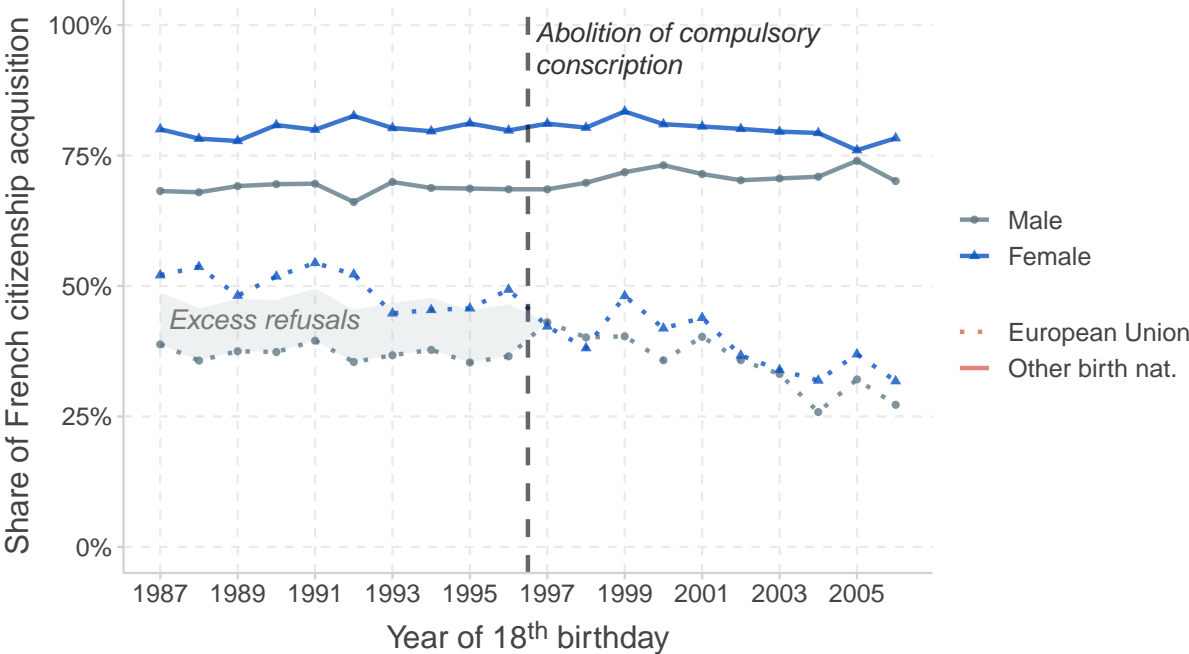


Figure A.10: Registration in electoral list

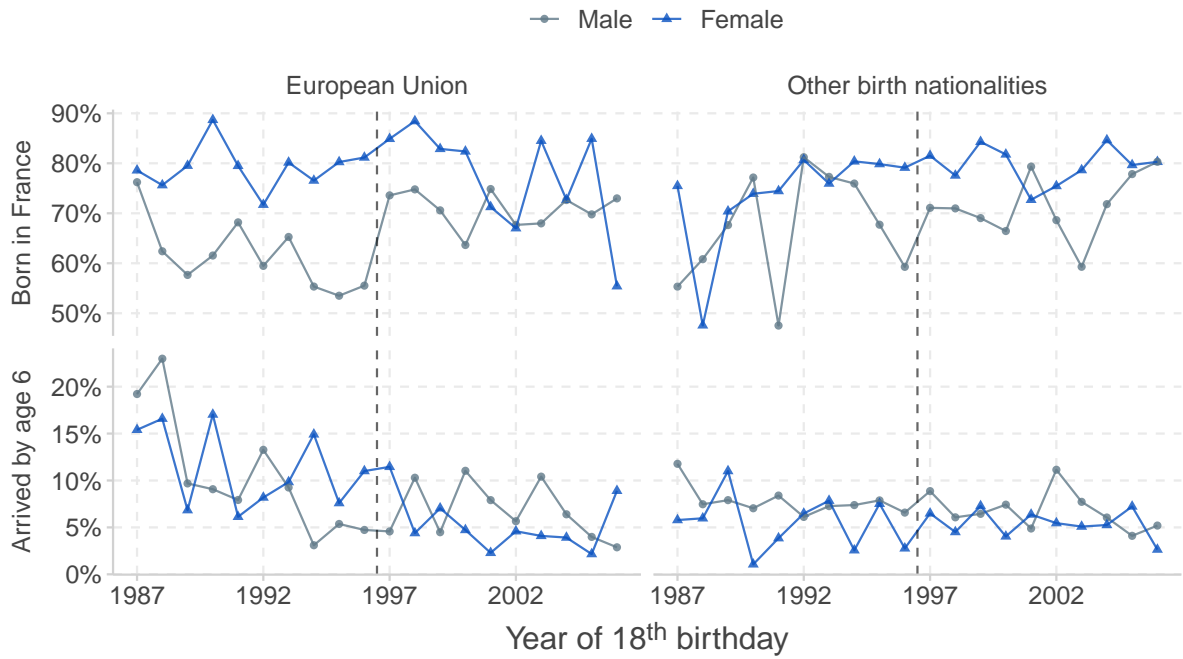


Figure A.11: Age at registration in electoral list

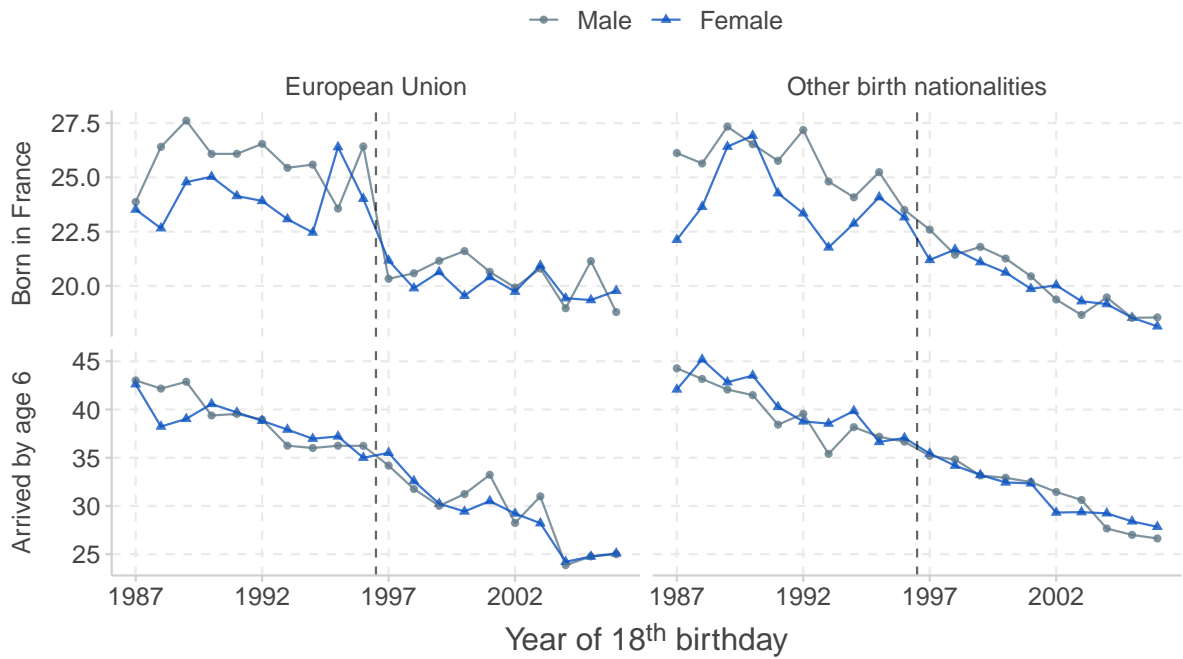


Figure A.12: Female labor market outcomes by their husband's birth cohort

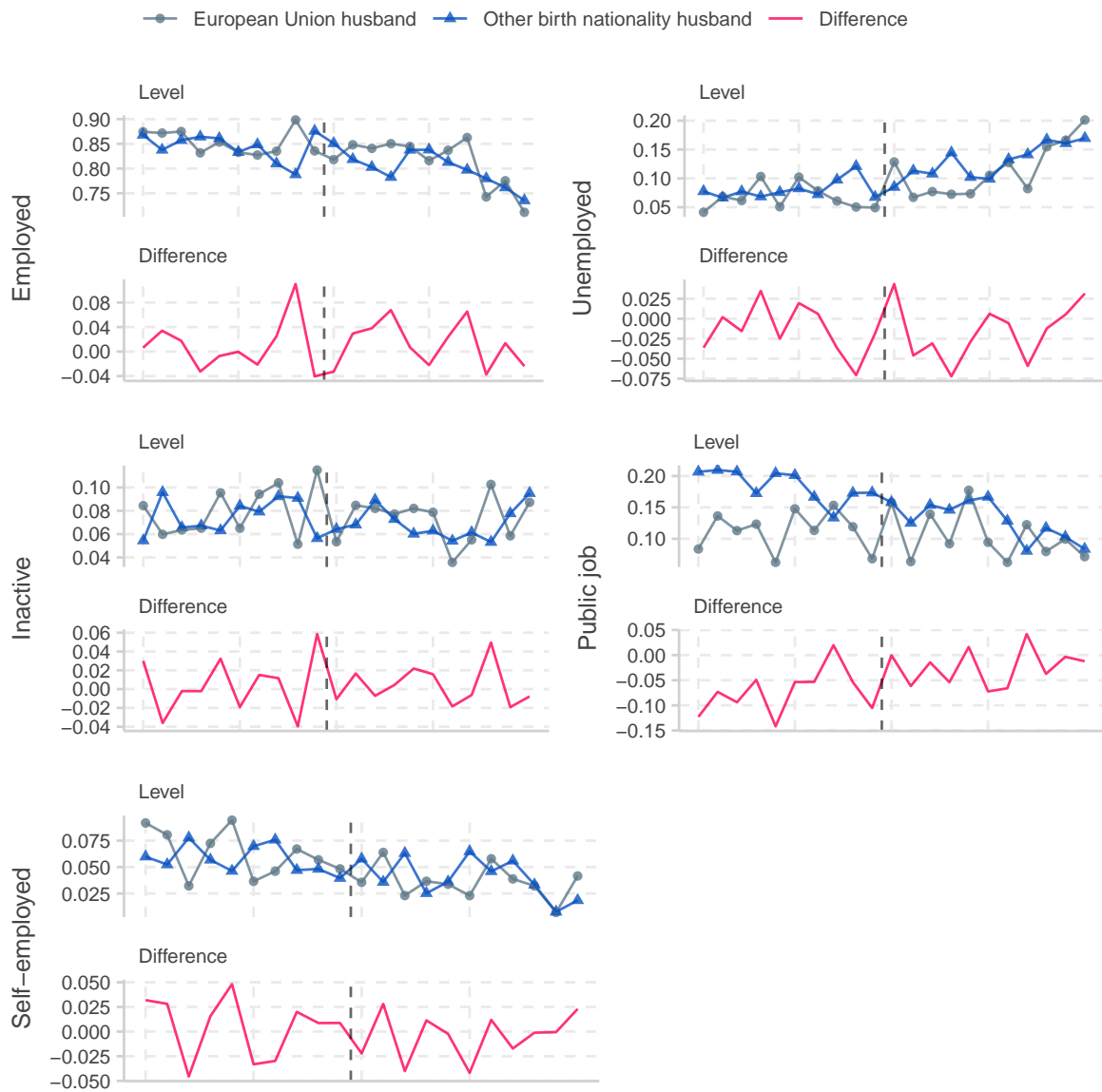


Figure A.13: Naturalization rates by duration of the military service of birth nationality

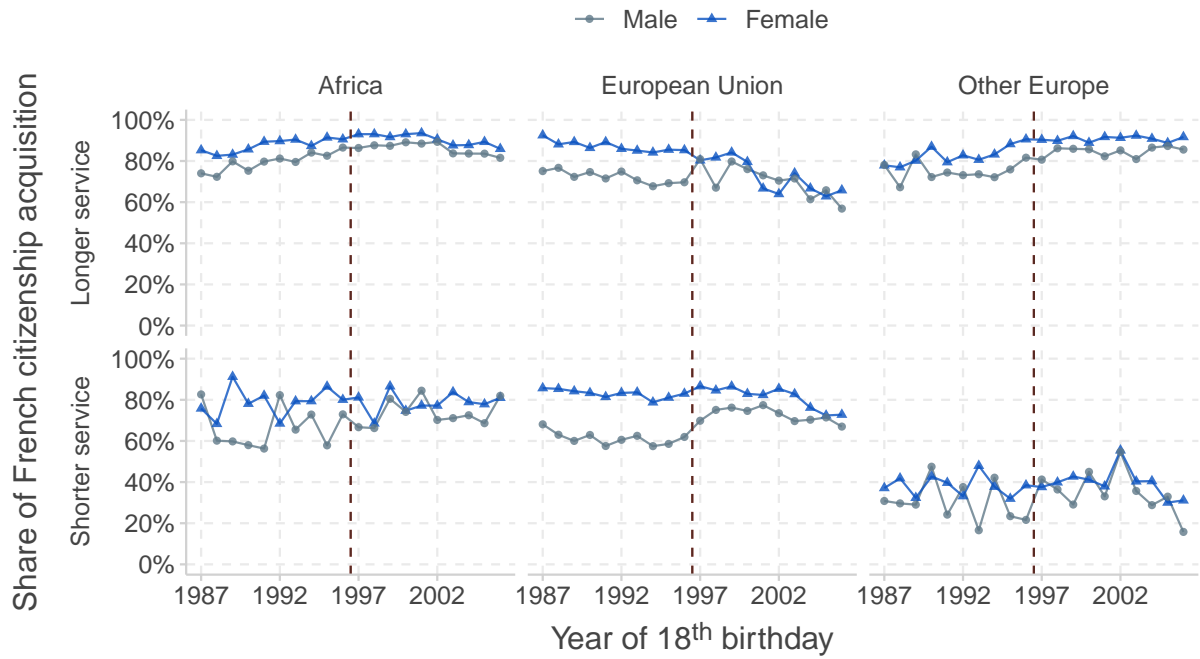


Figure A.14: Rates of presence in the 2014 census among 1990 and 1999 census populations

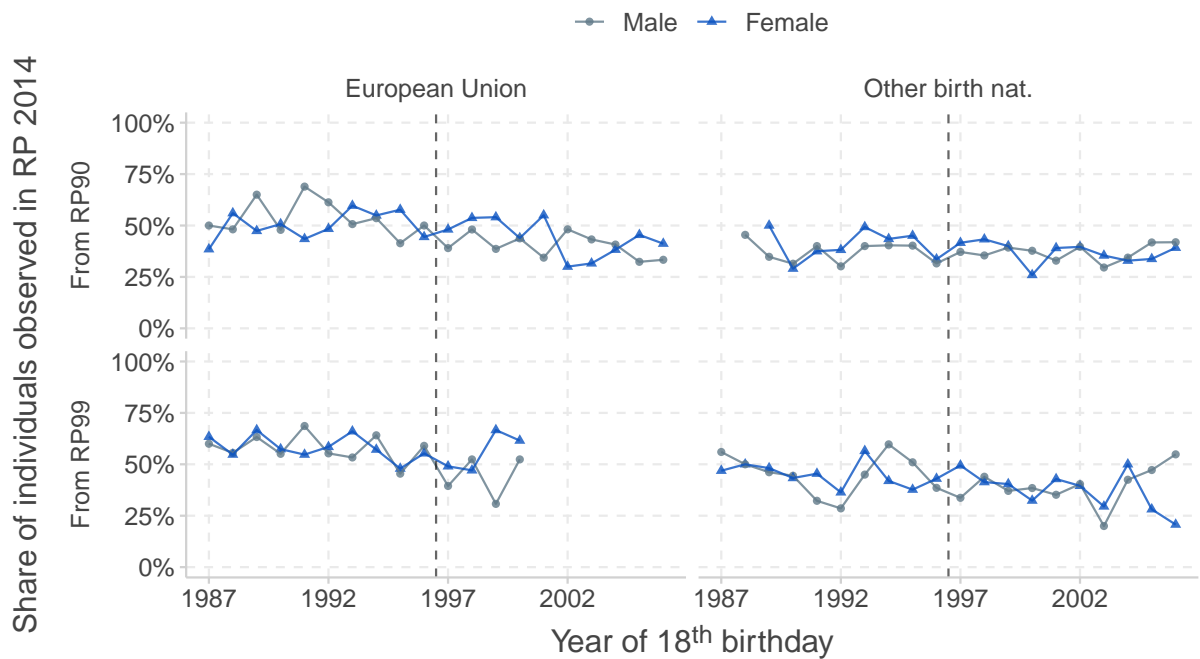


Figure A.15: Anticipation of the reform - Naturalization rates by month of birth

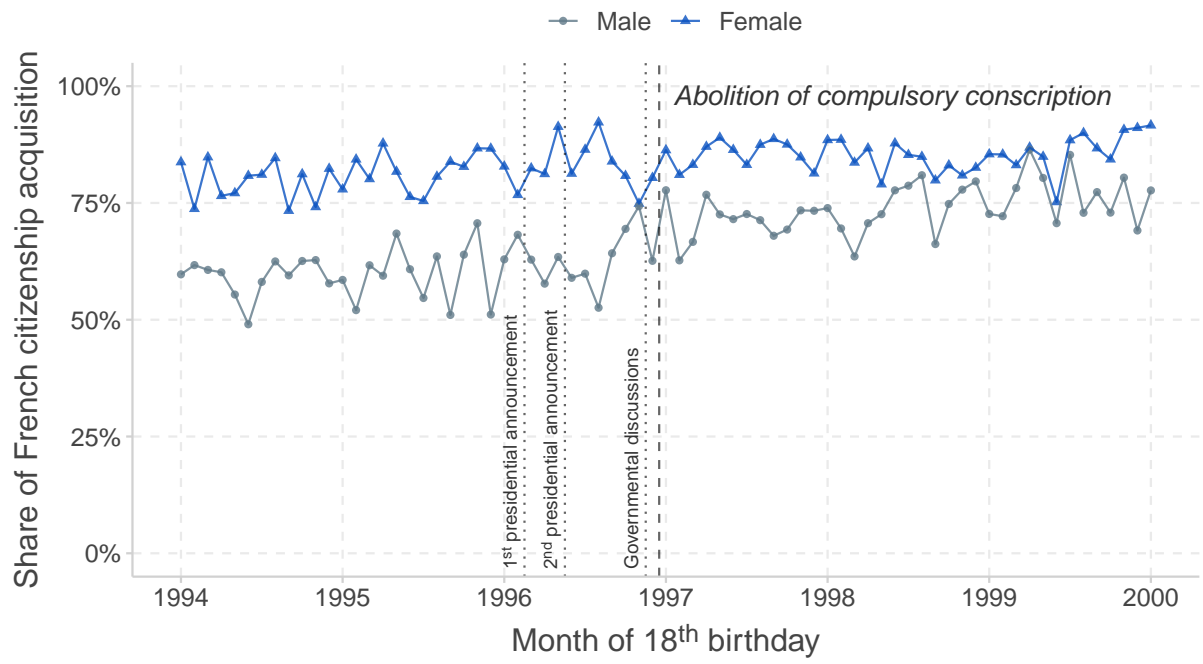




Figure A.16: Employment ITT - Every combination of control groups

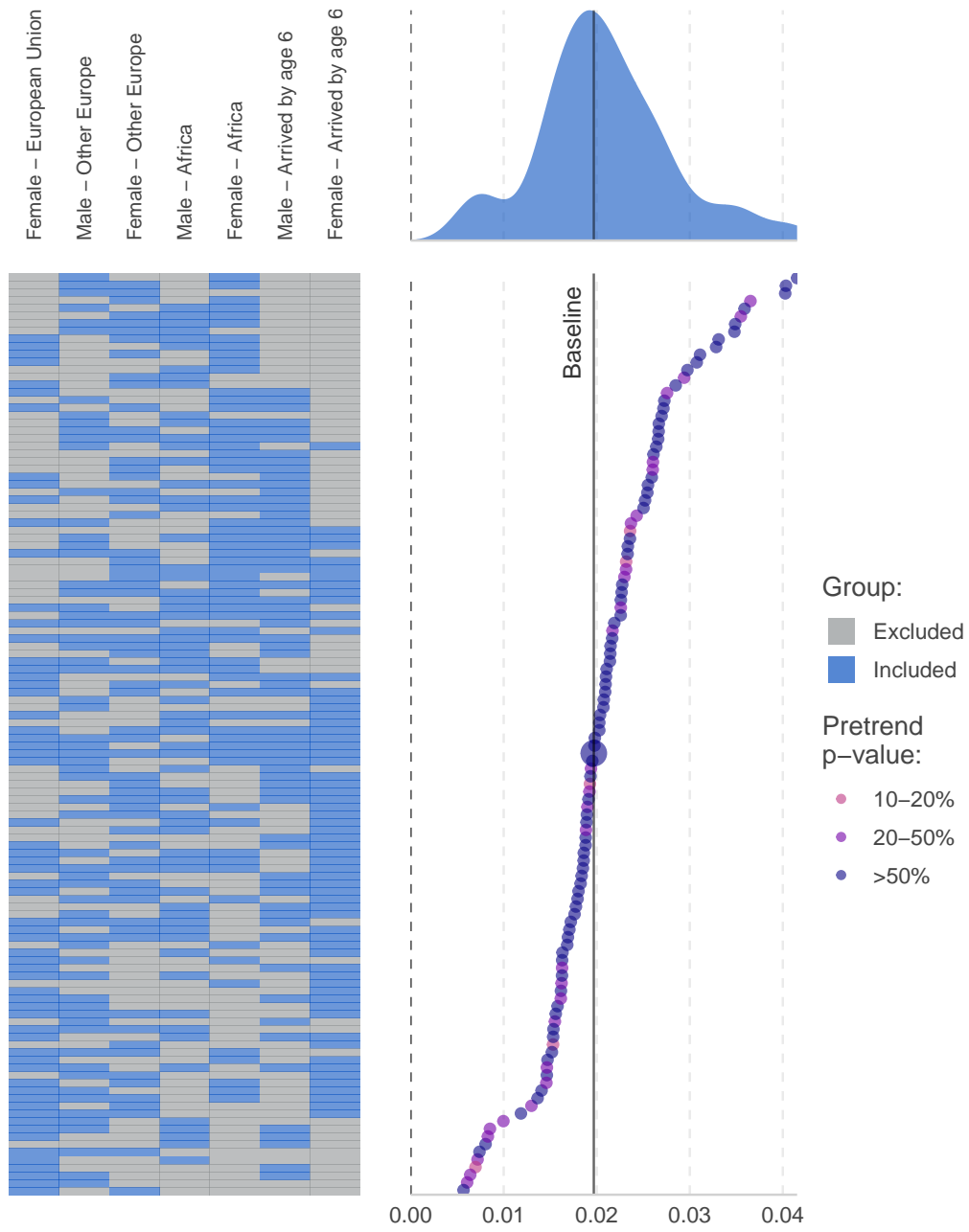


Figure A.17: Inactive ITT - Every combination of control groups

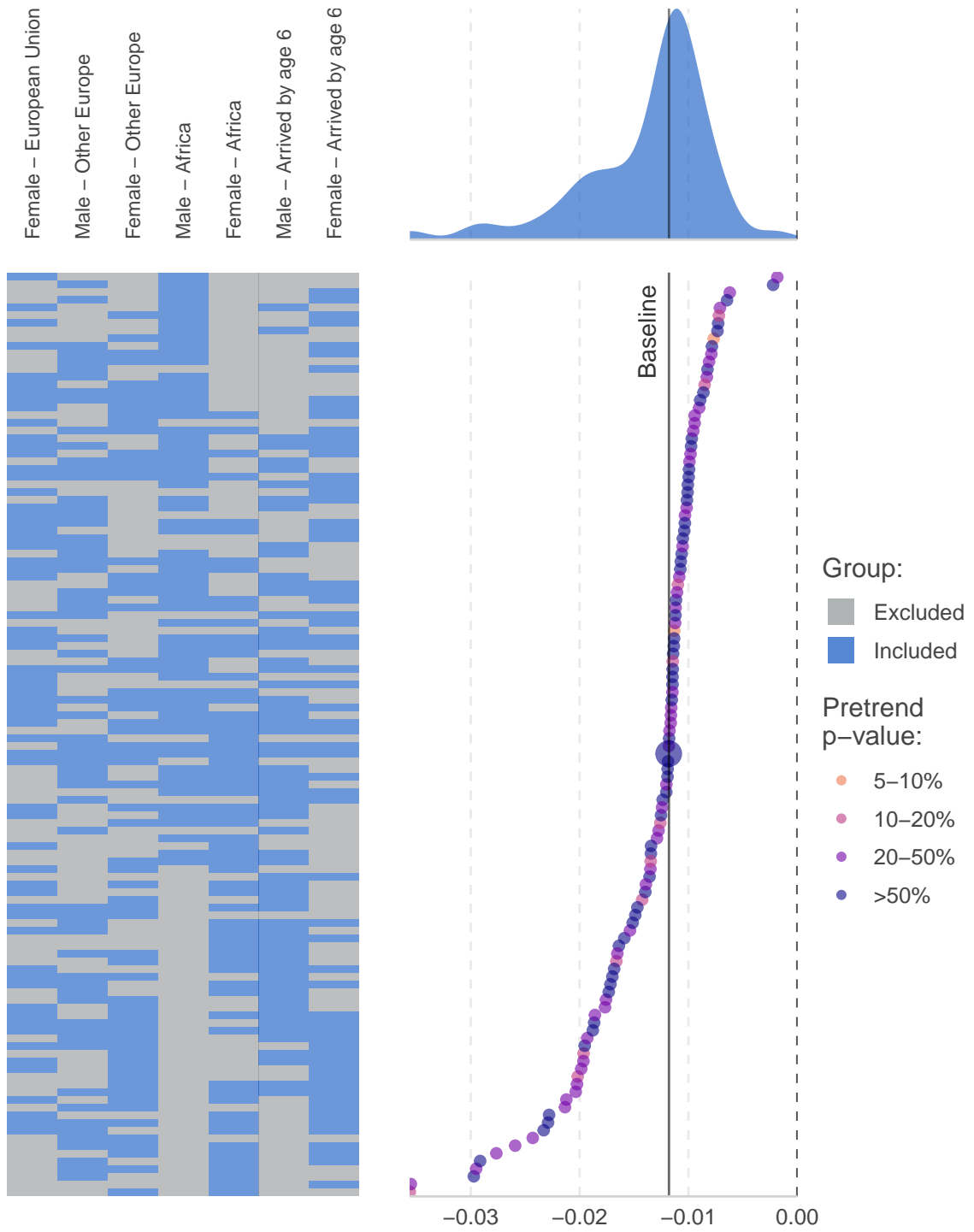


Figure A.18: Unemployed ITT - Every combination of control groups

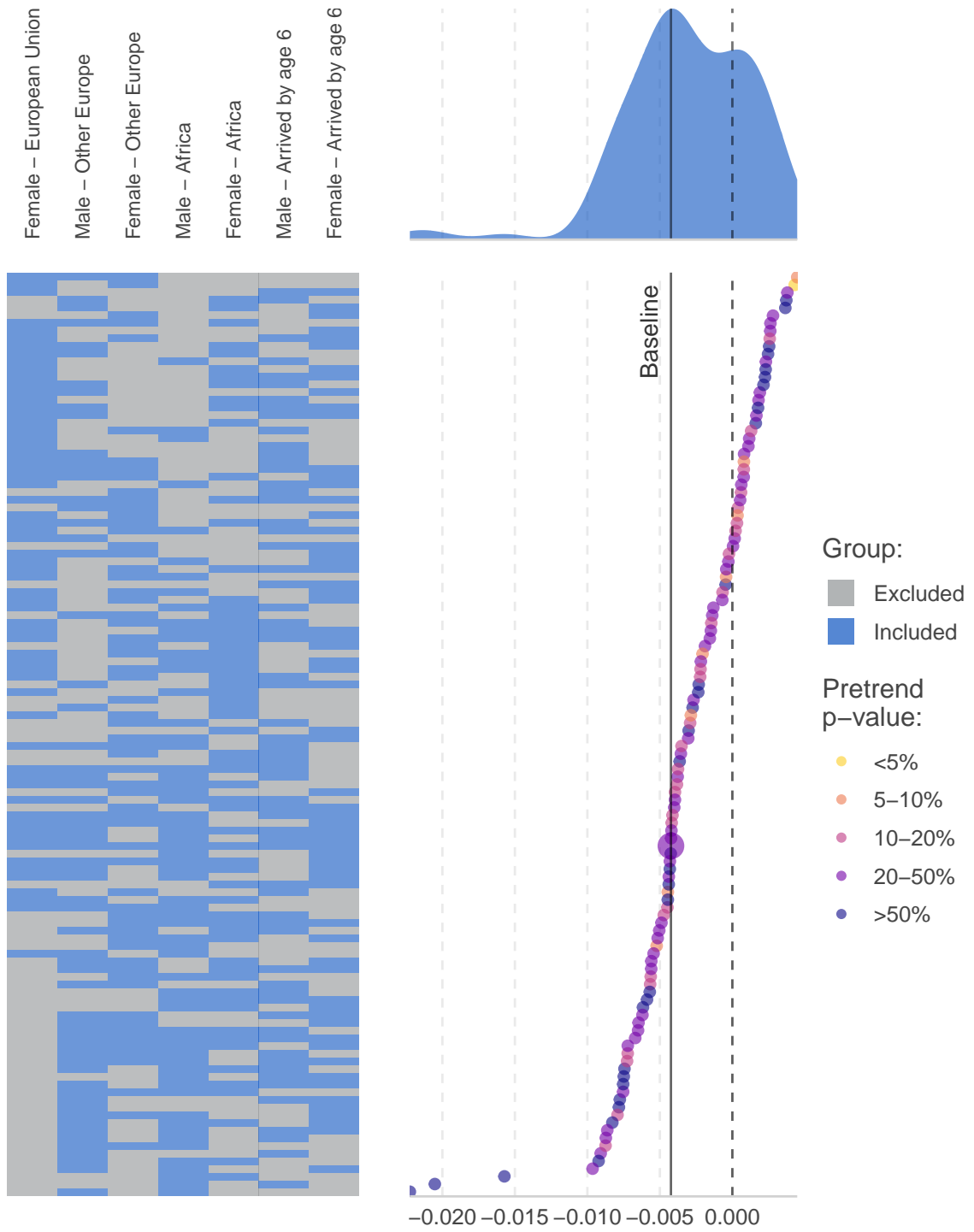


Figure A.19: Public job ITT - Every combination of control groups

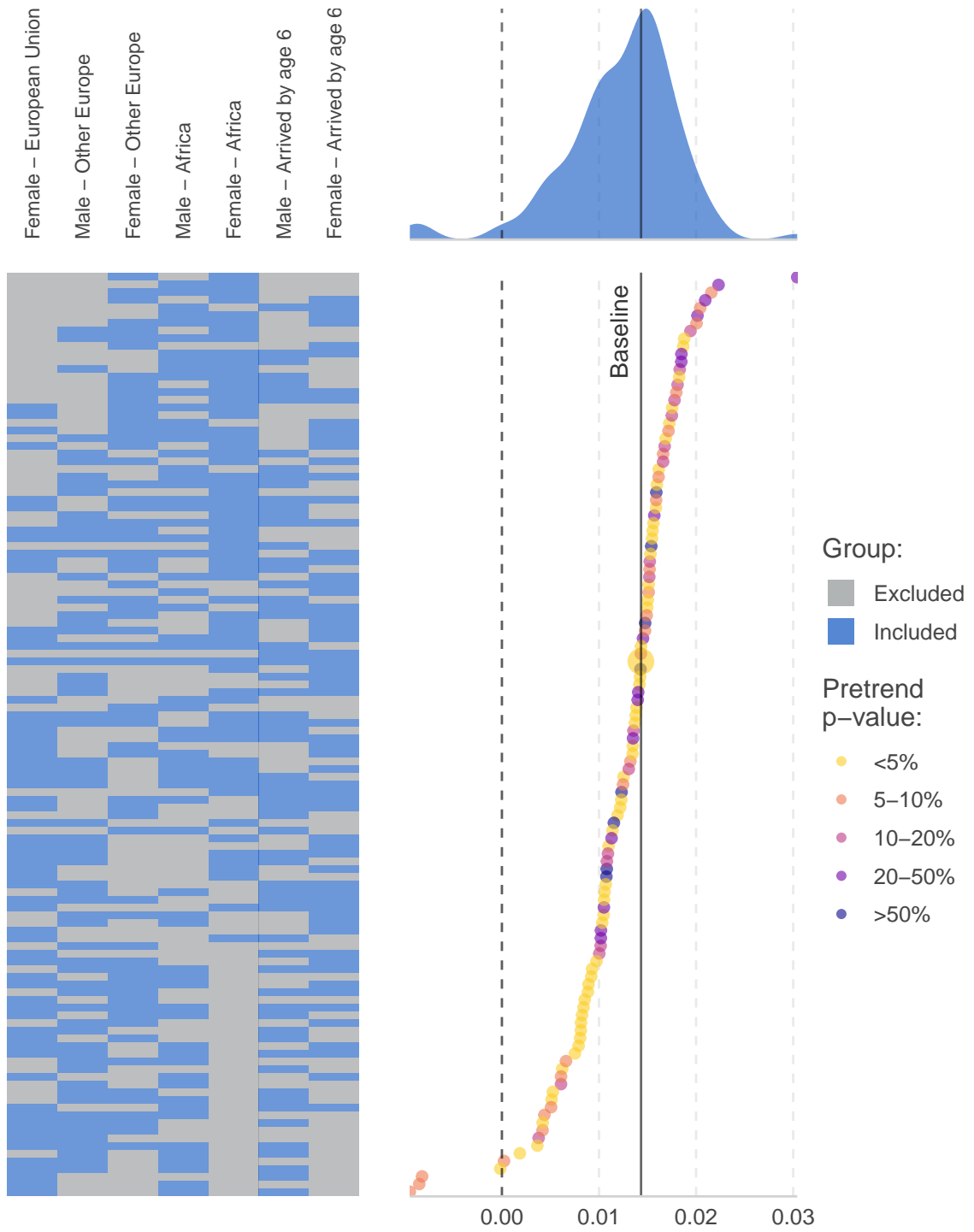
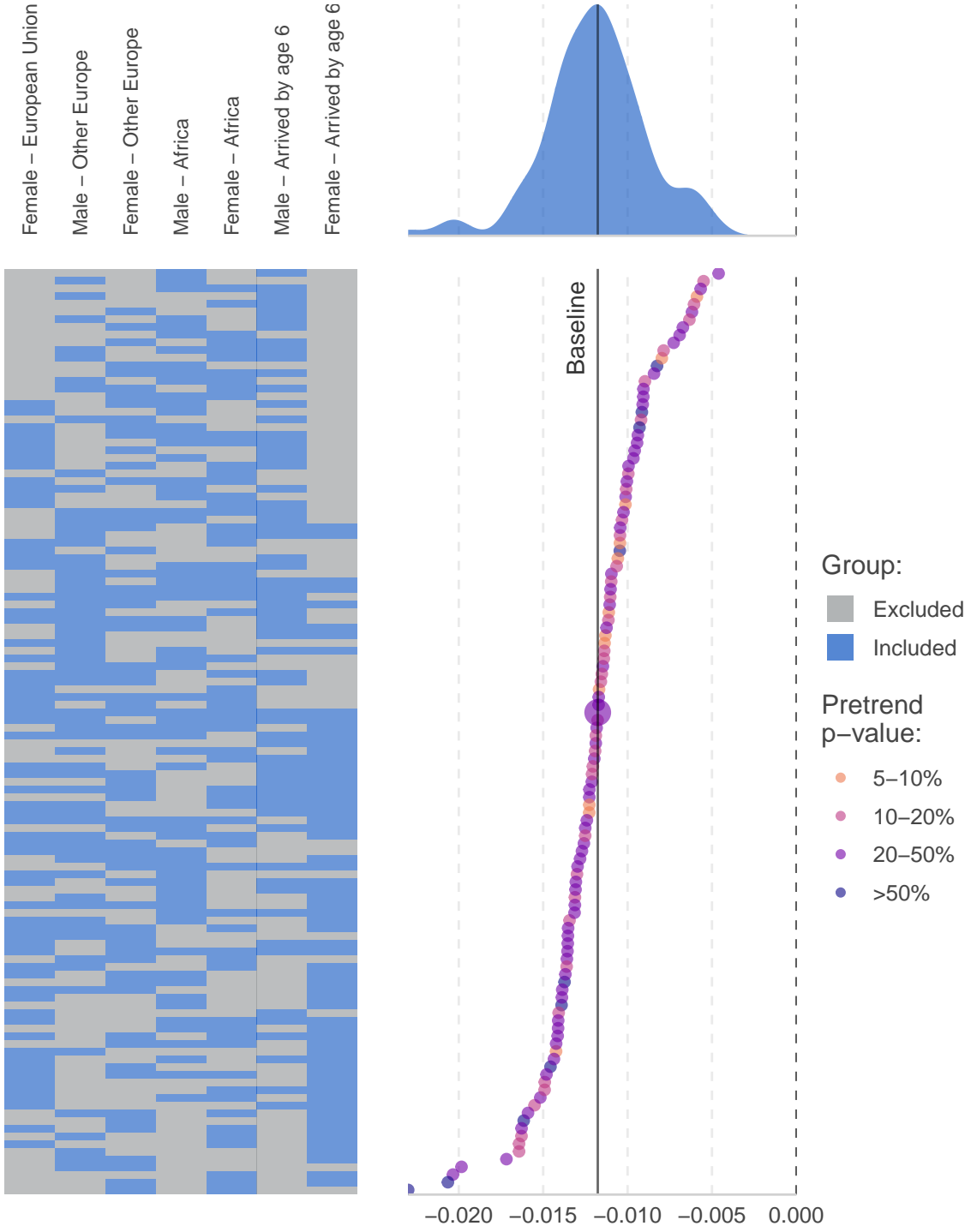


Figure A.20: Self-employed ITT - Every combination of control groups



## B Additional tables

Table B.1: Share of the most represented birth nationalities of individuals born in France without French citizenship

European Union		Other Europe		Africa		Other	
Portuguese	33.69%	Turkish	7.05%	Moroccan	15.97%	Cambodian	0.84%
Spanish	6.34%	Serbian	2.01%	Tunisian	7.61%	Laotian	0.72%
Italian	6.34%	Polish	0.72%	Algerian	4.07%	Vietnamese	0.53%
Belgian	0.72%	British	0.68%	Cameroonian	0.85%	Haitian	0.52%
German	0.65%	Swiss	0.66%	Congolese	0.7%	Chinese	0.51%
Other	0.64%	Other	1.35%	Other	3.89%	Other	2.92%
Total	48.37%	Total	12.48%	Total	33.11%	Total	6.04%

Table B.2: Effect of the abolition of compulsory conscription on naturalization

French citizenship acquisition	
Male	-0.171*** (0.004)
Post	0.026*** (0.004)
Post × Male	0.088*** (0.006)
Constant	0.844*** (0.003)
Observations	70,117
R2	0.037

Notes: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Window: ±5years.

Table B.3: Effect of the abolition of compulsory conscription on naturalization by education and birth nationality group

	European Union		Other Europe		Africa	
	Low edu.	High edu.	Low edu.	High edu.	Low edu.	High edu.
Post	0.025** (0.010)	-0.016** (0.008)	0.048*** (0.014)	0.051*** (0.019)	0.028*** (0.010)	-0.005 (0.008)
Male	-0.222*** (0.008)	-0.128*** (0.008)	-0.109*** (0.014)	-0.038* (0.021)	-0.062*** (0.010)	-0.065*** (0.009)
Post × Male	0.125*** (0.014)	0.082*** (0.012)	0.027 (0.019)	-0.026 (0.028)	0.019 (0.014)	-0.001 (0.012)
Constant	0.755*** (0.006)	0.900*** (0.005)	0.826*** (0.010)	0.803*** (0.014)	0.82*** (0.007)	0.930*** (0.006)
Observations	20,064	14,284	6,539	3,138	12,554	10,016
R <sup>2</sup>	0.048	0.021	0.021	0.007	0.007	0.011
F-stat.	340***	102***	47***	8***	30***	39***

Notes: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Window: ±5years.

Table B.4: Effect of the abolition of compulsory conscription on naturalization by education and birth nationality group

	European Union as in 1996		European Union as in 1994	
	Low edu.	High edu.	Low edu.	High edu.
Post	0.025** (0.010)	-0.016** (0.008)	0.025** (0.010)	-0.014* (0.008)
Male	-0.222*** (0.008)	-0.128*** (0.008)	-0.223*** (0.008)	-0.128*** (0.008)
Post × Male	0.125*** (0.014)	0.082*** (0.012)	0.124*** (0.014)	0.083*** (0.012)
Constant	0.755*** (0.006)	0.900*** (0.005)	0.755*** (0.006)	0.900*** (0.005)
Observations	20,064	14,284	20,042	14,229
R <sup>2</sup>	0.048	0.021	0.048	0.021
F-stat.	340***	102***	340***	102***

Notes: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Window: ±5years.

Table B.5: Effect of the abolition of compulsory conscription on naturalization on European Union citizens

	French citizenship acquisition		
	(1)	(2)	(3)
Male	-0.210*** (0.006)	-0.210*** (0.006)	-0.187*** (0.006)
Post	0.016** (0.007)	0.047*** (0.009)	0.033*** (0.009)
Post × Male	0.116*** (0.009)	0.116*** (0.009)	0.121*** (0.009)
Age		0.006*** (0.001)	0.007*** (0.001)
Higher Education			0.167*** (0.005)
Constant	0.824*** (0.004)	0.594*** (0.043)	0.498*** (0.042)
Observations	34,348	34,348	34,348
R2	0.048	0.049	0.084
F-stat.	577***	440***	629***

Notes: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Window: ±5years



Table B.6: Effect of naturalization on public-sector jobs

	Public job
ITT ( $\hat{\tau}$ )	0.014** (0.007)
LATE ( $\hat{\tau}/\hat{\gamma}$ )	0.119* (0.063)
Group weights ( $\hat{\omega}_i$ )	
Female - European Union	0.119
Male - Other Europe	0.187
Female - Other Europe	0.100
Male - Africa	0.131
Female - Africa	0.168
Male - Arrived by age 6	0.135
Female - Arrived by age 6	0.160
Time weights ( $\hat{\lambda}_t$ )	
1992	0
1993	0.808
1994	0
1995	0
1996	0.192
Education	X
Age	X
First-stage F-stat.	629***
Share of compliers ( $\hat{\gamma}$ )	0.121*** (0.009)

*Notes:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Bootstrapped standard errors in parentheses.

Table B.7: Effect of naturalization on self-employment

	Self-employment
ITT ( $\hat{\tau}$ )	-0.012 (0.007)
LATE ( $\hat{\tau}/\hat{\gamma}$ )	-0.097 (0.066)
Group weights ( $\hat{\omega}_i$ )	
Female - European Union	0.159
Male - Other Europe	0.054
Female - Other Europe	0.139
Male - Africa	0.167
Female - Africa	0.152
Male - Arrived by age 6	0.172
Female - Arrived by age 6	0.157
Time weights ( $\hat{\lambda}_t$ )	
1992	0
1993	0.273
1994	0
1995	0.288
1996	0.438
Education	X
Age	X
First-stage F-stat.	629***
Share of compliers ( $\hat{\gamma}$ )	0.121*** (0.009)

*Notes:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Bootstrapped standard errors in parentheses.

Table B.8: Effect of naturalization on self-reported experienced discrimination

	Citizenship Acquisition	Self-reported discrimination		About skin tone, origins nationality, or accent	
		ITT	IV	ITT	IV
Male	-0.161*** (0.040)	-0.234*** (0.049)	-0.238*** (0.045)	-0.200*** (0.045)	-0.211*** (0.041)
Post	-0.048 (0.036)	0.181*** (0.044)	0.180*** (0.043)	0.185*** (0.041)	0.182*** (0.040)
Post × Male	0.246*** (0.048)	-0.007 (0.058)		-0.016 (0.053)	
$\widehat{\text{Acquisition}}$			-0.030 (0.236)		-0.064 (0.218)
Constant	-10.65 (9.00)	32.26*** (10.98)	31.95*** (11.24)	39.41*** (10.10)	38.73*** (10.37)
Age	X	X	X	X	X
Education	X	X	X	X	X
Gender	X	X	X	X	X
EU citizenship	X	X	X	X	X
Born in France	X	X	X	X	X
Survey wave	X	X	X	X	X
Observations	1,612	1,605	1,605	1,604	1,604
R2	0.191	0.109	0.116	0.137	0.141

*Notes:* This table shows the results of a Two-stage Least-squares estimation of the effect of naturalization on the sense of belonging of naturalized second-generation immigrants. The first column documents the first-stage difference-in-differences specification, regressing citizenship acquisition on being a male, being born after 1979, i.e., in birth cohorts that were no longer subject to compulsory conscription, and on the interaction between the two. The first two columns show the results on self-reported discrimination experienced during the past 5 years, and the last two columns self-reported discrimination specifically about skin tone, origins, nationality, or accent. For these two outcomes, the first column shows the reduced form regression that identifies the Intention-to-treat effect, while the second column shows the Instrument Variable regression where the acquisition of French citizenship is instrumented by the first-stage difference-in-differences interaction. Standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. All specification in this table control for age, education, gender, EU citizenship, being born in France, and TeO survey wave. Source: Trajectories and Origins, INSEE-INED.

Table B.9: Effect of naturalization on the sense of belonging

	Citizenship Acquisition	I feel French		I feel at home in France	
		ITT	IV	ITT	IV
Male	−0.161*** (0.040)	−0.095* (0.053)	0.001 (0.051)	−0.024 (0.051)	0.009 (0.047)
Post	−0.048 (0.036)	−0.024 (0.048)	0.003 (0.048)	−0.051 (0.046)	−0.041 (0.046)
Post × Male	0.246*** (0.048)	0.142** (0.063)		0.049 (0.060)	
$\widehat{\text{Acquisition}}$			0.581** (0.264)		0.200 (0.247)
Constant	−10.65 (9.00)	−18.61 (11.95)	−12.52 (12.55)	−12.01 (11.46)	−9.87 (11.86)
Age	X	X	X	X	X
Education	X	X	X	X	X
Gender	X	X	X	X	X
EU citizenship	X	X	X	X	X
Born in France	X	X	X	X	X
Survey wave	X	X	X	X	X
Observations	1,612	1,591	1,591	1,595	1,595
R2	0.191	0.047	0.005	0.034	0.021

*Notes:* This table shows the results of a Two-stage Least-squares estimation of the effect of naturalization on the sense of belonging of naturalized second-generation immigrants. The first column documents the first-stage difference-in-differences specification, regressing citizenship acquisition on being a male, being born after 1979, i.e., in birth cohorts that were no longer subject to compulsory conscription, and on the interaction between the two. The first two columns show the results on the statement “*I feel French*”, and the last two columns on the statement “*I feel at home in France*”. For these two outcomes, the first column shows the reduced form regression that identifies the Intention-to-treat effect, while the second column shows the Instrument Variable regression where the acquisition of French citizenship is instrumented by the first-stage difference-in-differences interaction. Standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. All specification in this table control for age, education, gender, EU citizenship, being born in France, and TeO survey wave. Source: Trajectories and Origins, INSEE-INED.

Table B.10: Robustness of the ITT to the exclusion of the 1978 birth cohort

	With time weights		Without time weights	
	Including $t - 1$	Excluding $t - 1$	Including $t - 1$	Excluding $t - 1$
Employed	0.020** (0.009)	0.018** (0.009)	0.025*** (0.008)	0.022*** (0.008)
Public job	0.014** (0.007)	0.014* (0.008)	0.016*** (0.005)	0.014** (0.006)
Inactive	-0.012** (0.006)	-0.010* (0.006)	-0.017*** (0.004)	-0.017*** (0.004)
Self-employed	-0.012 (0.007)	-0.012 (0.008)	-0.012** (0.006)	-0.012* (0.007)
Unemployed	-0.004 (0.009)	-0.005 (0.008)	-0.005 (0.006)	-0.003 (0.007)

*Notes:* This table reports the results of Synthetic Difference-in-Differences specification estimating the effect of the increase in the naturalization rates of males born European Union citizens in France induced by the abolition of compulsory conscription on several labor market outcomes. The first two columns report the estimate for each outcome variable using the time weights as in [Arkhangelsky et al. \(2021\)](#), while the last two columns show the estimate resulting from an equal weighting of each birth cohort. The first column of each of these two sets includes all birth cohorts in the pre-period, while the last one is estimated without the 1978 birth cohort, i.e., the last one before the reform, which is potentially subject to anticipation effects. Bootstrapped standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ . Source: 2014 French Population Census.

Table B.11: Sensitivity of baseline results to the share of natives in the employment zone

Outcome	ITT (1)	ITT (2)	LATE (3)	LATE (4)
Employed	0.020** (0.009)	0.020** (0.009)	0.163* (0.084)	0.163** (0.079)
Public job	0.014** (0.007)	0.014** (0.007)	0.119* (0.063)	0.119* (0.064)
Inactive	-0.012* (0.006)	-0.012* (0.006)	-0.098* (0.057)	-0.098* (0.056)
Self-employed	-0.012 (0.007)	-0.012 (0.007)	-0.097 (0.066)	-0.097 (0.065)
Unemployed	-0.004 (0.008)	-0.004 (0.008)	-0.035 (0.069)	-0.035 (0.070)
%Natives in ZE		X		X

*Notes:* \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ . Bootstrapped standard errors in parentheses.

Table B.12: Robustness of the LATE to the exclusion of the 1978 birth cohort

	With time weights		Without time weights	
	Including $t - 1$	Excluding $t - 1$	Including $t - 1$	Excluding $t - 1$
Employed	0.163* (0.084)	0.149* (0.080)	0.207*** (0.070)	0.183*** (0.067)
Public job	0.119* (0.063)	0.119* (0.066)	0.130*** (0.048)	0.116** (0.050)
Inactive	-0.098* (0.057)	-0.083 (0.052)	-0.145*** (0.040)	-0.137*** (0.042)
Self-employed	-0.097 (0.066)	-0.097 (0.065)	-0.101* (0.056)	-0.103* (0.056)
Unemployed	-0.035 (0.069)	-0.037 (0.070)	-0.040 (0.058)	-0.027 (0.056)

Notes: \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ . Bootstrapped standard errors in parentheses.