

ARTICLE

“Welcome to France.” Can mandatory integration contracts foster immigrant integration?

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Abstract

European governments, struggling with incorporating diverse immigrant populations, introduced integration contracts. Through language training and compulsory civics courses, these contracts aim to induce new migrants to adopt the host society's culture, respect its values, and improve their labor market outcomes. Despite their popularity, little empirical evidence exists on whether integration contracts catalyze integration or trigger a backlash. To shed light on this question, we leverage the staggered introduction of France's integration contract across metropolitan departments between 2003 and 2006 to implement a regression discontinuity design. We use census data, labor force surveys, and our own survey of refugees to estimate the effect of the contract on integration outcomes. We find the integration contract facilitated employment in the short term without backlash but did not translate into long-lasting integration gains.

As European governments struggled to integrate increasingly diverse and growing immigrant populations, so-called “civic integration” policies emerged in the late 1990s as a novel and important policy lever (Goodman, 2011, 2013, 2014; Joppke, 2007a,b; Michalowski & van Oers, 2012). “Civic integration” policies rest on the idea that “basic knowledge of the host society's language, history, and institutions is indispensable to integration; enabling immigrants to acquire this basic knowledge is essential to successful integration” (Council of the European Union, 2004). In practice, they condition entry, permanent residence, or citizenship on acquiring “civic skills,” which include speaking the host country's language, learning about its history and culture, and adhering to its values. Relatively rare until the early 2000s, many

European countries that experienced waves of immigration in the past decades now implement some civic integration policies in the form of civic training, citizenship tests, or integration contracts (Goodman & Wright, 2015).

Despite the popularity of these policies in Europe, we still need to learn more about the effect of introducing integration requirements for permanent residence on actual integration. Surprisingly, the evidence on whether civic integration policies “help or hinder integration” (Strik, 2013) is relatively thin. The few scholars who have examined the quantitative effect of civic integration policies on economic and political integration in Europe have relied on cross-national variation in the intensity of civic integration policies overall, that is, at all stages of the integration process (entry,

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permanent residence, and naturalization) and reached opposite conclusions.¹ Goodman and Wright (2015) found that integration requirements did not affect employment, financial well-being, and social trust but positively affected political interest and efficacy. In contrast, Neureiter (2019) concluded that integration requirements had a strong and positive effect on economic integration but no impact on social and political integration. Moving away from cross-country comparisons, recent studies have analyzed the effectiveness of one component—language training—of the French integration contract, both on the extensive (Lochmann et al., 2019) and intensive margins (Pont-Grau et al., 2023), reporting no effect of the language training on the probability of finding employment.

Thus, we still need more evidence on the impact of specific civic integration policies on immigrant integration, which is the gap this study intends to fill. We analyze the impacts of France's integration contract (*Contrat d'accueil et d'intégration* [CAI]) on immigrant integration. Launched in July 2003, the CAI policy strongly encouraged all newly arrived non-EU migrants to sign a contract with the French state. Although signing the contract only became mandatory in 2007,² the policy reached over 90% in compliance in its first years of implementation.³ Immigrants who sign the contract must attend a mandatory 1-day civic training oriented to respecting the values of French society. As part of the program, they are also given the option to get language training and attend a "Living in France" information session. In 2004, these optional trainings were attended by roughly 20% to 30% of signatories.

To estimate the effect of the contract on integration, we leverage the discontinuity created by the staggered introduction of the contract in the 96 departments of Metropolitan France between 2003 and 2006. We estimate that the probability of signing the contract increased substantially for immigrants and even more so for refugees who arrived after the introduction of the contract (compared to those who came before). This discontinuity in the exposure to the policy allows us to estimate the effect of the policy using a regression discontinuity design, essentially comparing integration outcomes of immigrants who settled in France right before to those who arrived right after the introduction of the policy within the same depart-

ment. Using two large nationally representative data sets (French Census records and the French Labor Force Survey), we can precisely estimate potentially minor effects of the policy on standard integration outcomes (employment, naturalization, intermarriage). We also make use of the repeated feature of these government-produced surveys to estimate the effect of the integration contracts in the very short term (1 year after arrival), short term (2 to 5 years after arrival), medium term (6 to 10 years of arrival), and long term (more than 10 years after arrival). Finally, to enrich our integration outcomes, we partnered with the French asylum office to survey a representative sample of refugees and comprehensively measure multidimensional integration outcomes using the Immigration Policy Lab (IPL)-12 integration index (Harder et al., 2018).

This study yields two main findings. First, the French integration contract significantly increased the probability of employment 1 year after arrival. Estimates are substantial (+ 5.5 percentage points (pp)) relative to a low baseline (only 27% of newly arrived immigrants are employed). Second, the short-term employment boost we observe 1 year after arrival does not translate into increased integration success in the medium or long run. After 2 years of residence in France, the difference in employment is down to 3.7 pp. After 3 years or more of residence in France, we find no substantial differences between those who were encouraged and those who were not in our primary outcomes. In line with previous studies (Lochmann et al., 2019; Pont-Grau et al., 2023), descriptive evidence suggests that this positive effect is likely not due to language training.

Our study makes three core contributions by combining a rigorous research design that allows us to identify causal effects, extensive nationally representative surveys and original survey data on refugees, and a multidimensional set of integration measures. First, the short-term employment boost we document suggests not only that there are barriers to accessing the labor market among newly arrived immigrants but, importantly, that "labor market onboarding" can be accelerated. Second, the lack of any meaningful impact of immigrant integration of this policy in the medium and long run has considerable policy implications. Third, we contribute to the literature on the backlash effects of integration policies by showing that exposure to host country norms and standards, even if most likely not enabling, also does not seem to hinder immigrant integration.

BACKGROUND

In recent decades, many European countries have overhauled their integration policies by introducing

¹ Complementing the quantitative evidence discussed above, Bassel et al. (2021), Böcker and Strik (2011), Monforte et al. (2019), and Van Oers (2013) rely on interviews of immigrants and experts to examine the effects of civic integration policies across countries and at each of the different stages.

² Law No. 2005-35 of January 18, 2005, on programming for social cohesion gave a legislative framework to the contract and decided on its generalization throughout the territory. Law No. 2006-911 of July 24, 2006, relating to immigration and integration made it mandatory to sign the contract, which until then was only optional.

³ Data on take-up in 2004 and 2005 come from the *Journal Officiel Sénat* May 19, 2005, p. 1385.

mandatory language and civic education requirements for immigrants. The core idea behind these policies is to shift the responsibility for integration from the government to immigrants by making entry, long-term settlement, and naturalization contingent on language acquisition, civic knowledge, and a commitment to liberal Western values. While civic integration policies take place at different stages of the immigration process (entry, permanent residence, and naturalization), we focus in this study on stage 2. The so-called CAI specifically targets newly arrived immigrants intending to settle in France permanently. This represents about half of all France's new migrants—about 200,000 new residence permits (*titres de séjour*) each year, with about 100,000 signing the contract (as EU migrants and students, among others, are not subject to the CAI; Gagneron et al., 2013). Constrained by our research design, we evaluate this policy in its first years of implementation (2003–2006). In the beginning, it had three main components: a mandatory 1-day civics training, language training of up to 400 h for those whose French was deemed insufficient (with a “survival” target level that corresponds to an elementary mastery of French),⁴ and a 1-day “Living in France” information session for those interested. A 3-h skill assessment was introduced in 2009 (Office Français de l'Immigration et de l'Intégration, 2009) and is thus not part of our study.

Although signing the contract was not mandatory at first—the CAI only became compulsory in 2007, that is, after our study period (2003 to 2006)—the policy reached over 90% in compliance in its first years of implementation.⁵ This very high compliance can be explained by the fact that it was presented early on as an essential condition for renewing residency permits. Already in December 2002, the Prime Minister announced that the contract would feature an “obligation to attend [the sessions] to obtain rights and benefits” (Jardonnnet, 2002). Indeed, the first version of the CAI informed the signatory that “when deciding on issuing a residency permit, the prefect will take into account whether the immigrant has signed the contract [...]”⁶

The 1-day civic training is the only component of the French integration contract that all signatories must attend. In 2004, 99.1% of the signatories attended it. Its planners designed it as a textbook presentation of France's political regime and institutions, its symbols and doctrine (liberty, equality, and fraternity), and

the meaning of and conditions permitting access to French citizenship (Haut Conseil à l'intégration, 2003). Even though gender equality and *laïcité* are only two of the many topics listed in the curriculum, they became central in implementing the civic training (Gourdeau, 2015). In the PowerPoint presentation, “*Laïcité*” was added to the doctrine of the French republic: “Liberté, Égalité, Fraternité... *Laïcité*” (Gourdeau, 2019). A 2013 report by the Inspector General of Administration even recommended that “compulsory civic training should be simplified and shortened to half a day, with its content refocused on the essential messages that we want to convey (in particular *laïcité* and gender equality)” (Gagneron et al., 2013).

In addition to this mandatory component, officers at *Office Français de l'Immigration et de l'Intégration* (OFII) also prescribe language training to those whose French was deemed insufficient during an individual interview. In 2004, only 30% of contract signatories were assigned to receive some language training. The proportion is much higher among refugees because they are much less likely to come from francophone countries (Barrot & Dupont, 2020). While the number of hours assigned could go up to 400 h, immigrants receiving language training received 260 h on average (Lochmann et al., 2019). Contract signatories were also given the opportunity to attend a 1-day information session titled “Living in France” and designed to provide practical information to facilitate entry into the labor market and access to basic services, including the health care system, the school system, and social benefits. Twenty-two percent of the signatories participated in this training in 2004.

The French integration contract resembles integration agreements introduced elsewhere in Europe. The Netherlands was the first, in 1998, to introduce a 12-month integration course for newcomers. Austria, Belgium (Flanders), Denmark, Germany, and France followed, introducing similar mandatory integration requirements in the early 2000s (Carrera, 2006). Today, at least 16 European countries set clear criteria for fulfilling integration contracts (or agreements).⁷ There is significant variation in course duration between countries. The most recent systematic review of integration contracts (Garibay et al., 2013) reported that in 2013, the language courses lasted 120 h in Flanders, 200 in Luxembourg, 300 in Austria and Norway, 600 in Germany and the Netherlands, and 2,000 in Denmark. With 200 to 500 h of language training, the French policy stands in the middle of the distribution. This review also noted that the civic training lasted 8 h in France,

⁴ Level referred to as the “A1.1 level,” that is, the level below the lowest level of the framework (A1) of the Common European Framework of Reference for Languages (CEFR). Source: <https://www.france-education-international.fr/diplome/dil/?langue=fr>

⁵ Data on take-up in 2004 and 2005 come from the *Journal Officiel Sénat*, May 19, 2005, p. 1385, <https://www.senat.fr/questions/base/2005/qSEQ050517711.html>

⁶ Link to the CAI: <https://travail-emploi.gouv.fr/IMG/pdf/cai.pdf>

⁷ Austria, Belgium (Flanders), the Czech Republic, Denmark, Estonia, France, Germany, Greece, Italy, Latvia, Malta, Norway, Sweden, several cantons in Switzerland, the Netherlands, and the United Kingdom. In some countries, the integration contracts are only mandatory for a subset of immigrants, typically refugees, or non-EU immigrants. Hungary also introduced integration contracts for refugees in 2014 but eliminated the policy 2 years later.

30 in Germany, 60 in Flanders, and 75 in Austria. While France sits on the lower end of the spectrum when it comes to the length of civic training, other European countries have adopted a similar light-touched approach: Italy introduced a 10-h-long civic training in 2012, and Sweden introduced a two-and-a-half day training course for asylum seekers in 2021.⁸

These policies vary along two other important dimensions: whether the courses are free, as is the case in France, or not, and whether permanent residence is conditioned only on the attendance of the classes, as is the case in France, or on completing integration tests. The fact that courses are free of charge in France is convenient for evaluation purposes since we can rule out that possible adverse effects could come from the financial burden imposed by the constraint of having to finance courses out of pocket. However, the fact that renewal of residency permits was conditioned on attendance rather than tests in France could mean that immigrants paid less attention to the content, something to keep in mind when considering the generalizability of our findings.

We study the effect of this policy on the economic, social, and psychological integration of immigrants. As with most integration policies, improving immigrants' employment prospects is at the heart of civic integration policies across Europe. In a context of growing anxiety about societal fragmentation (Holtug & Mason, 2010), integration contracts have also been put forward as prominent tools to foster national cohesion across Europe. Alaoui and Pélabay (2020) argue that the French integration contract was specifically designed to "reaffirm that the French model alone can resist communitarianism." As mentioned earlier, Nicolas Sarkozy introduced the integration condition as a legal requirement for a residence permit. He saw it as a way to help French *Prefets* "prevent communities from turning in on themselves," quoted in Alaoui and Pélabay (2020, p. 118). In addition, country experts believe that "the psychological effects of the courses are probably more important than the language progress made by the immigrants who participate in the courses" (Böcker & Strik, 2011).

DATA

We use three main sources of data for our analyses. First, we use two nationally representative government-produced surveys: the French Census records and the French Labor Force survey.⁹ The use

of government-produced surveys presents two advantages. First, their very large sample size allows us to precisely estimate possibly small effects. Second, the fact that these surveys are run every year over a number of years allows us to observe individuals at varying distances from the cutoff to estimate effects in the short, medium, and long term of the contract. However, these surveys are also constrained by the limited breadth of the relevant outcomes they contain. Therefore, we conducted our own survey of a representative sample of refugees in France (provided by the French Asylum Office) to complement our analyses. The rationale for focusing on refugees is that they constitute one of the primary targets of the French integration contract, and in fact, they represent the largest group of immigrants assigned to the language training component of the contract.

We use the 2006, 2011, 2016, and 2019 Census main databases. These are 5-year rolling Censuses, meaning that the 2006 Census, for example, includes people interviewed between 2004 and 2008. Combining these data sets together yields a sample of 1,499,445 immigrants born outside the EU interviewed between 2004 and 2020 who (a) were either the reference person in their household or the spouse of the reference person, (b) arrived in France between 1997 and 2011, (c) were between the ages of 18 and 60, and (d) were living in Metropolitan France at the time of the survey. For robustness tests, we also use data on the 713,916 immigrants who meet the same criteria as above but were born in the EU to conduct placebo tests (immigrants born in the EU are excluded from the policy).

We complement our analyses with 2003 to 2020 Labor Force Surveys data. Since 2003, the Labor Force Survey has been a rolling survey taking place all year long. Sampled households are interviewed during six consecutive quarters, but we restrict our sample to the first interview for each immigrant. This yields a sample of 26,787 immigrants living in Metropolitan France who met the same criteria as above.

We also conducted an original survey of refugees who received refugee status in France. To construct a representative sample, we partnered with the French asylum office that provided us in September 2017 with a random sample of 500,000 asylum seekers who applied for refugee status in France between 1989 and 2015, about half of all asylum applications. Of these 500,000 asylum seekers, 98,372 were eligible for the survey (those who were granted refugee status, living in the 48 most populous departments [out of 96] in Metropolitan France, and from the 43 largest nationalities represented [out of 139], who were between 20 and 65 years old in 2017). Of these eligible refugees, we

⁸ <https://ec.europa.eu/migrant-integration/news/sweden-compulsory-introduction-course-all-asylum-seekers-en>.

⁹ We were granted access to the unrestricted version of these data via the "Comité du Secret Statistique" under project "CAIEVAL" (scss-3571-1). Scholars

interested in accessing these data sets to replicate our results must follow the same procedure.

sampled 18,000 refugees for our survey. We conducted a pilot from January to April 2018 in two departments (Essone and Val-de-Marne) and a national survey from August 2018 to April 2019.¹⁰ In total, we sent out 18,001 letters. To maximize our response rate, we partnered with the French postal services to hand deliver the survey instruments (two attempts) and to schedule a pick-up visit (two additional attempts). Of these letters, 11,737 addresses were still valid (sanctioned by a return letter from the French post office) and collected 1,720 responses (both paper survey and online) corresponding to a response rate of 9.6% of sampled refugees, and 14.7% of valid addresses. After restricting the sample to refugees who received their status between 1997 and 2011 and between the ages of 18 and 60, we have responses from 955 refugees in the sample.

Summary statistics for each of our main samples are displayed in Supporting Information (SI) Table B.1. The samples from the Census and the Labor Force Survey are similar as we would expect from nationally representative surveys. In both samples, immigrants born in the Maghreb form the largest immigrant group as they amount to 40% of each sample. As expected, because refugees are different from other channels of regular migration, the top three refugee-sending countries are Sri Lanka (13%), Democratic Republic of the Congo (DRC; 9%), and Russia (7%). Refugees are also less likely to be female (less than 40% in the French Refugee Survey compared to roughly 50% in the Census and the Labor Force Survey). By design, there are substantial differences in the number of years spent in France at the time of the survey between the census and the Labor Force Survey on the one hand (roughly 10 years) and the French Refugee Survey (almost 14 years on average). Refugees in our sample are also a bit older at the time of the survey (46 years old on average) than immigrants from the Census and Labor Force Survey (38 years old on average), even though they arrived in France at roughly similar ages (30 years old).

To capture immigrant integration on the economic, social, and psychological dimensions, we construct three main outcomes from all three surveys: whether the immigrant is employed, has a French-born partner, and is a French citizen. Overall, between 54% and 60% of immigrants and refugees in our samples are

employed (SI Table B.2). One year after arrival, short of 30% of immigrants are employed. But this proportion increases with years of residence: Within 2 to 5 years of arrival, about 44% of immigrants are employed and 60% of them are within 6 to 10 years of arrival (SI Table B.3). Mixed partnerships are relatively rare among the refugee population (only 2% overall, even though 62% are married), while much more frequent among the general immigrant population (20% to 30%). Refugees are just as likely to be naturalized than the larger immigrant population: roughly 30% in the full samples, 20% of those interviewed between 6 and 10 years after arrival, and 40% among those interviewed more than 10 years after arrival (SI Table B.3).

We enrich our measures with additional outcomes from the refugee survey (IPL-12 Integration Index; Harder et al., 2018) on all dimensions: A measure of equivalized income completes our economic outcomes. We also add two questions to capture better native-immigrant interactions: *"In the last 12 months, how often did you eat dinner with French people who are not part of your family?"* and *"Please think about the French people in your address book or your phone contacts. With how many of them did you have a conversation either by phone, messenger chat, or text exchange in the last 4 weeks?"* Our additional outcomes also include questions about psychological integration: *"How often do you feel like an outsider in France?"* and *"How connected do you feel with France?"*

RESEARCH DESIGN

We estimate the effect of the policy on economic, social, and psychological integration using a regression discontinuity design with multiple cutoffs. Two features of the policy implementation motivate this research design. First, assignment to the integration contract policy, that is, our treatment, is determined by the year in which immigrants obtained their first residency permit: Those who got their first residency permit in a department after the integration contract was introduced are assigned to receive the treatment, while those who got it before were not assigned. This assignment rule creates a discontinuity in the probability of being assigned to receive the treatment at the cutoff (the year of the introduction of the contract in the department that granted their first residency permit). Second, the staggered introduction of the integration contracts in France between 2003 and 2006 (represented in Figure 1) generates four different cutoffs: The policy was introduced in 12 departments in 2003, in 14 departments in 2004, in 35 departments in 2005, and in 35 departments in 2006. It was also introduced in 2008 for the overseas departments, but these are excluded from the analysis.

¹⁰ This research was conducted in full adherence to the Principles and Guidance for Human Subjects, Research (approved by the APSA Council, April 4, 2020). This research was reviewed and approved by the Stanford University Human Subjects Committee under IRB protocol 40172 on August 21, 2018, and the ETH Zurich Human Subjects Committee under IRB protocol EK 2018-N-107 on June 6, 2019. To protect the anonymity of participants, all survey instruments were sent via postal mail to participants directly by the French asylum office, such that the research team had access to neither the names nor the addresses of participants. We only recruited adult participants and collected written consent. They were informed that participation was voluntary.

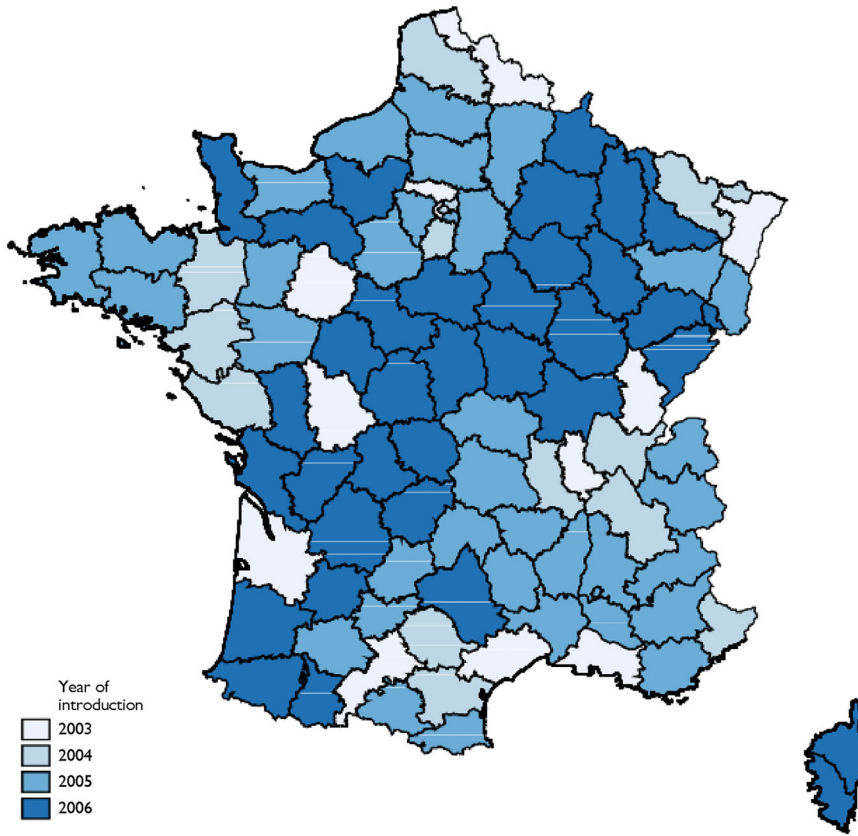


FIGURE 1 Staggered introduction of the Welcome and Integration Contract. *Note:* This map shows the year of introduction of the *Contrat d'accueil et d'intégration* (CAI) in France's departments.

We estimate the effect of the policy on outcome Y_i (described below) using the following specification:

$$Y_i = \tau D_i + \beta_0 \tilde{X}_i + \beta_1 (\tilde{X}_i \cdot D_i) + Z_i + \delta_d + \lambda_t + \epsilon_i, \quad (1)$$

with \tilde{X} being the running variable (year in which the immigrant received his or her first residency permit minus the year of introduction in the department that granted this residency permit) and D_i the encouragement variable equal to 1 if the immigrant arrived after the cutoff and 0 otherwise. We also include a vector of individual covariates Z_i (age at arrival, gender, and country of origin), department-fixed effects δ_d , because we do not know why some departments were chosen to receive the policy early on, and year of survey fixed effects (λ_t). We cluster standard errors by the interaction of department and year of arrival. We weight observations using survey weights provided by the survey producers for the Census and the Labor Force Survey, and entropy balancing weights for the French Refugee Survey.¹¹ To be sure, those who arrived earlier could sign the contract, generating treatment noncompliance; our estimand is consequently the local intention to treat effect at the cutoff averaged at

the four different cutoffs (corresponding to the four different years in which the contract was introduced).

We asked respondents to the French Refugee Survey about the department that granted their first residency permit and the year in which it was granted, but we do not observe either of these variables in the Census or Labor Force Survey. Instead, we proxy for department and year of first residency permit using the department of residence at the time of the survey and the year of arrival, respectively. This strategy presents some limitations.

First, we cannot directly identify immigrants who are not eligible to sign. Moreover, there are several instances where immigrants get a residency permit that makes them eligible for the integration contract several years after their arrival in France. Immigrants on a student visa, for instance, are excluded from the contract because only immigrants *with the intent of staying in the long term* in France are eligible to sign the contract. But they do become eligible later on if they stay in France after finishing their studies. This implies that we are coding as encouraged some individuals who are in fact, not, which might possibly bias our estimates toward zero. In 2015, students represented roughly 30% of all residency permits granted (Herbet, 2020). While nontrivial, this issue can relatively easily be dealt with by excluding, in a robustness test, immigrants who arrived in France at an age at which they could possibly have been a student (i.e.,

¹¹ We construct weights using entropy balancing (Hainmueller, 2012) to match our sample of respondents to the population of eligible refugees based on country of origin, age, department of residence, and the number of years spent in France.

keeping only immigrants who arrived in France above the age of 27).

Second, using the department of residence at the time of the survey presents the caveat that some immigrants may have moved since they got their first residency permit. For these, we would be misattributing encouragement status by using the department of residency at the time of the survey. To get around this issue, we exploit in our robustness checks the fact that in the Census, we know their department of residence 5 years prior. Overall, 5% of immigrants in our sample from the census moved department in the last 5 years. We can check the robustness of our results among immigrants who did not move departments in the last 5 years.

A third limitation arises from the fact that we use repeated cross-sectional data to estimate the effect of the policy. Some immigrants in France might leave the country such that the longer they are surveyed after arrival, the greater the possibility that attrition biases our estimates. To investigate the extent of this issue, we conduct balance tests at different points after arrival to see whether characteristics of control and encouraged immigrants change as the number of years spent in France increases, and we do not find this to be the case (SI Tables B.4, B.5, B.6, B.7, and B.8).

The main identification assumption of our research design is that the probability of being assigned to receive the treatment is discontinuous at the cutoff. We can test this assumption using our data from the French Refugee Survey because we asked respondents whether they signed the French integration contract. In this sample, we use the year in which refugees obtained refugee status (administrative data) as a proxy for the year in which they got their first residency permit and the department in which they signed their first residency permit (self-reported data) as a proxy for the department of arrival.¹² In Figure 2 (left panel), we plot the proportion of refugees who reported signing the contract as a function of the distance to the cutoff (difference between the year of arrival and the year of introduction in the department of arrival). This analysis confirms that there is a strong discontinuity at the cutoff. Using a linear but different slope model controlling for the department that delivered their first residency permit, we estimate that refugees who arrived just after the introduction are about 43 percentage points more likely to have signed the contract than those who arrived just before (Table 1, column 1).

To estimate the compliance with the contract among the population of immigrants, we combine data from OFII and the Census. On the right panel of Figure 2, we plot the proportion of immigrants who signed the contract as a function of the distance to

TABLE 1 Proportion of refugees and immigrants in samples who signed the contract as a function of whether they were “encouraged” to sign the contract (Compliance).

| | (1) Refugee Survey | (2) Census |
|--------------|-----------------------|------------------|
| Encouraged | .429** (.074) | .235** (.025) |
| Constant | .786* (.359) | .271** (.030) |
| Observations | 766 | 1,213 |

Note: The dependent variable is an indicator variable that equals one if the refugee reported signing the contract in column 1 and the proportion of non-EU immigrants who signed the contract in column 2. Robust standard errors are in parenthesis.

* $p < .05$, ** $p < .01$.

the cutoff. We estimate the proportion by department and weigh these estimates using the distribution of the immigrant population by department. We also report in Table 1 (column 2) our estimate of the size of the discontinuity using a linear but different slope model controlling for the department of arrival (we provide more details regarding these estimations in Appendix A of the SI).

Estimated that way, the discontinuity at the cutoff is somewhat smaller, about 24 percentage points (Table 1). One possible explanation for the fact that the compliance ratio at the cutoff is larger among refugees than immigrants is that the incentive to sign the contract for those who arrived before is greater for immigrants than for refugees: Immigrants who arrived before the introduction of the contract still had to sign the contract when they later applied for the 10-year residence card. But asylum seekers get the 10-year card directly when they are granted refugee status, which limits the incentive to sign the contract for those who arrived before.

Our research design rests on two additional assumptions. The first is that immigrants are not sorting around the threshold. Sorting around the threshold in this setting would mean that immigrants choose to arrive in different departments in order to benefit from or avoid the policy. Yet, in practice, two things make this behavior implausible. First, immigrants only have limited control over the timing of their first residency permit. We show in SI Table B.9 that, in the departments that introduced the CAI in 2006, the number of residency permits granted did not increase shortly after compared to shortly before the month in which the policy was introduced. Second, it is reasonable to assume that immigrants to France had imprecise knowledge over the introduction of the contract before its implementation in any department. The second additional assumption is that nothing else is changing at the cutoff. Two features of our design help us rule this out. First, the fact that we are averaging four

¹² We imputed the department of residence in 2017 (administrative data) when this information was missing in the survey (15% of respondents).

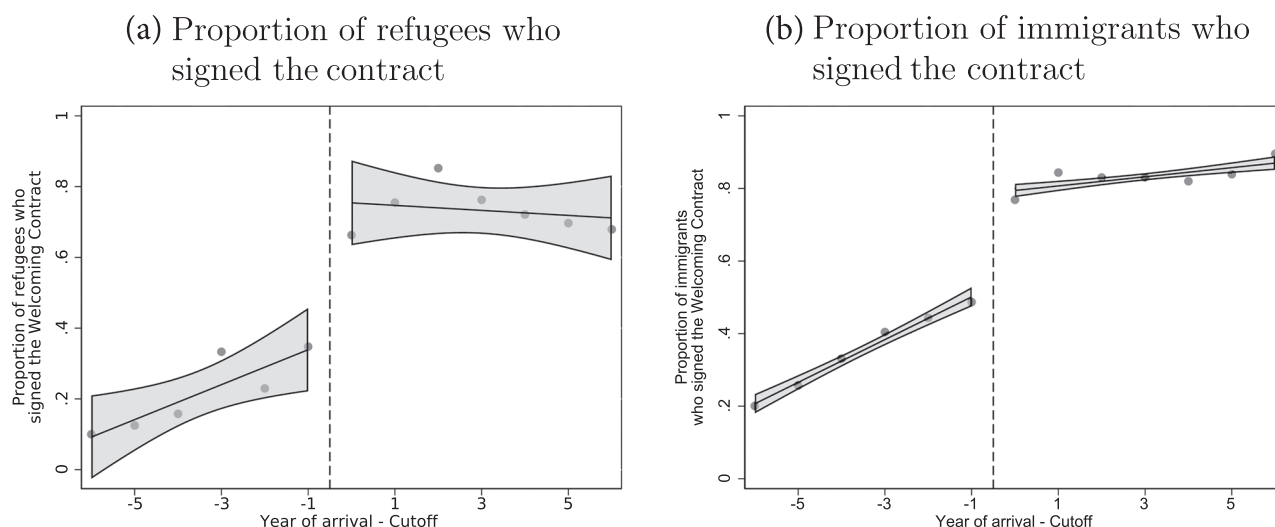


FIGURE 2 Compliance analysis: Proportion of refugees and immigrants who signed the contract as a function of the distance to the cutoff. *Note:* This figure displays the annual proportion of refugees (Panel a) and immigrants (Panel b) who signed the contract as a function of the distance to the cutoff (year of arrival minus year of introduction of the policy in the department of arrival). On the left panel, the proportion of refugees who signed the contract as a function of the distance to the cutoff is estimated using the French Refugee Survey. On the right panel, the proportion of immigrants who signed the contract is estimated using data from *Office Français de l'Immigration et de l'Intégration* (OFII) on the number of immigrants who signed the contract as a function of (a) department of arrival, (b) year of arrival, and (c) year of signature) and data from the 2011 Census.

local average treatment effects over four different cut-offs mitigates the concern that effects could be due to something else changing in France at the same time. Second, we can use the sample of Europeans who were not affected by the policy to conduct placebo tests (SI Table B.11).

RESULTS

Overall effect

We start by estimating the overall effect of the French integration contract on our main integration outcomes. On average, immigrants in the Census and the Labor Force Survey samples are interviewed 10 years after arrival, and refugees in the French Refugee Survey sample roughly 14 years after arrival (SI Table B.1). We first display our results graphically for these three main outcomes by plotting the smoothed values and the 95% confidence bands of a kernel-weighted local polynomial regression using the Epanechnikov kernel on each side of the discontinuity (Figure 3). Visually, we fail to detect a discontinuity in any of our three main outcomes, suggesting the absence of meaningful effects on immigrant integration overall.

We also report estimates of effect sizes for our main outcomes using a 5-year bandwidth in Table 2. These analyses confirm that the contract had no discernible effect on the integration dimensions we consider. Immigrants encouraged to sign the contract are .7 pp (s.e. = .2 pp) less likely to be employed during the survey in the Census and .2 pp less (s.e. = 1.4 pp) in

the Labor Force Survey. The estimate from the French Refugee Survey sample is also negative yet larger and less precisely estimated due to the smaller sample size: -12.6 percentage points (s.e. = 8.9 pp).

Our estimates of the contract on the probability of living with a French-born partner are all positive but, as before, negligible in size. They range from .5 pp (s.e.: .2 pp) in the Census to 3.5 pp (s.e.: 1.2 pp) in the Labor Force Survey. Estimates of the impact of the policy on our different outcomes capturing social integration are also positive though not statistically significant: 7.1 pp in the probability of having dinner with French people at least once a week (s.e.: 10.4 pp) and 16.5 pp in the likelihood of having at least three French people in their phone contacts (s.e.: 9.9 pp; SI Table B.10). Regarding citizenship acquisition, we similarly fail to detect any discernible effect of the contract on the probability of being French at the time of the survey. Our estimates are small and positive in the Census and the Labor Force Survey sample. They are negative in the French Refugee Survey sample. We similarly fail to detect a statistically significant effect on additional outcomes capturing psychological integration, though the estimates point toward increased attachment to France (SI Table B.10).

We conduct a series of robustness tests. First, to ensure that these null results are robust to alternative specifications, we first show that results are robust to using smaller bandwidths (SI Table B.11, columns 2 to 4), excluding immigrants who arrived the year of introduction in the relevant department (column 5), and to removing demographic controls (column 6). We also report estimates from our placebo group (immigrants

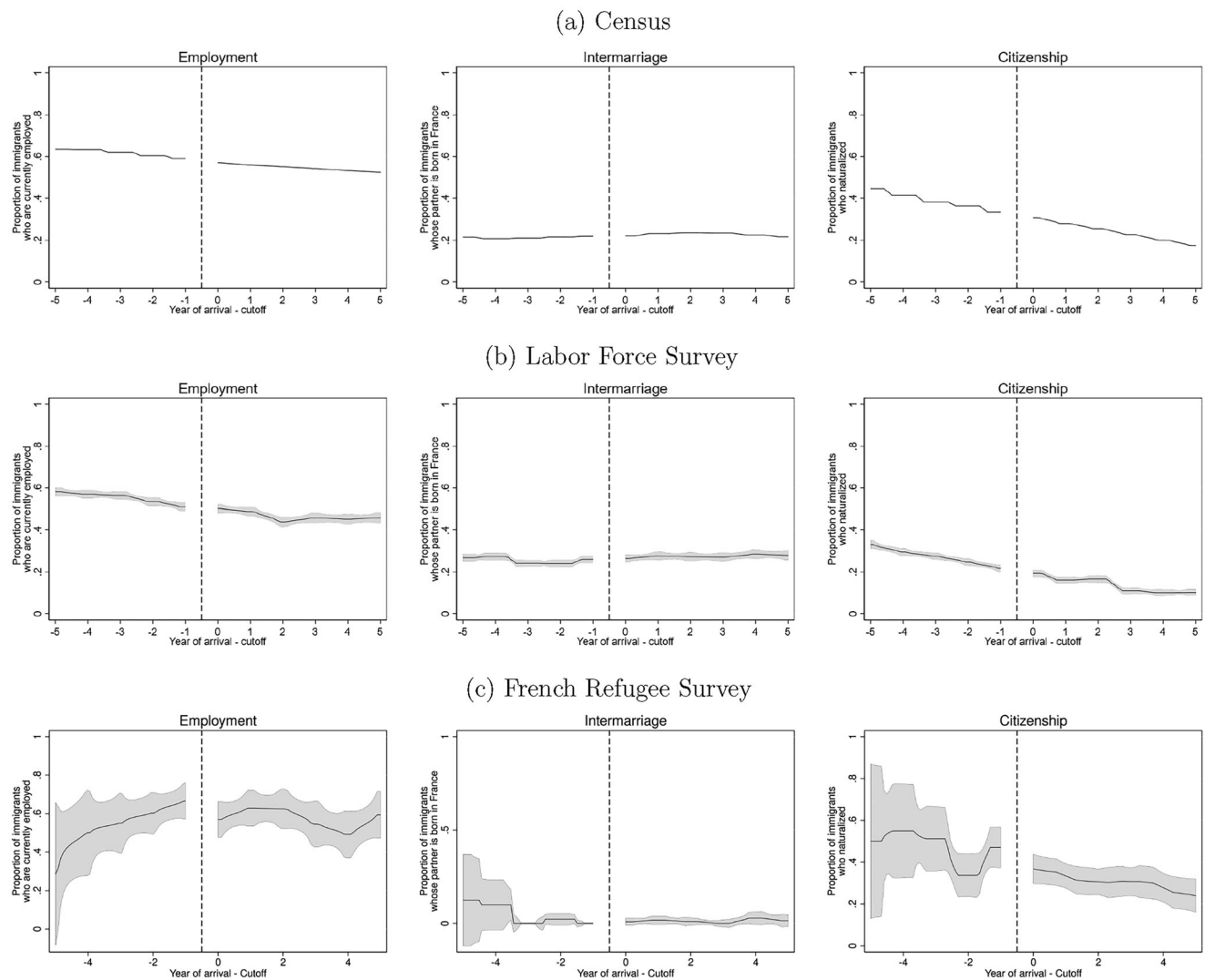


FIGURE 3 Overall effect of the French integration contract on economic, social, and psychological integration (Main outcomes). *Note:* This figure displays the results on the main outcomes from the Census data (Panel a), Labor Force Survey (Panel b), and the French Refugee Survey (Panel c). Each figure plots the smoothed values and the 95% confidence bands of a kernel-weighted local polynomial regression using the Epanechnikov kernel on each side of the discontinuity.

TABLE 2 Effect of the contract on economic, social, and psychological integration.

| | Census | | | Labor Force Survey | | | French Refugee Survey | | |
|----------------------|---------------------|------------------------|--------------------|--------------------|------------------------|--------------------|-----------------------|------------------------|-------------------|
| | Currently employed | Partner born in France | Is French | Currently employed | Partner born in France | Is French | Currently employed | Partner born in France | Is French |
| Encouraged | −0.007** (0.002) | 0.005* (0.002) | 0.002 (0.004) | −0.002 (0.014) | 0.036** (0.012) | 0.008 (0.013) | −0.126 (0.089) | 0.025 (0.016) | −0.052 (0.100) |
| Constant | 0.585** (0.002) | 0.196** (0.002) | 0.300** (0.003) | 0.524** (0.012) | 0.237** (0.009) | 0.183** (0.010) | 1.191** (0.375) | 0.261* (0.111) | 0.395 (0.327) |
| Mean of outcome | 0.585 | 0.219 | 0.322 | 0.514 | 0.264 | 0.212 | 0.589 | 0.015 | 0.356 |
| Number of encouraged | 551,234 | 551,234 | 551,234 | 9,980 | 9,980 | 9,980 | 495 | 531 | 508 |
| Observations | 1,200,176 | 1,200,176 | 1,200,176 | 21,479 | 21,479 | 21,479 | 739 | 791 | 762 |

Note: This table reports estimates from Equation (1).

* $p < .05$, ** $p < .01$.

from the European Union) in column 7. Second, to rule out that our estimates are null because we cannot exclude those who arrive as students in our samples, we show that results hold in the sample of immigrants who arrived in France above the age of 27 (SI Table B.11, column 8). Third, to address the concern that immigrants might move within France after they arrive, we show that results are similar when restricting our sample to individuals who did not move to another department in the last 5 years (SI Table B.11, column 9). Fourth, the nonrandom assignment of departments into early versus late adopters raises the concern that the policy may have been first introduced in places where it would be most effective. We do not find a statistically significant difference between early versus late adopters (SI Table B.12). Fifth, a concern specific to the French Refugee Survey is that refugees' decision to respond to our survey may be itself impacted by the policy, but we do not find this to be the case—encouraged refugees were not more likely to answer our survey (SI Table B.13).

Finally, we investigate whether spillovers between treated and control immigrants might attenuate our estimates. If this is the case, we would expect spillovers to be larger in more homogeneous places with respect to the country of origin. To test this, we estimate the ethnolinguistic fractionalization (ELF) in an immigrant's municipality of residence using the proportion of immigrants who were born in different countries. We then split immigrants in our sample into five equal-size groups with respect to the estimate of ELF in their municipality of residence. In SI Table B.14, we report the estimates of the interactions between the ELF quintiles and the encouraged variable. We find neither substantive nor significant differences between the estimate of the effect of the policy for immigrants in the first quintile (−.9 pp) and in other quintiles.

Overall, the French integration contracts did not strongly impact any of our main integration outcomes in the long run. Roughly 10 years after arrival, we do not find any meaningful difference in our integration outcomes between those encouraged to sign the contract and those not.

Short-term and long-term effects

We next consider the possibility that these overall null or very small effects are due to the fact that the policy only had short-term effects that dissipated over time. We investigate short- and long-term effects in SI Table B.15, dividing respondents to the Census and Labor Force Survey into four groups depending on whether they were surveyed (a) 1 year after arrival, (b) between 2 and 5 years after arrival, (c) between 6 and 10 years after arrival, or (d) more than 10 years after

arrival. This analysis in the Census sample presents one caveat: When restricting the sample to those interviewed 1 year after arrival, we only effectively use data from immigrants from departments that introduced the contract in 2004 or after. This is because the annual Census first took place in 2004, such that we do not observe immigrants who were not encouraged in departments that introduced the policy in 2003. The annual labor force survey started in 2003, so we do not face the same issue in that sample.

These subgroup analyses reveal that this policy substantially affected the probability of being employed in the short term. We find that immigrants encouraged to sign the contract were 5.5 pp (s.e.: 1.7 pp) more likely to be employed 1 year after arrival than immigrants who were not encouraged (Panel A of SI Table B.15). The estimate is larger (10.3 pp) but also noisier in the Labor Force Survey sample, as is to be expected from the small sample size for this subgroup (s.e.: 7.6 pp, $N = 911$). This represents a substantial increase compared to the average proportion of employed immigrants 1 year after arrival: 27% (SI Table B.3). Our robustness tests indicate that this effect is robust (SI Table B.16). Reassuringly as well, we find no evidence of very short-term effects on any other of our main outcomes: Partnerships take time to form, and immigrants only become eligible for naturalization after 10 years in France.

However, this positive employment effect quickly dissipates over time. The difference in the probability of being employed between immigrants who were encouraged and those who were not is down to 3 pp 2 years after arrival and very close to zero when considering immigrants who spent 3 or more years in France (Figure 4). Moreover, even among immigrants interviewed more than 5 or even 10 years after arrival (Panels C and D of SI Table B.15), we fail to detect any effects on our two other main outcomes. We note a positive and statistically significant effect on naturalization and mixed partnerships within 5 to 10 years of arrival in the Labor Force Survey, but these results are not corroborated by the estimations based on the Census data.

These additional analyses suggest that the contract had a strong positive effect on the probability that immigrants are employed 1 year after arrival. However, this effect did not translate into sustained employment gain, nor did it facilitate the integration of immigrants on other dimensions in the long run.

Backlash effect

We next test the hypothesis that complying with the French integration contract triggered a backlash among some immigrant groups (Strik, 2013). While a concern commonly raised by critics of civic integra-

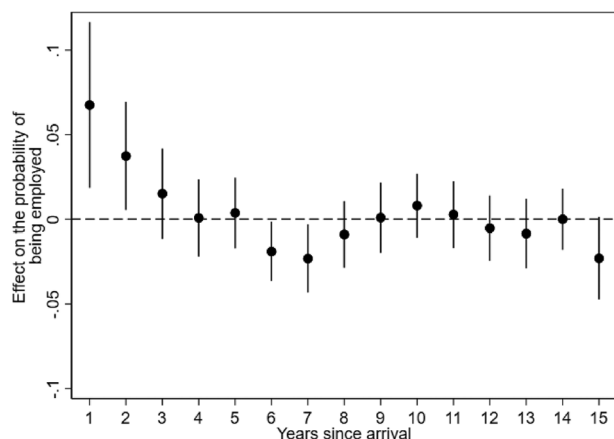


FIGURE 4 Effect on the probability of being currently employed by years since arrival (Census). *Note:* This figure displays the estimates and the 95% confidence intervals of the effect of the contract on the probability of being employed. We recover these estimates from an interaction model where we interact the treatment indicator and the running variables with the years since arrival. Department and demographic controls fixed effects are included but not shown. Standard errors are clustered two-way by the department and the year of arrival.

tion policies, this hypothesis has not been tested to date as existing studies have not gone beyond looking at the overall effect of these policies due to a lack of data availability. In the European Social Survey (ESS), for instance, only the (world) region of origin is available (Goodman & Wright, 2015; Neureiter, 2019) such that authors are unable to look at effect heterogeneity by country of origin.

We test this hypothesis by looking at the effect of the policy on Muslim immigrants specifically because some scholars argue that policies such as the French integration contract are implicitly targeted at Muslims (Alaoui & Pélabay, 2020; Joppke, 2012; Tiberj, 2014). To perform this analysis, we use the proportion of the population in the country of birth that identifies with Islam. The data come from the Association of Religion Data Archive's World Religion data set. The distribution of the proportion of the population who identified with Islam in the Census sample is displayed in SI Figure B.1. To assess the extent to which immigrants from predominantly Muslim-majority countries are impacted differently than immigrants from other countries, we estimate the following full interaction regression model where we interact the set of demographic controls (for the purpose of this analysis, we keep only the top 10 countries of origin and group all other countries into an "Other" category) with the running variable, the encouraged variable, and their interaction (Bansak, 2021):

$$Y_i = \tau D_i + \beta_0 \tilde{X}_i + \beta_1 (\tilde{X}_i \cdot D_i) + Z_i + (\tilde{X}_i \cdot D_i) Z_i + D_i Z_i$$

$$+ \tilde{X}_i D_i + \delta_d + \lambda_t + \epsilon_i. \quad (2)$$

In contrast to concerns about a Muslim backlash effect, we fail to detect any substantive or statistically significant differences between immigrants from Muslim-majority countries where more than 94% of the population identifies with Islam (Senegal, Algeria, Turkey, Tunisia, and Morocco) and our comparison group comprising countries not the 10 largest countries (Table 3), and the pattern is very similar 1 year after arrival (SI Table B.17).

MECHANISMS

What explains that the contract substantially increased the probability of being employed shortly after arrival but not in the medium and long run? In this section, we consider in turn two questions: What explains this short-term increase? Why did it not last?

Components of the contract

Which component of the French integration contracts most likely helped immigrants find employment shortly after arrival? Recall that the contract is a bundled policy that includes, in addition to mandatory civic training, optional language training, and a "Living in France" information session. Both components may facilitate immigrants' entry into the labor market and increase their probability of finding a job. To disentangle which component was more influential, we use survey data first to estimate the probability that immigrants from a given country or region of origin took part in the different components. In 2010, the statistical division of the Ministry of Interior surveyed newly arrived immigrants above the age of 18 who had signed the contract in 2009 (Elipa 1).¹³ The proportion of immigrants from different countries or regions who participated in the different pieces of training (conditional on having signed the contract) is displayed in Table 4. On average, only 13% of immigrants surveyed had started the language training a year after arrival, but this proportion ranges from 2% for immigrants from Madagascar and Cameroon to 44% and 41% for immigrants from Turkey and Sri Lanka, respectively. Overall, 29% had completed the living in France training by the time they were surveyed, and this proportion goes from 11% for immigrants from Congo and Mali to 45% and 41% for immigrants from Madagascar and Russia.

Next, we exploit this variation in exposure by country or region of origin to examine whether the effect

¹³ Elipa 1 is a panel survey with three waves conducted in 2010, 2011, and 2013. We only use data from the first wave.

TABLE 3 Heterogeneity by country of origin.

| | Currently employed (Census) (1) | Naturalized (Census) (2) | Partner born in France (Census) (3) |
|--------------------------------|---------------------------------------|--------------------------------|---|
| Encouraged | .002 (.016) | -.000 (.015) | -.008 (.013) |
| Encouraged × China (.03) | .024 (.028) | -.003 (.014) | -.018 (.025) |
| Encouraged × Madagascar (.05) | .037 (.019) | .034 (.023) | .025 (.023) |
| Encouraged × Russia (.12) | .029 (.020) | -.007 (.018) | .024 (.024) |
| Encouraged × Cameroon (.21) | .009 (.018) | -.003 (.016) | .001 (.018) |
| Encouraged × Ivory Coast (.38) | .011 (.015) | .007 (.017) | .006 (.017) |
| Encouraged × Senegal (.94) | .001 (.018) | .037 (.019) | .008 (.016) |
| Encouraged × Algeria (.99) | -.021 (.013) | .011 (.015) | .024 (.016) |
| Encouraged × Turkey (.99) | -.018 (.013) | -.004 (.016) | -.010 (.018) |
| Encouraged × Tunisia (.99) | -.006 (.014) | -.004 (.014) | -.000 (.021) |
| Encouraged × Morocco (.99) | -.009 (.011) | .003 (.012) | -.011 (.014) |
| Mean of outcome | .585 | .322 | .219 |
| Number of encouraged | 551,234 | 551,234 | 551,234 |
| Observations | 1,200,186 | 1,200,186 | 1,200,186 |

Note: This table reports estimates from Equation (2).

* $p < .05$, ** $p < .01$.

of the contract on employment is moderated by likely exposure to the language courses and the “living in France” training. We divide respondents into four groups depending on whether they have a low or high probability of exposure to both pieces of training (using the median of both distributions as the cut-offs to define the groupings). The results are shown in Figure 5.

Interestingly, we find that the contract’s effect was generally larger among immigrants with a very low likelihood (2% to 6%) of having taken the language training. Within this group of immigrants who likely did not participate in the language training, the effect was 11.8 pp (s.e.: 7.9 pp) and 17.8 pp (s.e.: 9.2 pp) depending on whether they were likely or unlikely to be exposed to the “living in France” training. In contrast, among the group of immigrants who likely participated in the language training component, the

effect of the contract on employment was only –3.0 pp (s.e.: 6.6 pp) and 5.9 pp (s.e.: 7.0 pp), respectively, among those who were least or most likely to be exposed to the “living in France” training. In addition, we find that among those immigrants who were less likely to be exposed to the “living in France” training, the difference in the effects among those who were less likely to be exposed to the language training and among those who were more likely to be exposed to it is statistically significant (14.7 pp, s.e. 7.5 pp).

Overall, these findings suggest that the language training component was not the driver behind the positive short-term effect of employment. Instead, the results suggest that the contract was most effective at boosting the employment of immigrants for whom the language was not a barrier to start with. To further examine this, we replicated the analyses to examine heterogeneity by quintiles of the Average Distance to

TABLE 4 Proportion of respondents who participated in the three different pieces of training by country or region.

| Country or region | Observations | Civic training | Language training | Living in France training |
|----------------------------|--------------|----------------|-------------------|---------------------------|
| Europe (including France) | 102 | .931 | .108 | .245 |
| Russia | 145 | .910 | .317 | .414 |
| CIS | 184 | .913 | .207 | .277 |
| Turkey | 357 | .916 | .440 | .361 |
| China | 270 | .919 | .267 | .178 |
| Sri Lanka | 189 | .921 | .413 | .328 |
| Asia (other) | 324 | .929 | .290 | .309 |
| Algeria | 1,165 | .967 | .066 | .324 |
| Morocco | 480 | .940 | .073 | .348 |
| Tunisia | 344 | .956 | .035 | .360 |
| Cameroon | 195 | .959 | .021 | .297 |
| Congo | 100 | .950 | .030 | .110 |
| Cote Ivoire | 250 | .960 | .040 | .244 |
| Guinea | 102 | .961 | .049 | .196 |
| Madagascar | 105 | .952 | .019 | .448 |
| Mali | 428 | .974 | .075 | .107 |
| Senegal | 198 | .934 | .045 | .247 |
| Sub-Saharan Africa (other) | 314 | .959 | .064 | .204 |
| DRC | 225 | .969 | .027 | .182 |
| Africa (other) | 245 | .971 | .171 | .253 |
| Haiti | 107 | .991 | .028 | .243 |
| America (other) | 278 | .928 | .076 | .313 |

Note: The table reports the proportion of respondents from each country or region who said that they (a) completed the civic training, (b) started the language training, and (c) completed the living in France training.

Source: Elipa 1 (Wave 1).

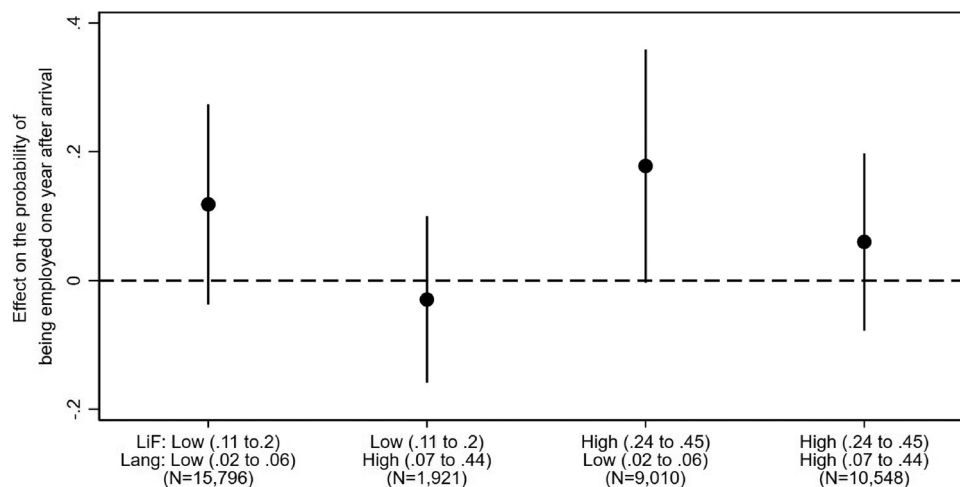


FIGURE 5 Effect on the probability of being employed 1 year after arrival by exposure to optional trainings. *Note:* This figure displays the effect of the policy in four different subgroups. In the figure, “LiF” stands for “Living in France” training, and “Lang” stands for language training. We recover these estimates and their 95% confidence intervals from a full interaction model in Equation (2), excluding country of origin fixed effects.

French (ADF; Fearon, 2003; Laitin & Ramachandran, 2016) in SI Figure B.2 and these findings also support this interpretation. In the lowest quintile (ADF below .48), where immigrants are most likely to be already proficient in French, the employment effect is close to 26 pp (s.e.: 14 pp) 1 year after arrival. These findings align with previous studies showing that language training did not increase the probability of finding employment (Lochmann et al., 2019; Pont-Grau et al., 2023). Beyond this, however, the data do not allow us to adjudicate between possible mechanisms: The contract might help those immigrants with facility in French overcome initial administrative barriers slowing down access to the labor market, provide practical help with job search, raise immigrants' self-confidence, or even change employers' perception of immigrants.

Short-term effects

Why are the effects of the contract so short-lived? One possible explanation is that the increase we see in the first year is driven by the fact that newly arrived immigrants who participated in the training are more likely to take on short-term contracts that are not renewed. The data do not support this interpretation. Indeed, in SI Table B.18, we find that the increase in employment in the first year is not driven exclusively by temporary contracts. Instead, we find a large effect (almost 3 pp) on permanent contracts that disappears 2 to 5 years after arrival. This suggests that the most likely explanation is that the control group catches up with the treatment group. If this is the case, the "effect" of the policy was to accelerate onboarding by a couple of months.

CONCLUSION

As a response to increases in the number of immigrants and refugees coming from outside Europe, the issue of civic integration of these new populations into the languages, cultures, and values of their host countries became an explicit policy goal. Many European countries that experienced these new immigration waves now require immigrants to sign a contract to attend civic training and language classes.

Until now, evidence is inconclusive about the return of these contracts on successful economic, social, and psychological integration. To address this gap, in this paper, we study the overall effect of the French integration contract on immigrant integration. We leverage unique features of the policy implementation to estimate the impact of the policy using a regression discontinuity design with multiple cutoffs. To capture the multifaceted impact of the French integration

contract, we combine the richness of a survey we conducted among refugees specifically for this purpose with the high statistical power permitted by extensive government surveys.

This study yields three main findings. First, we uncover substantial effects on employment in the very short term. We estimate that the French integration contract increased by 5.5 pp the probability of being employed 1 year after arrival, a substantial increase compared to a 27% baseline. This suggests that the French integration contract successfully alleviated barriers to entry into the labor market for some immigrants by an order of magnitude relatively unheard of for an integration policy.

Second, this initial employment boost was short-lived, and the policy's overall effect was minimal. Three years after arrival, integration levels of encouraged immigrants are similar to that of immigrants who were not. One likely explanation is that the policy simply accelerated immigrants' onboarding into the labor market by a few months. Overall, we find that the policy had no discernible effect on any of our integration outcomes. Third, we reject the backlash hypothesis: We find no evidence that immigrants exposed to the policy reduced their assimilation effort as a result.

It is important to remember that we study the effect of the contract as it was initially designed. Yet, the French integration contract was reformed twice since. In 2016, the CAI was renamed *Contrat d'intégration républicaine* and the mandatory training was extended to 2 days subsuming "Living in France" and the civic training. Also, the target level for the language training was raised to the A1 level of the CEFR (Barrot & Dupont, 2020). Three years later, the mandatory civic training was extended to 4 days, and the number of hours of language training increased by a factor of two to three. Today, immigrants scoring the lowest on the initial assessment test are prescribed 600 h of language courses. Yet, existing studies (Pont-Grau et al., 2023) suggest that longer language training hours are unlikely to have translated into increased employment: They find that while longer training hours increased the probability of having a permanent job for those already employed, it had no effect on the probability of being employed.

Overall, this study makes two important contributions. First, we provide the first country-level overall evaluation of a policy now implemented in many European countries. If light touch, the French integration contract resembles what is in place in several other European countries such that the results of this study will be relevant in other settings. Our findings suggest that integration contracts can be helpful in that they can accelerate the "labor market onboarding" of new immigrants. Yet, the lack of any discernible medium- to long-term effect on any of the dimen-

sions of integration success raises the question of the cost-effectiveness of these policies. While offset by the possible tax gains for those immigrants who entered the job market earlier, integration contracts are not inexpensive. In France, the cost of all three components combined amounted to 33 million Euros for 2009 (25 million for the language courses only) covering an average of 100,000 signatories (OFII, 2009, Annual Report). More research is needed to assess the cost/benefit returns to these policies, especially the ones relying on more intense versions of this policy, like in Denmark or Germany, or different features of the policy, conditioning residency permits on tests rather than attendance.

Second, another contribution of our study is that we are able to test for possible backlash effects of integration contracts. These contracts have an assimilationist side that might enrage vulnerable populations fearing the loss of their homeland cultures. However, existing studies on the effect of civic integration policies have not gone beyond looking at the overall effect of the policy often due to a lack of data availability (Goodman & Wright, 2015; Neureiter, 2019). Our findings suggest that even if not enabling, integration contracts do not seem to create a backlash hindering or slowing integration.

Overall, our results call into question one of the core principles of the European Union's integration policy: Imparting basic knowledge of the values of host countries' societies may not be "essential" for enabling immigrant integration.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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