Behind the Veil of Origin: Revisiting the Impacts of the French Headscarf Ban in Schools*

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Abstract

This paper examines the impact of prohibiting the Islamic veil in schools on economic and social outcomes of Muslim women. Using a difference-in-differences design, I show that the 1994 directive instructing school principals to ban the veil led to a substantial decline in educational attainment among affected cohorts, with persistent consequences for employment and marriage market outcomes. An analysis of mechanisms suggests that these effects stem primarily from heightened perceptions of discrimination and mistrust toward the French school system, rather than shifts in parental educational investments. I also show that misclassification of religion in prior work introduces substantial bias. Despite the adverse economic consequences, affected cohorts exhibit lower religiosity and camparable levels of integration later in life, pointing to a gradual assimilation towards the majority culture.

Keywords: headscarf ban, religious identity, women's education, social integration, marriage market, misclassification bias
JEL Codes: I28, J16, J15, Z12

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1 Introduction

Since the mid-20th century, the share of Muslims in the European population has been steadily rising, and their religious practices have increasingly come into tension with prevailing Western norms. In particular, the Islamic veil is often perceived both as a symbol of women's subordination and as a challenge to state secularism. In response, roughly one-third of European countries have adopted policies restricting the wearing of Islamic clothing in public spaces (Abdelgadir and Fouka 2020). While such regulations are often justified as preserving the majority culture, their effects on the targeted populations are theoretically ambiguous. On the one hand, if women do not choose to veil willingly, bans may liberate them from constraining social norms. On the other hand, if veiling provides significant religious benefits, prohibitions may lead to more social exclusion and segregation (Carvalho 2013, Jacquet and Montpetit 2024).

To address this question, I study the case of France, the only developed country to prohibit the Islamic headscarf in public schools for both pupils and employees.[1] In 1994, Minister of Education François Bayrou issued a circular (henceforth, the Bayrou circular) instructing school principals to ban the veil. The policy was gradually adopted by schools over the following years and was ultimately codified into law in 2004.

I extend the current evidence on veil bans by analyzing the effects of these two policy shocks on both economic and social integration. A key advantage of my approach is the ability to directly observe individuals' religion using unique survey data, rather than relying on origin as a proxy. Building on prior studies that employ origin-based measures (Abdelgadir and Fouka 2020, Maurin and Navarrete H 2023), I implement a difference-in-differences design comparing cohorts of Muslim women who reached the age of veiling before and after the policy.

Accurately identifying the treatment group in this setting reveals several new insights into the effects of religious prohibitions in secular contexts. Consistent with Maurin and Navarrete H (2023), I find a positive effect of the Bayrou circular for individuals of African origin. Yet for the actual treatment group—Muslim women—the effect is of opposite sign. I document a large negative impact of the circular issuance on Muslim women's likelihood of completing high school. The point estimates suggest a decline in the high-school completion rate of about 25% of the pre-treatment mean and is robust to a range of sensitivity checks. While this negative effect fades after five cohorts, I show using detailed data on respondents' educational trajectories that the circular also reduced total years of schooling, a result that persists in the medium run.

The stark contrast between estimates based on religious affiliation and those relying on origin highlights substantial bias from treatment misclassification in earlier studies. I

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[1]To my knowledge, Kazakhstan (since 2023) and Turkey (1997–2010) are the only other countries to have imposed such a ban on both students and teachers.

show that a wrongly signed estimate is consistent with recent findings in econometrics of difference-in-differences with a misclassified treatment (Denteh and Kédagni 2022, Negi and Negi 2025). Moreover, I find no clear effect of the 2004 law, consistent with the prohibition having already been widely implemented following the Bayrou circular.

I next investigate the mechanisms through which the headscarf ban reduced schooling. Consistent with qualitative evidence in Abdelgadir and Fouka (2020), Muslim women exposed to the directive were 13 percentage points (63%) more likely to report religious discrimination. This effect is not explained by their different origins, since discrimination based on this trait does not increase significantly. The Bayrou circular is also associated with an increase in mistrust of the French school system among affected Muslims, suggesting that the disruption in girls' educational trajectories reflects a hostile school environment.

An alternative explanation is that the ban prompted strategic reactions from identity-concerned Muslim parents who perceived the policy as a threat to their religious traditions (e.g. Dahl et al. 2022). However, I find little evidence for such parental responses. First, the effects are similar across families which devoted more effort in transmitting their religion, indicating that the impact is not concentrated among the most pious. Second, I show that Muslim parents did not reduce their educational investments in their daughters. Affected girls were no less likely to receive homework help from family members, nor more likely to attend private schools where the ban did not apply. Third, heterogeneity analyses show that the decline in schooling is concentrated among girls from more traditional families in an *economic* sense, rather than those in families making greater efforts to transmit religion.

Finally, I examine the long-run effects of the ministerial circular on religiosity, social integration, and economic outcomes. The decline in schooling translates into a 4.2 percentage point reduction in employment (6.5% of the mean) later in life, with spillovers to the marriage market. In line with evidence on assortative mating (Choo and Siow 2006, Chiappori et al. 2017), affected Muslim women are more likely to be married to an unemployed partner. As expected, given their earlier school exit, they are also more likely to have children.

Turning to cultural and social integration, I find no evidence of a religious backlash. In contrast with results for Turkey (Sakalli 2019) and Indonesia (Bazzi et al. 2025), exposed Muslim women report *lower* levels of religiosity in adulthood. Similarly, I find at most small effects on veiling behavior. One possible explanation is that the heightened political and media emphasis on secularism as a core Republican value (Winter 2009, Scott 2009) led religious minorities to reduce the intensity of their religious life. Moreover, I find no deterioration in various measures of social integration, including feelings of French identity, speaking French at home, same-faith marriage, or voting behavior. Thus, while the prohibition hindered Muslim women's economic integration, it may have simultaneously

advanced cultural assimilation.

This study first contributes to the literature on assimilation and integration policies. I show that the French headscarf ban reduced the schooling outcomes of Muslim girls, with evidence pointing to increased discrimination as the main channel. These findings echo previous work on other forms of *assimilationist* regulation, which often backfire (e.g. Lleras-Muney and Shertzer 2015, Fouka 2020). In the context of veil bans, Corekcioglu (2021) and Lu and Yurdakul (2025) document positive effects of lifting prohibitions in the Turkish public sector on female employment. By contrast, *integration* policies—such as easier access to citizenship—are typically associated with stronger labor market attachment and greater social integration of immigrants (see Gathmann and Garbers (2023) and Fouka (2022; 2024) for recent reviews, as well as Kamel (2025) on Arab immigrants following the *Dow v. United States* ruling). A notable exception is Dahl et al. (2022), who show that the introduction of automatic birthright citizenship in Germany lowered life satisfaction, self-esteem, and social integration among Muslim girls. Consistent with the broader evidence on assimilationist policies, my results suggest that headscarf bans are unlikely to promote economic integration either.

Second, this paper contributes to the literature on the interplay between education and identity. I show that restricting the ability of minorities to express their identity in schools can generate unintended consequences. Rather than fostering Muslim girls' integration into secular society, the French headscarf ban reduced their educational attainment. This finding is consistent with economic theory suggesting that veil bans may hinder the economic integration of pious women (Carvalho 2013), as well as with recent models showing that marginalized cultural communities may underinvest in human capital when the secular content of mainstream education is strengthened (Carvalho et al. 2017; 2024, Bisin et al. 2023). Related empirical work demonstrates that changes in school curricula can shape identity and attitudes (Cantoni et al. 2017, Squicciarini 2020, Bazzi et al. 2025). Closer to my setting, Sakalli (2019) shows that the secularization of Turkish schools lowered educational attainment and increased religiosity, particularly in pious districts prior to the reform.[2] My study differs in that I examine a minority context, where it was not the content of education but rather the conditions of schooling that were altered.

Third, this paper connects to the growing literature on reproducibility and replicability in economics. A key finding in this literature is that many published results fail to replicate, raising questions about the reliability of causal claims in empirical research (Gertler et al. 2018, Huntington-Klein et al. 2021; 2025, Brodeur et al. 2024). Related evidence of p-hacking and the lack of transparency in economics research (e.g. Brodeur et al. 2016; 2020) has fostered new research norms that treat replication as a crucial

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[2]Similarly, Benzer (2022) finds that the subsequent reintroduction of Islamic schools in Turkey, where the headscarf is permitted, improved girls' educational attainment.

diagnostic tool (Miguel 2021). I contribute to this literature by reassessing the impacts of a reform with significant implications for the integration of religious minorities in Western countries. Specifically, I perform a *direct replication* of two prior studies on the French headscarf ban using an alternative data source that allows for correct classification of the treatment group, while following similar empirical procedures.[3] I show that misclassifying the targeted group can substantially alter conclusions about the policy's effects on economic integration. While my results align with Maurin and Navarrete H (2023) when using origin as a proxy for Muslim affiliation, the effects on the actual treatment group are of opposite sign.

The rest of the article is structured as follows. Section 2 describes the institutional context. Section 3 presents the data sources along with summary statistics. Section 4 evaluates the impact of the ban on Muslim girls' educational attainment, presents my analysis of the underlying mechanisms, and discusses misclassification biases in previous papers. Section 5 analyzes impacts on long-term outcomes. Finally, Section 6 concludes.

2 Institutional context

2.1 The French headscarf ban in schools

The wearing of the Islamic veil has been a burning issue in France since at least three decades. In 1989, the "*affaire des foulards*" (headscarf affair) garnered nationwide attention when three girls were expelled from their middle school for refusing to remove their headscarves. The incident sparked heated debates and was followed by similar disputes in other schools. Eventually, the affair was settled after the highest French administrative court (the *Conseil d'État*) ruled in favor of the expelled girls (Scott 2009). In its ruling, the Council stated that banning the wearing of signs of religious affiliation by students in public schools was against their freedom of religion.

The 1994 Bayrou circular. Five years later, following the election of a right-wing government, the minister of education, François Bayrou issued a circular asking school principals to prohibit conspicuous religious symbols worn by students.[4] To justify the government's position, the document insists on the distinction between conspicuous symbols and discreet signs. The Minister argued that conspicuous symbols are "in themselves acts of proselytizing" and should thus be prohibited in public schools. This interpretation of the Council's ruling contrasts with its original meaning, which stated that as long as the student's behavior was not disrupting class activities, one should not be refused admission to school for wearing a veil. Despite some opposition from the *Conseil*

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[3]Different types of replications and their definitions are discussed in Dreber and Johannesson (2025).

[4]In France, a ministry circular is a governmental document which gives a clarification or an interpretation of the law or establishes guidelines for civil servants.

d'État, most school principals decided to follow Bayrou's recommendation and to adopt the ban over the following years.

To help implement the bans, Simone Veil, the minister of social affairs, appointed a woman of North-African origin, Hanina Chérifi, as mediator to handle problems on the ground. In the school year that followed the circular (issued in September 1994), around 3,000 cases required an intervention from the mediator with only 139 leading to exclusions. The number of interventions quickly dropped to 1,000 in 1996 and to about 150 in 2002, suggesting that most establishments implemented the ban soon after the circular was issued.

Despite these small numbers, all the media and political attention likely changed the schooling environment for Muslim girls beyond the management of these cases. There was, "during this period, renewed significant media coverage of the hijab issue, with [...] a shift in tone in the press commentary. [...] And once again, few women, apart from the occasional headscarved girl, were asked what they thought" (Winter 2009, p. 185-187). Such a one-sided debate might have created a hostile schooling environment against Muslim pupils. The potential impacts on the school environment are explored in Section 4.4.

The 2004 law. Partly fueled by concerns about terrorism, President Jacques Chirac appointed a commission in July 2003 to evaluate the possibility of enshrining the 1994 circular into law. Once again, the public debate was fierce. On the one hand, proponents of the ban argued that the policy could not only "free" Muslim girls from religious pressures, but also that headscarves "infringed on the liberty of conscience of other pupils" (Abdelgadir and Fouka 2020, p. 4). On the other hand, critics replied that it would rather impede the integration of Muslim girls by excluding them from public education.

The commission ultimately recommended a ban on conspicuous religious signs in public schools. The French government voted the ban in March 2004, which was enforced in October 2004. As for the ministry circular, the Islamic veil was the main target of the law and Muslim girls were the main group affected in practice. A report from Chérifi (2004) based on fieldwork in four school academies shows that, in 2004-2005, only 639 students showed up to school wearing a conspicuous religious sign, less than half than in the previous academic year. While most cases were resolved through dialogue, about 200 of these 639 students switched to private schools or opted for distance learning (Mattei and Aguilar 2016).

2.2 The French educational system

The headscarf ban targets pupils at the primary and secondary levels, but does not apply to students attending college. In France, pupils enter elementary school at age 6 and it

lasts for 5 years (until age 11). Then, they attend middle school (*collège*) for four years (until age 15) and school attendance is mandatory until age 16. Therefore, potential impacts of the ban should manifest after middle school. After middle school, students enter high school, either to pursue a vocational degree (CAP/BEP) in a professional high school (typically for two years) or to prepare for the *baccalauréat* in a general or a technological high school, which lasts three years). The *baccalauréat* is the diploma required for continuing in higher education. Following previous studies of the French headscarf ban, completion of this degree is the primary outcome of interest.

3 Data and descriptive analysis

3.1 Data sources and sample

My primary data source is the two waves of the *Trajectoires et Origines* surveys (henceforth TeO; Beauchemin et al. (2016; 2023)). Conducted in 2008–2009 and 2019-2020 by the French National Institutes for Demographic Studies, the TeO surveys targeted adults between 18 and 60 years old residing in metropolitan France. Purposefully oversampling immigrants and minorities, it includes 3,033 and 3,519 women who identify as Muslim in the first and second waves respectively. To my knowledge, these are the largest samples of this kind in France.[5] When including Muslim men and other religious groups, the entire surveys contain more than 21,000 observations each.

The TeO datasets are a comprehensive source of information on various aspects of respondents' lives, including living conditions (such as employment, education, housing, commune of residence, and health), social life (such as migration history, language use, family, and children), and public life (such as political views, experiences of discrimination, and social relationships). Of particularly value for this study is the religion section, which is a unique inclusion in a French survey of this scale since the collection of individual information on religion is closely monitored in France. This section includes variables such as religious affiliation, measures of religiosity, religious symbols worn, and intergenerational religious transmission. This is a key advantage over the French Labour Force Surveys (LFS), the data source used in previous studies (Abdelgadir and Fouka 2020, Maurin and Navarrete H 2023). Indeed, the LFS only offers a proxy for religious affiliation, namely the parents' place of (and nationality at) birth. I also observe origin in the TeO surveys, which I use it to replicate the previous papers.

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[5]Two surveys conducted by private firms, namely Institut Montaigne (2016) and Institut Français d'Opinion Publique [IFOP] (2019), have much smaller sample sizes—slightly above 1,000 individuals of Muslim origin, both genders included. Moreover, the TeO surveys are much richer on many other dimensions and response rates are higher given its mandatory nature.

Measurement of religion. A key contribution of this paper is to better identify the treatment and control groups than in previous studies of the French headscarf ban. The empirical analysis relies on an actual measure of religious affiliation along with measures of religiosity.[6] Response rates to the religion section of the survey are very high: religious affiliation is observed for more than 98% of the sample in each wave. Other sections of the survey were mandatory.

First, for religious affiliation, respondents are asked whether they currently have a religion. If they answer "yes", then they are asked which one and the interviewer notes the exact answer given. Particular denominations within each religious family are then pooled into 12 groups in the survey data. For the main empirical analysis, I further pool religious denominations into five groups to use as controls: Atheists (no religion), Catholics, Other Christians, Muslims, and Others.[7] The same questions are asked about other family members, namely the father, the mother, and the partner.

Second, the TeO surveys include a set of questions about individual religiosity. My preferred measure is the frequency of attendance of religious ceremonies, a standard measure of religiosity which focuses on religious practice (Iyer 2016). I also consider other measures of individual religiosity: the self-reported importance of religion in the respondent's life and whether she uses her religion to self-identify. To exploit the common variation in these measures, I also build a latent index of individual religiosity as in Jacquet and Montpetit (2024). Details are provided in Appendix A.1.

Third, for veiling behavior, I use the following question from the TeO survey:

> *In your daily life, do you wear in public a piece of clothing or jewelry that might evoke your religion?*   *(1) Never*   *(2) Sometimes*   *(3) Always*

If applicable, respondents were subsequently asked to report which religious symbols they wear. Answers were later sorted by the survey institute into four categories: jewelry, clothing, headcoverings, or others. Because they visibly signal religion and are the ones usually targeted by secular policies, I group the clothing and headcoverings categories together as *conspicuous symbols*.[8]

Educational attainment. My main outcome variable is schooling, which I measure in two ways. The TeO surveys report the specific grade level at which the individual left school up to the Master's (*BAC+5*) level. I use this information to reconstruct both high-school completion and the number of years of schooling (from elementary school),

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[6] I do not use veiled women as the treatment group because veiling behavior is measured at the time of the survey and thus much later in life. Evidence in Indonesia suggests that improved economic opportunities induced women to veil (Shofia 2022). Therefore, not only is veiling not a permanent exogenous trait, but is potentially an outcome in my setting.

[7] The "Others" category is mostly composed of Buddhists, Jews, and Hindu/Sikh.

[8] I use the terms "veiling" and "conspicuous symbols" interchangeably in the remainder of the paper.

capped at twenty years.[9]

Sample selection. The main empirical analysis focuses on the effects of the 1994 circular on girls reaching puberty (15 years old) around its issuance. Because individuals aged less than 20 years old might still be in high school, I focus on women aged at least 20 years old. This leads me to restrict the sample to individuals born between 1971 and 1987 so as to have similar numbers of observations on both sides of the cohort threshold. Given these restrictions, I end up with a working sample of 7,758 women—and 7,261 men for placebo exercises.

3.2 Descriptive analysis

I start the empirical analysis by comparing the characteristics of Muslim women to those of other French-born women. Table A1 contains these statistics. Muslim women display less favorable economic outcomes than their non-Muslim counterparts. In terms of educational attainment, the main outcome of interest, Muslim women are twice as likely to have no education and are also substantially less likely to pursue in higher education. Only one third of Muslim women have completed a college degree while more than half of non-Muslim women graduated in higher education. This translates into a difference of one year of schooling. Muslim women also substantially differ from other women on various aspects of their lives such as religious characteristics. For example, they display higher levels of religiosity and are more likely to report that their parents invested in their religious education.

To understand whether the headscarf ban might have played any role in Muslim women's education, I first plot the time series of my main outcomes of interest in Figure 1, separately for Muslim women and women of other religious affiliation. A striking pattern emerges: while the educational attainment of Muslim women born in the 1970s was somewhat catching-up over that of women of other religious groups, there is a significant break in this trend from the first cohort reaching the age of veiling after the issuance of the Bayrou circular. The drop in high-school completion rates (panel A) is substantial and it increases over the first three affected cohorts. Thus, at first glance, this suggests that the issuance of the circular abruptly delayed the ongoing reduction in educational gaps between the two groups. This negative trend appears to be short-lived as it then bounces back to that observed in the cohorts of the 1970s. For years of schooling (panel B), a similar decrease occurs, though less pronounced. However, in contrast with the trend in the high-school graduation rate, the drop somewhat persists in the medium run. To move towards a causal interpretation, I estimate difference-in-differences models in the next section.

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[9]Only about 11% of the sample has completed at least a Master's degree.

Figure 1: Evolution of educational attainment of Muslim and non-Muslim women across birth cohorts, 1971-1987
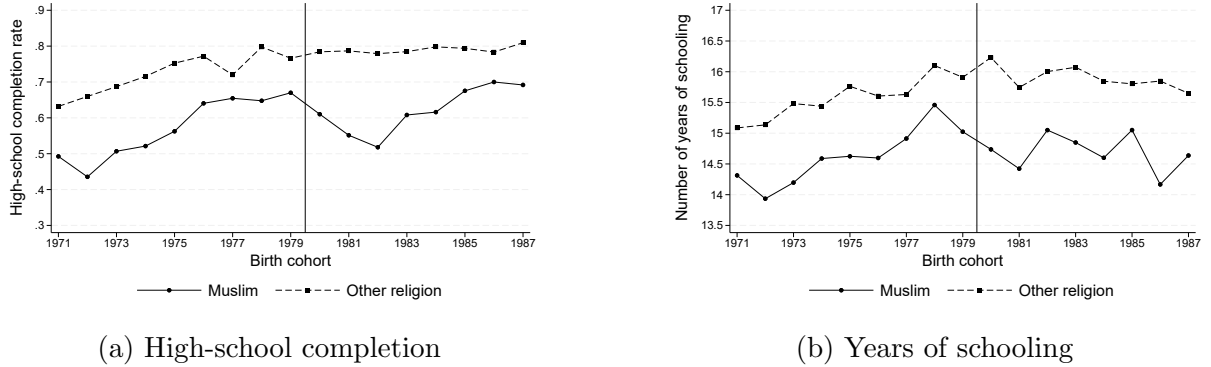
(a) High-school completion

(b) Years of schooling

Note: Evolution of the high-school completion rate across birth cohorts of Muslim and non-Muslim women born in France. On the right of the vertical line are cohorts who reach puberty after the 1994 Bayrou circular asking school principals to prohibit the headscarf in schools.

4 Impact on educational attainment

This Section covers the main empirical results. After describing the empirical strategy in Section 4.1, I present the main results in Section 4.2. Then I perform robustness checks (Section 4.3), investigate potential mechanisms (Section 4.4), and assess heterogeneity in policy impacts (Section 4.5). In Section 4.6 I compare my results with previous studies of the French headscarf ban and provide a detailed discussion of misclassification bias.

4.1 Empirical strategy

The main empirical specification is a standard difference-in-differences design, which compares cohorts of women of different religions reaching puberty (the age of veiling) before or after the 1994 circular was issued.

My approach differs from those of Abdelgadir and Fouka (2020) and Maurin and Navarrete H (2023) (henceforth AF and MN) in one crucial dimension, which is that I observe respondents' religious affiliation. I can thus identify Muslim women. In contrast, the empirical strategies used in those papers, summarized in Table 1, use proxies of religious affiliation, namely the father's country of birth or the father's nationality at birth. The age threshold chosen to define treated cohorts (third column) and the specific policy being studied (fourth column) also differ between the two studies.

In this paper, as in MN, I focus on the 1994 circular because most schools already implemented bans in 1994 (Chérifi 2004). As for the age threshold, I also follow MN and compare girls reaching puberty (the age of veiling) before and after the circular was issued. Formally, for individual i in birth cohort c, I estimate the following model:

$$Y_{i,c} = \alpha + \beta_1 \text{Muslim}_i \times \text{Post}_c + \beta_2 \text{Muslim}_i + \gamma_c + X'_{i,c}\delta + \varepsilon_{i,c} \tag{1}$$

where Post_c is an indicator variable taking the value of 1 if the individual is born after 1979 (i.e. reaches age 15 after 1994), Muslim_i indicates Muslim affiliation, and γ_c is a full set of cohort dummies. In my simplest specification, the vector of controls X contains a dummy for the second survey wave and dummies for the other religious groups. I also control for a Muslim-specific linear trend to account for the reduction of schooling gaps observed in pre-1980 cohorts in Figure 1.

In another specification, I further control for the respondents' living conditions when aged 15 years old. That is, I include indicators for whether the individual's father was working, the mother was working, and dummies for the *département* of residence. The main outcome variables Y are a dichotomous variable taking the value of one if individual i has completed high school (a *baccalauréat*) or the number of years of schooling.

Table 1: Empirical strategies used in previous studies of the French headscarf ban

Study	Policy	Age threshold	Data	Muslim proxy	Clustering level
Abdelgadir and Fouka (2020)	2004 law	19 y.o. (end of *baccalauréat*)	LFS 05-12	Father's birthplace	Father's birthplace
Maurin and Navarrete H (2023)	1994 circular	15 y.o. (puberty)	LFS 05-19	Father's nationality	Father's nationality $\times$ *département* of birth

Note: This table presents the empirical strategies used in previous studies of the impact of the French headscarf ban on the high-school graduation probability of Muslim women. The acronym LFS refers to the French Labour Force Surveys.

Inference. The appropriate method for calculating standard errors is yet another source of disagreement between the two previous studies. While AF cluster standard errors at the father's birthplace level, MN do not use this approach, arguing that it yields a small number of clusters. However, their clustering level may not be appropriate either since they do not cluster at the level at which treatment varies (Abadie et al. 2023). Here, treatment varies at the religion (or father's origin) levels, but not across the individual's birthplace within France.[10] Therefore, I rather cluster standard errors at the religion (or origin) level as in AF. Since this leaves me with few clusters, I also report p-values calculated using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) accounting for the small number of clusters in difference-in-differences designs.[11]

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[10] Maurin and Navarrete H (2023) justify their clustering level by saying that it accounts "for potential correlation of residuals within groups of individuals with a similar background" (p. 84). Such clustering strategy is at odds with standard practice in economics (Abadie et al. 2023).

[11] In difference-in-differences designs with few treated clusters, MacKinnon and Webb (2017) show that the wild cluster bootstrap procedure of Cameron et al. (2008) used in Abdelgadir and Fouka (2020) for robustness under-rejects the null hypothesis of null effects. The subcluster bootstrap of MacKinnon and Webb (2018) is preferred in this setting.

4.2 Main results

As shown in Section 3.2, the time series of educational attainment suggests that the 1994 circular induced a strong decline in schooling among Muslim girls. To confirm the graphical evidence, I estimate the difference-in-differences model (1). Results are reported in columns (1) and (2) of Table 2.

Table 2: Impact of the 1994 ministerial circular on educational attainment

			Proxies used in previous studies[†]			
Measure of treatment:	Religious affiliation		Father's nationality (MN)		Father's birthplace (AF)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: High school completion						
$\text{Muslim}_i \times \text{Post}_c$	-0.154***	-0.156***	0.047*	0.045*	0.073***	0.072***
	(0.015)	(0.012)	(0.023)	(0.024)	(0.020)	(0.020)
	[0.032]	[0.011]	[0.192]	[0.233]	[0.030]	[0.039]
Muslim_i	-0.232***	-0.207***	-0.240***	-0.204***	-0.082**	-0.077**
	(0.038)	(0.028)	(0.033)	(0.030)	(0.028)	(0.028)
	[0.014]	[0.005]	[0.061]	[0.054]	[0.128]	[0.123]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.611	0.611	0.625	0.625	0.641	0.641
N	6,905	6,905	6,818	6,818	6,872	6,872
R^2	0.039	0.076	0.046	0.080	0.043	0.078
Panel B: Years of schooling						
$\text{Muslim}_i \times \text{Post}_c$	-0.812***	-0.834***	0.278**	0.261*	0.330**	0.330**
	(0.237)	(0.195)	(0.113)	(0.142)	(0.102)	(0.109)
	[0.300]	[0.212]	[0.117]	[0.210]	[0.047]	[0.058]
Muslim_i	-1.041**	-0.876**	-1.892***	-1.662***	-0.345*	-0.359*
	(0.382)	(0.294)	(0.199)	(0.196)	(0.185)	(0.194)
	[0.168]	[0.145]	[0.020]	[0.020]	[0.262]	[0.257]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	14.956	14.956	14.965	14.965	15.152	15.152
N	6,670	6,670	6,589	6,589	6,639	6,639
R^2	0.034	0.070	0.048	0.079	0.045	0.077

Note: This Table reports regression estimates of the impact of the Bayrou circular on educational attainment of Muslim girls using various measures of the treated group. Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. In even columns, additional control variables are indicators for whether the individual's father was working, the mother was working, and dummies for the *département* of residence at age 15. Standard errors clustered at the religion (or father's origin) level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[†] MN refers to Maurin and Navarrete H (2023) and AF refers to Abdelgadir and Fouka (2020).

In Panel A, I first focus on the effect of the circular on the high-school completion rate, the main outcome studied in previous papers. Consistent with the observed trends,

I find that the ministerial circular causes an average decline in the probability of Muslim girls to complete high school of 15 percentage points. This effect is economically large in magnitude as it represents 25% of the pre-ban mean for this group. The point estimate remains unchanged when controlling for the woman's living conditions when she was 15 years old (column 2).

To further validate the impact on educational attainment, in Panel B, I exploit the richness of the TeO data to evaluate the impact on years of schooling. Precisely measuring years of completed education is not possible with the Labour Force Surveys and thus these results are novel in the literature on this reform. In line with the negative effect on high-school completion, the issuance of the circular is associated with a drop in years of schooling of more than 0.8 year (5.4% of the mean). In Appendix Table A9, I find that the smaller magnitude for years of schooling is in part due to Muslim girls switching to vocational high school—the CAP/BEP programs are one year shorter than the *baccalauréat*.

In columns (3) to (6) of Table 2, I assess how my results compare with previous studies. To do so, I estimate the impact of the circular using proxies for religious affiliation used in previous papers (see Table 1). Interestingly, I find that I can replicate the main result of Maurin and Navarrete H (2023) in the TeO data. They find that the issuance of the circular had a positive impact on girls of African origin, specifically of women whose father's has an African nationality. I obtain an estimate that is lower in magnitude, but similar to that found in that paper (4.7 versus 7.8 percentage points).

This result, however contrasts with Abdelgadir and Fouka (2020), who find no impact of the Bayrou circular. As shown in Table 1, this study uses an alternative age threshold to define treatment, namely adulthood. I interpret the lack of impact on cohorts reaching age 19 before and after the circular as stemming from the fact these cohorts already made their veiling and relevant educational choices. According to Muslim precepts, girls should veil at puberty, and hence the decision to veil is made earlier than in adulthood. The switch from middle school to high school is also a more important period for long-term schooling decisions.

One important point to note, however, is that the statistical significance of the results is quite sensitive to the method used to calculate standard errors. When accounting for the small number of clusters using the wild subcluster bootstrap, some estimates—especially for years of schooling—have high p-values and thus lose statistical significance. However, this correction of p-values was not made in the two previous papers (see Table 1). My results using the standard clustering approach are thus more comparable to the previous evidence.

Threats to identification. The main identification assumption for a causal interpretation is the standard parallel-trends assumption in that Muslim and non-Muslim

girls' educational attainment would have evolved the same absent the circular. To support
the parallel-trends assumption, I estimate an event-study model, which takes the form:

$$Y_{i,c} = \alpha + \sum_{\substack{c=1971 \\ c \neq 1979}}^{1987} \beta_c \, \text{Muslim}_i \times D_c + \gamma_r \text{Muslim}_i + \gamma_c + X'_{i,c}\delta + \varepsilon_{i,c} \qquad (2)$$

where D_c is an indicator taking the value of 1 if the individual is in birth cohort c. I use
the same set of controls as in the difference-in-differences analysis.

Results are reported in Figure 2. For high-school completion, except one aberrant
cohort (that of 1975), the treatment and control groups appear to evolve similarly in the
pre-treatment periods. Muslims girls catch-up on the control group in the early 1970s, but
the two groups evolve similarly from 1976 up to the first treated cohort. The graph also
reveals that the average impact in Table 2 masks important dynamics. Consistent with
the time series (Figure 1), the initially large negative impact vanishes from the 1985 birth
cohort. For the number of years of schooling, the pre-trends are quite similar. However,
for this outcome, the dynamics point to a rather persistent negative impact, rather than
simply a short-term phenomenon.

Another threat to identification arises if there is another shock differentially affecting
Muslim girls in the 1980 birth cohort. This is unlikely for two reasons. For one, the main
other episode that spurred discrimination against Muslims in this period is the September
2001 attacks in New York City, which occurred several years after the circular. Second,
the other change in the educational system for the 1980 cohort was a reform of vocational
high school. This policy reduced the number of years required to complete the vocational
degree by one year for some tracks in 2008 and then for all occupational tracks in 2009.
It also introduced catch-up exams for vocational high school students in their final year.
Although ethnic (and religious) minorities tend to be overrepresented in vocational tracks
(Belzil and Poinas 2010), there is no particular reason to believe that this policy change
would differentially affect male and female students. Among other robustness checks, I
use males as a placebo group in the following section to address this concern.

4.3 Robustness

Results of the main regression analysis suggest that banning the veil in French schools
worsens educational outcomes of Muslim girls. In this section I perform a set of sensitivity
analysis to validate the results, which are reported in Table 3.

First, there is empirical evidence that assimiliationist policies can cause exodus of
targeted groups (Saleh 2018, Saleh and Tirole 2021). We could thus be worried that
Muslim girls were more likely to study abroad as a response to the ban. In column (2),
I further restrict the sample to individuals who have completed all of their education

Figure 2: The dynamic impact of the 1994 Bayrou circular on Muslim girls' educational attainment
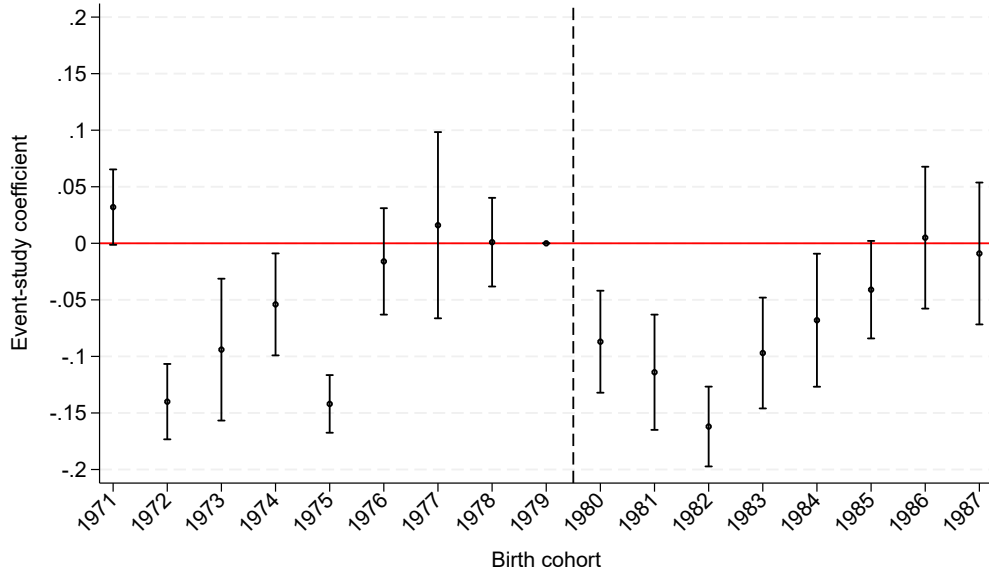
(a) High-school completion
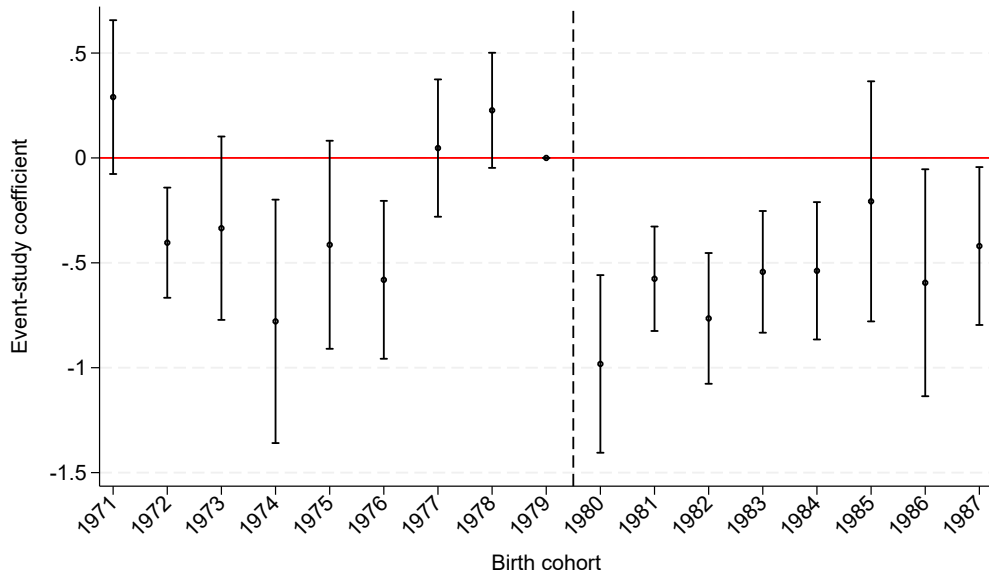
(b) Years of schooling

Note: These figures plot the coefficients of event-study regressions along with 95% confidence intervals. Outcomes variables are the high-school completion rate and the number of years of post-elementary schooling (capped at 20 years). Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. On the right of the vertical line are cohorts who are subject to the 1994 Bayrou circular asking school principals to prohibit the headscarf in schools.

in France. The estimated impacts are only slightly larger in magnitude when I use this different sample restriction.

Second, while Muslim girls were the main target of this policy, it prohibited all conspicuous religious symbols. Thus, the only group that is completely unaffected by the ban is atheists given that they have a null religiosity by definition. In the third column, I limit the control group to atheists only. As expected, the estimated drops in high-school completion rates and years of schooling are slightly larger in this specification.

Placebo analysis.   Next, in the last two columns, I estimate the difference-in-differences model using placebo groups that are unlikely to be affected by the ban. In column (4), I use the sample of men and consider Muslim men as the treatment group. As expected, since few Muslim men wear conspicuous religious symbols, I find no evidence of negative effect of the ban on this group. If anything, there is a small positive impact. However, as shown in Table A2, this positive effect shrinks when including additional control variables. The apparent negative impact on years of schooling is mainly due to a decrease in vocational high school achievement.

In the last column, I compare atheists to individuals reporting a non-Islamic religious affiliation. In principle, this group—mostly composed of Catholics—is potentially affected by the ban because it applies to all religious symbols. I find a small and statistically insignificant impact on this placebo group, which is in line with the Islamic veil being the main policy target.[12] Moreover, discreet symbols might be an appropriate substitute for conspicuous symbols for non-Muslims. Indeed, while most of the religious symbols worn by Muslim women are headscarves, women in other religious groups essentially only wear discreet symbols that are tolerated by law (see Table A1).

4.4   Mechanisms

The richness of the TeO surveys offers a unique opportunity to investigate the mechanisms through which the ban induces a decrease in schooling of Muslim girls. Given the limited content of the Labour Force Surveys (LFS) to explore such mechanisms, Abdelgadir and Fouka (2020) turn to the first wave of the TeO surveys and a qualitative analysis of twenty in-depth interviews. Despite their small sample and the fragility of their results (as shown, for example, in Maurin and Navarrete H (2023)), their analysis highlights potential channels. They argue that the intense public debates following the ban might have spurred discrimination against Muslims. Much of the public discourse adopted an anti-veiling and anti-Muslim tone (Scott 2009, Winter 2009). In turn, increased perceptions of discrimination might have reduced school performance through a feeling of alienation (Gould and Klor 2016).

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[12]The inclusion of all conspicuous symbols was seen by many as "a tokenistic attempt to hide the fact

Table 3: Robustness checks

	Baseline (1)	Schooling in France (2)	Muslims vs atheists (3)	Placebo groups	
				Muslim men (4)	Other religions (5)
Panel A: High school completion					
Muslim$_i$× Post$_c$	-0.150***	-0.157***	-0.167**	0.038***	
	(0.016)	(0.018)	(0.009)	(0.011)	
	[0.048]	[0.049]	[0.436]	[0.346]	
Muslim$_i$	-0.238***	-0.221***	-0.178**	-0.140***	
	(0.035)	(0.028)	(0.004)	(0.032)	
	[0.009]	[0.003]	[0.098]	[0.054]	
Not atheist$_i$× Post$_c$					-0.006
					(0.007)
					[0.699]
Not atheist$_i$					0.074*
					(0.035)
					[0.167]
Main controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.599	0.609	0.599	0.514	0.746
N	7,046	6,553	4,407	6,613	5,431
R^2	0.040	0.037	0.035	0.033	0.028
Panel B: Years of schooling					
Muslim$_i$× Post$_c$	-0.758**	-0.819***	-1.089**	-0.554**	
	(0.262)	(0.197)	(0.046)	(0.205)	
	[0.394]	[0.234]	[0.359]	[0.434]	
Muslim$_i$	-1.377***	-1.110**	-1.301***	-1.360***	
	(0.361)	(0.395)	(0.020)	(0.205)	
	[0.081]	[0.150]	[0.072]	[0.019]	
Not atheist$_i$× Post$_c$					-0.063
					(0.069)
					[0.680]
Not atheist$_i$					0.260
					(0.337)
					[0.570]
Main controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	14.646	15.046	14.646	14.373	15.651
N	6,808	6,322	4,250	6,421	5,225
R^2	0.039	0.045	0.041	0.028	0.024

Note: This Table reports results of robustness checks. In column (2) the sample is restricting to women who have studied in France. In column (3), I compare only Muslims and atheists. In columns (4) and (5), I consider placebo groups. Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. Standard errors clustered at the religion × born-post-1979 level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table 4, I explore the discrimination channel by using self-reported experiences of

that the law was really about the hijab" (Winter 2009, p. 224).

Table 4: Impact of the 1994 ministerial circular on experiences of discrimination

	Any experience (1)	Due to her religion (2)	Due to her origins (3)	At school (4)	Mistrust of French school (5)
Muslim$_i \times$ Post$_c$	0.086**	0.129***	0.035	0.015	0.075***
	(0.033)	(0.019)	(0.023)	(0.086)	(0.007)
	[0.446]	[0.187]	[0.499]	[0.935]	[0.021]
Muslim$_i$	0.125***	0.075	0.273***	-0.084	0.004
	(0.033)	(0.114)	(0.069)	(0.057)	(0.029)
	[0.069]	[0.732]	[0.034]	[0.373]	[0.935]
Main controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.364	0.206	0.691	0.345	0.118
N	7,074	1,724	1,724	2,335	7,008
R^2	0.038	0.165	0.207	0.042	0.010

Note: This Table reports regression estimates of the impact of the Bayrou circular on self-reported experiences of discrimination of Muslim girls. Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. Standard errors clustered at the religion level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

racism and discrimination as the outcome variable. In column (1) of Table 4, I show that treated cohorts are about 8.6 percentage points (24% of the pre-ban mean) more likely to report any experience of discrimination. However, since Muslim affiliation is correlated with being of African (especially Maghrebi) origin, these discriminatory treatments might not be solely related to religion.

Interestingly, the TeO survey also asked respondents about what was the source and the context of the alleged discrimination. In columns (2) to (4), I leverage this information and I find that treated cohorts are more likely to perceive being discriminated against because of their religion. The impact on religion-based discrimination is large, at almost two-thirds of the mean. While there is no evidence that these additional experiences of discrimination occurred at school (column 4), Muslim girls display higher rates of mistrust in the French school system (column 5). Overall, this evidence is consistent with an alienation of Muslim girls at school.

Another potential channel which may explain the negative impact I document could be a reaction to the ban from pupils' families. Dahl et al. (2022) find that, in reaction to an integration policy in Germany, Muslim parents were less likely to help their daughters with their homework and learning, with negative consequences on girls' self-esteem and life satisfaction. In theory, a similar disinvestment in girls' education could have occurred in the French context as a reaction to the perceived secularization of education (see Carvalho et al. 2024).

To help rule out this mechanism, I first evaluate the impact of the circular on whether girls received help at school from their family and peers. In columns (1) to (4) of Table 5,

Table 5: Impact of the 1994 ministerial circular on relatives' investments in girls' education and conflicts related to religion

	Was helped at school by				Type of school		
	Father (1)	Mother (2)	Siblings (3)	Friends (4)	Private (5)	Not in sector (6)	Rel. conflict (7)
$\text{Muslim}_i \times \text{Post}_c$	-0.098 (0.060) [0.479]	0.145*** (0.031) [0.118]	0.040 (0.084) [0.866]	-0.072 (0.054) [0.642]	0.009 (0.011) [0.738]	-0.026 (0.015) [0.535]	0.011 (0.019) [0.841]
Muslim_i	-0.135 (0.085) [0.317]	-0.180 (0.140) [0.417]	0.091 (0.073) [0.483]	-0.054 (0.050) [0.522]	-0.119*** (0.033) [0.054]	-0.105 (0.067) [0.325]	-0.024 (0.028) [0.617]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.283	0.276	0.714	0.348	0.071	0.165	0.183
N	3,329	3,375	3,231	3,427	7,074	7,063	7,063
R^2	0.029	0.093	0.051	0.026	0.192	0.014	0.006

Note: This Table reports regression estimates of the impact of the issuance of the Bayrou circular on Muslim girls' relatives' investments in their education and on the occurrence of conflicts with parents related to religion. Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. Standard errors clustered at the religion level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

I show the absence of significant impacts on whether the girl was helped by her father, her siblings, and her friends. If anything, mothers were more likely to support their daughters.

Second, the circular only applied to public schools and, therefore, private schools were forbidden to apply the ban. I check whether parents were more likely to send their daughters to private schools so as to avoid the veil ban. Results in column (5) suggest that this is not the case: the point estimate is very small and statistically indistinguishable from zero. Muslim parents were also as likely to send their daughters to another school than that of their catchment area (column 6). Thus, Muslim girls' disengagement from formal schooling does not appear to be driven by changes in behavior of their family members.

Third, in column (7), I evaluate whether the Bayrou circular triggered more conflict related to religion among Muslim families. I find no impact on this outcome either. Overall, these results suggest that strategic reactions from pious parents are unlikely to be driving the results.

4.5 Heterogeneous impacts

In Table 6, I investigate sources of heterogeneity in impacts of the ban on educational attainment. The survey contains several variables on the environment in which respondents grew up. I exploit this information for this analysis. This investigation serves

Table 6: Heterogeneous impacts on high-school completion rates

	Parental religious transmission		Parents' labor-force status		Language spoken at home	
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Muslim}_i \times \text{Post}_c$	-0.159*** (0.014) [0.017]	-0.161*** (0.018) [0.043]	-0.153*** (0.017) [0.046]	-0.019 (0.015) [0.674]	-0.153*** (0.016) [0.035]	-0.143*** (0.012) [0.013]
$\text{Muslim}_i \times \text{Post}_c \times \text{Religious education}_i$	0.016 (0.012) [0.601]					
$\text{Muslim}_i \times \text{Post}_c \times \text{Religious first name}_i$		0.225*** (0.041) [0.129]				
$\text{Muslim}_i \times \text{Post}_c \times \text{Mother was working}_i$			0.009 (0.006) [0.677]			
$\text{Muslim}_i \times \text{Post}_c \times \text{Father was working}_i$				-0.154*** (0.027) [0.120]		
$\text{Muslim}_i \times \text{Post}_c \times \text{Spoke French with mother}_i$					-0.005 (0.010) [0.846]	
$\text{Muslim}_i \times \text{Post}_c \times \text{Spoke French with father}_i$						-0.007 (0.007) [0.722]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.600	0.599	0.599	0.599	0.602	0.597
N	6,986	7,046	7,046	7,046	7,034	6,924
R^2	0.041	0.041	0.043	0.048	0.041	0.043

Note: This Table reports regression estimates of the heterogeneous impact of the Bayrou circular on educational attainment of Muslim girls. Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion dummies, a Muslim-specific linear trend, and the covariate the treatment is interacted with. Standard errors clustered at the religion level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

two purposes, namely further validating the role of parental influence and understanding how the familial context might shape long-run effects.

First, to further validate the absence of parental influence, I verify whether the estimated impact differs for women whose parents invested more in transmitting their religion. I use two measures of parental transmission of religion: whether the respondent reports religion as being "very important" in her education and religious name-giving.[13] Therefore, I have both a measure of *perceived* vertical transmission of religion and a measure based on revealed preferences of parents. I describe these variables in more detail in Appendix A.1.

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[13]Name-giving has been recognized as an important cultural transmission channel (Fryer and Levitt 2004, Abramitzky et al. 2020, Algan et al. 2022).

I report results of this heterogeneity analysis in columns (1) and (2) of Table 6. I find that stronger parental religious influence, if anything, slightly attenuates the negative effect on schooling. This holds for both measures of parental religious transmission. Thus, parental religious pressures do not appear to be the driving my results.

Second, another form of cultural transmission could be the language spoken at home (e.g. Fouka 2020). in columns (3) and (4), I show that impacts do not substantially vary

Third, in columns (5) and (6), I interact the treatment status with dichotomous variables indicating whether each parent was working when the respondent was 15 years old. I find, in particular, that the negative impact is concentrated among households in which the mother was not employed, but the father was. Together with the absence of impact of parental religious transmission, this result could indicate that the effects of the ban are concentrated among more traditional families, but not necessarily due to religious transmission.

4.6   Comparison to previous literature

Misclassification of treatment.   In the previous sections, I document large negative impacts of the Bayrou circular on academic achievement of Muslim girls. This result contrasts with previous evidence of positive effects on women of African origin in Maurin and Navarrete H (2023). While I show that the latter result holds in the TeO data, the impact is of opposite sign when religion is used to identify treatment. This suggests that using origin as a proxy for religion introduces a substantial misclassification bias. While classical measurement error would only induce an attenuation bias (towards zero), measurement error in religion is non-classical. With binary measures, any measurement error must be negatively correlated with their true values (Bound et al. 2001).[14]

What could explain such a large misclassification bias? Observing both the misclassified and the actual treatment indicators is a crucial advantage of the TeO data. It is rare to have this information in empirical applications.

First, when using both waves of TeO, I document that individuals of African origin are more religiously mixed than the authors claim. Only 71% of women whose father is an African national are actually Muslims in my regression sample.[15] One fourth of these second-generation immigrants are atheists and therefore unaffected by the Bayrou circular in my sample.

Second, recent advances in the literature on difference-in-differences with a

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[14] Abdelgadir and Fouka (2020) verify the robustness of their results to using alternative data sources, including the first wave of the Trajectories and Origins (TeO) survey in which religion is observed. However, by using only the first wave (of 2008-2009), they obtain a very small working sample of less than 2,000 observations in the regression analysis.

[15] This figure is even lower for Muslim women whose father is born in an African country, the proxy used in Abdelgadir and Fouka (2020). Less than 59% of this population is Muslim in my regression sample.

misclassified treatment show that misclassification can cause the DID estimand to be wrongly signed (Denteh and Kédagni 2022, Negi and Negi 2025). In the case of differential misclassification with asymmetric errors, Denteh and Kédagni (2022) show that the DID estimand may be wrongly signed even at relatively low rates of misclassification.[16] In the case studied here, the rates of false positives (non-Muslims of African origin) are 10.4-16.6% and those of false negatives (Muslims of non-African origin) are 11.3-19.3% under the two proxies. These are comparable to the minimal rates of asymmetric errors yielding a sign reversal in Denteh and Kédagni (2022). Moreover, in Appendix Table A5 I find that the rates of misclassification are correlated with my main schooling outcomes. This suggests that misclassification is unlikely to be non-differential, a necessary condition to guarantee attenuation bias (i.e. bias towards zero with no sign reversal).

Third, I further test for misclassification by interacting their measure of treatment (having a father who is an African national) with a Muslim dummy. Results are reported in Appendix Table A4. While the estimates are less precise in that specification, I find that the coefficient on the origin-religion interaction is negative. This thereby suggests that using father's nationality at birth as a proxy likely captures positive impacts on other religious groups.

Mechanisms. Regarding the mechanisms, Maurin and Navarrete H (2023) state that their results are consistent with the existence of a "silent majority" of Muslim girls who do not wish to veil and are under pressure to do so by their family. This argument is often used by proponents of veil bans who argue that such policies might liberate Muslim women from oppression. However, this point is very difficult to establish with the Labour Force Surveys used in that study (see De Giorgi 2023). These data lack information about the respondents' family or social circles.

I argue that this proposed mechanism is unlikely to hold for two reasons. First, qualitative evidence from interviews with Muslim women do not support this assertion. The vast majority of Muslim women claim that wearing the veil is their personal choice, some of them even doing so *against* their parents' will (Gaspard and Khosrokhavar 1995, Institut Montaigne 2016, Institut Français d'Opinion Publique [IFOP] 2019). An analysis of veiling patterns in France by Jacquet and Montpetit (2024) further suggests that private religious motives explain a larger share of veiling behavior than the religious environment.

Second, my results suggest that parental pressures are unlikely to be a key mechanism. I find no impact of the ministerial circular on parents' choices in Section 4.4. Moreover, the heterogeneity analysis in Section 4.5 indicates that girls raised in families that invested more in transmitting their religion do not differentially respond to the ban.

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[16]Misclassification with asymmetric errors is the case in which a misclassified dummy has different rates of false negatives and false positives. Differential misclassification refers to the case in which misclassification can be correlated with potential outcomes conditional on the true treatment.

Additional replications. Last, I end this Section by briefly discussing additional replications of (i) the impact of the Bayrou circular under AF's age threshold and sample, and (ii) the impact of the 2004 law under AF's and (iii) MN's threshold and sample in turn. For each replication, I consider the three measures of treatment as in Table 2. Results are reported in Appendix Tables A6 to A8. Overall, I mostly find statistically insignificant and inconsistent impacts when using these alternative specifications. I find no clear effect of the 2004 law, consistent with the prohibition having already been widely implemented following the Bayrou circular.

5   Long-term outcomes

Having established that the 1994 ministerial circular depressed schooling outcomes of Muslim girls, I now explore potential effects of this policy on their social and economic integration in the long-run. I focus on two groups of outcomes, namely economic conditions and cultural integration. While it can be expected that the decreased schooling translates into worse economic outcomes in the long-run, the potential impact on social integration is a priori ambiguous. On the one hand, there could be an identity backlash to the assimilationist policy (Sakalli 2019, Fouka 2020, Carvalho et al. 2024). On the other hand, religious minorities might reduce the intensity of their religious life when facing a more secular character of public education. For example, in the case of language policy, Clots-Figueras and Masella (2013) find that the introduction of Catalan as a teaching language in Catalan schools increased the number of people who identify as Catalan and who support Catalan independence.

Cultural integration. Long-run impacts on cultural integration in Table 8 are consistent with the latter interpretation. In columns (1) to (3), I estimate the impact of the Bayrou circular on measures of religiosity, namely the importance of religion in the respondent's life, the use of religion to self-identify, and a religiosity index (see Section 3.1). I find that treated Muslim women display lower levels of religiosity later in life. I further document an increase in veiling rates in public spaces (column 4), but this effect is not statistically significant under the bootstrap correcting for the few clusters. Therefore, the evidence overall points to a reduction in religiosity.

Economic theory suggests that veil bans may lead to increased segregation of pious women (Carvalho 2013). In columns (5) to (7), I further test this theoretical possibility by studying the impact of the Bayrou circular on social integration in the long-run. My first social-integration outcome (column 5) concerns friendships. I use a dichotomous variable indicating whether most of the woman's friends have the same religious affiliation. I find no evidence of this form of backlash. Similarly, affected cohorts are not more likely to report feeling French (column 6). In fact, if anything, I find they are more likely to

use French as main language at home (column 7). Moreover, I find some evidence of increased civic engagement among affected cohorts as they are more likely to have voted in the previous presidential election (column 8).

Overall, these results suggest that while the ban depressed schooling outcomes of Muslim girls, they display lower religiosity and similar levels of social integration in the long run.

Table 7: Impact of the Bayrou circular on long-run economic outcomes of Muslim girls

	Employment		Marriage market			
Dep. var:	Employed (1)	Works in public sector (2)	Lives in a couple (3)	Partner same rel. (4)	Partner works (5)	Has children (6)
Muslim$_i$× Post$_c$	-0.042***	-0.008	0.025	0.021	-0.054***	0.058***
	(0.013)	(0.014)	(0.018)	(0.018)	(0.014)	(0.015)
	[0.360]	[0.837]	[0.652]	[0.639]	[0.345]	[0.238]
Muslim$_i$	-0.122***	0.022	-0.021	0.303**	-0.014	0.071
	(0.025)	(0.022)	(0.086)	(0.126)	(0.013)	(0.082)
	[0.031]	[0.566]	[0.887]	[0.135]	[0.563]	[0.595]
Main controls	Yes	Yes	Yes	Yes		
Mean dep. var.	0.642	0.212	0.640	0.871	0.884	0.788
N	7,074	7,074	7,074	3,630	4,598	7,068
R^2	0.075	0.022	0.061	0.578	0.018	0.227

Note: This Table reports regression estimates of the impact of the issuance of the Bayrou circular on long-run economic integration of Muslim girls. Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. In column (4), the sample is restricted to women who currently have a partner. Standard errors clustered at the religion level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Economic outcomes. Last, I evaluate long-run economic impacts in Table 7. I estimate variants of equation (1) where the dependent variable is an employment measure, a marriage-market outcome, or fertility. I find that affected cohorts of Muslim women are less likely to be employed at the time of the survey (column 1). The estimated employment impact of 4.2 percentage points is large in magnitude at 6.5% of the mean employment of unaffected cohorts. In column (2), I show that this decrease is not specific public-sector employment for which conspicuous religious symbols are also prohibited. This suggests that the loss of human capital among affected cohorts translated into worse labor-market conditions in general. I note, however, that while partnership and same-faith marriage rates and are unchanged (columns 3 and 4), the ban increases the likelihood that Muslim women have an unemployed partner (column 5). This is consistent with ample evidence of assortative mating in the marriage market (e.g. Choo and Siow 2006, Chiappori et al. 2017). Finally, affected cohorts are more likely to have children (column 6), which may also drive the negative employment impacts of both parents.

Table 8: Long-term impact of the Bayrou circular on cultural integration of Muslim girls

Dep. var:	Religiosity				Social integration			
	Religion is very imp. (1)	Religion to self-define (2)	Religiosity index (3)	Veiling† (4)	Most friends of same religion (5)	Feels French (6)	Uses French at home (7)	Voted in last elec.‡ (8)
Muslim$_i$ × Post$_c$	-0.055***	-0.060***	-0.073***	0.025***	-0.020	0.015	0.052***	0.043**
	(0.007)	(0.006)	(0.008)	(0.002)	(0.024)	(0.010)	(0.009)	(0.017)
	[0.090]	[0.083]	[0.021]	[0.489]	[0.764]	[0.745]	[0.485]	[0.481]
Muslim$_i$	0.084	0.030	-0.057	0.119***	0.084	-0.048***	-0.072**	0.030
	(0.050)	(0.065)	(0.129)	(0.008)	(0.087)	(0.009)	(0.024)	(0.039)
	[0.362]	[0.820]	[0.794]	[0.001]	[0.568]	[0.031]	[0.165]	[0.652]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.431	0.213	0.252	0.150	0.574	0.901	0.811	0.882
N	7,065	6,967	7,074	7,064	5,116	7,037	2,597	6,172
R^2	0.273	0.102	0.173	0.147	0.049	0.030	0.106	0.014

Note: This Table reports regression estimates of the impact of the issuance of the Bayrou circular on cultural integration of Muslim girls. Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. Standard errors clustered at the religion level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
† Veiling is a dichotomous variable taking the value of 1 if the individual wears a conspicuous religious symbol (headcovering or clothing) in public spaces.
‡ This outcome is an indicator for voting in the first round of the last presidential election (with respect to the time of the survey).

6 Conclusion

In this paper, I use unique, rich observational data on religion in France to revisit the mixed evidence on the French headscarf ban. I find that the prohibition following the issuance of a ministerial circular is associated with a large decline in the educational attainment of exposed Muslim girls. Educational outcomes recover after the fifth cohort reaches veiling age under the prohibition. The impact on the number of years of schooling persists over the medium-run. I provide suggestive evidence that the negative impact of the ban operates through heightened discrimination against Muslims and increased mistrust of the French school system rather than via responses from parents. I also find suggestive evidence that the negative impacts on educational attainment translate into lower employment and impacts the marriage market in the long run. However, I find a decrease in religiosity among affected cohorts and, if anything, positive effects on social integration later in life.

This study also highlights that misclassifying the group targeted by an assimilationist policy might lead to incorrect conclusions. I find that the positive effects on educational attainment documented in Maurin and Navarrete H (2023) appear to be attributable to individuals of African origin who are not Muslim. It is therefore unlikely that they capture any impact of the religious prohibition.

Overall, my results suggest that forced assimilation policies such as headscarf bans are not a successful tool to foster the economic integration of minorities and immigrants. This result is consistent with Fouka (2020) and Sakalli (2019) who similarly find that these types of policies might backfire. However, at the same time, some well-intentioned integration policies might also hamper assimilation in contexts in which minorities are strongly attached to their traditional norms (Dahl et al. 2022). Therefore, in a context of increased global migrations from countries with non-Western cultures, more work is needed to better understand which policies can durably foster their integration.

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A Online Appendix

A.1 Measurement of religiosity and parental religious influence

The TeO datasets contains rich information on respondents' religious life. I first describe the variables I use to proxy for individual religiosity. I then detail how I combine those multiple measures into meaningful indices through a measurement system.

Individual religiosity. I measure individual religiosity using survey questions on the frequency of attendance of religious ceremonies, the self-reported importance of religion in the respondent's life, whether she uses her religion to self-identify, the respect of religious dietary restrictions, and religious marriage. I list details of these variables below:

Variable name	Values	Question	Type
attendance of religious ceremonies	never; for familial ceremonies only; for religious feasts only; one or twice a month; weekly	"How often do you attend religious ceremonies?"	ordinal
importance of religion in respondent's life	no importance; a little; quite important; very important	"What importance do you give to religion in your life today?"	ordinal
uses religion to self-identify	yes; no	"Among the following characteristics, which ones define you best? [...] Your religion?"	indicator
respect of dietary restrictions	never; sometimes; always; none (coded as a dummy if "always")	"In your daily life, do you respect your religion's dietary restrictions?"	indicator
religious marriage	yes; no	"Did you and your husband do a religious wedding?"	indicator

Measurement system. Since there is no natural way to combine the ordinal and indicator variables described above into meaningful indices, I formulate a measurement system. I am interested in a latent variable, *individual religiosity*, which I assume loads into the proxies listed above. I interpret those proxies as noisy measures of the associated unobserved, underlying concept. Denote by Z the vectors of proxies for individual religiosity. I assume ordinal relationships between measures $\{Z\}$ and the underlying factor IndivReligiosity$_i$ such that:

$$Z_{i,j} = \mu_{1,j} + \lambda_j \, \text{IndivReligiosity}_i + \varepsilon_{i,j} \tag{3}$$

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where ε is a measurement error assumed to be i.i.d. and to follow an ordinal logistic distribution. As the latent factor does not have a natural scale or location, to simplify interpretations, I normalize the means of IndivReligiosity$_i$ to zero, and its variance to one. I then predict the latent factor for each individual by calculating its empirical Bayes mean (Skrondal and Rabe-Hesketh 2009).

Parental religious transmission. I measure vertical religious pressures using two variables, namely the self-reported importance of religion in the respondent's education and religious name-giving.

Variable name	Values	Question	Type
importance of religion in education	no importance; a little important; quite important; very important	"What importance did religion have in the education you received in your family?"	ordinal
religious first name	yes; no	constructed by the author using respondent's first name	indicator

I classify as religious the names of the Islamic prophet's wives, Khadija, Sawda, Aicha, Hafsa, Zainab, Hind, Juwairiya, Safiya, Ramla, and Maimuna (Morsy 1989); and of his daughter Fatima. Variations in spelling are permitted. For male first names, I follow Sakalli (2019) by considering a name as religious if it is a variation of the prophet's name (Mohamed in French) or if it begins with "Abd-" ("servant of..." in Arabic).

A.2 Appendix Tables and Figures

Figure A1: Evolution of educational attainment of Muslim and non-Muslim men across birth cohorts, 1971-1987
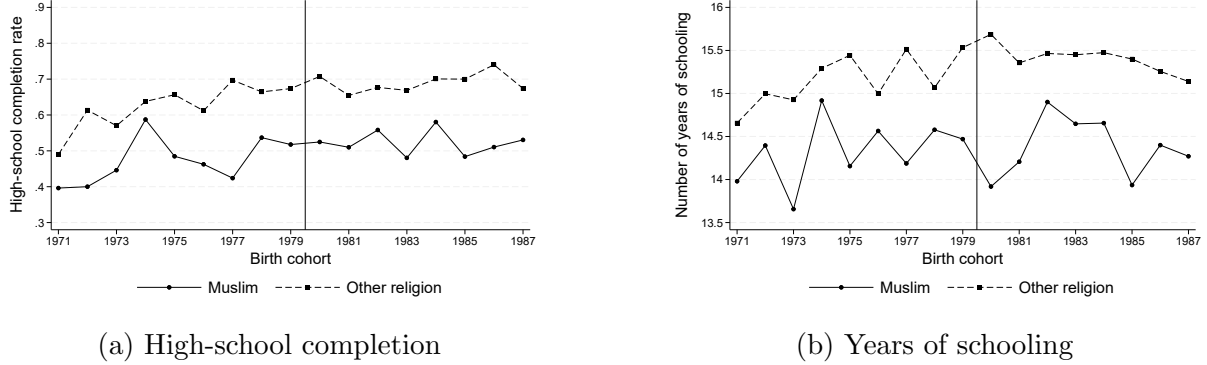
(a) High-school completion

(b) Years of schooling

Note: Evolution of the high-school completion rate across birth cohorts of Muslim and non-Muslim men born in France. On the right of the vertical line are cohorts who are subject to the 1994 Bayrou circular asking school principals to prohibit the headscarf in schools.

Table A1: Summary statistics

	Muslim women			Non-Muslim women		
	Mean	SD	N	Mean	SD	N
Demographics						
Age	32.933	7.539	1,860	35.039	7.364	5,898
Born in metropolitan France	0.853	0.354	1,860	0.919	0.273	5,898
Married	0.507	0.500	1,860	0.364	0.481	5,898
Highest degree completed						
No degree	0.13	0.34	1,619	0.06	0.23	5,453
CAP/BEP[†]	0.25	0.43	1,619	0.19	0.39	5,453
High school (*bacc*)	0.28	0.45	1,619	0.22	0.41	5,453
Higher education	0.33	0.43	1,619	0.53	0.39	5,453
Economic outcomes						
Employed	0.585	0.493	1,860	0.804	0.397	5,898
Unemployed	0.176	0.381	1,860	0.096	0.295	5,898
Inactive	0.187	0.390	1,860	0.064	0.244	5,898
Years of post-elementary schooling	14.71	3.14	1,440	15.73	3.01	4,879
Religious outcomes						
Religiosity index	0.343	0.791	1,860	-0.25	0.61	5,472
Religion is very important in life	0.469	0.499	1,854	0.135	0.342	2,886
Attends religious ceremonies regularly	0.065	0.247	1,856	0.140	0.347	2,886
Had conflict over religion with parents	0.177	0.382	1,860	0.148	0.355	5,898
Most friends are of the same religion	0.700	0.458	1,860	0.805	0.396	5,898
Religion was very important in education	0.432	0.495	1,852	0.139	0.346	5,831
Partner of same religion	0.553	0.497	1,860	0.226	0.418	5,898
Wears a religious symbol	0.286	0.452	1,854	0.234	0.424	2,888
Wears a conspicuous religious symbol	0.192	0.394	1,854	<0.01	0.049	2,888

Note: The data source is the Trajectories and Origins (TeO) surveys of 2008-2009 and 2019-2020.

[†] The CAP (*Certificat d'Aptitude Professionnelle*) and the BEP (*Brevet d'Études Professionnelles*) are vocational high-school degrees aimed at acquiring skills specific to a chosen occupation (such as plumbing, butchery, or bakery).

Table A2: Impact of the 1994 ministerial circular on educational attainment of men, additional controls

	High-school completion			Vocational high school		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Muslim}_i \times \text{Post}_c$	0.029**	0.030**	0.021**	-0.054***	-0.053***	-0.061***
	(0.010)	(0.011)	(0.009)	(0.009)	(0.010)	(0.010)
	[0.409]	[0.438]	[0.461]	[0.187]	[0.239]	[0.163]
Muslim_i	-0.148***	-0.143***	-0.125***	-0.109***	-0.105**	-0.105**
	(0.030)	(0.030)	(0.034)	(0.033)	(0.036)	(0.040)
	[0.038]	[0.045]	[0.087]	[0.161]	[0.190]	[0.227]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
Parents working	No	Yes	Yes	No	Yes	Yes
Dep. at 15 y.o.	No	No	Yes	No	No	Yes
Mean dep. var.	0.516	0.516	0.516	0.769	0.769	0.769
N	6,485	6,485	6,485	6,485	6,485	6,485
R^2	0.032	0.039	0.066	0.028	0.035	0.050

	Years of schooling		
	(7)	(8)	(9)
$\text{Muslim}_i \times \text{Post}_c$	-0.537**	-0.526**	-0.577**
	(0.197)	(0.211)	(0.208)
	[0.404]	[0.459]	[0.394]
Muslim_i	-1.210***	-1.186***	-1.018***
	(0.140)	(0.147)	(0.158)
	[0.007]	[0.008]	[0.018]
Main controls	Yes	Yes	Yes
Parents working	No	Yes	Yes
Dep. at 15 y.o.	No	No	Yes
Mean dep. var.	14.575	14.575	14.575
N	6,296	6,296	6,296
R^2	0.027	0.031	0.057

Note: This Table reports regression estimates of the impact of the Bayrou circular on educational attainment of Muslim boys. Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. In column (2), additional control variables are indicators for whether the individual's father was working and the mother was working. In column (3), dummies for the *département* of residence at age 15 are also controlled for. Standard errors clustered at the religion (or father's origin) level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Heterogeneity analysis of the impact on women of African origin

Measure of treatment[†]:	Father's nationality (MN) (1)	Father's birthplace (AF) (2)
Panel A: High school completion		
African origin$_i \times$ Post$_c$	0.030	0.022
	(0.041)	(0.041)
	[0.628]	[0.702]
African origin$_i \times$ Post$_c \times$ Muslim$_i$	-0.071**	0.023
	(0.024)	(0.022)
	[0.270]	[0.623]
Mean dep. var.	0.595	0.597
N	6,941	6,995
R^2	0.055	0.052
Panel B: Years of schooling		
African origin$_i \times$ Post$_c$	0.422**	0.123
	(0.141)	(0.255)
	[0.105]	[0.732]
African origin$_i \times$ Post$_c \times$ Muslim$_i$	-0.669***	0.186
	(0.131)	(0.190)
	[0.075]	[0.631]
Mean dep. var.	14.614	14.628
N	6,711	6,761
R^2	0.055	0.052

Note: Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, father's origins (nationality at birth or region of birth), and religion dummies and a Muslim-specific linear trend. Standard errors clustered at the religion (or father's origin) level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[†] MN refers to Maurin and Navarrete H (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A4: Heterogeneity analysis of the impact on women of African origin

Measure of treatment[†]:	Father's nationality (MN) (1)	Father's birthplace (AF) (2)
Panel A: High school completion		
African origin$_i \times$ Post$_c$	0.030	0.022
	(0.041)	(0.041)
	[0.637]	[0.706]
African origin$_i \times$ Post$_c \times$ Muslim$_i$	-0.071**	0.023
	(0.024)	(0.022)
	[0.279]	[0.617]
Mean dep. var.	0.595	0.597
N	6,941	6,995
R^2	0.055	0.052
Panel B: Years of schooling		
African origin$_i \times$ Post$_c$	0.422**	0.123
	(0.141)	(0.255)
	[0.113]	[0.731]
African origin$_i \times$ Post$_c \times$ Muslim$_i$	-0.669***	0.186
	(0.131)	(0.190)
	[0.071]	[0.636]
Mean dep. var.	14.614	14.628
N	6,711	6,761
R^2	0.055	0.052

Note: Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, father's origins (nationality at birth or region of birth), and religion dummies and a Muslim-specific linear trend. Standard errors clustered at the religion (or father's origin) level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[†] MN refers to Maurin and Navarrete H (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A5: Misclassification and educational attainment,
Muslim girls

Outcome:	High school (1)	Years of schooling (2)
$\varepsilon(\text{nationality})$	-0.057*** (0.018)	-0.330** (0.131)
$\varepsilon(\text{origins})$	-0.040*** (0.015)	-0.274** (0.107)

Note: This Table reports estimates of univariate regressions of schooling outcomes on the misclassification bias using the proxies in previous studies of the French headscarf ban. Following the notation in Denteh and Kédagni (2022), ε is an indicator taking the value of one if the treated group is misclassified under the considered proxy. The sample used is the same as in the main regression analysis. Robust standard errors in parentheses. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Replication of the impact of the Bayrou circular using AF age threshold and sample

| Measure of treatment: | Religious affiliation | | Proxies used in previous studies[†] | | | |
| | | | Father's nationality (MN) | | Father's birthplace (AF) | |
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: High school completion						
$\text{Muslim}_i \times \text{Post}_c$	0.122***	0.138***	0.059	0.076	0.033	0.045*
	(0.013)	(0.010)	(0.056)	(0.061)	(0.019)	(0.024)
	[0.064]	[0.014]	[0.527]	[0.467]	[0.272]	[0.213]
Muslim_i	-0.169**	-0.134**	-0.294***	-0.263***	-0.116**	-0.125***
	(0.061)	(0.048)	(0.030)	(0.027)	(0.038)	(0.037)
	[0.160]	[0.130]	[0.017]	[0.011]	[0.109]	[0.073]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.513	0.513	0.549	0.549	0.594	0.594
N	4,094	4,094	4,072	4,072	4,101	4,101
R^2	0.038	0.094	0.051	0.103	0.043	0.095
Panel B: Years of schooling						
$\text{Muslim}_i \times \text{Post}_c$	0.283	0.392*	0.556	0.635	0.437	0.495
	(0.197)	(0.210)	(0.390)	(0.463)	(0.323)	(0.312)
	[0.679]	[0.587]	[0.351]	[0.372]	[0.332]	[0.267]
Muslim_i	-0.963	-0.747	-1.961***	-1.774***	-0.408	-0.482
	(0.596)	(0.533)	(0.147)	(0.197)	(0.269)	(0.308)
	[0.365]	[0.397]	[0.002]	[0.011]	[0.332]	[0.312]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	14.365	14.365	14.463	14.463	14.880	14.880
N	3,967	3,967	3,945	3,945	3,974	3,974
R^2	0.033	0.068	0.045	0.076	0.039	0.070

Note: This Table reports replication results of the impact of the Bayrou circular on educational attainment of Muslim girls using various measures of the treated group and the empirical strategy in Abdelgadir and Fouka (2020). Means of the dependent variable in the treatment group over pre-1976 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. In even columns, additional control variables are indicators for whether the individual's father was working, the mother was working, and dummies for the *département* of residence at age 15. Standard errors clustered at the religion (or father's origin) level reported in parentheses. *p*-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[†] MN refers to Maurin and Navarrete H (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A7: Replication of 2004 law impact using MN age threshold and sample

| Measure of treatment: | Religious affiliation | | Proxies used in previous studies[†] | | | |
| | | | Father's nationality (MN) | | Father's birthplace (AF) | |
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: High school completion						
$\text{Muslim}_i \times \text{Post}_c$	-0.013	-0.021	0.041	0.035	0.101**	0.093**
	(0.014)	(0.012)	(0.034)	(0.038)	(0.039)	(0.040)
	[0.738]	[0.541]	[0.377]	[0.478]	[0.103]	[0.128]
Muslim_i	-0.332***	-0.305***	-0.234**	-0.186*	-0.002	0.028
	(0.029)	(0.049)	(0.082)	(0.086)	(0.063)	(0.050)
	[0.004]	[0.044]	[0.092]	[0.171]	[0.982]	[0.682]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.637	0.637	0.658	0.658	0.685	0.685
N	4,920	4,920	4,845	4,845	4,895	4,895
R^2	0.031	0.067	0.033	0.067	0.031	0.066
Panel B: Years of schooling						
$\text{Muslim}_i \times \text{Post}_c$	-0.028	0.052	0.429	0.469	0.721	0.680
	(0.147)	(0.088)	(0.273)	(0.287)	(0.553)	(0.548)
	[0.938]	[0.805]	[0.259]	[0.234]	[0.340]	[0.362]
Muslim_i	-1.853***	-1.507***	-1.750**	-1.314*	0.064	0.371
	(0.290)	(0.181)	(0.695)	(0.678)	(0.755)	(0.710)
	[0.029]	[0.007]	[0.124]	[0.208]	[0.950]	[0.695]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	15.099	15.099	15.217	15.217	15.453	15.453
N	4,651	4,651	4,581	4,581	4,627	4,627
R^2	0.071	0.116	0.081	0.121	0.076	0.118

Note: This Table reports replication results of the impact of the 2004 headscarf ban on educational attainment of Muslim girls using various measures of the treated group and the empirical strategy in Maurin and Navarrete H (2023). Means of the dependent variable in the treatment group over pre-1990 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. In even columns, additional control variables are indicators for whether the individual's father was working, the mother was working, and dummies for the *département* of residence at age 15. Standard errors clustered at the religion (or father's origin) level reported in parentheses. *p*-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[†] MN refers to Maurin and Navarrete H (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A8: Replication of 2004 law impact using AF age threshold and sample

| Measure of treatment: | Religious affiliation | | Proxies used in previous studies[†] | | | |
| | | | Father's nationality (MN) | | Father's birthplace (AF) | |
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: High school completion						
Muslim$_i \times$ Post$_c$	0.042***	0.033***	0.020	0.021	0.035	0.030
	(0.010)	(0.010)	(0.034)	(0.034)	(0.031)	(0.037)
	[0.216]	[0.281]	[0.654]	[0.644]	[0.390]	[0.528]
Muslim$_i$	-0.329***	-0.301***	-0.377***	-0.328***	-0.152*	-0.136
	(0.025)	(0.036)	(0.038)	(0.040)	(0.074)	(0.078)
	[0.001]	[0.004]	[0.002]	[0.005]	[0.215]	[0.265]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.607	0.607	0.642	0.642	0.669	0.669
N	6,097	6,097	6,023	6,023	6,079	6,079
R^2	0.038	0.080	0.046	0.084	0.044	0.083
Panel B: Years of schooling						
Muslim$_i \times$ Post$_c$	-0.277***	-0.284***	-0.221	-0.202	0.098	0.119
	(0.052)	(0.070)	(0.276)	(0.239)	(0.242)	(0.246)
	[0.108]	[0.185]	[0.547]	[0.528]	[0.743]	[0.699]
Muslim$_i$	-2.491***	-2.268***	-2.902***	-2.588***	-1.219**	-1.046**
	(0.216)	(0.203)	(0.311)	(0.288)	(0.418)	(0.422)
	[0.000]	[0.001]	[0.001]	[0.000]	[0.072]	[0.110]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	14.942	14.942	15.115	15.115	15.281	15.281
N	5,799	5,799	5,734	5,734	5,784	5,784
R^2	0.048	0.084	0.058	0.091	0.057	0.091

Note: This Table reports replication results of the impact of the 2004 headscarf ban on educational attainment of Muslim girls using various measures of the treated group and the empirical strategy in Abdelgadir and Fouka (2020). Means of the dependent variable in the treatment group over pre-1986 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. In even columns, additional control variables are indicators for whether the individual's father was working, the mother was working, and dummies for the *département* of residence at age 15. Standard errors clustered at the religion (or father's origin) level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[†] MN refers to Maurin and Navarrete H (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A9: Impact of the 1994 ministerial circular on completion of vocational high school

			Proxies used in previous studies[†]			
Measure of treatment:	Religious affiliation		Father's nationality (MN)		Father's birthplace (AF)	
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Muslim}_i \times \text{Post}_c$	0.068***	0.068***	-0.014	-0.014	-0.030**	-0.030**
	(0.006)	(0.006)	(0.016)	(0.016)	(0.011)	(0.011)
	[0.023]	[0.024]	[0.540]	[0.539]	[0.090]	[0.089]
Muslim_i	0.107***	0.107***	-0.020	-0.020	-0.015	-0.015
	(0.033)	(0.033)	(0.012)	(0.012)	(0.015)	(0.015)
	[0.114]	[0.109]	[0.591]	[0.584]	[0.577]	[0.574]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.198	0.198	0.180	0.180	0.172	0.172
N	7,046	7,046	6,960	6,960	7,014	7,014
R^2	0.010	0.010	0.011	0.011	0.012	0.012

Note: This Table reports regression estimates of the impact of the Bayrou circular on completion of vocational high school (as highest degree obtained) among Muslim girls using various measures of the treated group. Means of the dependent variable in the treatment group over pre-1980 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific linear trend. In even columns, additional control variables are indicators for whether the individual's father was working, the mother was working, and dummies for the *département* of residence at age 15. Standard errors clustered at the religion (or father's origin) level reported in parentheses. p-values computed using the wild subcluster bootstrap procedure of MacKinnon and Webb (2018) are reported in brackets. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[†] MN refers to Maurin and Navarrete H (2023) and AF refers to Abdelgadir and Fouka (2020).