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 outcomes and long-run integration of Muslim women. Using a difference-in-differences design, I show that the 1994 directive instructing school principals to ban the veil in French schools 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 using an origin proxy for religion reverses the sign of the estimated effect on education, highlighting that misclassification of a binary treatment can substantially affect the conclusions. Despite the adverse economic consequences, the affected cohorts exhibit stronger identification with France but also higher levels of religiosity, suggesting a mixed long-run impact on cultural assimilation.

Keywords: headscarf ban, religious identity, women's education, cultural 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). Current empirical evidence on veil bans is mixed,[1] in part because of the paucity of data on religious practices in countries where such prohibitions exist.

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.[2] 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. To identify the impact of these reforms, I implement a difference-in-differences design comparing cohorts of Muslim women who reached the mandatory schooling age (16 years old) before and after the policy.

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 above 18% of the pre-circular mean and is robust to a range of sensitivity checks. This large negative effect dissipates only after six cohorts have reached the compulsory-schooling age under the ban. I further show—using detailed data

[1] See Abdelgadir and Fouka (2020), Corekcioglu (2021), Maurin and Navarrete-Hernández (2023), and Lu and Yurdakul (2025).

[2] Very few other countries implemented headscarf bans for students. Turkey banned the wearing of the veil on university campuses in 1997, but the regulation was repealed by the pro-Islamic conservative Justice and Development Party (AKP) in 2010 (Corekcioglu 2021). To my knowledge, Kazakhstan (since 2023) is the only other country to ban headscarves for both teachers and pupils. Other European countries such as the Netherlands, Belgium, Denmark, Bulgaria, Norway, and Switzerland prohibit the wearing of face-covering garments (i.e. the niqab and burqa), but not the headscarf (i.e. the hijab). Recently, the provincial government of Québec (second-most populous Canadian province) has proposed Bill 94 which would ban the full-face veil for pupils and Austria legislated a ban on headscarves in schools for girls under the age of 14 to take effect at the start of the 2026-2027 school year.

on respondents' educational trajectories—that this impact translates into reduced total years of schooling.

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 10.2 percentage points (50%) more likely to report religious discrimination. The alleged discrimination is driven by perceptions of unfair treatment at school specifically, which increases by 12.3 percentage points (35%). 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. Also consistent with this discrimination channel, I also document similar increases in perceptions of discrimination among Muslim men.

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, no more likely to attend private schools where the ban did not apply, and if anything, less likely to have had religion-related conflicts with their parents when reaching adulthood. Third, heterogeneity analyses show that the decline in schooling is more pronounced among girls from more traditional families in an economic sense (male-breadwinner households), 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 leads to higher dropout rates and a substantial 10.5 percentage point reduction in employment later in life (16% of the mean) along with spillovers into the marriage market. Consistent with extensive 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 clear evidence of a religious backlash. In contrast with findings from Turkey (Sakalli 2019) and Indonesia (Bazzi et al. 2025), I detect no sharp increase in religiosity in adulthood. However, I document a rise in veiling rates in public spaces, which could indicate a symbolic response to the prohibition. I also document that affected cohorts are more likely to form most of their friendships within their own religious group. Despite these indications of increased segregation, these cohorts also express stronger identification with French society. Taken together, these results suggest a simultaneous strengthening of both national and religious identities (Beaman 2016, Abdelgadir and Fouka 2020).

I build on two earlier studies of the French headscarf prohibition, Abdelgadir and Fouka (2020) and Maurin and Navarrete-Hernández (2023), which relied on the Labour Force Surveys and therefore had to proxy for religion with origin.[3] I find that accurately identifying the targeted group in this setting reveals several new insights into the effects of religious prohibitions in secular contexts. Using a different data source than in those studies, I confirm the positive effect of the Bayrou circular on high-school graduation rates for individuals of African origin. Yet for the group actually targeted by the policy—Muslim women—the effect is of opposite sign. I show that these patterns also hold for the number of years of schooling, a new result in the literature on the French headscarf ban.

The contrast between estimates based on religious affiliation and those relying on origin illustrates how sensitive conclusions in this literature can be to the measurement of the treated group. A wrongly signed estimate under an origin proxy is consistent with recent econometrics results on difference-in-differences with a misclassified treatment (Denteh and Kédagni 2022, Negi and Negi 2025). Seen through this lens, my results and those of the earlier studies are complementary rather than contradictory: the negative effects I estimate for Muslim women echo the losses that Abdelgadir and Fouka (2020) document for cohorts in school at the time of the 2004 law. The positive estimates for women of African origin in Maurin and Navarrete-Hernández (2023) appear to reflect, at least in part, outcomes of non-Muslim women of African origin. For the 2004 law itself, I find a decline in years of schooling, broadly in line with Abdelgadir and Fouka (2020). However, there is no clear change in high-school completion rates—a pattern consistent with the delayed degree completion those authors document, and with much of the prohibition having already been implemented in the decade following the Bayrou circular (Chérifi 2004).

This study first contributes to the literature on assimilation and integration policies. I show that the French headscarf ban had negative impacts on schooling and long-run economic outcomes of Muslim women, 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.[4] A notable exception is Dahl et al. (2022), who show that the introduction

[3] Abdelgadir and Fouka (2020) noted this limitation and complemented their analysis with the first wave of the TeO survey, which records self-reported religion. Their TeO sample, however, contained less than 2,000 observations. Pooling both waves of the survey allows me to revisit this question with a sample four times larger.

[4] See Gathmann and Garbers (2023) and Fouka (2022; 2024) for reviews. Other recent examples

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 women's 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, Arold 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. 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 and political science. 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. 2026). 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 diagnostic tool (Miguel 2021, Ankel-Peters et al. 2023).

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.[5] The exercise illustrates, in an unusually transparent setting where both the proxy and the true treatment indicator are observed, how misclassifying a targetment group can shape the conclusions about a policy's effects on economic integration. My results align with Maurin and Navarrete-Hernández (2023) when using origin as a proxy for Muslim affiliation and their empirical specification, the

include Kamel (2025) on Arab immigrants following the *Dow v. United States* ruling, Maruthiah (2026) on American Indian Boarding Schools, Baudin et al. (2025) on mandatory allegiance statements for birthright citizenship eligibility in France, and Abou Daher et al. (2025) on the lifting of the female driving ban in Saudi Arabia.

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

are of opposite sign for the group the policy actually targeted.

The rest of the article is structured as follows. In the next section, I discuss the conceptual framework drawing from economic theory that guides my empirical analysis. Section 3 then describes the institutional context. Section 4 presents the data sources along with summary statistics. Section 5 evaluates the impact of the ban on Muslim women's educational attainment, presents my analysis of the underlying mechanisms, and discusses misclassification biases in previous papers. Section 6 analyzes impacts on long-term outcomes. Finally, Section 7 concludes.

2 Conceptual framework

How can a veil ban in a secular country impact long-run economic and cultural outcomes of Muslim women? The economic literature suggests several pathways through which minorities could respond to assimilationist policies like veil bans.

Economic theory of cultural transmission argues that individuals may underinvest in education to avoid its cultural content (Carvalho et al. 2024). As a response to a perceived strengthening of the secular content of education in France, Muslim pupils may drop out of school so as to preserve their religious identity. Similarly, identity-concerned Muslim parents who perceived the policy as a threat to their religious traditions could impose sanctions on their daughters who attend school unveiled. Examples of this avoidance strategy include Muslims under secular curricula in Turkey (Meyersson 2014, Sakalli 2019) and Indonesia (Bazzi et al. 2025), and cultural resistance among the Amish in the US (Choy 2020).

Perceived discriminatory treatment might also lead to cultural backlash by targeted minorities (e.g. Fouka 2020). Rather than inducing cultural assimilation, such policies may strengthen attachment to minority identity and increase investments in identity-preserving behavior. In the context of a veil ban affecting adolescent girls, this could translate into increased religiosity (Carvalho 2013), greater mistrust of majority institutions, and stronger reliance on co-religious friendship networks. At the same time, for Muslim girls who decide to pursue their education under the ban, the policy may reinforce identification with France due to higher exposure to non-Muslim peers (Beaman 2016).

An argument often heard in public debates in France states that Muslim girls are coerced into veiling (e.g. Maurin and Navarrete-Hernández 2023). If wearing the veil is imposed by pious parents or religious communities, it may be an inefficient and harmful social norm. A ban on such harmful norm could improve social welfare (Gulesci et al. 2025). For example, unveiling may make it easier to make friends at school or help avoid disruptions and discriminatory treatment in the classroom. This mechanism is consistent with findings by Dahl et al. (2022), who document that a German integration policy prompted cultural resistance by Muslim parents, resulting in negative consequences on

Muslim girls.

In the case of the French headscarf regulations, the current evidence focusing on outcomes of women of North-African origin is mixed and points at different mechanisms. While Abdelgadir and Fouka (2020) find that the 2004 ban depressed schooling outcomes of girls reaching adulthood after its enactment, Maurin and Navarrete-Hernández (2023) find that the Bayrou circular had a positive impact on educational attainment of girls reaching the age of 15 after the circular. While the former provide qualitative evidence that the impacts of the ban operate through increased discrimination against Muslim girls, the latter argue that their results are theoretically consistent with the existence of a "silent majority" of Muslim girls coerced into veiling.

In this paper, I provide an empirical assessment of these mechanisms using rich observational data on religious behavior and family circumstances. Before moving to the empirical analysis, I describe the policy change and the broader institutional context below.

3 Institutional context

3.1 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.[6] 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*

[6]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 pupils 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 5.4.

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 pupils 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).

3.2 The French educational system

The headscarf ban targets pupils at the primary and secondary levels, but does not apply to students attending university. 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 14-15) 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.

4 Data and descriptive analysis

4.1 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 the best of my knowledge, these are the largest samples of this kind in France.[7] 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). A unique feature for a French survey of this scope is a section on religion, since the collection of individual religious information is tightly regulated in France. This section includes information 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-Hernández 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 to replicate the previous papers.

[7] 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 properly identify the treatment and control groups. The empirical analysis relies on an actual measure of religious affiliation along with measures of religiosity.[8] 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. 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*.[9]

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), capped at twenty years.[10]

Sample selection. The main empirical analysis focuses on the effects of the 1994 circular on girls reaching the mandatory-schooling age (16 years old) around its issuance.

[8] 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.

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

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

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. I further restrict the sample to women born in France. Given these restrictions, I end up with a working sample of 7,758 women—and 7,261 men.

4.2 Descriptive analysis

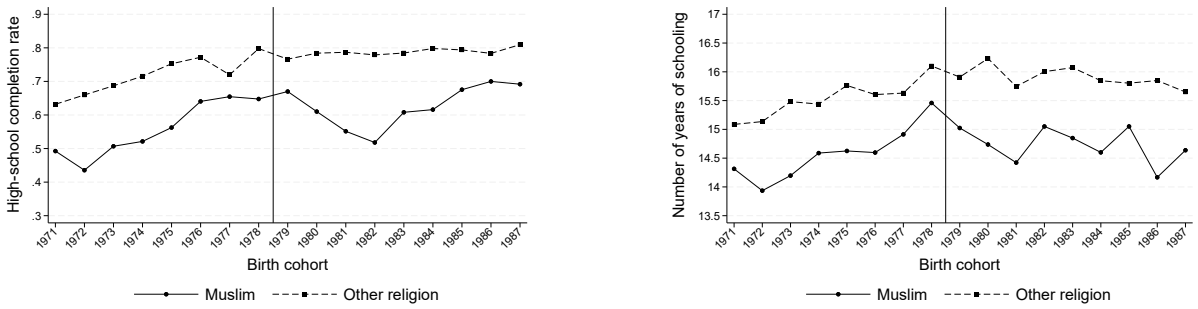
I start the empirical analysis by comparing the characteristics of Muslim women to those of other French-born women. Table A1 contains these statistics. Muslim women display less favorable economic outcomes than their non-Muslim counterparts. In terms of educational attainment, the main outcome of interest, 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 1980 cohort. The drop in high-school completion rates (panel A) is substantial and it increases over the following 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, and it coincides with the first cohort reaching the mandatory-schooling age after the circular issuance. 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.

5 Impact on educational attainment

This Section covers the main empirical results. After describing the empirical strategy in Section 5.1, I present the main results in Section 5.2. Then I perform robustness checks

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 the compulsory-schooling age (16 years old) after the 1994 Bayrou circular asking school principals to prohibit the headscarf in schools.

(Section 5.3), investigate potential mechanisms (Section 5.4), and assess heterogeneity in policy impacts (Section 5.5). In Section 5.6 I then compare my results with previous studies of the French headscarf ban.

5.1 Empirical strategy

The main empirical specification is a standard difference-in-differences design, which compares cohorts of women of different religions reaching the age at which school is no longer mandatory before or after the 1994 circular was issued.

My approach differs from those of Abdelgadir and Fouka (2020) and Maurin and Navarrete-Hernández (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 the first years after 1994 (Chérifi 2004). As for the cohort threshold defining treatment, the appropriate threshold depends on the question being asked. AF focuses on the 2004 law and define as treated the cohorts young enough to still be in secondary school after its enactment. Their threshold (age 19) is designed to guarantee that control cohorts should had completed secondary schooling and were therefore unexposed. The authors argue that this logic is internally consistent for their design, which estimates the effect of the law on cohorts in school when it arrived (Abdelgadir and Fouka 2023). My object of interest is different: the margin on which the actual bans

could affect educational attainment is the decision to leave school once attendance is no longer mandatory. The natural cutoff for this margin is the age at which dropping out becomes possible, namely 16 years old. I therefore consider individuals born 1979 or later as treated. MN instead use 15 years old, interpreted as the age of puberty and hence of veiling decisions. However, there is little evidence that veiling decisions are concentrated at that age. Moreover, puberty usually occurs earlier: De La Rochebrochard (1999) documents that the median age of puberty in France among the 1975-1978 cohorts is 13.1 years old.

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 1978 (i.e. reaches age 16 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-1979 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 × *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, MacKinnon et al. 2023). Here, treatment varies at the religion (or father's origin) level, but not over the individual's birthplace within France. 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 subculster bootstrap procedure of MacKinnon and Webb (2018) accounting for the small number of clusters in difference-in-differences designs.[11]

5.2 Main results

As shown in Section 4.2, the time series of educational attainment suggest that the 1994 circular induced a strong decline in schooling among Muslim women. To confirm the graphical evidence, I estimate the difference-in-differences model (1). Results are reported in columns (1) and (2) of Table 2.

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 women to complete high school of 11 percentage points. This effect is economically large in magnitude as it represents 18% of the pre-ban mean for this group. The point estimate remains similar 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 0.4 year (3% of the mean). In Appendix Table A2, I find that there is no discernible impact on completion of vocational high school (CAP/BEP), suggesting that these effects are driven by increased drop-out.

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). I find that, when using origin proxies, the effects on schooling are positive, smaller in magnitude, and not statistically significant.

In Appendix Table A3, I further show that I can replicate the main result of Maurin and Navarrete-Hernández (2023) in the TeO data when using their 15-year old age threshold. They find that the issuance of the circular had a positive impact on girls of

[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 procedure of MacKinnon and Webb (2018) is preferred in this setting.

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						
$\text{Muslim}_i \times \text{Post}_c$	-0.111***	-0.102***	0.016	0.022	0.022	0.030
	(0.011)	(0.018)	(0.017)	(0.018)	(0.026)	(0.024)
	[0.026]	[0.122]	[0.534]	[0.389]	[0.523]	[0.343]
Muslim_i	-0.104***	-0.084***	-0.165***	-0.146***	0.012	0.010
	(0.031)	(0.026)	(0.038)	(0.037)	(0.031)	(0.028)
	[0.098]	[0.107]	[0.145]	[0.159]	[0.816]	[0.812]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.609	0.609	0.622	0.622	0.637	0.637
N	6,623	6,623	6,544	6,544	6,596	6,596
R^2	0.072	0.103	0.085	0.112	0.084	0.111
Panel B: Years of schooling						
$\text{Muslim}_i \times \text{Post}_c$	-0.446*	-0.458*	0.139*	0.169**	0.047	0.103
	(0.236)	(0.241)	(0.072)	(0.065)	(0.113)	(0.101)
	[0.527]	[0.515]	[0.212]	[0.103]	[0.751]	[0.431]
Muslim_i	-0.215	-0.099	-1.363***	-1.250***	0.240	0.192
	(0.310)	(0.247)	(0.245)	(0.254)	(0.180)	(0.181)
	[0.678]	[0.816]	[0.069]	[0.085]	[0.388]	[0.488]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	14.952	14.952	14.966	14.966	15.147	15.147
N	6,416	6,416	6,341	6,341	6,390	6,390
R^2	0.064	0.095	0.079	0.104	0.078	0.105

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-1979 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-Hernández (2023) and AF refers to Abdelgadir and Fouka (2020).

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).

Results in columns (5) and (6), however, are similar to Abdelgadir and Fouka (2020), who find no effect of the Bayrou circular. As shown in Table 1, their study defines treatment using an alternative age threshold, namely adulthood. The absence of an effect among cohorts reaching age 19 before and after the circular might reflect the fact that

any impact would likely have occurred earlier, that is, at the age of dropping-out. This pattern is consistent with AF's focus on the signal sent by the legislation themselves, rather than on actual veil bans and the drop-out margin I focus on.

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. AF address the small number of clusters with the wild cluster bootstrap of Cameron et al. (2008) as a robustness exercise (see Abdelgadir and Fouka 2023). However, for settings with few treated clusters, as is the case here, MacKinnon and Webb (2017) show that the wild bootstrap under-rejects the null hypothesis of null effects and that the subcluster variant of MacKinnon and Webb (2018) is preferred in this setting. MN rather cluster standard errors at the father's nationality × birthplace level, which may not be appropriate since this is not the level at which treatment varies (Abadie et al. 2023, Abdelgadir and Fouka 2023). My results using the standard clustering approach are thus more comparable with the main findings of two previous studies.

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

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

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 outlying cohort (1975), the treatment and control groups appear to evolve similarly in the pre-treatment periods. 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. I observe similar dynamics for the number of years of schooling, further validating my findings.

Another threat to identification arises if there is another shock differentially affecting Muslim women born in 1980. 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 Muslim pupils. This reform should, if anything, boost vocational high-school completion rates, but I find no impact on in margin (see Appendix Table A2).

5.3 Robustness

Results of the main regression analysis suggest that banning the veil in French schools worsens educational outcomes of Muslim women. In this section I discuss some sensitivity analysis, which is reported in Appendix Table A4.

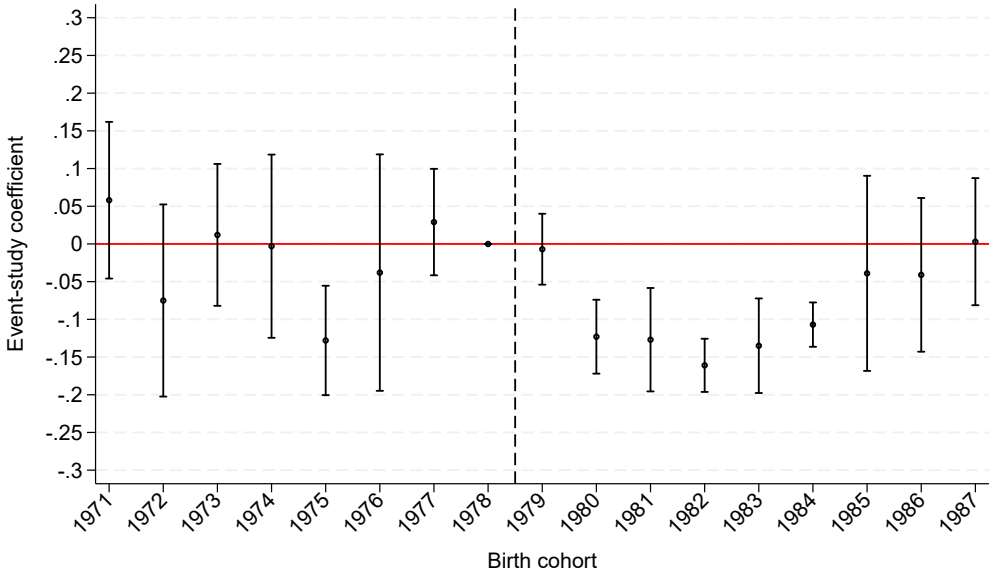
Alternative sample restrictions. There is empirical evidence that assimilationist policies can cause exodus of targeted groups (Saleh 2018, Saleh and Tirole 2021). We could thus be worried that Muslim pupils were more likely to study abroad as a response to the ban. In column (2), I further restrict the sample to individuals who have completed all of their education in France. The estimated impacts are only slightly lower in magnitude when I use this different sample restriction.

Another concern is that while Muslims were the main target of this policy, it prohibited all conspicuous religious symbols. Thus, non-atheists in the control group could be affected by the ban. The only group which is completely unaffected by the ban is the 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 only slightly larger in this specification.

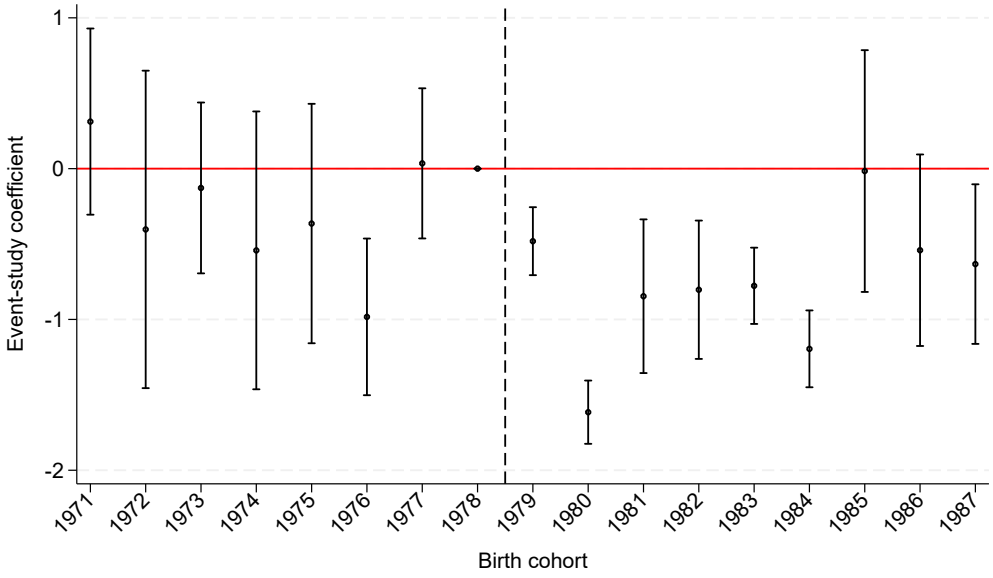
Placebo analysis. Last, in the fourth column, I estimate the difference-in-differences model using a placebo group that is unlikely to be affected by the ban. That is, I compare atheists to individuals reporting a religious affiliation other than Islam. In principle, this group—mostly composed of Catholics—is also concerned by the ban because it applies to all conspicuous religious symbols.

I find a precise null impact on this placebo treatment group, in line with the Islamic veil being the main policy target. The inclusion of all conspicuous symbols was seen by many as "a tokenistic attempt to hide the fact that the law was really about the hijab" (Winter 2009, p. 224). Moreover, discreet symbols might be an appropriate substitute for conspicuous symbols for non-Muslims. 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).

Figure 2: The dynamic impact of the 1994 Bayrou circular on educational attainment of Muslim women



(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 × département of birth, survey waves, religion, number of siblings, and number of older siblings (capped at ten) dummies. On the right of the vertical line are cohorts who reach the compulsory-schooling age after the issuance of the 1994 Bayrou circular asking school principals to prohibit the headscarf in schools.

5.4 Mechanisms

The richness of the TeO surveys offers a unique opportunity to investigate the mechanisms through which the ban induces a decrease in schooling among Muslim women. 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. Although the smaller sample available to them limited the precision of their analysis, it highlighted potential channels in line with the conceptual framework laid out in Section 2, which my larger two-wave sample allows me to examine systematically. 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 (e.g. Gould and Klor 2016) or prompted strategic reactions to perceived prejudice (e.g. Charles et al. 2025).

Table 3: 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)
$\text{Muslim}_i \times \text{Post}_c$	0.136***	0.102***	0.105***	0.123**	0.044***
	(0.019)	(0.021)	(0.029)	(0.046)	(0.006)
	[0.081]	[0.305]	[0.147]	[0.262]	[0.047]
Muslim_i	0.084**	0.052	0.273***	-0.047	-0.024
	(0.033)	(0.107)	(0.080)	(0.066)	(0.025)
	[0.172]	[0.792]	[0.046]	[0.631]	[0.555]
Main controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.361	0.205	0.694	0.349	0.118
N	6,784	1,664	1,664	2,258	6,731
R^2	0.047	0.175	0.235	0.050	0.013

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-1979 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Discrimination against Muslims. In Table 3, I explore the discrimination channel by using self-reported experiences of racism and discrimination as the outcome variable. In column (1) of Table 3, I show that treated cohorts are about 13.6 percentage points (38% of the pre-ban mean) more likely to report any experience of discrimination.

Interestingly, the TeO survey also asked respondents about 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 half of the pre-circular mean. Muslim affiliation being correlated with African (especially Maghrebi) origins, I unsurprisingly find a positive effect on origin-based discrimination as well. It is, however, much lower in magnitude as a share of the pre-circular mean (15%), suggesting that religion-based discrimination is the main factor. In column 4, I further find that these additional experiences of discrimination did occur at school. Muslim women also display higher rates of mistrust in the French school system (column 5), in line with increased hostility and a sense of alienation experienced by Muslim pupils.

To further validate the discrimination channel, I consider the impact of the Bayrou circular on Muslim men. If discrimination against Muslims increased following the headscarf ban more broadly, the Bayrou circular might have negative impacts on Muslim men as well. In Table 4, I estimate equation (1) in the sample of men and consider Muslim men as the treatment group.

In the first two columns, I focus on men's educational attainment. The circular is associated with a decrease in the number of years of schooling of Muslim men (column 2). I do not find that the negative impact on years of schooling is driven by lower high-school completion rates. In Appendix Table A5, I show that there is instead a small decrease in vocational high-school achievement among Muslim men.

In columns (3) to (7), I document that Muslim men are also more likely to report being discriminated against and to mistrust the French school system. The magnitude of impact is generally lower than for women, suggesting that the increased hostility was more targeted at female pupils. Muslim men are also more likely to associate discrimination with their origins than with their religion. A possible explanation could be that the political and media attention to religious symbols shifted perceptions of religious hostility from anti-Muslim sentiment to discrimination against the veil itself.

Alternative mechanisms. An alternative explanation for 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 women received help at school from their family and peers. In columns (1) to (4) of Table 5, I show the absence of significant impacts on whether the woman was helped by her father, her mother, her siblings, and her friends.

Second, the circular only applied to public schools and, therefore, private schools

Table 4: Impact of the 1994 ministerial circular on educational attainment and experiences of discrimination, men

	Educational attainment		Experiences of discrimination				
	High-school completion (1)	Years of schooling (2)	Any experience (3)	Due to his religion (4)	Due to his origins (5)	At school (6)	Mistrust of French school (7)
$\text{Muslim}_i \times \text{Post}_c$	0.005	-0.652***	0.056***	0.025	0.123**	0.079*	0.051***
	(0.013)	(0.071)	(0.014)	(0.023)	(0.044)	(0.037)	(0.008)
	[0.910]	[0.044]	[0.308]	[0.707]	[0.186]	[0.372]	[0.091]
Muslim_i	-0.152***	-1.330***	0.177***	-0.052	0.389**	-0.077*	-0.076
	(0.031)	(0.167)	(0.028)	(0.129)	(0.123)	(0.036)	(0.042)
	[0.037]	[0.008]	[0.012]	[0.793]	[0.049]	[0.219]	[0.313]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.514	14.373	0.490	0.196	0.825	0.397	0.132
N	6,613	6,421	6,364	1,534	1,534	2,434	6,311
R^2	0.033	0.028	0.138	0.120	0.192	0.048	0.011

Note: This Table reports regression estimates of the impact of the Bayrou circular on educational attainment (columns 1 and 2) and experiences of discrimination (columns 3 to 7) of Muslim men. Means of the dependent variable in the treatment group over pre-1979 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 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

were forbidden to apply the ban. Bazzi et al. (2025) show that female students' demand for Islamic schooling increased among cohorts exposed to a veil ban in public schools. I check whether Muslim parents in France reacted in a similar way by sending their daughters to private schools. Results in column (5) suggest that this is not the case: if anything, private-school attendance may have decreased. Muslim parents were only slightly more likely to send their daughters to another school than that of their catchment area (column 6). Therefore, it is unlikely that Muslim families sending their daughters to schools which have not yet implemented the ban would drive my results. 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, if anything, a reduction in such religious conflicts. Overall, these results suggest that strategic reactions from pious parents are unlikely to be driving the results.

5.5 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

Table 5: Impact of the 1994 ministerial circular on relatives' investments in girls' education

	Was helped at school by				Type of school		
	Father	Mother	Siblings	Friends	Private	Not in sector	Religious conflict
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{Muslim}_i \times \text{Post}_c$	-0.021	0.017	0.038	0.008	-0.016**	0.019***	-0.047***
	(0.043)	(0.026)	(0.049)	(0.021)	(0.006)	(0.006)	(0.014)
	[0.828]	[0.771]	[0.770]	[0.889]	[0.309]	[0.257]	[0.265]
Muslim_i	-0.047	-0.074	-0.002	-0.023	-0.108***	-0.085	-0.057
	(0.091)	(0.137)	(0.034)	(0.036)	(0.034)	(0.067)	(0.036)
	[0.742]	[0.714]	[0.973]	[0.685]	[0.076]	[0.396]	[0.350]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.281	0.277	0.716	0.349	0.072	0.161	0.184
N	3,325	3,371	3,228	3,423	6,784	6,776	6,778
R^2	0.048	0.132	0.293	0.035	0.191	0.017	0.012

Note: This Table reports regression estimates of the impact of the issuance of the Bayrou circular on Muslim girls' relatives' investments in their education. Means of the dependent variable in the treatment group over pre-1979 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

respondents grew up. I exploit this information for this analysis. This investigation serves 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 (at home) and name-giving.[12] For name-giving, my preferred measure is the transmission of either an Arabic-sounding first name.[13] Therefore, I have both a measure of *perceived* vertical transmission of religion and a measure based on revealed preferences of parents. I describe the construction of these variables in more detail in Appendix A.1.

I report results of this heterogeneity analysis in columns (1) and (2) of Table 6. I find that stronger parental religious influence, if anything, attenuates the negative effect on schooling, though this heterogeneous effect is not statistically significant at the 95% level. This pattern holds for both measures of parental religious transmission. Thus, parental religious pressures do not appear to be the driving my results.

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

[13]Arabic names typically signal the Muslim religion as most of those names come from the Quran (Algan et al. 2022).

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)
$\text{Muslim}_i \times \text{Post}_c$	-0.123***	-0.204***	-0.114***	-0.063***	-0.112***	-0.104***
	(0.010)	(0.057)	(0.015)	(0.012)	(0.012)	(0.011)
	[0.011]	[0.254]	[0.070]	[0.169]	[0.035]	[0.035]
$\text{Muslim}_i \times \text{Post}_c$	0.032*					
$\times$ Religious education$_i$	(0.016)					
	[0.422]					
$\text{Muslim}_i \times \text{Post}_c$		0.188*				
$\times$ Arabic first name$_i$		(0.095)				
		[0.424]				
$\text{Muslim}_i \times \text{Post}_c$			0.016**			
$\times$ Mother was working$_i$			(0.006)			
			[0.496]			
$\text{Muslim}_i \times \text{Post}_c$				-0.075***		
$\times$ Traditional family$_i^{\dagger}$				(0.011)		
				[0.123]		
$\text{Muslim}_i \times \text{Post}_c$					-0.004	
$\times$ Spoke French with mother$_i$					(0.011)	
					[0.877]	
$\text{Muslim}_i \times \text{Post}_c$						-0.005
$\times$ Spoke French with father$_i$						(0.007)
						[0.775]
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	0.040	0.044	0.043	0.040	0.040	0.043

Note: This Table reports regression estimates of the heterogeneous impact of the Bayrou circular on educational attainment of Muslim women. Means of the dependent variable in the treatment group over pre-1979 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
† Traditional family$_i$ takes the value of one if the respondent's father was working and her mother was not when aged 15 years old.

Second, in column (3), I interact the treatment status with dichotomous variables indicating whether the mother was working when the respondent was 15 years old. Maternal employment partly buffers the negative effect of the circular, which suggests that working mothers may have encouraged their daughters to pursue their education despite the prohibition. In column (4), I examine heterogeneity by family type. I define a dichotomous variable taking the value of one if, when aged 15 years old, the woman's mother was at home and the father was working, so as to capture traditional families in an economic sense. I find that, while the negative impact is concentrated among male-breadwinner households, the estimated negative impact on other families only shrinks by half.

Third, another form of cultural transmission could be the language spoken at home (e.g. Fouka 2020). In columns (5) and (6), I show that the impact of the circular on high-school graduation rates is unaffected by this household characteristic. Together with the absence of impact of parental religious transmission, these result indicate that the effects of the ban are concentrated among more traditional families, but not necessarily due to vertical religious or cultural transmission. Therefore, these results might simply reflect larger impacts among pious Muslim pupils, but I cannot check this in the data since religiosity and veiling measures are only observed later in life.

5.6 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 women. This result contrasts with previous evidence of positive effects on women of African origin in Maurin and Navarrete-Hernández (2023). While I show that their result holds in the TeO data when using their age threshold (see Appendix Table A3), 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 necessarily non-classical. With binary measures, any measurement error must be negatively correlated with their true values (Bound et al. 2001).

Two important remarks are in order. On the one hand, this concern applies to estimates that rely on origin proxies, which the Labour Force Surveys made unavoidable. AF were explicit about this limitation and verified their results in the first wave of the TeO survey using self-reported religion, with findings that point in the same direction as mine, though on a working sample of less than 2,000 observations. On the other hand, self-reported religion is measured several years after the policy and may itself respond to it. Reassuringly, in my analysis of long-term outcomes in the next secion, I find no impact of the circular on the use of religion for self-identification (Table 7, column 2).

What could explain such a large misclassification bias? The availability of both the misclassified and actual treatment indicators in a single dataset is a crucial advantage of the TeO data. It allows me to provide three insights on the sources of bias, which I detail below.

First, when using both waves of the survey, I document that individuals of African origin are more religiously mixed than is commonly assumed in this literature. Only 71% of women whose father is an African national are actually Muslims in my regression sample.[14] One fourth of these second-generation immigrants are atheists and therefore

[14]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.

unaffected by the Bayrou circular in my sample.

Second, recent advances in the literature on difference-in-differences with a 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.[15] 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 A6 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 A7. 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 second-generation immigrants of other religious groups or on atheists.

Mechanisms. Regarding the mechanisms, Maurin and Navarrete-Hernández (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, the Labour Force Surveys contain no information on respondents' families or social circles, so this assumption could not be tested in that study (see De Giorgi 2023). The richness of the TeO data directly allow me to test this.

Two sets of results suggest that this mechanism is unlikely to hold. 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.

[15] 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.

I find no impact of the ministerial circular on parents' choices and no increase in religion-related conflicts with parents in Section 5.4. Moreover, the heterogeneity analysis in Section 5.5 indicates that girls raised in families that invested more in transmitting their religion are, if anything, less affected by the reform.

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 (19 years old) and sample, and (ii) the impact of the 2004 law under AF's and (iii) MN's threshold (15 years old) and sample. For each replication, I consider the three measures of treatment as in Table 2. Results are reported in Appendix Tables A8 to A10. Overall, I mostly find statistically insignificant and inconsistent impacts when using these alternative specifications. These results are also indicative of misclassification bias, which should be even larger if the age threshold is also possibly misclassified. For the 2004 law, my estimates are less conclusive than for the circular: years of schooling decline among exposed Muslim women (Table A10), in line with AF, while high-school completion shows no clear change. This pattern is consistent with the delayed completion of secondary degrees documented by AF, and with much of the prohibition having already been implemented in the decade following the Bayrou circular, leaving the law itself with a smaller incremental effect on the drop-out margin. The dip in years of schooling visible for the 1986 cohort in Figure 1—the first cohort treated in AF's design—is also consistent with their findings.

6 Long-term outcomes

Having established that the 1994 ministerial circular depressed schooling outcomes of Muslim women, 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 7 suggest that both mechanisms might be at play. In columns (1) to (3), I estimate the impact of the

Bayrou circular on measures of religiosity, namely regular attendance (at least monthly) of worship events, the use of religion to self-identify, and a religiosity index (see Section 4.1 and Appendix A.1). I do not find a clear and consistent impact on religiosity later in life. While worship attendance increases, there is no discernible effect on my religiosity index. However, I document an increase in veiling rates in public spaces (column 4), which could suggest a form of religious backlash to the prohibition.

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. Consistent with the positive impact on veiling and worship attendance (column 1), Muslim women exposed to the circular are more likely to form friendships with other Muslims.

Despite these indications of increased segregation, I find that affected cohorts also increase their identification with the French society. I use two measures of cultural integration into the French society: self-reported feelings of French identity (column 6) and whether the respondent uses French as main language at home (column 7). I find positive impacts on both measures. Taken together, these results are consistent with a strengthening of both national and religious identities (Beaman 2016, Abdelgadir and Fouka 2020).

Table 7: Impact of the Bayrou circular on cultural integration of Muslim women

	Religiosity				Cultural integration		
Dep. var:	Monthly worship attendance	Religion to self-define	Religiosity index	Veiling†	Most friends of same religion	Feels French	Uses French at home
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{Muslim}_i \times \text{Post}_c$	0.029***	-0.009	-0.025	0.074***	0.054***	0.028***	0.098***
	(0.007)	(0.007)	(0.020)	(0.003)	(0.016)	(0.006)	(0.008)
	[0.104]	[0.797]	[0.683]	[0.079]	[0.273]	[0.353]	[0.184]
Muslim_i	-0.140**	0.048	-0.038	0.129***	0.105	-0.046***	-0.062**
	(0.056)	(0.064)	(0.130)	(0.008)	(0.087)	(0.008)	(0.023)
	[0.126]	[0.705]	[0.858]	[0.000]	[0.467]	[0.027]	[0.200]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.047	0.213	0.252	0.150	0.574	0.901	0.811
N	7,066	6,967	7,074	7,064	5,116	7,037	2,597
R^2	0.080	0.101	0.173	0.149	0.049	0.030	0.107

Note: This Table reports regression estimates of the impact of the issuance of the Bayrou circular on cultural integration of Muslim women. Means of the dependent variable in the treatment group over pre-1979 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.

Economic outcomes. Last, I evaluate long-run economic impacts in Table 8. I estimate variants of equation (1) where the dependent variable is an employment measure, a marriage-market outcome, or fertility. I find that the increase in school drop-out rates (column 7) translates into reduced employment at the time of the survey (column 1). The estimated employment impact of 10.5 percentage points is large in magnitude at more than 16% of the mean employment of unaffected cohorts. In column (2), I show that this decrease is not specific to public-sector employment for which conspicuous religious symbols are also prohibited. The negative impact on public-sector employment accounts for about 60% of the total drop, suggesting that the loss of human capital among affected cohorts translated into worse labor-market conditions in general. Moreover, while same-faith marriage rates are unchanged (column 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, given their earlier school exit, affected cohorts are more likely to have children (column 6), which may also drive the negative employment impacts on both Muslim women and their partner.

Table 8: Impact of the Bayrou circular on long-run economic outcomes of Muslim women

	Employment		Marriage market				
Dep. var:	Employed	Works in public sector	Lives in a couple	Partner same rel.	Partner works	Has children	School drop-out†
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{Muslim}_i \times \text{Post}_c$	-0.105***	-0.060***	0.081***	-0.041	-0.035***	0.059***	0.046**
	(0.022)	(0.018)	(0.023)	(0.023)	(0.005)	(0.015)	(0.020)
	[0.221]	[0.288]	[0.279]	[0.463]	[0.125]	[0.240]	[0.480]
Muslim_i	-0.134***	0.010	-0.008	0.284**	-0.005	0.066	0.104**
	(0.026)	(0.022)	(0.081)	(0.125)	(0.011)	(0.081)	(0.041)
	[0.023]	[0.794]	[0.948]	[0.150]	[0.790]	[0.603]	[0.169]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.642	0.212	0.640	0.871	0.884	0.788	0.305
N	7,074	7,074	7,074	3,630	4,598	7,068	0.305
R^2	0.075	0.022	0.061	0.578	0.018	0.227	0.014

Note: This Table reports regression estimates of the impact of the issuance of the Bayrou circular on long-run economic outcomes of Muslim women. Means of the dependent variable in the treatment group over pre-1979 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
† School drop-out is a dummy variable taking the value of one if the individual reports having stopped her schooling journey earlier than desired.

7 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 women. Educational outcomes recover only after several cohorts reach the age of compulsory schooling under the prohibition. 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. Last, I find mixed results on long-run religious and cultural integration, suggesting a strengthening of both national and religious identities.

This study also carries a measurement lesson: how the targeted group is identified can shape conclusions about the effects of an assimilationist policy. The positive effects on educational attainment estimated under origin proxies in Maurin and Navarrete-Hernández (2023) appear to be driven, at least in part, by individuals of African origin who are not Muslim, and are unlikely to capture any impact of the religious prohibition on the women it targeted. My results are instead consistent with Abdelgadir and Fouka (2020): cohorts of Muslim women exposed to the 1994 circular at the drop-out margin, and cohorts in school at the enactment of the 2004 law, both experienced educational losses through a hostile school environment, followed by a gradual recovery among cohorts entering school with the prohibition already in place.

Overall, the findings 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 $\text{IndivReligiosity}_i$ such that:

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

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 name-giving behavior.

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
Arabic-sounding first name	yes; no	TeO1 and constructed by the author using respondent's first name	indicator

The first TeO survey contains a classification of first names by likely origin. This variable is not available in TeO2, so I use all first names associated with an Arabic origin in TeO1 to impute a value to respondents in TeO2 using their first names.

A.2 Appendix Tables

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 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)
$\text{Muslim}_i \times \text{Post}_c$	0.013	0.016	-0.009	-0.014	-0.013	-0.019
	(0.014)	(0.015)	(0.011)	(0.010)	(0.014)	(0.011)
	[0.746]	[0.701]	[0.564]	[0.361]	[0.462]	[0.213]
Muslim_i	0.048	0.044	-0.052***	-0.054***	-0.057***	-0.053***
	(0.031)	(0.030)	(0.014)	(0.013)	(0.016)	(0.015)
	[0.386]	[0.390]	[0.201]	[0.149]	[0.066]	[0.066]
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
15 y.o. controls	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.206	0.206	0.187	0.187	0.177	0.177
N	6,623	6,623	6,544	6,544	6,596	6,596
R^2	0.024	0.043	0.030	0.049	0.031	0.050

Note: This Table reports regression estimates of the impact of the Bayrou circular on completion of any high-school degree (including CAP/BEP) 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-Hernández (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A3: Impact of the 1994 ministerial circular on educational attainment using a 15-year old age threshold

			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-Hernández (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A4: Robustness checks

		Subsamples		Placebo
	Baseline	Schooling in France	Muslims vs atheists	Other religions
	(1)	(2)	(3)	(4)
Panel A: High school completion				
$\text{Muslim}_i \times \text{Post}_c$	-0.110***	-0.102***	-0.115**	
	(0.014)	(0.016)	(0.008)	
	[0.066]	[0.112]	[0.584]	
Muslim_i	-0.215***	-0.193***	-0.150***	
	(0.035)	(0.028)	(0.001)	
	[0.014]	[0.007]	[0.017]	
$\text{Not atheist}_i \times \text{Post}_c$				-0.000
				(0.007)
				[0.980]
Not atheist_i				0.070*
				(0.034)
				[0.174]
Main controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.599	0.609	0.599	0.746
N	7,046	6,553	4,407	5,431
R^2	0.039	0.036	0.034	0.028
Panel B: Years of schooling				
$\text{Muslim}_i \times \text{Post}_c$	-0.565*	-0.457**	-0.932**	
	(0.263)	(0.202)	(0.040)	
	[0.508]	[0.474]	[0.441]	
Muslim_i	-1.262***	-0.945**	-1.167***	
	(0.352)	(0.397)	(0.004)	
	[0.092]	[0.195]	[0.018]	
$\text{Not atheist}_i \times \text{Post}_c$				-0.087
				(0.059)
				[0.520]
Not atheist_i				0.278
				(0.341)
				[0.555]
Main controls	Yes	Yes	Yes	Yes
Mean dep. var.	14.646	15.046	14.646	15.651
N	6,808	6,322	4,250	5,225
R^2	0.038	0.045	0.040	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 column (4), I use non-atheist individuals with another religious affiliation as a placebo group. Means of the dependent variable in the treatment group over pre-1979 cohorts are reported. Control variables are full sets of birthyear, survey waves, and religion (or father's origins) dummies and a Muslim-specific (or non-atheist-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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: 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)
$\text{Muslim}_i \times \text{Post}_c$	0.019	0.017	0.014	-0.010	-0.011	-0.016*
	(0.013)	(0.011)	(0.010)	(0.010)	(0.008)	(0.008)
	[0.649]	[0.629]	[0.645]	[0.788]	[0.704]	[0.608]
Muslim_i	-0.153***	-0.150***	-0.129***	-0.092**	-0.089**	-0.088*
	(0.030)	(0.030)	(0.034)	(0.034)	(0.037)	(0.042)
	[0.029]	[0.035]	[0.076]	[0.215]	[0.252]	[0.310]
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	0.034	0.050
Panel B: Years of schooling						
$\text{Muslim}_i \times \text{Post}_c$	-0.496***	-0.502***	-0.493***			
	(0.076)	(0.082)	(0.083)			
	[0.108]	[0.128]	[0.123]			
Muslim_i	-1.145***	-1.126***	-0.939***			
	(0.116)	(0.122)	(0.145)			
	[0.003]	[0.005]	[0.017]			
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-1979 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 A6: Univariate associations between misclassification and educational attainment, Muslim girls

Outcome:	High school (1)	Years of schooling (2)
ε(nationality)	-0.057***	-0.330**
	(0.018)	(0.131)
	[0.002]	[0.011]
ε(origins)	-0.040***	-0.274**
	(0.015)	(0.107)
	[0.009]	[0.010]
ε(father's religion)	-0.109***	-0.479***
	(0.026)	(0.167)
	[0.000]	[0.004]

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 or the father's religion. 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 our main regression analysis. Robust standard errors in parentheses. Level of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Heterogeneity analysis of the impact on women of African origin or with a Muslim father

Measure of treatment†:	Father's religion (1)	Father's nationality (MN) (2)	Father's birthplace (AF) (3)
Panel A: High school completion			
Muslim father$_i$× Post$_c$	-0.013		
	(0.036)		
	[0.856]		
Muslim father$_i$× Post$_c$× Muslim$_i$	-0.056		
	(0.039)		
	[0.583]		
African origin$_i$× Post$_c$		0.079***	0.045*
		(0.021)	(0.023)
		[0.075]	[0.263]
African origin$_i$× Post$_c$× Muslim$_i$		-0.176***	-0.086***
		(0.020)	(0.025)
		[0.014]	[0.216]
Mean dep. var.	0.596	0.595	0.597
N	6,745	6,941	6,995
R^2	0.047	0.057	0.055
Panel B: Years of schooling			
Muslim father$_i$× Post$_c$	0.116		
	(0.212)		
	[0.750]		
Muslim father$_i$× Post$_c$× Muslim$_i$	-0.506***		
	(0.071)		
	[0.039]		
African origin$_i$× Post$_c$		0.728***	0.250
		(0.124)	(0.225)
		[0.023]	[0.481]
African origin$_i$× Post$_c$× Muslim$_i$		-1.228***	-0.538
		(0.256)	(0.351)
		[0.108]	[0.536]
Mean dep. var.	14.631	14.614	14.628
N	6,519	6,711	6,761
R^2	0.047	0.056	0.053

Note: Means of the dependent variable in the treatment group over pre-1979 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-Hernández (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A8: Replication of the impact of the Bayrou circular using AF age threshold and sample

			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						
$\text{Muslim}_i \times \text{Post}_c$	0.122***	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-Hernández (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A9: Replication of 2004 law impact using MN age threshold and sample

			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						
$\text{Muslim}_i \times \text{Post}_c$	-0.013	-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-Hernández (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-Hernández (2023) and AF refers to Abdelgadir and Fouka (2020).

Table A10: Replication of 2004 law impact using AF age threshold and sample

			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						
$\text{Muslim}_i \times \text{Post}_c$	0.042***	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						
$\text{Muslim}_i \times \text{Post}_c$	-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-Hernández (2023) and AF refers to Abdelgadir and Fouka (2020).