

No. 2226

September 2026

Losing their religion

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Abstract

This paper studies whether governments can change social norms that constrain women's education and work, and through which mechanism. Albania's 1967 religion ban, which created the world's first atheist state, provides an extreme natural experiment: religious practice was outlawed nationwide, but a 1948 classification of historic monuments determined which religious buildings remained standing. We find larger increases in female employment and schooling where no monument survived. The public visibility of the old religious order appears to be the mechanism: effects are largest where no monument remained, smaller with rural-only monuments, and smallest with urban monuments; they also decline as monument count rises, regardless of denomination. We establish how social norms can outlive formal authority.

Keywords: social norms; gender; religion; female employment; education

JEL codes: Z13; J16; Z12; N34

This paper was produced as part of the Labour Programme. The Centre for Economic Performance is financed by the Economic and Social Research Council.

We would like to thank participants in numerous seminar audiences at Oxford University, London School of Economics, University College London, Zurich University, Baylor University, DIW Berlin, Padova University, St Andrews University, City University of New York, Bristol University, Birkbeck University of London, Surrey University, Bath University, York University, UC Davis, Duisburg University, NTNU University, University of Houston, and Luxembourg University, as well as conference participants at ASREC, IWAE, and the Riinvest Institute conference in Prishtina, Kosova. Special thanks to Amarildo Harka, Camilo Anton Cepeda Francese, and Liri Kuci for their excellent research assistance, and to Chari Amalavoyal, Sam Arenberg, Gharad Bryan, Jean-Paul Carvalho, Uran Ferizi, Colin Green, Dan Hamermesh, Indrit Hoxha, Iraj Hashi, Iftikhar Hussain, David Jaeger, Noel Malcolm, Sam Marden, Guy Michaels, Nathan Nunn, Steve Pischke, John Van Reenen, Andreas Uthemann, Tom Vogl, Jonathan White, Lea Ypi, and Maurizio Zanardi for comments on an earlier draft of this paper. Thanks also to Afrim Krasniqi and Juliana Rexha for very helpful insights on Albania. This paper investigates the economic consequences of the banning of religion and does not take a stand on the ethics or morality of the policy. The authors received no external financial support specifically for this research and have no financial relationships or other potential conflicts of interest to disclose.

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Published by
Centre for Economic Performance
London School of Economic and Political Science
Houghton Street
London WC2A 2AE

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Can governments reshape social norms that constrain economic activity? We study this question in the context of religious norms surrounding women’s education and work. Religious institutions not only organize private belief, they can also sustain shared expectations about appropriate behavior within the household and in public life. These expectations are especially important for women’s schooling and employment, where individual choices can depend on what others do and regard as acceptable. Formal religious authority can be abolished immediately, but the social expectations it sustained may still persist if households continue to believe that others will enforce the old order. We examine whether women’s education and work change more when the institutions that sustained those norms also disappear from local public view.

Albania’s 1967 religion ban provides an extreme natural experiment. The communist government outlawed religious practice nationwide, abolished the legal status of religious institutions, and closed every mosque, church, and monastery for worship, declaring Albania the world’s first atheist state. Most religious buildings were demolished or converted to secular use. Yet, a small number had been designated before the reform as historic monuments and thus remained standing, although worship was also prohibited in them (Gjeçovi, 2008; Bickert, 2018). This pre-existing classification determined whether a municipality retained a protected religious monument after the ban: some were left with at least one, while others with none. We exploit this variation to compare changes in women’s education and work between the two groups before and after the ban.

This setting separates two margins that are usually difficult to distinguish empirically. Formal religious authority and worship were abolished nationwide, while the public presence of the old religious order varied locally. This local visibility margin is central to our empirical design: a protected monument kept the old order visible in local public life even after it ceased to function as a religious institution. The source of this variation predates the reform by nearly two decades. In 1948, the Institute of Sciences compiled Albania’s first national heritage inventory, classifying monuments on historical, architectural, and cultural grounds. Of the 2,169 religious buildings closed at the time of the reform (Bickert, 2018), 130—about 6 percent—had been protected in this way. Thus, every municipality faced the same ban. What differed was whether any of its religious buildings had been classified for protection in 1948. The mechanism we test is that the complete disappearance of the old religious order from local public view weakened the social enforcement of traditional gender roles.

The setting is especially informative because Albania had already attempted to change women’s roles before 1967. The communist regime introduced formal gender equality, secular education, public campaigns, and policies encouraging women’s employment and schooling (Zickel and Iwaskiw, 1994; Këlliçi and Danaaj, 2016). These policies lowered practical barriers to participation, but they did not necessarily change expectations about what other households would regard as acceptable. Albania’s industrialization strategy also increased demand for

labor outside the household, and state planning documents increasingly described women as an underused source of labor (Këlliçi and Danaj, 2016). By 1966, the government explicitly identified religion and the backward customs it sustained as main obstacles to female emancipation and industrialization (Gjeçovi, 2008; Këlliçi and Danaj, 2016). In the regime’s view, earlier reforms were insufficient: changing women’s roles also required confronting the social order that sustained them.

We formalize this mechanism in a social-equilibrium model. Female participation carries a social cost, reflecting stigma and social disapproval, when households believe that traditional roles remain socially enforced through others’ behavior and judgments. A national decree need not eliminate those beliefs. Where no protected religious monument remained standing after the reform, households may have perceived a clearer break with the old order, generating a coordinated shift in expectations about how others would behave and what they would regard as acceptable. The model predicts larger post-reform increases in female employment and schooling in these municipalities. It also shows how these changes can build gradually over time as households update those beliefs and adjust their own participation decisions accordingly.

We test these predictions using hand-transcribed archival microfilm records from the National Archives of Albania and the National Statistics Institute. We assemble a municipality-year panel covering all 26 Albanian municipalities from 1956 to 1988, with annual information on female employment, schooling, industrial and agricultural production, population, and pre-reform religious institutions. An unusually informative feature of the archival data is the availability of municipality-level targets from the central government’s 1966 Five-Year Plan for industrial production, circulation, consumption, school supply, cultural houses, and health-care beds. Set before the religion ban but covering the years that followed, these targets record each municipality’s planned trajectory and provide a municipality-specific counterfactual benchmark. For industrial production, the same accounts also report realized output, allowing us to compare each municipality’s actual performance with what had been planned for it. We use the remaining targets to test whether municipalities without a protected religious monument were targeted for faster planned expansion in broader economic activity or public-service provision.

Using a difference-in-differences design, we find substantially larger post-reform increases in female employment and schooling in municipalities where no protected religious monument remained standing (treated) than in municipalities where at least one remained (control). Female employment rose by 0.610 log points more in treated than in control municipalities. Female secondary-school enrollment rose by 0.854 log points more, while female primary-school enrollment rose by 0.191 log points more. The larger secondary-school response is consistent with the mechanism: primary schooling was compulsory, whereas secondary schooling left households greater discretion over girls’ continued education

and therefore more scope for the social enforcement of traditional gender roles. Event-study estimates show no evidence of differential pre-trends for any of the three outcomes. After 1967, the effects emerge gradually and begin to plateau, as the model predicts, suggesting adjustment toward a new equilibrium rather than a one-time level shift. Because the national ban applied everywhere, these estimates do not capture its aggregate effect; instead, they identify whether outcomes changed more where the old religious order disappeared from local public view.

The effects extend to industrial production. Treated municipalities were not assigned faster planned industrial growth in the 1966 Five-Year Plan, yet after the reform they performed better relative to those pre-set targets. Industrial production relative to plan rose by 0.196 log points more in treated than in control municipalities. Realized industrial production also increased more in treated municipalities, although the estimate is less precise. These gains do not appear to come at the expense of agriculture, which shows no robust offsetting decline.

What explains the larger post-reform response where no protected religious monument remained? In the model, the public presence of the old religious order sustains expectations that traditional gender roles remain socially enforced; its disappearance weakens those expectations and lowers the social cost of women's schooling and employment. The empirical patterns follow this logic. Post-reform increases in female employment and schooling are smallest where a protected religious monument stood in an urban area, larger where monuments survived only in rural areas, and largest where none remained. The same gradient appears in the number of protected monuments: increases are smaller where more survived. Among municipalities with a monument, outcomes do not differ systematically according to whether it matched the locally dominant faith. Even where the only monument belonged to a non-dominant faith—a church in a predominantly Muslim municipality, or a mosque in a Christian one—the estimated increases are smaller than where no monument remained. Thus, the evidence supports the model's central mechanism: the public visibility of the old religious order helped sustain the social enforcement of traditional gender roles even after formal religious authority had been abolished.

The main identification concerns are that the absence of a protected religious monument in a municipality may correlate with differential government targeting around the reform or proxy for pre-existing historical, economic, or cultural differences across municipalities. The Five-Year Plan directly addresses differential targeting: before the religion ban, treated municipalities were not assigned faster planned growth in economic activity or public-service provision. In addition, none of the repurposed religious buildings became schools, making a mechanical school-supply explanation unlikely. To assess whether treatment status instead reflects pre-existing municipal differences, we first compare pre-reform religiosity. We find that religious buildings and clerics per capita are similar across treated and control municipalities, and allowing these measures to predict differential

post-reform changes leaves the estimates largely unchanged. Protected non-religious monuments from the same 1948 classification are similar across the two groups, making it less likely that treatment status reflects pre-existing historical differences. Controlling for the interaction of their pre-reform density with the post-reform indicator leaves the estimates intact. Pre-reform state cultural infrastructure and female political representation are similarly balanced, and conditioning on their interactions with the post-reform indicator leaves the results qualitatively unchanged.

The post-reform increases are also disproportionately female: women’s shares of employment and secondary-school enrollment rise more in treated municipalities, so the results do not reflect municipality-wide growth in employment or schooling alone. The evidence provides little support for differential government targeting, pre-existing municipal differences, or municipality-wide growth as explanations for the findings.

A separate concern is convergence from lower initial levels. Treated municipalities began with lower pre-reform female employment and secondary-school enrollment, so the post-reform differences could reflect convergence rather than the reform. To address this, we control for municipality-specific growth paths extrapolated using only pre-1967 data. The estimates remain positive and similar in magnitude to the baseline. The findings are also robust to differences in sample composition and municipal size, including pre-reform population controls, population weighting, matched samples, samples excluding Tirana, and leave-one-treated-municipality-out exercises. Results are also robust to region-specific trends, alternative functional forms, randomization inference, and alternative treatment definitions.

Finally, exploratory individual-level evidence from the 2008–09 Albania Demographic and Health Survey extends the analysis to longer-run female outcomes. Differences in literacy, number of children, ideal number of children, and household power between women from treated and control municipalities are largest among cohorts born before the reform, narrow across successive cohorts, and have largely disappeared among those born around two decades after it. These cohort patterns suggest that the post-reform changes had lasting consequences for women’s literacy, fertility, and power within the household.

The paper makes three main contributions. First, we contribute to the economics of culture and social norms by empirically identifying a novel mechanism through which institutional change can alter socially enforced behavior. Existing work emphasizes the persistence of culture and norms over time and their long-run economic consequences (Banfield, 1967; Putnam, 1993; Inglehart and Baker, 2000; Fernández, 2007; Giuliano, 2007; Tabellini, 2008; Algan and Cahuc, 2010; Voigtländer and Voth, 2012; Alesina, Giuliano and Nunn, 2013). Related work models social behavior as shaped by conformity and

social sanctions (Akerlof, 1980; Bernheim, 1994; Bénabou and Tirole, 2011) and shows that social norms can erode when privately held views become publicly recognized (Bursztyn, Egorov and Fiorin, 2020). We show that the continued public visibility of the institutions associated with an old social order can help sustain social enforcement even after their formal authority has been removed. This mechanism does not require private religious belief itself to change, only expectations about whether others will continue to enforce the old order.

Second, we contribute to research on gender norms and female participation by showing that expanding women’s rights and opportunities need not eliminate the constraints created by socially enforced gender roles. Despite earlier reforms expanding women’s rights and opportunities, women’s schooling and employment increased substantially more after the religion ban where the old religious order disappeared from local public view. A large literature studies how gender roles shape women’s education and labor supply (Goldin, 1990; Becker and Woessmann, 2008; Alesina, Giuliano and Nunn, 2013; Jayachandran, 2015; Giuliano, 2020; Miho, Jarotschkin and Zhuravskaya, 2024). Related work shows that women’s employment can be constrained by beliefs about what others regard as acceptable, and that correcting those beliefs can increase participation (Bursztyn, González and Yanagizawa-Drott, 2020). Our results show that such constraints can also weaken without any direct intervention on beliefs when the institutions that sustained them disappear from public view.

Third, we contribute to the economics of religion by showing that religion can shape economic behavior through the social enforcement of gender norms, even after formal religious authority and worship have been abolished nationwide. Since Weber’s classic argument linking Protestantism to economic behavior (Weber, 1991, originally published 1905), prior work has identified several channels through which religion affects economic outcomes (Iannaccone, 1998; Guiso, Sapienza and Zingales, 2003; Botticini and Eckstein, 2005; McCleary and Barro, 2006; Ekelund, Hébert and Tollison, 2002; Gruber and Hungerman, 2008; Becker and Woessmann, 2009; Cantoni, 2015; Campante and Yanagizawa-Drott, 2015; Bénabou, Ticchi and Vindigni, 2015; Cantoni, Dittmar and Yuchtman, 2018; Squicciarini, 2020; Becker, Rubin and Woessmann, 2024). We add a distinct channel: religion can matter not only through belief, practice, human capital, or formal institutions, but also through the social authority sustained by the old religious order’s continued visibility in public life.

Although Albania’s 1967 religion ban is an extreme setting, the underlying mechanism is broader and not specific to religion. When behavior depends on beliefs about what others will do or regard as acceptable, a norm may survive the loss of formal authority if the institutions or symbols that once sustained it remain visible in local public life. When those visible reminders

disappear, households may instead come to believe that the old norm has lost its force. Albania provides unusually clean evidence on this mechanism because religious authority was abolished nationwide, while the local visibility of the old religious order varied. The same logic may apply wherever formal rules change faster than the public markers of the old order, as when symbols of a former regime or monuments to past hierarchies remain in place after legal reform.

The remainder of the paper is organized as follows. Section I provides historical background, and Section II develops the model and derives its empirical predictions. Sections III and IV describe the data and empirical strategy. Section V presents the main results and evidence on the mechanism, and Section VI examines alternative explanations and robustness. Section VII considers interpretation, longer-run outcomes, and wider implications. Section VIII concludes.

I. Background: Religion, Communism, and the Visible Break with the Old Order

A. The Inherited Order and the Economic Case for Change

Albania was a multi-faith society when the Communist Party came to power in 1944 (Zickel and Iwaskiw, 1994). In 1946, the population was religiously diverse: 68.9 percent was Muslim, 20.7 percent Orthodox, and 10.3 percent Catholic. This diversity also had a regional dimension, with Muslim communities concentrated mainly in the center, Catholic communities in the north, and Orthodox communities in the south. Yet, the social norms associated with these religions were similarly gendered across communities. Evidence from the 1930s describes women’s public roles as constrained by household-centered expectations, patriarchal family authority, and customary norms that tied women to marriage and domestic family life (Plasari, 1930). In both Muslim and Christian communities, marriage was associated with a household-centered role for women, while men held authority over public and economic life.¹ For the communist state, the inherited order was a gender order as well as a religious one.

The state had strong economic reasons to challenge that order. Albania prioritized industrialization and sought to reduce dependence on agriculture; industrial production tripled between 1950 and 1955 (Këlliçi and Danaj, 2016). By the early 1960s, the regime’s industrialization strategy had intensified demand for labor outside private household production. State planning documents increasingly described women as an underused source of labor needed for the next phase of industrialization (Këlliçi and Danaj, 2016).² Thus, female employment was not only an ideological objective, but also an economic one.

¹This association between women and the domestic sphere was not unique to Islam or to Albania. Catholic social teaching also articulated a household-centered role for women: in *Rerum Novarum*, Leo XIII wrote that “a woman is by nature fitted for home-work” (Leo XIII, 1891, para. 42).

²Këlliçi and Danaj (2016) cite AQSH 14 AP/str, KQPPSH, D.2, V.1963, Fl. 9, p. 1, and AQSH, Fond 496, K.M., D.270, V.1967, pp. 92, 106, 120, and 128.

B. Gender Reform Before 1967

The regime had already attempted to change women’s roles before the religion ban. The 1946 Constitution guaranteed freedom of worship, secularized education, and declared equal rights for men and women. The government subsequently pursued more direct measures to increase women’s participation in public life such as propaganda, expanded schooling and childcare provision, and campaigns encouraging women into education and employment. These policies were part of a broader socialist emancipation agenda in which female employment was presented both as liberation and as the mobilization of a necessary productive labor force (Këllici and Danaj, 2016). Yet the regime itself treated these measures as incomplete. By the mid-1960s, female employment and post-compulsory schooling remained below the state’s objectives (Këllici and Danaj, 2016). Formal political life was still male-dominated: in 1966, women held about 16 percent of seats in the People’s Assembly. This gap between policy effort and behavioral change is central to interpreting the 1967 reform. Schooling and employment opportunities could lower practical barriers to participation, but they did not necessarily change what households believed others would regard as acceptable. If traditional gender roles, reinforced by customary and religious authority, remained socially enforced, a household that broke with them alone could still face social costs. The 1967 campaign mattered not only because it changed formal law, but because the state publicly attacked the religious institutions and symbols that had helped sustain the old social order.

C. The 1967 National Religion Ban

The shift from conventional reform to an anti-religious campaign became explicit at the beginning of 1967. The government identified religion and its “backward customs” as obstacles to female emancipation and industrialization. The state treated religion not only as private belief, but as part of a social order that sustained women’s household-centered role (Gjeçovi, 2008; Këllici and Danaj, 2016). That year, the regime outlawed religious practice and proclaimed Albania the world’s first atheist state. Religious institutions lost legal standing under Decree No. 4337, and all religious buildings were closed for worship.³ The reform also transformed the religious built environment. Most religious buildings were demolished or de-sanctified and converted to secular use. This removed the old religious order from local public view in many municipalities, even though a small number of protected religious monuments remained physically intact (Bickert, 2018). Figure 1 illustrates this transformation using the Mosque of Durrës, which was repurposed as *Pallati i Rinisë* (the Palace of Youth) after the ban. The example shows how secular re-purposing could alter both the function and public meaning of a formerly religious building.

³The campaign was highly coercive. Clergy who did not comply with the ban were persecuted, imprisoned, sent to labor camps, and in some cases killed.

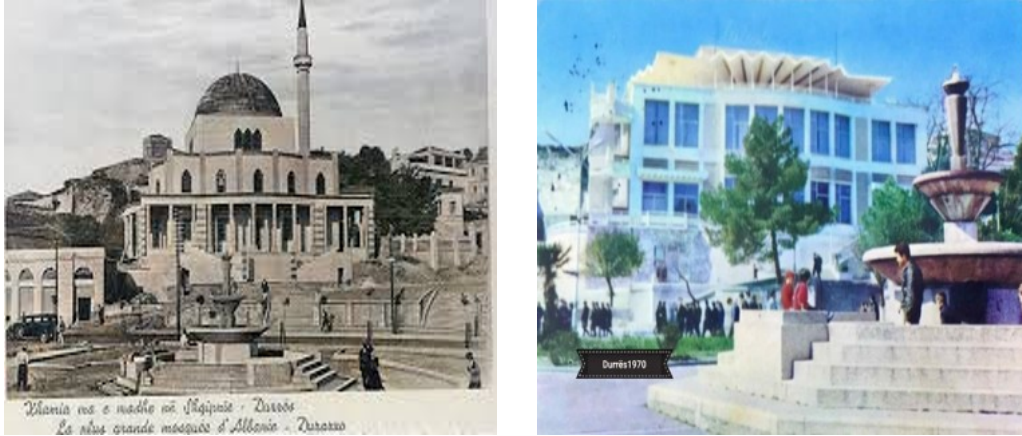


FIGURE 1. RELIGIOUS BUILDINGS AND THE 1967 RELIGION BAN

Note: The left panel shows the Mosque of Durrës before the 1967 religion ban. The right panel shows the same site after the mosque was de-sanctified and repurposed as *Pallati i Rinisë* (the Palace of Youth).

The 1967 reform combined two margins that are usually difficult to separate. The first was national legal suppression: religious practice and religious institutions were prohibited throughout Albania. The second was local public visibility: municipalities differed in whether the old religious order remained visible in local public life. The next subsection describes the pre-existing heritage classification that generated this local variation.

D. Protected Monuments and the Source of Variation

Local variation in monument removal came from a pre-existing heritage classification. Albania's first national list of protected monuments was compiled in 1948 by the Institute of Sciences, the country's principal scientific institution at the time, nearly two decades before the religion ban. The list was a general heritage inventory covering monuments across municipalities, including fortifications and ancient settlements, churches, mosques, tekkes, as well as bridges and other historic structures. Monuments were identified and evaluated by scholars and heritage experts within the Institute of Sciences, rather than by municipal economic planners or officials responsible for the later anti-religious campaign. The classification was based on historical, architectural, and cultural significance.

We do not require the heritage classification to have been random; its timing, the Institute's role in preserving cultural heritage, and the classification's stated preservation criteria make it unlikely that monuments were selected in anticipation of municipality-level responses to the 1967 religion ban. Of the 2,169 religious buildings closed at the time of the reform (Bickert, 2018), 130, or about 6 percent, were protected as cultural monuments. We identify these protected religious

monuments from official monument lists.⁴

These protected buildings were closed for worship, like all religious buildings, but remained physically intact in the municipality. Thus, pre-reform protection status, rather than contemporaneous local discretion, governed whether a religious monument remained standing after the reform. In treated municipalities, no protected religious monument remained standing after the reform. In control municipalities, at least one protected religious monument remained standing (Figure 2). Section II formalizes this distinction and derives the empirical predictions tested below.

⁴The monument inventory is constructed from the official county-level lists of Albanian historical and cultural monuments published by the Instituti Kombëtar i Trashëgimisë Kulturore (National Institute of Cultural Heritage). These inventories record both the pre-reform protection decisions and subsequent protection decisions for individual monuments. The protected religious monuments identified before the reform continue to appear in the state monument inventories after the 1967 campaign.

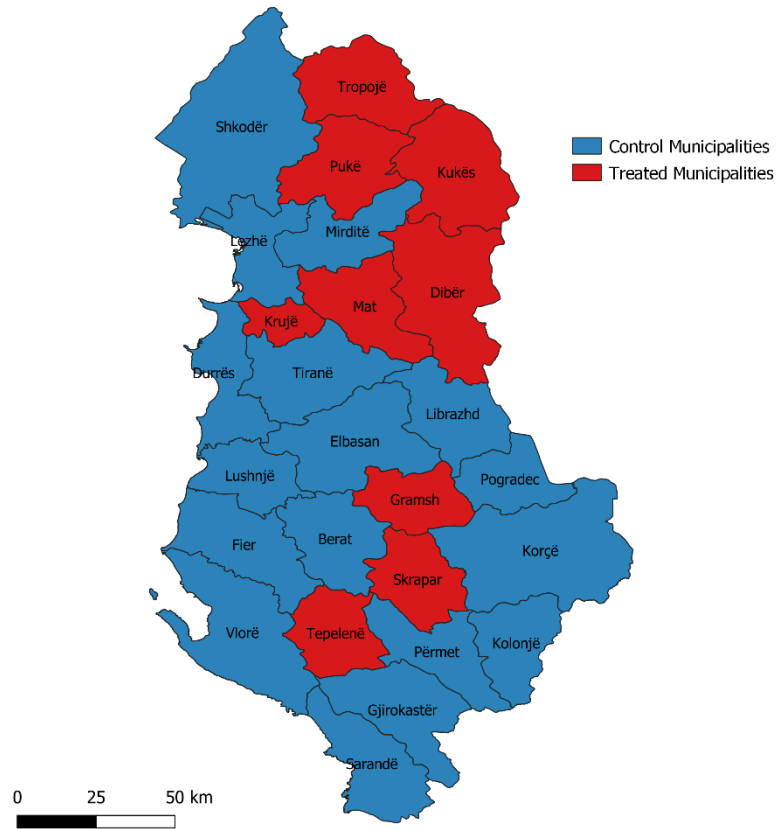


FIGURE 2. ALBANIA—TREATMENT AND CONTROL MUNICIPALITIES

Note: Treated municipalities are those in which no protected religious monument remained standing after the 1967 religion ban. Control municipalities are those in which at least one protected religious monument remained standing. Religious practice was banned nationwide in both groups; the map captures local variation in whether any protected religious monument remained physically intact.

II. A Model of Visible Removal and Social Equilibrium Change

This section formalizes a mechanism through which the local disappearance of the old religious order can affect women’s public participation. Albania’s 1967 religion ban applied nationwide, but whether the old order remained publicly visible—through a surviving protected monument—varied across municipalities. A household’s participation decision depends not only on private incentives but also on expectations about what others will do and what they will regard as acceptable. Variation in the local public visibility of the old order can shift those expectations.

A. Setup

There is a continuum of households of measure one, indexed by $i \in [0, 1]$. Each household decides whether the woman in the household remains in the traditional household role or participates in public life. Let $a_i \in \{H, W\}$, where H denotes the household role and W denotes participation in work or schooling. Aggregate female participation is:

$$x = \int_0^1 \mathbf{1}\{a_i = W\} di.$$

Household i receives a private benefit θ_i from participation, drawn from a continuously differentiable cumulative distribution F with density f , where $f > 0$ over the relevant range. Participation requires a private cost $c > 0$ ⁵ and a social cost $s(x, V, \tau)$, which captures community stigma and is continuously differentiable in x . The variable $\tau \in \{0, 1\}$ denotes the reform period, with $\tau = 0$ before the 1967 religion ban and $\tau = 1$ from 1967 onward. Let $V \in \{0, 1\}$ denote the public visibility of the old religious order, with $V = 1$ denoting that it remains publicly visible and $V = 0$ its local disappearance. The variable $R \in \{0, 1\}$ is a fixed municipality characteristic determined by the pre-reform heritage classification: $R = 1$ if at least one protected religious monument remained publicly visible after the reform, and $R = 0$ if none remained. We refer to $R = 0$ as complete local removal and to $R = 1$ as continued monument visibility.⁶

⁵The main text holds c fixed to isolate the social-cost channel. Appendix A allows for a time-invariant municipality-specific component in private participation costs, which permits municipalities to have different pre-reform participation levels without changing the comparative static with respect to public visibility. The reform may also have affected private costs or returns, but the model abstracts from those channels. Any nationwide component would be common across municipalities and could not by itself generate the differential response between municipalities with and without a surviving protected religious monument.

⁶The model treats visibility as binary. A natural extension would allow it to vary in degree, with social costs increasing in visibility, yielding a monotone analogue of the binary comparative static. Section V.C examines this using the location and number of surviving monuments, as well as alternative removal thresholds.

Public visibility is then given by:

$$V(R, \tau) = \begin{cases} 1, & \tau = 0, \\ R, & \tau = 1. \end{cases}$$

Before the reform, the old religious order was publicly visible in all municipalities. After the reform, it remained publicly visible only in municipalities with at least one surviving protected religious monument.

The social cost satisfies

$$\begin{aligned} s_x(x, V, \tau) &< 0 && \text{for all } x \in [0, 1], V \in \{0, 1\}, \tau \in \{0, 1\}, \\ s(x, 1, 1) &< s(x, 1, 0) && \text{for all } x \in [0, 1], \\ s(x, 0, 1) &< s(x, 1, 1) && \text{for all } x \in [0, 1]. \end{aligned}$$

The first restriction captures strategic complementarity: the social cost of female participation falls as participation becomes more common. The second captures a nationwide post-reform reduction in the social cost of participation even where a monument survived. The third is the local public visibility channel: after the reform, the social cost is lower where no monument survived.

Household i participates if $\theta_i \geq c + s(x, V(R, \tau), \tau)$. For a given R and τ , equilibrium participation $x^*(R, \tau)$ satisfies:

$$(1) \quad x^*(R, \tau) = 1 - F(c + s(x^*(R, \tau), V(R, \tau), \tau)).$$

Define the participation response as:

$$G(x; R, \tau) \equiv 1 - F(c + s(x, V(R, \tau), \tau)).$$

Because G is continuous and maps $[0, 1]$ into itself, the intermediate value theorem implies that at least one equilibrium exists. The slope of G with respect to x is:

$$-s_x(x, V, \tau)f(c + s(x, V, \tau)) > 0.$$

The positive slope implies that participation choices are strategic complements. If the complementarity is sufficiently strong, so that $-s_x(x, V, \tau)f(c + s(x, V, \tau)) > 1$ for some x , the model can generate multiple equilibria. Under the adjustment process below, an equilibrium is locally stable when the slope of G at that fixed point is less than one. Figure 3, Panel A illustrates this case.

B. Public Visibility and Participation Change

Consider a given municipality after the reform under two counterfactual states of public visibility: one in which at least one protected religious monument remains publicly visible, $R = 1$, and one in which no protected religious monument remains publicly visible, $R = 0$. Holding the national reform period

fixed at $\tau = 1$, we compare the post-reform participation response under complete local removal with the response under continued monument visibility. Since $s(x, 0, 1) < s(x, 1, 1)$ for all $x \in [0, 1]$, it follows that

$$1 - F(c + s(x, 0, 1)) > 1 - F(c + s(x, 1, 1))$$

for all $x \in [0, 1]$. Complete local removal shifts the participation response upward. If the shift is large enough, the low-participation equilibrium can disappear and participation can move toward a higher-participation outcome, as shown in Figure 3, Panel B.

Because $V(R, 0) = 1$ in either counterfactual state of public visibility, let x^{pre} denote the municipality's own pre-reform participation level and compare the two post-reform paths beginning from that same initial condition. Let k index successive stages of adjustment. A simple adjustment path is:

$$(2) \quad x_{k+1}(R) = 1 - F(c + s(x_k(R), V(R, 1), 1)), \quad x_0(R) = x^{\text{pre}}.$$

The post-reform participation response under complete local removal lies everywhere above the response under continued monument visibility. Appendix A shows that the adjustment paths converge and that, from the municipality's own pre-reform participation level, the long-run participation level is strictly higher under complete local removal. This adjustment process permits a gradual rather than instantaneous response.

Let $x^{\text{post}}(0)$ denote this long-run participation level under complete local removal and $x^{\text{post}}(1)$ the counterfactual long-run participation level under continued monument visibility. The model implies:

$$(3) \quad \Delta^M = x^{\text{post}}(0) - x^{\text{post}}(1) > 0.$$

Since $No\ Monuments = 1 - R$, the model predicts a positive coefficient on $No\ Monuments \times Post$ for female participation outcomes. The empirical difference-in-differences design uses changes in control municipalities to estimate the counterfactual post-reform change for treated municipalities under the parallel-trends assumption.

The model does not require any change in private religious belief. It requires only that complete local removal change beliefs about what other households will do and what they will regard as acceptable. The disappearance of the old order and any signal of state commitment embedded in that act would operate through the same channel: a weakening of perceived social authority and social enforcement.

C. Empirical Predictions

The model yields two main empirical predictions.

First, municipalities in which no protected religious monument remained pub-

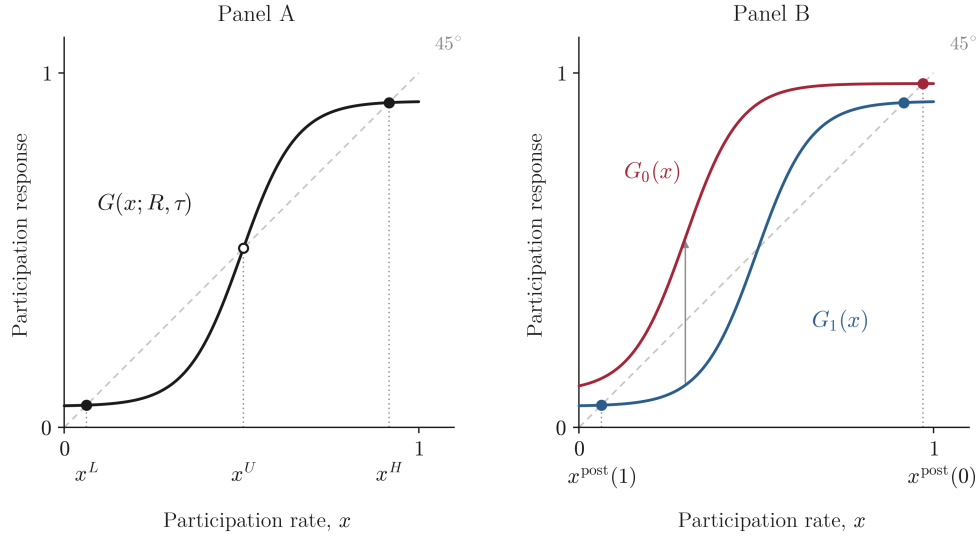


FIGURE 3. PUBLIC VISIBILITY AND PARTICIPATION CHANGE

Note: $G(x; R, \tau) = 1 - F(c + s(x, V(R, \tau), \tau))$ is the participation response; equilibria are intersections with the 45-degree line. Panel A illustrates multiple equilibria under sufficiently strong strategic complementarity. Filled markers denote the locally stable equilibria x^L and x^H , and the open marker denotes the unstable equilibrium x^U . Panel B compares the same municipality after the reform under the two counterfactual states of public visibility. The blue curve, $G_1(x) = 1 - F(c + s(x, 1, 1))$, corresponds to continued monument visibility, while the red curve, $G_0(x) = 1 - F(c + s(x, 0, 1))$, corresponds to complete local removal. Because $s(x, 0, 1) < s(x, 1, 1)$, $G_0(x) > G_1(x)$ for all x . Under continued monument visibility, the adjustment process beginning from the low pre-reform participation level selects $x^{\text{post}}(1)$; G_1 also admits a high stable equilibrium. In the configuration shown, complete local removal eliminates the low equilibrium and the same initial condition instead leads to the higher outcome $x^{\text{post}}(0)$. The private cost c is held fixed, and the curves are illustrative rather than calibrated.

licly visible should experience larger post-reform increases in female participation. We examine this prediction using female employment and female school enrollment. The mechanism should be most informative on margins over which households retain meaningful discretion, since the size of the participation response depends on how many households are close to the participation threshold.⁷ In Albania, secondary schooling provides a particularly informative margin because primary schooling was compulsory, whereas households retained greater discretion over girls’ continued education.

Second, the differential participation response need not occur immediately. Under the adjustment process, participation can build gradually as higher participation reduces social costs and induces additional households to participate. Because the adjustment paths converge to fixed points, these adjustments eventually diminish and the response stabilizes.

A further implication concerns the identity of surviving monuments. Because the model indexes public visibility by whether any protected religious monument remains publicly visible, rather than by its denomination, it does not predict a separate effect depending on whether the surviving monument matches the municipality’s locally dominant faith. Section V.C tests this implication.

More generally, the mechanism applies when an old social order remains publicly visible, behavior depends on beliefs about what others regard as acceptable, and changes in that visibility are widely observed within the local community. Section IV presents the corresponding empirical specifications.

III. Data and Descriptive Evidence

We assemble multiple municipality-level datasets on Albania before and after the 1967 religion ban using archival and administrative sources. The administrative panel is the core dataset, covering female employment, female primary and secondary school enrollment, and industrial production over time. We complement these data with information on pre-reform religious institutions and religious composition, official monument inventories used to construct the treatment measure, and records from the State Planning Commission’s Fourth Five-Year Plan. The planning records provide a rare opportunity to observe directly the state’s counterfactual expectations about municipal growth. The Commission’s planning files report municipality-level targets for 1967 and 1970.⁸ These targets were assigned before the reform and allow us to test whether treated municipalities were expected to grow faster and to compare realized post-reform production with

⁷Let $G_0(x) = 1 - F(c + s(x, 0, 1))$ and $G_1(x) = 1 - F(c + s(x, 1, 1))$ denote the two post-reform participation responses. By the mean value theorem, $G_0(x) - G_1(x) = f(\xi)[s(x, 1, 1) - s(x, 0, 1)] > 0$ for some ξ between $c + s(x, 0, 1)$ and $c + s(x, 1, 1)$. Thus, a given reduction in social cost induced by lower public visibility generates a larger response when more households lie near the participation threshold. This provides intuition about the relative informativeness of discretionary margins; the model does not impose a formal ranking across the empirical outcomes.

⁸The archival cover identifies the dossier as State Planning Commission file no. 74, marked “Secret,” for the Fourth Five-Year Plan, 1966–1970.

those targets.

A. Administrative Records, Religion, and Monuments

The main administrative data were hand-transcribed from microfilm records held in the National Archives of Albania and the National Statistics Institute. These records were classified as “Secret” during the communist period and were used by the state to monitor local population, employment, schooling, and production outcomes. We use them to construct a municipality-level panel covering Albania’s 26 municipalities from 1956 to 1988. The main outcomes are female employment, female primary and secondary school enrollment,⁹ and industrial production.

The records also contain detailed information on religion before the ban. For each municipality, we collected information on the number of religious buildings, the religious affiliation of those buildings, the number of clerics, and the religious composition of the population in 1946. These measures allow us to characterize pre-reform religiosity and to test whether municipalities with and without a surviving protected monument differed in religiosity before the reform.

We complement these records with official monument inventories from Albania’s cultural-heritage preservation authorities, which identify protected religious buildings and their status after the reform. At the time of the reform, Albania had 2,169 religious buildings. A smaller set of 130 buildings, approximately 6 percent, remained standing because they had been designated as protected cultural monuments under the heritage-preservation framework established in 1948. These buildings were closed for worship and stripped of their sacred function, but remained physically intact and visible in the municipality.

Buildings without protected status were either physically destroyed or desanctified and repurposed for secular civilian use, mainly as warehouses, but also as sports halls, or youth centers. None were converted to schools or to industrial or production use. Thus, the protected-monument designation generated the post-reform variation used in the empirical analysis: after religious worship was banned everywhere, some municipalities still contained a visible protected religious monument, while others did not.

Treated municipalities are those in which no protected religious monument remained standing after the reform. Control municipalities are those in which at least one remained (Figure 2). Treatment captures whether a protected religious monument remained standing in the municipality after the reform, rather than exposure to the national ban itself.

⁹Basic education was compulsory throughout this period (World Bank Group, 1995), while secondary education remained non-compulsory. In practice, enforcement of compulsory schooling was imperfect, particularly for girls, leaving some scope for household discretion despite the legal requirement (Miço, 2020).

B. State Planning Data

The State Planning Commission data are central to the empirical design. We supplement the administrative panel with municipality-level records from the Fourth Five-Year Plan (1966–1970), prepared before the religion ban and ratified at the Fifth Party Congress. The planning records report pre-reform values and planned targets across a broad set of outcomes, as well as realized industrial output. Table 1 reports pre-reform values for 1965–66 alongside the planned target for industrial production, while Appendix Figure A2 plots the full set of pre-reform values and subsequent plan targets. The plan reports official targets for industrial production, circulation, and consumption for 1967 and 1970.¹⁰ The planning records also contain targets for school supply and for cultural houses and cultural hearths (*shtëpi dhe vatra kulture*), which were local state institutions used for cultural programming and political education, as well as health-care beds, which capture local public-service capacity.

Because the plan was set before the religion ban, it records the state’s expectations about subsequent municipal growth. A central concern for the difference-in-differences design is that municipalities without surviving protected monuments may already have been expected to grow faster for reasons unrelated to the reform, such as planned capital investment, infrastructure, or industrial capacity. In most settings, these counterfactual expectations are unobserved. In Albania’s command economy, they were recorded explicitly in the state’s planning records.

We use the planning data in two ways. First, we test whether municipalities without surviving protected monuments were assigned faster planned expansion in economic activity or public-service provision under the Five-Year Plan. Second, for industrial production, we compare realized production with official targets and measure performance relative to plan. We also use agricultural production data to assess whether the industrial-production result reflects general municipal growth or sectoral reallocation.

C. Pre-Reform Characteristics and Descriptive Trends

Table 1 reports pre-reform municipality characteristics by treatment status.

¹⁰Circulation measures income from the sale of goods and services in each municipality, while consumption measures spending on consumable goods.

TABLE 1—PRE-REFORM MUNICIPAL CHARACTERISTICS BY TREATMENT STATUS

	Control Municipalities		Treated Municipalities	
	Mean	Standard Deviation	Mean	Standard Deviation
Totals				
Population 1946 (000s)	51.96	26.09	26.53	13.95
Ln Employment	7.40	1.36	5.64	0.68
Ln Primary School Enrollment	8.53	0.81	7.77	0.5
Ln Female Secondary Enrollment	5.39	1.73	3.24	0.86
Ln Industrial Production (MLek)	4.05	1.48	2.50	1.11
5-Year Plan in 1965-66				
<i>Financial (MLek)</i>				
Ln Planned Industrial Production	4.86	1.22	3.44	0.93
Ln Circulation	4.54	0.95	3.43	0.56
Ln Consumption	3.59	0.65	3.17	0.58
<i>Infrastructure (per 1,000)</i>				
Schools	2.10	1.16	2.90	1.16
Cultural Houses	1.16	0.60	1.44	0.58
Health Beds	5.08	3.88	5.08	6.00
Religiosity in 1946 per 1,000				
Religious Buildings	2.34	1.90	2.15	1.10
Clerics	1.56	0.74	1.49	0.58
Protected Religious Monuments	0.15	0.14	0.00	0.00
Protected Non-Religious Monuments	0.28	0.3	0.19	0.23
Pre-Reform Politics per 1,000				
Female Deputies	0.04	0.02	0.04	0.01
No. of Municipalities	17		9	

Note: The table reports municipality-level means and standard deviations separately for control and treated municipalities. Treated municipalities are those in which no protected religious monument remained standing after the 1967 reform. Control municipalities are those in which at least one protected religious monument remained standing. Religious-building and cleric measures are measured before the reform and reported per 1,000 inhabitants. Cultural houses, health-care beds and female deputies are also measured before the reform and reported per 1,000 inhabitants. The Five-Year Plan variables report pre-reform municipality-level values for 1965–66 and, for industrial production, the corresponding planned target; MLek denotes million lek. The row on protected religious monuments reports the number of buildings that had received protected heritage status before the reform. This status determined whether a protected religious monument remained standing after the reform, so the row defines the source of treatment variation rather than a pre-reform balance condition.

Treated municipalities were smaller and had lower baseline levels of female employment, female secondary school enrollment, and industrial production. The Five-Year Plan variables do not indicate that treated municipalities were favored in the state’s pre-reform plans. Planned industrial production is lower in treated municipalities, consistent with their smaller size and lower baseline production. This descriptive evidence provides no indication that treated municipalities were selected for faster planned growth; Section VI.A tests this directly using planned growth in industrial production, circulation, and consumption.

The table also reports pre-reform measures of religiosity, historical heritage, state cultural infrastructure, and female political representation. Religious buildings and clerics per capita capture pre-reform religiosity. Treated and control municipalities had comparable values on both measures. Protected non-religious monuments per 1,000 inhabitants were also similar across groups, averaging 0.28 in control municipalities and 0.19 in treated municipalities. Cultural houses and cultural hearths per capita capture the local density of state cultural infrastructure and were also similar across treated and control municipalities. Female deputies per 1,000 inhabitants capture pre-reform female political representation; this measure was nearly identical across groups, averaging 0.04 in both treated and control municipalities. We use these pre-reform measures below to test whether the main results reflect differences in pre-reform religiosity, historical heritage, state cultural presence, or female political representation rather than the post-reform disappearance of protected religious monuments from public view.

Figure 4 plots raw annual indices for treated and control municipalities for three female outcomes: female employment, female primary school enrollment, and female secondary school enrollment. The series are indexed to 1966 or the last pre-reform year with available data, and the vertical line marks the 1967 religion ban. In all three panels, treated and control municipalities move closely together before the reform. After the religion ban, the series diverge, with treated municipalities showing larger increases in all three outcomes.

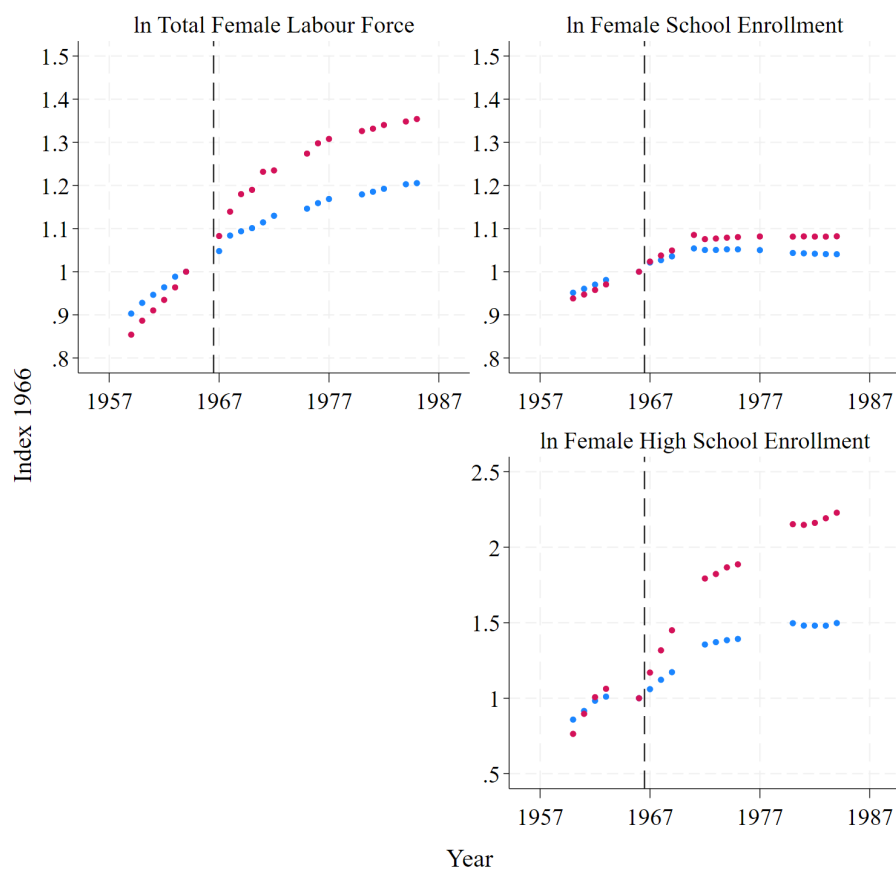


FIGURE 4. FEMALE EMPLOYMENT AND SCHOOL ENROLLMENT

Note: Panels plot annual indices for female employment, female primary school enrollment, and female secondary school enrollment separately for treated and control municipalities. Each series is indexed to 1, with 1966—or the last pre-reform year with available data—as the base year. The vertical dashed line marks 1967, the year of the religion ban. Red series correspond to treated municipalities; blue series correspond to control municipalities. Raw data are from the National Archives of Albania and the National Statistics Institute, 1956–1988.

Figure 5 plots corresponding series for planned industrial production, realized industrial production, and agricultural production.

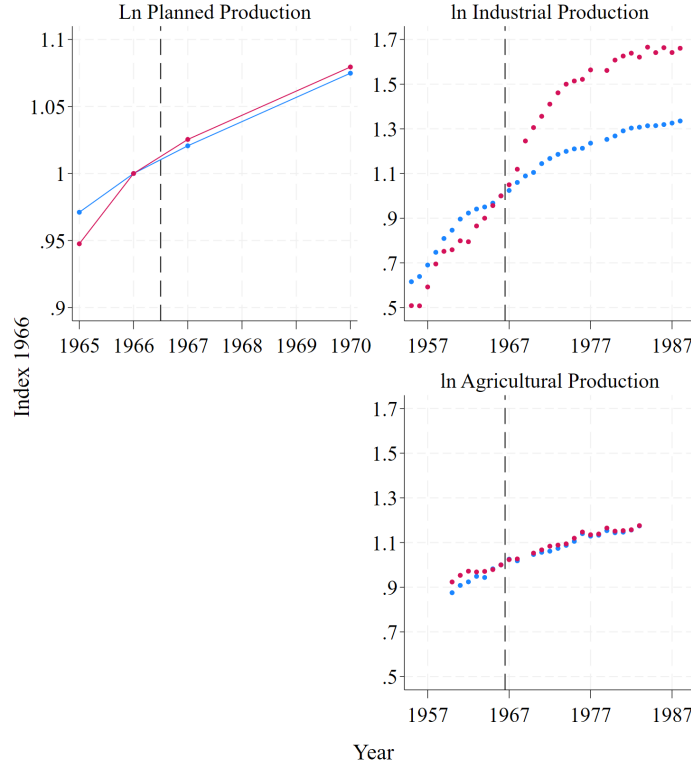


FIGURE 5. INDUSTRIAL PLANS, PRODUCTION, AND AGRICULTURE

Note: Panels plot annual indices for planned industrial production, realized industrial production, and agricultural production separately for treated and control municipalities. Each series is indexed to 1, with 1966—or the last pre-reform year with available data—as the base year. The vertical dashed line marks 1967, the year of the religion ban. Red series correspond to treated municipalities; blue series correspond to control municipalities. Planned production is observed only for the Five-Year Plan years; connecting lines are for visual clarity and do not represent annual observations. Raw data are from the National Archives of Albania, the National Statistics Institute, and the State Planning Commission records.

Planned industrial production moves similarly for treated and control municipalities over the Five-Year Plan period, consistent with treated municipalities not being assigned faster planned industrial growth before the religion ban. By contrast, realized industrial production diverges after 1967, with treated municipalities showing larger increases. Agricultural production shows no comparable divergence: treated and control municipalities track each other closely before and after the reform.

IV. Empirical Strategy

We use a municipality-level difference-in-differences design as our main empirical strategy. As described in Section I, our identification exploits the pre-reform classification of religious buildings as protected cultural monuments under the heritage-preservation framework established in 1948, almost two decades before the 1967 religion ban. Treated municipalities are the nine municipalities in which no protected religious monument remained standing after the reform; control municipalities are the seventeen municipalities in which at least one protected religious monument remained standing (Figure 2).

We estimate equations of the form:

$$(4) \quad Y_{ct} = \beta(\text{NoMonuments}_c \times \text{Post}_t) + \gamma X_{ct} + \delta_t + \mu_c + \varepsilon_{ct}.$$

where Y_{ct} denotes the outcome in municipality c and year t ; NoMonuments_c equals one for municipalities in which no protected religious monument remained standing after the reform and zero otherwise; Post_t equals one for years after 1966. The specification includes year fixed effects δ_t , municipality fixed effects μ_c , and X_{ct} , log municipal population. The coefficient of interest, β , measures the average post-reform change in outcomes in municipalities where no protected religious monument remained standing, relative to municipalities where at least one did.

For the female outcomes, Y_{ct} is the log number of employed women or of female pupils enrolled in primary or secondary school. Because control municipalities tend to be larger than treated municipalities, our main specifications control for log municipal population. We also report estimates for the female share of employment and schooling as complementary evidence on whether women’s participation increased relative to the corresponding municipal totals.

The Five-Year Plan provides an additional and unusually direct source of evidence. We estimate equation (4) using planned industrial production to test whether treated municipalities were assigned faster planned growth. We then use production relative to plan, defined as log realized industrial production minus log planned industrial production in the Five-Year Plan years, to test whether treated municipalities performed better relative to the targets set before the reform. Because these targets were fixed before the religion ban, they provide the state’s own municipality-specific expectations about subsequent industrial growth.

To examine the mechanism, we estimate variants of equation (4) that exploit differences in how much of the old religious order remained present in local public life after the reform. We first distinguish municipalities in which a protected religious monument survived in an urban area, survived only in rural areas, or did not survive at all. Monuments surviving only in rural areas were encountered by a smaller share of the municipality’s population than monuments surviving in urban areas. If public visibility is the mechanism, post-reform increases in female employment and schooling should be smallest where an urban monument remained, larger

where monuments survived only in rural areas, and largest where none remained. We complement this location-based comparison by examining whether the effects vary with the number of surviving protected religious monuments and with the share of religious buildings destroyed or de-sanctified. Finally, we test whether post-reform changes depend on the denomination of the surviving monument, which helps distinguish public visibility from denomination-specific attachment.

Standard errors are clustered at the municipality level. Given the small number of municipalities—26 in total, of which nine are treated—we supplement conventional clustered inference with wild-cluster bootstrap and randomization-inference p-values for the main female outcomes. The randomization-inference p-values are two-sided and based on 5,000 placebo reassignments of treatment across municipalities, preserving the observed number of treated municipalities.

To test for differential pre-trends and examine the timing of the treatment effects, we use the following event-study specification:

$$(5) \quad Y_{ct} = \sum_{\substack{\tau=-10 \\ \tau \neq 0}}^{22} \beta_{\tau} \mathbf{1}\{t - 1966 = \tau\} \text{NoMonuments}_c + \gamma X_{ct} + \delta_t + \mu_c + \varepsilon_{ct},$$

where τ indexes years relative to 1966, the omitted reference year. The event window extends up to ten years before and twenty-two years after 1966, depending on data availability for each outcome. We report a joint F-test of the pre-reform coefficients β_{τ} for each outcome to test whether treated and control municipalities were already diverging before the reform.

The identifying assumption is that, in the absence of differences in the post-reform public visibility of the old religious order, treated and control municipalities would not have experienced differential changes in the outcomes. The historical source of treatment helps motivate this assumption: treatment status was generated by heritage classifications dating back to 1948, rather than by contemporaneous municipal choices or differential exposure to the national religion ban.

The remaining concerns are that heritage classification may be correlated with pre-existing local demand, institutions, or social structures; that treated municipalities, which were smaller and had lower baseline outcome levels, may have been converging toward control municipalities for reasons unrelated to the reform; or that treated municipalities may have received greater state investment after the reform that was not reflected in the pre-reform plans. Section VI addresses these concerns. In addition to industrial production, we use planned growth in circulation and consumption, together with planned expansion in school supply, cultural houses and cultural hearths, and health-care beds, to test whether treated municipalities were targeted for faster economic or public-service development. We also examine pre-reform religiosity, protected non-religious monuments, state cultural infrastructure, and female political representation; allow baseline population to predict differential time paths and regions to follow separate linear trends; use

non-industrial labor and agricultural production to test for sectoral reallocation; and assess robustness to municipality size, population weighting, Poisson pseudo-maximum likelihood estimation, matching, and leave-one-treated-municipality-out checks. As an additional check on convergence, we estimate specifications that control for extrapolated pre-reform outcome trajectories, constructed from linear trends fitted separately for each municipality and outcome using only observations before 1967.

V. Results

A. Female Employment and School Enrollment

Table 2 reports difference-in-differences estimates for female employment and school enrollment.

TABLE 2—FEMALE EMPLOYMENT AND FEMALE SCHOOL ENROLLMENT

	(1) Fixed Effects	(2) Population Controls
Panel A: Employment		
No Monuments $\times$ Post	0.648	0.610
Standard Error	(0.117)	(0.123)
<i>p</i> -value	0.000	0.000
Observations	513	513
Panel B: Secondary School Enrollment		
No Monuments $\times$ Post	0.931	0.854
Standard Error	(0.222)	(0.249)
<i>p</i> -value	0.000	0.002
Observations	439	439
Panel C: Primary School Enrollment		
No Monuments $\times$ Post	0.265	0.191
Standard Error	(0.070)	(0.067)
<i>p</i> -value	0.001	0.008
Observations	494	494
Municipality and Year FE	✓	✓
Log Population		✓

Note: The table reports difference-in-differences estimates from equation (4). The dependent variables are log female employment, log female secondary-school enrollment, and log female primary-school enrollment. The reported coefficient is on No Monuments $\times$ Post. No Monuments equals one for municipalities in which no protected religious monument remained standing after the reform, and zero otherwise. Post equals one from 1967 onward. All specifications include municipality and year fixed effects. Column 2 adds log municipal population. Standard errors clustered by municipality are reported in parentheses, with *p*-values below.

Female employment increases more in treated municipalities after the reform: the preferred coefficient in Panel A, Column 2, is 0.610 log points. The coefficient remains positive and statistically significant when we allow for region-specific linear trends (Appendix Table A1).¹¹

¹¹We report region-specific linear trends as a robustness check rather than in the baseline because the treatment effects build gradually over time (Figure 6), and such trends estimated over the full panel may absorb part of the post-reform response (Wolfers, 2006).

Female schooling also increases more in treated municipalities, with a considerably larger response at the secondary level: the preferred coefficient in Column 2 is 0.854 log points for secondary-school enrollment (Panel B), compared with 0.191 log points for primary-school enrollment (Panel C). Primary schooling was compulsory during this period, whereas secondary schooling was not and thus left households more discretion over girls' continued education, which may explain the larger secondary-school response.

Figure 6 reports the corresponding event-study estimates. The pre-reform coefficients allow us to test whether treated and control municipalities were already on different paths before 1967. For female employment, these coefficients are small and neither individually nor jointly significant.¹² After 1967, the coefficients increase gradually and then stabilize at a higher level, as Prediction 2 of the model implies, rather than shifting once at the reform.

The school enrollment event studies likewise show no differential pre-reform trends: the pre-reform coefficients are not jointly significant for either outcome. After the reform, secondary-school enrollment increases in treated municipalities relative to control municipalities and continues to rise through much of the post-reform period, although the increase slows toward the end of the sample. Primary-school enrollment also increases, but by less.

¹² $F(5, 25) = 1.62, p = 0.191$.

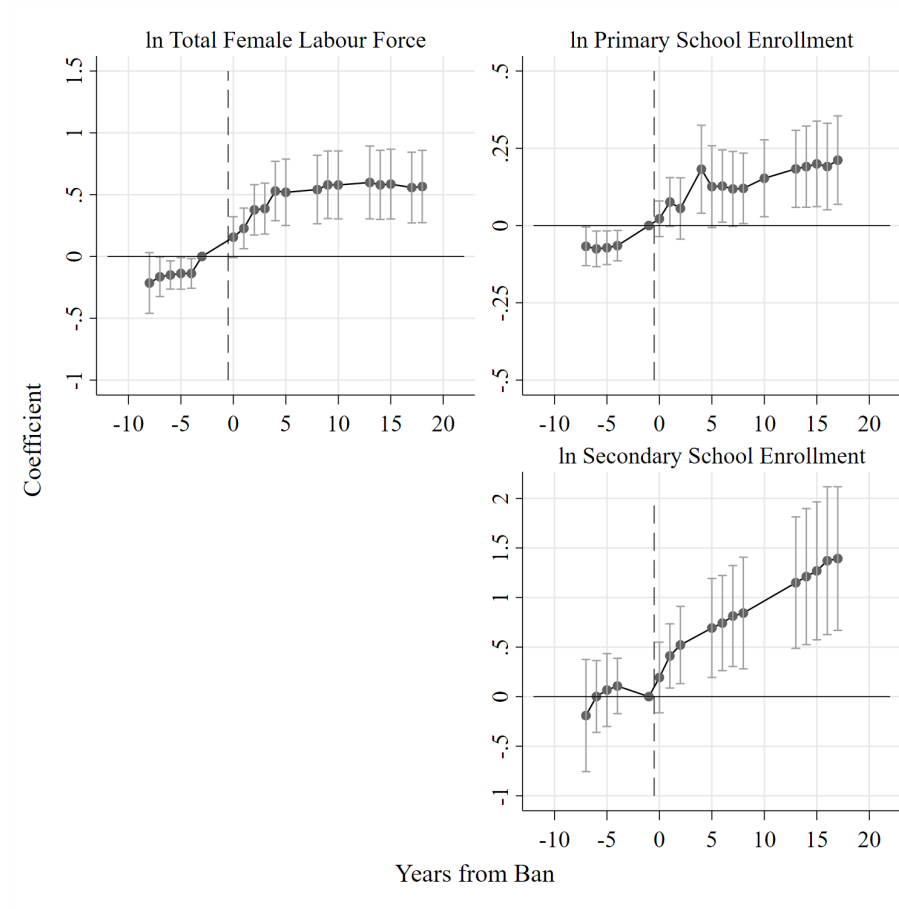


FIGURE 6. EVENT-STUDY ESTIMATES FOR FEMALE EMPLOYMENT AND FEMALE SCHOOL ENROLLMENT

Note: Panels plot event-study coefficients from equation (5) for log female employment, log female secondary-school enrollment, and log female primary-school enrollment. Event-study regressions include municipality and year fixed effects and log municipal population. The omitted year is 1966, the last pre-reform year. The vertical dashed line marks the 1967 reform. Confidence intervals are constructed at the 95 percent level using standard errors clustered by municipality. Raw data are from the National Archives of Albania.

B. Production and the Five-Year Plan

The Five-Year Plan records provide a check on the counterfactual parallel-trends assumption: they show whether treated municipalities were targeted for faster industrial growth before the reform. Table 3 reports the production results. Panels A and B use State Planning Commission data for 1965, 1966, 1967, and 1970 from the Five-Year Plan records. Panel A uses planned industrial production, while Panel B uses production relative to plan, defined as log realized industrial production minus log planned industrial production. Panel C uses realized industrial production from the annual administrative records, covering 1956 to 1988.

TABLE 3—PRODUCTION AND THE FIVE-YEAR PLAN

	(1) Fixed Effects	(2) Population Controls
Panel A: Planned Industrial Production		
No Monuments $\times$ Post	-0.014	-0.023
Standard Error	(0.106)	(0.098)
<i>p</i> -value	0.894	0.817
Observations	104	104
Panel B: Production Relative to Plan		
No Monuments $\times$ Post	0.201	0.196
Standard Error	(0.094)	(0.089)
<i>p</i> -value	0.042	0.037
Observations	104	104
Panel C: Industrial Production		
No Monuments $\times$ Post	0.426	0.390
Standard Error	(0.246)	(0.224)
<i>p</i> -value	0.096	0.094
Observations	858	848
Municipality and Year FE	✓	✓
Log Population		✓

Note: The table reports difference-in-differences estimates from equation (4). The dependent variables are log planned industrial production from the Five-Year Plan records in Panel A, log realized industrial production minus log planned industrial production in Panel B, and log realized industrial production from the annual administrative records in Panel C. Panels A and B are observed only in Five-Year Plan years, which accounts for the smaller number of observations. Population data are unavailable for the following municipalities in 1956: Gramsh, Kolonje, Lezhe, Librazhd, Mirdite, Permet, Pogradec, Puke, Sarande, Tepelene. All specifications include municipality and year fixed effects. Column 2 adds log municipal population. Standard errors clustered by municipality are reported in parentheses, with *p*-values below.

Panel A shows no evidence that treated municipalities were assigned faster planned industrial growth: the preferred coefficient in Column 2 is -0.023 log points and is not statistically significant. However, performance against those plans did differ: industrial production relative to plan increased by 0.196 log points more in treated municipalities than in control municipalities (Panel B, Column 2), and the coefficient is statistically significant. Together, Panels A and B show that the stronger realized performance of treated municipalities does not reflect higher pre-reform production targets. Panel C shows a positive but im-

precisely estimated post-reform difference in realized industrial production. The preferred coefficient in Column 2 is 0.390 log points. Unlike the planning records, the annual administrative data allow us to examine differential pre-reform trends and whether the post-reform difference persists over time.

Figure 7 reports the corresponding event-study estimates. Realized industrial production shows no evidence of a differential pre-reform trend. After 1967, the coefficients become positive and remain so through the end of the sample, although the confidence intervals are wide.

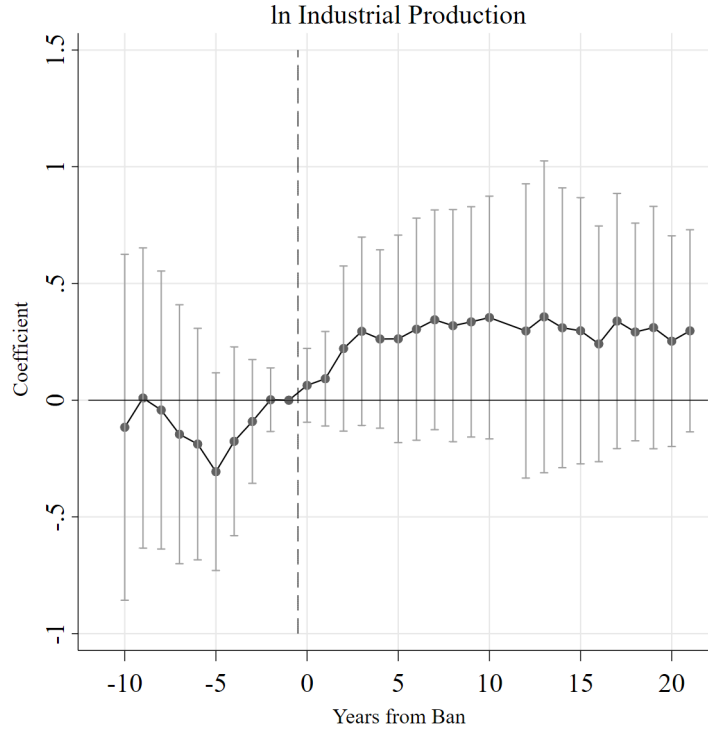


FIGURE 7. EVENT-STUDY ESTIMATES FOR INDUSTRIAL PRODUCTION

Note: The figure plots event-study coefficients from equation (5) for log realized industrial production from the annual administrative records. The event-study regression includes municipality and year fixed effects and log municipal population. The omitted year is 1966, the last pre-reform year, and the vertical dashed line marks the 1967 reform. Confidence intervals are constructed at the 95 percent level using standard errors clustered by municipality.

C. Mechanism: Public Visibility of the Old Religious Order

We examine the mechanism through which monument survival affected female employment and schooling by testing whether post-reform responses are stronger

where less of the old religious order remained publicly visible. We do so in three ways. First, we distinguish municipalities in which monuments survived in urban areas from those in which they survived only in rural areas. Second, we estimate how post-reform changes in female employment and schooling vary with the extent of monument survival, using both alternative removal thresholds and the number of surviving protected religious monuments. Third, we ask whether these changes depend on the denomination of the monument that survived.

LOCATION OF THE SURVIVING MONUMENT.

The baseline treatment captures whether a protected religious monument survived, but not how visible it was within the municipality. To capture this variation, we classify municipalities according to where protected religious monuments survived: in an urban area, only in rural areas, or not at all.¹³ Urban monuments stood where larger concentrations of residents lived and worked, while monuments surviving only in rural areas were encountered by a smaller share of the municipality’s population. We use municipalities with a surviving urban monument as the reference group and estimate separate post-reform changes in the three female outcomes for the rural-only and no-monument groups. The visibility mechanism implies that female employment and schooling should rise least where an urban monument remained, more where monuments survived only in rural areas, and most where no monument survived. Given the small number of municipalities in these groups, we supplement cluster-robust standard errors with randomization-inference p -values that preserve the observed group sizes.

¹³This distinction does not simply separate rural municipalities from urban ones. Durrës, for example, belongs to the rural-only group because its surviving protected religious monument lay outside the urban area, although Durrës is one of Albania’s largest cities.

TABLE 4—POST-REFORM RESPONSES BY LOCATION OF SURVIVING PROTECTED RELIGIOUS MONUMENTS

	Employment (1)	Secondary School Enrollment (2)	Primary School Enrollment (3)
Rural-Only Survival $\times$ Post	0.479	0.907	0.228
Standard Error	(0.131)	(0.322)	(0.072)
RI p -value	0.032	0.021	0.012
No Monument $\times$ Post	0.782	1.182	0.274
Standard Error	(0.104)	(0.226)	(0.059)
RI p -value	0.0002	0.0006	0.0004
Observations	513	439	494
Municipality and Year FE	✓	✓	✓
Log Population	✓	✓	✓

Note: Municipalities are classified according to where protected religious monuments survived after the 1967 reform. Municipalities retaining at least one monument in an urban area are the omitted reference group. The rural-only group consists of municipalities with at least one surviving monument in a rural area and none in an urban area; the no-monument group consists of municipalities in which no protected religious monument survived. The urban, rural-only, and no-monument groups contain 11, 6, and 9 municipalities, respectively. All outcomes are in natural logarithms. All specifications include municipality and year fixed effects and log municipal population. Standard errors clustered by municipality are reported in parentheses. Randomization-inference p -values are based on 5,000 placebo reassignments of municipalities across the three groups, preserving the observed group sizes, with the specification re-estimated for each assignment. The randomization-inference p -value is two-sided and equals the share of placebo estimates at least as large in absolute value as the observed estimate.

Table 4 shows this pattern across all three female outcomes. For each outcome, the rural-only group experiences a significantly larger post-reform increase than the urban-monument group, and the point estimate is larger still where no monument survived. Both the rural-only and no-monument coefficients remain statistically significant under randomization inference for all three outcomes.¹⁴

As Column 1 shows, relative to municipalities retaining an urban monument, female employment increased by 0.479 log points more in rural-only municipalities and by 0.782 log points more where no protected religious monument survived. Thus, the estimates show a larger post-reform response where less of the old religious order remained visible in local public life.

The rural-only result is also robust to excluding municipalities whose surviving monument was only a monastery. Two of the six rural-only municipalities fall into this category, and dropping them leaves the female-employment coefficient positive and statistically significant.¹⁵ In Appendix Table A3, reclassifying municipalities with no surviving urban monument, or no surviving non-monastery monument, as treated yields larger estimates than the baseline for all three female outcomes. These results suggest that what mattered was not simply whether a protected religious monument survived somewhere within the municipality, but

¹⁴The rural-only group contains six municipalities. Dropping each municipality in turn leaves the rural-only coefficient positive and statistically significant at the five percent level for all three female outcomes.

¹⁵The coefficient is 0.446 ($p = 0.013$).

how visible the old religious order remained in local public life.

EXTENT OF MONUMENT SURVIVAL.

As noted in Section II, extending the model to allow visibility to vary in degree yields a monotone prediction: female employment and schooling should increase most where no protected religious monument survived, and less as more monuments remained standing. We test this using two measures. First, we replace the baseline definition of complete removal with treatment definitions requiring municipalities to fall at or above the 50th or 75th percentile of the proportion of religious buildings destroyed or de-sanctified. These thresholds correspond to approximately 95 and 98 percent of religious buildings being destroyed or de-sanctified, respectively.¹⁶ These broader definitions therefore include municipalities in which some religious buildings remained after 1967. Second, we use the number of surviving protected religious monuments directly.

TABLE 5—ALTERNATIVE THRESHOLDS FOR RELIGIOUS-MONUMENT REMOVAL

	Employment (1)	Secondary School Enrollment (2)	Primary School Enrollment (3)
Panel A: 100% Destroyed (Main)			
Treated	0.610	0.854	0.191
Standard Error	(0.123)	(0.249)	(0.067)
<i>p</i> -value	0.000	0.002	0.008
Observations	513	439	494
Panel B: 98% Destroyed (75th pctl)			
Treated	0.473	0.783	0.190
Standard Error	(0.111)	(0.219)	(0.066)
<i>p</i> -value	0.000	0.001	0.008
Observations	513	439	494
Panel C: 95% Destroyed (50th pctl)			
Treated	0.296	0.509	0.166
Standard Error	(0.152)	(0.274)	(0.058)
<i>p</i> -value	0.062	0.075	0.009
Observations	513	439	494
Municipality and Year FE	✓	✓	✓
Log Population	✓	✓	✓

Note: The table reports difference-in-differences estimates from equation (4) using alternative definitions of treatment based on the extent of religious-building removal. The baseline specification defines treatment as complete local removal: municipalities in which no protected religious monument remained standing after the reform. The other definitions of treatment are that the municipality falls at or above the 50th or 75th percentile of the proportion of buildings destroyed or de-sanctified (corresponding to 95 and 98 percent of religious buildings, respectively). The baseline corresponds to 100 percent of religious buildings destroyed or de-sanctified. All specifications include municipality and year fixed effects and log municipal population. Standard errors clustered by municipality are reported in parentheses, with *p*-values below.

¹⁶The main analysis defines treatment as complete removal, corresponding to 100 percent of religious buildings being destroyed or de-sanctified. The median municipality lost approximately 95 percent of its religious buildings.

Table 5 displays the corresponding coefficients. The estimated effects are largest under the baseline definition of complete local removal, and are meaningfully smaller when the treatment allows for 2–5 percent of religious buildings to remain. The attenuation is particularly pronounced for female employment and secondary-school enrollment. Thus, the post-reform response strengthens as the treatment definition approaches complete disappearance of the old religious order from the local public environment.

TABLE 6—NUMBER OF SURVIVING PROTECTED RELIGIOUS MONUMENTS

	Employment (1)	Secondary School Enrollment (2)	Primary School Enrollment (3)
Panel A: Linear			
Monuments $\times$ Post	-0.030	-0.052	-0.013
Standard Error	(0.007)	(0.011)	(0.003)
Panel B: Quadratic			
Monuments $\times$ Post	-0.111	-0.151	-0.038
Standard Error	(0.024)	(0.040)	(0.011)
Monuments ² $\times$ Post	0.003	0.004	0.001
Standard Error	(0.001)	(0.001)	(0.000)
<i>Marginal Effect</i>	-0.087	-0.122	-0.030
Standard Error	(0.018)	(0.030)	(0.008)
Observations	513	439	494
Municipality and Year FE	✓	✓	✓
Log Population	✓	✓	✓

Note: The table reports difference-in-differences estimates from equation (4), replacing the baseline No Monuments $\times$ Post treatment with the number of surviving protected religious monuments interacted with the post-reform indicator. The count measure equals the number of protected religious monuments that remained standing in the municipality after the reform. Panel A reports linear specifications. Panel B additionally interacts the square of the number of surviving protected monuments with the post-reform indicator, allowing the relationship to be nonlinear. The marginal effect is evaluated at the mean number of surviving monuments. All specifications include municipality and year fixed effects and log municipal population. Standard errors clustered by municipality are reported in parentheses.

Table 6 shows a similar gradient using the number of surviving monuments. Conditional on log municipal population, municipalities with more surviving protected religious monuments experience smaller post-reform increases in female employment and schooling. Allowing the relationship to be nonlinear yields the same qualitative pattern.

DENOMINATION OF THE SURVIVING MONUMENT.

The model implies that if surviving monuments affected outcomes because they kept the old religious order visible, what mattered should have been their presence, rather than their denomination. Worship was banned everywhere after 1967, and surviving protected religious monuments no longer had any formal religious function. Yet a standing protected monument could keep the old religious

order present in local public life because it remained a visible symbol of religious institutions and the social norms they embodied.

Table 7 presents the results. Panel A restricts the sample to municipalities in which at least one protected religious monument remained standing after the reform and compares municipalities where the only surviving monuments did not match the locally dominant religion with municipalities where it did. If attachment to one's own faith's buildings drove the results, post-reform changes in female employment and schooling should have differed depending on whether the surviving monument matched the locally dominant religion. The estimates provide no evidence of such a difference.

Panel B compares municipalities where the only protected religious monument did not match the locally dominant religion with municipalities where no protected religious monument remained standing (our main treatment group). Municipalities that retained a non-dominant monument experienced significantly smaller increases in female employment than those where no monument remained. For the education outcomes, the coefficients are also negative but not statistically significant.

TABLE 7—DENOMINATION OF THE SURVIVING PROTECTED RELIGIOUS MONUMENT

	Female Employment (1)	Secondary School Enrollment (2)	Primary School Enrollment (3)
Panel A: Denomination of the Surviving Monument			
Non-Matching $\times$ Post	0.083	0.346	0.047
Standard Error	(0.181)	(0.403)	(0.108)
<i>p</i> -value	0.652	0.403	0.670
Observations	338	289	323
Panel B: No Monument versus Non-Dominant Monument			
Non-Dominant Monument $\times$ Post	-0.512	-0.472	-0.166
Standard Error	(0.158)	(0.368)	(0.108)
<i>p</i> -value	0.006	0.222	0.149
Observations	275	235	266
Municipality and Year FE	✓	✓	✓
Log Population	✓	✓	✓

Note: The table reports difference-in-differences estimates from equation (4). Panel A restricts the sample to municipalities in which at least one protected religious monument remained standing after the reform and compares the 4 municipalities where the only surviving monuments did not match the locally dominant religion with the 13 municipalities where it did. The coefficient reported is Non-Matching $\times$ Post. Panel B restricts the sample to the 9 municipalities with no protected religious monument and the 4 municipalities where the only surviving protected religious monument did not match the locally dominant religion. The coefficient reported is Non-Dominant Monument $\times$ Post, with the complete-removal municipalities as the omitted category; negative coefficients therefore indicate weaker post-reform responses where a non-dominant monument survived, relative to complete removal. Standard errors clustered by municipality are reported in parentheses, with *p*-values below.

The location, threshold, count, and denomination results reinforce the same interpretation. Female employment and schooling increase more where surviving monuments were less publicly visible and less where more monuments survived,

while the denomination of a surviving monument does not predict post-reform changes. Together, the evidence supports public visibility of the old religious order as the mechanism linking monument survival to these outcomes.

VI. Alternative Explanations and Robustness

The main results show that municipalities in which no protected religious monument remained standing experienced larger post-reform increases in female employment and schooling, particularly secondary-school enrollment, and stronger industrial performance relative to plan. The female responses were larger where less of the old religious order remained publicly visible. This section examines whether other explanations could account for these findings: differential state targeting; pre-existing religious, historical, and cultural differences; state mobilization; municipality-wide growth; and sectoral reallocation. It concludes with further robustness checks.

A. State Targeting and the Five-Year Plan

A central identification concern is that treated municipalities may have been targeted by the state for faster post-reform development. Because the Five-Year Plan reports municipality-level targets set before the religion ban for outcomes realized afterward, it allows us to test this possibility directly. Section V.B showed that treated municipalities were not assigned faster planned industrial growth. We extend this test to broader measures of planned economic activity. Appendix Figure A2 shows no differential planned growth in circulation, which measures income from the sale of goods and services, while planned consumption growth is, if anything, lower in treated municipalities. The latter result runs against an interpretation based on favorable state targeting. A separate concern is that treated municipalities may have been scheduled for faster expansion in public services, particularly school supply, which could account for the female-schooling results independently of changes in household participation. Treated and control municipalities had similar planned growth in the number of schools and in health-care beds, providing no indication that treated municipalities were favored in local public-service provision. Thus, the planning records provide no evidence that treated municipalities were targeted for faster economic or public-service development before the reform.

B. Pre-Reform Religiosity, Historical and Cultural Characteristics, and State Mobilization

PRE-REFORM RELIGIOSITY.

An alternative interpretation is that treatment status reflects pre-existing religious intensity rather than the local disappearance of the old religious order from public view. Municipalities in which no protected religious monument remained

standing may have differed in religiosity before 1967, and these differences could have generated different post-reform trajectories for reasons unrelated to the local disappearance of the old religious order.

We test this possibility by allowing pre-reform religiosity to predict differential post-reform changes. We interact pre-reform religious buildings per 1,000 inhabitants and clerics per 1,000 inhabitants with the post-reform indicator and add these interactions to equation (4), first separately and then jointly. If the estimated effects were driven by pre-existing religious intensity, the coefficient on No Monuments $\times$ Post should be sensitive to these controls. Table 8 shows no evidence that observable pre-reform religiosity explains the estimated female employment and schooling effects.

TABLE 8—BASELINE RELIGIOSITY AND FEMALE OUTCOMES

	(1) Baseline	(2) Religious Buildings $\times$ Post	(3) Clerics $\times$ Post	(4) Buildings and Clerics $\times$ Post
Panel A: Female Employment				
No Monuments $\times$ Post	0.610	0.610	0.606	0.607
Standard Error	(0.123)	(0.124)	(0.112)	(0.112)
<i>p</i> -value	< 0.001	< 0.001	< 0.001	< 0.001
Observations	513	513	513	513
Panel B: Secondary School Enrollment				
No Monuments $\times$ Post	0.854	0.829	0.862	0.839
Standard Error	(0.249)	(0.263)	(0.221)	(0.234)
<i>p</i> -value	0.002	0.004	0.001	0.001
Observations	439	439	439	439
Panel C: Primary School Enrollment				
No Monuments $\times$ Post	0.191	0.190	0.191	0.190
Standard Error	(0.067)	(0.068)	(0.062)	(0.063)
<i>p</i> -value	0.008	0.009	0.005	0.006
Observations	494	494	494	494
Municipality and Year FE	✓	✓	✓	✓
Log Population	✓	✓	✓	✓
Religious Buildings $\times$ Post		✓		✓
Clerics $\times$ Post			✓	✓

Note: The table reports difference-in-differences estimates from equation (4). The coefficient shown is on No Monuments $\times$ Post. The dependent variables are logged female employment in Panel A, logged female secondary school enrollment in Panel B, and logged female primary school enrollment in Panel C. All specifications include municipality and year fixed effects and log municipal population. Column 1 reports the preferred specification. Columns 2 and 3 add, respectively, pre-reform religious buildings per 1,000 inhabitants and pre-reform clerics per 1,000 inhabitants interacted with Post. Column 4 includes both interactions. Standard errors clustered by municipality are reported in parentheses. *p*-values are reported below the standard errors.

HISTORICAL AND CULTURAL CHARACTERISTICS.

A concern specific to the source of treatment variation is that municipalities in which protected religious monuments survived may have differed in broader

historical or cultural characteristics from municipalities in which none remained. Because preservation status was assigned on historical, architectural, and cultural grounds, treatment status could capture other pre-existing differences rather than the post-reform public visibility of the old religious order. We examine this possibility using protected non-religious monuments from the same pre-reform national heritage classification.

As shown in Table 1, control municipalities had 0.28 protected non-religious monuments per 1,000 inhabitants, compared with 0.19 in treated municipalities; the difference is not statistically significant ($p = 0.347$).

To allow broader historical and cultural characteristics to generate differential post-reform changes, we include the pre-reform density of protected non-religious monuments interacted with the post-reform indicator in equation (4). Table 9 reports the results.

TABLE 9—PROTECTED NON-RELIGIOUS MONUMENTS AND FEMALE OUTCOMES

	(1) Baseline	(2) Heritage $\times$ Post
Panel A: Employment		
No Monuments $\times$ Post	0.610	0.586
Standard Error	(0.123)	(0.129)
p -value	0.000	0.000
Observations	513	513
Panel B: Secondary School Enrollment		
No Monuments $\times$ Post	0.854	0.826
Standard Error	(0.249)	(0.264)
p -value	0.002	0.006
Observations	439	439
Panel C: Primary School Enrollment		
No Monuments $\times$ Post	0.191	0.177
Standard Error	(0.067)	(0.069)
p -value	0.008	0.017
Observations	494	494
Municipality and Year FE	✓	✓
Log Population	✓	✓
Heritage $\times$ Post		✓

Note: The dependent variables are logged female employment in Panel A, logged female secondary-school enrollment in Panel B, and logged female primary-school enrollment in Panel C. Column 2 adds the number of protected non-religious monuments designated before 1967 per 1,000 inhabitants, using municipality population in 1966, interacted with the post-reform indicator. All specifications include municipality and year fixed effects and log municipal population. Standard errors clustered by municipality are reported in parentheses.

Adding this interaction changes the estimated female employment and schooling effects little, and all three remain statistically significant. The treatment pattern also does not reproduce the historical geography of the *Kanun*, Albania's body of customary law (Hasluck, 1954). Shkodër, Lezhë, and Mirditë—areas where it was particularly influential—are all control municipalities, whereas treated mu-

nicipalities such as Gramsh, Skrapar, and Tepelenë lie outside its core areas.

STATE CULTURAL AND FEMALE POLITICAL MOBILIZATION.

A related concern is that treated municipalities may have differed before the reform in the intensity of state efforts to reshape local culture and women’s public roles. Cultural houses were central local institutions for cultural programming and political education before 1967, and contemporary documents show that cultural, literary, and artistic activity was treated as part of the regime’s broader project of ideological education (Meta, Krasniqi and Bello, 2018). Female political representation provides a second proxy for these efforts, since greater representation of women in public office could itself form part of the state’s attempt to promote women’s participation in public life. If such efforts were stronger in treated municipalities, the main results could partly reflect differences in state mobilization rather than the local disappearance of the old religious order from public view.

We examine this concern using two pre-reform measures: cultural houses and cultural hearths (*shtëpi dhe vatra kulture*) per 1,000 inhabitants in 1966, and women elected to the People’s Assembly in 1966 per 1,000 inhabitants. To allow municipalities with different pre-reform levels of state cultural and female political mobilization to follow different post-reform paths, we interact each measure with the post-reform indicator and include the interactions in equation (4), first separately and then jointly.¹⁷

Table 10 reports the results. Controlling for female political representation leaves the estimates essentially unchanged. Specifications including cultural houses yield smaller positive coefficients that remain statistically significant; these specifications exclude Tirana because cultural-house data are unavailable for Tirana.

¹⁷Appendix Figure A2 shows that treated and control municipalities had similar planned expansion of Cultural Houses in the 1966 Five-Year Plan.

TABLE 10—STATE CULTURAL AND FEMALE POLITICAL MOBILIZATION AND FEMALE OUTCOMES

	(1) Baseline	(2) Female Deputies	(3) Cultural Houses	(4) Both Controls
Panel A: Female Employment				
No Monuments $\times$ Post	0.610	0.624	0.436	0.439
Standard Error	(0.123)	(0.119)	(0.089)	(0.099)
<i>p</i> -value	0.000	0.000	0.000	0.000
Observations	513	513	494	494
Panel B: Female Secondary School Enrollment				
No Monuments $\times$ Post	0.854	0.882	0.451	0.426
Standard Error	(0.249)	(0.245)	(0.198)	(0.246)
<i>p</i> -value	0.002	0.001	0.032	0.096
Observations	439	439	422	422
Panel C: Female Primary School Enrollment				
No Monuments $\times$ Post	0.191	0.195	0.128	0.125
Standard Error	(0.067)	(0.068)	(0.059)	(0.068)
<i>p</i> -value	0.008	0.009	0.039	0.078
Observations	494	494	475	475
Municipality and Year FE	✓	✓	✓	✓
Log Population	✓	✓	✓	✓
Female Deputies $\times$ Post		✓		✓
Cultural Houses $\times$ Post			✓	✓

Note: The table reports difference-in-differences estimates from equation (4). The coefficient shown is on No Monuments $\times$ Post. No Monuments equals one for municipalities in which no protected religious monument remained standing after the reform. The dependent variables are logged female employment in Panel A, logged female secondary-school enrollment in Panel B, and logged female primary-school enrollment in Panel C. All specifications include municipality and year fixed effects and log municipal population. Column 1 reports the baseline specification. Column 2 adds female deputies per 1,000 inhabitants in 1966 interacted with Post. Column 3 adds cultural houses and cultural hearths per 1,000 inhabitants in 1966 interacted with Post. Column 4 includes both interactions. Tirana is excluded from specifications including the cultural-houses interaction because its cultural-institution count is not available, which accounts for the smaller number of observations in Columns 3 and 4. Standard errors clustered by municipality are reported in parentheses and *p*-values are reported in the following row.

C. Gender Specificity

An alternative explanation is that treated municipalities simply grew faster after the reform, raising employment and schooling for men and women alike. The data do not provide municipality-year population by age and sex, so we cannot construct gender-specific participation rates. However, we can examine whether women’s participation increased relative to the corresponding municipal totals.

This comparison is informative because increases in total employment or school enrollment need not increase women’s share of either outcome. However, if the reform weakened gender-specific constraints on women’s work and schooling, these shares should rise relative to the corresponding totals. Thus, we construct the log female share of employment and the log female share of secondary-school enrollment by subtracting the log total outcome from the corresponding log female outcome, and re-estimate equation (4) using these shares as dependent variables.

Table 11 reports the results. The female share of employment increases by 0.207 log points, equivalent to about 23 percent, while the female share of secondary-school enrollment increases by 0.398 log points, equivalent to about 49 percent. Both coefficients are statistically significant. The female-share evidence supports a gendered response, with the clearest increase on the secondary-schooling margin.

TABLE 11—FEMALE SHARES IN EMPLOYMENT AND SECONDARY SCHOOLING

	Female Employment Share (1)	Female Secondary Enrollment Share (2)
No Monuments $\times$ Post	0.207	0.398
Standard Error	(0.102)	(0.161)
p -value	0.064	0.031
Observations	513	439
Municipality and Year FE	✓	✓
Log Municipal Population	✓	✓

Note: The table reports difference-in-differences estimates from equation (4). The dependent variable in Column 1 is the log female share of employment, defined as log female employment minus log total employment. The dependent variable in Column 2 is the log female share of secondary-school enrollment, defined as log female secondary-school enrollment minus log total secondary-school enrollment. Treated municipalities are those in which no protected religious monument remained standing after the reform. All specifications include municipality and year fixed effects and log municipal population. Standard errors clustered by municipality are reported in parentheses and p -values are reported in the following row.

Figure A3 reports the corresponding event-study estimates. For the female employment share, the event study is normalized to 1964, the last available pre-reform year in the employment-share series. The joint test of the pre-reform coefficients does not reject the null of no differential pre-reform trends.¹⁸ The

¹⁸The joint pre-trend test gives $F(5, 25) = 1.02$ and $p = 0.425$.

post-reform coefficients are generally positive, particularly in later years, although they are less precisely estimated than those for the secondary-schooling share. For the female secondary-school enrollment share, the event study is normalized to 1966 and likewise shows no joint evidence of differential pre-reform trends.¹⁹ After the reform, the coefficients are positive and increase over time, becoming statistically significant in the later post-reform years.

We interpret the female-share estimates as supporting evidence rather than as direct estimates of gender differences. A municipality-wide shock could still raise female shares if women responded more strongly than men. One such shock would be an expansion of school capacity. Archival records of building repurposing show, however, that none of the repurposed religious buildings in the sample were converted into schools, and the planning records show no faster planned growth in school numbers in treated municipalities (Section VI.A).

D. Agriculture and Sectoral Reallocation

A remaining concern is that the production and female-employment results may reflect agricultural reorganization or sectoral reallocation rather than the mechanism emphasized above. Treated municipalities may, for example, have increased industrial activity by shifting labor or resources out of agriculture and into industry. A related concern is that the female-employment response could reflect changes in cooperative agricultural employment. Collectivization, however, preceded the religion ban and was nearly completed by the early 1960s.²⁰ Moreover, the female-employment event study shows a gradual post-reform increase rather than a discrete jump in 1967–1968, making a reclassification of cooperative workers timed to the reform less likely.

We examine the sectoral-reallocation interpretation using the non-industrial labor force and agricultural production. If industrial gains in treated municipalities reflected a shift of labor from non-industrial activities into industry, the non-industrial labor force should fall. Likewise, a substantial shift of labor or resources out of agriculture should be accompanied by a decline in agricultural production.

¹⁹The joint pre-trend test gives $F(4, 25) = 1.22$ and $p = 0.327$.

²⁰By 1961, nearly 90 percent of land had been collectivized into agricultural cooperatives (Kelliçi and Danaj, 2016).

TABLE 12—SECTORAL REALLOCATION: NON-INDUSTRIAL LABOR AND AGRICULTURAL PRODUCTION

	(1) FE	(2) Population Controls	(3) Region Trends
Panel A: Non-Industrial Labor Force			
No Monuments $\times$ Post	0.391	0.354	0.234
Standard Error	(0.136)	(0.135)	(0.126)
<i>p</i> -value	0.008	0.015	0.074
Observations	616	616	616
Panel B: Agricultural Production			
No Monuments $\times$ Post	-0.193	-0.297	-0.080
Standard Error	(0.132)	(0.157)	(0.074)
<i>p</i> -value	0.156	0.070	0.286
Observations	598	598	598
Municipality and Year FE	✓	✓	✓
Log Population		✓	✓
Region Linear Trends			✓

Note: The table reports difference-in-differences estimates from equation (4). The coefficient shown is on No Monuments $\times$ Post. The dependent variable in Panel A is the logged non-industrial labor force, computed as total labor force minus the industrial labor force. The dependent variable in Panel B is logged agricultural production. All specifications include municipality and year fixed effects. Column 2 adds log municipal population. Column 3 adds region-specific linear time trends. Standard errors clustered by municipality are reported in parentheses. *p*-values are reported below the standard errors.

Table 12 does not support a simple sectoral-reallocation interpretation. Panel A shows that the non-industrial labor force rises rather than falls: the coefficient is 0.354 in the population-adjusted specification and remains positive when region-specific linear trends are included. Panel B shows a negative coefficient for agricultural production in the population-adjusted specification, but Appendix Figure A4 shows no discrete decline around the reform, and the coefficient becomes small once region-specific trends are included.

E. Convergence, Sample Composition, Functional Form, and Inference

We conclude by examining whether the main results depend on particular features of the baseline specification or sample. We address three possibilities: that the estimates reflect convergence; that they are sensitive to sample composition and weighting; and that they depend on functional form or the method of inference.

CONVERGENCE AND ALTERNATIVE TIME PATHS.

Because treated municipalities started from lower pre-reform outcome levels, the estimates could in principle reflect convergence rather than the reform. To address this, we control for each municipality’s extrapolated pre-reform growth path, obtained by fitting a linear trend separately for each municipality and outcome using only observations before 1967, so that the control is predetermined with respect to the reform. The treatment effects remain positive, statistically

significant, and of broadly similar magnitude to the baseline estimates across all four outcomes (Appendix Table A4). Together with the event-study estimates and the matched-sample comparisons below, these results make convergence an unlikely explanation for the post-reform differences.

The baseline specification controls for contemporaneous municipal population, which could itself respond to the reform. Appendix Table A2 therefore replaces contemporaneous log municipal population with interactions between log 1966 municipal population and year indicators. This uses only pre-reform population variation while allowing municipalities of different baseline sizes to follow flexible year-specific paths. The main conclusions are unchanged.

As a further check, we allow regions to follow separate linear time trends, accounting for the possibility that treated and control municipalities were exposed to different regional time paths. This is a demanding specification because treatment is regionally clustered and the estimated responses build gradually after the reform, so region-specific trends may absorb part of the post-reform response. The main findings remain robust to this specification (Appendix Table A1).

SAMPLE COMPOSITION AND WEIGHTING.

Appendix Table A10 restricts the comparison to treated and control municipalities with similar observed pre-reform characteristics using propensity-score matching. One specification matches on pre-reform population, female secondary-school enrollment, and female employment; a second additionally matches on pre-reform religious buildings and clerics per 1,000 inhabitants. The estimates for female employment and female secondary-school enrollment remain positive and statistically significant in both matched samples. These estimates provide additional evidence that the main findings are not driven by pre-reform differences in population, outcome levels, or religiosity.

With only nine treated municipalities, the estimates could be sensitive to a single treated unit. Appendix Table A5 re-estimates the main female-outcome specifications after dropping each treated municipality one at a time. The results are stable across these samples and are not driven by any single treated municipality.

We next consider whether the estimates are driven by Tirana, which is both the capital and by far the largest municipality in the sample. Appendix Table A7 re-estimates the main specifications after excluding Tirana, with similar results. We then examine whether the results depend on giving municipalities equal weight in the baseline specification. Appendix Table A8 uses 1966 municipal population as analytic weights. The main female-outcome results are robust to population weighting.

FUNCTIONAL FORM AND INFERENCE.

The baseline female-outcome specifications use log counts and could therefore be sensitive to the log transformation, particularly where baseline counts are

small. Appendix Table A6 re-estimates the specifications using Poisson pseudo-maximum likelihood on the underlying count outcomes (Santos Silva and Tenreiro, 2006). The estimated effects remain positive and statistically significant under this alternative functional form.

Given the small number of municipalities, Table 13 reports randomization-inference (RI) and wild-cluster bootstrap (WCB) p -values for the main female outcomes. The RI p -values are based on 5,000 placebo reassignments of treatment status across municipalities, preserving the observed number of treated municipalities. For each of the outcomes, fewer than 1 percent of placebo assignments produce an estimate at least as large in absolute value as the observed estimate. We complement this with WCB p -values, imposing the null hypothesis of no treatment effect and recomputing the test statistic over 1,000 bootstrap replications. The WCB p -values are also small and lead to the same inference as the conventional clustered results.

TABLE 13—ROBUSTNESS OF INFERENCE

Outcome	Coef.	RI p -value	WCB p -value	Observations
Female employment	0.610	< 0.001	< 0.001	513
Female secondary enrollment	0.854	0.0068	0.002	439
Female primary enrollment	0.191	0.0082	0.012	494

Note: The table reports randomization-inference and wild cluster bootstrap-t tests for the main female outcomes. The coefficient shown is on No Monuments $\times$ Post. For RI - Treatment is randomly reassigned across municipalities while preserving the observed number of treated municipalities, 5,000 times using the baseline difference-in-differences specification. The randomization-inference p -value is two-sided and equals the share of placebo estimates at least as large in absolute value as the observed estimate. For WCB - We impose the null hypothesis of no treatment effect, re-weighting residuals using Rademacher weights, and recomputing the test statistic over 1,000 bootstrap replications.

The evidence in this section does not support explanations based on differential state targeting, pre-reform religiosity, broader historical or cultural differences, state mobilization, municipality-wide growth, or sectoral reallocation, and the main results are robust to alternative specifications, samples, and methods of inference.

VII. Discussion

Women’s employment and schooling changed most where the old religious order disappeared from public view, rather than only in law. Although our setting concerns religion, the mechanism is more general: public symbols can sustain the expectations associated with a social order even after its formal authority has been removed. This connects to a sociological tradition that views public symbols

as carriers of shared meaning and social authority (Durkheim, 1912; Geertz, 1973; Bourdieu, 1991).

To explore the longer-run implications of these changes beyond the period covered by our archival data, we turn to individual-level data from the 2008–09 Albania Demographic and Health Survey (DHS). Figure 8 compares women from treated and control municipalities within each birth cohort, from women born nine years before the 1967 reform to those born twenty years after it. Municipality treatment status is based on respondents’ place of residence in 1990, when internal migration remained heavily restricted and before the large migration flows of the post-communist transition. We examine literacy, number of children, ideal number of children, and household power. Household power is particularly informative because it captures women’s role in household decision-making, providing the most direct evidence among these outcomes on gender relations within the household.²¹

The largest differences between treated and control municipalities appear among the earliest cohorts. Women from treated municipalities born nine years before the reform have substantially lower literacy, more children, a higher ideal number of children, and lower household power than women from control municipalities in the same birth cohort. These within-cohort differences narrow across successive birth cohorts, with convergence becoming increasingly apparent among cohorts born around and after the reform. The gaps in literacy and ideal number of children largely disappear among cohorts born around 1967, while the gaps in number of children and household power close more gradually among post-reform cohorts.

Across all four outcomes, differences between women from treated and control municipalities have largely disappeared among cohorts born up to twenty years after the reform. Because these women are observed individually in the 2008–09 DHS, well beyond the end of our archival panel and into the post-communist period, these cohort patterns extend the archival evidence to longer-run literacy, childbearing outcomes, and women’s position within the household.

The mechanism is not confined to Albania and remains relevant today. In China, Islam remains a formally recognized religion, while the policy of religious sinicization has altered its public presence: domes, minarets, and other visibly Islamic features have been removed from mosques in several parts of the country (Ridgeon, 2020; Human Rights Watch, 2023). Some mosques have also been closed or converted to secular uses (Human Rights Watch, 2023). Public presence can

²¹Literacy is measured by whether the respondent can read a complete sentence. Number of children is the respondent’s reported number of children, ideal number of children is the respondent’s reported ideal number of children, and household power measures the respondent’s role in household decision-making. Lower household power among later cohorts is expected because these women were younger when interviewed and at an earlier stage of the household life cycle; the relevant comparison is therefore the treated–control gap within each birth cohort. For the youngest cohorts, number of children does not represent completed fertility, but the within-cohort comparison shows whether treated women had accumulated more children than control women by the same age. Figure 8 reports mean outcomes separately by birth cohort for women from treated and control municipalities.

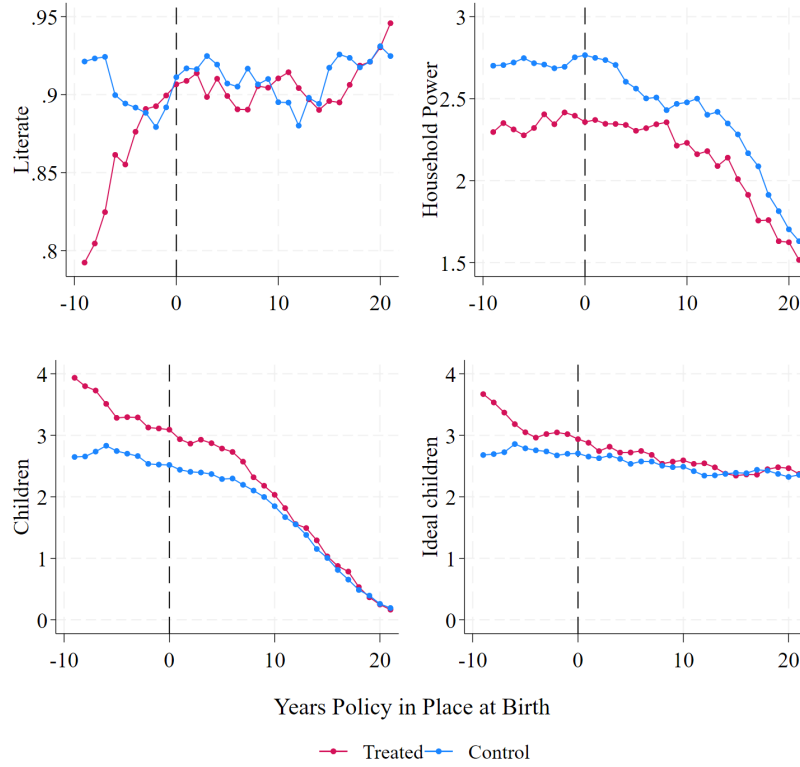


FIGURE 8. DHS SOCIAL NORMS

Note: The panels plot cohort averages from the 2008–09 Albania Demographic and Health Survey for literacy, number of children, ideal number of children, and household power. Household power is constructed based on female respondents answers on who has the final say at home on: making large household purchases; making household purchases for daily needs; visits to family or relatives; food to be cooked each day; and on deciding what to do with the money husband earns. Answering affirmative the respondent (woman) alone has a value of 1, the respondent and husband together has a value of 0.5, and 0 when the power is with someone else in the household. Literacy is measured by whether the respondent can read a complete sentence. Cohorts range from women born nine years before to twenty years after the 1967 religion ban. Treated municipalities are those in which no protected religious monument remained standing after the reform; control municipalities are those in which at least one protected religious monument remained. Municipality status is based on place of residence in 1990. Red markers denote treated municipalities and blue markers denote control municipalities. The sample contains 6,532 women, of whom 2,205 are from treated municipalities and 4,327 are from control municipalities.

also move in the other direction. In Afghanistan under the Taliban, restrictions on women’s schooling, work, and public roles have accompanied the reassertion of conservative religious authority in public life (Thomas and Collins, 2021; United Nations Assistance Mission in Afghanistan, 2025).

VIII. Conclusion

This paper studies whether dismantling the institutions that sustain a social order can change the norms governing economic behavior. Albania’s 1967 religion ban provides a rare setting in which to examine this question. Religious authority and worship were abolished nationwide, but municipalities differed in whether a religious building protected as a national cultural monument in 1948 remained standing after the reform. Using hand-transcribed administrative records for all 26 Albanian municipalities from 1956 to 1988, we exploit this predetermined variation to study changes in women’s employment and schooling. We find substantially larger post-reform increases in both outcomes where no protected religious monument remained. The mechanism evidence shows a clear visibility gradient: the increases are largest where no monument remained, smaller where monuments survived only in rural areas, and smallest where an urban monument remained. The effects emerge gradually, with no evidence of differential pre-reform trends. Municipalities without a surviving monument were not targeted for faster economic or educational expansion in the government’s 1966 Five-Year Plan, yet after the reform they performed better relative to pre-set industrial production targets. Exploratory evidence from the 2008–09 Albania Demographic and Health Survey extends the analysis to longer-run female outcomes, with differences between treated and control municipalities largely disappearing for cohorts born around and after the reform.

The results have three implications for how institutional change affects social norms. First, formal and socially recognized authority need not disappear together: legal rules can change immediately while expectations about what others will do and regard as acceptable persist. Second, public symbols can help sustain those expectations even after the institutions associated with them have lost their formal authority. Third, reforms aimed at changing entrenched social norms may have larger effects when they create a publicly visible break with the old social order, rather than changing formal rules alone. Although our setting concerns religion and women’s participation, this mechanism is more general and may apply wherever institutions and public symbols sustain shared expectations about socially acceptable behavior.

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The main text suppresses time-invariant municipal heterogeneity in order to isolate the public-visibility mechanism. This appendix shows that allowing municipalities to have different pre-reform participation levels does not alter the model's central comparative static.

For municipality m , let the fixed private participation cost be $c_m > 0$. Define the post-reform participation response under counterfactual public-visibility state $v \in \{0, 1\}$ as

$$G_{m,v}(x) = 1 - F(c_m + s(x, v, 1)).$$

Because $s_x(x, v, 1) < 0$ and $f > 0$, $G_{m,v}(x)$ is increasing in x . The public-visibility restriction

$$s(x, 0, 1) < s(x, 1, 1)$$

also implies

$$G_{m,0}(x) > G_{m,1}(x) \quad \text{for every } x \in [0, 1].$$

Municipality m may have its own selected pre-reform equilibrium, denoted x_m^{pre} , satisfying

$$x_m^{\text{pre}} = 1 - F(c_m + s(x_m^{\text{pre}}, 1, 0)).$$

The presence of c_m allows the pre-reform participation response itself to differ across municipalities. Municipalities may therefore have different pre-reform participation levels without altering the public-visibility comparative static.

For each counterfactual post-reform public-visibility state v , define

$$x_{m,k+1}(v) = G_{m,v}(x_{m,k}(v)), \quad x_{m,0}(v) = x_m^{\text{pre}}.$$

For each municipality m and public-visibility state v , the adjustment sequence $\{x_{m,k}(v)\}_{k \geq 0}$ converges to a fixed point of $G_{m,v}$. Moreover, the limiting participation levels satisfy

$$x_m^{\text{post}}(0) > x_m^{\text{post}}(1).$$

PROOF:

Fix municipality m and public-visibility state v . If $x_{m,1}(v) \geq x_{m,0}(v)$, then, because $G_{m,v}$ is increasing,

$$x_{m,2}(v) = G_{m,v}(x_{m,1}(v)) \geq G_{m,v}(x_{m,0}(v)) = x_{m,1}(v).$$

Iteration implies that the sequence is non-decreasing. If instead $x_{m,1}(v) \leq x_{m,0}(v)$, the same argument implies that the sequence is non-increasing. Since participation lies in $[0, 1]$, the sequence is bounded and hence converges. Continuity of $G_{m,v}$ implies that its limit is a fixed point.

It remains to compare the two counterfactual paths. Since

$$x_{m,0}(0) = x_{m,0}(1) = x_m^{\text{pre}},$$

we have

$$x_{m,1}(0) = G_{m,0}(x_m^{\text{pre}}) > G_{m,1}(x_m^{\text{pre}}) = x_{m,1}(1).$$

Suppose that $x_{m,k}(0) \geq x_{m,k}(1)$. Then

$$\begin{aligned} x_{m,k+1}(0) &= G_{m,0}(x_{m,k}(0)) \\ &\geq G_{m,0}(x_{m,k}(1)) \\ &> G_{m,1}(x_{m,k}(1)) \\ &= x_{m,k+1}(1), \end{aligned}$$

where the weak inequality follows from the monotonicity of $G_{m,0}$ and the strict inequality follows from $G_{m,0}(x) > G_{m,1}(x)$ for every x . Hence, by induction,

$$x_{m,k}(0) > x_{m,k}(1) \quad \text{for every } k \geq 1.$$

Taking limits gives

$$x_m^{\text{post}}(0) \geq x_m^{\text{post}}(1).$$

If the two limits were equal to some L , continuity would imply

$$L = G_{m,0}(L) = G_{m,1}(L),$$

which contradicts

$$G_{m,0}(L) > G_{m,1}(L).$$

Therefore,

$$x_m^{\text{post}}(0) > x_m^{\text{post}}(1).$$

APPENDIX FIGURES

Figure A1 illustrates the adjustment process, with both counterfactual paths beginning from the municipality's own pre-reform participation level.

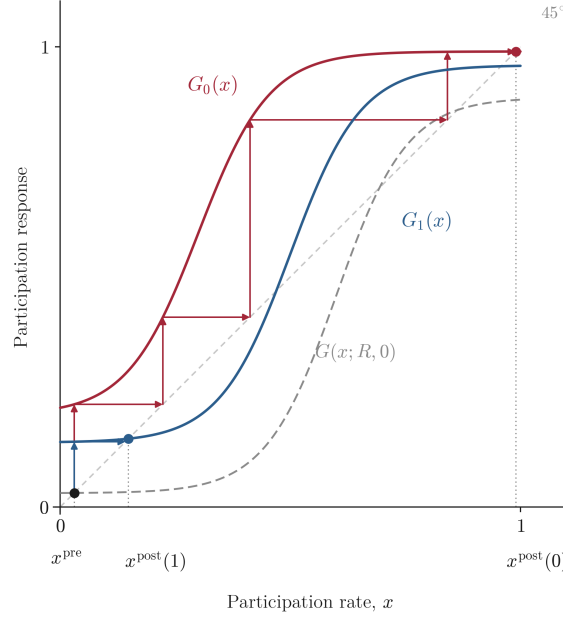


FIGURE A1. ADJUSTMENT PROCESS AFTER THE REFORM

Note: Each stage of adjustment moves vertically to the participation response and horizontally to the 45-degree line. The grey dashed curve is the pre-reform response, $G(x; R, 0) = 1 - F(c + s(x, 1, 0))$, which is common to both visibility states; its fixed point is the municipality's pre-reform participation level x^{pre} . Both counterfactual paths begin from this same initial condition. The blue curve, $G_1(x) = 1 - F(c + s(x, 1, 1))$, is the post-reform response when at least one protected religious monument remains visible. The red curve, $G_0(x) = 1 - F(c + s(x, 0, 1))$, is the post-reform response under complete local removal. Because $s(x, 1, 1) < s(x, 1, 0)$, participation rises after the reform under continued visibility; because $s(x, 0, 1) < s(x, 1, 1)$, the response is larger under complete local removal. The adjustment paths therefore converge in the illustrated configuration to $x^{\text{post}}(1)$ and $x^{\text{post}}(0)$, respectively, with $x^{\text{post}}(0) > x^{\text{post}}(1)$. The cobweb path illustrates gradual adjustment, with successive changes diminishing as the selected equilibrium is approached. The municipality's private participation cost is held fixed, and the curves are illustrative rather than calibrated.

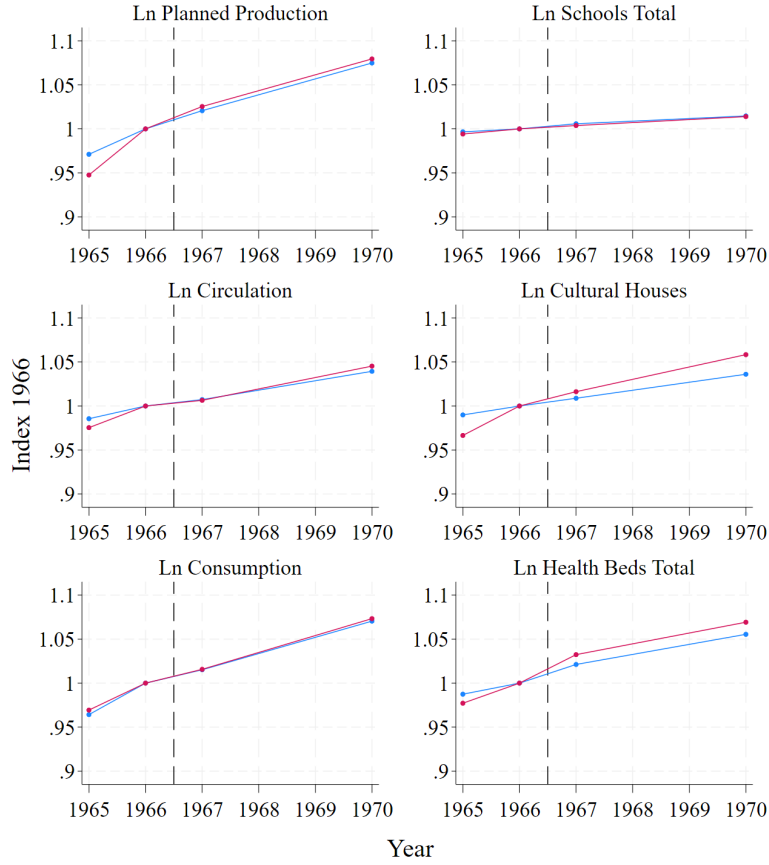
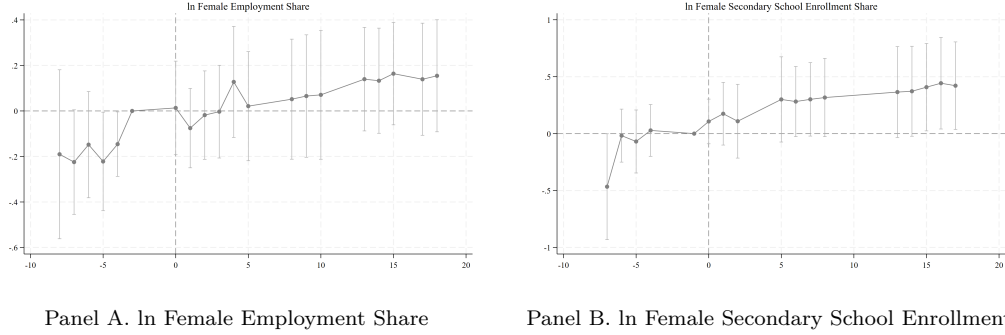


FIGURE A2. PLANNED TARGETS IN THE 1966 FIVE-YEAR PLAN

Note: The figure plots mean planned targets from the 1966 Five-Year Plan for: Industrial production, Circulation, Consumption, Schools, Cultural Houses, and Health Beds. Each series is indexed to 1 in 1966. The vertical dashed line marks 1967, the year of the religion ban. Red markers denote municipalities in which no protected religious monument remained standing after the reform; blue markers denote municipalities in which at least one protected religious monument remained standing. Circulation and Consumption are defined in Section III.

FIGURE A3. EVENT-STUDY DIAGNOSTICS FOR FEMALE SHARES



Note: The figure plots event-study coefficients for female shares in employment and secondary schooling. Panel A uses the log female employment share and is normalized to 1964, the last available pre-reform year in the employment-share series. Panel B uses the log female secondary-school enrollment share and is normalized to 1966. The horizontal axis reports years from the 1967 religion ban. Each specification includes municipality and year fixed effects. Confidence intervals are constructed at the 95 percent level using standard errors clustered by municipality.

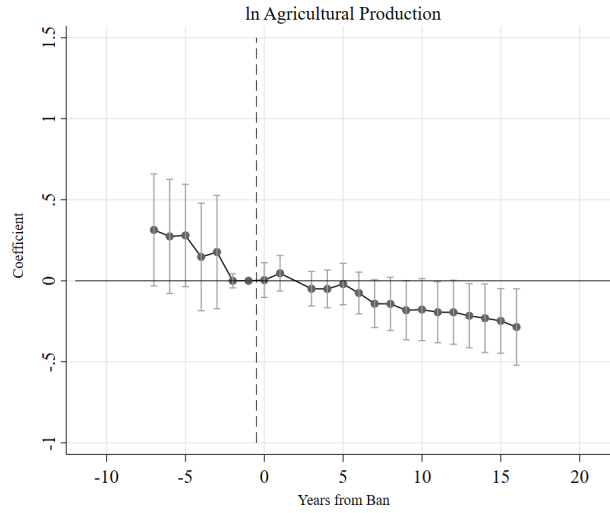


FIGURE A4. EVENT-STUDY ESTIMATES FOR AGRICULTURAL PRODUCTION

Note: The figure plots event-study coefficients from equation (5) for logged agricultural production. The specification includes municipality and year fixed effects and log municipal population. The omitted year is 1966, the last pre-reform year, and the vertical dashed line marks the 1967 reform. Confidence intervals are constructed at the 95 percent level using standard errors clustered by municipality.

APPENDIX TABLES

TABLE A1—FEMALE EMPLOYMENT AND SCHOOL ENROLLMENT WITH REGIONAL TIME TRENDS

	(1) Fixed Effects	(2) Population Controls	(3) Regional Trends
Panel A: Employment			
No Monuments $\times$ Post	0.648	0.610	0.453
Standard Error	(0.117)	(0.123)	(0.109)
<i>p</i> -value	0.000	0.000	0.004
Observations	513	513	513
Panel B: Secondary School Enrollment			
No Monuments $\times$ Post	0.931	0.854	0.517
Standard Error	(0.222)	(0.249)	(0.215)
<i>p</i> -value	0.000	0.002	0.048
Observations	439	439	439
Panel C: Primary School Enrollment			
No Monuments $\times$ Post	0.265	0.191	0.125
Standard Error	(0.070)	(0.067)	(0.051)
<i>p</i> -value	0.001	0.008	0.062
Observations	494	494	494
Municipality and Year FE	✓	✓	✓
Log Population		✓	✓
Regional Time Trends			✓

Note: The table reports difference-in-differences estimates from equation (4). The dependent variables are logged female employment, logged female secondary school enrollment, and logged female primary school enrollment. Column 3 additionally controls for regional time trends, based on the 12 regions which nest the municipalities. Due to degrees of freedom we do not condition on municipality-specific time trends. All specifications include municipality and year fixed effects. Column 2 adds log municipal population. Standard errors clustered by municipality are reported in parentheses and *p*-values are reported in the following row.

C1. Predetermined Population Trends

Appendix Table A2 examines whether the main female-outcome results are sensitive to conditioning on contemporaneous municipal population. Instead of controlling for post-reform population, the specifications interact log 1966 municipal population with year indicators, using only pre-reform population variation. Because these interactions are estimated separately for each year rather than as a single linear trend, they do not impose a single pre-to-post path through the gradual post-reform build-up documented in the event-study estimates. The female-employment and schooling estimates remain positive and in line with the main results, and the contrast between the compulsory and discretionary schooling margins is preserved: the coefficient is smaller for primary enrollment than for secondary enrollment, as in Table 2.

TABLE A2—ROBUSTNESS TO PREDETERMINED POPULATION TRENDS

	Employment (1)	Secondary Enrollment (2)	Primary School Enrollment (3)
No Monuments $\times$ Post	0.471	0.514	0.215
Standard Error	(0.124)	(0.195)	(0.098)
<i>p</i> -value	0.001	0.014	0.038
Observations	513	439	494
Municipality and Year FE	✓	✓	✓
Log 1966 Population $\times$ Year FE	✓	✓	✓

Note: The table reports robustness checks for the female-outcome specifications in Table 2. The dependent variables are logged female employment in Column 1, logged female secondary school enrollment in Column 2, and logged female primary school enrollment in Column 3. The specifications replace contemporaneous log municipal population with interactions between log 1966 municipal population and year indicators. All regressions include municipality and year fixed effects. Standard errors clustered by municipality are reported in parentheses.

C2. *Leave-One-Treated-Municipality-Out Checks*

Appendix Table A5 examines whether the main female-outcome results are driven by any single treated municipality. The table re-estimates the preferred female-outcome specifications after dropping each treated municipality one at a time. The estimates remain positive and statistically significant for female employment, female secondary enrollment, and female primary school enrollment in every leave-one-out sample.

TABLE A3—ALTERNATIVE DEFINITION OF TREATMENT BASED ON VISIBILITY TO LOCAL POPULATION

	Employment (1)	Secondary School Enrollment (2)	Primary School Enrollment (3)
Panel A: Overall			
Treated	0.610	0.854	0.191
Standard Error	(0.123)	(0.249)	(0.067)
<i>p</i> -value	0.000	0.002	0.008
Observations	513	439	494
Panel B: No Surviving Urban Monument			
Treated	0.657	1.064	0.255
Standard Error	(0.104)	(0.231)	(0.053)
<i>p</i> -value	0.000	0.000	0.000
Observations	513	439	494
Panel C: No Surviving Non-monastery Monument			
Treated	0.628	0.873	0.217
Standard Error	(0.122)	(0.262)	(0.063)
<i>p</i> -value	0.000	0.003	0.002
Observations	513	439	494
Municipality and Year FE	✓	✓	✓
Log Population	✓	✓	✓

Note: The table reports robustness checks using alternative definitions of the treatment. The dependent variables are logged female employment in Column 1, logged female secondary school enrollment in Column 2, and logged female primary school enrollment in Column 3. Panel A reports the main estimates. Panel B expands the main treatment definition to include municipalities where the only monuments of culture were rural. Panel C expands the treatment definition to include municipalities where the only monuments of culture were Monasteries. All regressions include municipality and year fixed effects. Standard errors clustered by municipality are reported in parentheses.

TABLE A4—TESTING THE CONVERGENCE EXPLANATION USING MUNICIPALITY-SPECIFIC PRE-REFORM GROWTH PATHS

	Employment (1)	Secondary Enrollment (2)	Primary Enrollment (3)	Industrial Production (4)
No Monuments $\times$ Post	0.537	0.771	0.133	0.400
Standard Error	(0.089)	(0.241)	(0.057)	(0.210)
Baseline estimate	0.610	0.854	0.191	0.390
Observations	513	439	494	848
Municipality and Year FE	✓	✓	✓	✓
Log Population	✓	✓	✓	✓
Municipality-Specific Pre-Reform Growth Path	✓	✓	✓	✓

Note: The table reports difference-in-differences estimates that control for municipality-specific pre-reform growth paths. For each outcome, a linear trend is estimated separately for each municipality using only observations before 1967 and extrapolated over the full sample period. The resulting control is therefore constructed entirely from pre-reform information and is predetermined with respect to the reform. The baseline-estimate row reports the corresponding preferred estimates from the main specifications for comparison. All specifications include municipality and year fixed effects and log municipal population. All outcomes are in natural logarithms. Standard errors clustered by municipality are reported in parentheses. Because the municipality-specific pre-reform slopes are estimated in a first step, the reported standard errors condition on those estimated slopes.

TABLE A5—LEAVE-ONE-TREATED-MUNICIPALITY-OUT ESTIMATES

Dropped Treated Municipality	Female Employment (1)	Female Secondary Enrollment (2)	Female Primary School Enrollment (3)
Dibër	0.624 (0.131)	0.885 (0.264)	0.189 (0.072)
Gramsh	0.603 (0.130)	0.822 (0.256)	0.202 (0.069)
Krujë	0.645 (0.120)	0.918 (0.242)	0.205 (0.067)
Kukës	0.597 (0.129)	0.914 (0.253)	0.178 (0.070)
Mat	0.630 (0.130)	0.803 (0.258)	0.185 (0.072)
Pukë	0.636 (0.126)	0.852 (0.255)	0.159 (0.061)
Skrapar	0.581 (0.124)	0.790 (0.244)	0.200 (0.069)
Tepelenë	0.624 (0.130)	0.897 (0.259)	0.215 (0.066)
Tropojë	0.556 (0.113)	0.823 (0.255)	0.186 (0.070)
Municipality and Year FE	✓	✓	✓
Log Population	✓	✓	✓

Note: The table reports estimates from regressions that drop one treated municipality at a time. The dependent variables are logged female employment in Column 1, logged female secondary school enrollment in Column 2, and logged female primary school enrollment in Column 3. Each specification includes the treatment-by-post indicator, contemporaneous log municipal population, municipality fixed effects, and year fixed effects. Standard errors clustered by municipality are reported in parentheses. All reported estimates are statistically significant at the 5 percent level.

C3. Poisson Pseudo-Maximum Likelihood Estimates

Appendix Table A6 reports Poisson pseudo-maximum likelihood estimates for the main female outcomes.

TABLE A6—POISSON PSEUDO-MAXIMUM LIKELIHOOD ESTIMATES FOR FEMALE OUTCOMES

	Female Employment (1)	Female Secondary Enrollment (2)	Female Primary School Enrollment (3)
No Monuments $\times$ Post	0.836	1.585	0.259
Standard Error	(0.106)	(0.216)	(0.050)
<i>p</i> -value	0.000	0.000	0.000
Observations	513	439	494
Municipality and Year FE	✓	✓	✓
Log Municipal Population	✓	✓	✓

Note: The table reports Poisson pseudo-maximum likelihood estimates for count outcomes. The dependent variables are female employment in Column 1, female secondary school enrollment in Column 2, and female primary-school enrollment in Column 3, each measured in levels rather than logs. All regressions include municipality and year fixed effects and log municipal population. Standard errors clustered by municipality are reported in parentheses.

C4. Additional Specification, Weighting, and Matching Checks

The following tables report additional checks for sample composition, population weighting, and matched-sample comparisons.

TABLE A7—EXCLUDING TIRANA THE CAPITAL: FEMALE OUTCOMES

	(1) All Municipalities	(2) All Municipalities	(3) Exclude Tirana	(4) Exclude Tirana
Panel A: Employment				
No Monuments $\times$ Post	0.648	0.610	0.620	0.534
Standard Error	(0.117)	(0.123)	(0.117)	(0.104)
<i>p</i> -value	0.000	0.000	0.000	0.000
Observations	513	513	494	494
Panel B: Secondary School Enrollment				
No Monuments $\times$ Post	0.931	0.854	0.863	0.677
Standard Error	(0.222)	(0.249)	(0.219)	(0.209)
<i>p</i> -value	0.000	0.002	0.001	0.003
Observations	439	439	422	422
Panel C: Primary School Enrollment				
No Monuments $\times$ Post	0.265	0.191	0.269	0.151
Standard Error	(0.070)	(0.067)	(0.073)	(0.059)
<i>p</i> -value	0.001	0.008	0.001	0.018
Observations	494	494	475	475
Municipality and Year FE	✓	✓	✓	✓
Log Population		✓		✓

Note: The table reports difference-in-differences estimates from equation (4). The coefficient shown is on No Monuments $\times$ Post. Columns 1 and 2 use all municipalities. Columns 3 and 4 exclude Tirana. All specifications include municipality and year fixed effects. Columns 2 and 4 add log municipal population. Standard errors clustered by municipality are reported in parentheses. *p*-values are reported below the standard errors.

TABLE A8—POPULATION-WEIGHTED ESTIMATES: FEMALE OUTCOMES

Outcome	Coef.	SE	<i>p</i> -value	Observations
Employment	0.649	(0.117)	0.000	513
Secondary Enrollment	0.943	(0.247)	0.001	439
Primary Enrollment	0.215	(0.063)	0.002	494

Note: The table reports population-weighted difference-in-differences estimates for the female outcomes. The coefficient shown is on No Monuments $\times$ Post. All specifications include municipality and year fixed effects and log municipal population. Analytic weights are based on 1966 municipal population. Standard errors clustered by municipality are reported in parentheses.

TABLE A9—POPULATION-WEIGHTED ESTIMATES: PRODUCTION OUTCOMES

Outcome	Coef.	SE	<i>p</i> -value	Observations
Planned Industrial Production	0.006	(0.109)	0.955	104
Production Relative to Plan	0.144	(0.064)	0.033	104
Industrial Production	0.473	(0.260)	0.080	848

Note: The table reports population-weighted difference-in-differences estimates for the production outcomes. The coefficient shown is on No Monuments $\times$ Post. All specifications include municipality and year fixed effects and log municipal population. Analytic weights are based on 1966 municipal population. Standard errors clustered by municipality are reported in parentheses.

TABLE A10—PROPENSITY-SCORE MATCHING

	Industrial Production (1)	Female Employment (2)	Female Primary Enrollment (3)	Female Secondary Enrollment (4)
Panel A: Population and Female Participation Match				
No Monuments $\times$ Post	0.701	0.487	0.136	0.922
Standard Error	(0.298)	(0.135)	(0.073)	(0.326)
<i>p</i> -value	0.030	0.002	0.077	0.011
Observations	652	398	380	340
Panel B: Additional Religiosity Match				
No Monuments $\times$ Post	0.432	0.437	0.103	0.729
Standard Error	(0.334)	(0.115)	(0.081)	(0.316)
<i>p</i> -value	0.212	0.001	0.219	0.032
Observations	652	398	380	340
Municipality and Year FE	✓	✓	✓	✓
Log Population	✓	✓	✓	✓

Note: The table reports matched-sample difference-in-differences estimates. The coefficient shown is on No Monuments $\times$ Post. The matched samples are estimated on common support using nearest-neighbour matching with a 0.06 caliper and no replacement. Panel A matches on 1960 population, female secondary enrollment, and female employment. Panel B additionally matches on 1960 religious buildings per capita and clerics per capita. All specifications include municipality and year fixed effects and log municipal population. Standard errors are clustered by municipality.