Losing Their Religion

By Richard Murphy and Shqiponja Telhaj*

Can states reshape social norms that constrain economic activity? We study whether dismantling the institutions that sustain a social order can change women's education and work. Albania's 1967 religion ban provides an extreme natural experiment: the government proclaimed the world's first atheist state, outlawed religious practice nationwide, and destroyed or repurposed most religious buildings. However, religious buildings protected as national cultural monuments in 1948 remained standing in some municipalities, generating predetermined variation in whether the old religious order disappeared completely from local public view. Using hand-transcribed administrative records from 1956 to 1988, we find substantially larger increases in female employment and schooling where no protected religious monument remained, especially in non-compulsory secondary schooling. Event-study estimates show no differential pre-trends; the effects emerge gradually and extend to industrial production. The government's 1966 Five-Year Plan, set before the ban but covering the post-reform years, shows that these municipalities were not targeted for faster economic or educational expansion, yet after the ban 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 reveal how states can reshape entrenched social norms by removing the public institutions that sustain social authority.

JEL: N34, Z12, O14, P21
Keywords: Religion, social norms, female participation, communism, Albania.

Can states 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 are expected to do and what others will regard as acceptable. Formal religious authority can be abolished immediately, but the social expectations it sustained may persist if households continue to believe that others will enforce the old order. We test whether women's education and work change more when the institutions that sustained those norms also disappear from public view.

Albania's 1967 religion ban provides an extreme natural experiment: the state declared Albania the world's first atheist state, outlawed religious practice nationwide, abolished the legal status of religious institutions, and closed every mosque, church, and monastery for worship. Most religious buildings were demolished or converted to secular uses. However, a small number had been listed before the reform as monuments of national cultural heritage and remained intact, although they too ceased to function as places of worship. The legal ban was national, but municipalities differed in whether any protected religious monument remained standing in the local public environment (Gjeçovi, 2008; Bickert, 2018).

This difference is central to our empirical design because a protected religious monument could matter even after it ceased to function as a religious institution: legal abolition removed formal religious authority everywhere, but a standing monument kept the old order visible in local public life. Our principal interpretation is that the complete disappearance of the old religious order from public view weakened the social authority of religiously reinforced gender norms. The variation exploited in the empirical design originates in national heritage classifications made nearly two decades before the reform. In 1948, the Institute of Sciences compiled Albania's first broad 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

* 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, IWAEE, and the Riinvest Institute in Prishtina. Special thanks to Amarildo Harka, Camilo Anton Cepeda Francese, and Liri Kuci for their excellent research assistance, and to Chari Amalavoyal, Sam Arenberg, Jean-Paul Carvalho, Uran Ferizi, Colin Green, Dan Hamermesh, Indrit Hoxha, Iraj Hashi, 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. Richard Murphy: Department of Economics, University of Texas at Austin, Population Research Center, NBER and CEP/LSE, IZA, CESifo. Shqiponja Telhaj: Department of Economics, University of Sussex and Centre for Economic Performance, London School of Economics. The authors received no external financial support specifically for this research and have no financial relationships or other potential conflicts of interest to disclose.

percent — had been protected in this way. Therefore, municipal treatment status did not arise from differential exposure to legal abolition; it reflected pre-reform national heritage classifications that determined whether a municipality retained a protected religious monument after the reform.

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 Danaj, 2016). These policies lowered some practical barriers to participation, but they did not necessarily change expectations about what other households would regard as acceptable.

The limits of these earlier reforms mattered economically as well. Albania's industrialization strategy 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 obstacles to female emancipation and industrialization (Gjeçovi, 2008; Këlliçi and Danaj, 2016). In the regime's view, those earlier reforms were insufficient. Changing women's roles also required confronting the social order that sustained them. The 1967 reform was distinctive in precisely this respect: it abolished formal religious authority and worship nationwide, but the complete disappearance of the old religious order from local public view occurred only in municipalities with no surviving protected religious monument.

We formalize this interpretation 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 where no protected religious monument remained standing. The model also shows how these effects 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. These records allow us to construct a municipality-year panel covering all 26 Albanian municipalities from 1956 to 1988, with annual information on female employment, schooling, industrial production, agricultural production, and population, as well as measures of pre-reform religious institutions. A distinctive feature of the archival data is the availability of municipality-level targets from the government's 1966 Five-Year Plan. These targets were set before the 1967 religion

ban but covered the post-reform years. For industrial production, the planning accounts also report realized industrial output alongside these targets. This allows us to compare each municipality's realized industrial output with its planned level, providing a municipality-specific counterfactual benchmark rather than relying only on extrapolated pre-reform trends. We also use targets for circulation, consumption, and school supply to test whether treated municipalities were assigned faster planned expansion in other economic outcomes or school supply.

Using a difference-in-differences design, we find that municipalities in which no protected religious monument remained standing experienced substantially larger post-reform increases in female employment and schooling. Female employment rose by 0.610 log points relative to municipalities in which at least one protected monument remained. Female secondary-school enrollment rose by 0.854 log points, while female primary-school enrollment rose by 0.191 log points. The larger response at the secondary level is what a norms-based mechanism would imply, since secondary schooling was the margin over which households had greater discretion. Event-study estimates show no evidence of differential pre-trends before the reform. After 1967, the effects build gradually and begin to plateau, as the model predicts, reflecting adjustment toward a new equilibrium through changing social expectations rather than a one-time level shift.

Treated municipalities were not assigned higher planned industrial growth in the government's 1966 Five-Year Plan, but after the reform they performed better relative to plan. Industrial production relative to plan rose by 0.196 log points more in treated than in control municipalities. Annual industrial production also increased. These gains do not appear to come at the expense of agriculture: agricultural production shows no offsetting decline.

The main identification concern is that monument survival may proxy for pre-existing municipal differences or correlate with differential state targeting. Several results make these explanations unlikely. The government's 1966 Five-Year Plan shows that treated municipalities were not assigned higher planned industrial growth, and planned consumption and circulation do not indicate broader targeting. Planned school-supply targets also provide no evidence that treated municipalities were scheduled for faster expansion in school supply. Observable measures of pre-reform religiosity—religious buildings and clerics per capita—are similar across treated and control municipalities, and the estimates are robust to allowing baseline religiosity to predict differential post-reform changes. Protected non-religious monuments from the same 1948 heritage classification also do not differ significantly between treated and control municipalities, and accounting for their pre-reform density leaves the estimates essentially unchanged, making broader historical or cultural differences an unlikely explanation. Pre-reform female political representation is also similar across the two groups, and the results are robust to controls for state cultural infrastructure and female deputies. Given the municipality-level design, we complement the baseline estimates with randomization inference, leave-one-treated-municipality-out checks, alternative treatment

definitions, population weighting, matching, and specifications that use only pre-reform population variation.

To explore potential mechanisms, we test whether the response reflects attachment to the locally dominant faith or the continued visibility of the old religious order. Among municipalities that retained a protected religious monument, post-reform outcomes do not differ systematically by whether the surviving monument matched the locally dominant faith. Moreover, municipalities with no surviving protected monument experienced larger post-reform increases than those whose only surviving protected monument belonged to a non-dominant faith. Even a church in a predominantly Muslim municipality, or a mosque in a Christian one, appears to have been sufficient to keep the old religious order visible in local public life. The relevant margin is not whose faith the building represented, but whether any protected religious monument remained. Consistent with this interpretation, the effects are largest when no protected religious monument remained and decline monotonically as the number of surviving protected monuments increases.

Overall, the evidence shows that the relevant empirical margin is the complete disappearance of the old religious order from local public view. The results are unlikely to be driven by pre-reform religiosity, differential state targeting or mobilization, broader historical or cultural differences, denomination-specific attachment, or sectoral reallocation.

Finally, exploratory individual-level evidence from the 2008–09 Albania Demographic and Health Survey, collected two decades after our archival panel ends, extends the analysis to longer-run 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 and narrow across successive cohorts, becoming much smaller among cohorts born around and after the reform, indicating that the reform's effects persisted well beyond the period covered by our archival panel.

The paper makes three contributions. First, it contributes to the economics of culture and social norms. Existing work has emphasized 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 contribute by identifying a mechanism through which formal reform can change social behavior: when the institutions that sustained the old social order are removed from local public view, beliefs about whether that order remains socially enforced can change. The mechanism does not require private religious belief itself to change, only beliefs about whether others will continue to enforce the old order. The denomination-specific results reinforce this interpretation: the response does not

depend on whether a surviving monument matches the locally dominant faith.

Second, the paper contributes to research on gender norms and female participation. 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). We show that policies expanding women's rights and opportunities need not change behavior when traditional gender roles remain socially enforced. Before 1967, Albania had already introduced legal equality, secular education, childcare, and policies promoting women's employment; after the religion ban, women's schooling and employment increased substantially more where the institutions reinforcing those roles disappeared from local public view.

Third, the paper contributes to the economics of religion. 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 contribute 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. The results show that 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 everyday life. When those visible reminders disappear, people 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 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 Section VI examines alternative explanations and robustness. Section VII considers interpretation, longer-run persistence, 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 (Zickel and Iwaskiw, 1994). On the eve of communist rule, the population was religiously diverse: in 1942, 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 inherited by 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)[1]. 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.[2] Formal political life remained male-dominated: in 1966, women held about 16 percent of seats in the People's Assembly. 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).[3] Female employment was therefore not only an ideological objective, but also an economic one.

B. Conventional 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 regime also pursued more direct reforms to increase women's participation in public life. It used legal reform, propaganda, expanded schooling and childcare provision, and campaigns encouraging women into education and employment. These policies were part of

[1]The treatment classification does not reproduce the historical geography of the *Kanun*. Shkodër, Lezhë, and Mirditë—areas in which customary law was particularly influential—are all controls, whereas treated municipalities such as Gramsh, Skrapar, and Tepelenë were not core Kanun areas. Differences in customary-law exposure are unlikely to explain the treatment pattern (Hasluck, 1954).

[2]This 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).

[3]Këlliçi 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.

a broader socialist emancipation agenda in which female employment was presented both as liberation and as the mobilization of a necessary productive labor force (Këlliçi 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 (Këlliçi and Danaj, 2016), and party discourse increasingly identified patriarchal customs and religious authority as obstacles to women's emancipation.

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 other households would regard as acceptable. If traditional gender roles, reinforced by customary and religious authority, remained socially enforced, unilateral deviation could still be costly. 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 end of 1966. The government increasingly identified religion and "backward customs" as obstacles to female emancipation and industrialization. Enver Hoxha, the communist leader, argued that women's emancipation could not be achieved by decree alone, but had to be realized through work, schooling, and education. Accordingly, the state came to treat religion not only as private belief, but as part of a social order that sustained women's household-centered role (Gjeçovi, 2008; Këlliçi and Danaj, 2016).

In 1967, 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.[4] The reform also transformed the religious built environment. Most religious buildings were demolished, converted to secular uses, or de-sanctified. 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 the transformation of the Mosque of Durrës. After the 1967 religion ban, the mosque no longer functioned as a place of worship and was repurposed as *Pallati i Rinisë* (the Palace of Youth). The example shows what de-sanctification and secular repurposing meant in practice: the national ban not only ended a building's religious use, but could also change its public function and meaning.

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:

[4]The 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 figure illustrates how secular repurposing altered both the function and public meaning of a formerly religious building after the ban.

municipalities differed in whether the old religious order remained visible in local public life. The next subsection describes the pre-existing heritage classifications 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. The technical identification and evaluation of monuments were undertaken 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 classifications were 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 county-level monument lists.[5]

These protected buildings were closed for worship, like all religious buildings, but remained physically present in the municipality. Thus, pre-reform protection status governed whether a religious monument remained standing after the reform, rather than contemporaneous local discretion. This institutional detail is central to the empirical design. Because religious practice was banned everywhere, municipalities did not differ in legal exposure to the reform. They differed in whether any protected religious monument remained visible in local public life.

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.

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. When women's participation decisions are socially interdependent, a household's choice depends not only on private incentives but also on expectations about the behavior and judgments of others. 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 $f > 0$

[5]The 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.

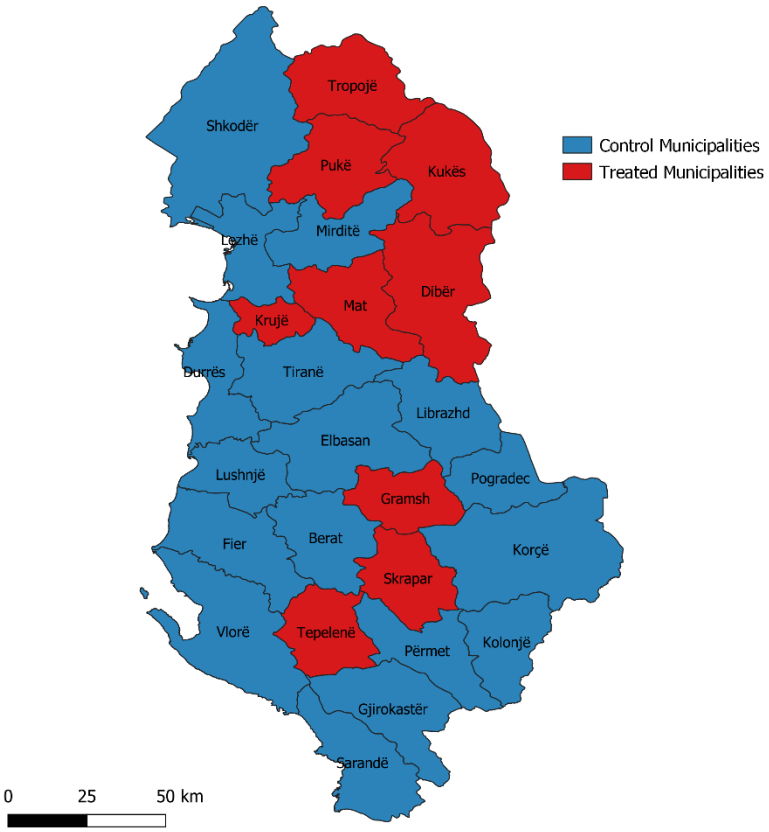


Figure 2. Albania—Treatment and Control Municipalities

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 the visible survival of protected religious monuments.

over the relevant range. Participation requires a private cost $c > 0$ [6] 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.

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 protected religious monument remained publicly visible. The third is the local public visibility channel: after the reform, the social cost is lower where no protected religious monument remained publicly visible.

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:

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

Define the participation response as:

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

[6] The 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.

Because G is continuous and maps $[0,1]$ into itself, 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 $V(R,0) = 1$ under 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:

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

The post-reform participation response under complete local removal lies everywhere above the response under continued public visibility. Appendix A shows that the adjustment paths converge and that, from the municipality's own pre-reform participation level, the limiting 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 the post-reform participation level selected by the adjustment process under complete local removal and $x^{\text{post}}(1)$ the counterfactual post-reform level selected when a protected religious monument remains publicly visi-

ble. The model implies:

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

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.

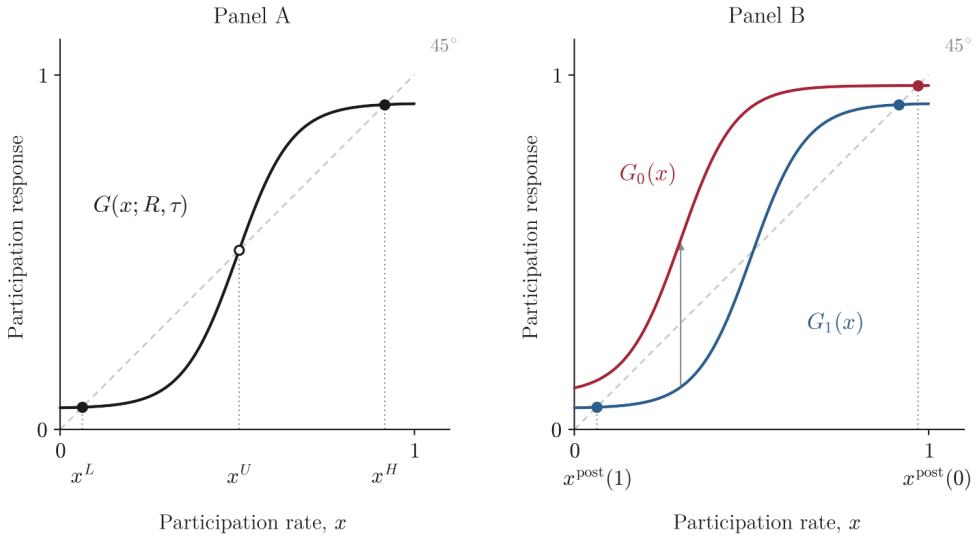


FIGURE 3. PUBLIC VISIBILITY AND PARTICIPATION CHANGE

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 public visibility of at least one protected religious monument, 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 public 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.

C. Empirical Predictions

The model yields two main empirical predictions.

First, municipalities in which no protected religious monument remained publicly 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 VI.D 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 explains how the employment and schooling outcomes map onto the model's concept of female participation and presents the corresponding empirical specifications.

III. Data and Descriptive Evidence

We combine archival and planning data to construct multiple datasets on Albania before and after the 1967 religion ban. The administrative panel is the core dataset. It allows us to study municipality-level changes in female employment, schooling, and production over time, with female employment as the central outcome. The State Planning Commission records provide a rare opportunity to observe counterfactual expectations directly. The Commission's planning files, dated December 1966,[8] report municipality-level targets for 1967 and 1970. Be-

[7]Let $G_0(x) = 1 - F(c + s(x, 0, 1))$ 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.

[8]The archival cover identifies the dossier as State Planning Commission file no. 74, marked "Secret," for the Fourth Five-Year Plan, 1966–1970. It records that the file was opened on 23 December 1966.

cause these targets were set before the religion ban, they allow us to test whether treated municipalities were expected to grow faster and to compare realized post-reform production with targets assigned before the reform.

A. Archival Data: Administrative Records, Religious Buildings and State Plans

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 [9] and secondary school enrollment, and industrial production.

This ordering reflects the policy setting. The communist state sought to industrialize rapidly and viewed women as an underused half of the labor force. Female labor mobilization was therefore not only an ideological objective, but also an economic one. The empirical analysis first examines whether women's employment and schooling increased more in municipalities where the old religious order disappeared fully from public view. It then investigates whether these changes were followed by downstream changes in 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 describe pre-reform religiosity and to test whether municipalities with and without a surviving protected monument differed in observable religious intensity 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. Most were destroyed, converted to secular uses, or de-sanctified. 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.

Importantly, buildings without protected status were either physically destroyed or de-sanctified and repurposed for secular civilian use, such as warehouses, sports halls, or youth centers. None were converted to industrial or production use. The protected-monument designation therefore generated the post-reform variation

[9] Basic 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).

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 there were no protected religious monuments. Control municipalities are those in which at least one protected religious monument remained physically intact. This distinction matters because the national ban eliminated legal worship everywhere. What varied was whether households continued to observe a protected religious monument in their own municipality. Therefore, the treatment captures the complete local removal of the old religious order from public view, not exposure to the legal ban itself.

The State Planning Commission data are central to the empirical design. We supplement the administrative panel with municipality-level records from the 1966 Five-Year Plan, prepared before the religion ban and ratified at the Fifth Party Congress. The planning records report both realized values and planned targets across a broad set of outcomes. Table 1 reports pre-reform values for 1965–66 alongside the planned target for industrial production, while Appendix Figure A2 plots the broader 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 broader local public-service capacity. We use these measures later to evaluate alternative explanations based on differential public investment and state cultural mobilization. The plan provides the state's own pre-reform expectations about municipal growth. This is important because a central identification concern with a DID approach 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 production plans.

We use the planning data in two ways. First, we test whether municipalities without surviving protected monuments were assigned faster planned expansion in industrial production, circulation, consumption, and school supply under the 1966 Five-Year Plan, which was set before the reform but covered the post-reform years. 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 broader municipal growth or sectoral reallocation. Table 1 reports pre-reform municipality characteristics by treatment status.

[10]Circulation 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 (000s)				
Population (1946)	51.96	26.09	26.53	13.95
Employment	3.39	4.21	0.36	0.32
Primary School Enrollment	6.64	4.31	2.69	1.52
Female Secondary Enrollment	0.67	1.09	0.04	0.03
Industrial Production per Capita	1.38	0.98	0.55	0.47
5-Year Plan in 1965-66				
Financial (MLek)				
Ln Industrial Production	4.05	1.48	2.50	1.11
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
Infrastructure (per 1,000)				
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 religious monument remained physically visible in the municipality after worship was banned, 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 per capita. The 1966 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 levels for treated municipalities, consistent with their smaller size and lower baseline production, while the planned difference is not statistically significant. The descriptive evidence therefore 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 that proxy for local social structure, state cultural infrastructure, and female political mobilization. Religious buildings and clerics per capita capture observable religious intensity before the reform. 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. 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 local social structure, state cultural presence, or female political mobilization rather than the post-reform disappearance of protected religious monuments from public view.

Exploiting the annual structure of the archival panel data, 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 participation.

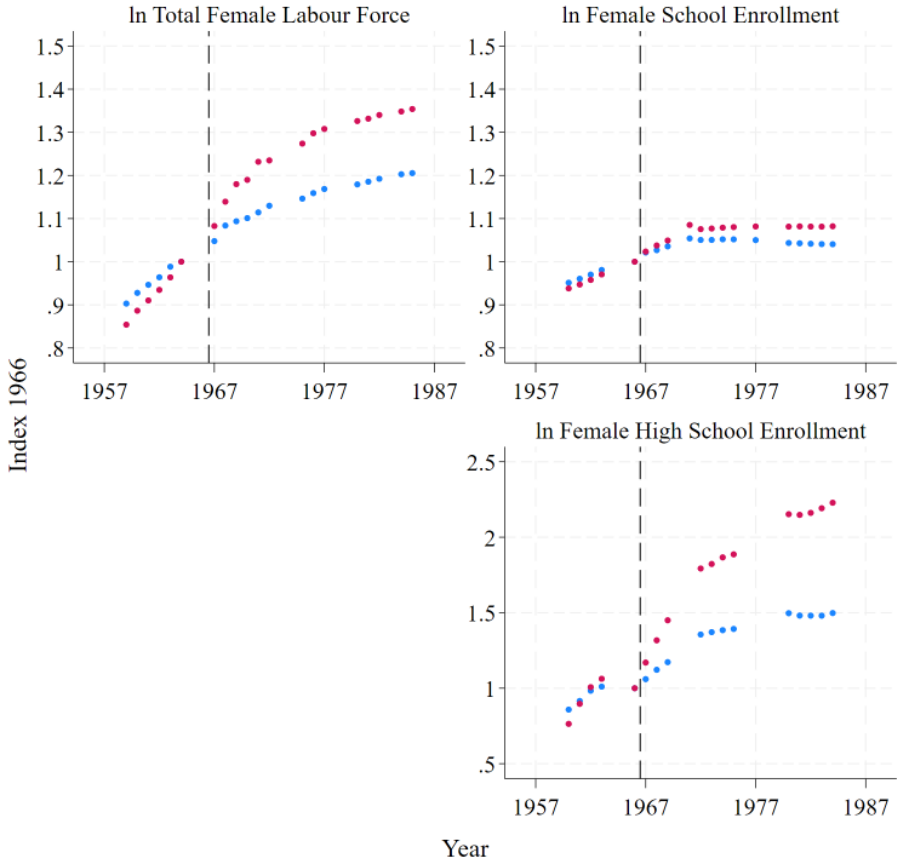


FIGURE 4. FEMALE EMPLOYMENT AND SCHOOL ENROLLMENT

Note: Panels plot annual indices for female employment, female primary school enrollment, and female secondary school enrollment by municipality-year. 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 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. 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. Planned industrial production moves similarly for treated and control municipalities over the Five-Year Plan period, consistent with the state not assigning faster expected industrial growth to treated municipalities 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.

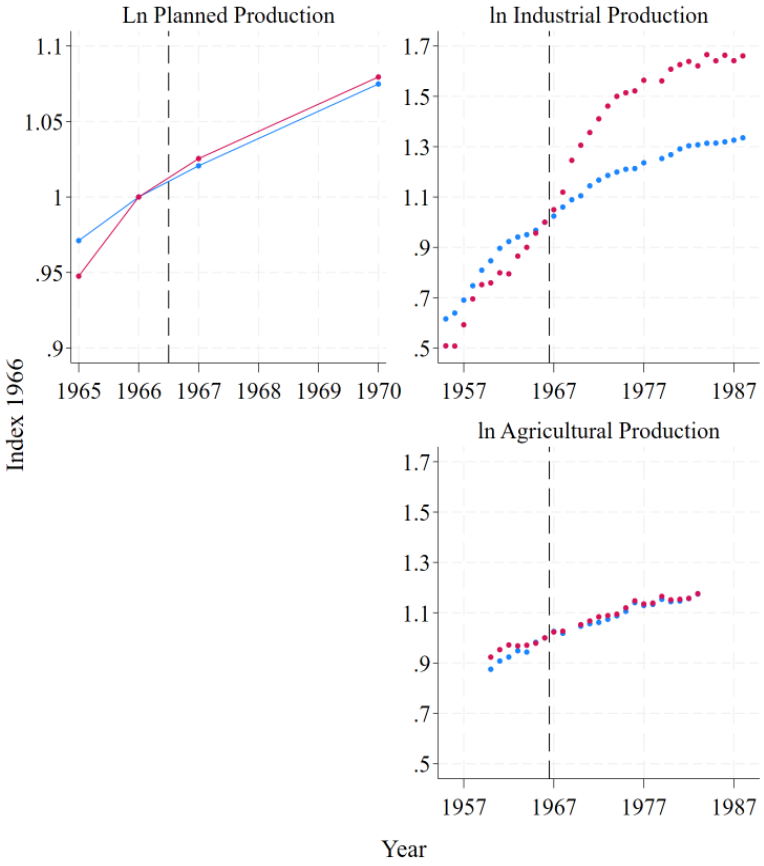


Figure 5. Industrial Plans, Production, and Agriculture

Note: Panels plot annual indices for planned industrial production, realized industrial production, and agricultural production by municipality-year. 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 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. 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.

IV. Empirical Strategy

We use a municipality-level difference-in-differences design as the main empirical strategy. The administrative panel spans 1956 to 1988 and compares changes across municipalities. The production analysis incorporates the government's pre-reform Five-Year Plans directly: we test whether treated municipalities were assigned faster planned industrial growth before the reform and whether realized production exceeded targets set before the reform.

A. Difference-in-Differences Specification

As described in Section I, our identification exploits the pre-reform heritage classification of religious buildings as monuments of national cultural heritage in 1948, almost two decades before the 1967 religion ban. Because religious practice was banned everywhere, treatment is not exposure to the legal ban itself, but whether the old religious order disappeared fully from public view after the reform. 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 (see Figure 2). We estimate equations of the form:

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

Here, 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 a time-varying control X_{ct}, for log municipal population. The coefficient of interest is β.

To map this specification to the model, recall that the model is written in terms of female participation in public life, which includes work and schooling. Because the control municipalities tend to be larger than the treated municipalities, our main specifications use logged municipality-year counts and include municipality fixed effects, year fixed effects, and log municipal population. We also provide estimates for the female share of employment and schooling as complementary evidence on whether women's participation increased relative to the corresponding municipal totals.

To test directly for differential pre-trends and examine the timing of the treatment effect, we estimate the following event study equation:

$$Y_{ct} = \beta_0 + \sum_{\substack{\tau=-10 \\ \tau\neq 0}}^{20} \beta_\tau \, 1\{t - 1966 = \tau\} \, \text{NoMonuments}_c + \gamma X_{ct} + \delta_t + \mu_c + \varepsilon_{ct}. \tag{5}$$

where τ indexes years relative to 1966, the omitted reference year, running from approximately -10 to $+20$ depending on data availability for each outcome, and omitting $\tau = 0$ as the reference year. 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 outcome. The historical source of treatment is useful for this assumption: post-reform treatment status was generated by heritage classifications dating back to 1948, not by contemporaneous municipal choices or by differential exposure to the national religion ban.

The remaining concerns are that this classification may still 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 differential post-reform state targeting or investment not visible in the 1966 plans. Section VI addresses these concerns directly. We use the 1966 Five-Year Plans to test for differential targeting in planned industrial production, school supply, circulation, and consumption; examine baseline religiosity, protected non-religious monuments, state cultural infrastructure, and female political representation; allow baseline population to predict differential time paths; and examine denomination-specific attachment, agricultural production as a test for sectoral reallocation, alternative treatment definitions, municipality size, population weighting, matching, and randomization inference.

V. Results

A. Female Employment and School Enrollment

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

The female-employment estimates in Panel A are large and robust across specifications: female employment within treated municipalities rose by 0.610 log points more following the reform than employment within control municipalities. The estimates remain large and statistically significant under more demanding specifications, including region-specific linear trends in Appendix Table A1.[11] This pattern is consistent with the model's central prediction that female employment increased more where the old religious order disappeared fully from public view.

[11] We report region-specific linear trends as a robustness specification rather than in the baseline because the treatment effects build gradually over time (Figure 6), and group-specific linear trends estimated over the full panel may therefore absorb part of the dynamic post-reform response (Wolfers, 2006). The estimates nonetheless remain positive and statistically significant with region-specific trends included (Appendix Table A1).

Table 2—Female Employment and Female School Enrollment

	(1) Fixed Effects	(2) Population Controls
Panel A: Employment		
No Monuments × Post	0.648	0.610
Standard Error	(0.117)	(0.123)
p-value	0.000	0.000
Observations	513	513
Panel B: Secondary School Enrollment		
No Monuments × Post	0.931	0.854
Standard Error	(0.222)	(0.249)
p-value	0.000	0.002
Observations	439	439
Panel C: Primary School Enrollment		
No Monuments × Post	0.265	0.191
Standard Error	(0.070)	(0.067)
p-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 logged female employment, logged female secondary school enrollment, and logged female primary school enrollment. The difference-in-differences coefficient reported is No Monuments × Post. No Monuments equals one for municipalities in which no protected religious monument remained standing after the reform, and zero for municipalities in which at least one protected religious monument remained standing. Post equals one for years after 1966. 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.

Female schooling also increased more in treated municipalities, with the strongest response at the secondary level. The preferred estimate for female secondary enrollment is 0.854 (Panel B). This is the more informative schooling margin because secondary school was not compulsory during this period, so it more directly reflects household decisions about girls' continued education and future roles. The effect on female primary school enrollment is also positive but smaller, with a preferred estimate of 0.191 (Panel C). This more modest response is consistent with primary schooling being compulsory during this period. [12]

Figure 6 reports the corresponding event-study estimates. The female-employment panel tests the model's central prediction and provides evidence on the model's pre-reform restriction that future monument survival did not predict differential female-participation paths before the reform. The pre-reform coefficients are small and statistically indistinguishable from zero, and the joint test of the pre-reform coefficients does not reject the null that they are jointly zero.[13] After 1967, the coefficients rise gradually and then stabilize at a higher level. This pattern is consistent with Prediction 2 of the model, which implies gradual adjustment as households update expectations and participation converges toward a new equilibrium, rather than a one-time level shift immediately after the reform.

The female-schooling panels mirror the pattern in Table 2: the post-reform response is stronger for secondary enrollment than for primary enrollment. Secondary enrollment continues to rise through most of the post-reform period, although the rate of increase slows toward the end of the sample. Secondary schooling is the margin over which households had greater discretion, since secondary school was not compulsory and reflected decisions about girls' continued education. Primary school enrollment also increases, but the response is smaller, consistent with compulsory primary schooling leaving less scope for household choice. Joint tests of the pre-reform coefficients likewise do not reject the null of no differential pre-trends for either schooling outcome. The stronger response at the secondary-school margin is consistent with the model's threshold intuition: participation responds more strongly on margins where households retain greater discretion and more households may be close to the participation threshold.

[12] Appendix Table A2 shows that the female employment and schooling estimates remain positive and substantial when contemporaneous population is replaced with interactions between log 1966 municipal population and year indicators.

[13] The joint test of the pre-reform coefficients gives $F(5, 25) = 1.62$, with Prob $> F = 0.191$.

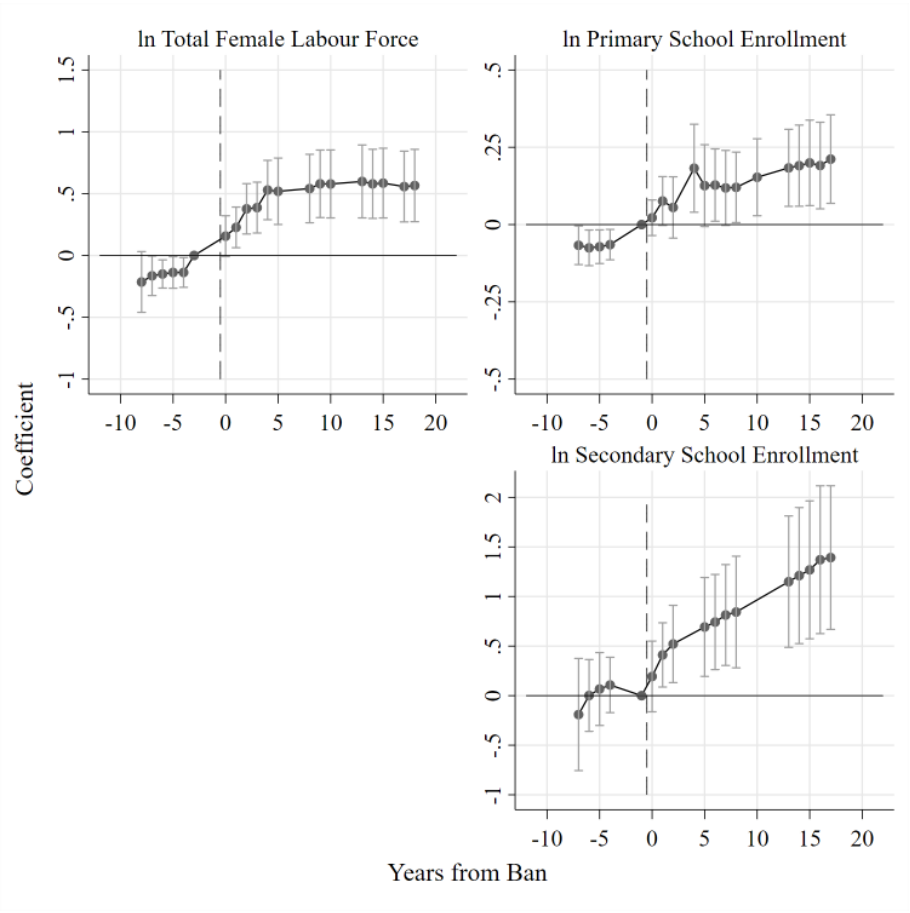


Figure 6. Event-Study Estimates for Female Employment and Female School Enrollment

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 1966 Five-Year Plans*

Table 3 reports difference-in-differences estimates for production. Panels A and B use State Planning Commission Five-Year Plan records. Panel A uses planned industrial production, while Panel B uses production relative to plan, defined as realized log industrial production minus planned log industrial production. Panel C uses realized industrial production from the annual administrative production records. The planning results provide a direct check on differential

Table 3—Production and the 1966 Five-Year Plans

	(1) Fixed Effects	(2) Population Controls
Panel A: Planned Industrial Production		
No Monuments × Post	-0.014	-0.023
Standard Error	(0.106)	(0.098)
p-value	0.894	0.817
Observations	104	104
Panel B: Production Relative to Plan		
No Monuments × Post	0.201	0.196
Standard Error	(0.094)	(0.089)
p-value	0.042	0.037
Observations	104	104
Panel C: Industrial Production		
No Monuments × Post	0.426	0.390
Standard Error	(0.246)	(0.224)
p-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 logged planned industrial production from the 1966 Five-Year Plans in Panel A, realized log industrial production minus planned log industrial production in Panel B, and logged realized industrial production from the annual administrative production 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 and *p*-values are reported in the following row.

state targeting and also support the plausibility of the counterfactual parallel-trends assumption. Panel A shows no evidence that treated municipalities were assigned faster planned industrial growth. Thus, the state's pre-reform plans do not indicate that treated municipalities were expected to grow faster. Production relative to plan tells a different story: Panel B shows that plans did not differ across treated and control municipalities, but performance relative to those plans did. Production relative to plan in treated municipalities increased by 0.196 log points more than in control municipalities.

The annual administrative production series in Panel C also shows a positive effect on realized industrial production. The estimate is statistically significant. Because the administrative series is observed annually over a much longer period,

it also shows that the production response is sustained across multiple post-reform years. Realized industrial production displays no differential pre-trend before 1967; after the reform, the coefficients turn positive and remain so through the end of the sample, although the confidence intervals are wide.

Figure 7 reports event-study estimates for realized industrial production.

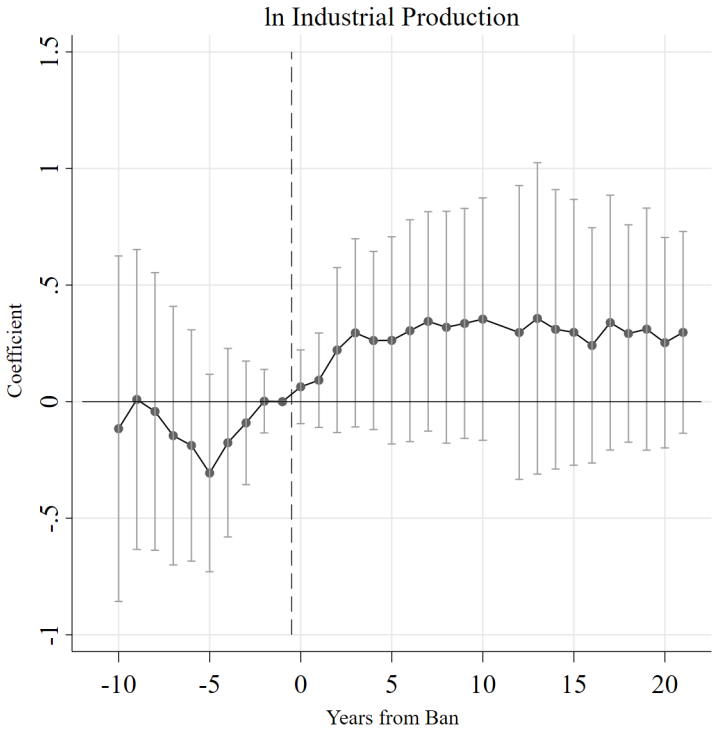


Figure 7. Event-Study Estimates for Industrial Production

Note: The figure plots event-study coefficients from equation (5) for logged 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.

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, as well as stronger industrial performance relative to plan. This section examines the main alternative explanations for these findings, provides additional evidence for inter-

preting them, and assesses the robustness of the empirical design. The analysis considers pre-reform targeting and pre-existing religious, historical, and cultural characteristics; the female response and what monument survival captures; sectoral alternatives; and additional robustness checks.

A. State Targeting and the Five-Year Plans

A central identification concern is that treated municipalities may have been targeted by the state for faster post-reform development. The 1966 Five-Year Plan provides a direct test of this possibility because it records municipality-level targets set before the religion ban for outcomes realized afterward. As shown in Table 3, treated municipalities were not assigned faster planned industrial growth. Therefore, the planning evidence provides direct evidence against differential state targeting and also supports the plausibility of the counterfactual parallel-trends assumption.

We extend this test to broader measures of planned economic activity. Appendix Figure A2 shows no differential planned growth in circulation, while planned consumption growth is lower in treated municipalities, running against an interpretation based on favorable state targeting. The absence of differential planned growth is not specific to industrial production. A separate concern is that treated municipalities may have been scheduled for faster expansion in school supply, which could account for the female-schooling results independently of changes in household participation. The top right panel shows that treated and control municipalities had the same planned growth for the number of schools. Planned health-care-bed provision is also similar across the two groups, providing no indication that treated municipalities were favored in broader local public-service provision.

The Five-Year Plan records provide no evidence that treated municipalities were assigned faster planned growth in industrial production, circulation, or consumption, nor scheduled for faster expansion in school supply. This also supports the interpretation that the post-reform increase in production relative to plan reflects stronger performance in treated municipalities, rather than more favorable pre-reform targets.

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

Pre-reform religiosity.

A natural alternative interpretation is that treatment status proxies for pre-existing religious intensity rather than the local disappearance of the old religious order from public view. In particular, municipalities in which no protected religious monument remained standing after the reform may have differed before 1967 in observable religious intensity, and these differences may have generated

different post-reform trajectories for reasons unrelated to the local disappearance of the old religious order.

We test this possibility by allowing baseline religiosity to predict differential post-reform changes. We interact pre-reform religious buildings per capita and clerics per capita 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 × Post should be sensitive to these controls. Table 4 shows no evidence that observable pre-reform religiosity explains the estimated female employment and schooling effects.

Table 4—Baseline Religiosity and Female Outcomes

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

Note: The table reports difference-in-differences estimates from equation (4). The coefficient shown is on No Monuments × 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 across municipalities 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 2.8 protected non-religious monuments per 10,000 inhabitants, compared with 1.9 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 5 reports the results.

Table 5—Protected Non-Religious Monuments and Female Outcomes

	(1) Baseline	(2) Heritage × Post
Panel A: Employment		
No Monuments × 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 × 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 × 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 × 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 10,000 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.

The coefficient on No Monuments × Post is 0.586 for female employment, compared with the baseline estimate of 0.610; 0.826 for secondary-school enrollment, compared with 0.854; and 0.177 for primary-school enrollment, compared with 0.191. All three estimates remain positive and statistically significant. The female employment and schooling effects do not appear to be explained by differential post-reform trajectories associated with broader pre-existing historical or cultural characteristics.

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 under communism 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 complementary 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 differential socialist mobilization rather than the local disappearance of the old religious order from public view.

We examine this concern using two pre-reform measures. The first is the number of cultural houses and cultural hearths (*shtëpi dhe vatra kulture*) per 1,000 inhabitants in 1966. The second is the number of 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. [14]

Table 6 reports the results. The coefficients on No Monuments × Post remain positive and statistically significant for all three female outcomes across these specifications. The female employment and schooling results do not appear to be explained by differential post-reform trajectories associated with pre-reform state cultural mobilization or female political representation.

[14] Appendix Figure A2 shows that treated and control municipalities had similar planned expansion of Cultural Houses in the 1966 Five-Year Plans.

Table 6—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 × Post	0.610	0.624	0.436	0.439
Standard Error	(0.123)	(0.119)	(0.089)	(0.099)
p-value	0.000	0.000	0.000	0.000
Observations	513	513	494	494
Panel B: Female Secondary School Enrollment				
No Monuments × Post	0.854	0.882	0.451	0.426
Standard Error	(0.249)	(0.245)	(0.198)	(0.246)
p-value	0.002	0.001	0.032	0.096
Observations	439	439	422	422
Panel C: Female Primary School Enrollment				
No Monuments × Post	0.191	0.195	0.128	0.125
Standard Error	(0.067)	(0.068)	(0.059)	(0.068)
p-value	0.008	0.009	0.039	0.078
Observations	494	494	475	475
Municipality and Year FE	✓	✓	✓	✓
Log Population	✓	✓	✓	✓
Female Deputies × Post		✓		✓
Cultural Houses × Post			✓	✓

Note: The table reports difference-in-differences estimates from equation (4). The coefficient shown is on No Monuments × 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 recorded at the city rather than district level, 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

The main results show substantial post-reform increases in female employment and female secondary-school enrollment. A natural question is whether these effects reflect a specifically female response or increases in employment and schooling that affected the municipality more generally. The data do not provide municipality-year population by age and sex, so we cannot construct comparable gender-specific participation rates. We can, however, 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. By contrast, if the reform weakened gender-specific constraints on women's work and schooling, female participation 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 composition outcomes as dependent variables.

Table 7 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 estimates are statistically significant. The female-share evidence supports a gendered response, with the clearest increase on the secondary-schooling margin.

Table 7—Female Shares in Employment and Secondary Schooling

	Female Employment Share (1)	Female Secondary Enrollment Share (2)
No Monuments × 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 diagnostics. 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.[15] The 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.[16] 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 complementary evidence rather than as direct estimates of gender differences. A municipality-wide opportunity shock could still increase female shares if women's responses were disproportionately large. Archival records of building repurposing show, however, that none of the repurposed religious buildings in the sample were converted into schools. Together with the Five-Year Plan evidence on school supply, this makes a direct schooling-capacity explanation for the secondary-schooling response less likely.

D. What Does Monument Survival Capture?

The baseline treatment distinguishes municipalities in which the old religious order disappeared completely from the local public environment after the 1967 reform from those in which at least one protected religious monument remained visible. We examine what this distinction captures along two dimensions. First, we ask whether post-reform responses depend on the denomination of the monument that survived. Second, we ask whether responses vary with the extent to which protected religious monuments remained visible after the reform.

Denomination of the surviving monument.

The model implies that the relevant empirical distinction is whether any protected religious monument remained visible in the municipality, rather than the denomination of that monument. Worship was banned everywhere after 1967, and surviving protected buildings no longer had any formal religious function. Yet a standing protected monument could keep the old religious order present in the local public environment.

Table 8 reports two tests of this implication. 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 surviving monument did not match the locally dominant religion with municipalities where it did. The estimates show no systematic differences by denomination conditional on monument survival. If attachment to one's own faith's buildings drove the results, municipalities that retained only a monument of another denomination should have responded differently from municipalities that retained a monument of the dominant faith. They did not.

[15]The joint pre-trend test gives $F(5, 25) = 1.02$ and $p = 0.425$.
[16]The joint pre-trend test gives $F(4, 25) = 1.22$ and $p = 0.327$.

Panel B compares municipalities where a protected religious monument survived but did not match the locally dominant religion with municipalities where no protected religious monument remained standing. This comparison asks whether the survival of even a non-dominant monument muted the post-reform response relative to complete local removal. The estimates are negative for female employment, secondary-school enrollment, and primary-school enrollment, indicating weaker post-reform responses where a non-dominant monument survived. The denomination evidence therefore supports the interpretation that the relevant distinction is monument survival rather than denomination-specific attachment: conditional on survival, denomination does not predict systematically different responses, while even a non-dominant monument is associated with a weaker response than complete local removal.

Table 8—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 × Post	-0.238	-0.256	-0.079
Standard Error	(0.204)	(0.219)	(0.083)
p-value	0.264	0.261	0.354
Observations	338	389	303
Panel B: No Monument versus Non-Dominant Monument			
Non-Dominant Monument × Post	-0.512	-0.469	-0.163
Standard Error	(0.219)	(0.244)	(0.061)
p-value	0.023	0.058	0.010
Observations	275	266	235
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 surviving monument did not match the locally dominant religion with the 13 municipalities where it did. The coefficient reported is Non-Matching × Post. Panel B restricts the sample to the 9 municipalities with no protected religious monument and the 4 municipalities where the surviving protected religious monument did not match the locally dominant religion. The coefficient reported is Non-Dominant Monument × 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 and p-values are reported in the following row.

Extent of monument survival.

The denomination results support the view that the identity of the surviving monument is not the relevant distinction. We next examine whether the amount of the old religious order that remained visible in public life matters. If complete local disappearance is the salient empirical margin, the post-reform response should be strongest where no protected religious monument survived and should weaken as progressively more of the pre-reform religious landscape remains visible.

We examine this implication in two complementary ways. First, we replace the baseline definition of complete removal with treatment definitions requiring that a municipality falls 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.[17] These broader definitions include municipalities in which more of the pre-reform religious landscape remained after 1967. Second, we use the number of surviving protected religious monuments directly. If greater monument survival preserved more of the old religious order in the local public environment, post-reform responses should be weaker where more monuments remained visible.

Table 9—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)
P-value	0.000	0.002	0.008
Observations	513	439	494
Panel B: 98% Destroyed (75th pctile)			
Treated	0.473	0.783	0.190
Standard Error	(0.111)	(0.219)	(0.066)
P-value	0.000	0.001	0.008
Observations	513	439	494
Panel C: 95% Destroyed (50th pctile)			
Treated	0.296	0.509	0.166
Standard Error	(0.152)	(0.274)	(0.058)
P-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 main analysis is estimated on the basis of 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.

Table 9 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 re-

[17]The 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.

main. The attenuation is particularly pronounced for female employment and secondary-school enrollment. The post-reform response therefore strengthens as the treatment definition approaches complete disappearance of the old religious order from the local public environment.

Table 10—Number of Surviving Protected Religious Monuments

	Employment (1)	Secondary School Enrollment (2)	Primary School Enrollment (3)
Panel A: Linear			
Monuments × Post	-0.030	-0.052	-0.013
Standard Error	(0.007)	(0.011)	(0.003)
Panel B: Quadratic			
Monuments × Post	-0.111	-0.151	-0.038
Standard Error	(0.024)	(0.040)	(0.011)
Monuments^2 × Post	0.003	0.004	0.001
Standard Error	(0.001)	(0.001)	(0.000)
Marginal Effect	-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 × 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. All specifications include municipality and year fixed effects and log municipal population. Standard errors clustered by municipality are reported in parentheses.

Table 10 provides complementary evidence on the same empirical margin. Because higher values indicate greater continued visibility of the old religious order, the expected relationship runs in the opposite direction. The estimates follow this pattern: 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. Thus, the response weakens as more of the protected religious landscape remains visible.

The denomination, threshold, and count results reinforce the same interpretation. The denomination of a surviving monument does not systematically predict the post-reform response, while responses weaken as more protected religious presence remains and are strongest where that presence disappears completely. The evidence therefore supports complete local disappearance of the old religious order, rather than denomination-specific attachment or the particular binary coding of treatment, as the relevant empirical margin.

E. 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.[18] Moreover, the female-employment event study shows a gradual post-reform increase rather than a discrete jump in 1967–1968, making an administrative reclassification 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.

Table 11—Sectoral Reallocation: Non-Industrial Labor and Agricultural Production

	(1) FE	(2) Population Controls	(3) Region Trends
Panel A: Non-Industrial Labor Force			
No Monuments × Post	0.391	0.354	0.234
Standard Error	(0.136)	(0.135)	(0.126)
p-value	0.008	0.015	0.074
Observations	616	616	616
Panel B: Agricultural Production			
No Monuments × Post	-0.193	-0.297	-0.080
Standard Error	(0.132)	(0.157)	(0.074)
p-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 × 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 11 does not support a simple sectoral-reallocation interpretation. Panel A shows that the non-industrial labor force rises rather than falls. In the population-

[18] By 1961, nearly 90 percent of land had been collectivized into agricultural cooperatives (Këlliçi and Danaj, 2016).

adjusted specification, the coefficient is 0.354 and remains positive when region-specific linear trends are included. Panel B shows no robust decline in agricultural production. Although the coefficient is negative in the population-adjusted specification, it becomes small and statistically insignificant once region-specific trends are included. Appendix Figure A4 likewise shows no discrete decline in agricultural production around the reform. Overall, these results do not support a simple sectoral-reallocation explanation for the industrial response.

F. Specification, Weighting, and Inference

We conclude with robustness checks addressing specification, inference, sample composition, and weighting. These checks leave the treatment definition unchanged and examine whether the main female-employment and schooling results depend on particular features of the baseline specification or sample.

Predetermined population.

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.

Influential treated municipalities.

With only nine treated municipalities, the estimates could be sensitive to a single treated unit. Appendix Table A3 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.

Functional form.

The baseline female-outcome specifications use logged counts and could therefore be sensitive to the log transformation, particularly where baseline counts are small. Appendix Table A4 re-estimates the specifications using Poisson pseudo-maximum likelihood on the underlying count outcomes (Santos Silva and Tenreyro, 2006). The estimated effects remain positive and statistically significant under this alternative functional form.

Inference with few municipalities.

Given the small number of municipalities, we apply randomization inference to our main results. Table 12 reports tests that randomly reassign treatment across

municipalities while preserving the observed number treated and re-estimate the baseline specification for each placebo assignment. The randomization-inference results confirm the conclusions for the main female outcomes.

Table 12—Randomization-Inference Tests for Female Outcomes

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

Note: The table reports randomization-inference tests for the main female outcomes. The coefficient shown is on No Monuments × Post. Treatment is randomly reassigned across municipalities while preserving the observed number of treated municipalities, and the baseline difference-in-differences specification is re-estimated for each placebo 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. The procedure uses 5,000 placebo assignments. All specifications include municipality and year fixed effects and log municipal population.

Region-specific trends.

Appendix Table A1 allows regions to follow separate linear time trends, addressing the possibility that treated and control municipalities were exposed to different regional trajectories. 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.

Municipality size and population weighting.

We first check that the estimates are not driven by Tirana, which is both the capital and by far the largest municipality in the sample. Appendix Table A5 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 A6 uses 1966 municipal population as analytic weights. The main female-outcome results are robust to population weighting.

Matched-sample comparisons.

Finally, Appendix Table A8 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 capita. The female employment and secondary school enrollment estimates remain positive and statistically significant

in both matched samples. The matched-sample estimates provide complementary evidence that the main findings are not specific to the unmatched comparison between treated and control municipalities.

VII. Discussion

The evidence points to a broader distinction between formal and socially recognized authority. Religious authority was abolished nationwide, yet women's employment and schooling changed most where the old religious order was also removed from local public view. 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. Our findings therefore identify a role for public symbols in the persistence of social norms. This idea 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, 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 and it provides the most direct evidence among these outcomes on gender relations within the household.[19]

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, the convergence persists among

[19]Literacy 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.

cohorts born as much as 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, the cohort patterns show that the changes associated with the reform persisted in longer-run literacy, childbearing outcomes, and women's position within the household.

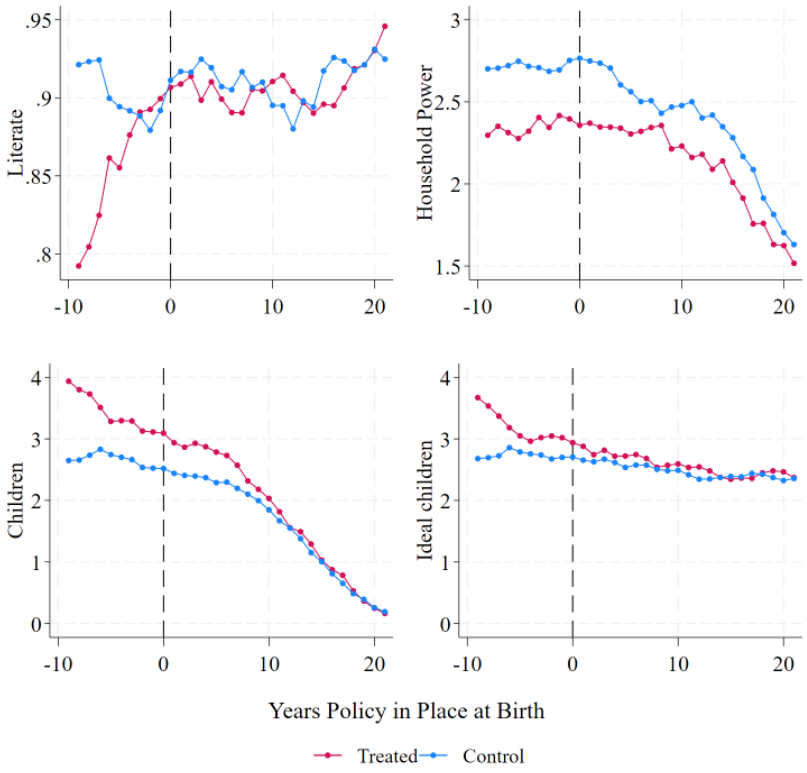


FIGURE 8. DHS SOCIAL NORMS

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. Literacy is measured by whether the respondent can read a complete sentence. Household power measures the respondent's role in household decision-making. 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.

The mechanism documented here can also work in the opposite direction. 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 everyday life; conversely, a stronger public presence of those institutions can reinforce the norm. Afghanistan under the Taliban illustrates this opposite direction: restrictions on women's schooling, work, and public roles have accompanied the reassertion of conservative religious authority in public life (Thomas and Collins, 2021). Albania and Afghanistan are very different settings, but the contrast points to a broader principle: the public presence of institutions associated with a social order can help sustain the expectations on which that order depends.

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 effects emerge gradually, with no evidence of differential pre-reform trends. Municipalities in which no protected monument remained 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 point to 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 commonly observed 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.

REFERENCES

Akerlof, George A. 1980. "A Theory of Social Custom, of Which Unemployment May Be One Consequence." *The Quarterly Journal of Economics*, 94(4): 749–775.

Alesina, Alberto, Paola Giuliano, and Nathan Nunn. 2013. "On the Origins of Gender Roles: Women and the Plough." *The Quarterly Journal of Economics*, 128(2): 469–530.

Algan, Yann, and Pierre Cahuc. 2010. "Inherited Trust and Growth." *American Economic Review*, 100(5): 2060–2092.

Banfield, Edward C. 1967. *The Moral Basis of a Backward Society.* New York:Free Press.

Becker, Sascha O., and Ludger Woessmann. 2008. "Luther and the Girls: Religious Denomination and the Female Education Gap in Nineteenth-Century Prussia." *Scandinavian Journal of Economics*, 110(4): 777–805.

Becker, Sascha O., and Ludger Woessmann. 2009. "Was Weber Wrong? A Human Capital Theory of Protestant Economic History." *The Quarterly Journal of Economics*, 124(2): 531–596.

Becker, Sascha O., Jared Rubin, and Ludger Woessmann. 2024. "Religion and Growth." *Journal of Economic Literature*, 62(3): 1094–1142.

Bénabou, Roland, and Jean Tirole. 2011. "Identity, Morals, and Taboos: Beliefs as Assets." *The Quarterly Journal of Economics*, 126(2): 805–855.

Bénabou, Roland, Davide Ticchi, and Andrea Vindigni. 2015. "Religion and Innovation." *American Economic Review*, 105(5): 346–351.

Bernheim, B. Douglas. 1994. "A Theory of Conformity." *Journal of Political Economy*, 102(5): 841–877.

Bickert, Matthias. 2018. "Re-sanctification of Space in Albania: The Case of Sunni Islam." In *Balkan and Baltic States in United Europe: Histories, Religions, and Cultures II.* , ed. Ekaterina Anastasova and Svetoslava Toncheva, 95–120. Sofia:Paradigma Publishing House.

Botticini, Maristella, and Zvi Eckstein. 2005. "Jewish Occupational Selection: Education, Restrictions, or Minorities?" *The Journal of Economic History*, 65(4): 922–948.

Bourdieu, Pierre. 1991. *Language and Symbolic Power.* Cambridge, MA:Harvard University Press. Edited by John B. Thompson; translated by Gino Raymond and Matthew Adamson.

Bursztyn, Leonardo, Alessandra L. González, and David Yanagizawa-Drott. 2020. "Misperceived Social Norms: Women Working Outside the Home in Saudi Arabia." *American Economic Review*, 110(10): 2997–3029.

Bursztyn, Leonardo, Georgy Egorov, and Stefano Fiorin. 2020. "From Extreme to Mainstream: The Erosion of Social Norms." *American Economic Review*, 110(11): 3522–3548.

Campante, Filipe, and David Yanagizawa-Drott. 2015. "Does Religion Affect Economic Growth and Happiness? Evidence from Ramadan." *The Quarterly Journal of Economics*, 130(2): 615–658.

Cantoni, Davide. 2015. "The Economic Effects of the Protestant Reformation: Testing the Weber Hypothesis in the German Lands." *Journal of the European Economic Association*, 13(4): 561–598.

Cantoni, Davide, Jeremiah Dittmar, and Noam Yuchtman. 2018. "Religious Competition and Reallocation: The Political Economy of Secularization in the Protestant Reformation." *The Quarterly Journal of Economics*, 133(4): 2037–2096.

Durkheim, Emile. 1912. *The Elementary Forms of Religious Life.* New York:Free Press. Translated by Karen E. Fields (1995); originally published as Les formes élémentaires de la vie religieuse (Paris: Alcan, 1912).

Ekelund, Robert B., Robert F. Hébert, and Robert D. Tollison. 2002. "An Economic Analysis of the Protestant Reformation." *Journal of Political Economy*, 110(3): 646–671.

Fernández, Raquel. 2007. "Women, Work, and Culture." *Journal of the European Economic Association*, 5(2–3): 305–332.

Geertz, Clifford. 1973. *The Interpretation of Cultures: Selected Essays.* New York:Basic Books.

Giuliano, Paola. 2007. "Living Arrangements in Western Europe: Does Cultural Origin Matter?" *Journal of the European Economic Association*, 5(5): 927–952.

Giuliano, Paola. 2020. "Gender and Culture." *Oxford Review of Economic Policy*, 36(4): 944–961.

Gjeçovi, Xhelal, ed. 2008. *Historia e popullit shqiptar në katër vëllime.* Vol. 4, Tiranë:Botimet Toena.

Goldin, Claudia. 1990. *Understanding the Gender Gap: An Economic History of American Women. NBER Series on Long-Term Factors in Economic Development*, New York:Oxford University Press.

Gruber, Jonathan, and Daniel M. Hungerman. 2008. "The Church versus the Mall: What Happens When Religion Faces Increased Secular Competition?" *The Quarterly Journal of Economics*, 123(2): 831–862.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales. 2003. "People's Opium? Religion and Economic Attitudes." *Journal of Monetary Economics*, 50(1): 225–282.

Hasluck, Margaret. 1954. *The Unwritten Law in Albania.* Cambridge:Cambridge University Press.

Iannaccone, Laurence R. 1998. "Introduction to the Economics of Religion." *Journal of Economic Literature*, 36(3): 1465–1495.

Inglehart, Ronald, and Wayne E. Baker. 2000. "Modernization, Cultural Change, and the Persistence of Traditional Values." *American Sociological Review*, 65(1): 19–51.

Jayachandran, Seema. 2015. "The Roots of Gender Inequality in Developing Countries." *Annual Review of Economics*, 7: 63–88.

Këlliçi, Klejd, and Ermira Danaj. 2016. "Promoting Equality, Perpetuating Inequality: Gender Propaganda in Communist Albania." *History of Communism in Europe*, 7: 39–61.

Leo XIII. 1891. "Rerum Novarum: Encyclical of Pope Leo XIII on Capital and Labor."

McCleary, Rachel M., and Robert J. Barro. 2006. "Religion and Economy." *Journal of Economic Perspectives*, 20(2): 49–72.

Meta, Beqir, Afrim Krasniqi, and Hasan Bello, ed. 2018. *Indoktrinimi komunist përmes kulturës, letërsisë dhe artit: Dokumente historike: Vëllimi I: 1945–1968.* Tiranë:Instituti i Historisë and Konrad-Adenauer-Stiftung.

Miho, Antonela, Alexandra Jarotschkin, and Ekaterina Zhuravskaya. 2024. "Diffusion of Gender Norms: Evidence from Stalin's Ethnic Deportations." *Journal of the European Economic Association*, 22(2): 475–527.

Miço, Heliona. 2020. "Addressing the Right to Education in Albania Before and After the Communist Regime." 91–117. Tirana, Albania:Epoka University. Conference held 18–19 November 2020, Tirana, Albania.

Plasari, Kaliopi. 1930. "Women's Rights." *Albanian*, 1(8): 175–178.

Putnam, Robert D. 1993. "The Prosperous Community: Social Capital and Public Life." *The American Prospect*, 4(13): 35–42.

Santos Silva, J. M. C., and Silvana Tenreyro. 2006. "The Log of Gravity." *The Review of Economics and Statistics*, 88(4): 641–658.

Squicciarini, Mara P. 2020. "Devotion and Development: Religiosity, Education, and Economic Progress in Nineteenth-Century France." *American Economic Review*, 110(11): 3454–3491.

Tabellini, Guido. 2008. "The Scope of Cooperation: Values and Incentives." *The Quarterly Journal of Economics*, 123(3): 905–950.

Thomas, Clayton, and Sarah R. Collins. 2021. "Afghan Women and Girls: Status and Congressional Action." Congressional Research Service In Focus IF11646.

Voigtländer, Nico, and Hans-Joachim Voth. 2012. "Persecution Perpetuated: The Medieval Origins of Anti-Semitic Violence in Nazi Germany." *The Quarterly Journal of Economics*, 127(3): 1339–1392.

Weber, Max. 1991. *The Protestant Ethic and the Spirit of Capitalism.* London:HarperCollins.

Wolfers, Justin. 2006. "Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results." *American Economic Review*, 96(5): 1802–1820.

World Bank Group. 1995. "Albania: Education Sector Strategy." World Bank Group.

Zickel, Raymond E., and Walter R. Iwaskiw. 1994. *Albania: A Country Study.* Washington, DC:Federal Research Division, Library of Congress.

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

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

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

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

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,

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

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.

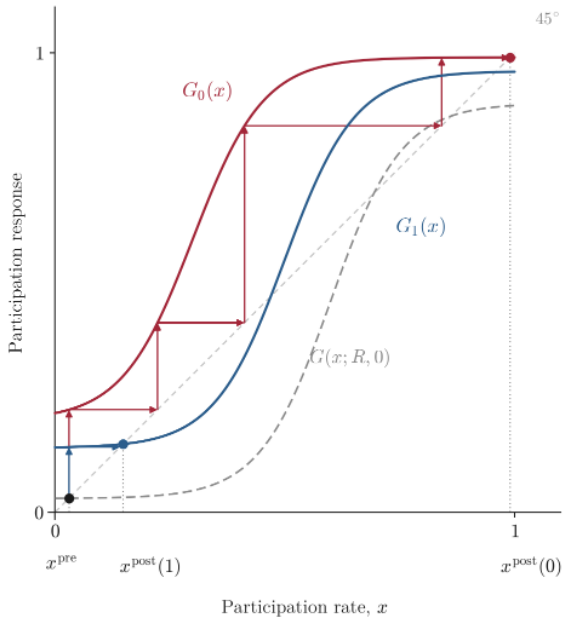


Figure A1. Adjustment Process after the Reform

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.

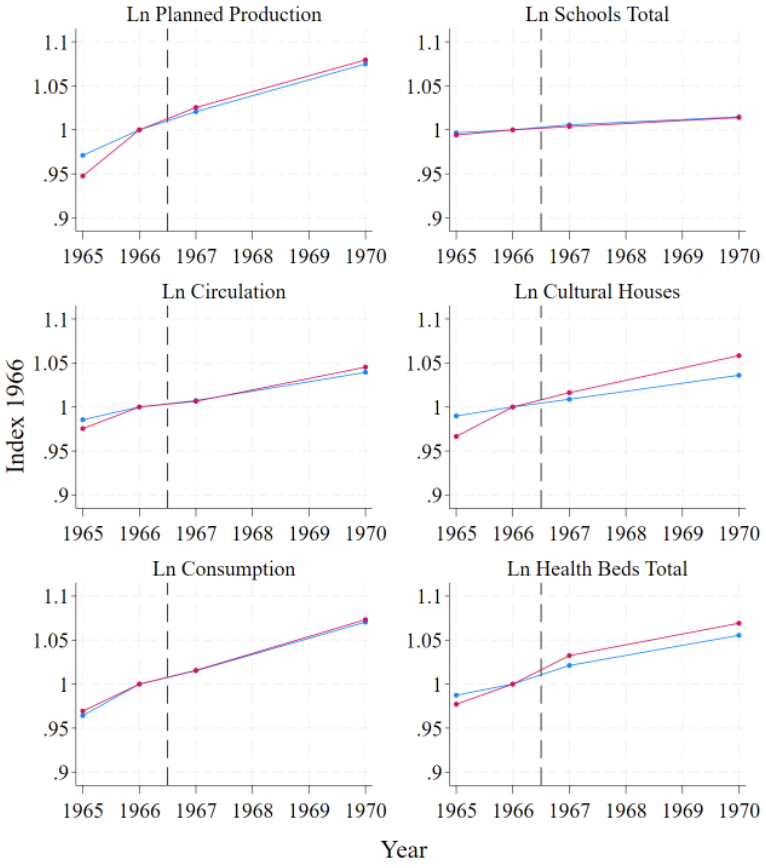
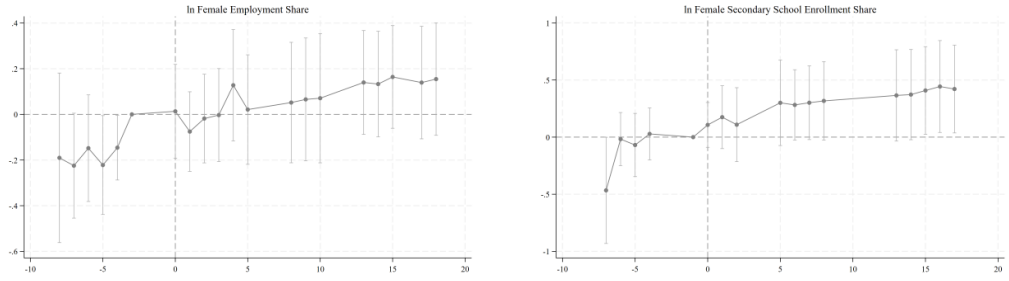


Figure A2. Planned Targets in the 1966 Five-Year Plans

Note: The figure plots mean planned targets from the 1966 Five-Year Plans 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



Panel A. ln Female Employment Share

Panel B. ln Female Secondary School Enrollment Share

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.

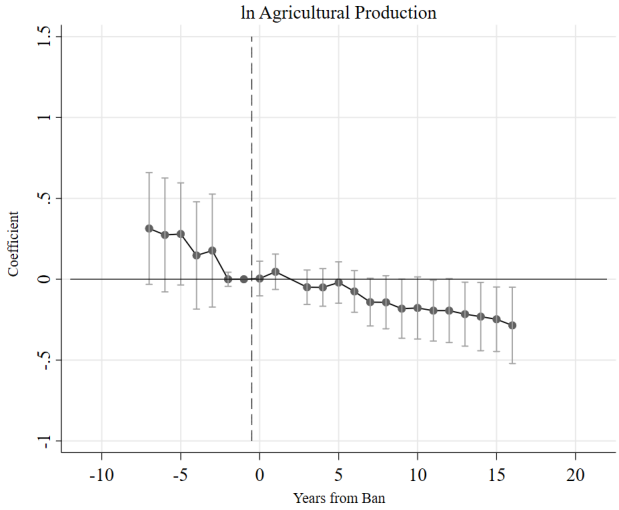


Figure A4. Event-Study Estimates for Agricultural Production

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 × Post	0.648	0.610	0.453
Standard Error	(0.117)	(0.123)	(0.109)
p-value	0.000	0.000	0.004
Observations	513	513	513
Panel B: Secondary School Enrollment			
No Monuments × Post	0.931	0.854	0.517
Standard Error	(0.222)	(0.249)	(0.215)
p-value	0.000	0.002	0.048
Observations	439	439	439
Panel C: Primary School Enrollment			
No Monuments × Post	0.265	0.191	0.125
Standard Error	(0.070)	(0.067)	(0.051)
p-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

	Female Employment (1)	Female Secondary Enrollment (2)	Female Primary School Enrollment (3)
No Monuments × Post	0.471	0.514	0.215
Standard Error	(0.124)	(0.195)	(0.098)
p-value	0.001	0.014	0.038
Observations	513	439	494
Municipality and Year FE	✓	✓	✓
Log 1966 Population × 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 A3 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—Leave-One-Treated-Municipality-Out Estimates

Dropped Treated Municipality	Female Employment (1)	Female Secondary Enrollment (2)	Female Primary School Enrollment (3)
Dibër	0.624	0.885	0.189
	(0.131)	(0.264)	(0.072)
Gramsh	0.603	0.822	0.202
	(0.130)	(0.256)	(0.069)
Krujë	0.645	0.918	0.205
	(0.120)	(0.242)	(0.067)
Kukës	0.597	0.914	0.178
	(0.129)	(0.253)	(0.070)
Mat	0.630	0.803	0.185
	(0.130)	(0.258)	(0.072)
Pukë	0.636	0.852	0.159
	(0.126)	(0.255)	(0.061)
Skrapar	0.581	0.790	0.200
	(0.124)	(0.244)	(0.069)
Tepelenë	0.624	0.897	0.215
	(0.130)	(0.259)	(0.066)
Tropojë	0.556	0.823	0.186
	(0.113)	(0.255)	(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 A4 reports Poisson pseudo-maximum likelihood estimates for the main female outcomes.

Table A4—Poisson Pseudo-Maximum Likelihood Estimates for Female Outcomes

	Female Employment (1)	Female Secondary Enrollment (2)	Female Primary School Enrollment (3)
No Monuments × Post	0.836	1.585	0.259
Standard Error	(0.106)	(0.216)	(0.050)
p-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 A5—Excluding Tirana the Capital: Female Outcomes

	(1) All Municipalities	(2) All Municipalities	(3) Exclude Tirana	(4) Exclude Tirana
Panel A: Employment				
No Monuments × Post	0.648	0.610	0.620	0.534
Standard Error	(0.117)	(0.123)	(0.117)	(0.104)
p-value	0.000	0.000	0.000	0.000
Observations	513	513	494	494
Panel B: Secondary School Enrollment				
No Monuments × Post	0.931	0.854	0.863	0.677
Standard Error	(0.222)	(0.249)	(0.219)	(0.209)
p-value	0.000	0.002	0.001	0.003
Observations	439	439	422	422
Panel C: Primary School Enrollment				
No Monuments × Post	0.265	0.191	0.269	0.151
Standard Error	(0.070)	(0.067)	(0.073)	(0.059)
p-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 × 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 A6—Population-Weighted Estimates: Female Outcomes

Outcome	Coef.	SE	*p*-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 × 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 A7—Population-Weighted Estimates: Production Outcomes

Outcome	Coef.	SE	*p*-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 × 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 A8—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 × Post	0.701	0.487	0.136	0.922
Standard Error	(0.298)	(0.135)	(0.073)	(0.326)
p-value	0.030	0.002	0.077	0.011
Observations	652	398	380	340
Panel B: Additional Religiosity Match				
No Monuments × Post	0.432	0.437	0.103	0.729
Standard Error	(0.334)	(0.115)	(0.081)	(0.316)
p-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 × 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.