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Script Reform, Human Capital and Social Change: Evidence From Uzbekistan's Latin Alphabet Transition

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ABSTRACT

Script reforms are common nation-building tools, yet rigorous evidence on their effects is scarce. This paper examines Uzbekistan's transition from Cyrillic to Latin script, mandated in 1993 and implemented in schools from 1996. I study heterogeneous effects across initial conditions using a difference-in-differences design that exploits variation in exposure across birth cohorts and pre-reform district characteristics. The reform had no significant average effect on literacy, but it generated substantial heterogeneity: districts with lower baseline literacy gained 0.09 standard deviations per standard deviation of the baseline literacy gap, whereas districts with more Russian speakers experienced relative losses of about 0.05 standard deviations. These patterns reveal the redistributive politics of nation-building. The reform also reshaped labour markets: Latin-exposed cohorts moved away from state employment and, less sharply, from entrepreneurship. The same cohorts became more likely to express intolerance towards speakers of other languages, an effect specific to the linguistic margin. The findings suggest that script reforms do not deliver aggregate literacy improvements but have significant distributional consequences and may heighten ethnolinguistic divisions.

JEL Classification: I25, J24, O15, P36, Z13

1 | Introduction

Language and script policies have been central tools of nation-building throughout history. From Atatürk's adoption of the Latin alphabet in Türkiye in 1928 (Lewis 1999; Göksel 2019) to recent debates over Kazakh script reform, governments have used orthographic changes to reshape national identity, facilitate modernisation and distance themselves from former colonial or imperial powers. Proponents argue that script reforms improve literacy by aligning written language with modern communication systems; critics contend they are costly symbolic exercises

that disrupt education and sever connections to cultural heritage. Yet despite the prevalence of such reforms, rigorous evidence on their effects remains scarce.

This paper addresses this debate by studying Uzbekistan's alphabet reform, part of the country's broader post-independence nation-building project. The law mandating transition from Cyrillic to Latin script was passed in September 1993, with full implementation in schools beginning in September 1996. The reform provides a natural experiment setting for several reasons. First, the timing of school implementation was exogenous to

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individual characteristics: all schools began teaching in Latin script from September 1996, creating discontinuous variation in exposure across birth cohorts. Second, the reform occurred within a stable institutional environment, as Uzbekistan maintained continuity in other educational policies during this period. Third, rich survey data allow me to measure multiple outcomes (literacy, educational attainment, occupational choice and social attitudes) for cohorts with varying degrees of Latin exposure.

I exploit this variation using a difference-in-differences framework that interacts post-reform cohort indicators with pre-reform district characteristics, following the identification strategy of Bleakley (2007, 2010), Lucas (2010), Bhalotra and Venkataramani (2015). This approach recognises that the reform's impact likely varied across districts depending on their initial conditions. I develop a simple theoretical framework showing why such heterogeneity should arise: districts with lower baseline literacy had more room for improvement and less to lose from disruption, whereas districts with larger Russian-speaking populations faced greater costs from the transition away from Cyrillic materials. The framework generates testable predictions that I take to the data.

The central finding of this paper is that *the reform had no significant average effect on literacy*: the coefficient on the simple post-reform indicator is close to zero and statistically insignificant. This null result is itself substantively important. It suggests that script reforms, despite their high symbolic value and substantial implementation costs, may not deliver the human capital benefits. Section 7.6 unpacks why: the null is the equilibrium outcome of offsetting gains and losses across districts, not evidence that the reform was inert.

However, this null average effect masks substantial and policy-relevant heterogeneity. Districts with lower pre-reform literacy experienced significantly larger gains: a one standard deviation increase in the baseline literacy gap is associated with a 0.09 standard deviation improvement in informational literacy and a 0.08 standard deviation improvement in general literacy for post-reform cohorts. Conversely, districts with higher Russian-speaking populations saw relative declines of approximately 0.05 standard deviations (significant at the 10 percent level), consistent with greater disruption in areas where Cyrillic materials and Russian-language resources were more prevalent.

These heterogeneous effects reveal the *redistributive politics of nation-building*. The reform transferred human capital from Russian-integrated areas to previously disadvantaged Uzbek-majority districts. Rather than a uniform modernisation effort, the alphabet reform had the distribution of gains and losses aligning with ethnic and linguistic cleavages.

Second, the reform altered occupational sorting. Latin-exposed cohorts are 8.2 percentage points less likely to work in the state sector, and 4.4 percentage points less likely to be entrepreneurs. These effects are strongly amplified by baseline district characteristics: a one standard deviation increase in the pre-reform state employment share adds a further 15.3 percentage point decline in state employment among treated cohorts. The pattern suggests that Latin literacy reduced comparative advantage in traditional bureaucratic careers,

which still operated largely in Cyrillic during the transition period, whereas communication gaps with Cyrillic-literate elders may have raised the costs of intergenerational business transfer (Lindquist et al. 2015).

Third, using data from the Life in Transition Survey (LiTS III), I examine effects on social attitudes. While I find no significant average effects on trust in government or general tolerance, Latin-exposed cohorts are 10.9 percentage points more likely to express intolerance towards speakers of a different language, an effect that is marginally significant ($p = 0.052$) and specific to the linguistic margin. This finding suggests that script reform may have heightened ethnolinguistic boundaries rather than promoting cosmopolitan openness.

These findings contribute to several strands of literature. Most directly, this paper adds to the growing literature on the effects of language and script policies (Aspachs-Bracons et al. 2008; Clots-Figueras and Masella 2013; Chen 2013; Gang et al. 2023). While existing work has focused primarily on language-of-instruction policies in multilingual contexts, I provide novel evidence on the effects of orthographic reform within a single language. The paper also contributes to the broader literature on nation-building and identity formation (Alesina and Reich 2015; Fouka 2016; Bandiera et al. 2019), showing that even symbolic policies can have real economic consequences through their effects on human capital and occupational choice. Finally, the null average effect contributes to policy debates about the value of script reforms, suggesting that their costs may outweigh their benefits for aggregate human capital formation.

The remainder of the paper is organised as follows. Section 2 reviews the related literature. Section 3 develops a theoretical framework generating predictions about heterogeneous effects. Section 4 provides background on Uzbekistan's alphabet reform. Section 5 describes the data. Section 6 presents the empirical strategy. Section 7 reports the main results on literacy and education. Section 8 examines occupational choice. Section 9 analyses attitudes. Section 10 concludes.

2 | Related Literature

This paper connects three strands of economic research: the economics of language and identity, nation-building policies and human capital formation.

2.1 | Language, Identity and Economic Outcomes

Economists have increasingly recognised language as a fundamental determinant of economic behaviour and outcomes. Chen (2013) demonstrates that grammatical features of languages, specifically the distinction between future and present tense, affect savings behaviour and health decisions. Gay et al. (2017) shows that native language influences risk preferences. These findings suggest that linguistic structures shape how individuals process information and make decisions.

Language also serves as a powerful marker of social identity. Building on the theoretical framework of Akerlof and Kranton (2000), which models identity as an argument in utility functions, empirical work has documented how language

shapes group identification and intergroup relations. Clots-Figueras and Masella (2013) exploit the 1983 introduction of Catalan-Spanish bilingual education to show that exposure to regional language instruction strengthens regional identity. Their identification strategy, which I adapt for this paper, uses years of compulsory education under the new language regime as an exogenous source of variation. They find that each additional year of Catalan instruction increases the probability of feeling “Catalan” by approximately 3.5 percentage points.

Fouka (2016) studies the opposite phenomenon: the effects of language *restriction* policies. Exploiting anti-German sentiment during World War I, she shows that states that banned German-language instruction experienced a backlash effect, with affected cohorts *more* likely to retain German names and marry within their ethnic group. This finding highlights that language policies can have unintended consequences that run counter to policymakers’ intentions.

Aspachs-Bracons et al. (2008) and Masella (2013) further document how language policies interact with national identity formation. Their work emphasises that language is not merely a communication tool but a carrier of cultural values and group membership signals.

2.2 | Nation-Building and State Capacity

A growing literature examines how states use various tools to construct national identity and social cohesion. Alesina et al. (2019) provide a theoretical framework showing that nation-building investments can have positive effects on economic development when they foster trust and reduce ethnic tensions, but can be costly when imposed on heterogeneous populations.

Bandiera et al. (2019) study compulsory schooling during the age of mass migration to the United States, showing that common school curricula reduced ethnic fragmentation and increased political integration. Bazzi et al. (2019) examine how Indonesia’s transmigration programme (which resettled individuals across ethnic boundaries) affected national identity formation. Cantoni et al. (2017) document how curriculum changes in China shaped political attitudes and ideology.

Within the Central Asian context, Edgar (2004) and Hirsch (2005) analyse Soviet-era nation-building policies, including the creation of national languages and alphabets. These works provide essential historical context for understanding post-independence reforms.

Alphabet reform specifically has received attention from historians and linguists (Uzman 2010; Schweitzer 2020), but systematic economic evaluation has been lacking. Bartholomäe (2016) discusses ongoing debates about Latin script adoption in Tatarstan, noting that proponents view alphabet choice as a tool for strengthening ethnic identity distinct from Russian national identity.

2.3 | Human Capital and Literacy

The economics of education literature has established robust links between literacy and labour market outcomes (Hanushek

and Woessmann 2012). However, most research focuses on *levels* of literacy rather than the *medium* through which literacy is acquired.

This paper speaks to research on how educational inputs affect skill formation. Stephens and Yang (2014) show that compulsory schooling laws in the United States increased educational attainment with meaningful returns. Duflo (2001) documents how school construction programs in Indonesia affected educational attainment and wages.

Closest to the policy margin studied here is the work on the language of instruction: J. Angrist and Lavy (1997) document how a switch from French to Arabic instruction in Morocco lowered the returns to schooling, Ramachandran (2017) finds large literacy gains from mother-tongue instruction in Taylor and von Fintel (2016) and Cappellari and Di Paolo (2018) provide further evidence from South Africa and Catalonia. The specific question of how alphabet choice affects learning outcomes, holding the language of instruction fixed, has received far less attention. Moore et al. (2014) and Lupyan and Bergen (2016) provide evidence from psychology that written language systems affect cognitive processing, but economic implications remain underexplored.

This paper makes several contributions to the literature. First, I provide the first estimates of a script reform’s effect on directly measured literacy skills, using a difference-in-differences design with pre-reform district characteristics as treatment intensity measures. Second, I document that the reform’s null average effect masks substantial heterogeneity, benefiting low-literacy districts while harming Russian-integrated areas, highlighting the redistributive politics of nation-building. Third, I trace downstream effects on occupational choice and social attitudes, showing that script reforms have consequences beyond immediate literacy outcomes. Fourth, the finding of increased linguistic intolerance contributes to debates about whether nation-building promotes or undermines social cohesion.

3 | Theoretical Framework

This section develops a simple framework to understand how script reform affects human capital accumulation and why effects may vary across districts with different initial conditions.

3.1 | Setup

Consider a model where individual i in district d from birth cohort c accumulates human capital H_{idc} through schooling (Cunha and Heckman 2007). Human capital production depends on (i) individual ability a_i , (ii) school quality q_d and (iii) the “script environment” S_{dc} , which captures the alignment between the script used in schools and the script prevalent in the broader society.

Before the reform, all schools teach in Cyrillic, and society operates in Cyrillic: $S_{dc} = 1$ (perfect alignment). After the reform, schools switch to Latin while society remains predominantly Cyrillic, creating a mismatch: $S_{dc} < 1$ for post-reform cohorts.

Human capital production follows:

$$H_{idc} = a_i \cdot q_d \cdot f(S_{dc}, L_d^0) \quad (1)$$

where $f(\cdot)$ captures how the script environment interacts with baseline district literacy L_d^0 , and $\frac{\partial f}{\partial S} > 0$ (better alignment improves learning).

3.2 | Heterogeneous Effects by Baseline Literacy

The key insight is that $\frac{\partial^2 f}{\partial S \partial L^0}$ may be positive, that is, the cost of script mismatch is *lower* in districts with lower baseline literacy. Several mechanisms generate this prediction:

3.2.1 | Room for Improvement

In districts with low baseline literacy, less script-dependent human capital is exposed to the reform. Even if Latin instruction is less efficient than Cyrillic would have been, the resulting loss is smaller because these districts started from a low base. Formally, if $f(S, L^0) = S^\alpha \cdot g(L^0)$ where $g'(L^0) > 0$, then:

$$\frac{\partial H}{\partial \text{Reform}} = a_i \cdot q_d \cdot [f(S_{\text{post}}, L_d^0) - f(1, L_d^0)] \quad (2)$$

This difference is negative for all districts but smallest in magnitude for low- L^0 districts since $S_{\text{post}} < 1$.

3.2.2 | Substitution versus Disruption

In high-literacy districts, students could have learnt effectively under the old system. The reform substitutes a well-functioning Cyrillic education with a less-tested Latin system, a clear disruption. In low-literacy districts, the counterfactual was poor education regardless of script, so Latin instruction may represent a fresh start with new materials and renewed pedagogical attention.

3.2.3 | Parental Complementarity

In high-literacy districts, parents are more educated and more likely to assist with homework. Script mismatch between parents (Cyrillic-literate) and children (Latin-learning) disrupts this intergenerational transfer. In low-literacy districts, parental assistance was limited regardless of script. This channel parallels the broader evidence that skills and attitudes are transmitted within families (Dohmen et al. 2012).

3.3 | Heterogeneous Effects by Russian-Speaking Population

Districts with larger Russian-speaking populations face additional disruption costs:

3.3.1 | Material Availability

Russian-speaking areas had greater access to Cyrillic-script books, newspapers and educational materials. The switch to Latin rendered these resources less useful, reducing the effective educational environment.

3.3.2 | Language Spillovers

Russian and Uzbek share Cyrillic script under the old system. In mixed-language districts, Cyrillic facilitated cross-linguistic communication and created positive spillovers for literacy. Latin script breaks this link.

3.3.3 | Political Economy

The reform was also framed as nation-building and distancing from Russian influence. Russian-speaking areas may have experienced less enthusiastic implementation or active resistance.

Taken together, these forces generate three predictions that map onto the difference-in-differences specification in Equation (8). Let $\Delta_d \equiv f(S_{\text{post}}, L_d^0) - f(1, L_d^0)$ denote the district-level treatment effect implied by Equation (2), and let R_d denote the district's Russian-speaking share.

First, the model imposes no restriction on the sign of the population-average effect. Because Δ_d differs across districts and the underlying forces work in opposite directions, the average effect $\mathbb{E}[\Delta_d]$ can be close to zero even when the reform changes learning substantially within districts. A null *average* literacy effect is therefore consistent with, rather than evidence against, a reform that reshapes the distribution of learning across districts.

Second, the treatment effect is larger where baseline literacy is lower. Because the disruption cost of script mismatch rises with baseline literacy, through the substitution and parental-complementarity channels above, Δ_d is decreasing in L_d^0 ,

$$\frac{\partial \Delta_d}{\partial L_d^0} < 0, \quad (3)$$

or equivalently increasing in the baseline literacy gap $1 - L_d^0$. In the empirical model, this corresponds to a positive coefficient on the interaction between post-reform exposure and the district literacy gap.

Third, the treatment effect is smaller, and may be negative, where the Russian-speaking population is larger. The material-availability, language-spillover and political-economy channels raise the cost of the switch in Russian-integrated districts, so

$$\frac{\partial \Delta_d}{\partial R_d} < 0. \quad (4)$$

In the empirical model, this corresponds to a negative coefficient on the interaction between post-reform exposure and Russian-speaking share.

3.4 | Occupational Choice and Social Attitudes

The framework extends naturally to occupational sorting. Let workers choose between state employment (G), private employment (P) and entrepreneurship (E) based on comparative advantage.

State sector jobs traditionally required Cyrillic literacy for bureaucratic tasks, form-filling, report-writing and document processing. Even after Latin was introduced in schools, government operations continued largely in Cyrillic throughout the 1990s and 2000s. Latin-educated workers thus faced a comparative disadvantage in state employment:

$$\Pr(G|\text{Latin}) < \Pr(G|\text{Cyrillic}) \quad (5)$$

A similar mechanism applies to entrepreneurship. Running a business requires record-keeping, correspondence with suppliers and customers, and interaction with tax and licencing authorities, all of which continued to operate largely in Cyrillic during the transition. Latin-educated entrants therefore faced higher fixed costs of formal business activity, and communication frictions with Cyrillic-literate parents could plausibly weaken the intergenerational transfer of family businesses.

Finally, script can function as a boundary (Wimmer 2002) that heightens group distinctions. The reform created a visible marker separating Latin-educated youth from Cyrillic-educated elders and Russian speakers. This boundary may strengthen in-group identification while fostering suspicion of out-groups, particularly those distinguished by language.

4 | Background

4.1 | Historical Context: Why a Script Reform?

Uzbekistan is one of the few countries to have undergone four different alphabet reforms within a single century, reflecting the region's complex colonial history. Before Russian conquest in the 19th century, Uzbek was written in Arabic–Persian script (Chagatai), a literary tradition stretching back to the mediaeval Turkic courts of Samarkand and Herat. Following the Bolshevik consolidation of Central Asia, Soviet authorities introduced a unified Latin alphabet (*Yanalif*) in 1928–1929 as part of the broader *korenizatsiya* (indigenisation) campaign. The First Turkological Congress in Baku (1926) had endorsed Latinisation explicitly as an instrument of modernisation: it would, congress delegates argued, sever Central Asian Muslims from a perceived “backward” clerical Arabic-script tradition while distancing them from neighbouring non-Soviet Turks who still used Arabic script (Edgar 2004; Hirsch 2005).

That first Latin period was deliberately short. In 1940, Stalin mandated a wholesale switch to Cyrillic across all Turkic-language Soviet republics. Two motivations are well documented in the historical literature. First, by the late 1930s, the Kemalist Turkish Republic had itself adopted a Latin alphabet (1928), and the Soviet leadership feared that a shared Latin script would facilitate pan-Turkic mobilisation across the southern frontier of the USSR. Second, Cyrillic dramatically reduced the cost of Russian-language acquisition and of producing bilingual administrative and educational materials, accelerating russification of Central Asian elites (Landau and Kellner-Heinkele 2001). A decree of 8 May 1940 transferred Uzbek to Cyrillic; by July of the same year, a unified Cyrillic orthography was adopted. For the next five decades, Cyrillic was the only script taught in Uzbek schools, and reported adult literacy rose from 78.7% in 1939 to 99.7% by 1970 within the highly

centralised Soviet education system (Spechler 2008; Gerber and Hout 2003).

4.1.1 | Why Reform Again in 1993?

Three motives, well documented in primary sources and in the political-science literature on post-Soviet language policy, drove the Karimov government's decision to legislate a return to Latin:

1. *De-Russification and post-Soviet identity construction.* Cyrillic was a daily reminder of imperial subordination. The 1989 Uzbek-language law and the 1993 alphabet law together formed the symbolic core of a project to recast Uzbek as the unambiguous *state* language and to mark a discontinuity with the Soviet past (Landau and Kellner-Heinkele 2001; Fierman 1991).
2. *Pan-Turkic re-orientation.* The 1992 Istanbul summit of Turkic-speaking heads of state (Türkiye, Azerbaijan, Uzbekistan, Kazakhstan, Kyrgyzstan and Turkmenistan) endorsed a common 34-letter Latin alphabet as a coordination device for cultural and economic integration (Uzman 2010). The Uzbek law of 2 September 1993 cited entry into “the world communication system” and alignment with Latin-using Turkic kin states as explicit rationales.
3. *Modernisation and the global-economy argument.* Government statements emphasised the convergence of Latin with the alphabets used in international scientific, technical and commercial communication. A common claim, widely repeated in the early-1990s Uzbek press, was that Latin would lower the cost of acquiring foreign languages and computer literacy.

What the 1993 law did *not* primarily target was domestic literacy. Soviet-era literacy in Cyrillic was already near-universal; the marginal human-capital case for switching scripts was weak and was not central in the legislative debate (Schweitzer 2020). This is important for interpreting the empirical results below: the reform's stated goals were symbolic and geopolitical, not pedagogical, which is consistent with the null *average* effect on literacy that I document below.

Following the September 1993 law, Tashkent revised the alphabet in 1995 (replacing several diacritics with digraphs) and committed to phased implementation.¹ *Full implementation in schools began in September 1996*: from that date, all primary and secondary education was conducted in Latin. The original target for full social transition was 2010; this has repeatedly slipped, and a 2023 deadline was again missed. The reform was thus dual-purpose by design: a symbolic break from the Soviet past, paired with a long-horizon and frequently delayed plan to retool the pedagogical apparatus.

4.2 | The Reform in Comparative Post-Soviet Perspective

Uzbekistan's 1993 law is part of a broader *post-Soviet de-Cyrillicisation wave* across the Turkic and Romance-speaking former republics. Table 1 summarises the principal cases. Three patterns are notable.

TABLE 1 | Post-soviet de-cyrrillisation reforms: A comparison.

Country	Law/decreed	School rollout	Stated rationale	Status (2025)
Moldova	1989	1989–1991	Reunify with Romanian; de-Russification	Complete
Azerbaijan	Dec 1991	1992–2001	Pan-turkic, de-russification	Complete
Turkmenistan	Apr 1993	1993–2000	Nation-building under Niyazov	Largely complete
Uzbekistan	Sep 1993	1996–present	De-russification, turkic alignment	Incomplete (mixed-use)
Kazakhstan	Oct 2017	2023–2031	Digital economy, turkic alignment	Phased rollout
Mongolia (proposed) ^a	2020 plan	2025–(planned)	Cultural revival (to traditional Mongolian)	Pre-implementation
Tatarstan (RU)	1999 attempt	Blocked 2002	Tatar nation-building	Blocked by federal law

^aMongolia's case differs in direction (Cyrillic to traditional Mongolian script rather than Latin) but shares the same political logic of post-Soviet identity reconstruction. Sources: Landau and Kellner-Heinkele (2001); Uzman (2010); Bartholomäe (2016); Smagulova (2017); Clement (2018); Garibova (2009); national legislative records.

First, the wave clusters in the immediate post-independence years (1989–1995), when symbolic distancing from Moscow was most politically salient. Moldova (1989) and Azerbaijan (1991) moved before the formal dissolution of the USSR; Turkmenistan (1993) and Uzbekistan (1993) followed once independent statehood was secured. Kazakhstan (decree 2017, school rollout 2023–2031) is a much later mover, motivated more by Turkic-world coordination and digital-economy arguments than by de-Russification pressure (Bartholomäe 2016; Smagulova 2017).

Second, every case combines an explicit nation-building rationale with a long, often-revised implementation schedule. Azerbaijan's transition was de jure complete by 2001, but Cyrillic Russian-language media remained dominant in Baku for years after; meanwhile, Turkmenistan's transition was undermined by chronic textbook shortages (Clement 2018). Implementation gaps were present in Uzbekistan as well; indeed, it was still legislating orthographic revisions to its Latin alphabet as recently as July 2026 (Gazeta.uz 2026), more than three decades after the founding law.

Third, where independent evaluations exist, the human-capital effects appear modest. Survey work in Azerbaijan documents a generation gap in script fluency closely paralleling Uzbekistan's, and qualitative evidence reports comparable disruption to intergenerational reading practices (Garibova 2009). The novelty of the present paper is to provide the first estimate of script-reform effects within the post-Soviet wave; the null average effect I estimate, paired with substantial heterogeneity by district characteristics, is consistent with the patchy implementation documented elsewhere.

The Azerbaijan case is the closest analogue and serves as a natural placebo/comparator. Like Uzbekistan, Azerbaijan moved from Cyrillic to a custom Latin alphabet, conducted the school rollout over roughly a decade and continued to operate large parts of public life in Cyrillic for years afterwards.² The contrast supports the language-as-marker interpretation in Section 9: it is not the script switch per se but the conditions under which the switch happened in Uzbekistan (delayed implementation, large Russian-speaking minority, school-society script gap) that produce the heightened linguistic boundary.

4.3 | The Reform and Its Implementation

The 1993 law set an ambitious timeline for transition, initially targeting completion by 2010. However, implementation proved gradual. The alphabet was revised in May 1995, with basic spelling rules approved in August 1995. The new Latin alphabet included unique characters designed for Uzbek phonetics.

4.3.1 | Full Implementation in Schools Began in September 1996

From this date, all primary and secondary education was conducted using Latin script: new textbooks were printed in Latin, teachers received training in the new alphabet and students were taught to read and write exclusively in Latin from their first year of schooling. This sharp implementation date provides the basis for my identification strategy.

Crucially, the broader society continued to operate primarily in Cyrillic throughout the transition period, and to a significant extent, still does today. Newspapers, government documents, street signs and older generations remained Cyrillic-dominant. This created a unique situation where school-age children learnt in one script while being surrounded by another in daily life. Recent surveys suggest that even now, approximately 34% of Uzbeks prefer Cyrillic, whereas 38% prefer Latin and 28% express no preference (Saida 2023).

The reform's differential impact across birth cohorts is mechanical: children who entered first grade in or after September 1996 received their entire education in Latin script (11 years of exposure), whereas those who had already completed schooling before 1996 had zero Latin exposure. Intermediate cohorts received partial exposure depending on their grade level when implementation began. Figure A1 illustrates this variation in treatment intensity across birth cohorts.

5 | Data

5.1 | Main Data: CALISS

The primary data source is the Central Asia Labor and Skills Survey (CALISS), conducted in Uzbekistan in 2013 (World Bank 2013). CALISS is a nationally representative household survey that includes direct assessments of cognitive skills,

including literacy and numeracy, following the methodology of the OECD's Programme for the International Assessment of Adult Competencies (PIAAC).

The survey covers individuals aged 16–65 and includes detailed information on demographic characteristics, educational attainment, employment and household composition. Crucially, the survey records birth year and birth quarter, allowing me to construct precise measures of exposure to the Latin script reform.

The key outcome variables are:

- **Informational Literacy:** A factor score derived from tasks involving the use of information from texts to accomplish specific goals (e.g., following written instructions, extracting information from documents).
- **General Literacy:** A broader factor score encompassing reading comprehension, vocabulary and text interpretation.
- **Tertiary Education:** An indicator for completion of post-secondary education.
- **State Employment:** An indicator for employment in the public sector.
- **Entrepreneurship:** An indicator for self-employment or family business ownership.

I restrict the sample to individuals born between 1965 and 1994, yielding a sample of 4070 observations across 75 districts. This window ensures sufficient variation in treatment status while avoiding cohorts too young to have completed education or too old to be reliably surveyed.

5.2 | Supplementary Data: LiTS III

To examine effects on attitudes, I use the Life in Transition Survey III (LiTS III), conducted in 2016 by the European Bank for Reconstruction and Development (EBRD) (EBRD 2016). The Uzbekistan sample includes 938 respondents with information on trust, tolerance and social attitudes.

Key attitude measures include:

- **Trust in Government:** Binary indicator for high trust in the national government.

TABLE 2 | Summary statistics by reform exposure (CALISS).

	Pre-reform		Post-reform	
	Mean	SD	Mean	SD
Informational literacy (factor score)	0.179	0.763	0.145	0.731
Literacy (factor score)	0.038	0.714	0.029	0.701
Tertiary education	0.098	0.298	0.111	0.314
State employment	0.553	0.498	0.478	0.500
Female	0.673	0.469	0.666	0.472
Urban	0.344	0.475	0.312	0.464
Household size	5.884	2.083	6.924	2.676
Good Russian proficiency	0.362	0.481	0.261	0.439
Observations	1304		2766	

Note: Pre-reform cohorts have 0 years of Latin-script schooling; post-reform cohorts have at least 1 year of Latin exposure.

- **Trust in President:** Binary indicator for high trust in the president.
- **Tolerance Index:** Count of social groups the respondent would accept as neighbours (0–4 scale).
- **Intolerance towards Different Languages:** Binary indicator for not wanting neighbours who speak a different language.

5.3 | Summary Statistics

Table 2 presents summary statistics by reform exposure status. Pre-reform cohorts (0 years of Latin exposure) and post-reform cohorts (any Latin exposure) are broadly similar on observable characteristics, though post-reform cohorts have larger household sizes and lower Russian proficiency, patterns consistent with demographic trends and the reform's intended effect of reducing Russian-language dominance.

Table 3 shows the distribution of baseline district characteristics, computed using only pre-reform cohorts to avoid the “bad controls” problem. There is substantial variation across Uzbekistan's 75 districts in baseline literacy, Russian-speaking share, urbanisation and state employment, variation that I exploit to identify heterogeneous treatment effects.

TABLE 3 | Baseline district characteristics (pre-reform cohorts only).

	Mean	SD	Min	Max
Russian-speaking share	0.774	0.184	0.200	1.000
Urban share	0.360	0.483	0.000	1.000
State employment share	0.585	0.236	0.000	1.000
Baseline literacy (factor score)	0.035	0.485	−1.332	1.054
Baseline Info. Literacy (factor score)	0.182	0.547	−1.538	1.413
Districts	75			

Note: District-level means computed using only cohorts with zero Latin exposure (born before 1979).

6 | Empirical Strategy

6.1 | Treatment Variable Construction

I construct years of Latin-script exposure based on the school entry rule (children must be 7 by September to start school that year) and the school implementation date (September 1996). Although the law was passed in 1993, actual Latin-script instruction in schools began in September 1996 following the 1995 alphabet revisions and preparation of teaching materials. For an individual born in year b and quarter q :

$$\text{SchoolEntry}_i = \begin{cases} b + 7 & \text{if } q \in \{1, 2, 3\} \\ b + 8 & \text{if } q = 4 \end{cases} \quad (6)$$

$$\text{YearsLatin}_i = \max\{0, \min\{11, 11 - (1996 - \text{SchoolEntry}_i)\}\} \quad (7)$$

This yields continuous treatment intensity ranging from 0 (for cohorts who completed schooling before 1996) to 11 (for cohorts who entered school in 1996 or later). I also define a binary indicator $\text{Post}_i = \mathbf{1}[\text{YearsLatin}_i > 0]$.

6.2 | Identification Strategy

A simple comparison of post-reform and pre-reform cohorts would conflate the reform's effect with general cohort trends. To address this concern, I adopt a difference-in-differences framework that exploits variation in "treatment intensity" across districts, following Bleakley (2010); Bhalotra and Venkataramani (2015).

The key insight is that the reform's impact should vary with pre-reform district characteristics. Districts with lower baseline literacy have more "room for improvement" and should benefit more from any positive effects of the reform. Conversely, districts with higher Russian-speaking populations, where Cyrillic materials were more prevalent, should experience greater disruption.

The main estimating equation is:

$$Y_{idt} = \alpha + \beta_1 \text{Post}_i + \beta_2 (\text{Post}_i \times \text{BaselineIntensity}_d) + \gamma X_i + \delta_t + \theta_d + \varepsilon_{idt} \quad (8)$$

where Y_{idt} is the outcome for individual i in district d and birth cohort t ; Post_i indicates any Latin exposure; $\text{BaselineIntensity}_d$ is a pre-reform district characteristic (e.g., baseline literacy gap, Russian-speaking share); X_i are individual controls; δ_t are age fixed effects and θ_d are district fixed effects.

The coefficient of interest is β_2 , which captures how the reform's effect varies with baseline district characteristics. A positive β_2 on baseline literacy gap would indicate that the reform had larger positive effects in districts with lower pre-reform literacy.

The baseline intensity measures must be computed using only pre-reform cohorts (those with $\text{YearsLatin} = 0$). Using all cohorts would create a mechanical correlation between post-reform outcomes and the "baseline" measure, biasing β_2 towards zero.

6.3 | Placebo Tests

To validate the identification strategy, I conduct placebo tests using a fake implementation date of 1992, critically, *before* the

law was announced in September 1993. This ensures the placebo cohorts could not have been affected by anticipation effects or early preparation following the announcement. If the estimated effects reflect true reform impacts rather than pre-existing trends, the placebo coefficients should be close to zero and statistically insignificant.

6.4 | Threats to Identification

Several concerns could threaten the interpretation of my estimates:

6.4.1 | Cohort Trends

If literacy was improving (or declining) differentially across districts for reasons unrelated to the reform, this could bias my estimates. The placebo test addresses this concern directly: the insignificant placebo coefficients suggest no pre-existing differential trends. The dynamic event-study specification reported in Section 7.3 (Figure 1) provides direct evidence that the cohort $\times$ baseline literacy gap interactions are flat for pre-reform cohorts (joint F -tests do not reject zero pre-trends; $p = 0.91$ and $p = 0.76$).

6.4.2 | Selective Migration

If the reform induced selective migration, for example, if educated families in Russian-speaking areas relocated after the reform, this could confound the district-level analysis. I address this by controlling for current urban residence and by noting that internal migration in Uzbekistan was relatively restricted during the 1990s and 2000s due to the *propiska* (residence registration) system inherited from the Soviet era.

6.4.3 | Concurrent Reforms

Other educational or economic changes coinciding with the 1996 implementation could confound my estimates. However, Uzbekistan was characterised by policy continuity during this period, with the Karimov government maintaining centralised control over education. The 1993 law specifically addressed script, not curriculum content or school resources. Moreover, if concurrent reforms affected all districts equally, they would be absorbed by the cohort fixed effects; only reforms that differentially affected high- versus low-literacy districts would bias the interaction coefficients.

6.4.4 | Measurement of Baseline Characteristics

My identification relies on baseline district characteristics computed from pre-reform cohorts. If these measures are noisy, my estimates would be attenuated towards zero, making my significant findings conservative.

6.4.5 | SUTVA Violations

If there are spillovers between treated and untreated cohorts within households, for example, if younger siblings help older siblings learn Latin, or if Latin-educated children teach their Cyrillic-educated parents, this could attenuate or amplify my estimates depending on the direction of spillovers. The negative coefficient on Russian-speaking share suggests that any positive

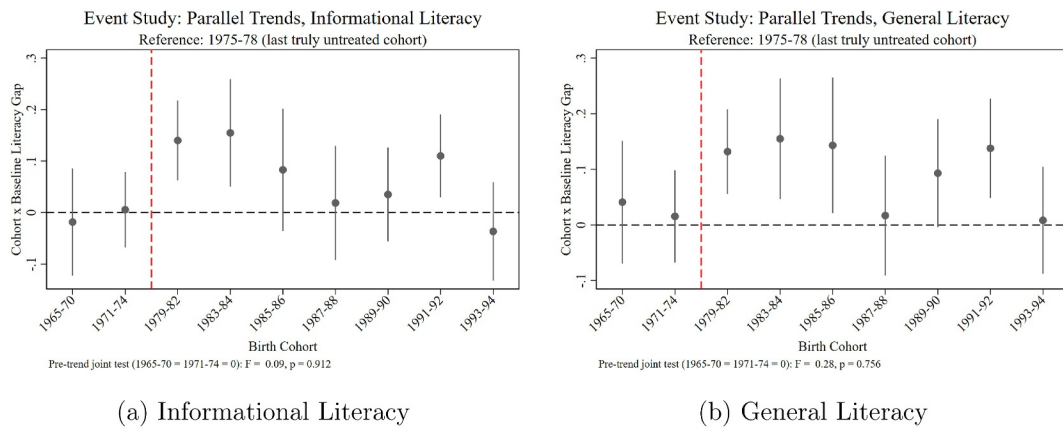


FIGURE 1 | Event study: Parallel pre-trends and dynamic treatment effects. (a) Informational literacy. (b) General literacy. Each marker reports $\hat{\beta}_c$ from Equation (9), the coefficient on cohort c interacted with baseline literacy gap. Reference category: 1975–78 cohort (last cohort fully pre-reform; mean Latin exposure 0.07 years). Vertical dashed line marks the September 1996 implementation cutoff. Whiskers are 95% confidence intervals based on standard errors clustered at the district level.

spillovers were insufficient to overcome disruption effects in those areas.

7 | Results: Literacy and Education

7.1 | Main Results

Table 4 presents the main difference-in-differences results for literacy outcomes. Column (1) shows that the simple post-reform effect is small and insignificant, indicating no average impact of Latin exposure on informational literacy.

However, columns (2)–(4) reveal substantial heterogeneity. The interaction between post-reform status and baseline literacy gap is strongly positive and significant: $\beta_2 = 0.089$ for informational literacy and $\beta_2 = 0.081$ for general literacy. This indicates that districts with lower pre-reform literacy experienced significantly larger improvements from the reform.

The interaction with Russian-speaking share is negative (-0.055 for informational literacy; -0.049 for literacy) and significant at the 10 percent level, consistent with greater disruption in districts where Cyrillic materials were more prevalent.

To interpret the magnitudes, consider the effect of moving from a district at the 25th percentile of the baseline literacy gap to one at the 75th percentile, a difference of approximately one standard deviation. The differential post-reform gain is 0.089 standard deviations of informational literacy and 0.081 standard deviations of general literacy. Effects of this size are meaningful but moderate: they sit at the lower end of the range typically reported for successful school-based interventions, and they are differential gains across districts rather than aggregate improvements.

7.2 | Placebo Test

Table 5 presents placebo tests using a fake reform date of 1992, critically, *before* the law was announced in September 1993. This timing rules out any anticipation effects or early preparation that may have begun after the 1993 announcement. The sample is restricted to pre-reform cohorts only (those with zero actual

Latin exposure, born 1965–1978). If the main results reflected pre-existing trends rather than true reform effects, one would expect significant coefficients on the placebo treatment.

The results strongly support the identification strategy. The placebo post indicator is small and statistically insignificant (0.063 for informational literacy, 0.062 for literacy). Most importantly, the placebo interaction term ($\text{Post}_{\text{placebo}} \times \text{Baseline Literacy Gap}$) is close to zero and highly insignificant: -0.023 for informational literacy and -0.051 for general literacy.

7.3 | Parallel Trends and Dynamic Effects

A central concern in any difference-in-differences design is that the identifying variation (here, the interaction of the post-reform indicator with pre-reform district characteristics) not be contaminated by pre-existing differential trends across districts. To address this concern directly, I estimate an event-study specification that allows the cohort-by-baseline-intensity interaction to vary flexibly across narrow birth-year bins. The estimating equation is

$$Y_{idt} = \sum_{c \neq \text{ref}} \beta_c \mathbf{1}[c_i = c] \times \text{LitGap}_d + \mu_c + \theta_d + \gamma X_i + \varepsilon_{idt}, \quad (9)$$

where c indexes 2-year birth cohort bins from 1965–70 to 1993–94. The reference category is the 1975–78 cohort, the last cohort whose schooling was *entirely* pre-reform (mean Latin exposure: 0.07 years). The next cohort (1979–82) already received 1–4 years of Latin schooling and is therefore partially treated. Anchoring on the truly untreated cohort is the conservative choice: it isolates dynamic deviations relative to a clean pre-reform baseline rather than comparing one partially-treated group to another (J. D. Angrist and Krueger 1991; Sun and Abraham 2021).

Figure 1 plots the $\hat{\beta}_c$ for informational literacy and general literacy, with 95% confidence intervals. The two truly pre-reform interactions (1965–70 and 1971–74) cluster tightly around zero and are individually insignificant in both outcomes. A joint F -test that these two pre-reform interactions equal zero comfortably fails to

TABLE 4 | Effect of latin exposure on literacy: difference-in-differences.

	(1) Post only	(2) Post $\times$ LitGap	(3) Post $\times$ Russian	(4) Post $\times$ Urban
Panel A: informational literacy				
Post (any Latin)	−0.021 (0.034)	−0.021 (0.033)	−0.023 (0.034)	−0.021 (0.034)
Post $\times$ baseline literacy gap		0.089*** (0.027)		
Post $\times$ Russian share			−0.055* (0.031)	
Post $\times$ urban share				−0.027 (0.031)
R-squared	0.497	0.498	0.497	0.497
Panel B: literacy				
Post (any Latin)	−0.015 (0.034)	−0.015 (0.034)	−0.017 (0.035)	−0.015 (0.034)
Post $\times$ baseline literacy gap		0.081*** (0.027)		
Post $\times$ Russian share			−0.049* (0.028)	
Post $\times$ urban share				−0.008 (0.031)
R-squared	0.446	0.448	0.447	0.446
Observations	4070	4070	4070	4070
Age FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes

Note: Standard errors clustered at district level in parentheses. Baseline intensity measures computed from pre-reform cohorts only (years_latin = 0). All specifications include controls for gender, household size, wealth quintile, urban residence and Russian proficiency.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

TABLE 5 | Placebo test: Fake reform year (1992, before law announced).

	(1) Info lit	(2) Literacy	(3) Info lit	(4) Literacy
Post _{placebo} (born ≥ 1975)	0.063 (0.061)	0.062 (0.061)	0.063 (0.061)	0.062 (0.062)
Post _{placebo} $\times$ literacy gap			−0.023 (0.041)	−0.051 (0.045)
Observations	1304	1304	1304	1304
R-squared	0.565	0.507	0.565	0.508
Sample	Pre-reform	Pre-reform	Pre-reform	Pre-reform

Note: Sample restricted to cohorts with zero actual Latin exposure (born 1965–1978). Placebo treatment assigns “post” status to cohorts born 1975 or later, who would have been in school in 1992 if implementation occurred then. This predates the September 1993 law announcement, ruling out anticipation effects. The insignificant interaction coefficients ($p = 0.57$ and $p = 0.27$) confirm no pre-existing differential trends, validating the main result where Post $\times$ Literacy Gap = 0.089***. Standard errors clustered at district level.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

reject: $F_{2,74} = 0.09$, $p = 0.91$ for informational literacy; $F_{2,74} = 0.28$, $p = 0.76$ for general literacy. The post-reform interactions behave differently. They are positive in six of the seven post-reform bins for informational literacy and in all seven for general literacy. The effect appears immediately with the first partially exposed bin (1979–82, 1–4 years of Latin schooling), at 0.140*** for informational literacy and 0.132*** for general literacy, and remains large for the 1983–84 bin (0.155*** in both outcomes). Several middle bins are individually imprecise, reflecting the narrow two-year cells, and the interactions are again significant for the 1991–92 bin (0.110*** and 0.138***). The youngest bin (1993–94) is close to zero; these respondents were only 19–20 years old at the time of the 2013 survey, so their measured adult skills are least comparable to those of older cohorts.

Two facts deserve emphasis. First, the absence of a pre-reform trend rules out the most natural confounding story, namely that high-literacy-gap districts were on a differentially improving trajectory for reasons unrelated to the script reform. Second, the treatment effect appears as soon as a cohort receives any Latin instruction rather than growing linearly in years of exposure: the first partially treated bin is statistically indistinguishable from the fully treated bins. This pattern is consistent with the reform operating as a school-level regime change, in which new textbooks, retrained teachers and renewed pedagogical attention arrived as a bundle, rather than as a per-year dose of Latin exposure. The continuous-treatment check in Section 7.5 reinforces this reading. Together, the flat pre-trends and the timing of the post-reform response support the interpretation of the main DiD coefficients.

7.4 | Restricting the Comparison Window

As an additional check, I restrict the sample to birth cohorts 1975–1994, so that identification relies only on cohorts close in age to the treated group. Table 6 shows that the Post $\times$ baseline literacy gap interaction is, if anything, larger in this window (0.133 for informational literacy; 0.126 for literacy), indicating

TABLE 6 | Restricted comparison window: Birth cohorts 1975–1994.

	(1) Info literacy	(2) Literacy
Post $\times$ literacy gap	0.133*** (0.041)	0.126*** (0.041)
Post (any Latin)	−0.031 (0.055)	−0.016 (0.058)
Young at reform (born 1983–93)	0.023 (0.034)	−0.027 (0.035)
Observations	3147	3147
R-squared	0.496	0.454
Age FE	Yes	Yes
District FE	Yes	Yes

Note: Sample restricted to 1975–1994 birth cohorts. The triple interaction Post $\times$ Young $\times$ Literacy Gap and the Young $\times$ Post term are collinear with treatment conditional on age fixed effects in this window and cannot be separately estimated. Standard errors clustered at district level.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

that the main results are not an artefact of comparisons with much older cohorts. A full triple-difference specification that additionally interacts treatment with a “young at reform” indicator (born 1983–1993, i.e., in school during the transition) cannot be separately identified here: conditional on age fixed effects within this window, the indicator and its interactions are collinear with treatment status, so I report the restricted-window difference-in-differences instead.

7.5 | Robustness Checks

I conduct several robustness checks to ensure the main findings are not driven by specification choices, outliers or inference assumptions.

7.5.1 | Dose-Response With Continuous Treatment

Rather than using a binary post-reform indicator, I re-estimate the baseline specification using a continuous measure of exposure, defined as years of Latin-script schooling (0–11). Column (2) of Table 7 reports the interaction between years of Latin exposure and the baseline literacy gap. The per-year interaction is small and statistically insignificant: 0.002 (standard error 0.002) for informational literacy, and 0.004 (standard error 0.003) for general literacy in the companion regression. The absence of a linear per-year gradient, combined with the event-study finding that the first partially exposed cohorts gain as much as fully exposed ones, indicates that the reform operated as a discrete regime change at the school level rather than as a treatment accumulating smoothly with each additional year of Latin schooling. The binary post-reform indicator is therefore the appropriate parameterisation, and I interpret the continuous specification as a diagnostic of functional form rather than as the preferred estimate.

7.5.2 | Alternative Clustering

With 75 district clusters, standard cluster-robust inference should be reliable, but I verify this using several approaches. First, I cluster standard errors at the region level (14 clusters), which is more conservative. The interaction coefficient remains significant at the 1% level with nearly identical point estimate (Table 7 Column 3). Second, I implement wild cluster bootstrap with 999 replications (Cameron et al. 2008). The bootstrap p -values are 0.002 at the district level and 0.000 at the region level, confirming significance under this more robust inference procedure.

7.5.3 | Leave-One-Out Sensitivity

I re-estimate the main specification dropping each district in turn to verify that no single district drives the results. The distribution of 75 leave-one-out coefficients has mean 0.091, standard deviation 0.003 and range [0.083, 0.104]. All 75 leave-one-out estimates remain significant at the 5% level. The narrow range confirms that the main finding is not driven by outlier districts.

7.5.4 | Bandwidth Sensitivity

The main analysis uses birth cohorts 1965–1994. Column (4) of Table 7 shows that restricting to ± 15 years around the 1989 cutoff yields a coefficient of 0.093 (significant at the five percent

TABLE 7 | Robustness checks: Main literacy results.

	(1) Baseline	(2) Continuous	(3) Region SE	(4) BW $\pm$ 15	(5) No controls
Post $\times$ literacy gap	0.091*** (0.027)		0.091*** (0.026)	0.093** (0.039)	0.094*** (0.027)
Years Latin $\times$ Lit gap		0.002 (0.002)			
Observations	4070	4070	4070	3244	4070
R-squared	0.496	0.495	0.496	0.491	0.493
Wild bootstrap p	0.002		0.000		

Note: Dependent variable: Informational Literacy (standardised). Column (1) reports the baseline specification. Column (2) replaces the post-reform indicator with continuous years of Latin exposure (0–11). Column (3) clusters standard errors at the region level (14 clusters). Column (4) restricts the sample to ± 15 years around 1989. Column (5) excludes individual controls. Wild bootstrap p -values are computed with 999 replications. All specifications include age and district fixed effects.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

level). Narrower bandwidths are not informative in this design because the baseline district characteristics are computed from pre-reform cohorts (born before 1979); restricting to very narrow windows around 1989 eliminates the cohorts that identify the baseline intensity measure.

7.5.5 | Controls

Column (5) shows that excluding individual controls yields a nearly identical coefficient (0.094), suggesting the results are not driven by differential selection on observables.

7.5.6 | Coefficient Stability and Oster (2019) Bounds

Adding the full set of individual controls moves the interaction coefficient only from 0.094 to 0.091, whereas the R^2 rises from 0.493 to 0.496. Applying the Oster (2019) bounding exercise mechanically to these quantities, with $R_{\max} = 1.3 \times R^2$ and $\delta = 1$, yields a bias-adjusted coefficient of -0.043 and an identified set of $[-0.043, 0.091]$, which includes zero. I report this bound for completeness but I highlight that in specifications saturated with age and district fixed effects, the baseline R^2 is mechanically high and the incremental explanatory power of the individual controls is close to zero (0.004 here); the Oster adjustment scales the small coefficient movement by the large ratio of unexplained to newly explained variance, so the bound becomes uninformative precisely when the observables barely matter. As Oster (2019) notes, the procedure is most informative when observables meaningfully shift explanatory power, which is not the case in this design. The more relevant evidence against confounding is therefore design-based: the flat pre-reform event-study interactions, the 1992 placebo and the near-invariance of the point estimate across control sets.

7.6 | Mechanisms Behind the Null Average Effect

The main result of this paper, that the reform had no significant average effect on literacy despite very large differential effects across districts, naturally raises the question of *why* the average is null. This subsection presents three pieces of evidence that, taken together, identify the dominant mechanism.

7.6.1 | Implementation: Urban versus Rural Rollout

Anecdotal accounts (Schweitzer 2020; Saida 2023) document that Latin-script textbooks, retrained teachers and supplementary materials reached urban schools faster than rural ones. Columns 3–6 of Table 8 split the sample by urban residence. The Post $\times$ LitGap interaction is more than twice as large in urban districts (0.132*** for informational literacy and 0.131*** for general literacy) as in rural districts (0.057 and 0.043, both insignificant). The contrast is consistent with the implementation-gap story: where the reform was delivered fastest, its heterogeneous effect was strongest; where delivery lagged, the pre-existing literacy gap continued to depress measured outcomes. Although the urban–rural difference is not always statistically significant given the unequal sample sizes (1311 vs. 2758), the pattern is stable across specifications and aligns with descriptive accounts of phased implementation.

7.6.2 | Individual-Level Russian Proficiency: A Complementarity, Not a Substitute

A common reading of the Uzbek transition holds that de facto exposure to Latin varied at the individual level: Russian-fluent students would have continued to read Cyrillic in newspapers, family correspondence and signage, and would therefore be partly insulated from the school-side script switch. I test this hypothesis using individual self-reported Russian proficiency (*goodRus*) interacted with treatment in columns 1–2 of Table 8.

The data reject the substitute interpretation. The Post $\times$ *goodRus* coefficient is positive and significant (0.119** for informational literacy, 0.166*** for general literacy): Russian-fluent students post-reform actually score *higher* in literacy, not lower. The triple interaction Post $\times$ LitGap $\times$ *goodRus* is small and statistically insignificant in both outcomes (0.036 and 0.066, $p > 0.20$), so the heterogeneous gain across districts does not differ meaningfully by individual Russian proficiency. I interpret this pattern as a *complementarity* between Russian-language human capital and the new Latin-script curriculum. Russian-fluent students plausibly entered the reform with greater general literacy skill, more access to print materials at home, and a stronger habit of reading, all of which carried over

TABLE 8 | Mechanisms: De-facto cyrillic exposure and urban/rural implementation.

	(1) InfoLit Full	(2) Lit Full	(3) InfoLit Urban	(4) Lit Urban	(5) InfoLit Rural	(6) Lit Rural
Post (any Latin)	−0.059* (0.033)	−0.069* (0.035)	−0.051 (0.060)	−0.042 (0.063)	−0.022 (0.035)	−0.017 (0.037)
Post × LitGap	0.073** (0.030)	0.054* (0.029)	0.132*** (0.039)	0.131*** (0.038)	0.057 (0.035)	0.043 (0.037)
Post × goodRus	0.119** (0.049)	0.166*** (0.054)				
Post × LitGap × goodRus	0.036 (0.052)	0.066 (0.061)				
Observations	4070	4070	1311	1311	2758	2758
R-squared	0.499	0.449	0.537	0.547	0.517	0.444

Note: Columns 1–2 estimate the full sample with a triple interaction with individual Russian proficiency (*goodRus*). Columns 3–4 restrict to urban respondents; 5–6 to rural. All specifications include age and district fixed effects, individual controls (gender, household size and wealth quintile) and standard errors clustered at the district level. The full-sample Post × LitGap coefficient is smaller than the headline 0.089 in Table 4 because part of the heterogeneous gain in this expanded specification is absorbed by the additional Post × goodRus term, which is itself positive and significant (a complementarity between Russian-language human capital and Latin-script schooling). The triple interaction is statistically indistinguishable from zero in both outcomes.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

TABLE 9 | Schooling-attainment channel.

	(1) Tertiary	(2) InfoLit (with tertiary control)
Post (any Latin)	−0.005 (0.011)	−0.019 (0.033)
Post × LitGap	0.005 (0.011)	0.087*** (0.027)
Tertiary completion (1 = yes)		0.404*** (0.071)
Observations	4070	4070
R-squared	0.218	0.510

Note: Column 1: regressing tertiary completion on the same DiD specification yields a Post × LitGap coefficient of 0.005 ($p > 0.6$). The reform did *not* differentially raise tertiary completion in low-baseline-literacy districts, ruling out a mechanical schooling-attainment channel. Column 2: re-estimating the informational-literacy regression with tertiary completion as an additional control. Tertiary completion is, as expected, a strong individual predictor of literacy, but the Post × LitGap coefficient (0.087***) is essentially identical to the headline 0.089***, confirming that the heterogeneous gain operates within years of schooling rather than through extending them. Standard errors clustered at district level.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

to Latin Uzbek. The home-environment Cyrillic exposure does not appear to have crowded out the school-side script effect; if anything, the literate-household channel dominates.

7.6.3 | Schooling Attainment Versus Skill Formation

A final possibility is that the reform raised *years of schooling* (so that aggregate human capital rose) but had a wash effect on *conditional* literacy skill, because disruption costs offset the additional years. Column 1 of Table 9 regresses tertiary completion on the same DiD specification: the Post × LitGap

coefficient is 0.005 ($p > 0.6$). The reform did *not* raise tertiary completion in low-baseline-literacy districts. Column 2 re-estimates the informational-literacy regression including tertiary completion as an additional control. Tertiary completion is, unsurprisingly, a strong predictor of literacy ($\hat{\beta} = 0.404***$), but the Post × LitGap interaction is essentially unchanged at 0.087***, compared with 0.089*** in the headline specification. The heterogeneous gain in measured skill is therefore not a mechanical reflection of cohorts staying in school longer; it is a within-schooling effect on the conversion of schooling into reading skill.

7.6.3.1 | Synthesis. Taken together, the three pieces of evidence point to a single dominant mechanism: *implementation intensity at the school level*, not individual-level Cyrillic insulation, drives the heterogeneous gains. The reform was effective at the margin (in low-baseline-literacy districts where the school-side investment in textbooks, teachers and materials was both most needed and most concentrated, and where urban delivery was fastest) and disruptive in Russian-integrated districts where the legacy Cyrillic infrastructure offered better outside options. The two effects roughly offset on aggregate, producing the null average. The null is therefore not an artefact of a uniformly ineffective intervention; it is the equilibrium outcome of a real *redistribution* of human capital along district-level implementation and ethnolinguistic lines.

8 | Results: Occupational Choice

8.1 | State Employment and Entrepreneurship

Table 10 presents results for occupational outcomes among employed respondents. The reform shifted cohorts away from both state employment and entrepreneurship, with the state-sector effect estimated much more precisely. Latin-exposed cohorts are 8.2 percentage points less likely to work in the state

TABLE 10 | Effect of latin exposure on occupational choice.

	(1) State	(2) State	(3) State	(4) Entrep	(5) Entrep	(6) Entrep
Post (any Latin)	−0.082*** (0.030)	−0.097*** (0.026)	−0.082*** (0.029)	−0.044* (0.024)	−0.022 (0.022)	−0.045* (0.024)
Post × base state		−0.153*** (0.017)				
Post × literacy gap			−0.033 (0.021)			−0.019 (0.026)
Post × base Entrep					−0.112*** (0.017)	
Observations	2024	2020	2024	2024	2020	2024
R-squared	0.138	0.154	0.139	0.180	0.192	0.181
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample restricted to employed individuals. Baseline intensity measures computed from pre-reform cohorts only. Controls: gender, household size and wealth quintile; urban residence is absorbed by the district fixed effects. Standard errors clustered at district level.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

sector (column 1) and 4.4 percentage points less likely to be entrepreneurs (column 4).

These effects are strongly amplified by baseline district characteristics. In districts with higher pre-reform state employment, the shift away from state jobs is much larger: the interaction coefficient is -0.153 , implying that a one standard deviation increase in the baseline state employment share is associated with an additional 15.3 percentage point decline in state employment among treated cohorts. Similarly, the decline in entrepreneurship is concentrated in districts with higher baseline entrepreneurship: the interaction coefficient is -0.112 , and it absorbs the average effect, which falls to an insignificant -0.022 once the interaction is included (column 5). By contrast, the baseline literacy gap does not predict occupational responses (columns 3 and 6), suggesting that this channel operates through the sectoral composition of local labour markets rather than through the literacy margin.

8.2 | Interpretation

The occupational results point to a mechanism operating through institutional frictions rather than changes in preferences or skills. In particular, the decline in state employment is consistent with reduced comparative advantage in bureaucratic occupations. Government jobs rely heavily on written interaction with formal institutions, including laws, tax forms, administrative records and official correspondence. During the transition period, much of this material continued to be produced in Cyrillic. As a result, cohorts educated primarily in the Latin script may have faced higher costs when navigating Cyrillic-based bureaucratic environments, reducing their likelihood of entering or remaining in state employment.

A similar logic applies to entrepreneurship. Formal business activity requires engagement with regulatory institutions, such as business registration offices, tax authorities and licencing agencies, which also continued to operate largely in Cyrillic. Limited fluency in Cyrillic-based written materials may have increased the fixed costs of operating a business for Latin-educated individuals, discouraging entry. In addition, entrepreneurship is transmitted strongly within families (Lindquist et al. 2015), and a script gap between Cyrillic-literate parents and Latin-educated children plausibly weakened this channel of transfer.

The amplification of these effects in districts with higher baseline exposure to state employment and formal economic activity is consistent with institutional path dependence. Where administrative systems and economic interactions were more deeply embedded in Cyrillic-based practices, the short-to medium-run frictions induced by the script transition were correspondingly larger.

9 | Results: Attitudes

9.1 | Trust and Tolerance

Table 11 presents results from the LiTS III data on attitudes. Attitudes are known to respond to the institutional environment experienced during formative years (Campa and Serafinelli 2019), which motivates asking whether script exposure left a similar imprint. The average effects of Latin exposure on trust and tolerance are generally small and insignificant. Trust in government and trust in the president show no significant change, and the overall tolerance index is unaffected (Figure A2 and A3).

However, the interaction terms reveal important heterogeneity. The interaction between post-reform status and baseline trust in government is strongly negative (-0.047), suggesting that in regions with higher initial trust, the reform actually reduced

trust among treated cohorts. Similarly, the tolerance interaction is positive (0.102), indicating that in more tolerant regions, the reform increased tolerance further.

9.2 | Language-Based Intolerance

Perhaps the most striking attitude result concerns intolerance towards speakers of different languages. Table 12 shows that Latin-exposed cohorts are 10.9 percentage points more likely to say they would not want neighbours who speak a different language. The effect is marginally significant ($p = 0.052$) and should be read with the modest LiTS sample in mind, but it is specific to the linguistic margin: there are no significant effects on intolerance towards different religions, immigrants or unmarried couples.

This finding suggests that the script reform, by creating a visible marker distinguishing Latin-educated youth from Cyrillic-educated elders and Russian speakers, may have heightened awareness of linguistic boundaries and strengthened in-group and out-group distinctions based on language. The Azerbaijan comparator analysis (Appendix Tables A1 and A2) shows no such effect where the reform was completed quickly and the Russian-speaking minority was smaller, reinforcing the interpretation that the conditions of implementation, rather than the script change itself, generate the linguistic boundary.

10 | Conclusion

This paper provides the first assessment of a national script reform's effects on human capital, labour markets and social attitudes. Uzbekistan's 1996 transition from Cyrillic to Latin script at schools, part of a broader post-Soviet de-Cyrillisation movement, is important for evaluation due to its potential consequences beyond human capital formation.

The central finding is that the reform *had no significant average effect on literacy*, a result that speaks directly to policy debates surrounding ongoing transitions in Kazakhstan and potential future reforms elsewhere. Script reforms are often justified on modernisation grounds, but the Uzbek experience suggests these benefits do not materialise in aggregate literacy improvements, at least in the medium term.

However, this null average effect conceals substantial heterogeneity. Districts with lower pre-reform literacy experienced significant gains (0.089 SD per SD of baseline literacy gap), consistent with the marginal returns to educational investment being higher when starting from a low base. Conversely, districts with larger Russian-speaking populations experienced relative losses (−0.055 standard deviations, significant at the 10 percent level), reflecting the disruption costs of transitioning away from established Cyrillic materials and networks. These patterns reveal the *redistributive politics of nation-building*: the reform

TABLE 11 | Effect of latin exposure on attitudes (LiTS III).

	(1) Gov	(2) Gov	(3) Pres	(4) Pres	(5) Toler	(6) Toler	(7) Neighb	(8) Foreign
Post (any Latin)	−0.004 (0.019)	0.000 (0.022)	−0.023* (0.011)	−0.023 (0.013)	−0.203 (0.152)	−0.203 (0.152)	0.001 (0.032)	−0.078 (0.056)
Post × base trust Gov		−0.047*** (0.005)						
Post × base trust pres				−0.006** (0.002)				
Post × base tolerance						0.102** (0.045)		
Observations	874	874	918	918	938	938	924	759
R-squared	0.027	0.038	0.025	0.026	0.340	0.342	0.024	0.036

Note: LiTS III Uzbekistan sample, birth cohorts 1965–1994. Standard errors clustered at region level.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

TABLE 12 | Effect of latin exposure on specific tolerance items.

	(1) Diff language	(2) Diff religion	(3) Immigrant	(4) Unmarried
Post (any Latin)	0.109* (0.049)	0.005 (0.059)	0.012 (0.026)	0.077 (0.074)
Observations	938	938	938	938
R-squared	0.199	0.250	0.073	0.243

Note: Outcome: Would NOT want as neighbour (1 = mentioned). Higher values indicate greater intolerance. Standard errors clustered at region level.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

systematically transferred human capital from Russian-integrated areas to Uzbek-majority districts with historically low literacy.

Beyond literacy, the reform reshaped labour markets and social attitudes. Latin-educated cohorts moved away from state employment (by 8.2 percentage points) and, less sharply, from entrepreneurship (by 4.4 percentage points), occupations in which Cyrillic literacy and intergenerational knowledge transfer were particularly valuable. The decline in state employment aligns with the bureaucracy's continued reliance on Cyrillic during the transition period, whereas the reduced entrepreneurship is consistent with the disrupted transmission of tacit knowledge between Cyrillic-literate parents and Latin-educated children.

Perhaps most striking is the finding of increased linguistic intolerance among Latin-exposed cohorts, a marginally significant effect of 10.9 percentage points. Rather than promoting cosmopolitan openness within the country, the reform appears to potentially have created a boundary distinguishing the Latin generation from Cyrillic elders and Russian speakers, which may have heightened ethnolinguistic in-group preference. The effect is specific to language-based intolerance: there are no corresponding effects on religious or immigrant-based intolerance, consistent with script functioning as a salient marker of linguistic group membership. The absence of any comparable effect in Azerbaijan strengthens the reading that this is a consequence of Uzbekistan's protracted and incomplete implementation rather than of Latinisation as such.

10.1 | Policy Implications

These findings carry several policy implications, each of which generalises beyond Uzbekistan to other ongoing or contemplated script reforms.

10.1.1 | Implementation, Not Legislation, Drives Outcomes

The most important implication is that the gap between the de jure adoption of a new script and its de facto penetration of daily life is the principal mediator of the reform's effects. Uzbekistan announced Latin in 1993 and rolled it into schools in 1996, yet 30 years later the public sphere remains effectively bilingual in script, and the reform's measured effects on average literacy are zero. Governments contemplating analogous reforms (notably Kazakhstan, with its 2017–2031 phased rollout) should plan and budget for the sustained effort required to retire Cyrillic from media, government and signage; absent that effort, the school-side investment yields a weighted average of small gains and small losses. The implication is to potentially “budget for the long tail of implementation.”

10.1.2 | Reforms Have Predictable Distributional Winners and Losers

The 0.09 SD heterogeneous gain in low-baseline-literacy districts and the –0.05 SD loss in Russian-integrated districts show: complementary investments (textbook subsidies, teacher retraining and supplementary materials in both scripts during transition) should be *targeted* to the districts and demographic

groups most exposed to the disruption costs. In Uzbekistan, this would have meant targeting Russian-speaking and urban communities rather than treating the reform as uniform across districts.

10.1.3 | Age of Exposure Matters in Non-Obvious Ways

The event-study estimates show that gains appear with the very first partially exposed cohorts, whereas several mid-transition cohorts (born 1985–88) display smaller and statistically imprecise interactions, and the continuous-treatment check finds no linear per-year gradient. The reform behaved as a regime change whose payoff depended on when a cohort passed through school relative to the supply of Latin textbooks and retrained teachers, not as a treatment that accumulates smoothly with years of exposure. This argues for either a sharp-cutoff rollout (an all-grades transition in a single year, with materials pre-positioned) or an explicit transitional curriculum that teaches both scripts to bridge cohorts, an option that Uzbekistan did not pursue and that available evidence on Azerbaijan suggests was similarly under-resourced. A gradual rollout of the kind Kazakhstan has announced would, on this evidence, tend to concentrate disruption on mid-transition cohorts.

10.1.4 | Script Choice Is Not a Substitute for Language-of-Instruction Policy

Existing evidence on educational outcomes from language reforms (Aspachs-Bracons et al. 2008; Clots-Figueras and Masella 2013; Ramachandran 2017) concerns the language of instruction, not the script in which a language is rendered. The results here suggest that script reform alone, holding the language of instruction constant, delivers far smaller human capital effects than a language-of-instruction change. Policymakers framing script reform as a route to improved educational outcomes conflating two distinct policy levers. If literacy improvement is the goal, language and curriculum reforms dominate orthographic ones in expected effect size.

10.1.5 | Identity and Social-Cohesion Costs Should Be Priced in

The increase in language-based intolerance among Latin-exposed cohorts (10.9 percentage points more likely to refuse a different-language neighbour) is a real social cost not captured by any human-capital evaluation. Policymakers should expect script reform, like any visible identity-marker policy, to harden ingroup/outgroup boundaries between the cohort that learnt the new script and those that did not. In multi-ethnic states (Kazakhstan, with its large Russian minority; Tatarstan, whose 1999 Latin-script law was federally blocked precisely on these grounds), the social-cohesion cost may be the binding constraint.

10.1.6 | The Costs of Reform Are Real, Recurring and Almost Entirely Unmeasured

This paper measures the reform's effects on literacy and, in turn, on educational attainment, occupational choice and social attitudes. These are the outcomes on which script reforms are typically justified, and therefore the first that any evaluation should establish; measuring them is the necessary starting point rather than the whole account. The cost side of the ledger is at least as important and far less visible. A script transition involves

substantial one-off and recurring outlays, including reprinting textbooks and official forms, retraining teachers, converting administrative records and replacing signage, currency and documents, alongside private adjustment costs for firms that re-tool correspondence, accounting and customer-facing materials. That these costs recur rather than resolve is illustrated by the July 2026 law refining four letters of the Latin alphabet three decades after the founding reform (Gazeta.uz 2026), which reopens part of the conversion process. As in most script reforms internationally, the Uzbek transition was pursued mainly on symbolic and geopolitical grounds, and the detailed cost and monitoring data that a formal cost-benefit assessment would require are not routinely collected to my knowledge, in Uzbekistan or in comparable settings. The broader lesson concerns evidentiary infrastructure: quantifying these costs and weighing them against the human-capital effects is important for future research, and for governments contemplating similar reforms since they can support this effort by building cost estimation and data collection into the process from the outset.

10.1.7 | Lessons for the Kazakhstan and Mongolia Transitions

Two ongoing reforms illustrate the value of these implications. Kazakhstan adopted a Latin-alphabet decree in October 2017, revised the alphabet twice (2018, 2021) and now plans school rollout from 2023 with full transition by 2031. The Uzbek evidence suggests that (i) the long rollout will lock in transition-cohort disruption costs for the better part of a generation, and (ii) the modest projected human-capital benefits are unlikely to be realised through the stated “digital economy” and “educational competitiveness” channels; those goals are better served by curriculum and language-of-instruction reforms. Mongolia’s planned transition (Cyrillic to traditional Mongolian, announced 2020 with a 2025 implementation target) faces the same trade-offs in inverse direction: a symbolic break with Soviet-era literacy infrastructure that should not be expected to deliver aggregate human-capital gains.

In sum, script reforms are powerful symbolic instruments, but the evidence here does not support treating them as engines of literacy or modernisation. The policy case for them stands or falls on identity, geopolitical alignment and long-run cultural objectives, not on near-term human-capital returns. When such reforms are undertaken, complementary policies that target the districts and groups bearing the heaviest disruption costs are essential to preserving social cohesion through the transition.

Several limitations warrant acknowledgement. First, the CALISS literacy measures capture skills 17 years after implementation; longer-term effects as Latin becomes more dominant in Uzbek society may differ. Second, I cannot directly observe implementation quality, teacher training or textbook availability at the district level, limiting my ability to pin down mechanisms precisely. Third, the LiTS III sample for attitude analysis is relatively small, and the linguistic intolerance result is only marginally significant.

Future research could extend this analysis in several directions. The ongoing script transitions in Kazakhstan (begun 2017) and Mongolia (Cyrillic to traditional Mongolian, announced 2020)

provide opportunities for prospective evaluation with pre-reform baseline data. Within-family analysis comparing siblings with differential exposure could strengthen causal identification. And as Uzbekistan’s transition approaches completion, researchers can examine whether initial disruption effects fade as society converges on a single script. More broadly, literacy is only one dimension of the reform’s welfare footprint; a full accounting would also require data on implementation expenditure, firm-level adjustment costs, migration and labour-market sorting, media consumption and the intergenerational transmission of language and skill, few of which are systematically collected in Uzbekistan or comparable settings. Building that evidence base, ideally through prospective data collection around the Kazakhstan and Mongolia transitions, would let policymakers set the human-capital and social effects established here against the full costs of transition, turning the evidence on script reform into a complete cost-benefit basis for reform design. Understanding the long-run dynamics of script reform remains an important area for future work.

Conflicts of Interest

The author declares no conflicts of interest.

Data Availability Statement

The data that support the findings of this study are available from GIZ, World Bank and IFPRI. Restrictions apply to the availability of these data, which were used under license for this study. Data are available from the author(s) with the permission of GIZ, World Bank and IFPRI.

Endnotes

¹ As this paper was under revision, Uzbekistan’s Legislative Chamber passed a bill (July 2026, pending Senate approval) replacing the digraphs *sh*, *ch*, *o’* and *g’* with the single letters *ş*, *ç*, *ö* and *ğ*, partially reversing the 1995 revision that had introduced those digraphs amid cooling relations with Türkiye (Gazeta.uz, 2026). More than three decades after the 1993 law, the script itself remains under active revision and Cyrillic remains in widespread parallel use, underscoring the central point of this paper that de-Cyrillisation is a protracted, politically contingent process rather than a discrete event.

² I extend the attitude analysis to the LiTS III 2016 Azerbaijan subsample, which carries the *same* attitude battery (trust in government, president, neighbours and foreigners, and intolerance towards different-language, different-religion, immigrant and unmarried neighbours) used for the Uzbekistan analysis in Section 9. Building treatment around Azerbaijan’s 1992 school rollout (Post = 1[years_latin > 0], $n = 1,043$ –1,127 across outcomes, birth cohorts 1955–1990, standard errors clustered at the eight LiTS regions), the Azerbaijan estimates are uniformly null. In particular, the post effect on intolerance towards different-language neighbours is -0.003 (SE 0.031, $p = 0.94$), in contrast to Uzbekistan’s $+0.109$ ($p = 0.052$) on the same outcome. The Azerbaijan trust outcomes are also indistinguishable from zero. I interpret this contrast as informative: the Uzbek language-intolerance result is country-specific, not a generic feature of post-Soviet Latin reforms. Appendix Tables A1 and A2 report the full Azerbaijan results.

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Appendix A: Additional Figures and Tables

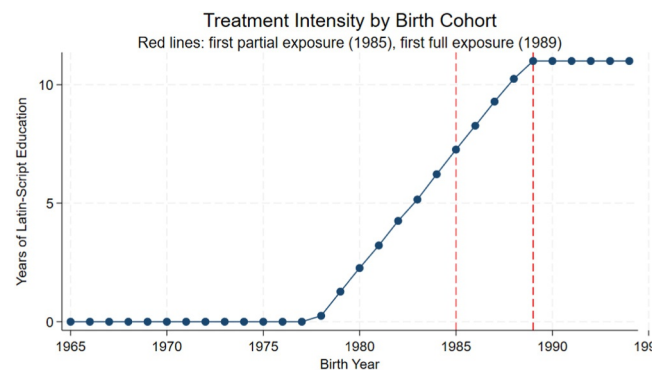


FIGURE A1 | Treatment Intensity by Birth Cohort. Years of Latin-script education by birth year, computed from the school-entry rule and the September 1996 implementation date. Cohorts born before 1979 completed schooling before the reform and have zero Latin exposure. Cohorts born 1979–1988 have partial exposure (1–10 years). Cohorts born 1989 or later have full exposure (11 years). The vertical dashed lines mark the 1985 cohort, the first with a majority of schooling (seven of eleven years) in Latin, and the 1989 cohort, the first with full exposure.

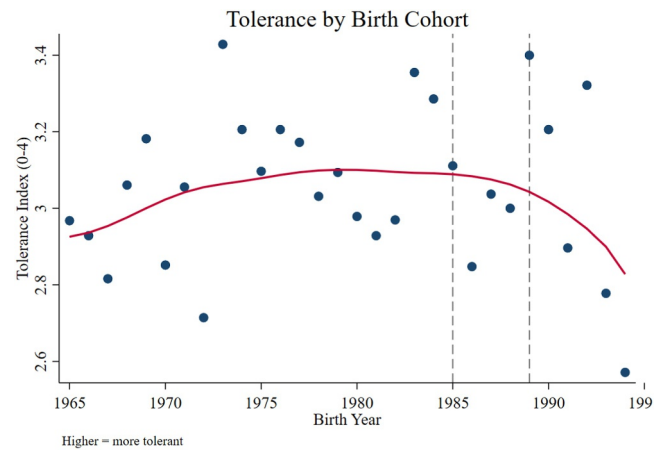


FIGURE A2 | Tolerance by Birth Cohort. Mean tolerance index (0–4, higher = more tolerant) by birth year with lowess smoother. Vertical dashed lines indicate first partial (1985) and full (1989) Latin exposure. Source: LiTS III.

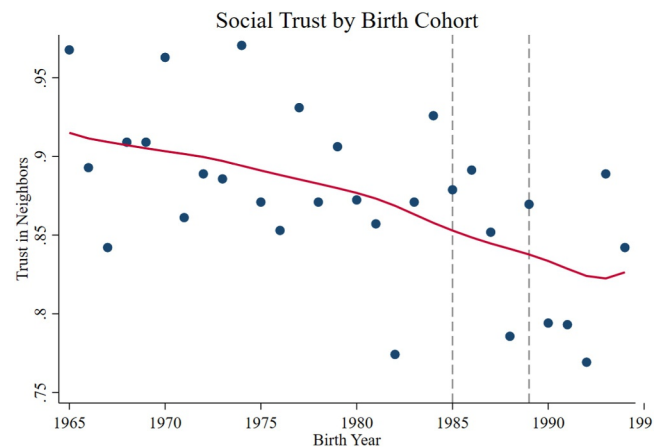


FIGURE A3 | Trust in Neighbours by Birth Cohort. Share reporting high trust in neighbours by birth year with lowess smoother. Vertical dashed lines indicate first partial (1985) and full (1989) Latin exposure. Declining trend visible among post-reform cohorts. Source: LiTS III.

Survey Questions for Literacy Assessment

The literacy and informational literacy modules from CALISS are reproduced with permission from IFPRI. See survey documentation for complete item wording.

Literacy assessment components:

- Vocabulary recognition (5 items)
- Idiom comprehension (1 item)
- Complex sentence interpretation (1 item)
- Narrative passage comprehension (5 items based on 257-word text)
- Document literacy: medicine instructions (2 items)
- Document literacy: bus schedule (2 items)

Informational literacy assessment components:

- Text comprehension (3 items)
- Table interpretation (2 items)
- Graph reading (2 items)
- Advertisement comprehension (2 items)
- Government notice comprehension (4 items)

TABLE A1 | Azerbaijan comparator: Latin reform and trust (LiTS III, 2016).

	(1) Trust gov	(2) Trust pres	(3) Trust neigh	(4) Trust foreign	(5) Tolerance
Post (any Latin)	−0.025 (0.062)	−0.049 (0.057)	−0.017 (0.035)	0.006 (0.031)	−0.000 (0.023)
Observations	1043	1091	1111	1095	1127
R-squared	0.124	0.218	0.020	0.026	0.188

Note: Azerbaijan subsample of LiTS III (2016), birth cohorts 1955–1990. Treatment is constructed from Azerbaijan's 1992 school rollout using the same school-entry rule as for Uzbekistan; Post = 1[years of Latin schooling > 0]. Outcomes (1)–(4) equal one if the respondent reports trust or complete trust; outcome (5) is a composite tolerance index. All specifications include female, urban, linear and quadratic birth-year trends and region fixed effects. Standard errors clustered at the region level (8 regions).

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

TABLE A2 | Azerbaijan comparator: Specific intolerance items (LiTS III, 2016).

	(1) Diff language	(2) Diff religion	(3) Immigrant	(4) Unmarried
Post (any Latin)	−0.003 (0.031)	0.029 (0.060)	0.025 (0.025)	−0.052 (0.033)
Observations	1127	1127	1127	1127
R-squared	0.041	0.093	0.133	0.237

Note: Outcome equals one if the respondent named the group as one they would not want as neighbours. Sample and specification as in Table A1. The Uzbekistan estimate for the same different-language outcome is +0.109 ($p = 0.052$; Table 12), so the contrast between the two countries is the substantive point. Standard errors clustered at the region level (8 regions).

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.