"The Classroom of Babel": Effect of Local Language Instruction on Human Capital in Uganda*

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September 28, 2026

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Abstract

This paper studies whether teaching the first three grades of primary school in a local language, rather than English, raises later human capital and labor-market outcomes, and who gains. I examine Uganda's 2007 Thematic Curriculum, which switched Primary 1–3 nationwide from English to local-language instruction. Using the 10 percent sample of the 2024 Uganda Census, I estimate January school-entry discontinuities at 19 birth-cohort cutoffs and compare them across cohorts in a difference-in-discontinuities design. In pre-reform cohorts, being born just after January 1 means about two fewer years of schooling. That gap does not change at the statutory transition cutoffs (1998–2000), but it narrows by 0.72 years from the 2001 cohort onward, alongside 5.3 and 5.7 percentage-point smaller gaps in literacy and primary completion. The schooling change is robust to bandwidth choice, to dropping any cutoff, and to restricting to documented birth dates; it halves net of a common linear trend. Employment changes little, with no sign of a penalty from less early English. The gain is broad-based: it is similar whether or not a child's home language matched the area's language of instruction, although language match predicted large attainment gaps before the reform. A first earnings projection and a sufficient-statistics welfare framework close the paper.

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1 Introduction

Educational attainment in Sub-Saharan Africa remains low: one in five primary-age children is out of school (UNESCO Institute for Statistics 2019). Most children in the region begin school in a language they do not speak at home. Teaching the early grades in a familiar language may improve comprehension and foundational learning, and those gains may accumulate into literacy and further schooling. English or French, however, remains the language of later schooling and of formal work. Education systems therefore face a real trade-off in the early grades, between instruction children understand and early exposure to the language they will need later. Many countries now teach lower primary in local languages, but evidence on the long-run consequences of such national reforms remains limited.

This paper asks three questions. First, does early instruction in a local language raise later human capital, measured by schooling and literacy? Second, does it lead to stronger labor-market outcomes, or does less early English offset the gains? Third, who gains: do children taught in their home language benefit more than children taught in a local language that is not their own?

I study Uganda's 2007 Thematic Curriculum, which switched Primary 1 through Primary 3 nationwide from English to local-language instruction, with a transition year in Primary 4 and English thereafter. Before the reform, lower primary was taught in English; the reform is therefore an extensive-margin switch. I use the 10 percent sample of the 2024 Uganda Census, about 1.6 million people born between 1989 and 2011, and observe completed schooling, literacy, primary completion and employment for the cohorts on both sides of the reform.

The design exploits the school-entry rule. Under statutory entry at age six with a January birthday cutoff, children born in December and January of adjacent years enter Primary 1 a year apart. Because all three lower-primary grades switched in 2007, the January 1998, 1999 and 2000 cutoffs separate children with 0 and 1, 1 and 2, and 2 and 3 years of intended local-language instruction. A single January discontinuity is not the policy effect: every cutoff carries a large background gap, because December-born children enter school a year earlier than January-born children. I therefore estimate the discontinuity at each of 19 January cutoffs from 1991 to 2009 and compare them across cohorts, a difference-in-discontinuities design (Grembi et al. 2016; Picchetti et al. 2024) applied to multiple cutoffs (Cattaneo et al. 2016).

Four findings emerge. First, every January cutoff carries a large gap: in pre-reform cohorts, being born just after January 1 means about two fewer years of schooling. Second, the gap does not change at the statutory transition cutoffs: relative to 1991–1997, the 1998–2000 average is −0.14 years (95 percent confidence interval $[-0.32, 0.05]$). Third, the gap shrinks from the 2001 cohort onward, by 0.72 years of schooling, 5.3 percentage points in literacy and 5.7 percentage points in primary completion. The schooling change is robust to alternative bandwidths, to dropping any single cutoff, to pooling all cutoffs in one regression, and to restricting to documented birth dates; it halves net of a common linear trend. Employment moves by 1.6 percentage points, and this change is smaller and less stable, but there is no evidence of a penalty from less early English. Fourth, the gain is broad-based. It is similar for ethnic groups that are the local majority and for local minorities, and similar whether or not a child's own language is the language of instruction in their birth area, although language match predicted 0.5 to 1.1 fewer years of schooling for mismatched children before the reform.

The paper contributes first to the literature on language of instruction and human capital. Ethiopia's 1994 mother-tongue reform raised schooling by 0.65 years and primary completion by 5 percentage points (Ramachandran 2017), raised reading (Argaw 2016), and improved enrollment and on-time progression (Seid 2016). Cross-border comparisons find better reading where schools use an indigenous language (Laitin and Ramachandran 2022). Program evaluations find large learning effects: in Cameroon, 1.1 to 1.4 standard deviations, still 0.4 to 0.6 in grade 5 (Laitin et al. 2019); in Uganda, the Northern Uganda Literacy Project raised reading by 0.64 standard deviations, while a cheaper version had little effect, and gains persisted eight years later (Kerwin and Thornton 2021; Buhl-Wiggers et al. 2023); Uganda's School Health and Reading Program raised reading in 9 of 12 languages (Brunette et al. 2019). A randomized comparison in a Malawian refugee camp found suggestive benefits for social studies but not mathematics (H. B. Kim and S. Kim 2026). This paper adds evidence from a nationwide extensive-margin switch, with adult outcomes for the full census, and it measures who gains by the child's own language relative to the area's language of instruction rather than by ethnicity alone.

The paper also contributes to work on the labor-market value of language skills. Arabization in Morocco lowered the returns to schooling through lost French (Angrist and Lavy 1997); two additional years of mother-tongue instruction in South Africa raised schooling and wages by 1.5

to 2 percent (Eriksson 2014). English instruction did not raise English proficiency in Puerto Rico (Angrist et al. 2008), and English-first instruction lowered later English by 0.17 standard deviations in South Africa (Taylor and von Fintel 2016). I report schooling, literacy and employment for the same cohorts, and find gains without an employment penalty. Finally, the paper relates to research using school-entry cutoffs, which documents lasting relative-age effects (Bedard and Dhuey 2006; Black et al. 2011) and, in Uganda, the interaction of Universal Primary Education with school entry (Grogan 2009). A reform timed by the entry cutoff inherits the cutoff's large recurring gap. The design here addresses this by comparing discontinuities across 19 cutoffs, with inference that respects the discrete running variable (Kolesár and Rothe 2018).

The remainder of the paper proceeds as follows. Section 2 describes Uganda's language environment, the reform and intended exposure. Section 3 presents a conceptual framework and its predictions. Section 4 describes the data, and Section 5 the design. Section 6 reports the main results, Section 7 threats and robustness, and Section 8 the timing of the change. Section 9 asks who gains, Section 10 develops a first welfare calculation, and Section 11 concludes.

2 Background

2.1 Language and primary education in Uganda

Uganda has 41 living indigenous languages, and English has been the official language since independence in 1962. English is the language of later schooling, government and much formal-sector work, while most children enter school speaking a local language. Primary completion is low: 68 percent of the 2006 cohort and 65 percent of the 2016 cohort left school before Primary 7 (The World Bank 2026).

Local-language instruction in the early grades is not a new idea in Uganda. The 1992 Government White Paper recommended it, but the recommendation was largely not implemented, and English remained the medium of instruction (Uganda Education Policy Review Commission 1992; Ssentanda 2014). In 1997, Universal Primary Education (UPE) abolished primary fees, producing an enrollment surge and widespread late entry (Deininger 2003; Grogan 2009). UPE affected every cohort in the analysis, including the pre-reform comparison cohorts.

2.2 The 2007 Thematic Curriculum

The government introduced the Thematic Curriculum after a 2006 pilot (Altinyelken 2010). The curriculum aimed at "rapid development of literacy, numeracy and life skills at lower primary." On language, it stated that "wherever possible, the child should learn in his/her home language or at least a language that is familiar to the child," and that "all learning materials used in these three years will be provided in the language of instruction." Where "there is no predominant local or area language," schools could teach in English. Primary 4 was a transition year to English and to subject-based teaching (National Curriculum Development Centre 2008).

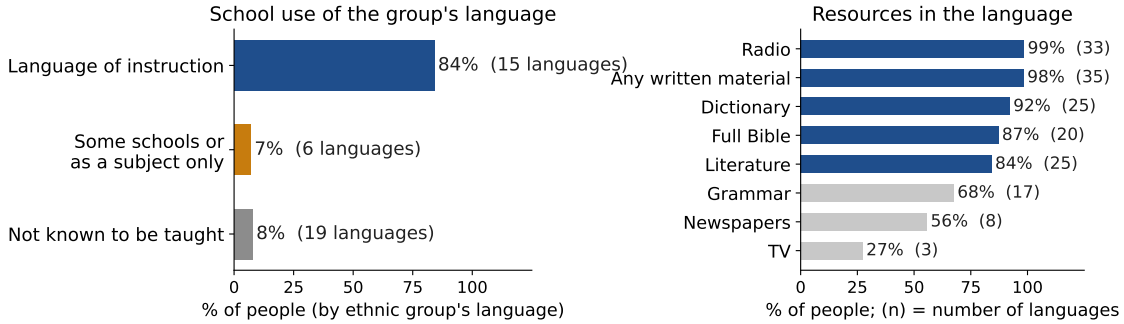
In 2007, Primary 1, 2 and 3 all switched to local-language instruction at once; the reform was not phased in one grade per year. The Primary 3 thematic curriculum materials were themselves piloted in 2008 (National Curriculum Development Centre 2008), so the language switch preceded the full set of materials. There was no national list assigning a language to each area: schools and districts chose the area's predominant local language. Implementation was uneven. Materials "were only slowly distributed to the school level and were not developed in all of the mother tongues that teachers were using" (Piper and Miksic 2011, quoted in UNICEF (2017)), and teacher preparation and classroom practice varied, including between urban and rural schools (Altinyelken et al. 2014). The reform was thus a literacy-and-numeracy reform delivered in local languages, with thin materials at first. Because language changed together with pedagogy and materials, the treatment studied here is the curriculum package, with language at its center.

Figure 1 shows the resulting language landscape. Of the 41 indigenous languages, 15 are used for instruction under the reform; they cover 84 percent of children's ethnic groups, and 8 percent of children belong to groups whose language is not known to be taught. Almost every group's language has radio and some written material, but newspapers exist in only eight.

2.3 Intended exposure across cohorts

The fixed implementation date creates variation in intended exposure across school-entry cohorts. Under statutory entry at age six with a January 1 birthday cutoff, a child born in year b enters Primary 1 in year $b + 7$. Because all three lower-primary grades switched in 2007, a child who entered Primary 1 in 2007 was intended to receive three years of local-language instruction, a 2006

Figure 1: Uganda's Languages and Languages of Instruction



Notes: Share of the analysis sample (N = 1.28 million, born about 1990–2010) by ethnic group, mapped to a language with a tribe-to-language crosswalk; in the 2024–25 Malaria Indicator Survey, 89–96 percent of women report the mapped language as their native language. Ethnologue describes current use, not use in 2007 (Ethnologue 2026).

entrant two years (Primary 2 and 3), a 2005 entrant one year (Primary 3), and earlier entrants none. Table 1 summarizes the mapping. The January 1998, 1999 and 2000 cutoffs change intended exposure from 0 to 1, 1 to 2, and 2 to 3 years. Before 1998 and after 2000, children on both sides of a January cutoff have the same intended exposure.

The mapping is an assignment rule, not a claim that every child entered on time and progressed one grade per year. The birthday rule is the statutory age-six convention; the original admission circulars for the relevant years have not yet been located. The relationship between intended and realized exposure is examined in Section 8.

Table 1: Intended Exposure to Local-Language Instruction by Birth Cohort

		Years of exposure to local language instruction if highest grade completed is:				
Year of birth	Year of primary school entrance	<Grade 1	Grade 1	Grade 2	Grade 3	Grade 4 or more
1997 or before	2004 or before	0	0	0	0	0
1998	2005	0	0	0	1	1
1999	2006	0	0	1	2	2
2000 or after	2007 or after	0	1	2	3	3

Notes: Shaded cells indicate years in which local-language instruction is intended. Primary 4 is treated as a transition year. The mapping assumes entry into Primary 1 at age six under a January 1 birthday cutoff, one-grade-per-year progression, and the 2007 switch of Primary 1–3 to local-language instruction.

3 Conceptual Framework

The framework combines a familiar-language productivity channel with dynamic skill formation and a separate return to English, following standard human-capital and skill-formation models (Becker 1964; Ben-Porath 1967; Cunha and Heckman 2007; Cunha et al. 2010). Let $L_i \in \{0, 1\}$ denote the language of instruction in Primary 1–3, with $L_i = 1$ for local language and $L_i = 0$ for English. Let h_{i0} denote skill at school entry, s_{i0} early schooling inputs, and h_{i1} foundational skill after Primary 3:

$$h_{i1} = A(L_i)\, s_{i0}^{\alpha}\, h_{i0}^{1-\alpha}, \qquad 0 < \alpha < 1. \tag{1}$$

$A(L)$ is the productivity of class time. If the child understands the language of instruction, $A(1) > A(0)$: the same inputs produce more foundational learning.

Later schooling, from Primary 4 onward, is in English and builds on foundational skill:

$$H_i = B\, h_{i1}^{\rho}\, s_{i1}^{1-\rho}, \qquad 0 < \rho < 1, \tag{2}$$

where H_i is adult human capital (schooling and literacy), s_{i1} later inputs, and ρ governs how much early skill carries forward. When early skill persists and complements later investment, a gain in h_{i1} raises literacy and makes continued schooling more productive.

In the labor market, let e_i denote English proficiency, which may be lower with less early English, and Y_i latent productivity:

$$Y_i = \omega H_i + \pi e_i + \varepsilon_i, \qquad \text{Employed}_i = \mathbf{1}\{Y_i > \bar{Y}\}, \qquad \omega, \pi \geq 0. \tag{3}$$

Local-language instruction may raise later English through stronger foundations, or lower it through less early exposure, so the net labor-market effect is ambiguous. Because the reform bundled language with pedagogy and materials, $A(L)$ reflects the curriculum package.

Table 2 states three predictions and how each is tested. Prediction P3 is the framework's distinctive implication: if the familiar-language channel operates through comprehension, the gain should be larger when the language of instruction is the child's home language.

Table 2: Predictions and How They Are Tested

Prediction	Test
P1. $A(1) > A(0)$ raises h_{i1} and, through ρ, H_i: literacy and schooling rise with exposure	Difference-in-discontinuities for literacy, years of schooling and primary completion (Section 6)
P2. Employment moves with $\omega\,\Delta H + \pi\,\Delta e$: ambiguous if early English falls	Difference-in-discontinuities for employment (Section 6)
P3. $A(1) - A(0)$ is larger when the language of instruction is the home language	Heterogeneity by likely language match: local ethnic majority, then own language versus the area's language of instruction (Section 9)

4 Data

The main source is the 10 percent sample of the 2024 Uganda Census, which records year and month of birth for everyone. I retain respondents with a valid birth month whose reported age is within one year of the age implied by their birth year. The analysis sample contains 1,621,282 people born between 1989 and 2011, which supports January cutoffs from 1991 to 2009; respondents at those cutoffs were about 15 to 33 years old at the census.

Table 3 describes the outcomes. Years of schooling is an index from 0 to 14 constructed from the highest level completed: no schooling and pre-primary are 0, Primary 1–7 are 1–7, senior secondary grades are 8–13, and post-secondary qualifications are 14; the bundled junior-secondary category is assigned 8.[1] Primary completion is an indicator for having completed at least Primary 7; it is not reported in Table 3 because the saved outputs contain only local means for it. Literacy equals one if the respondent can read and write with understanding in any language. Employment equals one for respondents who are working, including on their own farm. Schooling, literacy and primary completion are the primary outcomes; employment is exploratory, because it changes steeply with age over this range.

Two measurement issues matter for interpretation. First, census literacy is self-reported and lenient. In a direct reading test in the 2024–25 Malaria Indicator Survey, 77 percent of women aged 15–49 read at least part of a sentence and 56 percent a whole sentence, against 82 percent reporting literacy in the census (Appendix Table A1). A reading test in the 2016 Demographic and Health Survey nonetheless shows the same recurring January gap as census literacy. Second, birth

[1] The estimates use the corrected common coding developed for comparisons with the 2014 census. It differs from the coding in earlier versions of this paper only in assigning zero to 1,856 respondents in nursery categories that had been carried as numerical values; the main contrast moves from 0.715 to 0.724 years.

Table 3: Analysis Sample and Outcomes

Outcome	Mean	Definition
Years of schooling	6.5	Index 0–14 from highest level completed
Literacy	82.1%	Can read and write with understanding (any language)
Employment	66.9%	Working, including own-farm activity
Observations	1,621,282 (born 1989–2011; January cutoffs 1991–2009)	

Notes: 2024 Uganda Census, 10 percent sample; respondents with a valid birth month and reported age within one year of the age implied by birth year. Means are over the analysis sample.

dates are imperfect: reported dates heap on January and on the first day of the month. Section 7 examines how this affects the estimates.

I also use the 2002 and 2014 censuses (IPUMS International extracts), the 2016 Demographic and Health Survey and the 2024–25 Malaria Indicator Survey, to show that the January gap recurs across sources, to describe grade progression when the reform cohorts were of school age, and to link birthplace to the ethnic composition of districts before the reform.

5 Empirical Design

5.1 A single January cutoff

Let R_{ic} denote individual i's birth month relative to January of cutoff year c, centered so that December is -0.5 and January is $+0.5$, and let $D_{ic} = \mathbf{1}\{R_{ic} \geq 0\}$ indicate birth in or after January of year c. For each outcome and cutoff I estimate

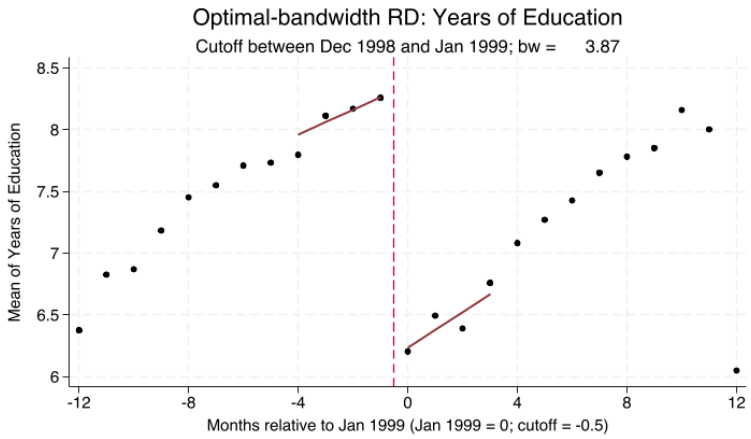
$$Y_i = \alpha_c + \tau_c D_{ic} + f_c(R_{ic}) + \varepsilon_i, \tag{4}$$

where f_c is a local linear function with separate slopes on each side, estimated with triangular weights and a mean-squared-error-optimal bandwidth selected separately for each outcome and cutoff (Calonico et al. 2014; Calonico et al. 2020).

Figure 2 shows the January 1999 cutoff. The discontinuity is large: $\tau_c \approx -2$ years of schooling. The same is true at every cutoff, because December-born children enter Primary 1 a year before January-born children. A single τ_c is therefore not the policy effect.

The recurring gap is not specific to the 2024 census. Pooling all cutoffs each source supports

Figure 2: Years of Schooling Around the January 1999 Cutoff



Notes: Monthly means of years of schooling by birth month relative to January 1999 and local linear fits with the MSE-optimal bandwidth. December is −1 and January is 0 on this axis; the cutoff lies between them. Drawn with the original schooling coding; single-cutoff graphs for 1997–2001 are in Appendix B.

into one regression with cutoff-specific intercepts and slopes and a single January jump, the average gap in years of schooling is −1.75 (standard error 0.05) over 40 cutoffs from 1970 to 2009 in the 2024 census, −2.08 (0.12) among women in the 2016 Demographic and Health Survey, and −1.52 (0.20) in the 2024–25 Malaria Indicator Survey. It is smaller in the 2002 census, −0.43 (0.03) over cutoffs from 1908 to 1996.

5.2 Difference-in-discontinuities

Each cutoff's discontinuity combines a background gap and the policy:

$$\widehat{\tau}_c = \lambda_c + \theta\,\Delta E_c + u_c, \tag{5}$$

where λ_c is the background January gap and ΔE_c the change in intended exposure at cutoff c: one at the January 1998–2000 cutoffs and zero otherwise. I compare period averages of the cutoff-specific estimates with the pre-reform average,

$$\widehat{\Delta}_g = \overline{\tau}_g - \overline{\tau}_{1991\text{–}97}, \qquad g \in \{1998\text{–}2000,\ 2001\text{–}2009\}, \tag{6}$$

following the logic of difference-in-discontinuities (Grembi et al. 2016; Picchetti et al. 2024). The identifying assumption is that the background gap λ_c is stable across cohorts. Under statutory

entry, the transition contrast ($g = 1998$–2000) measures the effect of one additional intended year, and the post contrast ($g = 2001$–2009) compares cutoffs at which both sides are equally exposed with cutoffs at which neither side is exposed; Section 8 returns to what the latter can and cannot reflect.

For inference, I treat each $\widehat{\tau}_c$ as one observation and compute standard errors from the dispersion of the estimates across cutoffs within each period, $\sqrt{s_g^2/n_g + s_{\text{pre}}^2/n_{\text{pre}}}$, with Welch–Satterthwaite degrees of freedom. The reported cutoff-level standard errors capture only sampling error within a cutoff; with a running variable observed only by month, pre-reform estimates vary about 1.8 times more than those standard errors imply (Kolesár and Rothe 2018). Between-cutoff inference accounts for this.

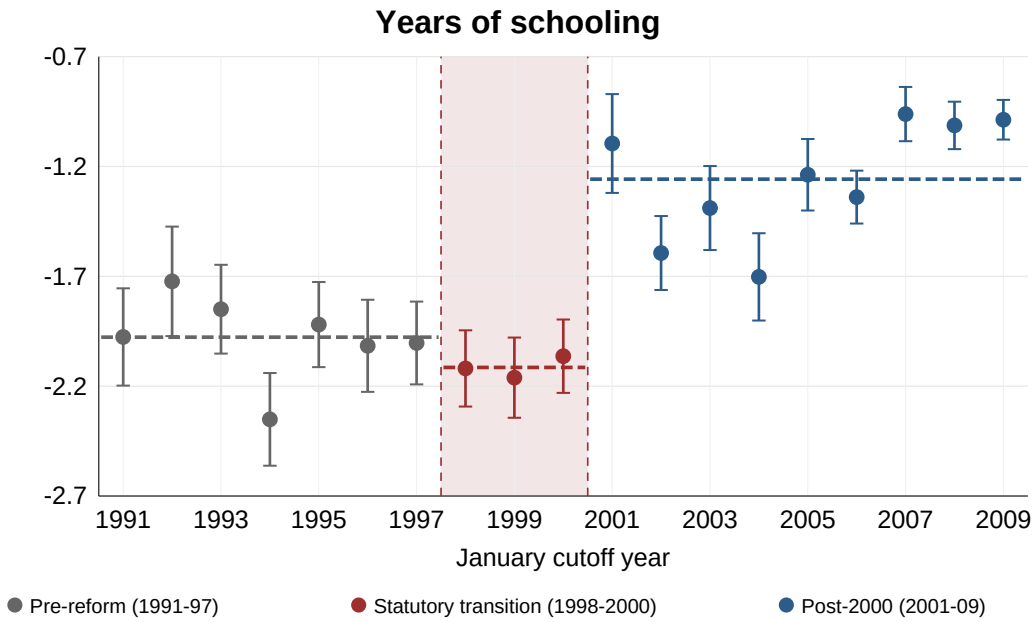
6 Main Results

Figures 3–5 plot the January discontinuity at each cutoff for schooling, literacy and employment, and Table 4 reports the difference-in-discontinuities estimates. Each point in the figures is the coefficient from a separate regression around one January cutoff; dashed segments are period averages.

In pre-reform cohorts, the January gap in schooling averages -1.97 years. At the three transition cutoffs it averages -2.11: the transition contrast is -0.14 years with a 95 percent confidence interval of $[-0.32, 0.05]$, so there is no favorable change where intended exposure changes under statutory entry. From the 2001 cutoff onward, the gap shrinks to -1.24 on average, a change of 0.72 years relative to the pre-reform average. This is 11 percent of mean schooling and closes about 37 percent of the pre-reform gap. Literacy follows the same pattern: no change at the transition, and a 5.3 percentage-point smaller gap after 2000. Primary completion, an outcome most cohorts have finished, shows a 5.7 percentage-point smaller gap (Appendix Figure A3). These patterns are consistent with prediction P1.

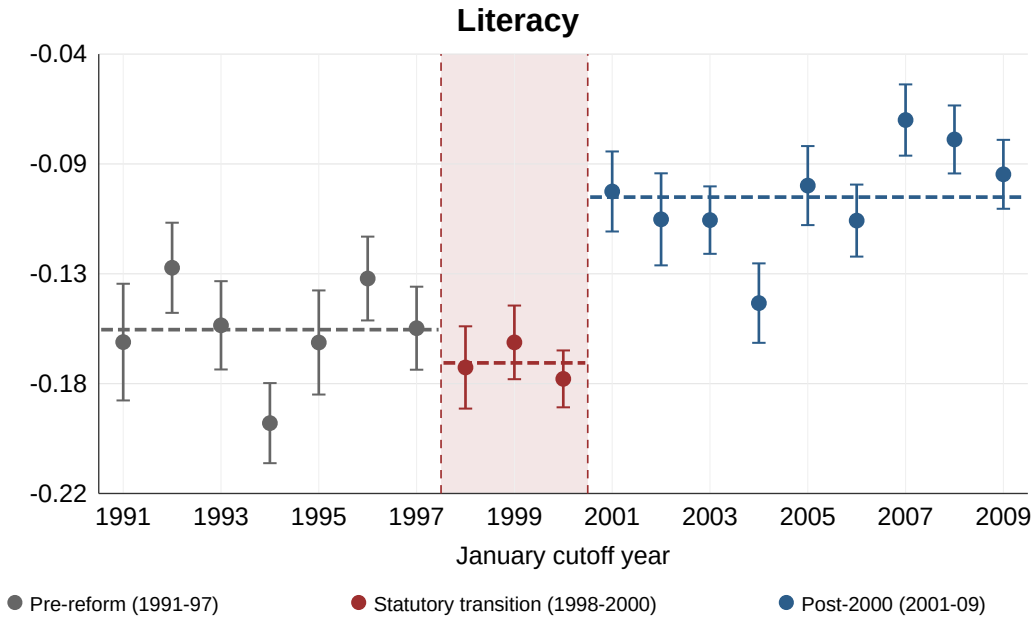
Employment moves by 1.6 percentage points after 2000, a smaller and, as Section 7 shows, less stable change. Consistent with the ambiguity in prediction P2, there is no evidence of an employment penalty from less early English.

Figure 3: Years of Schooling: January Discontinuities by Cutoff



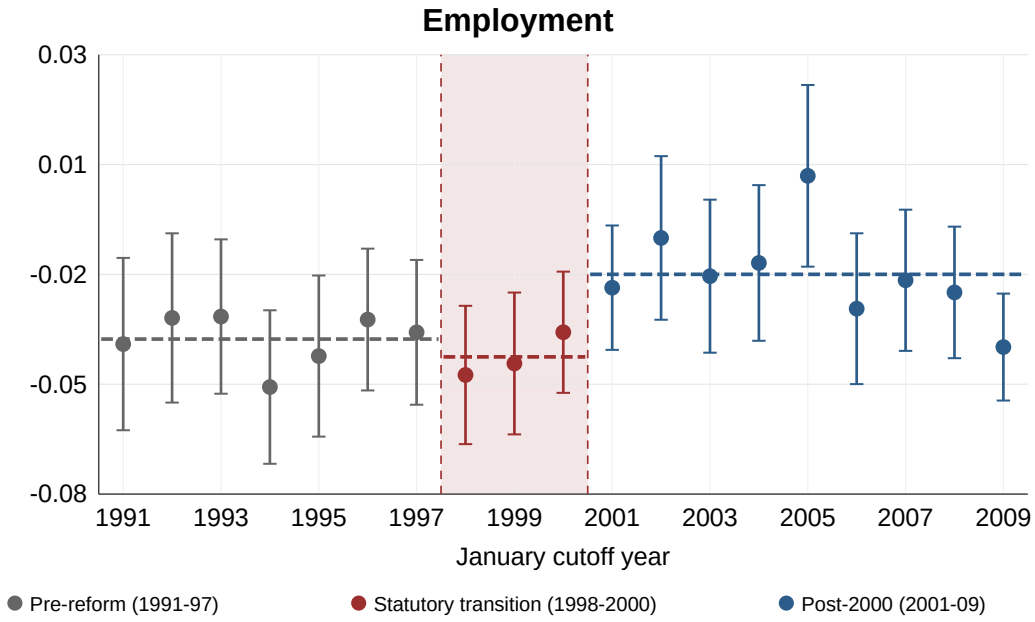
Notes: Each point is a separate January-cutoff local linear RD coefficient with an outcome- and cutoff-specific MSE-optimal bandwidth; bars are 95 percent confidence intervals from the cutoff regression. Dashed segments are unweighted period averages. The shaded region marks the January 1998–2000 transition cutoffs under statutory entry.

Figure 4: Literacy: January Discontinuities by Cutoff



Notes: See Figure 3. Literacy is a proportion.

Figure 5: Employment: January Discontinuities by Cutoff



Notes: See Figure 3. Employment is a proportion.

Table 4: Difference-in-Discontinuities Estimates

	Average January jump			Difference from pre-reform	
	1991–97	1998–2000	2001–09	Transition − Pre	Post − Pre
Years of schooling	−1.967	−2.106	−1.244	−0.139	**0.724*****
				(0.079)	(0.117)
Primary completion	−0.195	−0.199	−0.138	−0.004	**0.057*****
				(0.008)	(0.009)
Literacy	−0.155	−0.168	−0.101	−0.013	**0.053*****
				(0.009)	(0.011)
Employment	−0.037	−0.042	−0.021	−0.004	**0.016*****
				(0.004)	(0.005)
Number of cutoffs	7	3	9		

Notes: Unweighted averages of cutoff-specific local linear RD estimates with MSE-optimal bandwidths, 2024 Census. Parentheses: standard error of the difference in averages from the spread of estimates across cutoffs, $\sqrt{s_g^2/n_g + s_{\text{pre}}^2/n_{\text{pre}}}$ (Welch); tests use t with Welch–Satterthwaite degrees of freedom (about 7–8 for the transition and 11–14 for the post contrast). Primary completion, literacy and employment are proportions. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; bold: $p < 0.05$.

7 Threats and Robustness

Table 5 lists the main threats to interpreting the post-2000 change and the responses; Table 6 reports the robustness checks.

Table 5: Threats to Interpretation and Responses

Threat	Response
Birth dates heaped on January and on day 1	Donut RD: schooling holds, literacy and employment do not; registered births with a known day other than the 1st: 0.65 (Appendix Tables A2 and A5)
Within-year reporting gradient	Discussed in Section 7.2; the 2014 census and documented dates bound its role
Discrete running variable	Standard errors from the spread across cutoffs; all cutoffs in one regression with birth-month clusters: 0.78
Entry age not strictly enforced	Intended exposure is a reduced form; grade-progression evidence in Section 8
Cutoffs span ages 15–33 in 2024	Gains also in outcomes everyone has finished (ever attended, reached Primary 4)
Composition shifts at the cutoff	Female share and registration drop at January and shift after 2000, but predict *lower* schooling; with covariates as controls: 0.81 (Appendix Table A3)
Self-reported literacy	A direct reading test (DHS 2016) shows the same January gap
Concurrent change (UPE 1997, bundled pedagogy)	Interpreted as the curriculum package; UPE affects pre-1998 cohorts too

7.1 Robustness

Table 6 shows that the schooling change is stable across specifications. With fixed bandwidths from 3 to 12 months it ranges from 0.68 to 0.77. Dropping any single post-2000 cutoff leaves it between 0.69 and 0.78. Estimating all 19 cutoffs in one regression with cutoff-specific intercepts and slopes and period interactions, with standard errors clustered by birth month, gives 0.78 (0.07). Placebo "transitions" placed in three-year windows inside 1991–1997 range from −0.21 to 0.22, and the actual transition contrast lies inside that range.

The trend-and-break rows regress the 19 cutoff estimates on a linear cohort trend and level shifts at 1998 and 2001. The shift from the transition cutoffs to 2001 onward is 0.68 (0.19). Relative to the pre-reform period net of a common linear trend fitted to all 19 cutoffs, the change is 0.39 (0.29): about half the main estimate and not statistically significant. Net of the pre-reform trend alone, extrapolated forward, it is larger than the main estimate. How much of the change survives

therefore depends on what counterfactual path one assumes for the background gap, and I report both.

Literacy and primary completion are robust in direction across bandwidths, although literacy shrinks with wider bandwidths. Employment is not robust: its change falls to about zero at 9- and 12-month bandwidths and becomes negative in donut specifications. For literacy and employment, the gains sit largely in the December and January cells, where birth dates heap.

Table 6: How Robust Is the Post-2000 Change?

	Schooling	Primary completion	Literacy	Employment
Post − Pre, by specification				
Main: MSE-optimal bandwidth	**0.724***** (0.117)	**0.057***** (0.009)	**0.053***** (0.011)	**0.016***** (0.005)
Fixed bandwidth, 3 months	**0.767***** (0.113)	**0.062***** (0.009)	**0.054***** (0.010)	**0.018***** (0.005)
Fixed bandwidth, 6 months	**0.679***** (0.127)	**0.045***** (0.010)	**0.040***** (0.011)	0.008* (0.004)
Fixed bandwidth, 9 months	**0.694***** (0.140)	**0.042***** (0.011)	**0.033**** (0.011)	0.001 (0.004)
Fixed bandwidth, 12 months	**0.692***** (0.143)	**0.039***** (0.011)	**0.028**** (0.010)	−0.002 (0.004)
Leave one cutoff out	[0.686, 0.780]	[0.053, 0.059]	[0.049, 0.059]	[0.013, 0.018]
One regression, birth-month clusters	**0.780***** (0.074)	**0.061***** (0.006)	**0.055***** (0.007)	**0.011***** (0.004)
Trend-and-break regression across 19 cutoffs				
Shift from 1998–2000 to 2001+	**0.680***** (0.186)	**0.074***** (0.014)	**0.051***** (0.012)	**0.027***** (0.007)
Post vs. pre, net of common linear trend	0.390 (0.295)	**0.081***** (0.020)	0.025 (0.018)	**0.029**** (0.010)
Post vs. pre, net of extrapolated 1991–97 trend	**1.016***** (0.243)	0.074* (0.031)	0.053 (0.032)	0.017 (0.009)
Common linear trend (per cohort)	0.030 (0.023)	−0.002 (0.002)	0.003 (0.002)	−0.001 (0.001)
Joint test: both shifts = 0 (p)	<0.001	<0.001	<0.001	0.005
Placebo "transitions" within 1991–97	[−0.209, 0.224]	[−0.011, 0.014]	[−0.024, 0.013]	[−0.007, 0.006]

Notes: 2024 Census, corrected common outcome coding. Post − Pre and leave-one-out: Welch standard errors across cutoffs. One regression: all 19 cutoff windows estimated jointly with cutoff-specific intercepts and slopes, median bandwidth, standard errors clustered by birth month (114–190 clusters). Trend-and-break: $\widehat{\tau}_c$ on a linear trend, $\mathbf{1}\{c \geq 1998\}$ and $\mathbf{1}\{c \geq 2001\}$, robust standard errors, $t(15)$; "net of common linear trend" is the sum of the two shifts. Extrapolated trend: $\widehat{\tau}_c$ fitted on 1991–97 and projected to 2001–09. Placebo: each three-year window inside 1991–97 compared with the remaining pre-reform cutoffs. Donut and documented-date checks, available on the original coding, are in Appendix Table A5. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; bold: $p < 0.05$.

7.2 Birth-date reporting

The most important threat concerns birth-date reporting. Reported dates heap on January and on the first of the month: 37 percent of January births are recorded on the 1st, against 3.5 percent in June. More subtly, in the 2024 census schooling rises steadily across the birth year, from January-born to December-born, and then resets at the next January (Appendix Figure A1). Across the 19 cutoffs, the size of the fitted January discontinuity closely tracks the slope of this within-year gradient. A gradient of this kind is not what a January entry rule alone predicts, and one plausible source is that poorly documented birth dates default toward early months among less-educated respondents, less so for later cohorts born after birth registration improved. If that is the source,

part of the January gap, and part of its change over cohorts, reflects reporting rather than school entry.

Three pieces of evidence bound this concern. First, restricting to registered births with a known day of birth other than the 1st, the post-2000 change in schooling is 0.65 (0.15), close to the estimate in the full sample (Appendix Table A2). Second, the change also appears in outcomes that all cohorts have completed and that are less sensitive to exact age, such as ever attending school and reaching Primary 4. Third, comparing cohorts at the same age across censuses (Appendix Figure A2), the January gap among adults is considerably smaller in the 2014 census than in the 2024 census (-0.64 against -1.83 at ages of roughly 20 to 26), while in the 2014 census, children observed near school age show a clear January gap without a within-year gradient. The recurring gap therefore has a genuine school-entry component, but its level in the 2024 census is inflated by reporting. The documented-date estimate is the most conservative estimate of the post-2000 change; obtaining the census's birth-date editing and allocation flags would allow a direct test.

8 Timing: Why After 2000?

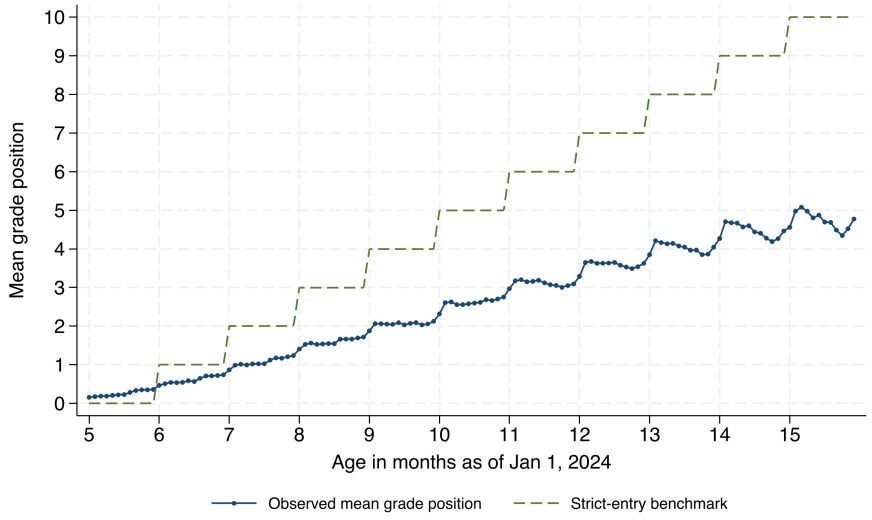
Under statutory entry, the transition cutoffs are 1998–2000 and nothing should change after 2000. The change instead appears from the 2001 cutoff onward and grows with recency: relative to the pre-reform average, the schooling gap is 0.54 (0.15) years smaller at the 2001–2004 cutoffs and 0.88 (0.11) smaller at the 2005–2009 cutoffs (Appendix Table A4). Under statutory entry every post-2000 cohort is equally exposed, so the change should be flat across post cutoffs. The growth also appears in outcomes everyone has finished.

School entry in Uganda is late and loose. Figure 6 compares mean grade by age among children in 2024 with the statutory benchmark: grade lags the benchmark at every age, and the same holds in the 2002 and 2014 censuses when the reform cohorts were of school age (Appendix Figure A8). Late entry, however, cannot by itself explain a change after 2000. Holding the reform date fixed, a child who enters Primary 1 a year late was born a year earlier than an on-time entrant in the same class. Late entry therefore moves the exposure transition to *older* birth cohorts, before 1998, not to later ones; grade repetition has the same effect.

Two explanations move in the right direction. The first is lagged classroom adoption. Materials

reached schools slowly and the Primary 3 materials were piloted only in 2008, so the full package may have reached a child's entire Primary 1–3 only for later entry cohorts: if delivery lagged the formal switch by a year, the first fully treated cohort is the 2008 entrants, born in 2001. A reform that matured over several years would also produce a gain that grows with recency. The second is changing reporting and composition, discussed in Section 7.2: birth registration improved for later cohorts and the within-year reporting gradient weakened. The documented-date estimates suggest that reporting does not account for the whole change, but separating the two requires historical school-level implementation records and the census birth-date flags.

Figure 6: School Progression and the Statutory Benchmark



Notes: Mean grade position by age in months as of January 1, 2024, among children aged 5–15. Children not attending school and children in pre-primary are coded zero. The dashed line is the benchmark under entry at age six and one-grade-per-year progression.

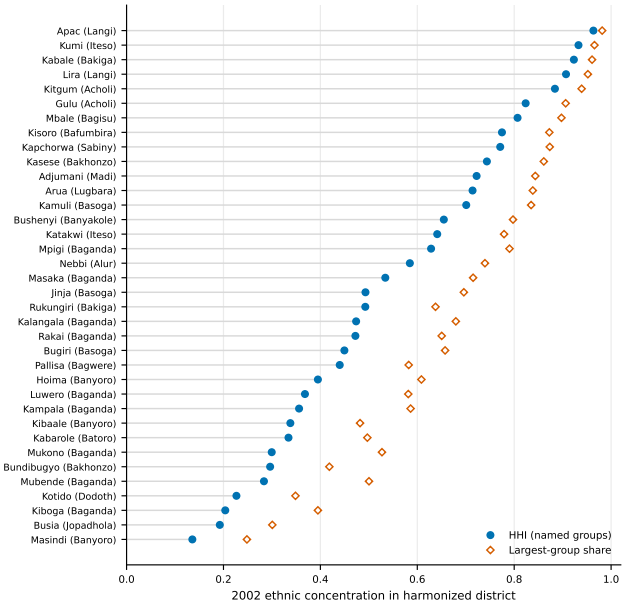
9 Who Gains?

Prediction P3 says the gain should be largest where the language of instruction is the child's home language. Schools taught in their area's main language. Children from the local majority group were therefore likely taught in their home language, while local minorities faced a new mismatch: before the reform everyone was taught in English, and after it minorities were taught in a local language that was not their own.

9.1 Measuring likely language match

I measure likely match in two ways. The first uses ethnicity: I link 2024 birthplace to 36 harmonized districts with 2002 census ethnic shares (94 percent of the sample links) and define a person's group as the local-largest group if their own ethnicity is the largest group in their birth district. Figure 7 shows the ethnic concentration of districts. The second uses language directly: I map each person's ethnicity to a language and each birth area to its likely language of instruction, the largest language of instruction in the area, and classify each person by relatedness tier: own language is the area's language of instruction; same Ethnologue subgroup; same branch; different branch. Tiers are ordinal relatedness categories, not a distance measure. The language of instruction is inferred, not observed, because no national list of area languages exists for 2007; ethnicity is not always language, and birthplace is not always school location (Appendix C).

Figure 7: Ethnic Concentration of Birth Districts, 2002



Notes: Herfindahl index of ethnic shares, 2002 Uganda Census (IPUMS), 36 harmonized districts: the probability that two residents of a district share an ethnicity.

9.2 Heterogeneous January jumps

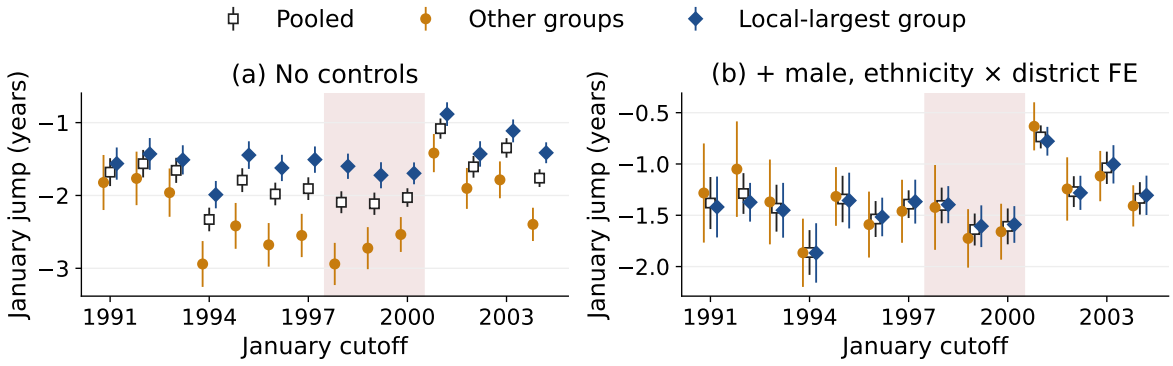
I estimate the same RD as the main design with the January jump allowed to differ by group. For each cutoff $c \in \{1991, \ldots, 2004\}$, among individuals born within six months of January of year c,

$$Y_i = \alpha + \tau D_i + \delta\,(D_i \times G_i) + \lambda G_i + f(r_i; D_i, G_i) + \varepsilon_i, \tag{7}$$

where $G_i = 1$ if the person's ethnicity is the largest group in their 2002 birth district, r_i is birth month relative to January, f has separate linear slopes for each side and group, and weights are triangular. τ is the January jump for other groups, $\tau + \delta$ for the local-largest group, and δ the difference in jumps. Version (a) has no controls; version (b) adds a male indicator and ethnicity-by-district fixed effects, with standard errors clustered by district. As in the main design, 1991–1997 are pre-reform cutoffs.

Figure 8 and Table 7 show that everyone gains by a similar amount. Without controls, the groups differ at every cutoff, before and after 2000; with ethnicity-by-district fixed effects the two series coincide, and both rise after 2000. In schooling, other groups gain 0.32 years and the local-largest group 0.39; the difference, $\widehat{\delta}$, does not change (+0.07, standard error 0.08). For literacy and employment the gains in this sample are small and the same for both groups. The raw group gap predates the reform and disappears with fixed effects.[2]

Figure 8: Heterogeneous January Jumps in Schooling: Everyone Gains by a Similar Amount



Notes: January jump in years of schooling by cutoff for the pooled sample, other groups, and the local-largest group. Panel (a): no controls, robust standard errors. Panel (b): male and ethnicity × district fixed effects, standard errors clustered by district. Heterogeneity sample: 36 linked districts, six-month bandwidth.

[2]The pooled change in this sample (0.37) is smaller than the main estimate because the heterogeneity sample uses the 36 linked districts, a fixed six-month bandwidth and post-reform cutoffs to 2004.

Table 7: Heterogeneous January Jumps: Both Groups Gain, No Gap in Any Outcome

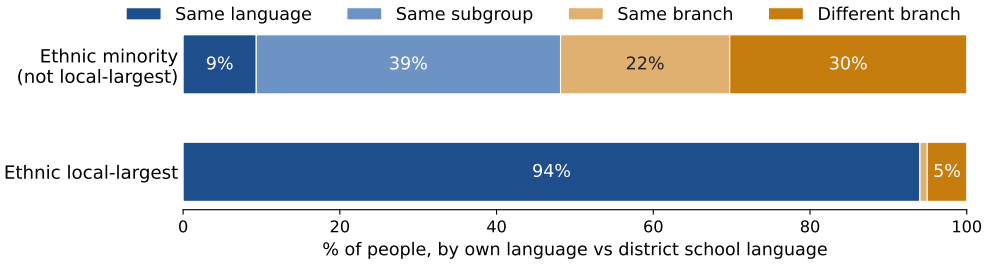
Post 2001–04 − pre 1991–97	Schooling (years)	Literacy (pp)	Employment (pp)
Pooled	+0.37* (0.15)	+1.2 (1.1)	+0.4 (0.4)
Other groups	+0.32 (0.19)	+1.4 (1.4)	+0.2 (0.6)
Local-largest group	**+0.39**** (0.14)	+1.2 (1.0)	+0.4 (0.8)
$\widehat{\delta}$ = largest − others, (b)	+0.07 (0.08)	−0.3 (0.5)	+0.2 (1.2)
(a) no controls	−0.06 (0.18)	−1.2 (1.6)	+0.1 (1.4)

Notes: Rows 1–3 and $\widehat{\delta}$ (b): male and ethnicity × district fixed effects. Heterogeneity sample (36 districts, six-month window, post-reform cutoffs to 2004), so levels differ from the main estimates. Welch standard errors across cutoffs. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; bold: $p < 0.05$.

9.3 Language match

An ethnic minority is often not a language minority. Figure 9 shows the relatedness tiers by ethnic status: many local minorities speak a language in the same subgroup or branch as their area's language of instruction, and some local majorities speak a language that is not itself taught.

Figure 9: Is an Ethnic Minority a Language Minority?

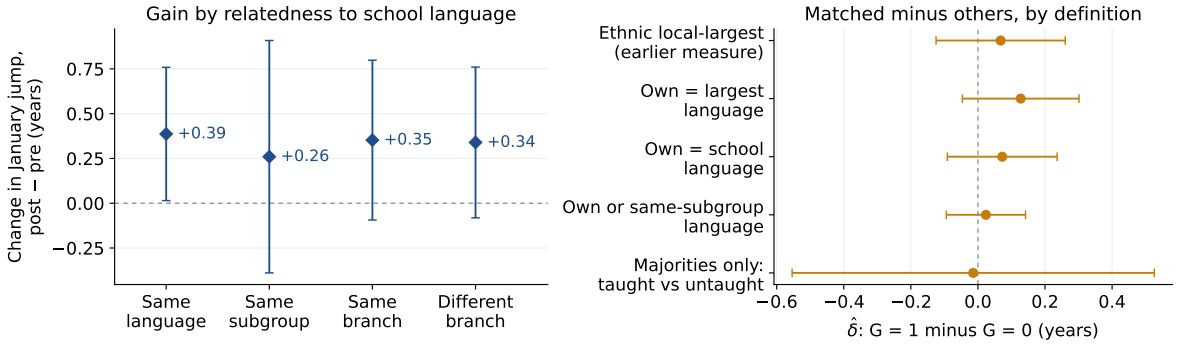


Notes: Relatedness of each person's language to the area's language of instruction, by whether their ethnicity is the local-largest group. Tier 1: same language; 2: same Ethnologue subgroup; 3: same branch; 4: different branch.

Figure 10 shows that gains did not depend on language match. The post-2000 change in the January schooling jump is +0.39, +0.26, +0.35 and +0.34 years across the four tiers, and the change per tier step is −0.02 (0.02). The difference in jumps is close to zero under every definition of match, including own language equal to the area's language of instruction (+0.07, standard error 0.07); for literacy and employment every difference is at most 0.04 in absolute value (Appendix Table A7). The same holds at the finer geography of 136 birth districts (Appendix Figure A10).

The null is informative, because language match mattered before the reform. Figure 11 shows that in cohorts born 1986–1997, all taught in English, children whose language differs from their area's language of instruction had 0.5 to 1.1 fewer years of schooling and 4 to 8 percentage points lower literacy than matched children in the same birth area. After the reform, their gain was similar

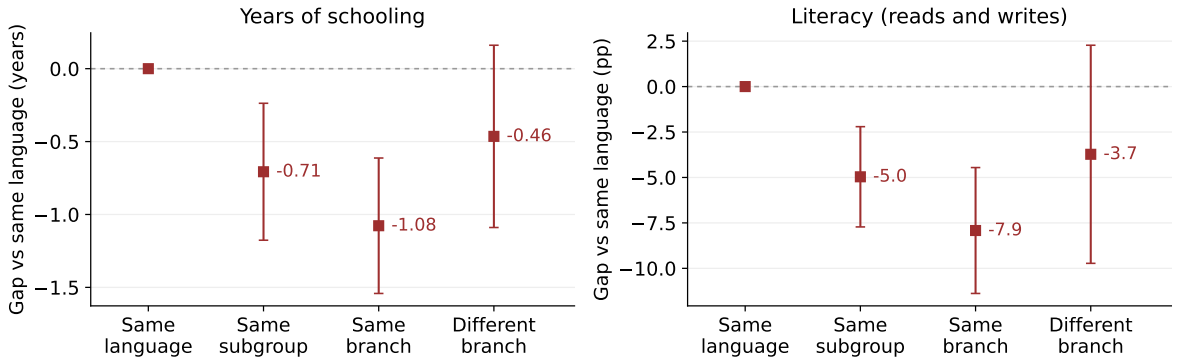
Figure 10: Gains Did Not Depend on Language Match (36 Birth Areas)



Notes: Left: change in the January schooling jump, post 2001–04 minus pre 1991–97, by relatedness tier. Right: difference in jumps between matched and other children under alternative definitions of match. Specification (b): male and ethnicity × area fixed effects, clustered by area. 95 percent Welch confidence intervals across cutoffs.

to the matched group's. Mismatched children started behind, and the reform neither widened nor closed that gap.

Figure 11: Language Match Predicted Levels, Not the Gain (36 Areas)



Notes: Gaps relative to same-language children, cohorts born 1986–97, with birth-area fixed effects and 95 percent confidence intervals. These are cross-sectional differences between groups, not effects of the reform. Not monotone in tier.

9.4 What heterogeneity tells us

Prediction P3 is not supported: the gain was broad-based. Ethnic minorities and local-largest groups gained alike, in schooling, literacy and employment. Measured directly as own language against the area's language of instruction, match makes no difference either, across relatedness tiers and language definitions, at 36 areas and at 136 districts. Local majorities whose own language is not taught (Alur, Jopadhola, Sabiny and Madi) gained as much as taught ones. Other designs agree: there is no gradient by district ethnic concentration, and urban-born children, where English

remained permitted, gained at least as much as rural-born children (Appendix C).

Four hypotheses could explain a broad-based gain. First, familiarity without mother tongue: the area's main language is often a local lingua franca that minorities hear and use daily, and it can still be more familiar than English. Second, the package, not only language: new pedagogy, themes and a literacy-and-numeracy focus reached every child. Third, possible exemptions in urban and private schools, which could keep English, yet urban-born children gained as much. Fourth, catch-up: groups starting lower have more room to gain from any expansion. The gain was not larger for closely related languages (same subgroup +0.26; different branch +0.34), which argues against a simple comprehension-distance account. Two extensions would sharpen the test: treating the 26 languages the National Curriculum Development Centre now considers feasible as possible languages of instruction, and using reported home language rather than ethnicity.

10 Welfare: A First Look and a Framework

10.1 An earnings projection

From the framework, earnings are $Y = \omega H + \pi e$, so a schooling gain ΔH raises earnings by $\omega\,\Delta H$ in every working year; $\pi\,\Delta e$ is the English channel, for which the results show no employment penalty. I value the gain as the present value, at school entry, of a permanent earnings increase of $\omega\,\Delta H$ over 40 working years starting 12 years after Primary 1 entry, discounted at 5 percent, with baseline earnings Y_0 equal to GDP per capita (US$1,073):

$$E = \omega\,\Delta H \times \sum_{t=12}^{51} 1.05^{-t} \times Y_0 = \omega\,\Delta H \times 10.03 \times Y_0. \tag{8}$$

Returns to schooling ω of 5 percent (a low value) and 9.7 percent (the world average) come from Patrinos (2016). Table 8 reports three scenarios. The implied earnings gain is 2.0 to 7.0 percent a year, with a present value of US$210 to US$760 per child.

This is a long-run projection, not an estimate. It applies a Mincer return to the schooling gain for cohorts who are 15 to 23 years old today and whose employment shows no robust gain yet. It is also gross: it values the extra years at their full return and ignores their cost, so it is an upper bound on what families would pay.

Table 8: A First Look at the Earnings Side

Scenario	ΔH (years)	Return ω	Implied earnings gain	
			per working year	present value E
Low	0.39 (net of trend)	5%	+2.0% (US$21)	US$210
Middle	0.72	5%	+3.6% (US$39)	US$390
High	0.72	9.7%	+7.0% (US$75)	US$760

Notes: US dollars at school entry. Baseline earnings Y_0 equal GDP per capita (US$1,073); 40 working years from year 12 after Primary 1 entry; discount rate 5 percent (at 3 percent, multiply by 1.66). Returns from Patrinos (2016). "Net of trend" is the post-minus-pre change net of a common linear trend (Table 6).

Table 9 places the schooling gain against Uganda's Northern Uganda Literacy Project and long-run effects of other programs. The gain here is close to Ethiopia's national reform and to PROGRESA's projected schooling effect.

Table 9: Benchmarks: Uganda's NULP and Long-Run Effects Elsewhere

Policy or program	Design	Schooling and learning	Earnings, cost, other
This paper: Uganda 2007, P1–P3	DiDisc	+0.72 years; literacy +5.3 pp	Projected +2.0–7.0%; employment ≈ 0
A. Uganda: NULP, Leblango in P1–P3 (Kerwin and Thornton 2021; Buhl-Wiggers et al. 2023)			
Full program	RCT	Reading +0.64 SD; after 8 years: Leblango +0.71, English +0.55 SD	No retention effect; modest grade progression; US$20 per pupil-year
Reduced-cost version	RCT	Reading +0.13 SD (n.s.), writing −0.33 SD; after 8 years: +0.38, +0.25 SD	US$7 per pupil-year
B. Long-run effects elsewhere			
Mother tongue, Ethiopia 1994 (Ramachandran 2017)	DiD	+0.65 years; completion +5 pp	Reading +11 pp (Argaw 2016)
Two more years of mother tongue, South Africa (Eriksson 2014)	DiD	+0.1–0.2 years; literacy +1–3 pp	Wages +1.5–2%
School construction, Indonesia (Duflo 2001)	DiD	+0.12–0.19 years per school per 1,000 children	Wages +1.5–2.7%
PROGRESA, Mexico (Schultz 2004)	RCT	+0.66 years (projected)	Return 8% (projected)

Notes: Headline estimates, each in its own units. NULP costs in 2013 US dollars.

10.2 A sufficient-statistics framework

The natural target for a welfare statement is the marginal value of public funds (Hendren and Sprung-Keyser 2020),

$$\text{MVPF} = \frac{\text{WTP}}{c + g\,\Delta H - t\,E}, \tag{9}$$

where c is the program cost per child, g the public cost per pupil-year, and $t\,E$ the tax on the extra earnings. By the envelope theorem (Chetty 2009), freely chosen extra years of schooling are worth approximately zero at the margin; what counts is better learning, and any change in English, over all years attended. A sufficient statistic for willingness to pay is the fee reduction that would have raised schooling as much as the reform did, WTP $\approx \bar{H}\,\Delta H/|\partial H/\partial p|$, where the price response $\partial H/\partial p$ can be taken from the 1997 fee abolition. Households' own valuation of the English trade-off is already reflected in ΔH. The envelope value gives a lower bound on willingness to pay, and the earnings value E an upper bound when every extra year is constrained.

The same few statistics price alternative policies (Table 10). English-only instruction, the pre-reform status quo, breaks even with the reform only if the English penalty is at least $\omega\,\Delta H$, or 2 to 7 percent of earnings. Teaching every child in their own language raises willingness to pay among mismatched children by $\widehat{\delta}$ and adds the cost of new orthographies, materials and teachers; with $\widehat{\delta} = +0.07$ years, this is worth US\$40–70 in lifetime earnings per mismatched child, at most US\$130–250 at the upper end of the confidence interval (Appendix D). Completing the calculation requires the price response, costs and returns, which is the next step of this project.

Table 10: Counterfactual Policies with the Sufficient Statistics

Counterfactual policy	What changes in the MVPF	Evidence needed
English-only (status quo)	Willingness to pay net of an English penalty $\pi\,\Delta e$; breaks even only if the penalty $\geq$ $\omega\,\Delta H$ (2–7% of earnings)	English tests; Morocco, South Africa, NULP bounds
Every child in their own language (15 $\rightarrow$ 26 languages)	Willingness to pay of the mismatched rises by $\widehat{\delta}$; cost adds orthographies, materials, teachers	$\widehat{\delta}$ by language match (this paper); cost per new language
Fuller materials and training	ΔH scaled by input intensity; higher c	NULP full versus reduced (0.64 versus 0.13 SD)
On-time entry (full exposure)	ΔH divided by the share actually exposed	Grade-for-age first stage (2002, 2014 censuses)
A fee cut of equal cost	Price equivalent: $\bar{H}\,\Delta H/\lvert\partial H/\partial p\rvert$ versus the same spending on fees	UPE 1997 price response

11 Conclusion

This paper asks whether teaching Primary 1–3 in a local language instead of English raises later human capital and labor-market outcomes, and who gains. Because every January school-entry cutoff carries a large recurring gap, I compare discontinuities across 19 cutoffs in the 2024 Uganda

Census.

The January gap does not change at the statutory transition cutoffs, but it shrinks by 0.72 years of schooling from the 2001 cohort onward, with smaller gaps in literacy and primary completion. The schooling change is robust to bandwidths, to dropping any cutoff and to documented birth dates, and halves net of a common linear trend; literacy is less robust. There is no robust employment gain, and no sign of a penalty from less early English. The gain is broad-based: similar whether or not the child's language matched the area's language of instruction, although match predicted large gaps before the reform. The timing is not explained by late entry, which would move the transition to older cohorts; it is consistent with a reform that reached classrooms with a lag and matured over time, and it is partly confounded by improvements in birth-date reporting.

Three extensions follow. On welfare, the sufficient statistics for a marginal value of public funds are the price response from the 1997 fee abolition, returns to schooling and program costs. On outcomes, the January patterns in health and household living standards are estimated, and their policy contrasts are next. On language match, the analysis can be repeated treating the 26 languages the National Curriculum Development Centre now considers feasible as possible languages of instruction. Historical school-level implementation records and the census birth-date flags would sharpen the timing of exposure.

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A Measurement and Birth-Date Diagnostics

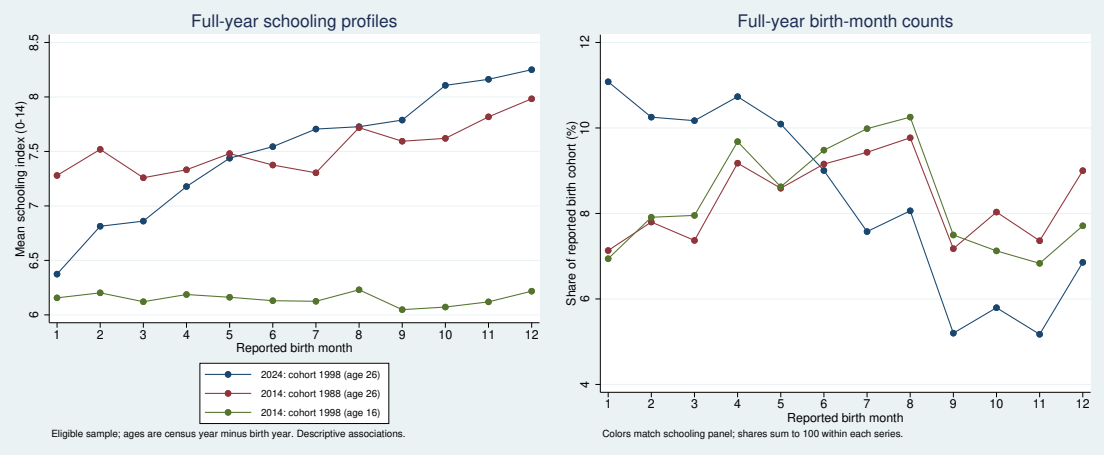
This appendix reports the literacy measure, the within-year schooling profile, the age-matched comparison of the 2014 and 2024 censuses, and the documented-date and composition checks discussed in Section 7.

Table A1: How Good Is Census Literacy?

Levels: self-report versus a reading test	
Census 2024: "can read and write"	82.1%
MIS 2024–25 (women 15–49): reads part of a sentence or more	77.2%
reads a whole sentence	55.6%
born 1990–94: part / whole	76.2 / 53.9%
born 2002–04: part / whole	80.2 / 58.0%
January gap in a reading test (DHS 2016, reads a whole sentence)	
Average jump, 1992–97 cutoffs	−0.21
Average jump, 1998–2000 cutoffs	−0.19
Jump at 2001	−0.08
Census literacy, 1991–97 average jump	−0.16

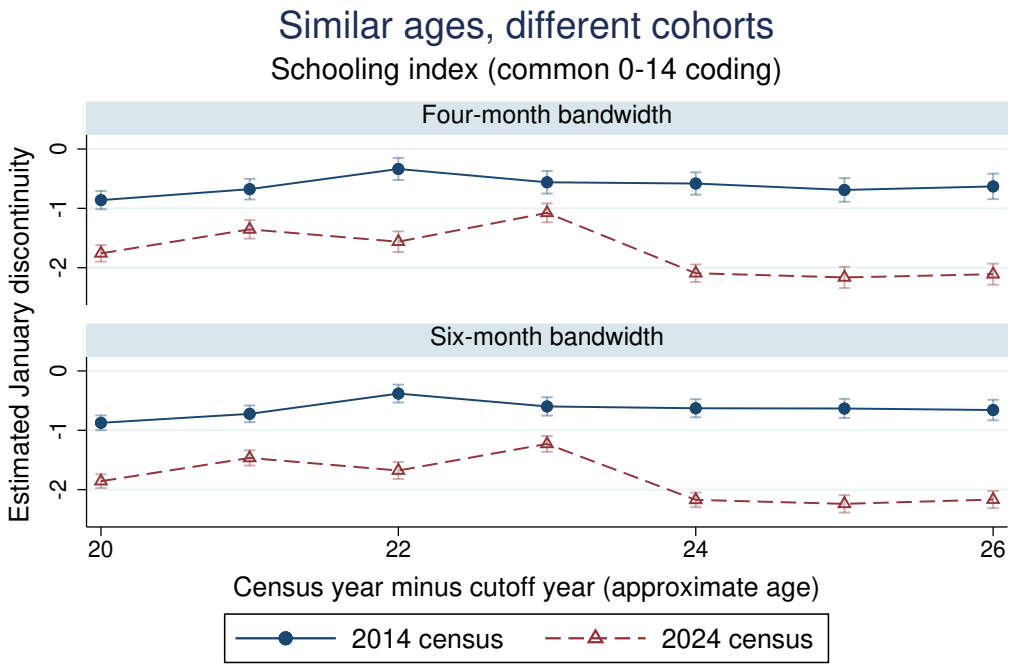
Notes: MIS: survey-weighted, 9,238 non-refugee women; card language not recorded; higher-educated women coded as readers. DHS 2016: women and men, local linear RD, MSE-optimal bandwidth. Census literacy is closer to "can read something" than to fluent reading, but a direct test shows the same recurring January gap.

Figure A1: Schooling Across the Birth Year



Notes: Mean schooling by birth month within birth years, census comparison. In the 2024 census, schooling rises across the birth year and resets at January; the fitted January discontinuity closely tracks the slope of this gradient.

Figure A2: Approximate Age-Matched January Discontinuities, 2014 and 2024 Censuses



Notes: January discontinuities in schooling for cohorts observed at approximately the same age in the 2014 and 2024 censuses, common schooling coding.

Table A2: Documented Birth Dates and Finished Outcomes

Schooling, post − pre	2001–09 cutoffs	2001–04	2005–09
All	**0.69***** (0.14)	**0.41**** (0.15)	**0.92***** (0.13)
Registered births	**0.78***** (0.16)	**0.48**** (0.17)	**1.01***** (0.13)
Day of birth known, not the 1st	**0.55***** (0.15)	0.28 (0.16)	**0.76***** (0.14)
Registered and day not the 1st	**0.65***** (0.15)	0.39* (0.17)	**0.86***** (0.13)
Finished outcome (pp), sample	**Pre-reform jump**	**2001–04**	**2005–09**
Ever attended, all	−12.1	+1.5 (1.0)	**+3.2***** (0.8)
Ever attended, registered and day not 1st	−8.5	+1.1 (1.1)	**+2.5**** (0.8)
Reached P4, all	−15.1	+1.7 (1.3)	**+3.9***** (1.1)
Reached P4, registered and day not 1st	−10.7	+1.5 (1.3)	**+3.3***** (1.0)
Reached P7, all	−20.3	+2.8 (1.6)	**+4.8***** (1.0)†
Reached P7, registered and day not 1st	−16.0	+2.3 (2.0)	**+4.2***** (1.1)†

Notes: Local linear RD at each cutoff, 12-month window, triangular kernel, robust standard errors; Welch standard errors across cutoffs. Original schooling coding. †Ages 15–19 at the 2005–09 cutoffs; some still in primary. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$; bold: $p < 0.05$.

Table A3: Composition at January, All 19 Cutoffs

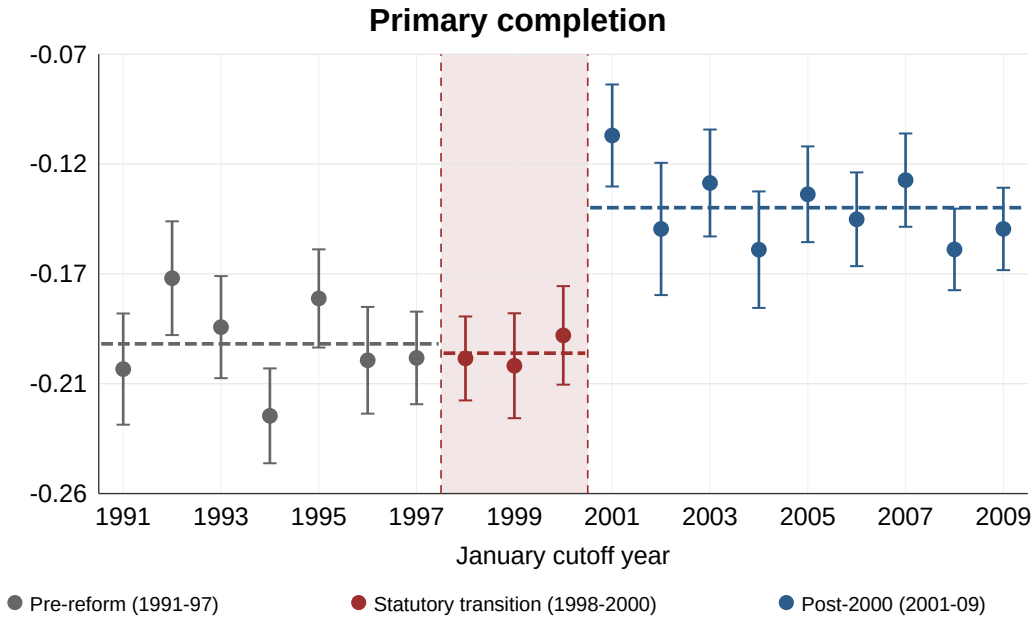
January jump in...	Pre (1991–97)	Post − Pre (2001–09)
Female (pp)	−6.7	**+1.8**** (0.6)
Birth registered (pp)	−5.3	**−2.0***** (0.5)
Mother alive (pp)	−2.9	**−0.8**** (0.4)
Father alive (pp)	−2.1	**−1.4**** (0.5)
Catholic / Muslim (pp)	−0.5 / +2.0	+0.2 (0.5) / −0.4 (0.6)
Predicted schooling, all covariates	−0.066	**−0.054***** (0.012)
Schooling	−2.14	**+0.69***** (0.14)
Schooling, covariates as controls	−2.09	**+0.81***** (0.13)

Notes: Local linear RD at each cutoff, 12-month window, triangular kernel, robust standard errors; Welch standard errors across cutoffs. Predicted schooling uses weights fit on births 1990–96. Original schooling coding. Composition at January is not stable across cutoffs, but the shift predicts lower schooling, and controls raise the estimate. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; bold: $p < 0.05$.

B Additional Results and Robustness

This appendix reports the primary-completion coefficient plot, the split of the post period, the donut and documented-date checks available on the original coding, bandwidth and donut figures, the January discontinuities back to the 1970 cohort, the replication in the Malaria Indicator Survey, and age–grade profiles in the 2002 and 2014 censuses.

Figure A3: Primary Completion: January Discontinuities by Cutoff



Notes: See Figure 3. Primary completion (reached Primary 7) is a proportion.

Table A4: Splitting the Post Period and the Dispersion of Cutoff Estimates

	Schooling	Primary completion	Literacy	Employment
Post − Pre, 2001–04 cutoffs	**0.535**** (0.153)	**0.061***** (0.013)	**0.039**** (0.012)	**0.018***** (0.004)
Post − Pre, 2005–09 cutoffs	**0.875***** (0.106)	**0.054***** (0.009)	**0.065***** (0.010)	0.014 (0.007)
SD of pre-reform $\widehat{\tau}_c$ / reported SE	1.81	1.77	2.16	0.65

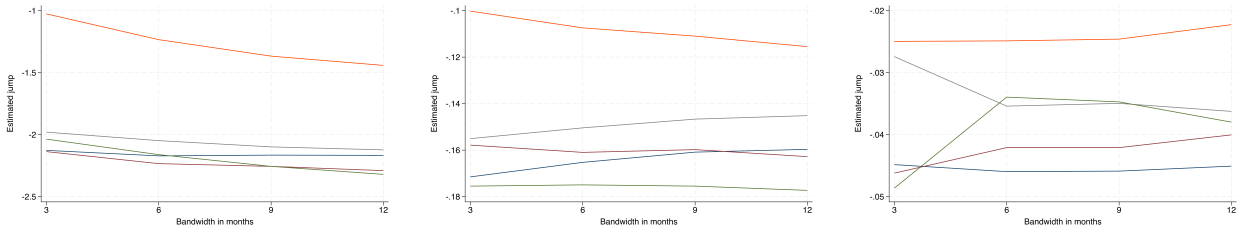
Notes: Corrected common coding, MSE-optimal bandwidth, Welch standard errors across cutoffs. Under statutory entry every post-2000 cohort is equally exposed, so the post contrast should be flat across post cutoffs. The last row compares the spread of pre-reform cutoff estimates with the standard errors reported by the cutoff regressions. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; bold: $p < 0.05$.

Table A5: Donut and Documented-Date Checks (Original Coding)

Post − Pre	Schooling	Literacy	Employment
Main, MSE-optimal bandwidth (original coding)	**0.715***** (0.120)	**0.055***** (0.011)	**0.016***** (0.005)
Donut, drop December and January, bandwidth 6	**0.527***** (0.156)	0.013 (0.013)	−0.010 (0.009)
Donut, drop December and January, bandwidth 12	**0.647***** (0.163)	0.012 (0.011)	**−0.014**** (0.007)
Registered births, day of birth not the 1st	**0.65***** (0.15)		

Notes: Estimated on the original schooling coding (Section 4). Welch standard errors across cutoffs. Documented dates: 12-month window (Appendix Table A2). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; bold: $p < 0.05$.

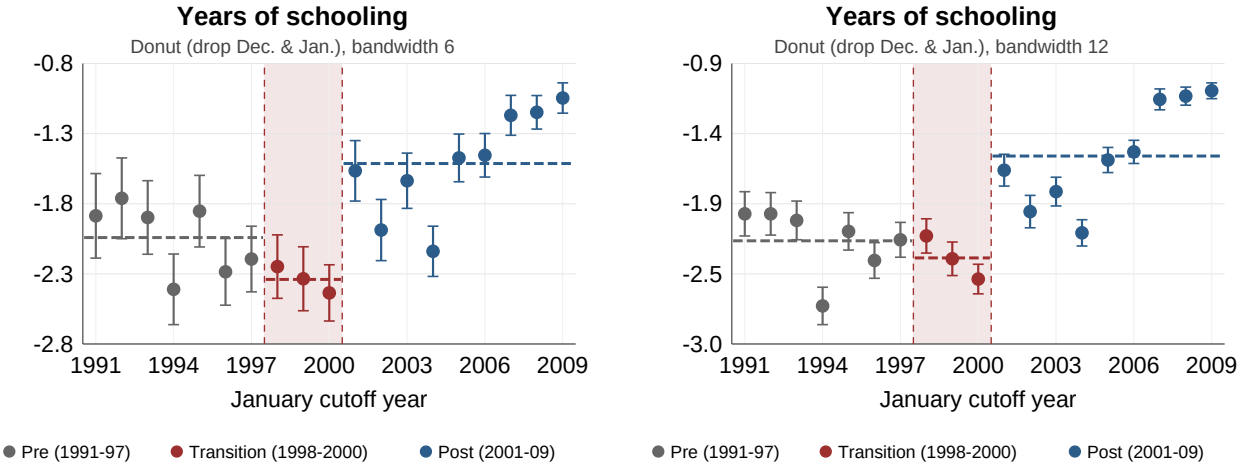
Figure A4: Sensitivity to Bandwidth (3–12 Months)



(a) Years of schooling (b) Literacy (c) Employment

Notes: Cutoff-specific RD estimates over bandwidths of 3 to 12 months, original coding.

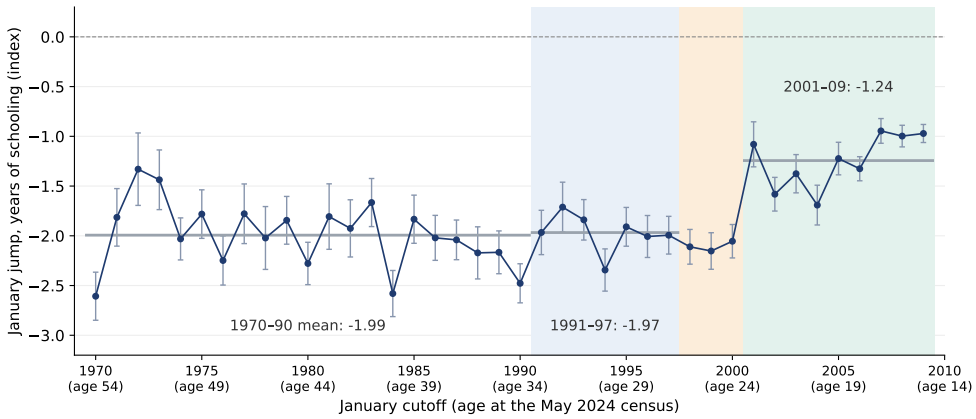
Figure A5: Donut RD, Years of Schooling (Drop December and January)



(a) Bandwidth 6 months (b) Bandwidth 12 months

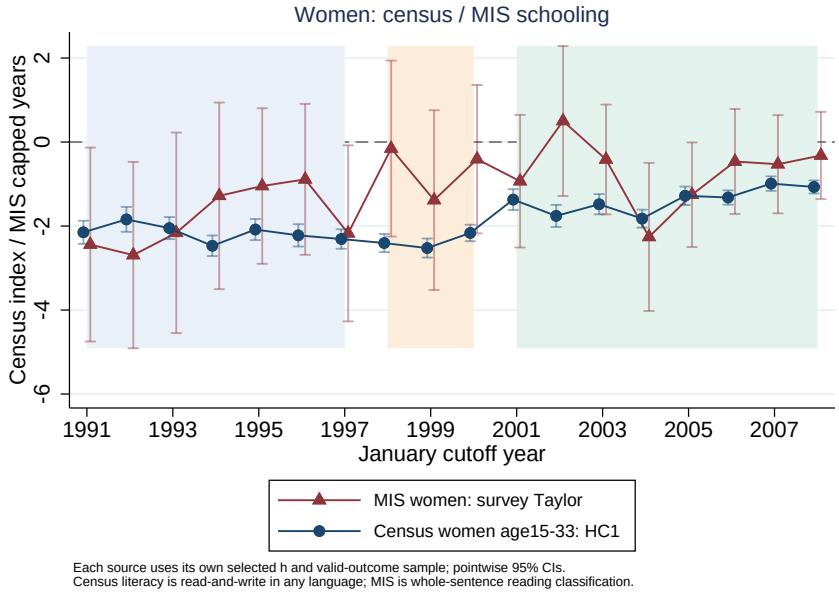
Notes: January-cutoff estimates after removing the December and January birth cells, original coding. The donut removes the two cells nearest the threshold and extrapolates across the gap; it is a sensitivity check, not a correction for full-year reporting patterns.

Figure A6: January Discontinuities Back to the 1970 Cohort



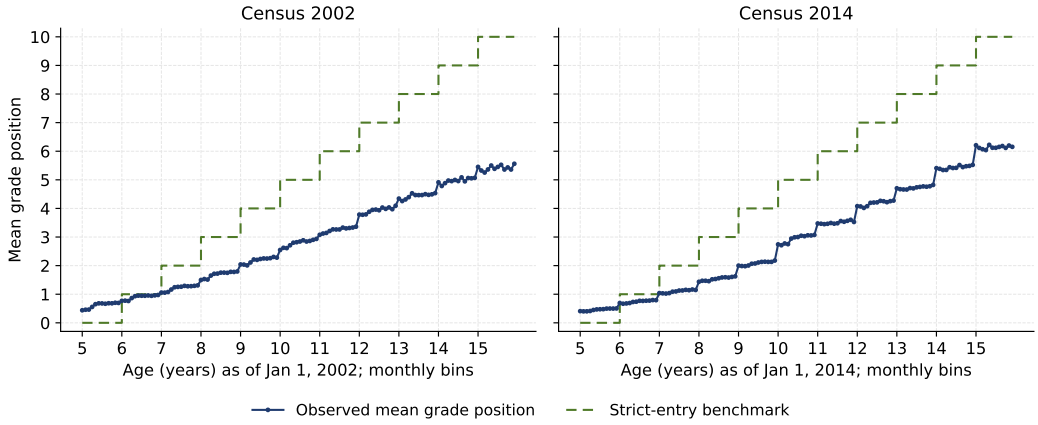
Notes: January-cutoff estimates for years of schooling in the 2024 census, cutoffs 1970–2009.

Figure A7: A Second Survey: MIS 2024 Women and the Census



Notes: January-cutoff estimates of years of schooling, 2024 census women and 2024–25 Malaria Indicator Survey women. Post − pre: census women 0.81 (0.12); MIS 1.02 (0.49), with a joint test of both shifts $p = 0.12$; DHS 2016 women, one post cutoff, 1.14 (0.41). MIS and DHS: completed years capped at 14, all cutoffs in one regression at a common bandwidth, survey standard errors.

Figure A8: Age–Grade Profiles in the 2002 and 2014 Censuses



Notes: Mean grade by age among children in the 2002 and 2014 censuses, against the statutory benchmark of entry at age six and one grade per year.

C Heterogeneity: Additional Results

C.1 Linking birthplace and constructing language match

I link 2024 birthplace to 36 harmonized districts of the 2002 census. Of household records born 1985–2004, 87.8 percent name a district in the IPUMS harmonized label, 6.6 percent name a newer district linked through its documented parent district, 2.4 percent are in ten cities excluded pending a boundary check, 0.3 percent have unknown birthplace and 2.9 percent were born abroad, for a mapped sample of 94.4 percent. Every district has all 20 cohorts.

Each person's own language comes from their ethnicity (`HH_P10`) mapped to an Ethnologue language; 97.5 percent of persons are mapped with high confidence, and in the Malaria Indicator Survey women report the mapped language 89–96 percent of the time for large groups. The area's language of instruction is the largest language of instruction in the birth area (2002 shares) or, at 136 districts, the birth district (2024 adults born 1940–84). In 4 of 36 areas and 23 of 136 districts, the language of instruction differs from the largest language. Tiers are ordinal relatedness categories from the Ethnologue classification. The language of instruction is inferred rather than observed: the 2007 curriculum names no languages or districts (National Curriculum Development Centre 2008), and the School Health and Reading Program later worked in 12 languages by district (RTI International 2014).

Table A6: Heterogeneous Jumps in Schooling: Pre and Post Levels

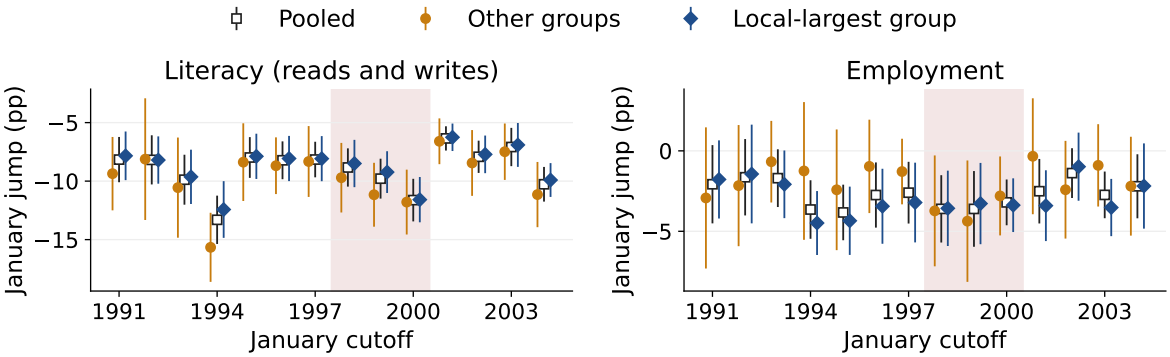
January jump (years), mean across cutoffs, spec (b)	Pre 1991–97	Post 2001–04	Post − Pre
Pooled	−1.46	−1.09	+0.37* (0.15)
Other groups	−1.42	−1.10	+0.32 (0.19)
Local-largest group	−1.48	−1.09	**+0.39**** (0.14)
$\widehat{\delta}$ = local-largest − others: (b) with FE	−0.06	0.01	+0.07 (0.08)
(a) no controls	0.72	0.67	−0.06 (0.18)

Notes: Mean January jump across cutoffs, specification (b) unless noted. Welch standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; bold: $p < 0.05$.

C.2 District intensity, cohort comparisons, and urban birthplaces

Two cohort designs complement the RD. The first is a continuous-treatment difference-in-differences with district ethnic concentration: $Y_{idt} = \alpha_d + \lambda_t + \sum_b \beta_b(\mathbf{1}\{t \in b\} \times \text{HHI}_d) + \gamma\,\text{Male}_i + \varepsilon_{idt}$, with birth-year bins 1990–94, 1995–99 and 2000–04 relative to 1985–89. If match mattered, $\beta_{2000\text{–}04} > 0$;

Figure A9: Heterogeneous January Jumps, Literacy and Employment



Notes: As Figure 8, specification (b).

Table A7: Difference in Jumps Under Alternative Definitions of Language Match

$\widehat{\delta}$ (b), post 2001–04 − pre 1991–97	Years of schooling	Reads and writes	Employed
Ethnic local-largest, 2002 (earlier analysis)	+0.07 (0.08)	−0.003 (0.005)	+0.002 (0.012)
Own language = largest language in district	+0.13 (0.08)	+0.003 (0.005)	+0.004 (0.010)
Own language = district school language	+0.07 (0.07)	−0.003 (0.005)	+0.008 (0.013)
Own or same-subgroup language	+0.02 (0.05)	−0.014 (0.009)	−0.005 (0.022)
Own language is an language of instruction (Ethnologue)	−0.01 (0.08)	−0.018 (0.011)	+0.012 (0.022)
Local-largest only: own language taught	−0.01 (0.22)	−0.028 (0.024)	+0.037 (0.022)

Notes: Specification (b), 36 areas; post 2001–04 minus pre 1991–97. Welch standard errors across cutoffs.

Table A8: Change in the January Jump by Relatedness Tier

Post 2001–04 − pre 1991–97, spec (b)	Years of schooling	Reads and writes	Employed
Tier 1: own = school language	+**0.39**** (0.14)	+0.011 (0.010)	+0.006 (0.008)
Tier 2: same subgroup	+0.26 (0.25)	+0.000 (0.020)	−0.017 (0.014)
Tier 3: same branch	+0.35 (0.18)	+0.012 (0.013)	+0.011 (0.020)
Tier 4: different branch	+0.34* (0.15)	+0.030 (0.015)	+0.007 (0.019)
Change per tier step (linear)	−0.02 (0.02)	+0.004 (0.003)	−0.000 (0.007)

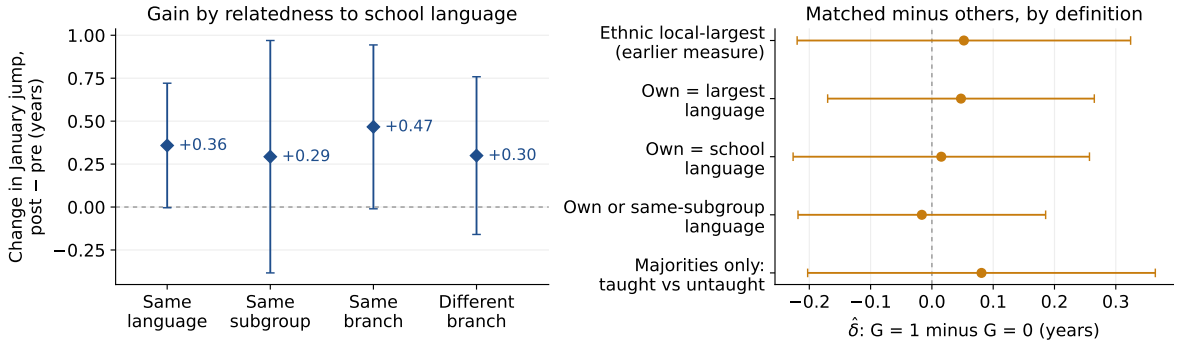
Notes: Specification (b), 36 areas. Welch standard errors across cutoffs. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; bold: $p < 0.05$.

Table A9: Pre-Reform Gaps by Language Match (Born 1986–97)

Born 1986–97, gap vs tier 1	Years of schooling		Reads and writes		Employed	
Fixed effects	Cohort	+ District	Cohort	+ District	Cohort	+ District
Tier 2: same subgroup	−0.31 (0.37)	−0.71*** (0.23)	−0.016 (0.027)	−0.050*** (0.014)	+0.026** (0.010)	−0.003 (0.006)
Tier 3: same branch	−0.89*** (0.31)	−1.08*** (0.23)	−0.066** (0.028)	−0.079*** (0.017)	−0.015 (0.011)	−0.008 (0.007)
Tier 4: different branch	−0.88** (0.35)	−0.46 (0.31)	−0.089*** (0.030)	−0.037 (0.030)	−0.049*** (0.013)	−0.015 (0.009)
Per tier step (linear)	−0.32*** (0.10)	−0.29*** (0.11)	−0.030*** (0.008)	−0.022** (0.009)	−0.013*** (0.004)	−0.005* (0.003)
Tier 1 mean	7.37		0.821		0.640	

Notes: Gaps relative to tier 1 (own language is the area's language of instruction), with cohort and birth-area fixed effects; standard errors clustered by area. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

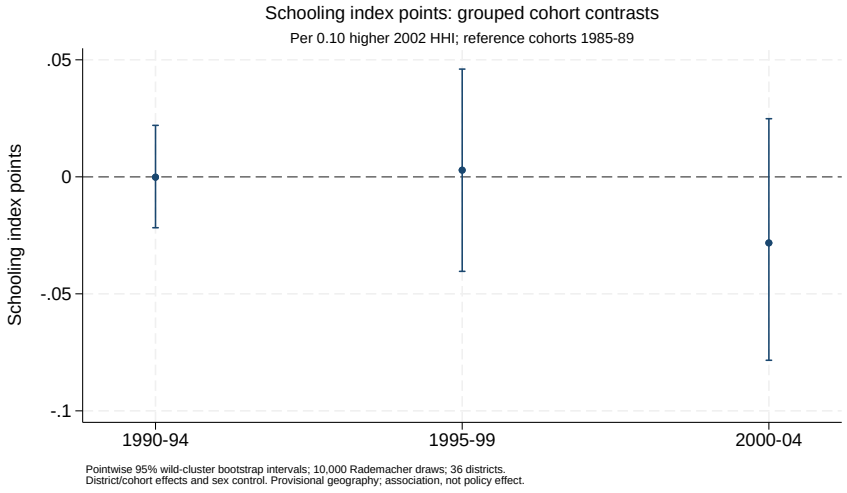
Figure A10: Gains and Language Match, 136 Birth Districts



Notes: As Figure 10, with 136 birth districts and the language of instruction defined from 2024 adults born 1940–84.

Figure A11 shows no gradient. The second is a triple difference comparing, for the same ethnicity, the change between cohorts born 2002–04 and 1990–94 where the group is the local majority and where it is a minority, net of each district's change: $\widehat{\delta} = -0.106$ (0.045) with ethnicity-by-district, ethnicity-by-cohort and district-by-cohort fixed effects (N = 519,908; wild district bootstrap p = 0.041). This design compares cohort levels observed at different ages, and its magnitude is sensitive to the choice of cohort windows; it points, if anything, toward catch-up by lower-baseline groups rather than larger gains for matched children.

Figure A11: District Ethnic Concentration and Schooling Across Cohorts

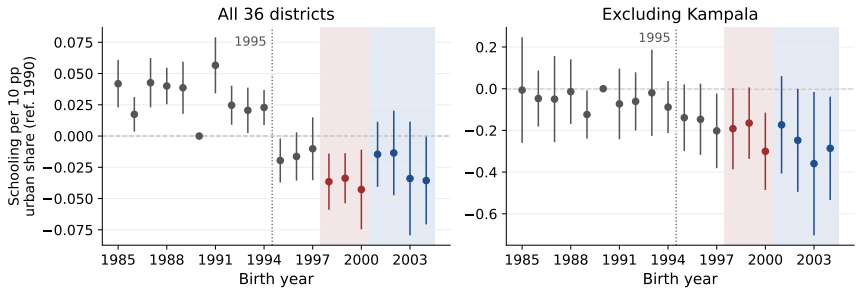


Notes: Coefficients β_b per 0.10 higher district HHI, relative to cohorts born 1985–89; district and birth-year fixed effects; standard errors clustered by district (36).

Urban birth districts, where English remained permitted, provide a check on exemptions. With specification (b), the post-minus-pre change in the January jump is +0.33 (0.16) for rural-born

children and +0.68 (0.24) for urban-born children (Kampala, Jinja and Gulu); the difference is +0.35 (0.25). Figure A12 shows that in a cohort comparison, the urban–rural gap opens before exposure, consistent with pre-existing convergence rather than the language switch.

Figure A12: Urban Share of Birth District and Schooling Across Cohorts



Notes: Cohort profile of schooling per 10 percentage points of 2002 urban share of the birth district; district and birth-year fixed effects, clustered by district. Urban share: median 7 percent; Kampala 100, Gulu 25, Jinja 22 percent.

D Welfare: Worked Example

Teaching every child in their own language would affect the 32 percent of children whose language differs from their area's language of instruction (tiers 2–4). Their gain would be the matched-minus-mismatched difference in the reform-era gain, $\widehat{\delta} = +0.07$ years (95 percent confidence interval up to +0.24). Valued with Equation (8) and ω between 5 and 9.7 percent, this is US$40–70 in lifetime earnings per mismatched child, at most US$130–250; averaged over all children it is +0.02 years (at most +0.08), against a reform-era gain of +0.37 in the same sample. Adding languages therefore pays in earnings terms only if it costs less than about US$40–70 per mismatched child (at most US$250). The National Curriculum Development Centre now proposes 26 languages, up from 15; the gains from additional languages are likely modest relative to the cost of new orthographies, materials and teachers. The calculation ignores peer and classroom spillovers.