

Violent Conflict and Family Investment in Education

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Abstract: When does violent conflict intensify rather than suppress investment in children's education? We argue that under conflicts characterized by selective violence and partial preservation of civilian infrastructure, families respond to uncertainty by reallocating resources toward portable forms of capital—most notably, education. Using retrospective data from Spain and a regression discontinuity design, we compare cohorts who reached school age during the Civil War (1936–1939) with adjacent birth cohorts. The baseline regression discontinuity design provides little robust evidence of an overall total effect of war exposure. Exploratory conditional specifications that adjust for educational supply show a +14 percentage point discontinuity in primary school completion and a +6 percentage point discontinuity in lower secondary completion. Because educational supply may itself have been affected by the war, these conditional estimates are consistent with a positive demand-side response that may have been offset in the total effect by supply-side constraints. At the primary level, gains were largest among children from less educated households, suggesting partial equalization of educational opportunity. These findings propose a conditional theory of how violent disruption reshapes family investment strategies and, through them, patterns of educational inequality.

Keywords: family investment strategies; violent conflict; educational attainment; human capital; social inequality; Spanish Civil War

Reproducibility Package: All documentation and code necessary to reproduce the empirical results presented in this manuscript is publicly available at <https://digital.csic.es/handle/10261/434731>. The replication materials provide access to the merged dataset used in the analyses, which combines processed microdata from the INE Sociodemographic Survey (1991) with provincial educational supply variables and economic indicators. The merged dataset is publicly available at <https://doi.org/10.20350/digitalCSIC/20220>. The historical educational supply data are archived separately at <https://doi.org/10.20350/digitalCSIC/18749> and should be cited when these data are used. All replication materials and datasets are publicly accessible without restrictions.

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Violent conflict is one of the most severe disruptions families can face, yet its consequences for children's education are more varied than the dominant literature suggests. Most research documents the expected: wars destroy schools, displace families, and force parents to prioritize immediate survival over long-term investment in children's human capital (Akbulut-Yuksel 2014; Justino 2016; Kesternich et al. 2014). But a smaller body of evidence points in a different direction. Under some conflicts, school enrollment rises, educational attainment improves, and families appear to intensify rather than withdraw investment in children's schooling (Arcand and Wouabe 2009; de Groot and Göksel 2011; Valente 2014).

Why? Existing research has not provided a satisfying answer. Studies suggesting positive educational effects tend to treat them as context-specific anomalies rather than outcomes generated by identifiable mechanisms. The conditions under which violent conflict might accelerate rather than suppress human capital formation remain theoretically underdeveloped.

We argue that the key lies in how conflict reshapes the relative value of different forms of capital. When physical and financial assets become vulnerable to destruction, confiscation, or devaluation, education acquires distinctive value as a portable and non-confiscable resource—one that families can invest in precisely because it cannot be taken away. At the same time, research on parenting under conflict shows that moderate violence, rather than paralyzing families, can trigger more protective and future-oriented parenting strategies that place greater weight on children's long-term development (Slone and Mann 2016). Both mechanisms operate only under a specific condition: the conflict must preserve sufficient continuity in civilian and educational life to allow families to act on their preferences. In highly destructive wars characterized by indiscriminate violence against civilians (Kalyvas 2006),¹ mass displacement, and institutional collapse, these adaptive responses are crowded out by the imperative of survival. In less disruptive conflicts, where violence is selective, schools remain open, and families retain meaningful agency, wartime uncertainty may intensify rather than suppress educational demand. This conditional argument generates a clear empirical prediction: the positive effect of conflict on education should be stronger, where violence is more intense, but only up to the threshold at which destruction overwhelms adaptation.

We test this prediction using evidence from the Spanish Civil War (1936–1939; henceforth SCW), a conflict that combined high levels of selective political violence with comparatively limited destruction of civilian and educational infrastructure. Violence during the war was highly selective, directed primarily toward political and military targets rather than indiscriminately against civilians (Ledesma and Leira-Castiñeira 2025). Although the conflict generated substantial mortality, children were not systematically targeted, and mortality patterns displayed pronounced gender selectivity, with deaths disproportionately affecting men, whereas adult women experienced no significant increase in mortality (Ortega and Silvestre 2006). This gender-based disparity underscores the targeted character of violence. Importantly, educational institutions continued operating across large parts of the country throughout the war (Sierra Blas, 2009). These characteristics (selective violence, preserved civil institutions, and limited direct threats to children) make the SCW a particularly useful case for testing our theoretical framework (see Supplemental Annex for historical background).

Using retrospective data from the 1991 Spanish Socio-Demographic Survey and a regression discontinuity design, we compare cohorts who reached school age during the war with adjacent birth cohorts. The baseline, uncontrolled discontinuity offers little robust evidence of an overall total effect of war exposure at either educational transition. Once we account for the contemporaneous contraction in educational supply, however, we find a significant positive discontinuity in the probability of completing both primary and lower secondary education. At the exploratory level, this is consistent with a family-level demand-side response that was largely offset by wartime disruptions to schooling provision.

The potential associations between education and war exposure also differed across social backgrounds. For primary education, gains were strongest among children from less educated households, indicating that wartime adaptation partially equalized access to basic schooling. For lower secondary education, positive correlations emerged across social backgrounds, although estimated gains were larger among more advantaged families. This pattern suggests that wartime adaptation intensified educational demand broadly, while pre-existing social resources continued to shape families' capacity to sustain educational progression at higher educational transitions.

Literature Review

The dominant finding in the literature on war and education is that violent conflict depresses educational attainment. Research across diverse settings consistently shows that armed conflicts disrupt schooling through displacement, institutional collapse, infrastructure destruction, and heightened household insecurity (Akbulut-Yuksel 2014; Justino 2016; Kesternich et al. 2014). Children exposed to war often experience interruptions in school attendance, lower educational quality, and long-term reductions in human capital accumulation. These negative effects have been documented across a wide range of conflicts, including the Rwandan genocide (Akresh and de Walque 2008), civil wars in Sub-Saharan Africa (Unfried and Kis-Katos 2023), the Cambodian conflict (Merrouche 2011), violence in Colombia (Duque 2024), and the Balkan wars (Swee 2015). Although the intensity and mechanisms vary across contexts, the prevailing conclusion is that violent conflict undermines educational trajectories and produces durable inequalities in educational attainment.

Existing research identifies both supply- and demand-side mechanisms linking conflict to educational decline (Justino 2016). On the supply side, wars destroy schools, transportation systems, and educational administration, reducing both access to and the quality of schooling. Educational systems are further weakened by the displacement or loss of teachers, shortages of educational materials, and broader institutional collapse. On the demand side, conflict increases poverty, insecurity, forced migration, and health shocks, leading families to prioritize immediate survival over long-term investments in children's education (Bertoni et al. 2019; Muthannam, Almahfali, and Haider 2022). In highly violent contexts, families may withdraw children from school because of economic necessity, fears regarding physical safety, or the direct disruption of everyday life. Research on parenting under conflict additionally suggests that extreme violence and chronic insecurity can profoundly alter family functioning and children's developmental environments (Slone and Mann 2016).

At the same time, a smaller but growing body of scholarship suggests that the relationship between conflict and education is more heterogeneous than the dominant literature implies. Some studies report positive educational effects under specific conflict conditions. Arcand and Wouabe (2009), for example, document increased school enrollment during the Angolan Civil War, whereas Valente (2014) shows that Nepal's Maoist insurgency expanded educational opportunities for girls. de Groot and Göksel (2011) similarly argue that conflict may increase educational

demand when education becomes valuable as a portable asset facilitating migration and adaptation under uncertainty. Beyond household-level mechanisms, political economy research has also shown that states sometimes respond to conflict by expanding educational systems as part of broader projects of state-building and political consolidation (Aghion et al. 2019; Paglayan 2022).

However, current explanations for these positive effects remain fragmented and theoretically underdeveloped. Most studies treat positive educational outcomes during conflict as context-specific anomalies rather than as outcomes generated by identifiable social mechanisms. In particular, limited attention has been paid to how families adapt strategically to wartime uncertainty under conflicts that preserve sufficient continuity in civilian life. Prior research rarely distinguishes systematically between conflicts characterized by indiscriminate violence and institutional collapse and those in which violence remains more selective and educational infrastructure continues partially functioning. Yet this distinction may be crucial for understanding variation in educational outcomes across conflicts.

We argue that under less disruptive forms of violence, where families retain sufficient agency to reorganize long-term investment strategies, they may intensify investments in children's education as part of broader strategies of protection, adaptation, and asset preservation. Under these conditions, wartime uncertainty may strengthen rather than weaken educational demand. This framework helps explain why some conflicts generate severe educational collapse, whereas others may partially accelerate investments in human capital.

Theoretical Framework

Our central theoretical claim is that violent conflict can alter the relative attractiveness of different forms of capital investment. Families hold portfolios of assets—physical property, financial savings, social networks, and human capital—whose relative value shifts under different institutional conditions. When physical and financial assets become vulnerable to confiscation, destruction, or devaluation, education acquires distinctive value as a portable and non-confiscable resource. Unlike land, property, or savings, human capital cannot be seized by an occupying force or destroyed by bombardment. Under conditions of asset insecurity, families that previously underinvested in education relative to physical capital may rationally reallocate resources toward schooling, not because their preferences have changed but because the relative returns to different asset classes have shifted.

A complementary mechanism operates through shifts in parenting orientations. Psychological research on families in conflict zones shows that parents exposed to moderate violence, as distinct from extreme, survival-threatening violence, tend to adopt more protective and future-oriented parenting strategies, placing greater emphasis on children's long-term development and educational trajectories (Akesson and Sousa 2020; Eltanamly et al. 2021; Slone and Mann 2016; Slone and Shoshani 2017; Sriskandarajah, Neuner, and Catani 2015). Such adaptive parenting responses can serve as crucial compensators for traumatic disorders (Slone and Mann 2016). Rather than withdrawing from investment in children's futures, these parents intensify it as a form of protective response to uncertainty. This mechanism reinforces the asset reallocation logic. Through different pathways, both mechanisms (shifts

in parenting preferences and recalibration of investment strategies) lead families to prioritize their children's education more heavily.

Scope conditions: Both mechanisms produce measurable educational improvements only under a specific set of conditions. First, violence must remain sufficiently selective to preserve families' capacity for strategic action. When indiscriminate violence directly threatens children's survival, immediate safety crowds out long-term investment. Second, educational and civilian infrastructure must remain sufficiently functional to allow families to act on their preferences: schools must be open, teachers must be present, and basic continuity of civilian life must be maintained. These conditions define what we term less disruptive conflicts. Third, the educational transition in question must not yet be universalized: these mechanisms operate where there is room for educational expansion, not where enrollment is already near-universal.

More systematically, we propose that two simultaneous and educationally relevant forces work in opposing directions during wars. On the one hand, violence frequently destroys infrastructure and disrupts civilian life, thereby constraining families' ability to invest in education. On the other hand, exposure to violence can prompt shifts in family preferences toward prioritizing children's education more heavily. Figure 1 visually shows these dynamics regarding violence's impact on education. Panel A depicts the positive relationship between violence intensity and the shift in parenting strategies favoring human capital investment. We argue that the intensity of violence increases families' protective parenting and inclination to invest in human capital, but only up to a threshold—once the preferred level of educational investment reaches a sufficiently high point, further increases in violence intensity do not generate additional preference changes. Panel B illustrates the negative association between violence and the preservation of infrastructure and civilian life. Consistent with economic research linking violence intensity to Gross Domestic Product (GDP) per capita growth (Mueller 2013; Mueller and Tobias 2016), we contend that below a certain intensity threshold, violence may exert a limited impact on civil life and infrastructure, thereby allowing for the preservation of educational resources and the continued provision of schooling. However, once the intensity of violence surpasses this threshold, conflicts become highly disruptive, severely damaging infrastructure and profoundly disrupting daily life.

Our argument suggests that depending on the intensity and nature of violence, conflicts can produce either positive or negative effects on children's education and other aspects of their well-being (Authors, 2025). In less disruptive wars (see Fig. 2), where violence is relatively moderate and does not result in wholesale destruction of daily life or educational infrastructure, families retain meaningful flexibility to shift toward more protective childrearing approaches and reallocate investments toward human capital. Consequently, we expect a net positive impact on education. However, when violence escalates to more extreme levels—directly threatening children's survival, destroying much of the infrastructure, severely disrupting civic life, and significantly hindering the provision of education—families face substantial constraints on their capacity to adjust their nurturing practices and investments in accordance with their preferences. Under these extreme war conditions, parents generally focus on immediate safety and, above all, keeping their children alive (El-Khani et al. 2016). The widespread destruction of civil and



Figure 1: How violence affects education: two competing forces.

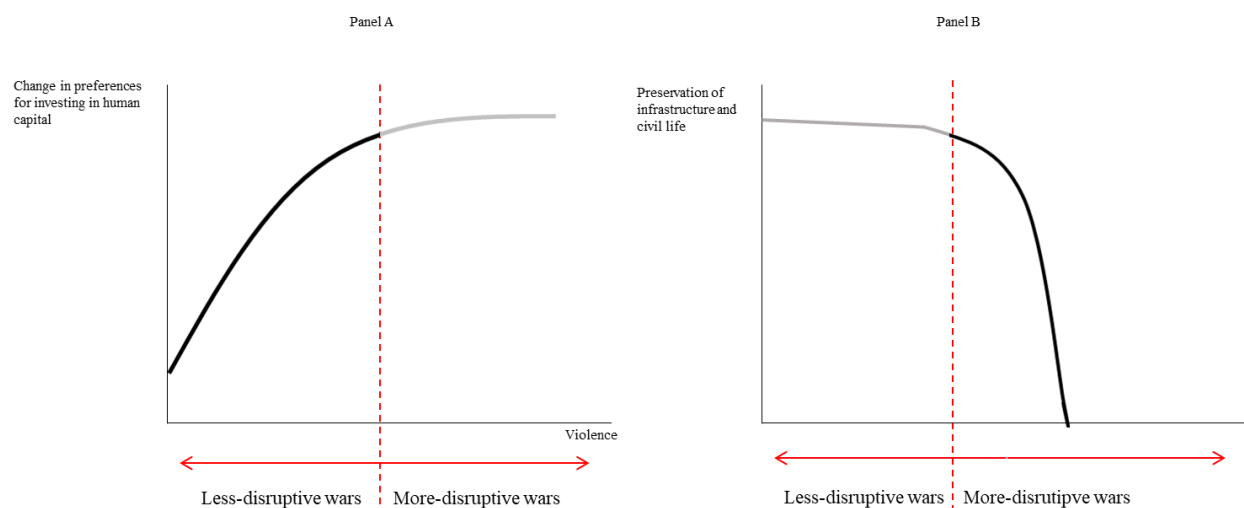


Figure 2: Two clusters of wars.

educational infrastructure severely disrupts the provision of educational services, preventing regular school attendance. As a result, we expect negative effects to prevail, leading to profoundly detrimental impacts on education.

Hypotheses

Our article focuses on less disruptive wars and contends that children exposed to such conflict are likely to experience positive shifts in their educational trajectories. The outcome variables in our study are the attainment of primary and lower secondary education. We expect that children who reach the typical age for starting these educational levels during the war will have a higher probability of completing them compared to children from earlier generations. Children from earlier cohorts, though still relatively young during the conflict, are expected to be less influenced by war-induced preference shifts, as their families likely made critical educational decisions prior to the war's onset.

An important scope condition is that these expectations apply primarily in contexts where educational expansion at the relevant level has not yet been fully achieved. For primary education, we hypothesize

H1: Children who reach the age of starting primary school (six years old) during a less disruptive conflict are more likely to complete primary education compared to those from earlier generations.

For lower secondary education, the children potentially affected are those who have completed primary education and reach the appropriate age to start lower secondary during the war. These children are at a critical transition point and are therefore most susceptible to war-induced changes in parental investment. Hence

H2: Children who are of the age to start lower secondary during the war (12 years old) and have already completed primary education have a higher probability of completing lower secondary education compared to children from previous generations.

The third prediction is theoretically more distinctive, as it allows us to discriminate between the proposed mechanism and alternative explanations. If the effect operates through family responses to asset insecurity and violence-induced protective parenting, it should be stronger in areas where conflict was more intense—because higher violence more powerfully activates both mechanisms. Alternative explanations based on reduced child labor demand or supply-side reorganization of schooling do not generate this prediction:

H3: The positive effect of war exposure on educational attainment should be monotonically stronger in provinces with higher conflict intensity.

The notion that war can constitute a transformative event implies that the changes it induces are likely to persist over time. Once parents reorient their investment priorities toward their children's education or adopt more protective and future-oriented parenting approaches, these changes will endure beyond the conflict period. Accordingly, we focus on outcome variables—completion of primary and lower secondary education—that reflect sustained parental investments rather than temporary wartime adjustments.

Methods

To examine the educational consequences of war exposure, we employ a regression discontinuity design (RDD) based on birth cohorts reaching school age during the SCW. The onset of the conflict in July 1936 created a sharp historical discontinuity that allows the comparison between adjacent cohorts exposed and unexposed to wartime conditions at critical educational transitions. For primary education, we focus on cohorts born between 1930 and 1933, who reached the typical school-entry age of six during the war years. For lower secondary education, we analyze cohorts born between 1924 and 1927, who reached the transition into lower secondary schooling—conditional on having completed primary—during the conflict.

The RDD framework estimates whether educational attainment changed discontinuously for cohorts exposed to wartime uncertainty relative to immediately preceding cohorts, while accounting for underlying cohort trends on either side of the cutoff. This approach isolates localized variation around the onset of the war rather than relying on average differences across broader populations.

In the general specification of an RDD model, we observe a continuous running variable Z —in this case, birth cohorts—that determines the assignment to a treatment or condition at a given threshold, $Z = z_0$. The outcome variable Y is assumed to follow a linear functional form on either side of the cutoff. The specification of our RDD models can be expressed as follows:

$$Y_i = \alpha + \tau D_i + f(Z_i) + \beta X_i + \varepsilon_i, \quad (1)$$

where Y_i denotes the outcome for individual i ; α is the constant term; D_i represents the cutoff point, with $D_i = 1$ if $Z_i \geq z_0$ and $D_i = 0$ otherwise; τ is the treatment effect of interest (in this case, the impact of war exposure on educational attainment); $f(Z_i)$ is a function capturing the variation on either side of the selected threshold; βX_i represents a set of covariates and slopes modeling their association with the outcome variable; and ε_i is the conventional error term. Following Calonico et al. (2019), we augment the standard local polynomial estimator with covariates entering in an additive-separable, linear-in-parameters specification, using a triangular kernel and nearest-neighbor variance estimation.

Identification Strategy and Covariate Adjustment

The identifying assumption of the RDD is that potential outcomes vary smoothly at the cohort cutoff in the absence of treatment. Under this assumption, any discontinuous change in educational attainment at the threshold can be attributed to war exposure. There is no historically documented mechanism through which families could have strategically manipulated their children's birth cohort assignment relative to the war's onset, making sorting at the threshold implausible.

Our primary specification estimates an unadjusted baseline RDD model to capture the total effect of war exposure on educational attainment. We then present exploratory specifications that include province-level GDP per capita and measures of educational supply, specifically the ratio of public primary school teachers to the relevant age population at the provincial level in the year each cohort reached school-entry age. Although these specifications should not be interpreted as causal estimates, the inclusion of these covariates is both theoretically and empirically motivated. Our theoretical framework posits two simultaneously operating forces: conflict-induced shifts in family investment preferences that increase educational demand, and war-related disruptions that constrain educational supply. Presenting the unadjusted discontinuity separately from conditional specifications allows us to document whether these supply-side constraints were present.

Empirically, we find that both GDP per capita (Fig. A.1) and educational supply (Figs. A.2 and A.3) show statistically detectable differences at the cohort cutoffs, reflecting the broader historical context of the period. Educational supply—measured as the teacher-to-population ratio—declines at the primary education

cutoff, consistent with the disruption of the Republican school expansion program following the outbreak of war. This means that the war-exposed cohorts faced, if anything, a less favorable educational supply environment than immediately preceding cohorts. Because GDP per capita and educational supply were themselves affected by war exposure, these variables lie on post-treatment pathways; the conditional specifications are therefore presented as exploratory analyses examining whether the discontinuity remains after conditioning on contemporaneous provincial characteristics. That the discontinuity becomes larger despite deteriorating supply-side conditions provides suggestive evidence of family-driven demand responses.

Discrete Running Variable

The running variable in our design is birth cohort, which is discrete by nature. Because individuals share birth years, the running variable takes a limited number of distinct values within the estimation window. Following Cattaneo, Idrobo, and Titiunik (2024), we apply the mass points adjustment throughout, which corrects inference for the presence of repeated values. Standard manipulation tests such as the McCrary density test are not applicable in this context: with a discrete running variable, the test mechanically detects mass points that reflect the cohort structure of the data rather than any strategic sorting. More substantively, manipulation of birth cohort assignment relative to a war that began unexpectedly in July 1936 is historically implausible.

Bandwidth Selection

The analyses use a bandwidth of 11 birth cohorts on either side of the cutoff, which balances precision and statistical power while preserving comparability between cohorts near the threshold. This choice is validated by two complementary procedures. First, the data-driven Mean Squared Error-optimal bandwidth selector of Calonico Cattaneo, and Titiunik (2014) yields $h = 11$ for both outcomes, indicating that our manually selected bandwidth coincides with the data-driven optimum. Second, we replicate all main estimates across a range of alternative bandwidths from $h = 9$ to $h = 17$ for primary education, and from $h = 11$ to $h = 17$ for lower secondary education—the minimum viable bandwidth for the latter given the cohort structure of the data. Estimated effects are stable across all specifications, ranging from 0.13 to 0.15 for primary education and from 0.047 to 0.063 for lower secondary education.

Data

Having access to reliable and representative microlevel data obtained from sampling populations during wartime is obviously rare. For this article, we use a unique retrospective dataset registering the life course of Spaniards born throughout most of the twentieth century: the Spanish 1991 Socio-Demographic Survey (*Encuesta Sociodemográfica*), conducted by the National Institute of Statistics (INE) in 1991

(henceforth ES-91). The survey offers extensive retrospective data from a representative sample covering all Spanish regions and cohorts, with a total sample size of 157,100 individuals.² Data collection was carried out through individual, household, and specific events questionnaires. Given the focus of our research, ES-91 is particularly well suited for analyzing how Spaniards reacted to the shock of their Civil War (1936–1939). It covers individuals born across the full range of cohorts relevant to our analysis, and we address potential concerns about selective mortality directly in the *Robustness Checks* section below.

The dependent variables in the statistical analyses are binary indicators reflecting respondents' completion of specific educational levels. For primary education, the variable is coded as zero if the respondent did not complete primary school and one if they did. For lower secondary school, the coding follows a similar approach but is applied only to respondents who had completed primary education. Regarding the independent variables, the primary variable of interest is the respondent's birth year. Given our focus on primary and lower secondary education during the SCW, we specifically recode the birth year variable to suit our analytical framework. This recoding aims to identify individuals who were of the appropriate ages to begin primary and lower secondary education during the war years (1936–1939). These individuals are expected to experience a discontinuity in their educational attainment, thereby representing the birth cohorts at the cutoff points in our main specification models. The recoding is as follows:

- Primary education: Individuals born between 1930 and 1933 are targeted, as they were six years old—the typical starting age for primary education—during the war.
- Lower secondary education: Individuals born between 1924 and 1927 are identified as the birth cohorts at the cutoff point, as they were 12 years old during the war—the usual starting age for lower secondary education.

For respondents with other birth years, we create broad categories, grouping birth years in intervals of three (see Fig. 3 for the distribution of respondents by these categories of birth cohorts and by year of birth).

To examine whether war exposure produced heterogeneous effects across different family backgrounds, we re-estimate our regression models stratified by level of parental education (using paternal education when available, and maternal education otherwise). This variable is trichotomous, scoring 0 if the respondent's father did not complete primary school, 1 if he did, and 2 if lower secondary school or more was attained. As control variables, all estimations include the public supply of primary education at the province-year level when respondents were seven years old. This supply-related indicator includes publicly subsidized schools, whether operated directly by the state or by nongovernmental institutions such as the Catholic Church. Particularly, for each province and year, we calculate the ratio of the number of public primary teachers to the population aged 6–12 (for analyses on primary education) or to the total population (for analyses on lower secondary schooling). Each respondent is then assigned the value of this indicator corresponding to when they were seven years old, ensuring that the indicator reflects the educational environment at the time they would have started primary school.³ By

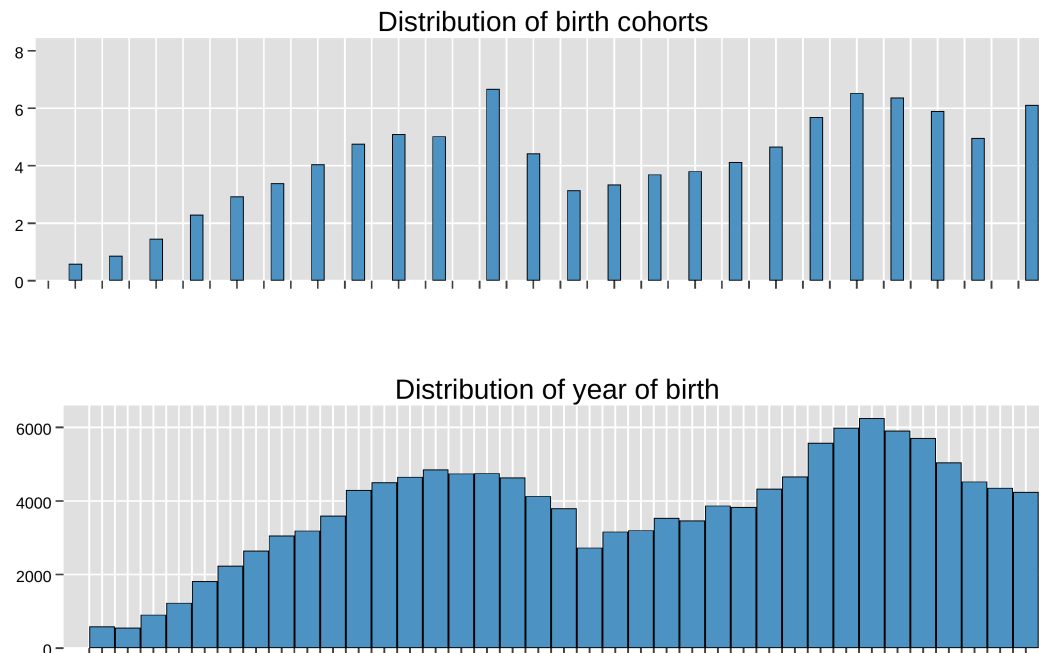


Figure 3: Description of the ES-91 sample.

Table 1: Summary of Variables (Analytical Sample).

		N	Mean	SD	Min	Max
Dependent variables	Completes primary	137,101	0.721	0.449	0	1
	Completes lower secondary	137,101	0.357	0.479	0	1
Female		137,101	0.533	0.499	0	1
Father's education	Less than primary	137,101	0.705	0.456	0	1
	Primary	137,101	0.174	0.379	0	1
	Secondary or more	137,101	0.121	0.326	0	1
Public supply of education	Teachers/pop 6–12	133,338	17.57	9.422	3.084	145.5
	Teachers/total pop	133,417	2.408	1.248	0.502	24.04
War intensity	War deaths (% of population)	137,101	0.55	0.60	0.03	3.27
GDP per capita	Thousands €(constant prices)	131,551	4.277	2.671	1.047	12.6

including measures of educational supply in our statistical models, we account for a major channel through which the state may influence educational outcomes. These variables are included only in exploratory conditional specifications intended to describe how the discontinuity changes after conditioning on contemporaneous provincial characteristics. Finally, our models also control for the respondents' sex (1: female) and real GDP per capita in constant prices (expressed in thousands of euros) at the province-cohort level.⁴ Table 1 provides descriptive information on these variables.

Table 2: Discontinuity in the Probability of Completing Primary Education.

	M1	M2	M3	M4	M5	M6
RD estimate	0.017	0.063*	0.142*	0.135*	0.115*	0.065*
(SE)	(0.013)	(0.013)	(0.013)	(0.014)	(0.030)	(0.035)
Robust <i>p</i> value	0.803	0.410	0.003	0.015	0.917	0.268
<i>N</i>	140,687	133,022	129,940	90,931	22,497	15,084
Controls	None	+GDP, sex	+Supply	All	All	All
Sample	Full	Full	Full	Father <prim.	Father prim.	Father sec.+

Note: Standard errors in parentheses. Robust *p* values reported.

* $p < 0.1$ (conventional). Models 4–6 include all controls. Bandwidth $h = 11$.

Table 3: Discontinuity in the Probability of Completing Lower Secondary Education.

	M1	M2	M3	M4	M5	M6
RD estimate	0.039*	0.057*	0.062*	0.039*	0.034	0.105*
(SE)	(0.013)	(0.013)	(0.013)	(0.012)	(0.035)	(0.057)
Robust <i>p</i> value	0.251	0.004	0.005	0.013	0.795	0.997
<i>N</i>	101,443	95,873	93,166	57,100	20,726	14,471
Controls	None	+GDP, sex	+Supply	All	All	All
Sample	Full	Full	Full	Father <prim.	Father prim.	Father sec.+

Note: Standard errors in parentheses. Robust *p* values reported.

* $p < 0.1$ (conventional). Sample restricted to respondents who completed primary education. Models 4–6 include all controls. Bandwidth $h = 11$.

Results

Main Results on Primary and Lower Secondary Education

Tables 2 and 3 present the results of our main models for primary and lower secondary education, respectively. Each table presents an unadjusted baseline RDD estimate (model 1; total effect) followed by various exploratory conditional specifications, including a model controlling for sex and GDP per capita (model 2), a fully specified model that additionally controls for educational supply (model 3), and three stratified models by paternal education level (models 4–6).

For primary education, the unadjusted discontinuity (model 1) reveals a positive but imprecisely estimated jump of 0.017 in the probability of completing primary education among cohorts born between 1930 and 1933. This uncontrolled estimate represents our best estimate of the total (unconditional) effect of war exposure and provides little robust evidence of an overall positive effect. Figure 4 shows this unconditional discontinuity. When we introduce controls for sex and GDP per capita (model 2), the estimated discontinuity increases to 0.063 and becomes statistically significant. The most critical finding, albeit exploratory, emerges under the fully specified model (model 3), which additionally controls for our measure of public educational supply. In this specification, the estimated discontinuity rises to 0.142, indicating that, net of the supply-side contraction, war-exposed cohorts show

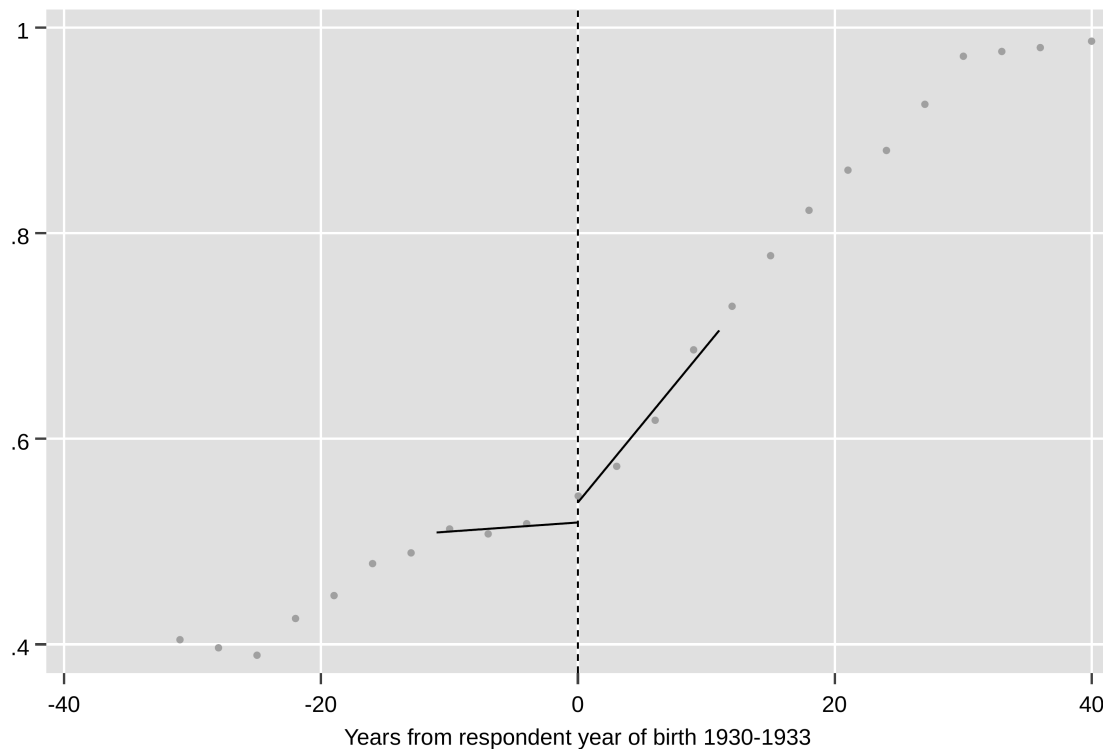


Figure 4: Probability of attaining primary school. *Note:* This figure displays the binned means (gray dots) of the outcome variable as a function of the running variable to threshold. The solid lines represent the local polynomial regression fit (first order and $p = 1$), using a manual bandwidth $h = 11$ and a triangular kernel. The vertical jump at the cutoff $c = 0$ illustrates the estimated treatment effect. The regression functions shown are not adjusted for covariates.

a 14 percentage point increase in the conditional probability of completing primary school relative to immediately preceding cohorts.

This increase is theoretically meaningful and reflects an important, albeit exploratory, empirical pattern. The ratio of public primary school teachers to the school-age population declines at the cohort cutoff of 1933, reflecting the disruption of the Republican school expansion program following the war's outbreak. War-exposed cohorts thus faced, if anything, a less favorable supply environment than immediately preceding cohorts. The finding that the discontinuity becomes substantially larger after conditioning on educational supply is consistent with positive demand-side responses operating during the conflict. Because war-exposed cohorts faced deteriorating supply conditions, the unadjusted discontinuity likely understates the magnitude of the demand-side response: had supply conditions been comparable across cohorts, the observed association would plausibly be larger still. Note, however, this conditional specification remains exploratory rather than causal.

Figure 5 shows the unconditional discontinuity. As with primary education, the uncontrolled estimate (0.039) provides little robust evidence of an overall total effect, despite reaching conventional significance. The estimate rises to 0.058 with

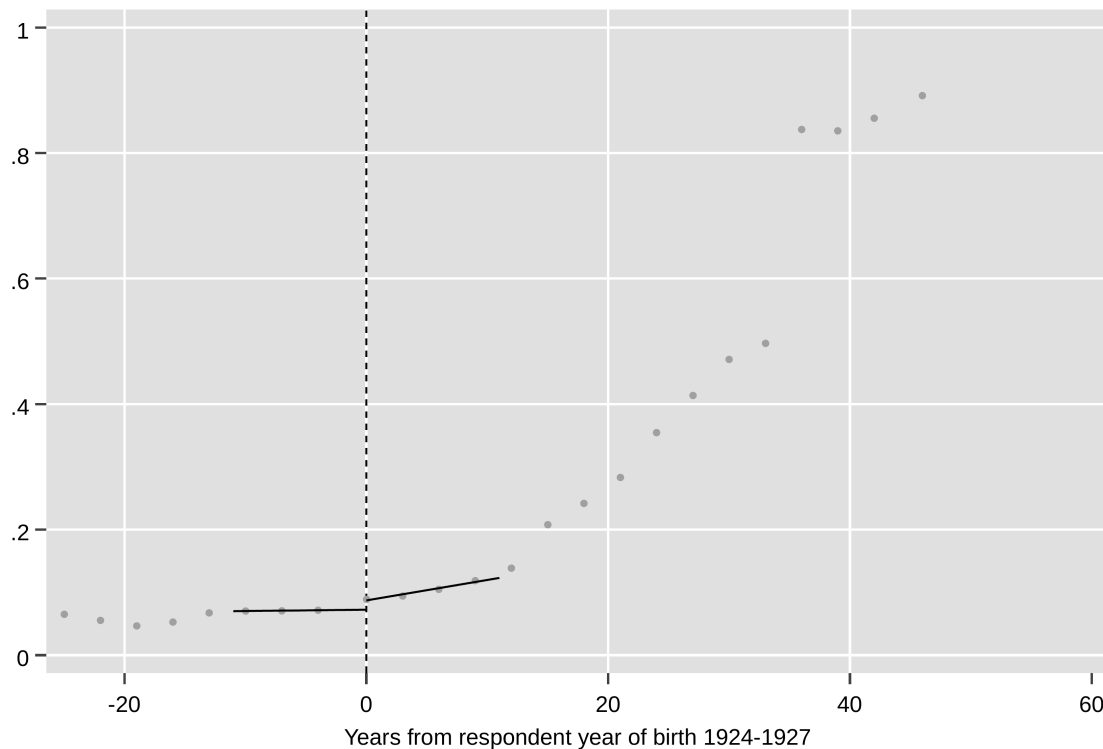


Figure 5: Probability of attaining lower secondary school among those who completed primary. *Note:* This figure displays the binned means (gray dots) of the outcome variable as a function of the running variable to threshold. The solid lines represent the local polynomial regression fit (first order, $p = 1$), using a manual bandwidth $h = 11$ and a triangular kernel. The vertical jump at the cutoff $c = 0$ illustrates the estimated treatment effect. The regression functions shown are not adjusted for covariates.

demographic and economic controls and to 0.063 under the fully specified model. War-exposed cohorts who had already completed primary education were thus approximately 6 percentage points more likely to continue into lower secondary school, a tentative approach to document an exploratory conditional estimate net of the supply-side contraction, not an estimate of the total effect. Although smaller in absolute magnitude than the primary education estimate, this finding is notable given the considerably lower baseline completion rates for lower secondary education in this period and the additional selective barriers that characterized entry into post-primary schooling.

Heterogeneous Effects by Social Background

Models 4 through 6 in Tables 2 and 3 disaggregate the associations by paternal education level. For primary education, an equalizing pattern appears to emerge. The estimated discontinuity is largest and most precisely estimated among children of fathers without primary education (0.135), remains positive among children of fathers with primary education (0.115), and is smaller among children of fathers with secondary or higher education (0.065). The gradient across groups suggests

that war-induced shifts in educational investment were disproportionately concentrated among less advantaged households—families that had previously invested less in children's schooling.

For lower secondary education, the pattern differs in a theoretically informative way. The most precise estimate is obtained among children from the least educated households (0.040), whereas for children of more educated fathers the correlation is positive but estimated with greater uncertainty given the considerably smaller sample sizes in those subgroups. The concentration of the most robust estimates among less advantaged families at both educational transitions suggests that the war's impact on educational demand was not limited to those with pre-existing resources but extended meaningfully to households that had historically been at the margins of educational participation.

We caution against overinterpreting the point estimates for smaller subgroups, particularly children of fathers with secondary or higher education, where sample sizes are substantially reduced and robust confidence intervals are wide. The substantively important finding is the direction and relative magnitudes across groups, rather than the precise point estimates for any individual subgroup.

Violence Intensity As a Test of the Proposed Mechanism

A central implication of our theoretical framework is that the educational response conditional on controls should be stronger, where the conflict was more intense, because higher violence more powerfully triggers the asset-reallocation and protective parenting mechanisms we propose. We test this prediction by stratifying the sample into terciles of provincial war casualties per capita⁵ and re-estimating the main RDD models within each stratum. Figure 6 shows the estimated discontinuities for primary education, and Figure 7 for lower secondary education across violence terciles.⁶

The uncontrolled discontinuities show a pattern directionally consistent with the proposed mechanism. For primary education, point estimates rise across terciles of violence intensity: -0.011 in the lowest tercile, 0.016 in the middle tercile, and 0.052 in the highest tercile; none reaches robust statistical significance. For lower secondary education, the pattern is similar: uncontrolled estimates rise from 0.025 to 0.040 and 0.058 across terciles, again without robust significance. This mirrors the weak evidence for an overall total effect already reported in Tables 2 and 3 (model 1).

Once we account for the contemporaneous contraction in educational supply, the same exploratory adjustment reported in Tables 2 and 3, the violence gradient becomes considerably sharper and, for the highest-violence tercile in primary education, robustly significant. We interpret this pattern as consistent with the violence-intensity gradient predicted by our theoretical framework, although the conditional estimates should be interpreted as exploratory.

We further verify the robustness of these findings by replicating the violence-stratified analysis using alternative thresholds for classifying high-violence provinces (top 20 percent, top 25 percent, and top 33 percent), and the median. The estimated effect for high-violence provinces in primary education remains stable

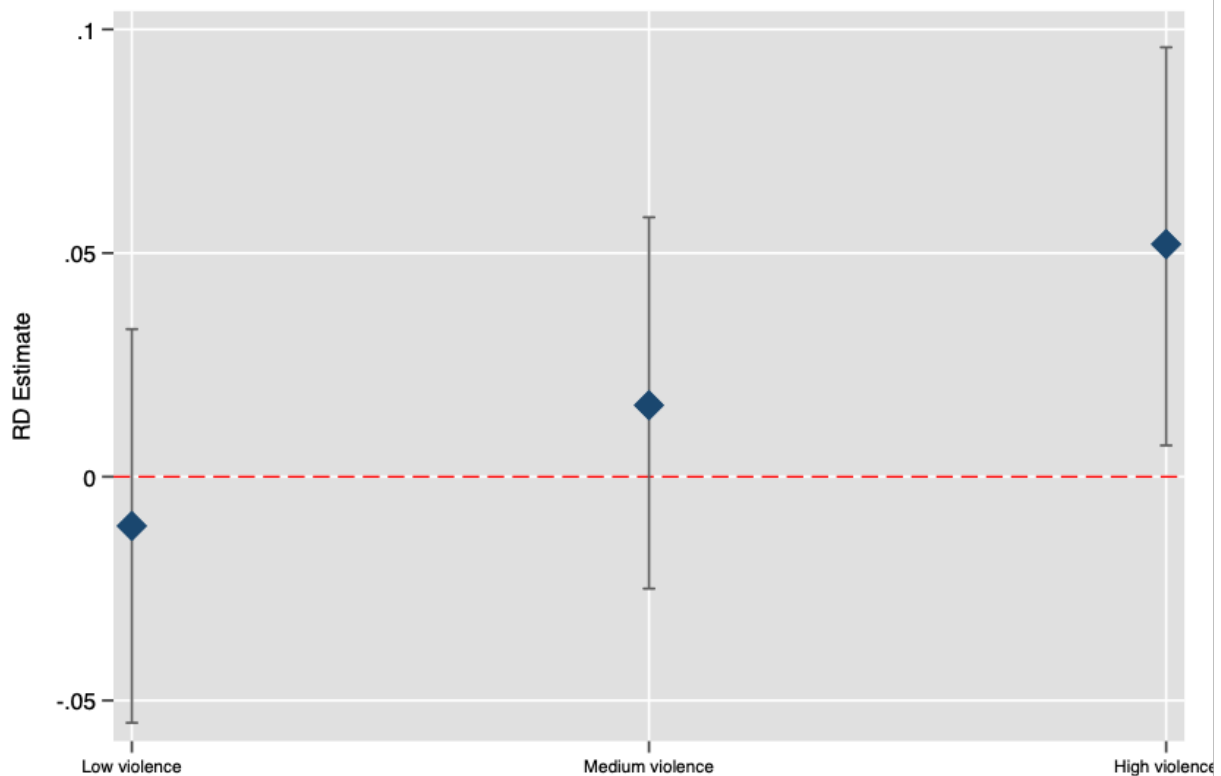


Figure 6: Violence gradient in primary education by provincial war intensity. *Note:* Each point represents the RD estimate of the effect of war exposure on the probability of completing primary education, estimated separately for provinces in each tercile of war casualties per capita. Bars represent 95 percent robust confidence intervals. Running variable: birth cohort distance to the 1933 cutoff. Bandwidth $h = 11$ and a triangular kernel.

across all threshold definitions, ranging from 0.198 to 0.221, confirming that the result does not depend on the specific cutoff chosen (see Table A.1).

Robustness Checks

The analyses reported above rest on a set of identifying assumptions that we now subject to systematic scrutiny. We address three potential threats to the validity of our estimates: selective mortality of respondents' fathers, sensitivity to bandwidth choice, and the specificity of the observed discontinuities to the war-affected cohorts.

Survival bias in the observed sample A potential concern with retrospective survey data from 1991 is selective mortality of the respondents' fathers. The fathers of our treated cohorts were born approximately between 1895 and 1915, placing them between 21 and 41 years old in 1936, the prime combat age. If in high-violence provinces the men who survived were positively selected on socioeconomic status because more educated or wealthier men were better able to avoid combat,

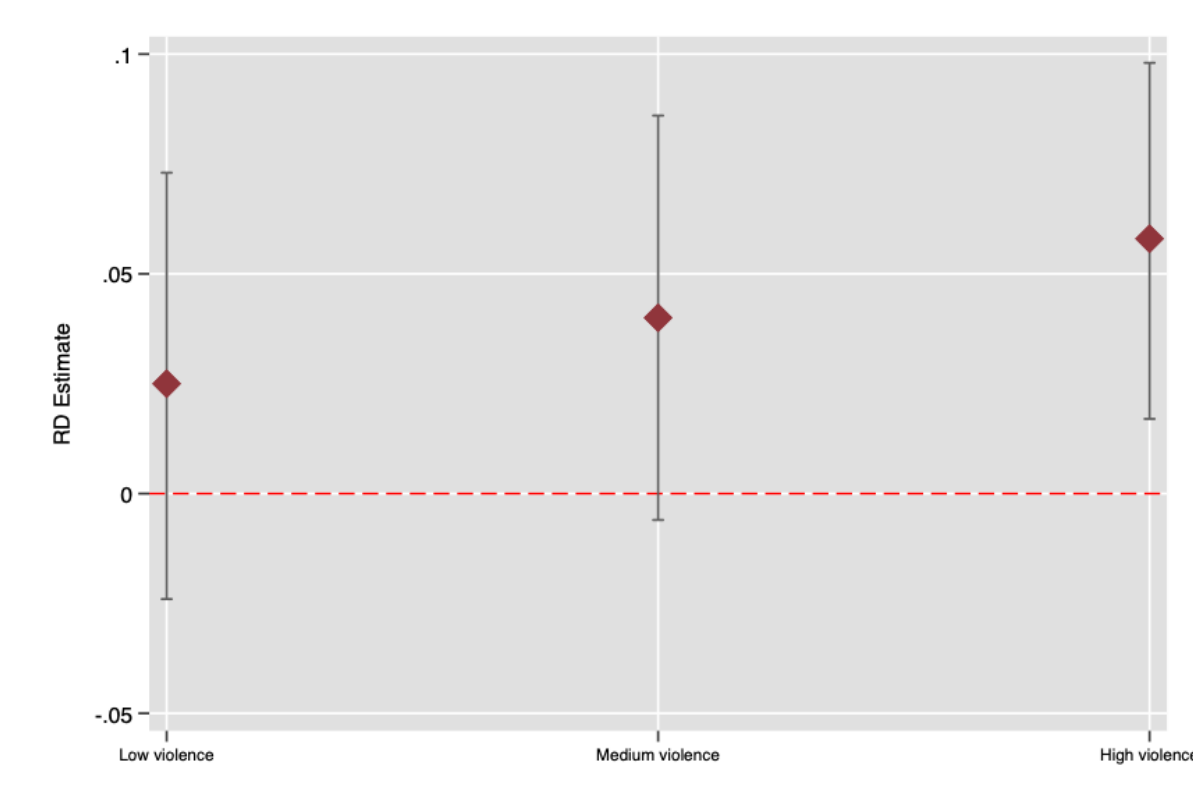


Figure 7: Violence gradient in lower secondary education by provincial war intensity. *Note:* Each point represents the RD estimate of the effect of war exposure on the probability of completing lower secondary education, estimated separately for provinces in each tercile of war casualties per capita. Sample restricted to respondents who completed primary education. Bars represent 95 percent robust confidence intervals. Running variable: birth cohort distance to the 1927 cutoff. Bandwidth $h = 11$ and a triangular kernel.

emigrate, or survive political repression, then the respondents observed in the ES-91 in high-violence areas would be disproportionately drawn from higher-SES families. Because parental education is a strong predictor of children's educational attainment, this compositional shift could inflate our regression discontinuity (RD) estimates in high-violence provinces even in the absence of any genuine behavioral response.

We address this concern with several empirical checks. First, we use information on the year of paternal death available in the ES-91 to test directly whether wartime paternal mortality was higher in high-violence provinces and, crucially, whether it was selective by educational level. Within the RD bandwidth window, fathers of respondents in high-violence provinces were indeed more likely to have died during the war years 1936–1939 ($\beta = 0.017^{**}$), confirming that our violence measure captures genuine differential mortality. However, among fathers who died during the war, the distribution of paternal education is virtually identical across high- and low-violence provinces: 77.5 percent versus 78.9 percent had less than primary education, 12.2 percent versus 11.8 percent had completed primary, and 10.3 percent versus 9.2 percent had lower secondary or more ($\chi^2 = 0.64$, n.s.). This comparison is conditional on paternal death having occurred and addresses a specific

compositional threat: whether the fathers who died differed systematically in education across violence levels. It does not speak to whether education predicts the likelihood of death differently by violence level, a distinct question we address next.

Second, we test whether the relationship between paternal education and wartime death differs by violence intensity, interacting paternal education with the high-violence indicator within the RD bandwidth window, controlling for birth cohort and clustering standard errors by province. The interaction terms are small and statistically indistinguishable from zero for both the “primary” and “lower secondary or more” categories relative to fathers with less than primary education, indicating that the education–mortality gradient does not differ systematically between high- and low-violence provinces.

Third, we test directly for a compositional discontinuity in paternal education at the war cutoffs, estimating our main RD specification with paternal education as the outcome variable. We find no significant discontinuity at either the primary education cutoff or the secondary education cutoff (Table A.2). The absence of any jump in the composition of paternal education at either threshold provides direct evidence against sample selection at the cutoffs themselves.

Fourth, we test whether treated cohorts in high-violence provinces are disproportionately drawn from higher-education families by regressing paternal education on violence intensity within the RD bandwidth window, controlling for birth cohort and clustering standard errors by province. We find no such evidence: the coefficient on violence intensity is small and statistically indistinguishable from zero both for the primary education sample and for the secondary education sample. The distribution of paternal education is virtually identical across provinces with different violence intensity, with approximately 81 percent of fathers having less than primary education in both high- and low-violence provinces.

Fifth, we examine whether war cohorts are systematically underrepresented in high-violence provinces relative to adjacent cohorts, which would constitute direct demographic evidence of differential attrition. Figure A.4 plots cohort size separately for high- and low-violence provinces across the full range of birth cohorts, for the primary (left panel) and secondary (right panel) cutoffs. Both panels show a decline in cohort size around the respective cutoffs—a well-documented demographic consequence of the war and immediate postwar period that reflects reduced births rather than selective mortality of survivors. Critically, this decline is parallel across high- and low-violence provinces: the two lines track each other closely throughout, with no disproportionate dip in high-violence areas around the treated cohorts. If anything, the relative decline is slightly more pronounced in low-violence provinces, which is the opposite of what differential attrition would predict.

These checks indicate that the observed sample is not compositionally biased by differential paternal survival across violence levels. This does not rule out, however, that paternal wartime death operated as a substantive mechanism shaping educational outcomes.

Father’s death as a competing mechanism Beyond its bearing on sample composition, paternal wartime death is theoretically relevant in its own right, as a

channel through which war could plausibly suppress rather than intensify educational investment. If the loss of a father reduced household income at the point when children's continued schooling depended on it, this channel would operate in direct opposition to the asset-reallocation and protective-parenting mechanisms we propose.

The evidence reported above is informative on this point. Fathers in high-violence provinces were more likely to die during the war years (0.017**), meaning that this suppressive channel was, if anything, more strongly present in precisely the provinces, where we also observe the largest positive discontinuities. The positive, monotonically increasing effect persists across violence terciles despite a more intense competing mechanism operating in those same high-violence provinces.

We do not enter paternal death as a covariate in the main specifications, since wartime mortality occurs after cohort assignment and is therefore post-treatment; conditioning on it would risk biasing rather than clarifying the total effect of war exposure. We instead treat it as a theoretically anticipated counteracting mechanism whose presence works, if anything, against our findings.

Bandwidth sensitivity We replicate all main estimates across a range of alternative bandwidths (Table A.3). For primary education, estimated effects range from 0.133 at $h = 9$ to 0.153 at $h = 15$ and $h = 17$, all robustly significant. For lower secondary education—where the minimum viable bandwidth given the discrete cohort structure of the data is $h = 11$ —estimates range from 0.063 at $h = 11$ to 0.047 at $h = 17$, remaining significant across all specifications. The data-driven optimal bandwidth selected by the mean squared error procedure coincides with $h = 11$ for both outcomes, validating our choice of baseline bandwidth. Taken together, these results confirm that the findings are not sensitive to bandwidth choice.

Placebo tests To verify that the observed discontinuities are specific to the cohorts directly affected by the war, we replicate the main analysis using placebo cutpoints defined by birth cohorts born before and after the war-affected cohorts. If the war is the driving mechanism, no significant discontinuities should emerge at these alternative thresholds. Consistent with this expectation, placebo estimates for adjacent cohorts are generally small, unstable in sign, and statistically insignificant, whereas the estimate at the true cutoff stands out clearly as the largest and most consistent discontinuity in the series. One exception worth noting is a marginally negative significant effect at the 1924–1926 placebo cutoff in high-violence provinces, which coincides with cohorts entering primary school during the politically turbulent years of the early Second Republic; we interpret this as reflecting the historically elevated uncertainty of that period rather than a threat to our identification. Full placebo results are reported in Tables A.4 and A.5.

Conclusion

This article argues that the educational consequences of violent conflict depend not only on the destructive capacity of war, but also on the extent to which families retain the ability to reorganize long-term investment strategies under conditions

of uncertainty. Using evidence from the Spanish Civil War, we show that baseline estimates provide limited evidence of an overall total effect of war exposure. Exploratory conditional analyses suggest that, after conditioning on contemporaneous changes in educational supply and provincial GDP per capita, the discontinuity becomes positive and sizeable. Because these covariates may themselves have been affected by the war, these conditional estimates should be interpreted as exploratory rather than causal. However, these findings challenge the dominant assumption that violent conflict uniformly suppresses educational attainment and instead suggest that some conflicts may intensify investments in human capital when civilian and educational infrastructures remain partially functional.

Our findings further show that these adaptive responses were socially stratified in ways that matter for understanding educational inequality. At the primary level, educational gains were strongest among children from less educated households, suggesting that wartime adaptation partially equalized access to basic schooling, a finding consistent with research showing that universal transitions are more susceptible to equalizing shocks than selective ones (DiPrete and Eirich 2006). At the transition into lower secondary education, the pattern becomes more stratified. Although positive effects appear across social groups, families with greater educational resources seem better positioned to convert wartime uncertainty into sustained educational progression at more selective transitions. Conflict therefore did not simply alter average attainment levels but interacted with existing structures of inequality in ways that varied across educational stages. Violent conflict therefore did not simply alter average educational outcomes but also reshaped the distribution of educational opportunity across social groups, compressing inequality at one transition while leaving it intact at the next.

More broadly, the article proposes a conditional theory of how extreme uncertainty reshapes family investment behavior. The mechanism we identify—families reallocating resources toward portable, non-confiscable assets when conventional wealth is threatened—is not unique to violent conflict. Economic crises, hyperinflation, forced displacement, and property confiscation all share the same underlying logic: when physical capital becomes insecure, human capital becomes relatively more attractive. This suggests that the dynamics we document in wartime Spain may generalize to other contexts in which families face severe and sudden threats to their asset base. Future research should examine whether similar patterns emerge under economic shocks, political instability, or forced migration, and whether the stratified nature of the educational response we find is a general feature of how inequality is reproduced under conditions of extreme uncertainty.

Appendix

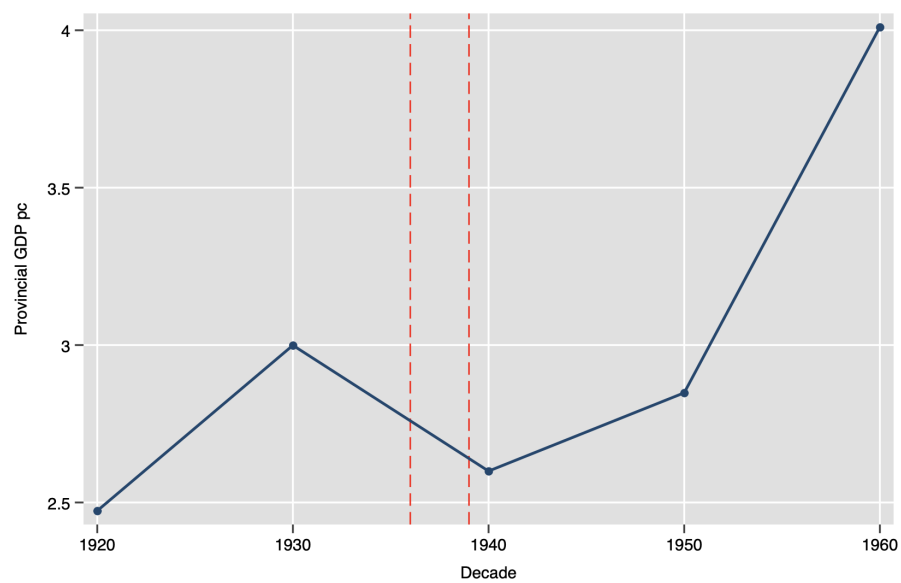


Figure A.1: Evolution of GDP per capita 1920–1960.

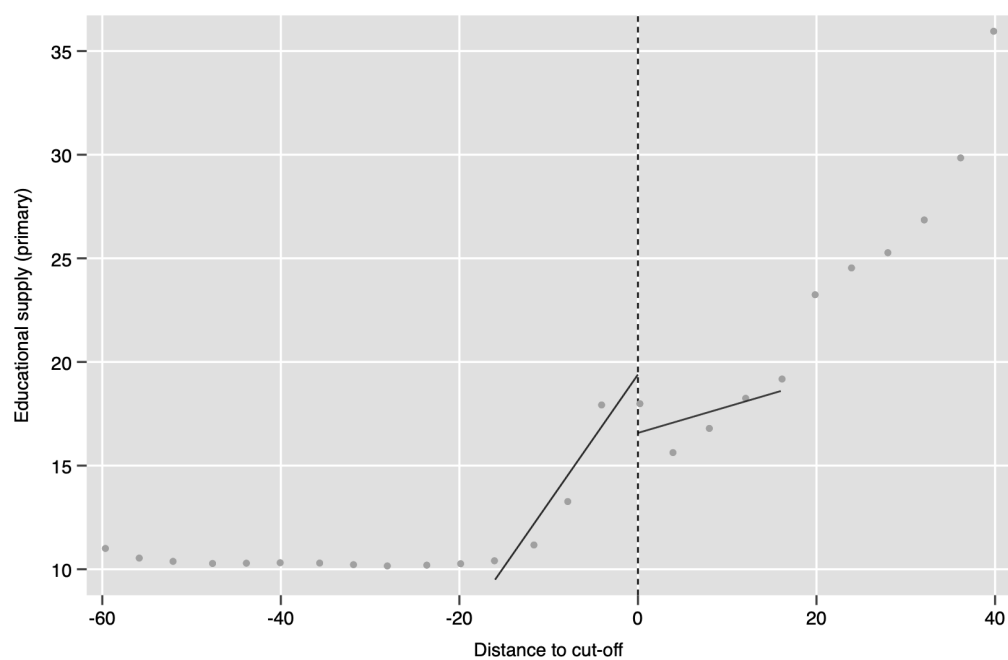


Figure A.2: RDD, educational supply: primary education.

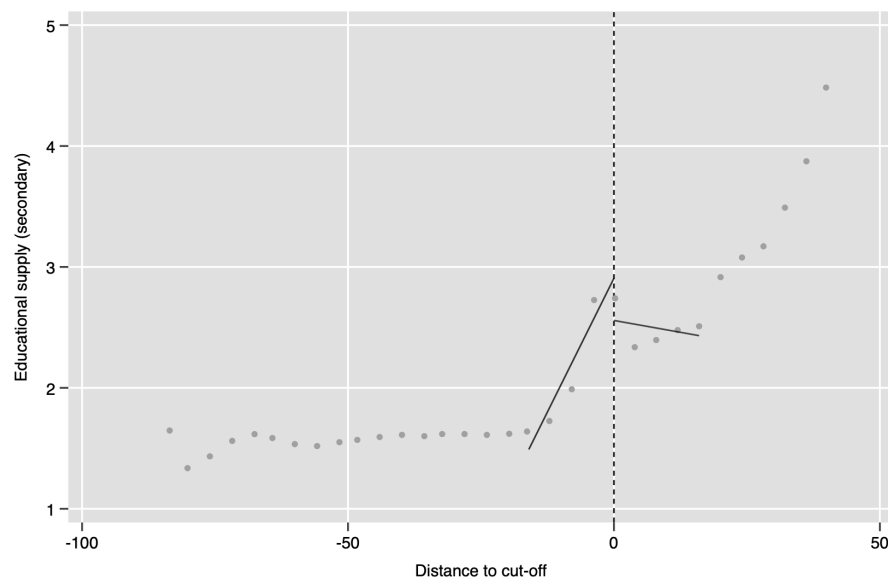


Figure A.3: RDD, educational supply: secondary education.

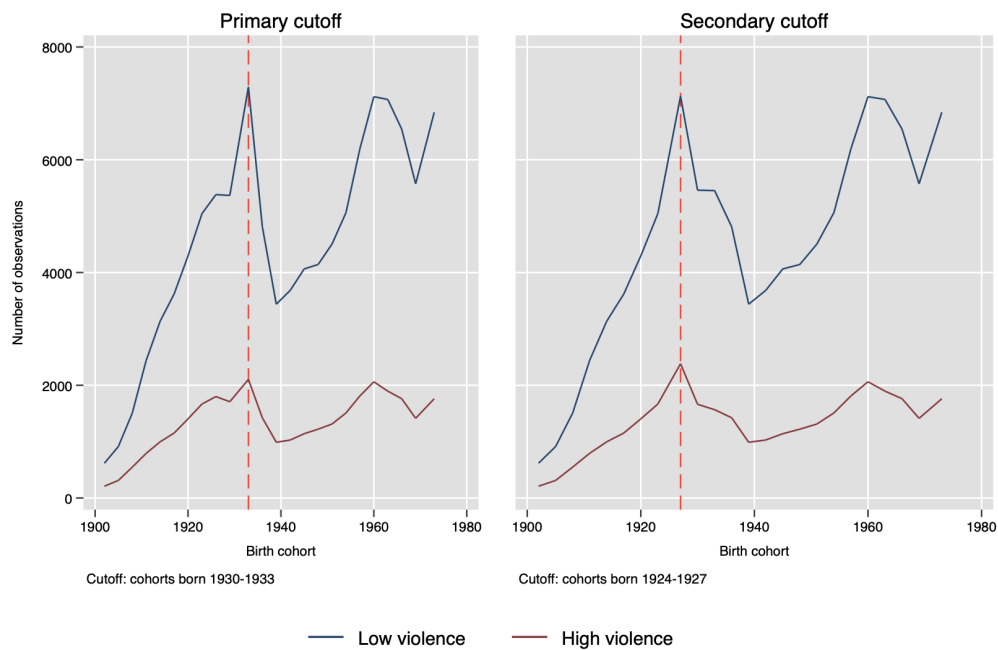


Figure A.4: Cohort size by violence intensity.

Table A.1: Robustness to Violence Threshold Definition.

	Top 20%	Top 25%	Top 33%	Median 50%
<i>Primary education</i>				
RD estimate	0.216*	0.206*	0.221*	0.198*
(SE)	(0.026)	(0.024)	(0.022)	(0.017)
N	27,096	31,482	39,939	66,247
<i>Lower secondary education</i>				
RD estimate	0.065*	0.072*	0.082*	0.067*
(SE)	(0.025)	(0.024)	(0.020)	(0.018)
N	20,651	23,115	29,417	48,012

Note: Standard errors in parentheses. Each column restricts the sample to provinces above the indicated violence threshold. All models include full controls.

* $p < 0.1$.

Table A.2: RD Estimates: Paternal Education at the War Cutoffs.

	Primary Cutoff (1933)	Secondary Cutoff (1927)
RD estimate	-0.014	0.039
(SE)	(0.015)	(0.025)
Robust p value	0.968	0.273
N	139,001	100,418
Bandwidth	$h = 11$	$h = 11$

Note: Outcome is paternal education (0 = less than primary, 1 = primary, and 2 = lower secondary or more). Standard errors in parentheses. Robust p values reported following Calonico et al. (2014). Triangular kernel and mass points adjustment. No significant discontinuity in paternal education composition is detected at either cutoff, indicating no compositional selection in the observed sample at the treatment threshold.

Table A.3: Bandwidth Sensitivity.

	$h = 9$	$h = 11$	$h = 13$	$h = 15$	$h = 17$
<i>Primary education</i>					
RD estimate	0.133*	0.142*	0.147*	0.153*	0.152*
(SE)	(0.017)	(0.013)	(0.012)	(0.010)	(0.009)
<i>Lower secondary education</i>					
RD estimate	—	0.063*	0.059*	0.054*	0.047*
(SE)	—	(0.013)	(0.012)	(0.010)	(0.010)

Note: Standard errors in parentheses. All models include full controls. Minimum viable bandwidth for lower secondary education is $h = 11$ given the discrete cohort structure of the running variable.

* $p < 0.1$ (conventional). For lower secondary education, $h = 9$ is not viable given the discrete cohort structure; estimation begins at $h = 11$.

Table A.4: Discontinuity in the Evolution of the Probability of Completing Primary School by Level of Provincial Violence during the Civil War and Placebo Cohorts.

	1920–1923	1924–1926	1927–1929	1930–1933	1934–1936	1937–1939	1940–1942
<i>High violence</i>							
Discontinuity	–0.037 (0.024)	–0.11* (0.022)	0.00052 (0.022)	0.21* (0.024)	–0.0032 (0.020)	–0.042* (0.023)	0.015 (0.024)
<i>N</i>	31,482	31,482	31,482	31,482	31,482	31,482	31,482
<i>Low violence</i>							
Discontinuity	–0.032 (0.025)	–0.034 (0.022)	0.011 (0.022)	0.061* (0.025)	0.0034 (0.019)	0.00050 (0.022)	0.019 (0.022)
<i>N</i>	37,695	37,695	37,695	37,695	37,695	37,695	37,695

Note: Standard errors in parentheses.

* $p < 0.1$. Threshold for violence intensity set at top 25 percent.

Table A.5: Discontinuity in the Evolution of the Probability of Completing Lower School by Level of Provincial Violence during the Civil War and Placebo Cohorts.

	1915–1917	1918–1920	1920–1923	1924–1927	1928–1930	1931–1933	1934–1936
<i>High violence</i>							
Discontinuity	–0.018 (0.026)	–0.016 (0.024)	0.0036 (0.022)	0.062* (0.024)	0.011 (0.019)	–0.068* (0.023)	0.0038 (0.022)
<i>N</i>	23,115	23,115	23,115	23,115	23,115	23,115	23,115
<i>Low violence</i>							
Discontinuity	–0.025 (0.019)	–0.013 (0.017)	0.0016 (0.015)	0.048* (0.016)	–0.011 (0.012)	–0.014 (0.013)	–0.0030 (0.013)
<i>N</i>	74,328	74,328	74,328	74,328	74,328	74,328	74,328

Note: Standard errors in parentheses.

* $p < 0.1$. Threshold for violence intensity set at top 25 percent.

Notes

- 1 See Balcells and Stanton (2021) for an analysis of the research on violence against civilians during armed conflicts, as well as a classification of different patterns of such violence.
- 2 The original ES-91 sample comprises 157,100 respondents. We exclude individuals born after 1973 (16,413 cases), yielding a working sample of 140,687. Further exclusions due to missing data on key variables reduce the analytical sample size, which varies by model specification.
- 3 The historical series at the provincial level for both variables cover the period from the late nineteenth century to the last cohort under analysis. Teacher counts were compiled from the Statistical Yearbook of Spain (INE, various years) and additional historical statistical sources, such as *Estadística Escolar de España* (1909), *Estadística de los Establecimientos de Enseñanza* (various years), and *Estadística de la Enseñanza Primaria* (various years). For gaps in these time series, we applied linear interpolation, with one exception. A substantial gap exists immediately before and during the Spanish Civil War: annual teacher data are available through 1933, with the next observation in 1940. For 1934–1935, we employed growth-rate projection based on pre-war trends rather than linear interpolation. From 1935 to 1940, linear interpolation was used to estimate values during the conflict years. The population data are sourced from the Spanish census (INE), conducted at ten-year intervals. Each respondent is assigned educational supply values based on their province of residence and age at seven, capturing the educational infrastructure of their childhood environment.
- 4 Data on the GDP per capita have been obtained from estimates of the Spanish real GDP by Prados de la Escosura (2017) and of regional shares of the GDP by Diez-Minguela, Martínez-Galarraga, and Tirado-Fabregat (2018).
- 5 We use provincial-level civil registry data on deaths from military operations compiled by Salas Larrazábal (1977) and adjusted by total provincial population to create a per capita measure.
- 6 The confidence intervals in Figures 6 and 7 appear asymmetric around the displayed point estimates. This is a direct consequence of the robust bias-corrected (RBC) inference procedure proposed by Calonico et al. (2014). As shown in their Theorem 1, the RBC confidence interval is centered around the bias-corrected estimator $\tau_{bc}^{\wedge}$ —which subtracts an estimate of the asymptotic leading bias from the conventional local-linear estimator—rather than around the conventional point estimate displayed in the figure. Because the bias correction shifts the center of the interval away from the conventional estimate, the resulting interval appears asymmetric when plotted against the latter. Throughout the article, we display conventional point estimates for ease of interpretation while basing all inference on the RBC procedure, which offers correct coverage under MSE-optimal bandwidth choices (Calonico et al. 2014).

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