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Andrea Bernini, Sven A. Hartmann

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Institute for Labour Law and Industrial Relations in the European Union (IAAEU)
54296 Trier
www.iaaeu.de

OPTING OUT OF AUTOCRACY: FOREIGN MEDIA, INSTITUTIONAL LEGITIMACY, AND STATE-DEPENDENT HUMAN CAPITAL*

Andrea Bernini† Sven A. Hartmann‡

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Abstract

When autocracies tie human capital to political loyalty, schooling functions as a costly signal contingent on regime survival. Exploiting topography-driven variation in East Germans' reception of West German television, we examine how foreign media, by eroding regime legitimacy, reshapes schooling. Exposure reduced schooling by 0.5 years and post-primary attainment by 10.7 percentage points—voluntary withdrawal driven by collapsing legitimacy, not screening, cognition, time use, preferences, or economic anxiety. After reunification, the employment and income gaps closed, the exposed proved less overeducated, and a 1959 reform raising attainment moved neither earnings nor employment—confirming the forgone schooling was political, not productive.

Keywords: Human capital, mass media, authoritarian institutions, institutional legitimacy, signaling.
JEL classification: I21, J24, P16, D83, O15, N44.

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†University of Houston. abernini@uh.edu.
‡Trier University & IAAEU. hartmann@iaaeu.de.

> *"We'd always been irritated that sports had clashed with the ever-exciting* Little House on the Prairie *or the 1950s Western series* Fury*. Everyone in the GDR watched Western TV shows, which could be picked up by fiddling with the TV antennas. We just had to do it secretively, and we were careful never to mention our favorite shows in front of our teachers."*
>
> — Hensel (2004)

1 Introduction

In the standard Becker (1964) model, individuals invest in schooling to maximize the present value of expected labor market returns. In many institutional environments, however, those returns are explicitly political. Authoritarian regimes design education systems to do more than impart productive skills: schooling enforces ideological conformity, screens for regime loyalty, and gates access to scarce state-allocated rents (Rodden 2002; Levitsky and Way 2010; Kim 2013; Lankov 2015; Cantoni et al. 2017; Guriev and Treisman 2022). When educational advancement and economic rewards both require credible signals of political alignment, the private value of schooling depends on the perceived legitimacy and durability of the state, not on the productive content of what is taught. Modern autocracies recognize this fragility: they invest heavily in digital firewalls and selective censorship precisely to insulate citizens from foreign narratives that might destabilize the political return to compliance. What happens to human capital investment when these information barriers fail?

We study this question using the German Democratic Republic (GDR), one of the most explicitly ideologically engineered education systems of the twentieth century, and a natural experiment that delivered quasi-random variation in foreign media access. Under socialism, schooling in the GDR was a constitutive instrument of political control: curricula were grounded in Marxist–Leninist doctrine; advancement to elite tracks (*Erweiterte Oberschule*, university) and entry into premium occupations were strictly conditional on demonstrated loyalty; and youth organizations such as the Free German Youth (FDJ) functioned as institutional gatekeepers (Fulbrook 2005; Steiner 2010; Glaeser 2000). As one contemporary memoir records, "you could be as mediocre as you wanted in math or history, as long as you got good grades in the things that really mattered: conduct, orderliness, enthusiasm, and application" (Hensel 2004). East German schooling thus produced state-dependent human capital: credentials whose value was inseparable from the regime that issued them.

To identify the causal effect of external cultural exposure on educational investment, we exploit a natural experiment: geographic variation in West German television reception within the GDR. Because broadcast coverage was determined by topography and the placement of West German transmitters rather than by individual choice, local economic conditions, or socialist policy, signal availability provides a plausibly exogenous shock

to the cultural information environment of East German students (e.g., Kern and Hainmueller 2009; Crabtree, Darmofal, and Kern 2015; Bursztyn and Cantoni 2016). Western programming projected an alternative normative order built on individual freedom, consumerism, and democratic accountability, directly at odds with the GDR's collectivist ideology of socialist duty.

Roughly 85 percent of East German households received Western signals; the principal exceptions were the southeastern district of Dresden (the so-called *Tal der Ahnungslosen*, "Valley of the Clueless") and parts of the northeast, where natural terrain blocked reception. We combine a high-resolution Longley–Rice signal-propagation model with the 1990 wave of the German Socio-Economic Panel (SOEP), which surveyed East German households in the summer of 1990, only months after the Wall fell but before any reunification policy could affect the schooling decisions we measure. Because the SOEP records pre-reunification residence at the county level, we can assign treatment based on individuals' formative-years location rather than on potentially endogenous post-1989 mobility.

Three findings organize the paper. First, exposure to West German television sharply depressed educational attainment. In our preferred specification with district fixed effects and individual controls, exposed individuals are 10.7 percentage points less likely to attain post-primary education (a 17.7 percent decline relative to the sample mean) and complete 0.5 fewer years of schooling, a roughly 4 percent decline. The estimates are stable across geographic regression-discontinuity specifications that compare individuals on either side of the signal boundary, ordered-probit, logit, and Poisson functional forms, entropy-balancing and coarsened-exact-matching reweighting, and sample restrictions that drop migrants, border counties, Berlin, and recent movers; they remain negative across a range of alternative signal-strength cutoffs. Placebo tests on cohorts who completed schooling *before* the GDR was founded and *before* West German broadcasting began rule out persistent regional confounders.

Second, the channel is institutional, not cognitive or behavioral. Using a declassified 1989 youth survey in which respondents reported views anonymously, we instrument self-reported viewership with topographic signal access. Exposure eroded trust in state policy across information, domestic, cultural, and scientific domains and confidence in the socialist system's long-run prospects; it weakened identification with the GDR, reduced membership in the Socialist Unity Party (SED, the regime's ruling party and gatekeeper to premium occupations), and raised emigration intent. Crucially, exposure left the probability of holding an FDJ leadership position unchanged. This null is the crux of the demand- versus supply-side distinction: because holding such a position was the active signal of loyalty on which the regime rationed access to elite educational tracks, a state screening out politically unreliable students would have driven exposed students out of these roles—yet their formal standing was untouched. We interpret this divergence, di-

minished ideological attachment without a loss of formal standing, as the hallmark of the demand channel. It indicates that students voluntarily divested from a system they no longer trusted, rather than being mechanically excluded by state gatekeepers.

We corroborate this reading with formal complaint letters (*Eingaben*), one of the few officially sanctioned channels through which GDR citizens could register grievances with state authorities. Unlike survey responses, these letters were behaviorally costly to submit and survive in administrative archives for the full population, providing a revealed-preference measure of legitimacy erosion independent of any survey evidence. Exposed counties filed significantly more complaints about public education and youth organizations, precisely the institutions through which the regime priced political loyalty in schooling. As alternative mechanisms that could explain lower educational attainment, we rule out, in turn, cognitive depreciation, time crowding-out, preference shifts, and economic anxiety. The decline in schooling traces to a collapse in the perceived legitimacy of the state's reward structure.

Third, the post-reunification trajectory of exposed cohorts confirms that what they declined to invest in was political rather than productive capital. In 1990, the final year of socialist labor allocation, exposed individuals were 4.5 percentage points less likely to hold full-time jobs, consistent with their exclusion from state-allocated premium positions. After the socialist regime collapsed and political rents disappeared, however, the employment gap closed within five years and did not re-emerge. The income gap followed the same arc, and exposed individuals proved *less* prone to overeducation and educational mismatch, indicating that the schooling they forwent would have amounted to excess credentialing in a market economy. By contrast, exposed cohorts continued to sort away from formerly state-dominated occupations, including public administration, education, security, and the party apparatus, for roughly a decade. Our reading of these dynamics is that the marginal year of GDR education that exposed students chose to forgo embodied a near-zero productive return and a positive political premium that disappeared with the regime. To reinforce this decomposition, we leverage a complementary regression-discontinuity design built around the 1959 GDR schooling reform, which mandated a two-year increase in compulsory schooling. This policy-driven variation in education recovers a similarly near-zero productive return. This implies that the GDR schooling system generated substantial inefficiencies, extracting time and ideological compliance in exchange for credentials with purely regime-specific returns.

The paper contributes to three literatures. First, an influential strand of the literature finds that exposure to television and other mass media raises human capital, aspirations, and developmental outcomes by providing role models, lowering information frictions, or shifting the perceived returns to schooling (Gentzkow and Shapiro 2018; Jensen and Oster 2009; La Ferrara, Chong, and Duryea 2012; Kearney and Levine 2019; DellaVigna and

La Ferrara 2015; Bjorvatn et al. 2020). We document the opposite: in our setting, media exposure *reduces* educational investment. The contrast reflects a boundary condition on the canonical result. The sign of the media-on-education effect depends on the alignment between media content and the institutional structure that mediates educational returns. When the state is the residual claimant on those returns, media content that erodes state legitimacy depresses rather than raises investment. This reversal, and a framework for understanding when and why it arises, is the first contribution.

A second strand of work shows that culture shapes economic behavior (Bisin and Verdier 2001, 2011; Algan et al. 2012; Alesina and Giuliano 2015; Guiso, Sapienza, and Zingales 2003) and that cultural shocks and institutional change can reshape preferences and economic behavior (Dell, Pei, and Olken 2018; Cavalli and Grätz 2022; Doepke and Klasing 2026). We contribute to this literature by providing causal evidence on beliefs about institutional legitimacy as a mechanism linking cultural exposure to economic decisions. Our setting is well suited for this purpose: it combines a clean instrument with direct measurement of the intermediate belief (trust in regime institutions, in the same population as the main outcome) and long-run labor market data that allow us to assess the interpretation. The results are consistent with a causal sequence from media exposure to eroded legitimacy beliefs, to reduced schooling investment, to a labor-market penalty that closes once the political return disappears. The evidence suggests that beliefs about institutional legitimacy shape not only political behavior but economic decisions as well.

Third, a rich literature uses West German broadcasts to identify cultural effects in the GDR.[1] We extend this frontier to the domain of human capital and, crucially, exploit reunification to decompose the marginal return to authoritarian schooling into its productive and political components. Because reunification stripped the state-dependent return to zero *ex post*, the subsequent convergence of exposed and unexposed workers in the labor market suggests that the productive component of the marginal forgone year of schooling was approximately zero, with the political premium accounting for most of the return. We confirm this near-zero productive return with a second, independent design that exploits a GDR compulsory-schooling reform unrelated to media exposure. This decomposition contributes direct quantitative evidence on the political-rents content of authoritarian education and complements work on the costs of ideologically engineered institutions (Cantoni et al. 2017; Guriev and Treisman 2022).

[1] The literature exploiting this natural experiment spans several domains: political attitudes and behavior (Kern and Hainmueller 2009; Kern 2011; Crabtree, Darmofal, and Kern 2015; Friehe, Müller, and Neumeier 2020; Hornuf, Rieger, and Hartmann 2023); consumption (Hyll and Schneider 2013; Bursztyn and Cantoni 2016); entrepreneurship (Slavtchev and Wyrwich 2023); fertility and family formation (Hartmann 2024); environmental attitudes (Hartmann 2026); crime (Friehe, Müller, and Neumeier 2018); and health behaviors (Bernini and Hartmann 2025).

Section 2 reviews the institutional setting. Section 3 develops the conceptual framework and derives three testable predictions. Section 4 describes the data and identification strategy. Sections 5–7 present the main results, mechanism evidence, and post-reunification dynamics. Section 8 concludes.

2 Historical and Institutional Background

Education and ideology in the GDR. The GDR education system was engineered to manufacture political loyalty: schooling was its foundational instrument of ideological reproduction. The ruling SED monopolized educational policy and embedded ideological conformity into a highly centralized, state-run system (Fulbrook 2005; Steiner 2010). As Rodden (2002) observes, "no area of East German society more decisively formed the 'socialist citizen' than education."

The institutional backbone of the system was the *Polytechnische Oberschule* (POS), a ten-year general school that combined academic instruction, vocational training, and explicit political indoctrination. Curricula were grounded in Marxist–Leninist doctrine, with mandatory courses in socialist civics (*Staatsbürgerkunde*), political economy, and the history of the workers' movement (Davis 2013). Ideology permeated all subjects: literature, history, and geography were systematically recast to assert the moral and historical superiority of socialism over capitalism (Brock 2009; Rodden 2006). Centrally produced textbooks and party-approved pedagogy enforced uniformity. We document representative textbook excerpts and visual materials in Appendix C.

Crucially, the GDR education system was a political screening mechanism. Access to the academic upper-secondary track (*Erweiterte Oberschule*, EOS) and to subsequent entry into premium occupations depended on political reliability rather than academic merit alone (Glaeser 2000). State authorities and teachers evaluated students on 'socialist morality' and on participation in regime-sponsored institutions. The FDJ acted as an institutional gatekeeper: nominally voluntary, membership and active engagement were de facto prerequisites for socioeconomic advancement. Dissent or ideological apathy truncated professional opportunities. Within this framework, educational attainment was a costly signal of political loyalty whose private value depended on the perceived legitimacy and durability of the regime.

West German cultural spillovers. To preserve this ideological monopoly, the GDR fiercely restricted access to alternative information. For this purpose, the Ministry for State Security (*Ministerium für Staatssicherheit*, Stasi) imposed import bans on Western print media, enforcing them through strict border control. The Stasi also maintained its own departments in the post offices, where citizens' mail coming from outside the GDR

was systematically opened. By the 1980s, however, an estimated 85% of East German households had regular access to West German television. Initial attempts to block the signals from the West German TV towers failed (Boyd 1983; Kuschel 2016). Because broadcast coverage was determined by topography and the placement of Western transmitters rather than by individual choice, local economic conditions, or GDR policy, the resulting variation provides a plausibly exogenous shock to cultural exposure. The principal exceptions were the southeastern district of Dresden, famously dubbed the "Valley of the Clueless," and parts of the northeast, where natural terrain blocked signal reception.

To document the differences between the West and East German broadcasting systems systematically, we collected detailed information on the programming of the two national West German television channels (ARD and ZDF) and the two national GDR channels (DDR 1 and DDR 2) between 1970 and 1989 using two historical television archives (see Appendix D for a detailed description). The resulting dataset comprises 37,550 individual broadcasts across 471 unique broadcast days. Our content analysis reveals substantial differences in the information environments offered by the two television systems. Compared with GDR television, West German television devoted considerably larger shares of programming to news, political magazines, religious broadcasts, and entertainment formats such as television series, whereas GDR television placed greater emphasis on educational and children's programming, consistent with its role as an instrument of political socialization. Moreover, West German television reported regularly on political, economic, and social developments in the GDR through recurring news and political magazine formats, thereby confronting viewers with perspectives largely absent from official GDR media.

West German programming broadcast a cultural paradigm directly at odds with the GDR's collectivist foundations. News promoted liberal-democratic norms; entertainment and advertising glorified personal freedom, consumer choice, and individual self-expression. For East German youth, television was a window onto an alternative social order that explicitly contested the moral authority of the state: without leaving their living rooms, students could juxtapose official classroom rhetoric with the world depicted on Western screens (Davey 1987). Contemporary accounts suggest that exposed students often developed 'value pluralities,' performing outward compliance in school while privately absorbing the aspirations modeled by Western media (Gleye 1991). These are precisely the conditions under which our framework predicts withdrawal from state-mediated education: foreign content simultaneously erodes the perceived value of regime credentials and raises the psychological cost of signaling conformity.

A growing literature documents that Western broadcasts altered cultural spillovers across a range of outcomes, from political attitudes and electoral behavior to consumption, entrepreneurship, fertility, and health. We extend it to the domain of educational investment. Human capital is a particularly consequential margin: unlike consumption

or attitudes, schooling decisions are irreversible, accumulate over time, and determine long-run earnings capacity. Moreover, by exploiting reunification to strip the political premium from schooling *ex post*, we can identify not just whether Western television changed education, but *why* it changed it and *what the forgone schooling was worth*.

3 Conceptual Framework

We formalize the schooling decision in an authoritarian setting to organize the empirical analysis and to clarify what parameters our design recovers.

Setup. A representative agent i chooses whether to invest in advanced (post-primary) state education, $e_i \in \{0, 1\}$. Investing yields an expected lifetime return composed of two parts: a productive wage w that accrues regardless of regime, and a regime-specific political premium R that captures access to elite occupations, university tracks, and state-allocated housing reserved for credentialed political loyalists. The expected value of R depends on the agent's subjective belief in the legitimacy and durability of the regime, $\theta_i \in [0, 1]$.

Acquiring education entails a cost $C(c_i, \psi_i)$, where c_i is innate academic ability and ψ_i measures ideological alignment with the state. Because advanced schooling required active participation in socialist youth organizations and mastery of Marxist–Leninist doctrine, agents who internally reject regime ideology bear a higher psychological and moral cost of outward conformity. The agent maximizes expected utility:

$$U_i(e_i) = \big(w + \theta_i R\big)e_i - C(c_i, \psi_i)e_i, \tag{1}$$

and chooses $e_i = 1$ if and only if

$$w + \theta_i R \geq C(c_i, \psi_i). \tag{2}$$

The cultural shock. Let $T_i \in \{0, 1\}$ index exposure to West German television. Western media affects the schooling decision through two distinct channels.

(i) The legitimacy channel. Uncensored Western news and entertainment expose the relative economic underperformance of the socialist state and broadcast competing political norms, lowering the perceived legitimacy and expected survival of the regime: $\theta_i(T_i = 1) < \theta_i(T_i = 0)$. This depresses the expected value of the political premium R.

(ii) The ideological-cost channel. Western programming models individualistic, consumer-oriented values that contradict socialist collectivism. As exposed agents internalize these

norms, the cost of credibly signaling socialist conformity rises: $C(c_i, \psi_i \mid T_i = 1) > C(c_i, \psi_i \mid T_i = 0)$.

The total effect of West German television exposure on the net benefit of advanced education is therefore

$$\frac{\partial\big(w + \theta_i R - C(c_i, \psi_i)\big)}{\partial T_i} = \underbrace{R\,\frac{\partial \theta_i}{\partial T_i}}_{<0} - \underbrace{\frac{\partial C(c_i, \psi_i)}{\partial T_i}}_{>0} \;<\; 0. \tag{3}$$

Both channels point in the same direction. Holding the productive return w fixed, exposure unambiguously reduces the expected utility of investment in state-dependent education.[2] Equation (3) yields three predictions that map onto our empirical sections:

P1: Main effects. Marginal agents withdraw from advanced schooling when the legitimacy or ideological-cost channels operate. We test this in Section 5.

P2: Mechanism. The decline in schooling reflects a fall in θ_i and a rise in the cost of conformity, ψ_i, rather than cognitive or preference-based shifts. We test this in Section 6.[3]

P3: Post-reunification labor markets. If the regime collapses, the political premium vanishes ($R = 0$) and only the productive return w is realized. If GDR education was primarily political signaling, the labor-market disadvantage of exposed individuals (their lower probability of holding the desirable, state-allocated jobs) should close with the collapse of the regime, even though the schooling shortfall itself persists. Persistent occupational sorting away from formerly state-dominated sectors, by contrast, should remain. We test this in Section 7. An exogenous, policy-driven increase in compulsory schooling in 1959 delivers a complementary test of the same parameter: if $w \approx 0$, such an increase should raise attainment without raising income or employment.

[2] The model is partial equilibrium: each agent's schooling decision is taken independently, abstracting from the signaling-game interaction in which the payoff to compliance depends on the aggregate enrollment rate. In a full signaling equilibrium, if Western television reduces compliance broadly, the screening value of the GDR credential falls for all students, including those in unexposed counties. This general-equilibrium feedback would compress the schooling gap between treated and control units, so our reduced-form estimates are best interpreted as conservative lower bounds on the individual-level effect of legitimacy erosion.

[3] Because $\theta_i \downarrow$ and $\psi_i \uparrow$ both reduce the net benefit of schooling, the education estimates identify their joint effect, which we term *institutional alienation*. The mechanism analysis nonetheless speaks to each channel separately: trust in regime policies and confidence in the socialist system's future load on $\theta_i \downarrow$; identification with GDR institutions, party membership, and emigration intent load on $\psi_i \uparrow$. Both are found operative. Since they predict the same sign for schooling, the main estimates remain a composite.

4 Data and Identification Strategy

Estimating the causal effect of external media exposure on human capital in an authoritarian setting is complicated by a selection problem: individuals who seek out foreign media likely differ systematically in unobserved preferences, family background, and, crucially in this context, latent political attitudes that independently shape schooling decisions under the regime. We address this endogeneity by exploiting quasi-random geographic variation in West German television signal reception generated by topography and transmitter placement: factors determined by engineering and geography rather than by individual demand, local economic conditions, or GDR policy.

4.1 Measuring West German television exposure

We measure exposure using a high-resolution analog signal-propagation model. Following Crabtree, Darmofal, and Kern (2015) and Bursztyn and Cantoni (2016), we classify a county as treated if predicted average signal strength exceeds -86.5 dBm, the threshold required for reliable analog reception with standard household antennas. The Longley–Rice model underlying this prediction incorporates precise transmitter coordinates, broadcast frequencies, and detailed local topography to simulate how signals propagate across terrain. Because SOEP respondents are identified at the county rather than municipality level, we assign treatment at the county level. Figure Ia maps the resulting coverage pattern and transmitter locations.[4]

To verify that geographic signal availability translates into actual viewing behavior, we use a declassified 1989 survey conducted by the *Zentralinstitut für Jugendforschung* (ZIJ) shortly before the fall of the Berlin Wall (GESIS, ZA6008 1989). The survey collected granular, self-reported media consumption data for 3,564 East Germans across eight of the GDR's fourteen districts.[5] Figure A.1 documents a sharp discontinuity in viewing habits across the signal boundary: in unexposed districts, 82.5% of respondents reported almost never watching West German television (67.8% never, 14.7% rarely), compared to just 5.4% in covered regions, where two-thirds (66.3%) reported watching daily. Table I formalizes this first-stage relationship for the 3,416 respondents with complete covariates.

[4] Figure Ib illustrates the physical infrastructure underlying the variation: a West German broadcast transmitter sited on an elevated ridge along the inner-German border. Transmitters were deliberately positioned on high ground to maximize signal reach into East German territory. The Longley–Rice model captures precisely how this elevation advantage interacted with GDR terrain to generate the sharp, topography-driven discontinuities in reception that our identification strategy exploits.

[5] ZIJ surveys were classified as confidential during the socialist era and were declassified after reunification (Kern and Hainmueller 2009). To minimize preference falsification, questionnaires were administered anonymously in group settings and collected in sealed urns (Friedrich 1990). While preference falsification remains a possibility despite preventative measures, it is unlikely to vary systematically between the treatment and control groups (Kern and Hainmueller 2009).

Signal access raises an ordinal viewership index (1 = never, 5 = daily) by 2.8 points, from near-non-exposure to near-daily consumption, and increases the probability of watching at least once a week by 77 percentage points. The estimates are virtually unchanged when we condition on individual-level covariates.

The ZIJ data are powerful for first-stage validation but have three limitations: geographic identifiers are restricted to the district (instead of county) level, the survey omits the northeastern control region, and the sample skews young (mean age 23).

4.2 Measuring educational attainment

We construct our main estimation sample from the 1990 wave of the SOEP (2025). Conducted in the summer of 1990, just after the Wall fell but before formal reunification on 3 October, this wave is uniquely well suited for our purposes: it captures rich individual-level demographic and educational data reflecting decisions made entirely within the institutional framework of the GDR.

We restrict attention to the East German sample, initially comprising 4,453 individuals across 2,179 households. We drop 274 individuals still enrolled in formal education or active apprenticeships at the time of the interview, yielding a final estimation sample of 4,179 adults whose schooling is observed at completion. We assign treatment based on each respondent's pre-reunification county of residence; given the GDR's strict constraints on internal mobility, this is a reliable proxy for residence during formative schooling years.

We measure educational attainment along two margins. The extensive margin is a binary indicator for completing any credential beyond the primary level, the threshold that separates the basic polytechnic cycle from all further qualification tracks in the GDR system. The intensive margin is total years of completed schooling, constructed by mapping each GDR credential to its statutory duration: e.g., the standard *Polytechnische Oberschule* (POS) maps to 10 years, the academic-track *Erweiterte Oberschule* (EOS) to 12 years; the full credential-to-year mapping appears in Figure A.2.

For the long-run analysis in Section 7, we follow these individuals through the longitudinal SOEP from 1990 to 2010 and measure four families of outcomes: full-time employment; employment in a *state-dominated* occupation (military and security services; political administration and public service; education; law, social sciences, and social services; media, culture, and information; and religious occupations, coded from ISCO-88 and listed in Table A.2); the natural logarithm of net household income; and educational mismatch. Mismatch is constructed with the realized-matches method: within each three-digit occupation and survey year, a worker is classified as overeducated, matched, or undereducated as their years of education lie above, within, or below one standard deviation of the occupational mean, and we record both the incidence of mismatch and overeducation and

their intensity in years (Appendix A.2). Finally, the regression-discontinuity design in Figure IV uses the respondent's birth year as the running variable around the GDR's 1959 extension of compulsory schooling to the ten-year POS, which became binding for cohorts born from 1944 onward (Table B.10).

4.3 Descriptive statistics and balance

Table II reports summary statistics. Educational attainment in the GDR was high by international standards: 60.3% of the sample completed post-primary education, with a mean of 12.7 years of schooling. Treatment is widespread, reflecting the topographic dominance of West German signal coverage: 192 of the 217 counties, and the great majority of individuals in the sample, received reliable signals. A raw comparison of means already reveals a meaningful educational deficit in exposed regions: individuals with Western television access were 6.4 percentage points less likely to attain post-primary education and completed 0.23 fewer years of schooling, both differences significant at the 1% and 5% levels, respectively. Pre-determined covariates (gender, age, marital status, migration background) are tightly balanced across treatment and control. The only detectable difference is a small (0.13-person) imbalance in household size, consistent with documented effects of Western television on fertility (Hartmann 2024) and absorbed by our preferred specification. Panel B shows that the same pattern holds within the 100 km geographic regression-discontinuity (GRD) subsample.[6]

4.4 Empirical strategy

We estimate the reduced-form effect of West German television exposure on educational attainment with the following baseline specification:

$$Y_{ic} = \alpha + \beta \, \mathrm{TV}_c + \gamma X_i' + \delta_d + \varepsilon_c, \tag{4}$$

where Y_{ic} is an educational outcome for individual i in county c; TV_c is a binary indicator equal to one if county c had reliable West German signal reception; X_i is a vector of individual covariates (gender, age, age^2, marital status, household size, migration background); and δ_d are district fixed effects that absorb broader regional heterogeneity. We cluster standard errors at the county level to allow for arbitrary spatial correlation within the unit at which treatment is assigned. The coefficient of interest, β, captures the causal effect of cultural exposure on educational investment.

[6] Table B.5 shows that our results are robust to addressing residual imbalance via entropy balancing and coarsened exact matching.

The identifying assumption is that, in the absence of West German broadcasts, educational outcomes in exposed and unexposed counties would have evolved in parallel. We support this assumption with three lines of evidence.

Pre-treatment balance and parallel trends. Population-weighted district-level data from the 1955 and 1989 GDR Statistical Yearbooks confirm that treatment and control regions were structurally comparable before the treatment period and remained comparable throughout it. Panel A of Table B.1 reports baseline differences in 1955 (the earliest year with comprehensive administrative records). We find no systematic disparities in population density, demographic composition, suicide rates, retail sales, or educational inputs (students per teacher and students per class). Panel B replicates the comparison in 1989, just months before reunification: treated and control districts remain statistically indistinguishable across health and economic indicators. Panel C documents that these characteristics evolved in parallel between 1955 and 1989, ruling out differential pre-trends. Panel D verifies balance in pre-socialist religious composition (Protestant and Catholic shares in 1925 and the Jewish share in 1910), a stringent check given the well-documented link between historical denominational structure and long-run literacy and institutional attitudes (Becker and Woessmann 2009). Together, these checks demonstrate that the GDR maintained substantial spatial homogeneity along pre-determined characteristics relevant for human capital.

Institutional constraints on residential sorting. The most consequential threat to our design is endogenous sorting: individuals with stronger latent demand for Western content or lower educational aspirations might selectively have located in signal-covered counties. The GDR institutional environment, however, made such sorting largely infeasible. The state tightly regulated internal mobility through centralized labor allocation, mandatory residence permits, and a chronic, state-managed housing shortage (Hyll and Schneider 2013). Between 1970 and 1988, the annual rate of cross-county migration was just 2.5 percent, implying that the average GDR resident crossed a county boundary roughly once in a lifetime (Grundmann 1998).

To guard against contamination from the selective relocation that followed the Wall's opening, we assign treatment based on each respondent's pre-1989 county of residence and restrict the sample to individuals for whom that location is recorded. Table B.2 further

confirms that our results are robust to dropping individuals who changed residence in the two years preceding the 1990 SOEP interview, effectively ruling out 'last-minute' sorting.[7]

Geographic regression discontinuity. As a third line of defense, we implement a geographic regression-discontinuity (GRD) design that compares individuals living in unexposed counties to individuals in exposed counties within 100 km of the signal boundary. The narrow geographic bandwidth absorbs unobserved spatial gradients (e.g., proximity to the West, industrial structures, agglomeration patterns) that might otherwise correlate with both signal access and educational opportunity. The GRD is our preferred robustness check; we further demonstrate that the estimates are stable across bandwidths from 50 to 150 km in Figure B.1.

5 Western Television and Educational Attainment

Table III presents our baseline estimates of the effect of West German television exposure on educational attainment. We restrict our sample to 4,179 East German residents who completed their formal schooling prior to reunification. Columns (1)–(3) report average marginal effects from probit models for the probability of completing post-primary education, while columns (4)–(6) report OLS estimates for completed years of schooling. All specifications include district fixed effects. Specifically, columns (1) and (4) introduce the baseline treatment effect without additional covariates; columns (2) and (5) add the full vector of individual controls; and columns (3) and (6) impose the 100 km GRD bandwidth.

Consistent with prediction P1 (Section 3), exposure depresses educational investment at both margins. In our preferred specification (column 2), TV exposure reduces the likelihood of completing post-primary education by 10.7 percentage points, a 17.7 percent decline relative to the sample mean of 60.3 percent. The intensive-margin estimate (column 5) is correspondingly large: exposure reduces total schooling by 0.5 years, or roughly 4 percent of the sample mean of 12.7 years. In the GRD subsample (columns 3 and 6), the point estimates are virtually identical, confirming that broader regional heterogeneity cannot account for the deficit. The geographic stability of the estimates is reinforced

[7] Two potential sources of spatial interference merit attention: if Western TV induces out-migration from exposed counties, the control-county sample may become positively selected on regime loyalty, attenuating the estimated gap; peer effects could also transmit the legitimacy shock across county boundaries. Two features of our design limit these concerns. First, the two control regions, the Dresden districts in the southeast and the northeastern coastal districts, are geographically separated, making cross-contamination between them implausible. Second, in unreported analysis we interact the treatment indicator with a dummy for the northern half of the GDR; the interaction is statistically insignificant. If either control region were contaminated by spillovers specific to its location, the estimates would diverge across the two halves of the country; they do not.

visually in Figure B.1, which traces our coefficient of interest β across bandwidths from 50 to 150 km: the coefficients are flat and confidence intervals consistently exclude zero.

These findings contrast with the canonical media-on-education effect, in which information access raises human capital by lowering frictions or supplying aspirational role models. In the language of Section 3, those mechanisms operate through the productive wage w; in the GDR, exposure instead operates through θ and ψ, eroding the perceived legitimacy of the political premium R and raising the cost of credibly signaling conformity. Because GDR credentials primarily signaled political loyalty rather than productive capability, marginal students rationally withdrew from a system whose returns were contingent on regime survival. Section 6 provides direct evidence on these channels. We first verify that the baseline estimates are not driven by spurious regional, demographic, or modeling artifacts.

Cohorts unexposed during formative years. If the estimated effect reflects exposure during schooling years, we should observe no impact on cohorts who completed their education before West German television could have reached them. Table B.4 confirms this falsification using two placebo cohorts.[8] The first comprises individuals who finished schooling before the GDR itself was founded; the second, the group who finished before West German public broadcasting (ARD) began transmitting across the inner-German border in the mid-1950s. For both placebo cohorts and for both outcomes (post-primary attainment and years of schooling), the estimated coefficient on signal access is small and statistically indistinguishable from zero. Because these cohorts inhabited the same treated and control districts as our main sample but could not have been exposed during their schooling years, the contrast with the baseline rules out time-invariant regional confounders that would affect exposed and unexposed cohorts symmetrically.

Spatial confounding and physical spillovers. A concern in spatial designs within divided Germany is the threat of two-way physical spillovers. Counties closer to the inner-German border may have experienced greater cross-border smuggling, informal interaction, or compensatory regime investment. To address this, we exploit the highly non-linear nature of our treatment variable: because Longley–Rice signal propagation depends on idiosyncratic topography (mountains, valleys, transmitter geometry), reception is sharply decoupled from simple linear distance to the border. Table B.3 introduces, one at a time, a continuous control for log distance to the inner-German border, individual religious affiliation, parental educational attainment, and an indicator for having social ties in West Germany. The television coefficient is stable across every permuta-

[8] Appendix B.7 further shows the effect does not vary detectably with the length of the potential exposure window.

tion. Distance to the border enters with a small positive and weakly significant coefficient that disappears within the signal-covered region, where idiosyncratic terrain rather than border proximity determines which counties receive a usable signal; West German social ties enter insignificantly. The GRD design, complemented by the bandwidth-sensitivity analysis in Figure B.1, reinforces the conclusion that the estimate captures the reception discontinuity rather than a smooth gradient in proximity to the West.

Sample composition and selective migration. Table B.2 sequentially restricts the estimation sample to address concerns about boundary-specific dynamics and selective mobility. We drop, in turn: (i) respondents in counties directly bordering West Germany; (ii) Berlin and its surrounding commuter counties; (iii) individuals who report having migrated to West Germany after reunification; and (iv) individuals who changed residence in the two years before the 1990 interview. The estimated television coefficient is stable across all four exclusions, each within a fraction of a standard error of the baseline. The results are therefore not an artifact of border counties, of Berlin, or of selective post-reunification migration.

Reweighting, functional form, and treatment definition. Although covariates are well balanced, Table B.5 re-estimates the baseline under reweighting and alternative functional forms. Entropy balancing and coarsened exact matching, which explicitly equalize the covariate distribution between treatment and control, leave the estimates essentially unchanged. The conclusions are likewise invariant to functional form (e.g., a logit for the binary outcome and a generalized-Poisson model for years of schooling). Tables B.6 and B.7 exploit the ordinal structure of educational data via ordered-probit models on the condensed and full CASMIN classifications and on alternative GDR-specific and ISCED scales; across all of them, exposure raises the probability of terminating schooling at the lowest levels and lowers the probability of attaining higher-secondary or tertiary credentials. Finally, Figure B.2 re-estimates the baseline under alternative signal-strength cutoffs (-86.5, -82.5, and -80.0 dBm): the penalty is negative throughout and statistically significant at the two lower thresholds, attenuating only under the most restrictive -80.0 dBm definition, which reclassifies marginal-reception counties as untreated and so attenuates the estimate toward zero.[9]

[9] Appendix B.7 examines whether the effect varies by gender. The interaction of signal access with an indicator for being female is statistically insignificant for both outcomes (Table B.8), so the schooling response is symmetric across men and women. This symmetry is consistent with demand-side withdrawal rather than gender-specific supply-side screening, under which the penalty would concentrate among the group facing stiffer loyalty requirements for admission to the academic upper-secondary track.

6 Mechanism: Institutional Alienation

Why did exposure to Western media reduce human capital investment? The framework of Section 3 identifies two channels: a fall in the perceived legitimacy of the regime ($\theta_i \downarrow$) and a rise in the moral cost of ideological compliance ($\psi_i \uparrow$). We establish the mechanism in three steps. First, using a confidential survey fielded months before the Wall fell, we show that Western TV eroded trust in regime institutions and weakened political identification with the state. Second, we corroborate the survey findings with an independent behavioral measure: the volume and content of formal complaint letters that GDR citizens filed with state authorities. Third, we rule out four alternative mechanisms: cognitive depreciation, leisure substitution, preference shifts, and economic anxiety. Together, the evidence points to a single operative cause: institutional alienation.

6.1 Trust, confidence, and political affiliation

To identify the legitimacy channel, we estimate two-stage least squares (2SLS) models that instrument self-reported West German television consumption with topographic signal reception.[10] Table IV reports first- and second-stage estimates for twelve attitudinal outcomes, grouped into three panels. The instrument is strong throughout: the first-stage coefficient on signal reception is about 0.77, and the first-stage F statistic peaks at 292, far above conventional weak-instrument thresholds.

To document the informational environment systematically, we assembled a unique dataset on the programming of the two national West German and the two national GDR television channels from two historical online television archives (see Appendix D). The content analysis provides direct evidence on the informational environment underlying these results. West German television not only devoted substantially more airtime to news and political magazine formats than GDR television, but also reported regularly on political, economic, and social developments within the GDR. As a result, viewers were repeatedly confronted with alternative accounts of the same events that were simultaneously portrayed in schools and through state-controlled media. This made inconsistencies between official narratives and independent reporting more salient, providing a plausible explanation for a decline in trust in state information and institutions. At the same time,

[10] The mechanism estimates draw on the ZIJ survey (GESIS, ZA6070 1989), independent of the SOEP used for the schooling results; convergence of attitudinal evidence from one source and behavioral outcomes from the other provides independent corroboration of the channel. The exclusion restriction requires that topographic signal reception affect political attitudes only through television viewing. The main threat is that terrain features correlating with signal strength may also proxy for proximity to the West German border, which could shape attitudes through cross-border contact or informal interaction independently of television. This concern is mitigated by the evidence in Section 5.

Western entertainment programming exposed viewers to alternative lifestyles and value systems, increasing the psychological cost of ideological conformity.

Trust in regime policies. Panel A of Table IV measures trust in four substantive policy domains. Watching West German television reduces the probability of expressing trust in state-provided information, in domestic policy, in cultural policy, and in science and technology policy. Because trust in these domains is far from universal to begin with, these are economically large effects. They imply that exposure erodes the belief in the quality and reliability of state institutions, depressing the expected value of state-mediated rewards ($\theta_i \downarrow$).

Confidence in the socialist system. Panel B extends the legitimacy channel from current policy to the system's prospects. Exposure reduces confidence in the GDR's economic development, in socialist democracy, in the future of the consumer-goods industry, and in scientific and technological development. Western broadcasting thus did more than register dissatisfaction with the present: it undercut the belief that the socialist project would deliver in the future, precisely the belief on which the return to a regime-issued credential ($\theta_i \downarrow$) depended.

Identification, party affiliation, and emigration intent. Panel C shows that this loss of trust translated into behavioral detachment from the state. Exposure weakens identification with the GDR and alignment with its mode of governance, lowers membership in the ruling SED, and raises the intention to emigrate. These results document the second channel: as Western norms are absorbed, the moral and psychological cost of outward socialist conformity ($\psi_i \uparrow$) rises to a level that no longer justifies continued investment in the state system.

Withdrawal of demand vs. denial of access. Two mechanisms could generate the observed decline in schooling: a *demand-side* channel, in which students rationally choose less education because the state-mediated return to credentials no longer justifies the cost of ideological compliance; and a *supply-side* channel, in which students who fail to signal sufficient regime loyalty are denied access to higher tracks. Both are consistent with our reduced-form estimates.

We argue the demand-side channel dominates. The supply-side interpretation predicts that the state screens out ideologically unreliable students, reducing their FDJ participation. Table V shows the opposite: exposure leaves unchanged the probability of holding any of six FDJ leadership positions, from group leader to committee member to club chair, yet significantly reduces felt identification with the organization. Exposed individ-

uals continued to participate formally in the very institution through which the regime rationed access to elite educational tracks; what fell was not their presence but their belief. This is the signature of disengagement, not exclusion: individuals rationally maintained their formal standing while privately withdrawing their ideological commitment.

6.2 Corroborating evidence: citizen complaints

We corroborate the attitudinal evidence using formal complaint letters (*Eingaben*), one of the few officially sanctioned outlets through which GDR citizens could register grievances with state authorities.[11] Unlike survey responses, complaint letters are a behaviorally revealed, population-level measure: writing one entailed a cost and could not easily be concealed from authorities.

The legitimacy channel predicts that exposed counties should generate more complaints, specifically about the institutions through which the regime mediated educational rewards. Table VI regresses the county-level flow of complaints over 1980–1988 on West German signal reception, conditioning on county-level characteristics and year fixed effects. The predictions are confirmed. Exposed counties filed significantly more complaints about *public education* and on *youth affairs*. On the other hand, exposure leaves the *total* volume of complaints essentially unchanged (column 1): exposed and unexposed counties complain at statistically indistinguishable overall rates. The increase instead reflects a shift in *composition*, with public education and youth affairs absorbing a significantly larger share of all complaints in exposed counties (columns 3 and 5). The fact that the increase is specific to education and youth organizations, and not to other policy domains, provides strong out-of-survey corroboration of the legitimacy channel: Western television eroded confidence in the precise institutions that mediated the political return to schooling.

6.3 Ruling out alternative explanations

If, instead, television depressed human capital by crowding out study time or by impairing cognitive development, we should observe broad cognitive deficits and meaningful shifts in time use. Figure II shows that we do not.

Cognitive ability and personality. Figure IIa reports estimates for performance on the Symbol–Digit and Animal-Naming tests at the 30-, 60-, and 90-second marks, together with the Big Five personality traits. The cognitive coefficients are tightly clustered around

[11] Their content has been digitized and classified by topic by Albrecht et al. (2020).

zero with confidence intervals that exclude meaningful effects, ruling out a human-capital-depreciation story. Among the personality dimensions, only neuroticism shows a small, at most marginally significant increase, which we read not as a separate behavioral channel but as an empirical trace of $\psi_i \uparrow$: the psychological strain of navigating contradictory demands from a collectivist school system and an increasingly individualistic Western reference culture. All other traits are estimated at zero.

Preferences, leisure, and economic expectations. Figure IIb plots estimates for economic preferences (risk tolerance, patience, and altruism), leisure behaviors (socializing with friends, going to the cinema, attending cultural events, and practicing sports), and economic expectations (optimism about one's own future and the perceived risk of job loss, career deterioration, and a forced change of occupation). Not a single coefficient is statistically significant. Two implications follow. First, the time-displacement hypothesis finds no support: exposed individuals show no change in time allocated to socializing, cultural activities, or sport, ruling out any broad reallocation of time toward television. Second, the result rules out an economic-anxiety mechanism: Western broadcasts did not heighten labor-market insecurity in a way that would lower the expected productive wage w and depress schooling for reasons unrelated to legitimacy. The decline in human-capital investment is therefore attributable to institutional alienation ($\theta_i \downarrow$ and $\psi_i \uparrow$) rather than to cognition, preferences, time use, or economic fear.[12]

7 Long-Run Labor Market Consequences

The mechanism evidence establishes that Western television lowered θ_i (regime legitimacy) and raised ψ_i (the cost of ideological compliance) without shifting cognition or fundamental economic preferences: exposed individuals withdrew from state-mediated schooling for institutional, not productive, reasons. This motivates the test presented in this section. If GDR schooling primarily built productive skills, exposed cohorts should face persistent labor-market penalties once credentials are priced by the market. If instead GDR credentials primarily signaled political loyalty, the regime's collapse should rapidly close those gaps even as the credential shortfall persists, since the post-reunification labor market priced on productive rather than political value. We use the longitudinal

[12] An alternative interpretation is that Western television informed students about West German opportunities, inducing credential disinvestment in anticipation of emigration. This outside-option channel is observationally equivalent in the main schooling equation. Two features weigh against it as the dominant mechanism: exposure erodes trust in the GDR's own institutions and the socialist system's future, beliefs that are distinct from information about external alternatives; and occupational sorting away from state-dominated sectors persists among stayers after reunification (as discussed in the Section 7), consistent with legitimacy erosion but not with emigration anticipation. We treat the two channels as complements, with institutional alienation as the primary.

SOEP to trace trajectories in five-year windows from 1990 through 2010 and gauge the relative magnitudes of the productive (w) and political (R) components.[13] We bring two complementary research designs to bear. The first traces the television gap across the post-reunification transition in four sets of outcomes—full-time employment and occupational sorting, household income, and the incidence and intensity of educational mismatch (Figure III). The second exploits a cohort-based extension of compulsory schooling in 1959 that raised GDR education for reasons unrelated to television, delivering an independent estimate of its productive return (Figure IV).

Employment and occupational sorting. Figure IIIa overlays the effect of TV exposure on full-time employment and on holding a state-dominated occupation, each estimated in five-year windows. In 1990, the final year of the socialist labor-allocation system, exposure carries a distinct penalty: exposed individuals are 4.5 percentage points less likely to be employed full-time. This is consistent with their exclusion from state-allocated premium positions, which the regime administratively reserved for those who had demonstrated loyalty through formal educational and political channels. The post-reunification dynamics, however, reveal the underlying nature of the deficit. Following the transition to a market economy, the employment gap closes rapidly: by the mid-1990s exposed and unexposed individuals are statistically indistinguishable, and the point estimates are essentially zero (indeed slightly positive) through 2010. We read this as evidence that the marginal year of GDR schooling that students chose to forgo carried a near-zero productive return ($w \approx 0$): the pre-reunification penalty reflected the loss of state-mediated rents, not a deficiency in transferable skills. Once the state apparatus collapsed and the labor market stopped rewarding ideological loyalty, the penalty for the forgone education fell to zero.

Labor-market convergence does not, however, imply behavioral convergence. The state-dominated-occupation series in the same panel traces the probability of working in formerly state-dominated occupations: military and security services; political administration and public service, including the party and mass-organization apparatus; education; law, social sciences, and social services; media, culture, and information; and religious occupations. Table A.2 reports the full list of ISCO-88 occupation codes we classify as state-dominated. In 1990, exposed individuals are about 5 percentage points less likely to hold these positions. Striking is the persistence of the gap: it remains negative

[13] To ensure that the findings reported in Figure III are not driven by selective panel attrition, we repeat all specifications weighting each observation by the inverse of the number of periods the individual appears in the data. This balances the panel artificially so that every individual carries equal total weight in the estimation. Employing these weights, does not change our the results. In addition, following the robustness strategy reported in Table B.2, we exclude individuals who migrated from East to West Germany after reunification. This alternative sample restriction yields virtually identical results.

and statistically significant through the early 2000s and narrows only gradually thereafter, so that exposed cohorts continue to sort *away* from historically state-dominated sectors for roughly a decade after reunification, even as their employment converges. This sustained occupational divergence reveals that the cultural realignment induced by Western television outlived the institutions it originally targeted.

Income. Figure IIIb repeats the exercise for the natural logarithm of net household income, and the trajectory tracks the employment result. In 1990, under the socialist allocation system, exposed individuals lived in households with about 7 log points (roughly 7 percent) lower net income, consistent with their exclusion from the better-remunerated positions the state reserved for credentialed loyalists. The gap then narrows steadily over the post-reunification period, to about 4 log points by 2006–2010. The point estimates remain negative throughout but are imprecisely estimated, with confidence intervals that include zero once the labor market shifts from political to productive pricing. That the income penalty is concentrated in the socialist era and fades thereafter reinforces the central reading: what the forgone schooling priced was access to state-mediated rents, not transferable earnings capacity.

Educational mismatch and overeducation. If the marginal year of GDR schooling built little productive human capital, the credentials exposed students declined to acquire should have amounted, in effect, to excess education—schooling beyond what their eventual occupations required. Figures IIIc and IIId test this directly using a standard realized-matches measure: within each three-digit occupation and survey year, a worker is classified as overeducated, matched, or undereducated according to whether years of schooling exceed, lie within, or fall short of one standard deviation of the occupational mean (Appendix A.2). Figure IIIc reports the likelihood of mismatch and, separately, of overeducation; Figure IIId reports their intensity in years. Notably, because the estimates for overall mismatch and overeducation track each other almost perfectly, there is no evidence of undereducation; the mismatch is driven entirely by overeducation. Exposed individuals are markedly *less* likely to be mismatched or overeducated—by about 9 and 7 percentage points, respectively, in 1990—and accumulate roughly 0.1 fewer years of each. As with employment and income, the gaps are largest at reunification and attenuate over the following two decades. The interpretation is the mirror image of the schooling result: the extra education that unexposed cohorts obtained translated disproportionately into credentials their jobs did not draw on, so that opting out spared exposed students precisely the over-credentialing that carried no productive return. This is the microeconomic counterpart of $w \approx 0$; the forgone year was excess education, not foregone skill.

Decomposing the return to GDR education. Together, these post-reunification dynamics permit a decomposition of the marginal return to GDR schooling. Reunification effectively forced $R = 0$ *ex post* by abolishing the institutions that distributed regime rents. The rapid employment convergence of exposed cohorts, despite their lower credential stock, implies that $w \approx 0$ for the marginal year of education they declined to acquire. The income and mismatch results point the same way: the earnings penalty is concentrated under socialism and fades once pricing turns productive, and the schooling exposed students forwent would have manifested as over-credentialing rather than as scarce, remunerated skill. The full return to that marginal year was therefore, approximately, the political premium R.

Independent evidence from the 1959 schooling reform. The decomposition above rests on the television experiment, which recovers the return to the particular marginal year that exposed students chose to forgo. One might worry that this margin is special. We therefore corroborate $w \approx 0$ with a second, unrelated source of variation: the GDR's 1959 extension of compulsory schooling, which introduced the ten-year *Polytechnische Oberschule* for cohorts born from 1944 onward. Figure IV presents regression-discontinuity plots in birth year around the 1943–1944 cutoff. The first stage is large: the share completing 10 or more years of schooling rises by about 25 percentage points (Figure IVa) and total schooling by roughly two-thirds of a year (Figure IVb) at the threshold. Yet no comparable discontinuity appears in economic outcomes: neither full-time employment (Figure IVc) nor log household income (Figure IVd) shifts discernibly at the cutoff. Instrumenting attainment with reform exposure makes this explicit (Table B.10). Because the reform raised schooling for administrative reasons unrelated to television, signal reception, or individual demand, it identifies the productive return for a different population—cohorts mechanically induced to acquire more education—and reaches the same verdict. Two designs, exploiting entirely different variation, converge on a near-zero productive return to the marginal year of GDR schooling.[14]

This decomposition carries a concrete economic implication. The GDR education system extracted substantial costs $C(c_i, \psi_i)$ from students in the form of time, ideological conformity, and the psychological strain documented in Section 6, to produce credentials that generated essentially no transferable economic value at the margin. The reduction in schooling induced by Western media therefore represents savings in ideological rent-seeking rather than lost productive capital. A back-of-the-envelope calculation gives a

[14] The reform design carries less power than the television experiment: first-stage F-statistics are on the order of 10–12 in the five-year bandwidth, and the employment panel exhibits a pre-reform birth-year trend. We therefore read Figure IV as corroborating, rather than independently establishing, $w \approx 0$. The income discontinuity, where the pre-reform trend is flat and the first stage strong, is the cleanest of the four panels.

sense of scale. Signal coverage reached approximately 85 percent of each annual secondary-school cohort of roughly 238,000 students (Statistical Yearbook of the German Democratic Republic 1990). At a treatment effect of half a year per exposed student, and over the approximately 30 cohorts schooled during the era of West German broadcasting, the marginal politically-motivated component alone amounts to roughly 3 million student-years.[15] The regime diverted on the order of 3 million student-years from productive activity into politically motivated signaling, and this counts only the margin our design identifies.

For the broader political economy of authoritarianism, these estimates document a response to information penetration that is quieter than the standard story of media-induced revolt: rational, individual withdrawal from a signaling equilibrium whose aggregate efficiency cost was large but largely invisible. Coupling human capital investment to political loyalty distorts educational sorting and diverts student effort into non-transferable signaling; opening the information environment reveals that distortion and prompts marginal citizens to opt out. For modern autocracies, the implication is that ideological censorship serves an economic function alongside its political one, sustaining the rents that make state-mediated credentials valuable. When those barriers erode, the behavioral response can register in human capital decisions long before it appears in political mobilization.

Three features of the GDR setting may bound the generalizability of these estimates. Cultural and linguistic proximity to West Germany maximized the dissonance between domestic ideology and foreign content; where the information alternative is more culturally distant, the legitimacy shock is likely smaller. The GDR's sudden and publicly observable collapse made the terminal value of credentials common knowledge nearly simultaneously for all agents; in autocracies where collapse is gradual, the anticipated political premium may fall less sharply. And the near-complete coupling of credentials to state employment left almost no private-sector alternative; where private labor markets coexist with state employment, the productive return w is less compressed and the schooling response correspondingly attenuated. These three features delimit rather than disqualify the mechanism. In contemporary autocracies where tight credential-employment coupling persists and private-sector alternatives remain limited, modern information channels, including satellite television, VPN access, and social media, may generate similar dynamics.

[15] This calculation multiplies 238,000 students per cohort $\times$ 0.85 (coverage rate) $\times$ 0.5 years (treatment effect) $\times$ 30 cohorts (1960–1989) $\approx$ 3.03 million student-years. This is a lower bound: it counts only the marginal year identified by the natural experiment for the exposed sub-population, not the full political component of GDR credentials, which applied to all students regardless of signal access.

8 Conclusion

In authoritarian regimes, education is as much an instrument of political control as it is a productive technology. Beyond building human capital, schools socialize citizens, screen for regime loyalty, and gatekeep access to state-allocated rents. When external information undermines regime legitimacy, it fundamentally alters this investment calculus. Citizens confront a margin of choice absent from standard human-capital models: whether to keep paying ideological costs for credentials whose value depends on a regime they no longer believe in.

This paper studies that choice. Quasi-random variation in West German television reception within the GDR generated a cultural shock that exposed students to a normative framework directly at odds with their domestic educational system. Using individual-level data, we show that exposed individuals completed 0.5 fewer years of schooling and were 10.7 percentage points less likely to attain post-primary credentials. We trace this decline to a collapse in perceived regime legitimacy and a rise in the cost of ideological compliance, corroborated by attitudinal survey data and administrative complaint records, with cognition, time use, risk preferences, patience, and economic anxiety ruled out as alternatives. After reunification stripped political rents from the labor market, the labor market penalty associated with forgone education collapsed to zero, while sorting away from state-dominated occupations persisted for roughly a decade. An independent regression-discontinuity design exploiting the 1959 compulsory-schooling reform reinforces this reading: schooling raised by policy likewise yielded no detectable gain in earnings or employment. The marginal year of GDR schooling that exposed students chose not to acquire embodied a political signaling premium with no transferable productive return.

The findings carry implications that extend well beyond divided Germany. Modern autocracies invest in digital firewalls not merely to suppress dissent but to preserve the political premium on regime-sanctioned credentials. Our results show that when those barriers erode, the behavioral response need not be revolutionary. It can be quiet, individual, and rational: a half-year reduction in schooling per affected student that, aggregated over millions of young people, amounts to a substantial reallocation of human effort away from productive activity. Three questions follow for future research. How does media-induced institutional alienation shape long-run political participation and democratic consolidation after a regime collapses? Do cultural shocks of this kind transmit intergenerationally, altering the human capital choices of exposed cohorts' children? And how do modern autocracies adapt their censorship architectures and educational curricula in response to the erosion of information monopoly? These questions matter because the mechanism documented here, the political pricing of human capital, is not specific to divided Germany: wherever the state is the dominant employer and ideological confor-

mity gates occupational advancement, foreign information has the potential to reallocate investment across the productive and political margins of schooling.

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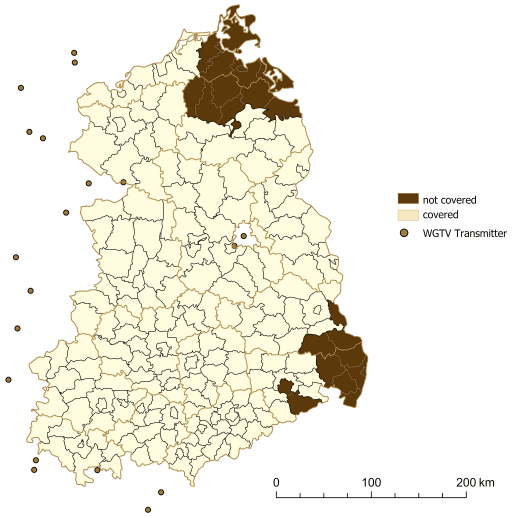
Figure I: West German television signal strength and transmitters

(a) Signal Strength Map

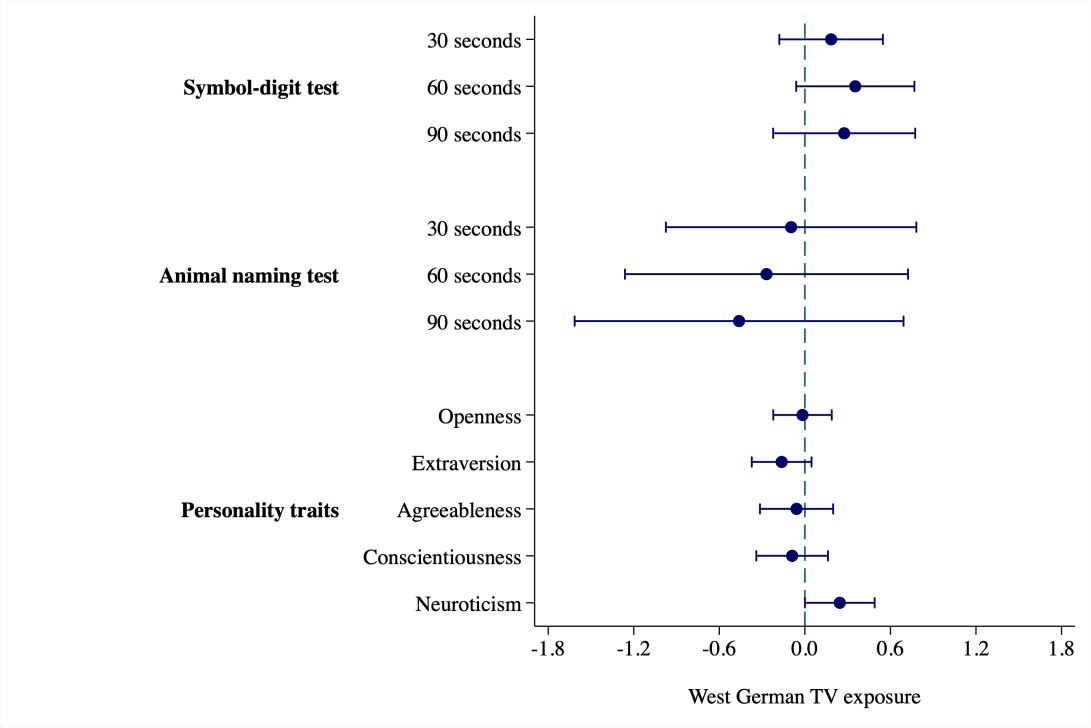


(b) Transmitter Locations

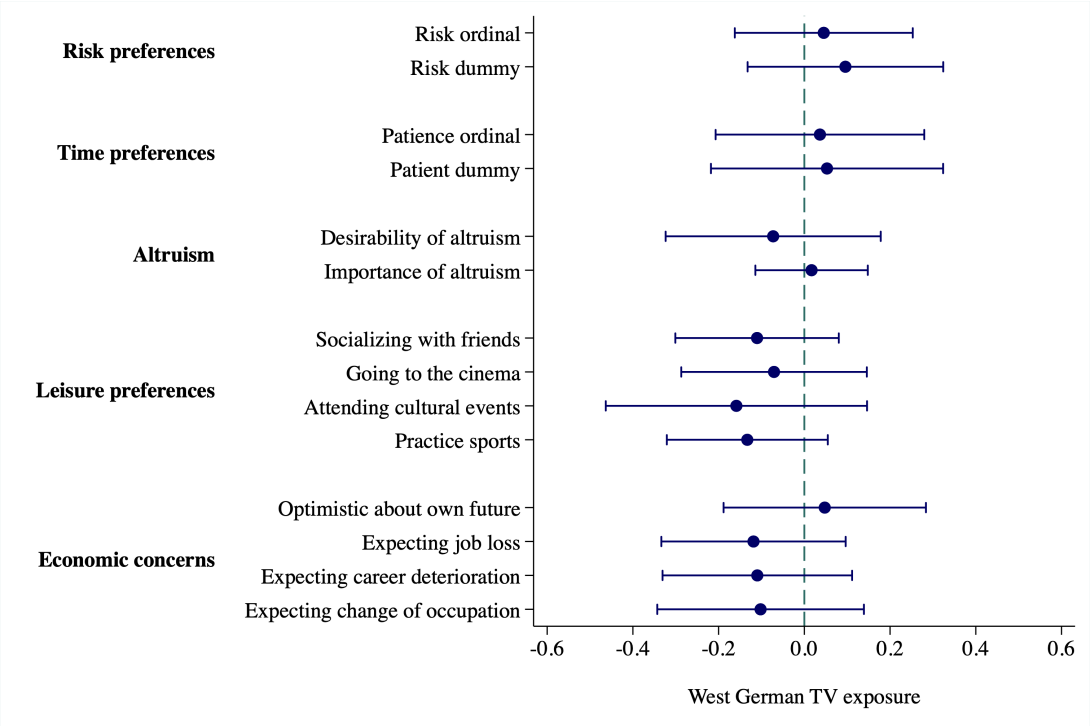
Notes: Figure Ia illustrates the continuous West German TV signal strength across pre-reunification GDR counties calculated via Longley–Rice propagation models. Darker counties represent the control area with no reception below the −86.5 dBm cutoff (25 counties), and lighter counties represent the treatment area with sufficient signal strength (192 counties). County boundaries are shown as gray lines. Figure Ib identifies the geographic placement of the primary West German broadcast transmitters along the inner-German border. Data sources: Crabtree, Darmofal, and Kern (2015), Bursztyn and Cantoni (2016), and authors' calculations.

Figure II: Ruling out alternative mechanisms

(a) Traits and Abilities

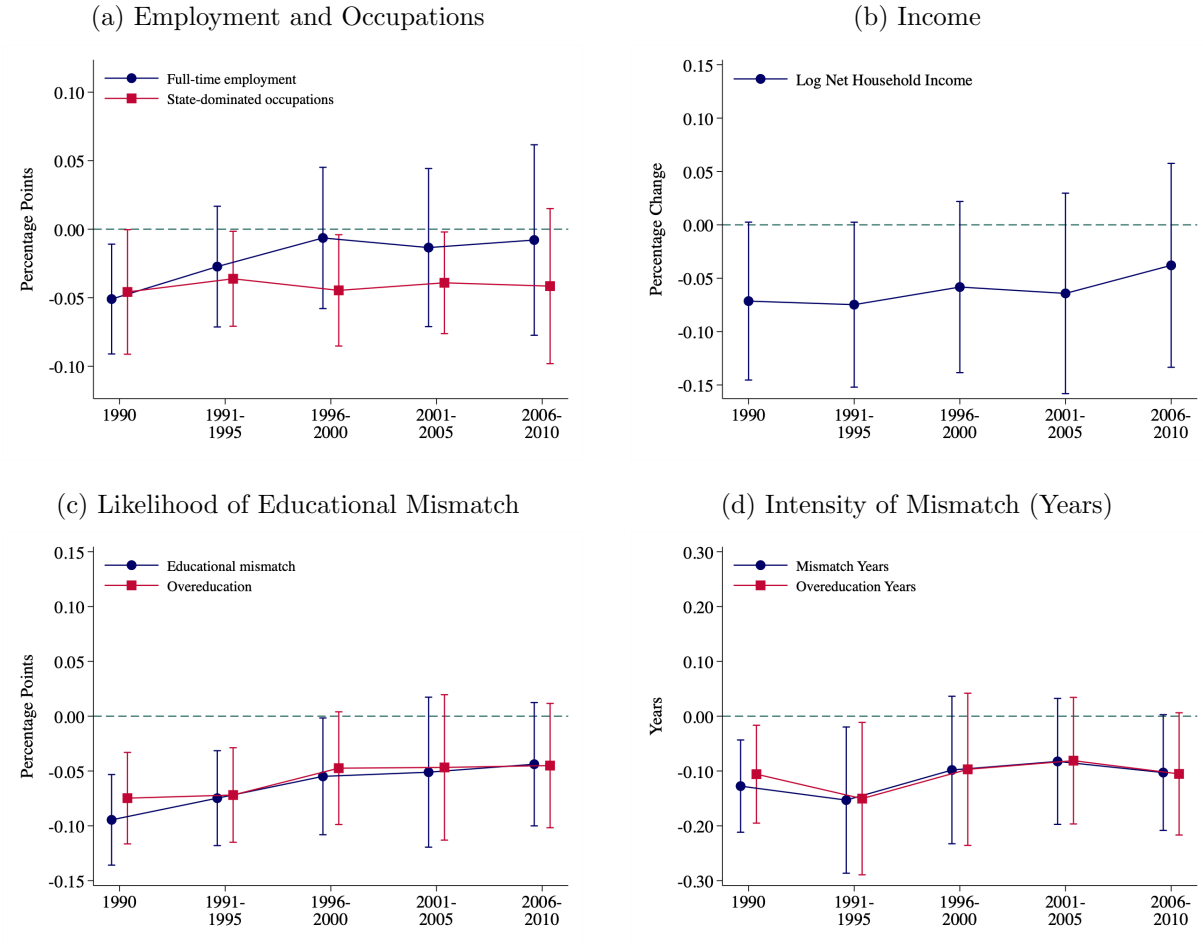


(b) Preferences, Behaviors, and Expectations



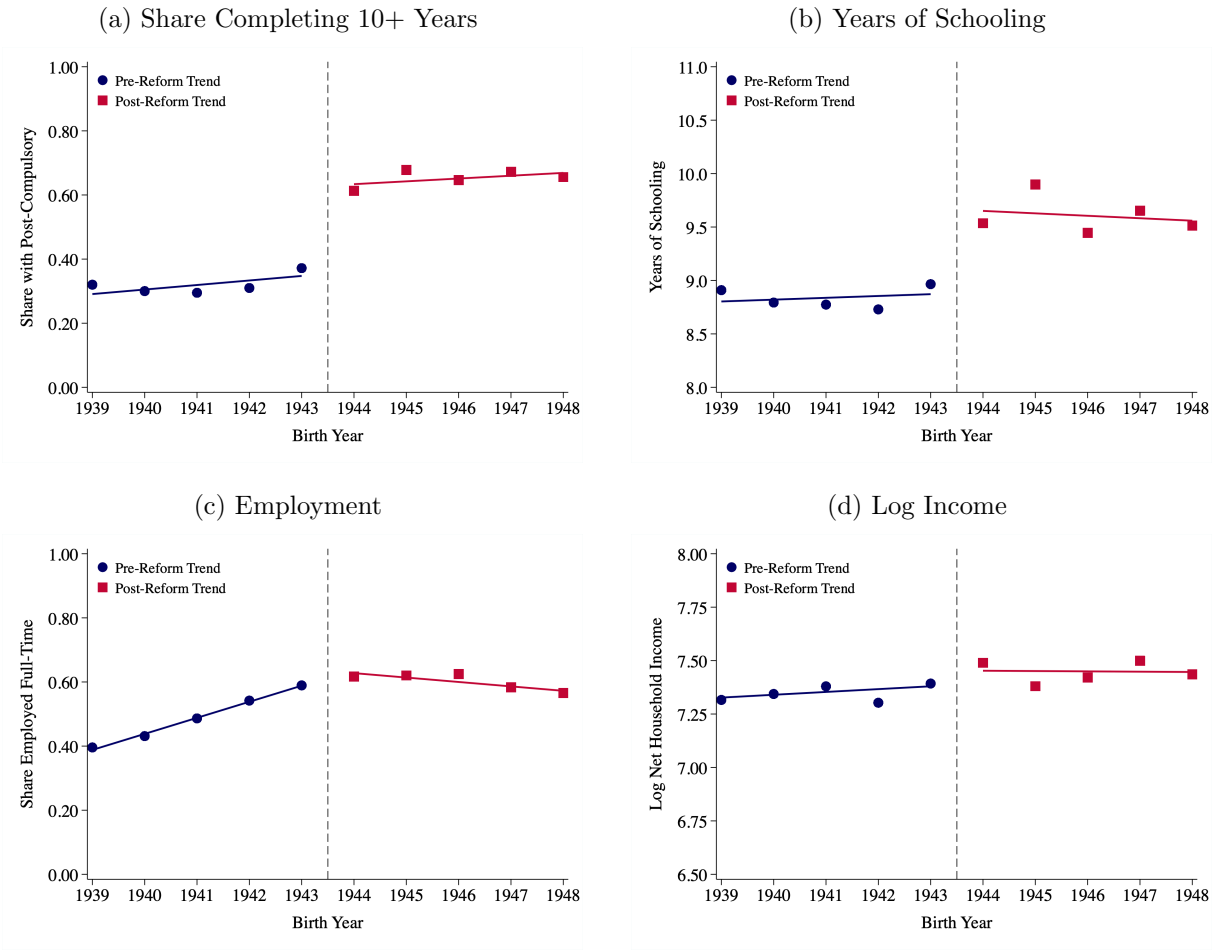
Notes: All figures show results from OLS regressions. The confidence intervals shown are set at the 95% level of statistical significance. All dependent variables are standardized. Additional controls include gender, age, age^2, single, household size, and migration background. In addition, we include district fixed effects. Standard errors are clustered at the county level. Data source: SOEP (2025).

Figure III: Post-Reunification Gaps in Employment, Income, and Educational Mismatch



Notes: Figures IIIa to IIId show average marginal effects over time from OLS regressions. The dependent variables in figure IIIa are dummy variables for full-time employment and employment in a state-dominated occupation. In figure IIIb, the dependent variable is the natural logarithm of net household income. In figure IIIc, the dependent variables are dummy variables equal to one if the respondent experiences educational mismatch or overeducation. The dependent variables in figure IIId measure a respondent's absolute number of mismatch years and overeducation years. Controls include gender, age, age^2, single, household size, and migration background; district fixed effects are included in all specifications. Standard errors are clustered at the county level. The error bars represent 90% confidence intervals. Data source: SOEP (2025), survey years 1990–2010.

Figure IV: Regression Discontinuity Estimates of the GDR Schooling Reform



Notes: The figure displays regression discontinuity plots estimating the impact of the GDR schooling reform. The running variable is the respondent's birth year, with the vertical dashed line indicating the policy cutoff between the 1943 and 1944 birth cohorts. Scatter points represent cohort averages. Solid lines represent linear trends fitted separately for the pre-reform (1939–1943) and post-reform (1944–1948) cohorts using underlying microdata. The dependent variables are an indicator for completing 10 or more years of schooling in figure IVa, total years of schooling in figure IVb, an indicator for full-time employment in figure IVc, and the natural logarithm of net household income in figure IVd. The sample is restricted to working-age individuals (age $\leq$ 65). Corresponding two-stage least-squares estimates, which instrument educational attainment with reform exposure, are reported in Table B.10. Data source: SOEP (2025).

Table I: West German TV signal and viewership

	Viewing Frequency		Weekly Viewership	
	(1)	(2)	(3)	(4)
West German TV signal	2.812***	2.803***	0.770***	0.769***
	(0.049)	(0.049)	(0.015)	(0.015)
Controls		✓		✓
Mean Y	3.929	3.929	0.784	0.784
R^2	0.602	0.606	0.499	0.502
Observations	3416	3416	3416	3416

Notes: Columns (1) and (2) show results from OLS regressions, while columns (3) and (4) report average marginal effects from probit models. The dependent variable in columns (1) and (2) is an ordinal variable indicating how regularly a respondent watched West German TV on the following scale: (1) *never*, (2) *rarely*, (3) *once a week*, (4) *several times a week*, and (5) *daily*. In columns (3) and (4), the dependent variable is a dummy variable equal to one if a respondent watched West German TV at least once a week. Controls include gender, age, age^2, and single. Heteroskedasticity-robust standard errors are shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: GESIS, ZA6008 (1989).

Table II: Summary statistics

	All Observations					West German TV		(6) – (7)
	Mean	S.D.	Min	Max	N	With	Without	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Overall Sample								
Above Primary	0.603	0.489	0.00	1.00	4179	0.595	0.659	−0.064***
Years	12.695	2.245	8.00	16.00	4179	12.665	12.896	−0.231**
Female	0.528	0.499	0.00	1.00	4179	0.527	0.537	−0.010
Age	43.167	15.828	17.00	95.00	4179	43.249	42.603	0.647
Single	0.145	0.352	0.00	1.00	4179	0.146	0.137	0.009
Household size	3.066	1.171	1.00	8.00	4179	3.050	3.175	−0.126**
Migration	0.034	0.181	0.00	1.00	4179	0.034	0.034	−0.000
Panel B: Geographic Regression Discontinuity Sample								
Above Primary	0.609	0.488	0.00	1.00	2709	0.596	0.659	−0.063***
Years	12.736	2.263	8.00	16.00	2709	12.697	12.896	−0.199*
Female	0.527	0.499	0.00	1.00	2709	0.524	0.537	−0.012
Age	43.221	15.661	17.00	95.00	2709	43.371	42.603	0.769
Single	0.153	0.360	0.00	1.00	2709	0.157	0.137	0.020
Household size	3.043	1.153	1.00	7.00	2709	3.011	3.175	−0.164***
Migration	0.033	0.178	0.00	1.00	2709	0.033	0.034	−0.001

Notes: This table shows summary statistics of the main estimation sample. Columns (1)–(5) show statistics for the whole sample, column (6) for individuals with West German TV reception, and column (7) for individuals without West German TV reception. Column (8) shows the difference in means (column 6 minus column 7). Panel A covers the full estimation sample; Panel B restricts attention to the 100 km geographic regression-discontinuity (GRD) subsample. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table III: The effect of West German TV exposure on educational attainment

	Educational Attainment					
	Above Primary			Years		
	(1)	(2)	(3)	(4)	(5)	(6)
West German TV	-0.133***	-0.107***	-0.108***	-0.504**	-0.489**	-0.493**
	(0.047)	(0.036)	(0.037)	(0.253)	(0.230)	(0.229)
Controls		✓	✓		✓	✓
GRD design			✓			✓
Mean Y	0.603	0.603	0.609	12.695	12.695	12.736
R^2	0.019	0.263	0.249	0.021	0.126	0.134
Observations	4179	4179	2709	4179	4179	2709

Notes: Columns (1) to (3) show average marginal effects from probit models, while columns (4) to (6) report results from OLS regressions. The dependent variable in columns (1) to (3) is a dummy variable equal to one if the respondent has at least an intermediate level of education based on the CASMIN classification (*Comparative Analysis of Social Mobility in Industrial Nations*). The dependent variable in columns (4) to (6) measures a respondent's number of years of education. In columns (3) and (6), we use a geographic regression discontinuity (GRD) design that only includes individuals who lived in counties without West German TV reception or in counties with reception within 100 km of the treatment-control border. Controls include gender, age, age^2, single, household size, and migration background; district fixed effects are included in all specifications. Standard errors are clustered at the county level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table IV: Undermining trust for the regime and moral costs

Panel A: Trust in Regime Policies				
	Information	Domestic	Cultural	Sci-Tech
	(1)	(2)	(3)	(4)
Second stage:				
West German TV	−0.056**	−0.074***	−0.062***	−0.095***
	(0.023)	(0.028)	(0.019)	(0.023)
First stage:				
Signal reception	0.770***	0.774***	0.772***	0.773***
	(0.045)	(0.045)	(0.045)	(0.045)
Mean Y	0.180	0.532	0.836	0.740
F statistic	289	292	291	290
Observations	3741	3738	3743	3731
Panel B: Confidence in Regime Development				
	Economic Development	Socialist Democracy	Consumer Goods Industry	Sci-Tech
	(5)	(6)	(7)	(8)
Second stage:				
West German TV	−0.038*	−0.079***	−0.086***	−0.081**
	(0.022)	(0.028)	(0.026)	(0.036)
First stage:				
Signal reception	0.773***	0.774***	0.772***	0.751***
	(0.045)	(0.045)	(0.045)	(0.058)
Mean Y	0.171	0.381	0.267	0.517
F statistic	292	291	291	166
Observations	3753	3738	3744	2252
Panel C: Moral costs of ideological compliance				
	Identification with GDR	Alignment with Governance	SED Member	Intent to Migrate
	(9)	(10)	(11)	(12)
Second stage:				
West German TV	−0.205***	−0.093**	−0.058**	0.104***
	(0.060)	(0.038)	(0.028)	(0.028)
First stage:				
Signal reception	0.772***	0.769***	0.771***	0.774***
	(0.045)	(0.045)	(0.045)	(0.045)
Mean Y	4.791	2.699	0.466	0.606
F statistic	290	286	287	290
Observations	3730	3722	3703	3723

Notes: The table shows results from two-stage least squares regressions using West German TV signal reception as the instrumental variable for West German TV consumption. Controls include gender, age, age^2, single, and the mother's qualification level. Standard errors are clustered at the individual level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: GESIS, ZA6070 (1989).

Table V: Engagement in the FDJ

	Holding a Position in the FDJ						
	Any Position (1)	Higher Mgmt. (2)	Group Leader (3)	Group Board (4)	Committee Member (5)	Club Leader (6)	FDJ Identity (7)
Second stage:							
West German TV	0.016	0.020	−0.000	−0.002	−0.001	0.004	−0.140*
	(0.027)	(0.014)	(0.014)	(0.026)	(0.016)	(0.015)	(0.075)
First stage:							
Signal reception	0.790***	0.785***	0.791***	0.785***	0.788***	0.793***	0.772***
	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)	(0.045)
Mean Y	0.391	0.079	0.070	0.320	0.097	0.086	3.545
F statistic	298	296	300	297	297	302	289
Observations	3655	3695	3687	3687	3676	3687	3725

Notes: The table shows results from two-stage least squares regressions using West German TV signal reception as the instrumental variable for West German TV consumption. Controls include gender, age, age^2, single, and the mother's qualification level. Standard errors are clustered at the individual level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: GESIS, ZA6070 (1989).

Table VI: Western TV and complaint letters

	Complaint letters				
	Total	Public Education		Youth Affairs	
		Number	Share	Number	Share
	(1)	(2)	(3)	(4)	(5)
West German TV	0.445	0.080**	0.752*	0.011***	0.106**
	(0.406)	(0.036)	(0.404)	(0.004)	(0.045)
Mean Y	6.633	0.205	2.656	0.034	0.460
Overall R^2	0.103	0.051	0.073	0.097	0.096
Observations	1885	1885	1885	1885	1885

Notes: Results are based on linear random-effects models. The dependent variable in column (1) is the total number of complaint letters per 100,000 inhabitants per day. In columns (2) and (4), the dependent variable is the number of complaint letters concerning public education and youth affairs, respectively, per 100,000 inhabitants per day. In columns (3) and (5), it is their share (in percent) of all complaint letters. Controls include log population density, share of females, and population size; year fixed effects are included in all specifications. Standard errors are clustered at the county level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: Albrecht et al. (2020), period: 1980–1988.

Online Appendix for

Opting Out of Autocracy: Foreign Media, Institutional Legitimacy, and State-Dependent Human Capital

Online Appendix Overview.

Appendix A describes the construction of the samples, defines every variable used in the empirical analysis, and reports descriptive material that supports the discussion in Sections 4 and 5.

Appendix B reports a comprehensive battery of robustness checks that probe the identifying assumption, the timing of the shock, spatial heterogeneity, sample composition, reweighting, and parametric assumptions.

Appendix C documents the explicitly ideological content of GDR schooling using contemporaneous textbooks, party-education materials, and visual propaganda. This evidence motivates the state-dependent human-capital interpretation of the main findings.

A Data, Variable Definitions, and Descriptive Evidence

A.1 Sample construction

SOEP sample. The main sample is built from the 1990 wave of the German Socio-Economic Panel (SOEP 2025), which surveyed East German households in June 1990, after the fall of the Berlin Wall in November 1989 but before the monetary, economic, and social union of July 1990 or any subsequent reform that could have affected schooling. We restrict attention to respondents who were resident in the GDR before reunification and for whom we observe county (*Kreis*) of residence in 1989. We further restrict to individuals who had completed their formal schooling by 1990, leaving a sample of 4,179 individuals. We assign West German television treatment at the county level using the Longley–Rice signal-propagation predictions of Crabtree, Darmofal, and Kern (2015): a county is classified as treated if the average predicted signal strength exceeds -86.5 dBm, the threshold at which standard analog reception was reliable in the late 1980s.

ZIJ sample. We also use two declassified 1989 surveys of the GDR's *Zentralinstitut für Jugendforschung* (GESIS, ZA6008 1989; GESIS, ZA6070 1989). The ZIJ collected detailed data on attitudes, political affiliation, and media consumption from a sample of approximately 4,000 East Germans. Crucially, the survey was anonymous and conducted under conditions explicitly designed to elicit truthful responses; Friedrich (1990) document the methodological precautions adopted by the ZIJ.

A.2 Variable definitions

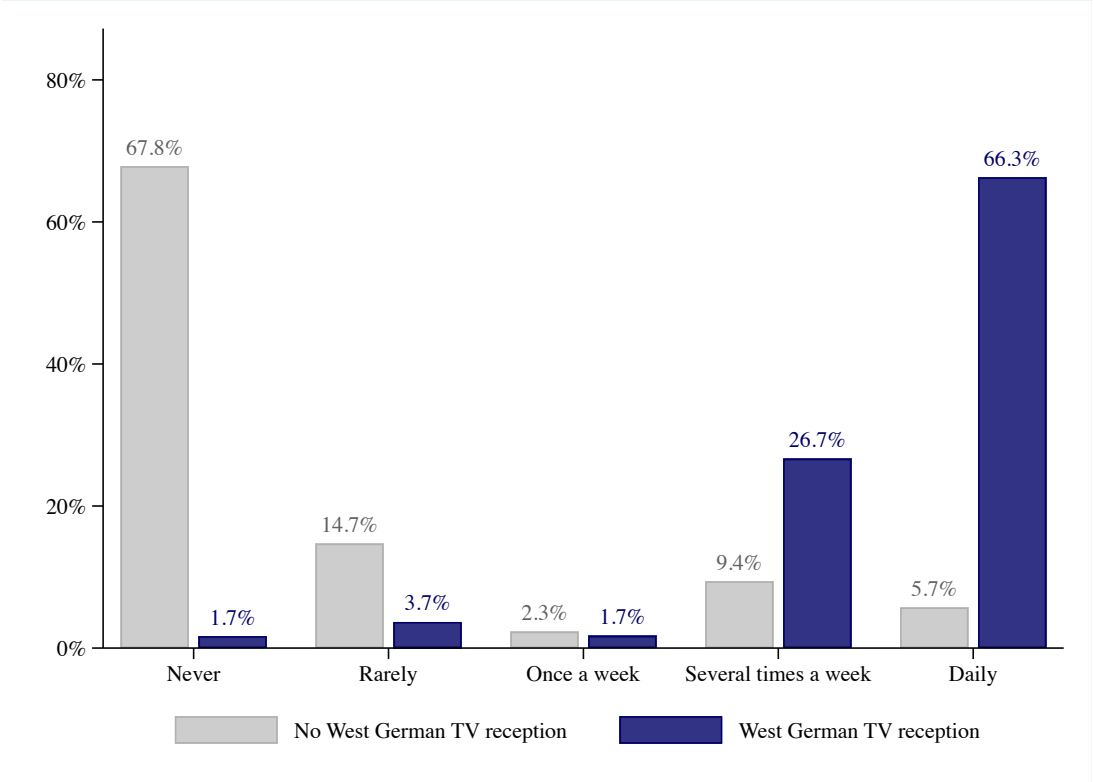
Table A.1 defines every variable used in the empirical analysis. The table is organized into eight panels: outcome variables on educational attainment (Panel A); the treatment variable (Panel B); mechanism variables capturing regime legitimacy and political affiliation (Panel C); mechanism variables ruling out alternative channels (Panel D); long-run labor-market outcomes (Panel E); individual-level controls (Panel F); district-level variables used in the balance and pre-trend tests (Panel G); and county-level complaint-letter variables used in Section 6.2 (Panel H).

Table A.2 lists the occupations we classify as 'state-dominated' in the long-run analysis of Section 7, grouped into six fields: military and security services; political administration and public service; education; law and social services; media, culture, and information; and religious occupations. These are predominantly occupations in which, under the GDR, employment and advancement were mediated by the state and its mass organizations and screened on political reliability. Occupations are coded from the ISCO-88 classification.

A.3 Television reception and self-reported viewership

Figure A.1 provides the descriptive counterpart to the first-stage regressions reported in Table I. The histograms show the empirical distribution of self-reported West German TV consumption among ZIJ respondents, separately for treatment and control districts. In treated districts, viewership is heavily concentrated in the "daily" and "several times a week" categories: 66.3 percent of respondents report watching daily, 26.7 percent several times a week, and more than 90 percent at least once a week. In control districts, by contrast, the modal response is "never" (67.8 percent), a further 14.7 percent watch only "rarely," and fewer than 20 percent report any meaningful weekly viewership. The shift is consistent with the technical limits of analog over-the-air reception: in the southeastern district of Dresden and parts of the northeastern coast, terrain and the placement of West German transmitters made stable Western reception physically infeasible. The figure makes clear that the binary treatment indicator captures a quantitatively large discontinuity in actual viewing behaviour, supporting the strong first-stage relationships reported in Table I.

Figure A.1: West German TV signal reception and self-reported viewership



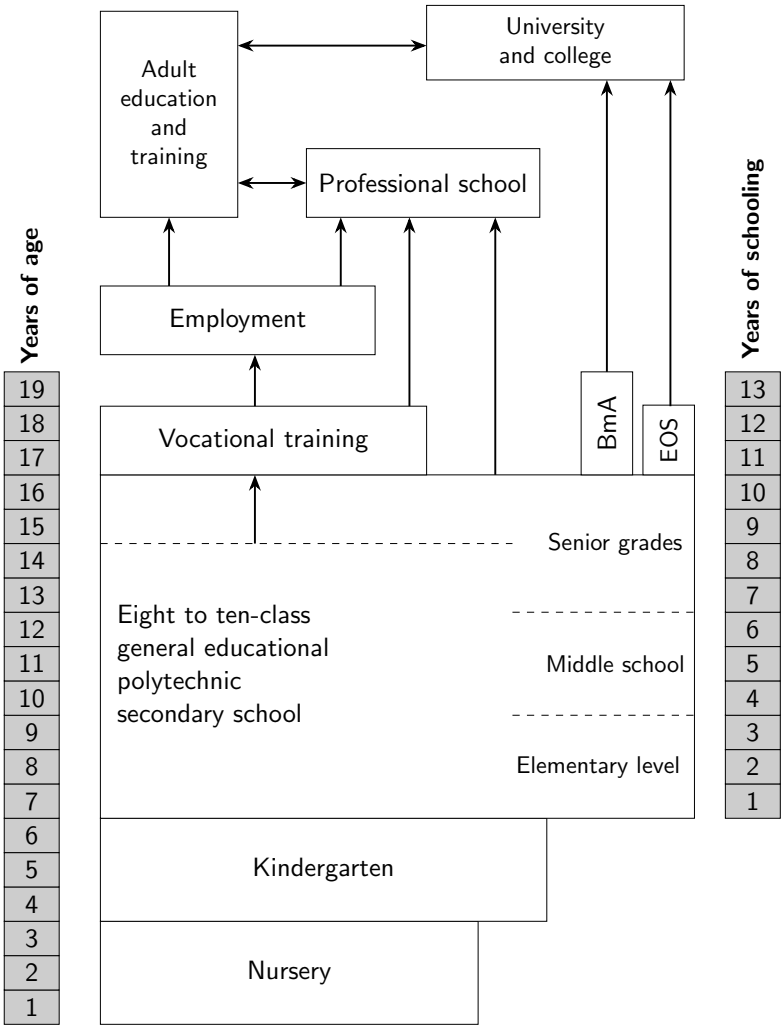
Notes: Distribution of self-reported viewing frequency of West German television among ZIJ respondents, separately for districts classified as treated (West German TV reception) and control (no reception). The horizontal axis reports the original five-category response set: *never*, *rarely*, *once a week*, *several times a week*, and *daily*. Treatment is assigned at the *Bezirk* level using the Longley–Rice signal predictions of Crabtree, Darmofal, and Kern (2015), with a threshold of −86.5 dBm. Data source: GESIS, ZA6008 (1989).

A.4 The architecture of GDR schooling

Figure A.2 summarizes the structure of the educational system that prevailed in the GDR throughout the period covered by our analysis. The system was organized around the *Polytechnische Oberschule* (POS), a ten-year general school combining academic instruction, vocational pre-training, and explicit political socialization. After completing the eighth grade (or, in some pathways, the tenth grade), students could enter vocational training (*Berufsausbildung*); a smaller share continued to vocational training combined with the high-school diploma (*Berufsausbildung mit Abitur*, BmA); a still smaller share (typically below 10 percent of a cohort) entered the *Erweiterte Oberschule* (EOS), the academically selective track that prepared students for university or college. Access to the EOS, BmA, professional schools, and universities was governed not only by academic performance but also by certified political reliability: admission was conditional on FDJ membership, on a positive recommendation from the school's pedagogical council, and, for elite tertiary institutions, on documented loyalty to the SED (Fulbrook 2005; Steiner 2010).

The figure highlights three institutional features that motivate our identification strategy and our interpretation of the results. First, the schooling sequence is highly stratified: each step beyond the eighth grade represents a binding selection point at which both academic merit and political conformity are evaluated. Second, the labour-market value of credentials beyond the POS was overwhelmingly mediated by state employers (public administration, state-owned enterprises, education, and the party apparatus), all of which screened on regime loyalty. Third, exit options outside the regime-approved pathway were limited: alternative private-sector careers, of the kind that exist in market economies, were largely unavailable. Together, these features imply that the marginal student facing a more credible Western alternative had a strong incentive to withdraw from the upper rungs of the schooling ladder, even absent any change in cognitive ability or productive returns, a mechanism we develop formally in Section 3 and document empirically in Sections 5–7.

Figure A.2: Structure of the educational system in the GDR



Notes: BmA: Vocational training with A level; EOS: Grammar school (A level)

Table A.1: Variable description

Variable	Description	Source
Panel A: Outcome variables (educational attainment)		
Above primary education	Dummy indicator equal to 1 if the respondent attained at least an intermediate secondary qualification (CASMIN level $\geq$ 1c). The complement (0) pools individuals with no qualification, with inadequately completed schooling, or with general elementary education only.	SOEP (2025), 1990 wave.
Years of education	Total number of completed years of formal schooling, including vocational and tertiary training, imputed from CASMIN credentials.	SOEP (2025), 1990 wave.
Condensed CASMIN scale	Three-level ordinal indicator distinguishing primary, intermediate, and higher education. Used as the dependent variable in the ordered probit specification reported in Table B.6, columns (1)–(2).	SOEP (2025), 1990 wave.
Full CASMIN scale	Nine-level ordinal indicator covering the full CASMIN classification, from *Inadequately completed* to *Higher tertiary education*. Used in Table B.6, columns (3)–(4).	SOEP (2025), 1990 wave.
GDR school degree	Three-level ordinal indicator based on the official GDR classification: completion of 8th grade, completion of 10th grade, and high-school diploma (*Abitur*). Used in Table B.7, columns (1)–(2).	SOEP (2025), 1990 wave.
ISCED scale (condensed and full)	Ordinal indicators based on the International Standard Classification of Education at three- and six-level disaggregation. Used in Table B.7, columns (3)–(6).	SOEP (2025), 1990 wave.
Panel B: Treatment variable (exposure to West German television)		
West German TV reception (binary)	Indicator equal to 1 if the respondent's pre-reunification county of residence had average predicted West German signal strength above -86.5 dBm. In the ZIJ analysis, treatment is assigned at the district (*Bezirk*) level; in the SOEP analysis, at the county (*Kreis*) level.	Crabtree, Darmofal, and Kern (2015); authors' calculations.
West German TV reception (continuous)	Share of the county's population (or land area) covered by signal strength above -86.5 dBm, computed from municipality-level Longley–Rice predictions and aggregated using two weighting schemes. Used in Figure B.2.	Bursztyn and Cantoni (2016); Statistisches Bundesamt; authors' calculations.
West German TV viewership (self-reported)	Five-point ordinal scale capturing how regularly respondents watched West German TV: (1) never, (2) rarely, (3) once a week, (4) several times a week, (5) daily. Also coded as a binary indicator equal to 1 if the respondent watched at least once a week.	GESIS, ZA6008 (1989).
Panel C: Mechanism variables (regime legitimacy and political affiliation, ZIJ 1989)		
Trust in regime: information	Indicator equal to 1 if the respondent expresses trust in official information about domestic and international affairs broadcast by GDR media.	GESIS, ZA6070 (1989).
Trust in regime: domestic policy	Indicator equal to 1 if the respondent expresses trust in the GDR's domestic policy.	GESIS, ZA6070 (1989).
Trust in regime: cultural sphere	Indicator equal to 1 if the respondent expresses trust in GDR cultural and media institutions.	GESIS, ZA6070 (1989).
Trust in regime: science and technology	Indicator equal to 1 if the respondent expresses trust in the regime's scientific and technological institutions.	GESIS, ZA6070 (1989).

Continued on next page

Table A.1 – continued from previous page

Variable	Description	Source
Confidence in economic development	Indicator equal to 1 if the respondent expresses confidence in the future economic development of the GDR.	GESIS, ZA6070 (1989).
Confidence in socialist democracy	Indicator equal to 1 if the respondent expresses confidence in the development of socialist democracy.	GESIS, ZA6070 (1989).
Confidence in the consumer-goods industry	Indicator equal to 1 if the respondent expresses confidence in the future development of the consumer-goods industry.	GESIS, ZA6070 (1989).
Confidence in science and technology	Indicator equal to 1 if the respondent expresses confidence in the GDR's scientific and technological development.	GESIS, ZA6070 (1989).
Identification with the GDR	Five-point Likert scale (1 = very weak, 5 = very strong) of self-reported identification as a citizen of the GDR.	GESIS, ZA6070 (1989).
Alignment with governance	Five-point Likert scale capturing the respondent's alignment with the GDR's socialist system of governance; higher values indicate stronger alignment.	GESIS, ZA6070 (1989).
Identification with the FDJ	Five-point Likert scale of identification with the Free German Youth (*Freie Deutsche Jugend*, FDJ).	GESIS, ZA6070 (1989).
SED membership	Indicator equal to 1 if the respondent reports being a member or candidate member of the ruling Socialist Unity Party (*Sozialistische Einheitspartei Deutschlands*, SED).	GESIS, ZA6070 (1989).
Intention to migrate	Indicator equal to 1 if the respondent reports intending to leave the GDR for the Federal Republic.	GESIS, ZA6070 (1989).
Panel D: Alternative-mechanism variables (SOEP 1990 and follow-ups)		
Cognitive ability (symbol-digit, animal-naming)	Standardized test scores from the Symbol–Digit Modalities Test and the semantic-fluency animal-naming task administered as part of the SOEP cognitive module.	SOEP 2006 cognitive module.
Risk preference	Eleven-point Likert scale ranging from 0 (very risk-averse) to 10 (very risk-seeking). Standardized in the regressions.	SOEP 2004.
Time preference (patience)	Eleven-point Likert scale ranging from 0 (very patient) to 10 (very impatient). Standardized in the regressions.	SOEP 2008.
Altruism	Eleven-point Likert scale of self-reported willingness to act altruistically. Standardized in the regressions.	SOEP 2010.
Personality (Big Five)	Self-reported Big-Five personality dimensions (openness, conscientiousness, extraversion, agreeableness, neuroticism), each averaged from the standard SOEP three-item inventory and standardized.	SOEP 2005.
Leisure: socializing	Frequency of meeting friends, relatives, or neighbors on a four-point scale, ranging from never to at least once a week; standardized.	SOEP 1990.
Leisure: cultural events	Frequency of attending concerts, theater, or lectures (four-point scale); standardized.	SOEP 1990.
Leisure: sporting events	Frequency of attending sporting events (four-point scale); standardized.	SOEP 1990.
Leisure: practising sports	Frequency of practising sports (four-point scale); standardized.	SOEP 1990.
Economic concerns: career deterioration, occupational change, job loss, mass layoffs	Indicators equal to 1 if the respondent expects, within the next two years, a deterioration of their career, a change of occupation, an individual job loss, or mass layoffs at their employer.	SOEP 1990.

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Table A.1 – continued from previous page

Variable	Description	Source
Optimism about own future	Four-point Likert scale measuring optimism about the respondent's personal future.	SOEP 1990.
Panel E: Long-run outcomes (post-reunification labor market)		
Full-time employment	Indicator equal to 1 if the respondent is full-time employed in the SOEP wave under consideration. Estimated separately for waves 1990, 1995, 2000, 2005, and 2010.	SOEP (2025).
Log monthly net labor income	Natural logarithm of monthly net labor earnings (deflated to 2010 Euros). Estimated separately for waves 1990, 1995, 2000, 2005, and 2010.	SOEP (2025).
State-dominated occupation	Indicator equal to 1 if the respondent works in an occupation classified as "state-dominated" in the GDR period: public administration, state-owned enterprises, education, party and mass-organization apparatus, healthcare, and security services.	SOEP (2025); authors' coding.
Educational mismatch	An indicator equal to 1 if the respondent's schooling deviates by more than one standard deviation from their three-digit (ISCO-88) occupation-by-wave mean, or the continuous number of years beyond this band (0 if matched). Estimated for waves 1990, 1995, 2000, 2005, and 2010.	SOEP (2025); authors' coding.
Overeducation	An indicator equal to 1 if the respondent's schooling exceeds the occupation-by-wave mean by more than one standard deviation, or the continuous number of years exceeding the upper edge of this band (0 otherwise). Estimated for waves 1990, 1995, 2000, 2005, and 2010.	SOEP (2025); authors' coding.
Panel F: Individual-level controls		
Age, age^2	Respondent's age and its square at the time of survey.	SOEP (2025); GESIS, ZA6070 (1989).
Female	Indicator equal to 1 if the respondent is female.	SOEP (2025); GESIS, ZA6070 (1989).
Single	Indicator equal to 1 if the respondent is unmarried at survey.	SOEP (2025).
Household size	Number of individuals in the respondent's household.	SOEP (2025).
Migration background	Indicator equal to 1 if the respondent has a direct or indirect migration background.	SOEP (2025).
Religious affiliation	Indicator equal to 1 if the respondent reports any religious affiliation in the 1990 SOEP wave.	SOEP (2025).
Father's education above primary	Indicator equal to 1 if the respondent's father attained at least an intermediate secondary qualification.	SOEP (2025).
Mother's education above primary	Indicator equal to 1 if the respondent's mother attained at least an intermediate secondary qualification.	SOEP (2025).
Distance to West German border (log)	Natural logarithm of the great-circle distance from the centroid of the respondent's pre-reunification county of residence to the closest point on the inner-German border.	Authors' calculations.
District fixed effects	Vector of indicators for the 14 administrative *Bezirke* of the GDR plus East Berlin.	SOEP (2025).
Panel G: District-level variables (Statistical Yearbook of the GDR)		

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Table A.1 – continued from previous page

Variable	Description	Source
Population density	Inhabitants per km^2.	Statistical Yearbook of the German Democratic Republic (1955); Statistical Yearbook of the German Democratic Republic (1990).
Share of females; share of females below 18	Share of females in the total population and among individuals younger than 18, respectively.	Statistical Yearbook of the German Democratic Republic (1955); Statistical Yearbook of the German Democratic Republic (1990).
Share of population below 18	Share of the population aged below 18.	Statistical Yearbook of the German Democratic Republic (1955); Statistical Yearbook of the German Democratic Republic (1990).
Students per teacher; students per class	Average number of students per teacher and per class in district schools (available for 1955 only).	Statistical Yearbook of the German Democratic Republic (1955).
Suicides per 100,000	Annual suicide rate per 100,000 inhabitants.	Statistical Yearbook of the German Democratic Republic (1955); Statistical Yearbook of the German Democratic Republic (1990).
Retail sales per capita	Annual retail sales per inhabitant (in 1985 GDR Marks).	Statistical Yearbook of the German Democratic Republic (1955); Statistical Yearbook of the German Democratic Republic (1990).
Hospital beds per 1,000	Number of hospital beds per 1,000 inhabitants (available for 1989 only).	Statistical Yearbook of the German Democratic Republic (1990).
Medical doctors per 1,000	Number of medical doctors per 1,000 inhabitants (available for 1989 only).	Statistical Yearbook of the German Democratic Republic (1990).
Share of foreigners	Share of foreign nationals in the district population (available for 1989 only).	Statistical Yearbook of the German Democratic Republic (1990).
Protestant share (1910, 1925)	Share of Protestants in the population from the 1910 and 1925 censuses, used to test pre-socialist religious balance.	Becker, Mergele, and Woessmann (2020).
Catholic share (1910, 1925)	Share of Catholics in the population from the 1910 and 1925 censuses.	Becker, Mergele, and Woessmann (2020).
Jewish share (1910, 1925)	Share of Jews in the population from the 1910 and 1925 censuses.	Becker, Mergele, and Woessmann (2020).
Panel H: Citizen complaint letters (*Eingaben*), county level, 1980–1988		
Complaints, total	Number of complaint letters filed per 100,000 inhabitants and day, pooling all topics.	Albrecht et al. (2020).
On public education	Number of complaint letters concerning public education per 100,000 inhabitants and day; Complaint letters concerning public education as a share (in %) of all complaint letters in the county-year.	Albrecht et al. (2020).

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Table A.1 – continued from previous page

Variable	**Description**	**Source**
On youth affairs	Number of complaint letters concerning youth affairs per 100,000 inhabitants and day; Complaint letters concerning youth affairs as a share (in %) of all complaint letters in the county-year.	Albrecht et al. (2020).

Table A.2: State-dominated occupations

ISCO-88	Occupation
Military & Security	
100	Soldiers
3450	Police Inspectors and Detectives
5162	Police Officers
5163	Prison Guards
5169	Protective Services Workers (NEC)
Political Administration & Public Service	
1110	Legislators, Senior Officials, and Managers
1140	Senior Officials of Special-Interest Organisations
1141	Senior Officials of Political Party Organisations
1142	Senior Officials of Employers', Workers', and Economic-Interest Organisations
2470	Public Service Administrative Professionals
3442	Government Tax and Excise Officials
3443	Government Social Benefit Officials
3449	Customs, Tax, and Related Government Associate Professionals (NEC)
Education	
2310	Higher Education Teaching Professionals
2320	Secondary Education Teaching Professionals
2331	Primary Education Teaching Professionals
2340	Special Education Teaching Professionals
2359	Other Teaching Professionals (NEC)
3320	Pre-Primary Education Teaching Associate Professionals
3340	Other Teaching Associate Professionals (NEC)
Law, Social Sciences & Social Services	
2421	Lawyers
2429	Legal Professionals (NEC)
2441	Economists
2442	Sociologists, Anthropologists, and Related Professionals
2446	Social Work Professionals
2411	Accountants
2412	Personnel and Careers Professionals
3460	Social Work Associate Professionals
Media, Culture & Information	
2451	Authors, Journalists, and Other Writers
2455	Film, Stage, and Related Actors and Directors
2432	Librarians and Related Information Professionals
Religious Occupations	
2460	Religious Professionals
3480	Religious Associate Professionals

Notes: The table lists the occupations classified as 'state-dominated' for the post-reunification analysis in Section 7, grouped by field. Occupations are coded using the ISCO-88 classification; the designation of an occupation as state-dominated is the authors' own. NEC = not elsewhere classified. Source: authors' coding.

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B Robustness Checks

This appendix subjects the baseline estimates of Table III to a comprehensive robustness analysis organized around the principal threats to identification. Section B.1 provides pre-treatment balance and parallel-trends evidence. Section B.2 probes sensitivity to selective migration, border counties, and Berlin. Section B.3 addresses the concern that the estimates reflect a spatial gradient in proximity to West Germany rather than a discontinuity at the signal boundary. Section B.4 uses the historical timing of West German broadcasting to run cohort placebo tests. Sections B.5 and B.6 vary the estimator, functional form, and treatment definition. Sections B.7 and B.8 test for effect heterogeneity by gender and exposure intensity and probe the sensitivity of inference to the level of clustering. Section B.9 steps outside the television experiment altogether, using a GDR compulsory-schooling reform as an instrument for educational attainment to provide an independent estimate of the productive return that underlies the decomposition in Section 7. Across all specifications, the estimated effect of West German television exposure remains negative, statistically significant, and quantitatively close to the baseline.

B.1 Pre-treatment balance and trends

The identifying assumption is that, conditional on district fixed effects and individual controls, West German signal access is uncorrelated with unobserved determinants of educational attainment. We probe this assumption using population-weighted, district-level data from the GDR Statistical Yearbooks of 1955 and 1990, supplemented with pre-socialist religious composition data from Becker, Mergele, and Woessmann (2020). Table B.1 reports four panels of evidence.

Panel A compares treated and untreated districts in 1955, the earliest year with comprehensive administrative records and before the saturation of West German broadcasting. Across population density, gender composition, age structure, schooling inputs (students per teacher, students per class), suicide rates, and retail sales per capita, the differences between treatment and control are uniformly small in magnitude and statistically indistinguishable from zero. Panel B repeats the comparison in 1989, immediately before reunification. Treated and untreated districts remain statistically indistinguishable in demographic composition, health-care infrastructure (hospital beds and medical doctors per 1,000), and economic activity.

Panel C explicitly tests for differential trends by comparing the change in each variable between 1955 and 1989. The estimated differences are uniformly small and statistically insignificant, ruling out the concern that treated and untreated districts were on diverging trajectories along observable dimensions over the three-and-a-half decades preceding our outcome measurement. Panel D adds a stringent historical balance check using pre-socialist religious composition. The Protestant, Catholic, and Jewish population shares in 1910 and 1925 are nearly identical between districts that later did and did not receive West German signals, a particularly informative check given the well-documented link between religious denomination and long-run human-capital investment (Becker and Woessmann 2009).

Taken together, the four panels establish that the GDR maintained substantial spatial homogeneity along pre-determined characteristics relevant for human capital. The identifying variation we exploit is therefore unlikely to reflect persistent regional heterogeneity correlated with Western reception.

B.2 Sample composition and selective migration

Even within East Germany, certain subpopulations could in principle drive the estimated effect: individuals living directly on the inner-German border whose schooling decisions might have been shaped by atypical proximity to the West; respondents in Berlin and the surrounding ring, where reunification dynamics were unusually intense; individuals who migrated to West Germany after 1990, raising the possibility of selective attrition; and respondents who had recently changed residence within the GDR, for whom assigned treatment may not reflect actual exposure during schooling.

Table B.2 addresses each of these concerns by sequentially excluding the relevant subpopulation. Column (1) drops respondents from counties on the inner-German border. Column (2) drops respondents from Berlin and the nine surrounding counties. Column (3) drops respondents who report having moved to West Germany in the post-reunification follow-up waves. Column (4) drops respondents who changed residence in the two years before the 1990 SOEP interview. Across all four restrictions, the estimated effects remain within a fraction of a standard error of the baseline. These results indicate that the baseline findings are not driven by selective migration, by border-county dynamics, or by the inclusion of Berlin.

B.3 Geographic proximity and omitted-variable concerns

Treated districts are not randomly scattered: by construction, they tend to be closer to the West German border. A natural concern is that unobserved gradients in geographic proximity, rather than the television signal per se, drive the result. We address this concern in two complementary ways.

First, we exploit quasi-experimental variation at the signal boundary itself. Columns (3) and (6) of Table III report estimates from a geographic regression discontinuity (GRD) design that retains only individuals living in counties within 100 km of the boundary separating reception from non-reception areas. Despite the substantial reduction in sample size, the estimates remain quantitatively close to the full-sample baseline. Figure B.1 probes the sensitivity of the GRD design to the choice of bandwidth. Figure B.1a plots the coefficient on West German television in the post-primary specification, and Figure B.1b does the same for years of schooling, varying the bandwidth from 150 km down to 50 km. The point estimates are remarkably stable across bandwidths, and their confidence intervals consistently exclude zero. The result is therefore not an artifact of the particular 100-km cutoff used in the baseline.

Second, Table B.3 addresses the related concern that omitted individual- and family-level characteristics could correlate with both television reception and educational outcomes. The

table introduces additional controls one at a time into the full-sample specification. Panel A reports estimates for the post-primary outcome; Panel B reports estimates for years of schooling. Columns (1) and (2) introduce the logarithm of distance to the inner-German border, first in the full sample and then within the signal-covered region. The coefficient on television exposure is essentially unchanged; distance carries at most a marginally significant positive coefficient in the full sample and loses all explanatory power once attention is restricted to covered counties (column 2), exactly as the GRD logic predicts. Column (3) adds a control for self-reported religious affiliation; although religion itself predicts years of schooling, the estimated effect of television exposure is again unchanged, so individual-level religious background does not mediate the effect. Column (4) adds dummies for father's and mother's education above the primary level. Parental education is, as expected, a powerful predictor of own attainment, but its inclusion leaves the television coefficient intact. Column (5) adds an indicator for having social ties (relatives or friends) in West Germany, a plausible correlate of latent demand for Western content; it enters insignificantly and again leaves the television coefficient unchanged. Finally, column (6) adds an indicator equal to one if the respondent was exposed to the GDR schooling reform that extended compulsory education from eight to ten years. As expected, exposure to the reform is associated with significantly higher educational attainment, reflecting its intended effect on years of schooling. Importantly, however, the estimated effect of West German television exposure remains virtually unchanged. This is consistent with the institutional design of the reform, which was implemented uniformly throughout the GDR and therefore did not vary systematically with access to West German television.

The robustness of the estimate to controls for distance, religion, parental schooling, West German social ties, and exposure to the GDR schooling reform indicates that the result is not driven by spatial gradients, individual-level cultural background, differential family human capital, or pre-existing Western connections.

B.4 Cohort timing and placebo tests

Another threat to identification is that unobserved characteristics of the treated districts could correlate with educational outcomes for reasons unrelated to West German broadcasting. We address this concern with cohort-based placebo tests that exploit the historical timing of the regime and of West German television. Western public broadcasting (ARD) expanded across the inner-German border only after the mid 1950s, and the GDR itself was founded in 1949; individuals who completed their formal schooling before these dates could not have been treated by signal exposure during their schooling years, regardless of where they lived.

Table B.4 reports two such placebos. For both placebo cohorts and for both outcomes, the estimated coefficient on West German signal access is small and statistically indistinguishable from zero. Because these cohorts lived in the same treated and control districts as our main sample but could not have been exposed during their formative years, the absence of any placebo effect anchors the baseline result to exposure during schooling rather than to time-invariant district characteristics.

B.5 Reweighting and alternative estimators

To guard against the influence of differences in the distribution of observable covariates between treated and untreated respondents, Table B.5 reports estimates obtained after explicitly balancing the two groups and under alternative functional forms. Columns (1) and (4) implement the entropy-balancing reweighting of Hainmueller (2012), which constructs unit weights that match the covariate moments between groups; columns (2) and (5) implement coarsened exact matching (Iacus, King, and Porro 2012), which retains only treated and control observations falling in the same coarsened covariate strata. The reweighted and matched samples reproduce the baseline almost exactly. Columns (3) and (6) vary the functional form: a logit model for the post-primary outcome and a generalized-Poisson model for years of schooling. Neither the choice of binary link function nor the linearity assumption for years of schooling materially affects the conclusions.

B.6 Ordinal outcomes and the definition of treatment

A further set of exercises exploits the ordinal structure of educational attainment and probes the definition of the treatment. Table B.6 estimates ordered-probit models on the condensed CASMIN scale (columns 1–2) and on the full CASMIN scale (columns 3–4). The coefficient on West German television is negative and significant in both; the marginal effects show that exposure raises the probability of remaining in the lowest education category and lowers the probability of attaining higher-secondary or tertiary qualifications. Table B.7 reproduces the exercise using three further classifications: the official GDR school-degree categories (columns 1–2), and the International Standard Classification of Education (ISCED) at condensed (columns 3–4) and full (columns 5–6) disaggregation. The negative effect is robust throughout: students in treated districts are more likely to terminate schooling at the eighth or tenth grade and less likely to attain the *Abitur*, professional degrees, or higher tertiary credentials.

Finally, Figure B.2 addresses the sensitivity of the results to the way West German signal access is operationalized. We re-estimate the baseline replacing the −86.5 dBm threshold with two alternative cutoffs, −82.5 and −80.0 dBm. The estimated effect remains negative for both outcomes across all three definitions and is statistically significant at the −86.5 and −82.5 dBm cutoffs. It attenuates toward zero only under the most restrictive −80.0 dBm definition, which requires an unusually strong signal to classify a county as treated and reclassifies marginal-reception areas as untreated, biasing the estimate toward zero through measurement error. The pattern reinforces the conclusion that we capture the substantive effect of access to Western broadcasting rather than an artifact of one particular operationalization of the treatment.

B.7 Effect heterogeneity by gender and exposure intensity

Table B.8 asks whether the schooling response varies systematically across individuals. Columns (1) and (4) interact signal access with an indicator for being female; columns (2)–(3) and (5)–(6)

interact it with the number of formative ('impressionable') years during which West German television was on air, measured from the onset of ARD cross-border transmission and, alternatively, over the respondent's full schooling window. None of the interaction terms is statistically significant, while the main effect of exposure remains negative and significant throughout. The effect is thus broadly homogeneous: it does not differ detectably between men and women (consistent with a demand-side response rather than gender-specific screening) and does not scale with the length of the potential exposure window.

B.8 Inference and the level of clustering

Because treatment is assigned at the county level and the control region comprises only 25 of the 217 counties, inference could in principle be sensitive to the level at which the standard errors are clustered. Table B.9 re-estimates the baseline specification clustering at three successively coarser levels: the county (our default), the spatial planning region (*Raumordnungsregion*), and the district (*Bezirk*). The point estimates are common across columns by construction. The post-primary effect remains significant at the 1% level under all three levels of clustering, and the years-of-schooling effect at the 5% level, weakening only to the 10% level under district clustering, where the number of clusters is smallest. Inference is therefore not an artifact of the level of clustering.

B.9 The 1959 GDR schooling reform

The decomposition in Section 7 infers that the marginal year of GDR schooling carried a near-zero productive return ($w \approx 0$) from the post-reunification convergence of television-exposed cohorts. Because that inference rests on the television experiment, we corroborate it here with an unrelated source of variation in schooling. In 1959, the GDR extended compulsory general education to the ten-year *Polytechnische Oberschule*; the new requirement bound cohorts born from 1944 onward, who reached the statutory school-leaving age from 1960 onward. Cohorts born just before and just after this threshold therefore faced sharply different compulsory-schooling requirements for reasons unrelated to West German television, signal geography, or individual demand.

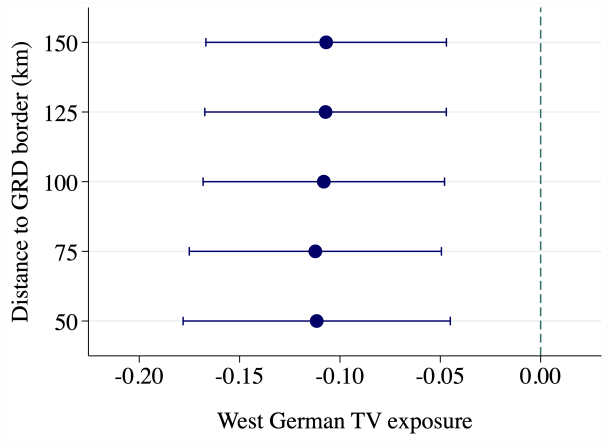
We exploit this discontinuity in a two-stage least-squares design that instruments educational attainment with an indicator for being born in 1944 or later, controlling for the running variable (birth year) and its square alongside the baseline individual controls and district fixed effects, with standard errors clustered at the county level. Attainment is measured two ways—an indicator for completing post-compulsory schooling and total years of schooling—and we examine two outcomes, log net household income and full-time employment, in the 1990 wave. Table B.10 reports estimates within a five-year bandwidth (Panel A, cohorts born 1939–1948) and a ten-year bandwidth (Panel B, cohorts born 1934–1953) around the cutoff; the specification is the instrumental-variables counterpart of the reduced-form regression-discontinuity plots in Figure IV.

The first stage is strong and precisely estimated. Reform exposure raises post-compulsory completion by about 0.25 in both bandwidths and total schooling by 0.66 and 0.57 of a year in the five- and ten-year windows, respectively, all significant at the 1 percent level; the first-stage F-statistics range from roughly 10 in the narrow bandwidth to 28 in the wider one. The second stage, by contrast, is flat throughout. An additional reform-induced year of schooling moves log income by 0.065 (five-year) and 0.046 (ten-year) and full-time employment by 0.070 and 0.038; the effects of completing post-compulsory schooling are likewise small (0.17 and 0.10 for income, 0.19 and 0.08 for employment). None of the eight second-stage coefficients is statistically significant.

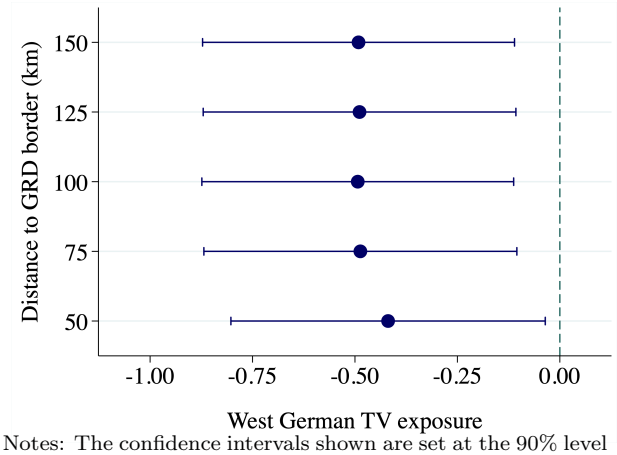
Two caveats temper the reading. First, the design has limited power: the confidence intervals are wide, and the per-year income estimates are not bounded tightly enough to exclude a modest positive return of the kind a market economy would reward. Second, as Figure IV shows, the employment outcome exhibits a mild pre-reform birth-year trend. We therefore treat the reform as corroborating, rather than independently establishing, the near-zero productive return: a substantial, exogenously imposed increase in GDR schooling produced no detectable gain in income or employment, exactly as the television-based decomposition implies.

Figure B.1: Distances in GRD design

(a) Above Primary

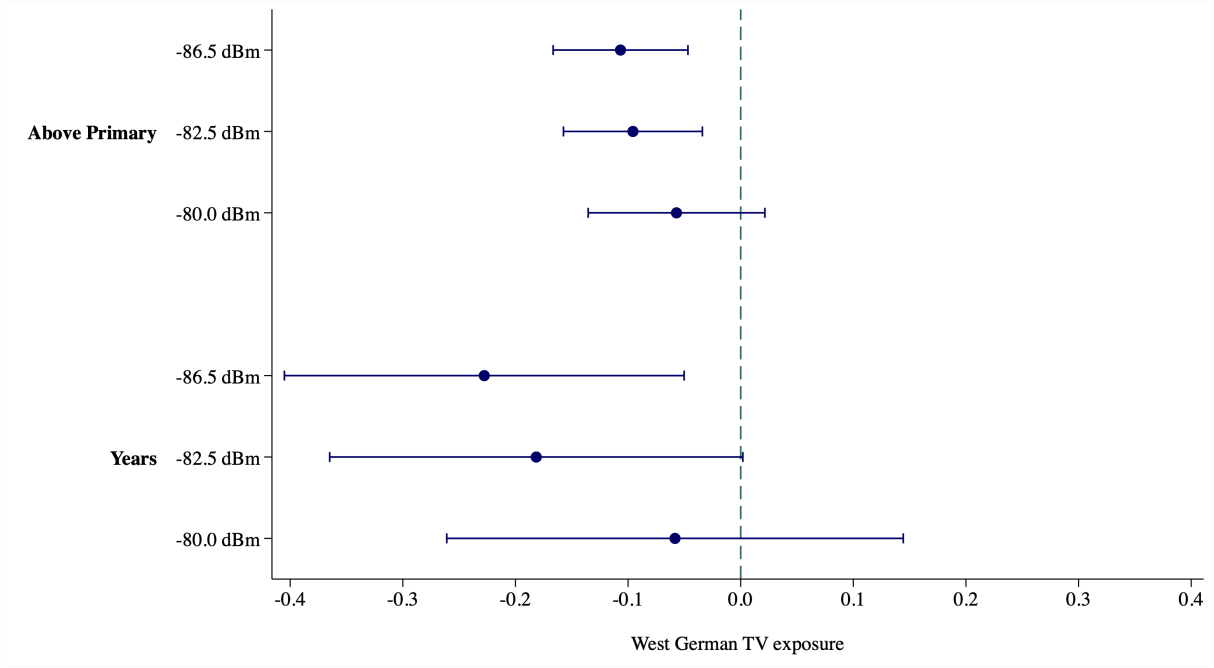


(b) Years



Notes: The confidence intervals shown are set at the 90% level of statistical significance. Additional controls include age, age^2, single, household size, migration background, and district fixed effects. Standard errors are clustered at the county level. Data source: SOEP (2025), survey year 1990.

Figure B.2: Alternative specification of the treatment indicator



Notes: The confidence intervals shown are set at the 90% level of statistical significance. The variable *Years* is standardized. Additional controls include gender, age, age^2, single, household size, and migration background, as well as district fixed effects. Standard errors are clustered at the county level. Data source: SOEP (2025), survey year 1990.

Table B.1: Differences between districts: Pre-trends

	With West German TV	Without West German TV	Difference		
	Mean	Mean	Difference	Std. err.	P-value
	(1)	(2)	(3)	(4)	(5)
Panel A: Balance in district characteristics in 1955: values					
Population density	207.17	203.18	3.99	74.20	0.96
Share of females *(%)*	57.20	57.04	0.16	0.93	0.87
Pop. share below 18 *(%)*	26.70	27.41	−0.71	1.61	0.67
Female share below 18 *(%)*	23.75	24.31	−0.56	1.73	0.75
Suicides per 100k	26.01	24.87	1.14	4.46	0.80
Retail sales per capita	1680.42	1684.31	−3.89	80.86	0.96
Students per teacher	17.13	17.02	0.11	0.38	0.78
Students per class	25.31	25.56	−0.25	0.33	0.67
Panel B: Balance in district characteristics in 1989: values					
Population density	176.14	181.24	−5.10	58.65	0.93
Share of females *(%)*	52.11	51.90	0.21	0.53	0.70
Pop. share below 18 *(%)*	22.42	23.82	−1.40	1.01	0.19
Female share below 18 *(%)*	20.99	22.36	−1.37	1.19	0.27
Share of foreigners *(%)*	1.13	1.12	0.01	0.28	0.98
Suicides per 100k	27.70	26.37	1.33	1.70	0.45
Hospital beds per 1,000	9.85	9.63	0.22	0.46	0.64
Medical doctors per 1,000	2.24	2.45	−0.21	0.23	0.38
Retail sales per capita	7544.16	7836.19	−292.03	188.77	0.15
Panel C: Balance in district characteristics 1955-1989: trends					
Population density	−24.77	−17.32	−7.45	15.75	0.65
Share of females *(%)*	−5.00	−5.03	0.03	0.61	0.96
Pop. share below 18 *(%)*	−4.31	−3.72	−0.59	0.75	0.44
Female share below 18 *(%)*	−2.78	−2.09	−0.69	0.72	0.35
Suicides per 100k	1.97	1.83	0.14	3.42	0.97
Retail sales per capita	5869.42	6147.95	−278.53	193.05	0.18
Panel D: Pre-treatment characteristics					
Protestant share in 1925 (%)	91.19	90.75	0.44	1.14	0.70
Catholic share in 1925 (%)	5.19	6.36	−1.17	1.03	0.26
Jewish share in 1910 (%)	0.22	0.22	0.00	0.05	0.99

Notes: The table compares district (*Verwaltungsbezirke*) characteristics between areas with and without West German TV reception. For each variable it reports the population-weighted mean in covered and uncovered districts, their difference, the standard error of the difference, and the p-value of a test of equality of means. Panel A reports characteristics measured in 1955 and Panel B in 1989; Panel C reports their change between 1955 and 1989; Panel D reports pre-socialist religious composition (denominational population shares in 1910 and 1925). East Berlin is excluded: as the GDR's only city district it was demographically incomparable to the others, and it was not an officially independent district until 1961; results are robust to its inclusion. Data sources: the Statistical Yearbooks of the German Democratic Republic (1955 and 1990) for Panels A–C, and Becker, Mergele, and Woessmann (2020) for Panel D.

Table B.2: Sensitivity to sample composition

	Excluding Individuals...			
	Border Counties	Berlin Area	Movers to the West	Recent Movers
	(1)	(2)	(3)	(4)
Panel A: Above Primary				
West German TV	-0.108^{***}	-0.109^{***}	-0.108^{***}	-0.106^{***}
	(0.037)	(0.038)	(0.038)	(0.040)
Mean Y	0.604	0.585	0.593	0.584
Pseudo R^2	0.260	0.261	0.262	0.255
Observations	3747	3689	3980	3738
Panel B: Years				
West German TV	-0.445^{*}	-0.489^{**}	-0.461^{**}	-0.419^{*}
	(0.231)	(0.229)	(0.220)	(0.231)
Mean Y	12.702	12.615	12.688	12.676
R^2	0.130	0.121	0.125	0.128
Observations	3747	3689	3980	3738

Notes: Panel A reports probit average marginal effects for attaining at least an intermediate level of education based on the CASMIN classification (*Comparative Analysis of Social Mobility in Industrial Nations*); Panel B reports OLS estimates for years of schooling. Each column drops a subpopulation: column (1) respondents in counties on the inner-German border; column (2) Berlin and the nine surrounding counties; column (3) respondents who report having moved to West Germany in the follow-up waves; and column (4) respondents who changed residence in the two years before the 1990 SOEP interview. All specifications include the baseline controls (gender, age, age^2, single, household size, and migration background) and district fixed effects. Standard errors are clustered at the county level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table B.3: Omitted variables and alternative samples

	Border Distance		Religion	Parents' Educ.	Social Ties	Reform
	Full	TV Sample				
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Above Primary						
West German TV	−0.106***		−0.106***	−0.118***	−0.108***	−0.100***
	(0.035)		(0.037)	(0.037)	(0.036)	(0.037)
Distance to Border (log)	0.014*	0.010				
	(0.008)	(0.008)				
Religious Affiliation			−0.004			
			(0.016)			
Father: Above Primary				0.172***		
				(0.023)		
Mother: Above Primary				0.079***		
				(0.029)		
West German Ties					0.013	
					(0.018)	
GDR schooling reform						0.216***
						(0.036)
Mean Y	0.604	0.596	0.604	0.636	0.603	0.603
Pseudo R^2	0.264	0.264	0.264	0.283	0.263	0.275
Observations	4164	3633	4151	3041	4176	4179
Panel B: Years						
West German TV	−0.485**		−0.486**	−0.546**	−0.496**	−0.469**
	(0.218)		(0.227)	(0.219)	(0.229)	(0.227)
Distance to border (log)	0.084**	0.053				
	(0.041)	(0.037)				
Religious affiliation			−0.267***			
			(0.087)			
Father: Above Primary				0.811***		
				(0.131)		
Mother: Above Primary				0.179		
				(0.155)		
West German ties					0.128	
					(0.084)	
GDR schooling reform						0.585***
						(0.145)
Mean Y	12.694	12.664	12.700	12.866	12.694	12.695
R^2	0.128	0.127	0.131	0.150	0.127	0.131
Observations	4164	3633	4151	3041	4176	4179

Notes: Panel A reports probit average marginal effects for attaining at least an intermediate level of education based on the CASMIN classification (*Comparative Analysis of Social Mobility in Industrial Nations*); Panel B reports OLS estimates for years of schooling. All specifications include the baseline controls (gender, age, age^2, single, household size, and migration background) and district fixed effects, and add one further control at a time: column (1) the log distance to the inner-German border (full sample); column (2) repeats this within the signal-covered sample only; column (3) an indicator for religious affiliation; column (4) indicators for the father's and mother's education above the primary level; column (5) an indicator for social ties in West Germany; and column (6) an indicator for exposure to the GDR schooling reform. Standard errors are clustered at the county level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table B.4: Placebo cohorts schooled before exposure was possible

	Educational Attainment			
	Above Primary		Years	
	Before GDR Cohort (1)	Before ARD Cohort (2)	Before GDR Cohort (3)	Before ARD Cohort (4)
West German TV	0.089	0.003	−0.139	−0.555
	(0.086)	(0.105)	(0.667)	(0.579)
Mean Y	0.193	0.201	11.305	11.407
R^2	0.145	0.128	0.342	0.317
Observations	430	543	430	543

Notes: Columns (1) and (2) show average marginal effects from probit models, while columns (3) and (4) report results from OLS regressions. The dependent variable in columns (1) and (2) is a dummy variable equal to one if the respondent has at least an intermediate level of education based on the CASMIN classification (*Comparative Analysis of Social Mobility in Industrial Nations*). The dependent variable in columns (3) and (4) measures a respondent's number of years of education. The two placebo samples comprise respondents who completed their schooling before the GDR was founded (1949) and before West German television (ARD) reached the inner-German border (mid-1950s), respectively; neither cohort could have been exposed to West German broadcasts during its schooling years. Controls include gender, age, age^2, single, household size, and migration background; district fixed effects are included in all specifications. Standard errors are clustered at the county level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table B.5: Covariate balancing and alternative estimators

	Educational Attainment					
	Above Primary			Years		
	Entropy (1)	CEM (2)	Logit (3)	Entropy (4)	CEM (5)	Poisson (6)
West German TV	-0.108***	-0.106***	-0.103***	-0.461***	-0.581***	-0.038**
	(0.029)	(0.034)	(0.036)	(0.158)	(0.221)	(0.018)
Mean Y	0.603	0.613	0.603	12.695	12.743	12.695
Observations	4179	3767	4179	4179	3767	4179

Notes: The table re-estimates the baseline effect of West German TV under covariate balancing and alternative estimators; columns (1)–(3) report average marginal effects on post-primary attainment and columns (4)–(6) estimates for years of schooling. Columns (1) and (4) use entropy-balancing weights that match covariate means across groups (Hainmueller 2012); columns (2) and (5) use coarsened exact matching (Iacus, King, and Porro 2012); column (3) reports a logit average marginal effect and column (6) the coefficient from a generalized-Poisson regression. All specifications include the baseline controls (gender, age, age^2, single, household size, and migration background) and district fixed effects. Standard errors (in parentheses) are clustered at the county level in columns (2), (3), (5), and (6) and are survey-linearized in the entropy-balancing columns (1) and (4). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table B.6: Ordered probit model

	Educational Attainment			
	Condensed CASMIN		CASMIN Scale	
	Coef. (1)	Marg. Effect (2)	Coef. (3)	Marg. Effect (4)
West German TV	−0.278**		−0.303**	
	(0.125)		(0.114)	
Primary level		0.091**		
		(0.039)		
Intermediate level		−0.010***		
		(0.002)		
Higher level		−0.081**		
		(0.038)		
Inadequately completed				0.000
				(0.000)
General elementary school				0.030***
				(0.011)
Basic vocational qualification				0.068**
				(0.028)
Intermediate general qualification				0.002*
				(0.001)
Intermediate vocational				−0.008***
				(0.002)
General maturity certification				−0.002**
				(0.001)
Vocational maturity certification				−0.003**
				(0.001)
Lower tertiary education				−0.039**
				(0.015)
Higher tertiary education				−0.050**
				(0.024)
Mean Y	1.837		4.941	
Pseudo R^2	0.093		0.071	
Observations	4179		4179	

Notes: The table reports ordered-probit estimates of the effect of West German TV on educational attainment measured on the CASMIN scale (*Comparative Analysis of Social Mobility in Industrial Nations*). Columns (1)–(2) use the condensed CASMIN scale and columns (3)–(4) the full scale. Columns (1) and (3) report the coefficient on the West German TV indicator from the ordered probit; columns (2) and (4) report the implied marginal effect on the probability of each education category. Controls include gender, age, age^2, single, household size, and migration background; district fixed effects are included in all specifications. Standard errors are clustered at the county level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table B.7: Ordered probit model: GDR and ISCED scales

	Educational Attainment					
	GDR School Degree		Condensed ISCED		ISCED Scale	
	Coef.	Marg. Effect	Coef.	Marg. Effect	Coef.	Marg. Effect
	(1)	(2)	(3)	(4)	(5)	(6)
West German TV	−0.322**		−0.285**		−0.260**	
	(0.134)		(0.125)		(0.125)	
8th grade		0.096**				
		(0.038)				
10th grade		−0.032***				
		(0.009)				
High school		−0.064**				
		(0.030)				
Elementary or less				0.037**		
				(0.014)		
Vocational				0.058**		
				(0.029)		
Higher education				−0.095**		
				(0.043)		
Gen. elementary						0.034**
						(0.015)
Middle vocational						0.055*
						(0.029)
Vocational + A-level						−0.002**
						(0.001)
Higher vocational						−0.008**
						(0.003)
Higher education						−0.079**
						(0.040)
Mean Y	1.713		2.200		3.746	
Pseudo R^2	0.162		0.077		0.059	
Observations	4117		4179		4179	

Notes: The table reports ordered-probit estimates of the effect of West German TV on educational attainment under three alternative classifications. Columns (1)–(2) use the official GDR school-degree scale, columns (3)–(4) the condensed International Standard Classification of Education (ISCED) scale, and columns (5)–(6) the full ISCED scale. Columns (1), (3), and (5) report the coefficient on the West German TV indicator from the ordered probit; columns (2), (4), and (6) report the implied marginal effect on the probability of each education category. Controls include gender, age, age^2, single, household size, and migration background; district fixed effects are included in all specifications. Standard errors are clustered at the county level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table B.8: Heterogeneity by gender and exposure intensity

	Educational Attainment					
	Above Primary			Years		
	(1)	(2)	(3)	(4)	(5)	(6)
West German TV	-0.096**	-0.115**	-0.111**	-0.416*	-0.411*	-0.421*
	(0.048)	(0.057)	(0.044)	(0.241)	(0.244)	(0.247)
West German TV× Female	-0.033			-0.137		
	(0.034)			(0.182)		
W.G. TV× Impr. Years (ARD)		0.002			-0.008	
		(0.005)			(0.020)	
W.G. TV× Impr. Years			0.002			-0.023
			(0.004)			(0.022)
Mean Y	0.603	0.603	0.603	12.695	12.695	12.695
R^2	0.317	0.334	0.324	0.126	0.131	0.127
Observations	4179	4179	4179	4179	4179	4179

Notes: This table shows results from OLS regressions. The dependent variable in columns (1) to (3) is a dummy variable equal to one if the respondent has at least an intermediate level of education based on the CASMIN classification (*Comparative Analysis of Social Mobility in Industrial Nations*); in columns (4) to (6) it is the respondent's number of years of education. Each column interacts West German TV reception with a moderator: columns (1) and (4) with an indicator for being female; columns (2) and (5) with the number of years (ages 6–16) during which West German broadcasting was available, counted from the onset of ARD transmission (1953); and columns (3) and (6) with the same measure counted from 1970. Controls include gender, age, age^2, single, household size, and migration background; district fixed effects are included in all specifications. Standard errors are clustered at the county level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table B.9: Clustering

	Educational Attainment					
	Above Primary			Years		
	County	ROR	District	County	ROR	District
	(1)	(2)	(3)	(4)	(5)	(6)
West German TV	−0.107***	−0.107***	−0.107***	−0.489**	−0.489**	−0.489*
	(0.036)	(0.021)	(0.028)	(0.230)	(0.209)	(0.245)
Mean Y	0.603	0.603	0.603	12.695	12.695	12.695
R^2	0.263	0.263	0.263	0.126	0.126	0.126
Observations	4179	4179	4179	4179	4179	4179

Notes: Columns (1) to (3) show average marginal effects from probit models, while columns (4) to (6) report results from OLS regressions. The dependent variable in columns (1) to (3) is a dummy variable equal to one if the respondent has at least an intermediate level of education based on the CASMIN classification (*Comparative Analysis of Social Mobility in Industrial Nations*). The dependent variable in columns (4) to (6) measures a respondent's number of years of education. Controls include gender, age, age^2, single, household size, and migration background; district fixed effects are included in all specifications. The point estimates are common within each outcome; the columns differ only in the level at which standard errors are clustered. Standard errors, shown in parentheses, are clustered at the county level in columns (1) and (4), at the level of spatial planning regions (*Raumordnungsregionen*) in columns (2) and (5), and at the district level in columns (3) and (6). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

Table B.10: GDR Schooling Reform

	Income		Full-Time Employment	
	(1)	(2)	(3)	(4)
Panel A: 5-Years Bandwidth				
Second stage:				
$\geq$ 10 Years	0.173		0.186	
	(0.161)		(0.197)	
Years of Schooling		0.065		0.070
		(0.061)		(0.075)
First stage:				
Reform	0.251***	0.662***	0.251***	0.662***
	(0.071)	(0.205)	(0.071)	(0.206)
Mean Y	6.937	6.937	0.815	0.815
F statistic	12.480	10.472	12.619	10.329
Observations	720	720	729	729
Panel B: 10-Years Bandwidth				
Second stage:				
$\geq$ 10 Years	0.103		0.084	
	(0.119)		(0.138)	
Years of Schooling		0.046		0.038
		(0.053)		(0.062)
First stage:				
Reform	0.254***	0.571***	0.255***	0.568***
	(0.049)	(0.142)	(0.048)	(0.141)
Mean Y	6.896	6.896	0.810	0.810
F statistic	27.254	16.248	27.829	16.104
Observations	1583	1583	1604	1604

Notes: This table shows results from two-stage least squares regressions using exposure to the GDR schooling reform as the instrumental variable for educational attainment. In columns (1) and (3) educational attainment is measured using a dummy variable capturing completion of 10 or more years of schooling. In columns (2) and (4) educational attainment is measured using years of schooling. Panel A (B) includes only individuals who started school during the five-year (ten-year) period before and after the introduction of the school reform. Controls include gender, age, age^2, single, household size, and migration background; district fixed effects are included. All specifications additionally control for linear and quadratic terms in the distance from the reform cutoff. Standard errors are clustered at the county level and shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data source: SOEP (2025), survey year 1990.

C The Ideological Content of GDR Schooling

This appendix documents the explicitly ideological character of GDR schooling through primary source materials: textbook excerpts, party-education pamphlets, and visual propaganda from classrooms and youth organizations. These materials ground the state-dependent human-capital interpretation of the main results. They establish that GDR schooling was conceived not only as a vehicle for transmitting productive skills but as a centrally directed instrument for shaping political beliefs, identities, and loyalties. Three subsections follow: party-education archival materials (Section C.1), a representative excerpt from the 1988 tenth-grade civics textbook (Section C.2), and a selection of visual materials from textbooks, FDJ publications, and party pamphlets (Section C.3).

C.1 Party education and ideological training

Beyond the formal school system, ideological formation in the GDR was reinforced by a comprehensive infrastructure of party-directed adult education. Archival materials from the early years of the regime illustrate how the SED explicitly conceptualized education as a central tool for building a "party of a new type," grounded in Marxist–Leninist doctrine. A 1950 pamphlet from the *Sozialistische Bildungshefte* (reproduced in Figure C.1) characterizes the introduction of Soviet-style party education (the *Parteilehrjahr*) as a core organizational task. The document repeatedly stresses that party members were expected to "constantly ... master the theory of Marxism–Leninism," and that political education was indispensable both for leadership and for practical action.

The pamphlet also makes clear that ideological training was intended to be systematic and hierarchical. At the lowest level, party members and sympathizers were to attend basic political courses; more advanced participants were assigned to study circles focused on canonical texts such as the history of the Communist Party of the Soviet Union and the biographies of Lenin and Stalin; higher levels of instruction included evening schools and an "Evening University of Marxism–Leninism" in Berlin, aimed at functionaries and other elites. Across all levels, instruction was standardized, monitored, and linked to formal evaluation. Continuous independent study of the works of Marx, Engels, Lenin, and Stalin was framed as a permanent obligation, not a supplement, of party membership. The document identifies the neglect of theory as a political danger: "practicality" (the tendency to prioritize immediate administrative or economic concerns over ideological work) is treated as a major weakness associated with opportunism, political error, and susceptibility to opposing views. The regime's educational project, in other words, was not informational but formative: it was meant to create politically reliable cadres who could interpret events through a Marxist–Leninist lens and act accordingly.

C.2 Textbook evidence: the socialist worldview

The GDR education system was centrally organized and closely aligned with the SED's objectives. Curriculum content, teaching practices, and extracurricular activities were structured to promote socialist ideology and a collective identity centered on the working class. The flagship vehicle for explicit ideological instruction was *Staatsbürgerkunde* (socialist civics), a compulsory subject that taught Marxist–Leninist doctrine and framed historical and contemporary events through the lens of class struggle and socialist progress.

The excerpt below, drawn from the first chapter of the 1988 tenth-grade GDR civics textbook, illustrates how schooling conveyed a comprehensive ideological framework that bound personal orientation to political conformity.[16]

> ***What is an era?***
>
> *Lenin defined an era in this way: "We cannot know the speed and success with which individual historical movements of a respective era will develop. We can and in fact do know, which class is at the center of this era or that era, what determines its essential content, the main direction of its development, the most important characteristics of the historical situation of the respective era, etc."*
>
> *Lenin's starting point for the definition of an era is the proper determination of the class that defines the character of the respective era.*
>
> *Applying this to our era: For a number of reasons, the working class is at the center. It is most directly involved in modern mass production, and is its most important force. The working class has also developed rapidly as a result of developments in science and technology in recent decades. In the middle of the 1980s it constituted a worldwide army of 660 million people.*
>
> *The working class is the main social force of our time. In advocating its own class interests, it also represents the fundamental interests for peace and social progress of all other productive classes and walks of life, which are the interests of humanity itself. In our era, the working class fulfills its historical mission on a world scale. Under the guidance of the Marxist–Leninist parties, its function is to eliminate the capitalist social order and, in alliance with other classes and walks of life, establish its own power while building up socialism and communism. Today the historical mission of the working class is bound more tightly than ever with world peace.*
>
> ***What is the character of our era?***
>
> *The program of the SED states: "Our era is the era of transition from capitalism to socialism."*
>
> *Our era began with the Great October Socialist Revolution in 1917, and it will last until socialist conditions have triumphed throughout the entire world.*

[16] We thank Bytwerk (1998) for sharing the textbook materials.

But as Lenin knew before the Great October Revolution, conditions for a socialist revolution would ripen at different paces in individual countries. In the beginning socialism would only triumph in one or a few nations. In the course of time, however, it will triumph in other countries, and eventually the entire world will become a part of the socialist community.

History has proven that Lenin was correct.

Whoever does not start with Lenin's principles will reach a false understanding of our era. Bourgeois opinion makers, for example, speak of this as an "era of industrial societies" in contrast to the earlier "agrarian societies." Others conclude that electronic and robot technologies in our era are resulting in an "era of technological societies." These narrow and one-sided approaches consider only development in production capacity, not the overall development of society.

Through such methods, bourgeois ideologues conceal actual social development. They never answer the crucial question: which class determines the character of an era and the direction in which society consequently goes. Bourgeois ideology is not able to give scientifically based answers to these questions, so it must admit the inevitable fall of capitalism and the victory of socialism.

What are the actual characteristics of our era?

With the transition from capitalism to socialism on a worldwide scale, humanity's age-old dreams of life without exploitation and poverty, life without injustice and war, are gradually being realized.

This development has a deep influence on the life of humans of our time, particularly in those countries in which socialism has already become reality, but also in those where this fundamental social transformation is still coming.

The influence of the individual on developments in his own country depends substantially on his knowledge and convictions regarding the course of history. The correct understanding of the present era is a prerequisite for correct personal orientation. To be on the side of history's victors — and who would not want to be — demands supporting the socialist party through mind, heart, and action!

The passage is striking in three respects relevant for our argument. First, it teaches a unified historical narrative in which socialism is presented as the necessary outcome of class dynamics. Second, it casts the rejection of this narrative, including the alternative interpretations explicitly entertained by Western analysts, as scientifically illegitimate. Third, and most directly relevant for our economic argument, it links personal orientation ("personal orientation," "mind, heart, and action") to political alignment with the SED. A student internalizing this curriculum is being told that intellectual maturity is, by construction, indistinguishable from regime loyalty. The marginal value of the credential certifying such conformity is therefore necessarily contingent on the survival of the regime that issues it.

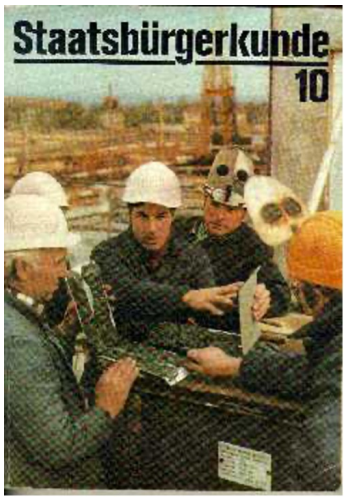
C.3 Visual evidence from educational materials

Figure C.1 reproduces a selection of visual materials used in GDR schools, youth organizations, and party education. We group them in three blocks. Panel (a) shows two covers from *Staatsbürgerkunde* textbooks. The materials illustrate how socialist civics was institutionalized as a mandatory subject and how ideological instruction was embedded directly in the school curriculum. Panel (b) reproduces materials associated with the Free German Youth (FDJ), the regime-aligned youth organization that served as both a complement to formal schooling and an institutional gatekeeper to higher tracks: discipline, collective identity, and political loyalty are explicit motifs. The repeated invocation of Soviet symbolism reinforces the international ideological orientation of the GDR project. Panel (c) reproduces representative pamphlets from the SED's party-education apparatus, illustrating the direct integration of school content with party doctrine.

Taken together, the textual and visual evidence in this appendix supports the interpretation that GDR education was explicitly designed to (i) promote a Marxist–Leninist worldview, (ii) legitimize the socialist state and its institutions, (iii) encourage identification with the working class and collective goals, and (iv) align individual aspirations with the political objectives of the regime. In this institutional environment, educational attainment functioned not only as a productive credential but also as a credible signal of ideological conformity, the precise feature that, in our model, makes investment in schooling sensitive to the perceived legitimacy of the state and to the credibility of alternative narratives transmitted by foreign media.

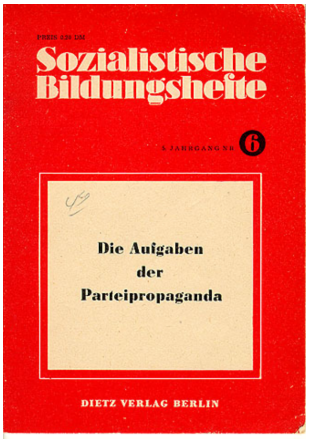
Figure C.1: Ideological messaging in GDR educational materials

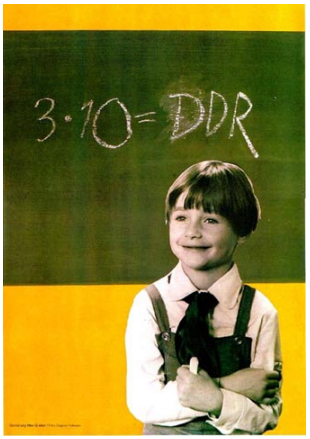
(a) Ideological Curriculum: Socialist Civics Textbooks



(b) Political Socialization: The Free German Youth (FDJ)

(c) Education as Party Instrument and Identity Formation





Notes: Panel C.1a shows two covers from *Staatsbürgerkunde*, the GDR's socialist civics curriculum, which institutionalized Marxist–Leninist ideology in formal schooling. Panel C.1b documents materials from the Free German Youth (FDJ), the state-sponsored youth organization that complemented schooling by fostering discipline, collective identity, and political loyalty. Panel C.1c reproduces representative pamphlets from the party-education apparatus that linked schooling to SED doctrine. Together, the figure illustrates how formal education, youth organizations, and party propaganda jointly promoted conformity to socialist norms and the formation of the "socialist citizen."

D The West and East German Television Information Environment

To characterize the information environment to which East Germans were exposed, we collected detailed information on television programming for the two public West German broadcasters (ARD and ZDF) and the two East German broadcasters (DDR 1 and DDR 2) between 1970 and 1989. Program schedules were obtained from the online television archives Retro-Media and ShoutWiki, which document historical broadcast schedules for both German television systems.

Although neither archive provides complete schedules for every day, they contain detailed information on the vast majority of regularly scheduled broadcasts, including the title, broadcast time, and a short program description. For ARD, we consider only the nationwide joint program and exclude the regional early-evening broadcasts of the individual state broadcasters. During this time slot, different regional stations transmitted distinct programs simultaneously; excluding these broadcasts ensures comparability with the nationally broadcast programming of ZDF, DDR 1, and DDR 2 and avoids double-counting concurrent regional content. We further restrict the analysis to days for which programming information is available for both the West and East German channels, allowing for a meaningful comparison of contemporaneous television content. The resulting dataset comprises 37,550 individual broadcasts across 471 unique broadcast days, providing a representative overview of television programming in both German states over the two decades preceding reunification.

We manually classified each broadcast into broad content categories, including news and magazines, educational programs, documentaries, children's programming, sports, movies, television series, music, religious programming, and other cultural content. The classification was based on the program descriptions contained in the archives and, where necessary, supplemented by information from contemporary online sources to ensure consistent coding across West and East German broadcasts.

Figure D.1 summarizes the composition of television programming in the two broadcasting systems. Several systematic differences emerge. West German television devoted substantially larger shares of programming to news, political magazines, religious broadcasts, and entertainment formats such as television series. In contrast, East German television allocated considerably larger shares to educational and children's programming. While both broadcasting systems offered a diverse mix of content, their composition differed markedly. West German television placed greater emphasis on information provision, political discussion, and entertainment, whereas East German television devoted substantially more airtime to educational and children's programming, consistent with the GDR's documented use of television as an instrument of political socialization among younger audiences.

These differences are consistent with the mechanism explored in the paper. Beyond providing access to alternative news coverage, West German television exposed East German viewers to recurring political magazine formats that offered perspectives unavailable in official GDR media. At the same time, imported television series and other entertainment programming presented al-

ternative social norms, lifestyles, and cultural values that were largely absent from East German broadcasting. The content analysis therefore complements our empirical findings by documenting that viewers with access to West German television were exposed to a systematically different informational and cultural environment.

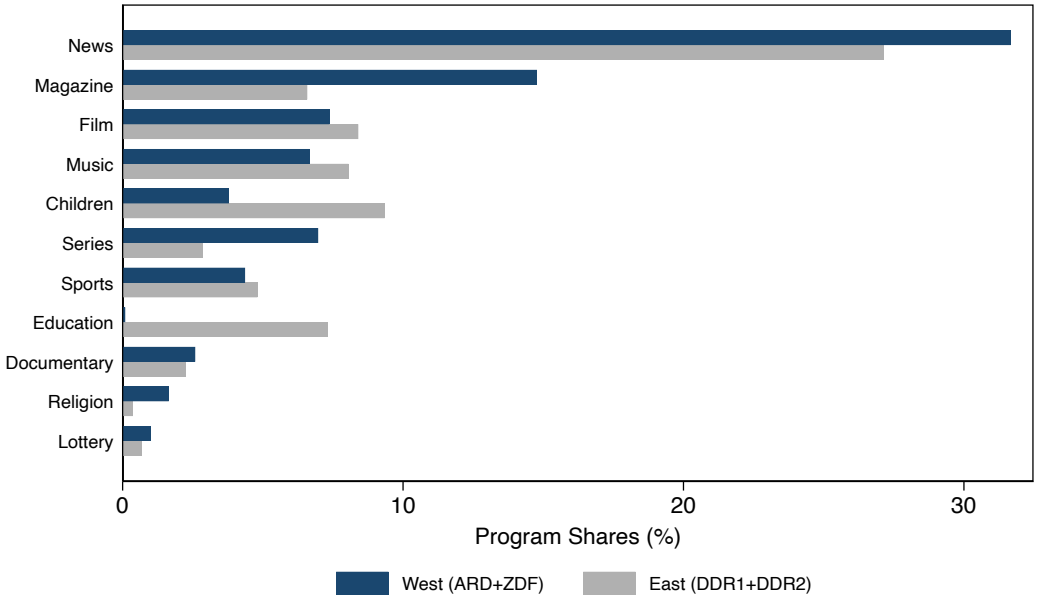
Beyond these broad differences in programming composition, West German television also reported regularly on developments in the GDR. For the analysis below, we manually identified broadcasts whose primary topic was political, economic, or social developments in the GDR. Figure D.2 shows that GDR-related broadcasts were a persistent feature of West German television throughout the 1970s and 1980s. Table D lists the programs that most frequently covered East German affairs together with illustrative examples of their reporting. Much of this coverage appeared in recurring political magazine formats, complementing regular news broadcasts with more in-depth reporting on political developments and everyday life in the GDR.

Figure D.2a shows the distribution of news, magazine, and political magazine broadcasts over the course of the day. These information-oriented formats were broadcast throughout the broadcast schedule, with particularly high concentrations around midday and during the evening hours. Thus, viewers with access to West German television were regularly exposed not only to alternative news reporting but also to in-depth political programming.

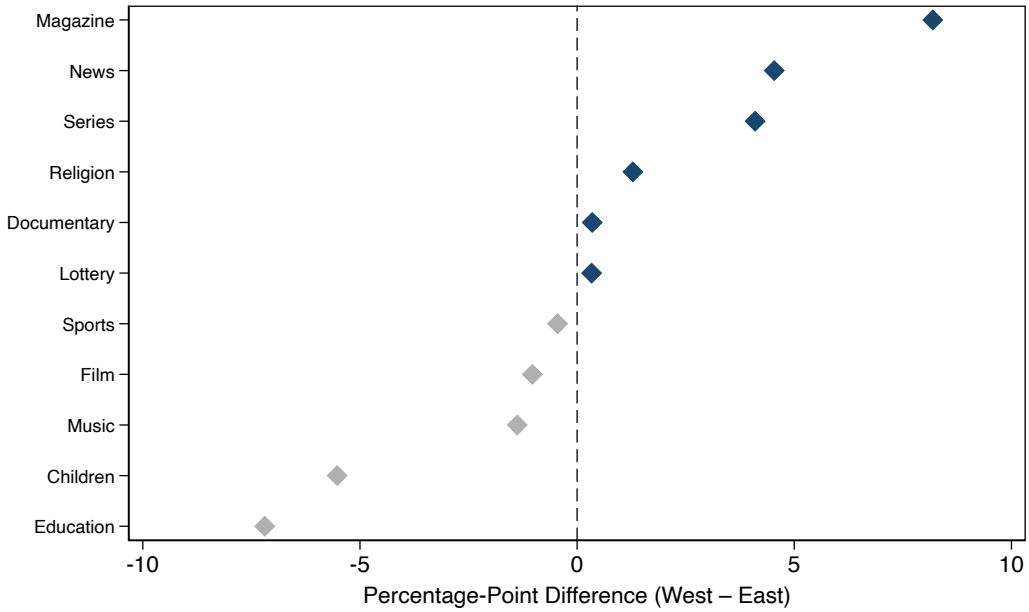
The regularity of this reporting is closely related to the mechanism studied in the paper. East German viewers were therefore exposed not merely to additional information but to competing interpretations of the same political system that they encountered in school, on GDR television, and through other state-controlled media. As discussed in Section 6, students could directly compare the official narratives presented in classrooms and on GDR television with alternative accounts provided by West German broadcasters. The systematic reporting documented in Figure D.2 therefore illustrates how West German television could make inconsistencies in official information more salient, contributing to the erosion of trust in the GDR regime documented in Section 2.

Figure D.1: Television content in West and East Germany

(a) Content Shares



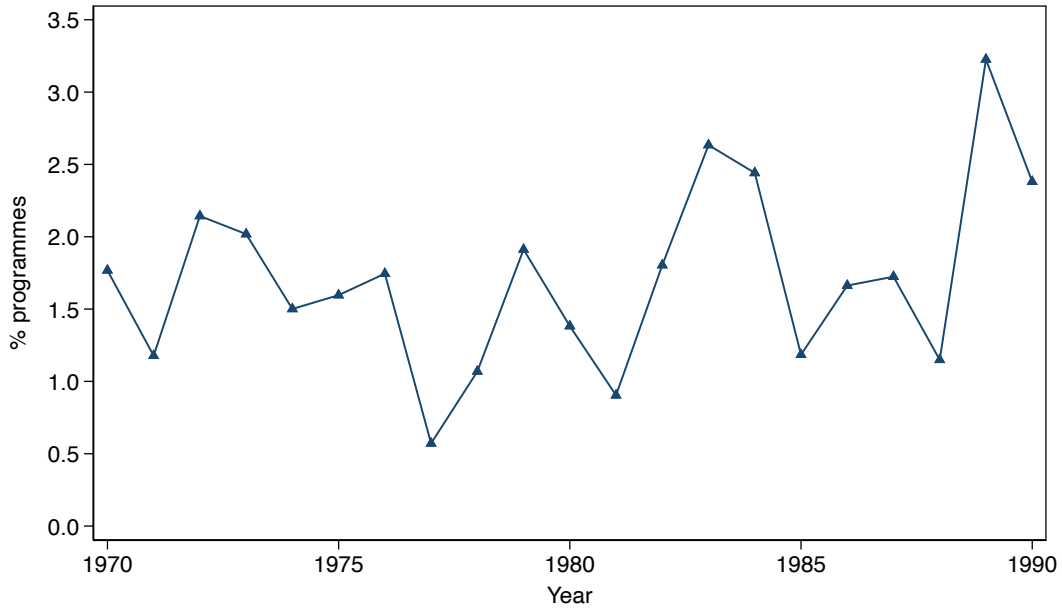
(b) Differences in TV Content



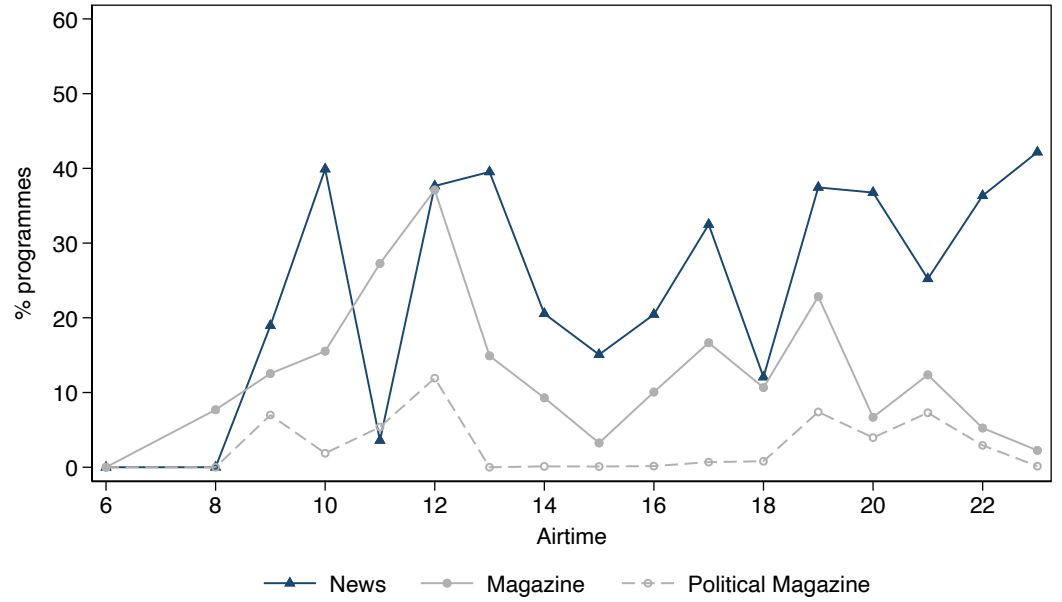
Notes: Only national TV channels. The figure compares the composition of television programming on the two public West German television channels (ARD and ZDF) and the two East German channels (DDR 1 and DDR 2) between 1970 and 1989. Broadcasts were manually classified into broad content categories using the historical program descriptions provided by the Retro-Media and ShoutWiki archives and supplemented, where necessary, by additional historical sources. Only the nationwide ARD program is included; regional early-evening broadcasts are excluded. Panel (a) reports the share of broadcasts by content category. Shares do not sum to 100 percent because several residual categories (e.g., quiz shows, cooking programs, and uncategorized broadcasts) are omitted. Panel (b) reports the percentage-point difference in content shares between West and East German television.

Figure D.2: Reporting on GDR in West German TV

(a) Development over time



(b) West German TV broadcasts reporting on GDR



Notes: Panel A summarizes GDR-related reporting on West German television between 1970 and 1989. GDR-related broadcasts were identified using the historical program descriptions and manually classified whenever the GDR constituted the primary subject of the broadcast. Panel (b) shows the distribution of news, magazine, and political magazine broadcasts over the course of the day.

Table D.1: Examples of Western TV programming covering GDR-related issues

Title	Channel	Type	Airtime	Timeline	Description
Political magazines on GDR-related topics					
Drüben	ZDF	Political magazine	Every other Sunday, initially at 7:40 p.m., later at 7:55 p.m., 20 minutes per episode	1966–1973	A political magazine covering politics, the economy, culture, and everyday life in the GDR. Based on opinion polls, West German citizens were asked about their knowledge of the GDR, while in the recurring segment *Zurück aus drüben* ('Back from Over There'), Western visitors shared their personal experiences and impressions of traveling to the GDR.
Kennzeichen D	ZDF	Political magazine	Wednesday or Friday noon (until 1984) and Thursday noon (since 1985), 45 minutes per episode	1971–2001	A political magazine addressing topics concerning both German states, intended to present an authentic portrayal of everyday life on both sides of the inner-German border. For example, the broadcast of May 24, 1989, reported on electoral fraud during the GDR local elections of May 7, 1989, featuring secret footage filmed by East German opposition activists Siegbert Schefke and Aram Radomski.
Kontraste	ARD	Political magazine	Thursday 9:45–10:15 p.m. every three weeks, 30 minutes per episode	since 1968	The program initially reported on developments in the Eastern Bloc before shifting its focus to inter-German relations in the mid-1980s. For example, on September 27, 1988, Kontraste aired a report on environmental pollution in the Bitterfeld region of the GDR. The report includes video footage produced by East German environmental activists.
News programming and magazines occasionally covering GDR-related topics					
Brennpunkt	ARD	Newscast	Irregular	Since 1971	A complement to ARD's regular news programming, broadcast when major events generated greater demand for in-depth coverage than could be accommodated by standard newscasts. For example, the program reported on the GDR's 40th anniversary (October 7, 1989) and the resignation of the SED Central Committee (December 3, 1989).
Heute	ZDF	Newscast	Several times a day, 20 minutes per episode	Since 1963	*Heute* is the main news program of ZDF.
Heute-Journal	ZDF	News magazine	Weekdays, 15–30 minutes per episode	Since 1978	*Heute-Journal* provides in-depth coverage of individual news topics, offering detailed background information and interviews.

Examples of Western TV programming covering GDR-related issues *(continued)*

Title	Channel	Type	Airtime	Timeline	Description
Monitor and Panorama	ARD	Political magazine	Thursday from 9:45–10:15 p.m., 30 minutes per episode	Monitor (since 1965) and Panorama (since 1961)	Monitor focused on German domestic politics, while Panorama covered a broader range of topics. The three TV magazines, Kontraste, Monitor, and Panorama broadcast alternately every Thursday evening.
Report	ARD	Political magazine	Tuesday evening, 45 minutes per episode	Mainz (since 1966), München (since 1962)	*Report Mainz* and *Report München* cover current political events and are broadcast alternately on Tuesdays.
Tagesschau	ARD	Newscast	Several times a day, 20 minutes per episode	Since 1952	*Tagesschau* is the main news program of ARD.
Tagesthemen	ARD	Newscast	Once per day, 20–35 minutes per episode	Since 1978	Unlike the *Tagesschau*, which is intended to provide only a news overview, the *Tagesthemen* aims to offer viewers additional information.
Tatsachen	ARD	TV report	Sunday afternoon, 30 minutes per episode	1973–1984	Reports on incidents of environmental pollution and on the work of environmental activists. For example, the broadcast of September 23, 1984, addressed the issue of cross-border sulfur emissions between the two German states.

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