When Schools Shape Demography: Schooling under Socialism, Gender Norms and the Fertility Decline in East Germany*

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Abstract

This paper argues that gender-egalitarian work norms may translate into lower fertility when they conflict with institutions organized around a male-breadwinner model—a norm–institution mismatch. East Germany provides a setting in which these forces became sharply misaligned: schooling under socialism emphasized women's continuous employment, while reunification weakened the institutional support for that model. Exploiting the strict East German school-entry cutoff, I find that one additional grade under the GDR school regime reduced fertility by about 0.07 children among cohorts forming families after reunification. Consistent with norm transmission, additional exposure strengthened women's personal work attachment, increased acceptance of maternal employment, and raised women's employment, without gains in educational attainment or household income. Consistent with the mismatch account, the fertility effect is substantially smaller in regions where post-reunification childcare availability remained higher. A within-person vignette experiment points to the same work–family margin: greater exposure increases responsiveness to childcare provision but not to broader economic conditions. Together, the evidence indicates that gender-egalitarian work norms reduce fertility primarily where institutional support for combining work and family is limited.

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1 Introduction

Fertility has fallen below replacement in nearly every high-income country, with far-reaching consequences for economic growth and the fiscal sustainability of welfare states (Jones, 2022). With standard economic factors leaving much of the recent decline unexplained (Becker, 1960; Chatterjee and Vogl, 2018; Cumming and Dettling, 2024), attention has increasingly shifted to cultural change and the transformation of gender norms (Doepke et al., 2023; Kearney and Levine, 2025). Yet the fertility implications of these norms may depend on the institutions that shape the work–family trade-off. Figure 1 illustrates this possibility: across 47 countries, stronger support for women's employment is associated with lower fertility only where formal childcare coverage is relatively low. This cross-country pattern raises a natural question: when do gender norms emphasizing women's continuous employment translate into lower fertility?

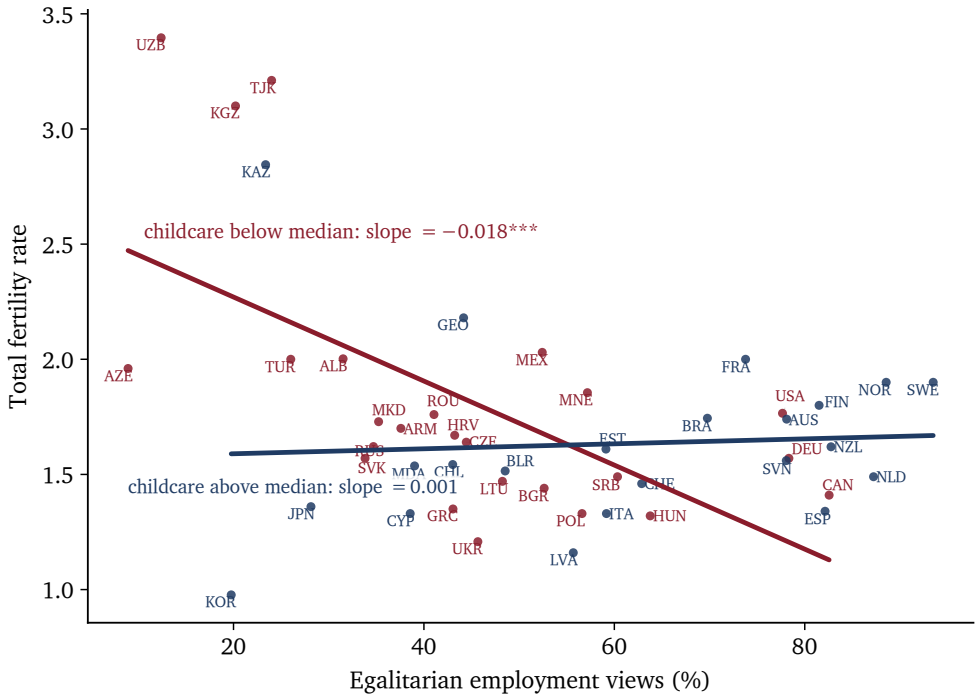


Figure 1. Egalitarian Employment Norms and Fertility, by Childcare Provision

Notes. Each point represents a country ($N = 47$). Egalitarian employment views are the share of respondents rejecting the World Values Survey statement that men should have priority when jobs are scarce. Fertility is the total fertility rate in the same survey year. Countries are split at the sample median of under-three formal childcare coverage. Lines show separate OLS fits below and above the childcare median. Appendix Table B.1 reports the underlying regressions and data sources.

This paper argues that norms emphasizing women's continuous employment lower fertility when they conflict with institutions built around a male-breadwinner model—a norm–institution mismatch. For individuals who regard continuous employment as personally important, parenthood may entail not only monetary costs and forgone earnings, but also a non-pecuniary loss when it requires them to scale back work and depart from an internalized work identity (Akerlof and Kranton, 2000). When employment and parenthood

are difficult to combine, the resulting work–family conflict can be resolved through either lower employment or lower fertility. Stronger attachment to employment makes adjustment through work more costly and can therefore shift adjustment toward lower fertility. Where care remains predominantly maternal, women face this trade-off most directly. Public childcare weakens this conflict by reducing the market work displaced by parenthood. The relevant mismatch is therefore not low childcare provision in absolute terms, but a gap between the support needed to reconcile employment-centered orientations with parenthood and the support actually available.

Testing this interaction is difficult because norms and institutions typically coevolve, making their separate and joint effects hard to identify. German reunification sharply disrupted this alignment. Before 1990, the German Democratic Republic (GDR) deliberately combined the two: schools explicitly promoted women's continuous employment, childcare covered more than 80 percent of children under age three, and female labor-force participation reached 91 percent. Retrospective evidence also points to schooling as an important source of these views. In a survey I fielded, GDR-schooled respondents most often identify school and teachers as the single most important source of their work–family views, and schooling shows a much larger East–West difference than parents, peers, media, or the workplace.

Reunification then transformed both sides of this system. It dismantled the socialist school regime, while funding cuts and facility closures led to a sharp decline in under-three childcare availability (Deutsches Jugendinstitut, 2002). Over the same period, East Germany experienced one of the steepest peacetime fertility declines on record, with the total fertility rate falling from 1.52 to 0.77 (Kreyenfeld and Vatterrott, 2018). Among GDR-schooled respondents who report that reunification changed their family plans, work–family compatibility and childcare rank well above financial insecurity as constraints on fertility. Reunification thus confronted cohorts socialized into norms emphasizing women's continuous employment with institutions far less supportive of combining employment and parenthood.

The empirical challenge is to isolate the effect of GDR schooling from the broader economic and institutional changes triggered by reunification. To do so while holding age at reunification nearly constant, I exploit the GDR's May 31 school-entry rule (Fuchs-Schündeln and Masella, 2016). Children born in May entered school that September, whereas those born in June entered one year later. Among the 1974–1983 cohorts, the cutoff therefore led May-born students to complete one additional compulsory-school grade under the GDR regime rather than the post-reunification regime. I benchmark this local May–June contrast against the adjacent 1970–1973 cohorts, for whom the same school-entry rule did not alter school-regime exposure.

The main analysis uses the 2018–2019 German Microcensus, a mandatory annual household survey covering more than 800,000 individuals each year. In these data, I find that the cutoff-induced increase in GDR school-regime exposure reduced later fertility by 0.072 children, or about six percent of the sample mean. It also lowered the probability of having at least one child in the household by 2.3 percentage points. The May–June gap among the comparison cohorts is effectively zero, so the difference-in-differences estimate of −0.080 closely matches the local contrast. Across all eleven adjacent birth-month pairs, May–June is the most negative contrast and the only one statistically distinguishable from zero.

Three longitudinal surveys—SOEP, NEPS, and FReDA—provide complementary validation. Defining East German origin by birthplace leaves the negative cutoff pattern intact, and May-born status does not significantly predict subsequent East–West residence. SOEP fertility histories further indicate that the result reflects fewer children rather than delayed childbearing: the age-at-first-birth contrast appears in both the exposed and the earlier cohorts, whereas the negative contrast in children ever born is specific to the exposed cohorts.

The fertility response also follows a clear grade profile: approximately −0.15 children when the marginal GDR-regime grade fell in grades 7–10, −0.07 in grades 4–6, and near zero in grades 1–3. This ordering is consistent with the historical intensification of employment-centered content in the upper grades, while also potentially reflecting age-specific receptiveness to socialization.

I next examine whether school-regime exposure shifted attitudes toward maternal employment, personal work attachment, and employment behavior. Greater exposure increases acceptance of maternal employment among women and men alike. Among women, it raises personal work attachment by 0.28 standard deviations and employment by 2.4 percentage points, while the corresponding estimates among men are close to zero. These shifts occur without gains in educational attainment or household income and without a change in retrospectively reported ideal family size.

The fertility consequences also depend on the institutional environment encountered after reunification. Across eastern states, the fertility effect attenuates from approximately −0.13 children at the 25th percentile of childcare availability to close to zero at the 75th percentile, with no corresponding gradient among the comparison cohorts. I complement this cross-state evidence with a within-person vignette experiment that varies institutional support and economic conditions among respondents from the same birth months and cohorts. Greater school-regime exposure produces a substantially larger expected-fertility response to improved institutional support, but no differential response to better economic conditions. The difference between these responses is statistically significant.

Respondents' own accounts point to the same work–family margin. Those with greater GDR-school exposure place greater weight on work–family compatibility and childcare

when describing their own fertility choices. By contrast, the corresponding estimates for financial security, labor-market security, housing, partnership circumstances, and the division of care and housework within the household are all close to zero. They also report requiring a higher level of childcare to realize a desired family size. Together, these findings support the view that stronger attachment to continuous employment translates into lower fertility primarily where institutions make employment and parenthood difficult to combine.

This paper makes three main contributions. First, it provides within-country evidence on what I call a norm–institution mismatch: the fertility consequences of norms emphasizing women's continuous employment depend on the institutional environment in which family decisions are made. Recent work argues that cultural change and shifting gender norms are central to the modern fertility decline (Myong, Park, and Yi, 2021; Doepke et al., 2023; Bloom, Kuhn, and Prettner, 2024; Kearney and Levine, 2025), while a related literature shows that family-policy institutions, especially public childcare, shape the work–family trade-off (Del Boca and Sauer, 2009; Baizan, Arpino, and Delclòs, 2016; Bauernschuster, Hener, and Rainer, 2016; d'Albis, Gobbi, and Greulich, 2017; Olivetti and Petrongolo, 2017; Guner, Kaya, and Sánchez-Marcos, 2024). German reunification provides a setting in which norms and the institutions supporting them were abruptly decoupled, allowing me to study their interaction within a single country. I show that stronger attachment to continuous employment translates into lower fertility when institutions make employment and parenthood difficult to combine.

Second, I contribute to the literature on the determinants and transmission of gender norms. A well-established literature shows that culturally transmitted beliefs shape women's work and fertility (Fernández, Fogli, and Olivetti, 2004; Fernández and Fogli, 2009; Cavapozzi, Francesconi, and Nicoletti, 2021), while more recent work finds that media, edutainment, and school-based interventions can shift such norms (Jensen and Oster, 2009; La Ferrara, Chong, and Duryea, 2012; Kearney and Levine, 2015; Banerjee, La Ferrara, and Orozco, 2019; Dhar, Jain, and Jayachandran, 2022; Edmonds, Feigenberg, and Leight, 2023). I complement this work by showing that sustained, state-directed schooling can shape work–family orientations that persist into adulthood. Greater GDR-school exposure increases acceptance of maternal employment and, among women, strengthens personal attachment to continuous employment and raises employment. These findings also point to schooling as an important determinant of the persistent East–West differences in gender norms and female labor supply documented in earlier work (Bauernschuster and Rainer, 2012; Campa and Serafinelli, 2019; Lippmann, Georgieff, and Senik, 2020; Jessen, 2022; Boelmann, Raute, and Schönberg, 2025).

Third, the paper contributes to research on curricular reform, ideological schooling, and the long-run consequences of socialism. A growing literature shows that school curricula

can shape identity, political preferences, and later-life behavior, especially when states use schools to transmit ideology (Fuchs-Schündeln and Masella, 2016; Cantoni et al., 2017; Bai and Li, 2020; Chen, Lin, and Yang, 2023; Braghieri and Eichmeyer, 2024; Costa-Font, García-Hombrados, and Nicińska, 2024; Li, Luo, and Wang, 2025; Meriläinen and Mitrunen, 2025), and that curricular reforms can have persistent economic effects (Arold, 2024; Arold, Woessmann, and Zierow, 2025). I add to this work by showing that ideological schooling can have demographic consequences whose magnitude depends on the institutional environment encountered in adulthood. The persistence of these effects decades after reunification also connects the paper to research on the legacy of socialism, which documents lasting effects of the East German regime on preferences and economic behavior (Alesina and Fuchs-Schündeln, 2007; Fuchs-Schündeln and Schündeln, 2020; Laudenbach, Malmendier, and Niessen-Ruenzi, 2026). The closest empirical study is Fuchs-Schündeln and Masella (2016), who use related school-entry cutoffs around reunification to study educational attainment and labor-market outcomes among cohorts born between 1971 and 1977.[1] I build on this source of variation but use a different comparison strategy, extend the analysis to the 1974–1983 cohorts, and examine fertility, gender attitudes, and female labor supply.[2]

Section 2 develops the conceptual framework. Sections 3–5 describe the institutional setting, data, and empirical strategy. Sections 6 and 7 present the fertility results and mechanism evidence. Section 8 concludes.

2 Norm–Institution Mismatch: A Conceptual Framework

This section formalizes how attachment to continuous employment can shape fertility when parenthood competes with market work for women's time. School-regime exposure affects the non-pecuniary value of employment, while institutional support determines how much market work parenthood displaces. When employment and parenthood are difficult to combine, stronger attachment to employment makes adjustment through work more costly and can therefore shift adjustment toward lower fertility. A norm–institution mismatch arises

1. They further distinguish between two margins: for cohorts born 1971–1973, reunification primarily relaxes non-meritocratic access restrictions to academic tracks and apprenticeships, whereas for cohorts born 1974–1977 it mainly reduces exposure to socialist teaching content and style.

2. Methodologically, I rely entirely on month-of-birth variation within East Germany, using earlier East German cohorts as an internal comparison group to net out generic consequences of school-entry timing. This differs from Fuchs-Schündeln and Masella (2016), who use West Germany as the comparison group. The within–East Germany design thus abstracts from institutional differences between East and West German schooling systems, including West Germany's different school-entry cutoff and non-trivial rates of early and delayed enrollment (Rüdiger, Kormann, and Peez, 1976).

when strong attachment to continuous employment meets institutions that make employment and parenthood difficult to combine. The framework links these forces to predictions about work attachment, employment, fertility, the grade profile, and the role of public childcare.

2.1 Schooling, Work Orientation, and the Cost of Children

Let s_{ig} indicate whether individual i completed grade g under the GDR school regime rather than under the post-reunification regime. I summarize the individual's attachment to continuous employment as

$$G_i = \sum_{g=1}^{10} \gamma_g s_{ig}, \qquad \gamma_g \geq 0. \tag{1}$$

The parameter γ_g captures how much completing grade g under the GDR regime contributes to this work orientation.[3] It may reflect both differences in curricular content and differences in receptiveness to socialization across ages and developmental stages (John et al., 2017).

Let $\chi_r \in [0,1]$ denote institutional support for combining employment and parenthood in region r. In the benchmark, χ_r represents the share of the maternal care requirement absorbed by institutions, with public childcare as its closest empirical counterpart. Define

$$T_r \equiv 1 - \chi_r$$

as the residual maternal care burden. An additional child requires T_r units of maternal time.[4] A woman divides a unit time endowment between market work ℓ, childcare, and residual non-market time m:

$$m = 1 - \ell - T_r n, \qquad m \geq 0. \tag{2}$$

I place the residual care burden on the woman's time constraint because mothers performed most childcare and housework in East Germany both before and after reunification (Einhorn, 1992; Rosenfeld, Trappe, and Gornick, 2004; Jessen, 2022), while the GDR's universal-worker model prescribed women's continuous employment without a corresponding reallocation of unpaid work to men (Anweiler, 1988; Kim, 2003).

3. The cardinalization of G_i is a normalization. Any monotone transformation leaves the sign predictions unchanged.

4. Appendix A.3 allows for a general maternal-time requirement with economies of scale across siblings and shows that the core fertility result and its disappearance under full institutional support continue to hold. It also extends the mechanism to the first-child margin.

Utility is quasilinear in consumption:

$$U_i \;=\; y + w\ell - \bar{c}\,n \;+\; v(n) \;+\; B(G_i)\,\ell \;+\; h(m), \tag{3}$$

where y is non-labor income, w the wage, and $\bar{c}$ the monetary cost of a child. Children yield utility $v(n)$ and residual non-market time yields utility $h(m)$, with $v' > 0 > v''$ and $h' > 0 > h''$. The term $B(G_i)\,\ell$, with $B(G_i) \geq 0$ and $B'(G_i) > 0$, captures the non-pecuniary value of market work associated with stronger attachment to continuous employment.[5] Quasilinearity suppresses wealth effects from non-labor income and isolates the opportunity-cost channel.

At an interior optimum, the labor-supply condition equates the marginal value of non-market time to the full marginal return to market work:

$$h'(m^*) \;=\; w + B(G_i). \tag{4}$$

The shadow value of the woman's time therefore depends not only on her wage but also on the non-pecuniary value she attaches to employment. Substituting equation (4) into the fertility condition gives

$$v'(n^*) \;=\; \bar{c} \;+\; T_r\,[\,w + B(G_i)\,]. \tag{5}$$

Equation (5) is the central expression of the framework. The marginal cost of a child consists of the direct monetary cost $\bar{c}$, forgone earnings $T_r w$, and the non-pecuniary value of the market work displaced by parenthood, $T_r B(G_i)$. The final term captures the norm–institution mismatch: stronger attachment to employment raises the cost of parenthood only to the extent that institutions leave a residual care burden.

The model therefore predicts an interaction rather than an unconditional fertility ranking across regions. The same increase in work attachment should have larger fertility consequences where institutions leave a greater residual care burden. Childcare availability may thus remain high in absolute terms yet still fall short of the level needed to reconcile continuous employment with parenthood for individuals with stronger work attachment.

Equation (5) also clarifies the distinction between the proposed mechanism and a conventional pecuniary-returns channel. The wage w and the non-pecuniary return $B(G_i)$ enter the opportunity cost of children through the same sum. If GDR school-regime exposure also

5. The formulation follows the identity framework of Akerlof and Kranton (2000), in which agents derive utility from actions that conform to internalized prescriptions for their social category. Accordingly, I use "norm" here to refer to an internalized prescription rather than an external social sanction. In this setting, the norm prescribes continuous, predominantly full-time employment as part of the female role, so the non-pecuniary return to work rises with time spent in market employment.

raised wages, it could generate a similar fertility response and childcare gradient. Distinguishing the two channels therefore requires evidence on whether school-regime exposure changes non-pecuniary work attachment or monetary returns to work.

The benchmark holds the direct utility from children fixed with respect to school-regime exposure. The proposed channel therefore operates through the opportunity cost of parenthood rather than through a direct change in the taste for children. The framework also implies stronger responsiveness to conditions that make employment and parenthood easier to combine, rather than a general tendency to respond more strongly to favorable environments.

The linear specification $B(G_i)\,\ell$ provides a tractable representation of the GDR ideal of continuous female employment. Appendix A develops the comparative statics and extensions.

2.2 Empirical Predictions

The framework yields three empirical predictions.

Prediction 1: Work orientation, employment, and fertility. If completing a grade under the GDR regime raises G_i, greater exposure should strengthen women's attachment to market work and increase women's employment. When institutional support is incomplete, $T_r > 0$, stronger work attachment raises the opportunity cost of children and therefore reduces fertility. Because the residual care burden falls primarily on women in this setting, responses in personal work attachment and employment should be stronger among women. By contrast, schooling may shift general attitudes toward maternal employment among both women and men.

Prediction 2: Grade profile. The fertility effect should be more negative when the marginal GDR-regime grade generates a larger increase in work attachment. Because employment-centered content was most explicit in the upper grades (Section 3.2), the effect should be strongest for grades 7–10 and weakest for grades 1–3, while also potentially reflecting age-specific receptiveness to socialization.

Prediction 3: Institutional complementarity. As institutional support rises, the norm-based opportunity cost $T_r B(G_i)$ declines. The fertility effect of GDR school-regime exposure should therefore be largest where post-reunification childcare availability was lowest and attenuate as support increases. Individuals with stronger work attachment should also require more institutional support to sustain a given family size.

Together, these predictions link school-regime exposure to fertility through work attachment and its interaction with the institutional environment.

3 Institutional Setting

German reunification provides an unusually sharp setting for studying the long-run consequences of schooling under East German socialism and how they depend on the institutional environment encountered in adulthood. Before 1990, East and West Germany maintained fundamentally different family-policy regimes and educational mandates. Reunification transformed East Germany's school system and work–family policy framework along West German lines, abruptly ending socialist schooling for affected cohorts. This section describes the institutional features on which the empirical design rests.

3.1 Divergent Gender Regimes Before Reunification

Following the postwar division, the two German states developed fundamentally different approaches to women's work and motherhood. These differences did not arise incidentally but were deliberately shaped through policy.

In the GDR, women's employment was both a constitutional right and a central ideological objective. This commitment was supported by near-universal public childcare and policies designed around continuous full-time employment. By the late 1980s, childcare covered roughly 95 percent of children aged 3–6 and over 80 percent of those under three. Enterprises frequently provided on-site care, mothers were entitled to shortened working hours with dismissal protection, and one-year fully paid parental leave had been available since 1976. These institutions underpinned a universal-worker model that sought to dissolve women's economic dependence on the traditional male breadwinner (Kim, 2003). By 1989, 91 percent of women participated in the labor force, predominantly in full-time work and largely irrespective of motherhood status (Trappe, 1996). Yet the universal-worker model did not extend to an egalitarian division of unpaid work: East German mothers continued to perform the majority of childcare and housework throughout the socialist period and after reunification (Rosenfeld, Trappe, and Gornick, 2004; Jessen, 2022).

West Germany, by contrast, maintained a policy environment that reinforced the male-breadwinner model. Childcare coverage for children under three remained below 3 percent. Joint taxation and dependent-spouse benefits created strong disincentives for secondary earners, and prevailing cultural norms stigmatized working mothers. Female employment stood at 56 percent in 1989 and was predominantly part-time among mothers (Rosenfeld, Trappe, and Gornick, 2004).[6]

6. Some systematic East–West differences spanning economic structures, political preferences, and gender roles predate the separation itself (Becker, Mergele, and Woessmann, 2020). The empirical design exploits within–East Germany variation generated by birth timing around the school-entry cutoff, and is therefore silent on these longer-run differences.

These institutional contrasts were mirrored in attitudes. Early post-reunification surveys show that East Germans were substantially more likely to support maternal employment (Panel A, Appendix Figure E.1). That gap persisted despite decades of shared federal institutions and media exposure (Panel B, Appendix Figure E.1), consistent with evidence that gender attitudes are especially malleable during childhood and adolescence but relatively stable thereafter (John et al., 2017). Yet that persistence is confined to cohorts socialized before reunification: among those born afterward, the gap disappears, suggesting that convergence in the childhood institutional environment was accompanied by convergence in attitudes (Appendix Figure E.2).

3.2 Schooling under Socialism and the School-Entry Cutoff

Schools were one of the GDR's main instruments for transmitting employment-centered work–family norms. For the empirical strategy, two features matter most: the school regime explicitly sought to transmit prescribed social objectives, and the school-entry rule was strictly enforced. Because this content became more explicit in the upper grades, the design also generates a testable grade profile in exposure to messages about women's employment and economic independence.

Schooling content and gender-role socialization. From the state's founding in 1949, gender equality was embedded as a foundational principle across GDR curricula, textbooks, and pedagogical practice (Anweiler, 1988; Block and Fuchs, 1993). The 1974 Youth Law codified this mandate, requiring schools to cultivate "socialist personalities" with explicit emphasis on gender-egalitarian work values and women's economic independence from the male breadwinner (Jugendgesetz der Deutschen Demokratischen Republik, 1974; Kim, 2003). Within the ten-year Polytechnic Secondary School that all students attended (see Appendix Figure E.3 for an overview of the system's structure), gender-egalitarian messages were not confined to one subject. Reflecting Education Minister Margot Honecker's directive that "there is no non-political subject" (Schmitt, 1978), they were integrated across disciplines: mathematics problems featured female engineers, literature emphasized women's economic contributions, and textbooks depicted women primarily in public rather than domestic roles (Schröter, 2002).[7]

West German schooling operated on fundamentally different principles. Official curricula emphasized fostering students' independent personality development (Ministerium für Kultus und Sport Baden-Württemberg, 1994), and the Beutelsbach Consensus established

7. To illustrate, the official German-language curriculum prescribed a lesson unit tied to International Women's Day (March 8, a state-celebrated holiday in the GDR) requiring teachers to discuss the professional and civic roles of students' mothers (Ministerium für Volksbildung der Deutschen Demokratischen Republik, 1990).

that students must be enabled to form their own opinions rather than be directed toward prescribed political or social objectives (Wehling, 1976). In practice, however, this formal neutrality was embedded in a male-breadwinner framework. Textbooks, for instance, were much more likely to depict women in household settings than in the labor force (Moser and Hannover, 2014; Fruehwirth, Heilemann, and Stoeger, 2024; Jehle et al., 2024).

Retrospective accounts from former students corroborate this contrast. In the retrospective survey of Germans aged 55–66 (Section 7.2), 72 percent of GDR-schooled respondents recalled their schools conveying an egalitarian role model, compared with 18 percent of West-schooled respondents (Appendix Figure C.1). The specific norms recalled mirror the curricular mandate closely. When asked to rate how strongly gender-egalitarian messages were presented as normal or self-evident in school, East Germans score substantially higher on all three elicited items: that women should be able to work regardless of family situation (4.3 vs. 2.0), that mothers of young children should work (4.2 vs. 1.9), and that having a profession is as important for women as for men (4.1 vs. 2.2), all on a 0–5 scale (Appendix Table C.2 and Appendix Figure C.2).

Grade-level intensification. Explicit employment-centered content became increasingly prominent in the middle and upper grades (see Appendix Table E.1 and Figure E.4 for detailed curriculum hours by grade and subject) (Schmitt, 1978). Grades 1–3 focused primarily on basic skills and contained relatively little dedicated content on women's employment and economic independence. From grades 4–6, the curriculum introduced female role models in science and production. In the upper grades (7–10), dedicated subjects such as civics and polytechnic education explicitly framed women's labor-force participation and economic independence as achievements of socialism (Kim, 2003).[8] Retrospective reports from East Germans corroborate this subject-level pattern: among East German respondents, 68 percent cite civics and 47 percent polytechnical education as the subjects in which gender-egalitarian norms were most salient, while natural science subjects and mathematics are mentioned by fewer than 7 percent (Appendix Figure C.3).

School-entry rule. While West Germany exhibits non-trivial rates of early and delayed enrollment around school-entry cutoffs (Rüdiger, Kormann, and Peez, 1976), enforcement in the GDR was strict: children turning six by May 31 entered school that September, and those born on or after June 1 waited one year.[9] When reunification ended the GDR school system, the cutoff generated variation in school-regime exposure for the 1974–1983 cohorts: May-

8. Evidence from the GDR era underscores the special role of civics: asked what most strongly shaped their classmates' worldviews, GDR students ranked civics second only to the family—ahead of peers, media, and every other school subject (Diecke, 1967; Schmitt, 1978).

9. Kindergarten operated separately through voluntary enrollment (Höltershinken, Hoffmann, and Prüfer, 1997).

born students had completed one additional GDR-regime grade relative to their June-born peers.

3.3 Reunification as an Abrupt Institutional Shock

Reunification abruptly terminated both the educational regime described above and the broader work–family institutions with which it had been aligned. In education, the first step was to suspend binding curricula for civics and history and give teachers discretion over lesson design, explicitly to de-ideologize the regime's core political subjects. By the 1990–1991 school year, West German textbooks had replaced socialist materials, and roughly 20 percent of teachers had been dismissed, often for political reasons (Fischer, 1992). The reform was not incremental. A system that had used schooling to transmit prescribed social objectives was replaced by one organized around the principle that students should form their own opinions (Wehling, 1976). In practice, materials portraying gender roles as interchangeable gave way to materials emphasizing individual personality development and more traditional conceptions of family life.[10]

The work–family policy environment changed just as abruptly. Childcare coverage for children under three fell from more than 80 percent in 1990 to between 29 and 54 places per 100 children by 1994, depending on the state, as subsidies disappeared and facilities closed or were converted (Deutsches Jugendinstitut, 2002). West German family policies also replaced GDR provisions, including parental-leave arrangements and tax incentives that made single-earner specialization more attractive. Over the same period, East German fertility fell from 1.52 to 0.77, while West German fertility remained near 1.4 (Appendix Figure E.5).

4 Data

The main empirical analysis uses the German Microcensus, whose large sample provides the precision required for the core fertility and heterogeneity analyses. I complement it with three nationally representative longitudinal surveys—the German Socio-Economic Panel (SOEP), the National Educational Panel Study (NEPS), and the Family Research and Demographic Analysis survey (FReDA)—that provide fertility outcomes and birthplace information, allowing me to examine the cutoff pattern using a birthplace-based definition of East

10. For example, Baden-Württemberg's 1994 primary-school framework stated that education should enable "boys and girls to discover their gender identity" (Ministerium für Kultus und Sport Baden-Württemberg, 1994), a sharp contrast to East German curricula that portrayed gender roles as interchangeable.

German origin. SOEP additionally provides a measure of work orientation unavailable in the Microcensus. Further evidence comes from two surveys fielded shortly after reunification and two original surveys designed to examine alternative explanations and the proposed mechanism.

4.1 German Microcensus

The main estimation sample comes from the 2018–2019 German Microcensus, a mandatory annual household survey covering one percent of the German population.[11] Pooling two adjacent waves increases precision in a design identified from a narrow birth-month comparison and allows age and birth-cohort effects to be controlled for flexibly.

I restrict the sample to individuals residing in East Germany, born between 1970 and 1983, and observed in 2018–2019, by which point most childbearing for these cohorts had already occurred. To implement the cutoff design, I further restrict the analysis to individuals born in May or June. The 1974–1983 cohorts constitute the exposed group, for whom the school-entry cutoff changed the regime under which the marginal compulsory-school grade was completed. The adjacent 1970–1973 cohorts serve as comparison cohorts: they were subject to the same school-entry rule but completed compulsory schooling before reunification.

The baseline outcome is the number of biological or adopted children residing in the household. I also consider two alternative fertility measures available in the Microcensus: the number of children in the family unit and, for women in the relevant survey wave, the number of children ever born. Appendix Tables B.2 and B.3 report summary statistics for the supplementary outcomes and the Microcensus sample.

The Microcensus records current residence rather than birthplace. I therefore define the East German analysis sample using current residence in the former GDR states. Section 6.1 examines the robustness of this classification using longitudinal surveys that observe both birthplace and current residence and reports cutoff estimates under alternative geographic definitions.

4.2 Longitudinal Surveys

I use the nationally representative SOEP, NEPS, and FReDA surveys for robustness checks and mechanism analyses (Blossfeld and Von Maurice, 2011; Goebel et al., 2019; Bujard et al., 2023). All three record birthplace and fertility outcomes, allowing me to define East

11. Research Data Center of the Federal Statistical Office and Statistical Offices of the Federal States of Germany, DOI: 10.21242/12211.2018.00.00.1.1.1 and 10.21242/12211.2019.00.00.1.1.1.

German origin independently of subsequent mobility and to replicate the cutoff analysis using a birthplace-based sample. SOEP additionally provides a career-orientation measure that is unavailable in the Microcensus.

To assess alternative explanations, I also use two surveys fielded shortly after reunification. The Shell Youth Study provides measures of optimism and economic outlook among adolescents and young adults (Fischer et al., 1999), while the DJI Youth Survey provides measures of financial concerns and attitudes toward the former GDR. The SOEP adds annual measures of economic worries and satisfaction from 1990 onward. Appendix B provides further details on the datasets and variable construction.

4.3 Two Original Surveys

Existing datasets do not capture several perceptions and stated responses central to the proposed mechanism. I therefore fielded two original surveys that serve complementary empirical purposes. The first is a *retrospective survey* of Germans who were in their family-forming years around reunification. It elicits the perceived sources of respondents' work–family views, whether reunification affected their family planning, and the constraints they associate with their fertility decisions.

The second is the *May–June cohort survey*, a vignette and autobiographical survey of East Germans born in the same months and cohorts used in the cutoff design. It experimentally varies institutional and economic conditions and elicits respondents' own work orientations, fertility considerations, and childcare requirements. Appendices C and D describe the sampling, fieldwork, and full survey instruments.

5 Identification Strategy

5.1 School-Entry Cutoff and School-Regime Exposure

Identification exploits the interaction between East Germany's school-entry rule and the abrupt school-system transition at reunification. Under the GDR enrollment rule, children who turned six on or before May 31 entered school that September, whereas children born on or after June 1 entered one year later. When reunification replaced the GDR school regime for cohorts still enrolled, this rule translated birth timing into differential exposure to schooling under East German socialism. For the 1974–1983 cohorts, the cutoff changed the regime under which the marginal compulsory-school grade was completed. May-born students were one grade ahead of June-born students and had therefore completed one

additional grade under the GDR regime before the school system changed. Total years of compulsory schooling were unchanged.

Throughout, I focus on East German individuals born in May or June, the narrowest month-of-birth contrast around the May 31 cutoff. Background characteristics are balanced within this window but become less so as more distant birth months are included (Appendix Table F.1). I therefore take the May–June contrast as the baseline local comparison, in the spirit of local-randomization approaches to cutoff designs (Cattaneo, Frandsen, and Titiunik, 2015; Cattaneo, Titiunik, and Vazquez-Bare, 2016).

The narrow window also limits season-of-birth differences, keeps age at reunification nearly constant while preserving the cutoff-induced difference in grade position, and minimizes differences in broader exposure to the GDR regime.

5.2 Empirical Specifications

I begin by comparing May- and June-born individuals among cohorts still in compulsory schooling at reunification:

$$Y_{ic} = \alpha_0 + \alpha_1 \,\mathrm{May}_i + \gamma_c + X_{ic}'\theta + \varepsilon_{ic}, \tag{6}$$

where Y_{ic} denotes the fertility outcome of individual i in birth cohort c, May_i is an indicator for being born in May rather than June, γ_c are birth-year fixed effects, and X_{ic} includes a female indicator and flexible age controls.[12] The coefficient α_1 is the paper's primary local cutoff estimate. Among the exposed 1974–1983 cohorts, May birth implies completion of one additional grade under the GDR school regime rather than the post-reunification school regime, so α_1 captures the local effect of that substitution rather than the effect of an additional year of education.

This estimate may also capture stable consequences of relative age at school entry. I address this concern using the adjacent 1970–1973 cohorts as a comparison group. These cohorts faced the same enrollment rule but had completed compulsory schooling before reunification, so May versus June birth changed relative age at school entry without changing school-regime exposure. A longitudinal survey confirms similarly strong compliance with the school-entry cutoff in the exposed and comparison cohorts (Appendix Figure F.1). The resulting difference-in-differences specification is:

$$Y_{ic} = \delta_0 + \delta_1 \left(\mathrm{May}_i \times \mathrm{Exposed}_c\right) + \delta_2 \,\mathrm{May}_i + \gamma_c + X_{ic}'\theta + u_{ic}, \tag{7}$$

12. I pool two waves of the German Microcensus (2018–2019), which allows me to identify age and birth cohort effects simultaneously.

where Exposed_c indicates the cohorts still in school during reunification. Its main effect is absorbed by the birth-year fixed effects γ_c. The interaction δ_1 differences out any May–June gap common to the exposed and comparison cohorts, isolating the component associated with differential school-regime exposure.

Standard errors are clustered at the cohort–month-of-birth level, the level at which treatment varies. Because the effective number of clusters is limited, I complement conventional cluster-robust inference with wild cluster bootstrap procedures (Cameron, Gelbach, and Miller, 2008; Roodman et al., 2019).

6 School-Regime Exposure and Fertility

This section establishes two main results. First, one additional grade under the GDR school regime reduced later fertility by about 0.07 children. Second, the effect is concentrated in the middle and upper grades, where the GDR curriculum placed greater emphasis on women's employment and economic independence. Figure 2 summarizes both patterns. Section 7 then examines whether the broader evidence is consistent with the proposed work–family mechanism and with institutional complementarity after reunification.

6.1 Average Fertility Effects at the Cutoff

Table 1 reports the baseline May–June estimates for the exposed 1974–1983 cohorts. One additional grade under the GDR school regime reduces fertility by 0.072 children (SE = 0.019; wild cluster bootstrap $p = 0.023$). A meaningful share of the effect appears on the extensive margin: the probability of having at least one child is 2.3 percentage points lower (Appendix Table F.2).

The adjacent 1970–1973 cohorts provide a direct comparison for cutoff-related effects that are unrelated to school-regime exposure. These cohorts faced the same enrollment rule but had already completed compulsory schooling before reunification, so May versus June birth did not change their exposure to the GDR school regime. Their May–June fertility gap is 0.007 children (SE = 0.012). Subtracting this gap from the exposed-cohort estimate yields a difference-in-differences estimate of −0.080 children (SE = 0.023; wild cluster bootstrap $p = 0.043$). The close agreement between the two estimates indicates that stable consequences of the enrollment cutoff, including generic relative-age effects, are unlikely to account for the fertility result.

A second check asks whether the fertility break appears specifically at the statutory school-entry threshold. Across the eleven adjacent birth-month pairs, the May–June

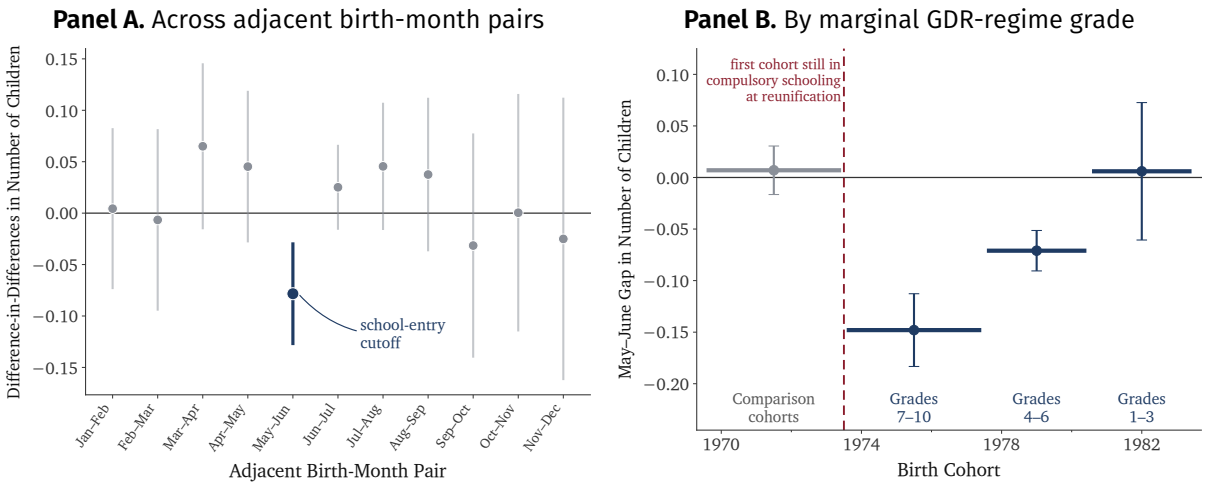


Figure 2. Fertility Effects at the School-Entry Cutoff and by Marginal Grade

Notes. German Microcensus 2018–2019, East German respondents. The dependent variable is the number of children in the household. Panel A reports the difference-in-differences estimate for each adjacent birth-month pair. May–June is the only pair spanning the statutory school-entry cutoff. Panel B reports local May–June estimates by the phase in which the marginal GDR-regime grade occurred: comparison cohorts (1970–1973), grades 7–10 (1974–1977), grades 4–6 (1978–1980), and grades 1–3 (1981–1983). Panel A estimates include birth-year fixed effects. Panel B specifications include age and birth-year fixed effects and a female indicator. Whiskers denote 95 percent confidence intervals based on standard errors clustered at the cohort–month-of-birth level. Appendix Table F.6 reports the Panel A estimates. Appendix Table F.11 reports the Panel B estimates and wild cluster bootstrap *p*-values. Appendix Table F.12 reports the corresponding difference-in-differences estimates by grade.

difference-in-differences estimate is the most negative and the only one statistically distinguishable from zero (Figure 2, Panel A, and Appendix Table F.6). The fertility break therefore appears at the school-entry cutoff rather than along a smooth season-of-birth gradient.

The result is also robust to alternative outcome definitions and specifications. Among women for whom children ever born are observed, the estimate is −0.092 (SE = 0.051). Estimates using children in the family, the 2020 Microcensus wave, state fixed effects, and state-specific cohort trends are similarly close to the baseline estimate (Appendix Tables F.3–F.5).

Table 1. Schooling under Socialism and Fertility

	Exposed cohorts (1974–1983) (1)	Comparison cohorts (1970–1973) (2)	Difference-in-differences (3)
May–June gap	−0.072***	0.007	−0.080***
	(0.019)	(0.012)	(0.023)
Wild cluster bootstrap *p*	[0.023]	—	[0.043]
Observations	5,407	2,289	7,696

Notes. German Microcensus 2018–2019, East German residents. The dependent variable is the number of children in the household. Column (1) reports the local May–June cutoff estimate for the exposed 1974–1983 cohorts, for whom the cutoff changes the regime under which the marginal grade was completed. Column (2) reports the corresponding May–June gap for the adjacent 1970–1973 comparison cohorts, who faced the same enrollment rule but completed compulsory schooling before reunification. Column (3) reports the difference between the two, estimated as the interaction in a pooled regression. All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level. Wild cluster bootstrap *p*-values using Webb weights and 999 replications are reported in brackets for columns (1) and (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

I next use the longitudinal surveys for two additional validations. First, defining East German origin by birthplace leaves the cutoff pattern intact: May–June is again the most negative of the eleven adjacent birth-month comparisons, with no comparable break at placebo boundaries (Appendix Figures F.2 and F.3 and Appendix Table F.7). Given the much smaller panel samples, I widen the birth-month window to improve precision. The resulting estimates remain negative in SOEP, NEPS, and FReDA and are statistically compatible with the Microcensus estimate (Appendix Table F.8). I therefore use the panels primarily to validate the direction and location of the effect, while relying on the much larger Microcensus for the baseline estimate.

The panels also validate the Microcensus residence-based definition of East German origin. Applying the same current-residence definition in the panel data yields an estimate of −0.064 children, close to the Microcensus estimate of −0.072 (Appendix Figure F.4 and Appendix Table F.9). May-born status also does not predict later East–West residence among East-born respondents.

The longitudinal data also provide fertility histories that help separate fertility timing from family size. These data point to fewer children rather than simply delayed childbearing (Appendix Table F.10).

6.2 Heterogeneity by Marginal Grade at Reunification

The average effect masks a pronounced grade profile. Panel B of Figure 2 groups cohorts by the phase in which the cutoff-induced marginal GDR-regime grade occurred. The May–June gap is close to zero among the 1970–1973 comparison cohorts, who had completed compulsory schooling before reunification. Among the exposed cohorts, the estimate is −0.148 children for the 1974–1977 cohorts, whose marginal grade fell in grades 7–10, −0.071 for the 1978–1980 cohorts, whose marginal grade fell in grades 4–6, and 0.006 for the 1981–1983 cohorts, whose marginal grade fell in grades 1–3 (Appendix Table F.11). The corresponding difference-in-differences estimates are −0.160, −0.080, and 0.008 (Appendix Figure F.5 and Appendix Table F.12). The underlying May and June series track each other closely among the comparison cohorts and begin to diverge with the first cohort for which the cutoff changes school-regime exposure (Appendix Figure F.6).

The fertility effect is thus largest when the marginal GDR-regime grade fell in grades 7–10, where employment-centered content was most explicit, and close to zero when it fell in grades 1–3 (Section 3.2). Because marginal grade is also tied to age at exposure, the profile may reflect both curricular intensity and age-specific receptiveness to socialization.

7 Evidence on Norm Transmission and Institutional Complementarity

This section examines whether the fertility effect reflects the norm–institution mismatch described in Section 2. The framework links school-regime exposure to fertility through two margins. Greater exposure should increase acceptance of maternal employment and strengthen personal work attachment and employment, with the latter two responses stronger among women. Its fertility consequences should be smaller where institutional support makes employment and parenthood easier to combine. The evidence is cumulative: the interpretation rests on the alignment of these predictions across distinct datasets and empirical designs rather than on any single auxiliary outcome. I first assess alternative explanations and then report retrospective survey evidence on respondents' work–family views and perceived constraints. I next examine both margins at the school-entry cutoff, beginning with work orientations and employment, before turning to state-level childcare variation and experimental and autobiographical evidence on institutional complementarity.

7.1 Alternative Explanations for the Fertility Effect

I first examine several alternative explanations for the fertility effect. Table 2 reports local May–June estimates for education, household resources, partnership, and fertility preferences using the specification from Table 1. A human-capital mechanism would predict gains in education or productive skills, yet greater school-regime exposure does not increase university completion or further training, and educational attainment is, if anything, slightly lower. I find no evidence of an increase in household or individual net income (Appendix Table F.15).[13] Nor do I find evidence of changes in family circumstances or desired family size. May-born status does not predict whether a partner is present in the household and leaves retrospectively reported ideal family size virtually unchanged (Appendix Table D.7).

Another possible explanation is that the cutoff changed the economic conditions students faced as they entered adulthood after reunification. Because May-born students were one grade ahead of June-born students, they also reached labor-market entry in a different calendar year. Yet labor-market conditions at entry differ little across the cutoff, and youth surveys fielded in the years after reunification show no May–June differences in financial concerns or expectations about the future (Appendix Tables F.16 and F.17). The same pattern appears in later years: in the SOEP, May- and June-born respondents do not differ systematically in worries about their own economic situation or in satisfaction with living

13. Fuchs-Schündeln and Masella (2016) provide complementary evidence for older cohorts who were in upper grades or vocational schooling at reunification: comparing East and West German cohorts born 1971–1977 in a difference-in-differences framework, they also document no change in female wages and a modest reduction in male wages and university completion.

Table 2. Schooling under Socialism and Alternative Explanations: Human Capital, Household Resources, and Family Circumstances

	Human capital			Household resources	Family circumstances	
	Educational attainment (1)	University degree (2)	Further training (3)	Household net income (EUR) (4)	Partner in household (5)	Ideal number of children (6)
May-born	−0.080*	−0.021	−0.006	−51.93	−0.007	0.039
	(0.042)	(0.012)	(0.007)	(33.45)	(0.016)	(0.086)
Observations	5,404	5,407	5,403	5,352	5,407	400

Notes. German Microcensus 2018–2019, East German residents in the exposed 1974–1983 cohorts, for columns (1)–(5), and the May–June cohort survey, restricted to 400 respondents in the exposed cohorts, for column (6). Each column reports the local May–June estimate from a separate regression. Educational attainment is measured by ISCED level (1–8). Further training indicates participation during the previous year. Household net income is constructed from bracket midpoints, with the top-coded category assigned 1.25 times its lower threshold. Ideal number of children is the number respondents report having considered ideal before family formation. Microcensus specifications include age and birth-year fixed effects and a female indicator, with standard errors clustered at the cohort–month-of-birth level. The survey specification includes birth-year fixed effects and respondent gender, with standard errors clustered at the birth-year × birth-month level. Individual net income is reported in Appendix Table F.15. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

standards and household income during the two decades after reunification. Taken together, the evidence does not point to meaningful May–June differences in economic circumstances or perceptions after reunification.

A related concern is that the regime change disrupted schooling itself. Disruption common to all pupils cancels in the May–June contrast, because May- and June-born children experienced the transition at nearly the same age. Disruption concentrated in particular grades could contribute to the slightly lower educational attainment in Table 2. On its own, however, such disruption does not naturally account for the higher employment and stronger work attachment observed among May-born women (Section 7.3).

Overall, these results provide little support for the main alternative explanations.

7.2 Retrospective Evidence on the Work–Family Margin

I fielded a retrospective survey of 900 Germans aged 55 to 66, 600 of whom were schooled in the GDR and 300 in West Germany (Appendix C). These cohorts were of family-forming age around reunification.[14]

The responses point to a clear work–family pattern. Sixty-six percent of GDR-schooled respondents, compared with 11 percent of West-schooled respondents, report that reunification changed their family plans (Appendix Table C.2, Panel C). Among GDR-schooled respondents reporting such a change, 73 percent identify either work–family compatibility or the loss of public childcare as the single most important reason, compared with 13 percent

14. To limit backward rationalization, all questions on schooling and socialization preceded the reunification and family-planning modules.

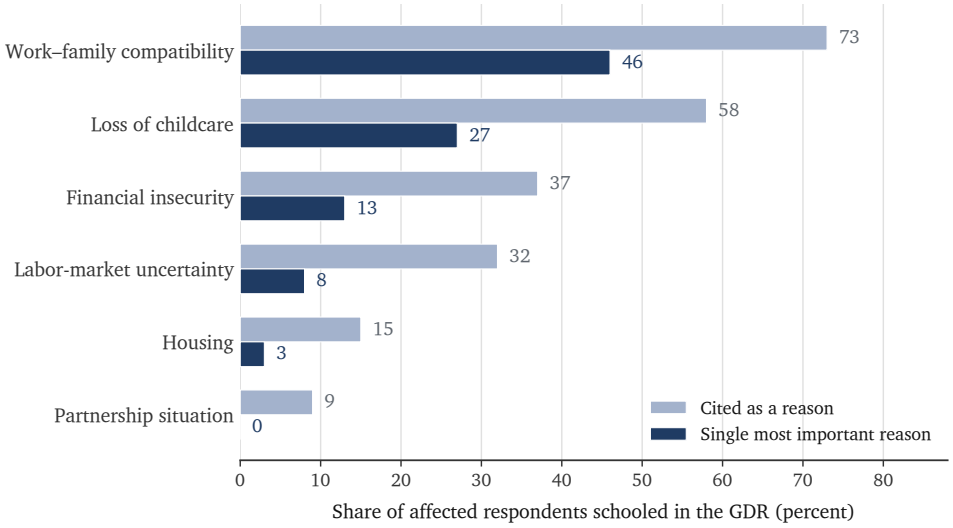


Figure 3. Retrospective Reports: Perceived Fertility Constraints

Notes. Retrospective survey of Germans aged 55–66. The figure is restricted to GDR-schooled respondents who report that reunification affected their family planning ($N = 396$). Bars show the share citing each reason and the share identifying it as the single most important reason. Multiple-response items were presented in randomized order. Appendix C reports the full instrument and results.

who identify financial insecurity (Figure 3). A separate life-course battery, elicited without explicit reference to reunification, yields the same ranking (Appendix Table C.2, Panels C and D).

The survey also points to schooling as a distinctive perceived source of these work–family views. Among GDR-schooled respondents, school and teachers are the most commonly cited main source, at 34 percent (Appendix Figure C.4), and 72 percent recall egalitarian gender-role models being conveyed in school (Appendix Figure C.1), consistent with the archival evidence in Section 3.2.

The same work–family pattern appears along two additional margins. Among women, 64 percent of GDR-schooled respondents report that having a child would have strongly constrained their career, compared with 35 percent of West-schooled women; among men, the corresponding shares are 21 and 20 percent (Appendix Figure C.6). GDR-schooled respondents also rate post-reunification childcare as substantially less adequate relative to their own expectations—1.7 rather than 3.0 on a zero-to-five scale—consistent with the large decline in childcare provision relative to the levels they had experienced under the GDR (Appendix Figure C.5).

Taken together, the retrospective evidence points to the two margins emphasized by the framework: schooling as a source of attachment to continuous employment and work–family compatibility as a constraint on fertility. I next examine these margins at the school-entry cutoff, beginning with work orientations and employment.

7.3 Work Orientations and Employment

I focus on two related but distinct dimensions of work orientation. Acceptance of maternal employment measures whether respondents view mothers' employment as compatible with children's well-being. Because it captures a general social norm rather than respondents' own employment choices, it may respond among both women and men. Personal work attachment instead captures the importance respondents place on maintaining their own employment and professional activity. It is the closest empirical counterpart to the nonpecuniary value of work in Section 2.

Table 3 presents the results. Acceptance of maternal employment increases among women and men alike. In the May–June cohort survey, May-born women score 0.197 standard deviations higher on the four-item acceptance index (SE = 0.109), while May-born men score 0.144 standard deviations higher (SE = 0.075). The two estimates do not differ statistically, and the pooled May–June difference is 0.194 standard deviations (SE = 0.060; Appendix Table F.18). Greater exposure to the GDR school regime therefore increased acceptance of maternal employment without a pronounced gender difference.

Table 3. Schooling under Socialism, Work Orientations, and Employment

	Acceptance of maternal employment		Personal work attachment		Employment probability	
	Women (1)	Men (2)	Women (3)	Men (4)	Women (5)	Men (6)
May-born	0.197*	0.144*	0.277***	−0.029	0.024*	0.009
	(0.109)	(0.075)	(0.079)	(0.113)	(0.012)	(0.008)
Observations	223	177	223	177	2,637	2,770

Notes. East German respondents in the exposed 1974–1983 cohorts. Each entry reports the local May-born minus June-born estimate. Columns (1)–(4) use the May–June cohort survey described in Appendix D, so that maternal-employment acceptance and personal work attachment are measured for the same respondents. Columns (5)–(6) use the Microcensus, with an indicator for being employed as the dependent variable. The acceptance index averages four items on mothers' employment, coded so that higher values indicate greater acceptance. The work-attachment index captures recalled attachment to continuous employment after leaving school. Outcomes in columns (1)–(4) are standardized to mean zero and unit standard deviation. Appendix Table F.18 reports the pooled estimates, a current-views version of the work-attachment index, and an independent SOEP measure of career orientation. All specifications include birth-year fixed effects. Microcensus specifications additionally include age fixed effects. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The estimated response in personal work attachment is larger among women. Among women, May-born respondents score 0.277 standard deviations higher on an index asking them to recall their attachment to continuous employment in the period after leaving school, while the corresponding estimate among men is −0.029. A parallel index of current work orientations yields a similarly large estimate for women, 0.276 standard deviations, and a smaller, imprecisely estimated response of 0.154 for men (Appendix Table F.18). A broader SOEP measure points in the same direction: among women, the importance attached to career success rises by 0.237 standard deviations (Appendix Table F.18). Together, these

results indicate that greater school-regime exposure strengthened women's attachment to continuous employment. This gender pattern is consistent with the GDR's universal-worker model, which placed particular emphasis on women's continuous employment.

Employment behavior moves in the same direction. In the Microcensus, May-born women from the exposed cohorts are 2.4 percentage points more likely to be employed, compared with 0.9 percentage points among men. The same gender pattern therefore appears in both personal work attachment and employment behavior, consistent with the maternal-employment margin emphasized by the framework.[15]

Taken together, these results help distinguish the proposed mechanism from a conventional pecuniary-returns channel. In the framework, both wages and the non-pecuniary value of employment raise the opportunity cost of children. The combination of stronger work attachment and higher female employment, without corresponding gains in education or income, therefore points toward the non-pecuniary value of work rather than higher productivity or monetary returns.

7.4 Institutional Complementarity: State-Level Childcare Variation

I next turn to the second margin: whether the fertility effect is smaller where post-reunification institutions provided more support for combining employment and parenthood. Public childcare is the closest empirical counterpart to institutional support in the framework because it reduces the maternal time burden associated with parenthood. Because higher pecuniary returns could generate a similar gradient (Section 2), I interpret this heterogeneity together with the preceding evidence on work attachment and monetary returns.

I examine this prediction using state-level variation in public childcare after reunification. The principal moderator is the number of public childcare places per 100 children under age three in 1994, the earliest consistently available post-transition measure. I also examine four alternative measures of state-level childcare support. Because the 1994 measure predates family formation for most exposed cohorts, it is less likely than later childcare provision to reflect their own fertility choices. At the same time, the broad cross-state ordering of childcare availability persisted beyond the immediate post-reunification period, and the results are robust to using later values instead (Appendix Table F.13). Coverage varied substantially across the eastern states, from 28.7 places per 100 children in Berlin to 54.1 in Brandenburg. These differences emerged as facilities closed and public funding contracted,

15. Among employed women, weekly hours are about one hour lower (Appendix Table F.15). This is consistent with the additional employment occurring partly in lower-hours jobs, which may help explain why higher employment does not translate into higher income.

while fiscal capacity and political decisions shaped how much childcare each state retained (Deutsches Jugendinstitut, 2002; Borchardt, 2023).

Column (1) of Table 4 shows a pronounced gradient. At the 25th percentile of 1994 childcare availability, the implied fertility effect is −0.127 children, with a standard error of 0.024. At the 75th percentile, the implied effect is −0.015, with a standard error of 0.045. The underlying treatment-by-childcare interaction is 0.011, with a standard error of 0.006 (Appendix Table F.13). The fertility response is therefore substantially larger where post-reunification childcare availability was lower and approaches zero where it was higher.

Columns (2)–(5) use alternative measures of institutional support: childcare-center density, public childcare spending per child, broader childcare capacity, and childcare's share of state youth-welfare expenditure. The implied effect at the 25th percentile ranges from −0.068 to −0.105 children and is significant throughout, while the implied effect at the 75th percentile is close to zero in every case.

Table 4. Schooling under Socialism and Fertility by Post-Reunification Childcare Support

	Measure of state-level childcare support, 1994				
	Under-3 childcare availability (1)	Childcare center density (2)	Public childcare spending per child (3)	Broad childcare capacity (ages 0 to 6.5) (4)	Childcare budget share (5)
Effect at 25th percentile	−0.127***	−0.095***	−0.105***	−0.068**	−0.070**
	(0.024)	(0.028)	(0.040)	(0.028)	(0.031)
Effect at 75th percentile	−0.015	−0.018	−0.030	−0.028	−0.022
	(0.045)	(0.043)	(0.038)	(0.034)	(0.037)
Observations	5,407	5,407	5,407	5,407	5,407

Notes. German Microcensus 2018–2019, East German respondents in the exposed 1974–1983 cohorts. The dependent variable is the number of children in the household. Each column uses a different state-level measure of childcare support, all measured in 1994, and reports the implied effect of one additional GDR-regime grade evaluated at the 25th and the 75th percentile of that measure, $\hat{\beta}_{\text{May}} + \hat{\beta}_{\text{May}\times M}\, Q_p(M)$, where M denotes the measure and $Q_p(M)$ its pth percentile. Appendix Table F.13 reports the underlying coefficients, the variable definitions and data sources, the percentile values, and the same exercise using 1998 values. All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Taken together, the cross-state estimates are consistent with institutional complementarity. The fertility response to GDR school-regime exposure is larger where childcare availability was lower and approaches zero where availability remained comparatively high. This gradient is absent among the 1970–1973 comparison cohorts, who faced the same school-entry cutoff but no differential school-regime exposure (Appendix Table F.14), making a general state-specific May–June fertility pattern less plausible. At the same time, the cross-state variation in childcare is observational. Although much of it likely reflects the supply-side changes described above, some may also reflect latent demand and other state-level differences.

7.5 Experimental and Autobiographical Evidence on Institutional Complementarity

I complement the cross-state evidence with the May–June cohort survey, which also provides the work-orientation measures used above. The survey includes 500 East Germans born in May or June between 1970 and 1983: 400 in the exposed cohorts and 100 in the comparison cohorts. A within-person vignette experiment tests whether the May–June difference in expected-fertility responses is larger for improved institutional support than for improved economic conditions. An autobiographical module then elicits the considerations respondents recall as important for their own fertility decisions and the childcare coverage they consider necessary for an employment-oriented woman to realize her desired family size.

Vignette experiment. Each respondent evaluated hypothetical post-reunification East German regions in two blocks. In the institutional-support block, regions differed in childcare availability, waiting-list pressure, and opening hours, while broader economic conditions were held fixed. In the economic-conditions block, regions differed in job security, unemployment, income predictability, and housing affordability, while childcare and family transfers were held fixed. Each block contained a less favorable and a more favorable region. Both block order and vignette order within each block were randomized.

After each vignette, respondents predicted lifetime fertility for a woman from their own birth cohort: her expected number of children, the likelihood of having at least one child, and the likelihood of having two or more children. Within each block, I take the difference between the more and less favorable vignette for each outcome and average the three standardized differences into an expected-fertility index. I then compare this within-person response between May- and June-born respondents. Any level difference in stated fertility between the two birth-month groups therefore drops out.

Figure 4, Panel A, summarizes the results. Among the exposed cohorts, May-born respondents show a larger expected-fertility response to improved institutional support, with a May–June difference of 0.165 standardized units (SE = 0.058). The corresponding difference for improved economic conditions is −0.027 (SE = 0.028). The difference between the two responses is 0.193 standardized units (SE = 0.069). Respondents with greater school-regime exposure therefore show a larger expected-fertility response to improved institutional support, without a corresponding differential response to the economic improvements described in the vignettes. All three components of the expected-fertility index move in the same direction (Appendix Table D.2).

The estimated May–June difference among the 1970–1973 comparison cohorts is small and imprecise, at 0.020 standardized units (SE = 0.080). Among the exposed cohorts, the differential response is largest when the marginal GDR-regime grade fell in grades 7–10 and

smallest when it fell in grades 1–3: the estimates are 0.348, 0.050, and 0.006 for grades 7–10, 4–6, and 1–3, respectively (Appendix Table D.4). This ordering parallels the grade profile of the effects on realized fertility.

Autobiographical evidence. The autobiographical module asks which considerations respondents recall as important for their own fertility decisions. In the exposed cohorts (Figure 4, Panel B), May-born respondents report placing greater weight on work–family compatibility, by 0.160 standard deviations (SE = 0.065), and on childcare for children under age three, by 0.179 standard deviations (SE = 0.060). The corresponding estimates for financial security, labor-market security, housing, and partnership circumstances are close to zero. The institutional-constraints index yields a May–June estimate of 0.169 standardized units, compared with −0.010 for the economic-constraints index (Appendix Table D.5). The same ranking appears when respondents name a single most important consideration (Appendix Table D.6). Taken together, the experimental and autobiographical evidence converges on the same work–family margin.

There is no corresponding May–June difference in the reported importance of the division of childcare and housework or in the perceived adequacy of partner-provided care (Appendix Table D.9). These estimates concern reported importance and adequacy, rather than the actual allocation of care within the household.

In a complementary descriptive exercise, adding measures of work attachment and acceptance of maternal employment attenuates the May-born coefficient in both the vignette and autobiographical specifications.[16]

Required childcare coverage. Respondents also report the minimum childcare coverage they consider necessary for an employment-oriented woman to realize her desired family size, as well as the coverage they recall in the region relevant to their own family decisions.[17] Among respondents in the exposed cohorts, the May–June difference in required coverage is 3.1 places per 100 children under age three (SE = 0.51). Among the 384 respondents who also assessed local availability, the difference in required coverage is 2.9 places, while the difference in recalled availability is only 0.10 (SE = 0.61). The resulting May–June difference in the perceived shortfall is 2.8 places per 100 children (SE = 0.91, Appendix Table D.8). The larger perceived shortfall therefore reflects a higher level of support considered necessary, not lower recalled availability.

This pattern matches the required-support implication of Prediction 3, formalized in Appendix A.2: stronger work attachment raises the institutional support needed to sustain a

16. The May-born coefficient falls from 0.165 to 0.125 in the vignette specification and from 0.169 to 0.118 in the autobiographical specification (Appendix Table D.10).

17. The item is framed as a system-level coverage rate rather than a per-child guarantee, since not every family requires a place simultaneously.

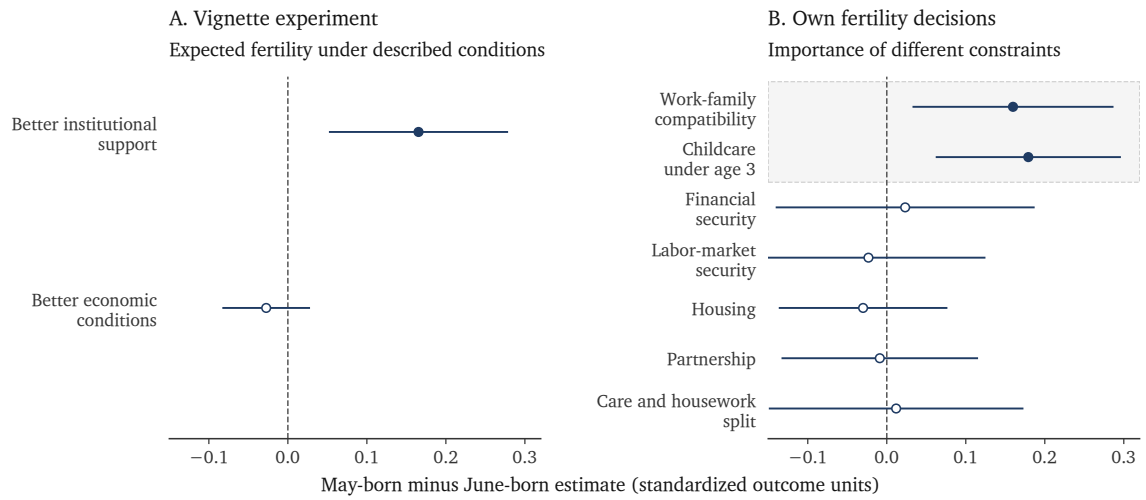


Figure 4. Schooling under Socialism and the Work–Family Margin: Experimental and Autobiographical Evidence

Notes. The figure reports May-born minus June-born estimates for the exposed 1974–1983 cohorts. Panel A shows responses to improved institutional support and improved economic conditions in the vignette experiment. The expected-fertility index combines standardized responses for expected number of children, likelihood of at least one child, and likelihood of two or more children. Panel B reports standardized importance ratings from respondents' own fertility decisions. All specifications include birth-year fixed effects and respondent gender. Whiskers denote 95 percent confidence intervals based on standard errors clustered at the birth-year × birth-month level. Filled circles indicate significance at the 5 percent level. Appendix Table D.3 reports the underlying estimates.

given family size. The mismatch is therefore relative rather than absolute. The same childcare environment may be sufficient for individuals with weaker work attachment but insufficient for those more strongly attached to continuous employment. Together with the household results above, these results point to public childcare as the more relevant institutional margin in this setting.

Overall, the evidence is consistent with a norm–institution mismatch: greater school-regime exposure strengthens women's attachment to employment, while its fertility consequences are weaker where institutional support makes employment and parenthood easier to combine.

8 Conclusion

This paper studies how childhood socialization interacts with adult institutions to shape fertility. German reunification provides a setting in which cohorts socialized under one work–family model made their fertility decisions under another. Exploiting the GDR's May 31 school-entry cutoff, I find that one additional grade under the GDR school regime reduced fertility by about 0.07 children among cohorts forming families after reunification.

The broader pattern of evidence is consistent with norm transmission and institutional mismatch. The fertility effect is concentrated in the middle and upper grades, where school

content most explicitly emphasized women's employment. Greater school-regime exposure also increases acceptance of maternal employment, strengthens women's attachment to continuous employment, and raises women's employment, without corresponding gains in educational attainment or household income and with little change in retrospectively reported ideal family size. Consistent with the mismatch mechanism, the fertility effect is substantially weaker in regions where post-reunification childcare availability remained higher.

Experimental and autobiographical evidence points to the same work–family margin. Greater school-regime exposure increases responsiveness of expected fertility to institutional support but not to more favorable economic conditions. Respondents with greater exposure also place greater weight on work–family compatibility and childcare in their own fertility decisions and report requiring more childcare to realize a desired family size. A separate retrospective survey reinforces this interpretation by pointing to schooling as an important source of work–family views and to work–family compatibility and childcare as central perceived constraints on fertility.

Taken together, the findings suggest that reunification created a mismatch between cohorts' attachment to continuous employment and the institutions they encountered in adulthood. Where childcare availability remained comparatively high, the fertility response was small. Where support was weaker, stronger attachment to employment made parenthood more costly and shifted adjustment toward lower fertility.

The findings speak to a broader debate about low fertility in high-income countries. Recent accounts emphasize cultural change as an important driver of fertility decline (Doepke et al., 2023; Kearney and Levine, 2025). The evidence here highlights a complementary point: the demographic consequences of changing gender norms depend on whether institutions adapt to the work–family model those norms imply. When norms and institutions evolve at different speeds, greater attachment to employment need not translate into lower labor supply. It may instead translate into lower fertility. Where work–family compatibility rather than household resources is the binding constraint, policies operating mainly through transfers may therefore have limited traction on their own (Bergsvik, Fauske, and Hart, 2021). More broadly, the results suggest that the demographic effects of cultural change depend on the institutional environment in which family decisions are made.

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Appendix A Derivations for the Conceptual Framework

This appendix derives the comparative statics underlying Section 2 and extends the linear benchmark to a general maternal-time requirement that allows for economies of scale across siblings and the first-child margin.

A.1 Linear Benchmark

Recall that attachment to continuous employment is summarized by

$$G_i = \sum_{g=1}^{10} \gamma_g s_{ig}, \qquad \gamma_g \geq 0, \tag{A.1}$$

where s_{ig} indicates whether woman i completed grade g under the GDR regime rather than the post-reunification regime. Let $\chi_r \in [0,1]$ denote institutional support for combining employment and parenthood and define

$$T_r \equiv 1 - \chi_r$$

as the residual maternal-time requirement per child. In the linear benchmark, the time constraint is

$$m = 1 - \ell - T_r n, \qquad m \geq 0. \tag{A.2}$$

Utility is

$$U_i = y + w\ell - \bar{c}n + v(n) + B(G_i)\ell + h(m), \tag{A.3}$$

with $v' > 0 > v''$, $h' > 0 > h''$, $B(G_i) \geq 0$, and $B'(G_i) > 0$. For an interior solution, the first-order conditions are

$$w + B(G_i) = h'(m^*), \tag{A.4}$$

$$v'(n^*) = \bar{c} + T_r h'(m^*). \tag{A.5}$$

Combining them gives

$$v'(n^*) = \bar{c} + T_r[w + B(G_i)]. \tag{A.6}$$

Thus the marginal cost of a child consists of the direct monetary cost $\bar{c}$, forgone earnings $T_r w$, and the non-pecuniary value of the market work displaced by parenthood, $T_r B(G_i)$.

A.2 Comparative Statics and Empirical Predictions

For the comparative statics, write $B \equiv B(G_i)$.

Prediction 1: Work orientation, employment, and fertility. Differentiating equation (A.6) with respect to B gives

$$\frac{\partial n^*}{\partial B} = \frac{T_r}{v''(n^*)} < 0 \qquad \text{for } T_r > 0. \tag{A.7}$$

Similarly, equation (A.4) implies

$$\frac{\partial m^*}{\partial B} = \frac{1}{h''(m^*)} < 0. \tag{A.8}$$

Using $\ell^* = 1 - m^* - T_r n^*$,

$$\frac{\partial \ell^*}{\partial B} = -\frac{\partial m^*}{\partial B} - T_r \frac{\partial n^*}{\partial B} > 0. \tag{A.9}$$

Stronger attachment to continuous employment therefore reduces fertility and increases labor supply. At an extensive-margin corner, a higher B also weakly increases the incentive to participate in market work.

Because B is increasing in G_i and G_i is linear in school-regime exposure,

$$\frac{\partial n^*}{\partial s_{ig}} = \frac{T_r}{v''(n^*)} B'(G_i)\gamma_g \leq 0, \tag{A.10}$$

with a strict inequality whenever $\gamma_g > 0$ and $T_r > 0$. The corresponding effect on labor supply is nonnegative.

Prediction 2: Grade profile. Holding the remaining determinants of fertility fixed, if upper-grade exposure has greater effective socialization intensity, $\gamma_U > \gamma_L$, and $T_r > 0$, equation (A.10) implies

$$\left|\frac{\partial n^*}{\partial s_{iU}}\right| > \left|\frac{\partial n^*}{\partial s_{iL}}\right|. \tag{A.11}$$

The fertility response to school-regime exposure is therefore larger when the marginal GDR-regime grade has greater effective socialization intensity.

Prediction 3: Institutional complementarity. Because $T_r = 1 - \chi_r$, equation (A.10) implies the boundary result

$$\lim_{\chi_r \to 1} \frac{\partial n^*}{\partial s_{ig}} = 0. \tag{A.12}$$

The fertility effect of school-regime exposure therefore vanishes when institutions fully absorb the maternal-time requirement.

Global monotonic attenuation in χ_r additionally depends on curvature. In the linear benchmark, $v''' \leq 0$ is sufficient. To see this, note that equation (A.6) gives

$$\frac{\partial n^*}{\partial \chi_r} = -\frac{w+B}{v''(n^*)} > 0,$$

so differentiating equation (A.7) with respect to χ_r yields

$$\frac{\partial}{\partial \chi_r}\left(\frac{\partial n^*}{\partial B}\right) = -\frac{1}{v''(n^*)} + \frac{T_r\,(w+B)\,v'''(n^*)}{[v''(n^*)]^3} > 0 \qquad \text{if } v''' \leq 0. \tag{A.13}$$

Because $\partial n^*/\partial B < 0$, equation (A.13) implies that the fertility response to stronger work attachment becomes less negative as institutional support rises. The same holds for the response to school-regime exposure in equation (A.10).

Required institutional support. For an interior target family size $\bar{n}$, define the level of institutional support that makes $\bar{n}$ satisfy the fertility condition. Provided

$$0 < v'(\bar{n}) - \bar{c} < w + B(G_i), \tag{A.14}$$

the implied residual maternal-time requirement is

$$T_i^{\text{req}}(\bar{n}) = \frac{v'(\bar{n}) - \bar{c}}{w + B(G_i)},$$

and the required level of institutional support is

$$\chi_i^{\text{req}}(\bar{n}) = 1 - \frac{v'(\bar{n}) - \bar{c}}{w + B(G_i)}. \tag{A.15}$$

Differentiating gives

$$\frac{\partial \chi_i^{\text{req}}(\bar{n})}{\partial G_i} = \frac{[v'(\bar{n}) - \bar{c}]B'(G_i)}{[w + B(G_i)]^2} > 0. \tag{A.16}$$

Individuals with stronger attachment to continuous employment therefore require more institutional support to sustain the same target family size. This formalizes the paper's distinction between absolute childcare provision and the level of support needed to reconcile employment and parenthood.

A.3 General Maternal-Time Requirement

The linear benchmark assumes that each additional child requires the same amount of maternal time. The core mechanism does not depend on this assumption. Let $\tau(n, \chi_r)$ denote the total residual maternal-time requirement and suppose that, for $n > 0$ and $\chi_r < 1$,

$$\tau_n > 0, \qquad \tau_\chi < 0, \qquad \tau_{n\chi} < 0, \qquad \tau(0, \chi_r) = 0, \qquad \tau(n, 1) = 0. \tag{A.17}$$

Children increase the maternal-time requirement, institutional support reduces both its total and marginal amount, and full support eliminates the residual requirement. Economies of scale across siblings correspond to $\tau_{nn} \leq 0$.

The time constraint becomes

$$m = 1 - \ell - \tau(n, \chi_r),$$

and the fertility condition is

$$v'(n^*) = \bar{c} + \tau_n(n^*, \chi_r)[w + B(G_i)]. \tag{A.18}$$

Differentiating with respect to B gives

$$\frac{\partial n^*}{\partial B} = \frac{\tau_n}{v''(n^*) - \tau_{nn}[w + B(G_i)]} < 0, \tag{A.19}$$

where the denominator is negative by the second-order condition. Thus stronger work attachment continues to reduce fertility in the presence of economies of scale across siblings, provided the second-order condition holds.

At a given family size, $\tau_{n\chi} < 0$ implies that institutional support reduces the norm-based component of the marginal cost of a child, $\tau_n(n, \chi_r)B(G_i)$. The total magnitude of $\partial n^*/\partial B$ need not decline monotonically over the full range of χ_r, because both n^* and the denominator in equation (A.19) vary with institutional support. In the linear benchmark, $v''' \leq 0$ is sufficient for monotonic attenuation, as equation (A.13) shows. Regardless of curvature, $\tau(n, 1) = 0$ implies $\tau_n(n, 1) = 0$, so the fertility response vanishes under full institutional support.

The mechanism also applies to the first-child margin. Let $V_0(B)$ and $V_1(B)$ denote maximized utility without and with one child. With interior labor supply, the labor-supply condition (A.4) pins down the same residual non-market time in both states, so

$$\ell_1^* = \ell_0^* - \tau(1, \chi_r).$$

The envelope theorem therefore gives

$$\frac{\partial (V_0 - V_1)}{\partial B} = \ell_0^* - \ell_1^* = \tau(1, \chi_r) > 0. \tag{A.20}$$

Stronger attachment to continuous employment therefore makes remaining childless relatively more attractive than having a first child. Because $\tau_\chi(1, \chi_r) < 0$, institutional support attenuates this first-child wedge, which disappears under full support.

Together, these extensions show that the core norm–institution mismatch mechanism does not depend on linear childcare costs or on fertility adjustment occurring only at the intensive margin.

Appendix B Supplementary Data Sources

This appendix describes the supplementary datasets used in the analysis alongside the German Microcensus.

B.1 National Education Panel Study (NEPS)

The National Education Panel Study (NEPS) is a large-scale longitudinal survey documenting educational trajectories and life courses in Germany (Blossfeld and Von Maurice, 2011). It follows six starting cohorts, ranging from newborns to adults, across repeated waves. I use Starting Cohort 6, which tracks the educational and professional careers of a representative adult sample born between 1944 and 1988. I use survey waves from 2007/2008 through 2022. NEPS provides retrospective educational biographies, including respondents' state of birth, which I use to define East German origin. In NEPS, fertility is measured as the number of children residing in the household.

I also use the NEPS school-episode records to document the school-entry rule (Appendix Figure F.1).

B.2 German Socio-Economic Panel (SOEP)

The German Socio-Economic Panel (SOEP) is a nationally representative longitudinal household survey covering a wide range of topics (Goebel et al., 2019). I use SOEP Core 1984–2022 (v. 39), which has followed individuals since 1984 with periodic sample refreshes. In the SOEP, fertility is measured as the number of children in the household. The survey also records the importance respondents attach to career success, which I use in Appendix F. For the years 1990–2010, I additionally use its annual items on worries about one's own

economic situation and job security and on satisfaction with life, the living standard, and household income (Appendix Table F.17).

B.3 Family Research and Demographic Analysis (FReDA)

The Family Research and Demographic Analysis (FReDA) survey is a large-scale panel designed to be nationally representative of individuals aged 18 to 49 in Germany (Bujard et al., 2023). The survey combines self-administered web and paper questionnaires. Data collection began in early 2021 with an initial sample of approximately 38,000 respondents. In FReDA, fertility is measured as the total number of biological children with the current partner and with former partners.

B.4 Additional Datasets

Shell Youth Study (1991). The Shell Youth Study is a nationally representative survey series of young people in Germany, focusing on social values, political orientations, and economic expectations. The 1991 wave, fielded in November 1991 shortly after reunification, covered 13- to 29-year-olds (Fischer et al., 1999). To focus on cohorts exposed to the school-regime transition in the main design, I restrict the analysis to East German respondents born between 1974 and 1978, the youngest cohorts covered by the survey, who were aged 13 to 17 at the time of interview. I use measures of optimism about respondents' personal future and society's future.

DJI Youth Survey (1992, 1997, and 2003). The DJI Youth Survey is a nationally representative study conducted by the German Youth Institute, covering adolescents and young adults aged 16 to 29 in East and West Germany. The survey collects information on financial circumstances, life satisfaction, and attitudes toward social change in the aftermath of reunification. I use respondents' satisfaction with their financial situation and their agreement that people were better off in the past.

World Values Survey. The World Values Survey is a repeated cross-national survey of social and political values (Inglehart et al., 2022). I use it for the cross-country pattern in Figure 1. Egalitarian employment views are measured as the country-level share of respondents who reject the statement that "when jobs are scarce, men should have more right to a job than women." Each country enters once, at its most recent available wave, and is matched to its total fertility rate in the same survey year (Azevedo, 2011) and the most recent available share of children under age 3 in formal childcare from Eurostat and UNECE time series and the OECD Family Database. Table B.1 reports the corresponding regressions and detailed data sources.

Table B.1. Egalitarian Employment Norms, Childcare Provision, and Fertility across Countries

	All countries (1)	Childcare below median (2)	Childcare above median (3)	Difference in slopes (4)	Countries
Baseline (Figure 1)	−0.0083**	−0.0182***	0.0011	0.0193***	47
	(0.0031)	(0.0050)	(0.0039)	(0.0063)	
Additional country-level controls					
+ Log GDP per capita	−0.0030	−0.0098	0.0015	0.0168**	47
	(0.0037)	(0.0058)	(0.0044)	(0.0062)	
+ Weeks of paid maternity leave	−0.0109***	−0.0215***	0.0010	0.0209***	43
	(0.0037)	(0.0052)	(0.0048)	(0.0069)	
+ Pre-primary enrollment ratio	−0.0038	−0.0120**	0.0023	0.0159**	45
	(0.0035)	(0.0052)	(0.0045)	(0.0060)	
+ Survey year	−0.0077**	−0.0179***	0.0015	0.0197***	47
	(0.0032)	(0.0049)	(0.0040)	(0.0062)	
Alternative norm measures					
Neutral responses excluded	−0.0098***	−0.0165***	−0.0002	0.0163***	47
	(0.0028)	(0.0039)	(0.0041)	(0.0058)	
Women respondents only	−0.0092***	−0.0176***	0.0003	0.0179***	47
	(0.0031)	(0.0046)	(0.0040)	(0.0061)	
Respondents aged 18–45	−0.0084***	−0.0188***	0.0005	0.0193***	47
	(0.0030)	(0.0047)	(0.0037)	(0.0060)	
Women aged 18–45	−0.0098***	−0.0182***	−0.0006	0.0175***	47
	(0.0029)	(0.0042)	(0.0039)	(0.0058)	

Notes. The table reports the country-level regressions behind Figure 1. The dependent variable is the total fertility rate from the World Bank World Development Indicators, retrieved with the *wbopendata* module (Azevedo, 2011). The regressor is the share of respondents rejecting the World Values Survey statement that "when jobs are scarce, men should have more right to a job than women" (Inglehart et al., 2022), expressed per percentage point, matching the horizontal axis of Figure 1. The baseline coefficient of −0.0182 implies that a 10-point increase in the share holding egalitarian employment views goes with 0.18 fewer children per woman. Norms and fertility are measured in each country's World Values Survey year, 1996 to 2022 with a median of 2017. The 47 countries are all those for which the norm measure, the total fertility rate, and a comparable under-three formal childcare measure are jointly available. Pre-primary enrollment is the gross enrollment ratio in pre-primary education, which covers older pre-school children rather than the under-three margin and can exceed 100 percent where over-age children are enrolled. Countries are split at the median of the most recent available share of children under age 3 in formal childcare, 30 percent, from Eurostat and UNECE time series and the OECD Family Database (Brazil: IBGE). Columns (1)–(3) report the slope in the pooled sample and within each childcare group. Column (4) reports the difference between the slope above and below the childcare median, estimated from the interaction of the norm measure with an indicator for above-median childcare. Each row is a separate specification. The alternative norm measures exclude neutral responses rather than counting them as non-egalitarian, or recompute the share of egalitarian responses within the indicated respondent group. The fertility measure and the childcare split are unchanged throughout. The number of countries in columns (2) and (3) is 25 and 22 in the baseline, 24 and 19 with maternity leave, and 24 and 21 with pre-primary enrollment. Each country contributes one observation, and conventional OLS standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2. Descriptive Statistics for Supplementary Outcome Variables

	Mean	Std. dev.	Min.	Max.	Obs.	Data Source	Waves
Number of Children	1.326	1.064	0	8	475	NEPS, FReDA, SOEP	2021
Currently Living in East Germany	0.751	0.432	0	1	3,133	NEPS, SOEP	1991–2021
Work-Orientation Measure							
Career Success Important for Life Fulfillment	0.000	1.000	−3.081	1.474	544	SOEP	1992, 1995, 2004, 2008, 2012, 2016, 2021
Future Outlook and Economic Attitudes							
Satisfaction with financial situation	4.722	2.667	0	10	579	DJI Youth Survey	1992, 1997, 2003
People were better off in the past	2.744	0.935	1	4	577	DJI Youth Survey	1992, 1997, 2003
Optimism about personal future	2.296	0.558	1	3	81	Shell Youth Study	1991
Optimism about society's future	1.765	0.426	1	2	81	Shell Youth Study	1991
Post-Reunification Economic Circumstances							
Worries about own economic situation	2.200	0.630	1	3	2,308	SOEP	1990–2010
Worries about job security	1.927	0.727	1	3	1,504	SOEP	1990–2010
Life satisfaction	6.960	1.639	0	10	2,318	SOEP	1990–2010
Satisfaction with living standard	6.880	1.783	0	10	1,577	SOEP	1990–2010
Satisfaction with household income	5.506	2.364	0	10	2,232	SOEP	1990–2010

Notes. Descriptive statistics for the supplementary outcomes used in the analysis. Samples correspond to the estimation samples for each outcome. For the outcomes of Appendix Table F.17, they are the May–June samples of column (1). The career-orientation measure is standardized to mean zero and unit standard deviation, with higher values indicating stronger career orientation. SOEP worries are coded so that higher values indicate greater concern, and the SOEP satisfaction items are measured on a 0–10 scale. The SOEP rows count person-year observations.

Table B.3. Descriptive Statistics: Microcensus Sample (2018–2019)

Variable	Obs.	Mean	Std. dev.	Min.	Max.
Demographics					
Female (=1)	5,407	0.488	0.500	0	1
Year of birth	5,407	1978.733	2.886	1974	1983
Month of birth (5 = May, 6 = June)	5,407	5.491	0.500	5	6
Age	5,407	39.402	2.988	34	45
Household and Labor Market					
Unemployed (=1)	5,402	0.067	0.256	0	1
Monthly household net income (EUR)	5,352	3,467.788	2,169.374	75	22,500
Monthly individual net income (EUR)	5,377	1,917.156	1,324.345	0	22,500
Weekly working hours	4,873	33.067	15.158	0	80
Education					
Highest ISCED education level	5,404	4.146	1.663	1	8
University degree (=1)	5,407	0.296	0.456	0	1
Further training last year (=1)	5,403	0.216	0.412	0	1
Family and Relationship Status					
Not married (=1)	5,407	0.493	0.500	0	1
Has children (=1)	5,407	0.676	0.469	0	1
Number of children in household	5,407	1.148	1.007	0	6
Number of children in family	5,407	0.999	1.018	0	6
Employment Rates					
Overall employment	5,407	0.901	0.298	0	1
Male employment	2,770	0.915	0.279	0	1
Female employment	2,637	0.887	0.317	0	1

Notes. German Microcensus 2018–2019. The sample consists of May- and June-born respondents from the exposed 1974–1983 cohorts who currently reside in East Germany. Income measures use bracket midpoints. The top-coded category of €18,000 or more is assigned €22,500. Weekly working hours are observed conditional on employment.

Appendix C Retrospective Survey

C.1 Sample and Fieldwork

I fielded a retrospective survey of German residents born between 1960 and 1970 through *Talk Online*, a major German online panel provider. The survey was conducted between February 27 and March 12, 2026, and the median completion time was 4 minutes and 23 seconds. To increase precision in East–West comparisons, the sample oversamples respondents who attended school in the former German Democratic Republic (GDR). The final sample comprises 900 respondents: 600 who attended school in the former GDR and 300 who attended school in West Germany. Respondents were screened out if they failed an embedded attention check, were born outside the 1960–1970 cohort range, or did not attend school in Germany.[18] Childhood region is defined using a retrospective question on the federal state in which respondents went to school. Respondents are classified as East German if the reported state is Brandenburg, Mecklenburg-Vorpommern, Saxony, Saxony-Anhalt, Thuringia, or East Berlin. All other German states are classified as West German.

C.2 Questionnaire Structure

The questionnaire comprises three substantive modules and a brief closing section. The full instrument is reproduced in English in Appendix Section C.4. The German originals of both surveys, exactly as shown to respondents, are collected at https://a-bertermann.github.io/questionnaires_german.pdf. I summarize here the design choices most relevant for interpretation.

Module ordering. To limit backward rationalization, all questions on schooling and socialization precede questions on reunification and family planning. Key items that rely on retrospective recall include an explicit "cannot assess / do not remember" option.

Schooling and socialization (Questions 10–14). This module measures the socialization channel underlying the paper's proposed mechanism. Respondents first rate how strongly five channels shaped their views on work–family compatibility: parents, school, peers, media, and the workplace. They then indicate which gender-role model they perceived their schooling to convey: egalitarian, traditional, none, or cannot remember. The most direct items ask respondents to rate, on a 0–5 scale, the extent to which three specific egalitarian norms were presented as normal or self-evident in school: that women should be able to work regardless of family situation, that mothers of young children should be employed,

18. An additional 9 respondents were excluded on the basis of a bot-detection check.

and that having a profession is as important for women as for men. These items map directly onto the gender-egalitarian content documented in Section 3.2. For respondents schooled in the GDR, a supplementary item asks in which subjects these norms were most salient, with response categories aligned to the GDR curriculum.

Reunification and family planning (Questions 15–19). This module elicits whether reunification affected family planning and, if so, how. Respondents who report an effect indicate whether it took the form of fewer children, more children, delayed timing, or both a change in number and timing, and report the resulting change in planned fertility. They then select the reasons for any reported change from a randomized list, including deterioration in work–family compatibility, loss of childcare, financial insecurity, labor-market uncertainty, housing, and partnership circumstances, and identify the single most important factor. Randomizing the order of the listed reasons reduces the scope for ordering effects.

Childcare and perceived constraints (Questions 20–22). The final substantive module captures perceived institutional and structural constraints on fertility independently of reunification. Respondents rate how well public childcare in their region after reunification matched their expectations on a 0–5 scale and report how strongly an additional child, or for childless respondents a first child, would have constrained their career development. A separate battery then asks respondents to rate the importance of eight factors—work–family compatibility, childcare availability, financial insecurity, career demands, housing, social support, health, and partnership stability—in shaping their fertility decisions over the life course, elicited without reference to reunification. The battery is administered separately to parents and childless respondents, with wording adapted accordingly.

C.3 Sample Characteristics and Descriptive Patterns

Appendix Table C.1 reports summary statistics for the analysis sample by childhood region. The stratified sampling design yields similar age and gender composition across the East and West German samples. Appendix Table C.2 reports the full survey results by childhood region, covering socialization channels for work–family views, perceived effects of reunification on family planning, and life-course fertility constraints.

Table C.1. Retrospective Survey: Sample Characteristics

	East ($N = 600$)	West ($N = 300$)	Δ	p-value
Panel A: Demographics (Q1–Q5)				
Birth year	1963.1	1963.3	−0.2	0.37
Female (%)	51.7	50.3	1.3	0.71
Childless (%)	23.0	16.0	7.0	0.01
Currently in East Germany (%)	76.2	3.7	72.5	<0.001
Panel B: Socioeconomic status (Q6–Q8)				
Employed full-time (%)	43.2	41.7	1.5	0.67
Employed part-time (%)	16.0	12.7	3.3	0.19
Self-employed (%)	3.8	8.7	−4.8	<0.01
Retired (%)	25.7	25.7	0.0	1.00
Not employed (%)	11.3	11.3	0.0	1.00
Currently married (%)	50.5	41.7	8.8	0.01
Divorced (%)	17.8	15.7	2.2	0.42
Net income ≥ 2,000 (%)	48.0	56.8	−8.8	0.02

Notes. Retrospective survey of 900 Germans aged 55–66, stratified by region of schooling. East refers to respondents schooled in the former GDR states, including East Berlin. West refers to respondents schooled in the former West German states, including West Berlin. Δ reports the raw East–West difference. p-values are from two-sided t-tests for continuous variables and χ^2 tests for proportions. See Appendix C.4 for the full instrument.

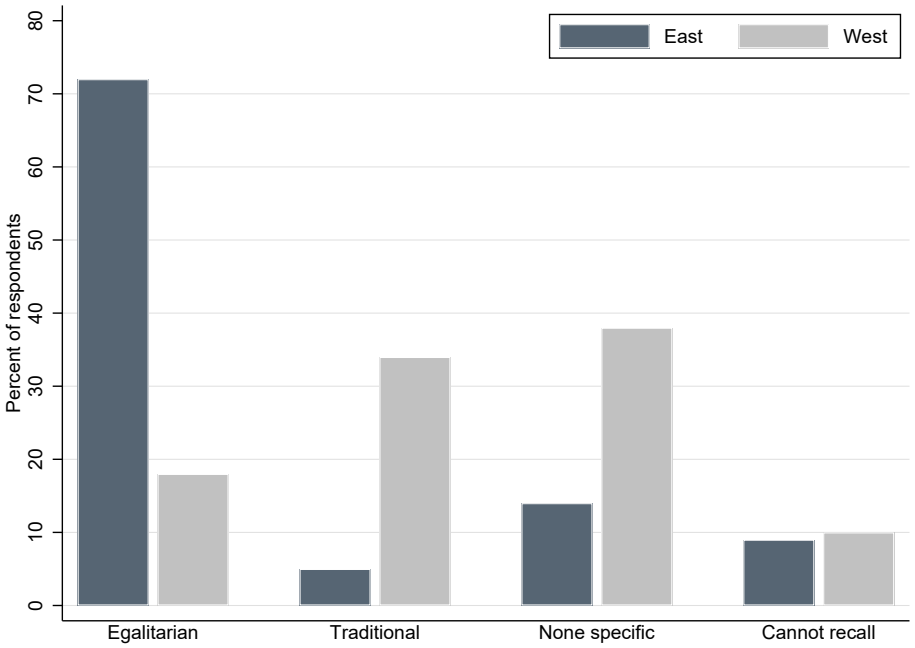


Figure C.1. Recalled Gender Role Models in School by Region

Notes. Data come from the retrospective survey described in Appendix C. The figure uses the full sample of 900 respondents aged 55–66, including 600 GDR-schooled and 300 West-schooled respondents, and shows the share recalling each type of gender-role model conveyed in school, by region. Response categories: egalitarian division of roles, traditional division of roles, no specific gender roles, cannot remember.

Table C.2. Survey Evidence on Socialization, Reunification Effects, and Fertility Constraints

	East	West	Δ	*p*-value
Panel A: Socialization channels (Q10–Q11)				
Share rating strongly formative (%):				
School and teachers	47	23	24***	<0.001
Parents and family	60	58	2	0.565
Peers and acquaintances	26	25	1	0.746
Media	20	18	2	0.474
Workplace	35	34	1	0.766
Single most important channel (%):				
School and teachers	34	14	20***	<0.001
Parents and family	32	43	−11***	0.001
Peers and acquaintances	8	10	−2	0.332
Media	6	7	−1	0.571
Workplace	12	16	−4	0.110
Cannot say	8	10	−2	0.332
Panel B: Gender role models in school (Q12–Q14)				
Recalled type of role model (%):				
Egalitarian	72	18	54***	<0.001
Traditional	5	34	−29***	<0.001
None specific	14	38	−24***	<0.001
Cannot remember	9	10	−1	0.627
School-transmitted norms (mean, 0–5):				
Mothers should work	4.2	1.9	2.3***	<0.001
Women employed regardless	4.3	2.0	2.3***	<0.001
Career equally important	4.1	2.2	1.9***	<0.001
Panel C: Reunification and family planning (Q15–Q19)				
Reunification affected planning (%)	66	11	55***	<0.001
Direction of effect (among affected East, Q16, %):				
Fewer children	76			
Timing only	12			
Both fewer and delayed	8			
More children	4			
Change in planned number of children (Q17):				
Mean change	−1.0			
Share reporting −1 (%)	65			
Share reporting ≤ −2 (%)	19			
Reasons cited (% cited, Q18 / % most important, Q19):				
Work–family compatibility	73 / 46			
Loss of childcare	58 / 27			
Financial insecurity	37 / 13			
Labor-market uncertainty	32 / 8			
Housing	15 / 3			
Partnership situation	9 / 0			
Panel D: Life-course fertility constraints (mean, 0–5, Q22/Q22′)				
Work–family incompatibility	3.6	2.6	1.0***	<0.001
Inadequate childcare	3.3	2.7	0.6***	<0.001
High professional demands	2.9	2.4	0.5***	<0.001
Financial insecurity	2.7	2.7	−0.0	0.94
Housing	2.4	2.6	−0.2*	0.07
Lack of family/social support	2.3	2.1	0.2	0.12
Missing or unstable partnership	1.8	1.9	−0.1	0.40
Health	1.4	1.5	−0.1	0.38
Panel E: Childcare adequacy and career–fertility trade-offs (Q20–Q21′)				
Childcare met expectations (mean, 0–5)	1.7	3.0	−1.3***	<0.001
Children strongly constrain career (%):				
Women	64	35	29***	<0.001
Men	21	20	≈ 0	0.83

Notes. Retrospective survey of 900 Germans aged 55–66, including 600 respondents schooled in the GDR and 300 schooled in West Germany. Panel A reports perceived sources of work–family views. Panel B reports recalled gender-role content in school. Panel C reports perceived effects of reunification on family planning and, among affected respondents, the direction and magnitude of those changes and the reasons cited. Panel D reports life-course fertility constraints on a 0–5 scale. Panel E reports perceived childcare adequacy and career constraints from children, where the career-constraint rows give the share rating 4 or 5 on the underlying 0–5 scale. Primed item numbers denote the version shown to childless respondents. Unprimed numbers denote the version shown to parents. Higher values indicate stronger perceived influence, constraint, or adequacy as indicated by the row label. *p*-values are from two-sided *t*-tests or χ^2 tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

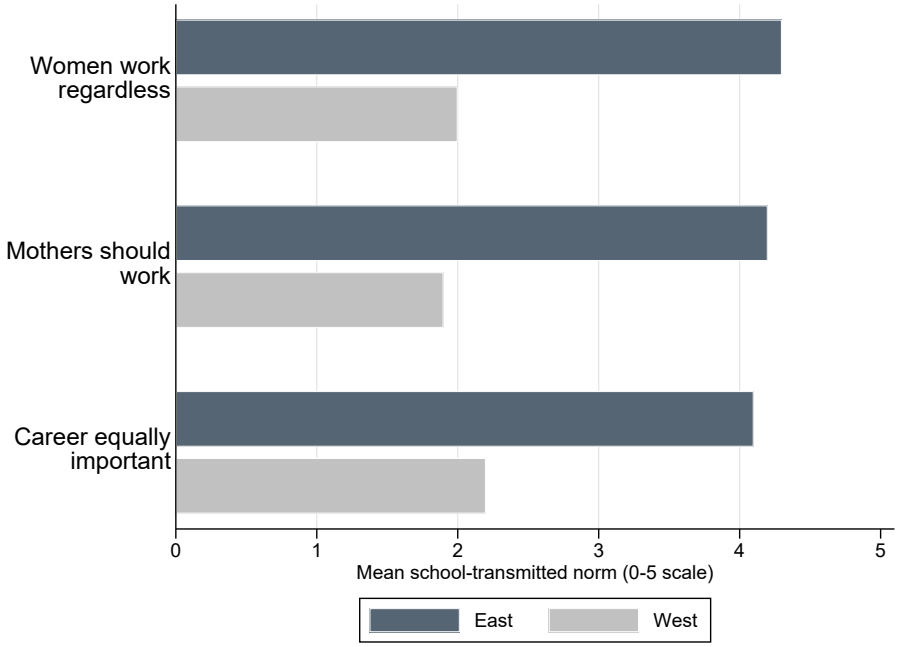


Figure C.2. School-Transmitted Employment-Centered Gender Norms by Region

Notes. Data come from the retrospective survey described in Appendix C. The figure uses the full sample of 900 respondents aged 55–66, including 600 GDR-schooled and 300 West-schooled respondents, and shows the mean rating of how strongly each norm was conveyed as normal or self-evident in school (0 = not at all, 5 = very strongly), by region.

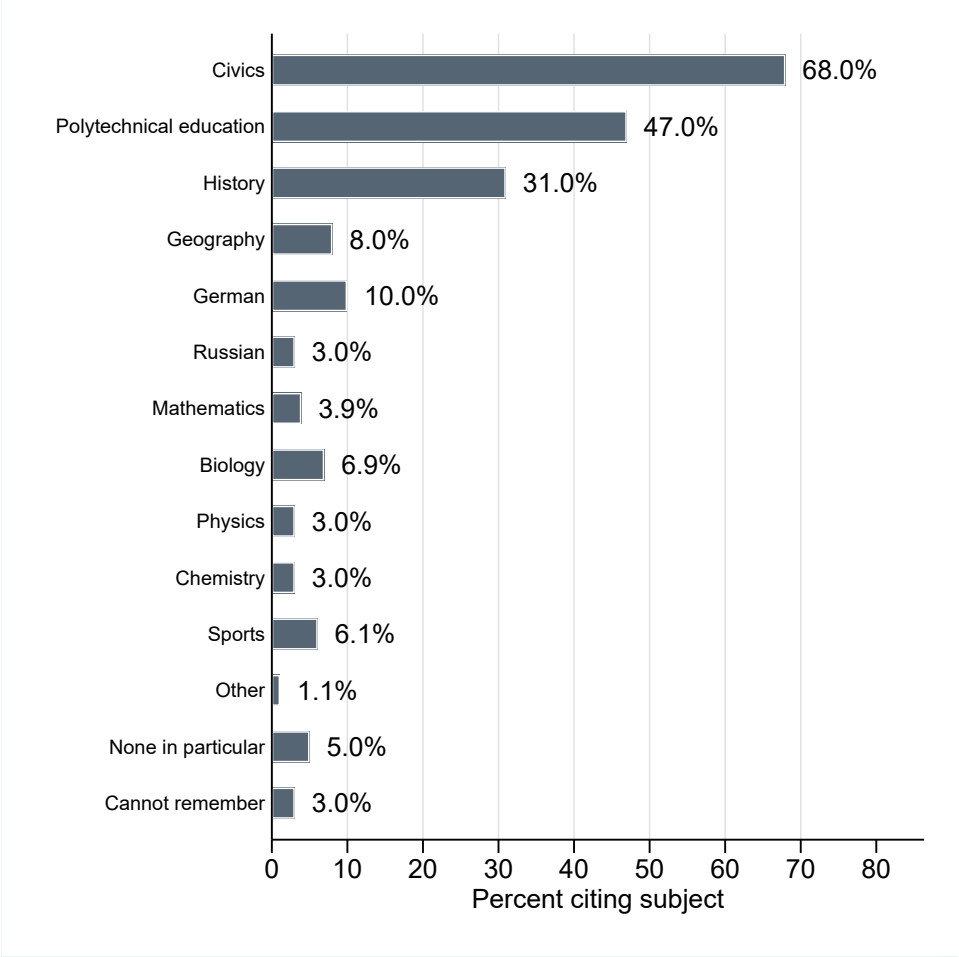
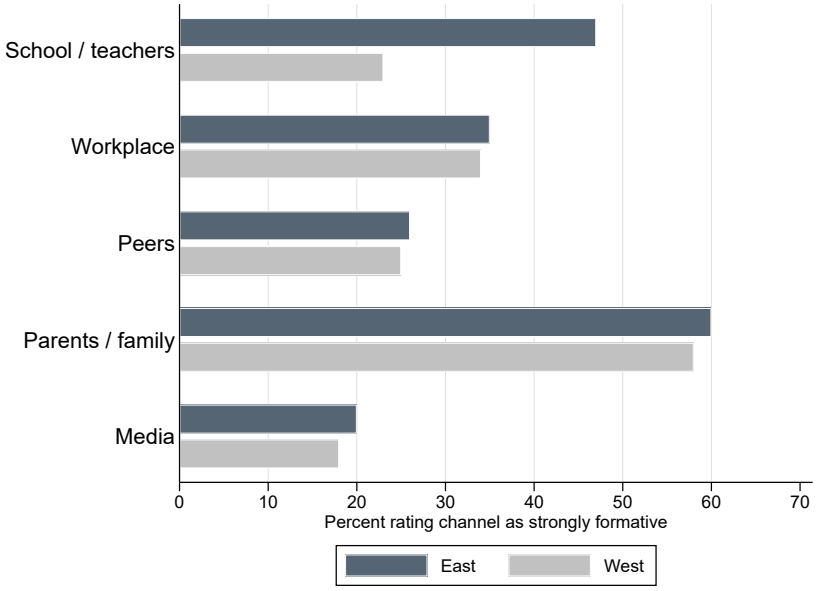
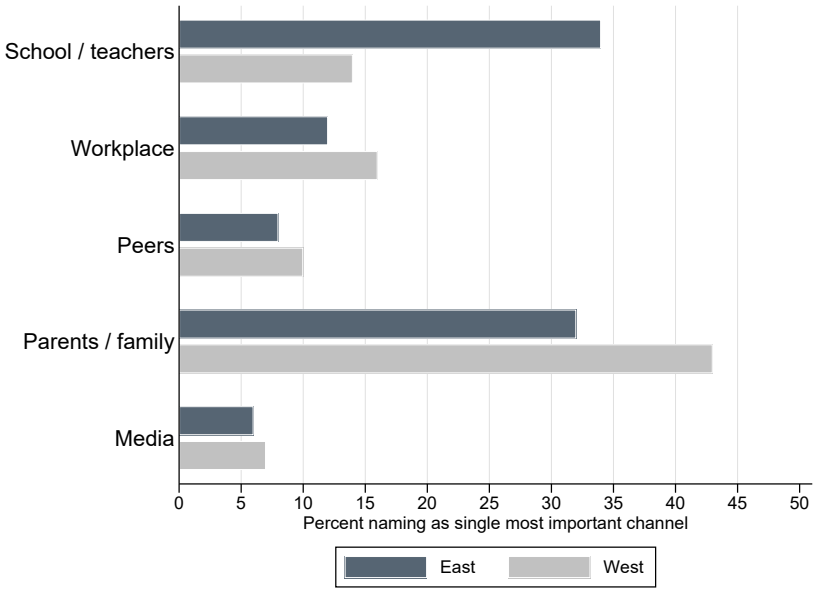


Figure C.3. School Subjects Associated with Gender-Egalitarian Norms

Notes. Data come from the retrospective survey described in Appendix C. The figure is restricted to the 600 GDR-schooled respondents aged 55–66 and shows the share citing each school subject as particularly salient in conveying gender-egalitarian employment norms. Multiple responses were allowed.



(a) Share rating each channel as strongly formative



(b) Share naming each channel as the single most important

Figure C.4. Socialization Channels for Work–Family Views

Notes. Data come from the retrospective survey described in Appendix C. The figure uses the full sample of 900 respondents aged 55–66, including 600 GDR-schooled and 300 West-schooled respondents. Panel (a) reports the share rating each channel as strongly shaping views on work–family compatibility (4 or 5 on a 0–5 scale), by region. Panel (b) reports the share identifying each channel as the single most important factor shaping those views (single choice), by region. The bars in panel (b) do not sum to 100 percent because the residual "cannot say" category is not plotted. Table C.2 reports it.

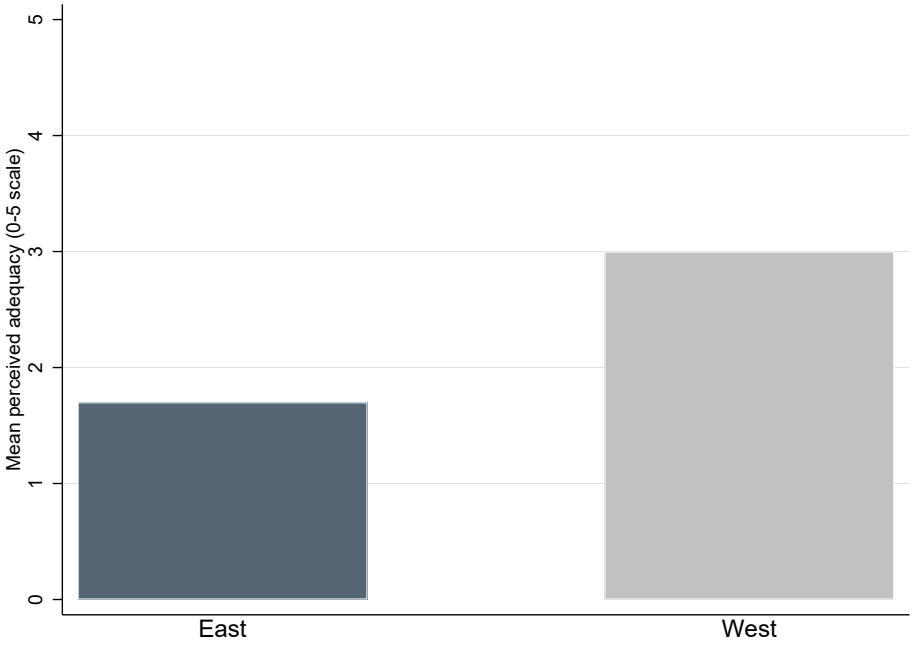


Figure C.5. Perceived Childcare Adequacy by Region

Notes. Data come from the retrospective survey described in Appendix C. The figure uses the full sample of 900 respondents aged 55–66, including 600 GDR-schooled and 300 West-schooled respondents, and shows the mean rating of how well post-1990 public childcare provision matched respondents' own expectations (0 = not at all, 5 = very well), by region.

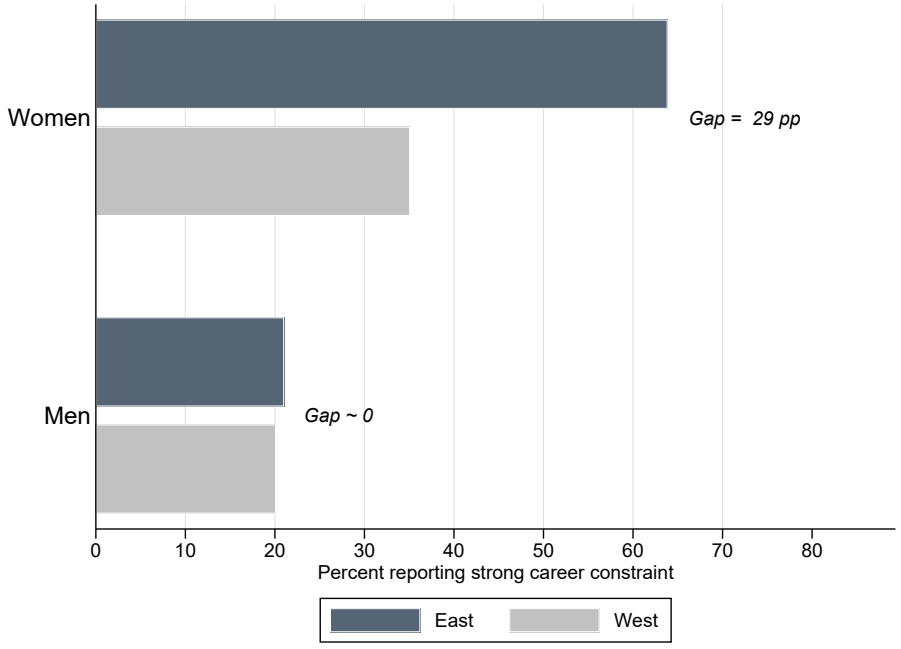


Figure C.6. Perceived Career–Fertility Trade-Off by Region and Gender

Notes. Data come from the retrospective survey described in Appendix C. The figure uses the full sample of 900 respondents aged 55–66, including 600 GDR-schooled and 300 West-schooled respondents, and shows the share reporting that an additional child, or a first child among childless respondents, would have strongly constrained career development (4 or 5 on a 0–5 scale), by region and gender.

C.4 Survey Instrument

This section reproduces the survey questionnaire in English translation (Section C.4.1) and provides a link to the original German instrument (Section C.4.2). Key items relying on retrospective recall include the response option "Cannot assess / do not remember." Items marked with † are optional. Routing conditions are indicated in italics. Filtered questions were shown only to the specified subgroup of respondents.

C.4.1 English Translation.

[Attention check; respondents who do not select the instructed option are flagged and excluded from the analysis]

0. To begin the survey, please select the option "A little".
Not at all; A little; Moderately; A lot; Very much

Section 1: Demographics

1. In which year were you born?
[Dropdown: 1955–1975]

2. What is your gender?
Male; Female; Diverse

3. Do you have children?
Yes; No

4. In which German federal state did you mostly attend school?
[Dropdown with all German federal states, plus Berlin (East), Berlin (West), and "Did not attend school in Germany"]

5. In which part of Germany is your current place of residence?
East Germany (former GDR); West Germany

6. Which of the following best describes your current employment status?
[Dropdown:] Employed full-time; Employed part-time; Self-employed; Unemployed; Homemaker; Retired (including pension or early retirement); Other employment status *[free-text field]*

7. What is your current marital status?
Single, never married; Married; Registered partnership; Divorced; Widowed; In a committed partnership, not married

8. What was your net income last month?
Less than 500 €; 500 to less than 1,000 €; 1,000 to less than 1,500 €; 1,500 to less than

2,000 €; 2,000 to less than 2,500 €; 2,500 to less than 3,000 €; 3,000 to less than 3,500 €; 3,500 to less than 4,000 €; 4,000 to less than 4,500 €; 4,500 to less than 5,000 €; 5,000 to less than 5,500 €; 5,500 to less than 6,000 €; 6,000 € or more; Prefer not to say; Don't know

[Bot-detection check; respondents whose entry is inconsistent with Question 1 are flagged and excluded from the analysis]

9. Please enter your month and year of birth in the text field.
[Single-line text field]

Section 2: Schooling and socialization

10. How strongly did the following factors shape your view on the compatibility of family and work?
[Matrix; 0 = not at all … 5 = very strongly; order randomized]

- Parents / family
- School / teachers
- Friends / acquaintances
- Media (TV, books, internet, …)
- Workplace / professional environment

11.† And which of these factors was most important overall?
Same answer categories as Question 10 + "Cannot say precisely"

12. What gender roles for men and women were conveyed at school, in your perception?
(1) Egalitarian role model (e.g., women employed regardless of family situation, women economically independent); (2) Traditional role model (e.g., man as main breadwinner, woman primarily responsible for household and children); (3) No specific gender roles; (4) Cannot remember.

13.† *(shown only for respondents who mostly attended school in the GDR, i. e. Question 4 ∈ {Berlin (East), Brandenburg, Mecklenburg-Vorpommern, Saxony, Saxony-Anhalt, Thuringia})*
In which school subjects were these gender roles conveyed in particular? *[Multiple selections possible]*
Civics (Staatsbürgerkunde); History; Geography; Polytechnical education; Physical education (sports); Biology; Chemistry; Physics; Mathematics; German; Russian; Other: *[free-text field]*; Not in any subject in particular; Cannot remember

14. To what extent were the following ideas conveyed as normal or self-evident in your school?
[Matrix; 0 = not at all … 5 = very strongly]

(a) Women should be able to work regardless of their family situation.

(b) Even mothers of young children should work.
(c) Having a job is as important for women as for men.

[Attention check; respondents who do not select the instructed option are flagged and excluded from the analysis] To confirm your attention, please select the option "A little".
Not at all; A little; Moderately; A lot; Very much

Section 3: Reunification, childcare, and perceived constraints

15. Did German reunification in 1990 influence your family planning?
Yes; No; Cannot remember / don't know

16. *(shown only if Question 15 = Yes)* In what way did reunification change your family planning?
Fewer children; More children; Same number, only timing changed; Both the number and the timing changed; Cannot say precisely

17. *(shown only if Question 15 = Yes)* By how many children did your originally planned number of children change as a result of reunification?
−3; −2; −1; 0; +1; +2; +3

18. *(shown only if Question 15 = Yes)* What were the reasons that reunification influenced your family planning? *[Multiple selections possible; order randomized]*
Worsened compatibility of work and family; Loss or deterioration of childcare provision; Financial uncertainty / unemployment; Uncertainty about future career prospects / the labor market; Housing situation; Partnership situation; Other reasons: *[free-text field]*; Cannot say precisely

19. *(shown only if Question 18 was answered)* And which of these was the most important reason?
[Same categories as Question 18, single choice]

20. Thinking about the years after 1990 in your region of residence at the time: How well did public childcare provision match your expectations?
0 = not at all … 5 = very well; cannot assess

21. *(shown only if Question 3 = Yes)* How strongly would another child have constrained your career development?
0 = not constrained at all … 5 = constrained very strongly; cannot assess

21'. *(shown only if Question 3 = No)* How strongly would a child have constrained your career development?
0 = not constrained at all … 5 = constrained very strongly; cannot assess

22. *(shown only if Question 3 = Yes)* Regardless of reunification: To what extent did the following factors constrain or complicate your decision about the number of children?
[Matrix; 0 = not at all … 5 = very strongly]

- Difficulties balancing family and work
- Insufficient availability of public childcare
- Lack of a partner or unstable partnership
- Financial uncertainty
- High job demands
- Housing situation
- Lack of support from family / social environment
- Health

22'. *(shown only if Question 3 = No)* Regardless of reunification: To what extent did the following factors constrain or complicate your decision to have children or not to have children?
[Matrix; 0 = not at all … 5 = very strongly; same sub-items as Question 22]

Section 4: Closing

23.† Would you like to tell us anything else?
Yes *[free-text field]*; No

C.4.2 German-Language Original. The survey was administered in German, and the English translation above is provided for reference. The original questionnaire, exactly as presented to respondents, is available in the combined questionnaire document at https://a-bertermann.github.io/questionnaires_german.pdf.

Appendix D May–June Cohort Survey: Vignette and Autobiographical Modules

D.1 Sample and Fieldwork

I fielded the May–June cohort survey among East Germans born in May or June between 1970 and 1983 through Talk Online, Bilendi, and Konkret. The survey was conducted between April 10 and April 18, 2026. The May–June birth-month restriction mirrors the main empirical design, while the cohort range includes both the exposed cohorts used in the main analysis and older comparison cohorts. The final sample comprises 500 respondents, stratified by birth cohort: 100 respondents in the 1970–1973 comparison cohorts, 160 in the

1974–1977 cohorts (grades 7–10 at reunification), 120 in the 1978–1980 cohorts (grades 4–6), and 120 in the 1981–1983 cohorts (grades 1–3).

Childhood region is defined using a retrospective question on the federal state in which respondents went to school. Respondents are classified as having attended school in East Germany if the reported state is Brandenburg, Mecklenburg-Vorpommern, Saxony, Saxony-Anhalt, Thuringia, or East Berlin. Respondents who did not satisfy the birth-month, cohort, or East-schooling restrictions are not included in the analysis sample.

D.2 Questionnaire Structure

The questionnaire comprises two vignette blocks, an autobiographical module, and a brief closing section. The full instrument is reproduced in English in Appendix Section D.5, and the German original is available at https://a-bertermann.github.io/questionnaires_german.pdf. I summarize here the design choices most relevant for interpretation.

Module ordering. Respondents first completed the vignette experiment and only afterward answered questions about their own fertility decisions and work attachment. This ordering limits the risk that autobiographical reflection primes responses to the hypothetical scenarios. Within the vignette experiment, the order of the institutional-support and economic-conditions blocks was randomized. The order of vignettes within each block was also randomized. Respondents were not told that May versus June birth defined treatment status, and the link between birth month and GDR school-regime exposure was unlikely to be transparent to them. This limits the scope for experimenter-demand effects along the treatment dimension. The economic-conditions block additionally provides an internal benchmark for engagement with the scenarios.

Institutional-support block. The institutional-support block measures responsiveness to the feasibility of combining employment and parenthood. Respondents evaluate hypothetical East German regions that differ in the availability and reliability of childcare for children under age three, while broader economic conditions are held fixed.

Economic-conditions block. The economic-conditions block measures responsiveness to economic conditions more generally. Regions differ in job security, unemployment, income predictability, and housing affordability, while childcare provision and family transfers are held fixed.

Vignette outcomes. After each vignette, respondents answered three questions about the female protagonist: how many children she would have over her life course, how likely she would be to have at least one child, and how likely she would be to have two or more

children under the described conditions. The primary vignette outcome is an expected-fertility index. I first standardize each high-minus-low component and then average the three standardized differences. The resulting index is not re-standardized, so coefficients are expressed in units of the component standard deviations. I also report the components separately to distinguish the overall number of children from the extensive and intensive fertility margins.

Autobiographical module. After the vignette blocks, respondents completed an autobiographical module about their own fertility decisions. The module asks respondents to rate the importance of institutional constraints, including work–family compatibility and childcare availability, and economic constraints, including financial security, labor-market security, and housing. It also includes household-dynamics items measuring whether the division of childcare and housework mattered for fertility decisions and whether care provided by the respondent's partner alone would have been sufficient for combining employment and family. These household-dynamics items are reported separately and are not part of the primary vignette index or the work-attachment index. Respondents also identify the single most important consideration in their own fertility decisions and answer items measuring work attachment and acceptance of maternal employment. To measure childcare reference points, respondents report the minimum number of public childcare places per 100 children under age three they consider necessary for a working mother to realize her desired family size. A later own-context question asks respondents to assess how many such places were available in the region relevant for their own family decisions. I use the difference between these two quantities to construct a perceived childcare gap, reported in Table D.8.

D.3 Sample Characteristics

Appendix Table D.1 reports summary statistics for the May–June cohort survey sample. The main analysis compares May-born and June-born respondents within the exposed cohorts. The key test compares the May–June response across the institutional-support and economic-conditions blocks. A larger differential response in the institutional-support block would indicate greater sensitivity specifically to the work–family environment rather than to difficult post-reunification conditions more generally.

Table D.1. May–June Cohort Survey: Sample Composition by Cohort Group and Birth Month

	Comparison cohorts		Exposed cohorts	
	June	May	June	May
Age at interview	53.48	53.54	46.47	46.52
Female respondent	0.460	0.420	0.570	0.545
Has any children	0.780	0.780	0.780	0.730
Number of children	2.06	1.98	1.60	1.49
Full-time employed	0.400	0.420	0.565	0.540
Part-time employed	0.120	0.220	0.160	0.165
Currently lives in East Germany	0.740	0.800	0.750	0.735
Correct childcare comprehension	0.960	0.940	0.930	0.955
Correct economic comprehension	0.980	0.980	0.955	0.965
Median completion time (minutes)	6.07	6.21	6.57	6.49
Observations	50	50	200	200

Notes. Respondent-level summary statistics for the May–June cohort survey by cohort group and birth month. Comparison cohorts were born in 1970–1973 and exposed cohorts in 1974–1983. Number of children refers to respondents' own children and is not restricted to children currently living in the household. The comprehension items record whether the respondent correctly identified which vignette region offered better childcare and which offered more favorable economic conditions. The exposed sample was recruited using equal intended quotas by birth year. Completion time is reported in minutes.

D.4 Results

Table D.2. Vignette Component Outcomes: Expected Fertility

	Institutional-support block			Economic-conditions block		
	Number of children	At least one child	Two or more children	Number of children	At least one child	Two or more children
May-born	0.132*	0.147*	0.214**	−0.025	−0.034	−0.022
	(0.069)	(0.077)	(0.086)	(0.045)	(0.039)	(0.050)
Observations	400	400	400	400	400	400

Notes. May-born minus June-born estimates for exposed 1974–1983 cohorts. Outcomes are standardized high-minus-low responses within each vignette block. The number-of-children outcome records expected total children. The other two outcomes record the perceived likelihood of at least one child and of two or more children on 0–100 scales. The institutional-support block varies childcare availability, waiting-list pressure, and opening hours. The economic-conditions block varies job security, unemployment, income predictability, and housing affordability. All specifications include birth-year fixed effects, respondent gender, block order, and within-block vignette order. Standard errors are clustered at the birth-year × birth-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.3. Vignette and Autobiographical Estimates: Detailed Coefficients

	May-born
Panel A. Vignette experiment	
Institutional support index	0.165***
	(0.058)
Economic conditions index	−0.027
	(0.028)
Difference: institutional − economic	0.193***
	(0.069)
Panel B. Importance of factors in own fertility decisions	
Work-family compatibility	0.160**
	(0.065)
Childcare under age 3	0.179***
	(0.060)
Financial security	0.023
	(0.084)
Labor-market security	−0.023
	(0.076)
Housing	−0.030
	(0.054)
Partnership	−0.009
	(0.063)
Care and housework split	0.012
	(0.082)
Observations	400

Notes. Estimates underlying Figure 4 for exposed 1974–1983 cohorts. Panel A reports May-born minus June-born estimates for the institutional-support and economic-conditions vignette indices. Each index averages standardized high-minus-low responses for expected number of children, likelihood of at least one child, and likelihood of two or more children. The institutional-minus-economic row is estimated from the within-respondent difference between the two vignette indices. Panel B reports corresponding estimates for standardized importance ratings in respondents' own fertility decisions. All specifications include birth-year fixed effects and respondent gender. Vignette specifications additionally include block-order and within-block order controls. Standard errors are clustered at the birth-year × birth-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.4. Vignette Response to Institutional Support by Cohort Group

	Comparison 1970–1973	Pooled 1974–1983	1974–1977 grades 7–10	1978–1980 grades 4–6	1981–1983 grades 1–3
May-born	0.020	0.164***	0.348***	0.050*	0.006
	(0.080)	(0.059)	(0.115)	(0.029)	(0.045)
Observations	100	400	160	120	120

Notes. Each column reports the May-born minus June-born estimate for the institutional-support index in the indicated cohort group. The 1970–1973 comparison cohorts completed compulsory schooling before reunification. The exposed cohort groups correspond to the phase in which the marginal GDR-regime grade occurred. All specifications include birth-year fixed effects, respondent gender, block order, and childcare-block vignette order. Standard errors are clustered at the birth-year × birth-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.5. Institutional and Economic Constraints in Own Fertility Decisions

	Exposed cohorts, 1974–1983		Comparison cohorts, 1970–1973	
	Institutional constraints	Economic constraints	Institutional constraints	Economic constraints
May-born	0.169***	−0.010	−0.115	0.071
	(0.043)	(0.037)	(0.109)	(0.119)
Difference: institutional − economic	0.179***		−0.186	
	(0.028)		(0.171)	
Observations	400	400	100	100

Notes. May-born minus June-born estimates for the exposed 1974–1983 cohorts and the adjacent 1970–1973 comparison cohorts. Institutional constraints are the standardized average of work–family compatibility and childcare importance in respondents' own fertility decisions. Economic constraints are the standardized average of financial security, labor-market security, and housing. Difference rows report within-respondent contrasts between the institutional- and economic-constraints indices. All specifications include birth-year fixed effects and respondent gender. Standard errors are clustered at the birth-year × birth-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.6. Single Most Important Factor in Own Fertility Decisions

	Work-family	Childcare	Care/ housework split	Financial	Labor-market	Housing	Partnership
May-born	0.064**	0.033*	−0.001	−0.022	−0.023	−0.015	−0.036
	(0.031)	(0.018)	(0.013)	(0.030)	(0.025)	(0.023)	(0.026)
Observations	400	400	400	400	400	400	400

Notes. Each column reports a linear-probability estimate of the May-born minus June-born difference in whether the respondent selected the indicated factor as the single most important consideration in their own fertility decisions. The sample consists of exposed 1974–1983 cohorts. All specifications include birth-year fixed effects and respondent gender. Standard errors are clustered at the birth-year × birth-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.7. Stated Ideal Family Size and School-Regime Exposure

	Exposed cohorts (1974–1983)			Comparison cohorts	Difference-in-
	Pooled (1)	Women (2)	Men (3)	(1970–1973) (4)	differences (5)
May-born	0.039	0.031	0.052	−0.018	0.057
	(0.086)	(0.109)	(0.131)	(0.152)	(0.174)
Observations	400	223	177	100	500

Notes. The outcome is the number of children respondents report having considered ideal before family formation. Columns (1)–(3) report the May-born minus June-born estimate for the exposed 1974–1983 cohorts, pooled and separately by gender. Column (4) reports the corresponding estimate for the adjacent 1970–1973 comparison cohorts. Column (5) reports the difference between the exposed- and comparison-cohort estimates. All specifications include birth-year fixed effects. Pooled specifications additionally include respondent gender. Standard errors are clustered at the birth-year × birth-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.8. Childcare Reference Points and Perceived Institutional Shortfall

	Required childcare	Recalled childcare availability	Perceived childcare gap	Cannot assess local availability
May-born	3.104***	0.098	2.816***	0.000
	(0.507)	(0.608)	(0.906)	(0.009)
Observations	400	384	384	400

Notes. May-born minus June-born estimates for exposed 1974–1983 cohorts. Required childcare is the stated number of public childcare places per 100 children under age three needed for an employment-oriented woman to realize her desired family size. Recalled childcare availability is respondents' recollection of childcare availability in the region relevant to their own family decisions and is subjective and retrospective rather than administrative coverage. The perceived childcare gap is the difference between required and recalled availability. Recalled availability and the gap are defined for respondents who provided an assessment of local availability. On these 384 respondents the required-childcare estimate is 2.914 (0.525), so the gap estimate equals required minus recalled availability within the same sample. The final column indicates inability to provide such an assessment. All specifications include birth-year fixed effects and respondent gender. Standard errors are clustered at the birth-year × birth-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.9. Household Dynamics and Private Care as Potential Substitutes

	Care and housework split importance	Partner care adequacy	Partner care sufficient	No partner / cannot assess
May-born	0.012	−0.012	−0.020	−0.020
	(0.082)	(0.068)	(0.024)	(0.022)
Observations	400	338	338	400

Notes. May-born minus June-born estimates for exposed 1974–1983 cohorts. Care and housework split importance is a standardized rating of how important the division of childcare and housework was for respondents' own fertility decisions. Partner-care adequacy is a standardized rating of how sufficient care by the respondent's partner alone would have been for combining employment and family. Partner care sufficient is an indicator for ratings of 7 or higher on the underlying 0–10 scale. Partner-care outcomes exclude respondents who selected "cannot assess / no stable partnership at the time." The final column indicates that response. All specifications include birth-year fixed effects and respondent gender. Standard errors are clustered at the birth-year × birth-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.10. Descriptive Mechanism Accounting: Work Attachment and Maternal Employment

	Vignette experiment		Own fertility decisions	
	Baseline	+ Mechanisms	Baseline	+ Mechanisms
May-born	0.165***	0.125**	0.169***	0.118***
	(0.058)	(0.053)	(0.043)	(0.035)
Work-attachment index		0.131***		0.214***
		(0.029)		(0.038)
Maternal-employment index		0.109***		0.117***
		(0.035)		(0.035)
Observations	400	400	400	400

Notes. May-born minus June-born estimates for exposed 1974–1983 cohorts in a suggestive mechanism-accounting exercise. The vignette outcome is the institutional-support index. The autobiographical outcome is the institutional-constraints index, defined as the standardized average of work–family compatibility and childcare importance in respondents' own fertility decisions. The mechanism specification adds the post-school work-attachment index and the maternal-employment index. All specifications include birth-year fixed effects and respondent gender. Vignette specifications additionally include block-order and within-block order controls. Standard errors are clustered at the birth-year × birth-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.5 Survey Instrument: English Translation

Introduction shown to respondents. *In the following, we describe several short situations involving hypothetical regions in East Germany. The regions are fictional, but they are based on typical living conditions at that time. Please read each description carefully. The same short questions will then follow for each region. There are no right or wrong answers—we are interested only in your personal assessment.*

Screening.

(1) **Quality check:** Please select answer option "3."
Response options: 1 / 2 / 3 / 4 / 5.

(2) **S1.** In which year were you born?
Dropdown: 1968–1985. Only 1970–1983 proceed.

(3) **S2.** In which month were you born?
Dropdown: all months. Only May and June proceed.

(4) **S3.** In which German federal state did you complete most of your schooling?
Dropdown: all federal states. Only former GDR states proceed.

Block A: Institutional support through childcare. Respondents saw two regional living situations in randomized order. The vignette text remained visible while respondents answered the outcome questions.

Low institutional-support vignette:
In this region, reliable childcare for children under age three is scarce. For every 100 children under age three, there are about 25 public childcare places. This means that only about one quarter of children receive a place; for most families, no place is available. Waiting lists are long. Even when a place is found, opening hours often end in the early afternoon and often do not fit a normal working day. Many families therefore have to look for additional private solutions or adjust working hours around childcare. Economic conditions—job opportunities, pay levels, and housing costs—correspond to a typical East German region of this period.

High institutional-support vignette:
In this region, childcare for children under age three is more reliably available. For every 100 children under age three, there are about 50 public childcare places. Because many families also rely on parental leave, grandparents, or private care, this is generally enough for families who want a place to obtain one. Waiting lists are shorter, and opening hours more often fit a normal working day, so families can plan work and childcare more reliably. Economic conditions—job opportunities, pay levels, and housing costs—correspond to a typical East German region of this period.

Block B: Economic conditions. Respondents saw two additional regional living situations in randomized order. The vignette text remained visible while respondents answered the outcome questions.

Difficult economic-conditions vignette:
In this region, economic conditions are difficult. Jobs are insecure. Unemployment is high. Income is difficult to plan, and housing costs take up a large share of household income. Public childcare for children under age three and family-related transfers correspond to a typical East German region of this period.

Stable economic-conditions vignette:
In this region, economic conditions are stable. Jobs are reliable. Unemployment is low. Income is easy to plan, and housing costs are affordable for many households. Public childcare for children under age three and family-related transfers correspond to a typical East German region of this period.

Outcome questions shown after each vignette. *For the next questions, please think about a woman from your birth cohort in East Germany. Base your assessment on the described living conditions.*

(1) **F1.** How likely is it that this woman would have had at least one child over her life course under the described living conditions?
Response scale: 0–100 slider, no starting value; 0 = very unlikely, 100 = very likely.

(2) **F2.** How likely is it that this woman would have had two or more children over her life course under the described living conditions?
Response scale: 0–100 slider, no starting value; 0 = very unlikely, 100 = very likely.

(3) **F3.** How many children would this woman have had over her life course under the described living conditions?
Response options: 0 / 1 / 2 / 3 / 4 or more.

Questions shown after both vignette blocks.

(1) **F4a.** Comparing the two regions with different childcare provision: in which region was childcare for children under age three better?
Response scale: first region clearly worse / first region somewhat worse / about the same / first region somewhat better / first region clearly better.

(2) **F4b.** Comparing the two regions with different economic conditions: in which region were general economic conditions more favorable?
Response scale: first region clearly less favorable / first region somewhat less favorable / about the same / first region somewhat more favorable / first region clearly more favorable.

(3) **F5.** Overall, how easy was it for you to imagine the regions described in the two blocks?
Response scale: 1 = not easy at all …5 = very easy.

Autobiographical module. *The following questions no longer concern the described regions, but rather your own life situation.*

(1) **E1.** Thinking about the time before you had children, or before you seriously thought about having children: how many children did you ideally want at that time?
Response options: 0 / 1 / 2 / 3 / 4 or more.

(2) **E2a.** Thinking about the life phase in which issues of work, partnership, and family were or could have been important for you: how important was work–family compatibility for whether you decided, or would have decided, to have a first or another child?
Response scale: 0 = not at all important …10 = very important.

(3) **E2b.** How important was childcare for children under age three?
Response scale: 0 = not at all important …10 = very important.

(4) **E2c.** How important was financial security or economic stability?
Response scale: 0 = not at all important …10 = very important.

(5) **E2d.** How important was the security of career prospects or the labor market?
Response scale: 0 = not at all important …10 = very important.

(6) **E2e.** How important was the housing situation?
Response scale: 0 = not at all important …10 = very important.

(7) **E2f.** How important was the partnership situation?
Response scale: 0 = not at all important …10 = very important.

(8) **E2g.** How important was the division of childcare and housework between mother and father, or between parents?
Response scale: 0 = not at all important …10 = very important.

(9) **E3.** Which of these factors was most important overall?
Single choice: work–family compatibility / childcare under age three / financial security / career and labor-market prospects / housing situation / partnership situation / division of childcare and housework.

Childcare reference point.

(1) **E4.** How many public childcare places for children under age three per 100 children would be necessary, in your view, for a woman who wants to remain employed to realize her desired family size?
Background: Not all families need a place at the same time—some use parental leave, others rely on private childcare. We are interested in the overall coverage rate at which

an employment-oriented woman can reliably expect to obtain a place.
Response scale: 0 = no places per 100 children, 100 = places for all children under age three. Respondents were told that they did not need to know the exact number and should give their best assessment.

Work attachment.

(1) **E5.** First, please think back to your period after school, without thinking of one exact year and regardless of whether you later had children. To what extent did the following statements apply to you at that time?
Response scale for E5a–E5d: 0 = does not apply at all …10 = fully applies; additional option "cannot assess."

a. **E5a.** Having one's own employment was a central part of my life plan.
b. **E5b.** Being financially independent was very important to me.
c. **E5c.** Work was more than just earning money.
d. **E5d.** I wanted to remain professionally active even if partnership or family later became important.

(2) **E6.** And how is it today? To what extent do the following statements apply to you today?
Response scale for E6a–E6d: 0 = does not apply at all …10 = fully applies; additional option "cannot assess."

a. **E6a.** Having one's own employment is a central part of my life plan.
b. **E6b.** Being financially independent is very important to me.
c. **E6c.** Work is more than just earning money.
d. **E6d.** I want to remain professionally active even when partnership or family are important.

Maternal employment.

(1) **E7a.** A mother with a young child can work without harming the relationship with the child.

(2) **E7b.** Mothers of young children should have the opportunity to remain professionally active.

(3) **E7c.** It is not generally worse for children if their mother works.

(4) **E7d.** It should be normal for mothers of young children to work if they want to.
Response scale for E7a–E7d: 0 = strongly disagree …10 = strongly agree; additional option "cannot assess."

Household dynamics and own childcare environment.

(1) **E8.** If you had had a small child under age three at the time and had wanted to remain employed yourself, how sufficient would care by your then partner alone have been for reconciling employment and family?
Response scale: 0 = not at all sufficient …10 = fully sufficient; additional option "cannot assess / no stable partnership at the time."

(2) **E9.** Thinking about the region in which you lived during the life phase of your family decisions, or in which a child under age three could have been relevant for you, approximately how many public childcare places for children under age three were available there per 100 children?
Response scale: 0 = no places per 100 children, 100 = places for all children under age three; additional option "cannot assess." Respondents were told that they did not need to know the exact number and should give their best assessment.

Demographics and debriefing at end of survey.

(1) **B1.** What is your gender?
Response options: male / female / diverse / prefer not to say.

(2) **B2.** How many children do you have today?
Response options: 0 / 1 / 2 / 3 / 4 or more.

(3) **B3.** What is your current employment status?
Response options: full-time / part-time / self-employed / not employed / retired / other.

(4) **B4.** Where do you live today?
Response options: East Germany / West Germany.

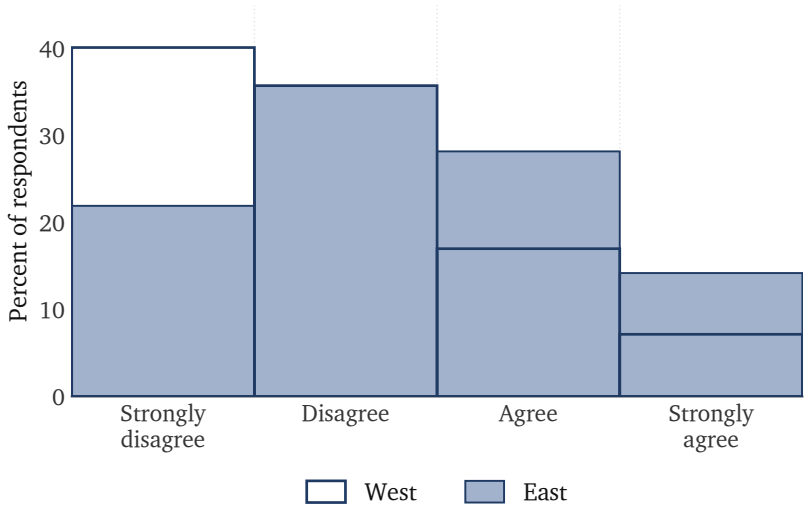
(5) **B5.** What do you think this study was mainly about?
Response format: open text; optional.

D.6 Survey Instrument: German Original

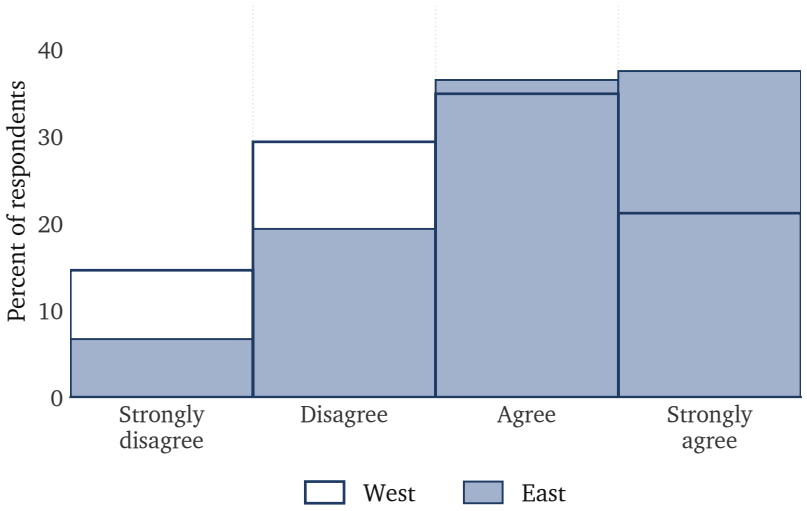
As for the retrospective survey, the May–June cohort survey was fielded in German. Its German original is contained in the same combined questionnaire document at https://a-bertermann.github.io/questionnaires_german.pdf.

Appendix E Historical and Descriptive Evidence

E.1 Fertility and Gender Attitudes in East and West Germany



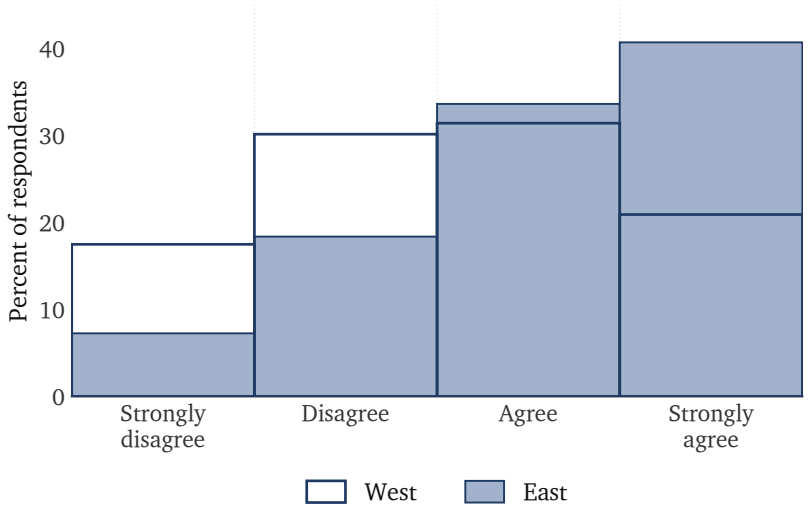
(a) 1991



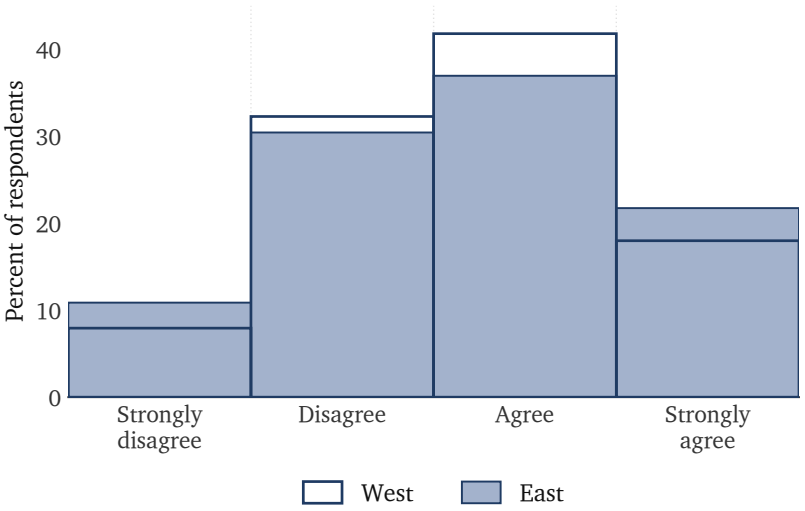
(b) 2016

Figure E.1. East–West Gap in Acceptance of Maternal Employment, by Survey Wave

Notes. The figure plots the distribution of responses to a survey item measuring acceptance of maternal employment in East and West Germany. The original item is "A small child is bound to suffer if his or her mother goes out to work." Responses are reverse-coded so that higher values indicate greater acceptance of maternal employment. Panel (a) restricts the sample to the 1991 survey wave, panel (b) to the 2016 survey wave. Source: German General Social Survey (ALLBUS), 1991 and 2016.



(a) Born Before Reunification



(b) Born After Reunification

Figure E.2. East–West Gap in Acceptance of Maternal Employment, by Birth Cohort

Notes. The figure plots the distribution of responses to a survey item measuring acceptance of maternal employment in East and West Germany, pooled across the 2012 and 2016 waves. The original item is "A small child is bound to suffer if his or her mother goes out to work." Responses are reverse-coded so that higher values indicate greater acceptance of maternal employment. Panel (a) restricts the sample to individuals born before reunification (1990 or earlier), and panel (b) to individuals born after reunification (1991 or later). In a regression of the reverse-coded maternal-employment measure on a West-Germany indicator with survey-year fixed effects, the West-minus-East gap is −0.519 (SE = 0.036) for cohorts born before reunification and 0.011 (SE = 0.149) for cohorts born after, indicating that West Germans are less accepting of maternal employment among cohorts born before reunification but that this gap closes for cohorts socialized after the institutional convergence. Source: German General Social Survey (ALLBUS), 2012 and 2016.

E.2 Structure and Curriculum of the GDR School System

Table E.1. Timetable for the Ten-Grade Secondary School, School Year 1971/72

Subject / Grade	1		2	3	4	5	6	7	8	9	10
	1st H.	2nd H.									
German	11	10	12	14	14	7	6	5	4+1	3	3+1
Russian	—	—	—	—	—	6	5	3	3	3	3
Mathematics	5	5	6	6	6	6	6	6	4	5	4
Physics	—	—	—	—	—	—	3	2	2	3	3
Astronomy	—	—	—	—	—	—	—	—	—	—	1
Chemistry	—	—	—	—	—	—	—	2	4	2	2
Biology	—	—	—	—	—	2	2	1	2	2	2
Geography	—	—	—	—	—	2	2	2	2	1	2
Handicraft	1	1	1	1	2	2	2	—	—	—	—
School garden	—	1	1	1	1	—	—	—	—	—	—
Polytechnic education, including	—	—	—	—	—	—	—	4	4	5	5
Introduction to social production	—	—	—	—	—	—	—	(1)	(1)	(2)	(2)
Technical drawing	—	—	—	—	—	—	—	(1)	(1)	(—)	(—)
Productive work	—	—	—	—	—	—	—	(2)	(2)	(3)	(3)
History	—	—	—	—	—	1	2	2	2	2	2
Civics	—	—	—	—	—	—	—	1	1	1	2
Drawing	1	1	1	1	2	1	1	1	1	1	—
Music	1	1	1	2	1	1	1	1	1	1	1
Physical education	2	2	2	2	3	3	3	2	2	2	2
Weekly hours	21	21	24	27	29	31	33	32	32+1	31	32+1
Optional											
Needlework	—	—	—	—	1	1	—	—	—	—	—
2nd foreign language	—	—	—	—	—	—	—	3	3	3	2
Weekly hours, including optional	21	21	24	27	30	32	33	35	35+1	34	34+1

Notes. Official timetable for the ten-grade general polytechnic secondary school, introduced in 1970/71 and applicable from 1971/72. Grade 1 is reported separately for the first and second half of the school year (1st H., 2nd H.). Dashes indicate no scheduled hours. Entries such as "4+1" denote additional instructional hours. Parenthesized values are the components of the polytechnic total in the row above and are not added again. The final row repeats the weekly total including the optional courses. Source: Schmidt (1988).

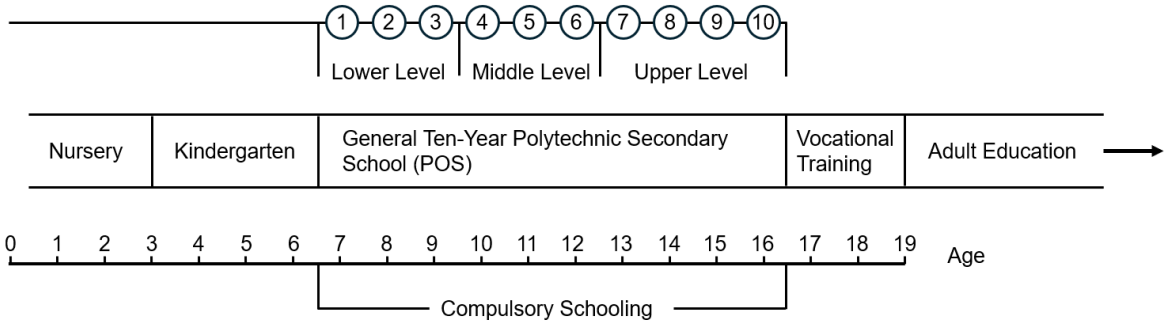


Figure E.3. Structure of the GDR Education System

Notes. This figure illustrates the structure of the education system in the German Democratic Republic (GDR). Children typically entered the General Ten-Year Polytechnic Secondary School (POS) at age 6 and progressed through lower, middle, and upper levels over ten grades. Compulsory schooling spanned from ages 6 to 16. After completing the POS, students could enter vocational training or further education. Source: Schmidt (1988).

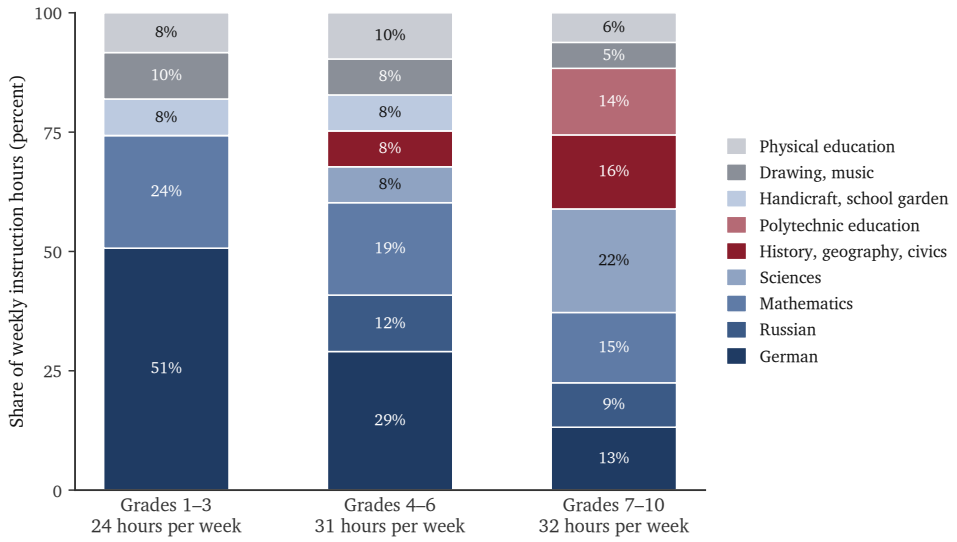


Figure E.4. Composition of Weekly Instruction Hours by Subject and Grade

Notes. Share of weekly instruction hours by subject group in grades 1–3, 4–6, and 7–10 of the GDR ten-grade polytechnic secondary school, 1971/72 timetable. Grade 1 enters as the mean of its two half-years, supplementary hours are included, and optional courses are excluded. The label under each bar gives the average weekly total. Appendix Table E.1 reports the underlying timetable. Source: Schmidt (1988).

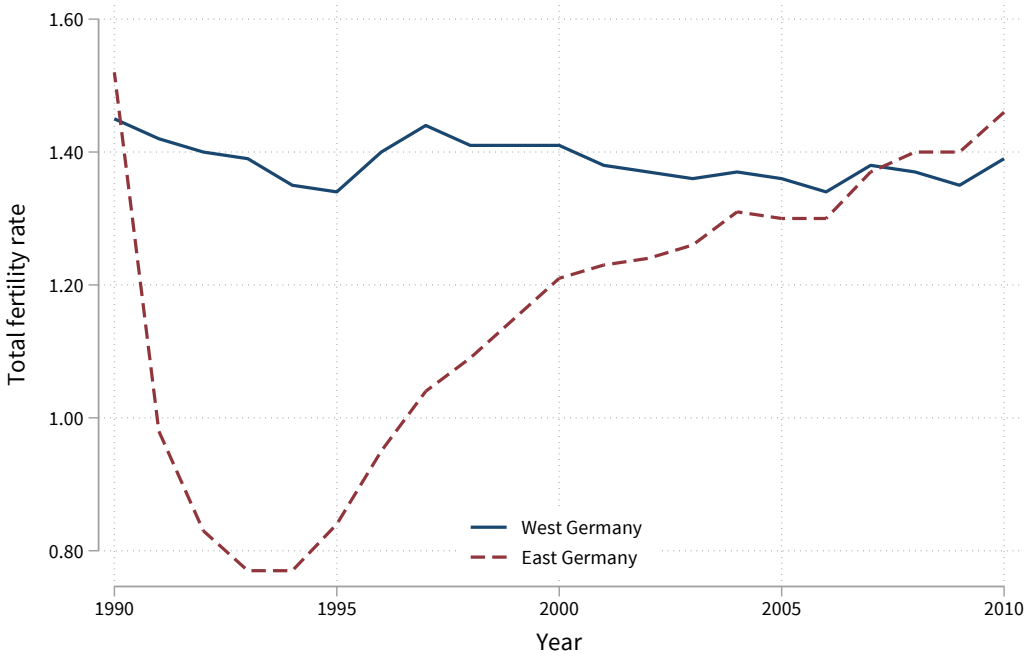


Figure E.5. Fertility after Reunification in East and West Germany

Notes. Annual total fertility rates in East and West Germany, 1990–2010. Source: Statistisches Bundesamt (2020).

Appendix F Supplementary Results and Robustness

Table F.1. Covariate Balance by Birth-Month Window in the Longitudinal Surveys

	Birth-month window					
Background characteristic	±1	±2	±3	±4	±5	All months
All three surveys						
Female	0.567	0.507	0.007	0.025	0.205	0.158
State of birth	0.526	0.268	0.138	0.091	0.369	0.206
SOEP and NEPS						
Parental education (years)	0.233	0.698	0.720	0.411	0.095	0.599
Parental occupational prestige (ISEI-88)	0.404	0.027	0.232	0.084	0.032	0.271
Number of siblings	0.100	0.553	0.064	0.079	0.010	0.025
SOEP and FReDA						
Urbanization	0.079	0.008	0.011	0.006	0.015	0.011
Minimum *p*-value	0.079	0.008	0.007	0.006	0.010	0.011
Observations in window	475	995	1,492	1,924	2,380	2,836

Notes. Each cell reports the *p*-value from a balance test comparing a background characteristic across the two sides of the May 31 cutoff within the indicated birth-month window. The sample consists of respondents born in East Germany in 1974–1983 and observed in SOEP, NEPS, or FReDA in 2021. Except for state of birth, each test regresses the covariate on an indicator for being born before the cutoff, with birth-year fixed effects and dataset fixed effects where observations are pooled across surveys. Equality of state composition across the six eastern states is assessed using Fisher's exact test, with Pearson's χ^2 test used where the exact calculation is infeasible. The female indicator and state of birth are available in all three surveys. Parental education, occupational prestige, and number of siblings are available in SOEP and NEPS, while urbanization is available in SOEP and FReDA. Parental education and occupational prestige (ISEI-88) use the higher observed value across parents. Urbanization is measured using FReDA's degree-of-urbanization classification and SOEP's settlement-type measure, both capturing the urban–rural character of respondents' place of residence. Each measure is standardized within survey before pooling. The final row reports the smallest *p*-value among the covariates available in the corresponding column and is not adjusted for multiple testing. Standard errors are clustered at the birth-year × cutoff-side level.

Table F.2. Schooling under Socialism and the Probability of Having Any Child

	Exposed cohorts (1974–1983) (1)	Comparison cohorts (1970–1973) (2)	Difference-in-differences (3)
May–June gap	−0.023***	0.011	−0.034***
	(0.005)	(0.011)	(0.012)
Wild cluster bootstrap *p*	[0.001]	—	[0.097]
Observations	5,407	2,289	7,696
Age fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes

Notes. German Microcensus 2018–2019, East German residents. The dependent variable is an indicator for at least one child in the household. Columns follow Table 1. Column (1) reports the exposed 1974–1983 cohorts, column (2) the adjacent 1970–1973 comparison cohorts, and column (3) the difference between the two from a pooled difference-in-differences regression. All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level. Brackets report wild cluster bootstrap *p*-values using Webb weights and 999 replications for columns (1) and (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

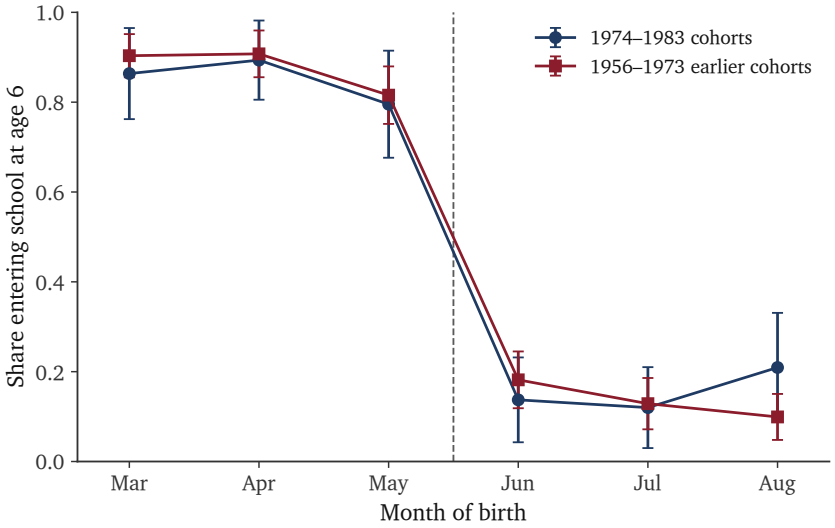


Figure F.1. Age at School Entry around the May 31 Cutoff

Notes. National Educational Panel Study (NEPS), Starting Cohort 6, East-born respondents born from March through August with observed school-entry information. Points show the share who entered school before age seven, by month of birth, for the 1974–1983 cohorts ($N = 279$) and the earlier 1956–1973 cohorts ($N = 811$), which comprise all earlier cohorts available in the corresponding analysis sample. Under the GDR school-entry rule, children who turned six on or before May 31 entered school in September of that year, whereas children born in June or later entered one year later. Whiskers denote 95 percent confidence intervals.

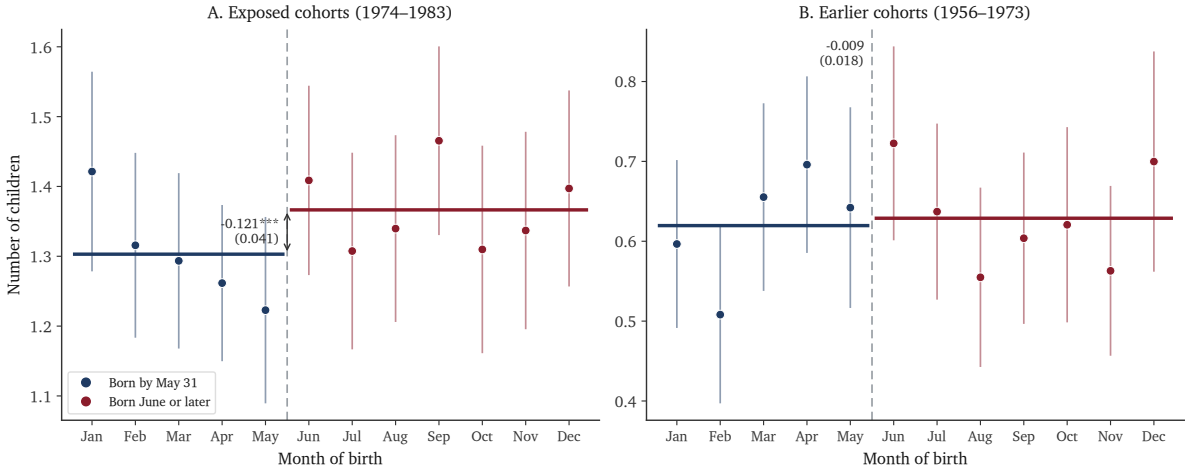


Figure F.2. Fertility by Month of Birth around the May 31 School-Entry Cutoff

Notes. SOEP, NEPS, and FReDA, wave 2021, East-born respondents. Points show residualized mean numbers of children by month of birth, controlling for birth-year, dataset, gender, and age fixed effects. Panel A reports the exposed 1974–1983 cohorts, and Panel B the earlier 1956–1973 cohorts, which comprise all earlier cohorts available in the panel sample. Whiskers denote 95 percent confidence intervals. Labelled estimates correspond to the all-month specifications in Table F.7.

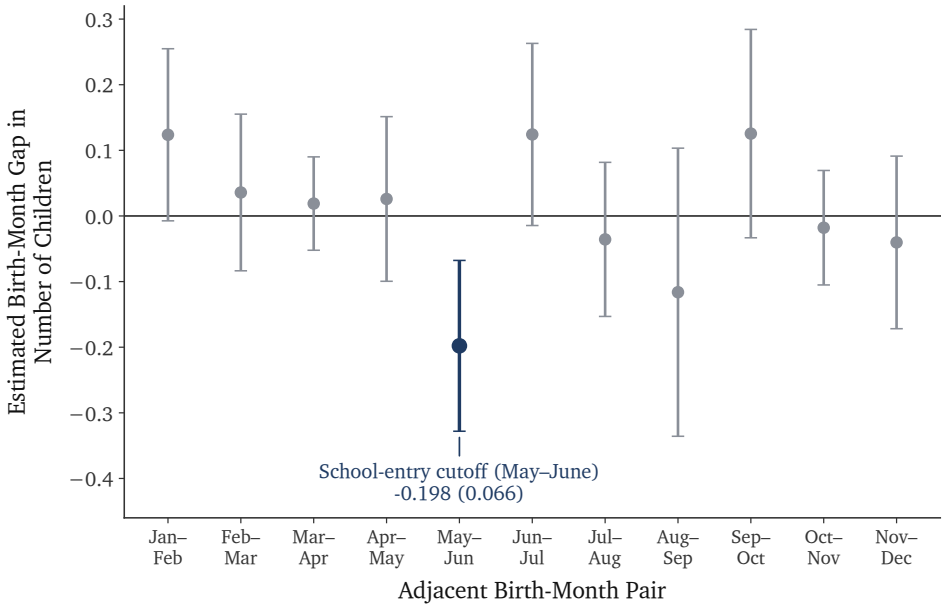


Figure F.3. Fertility Gaps across Adjacent Birth-Month Pairs

Notes. Each point reports the earlier-minus-later birth-month estimate for one adjacent birth-month pair among East-born respondents in the exposed 1974–1983 cohorts from SOEP, NEPS, and FReDA. All specifications include gender, birth-year, dataset, and age fixed effects. Whiskers denote 95 percent confidence intervals based on standard errors clustered at the birth-year × cutoff-side level. May–June is the only pair spanning the school-entry cutoff.

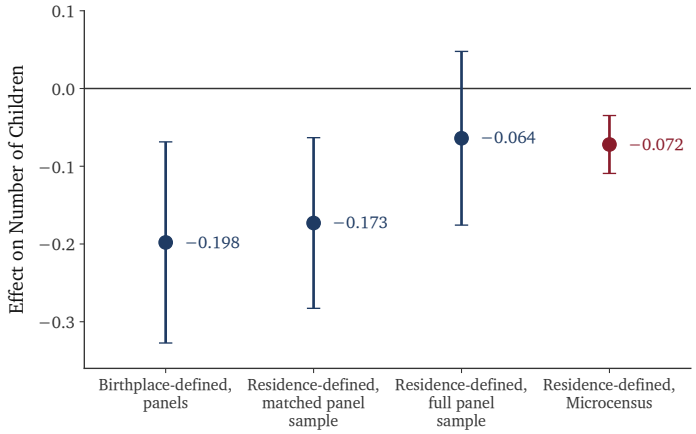


Figure F.4. Fertility Estimates across Alternative Definitions of East German Origin

Notes. Local May–June cutoff estimates for the exposed 1974–1983 cohorts under alternative geographic definitions. The first estimate defines East German origin by birthplace in SOEP, NEPS, and FReDA. The second applies the current-residence definition to the panel respondents for whom birthplace is also observed. The third applies the current-residence definition to the unrestricted panel sample. The fourth reports the corresponding Microcensus estimate using current residence. All specifications include birth-year and age fixed effects and a gender indicator. Panel estimates additionally include dataset fixed effects. Whiskers denote 95 percent confidence intervals.

Table F.3. Schooling under Socialism and Fertility: Robustness Checks

	Baseline (1)	State fixed effects (2)	State-specific cohort trends (3)
May-born	−0.072***	−0.075***	−0.074***
	(0.019)	(0.019)	(0.019)
Observations	5,407	5,407	5,407

Notes. Local May–June cutoff estimates for East German respondents in the exposed 1974–1983 cohorts. The dependent variable is the number of children in the household. Column (1) reports the baseline specification. Column (2) adds state fixed effects. Column (3) adds state-specific linear birth-cohort trends. All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.4. Schooling under Socialism and Fertility: Alternative Fertility Measures

	Number of children			
	In household (baseline) (1)	In household (2020 wave) (2)	In family (3)	Ever born (women, 2018) (4)
May-born	−0.072***	−0.078***	−0.075***	−0.092*
	(0.019)	(0.026)	(0.015)	(0.051)
Observations	5,407	2,847	5,407	1,230

Notes. Local May–June cutoff estimates for East German respondents in the exposed 1974–1983 cohorts. Column (1) uses children in the household. Column (2) uses the 2020 Microcensus wave. Column (3) uses the number of children in the family unit. Column (4) uses children ever born, available for women in the 2018 wave. All specifications include age and birth-year fixed effects and a female indicator where applicable. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.5. Schooling under Socialism and Fertility: Difference-in-Differences Robustness

	Baseline (1)	State fixed effects (2)	State-specific cohort trends (3)
May-born × exposed cohorts (1974–1983)	−0.080***	−0.082***	−0.088***
	(0.023)	(0.024)	(0.024)
Observations	7,696	7,696	7,696

Notes. Difference-in-differences estimates comparing the May–June gap among the exposed 1974–1983 cohorts with the corresponding gap among the adjacent 1970–1973 comparison cohorts. Column (1) reports the baseline specification. Column (2) adds state fixed effects. Column (3) adds state-specific linear birth-cohort trends. All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.6. Difference-in-Differences Estimates at Every Adjacent Birth-Month Pair

	Difference-in-differences		Observations	
Adjacent birth-month pair	Estimate	(SE)	Exposed cohorts (1974–1983)	Comparison cohorts (1970–1973)
January–February	0.004	(0.039)	5,037	2,182
February–March	−0.007	(0.045)	5,189	2,243
March–April	0.065	(0.041)	5,407	2,305
April–May	0.045	(0.037)	5,441	2,314
May–June	−0.078***	(0.025)	5,407	2,289
June–July	0.025	(0.021)	5,274	2,307
July–August	0.045	(0.031)	5,204	2,192
August–September	0.038	(0.038)	5,226	2,138
September–October	−0.032	(0.055)	5,068	2,017
October–November	0.000	(0.058)	4,734	1,832
November–December	−0.025	(0.070)	4,834	1,952

Notes. German Microcensus 2018–2019, East German respondents born 1970–1983. The dependent variable is the number of children in the household. Each row reports, for one adjacent birth-month pair, the difference between the earlier-minus-later birth-month gap among the exposed 1974–1983 cohorts and the same gap among the 1970–1973 comparison cohorts. These are the estimates plotted in Figure 2, Panel A. May–June is the only pair spanning the statutory school-entry cutoff. All specifications include birth-year fixed effects. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.7. Fertility Estimates across Birth-Month Windows in the Birthplace-Based Panels

	Cutoff window	Wider windows	
	May–June (1)	March–August (2)	All months (3)
Exposed cohorts (1974–1983)	−0.198***	−0.107**	−0.121***
	(0.066)	(0.046)	(0.041)
Wild cluster bootstrap p	[0.051]	[0.076]	[0.028]
Earlier cohorts (1956–1973)	−0.094	−0.010	−0.009
	(0.067)	(0.026)	(0.018)
Exposed − earlier	−0.104	−0.097*	−0.113**
	(0.093)	(0.052)	(0.044)
Wild cluster bootstrap p	[0.411]	[0.143]	[0.050]
Observations, exposed	475	1,492	2,836
Observations, earlier	358	1,128	2,294

Notes. SOEP, NEPS, and FReDA, wave 2021, East-born respondents. The dependent variable is the number of children. Column (1) reports the local May–June contrast. Columns (2) and (3) widen the birth-month window. The first row reports estimates for the exposed 1974–1983 cohorts. The second row reports estimates for all earlier cohorts observed in the data, born 1956–1973. The third row reports the difference between the two cohort groups from a pooled regression. Narrowing the group of earlier cohorts does not change the picture. Using the ten cohorts before the exposed window, born 1964–1973, the estimates are −0.081 (0.107), 0.013 (0.044), and −0.010 (0.032) across columns (1)–(3), and the corresponding differences in differences are −0.117 (0.124), −0.119 (0.063), and −0.112 (0.052). Using the adjacent 1970–1973 comparison cohorts, the group the Microcensus analysis uses, the estimates are 0.045 (0.085) and 0.057 (0.055) in the two wider windows, with differences in differences of −0.151 (0.093) and −0.179 (0.067). The local May–June cell holds only 135 observations in eight clusters and is uninformative at −0.131 (0.190). All specifications include gender, birth-year, dataset, and age fixed effects. Standard errors are clustered at the birth-year × cutoff-side level. Brackets report wild cluster bootstrap p-values using Webb weights and 9,999 replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.8. Fertility Estimates by Survey in the Birthplace-Based Panels

	Single survey			Leaving one survey out		
	SOEP (1)	NEPS (2)	FReDA (3)	no SOEP (4)	no NEPS (5)	no FReDA (6)
Born before June	−0.123	−0.246**	−0.098*	−0.128**	−0.102*	−0.153***
	(0.073)	(0.093)	(0.057)	(0.048)	(0.050)	(0.047)
Observations	455	221	2,160	2,381	2,615	676

Notes. SOEP, NEPS, and FReDA, wave 2021, East-born respondents in the exposed 1974–1983 cohorts. The dependent variable is the number of children. Columns (1)–(3) estimate the January–May versus June–December contrast separately by survey. Columns (4)–(6) re-estimate the pooled specification leaving one survey out at a time. All specifications include gender, birth-year, and age fixed effects. Pooled specifications additionally include dataset fixed effects. Standard errors are clustered at the birth-year × cutoff-side level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.9. Fertility Estimates across Alternative Definitions of East German Origin and Subsequent Residence

	Number of children				Lives in East today
	SOEP, NEPS, and FReDA			Microcensus	
	Birthplace-defined (1)	Residence-defined, birthplace observed (2)	Residence-defined, full panel sample (3)	Residence-defined (4)	East-born respondents (5)
May-born	−0.198***	−0.173***	−0.064	−0.072***	−0.032
	(0.066)	(0.056)	(0.057)	(0.019)	(0.031)
Observations	475	337	458	5,407	561

Notes. Exposed 1974–1983 cohorts in the May–June window. Columns (1)–(4) report local May–June fertility estimates under alternative definitions of East German origin. Column (5) tests whether May-born status predicts subsequent residence in East Germany among East-born respondents. Columns (1)–(4) are the four estimates plotted in Figure F.4. Columns (1)–(3) use SOEP, NEPS, and FReDA, wave 2021. Column (1) defines East German origin by birthplace. Column (2) applies the current-residence definition to the respondents for whom birthplace is also observed. Column (3) applies the current-residence definition to the unrestricted panel sample. Column (4) reports the corresponding German Microcensus 2018–2019 estimate, which uses current residence. The difference between columns (2) and (3) therefore reflects sample composition rather than the geographic classification of the same respondents. Column (5) regresses current residence in East Germany on May-born status among East-born panel respondents. All specifications include gender, birth-year, and age fixed effects. The panel specifications additionally include dataset fixed effects. Standard errors are clustered at the birth-year × cutoff-side level in the panels and at the cohort–month-of-birth level in the Microcensus. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.10. First-Birth Timing and Completed Fertility

	Exposed cohorts (1974–1983) (1)	Earlier cohorts (1956–1973) (2)	Exposed − Earlier (3)
Age at first birth (years)	−0.867***	−1.001***	0.135
	(0.179)	(0.204)	(0.269)
Observations	903	1,367	2,270
Children ever born	−0.091	0.022	−0.113*
	(0.058)	(0.026)	(0.063)
Observations	1,219	2,239	3,458

Notes. SOEP fertility histories, one observation per East-born respondent. Estimates compare January–May with June–December births within birth year, using all birth months because person-level fertility histories are too sparse in the local May–June window. Column (1) reports the exposed 1974–1983 cohorts, column (2) all earlier cohorts observed in the data, born 1956–1973, and column (3) the difference between the two, estimated from a pooled regression. Age at first birth is observed only among parents and should therefore be interpreted descriptively. All specifications include birth-year fixed effects and a gender indicator. Standard errors are clustered at the birth-year × cutoff-side level. Because the histories are time-invariant, the sample covers every East-born SOEP respondent ever interviewed rather than only respondents observed in the 2021 wave, so it is larger than the SOEP contribution to the panel estimates elsewhere in this appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.11. Schooling under Socialism and Fertility by Marginal GDR-Regime Grade

Outcome variable:	Number of Children			
Marginal GDR-regime grade:	Comparison (1970–1973) (1)	Grades 7–10 (1974–1977) (2)	Grades 4–6 (1978–1980) (3)	Grades 1–3 (1981–1983) (4)
May-born	0.007	−0.148***	−0.071***	0.006
	(0.012)	(0.018)	(0.010)	(0.034)
Wild cluster bootstrap *p*	—	[0.006]	[0.018]	[0.924]
Observations	2,289	2,004	1,606	1,797

Notes. Local May–June estimates by the phase in which the marginal GDR-regime grade occurred. Column (1) reports the 1970–1973 comparison cohorts, for whom the cutoff did not alter school-regime exposure. Columns (2)–(4) report the exposed cohort groups whose marginal GDR-regime grade fell in grades 7–10, grades 4–6, and grades 1–3, respectively. All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level. Wild cluster bootstrap *p*-values use Webb weights and 999 replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

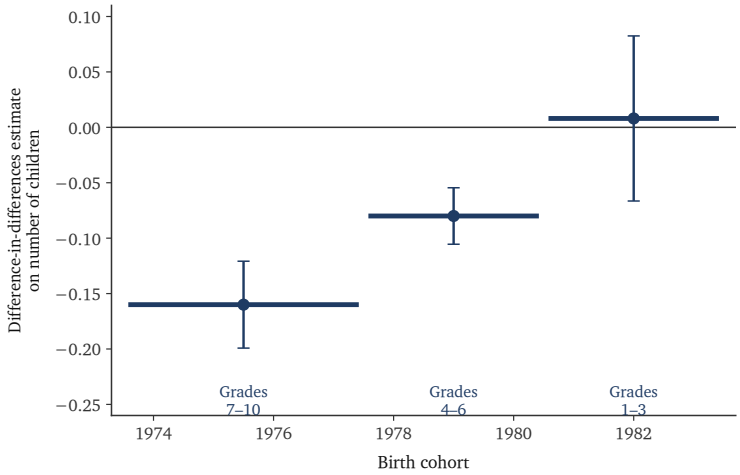


Figure F.5. Fertility Effects by Marginal GDR-Regime Grade: Difference-in-Differences

Notes. Difference-in-differences estimates by marginal-grade group, comparing the May–June gap in each exposed cohort group with the corresponding gap among the 1970–1973 comparison cohorts. The exposed groups correspond to cohorts whose marginal GDR-regime grade fell in grades 7–10, grades 4–6, and grades 1–3. All specifications include age and birth-year fixed effects and a female indicator. Whiskers denote 95 percent confidence intervals based on standard errors clustered at the cohort–month-of-birth level. Appendix Table F.12 reports the underlying estimates.

Table F.12. Schooling under Socialism and Fertility: Difference-in-Differences by Marginal GDR-Regime Grade

Outcome variable:	Number of Children		
Marginal GDR-regime grade:	Grades 7–10 (1974–1977) (1)	Grades 4–6 (1978–1980) (2)	Grades 1–3 (1981–1983) (3)
Difference-in-differences	−0.160***	−0.080***	0.008
	(0.020)	(0.013)	(0.038)
Wild cluster bootstrap p	[<0.001]	[0.004]	[0.923]
Observations	4,293	3,895	4,086

Notes. Difference-in-differences estimates by marginal-grade group. Each column compares the May–June gap in the indicated exposed cohort group with the corresponding gap among the 1970–1973 comparison cohorts. The exposed groups correspond to cohorts whose marginal GDR-regime grade fell in grades 7–10, grades 4–6, and grades 1–3, respectively. All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level. Wild cluster bootstrap p-values use Webb weights and 999 replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

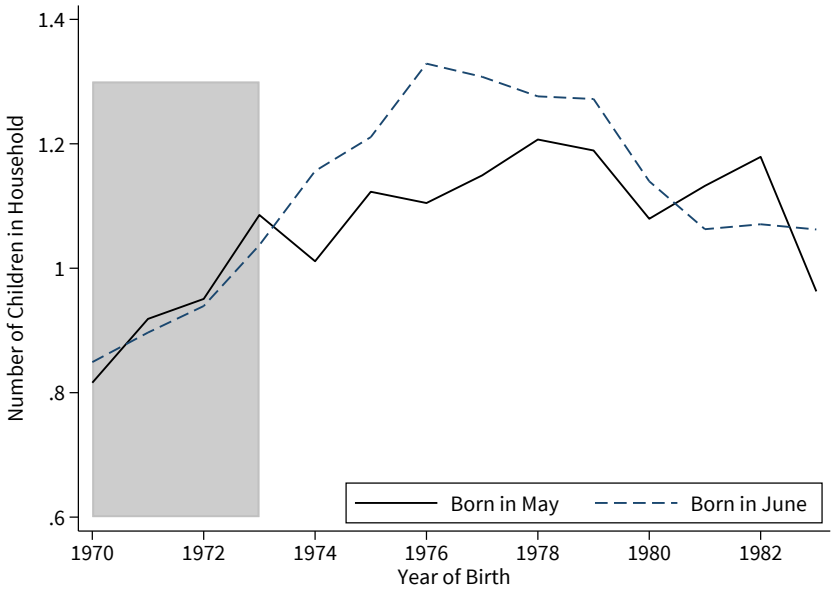


Figure F.6. May and June Fertility Levels by Birth Cohort

Notes. German Microcensus 2018–2019, East German respondents born in May or June. Points show cohort means of the number of children in the household for May births and June births. The shaded 1970–1973 cohorts completed compulsory schooling before reunification and serve as the comparison group. The 1974–1983 cohorts are exposed to cutoff-induced variation in school-regime exposure.

Table F.13. State-Level Childcare Heterogeneity: Coefficients and Implied Fertility Effects

		Regression coefficients			Childcare percentiles		Implied fertility effects		
Measure	Year	Childcare level	May-born	May-born × Childcare	25th	75th	25th pct.	75th pct.	Obs.
Under-3 childcare availability	1994	0.003 (0.004)	−0.493** (0.209)	0.011* (0.006)	32.80	42.90	−0.127*** (0.024)	−0.015 (0.045)	5,407
Under-3 childcare availability	1998	−0.002 (0.003)	−0.294** (0.139)	0.007 (0.004)	24.10	47.20	−0.136*** (0.037)	0.016 (0.070)	5,407
Childcare center density	1994	0.002 (0.003)	−0.407** (0.180)	0.024** (0.011)	13.10	16.33	−0.095*** (0.028)	−0.018 (0.043)	5,407
Childcare center density	1998	0.001 (0.002)	−0.452** (0.190)	0.026** (0.011)	14.88	16.84	−0.072** (0.033)	−0.022 (0.041)	5,407
Public childcare spending per child	1994	−0.004 (0.010)	−0.506*** (0.195)	0.033** (0.016)	12.04	14.29	−0.105*** (0.040)	−0.030 (0.038)	5,407
Public childcare spending per child	1998	−0.005 (0.009)	−0.383** (0.160)	0.025*** (0.008)	10.99	14.85	−0.104*** (0.026)	−0.006 (0.023)	5,407
Broad childcare capacity (ages 0 to 6.5)	1994	0.001 (0.003)	−0.615** (0.250)	0.007** (0.003)	75.20	80.70	−0.068** (0.028)	−0.028 (0.034)	5,407
Broad childcare capacity (ages 0 to 6.5)	1998	−0.001 (0.002)	−0.459** (0.205)	0.005* (0.003)	68.30	78.20	−0.086*** (0.031)	−0.032 (0.033)	5,407
Childcare budget share	1994	0.011 (0.036)	−0.847*** (0.324)	0.094** (0.040)	8.27	8.78	−0.070** (0.031)	−0.022 (0.037)	5,407
Childcare budget share	1998	0.008 (0.031)	−0.968*** (0.348)	0.113** (0.044)	7.81	8.48	−0.082*** (0.030)	−0.006 (0.034)	5,407

Notes. German Microcensus 2018–2019. Each row reports a separate regression for East German respondents in the exposed 1974–1983 cohorts. The dependent variable is the number of children in the household. The regression-coefficient columns report the coefficients on the childcare measure, the May-born indicator, and their interaction. The implied-effect columns report the May-born effect evaluated at the 25th and 75th percentiles of the childcare measure, $\hat{\beta}_{\text{May}} + \hat{\beta}_{\text{May}\times M}\, Q_p(M)$, computed from unrounded coefficients, so they need not be reproduced exactly by the rounded coefficients reported here. The 1994 rows correspond to the specifications reported in Table 4. The 1998 rows repeat the exercise using 1998 values of each measure. Under-three childcare availability is measured as places per 100 children under age three (Deutsches Jugendinstitut, 2002). Childcare-center density is the number of childcare centers per 1,000 children aged 0 to under 6.5 (Thüringer Landesamt für Statistik, 2007). Public childcare spending is annual spending per child aged 0 to under 12, in 100-EUR units (Statistisches Bundesamt, 2005, 2006). Broad childcare capacity is the number of places for children aged 0 to under 6.5 per 100 children in that age range (Thüringer Landesamt für Statistik, 2007). Childcare budget share is childcare net expenditure as a share of total state youth-welfare net expenditure, in 10-percentage-point units (Statistisches Bundesamt, 2005, 2006). All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level and are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.14. Comparison Cohorts and Fertility Across Alternative Measures of Childcare Support

	Measure of state-level childcare support				
	Under-3 childcare availability (1)	Childcare center density (2)	Public childcare spending per child (3)	Broad childcare capacity (ages 0 to 6.5) (4)	Childcare budget share (5)
Panel A. Childcare measures, 1994					
Effect at 25th percentile	0.013	0.006	0.015	0.008	0.018
	(0.018)	(0.019)	(0.022)	(0.018)	(0.022)
Effect at 75th percentile	0.009	0.012	0.021	0.004	0.009
	(0.015)	(0.018)	(0.020)	(0.017)	(0.020)
Panel B. Childcare measures, 1998					
Effect at 25th percentile	0.011	0.004	0.007	0.012	0.006
	(0.021)	(0.021)	(0.023)	(0.020)	(0.023)
Effect at 75th percentile	0.004	0.009	0.014	0.002	0.017
	(0.019)	(0.019)	(0.021)	(0.018)	(0.021)
Observations	2,289	2,289	2,289	2,289	2,289

Notes. German Microcensus 2018–2019, East German respondents in the adjacent 1970–1973 comparison cohorts, who completed compulsory schooling before reunification. The table applies the childcare-heterogeneity specifications of Table 4 to these cohorts. Each column reports the implied May–June gap evaluated at the 25th and 75th percentile of the indicated childcare measure, taken in 1994 (Panel A) or 1998 (Panel B). All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.15. Schooling under Socialism, Income, and Working Hours

Outcome:	All	Women	Men
Monthly household net income (EUR)	−51.927	−98.145	−9.022
	(33.450)	(76.933)	(45.003)
Observations	5,352	2,604	2,748
Monthly individual net income (EUR)	29.287	−14.488	69.754
	(30.020)	(34.135)	(51.386)
Observations	5,377	2,623	2,754
Log individual net income	0.010	−0.008	0.026
	(0.012)	(0.020)	(0.023)
Observations	4,750	2,280	2,470
Weekly working hours	−0.369	−0.966**	0.223
	(0.376)	(0.383)	(0.547)
Observations	4,873	2,338	2,535
Monthly income per weekly hour	−0.059	−1.592	1.416
	(1.470)	(1.992)	(2.469)
Observations	4,267	2,008	2,259

Notes. German Microcensus 2018–2019, East German residents in the exposed 1974–1983 cohorts. Each entry reports the local May–June estimate. Income outcomes are constructed from bracket midpoints. The top-coded category is assigned 1.25 times the top-code threshold. Weekly working hours are observed conditional on employment. The income-per-hour measure is monthly individual net income divided by usual weekly working hours and is not a directly observed hourly wage. All specifications include age and birth-year fixed effects. Pooled specifications additionally include a female indicator. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.16. Labor-Market Conditions at Graduation as an Alternative Channel

Panel A. Labor-market conditions at assigned labor-market entry

	Youth unemployment (%)	Overall unemployment (%)	Employment rate (%)
May-born	−0.084	−0.822	1.017
	(0.611)	(1.291)	(1.489)
Observations	5,407	5,407	5,407

Panel B. Fertility estimates

	Estimate
Baseline specification	−0.072***
	(0.019)
With labor-market controls	−0.055***
	(0.022)
Observations	5,407

Notes. German Microcensus 2018–2019, East German respondents in the exposed 1974–1983 cohorts. Labor-market entry is assigned to age 16 for May births and age 17 for June births. Panel A reports local May–June estimates for youth unemployment, overall unemployment, and the employment rate in the corresponding calendar year. The data are from the Federal Statistical Office, with 1990 values imputed from 1991. Panel B reports the baseline fertility estimate and the estimate obtained after adding the three labor-market measures from Panel A as controls. Because the assigned labor-market-entry year is determined by birth year and birth month, these measures are themselves functions of treatment conditional on birth-year fixed effects. Panel A should therefore be interpreted as describing cutoff-related differences in labor-market conditions rather than as a balance test, and the augmented specification in Panel B conditions partly on treatment-induced variation. All specifications include age and birth-year fixed effects and a female indicator. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.17. Post-Reunification Economic Perceptions and Circumstances

	Birth-month window		
	May–June (1)	March–August (2)	All months (3)
Panel A. Shell Youth Study, 1991			
Optimism about personal future	0.141	−0.020	−0.044
	(0.088)	(0.074)	(0.051)
Optimism about society's future	0.030	−0.021	−0.023
	(0.086)	(0.056)	(0.046)
Observations	81	207	367
Panel B. DJI Youth Survey, 1992–2003			
Satisfaction with financial situation	−0.220	−0.030	0.049
	(0.173)	(0.096)	(0.071)
Observations	579	1,844	3,495
People were better off in the past	−0.006	−0.043	−0.009
	(0.059)	(0.035)	(0.031)
Observations	577	1,836	3,480
Panel C. DJI Youth Survey, 1992, Early Exposed Cohorts			
Satisfaction with financial situation	−0.359	−0.186	−0.133
	(0.634)	(0.248)	(0.197)
Observations	86	324	600
People were better off in the past	0.014	0.033	0.036
	(0.051)	(0.102)	(0.076)
Observations	86	323	598
Panel D. SOEP, 1990–2010			
Worries about own economic situation	−0.037	0.075*	0.033
	(0.040)	(0.045)	(0.037)
Observations	2,308	6,635	12,589
Worries about job security	0.013	0.108*	0.038
	(0.078)	(0.059)	(0.042)
Observations	1,504	4,494	8,431
Life satisfaction	−0.025	−0.027	−0.027
	(0.054)	(0.044)	(0.033)
Observations	2,318	6,655	12,619
Satisfaction with living standard	0.019	−0.031	−0.042
	(0.065)	(0.046)	(0.034)
Observations	1,577	4,518	8,628
Satisfaction with household income	−0.035	−0.006	−0.018
	(0.042)	(0.038)	(0.032)
Observations	2,232	6,338	12,013
Persons	343	1,007	1,996

Notes. Shell Youth Study 1991 (Fischer et al., 1999), DJI Youth Survey (1992, 1997, and 2003 waves), and SOEP 1990–2010, East German respondents. Each entry reports the earlier-minus-later birth-month estimate within the indicated window. Column (1) is the local May–June contrast, and columns (2) and (3) widen the birth-month window. Panel A reports optimism about respondents' personal future and society's future in the 1991 Shell Youth Study, with no additional controls. Panels B and C report financial satisfaction and views on whether people were better off in the past in the DJI Youth Survey, with birth-year and survey-wave fixed effects. Panel D reports economic worries and satisfaction outcomes from the SOEP for respondents who lived in the GDR in 1989. Worries are coded so that higher values indicate greater concern, satisfaction items are measured on a 0–10 scale, and all SOEP outcomes are standardized within the sample. Worries about job security are observed for employed respondents only. SOEP specifications include birth-year and survey-year fixed effects and a gender indicator. Panel A covers the 1974–1978 cohorts, Panels B and D the 1974–1983 cohorts, and Panel C restricts the 1992 DJI wave to the early exposed 1974–1977 cohorts. SOEP observation counts are person-year observations, and the final row reports the number of unique respondents. Standard errors are clustered at the cohort–month-of-birth level, which in the SOEP nests repeated observations for the same individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.18. Schooling under Socialism and Alternative Measures of Work Orientations

	Pooled (1)	Women (2)	Men (3)
Panel A. Acceptance of Maternal Employment			
May–June cohort survey	0.194***	0.197*	0.144*
	(0.060)	(0.109)	(0.075)
Observations	400	223	177
Panel B. Personal Work Attachment			
May–June cohort survey: post-school attachment	0.133**	0.277***	−0.029
	(0.057)	(0.079)	(0.113)
Observations	400	223	177
May–June cohort survey: current attachment	0.199***	0.276***	0.154
	(0.058)	(0.068)	(0.117)
Observations	400	223	177
SOEP: career success important	0.214***	0.237**	0.145
	(0.058)	(0.085)	(0.179)
Observations	544	353	191

Notes. Local May–June cutoff estimates for East German respondents in the exposed 1974–1983 cohorts. Panel A reports the maternal-employment acceptance index from the May–June cohort survey. Panel B reports three measures of personal work attachment: recalled attachment after leaving school, current attachment, and the importance attached to career success in the SOEP. The Panel A row and the first row of Panel B correspond to the outcomes reported in Table 3. All outcomes are standardized to mean zero and unit standard deviation within survey. The May–June cohort survey is the original survey of East German respondents born in May or June in the cohorts used in the cutoff design (Appendix D). The SOEP measure is pooled across the 1992–2021 waves in which it was asked. All specifications include birth-year fixed effects. Pooled columns include a gender indicator, and the SOEP specification includes wave fixed effects. Standard errors are clustered at the cohort–month-of-birth level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.