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**Conference Paper**

# Daring More Agency: Educational Investments and Family Formation Effects of the West German Age of Majority Reform

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# Daring More Agency: Educational Investments and Family Formation Effects of the West German Age of Majority Reform

Anna L.M. Daelen[*†], Lidia Gutu[*‡], C. Katharina Spiess[*‡§]

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Educational investments are among the most consequential decisions in the transition to adulthood, yet little is known about how legal institutions governing decision-making authority affect these choices. We examine the causal effect of legal adulthood on women's educational attainment and family formation by exploiting the 1975 West German age-of-majority reform, which lowered the age of legal adulthood from 21 to 18 years. Because school-entry rules caused students to complete secondary school in different graduation cohorts across federal states, the reform generated plausibly exogenous variation in whether women attained legal adulthood before or after making postsecondary educational decisions. Using data from the German Microcensus and a difference-in-differences design, we find that reaching legal adulthood before leaving secondary school increased university degree attainment by almost 4 percentage points, while decreasing marriage and childbearing. Our findings identify legal adulthood as an important institutional determinant of human capital investments and demonstrate that changes in legal decision-making authority can have persistent consequences over the life course.

Keywords: legal capacity, legal adulthood, autonomy, university attainment, fertility, marriage

JEL Codes: I24, I23, J24

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# 1 Introduction

Educational investments made during late adolescence are among the most consequential decisions over the life course. They shape individuals' human capital, labor market careers, lifetime earnings, and family formation, and thereby contribute to the intergenerational transmission of economic opportunities (e.g., Becker 1964; Becker & Tomes 1986; Cunha & Heckman 2007; Heckman & Mosso 2014). Understanding why some young adults pursue higher levels of education than others has therefore long been a central question in economics. A large literature has identified numerous determinants of educational investments, including expected returns to schooling, financial constraints, parental resources, school quality, educational institutions, and peer effects (e.g., Card 1999; Hanushek & Woessmann 2011; Oreopoulos & Salvanes 2011). The institutional framework governing who has the legal authority to make educational decisions, however, has received considerably less attention.

Many educational choices with long-term economic consequences remain subject to legal restrictions until the age of majority has been reached. One important decision at the transition from adolescence to adulthood is the decision on postsecondary education: to enroll in university, start vocational training, or directly enter the labor market. If the timing of legal adulthood influences educational investments, legal capacity constitutes an additional determinant of human capital formation. Although the age of legal adulthood determines whether adolescents or their parents hold formal decision-making authority at a critical educational transition, along with transferring extensive economic rights, the corresponding institutional rules and reforms have received little attention in the economics literature.

This paper contributes to filling this gap by examining whether obtaining legal adulthood before leaving secondary school affects educational investments in the first place. Moreover, we investigate effects on family formation—a related major life event. We exploit the 1975 West German age-of-majority reform, which lowered the age of majority from 21 to 18 years, transferring a wide range of legal rights from parents to young adults. The reform changed legal decision-making authority without directly altering educational institutions or financial incentives, allowing us to isolate the effect of decision-making agency. Our analysis asks whether reaching legal adulthood before the transition to postsecondary education increased university attainment and whether these educational effects were accompanied by persistent

differences in marriage and fertility over the life course. We provide the first causal evidence that the timing of legal adulthood affects postsecondary education and family formation.

Our empirical analysis exploits a unique feature of the 1975 West German age-of-majority reform. Before the reform, individuals remained legal minors until age 21 and therefore needed parental consent for a wide range of legally binding decisions, such as decisions about postsecondary education. The reform lowered the age of majority to 18, granting adolescents full legal capacity while leaving the structure of secondary and postsecondary education unchanged. The reform is particularly consequential for students acquiring the general university entrance qualification, who graduated at about age 19 according to the statutory grade progression. Because students started and completed secondary school at different ages depending on state-specific school entry cutoff dates, some graduation cohorts reached legal adulthood before making postsecondary educational choices, whereas otherwise similar students remained subject to parental legal authority when graduating from school. This institutional setting generates plausibly exogenous variation in legal adulthood at the transition from secondary to postsecondary education, allowing us to identify its causal effect on educational attainment and family formation.

Our findings show that obtaining legal adulthood before leaving secondary school had persistent effects on both educational attainment and family formation. Women who reached legal adulthood before making postsecondary educational decisions became significantly more likely to obtain a university degree, increasing overall postsecondary degree attainment. These educational gains were accompanied by a lower likelihood of marriage and motherhood. Placebo tests support the credibility of the identification strategy and suggest that the estimated effects are driven by the earlier transfer of legal decision-making authority relative to the statutory school trajectory rather than by other graduation cohort differences.

The analysis focuses on women because legal autonomy was likely to be more consequential for their educational and family decisions: At the time of the reform, West Germany was characterized by a male breadwinner model, where institutions and gender norms substantially constrained women's decision-making (Section 2.3). Earlier legal capacity therefore plausibly expanded women's ability to make independent decisions about postsecondary education, marriage, and fertility. More broadly, women have historically faced greater legal and social restrictions on economic decision-making than men, both in

Germany and internationally (e.g., Tertilt et al. 2022). Thus, a change in legal agency is of particular interest in studying women's decision making.

We study educational attainment and family formation given that both are major life outcomes. Education, marriage, and fertility can affect one another, and are connected by social norms and institutional context (e.g., Mortimer et al. 2005). In particular, there is a well-established negative relationship between educational attainment and family formation (e.g., Ahn & Winters 2025; Nitsche & Brückner 2021; Leesch & Hiekel 2026; for a summary Vasireddy et al. 2023). For women of high educational background, as in our sample, further human capital investments can lead to a marriage penalty through reduced reproductive capital (Low 2024). Moreover, men tend to marry women with lower earnings and lower levels of education than themselves (Akerlof & Kranton 2000; Bertrand et al. 2021). Thus, all else equal, more university education among women may reduce the pool of potential partners. These assortative mating processes may be particularly relevant in the context of the male breadwinner model shaping gender norms in West Germany.

We contribute to three main strands of literature. First, we contribute to the economics of education by identifying legal adulthood as a previously overlooked institutional determinant of educational investments. A large economics literature has identified numerous determinants of educational investments, including financial incentives (e.g., Dynarski 2003), school quality and educational institutions (e.g., Deming et al. 2014), information about the costs and returns to college (e.g., Bettinger et al. 2012), parental resources and family background (e.g., Cameron & Heckman 2001), and risk attitudes (e.g., Boneva & Rauh 2018). In the German context, previous work has shown that changes in tuition fees or changes in study aid programs shaped the attainment of university degrees (e.g., Bietenbeck et al. 2023; Steiner & Wrohlich 2012). Moreover, studies show that an increase in knowledge on the costs and benefits of college enrollment led to an increase in degree attainment (e.g., Peter et al. 2021; Berkes et al. 2022).

Age at graduating from academic-track school varied in Germany also through other levers such as school duration. For instance, the German G8-reform—implemented in federal states after 2000—shortened the duration of academic-track school by a compression of the curriculum. As a result, more students acquired university entry degrees before reaching the age of majority (Huebener & Marcus 2017). Marcus and Zambre (2019) show that the compression of secondary schooling reduced university enrollment, investigating maturity

and higher stress levels as potential mechanisms. However, these studies evaluate reforms within the education system and do not focus explicitly on the role of legal adulthood at the transition to postsecondary education. Our paper is novel in isolating the effect of legal agency as a determinant of postsecondary education decisions, given that the school system remained unchanged for our cohorts of analysis.

Second, the paper contributes to a broader literature on legal institutions and economic behavior by demonstrating that changes in legal capacity influence important life-course decisions even when economic incentives and educational institutions remain unchanged. To date, the literature on changes in legal age thresholds has focused primarily on risky behaviors and their consequences, as well as voting rights and criminal responsibility. For example, a higher minimum legal drinking age has been shown to reduce alcohol consumption, mortality, and crime (Bagues & Villa 2024), and to improve educational outcomes (Carpenter & Dobkin 2011). Studies investigating the effects of lowering the voting age suggest positive effects on adolescents' political engagement, including turnout and pro-civic attitudes (Leininger et al. 2024; Eichhorn & Bergh 2021), while the influence in shaping adolescents' political interest shifted from parents to schools (Zeglovits & Zandonella 2013). More closely related, Geddes et al. (2012) observe that the rise in female labor market participation in the US before the turn of the century might be driven by cohorts of women who were endowed with economic rights in their youth. We provide causal evidence that the *timing* of receiving economic rights matters. In particular, we show that acquiring legal capacity prior to a major decision point shapes women's education and family formation outcomes.

Third, our paper contributes to the literature on family formation by showing that reforms affecting educational decisions during the transition to adulthood have long-run implications for marriage and fertility, highlighting how institutional changes in early adulthood can shape demographic behavior. So far, studies in the German setting have analyzed fertility effects mainly due to changes in school duration (e.g., Alm et al. 2025; Koebe & Marcus 2022). Alm et al. (2025) show that shorter, compressed schooling due to the G8-reform led to earlier labor market entry and childbearing. An increase in education due to longer compulsory schooling, in contrast, reduced completed fertility in West Germany (Cygan-Rehm & Maeder 2013). However, these studies do not consider changes in legal adulthood at the point when students make decisions about postsecondary education. This is the new focus of our study.

# 2 Institutional Setting

## 2.1 The age of majority reform

Our empirical strategy exploits a fundamental change in the legal allocation of decision-making authority between parents and their adolescent children. The Law for the Restructuring of the Age of Majority (*Gesetz zur Neuregelung des Volljährigkeitsalters*) lowered the legal age of majority in West Germany from 21 to 18 years (Bundesgesetzblatt 1974). The legislation was passed by the German parliament in March 1974, and came into effect on 1 January 1975. Everyone born between 1954 and 1956 reached legal adulthood on 1 January 1975, whereas younger cohorts became legal adults on their eighteenth birthday.

Before the reform, adolescents below age 21 were legal minors and therefore lacked full legal capacity. Parents or legal guardians retained authority over all major economic decisions requiring legal contracts: entering employment, enrolling in postsecondary education, managing financial assets, renting accommodation, taking out loans. Adolescents eligible for university studies consequently remained legally dependent on their parents despite assuming increasing economic and social responsibilities. For students completing the academic secondary-school track (*Gymnasium*, see Section 2.2), the reform implied that graduates could, for the first time, shape the transition to postsecondary education without the need for parental signatures or consent. Thus, the reform transferred formal decision-making authority over one of the most important human capital investments from parents to adolescents.

Most importantly, the age of majority reform conferred full contractual capacity, which is foundational to autonomous educational decision-making. Moreover, the reform harmonized the regular legal marriage age at 18 years for both men and women. For women, whose minimum legal marriage age had previously been 16 years, the increase was intended to reduce early marriages, which mainly occurred because of unintended pregnancies and could result from parental pressure (Deutscher Bundestag 1972). By contrast, most other legal domains remained unchanged, including voting rights, the minimum legal age of drinking and smoking, as well as criminal law.[1]

[1] Voting age had already been lowered to 18 by a law change in 1970. At the same time, passive suffrage was tied to the age of majority. Before and after the reform, the legal age for the purchase and consumption of tobacco and low-proof alcohol in public was 16, whereas a minimum age of 18 applied to high-proof spirits. Before and after the reform, defendants between 18 and 21 were put on trial in either juvenile or adult court on a case by case basis.

The reform also did not modify the economic returns to education, university admission requirements, tuition fees, or the structure of the German educational system. Neither did it alter the availability of public financial support for students. Instead, it changed who was legally entitled to choose among existing educational opportunities. Throughout this paper, we therefore interpret the reform as a reallocation of decision-making authority within families at an educational transition point, holding constant the education system.

The reform occurred in the context of deliberate societal liberalization. In his 1969 inaugural government policy statement, Chancellor Willy Brandt announced: "We want to dare more democracy". Among the intended reforms were a lower voting age and a reassessment of the age of legal adulthood (Deutscher Bundestag 1970). The age of majority reform was also motivated by the view that social and economic maturity had shifted to younger ages. By the early 1970s, many young adults between the ages of 18 and 21 had already completed vocational training, entered the labor market, established independent households, or fulfilled compulsory military service. Supporters therefore argued that legal rights should correspond to these existing responsibilities (Deutscher Bundestag 1972).

Support for the reform was broad but not unanimous. Opponents warned that reducing parental authority would expose adolescents to important decisions before they had acquired sufficient maturity, particularly during a period characterized by increasing intergenerational conflict. Proponents argued that "the sense of responsibility grows with the challenge of responsibility" (Deutscher Bundestag 1974). This debate reflects precisely the mechanism investigated in this paper: whether transferring decision-making authority from parents to adolescents affects investments with long-term economic consequences.[2]

### 2.2 The German education system

**The German school system.** The following description of the German education system refers to the institutional setting of the 1970s, although many features are preserved until today. While education policy in Germany falls under the responsibility of the federal states, the basic structure of the education system was largely comparable across states.

[2] A sociological evaluation conducted shortly after the reform concluded that, in approximately one-third of the cases examined, lowering the age of majority directly enabled adolescents to pursue greater autonomy in the face of parental resistance (Lauber et al., 1980). Additionally, parents may become more permissive in the first place once their children turn legal adults, given that the legal and moral responsibility is lifted off of parents (Lauber et al., 1980).

According to the statutory school entry rule, children entered primary school at age six or seven, depending on their month of birth and the school start cutoff date. During the period considered in this paper, cutoff dates differed across federal states and changed frequently over time (e.g., Koebe & Marcus 2022). Children born before the cutoff typically entered primary school in the calendar year they turned six, whereas children born after the cutoff started school one year later at age seven. Although provisions for early or delayed school entry existed, exceptions were relatively uncommon and most children entered school according to the statutory rules (Barabasch et al. 2025).

After four years of primary school, students were tracked into different secondary school types. While the lower secondary tracks (*Hauptschule* and *Realschule*) prepared students for vocational training, the academic track (*Gymnasium*) culminated in the *Abitur*, the general university entrance qualification, and prepared students for university studies.

**Legal adulthood and the transition to postsecondary education**. During the period under study, *Gymnasium* comprised 13 grades, and students graduated at about age 19. Graduation from *Gymnasium* marked the point at which young women decided whether to enroll in university or pursue alternative postsecondary pathways, most notably vocational training. Because these decisions were generally taken immediately after leaving school, the reduction of the age of majority from 21 to 18 directly affected the institutional framework under which education decisions were made. Prior to the reform, women who had not yet reached the age of 21 needed parental consent to enroll in university or to undertake other legally binding steps associated with their career transition. Following the reform, women could make these decisions independently from the age of 18 onwards.

The timing of postsecondary transitions further reinforces the relevance of graduating from school as a legal adult. The school year ended in July, university studies started in October, and vocational training programs generally began in September. Most graduates entered postsecondary education without substantial delays. For the 1976 school graduation cohort (earliest data available), 80 percent of women with a university degree started their studies within one year after academic-track graduation (Bundesministerium für Bildung und Forschung 2002).[3] By our empirical strategy we identify marginal cohorts of women who

[3] For men with a university degree, the share is lower at 72 percent (Bundesministerium für Bildung und Forschung 2002), because men were subject to a 15-months compulsory military service. Among men born in

turn legal adults either half a year before or half a year after graduating from school with an *Abitur* (Section 3.2).

**Postsecondary education in Germany.** Women graduating from *Gymnasium* faced two main, fundamentally different postsecondary options: enrolling in university or entering vocational education and training. University studies were largely self-directed, involved long study durations, and generally did not provide an earned income. Before Bachelor and Master degree programs were introduced in Germany in the early 2000s, university studies typically culminated in a single degree equivalent to a Master's degree, with median study durations of around six years (Statistisches Bundesamt 2023). Pursuing higher education therefore typically required young adults to establish an independent household, manage their own finances, and enter into contractual arrangements, all of which required full legal capacity. Vocational training, on the other hand, usually lasted three years and provided trainees with a regular income. Combining employment with formal instruction, vocational training qualified for a wide range of jobs, including careers in banking and public administration.

Importantly, financial barriers to higher education had already been substantially reduced before the reform. Tuition fees had been abolished in 1970, and the Federal Training Assistance Act (Bundesausbildungsförderungsgesetz, BAföG), introduced in 1971, provided means-tested financial support to students with the explicit objective of expanding access to higher education (Deutscher Bundestag 1971). In the early 1970s, up to 45 percent of students received financial assistance; from1974 onwards, a mandatory interest-free loan was tied to receiving financial assistance (Iost 2026). In addition, paid employment during semester breaks—and, to a lesser extent, during term time—was common (Bundesregierung 1974). Thus, in principle, students could attend university regardless of parental income.

University participation had expanded considerably since the late 1960s as part of broader efforts to increase educational attainment. However, vocational education remained the predominant pathway in the German population at the time of the reform, and university degrees were particularly rare among women (Reinberg & Hummel 2002). Section 3 shows descriptive statistics for our sample of women.

1955 or 1956, about 58 percent served in military or alternative service, also when conditioning on university entrance qualification (Daelen 2026).

### 2.3 Gender norms and family institutions

The age of majority reform took place during a period of profound social change in West Germany. While educational expansion and the women's movement increasingly challenged traditional gender roles, family life and labor market participation were still largely organized around the male breadwinner model. Women were generally expected to marry relatively early, interrupt employment for family formation, and assume primary responsibility for household work and childrearing (e.g., Trappe et al. 2015). These social norms were reflected in legal institutions that assigned distinct roles to men and women within marriage and the family. For instance, women could not take up employment without their husband's permission until the 1977 reform of marriage and family law (Böhmer 1977), which reflected a gradual shift towards greater autonomy for women.[4]

Although women's educational attainment had increased substantially by the early 1970s, higher education was still considerably less common among women than among men, and long educational careers and continuous labor market participation by women were far from the norm. For young women who had obtained an *Abitur*, parents and daughters may have assessed postsecondary education choices differently: vocational training was shorter, paid from the outset, and led to secure white-collar employment, all of which was readily compatible with early family formation, whereas university studies required a longer commitment without earned income and returns that depended on sustained labor market attachment. Parents may have placed greater weight on the possibility of early family formation or on prevailing social roles, whereas daughters increasingly faced expanding educational and occupational opportunities.

Against this institutional background, lowering the age of majority represented an important transfer of decision-making authority from parents to young women precisely when decisions about postsecondary education were made. By granting full legal capacity at age 18, it enabled young women graduating from *Gymnasium* to enroll in university, enter contracts, establish independent households, and organize their educational pathways without requiring

[4] Women's role as homemakers continued to be reflected by the principle of the family wage earned by the male breadwinner, as well as by joint taxation and the co-insurance of nonworking spouses in statutory social security. Public day care was limited to mornings and children of age three and above (Barth et al. 2020). Accordingly, increases in female labor market participation during the 1970s were mainly driven by part-time work (Trappe et al. 2015).

parental consent. The reform therefore reduced parents' formal ability to influence educational choices at a time when daughters' own preferences regarding higher education may increasingly have differed from those of their parents. By shifting whether women graduate from *Gymnasium* as legal adults or not, the age of majority reform provides a unique opportunity to examine whether increasing legal autonomy affected young women's investments in higher education.

# 3 Data and Empirical Strategy

## 3.1 Data

We use pooled cross sections from the German Microcensus covering the years 2005 to 2021.[5] The Microcensus is an annual survey conducted by the Federal Statistical Office that covers one percent of the German population and provides detailed information on educational attainment, labor market outcomes, family characteristics, and demographic background (Statistisches Bundesamt 2020). Participation is mandatory for sampled households, resulting in exceptionally high response rates and minimizing concerns about selective nonresponse or panel attrition. The Microcensus therefore provides highly representative information on education and family outcomes.

Our empirical analysis focuses on women born between July 1955 and June 1956 in West Germany who obtained an *Abitur*, the general university entrance qualification. For these women, who finished school at about age 19, the age of majority reform altered the legal framework at the transition to postsecondary education (see Section 2). We exclude women with lower secondary qualifications, who typically completed their educational transitions several years earlier regardless of the reform (at about age 16). We further exclude individuals who obtained the *Abitur* through second-chance education, since they initially obtained lower schooling degrees, transitioning to their first postsecondary education also several years before reaching the age of majority regardless of the reform.

Our identification strategy exploits variation in statutory school entry rules across federal states. Because the Microcensus does not record the federal state in which respondents

[5] In later analyses, we investigate the year of the current or most recent marriage. Therefore, we additionally use the Microcensus waves 1985 to 1990. Both schooling degrees and the age of marriage are included only in the data sets 1985, 1987, and 1989. We cannot use the 1985–1990 data for our identification strategy to analyze family formation during early adulthood because these earlier data sets do not contain birth months, and only some of them contain schooling degrees.

attended school, we follow the established approach in the literature and use the respondent's current federal state of residence as a proxy for the federal state in which she completed secondary education (Pischke 2007; Pischke & von Wachter 2008; Cygan-Rehm & Maeder 2013; Margaryan et al. 2021; Koebe & Marcus 2022). Thus, we assume that women—at the time of survey participation (ages 49 to 66)—reside in a federal state with the same school start cutoff as the federal state in which they started school.[6] We return to this assumption and discuss its implications in Section 3.3.

We restrict the analysis to West German women. Accordingly, we exclude respondents residing in East Germany or reporting educational qualifications obtained in the former German Democratic Republic (GDR). We further exclude the three city states—Berlin, Hamburg, and Bremen. East and West Berlin cannot be distinguished in the data; the city states exhibit higher inter-state mobility than the territorial states (Figure A1), while providing only limited variation in treatment assignment within our estimation window (Table 2). Finally, we exclude migrants and women without German citizenship because our identification strategy assumes exposure to the West German schooling system and its school entry regulations. Appendix Table A1 documents the sample construction in detail.

Our primary outcomes capture completed educational attainment. We distinguish between university degrees and vocational qualifications as the highest professional degree obtained. University degrees include qualifications from both traditional universities (*Hochschulen*) and universities of applied sciences (*Fachhochschulen*). In addition, we examine women's marital status and the number of children women have given birth to. Fertility information is available only for women and was collected in four Microcensus waves (2008, 2012, 2016, and 2018). Consequently, analyses of fertility outcomes rely on smaller samples than the analyses of educational attainment and marital status. Although responses to the fertility module were voluntary, approximately 90 percent of eligible women answered these questions. Appendix Table A6 provides no evidence of selective nonresponse.

Because the women in our sample are born between 1955 and 1956, they are between 49 and 66 years old when outcomes are observed (with age measured in years). Educational

[6] Treatment assignment is not affected by people moving within federal states, neither by moving between federal states during early adulthood if moving back to the federal state of school attendance in later adulthood (before participation in the Microcensus). Treatment assignment is also unaffected by moving between federal states which had the same school start cutoff, for example, between Lower Saxony, Schleswig-Holstein, Bremen, or North Rhine-Westphalia.

attainment and fertility can therefore be regarded as completed life-cycle outcomes, ruling out that our estimates reflect only temporary differences in educational enrollment or family formation.

Table 1 presents descriptive statistics. Among West German women of all schooling backgrounds in Panel A, 70 percent hold vocational training as their highest professional degree, 14 percent hold a university degree, and 15 percent have no postsecondary degree. Approximately 17 percent of women obtain the *Abitur*, among whom 74 percent hold a university degree, and 20 percent hold vocational training. Regarding family formation, 67 percent of women are married, 15 percent have never been married, and 29 percent remain childless.

### 3.2 Empirical approach and identification

Our treatment is defined by whether women have reached the age of legal majority before obtaining the *Abitur*, Germany's general university entrance qualification. All women in our main sample became legal adults on 1 January 1975 because of the reform, and graduate from school in 1974 or 1975, depending on birth month and federal state. Those graduating in July 1975 graduated as legal adults, while those graduating in July 1974 still needed parental consent to enroll immediately in postsecondary education. Treated women attained full legal capacity six months before graduating from the academic school track. By contrast, untreated women reached legal adulthood only six months after graduation. In the absence of parental consent, they could only begin postsecondary education in the following year, leading to an interruption in their educational trajectory of at least nine months.

Identification exploits variation in school entry rules across West German federal states. School entry cutoffs differed by state, implying that individuals with identical birth dates graduated in different calendar years depending on the state in which they attended school. This institutional variation generated plausibly exogenous differences in whether students reached legal adulthood by the statutory graduation date, conditional on birth month and federal state, given that the reform granted legal adulthood to all cohorts in our main sample on 1 January 1975.

Table 2 illustrates the assignment mechanism. The statutory graduation dates are determined by birth month and state-specific school entry cutoffs. For example, Bavaria used a

September 30 cutoff, whereas Baden-Württemberg used a December 31 cutoff. A woman born in October 1955 would graduate in July 1975 as a legal adult in Bavaria, but one year earlier as a legal minor in Baden-Württemberg. Thus, women born in the same month differed in legal adulthood at the statutory graduation date solely because of institutional differences in school entry regulations across federal states.

We employ a difference-in-differences model with two-way fixed-effect estimation, using the following specification:

$$(1)\quad Y_{ism} = \tau\, Treated_{ism} + \alpha_{is} + \beta_{im} + \gamma_{ism} + \epsilon_{ism},$$

where $Y_{ism}$ denotes the outcome variable of women $i$ in federal state $s$, who is born in year and month $m$. The dummy variable $Treated_{ism}$ indicates whether women $i$ had turned legally adult before the statutory date of receiving the general university entrance qualification. Thus, the treatment effect $\tau$ is an intention-to-treat effect. $\alpha_{is}$ are federal state fixed effects, capturing time-constant individual differences by federal state. $\beta_{im}$ are birth-year-and-month fixed effects, capturing seasonality and shocks common to all federal states, including season-of-birth effects (e.g., Buckles & Hungerman 2013), and macroeconomic trends or shocks (e.g., Akbulut-Yuksel et al. 2020). Finally, $\gamma_{ism}$ denote survey year fixed effects.

The estimation sample is restricted to the smallest window that provides at least one birth quarter of treatment and control observations in every federal state. As shown in Table 2, this window comprises women born between July 1955 and June 1956.

### 3.3 Identification assumptions

Our identification strategy relies on the assumption that, absent the reform, adjacent graduation cohorts would not have differed systematically in educational attainment or family formation. Because treatment assignment is determined exclusively by the interaction between birth date, state-specific school entry rules, and the nationwide implementation date of the reform, exposure is plausibly exogenous conditional on the included fixed effects.

We assess the validity of this assumption in several ways. First, we conduct placebo analyses that assign the reform to earlier cohorts (Section 4.2). These tests consistently yield estimates close to zero. Second, Table A2 shows that treatment did not affect the probability of obtaining the *Abitur*, neither for our estimation sample, nor for the preceding placebo reform

years in which all cohorts graduated as legal minors. Thus, we can rule out that being born before or after the cutoff affected whether women complete academic-track school; and we can rule out treatment effects on completing academic-track school. Third, Appendix B discusses potential confounding factors, including the education expansion, contemporaneous institutional reforms, and macroeconomic conditions. We argue that these factors affected the relevant graduation cohorts similarly and are therefore unlikely to explain our estimates.

A potential concern is that the Microcensus does not observe the federal state in which respondents attended school. As described in Section 3.1, we use respondents' current federal state of residence as a proxy. This approximation is justified by the relatively low level of interstate mobility in West Germany at the time. Previous studies report that current and earlier federal-state residence coincide for roughly 80 percent of individuals or more (Pischke 2007; Margaryan et al. 2021; Cygan-Rehm 2025). Any resulting measurement error is likely to attenuate the estimated treatment effect because treatment assignment is measured with error rather than systematically biased. Differential migration induced by the reform would threaten identification only if exposure to legal adulthood before graduation systematically affected interstate mobility. We view this scenario as unlikely. West German federal states all offered broad access to higher education, reducing incentives for reform-induced migration.[7] Moreover, being born before or after the school start cutoff is unrelated to later interstate mobility (Barabasch et al. 2025), and additional education does not affect interstate migration among women (Huebener 2025).

# 4 Results

We estimate Equation (1) by ordinary least squares. Standard errors are clustered at the federal-state level, reflecting the level at which school entry regulations are determined. Because the number of clusters is small at 8 federal states, we additionally report *p*-values based on the wild cluster bootstrap-*t* procedure following Cameron and Miller (2015).

## 4.1 Main results

Table 3 presents the main estimates of the effect of reaching legal adulthood before graduating from *Gymnasium*. Overall, the age of majority reform increased women's

[7] The federal states with smaller populations in our sample are Saarland or Schleswig-Holstein. But the largest neighboring federal states (Rhineland Palatinate for Saarland, and Lower Saxony for Schleswig-Holstein) had the same school start cutoffs.

postsecondary educational attainment. Women who had already reached legal adulthood at the time of completing secondary school were 3.2 percentage points more likely to obtain a postsecondary qualification than comparable women who graduated while still being legal minors. This increase is driven entirely by university education. The probability of completing a university degree rises by 3.7 percentage points, corresponding to 5 percent of the sample mean, whereas the probability of obtaining a vocational qualification remains unchanged.

These findings suggest that transferring legal decision-making authority from parents to daughters before the transition to postsecondary education increased investments in higher education. Rather than women shifting between vocational and university education, the reform increased overall postsecondary educational attainment, with gains concentrated in university completion.

The reform also had persistent consequences for family formation. Women who had already attained legal adulthood before leaving *Gymnasium* were 5.8 percentage points less likely to be married in adulthood, corresponding to an 8.7 percent reduction relative to the sample mean. This effect size on marriage is largely mirrored by an increase in the probability of being single, i.e., never having been married. We observe no effects on the probability of having another marital status in adulthood such as being divorced or widowed. The reform therefore appears to have primarily influenced the incidence of partnership formation.

Table 4 shows that the effects on education and marriage extend to fertility outcomes. Women who became legal adults before completing *Gymnasium* were 6 percentage points less likely to have children. The magnitude of the effect on childlessness largely corresponds to those on marriage, in line with marriage and motherhood being "tied events" in West Germany at the time (Kreyenfeld & Konietzka 2017). Thus, the fertility effect on the extensive margin is predominantly explained by partnership formation.

The results in Table 4 further suggest that the reform also affected the intensive fertility margin. Column 6 shows that women who graduated as legal adults had 0.24 fewer children on average compared to women who graduated as legal minors. Two findings indicate that this effect is too large to run through the extensive margin alone. First, relating the point estimate in Column 6 to the one in Column 2, assuming that the reform operates only through the extensive margin, implies that the women deterred from motherhood by the reform would have had 3.6 children

on average. The high counterfactual number of children is implausible given a sample mean of 2 children among mothers. Second, the treatment effect on having more than one child (Column 3) is larger in absolute terms than the effect on having at least one child (Column 2), which is algebraically impossible under a monotonic extensive-margin-only fertility effect. Taken together, the results and patterns are consistent with forgone and postponed family formation, as well as higher opportunity costs of additional children—all pointing to increased educational attainment as the underlying fundamental mechanism.

Beyond their statistical significance, our estimates are economically meaningful when viewed in the context of the broader economics of education literature. Previous research has shown that educational attainment responds to a wide range of policy interventions targeting the costs or returns to education, including financial aid, tuition reforms, information interventions, and changes in compulsory schooling laws (e.g., Dynarski 2003, 2008; Oreopoulos 2006; Peter et al. 2021; Berkes et al. 2022). Many of these interventions increase postsecondary educational attainment by a few percentage points, although larger effects have been found for major institutional reforms and for disadvantaged populations. Against this background, the estimated increase in university degree attainment associated with earlier legal adulthood is plausible. Because we only observe university degrees and not university enrollment, the effects on university degrees likely reflect a lower bound on the effect on initial educational choices.

The effects on family formation are hard to benchmark, given that the literature shows mixed effects of education on completed fertility (see Cygan-Rehm & Maeder 2013 for a discussion of the literature), and causal studies are scarce especially with respect to higher education. More broadly, higher education and career orientation is associated with later marriage and childbearing, which likely translate to lower completed family formation (e.g., Goldin 2006 for a summary), also in the German context (Blossfeld & Huinink 1991; Nitsche & Brückner 2021; Leesch & Hiekel 2026). Particular to the West German setting, an additional year of schooling reduces the probability of ever having children by 5 percentage points for women affected by an increase in compulsory schooling—where the large effects are likely explained by the high opportunity costs of childbearing (Cygan-Rehm & Maeder 2013). By the same token, our effects are plausible given that we focus on women who are eligible for university studies, for whom the opportunity costs of childbearing are particularly high.

**Robustness.** Given that participation in the Microcensus is mandatory, sample selection is a minor concern. Our results could be amplified by selection if treated women with lower

postsecondary education and higher fertility have a higher mortality or a higher probability to emigrate, and are therefore unobserved in the sample. Table A3 of the Appendix presents a robustness check of our main results excluding the data after 2019, exploiting the COVID-19 pandemic as a shock to mortality. The results are very similar.

### 4.2 Placebo analyses

Our identification strategy requires that adjacent graduation cohorts would not have differed systematically absent the reform (Section 3.3). We assess this assumption using a series of placebo reforms that assign treatment to cohorts graduating in the years before 1975, none of whom turned legal adult before leaving school. Appendix Tables A7 and A8 describe the construction of the placebo treatment assignments. As in the main specification, each placebo analysis uses the smallest birth-cohort window that contains at least one birth quarter of both treated and control observations within every federal state, while ensuring that none of the included cohorts is actually exposed to the reform. Consequently, the placebo estimates only capture school cohort differences other than legal adulthood at the statutory graduation date.

Figure 1 presents the corresponding placebo estimates for the main outcomes. Each coefficient compares the predicted graduation cohort in a given placebo year with the immediately preceding cohort, using the same identification strategy as in the main analysis. Across all specifications, the placebo effects are economically small and statistically insignificant. By contrast, the treatment effects associated with the actual reform are substantially larger in absolute terms. In particular, the reform year is the only one to display a coherent, systematic pattern of treatment effects throughout education and family formation outcomes: more education, fewer marriages, and fewer children. The placebo analyses confirm that the estimated treatment effects are not driven by broader trends in graduation cohorts or by birth relative to the school start cutoff date, but can be attributed to reaching the age of majority before obtaining the *Abitur*. Otherwise, we would expect to observe significant effects also in the placebo reform years.

We conduct an additional placebo test using women with lower secondary qualifications. Because these women typically completed school at around age 16 and entered vocational training before reaching the age of majority, the reform did not affect their educational transition, although school entry regulations generated the same school cohort variation as in our main sample. As shown in Appendix Table A4, treatment effects are uniformly close to

zero. The placebo analysis on women of lower school tracks suggests that our findings are unlikely to be driven by broader cohort trends, changes in social norms, or other contemporaneous developments unrelated to the reform.[8]

### 4.3 Interpretation

The estimated effects on educational attainment, marriage, and fertility should be interpreted as *total effects* of the age of majority reform. These outcomes are realized sequentially over the life course and may causally affect one another. Educational attainment may influence subsequent partnership formation and fertility decisions, and vice versa. Consequently, our estimates capture not only the direct effect of reaching legal adulthood before leaving secondary school, but also any indirect effects operating through educational and family choices.

A first interpretation of our findings is that the reform affected family formation primarily through its impact on women's educational attainment. As shown above, the reform substantially increased university completion without affecting vocational qualifications. Higher educational attainment may subsequently have altered partnership formation and fertility through several channels, including changes in women's labor market opportunities, the opportunity costs of family formation, and educational assortative mating (for a summary, e.g., Goldin 2006). These channels are well established in the demographic and economic literature, and likely were particularly relevant in the institutional context of 1970s West Germany, where traditional gender norms and the male breadwinner model continued to shape marriage-market behavior.[9]

Table 5 shows treatment effects on partner characteristics, considering the highest postsecondary degree of the partner.[10] Similar to the results for marital status, Column 1

[8] The effects for men with Abitur are also mostly economically small and statistically insignificant (Appendix Table A5). However, these results are hard to interpret because men's transition to postsecondary education is affected by compulsory military service. On the other hand, we expect that the reform affected men less because university studies were more common among men, and they were subject to fewer constraints and opportunity costs.

[9] For instance, Sprengholz et al. (2022) show for West Germany that situations where the wife earns more than her husband were systematically avoided, consistent with men disliking wives who earn more than themselves and therefore tend to marry women with lower income (Bertrand et al. 2015). Even in the years after the reunification, 50 percent of the West German population agree to the statement that it is "almost certain to cause problems" if a woman earns more money than her husband (Sprengholz et al. 2022).

[10] In this analysis, "partner" is defined as a spouse living in the same household, because educational degrees of partners are only observed for spouses living in the same household in the Microcensus data.

shows that women who reached legal adulthood before graduating from *Gymnasium* are around five percentage points less likely to have a partner at the time of our observation period. Considering the postsecondary degree of the partner, treated women are less likely to have a partner with vocational training, which is the predominant education level in the overall population. Despite higher university degree attainment, treated women are not more likely to have a partner with a university degree.

These patterns are consistent with educational assortative mating in the context of the male breadwinner model, where women with high human capital investments incur a marriage penalty through reduced reproductive capital (Low 2024), and men tend to marry women with lower earnings and lower levels of education than themselves (Akerlof & Kranton 2000; Bertrand et al. 2021; De Hauw et al. 2017). In particular, the results suggest that women's educational attainment had important implications for their long-term marriage market outcomes, contributing to increasing childlessness. For women born in West Germany between 1950 and 1964, childlessness rates doubled to 20 percent, and were particularly high among female academics (Kreyenfeld & Konietzka 2017). Our results are also in line with findings in other developed countries, where skilled women have lower marriage rates than unskilled ones, which could arise from norms against working women (Bertrand et al. 2021).

Another possible explanation for our results is that exercising agency enhances autonomous decision-making—which can be thought of as a competence related to self-efficacy and locus of control (Bandura 1977; Almlund et al. 2011). Legal decision-making authority was transferred during a formative period[11] in connection with an early opportunity to exercise agency by shaping a major educational transition as a legal adult. The experience of exercising agency may have enhanced self-efficacy (Bandura 2001), fostering more independent decision-making later in life. As a result, treated women may have been less willing to enter partnerships and roles they would otherwise have accepted. Although we cannot test this mechanism with our data, more autonomous decision-making may have reinforced the proposed mechanism of the higher opportunity costs of family formation through increased educational attainment.

[11] Research in cognitive science suggests that early adulthood is a formative period in which the executive brain functions develop (Fuhrmann et al., 2015).

Other explanations are unlikely to account for our findings. We see no evidence of early marriages driving our results, suggesting that the full contractual capacity at the educational transition—allowing women to plan and execute educational investments—is key to interpreting our findings. For women with *Abitur*, "shotgun marriages" below age 18 were much rarer than for women of lower schooling backgrounds (less than 0.1 percent for birth years 1955 to 1956), and this percentage remains constant across birth years. Similarly, strategic marrying below age 21 in order to become legally adult is not a relevant phenomenon in our setting, and we find no treatment effects on marriages below age 21.[12]

Finally, the reform also implied earlier independent access to oral contraceptives, which could increase educational attainment and delay family formation (Goldin & Katz 2002). However, this change affected both treated and control women within our estimation sample because all cohorts turned legal adults on 1 January 1975. Differences in contraceptive access therefore cannot explain the treatment effects identified by variation in legal adulthood at school completion.

# 5 Conclusion

Overall, our findings demonstrate that legal institutions can shape educational investments by determining when young people acquire the legal authority to make economically consequential decisions on their own. The institutional setting we study is the 1975 West German age of majority reform. Similar legal reforms were initiated throughout Europe in the 20th century (Council of Europe 1972), and around the world until recently, also in light of demographic challenges (The National 2026; Osumi 2022). Our results suggest that legal capacity is not merely a legal concept but an economically relevant institution that can have lasting consequences for human capital accumulation and subsequent demographic behavior.

Given that our treatment is defined as obtaining university entrance qualification as a legal adult, our findings speak to the age of majority, the duration of academic-track school, and school starting age. In Germany, where academic-track school duration is among the longest, reductions in school duration across federal states led to more "early birds" finishing school as legal minors (Huebener & Marcus 2017). However, several German states have since

[12] The dominant reason for early marriages was to legitimize unplanned pregnancies (Deutscher Bundestag 1972). Comparing birth cohorts who graduate as legal minors to those who graduate as legal adults, the fraction of marriages below age 21 remains constant.

reversed the reform, while others continue to offer an accelerated pathway to the *Abitur*. In many OECD countries with shorter school durations, it is more common that students finish upper secondary school as legal minors. While some countries seek to address the challenges of aging societies to labor markets and social security systems by reducing school duration, others have lowered school starting age (Thomsen 2015). While the maturity of graduates is frequently discussed in the context of school reforms, the age of majority at the transition to postsecondary education has received little attention.

Our study also contributes to a broader literature examining how institutions shape individual behavior. In particular, legal rules may influence human capital accumulation by determining who is entitled to make economically important decisions during key developmental stages. The timing of legal adulthood therefore deserves greater attention as a component of the institutional environment in which educational investments are made.

Our results do not imply that lowering the age of majority necessarily increases education. Rather, policymakers considering reforms affecting adolescents' legal rights should recognize that such reforms may influence educational trajectories and family formation. Although institutional settings differ across countries and over time, the findings illustrate that legal reforms can affect behavior through channels extending beyond their immediate legal objectives.

There might also be effects on other outcomes unobserved in this setting, such as attitudes, personality, or noncognitive skills, which are not captured in the German Microcensus or in most administrative data. Thus, further studies based on large enough surveys may consider these channels. Another limitation of our data is that the German Microcensus does not contain parental characteristics. Therefore, we cannot test for heterogeneity by parental education. However, we expect the effects to be larger if parents do not have a university degree and therefore might be more skeptical about university education. Another limitation is that the data do not contain educational biographies but only the highest degree obtained.

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Table 1: Descriptive statistics on women's education and family formation

| | mean | $N$ |
|---|---|---|
| **Panel A.** Women in West Germany overall | | |
| General university entrance qualification | 0.165 | 53,116 |
| Any postsecondary degree | 0.847 | 52,949 |
| University degree | 0.144 | 52,949 |
| Vocational degree | 0.700 | 52,949 |
| **Panel B.** Women with general university entrance qualification | | |
| Any postsecondary degree | 0.946 | 8,185 |
| University degree | 0.742 | 8,185 |
| Vocational degree | 0.204 | 8,185 |
| Single (never married) | 0.151 | 8,185 |
| Married | 0.665 | 8,185 |
| Divorced | 0.138 | 8,185 |
| Has given birth to children | 0.712 | 1,771 |
| Number of children | 1.414 | 1,715 |
| Number of children \| having children | 2.014 | 1,196 |
| Marriage at age 18 or younger | 0.001 | 4,135 |

*Notes*: Included are West German women born between July 1955 and June 1956. In Panel B, we condition on having the general university entrance qualification (Abitur), as in our main sample. In Panel A, women with Abitur and lower schooling qualifications are included; women with GDR-specific schooling degrees are excluded. For education and marital status, the source is the German Microcensus 2005–2021; for fertility, the source is the German Microcensus 2008, 2012, 2016, and 2018. For the age of marriage, the source is the German Microcensus 1985, 1987, and 1989, where the age of marriage is elicited for the current or last marriage. Own calculations, the means are weighted.

Table 2: Statutory graduation date, by federal state and quarter of birth

| | | | window of analysis | | | | | |
|---|---|---|---|---|---|---|---|---|
| Quarter of birth | (1955 Q1) | (1955 Q2) | 1955 Q3 | 1955 Q4 | 1956 Q1 | 1956 Q2 | (1956 Q3) | (1956 Q4) |
| Bavaria | July 1974 | July 1974 | July 1974 | **July 1975** | **July 1975** | **July 1975** | **July 1975** | **July 1976** |
| Baden-Württemberg | July 1974 | July 1974 | July 1974 | July 1974 | **July 1975** | **July 1975** | **July 1975** | **July 1975** |
| Hesse | July 1973 | July 1974 | July 1974 | July 1974 | **July 1975** | **July 1975** | **July 1975** | **July 1975** |
| Lower Saxony | July 1973 | July 1974 | July 1974 | July 1974 | July 1974 | **July 1975** | **July 1975** | **July 1975** |
| Rhineland-Palatinate | July 1973 | July 1974 | July 1974 | July 1974 | July 1974 | **July 1975** | **July 1975** | **July 1975** |
| Nord Rhine-Westphalia | July 1973 | July 1974 | July 1974 | July 1974 | July 1974 | **July 1975** | **July 1975** | **July 1975** |
| Schleswig-Holstein | July 1973 | July 1974 | July 1974 | July 1974 | July 1974 | **July 1975** | **July 1975** | **July 1975** |
| Saarland | July 1973 | July 1974 | July 1974 | July 1974 | July 1974 | **July 1975** | **July 1975** | **July 1975** |
| (Bremen) | July 1973 | July 1974 | July 1974 | July 1974 | July 1974 | **July 1975** | **July 1975** | **July 1975** |
| (Hamburg) | Mar 1974 | **Mar 1975** | **Mar 1975** | **Mar 1975** | **Mar 1976** | **Mar 1976** | **Mar 1976** | **Mar 1976** |
| (West Berlin) | Mar 1974 | **Mar 1975** | **Mar 1975** | **Mar 1975** | **Mar 1975** | **Mar 1976** | **Mar 1976** | **Mar 1976** |

*Notes*: This table shows the statutory graduation date, based on birth quarter and federal state, for obtaining the general university entrance qualification (*Abitur*). Everyone born 1955–1956 turned legal adult on 1 January 1975 by the age of majority reform. Cohorts marked in bold are assigned to treatment, i.e., turn legal adults before their statutory graduation date. In parentheses are the federal states and birth quarters which are excluded in the analysis. The school start cutoffs for cohorts born in 1955 to 1956 are as follows: September 30 for Bavaria, December 31 for Baden-Württemberg, March 31 for Lower Saxony, Rhineland-Palatinate, Nord-Rhine Westphalia, Schleswig-Holstein, Saarland, Bremen, and West Berlin. Hesse and Hamburg change their school cutoff from March 31 to December 31, starting from the first 1955 Q2 birth cohort. The statutory graduation date can vary between federal states despite having the same school start cutoff because of differences in overall school duration (details are provided in Appendix B2 on the short school years).
*Source*: Koebe & Marcus (2022), own representation.

Table 3: The effect of the age of majority reform on women's education and marital status

| | Highest postsecondary degree | | | Marital status | | |
|---|---|---|---|---|---|---|
| | any degree | university | vocational | single | married | divorced |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated | 0.032** | 0.037** | -0.005 | 0.052* | -0.058** | 0.007 |
| | (0.012) | (0.012) | (0.019) | (0.022) | (0.019) | (0.024) |
| Sample mean | 0.946 | 0.742 | 0.204 | 0.151 | 0.665 | 0.138 |
| Regular $p$-value | 0.029 | 0.021 | 0.801 | 0.051 | 0.019 | 0.768 |
| Bootstrapped $p$ | 0.125 | 0.094 | 0.828 | 0.055 | 0.023 | 0.797 |
| R-squared | 0.009 | 0.012 | 0.011 | 0.012 | 0.010 | 0.009 |
| Observations | 8185 | 8185 | 8185 | 8185 | 8185 | 8185 |

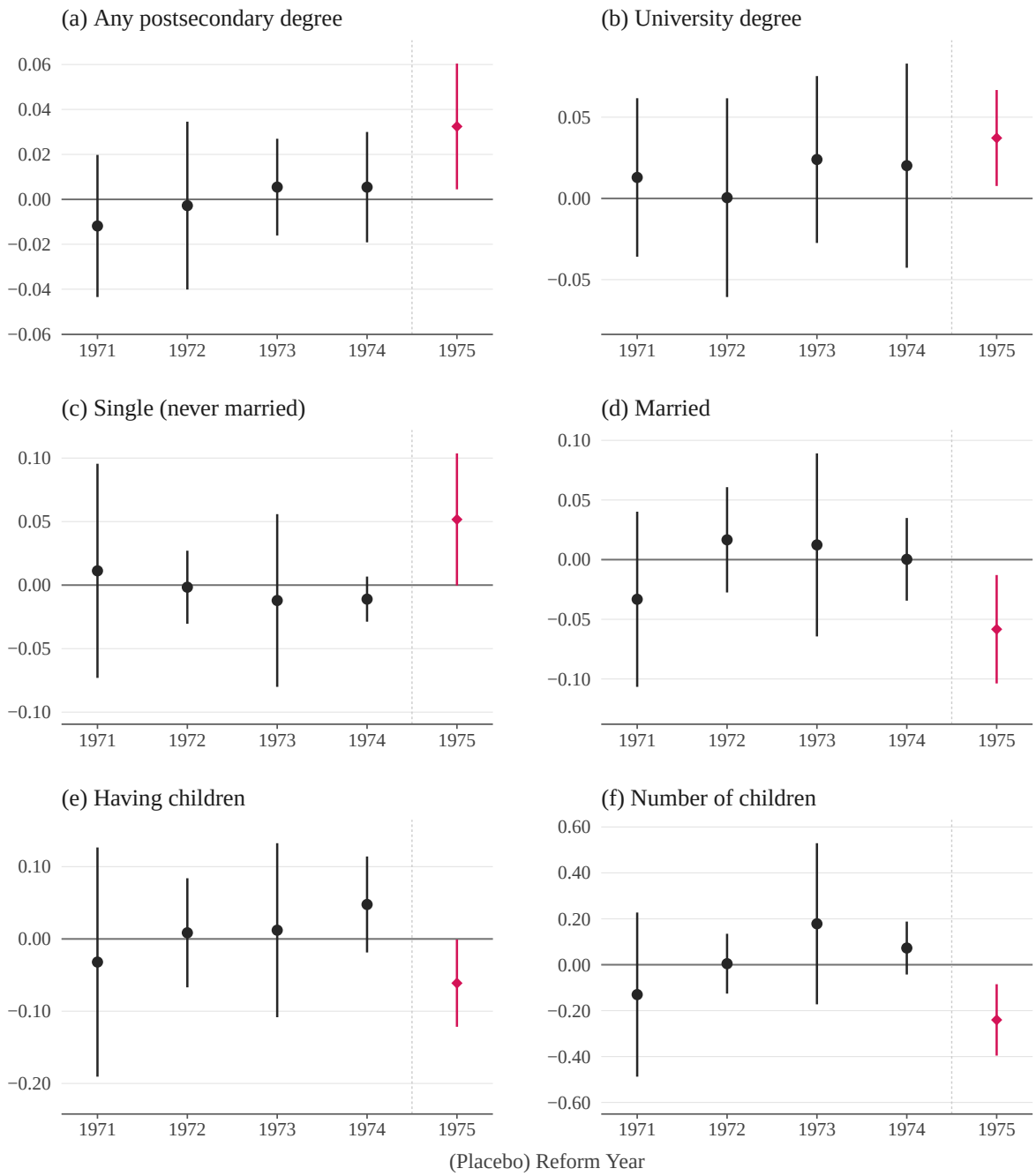
*Notes*: Treatment effects are from difference-in-differences models estimated by linear regression. "Treated" is a dummy variable for turning legally adult before the statutory date of graduating from school, which is calculated on the basis of birth month and federal state. The dependent variable is indicated in the top row and measured at age 49 or higher. "Any degree" is a dummy variable for having either a university or a vocational degree. The marital status "single" means never having been married. Included are women born between July 1955 and June 1956 holding the general university entrance qualification. All regressions include fixed effects for month of birth, federal state, and survey year. Standard errors are clustered at the federal state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: German Microcensus 2005–2021, own calculations, weighted.

Table 4: The effect of the age of majority reform on women's fertility

| | Having children | | | | | |
|---|---|---|---|---|---|---|
| | at least 1 child | | more than 1 child | more than 2 children | more than 3 children | Number of children |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated | -0.061** | -0.067** | -0.092* | -0.052 | -0.021 | -0.240*** |
| | (0.026) | (0.023) | (0.040) | (0.030) | (0.018) | (0.066) |
| Sample mean | 0.712 | 0.702 | 0.496 | 0.160 | 0.046 | 1.414 |
| Regular $p$-value | 0.048 | 0.023 | 0.053 | 0.125 | 0.289 | 0.008 |
| Bootstrapped $p$ | 0.031 | 0.023 | 0.172 | 0.238 | 0.461 | 0.016 |
| R-squared | 0.019 | 0.023 | 0.024 | 0.015 | 0.015 | 0.024 |
| Observations | 1771 | 1715 | 1715 | 1715 | 1715 | 1715 |

*Notes*: Treatment effects are from difference-in-differences models estimated by linear regression. "Treated" is a dummy variable for turning legally adult before the statutory date of graduating from school, which is calculated on the basis of birth month and federal state. The dependent variable is indicated in the top row and measured at age 49 or higher. The dependent variable in Column 1 is whether women have given birth to children. For comparability with subsequent specifications, the sample in Column 2 is restricted to women who also reported the number of children they have given birth to. Included are women born between July 1955 and June 1956 holding the general university entrance qualification. In Column 2, sample size is smaller because of nonreport on the number of children. All regressions include fixed effects for month of birth, federal state, and survey year. Standard errors are clustered at the federal state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* German Microcensus 2008, 2012, 2016, and 2018; own calculations, weighted.

Figure 1: Placebo analyses




*Notes:* The figure shows coefficients from difference-in-differences models estimated by linear regression, where each coefficient shows the difference in outcomes between graduating in a given year versus in the previous year. The coefficient for reform year 1975 is the treatment effect; coefficients for previous years are placebo effects. Placebo treatment variables are constructed as if the reform had taken place in a previous year, taking the respective time-varying and federal-state-specific school start cutoffs into account. The dependent variable is indicated at the top of each subfigure. All regressions include fixed effects for month of birth, federal state, and survey year. Confidence intervals are at the 95-percent level, based on the $t$-distribution with standard errors clustered at the federal state level. *Source:* German Microcensus 2005–2021 for education and family status; German Microcensus 2008, 2012, 2016, and 2018 for fertility; own calculations, weighted.

Table 5: The effect of the age of majority reform on women's partner characteristics

| | Partner | Partner with postsecondary degree | | |
|---|---|---|---|---|
| | | any degree | university | vocational |
| | (1) | (2) | (3) | (4) |
| Treated | -0.047** | -0.073*** | -0.005 | -0.068*** |
| | (0.018) | (0.018) | (0.022) | (0.015) |
| Sample mean | 0.685 | 0.619 | 0.439 | 0.181 |
| Regular $p$-value | 0.034 | 0.004 | 0.822 | 0.003 |
| Bootstrapped $p$ | 0.023 | 0.039 | 0.832 | 0.039 |
| R-squared | 0.012 | 0.009 | 0.012 | 0.013 |
| Observations | 8185 | 8185 | 8185 | 8185 |

*Notes*: Treatment effects are from difference-in-differences models estimated by linear regression. The dependent variable is indicated in the top row. In the data, having a partner is defined as living together with a spouse in the same household. "Treated" is a dummy variable for turning legally adult before the statutory date of graduating from school, which is calculated on the basis of birth month and federal state. Included are women born between July 1955 and June 1956 holding the general university entrance qualification. All regressions include fixed effects for month of birth, federal state, and survey year. Standard errors are clustered at the federal state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* German Microcensus 2005–2021, own calculations, weighted.

# Appendix A: Additional Tables and Figures

Table A1: Sample construction

| | | | $N$ |
|---|---|---|---|
| Women born in window of analysis | | | 87,013 |
| **Observations dropped** | | | |
| Nongerman citizens, migrants | -13,101 | -15.1% | 73,912 |
| Residence in federal states of East Germany (incl. Berlin) | -18,316 | -24.8% | 55,596 |
| Residence in remaining city states | -1,579 | -2.8% | 54,017 |
| GDR-specific schooling degrees | -952 | -1.8% | 53,065 |
| No general university entrance qualification (no *Abitur*) | -44,303 | -83.5% | 8,762 |
| Obtained *Abitur* through second-chance education | -577 | -6.6% | 8,185 |
| Main sample | | | 8,185 |

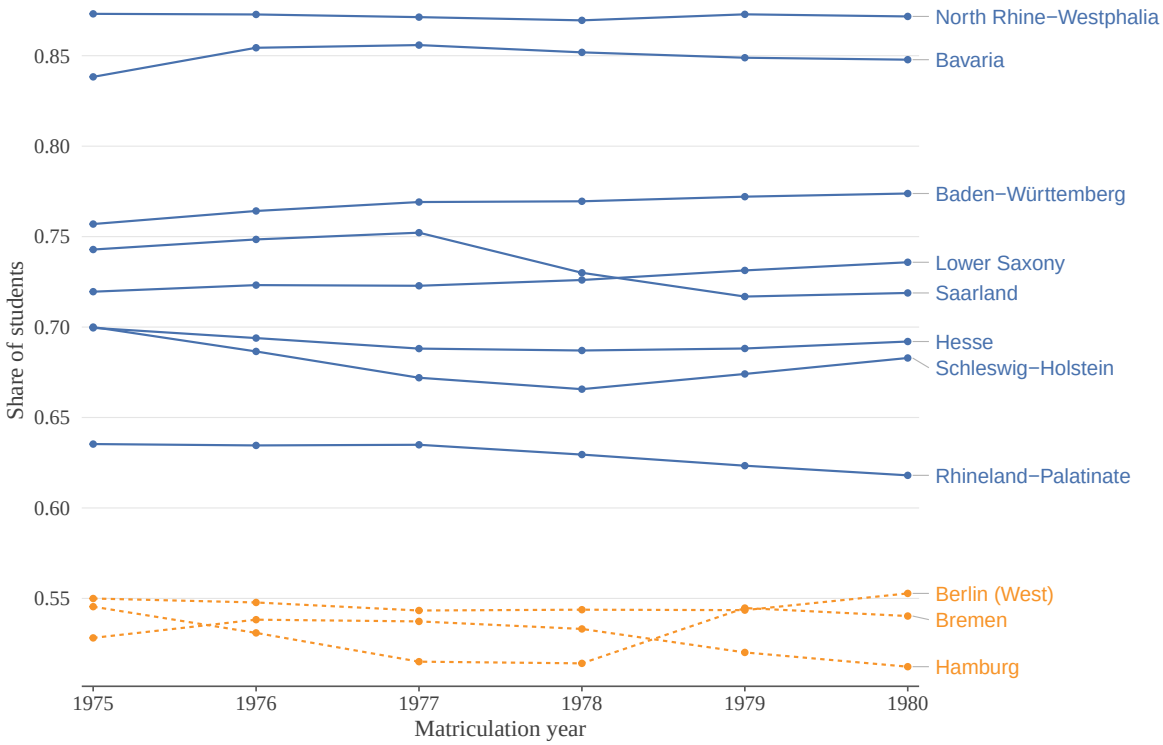
*Source*: German Microcensus 2005–2021. Own calculations, not weighted.

Table A2: The effect of the age of majority reform on obtaining the general university entrance qualification (*Abitur*)

| | Placebo samples | | | | Main sample |
|---|---|---|---|---|---|
| (Placebo) reform year: | 1971 | 1972 | 1973 | 1974 | 1975 |
| | (1) | (2) | (3) | (4) | (5) |
| Treated | -0.000 | 0.011 | 0.008 | -0.000 | -0.002 |
| | (0.007) | (0.010) | (0.008) | (0.008) | (0.014) |
| Sample mean | 0.123 | 0.134 | 0.143 | 0.161 | 0.165 |
| Regular $p$-value | 0.979 | 0.317 | 0.355 | 0.998 | 0.868 |
| Bootstrapped $p$ | 0.984 | 0.566 | 0.438 | 0.996 | 0.871 |
| R-squared | 0.004 | 0.004 | 0.006 | 0.004 | 0.005 |
| Observations | 47,689 | 61,024 | 49,082 | 50,196 | 53,116 |

*Notes*: Treatment effects are from difference-in-differences models estimated by linear regression. "Treated" is a dummy variable for turning legally adult before the statutory date of graduating from school, which is calculated on the basis of birth month and federal state. In Column 5, women born between July 1955 and June 1956 are included; for the placebo samples, women born in the respective windows of analysis are included. Women with *Abitur* or lower schooling qualifications are included, women with GDR-specific schooling degrees are excluded. All regressions include fixed effects for month of birth, federal state, and survey year. Standard errors are clustered at the federal state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: German Microcensus 2005–2021, own calculations, weighted.

Figure A1: Share of students studying in their home federal state




*Notes:* This graph shows the share of students who have their permanent address registered in the same federal state where they attend university, out of all students matriculated in each year.
*Source:* Statistisches Bundesamt (1975–1980), own calculations.

Table A3: Robustness of main results to reducing observation period (data until 2019)

| | Highest postsecondary degree | | | Marital status | | |
|---|---|---|---|---|---|---|
| | any degree | university | vocational | single | married | divorced |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated | 0.043** | 0.043*** | 0.000 | 0.072*** | -0.086*** | 0.006 |
| | (0.014) | (0.010) | (0.020) | (0.018) | (0.018) | (0.026) |
| Sample mean | 0.946 | 0.741 | 0.204 | 0.153 | 0.667 | 0.140 |
| Regular $p$-value | 0.016 | 0.004 | 0.999 | 0.005 | 0.002 | 0.819 |
| Bootstrapped $p$ | 0.102 | 0.086 | 0.992 | 0.016 | 0.016 | 0.836 |
| R-squared | 0.012 | 0.012 | 0.010 | 0.014 | 0.012 | 0.009 |
| Observations | 7072 | 7072 | 7072 | 7072 | 7072 | 7072 |

*Notes*: Treatment effects are from difference-in-differences models estimated by linear regression. "Treated" is a dummy variable for turning legally adult before the statutory date of graduating from school, which is calculated on the basis of birth month and federal state. The dependent variable is indicated in the top row and measured at age 49 or higher. "Any degree" is a dummy variable for having either a university or a vocational degree. The marital status "single" means never having been married. Included are women born between July 1955 and June 1956 holding the general university entrance qualification. All regressions include fixed effects for month of birth, federal state, and survey year. Standard errors are clustered at the federal state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: German Microcensus 2005–2019, own calculations, weighted.

Table A4: Placebo effects for women without university entrance qualification

| | Highest postsecondary degree | | | Marital status | | | Fertility | |
|---|---|---|---|---|---|---|---|---|
| | any | university | vocational | single | married | divorced | having children | number of children |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treated | -0.014 | -0.001 | -0.014 | 0.000 | -0.011 | 0.008 | 0.008 | 0.049 |
| | (0.014) | (0.001) | (0.015) | (0.005) | (0.012) | (0.005) | (0.014) | (0.049) |
| Sample mean | 0.829 | 0.002 | 0.825 | 0.067 | 0.726 | 0.133 | 0.83 | 1.629 |
| Regular $p$-value | 0.367 | 0.227 | 0.395 | 0.991 | 0.397 | 0.155 | 0.573 | 0.346 |
| Bootstrapped $p$ | 0.602 | 0.391 | 0.648 | 0.977 | 0.328 | 0.141 | 0.625 | 0.465 |
| R-squared | 0.002 | 0.002 | 0.002 | 0.001 | 0.004 | 0.001 | 0.002 | 0.002 |
| Observations | 163,483 | 163,483 | 163,483 | 163,483 | 163,483 | 163,483 | 36,148 | 35,274 |

*Notes*: Treatment effects are from difference-in-differences models estimated by linear regression. This sample contains only women born between July 1955 and June 1956 who do *not* hold the general university entrance qualification, but visited lower school tracks (*Haupt-, Realschule, Mittlere Reife*). The coefficient on "treated" gives the placebo treatment effect, capturing the assignment to different grade cohorts, none of whom graduate as legal adults. The dependent variable is indicated in the top row and measured at age 49 or higher. "Any degree" is a dummy variable for having either a university or a vocational degree. Students of lower school tracks could not attend university in general. All regressions include fixed effects for month of birth, federal state, and survey year. Standard errors are clustered at the federal state level and reported in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. *Source*: German Microcensus 2005–2021, own calculations, weighted.

Table A5: The effect of the age of majority reform on men's education and family formation outcomes

| DV: | Highest postsecondary degree | | | Marital status | | |
|---|---|---|---|---|---|---|
| | any | vocational | university | single | married | divorced |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated | -0.019* | -0.031 | 0.010 | 0.002 | -0.012 | 0.010 |
| | (0.009) | (0.026) | (0.023) | (0.010) | (0.022) | (0.016) |
| Sample mean | 0.948 | 0.157 | 0.790 | 0.136 | 0.759 | 0.092 |
| Regular $p$-value | 0.081 | 0.277 | 0.676 | 0.846 | 0.608 | 0.541 |
| Bootstrapped $p$ | 0.133 | 0.406 | 0.828 | 0.813 | 0.602 | 0.539 |
| R-squared | 0.009 | 0.011 | 0.014 | 0.010 | 0.008 | 0.007 |
| Observations | 10,202 | 10,202 | 10,202 | 10,202 | 10,202 | 10,202 |

*Notes*: Treatment effects are from difference-in-differences models estimated by linear regression. "Treated" is a dummy variable for turning legally adult before the statutory date of graduating from school, which is calculated on the basis of birth month and federal state. The dependent variable is indicated in the top row. "Any degree" is a dummy variable for having either a university or a vocational degree. Included are men born between July 1955 and June 1956 holding the general university entrance qualification. All regressions include fixed effects for month of birth, federal state, and survey year. Standard errors are clustered at the federal state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: German Microcensus 2005–2021, own calculations, weighted.

Table A6: Testing for selective nonreport on fertility

| DV: | Missing value | | |
|---|---|---|---|
| | having children | number of children | |
| | (1) | (2) | (3) |
| Treated | -0.007 | -0.004 | 0.011 |
| | (0.014) | (0.025) | (0.025) |
| Sample mean | 0.067 | 0.099 | 0.048 |
| Regular $p$-value | 0.624 | 0.870 | 0.660 |
| Bootstrapped $p$ | 0.625 | 0.867 | 0.672 |
| R-squared | 0.040 | 0.045 | 0.046 |
| Observations | 1897 | 1897 | 1252 |
| Conditional on having children | | | ✓ |

*Notes*: Treatment effects are from difference-in-differences models estimated by linear regression. Included are women born between July 1955 and June 1956 holding the general university entrance qualification. "Treated" is a dummy variable for turning legally adult before the statutory date of graduating from school, which is calculated on the basis of birth month and federal state. All regressions include fixed effects for month of birth, federal state, and survey year. Standard errors are clustered at the federal state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* German Microcensus 2008, 2012, 2016, and 2018, own calculations, weighted.

Table A7: Statutory graduation date, by federal state and quarter of birth

*Panel A: Placebo reform year 1971*

| | | | window of analysis | | | | | |
|---|---|---|---|---|---|---|---|---|
| Quarter of birth | (1951 Q1) | (1951 Q2) | 1951 Q3 | 1951 Q4 | 1952 Q1 | 1952 Q2 | 1952 Q3 | (1952 Q4) |
| Bavaria | July 1970 | July 1970 | July 1970 | **July 1971** | **July 1971** | **July 1971** | **July 1971** | **July 1972** |
| Baden-Württemberg | July 1969 | July 1970 | July 1970 | July 1970 | **July 1971** | **July 1971** | **July 1971** | **July 1971** |
| Hesse | July 1969 | July 1970 | July 1970 | July 1970 | July 1970 | **July 1971** | **July 1971** | **July 1971** |
| Lower Saxony | July 1969 | July 1970 | July 1970 | July 1970 | July 1970 | **July 1971** | **July 1971** | **July 1971** |
| Rhineland-Palatinate | July 1969 | July 1970 | July 1970 | July 1970 | July 1970 | **July 1971** | **July 1971** | **July 1971** |
| Schleswig-Holstein | July 1969 | July 1970 | July 1970 | July 1970 | July 1970 | **July 1971** | **July 1971** | **July 1971** |
| North Rhine-Westphalia | July 1969 | July 1969 | July 1970 | July 1970 | July 1970 | July 1970 | **July 1971** | **July 1971** |
| (Saarland) | July 1969 | July 1969 | July 1969 | July 1970 | July 1970 | July 1970 | July 1970 | July 1970 |
| (Bremen) | July 1969 | July 1970 | July 1970 | July 1970 | July 1970 | **July 1971** | **July 1971** | **July 1971** |
| (Hamburg) | Mar 1970 | **Mar 1971** | **Mar 1971** | **Mar 1971** | **Mar 1971** | **Mar 1972** | **Mar 1972** | **Mar 1972** |
| (West Berlin) | Mar 1970 | **Mar 1971** | **Mar 1971** | **Mar 1971** | **Mar 1971** | **Mar 1972** | **Mar 1972** | **Mar 1972** |

*Panel B: Placebo reform year 1972*

| | | | window of analysis | | | | | |
|---|---|---|---|---|---|---|---|---|
| Quarter of birth | (1952 Q1) | (1952 Q2) | 1952 Q3 | 1952 Q4 | 1953 Q1 | 1953 Q2 | 1953 Q3 | (1953 Q4) |
| Bavaria | July 1971 | July 1971 | July 1971 | **July 1972** | **July 1972** | **July 1972** | **July 1972** | **July 1973** |
| Baden-Württemberg | July 1971 | July 1971 | July 1971 | July 1971 | **July 1972** | **July 1972** | **July 1972** | **July 1972** |
| Hesse | July 1970 | July 1971 | July 1971 | July 1971 | July 1971 | **July 1972** | **July 1972** | **July 1972** |
| Lower Saxony | July 1970 | July 1971 | July 1971 | July 1971 | July 1971 | **July 1972** | **July 1972** | **July 1972** |
| Rhineland-Palatinate | July 1970 | July 1971 | July 1971 | July 1971 | July 1971 | **July 1972** | **July 1972** | **July 1972** |
| Schleswig-Holstein | July 1970 | July 1971 | July 1971 | July 1971 | July 1971 | **July 1972** | **July 1972** | **July 1972** |
| North Rhine-Westphalia | July 1970 | July 1970 | July 1971 | July 1971 | July 1971 | July 1971 | **July 1972** | **July 1972** |
| Saarland | July 1970 | July 1970 | July 1970 | July 1970 | July 1971 | **July 1972** | **July 1972** | **July 1972** |
| (Bremen) | July 1970 | July 1971 | July 1971 | July 1971 | July 1971 | **July 1972** | **July 1972** | **July 1972** |
| (Hamburg) | Mar 1971 | **Mar 1972** | **Mar 1972** | **Mar 1972** | **Mar 1972** | **Mar 1973** | **Mar 1973** | **Mar 1973** |
| (West Berlin) | Mar 1971 | **Mar 1972** | **Mar 1972** | **Mar 1972** | **Mar 1972** | **Mar 1973** | **Mar 1973** | **Mar 1973** |

*Notes*: This table shows the statutory graduation date, based on birth quarter and federal state, for obtaining the general university entrance qualification (*Abitur*). Everyone born 1955–1956 turned legal adult on 1 January 1975 by the age of majority reform. Cohorts marked in bold are assigned to treatment, i.e., turn legal adults before their statutory graduation date had the reform taken place in the respective placebo reform year. In parentheses are the federal states and birth quarters which are excluded in the analysis. The school start cutoffs for cohorts born in 1951 to 1953 are as follows: 30 June for Nord-Rhine Westphalia, September 30 for Bavaria, March 31 for Lower Saxony, Rhineland-Palatinate, Schleswig-Holstein, Hesse, Hamburg, Bremen, and West Berlin. Baden-Württemberg changed its cutoff from 15 April to 31 December (affecting cohorts born 1952); Saarland changed its cutoff from 30 September to 31 December and again to 31 March for the observed cohorts. The statutory graduation date can vary between federal states despite having the same school start cutoff because of differences in overall school duration (details are provided in Appendix B2 on the short school years). *Source*: Koebe & Marcus (2022), own representation.

Table A8: Statutory graduation date, by federal state and quarter of birth

*Panel A: Placebo reform year 1973*

| | | | window of analysis | | | | | |
|---|---|---|---|---|---|---|---|---|
| Quarter of birth | (1953 Q1) | (1953 Q2) | 1953 Q3 | 1953 Q4 | 1954 Q1 | 1954 Q2 | 1954 Q3 | (1954 Q4) |
| Bavaria | July 1972 | July 1972 | July 1972 | **July 1973** | **July 1973** | **July 1973** | **July 1973** | **July 1974** |
| Baden-Württemberg | July 1972 | July 1972 | July 1972 | July 1972 | **July 1973** | **July 1973** | **July 1973** | **July 1973** |
| Hesse | July 1972 | July 1972 | July 1972 | July 1972 | July 1972 | **July 1973** | **July 1973** | **July 1973** |
| Lower Saxony | July 1972 | July 1972 | July 1972 | July 1972 | July 1972 | **July 1973** | **July 1973** | **July 1973** |
| Rhineland-Palatinate | July 1972 | July 1972 | July 1972 | July 1972 | July 1972 | **July 1973** | **July 1973** | **July 1973** |
| Schleswig-Holstein | July 1972 | July 1972 | July 1972 | July 1972 | July 1972 | **July 1973** | **July 1973** | **July 1973** |
| Saarland | July 1972 | July 1972 | July 1972 | July 1972 | July 1972 | **July 1973** | **July 1973** | **July 1973** |
| North Rhine-Westphalia | July 1971 | July 1971 | July 1972 | July 1972 | July 1972 | July 1972 | **July 1973** | **July 1973** |
| (Bremen) | July 1971 | July 1972 | July 1972 | July 1972 | July 1972 | **July 1973** | **July 1973** | **July 1973** |
| (Hamburg) | Mar 1972 | **Mar 1973** | **Mar 1973** | **Mar 1973** | **Mar 1973** | **Mar 1974** | **Mar 1974** | **Mar 1974** |
| (West Berlin) | Mar 1972 | **Mar 1973** | **Mar 1973** | **Mar 1973** | **Mar 1973** | **Mar 1974** | **Mar 1974** | **Mar 1974** |

*Panel B: Placebo reform year 1974*

| | | | window of analysis | | | | | |
|---|---|---|---|---|---|---|---|---|
| Quarter of birth | (1954 Q1) | (1954 Q2) | 1954 Q3 | 1954 Q4 | 1955 Q1 | 1955 Q2 | (1955 Q3) | (1955 Q4) |
| Bavaria | July 1973 | July 1973 | July 1973 | **July 1974** | **July 1974** | **July 1974** | **July 1974** | **July 1975** |
| Baden-Württemberg | July 1973 | July 1973 | July 1973 | July 1973 | **July 1974** | **July 1974** | **July 1974** | **July 1974** |
| North Rhine-Westphalia | July 1972 | July 1972 | July 1973 | July 1973 | July 1973 | **July 1974** | **July 1974** | **July 1974** |
| Lower Saxony | July 1972 | July 1973 | July 1973 | July 1973 | July 1973 | **July 1974** | **July 1974** | **July 1974** |
| Rhineland-Palatinate | July 1972 | July 1973 | July 1973 | July 1973 | July 1973 | **July 1974** | **July 1974** | **July 1974** |
| Hesse | July 1972 | July 1973 | July 1973 | July 1973 | July 1973 | **July 1974** | **July 1974** | **July 1974** |
| Schleswig-Holstein | July 1972 | July 1973 | July 1973 | July 1973 | July 1973 | **July 1974** | **July 1974** | **July 1974** |
| Saarland | July 1972 | July 1973 | July 1973 | July 1973 | July 1973 | **July 1974** | **July 1974** | **July 1974** |
| (Hamburg) | Mar 1973 | Mar 1974 | Mar 1974 | Mar 1974 | Mar 1974 | **Mar 1975** | **Mar 1975** | **Mar 1975** |
| (Bremen) | July 1972 | July 1973 | July 1973 | July 1973 | July 1973 | **July 1974** | **July 1974** | **July 1974** |
| (Berlin West) | Mar 1973 | **Mar 1974** | **Mar 1974** | **Mar 1974** | **Mar 1974** | **Mar 1975** | **Mar 1975** | **Mar 1975** |

*Notes*: This table shows the statutory graduation date, based on birth quarter and federal state, for obtaining the general university entrance qualification (*Abitur*). Everyone born 1955–1956 turned legal adult on 1 January 1975 by the age of majority reform. Cohorts marked in bold are assigned to treatment, i.e., turn legal adults before their statutory graduation date had the reform taken place in the respective placebo reform year. In parentheses are the federal states and birth quarters which are excluded in the analysis. The school start cutoffs for cohorts born in 1953 to 1955 are as follows: September 30 for Bavaria, March 31 for Lower Saxony, Rhineland-Palatinate, Schleswig-Holstein, Saarland, Hesse, Hamburg, Bremen, and West Berlin, and 31 December for Baden-Württemberg. Nord-Rhine Westphalia changed its cutoff from 30 June to 31 March. The statutory graduation date can vary between federal states despite having the same school start cutoff because of differences in overall school duration (details are provided in Appendix B2 on the short school years). *Source*: Koebe & Marcus (2022), own representation.

# Appendix B. Discussion of the Exclusion Restriction

Our identification strategy requires that no other changes occurring at the 1975 reform threshold affected educational or family outcomes differentially for the treatment and control cohorts. This appendix discusses the most plausible alternative explanations and explains why they are unlikely to account for our findings.

## B1. Macroeconomic Conditions

Macroeconomic conditions at the time of leaving school may influence educational decisions and subsequent labor market outcomes. Graduating during a recession has been shown to reduce entry wages (Oreopoulos et al., 2012), and recent evidence for Germany suggests that recessions may also lower university enrollment (Leibing, 2026). However, the cohorts obtaining the university entrance qualification in 1974 and 1975 experienced almost identical macroeconomic conditions. Following the first oil crisis, the West German recession lasted from 1974 until the second quarter of 1975 (Heilemann, 2019), with GDP growth declining from 4.8 percent in 1973 to 0.9 percent in 1974 and –0.9 percent in 1975. Consequently, both treatment and control cohorts made their educational decisions under very similar economic conditions. If anything, the recession would be expected to affect the 1974 placebo cohort rather than the reform cohort. Consistent with this interpretation, the placebo analyses reported in Figure 1 show no significant effects for the placebo reform years.

## B2. Other Educational Reforms

Two broader developments during this period could potentially confound our estimates: the expansion of higher education and the introduction of short school years. Neither is likely to violate the exclusion restriction.

### Expansion of higher education

West Germany expanded higher education substantially during the late 1960s and early 1970s: between 1964 and 1978 the number of universities almost doubled from 27 to 53, accompanied by the establishment of universities of applied sciences and the reclassification of several institutions (Boelmann, 2024; Bartz, 2007). This expansion, however, predates the reform. University openings were steepest between 1965 and 1972 and essentially flat between 1974 and 1977, and enrollment rose steadily in the years after 1972 (Boelmann, 2024; BMFTR, 2025). Only two universities opened in 1975, and both were small: the first cohort at the newly founded University of Bayreuth comprised roughly 600 students (Universität Bayreuth, 2015). Thus, educational opportunities did not change discontinuously for the 1975 graduation cohort.

**Short School Years**

Most West German federal states introduced short school years during the 1960s to shift the beginning of the school year from spring to after the summer holidays (Pischke, 2007; Koebe & Marcus, 2022). Within federal states, all birth cohorts in our sample were exposed to this reform. Treatment and control cohorts therefore differ only in the school grade during which they experienced a short school year, rather than in exposure itself. Moreover, previous research finds no long-run effects on women's labor market outcomes (Cygan-Rehm, 2025) or family formation (Koebe & Marcus, 2022), while educational effects appear limited and concentrated among lower-achieving students (Pischke, 2007). It is therefore unlikely that the short school year reform explains the discontinuity observed at the age-of-majority threshold.

Taken together, neither macroeconomic conditions nor contemporaneous educational reforms changed discontinuously at the 1975 reform threshold. We therefore conclude that these developments are unlikely to provide an alternative explanation for our estimated effects.