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School Starting Age and the Gender Pay Gap over the Life Cycle

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Abstract

This paper replicates and extends the evidence on the lifetime effects of school starting age on earnings by Fredriksson and Öckert (2014) for Sweden. Using German data for individuals born between 1945 and 1965, we examine a more rigid system of ability tracking in secondary education, a potential driver of long-term effects. We confirm negligible effects of later school entry for men and positive effects for women. These gender differences arise despite similar effects on educational attainment. By unfolding the gender gaps over the lifecycle, assessing fertility decisions, and maternal employment around the first birth, we show that childbirth postponement and increased labor market attachment after the first birth seem to be plausible mechanisms.

JEL classification

I21, I24, I26

Keywords

school starting age, lifetime effects, education, gender gaps

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1 Introduction

School entry cutoffs determine the timing of school enrollment. An extensive literature discusses whether these cutoffs have long-run effects on student achievement and labor market outcomes. There seems to be a consensus that relatively older students have an initial advantage in learning outcomes (see, e.g., Bedard, 2006; Schneeweis and Zweimüller, 2014; Cook and Kang, 2020). These effects tend to diminish over time (e.g., Mühlenweg and Puhani, 2010; Dhuey et al., 2019), yet they do not necessarily entirely disappear, particularly in education systems that track students into different school types based on their academic performance.[1] This suggests the potential for long-run effects on labor market outcomes. However, the existing research on earnings typically finds small to undetectable effects (e.g., Black et al., 2011; Dustmann et al., 2017), although the effects may vary substantially over the life course (e.g., Fredriksson and Öckert, 2014; Larsen and Solli, 2017; Oosterbeek et al., 2021).

This paper re-evaluates and extends the evidence on the life-cycle effects of starting age (SSA) on labor market performance, first documented by Fredriksson and Öckert (2014) (hereafter FÖ). Specifically, applying a regression discontinuity (RD) approach to Swedish data for individuals born between 1935 and 1955, FÖ have shown that later school entry increases educational attainment and affects the allocation of labor supply over the career, particularly at its beginning and toward its end. However, despite the educational benefits, they find no overall net increase in lifetime earnings. In contrast, FÖ show that relatively older school starters may even experience some lifetime losses due to a relatively later entry into the labor market. Interestingly, the lifetime losses are driven by men, as FÖ find net gains from a later school start for women, which is intriguing and suggests implications for persistent gender gaps in the labor market (e.g., Blau and Kahn, 2017).[2]

[1] For a review of the SSA literature, see Dhuey and Koebel (2022). In countries without ability tracking, there are typically no effects on educational attainment (e.g., Black et al., 2011; Dobkin and Ferreira, 2010). The role of the tracking systems in educational responses to a later school start is clearly demonstrated in Fredriksson and Öckert (2014), who document that the effects diminish after ability tracking is postponed.

[2] Closely related studies from ability tracking systems such as Germany (Dustmann et al., 2017) and the Netherlands (Oosterbeek et al., 2021) generally confirm initial losses in age-specific earnings and no long-run effects. However, none of these studies assesses or explains the different SSA effects by gender.

We first replicate the main findings of FÖ by applying a similar RD design to German social security records containing labor market biographies for the 1945–1965 birth cohorts. We confirm the large negative earnings effects of a relatively later school start at the beginning of the career and no significant effects, on average, during the prime working years. Moreover, we successfully replicate the gender-specific patterns found by FÖ, confirming virtually no effect on men's lifetime earnings and a net benefit from a later school start for women. We show that these gender differences arise despite similar effects on schooling – a remarkable finding given the arguably more rigid tracking system in Germany than in Sweden at the time (e.g., earlier tracking and limited opportunities to change tracks).

We then extend the analysis beyond the results in FÖ by documenting the implications of SSA for the compression of the gender earnings gap over the life cycle. Finally, we examine the potential mechanisms behind the gender-specific responses, such as differential effects on postsecondary education, job and firm characteristics, fertility decisions, and labor market outcomes around the first birth. Our estimates suggest that delayed first childbirth and increased labor market attachment after birth, which occurs at critical career stages with relatively high returns to experience (e.g., Bhuller et al., 2017) appear to be plausible mechanisms behind the positive effects on women's lifetime earnings. These findings are consistent with recent literature emphasizing the role of children as an important source of gender inequality in the labor market (for a recent review, see Olivetti et al., 2024).

The paper is structured as follows: Section 2 presents the institutional details, Section 3 our data, and Section 4 the empirical strategy. We discuss our main results in Section 5 and conclude in Section 6.

2 Institutional Background

In Germany, the birth cohort and the specific cutoff date typically determine the year of school entry. Individuals born before the cutoff start school in the year they turn six, and individuals born after the cutoff start school in the year they turn seven. The cutoff dates may vary from state to state, as each state is responsible for its education policy. In the 1950s and 1960s, the period under study, the most common cutoff dates were March 31, June 30, and December 31, although all states moved the cutoff date at least once (see

Appendix Figure A.1 for details).[3] The cutoff dates are not necessarily binding, as some states explicitly allow earlier enrollment.[4] Nevertheless, most parents follow the default rules, showing that, on average, more than 70 percent of children enter school in the year scheduled by the cutoff.[5]

After four years of elementary school, usually at age 10, students are placed in one of three secondary school tracks based on their academic ability.[6] The basic track (*Hauptschule*) lasts an additional four to five years (depending on the applicable compulsory schooling law)[7], while the intermediate track (*Realschule*) lasts an additional six years. Both tracks prepare students for apprenticeships in blue-collar or white-collar occupations (Dustmann et al., 2017). In contrast, the academic track (*Gymnasium*) lasts an additional nine years and prepares students for university education. Importantly, as in Sweden, the length of compulsory schooling in Germany is grade-based and not age-based (e.g., in the US or UK), i.e., it is independent of an individual's school entry age.

3 Data

For the main analysis, we use individual register records from the Sample of Integrated Labour Market Biographies (vom Berge et al., 2021). The SIAB is a 2 percent sample of the population covered by the German social security system at least once between 1975 and 2019 due to employment, unemployment, or welfare assistance.[8] Apart from the large sample size, the main advantage of the data is that the information on employment biographies, earnings, and birthdates (year and month) is very accurate. We focus on German citizens from the former West German states (excluding Berlin) born between 1945 and 1965 to ensure long earnings histories. We measure their labor market outcomes

[3]Including indicators for the cutoff month does not affect our results (see Appendix Table A.3).

[4]Typically, children born in the first three months after the cutoff are eligible for early enrollment upon application (Kamb and Tamm, 2022; Görlitz et al., 2025). Other exceptions, e.g., based on lack of intellectual or emotional maturity, are scarce as they require complex administrative procedures and extensive paperwork.

[5]We provide detailed evidence on the compliance with the cutoffs in Appendix B.3.

[6]The tracking recommendation is based on the student's academic ability as perceived by the elementary school teacher. Most parents follow this recommendation (Fröhlich, 1974).

[7]During the period under study, several states extended compulsory schooling requirements from eight to nine years (Pischke and von Wachter, 2008; Cygan–Rehm, 2022). Some states also shifted the start of the school year from spring to fall, resulting in two shorter school years (Pischke, 2007; Cygan-Rehm, 2026). Controlling for these policy changes does not change our results (see Appendix Table A.3).

[8]The data cover about 80%w of Germany's workforce, excluding civil servants and the self-employed.

from age 15 to 64, which covers the potential working life span in Germany. The original earnings measure is stored as gross daily earnings in EUR, which we deflate to 2015 prices using the consumer price index (OECD, 2017). Payroll earnings data are generally highly reliable.[9] We transform the daily data into an annual panel including age-specific sum of earnings for each individual. Following FÖ, we normalize annual earnings by the cohort-specific average prime-age earnings (i.e., the sum at ages 30–54). Lifetime earnings correspond to the sum of earnings at ages 15–64.[10] Similarly, we calculate the total number of days worked at these ages to measure lifetime employment.

Information on educational attainment is limited and focuses mainly on post-secondary qualifications (such as high school graduation, university/college degree, and vocational training) from which we generate years of schooling. Like the Swedish data, the German social security records do not report an individual's school starting age (SSA). Therefore, as in FÖ, we rely on auxiliary survey data to provide complementary evidence on the discontinuity in SSA at the cutoff. Specifically, we use data from the National Educational Panel Study (NEPS; see Appendix B for details). We link both data sets to the relevant cutoffs collected by Cygan-Rehm (2026) by using an individual's year and month of birth, as well as a proxy for state of schooling. This may introduce some measurement error.[11]

Our SIAB estimation sample for the main analysis on labor market effects includes 306,145 individuals, of whom 49 percent are female (see Appendix Table A.1). Between the ages of 15 and 64, the average total earnings reach almost 910 thousand EUR (in 2015 prices), but there is a large gender gap: men accumulate more than twice as much labor income as women (1.2 million versus 580 thousand EUR). The gender gap in days worked is, on average, "only" 15 percent (9,418 versus 8,216). The coinciding high earnings gap and the lower employment gap suggest a much lower female labor supply at the

[9]Gross earnings are reported only up to the social-security contribution ceiling (relevant for old-age pensions and unemployment benefits); higher values are top-coded, affecting about 5% of spells. We impute these following Dauth and Eppelsheimer (2020). Results are similar using the raw top-coded values.

[10] The 1975–2019 window implies an unbalanced panel: cohorts born in 1945 are observed at ages 30–65, those born in 1965 at 15–54. To maximize the balanced sample, we use slightly different age bins than FÖ's 25–54 and 42–45. Appendix Table A.8 shows that our results are robust to this choice.

[11]Measurement error is likely limited due to low interstate mobility in these cohorts. NEPS data show no cutoff effect on regional mobility (Appendix B.4), implying remaining error likely attenuates estimates.

intensive margin, presumably due to prevailing social norms.[12] On average, one-quarter of individuals completed the academic track (high school equivalent), and 16 percent have a college/university degree with higher shares among men.

4 Empirical Strategy

Following FÖ, we exploit the legal cutoff rules for school enrollment as a source of exogenous variation in school starting age (SSA) within a parametric regression discontinuity (RD) design.[13] Similar to FÖ, we do not observe SSA in our labor market data. Thus, we focus on the intention-to-treat (ITT) effect of being born after the cutoff, but we also provide evidence on the first-stage effect of being born after the cutoff on SSA.[14] In the main analysis, we estimate the following reduced-form equation using the SIAB:

$$Y_{ics}^{a} = \beta^{a}\ After + f^{a}(m_{ics}) + \pi_{c}^{a} + x_{ics}'\delta^{a} + \varepsilon_{ics}^{a}, \tag{1}$$

where Y_{ics} is an outcome (e.g., earnings) of individual i from birth cohort c and the federal state s. The outcomes are measured at a specific age (range) a. The discrete running variable m_{ics} measures the relative distance between an individual's birth month and the relevant school-entry cutoff. We normalize m_{ics} to zero for the last birth month before the cutoff so that it equals one for the first month after the cutoff (i.e., runs from -5 to 6). In our preferred specification, we define $f(m_{ics})$ as a linear function of the running variable

[12] Most West German women in these cohorts worked part-time—a pattern still common and not confined to mothers of young children. For the 2000s, Dehos and Paul (2023) report that fewer than 15% (25%) of mothers worked $\geq$ 35 hours/week when their youngest child was aged 7–9 (12–15).

[13] RD methods have advanced since FÖ. Current recommendations often favor nonparametric local polynomial regressions with bias-corrected inference to address potential misspecification of the running-variable trend (e.g., Armstrong and Kolesár, 2018, 2020; Calonico et al., 2020). These approaches require sufficient support of the running variable to select optimal bandwidths and construct bias-corrected bounds. Similar to FÖ, our running variable takes only twelve values (birth months), yielding an insufficient number of mass points for honest-inference procedures to select the optimal bandwidth for bias-corrected intervals. We therefore rely on a parametric RD specification, complemented by local randomization inference as a natural alternative to local polynomial methods (Cattaneo and Escanciano, 2017).

[14] FÖ use a two-sample instrumental variable (IV) approach to directly scale the ITT estimates by the first-stage effect. This allows them to interpret the IV estimate as the effect of a one-year increase in SSA. Such an interpretation ignores the distinction between absolute and relative age effects (which may violate the assumptions of exclusion and monotonicity). Thus, most recent studies in the SSA literature focus on estimating and interpreting reduced-form effects rather than on the IV framework (e.g., Landersø et al., 2017; Dhuey et al., 2019; Landersø et al., 2020; Oosterbeek et al., 2021) and we follow this literature.

with different slopes on either side of the cutoff.[15] The main regressor of interest is the indicator $After = \mathbb{1}\{m_{ics} > 0\}$. Thus, β measures the local effect of being the oldest in class relative to being the youngest for individuals who comply with the cutoff regulations. π_c represent cohort fixed effects. For sensitivity tests, we include additional covariates in x_{ics}, such as e.g. federal-state fixed effects or the cutoff month fixed effects, as the cutoffs in Germany vary across states. Finally, ε_{isc} captures the unobserved heterogeneity. Following Kolesár and Rothe (2018), we estimate the heteroskedasticity-robust standard errors.[16]

The main identification assumption is that $f(m_{ics})$ is a continuous and smooth function with no other discontinuity at the cutoff other than a higher SSA. A potential threat could be that some parents time the child's birth in response to the expected school entry cutoff. To mitigate such concerns, Appendix Figure A.4 shows that the distribution of births around the cutoff is smooth. Consistent with the graphical inspection, we do not find a differential mass of births around the cutoff using the density tests for discrete running variables by Frandsen (2017). We also find no evidence that predetermined characteristics are imbalanced around the cutoff (see Appendix B.2). Therefore, it seems plausible that conditional on $f(m_{ics})$, the treatment indicator *After* is as good as randomly assigned.

5 Results

We begin by summarizing our key findings on the first-stage relationship in the NEPS data, which we discuss in detail in Appendix B. In general, we find a substantial discontinuity in school starting age (SSA) around the school entry cutoff, implying that the actual SSA increases at the cutoff by 0.375 years on average (significant at the 1% level).[17] The first stage effects and the proportions of (non-)compliers are almost identical for men and

[15] Appendix Figure A.3 plots the evolution of mean earnings at selected ages across m_{ics}. Each subfigure shows linear and quadratic trends fitted separately to the raw data on either side of the cutoff. The trends are fairly linear. In Section 5, we also show that our main findings remain robust when employing a quadratic specification in $f(m_{ics})$, narrowing the bandwidths around the cutoff, and dropping observations closest to the cutoff. The overall evidence strongly supports our choice of linear $f(m_{ics})$ in the main specification.

[16] We also use alternative inference procedures, clustering by the running variable and by state (given state-specific cutoffs). Results are similar across methods, with robust standard errors typically most conservative (Appendix Table A.3). Because our running variable has few mass points (birth months), honest-inference methods (Kolesár and Rothe, 2018; Armstrong and Kolesár, 2020) are not feasible, as they require sufficient support within the bandwidth to bound worst-case bias. As an alternative, Table A.4 implements the local randomization inference (Cattaneo et al., 2017), which strongly supports our main conclusions.

[17] This is consistent with Puhani and Weber (2008) and Dustmann et al. (2017), who study more recent German cohorts.

women. Thus, any differences in labor market responses to the cutoff rule cannot be attributed to gender differences in (non-)compliance. The point estimate of 0.375 implies that we can scale the subsequent reduced-form results from the SIAB by a factor of 2.7 to interpret them as instrumental variable (IV) estimates of starting school one year later.

Next, we turn to the labor market effects using the SIAB data. Figure 1 shows the estimated effects of the school entry cutoff on age-earnings profiles. Each age-specific estimate comes from a separate linear regression of annual earnings at a given age on the *After* dummy from our main model specification. The vertical dashed lines mark the prime-age interval 30-54.[18] For ease of interpretation, we follow FÖ and normalize individual age-specific earnings by the respective cohort-specific mean of total prime-age earnings.[19]

In Panel (a), we replicate the significant initial earnings disadvantage for individuals born after the cutoff, a finding that FÖ primarily interpret as a mechanical consequence of a later school start that postpones labor market entry and leads to an initial loss of experience. We find no statistically significant effects during the prime working ages, except for the negligible earnings premiums around age 32. This seems to be driven by a slight increase in labor supply (see Appendix Figure A.6). These patterns largely confirm the findings by FÖ for Sweden and the Netherlands (Oosterbeek et al., 2021). In contrast to these countries, however, we do not find any earnings gains towards the end of the working life due to increased labor supply just before the nominal retirement age of 65. The lack of reallocation of labor supply towards the end of the career may be related to relatively generous early-retirement schemes (see, e.g., Riphahn and Schrader, 2021).

The gender split in panels (b) and (c) shows that men drive the overall pattern of disappearing effects at prime working ages. Women born after the cutoff, however, experience earnings 2–2.5 percentage points higher throughout their careers, with particularly strong effects between ages 25 and 30. Except for the absence of substantial positive effects around retirement, our gender-specific estimates confirm FÖ, who document positive long-run

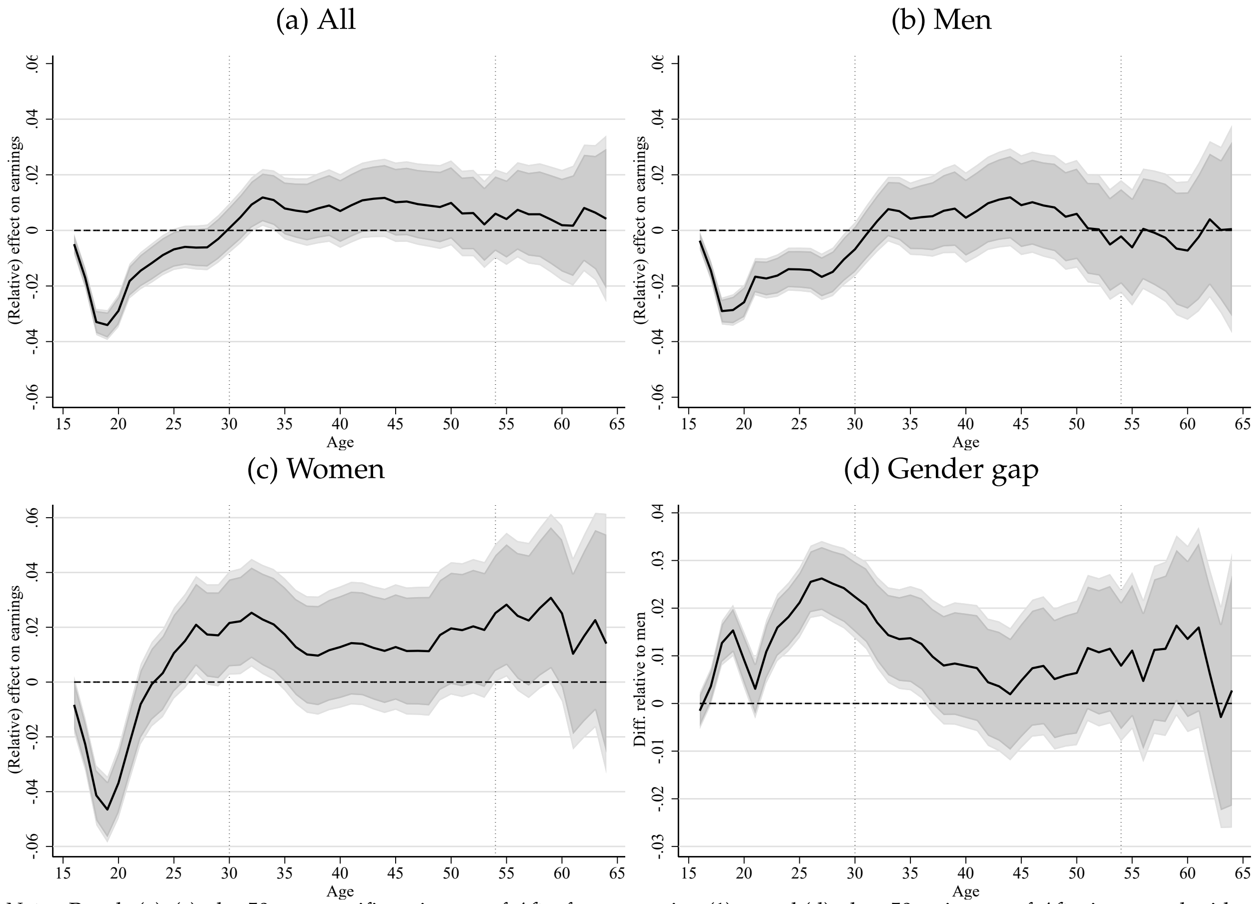
[18] Within this interval, the estimation samples include all individuals born between 1944 and 1963. Outside this range, our panel is unbalanced for birth cohorts. Therefore, these results should be treated with caution.

[19] Thus, the scaling factor in subfigures (a) and (d) differs from the gender-specific normalization in subfigures (b) and (c). Figure A.5 shows that using the full-sample normalization in all subfigures yields similar patterns. The alternative results are better suited for comparing the differences across the subsamples.

effects for women and virtually none for men. While life-cycle patterns are similar across countries, the effects for women are much larger in Germany than in Sweden. We return to this in Section 6.

The gender-specific effects suggest further implications for the overall gender pay gap. We extend the analysis beyond the results in FÖ to shed more light on this issue. Specifically, in panel (d), we extend the model specification by interacting the *After* indicator with a female dummy and plot the age-specific estimates of this interaction term. The figure shows a substantial effect of heterogeneity in favor of female earnings that emerges early in the career and persists, albeit attenuated, until the early 40s. The striking differences in earnings effects up to the mid-30s can largely be attributed to gender-specific labor supply responses (see panel (d) of Appendix Figure A.6).

Figure 1: Age-specific effects of being born after the cutoff on earnings



Notes: Panels (a)–(c) plot 50 age-specific estimates of *After* from equation (1); panel (d) plots 50 estimates of *After* interacted with a female dummy. Each estimate comes from a separate linear regression with cohort fixed effects and a linear running-variable trend with side-specific slopes. Dark (light) shading shows 90% (95%) confidence intervals based on robust standard errors. Vertical dashed lines mark the prime-age interval 30–54, where samples are balanced across cohorts. Figures use West German cohorts born 1945–1965. Age-specific earnings are measured relative to average cohort-specific (in panels b and c, also gender-specific) prime-age earnings.

Table 1 summarizes the lifetime consequences of the age-specific effects. The first four columns show the effects on the sum of earnings and days worked accumulated over the entire career (ages 15–64) and during the prime working years (ages 30–54).[20] The effects are measured in percentage points relative to the respective cohort-specific means. We multiply all estimates by 100 to improve readability. Panel A reports average effects from the full sample. The small negative coefficient in column 1 is mainly due to foregone earnings from a later entry into the labor market, as during the prime-age period, the earnings (column 2) and employment (column 4) effects are positive (yet small and insignificant).

Panel B shows that men drive the overall zero effects.[21] In contrast, the corresponding point estimates for women in Panel C are positive (though mostly insignificant), suggesting that they more than make up for the initial earnings losses. The marginally significant estimate in column 2 implies that women born after the cutoff enjoy an earnings premium of 1.9 percentage points in their prime working years.

The benefits for women are more evident in Panel D, where we estimate a parsimonious interacted specification that increases precision. Importantly, the estimate of the treatment–gender interaction is nearly identical in a fully interacted model, but much less precise.[22] The lifetime effect on female earnings is about 1.6 to 1.8 percentage points larger (and statistically significant) than that on male earnings. Labor supply responses can partially, but not fully, explain the compressing effect of SSA on the gender earnings gap, as the female excess in the effect on days worked is 0.5–0.7 percentage points.[23]

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[20] Pooled regressions using the age-specific outcomes yield similar results (see Appendix Table A.2). Averaging the coefficients from Figure 1a across the 30-54 age range yields an effect size of 0.8 ppt. This is slightly higher than the corresponding 0.5 ppt in Panel A, column 2 of Table A.2. This discrepancy is likely due to more model flexibility in Figure 1a as the RDD regressions are run separately for each age year.

[21] Our estimates for German men are also consistent with Dustmann et al. (2017). They study the earnings effects of full-time male workers in the 30s and early 40s from more recent German cohorts (1961–1976). We complement their results by documenting heterogeneous life-cycle effects for men and women.

[22] Appendix Table A.9 reports results from the fully interacted specification and a slightly less flexible variant. The point estimates are nearly identical across the different specifications, but the parsimonious model substantially improves precision. To further support the argument that there is no efficiency-consistency tradeoff due to misspecification in our main results, Table A.4 reports alternative results from the local randomization approach within narrower windows around the cutoff. They do not impose any functional-form assumptions and lead to very similar conclusions.

[23] These results are robust to the inclusion of additional covariates, quadratic running-variable trends, trimming the bandwidth around the cutoff, and alternative inference procedures (Appendix Table A.3).

Table 1: Lifetime effects of being born after the cutoff

	Sum of earnings (effects in pp)		Sum of employment days (effects in pp)		Education			
						Degrees (effects in pp)		
	Ages 15-64	Ages 30-54	Ages 15-64	Ages 30-54	Years of schooling	Academic track	College/ University	Vocational training
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All (306,145 observations/individuals)								
After	−0.019	0.507	−0.291	0.359	0.064***	0.948***	0.642**	−0.658**
	(0.645)	(0.698)	(0.357)	(0.386)	(0.015)	(0.316)	(0.267)	(0.308)
Panel B: Men (156,732 observations/individuals)								
After	−0.371	0.257	−0.624	0.169	0.061***	1.033**	0.958**	−0.792*
	(0.750)	(0.792)	(0.479)	(0.499)	(0.017)	(0.459)	(0.406)	(0.439)
Panel C: Women (149,413 observations/individuals)								
After	1.420	1.865*	0.164	0.693	0.067***	0.877**	0.332	−0.534
	(0.914)	(1.030)	(0.522)	(0.585)	(0.015)	(0.431)	(0.337)	(0.430)
Panel D: Gender difference in the effect (306,145 observations/individuals)								
After×female	1.816***	1.559**	0.739**	0.529	0.003	−0.162	−0.288	0.406
	(0.583)	(0.628)	(0.352)	(0.379)	(0.016)	(0.314)	(0.263)	(0.306)

Notes: All reported coefficients are estimates of the discontinuity in the respective outcome (given in the column head) for the school-entry cutoff or its interaction. All specifications include cohort fixed effects. Robust standard errors are reported in parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively.

Several possible explanations exist for why a later school start may help close the gender gap in lifetime earnings. First, columns 5–8 of Table 1 show that disproportionate effects on educational attainment do not directly explain the effect on the gender gap. While we find positive effects on years of schooling (similar to FÖ), academic track, and college completion, as well as a negative impact on vocational training, the magnitudes of these effects are either similar across genders or slightly less pronounced for females. In any case, the differences are statistically insignificant.

Second, as noted above, the substantial gender differences in initial earnings effects largely reflect differences in early-career labor supply (Appendix Figure A.6). Women born after the cutoff experience smaller experience losses than later-born men, who show larger effects on postsecondary education (Table 1), implying longer educational spells. Although employment differences cannot directly explain the earnings gap beyond the late 30s (Appendix Figure A.6), women's earnings may benefit from relatively higher returns to experience when earnings trajectories are steep (e.g., Aryal et al., 2022), with effects that carry over to the long run.

Third, we consider the role of job and firm characteristics (see Appendix Table A.5). Although we do not observe hours worked, the estimates do not support gender-specific effects on labor supply at the intensive margin, as we do not find differential effects on accumulated labor market experience in full-time jobs (columns 1 and 2) and multiple job-holding (columns 3 and 4) across genders. We observe that relatively older school entrants spend a larger share of their working lives in jobs with more complex skill requirements (columns 5 and 6), consistent with the positive effects of the cutoff rules on cognitive skills in Germany (Görlitz et al., 2022). However, these effects on job complexity are similar across genders. There are also no effects on labor market mobility across employers (columns 7 and 8) and firm size (columns 9 and 10) for either men or women.

Finally, any gender-specific effects of schooling laws on labor market outcomes could also arise from potential effects on women's fertility decisions and their interference with maternal labor supply.[24] Examining this channel (see Appendix Table A.6), we find no effects on the probability of becoming a mother or on the total number of children. Instead, our estimates imply a significant increase in the age at first birth by about a quarter of a year.[25] While the school-entry cutoff also shifts the age at labor market entry by a similar amount (column 4), mothers who started school later are better educated and older at the time of first birth. This may strengthen their attachment to the labor market.[26] As motherhood is a key determinant of the persistent gender wage gap (e.g., Olivetti and Petrongolo, 2016; Kleven et al., 2019; Olivetti et al., 2024), delayed fertility may positively affect earnings in the long run because mothers typically lose experience at career stages when earnings trajectories are steep and returns to experience high (Bhuller et al., 2017). Thus, women born after the cutoff accumulate valuable labor-market experience during critical, wage-forming periods, which appears to pay off in the long run.

[24]German social security data do not directly report fertility outcomes. However, Müller et al. (2022) developed a procedure that allows relatively accurate inference about births from a woman's maternity leave spells as reported by her employer. For a recent application, see, e.g., Zimmert and Zimmert (2024).

[25]These results are consistent with Fredriksson et al. (2022), who find that SSA increases maternal age at birth but does not affect the number of children in Finland.

[26]In line with this argument, Appendix Figure A.6 shows positive employment effects for women around the average age of first childbirth (of 27 years old). In Appendix Table A.7, we directly show that the enrollment cutoff affects maternal labor market outcomes, particularly after birth.

6 Conclusion

Whether school starting age (SSA) leaves a persistent imprint on adult earnings is a much-investigated topic in labor economics. Applying a regression discontinuity design to Swedish data, Fredriksson and Öckert (2014) were the first to document that the direction and magnitude of the effects can vary substantially across the working life and by gender. We replicate their findings for Germany and find very similar patterns. We also go beyond a narrow replication by showing that the differential SSA effects for men and women compress the gender gap in lifetime earnings. We show that this cannot be attributed to gender-specific compliance with school entry rules, direct heterogeneous effects on educational attainment, or differential sorting into jobs or firms. Instead, we provide evidence that the substantial benefit from a later school start for women relative to men emerges mostly until the mid-30s as a consequence of increased female labor supply around the first birth, which typically coincides with critical wage-forming periods.

However, while the negligible effects for men are comparable across countries, the magnitude of the positive effect on women's earnings in their prime working years appears to be larger in Germany than in Sweden.[27] Interestingly, these cross-country differences arise despite remarkably similar effects of SSA on years of schooling.[28] Our results point to increased maternal education, the delay in first birth, and increased labor market attachment after childbirth as potential mechanisms behind the positive effect in Germany. This is consistent with motherhood being a key determinant of gender gaps in labor markets (e.g., Olivetti et al., 2024). This channel may be crucial for (West) German women who face conservative social norms regarding maternal employment, a limited supply of public childcare, and high penalties for motherhood (see, e.g., Gangl and Ziefle, 2009, Kleven et al., 2019, Boelmann et al., 2024). This paper cannot provide empirical evidence on these issues. Still, our results suggest a new avenue for research on potential explanations for cross-gender and cross-country differences in the impact of SSA on earnings.

[27] For comparison with the IV estimates in FÖ, we relate our reduced-form effects at ages 30–54 in Table 1 to the corresponding first-stage effects in Appendix Table B.5. For example, for German women, our estimates imply that a one-year increase in SSA increases prime-age earnings by (1.865/0.36=) 5.1 percentage points. For Swedish women, FÖ estimate a 1 percentage point increase in earnings at ages 25–54.

[28] FÖ find that starting school one year later increases female educational attainment by 0.181 years of schooling. Our estimates for German women suggest an increase of 0.183 (=0.067/0.367) years of schooling.

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Online Appendix – not for publication

A Additional tables and figures

Table A.1: Descriptive statistics

Variable	All		Men		Women	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Individual characteristics:						
Female	0.488	0.500	0.000	0.000	1.000	0.000
Year of birth	1956.088	5.934	1956.019	5.950	1956.160	5.916
Month of birth	6.407	3.430	6.409	3.433	6.404	3.428
Schleswig-Holstein	0.044	0.205	0.043	0.203	0.045	0.206
Hamburg	0.026	0.158	0.025	0.157	0.026	0.158
Lower Saxony	0.125	0.331	0.126	0.331	0.124	0.330
Bremen	0.008	0.088	0.008	0.088	0.008	0.088
North Rhine-Westphalia	0.283	0.451	0.284	0.451	0.282	0.450
Hesse	0.092	0.289	0.091	0.287	0.093	0.290
Rhineland-Palatinate	0.070	0.255	0.071	0.256	0.070	0.255
Baden-Wuerttemberg	0.154	0.361	0.153	0.360	0.155	0.362
Bavaria	0.186	0.389	0.186	0.389	0.186	0.389
Saarland	0.013	0.111	0.013	0.114	0.012	0.109
Labor Market Outcomes:						
Earnings (in 2015 KEUR)						
Sum across ages 15–64	909.691	812.354	1225.597	915.017	578.311	509.313
Sum across ages 30–54	643.400	622.206	890.863	701.399	383.815	381.527
Employment (in days)						
Sum across ages 15–64	8831.284	4393.299	9417.553	4509.889	8216.296	4180.140
Sum across ages 30–54	5843.760	3108.426	6375.575	3143.009	5285.893	2971.143
Educational Outcomes:						
Year sof schooling	9.945	1.664	10.027	1.729	9.858	1.587
Academic track certificate	0.254	0.435	0.285	0.451	0.221	0.415
College/University degree	0.158	0.364	0.197	0.398	0.117	0.321
Vocational training	0.772	0.419	0.755	0.430	0.790	0.407
Observations/Individuals:	306,145		156,732		149,413	

Table A.2: Results from pooled regressions of age-specific outcomes

	Age-specific earnings (effects in percentage points)		Age-specific employment days (effects in percentage points)	
	Obs. pooled over whole career (ages 15-64)	Obs. pooled over prime working ages (30-54)	Obs. pooled over whole career (15-64)	Obs. pooled over prime working ages (30-54)
Panel A: All (306,145 individuals)				
$After$	-0.014	0.507	-0.265	0.359
	(0.543)	(0.698)	(0.321)	(0.386)
Observations	12,868,061	7,653,625	12,868,061	7,653,625
Panel B: Men (156,732 individuals)				
$After$	-0.292	0.257	-0.549	0.169
	(0.616)	(0.792)	(0.423)	(0.449)
Observations	6,583,536	3,918,300	6,583,536	3,918,300
Panel C: Women (149,413 individuals)				
$After$	1.229	1.865^{*}	0.148	0.693
	(0.816)	(1.030)	(0.482)	(0.585)
Observations	6,284,525	3,735,325	6,284,525	3,735,325
Panel D: Gender difference in the effect (306,145 individuals)				
$After \times female$	1.610^{***}	1.559^{***}	0.710^{**}	0.529
	(0.493)	(0.628)	(0.317)	(0.379)
Observations	12,868,061	7,653,625	12,868,061	7,653,625

Notes: All reported coefficients are estimates of the discontinuity in the respective outcome (given in the column head) for the school-entry cutoff and/or its interaction. All specifications include cohort fixed effects. Standard errors reported in the parentheses are clustered at the individual level. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.3: Robustness checks for the interaction effect

	Sum of earnings (effects in percentage points)		Sum of employment days (effects in percentage points)	
	Whole career (ages 15-64)	Prime working ages (30-54)	Whole career (15-64)	Prime working ages (30-54)
<u>Main results</u> (Obs./Ind. 306,145)				
$After \times female$	1.816***	1.559**	0.739**	0.529
	(0.583)	(0.628)	(0.352)	(0.379)
Alternative clustering levels for the std.err. [brackets: state & cohort (two-way)]; {braces: state}				
	[0.455]***	[0.455]***	[0.351]**	[0.405]
	{0.513}***	{0.574}***	{0.334}**	{0.424}
<u>*Panel A:* Adding Fixed Effects for the cutoff month</u>				
$After \times female$	1.808***	1.553**	0.738**	0.529
	(0.583)	(0.628)	(0.352)	(0.378)
<u>*Panel B:* Adding federal state fixed effects</u>				
$After \times female$	1.769***	1.515**	0.716**	0.509
	(0.582)	(0.627)	(0.351)	(0.378)
<u>*Panel C:* Adding controls for short school years and compulsory schooling reform exposure</u>				
$After \times female$	1.813***	1.556**	0.737**	0.527
	(0.583)	(0.628)	(0.352)	(0.379)
<u>*Panel D:* Federal state by birth month fixed effects</u>				
$After \times female$	1.773***	1.517**	0.717**	0.510
	(0.582)	(0.627)	(0.351)	(0.378)
<u>*Panel E:* Adding federal state by cohort fixed effects</u>				
$After \times female$	1.755***	1.499**	0.714**	0.514
	(0.582)	(0.627)	(0.351)	(0.378)
<u>*Panel F:* Adding quadratic trends</u>				
$After \times female$	1.816***	1.558**	0.740**	0.530
	(0.583)	(0.628)	(0.352)	(0.379)
<u>*Panel G:* Donut specification: dropping $-/+$ 1 month around the cutoff</u> (Obs./Ind. 251,989)				
$After \times female$	2.350***	2.131***	0.870**	0.693*
	(0.644)	(0.693)	(0.388)	(0.417)
<u>*Panel H:* Narrowing the bandwidth to 5 months around the cutoff</u> (Obs./Ind. 253,056)				
$After \times female$	2.135***	1.823***	0.907*	0.660
	(0.634)	(0.682)	(0.382)	(0.411)

Notes: All reported coefficients are estimates of the discontinuity in the respective outcome (given in the column head) for the school-entry cutoff and/or its interaction. All specifications include cohort fixed effects if not stated differently. Robust standard errors are reported in the parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.4: Comparison of the main results and the local randomization approach

		Local randomization, bandwidth		
	(1) Main	(2) ±3	(3) ±4	(4) ±5
Panel A: Dep. var.: *female* (balancing check)				
$After$	0.003	0.001	0.003	0.001
	(0.004)	(0.002)	(0.002)	(0.002)
Panel B: Dep. var.: sum of earnings, ages 15-64				
$After \times female$	1.816***	1.597**	1.452**	2.136***
	(0.583)	(0.804)	(0.700)	(0.634)
		[0.045]	[0.050]	[0.01]
Panel C: Dep. var.: sum of earnings, ages 30-54				
$After \times female$	1.559**	1.249	1.149	1.823***
	(0.628)	(0.866)	(0.754)	(0.682)
		[0.150]	[0.130]	[0.01]

Notes: Panel A reports a balancing test using the female indicator as the dependent variable. The small and statistically insignificant coefficient indicates gender balance in the sample, consistent with the local randomization assumption (Cattaneo and Escanciano, 2017). Panels B and C report results earnings at ages 15–64 and 30–54, respectively. Column (1) presents estimates from our baseline specification. Columns (2)–(4) report results from local randomization regressions estimated in various symmetric windows around the cutoff, treating treatment assignment as good as random within the respective window and omitting any functional-form assumptions on the running variable. Inference in columns (2)–(4) is based on permutation tests that randomly reassign treatment within a specific window while holding the number of treated observations fixed. This approach does not rely on assumptions about the functional form of the running-variable trend and is valid under the local randomization framework rather than the continuity assumption of the conditional expectation function. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. In column (1), significance levels are based on robust standard errors reported in parentheses. In columns (2)–(4), stars correspond to permutation p-values (reported in brackets) obtained from 199 random permutations.

Table A.5: Effects on job and employer characteristics

	Share full time		Share multiple jobs		Share complex jobs		No. of employers		Firm size (in 100 employees)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age bracket:	15-64	30-54	15-64	30-54	15-64	30-54	15-64	30-54	15-64	30-54
Panel A: All (Obs./Ind. 306,145)										
After	−0.319	−0.285	−0.070	−0.074	0.526**	0.445*	−0.013	0.011	−0.229	−0.184
	(0.243)	(0.284)	(0.065)	(0.073)	(0.244)	(0.261)	(0.033)	(0.022)	(0.264)	(0.277)
Y-Mean	74.09	70.88	3.304	3.395	19.64	20.47	5.521	3.410	9.523	9.269
Panel B: Men (Obs./Ind. 156,732)										
After	−0.105	−0.185	−0.042	−0.048	0.640*	0.488	0.017	0.010	−0.190	−0.118
	(0.201)	(0.233)	(0.075)	(0.083)	(0.376)	(0.402)	(0.050)	(0.033)	(0.469)	(0.496)
Y-Mean	91.90	91.90	2.586	2.605	25.25	26.50	5.792	3.565	13.40	13.33
Panel C: Women (Obs./Ind. 149,413)										
After	−0.351	−0.142	−0.110	−0.114	0.453	0.453	−0.044	0.012	−0.226	−0.208
	(0.363)	(0.421)	(0.106)	(0.120)	(0.295)	(0.317)	(0.041)	(0.028)	(0.217)	(0.218)
Y-Mean	55.39	48.83	4.057	4.225	13.76	14.14	5.238	3.247	5.456	5.011
Panel D: Interacted (Obs./Ind. 306,145)										
After	−0.172	−0.281	−0.086	−0.091	0.638**	0.518*	−0.016	0.009	−0.335	−0.285
	(0.204)	(0.237)	(0.067)	(0.076)	(0.280)	(0.300)	(0.038)	(0.025)	(0.326)	(0.344)
After× *female*	−0.109	0.217	0.024	0.027	−0.171	−0.086	0.010	0.005	0.259	0.254
	(0.207)	(0.240)	(0.065)	(0.073)	(0.238)	(0.255)	(0.032)	(0.022)	(0.258)	(0.271)
Y-Mean	74.09	70.87	3.304	3.395	19.64	20.5	5.521	3.410	9.523	9.269

Notes: All reported coefficients are estimates of the discontinuity in the respective outcome (given in the column head) for the school-entry cutoff and/or its interaction. All specifications include cohort fixed effects if not stated differently. Robust standard errors are reported in the parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.6: Effects on fertility outcomes for women

	Motherhood indicator (effects in pp) (1)	Number of children (2)	Age at first birth (cond. on motherhood) (3)	Age at labor market entry (4)
$After$	-0.002	0.003	0.244^{***}	0.237^{***}
	(0.005)	(0.008)	(0.069)	(0.072)
Y-Mean:	0.471	0.644	27.164	24.85
Obs./Ind.:	149,413	149,413	70,410	149,413

Notes: All reported coefficients are estimates of the discontinuity in the respective outcome (given in the column head) for the school-entry cutoff and/or its interaction. All specifications include cohort fixed effects if not stated differently. Robust standard errors are reported in the parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.7: Effects on earnings and employment around birth for mothers

	Effects (in percentage points)					
	from five years to one year before birth		from the year of birth to five years treheafter		from the year of birth to ten years treheafter	
	Earnings (1)	Employment (2)	Earnings (3)	Employment (4)	Earnings (5)	Employment (6)
$After$	1.012	0.208	3.312^{*}	2.239^{*}	3.846^{**}	2.416^{*}
	(1.353)	(1.091)	(1.750)	(1.329)	(1.752)	(1.262)
Obs./Ind.:	70,410	70,410	70,410	70,410	70,410	70,410

Notes: All reported coefficients are estimates of the discontinuity in the respective outcome (given in the column head) for the school-entry cutoff and/or its interaction. All specifications include cohort fixed effects if not stated differently. Robust standard errors are reported in the parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.8: Long-run effects of being born after the cutoff across different age groups

	Sum of earnings (effects in pp)		Sum of employment days (effects in pp)	
	Ages 25-54 (1)	Ages 41-45 (2)	Ages 25-54 (3)	Ages 41-45 (4)
Panel A: All (Obs./Ind. 306,145)				
After	0.326	0.674	0.233	0.063
	(0.656)	(0.776)	(0.371)	(0.461)
Panel B: Men (Obs./Ind. 156,732)				
After	−0.004	0.627	−0.969	−0.057
	(0.741)	(0.903)	(0.476)	(0.598)
Panel C: Women (Obs./Ind. 149,413)				
After	1.831*	1.500	0.722	0.158
	(0.966)	(1.151)	(0.565)	(0.714)
Panel D: Gender difference in the effect (Obs./Ind. 306,145)				
After×female	1.892***	1.042	0.852**	0.494
	(0.587)	(0.711)	(0.363)	(0.459)

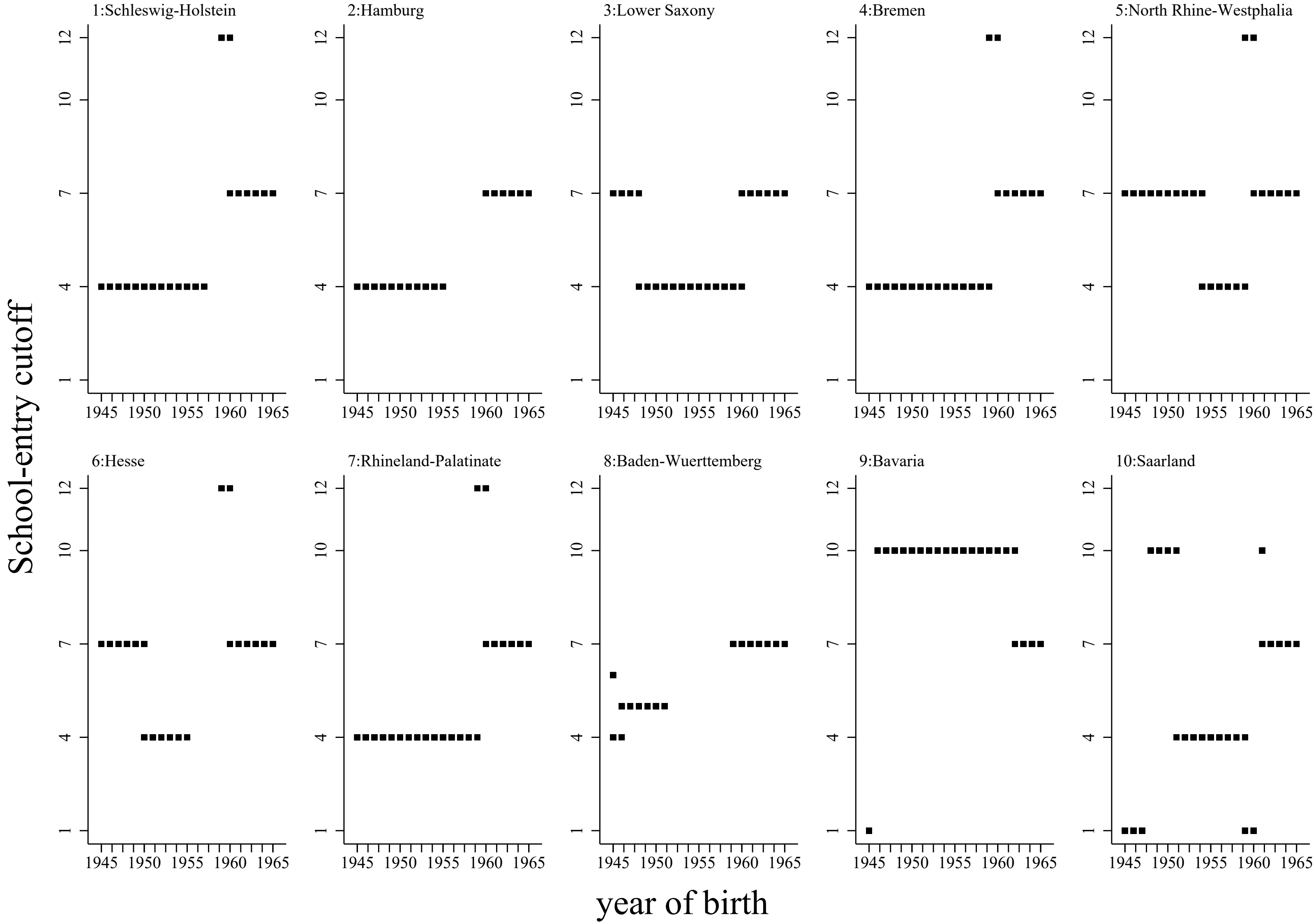
Notes: All reported coefficients are estimates of the discontinuity in the respective outcome (given in the column head) for the school-entry cutoff or its interaction. All specifications include cohort fixed effects. Robust standard errors are reported in the parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.9: Robustness of the gender difference to more flexible model specifications

	Sum of earnings (effects in pp)		Sum of employment days (effects in pp)	
	Ages 15-64 (1)	Ages 30-54 (2)	Ages 15-64 (3)	Ages 30-54 (4)
Panel A: Main specification (Obs./Ind. 306,145)				
After×female	1.816***	1.559**	0.739**	0.529
	(0.583)	(0.628)	(0.352)	(0.379)
Panel B: Fully-interacted model incl. gender-specific cohort fixed effects (Obs./Ind. 306,145)				
After×female	1.790	1.608	0.788	0.524
	(1.182)	(1.299)	(0.708)	(0.769)
Panel C: Fully-interacted model excl. gender-specific cohort fixed effects (Obs./Ind. 306,145)				
After×female	1.787	1.604	0.786	0.523
	(1.182)	(1.299)	(0.708)	(0.769)

Notes: All reported coefficients are estimates of the discontinuity in the respective outcome (given in the column head) for the school-entry cutoff or its interaction. All models include cohort fixed effects as specified in equation 1. The fully-interacted model in Panel B additionally includes gender-specific trends in the running variable on both sides of the cutoff, as well gender-specific cohort fixed effects. The model in Panel C includes gender-specific trends in the running variable on both sides of the cutoff but omits the gender-specific cohort fixed effects. Robust standard errors are reported in parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively.

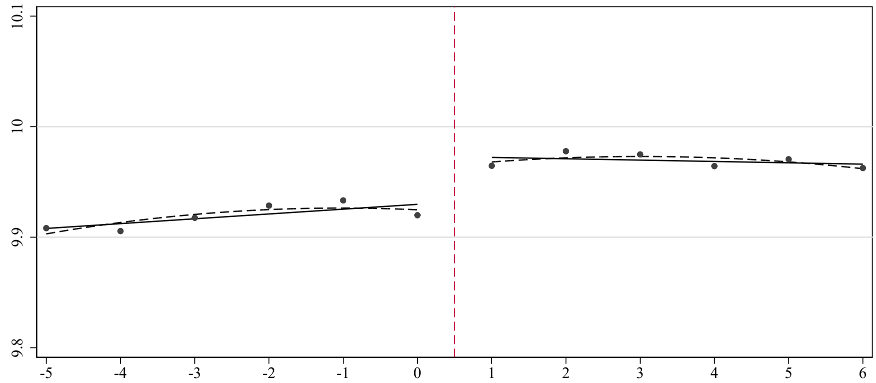
Figure A.1: The month of school entry by birth cohort and federal state



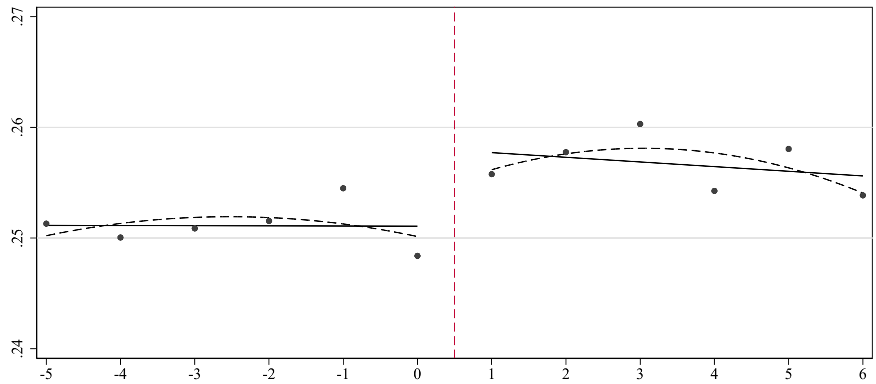
Notes: Based on data collected from primary sources. For details, see Cygan-Rehm (2026). The state-specific cutoff rules vary by birthdate (year and month).

Figure A.2: Educational outcomes by the distance to the cutoff

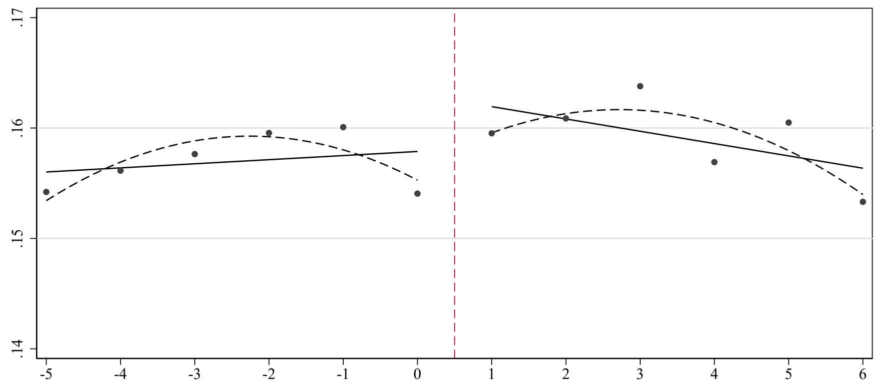
(a) Years of schooling



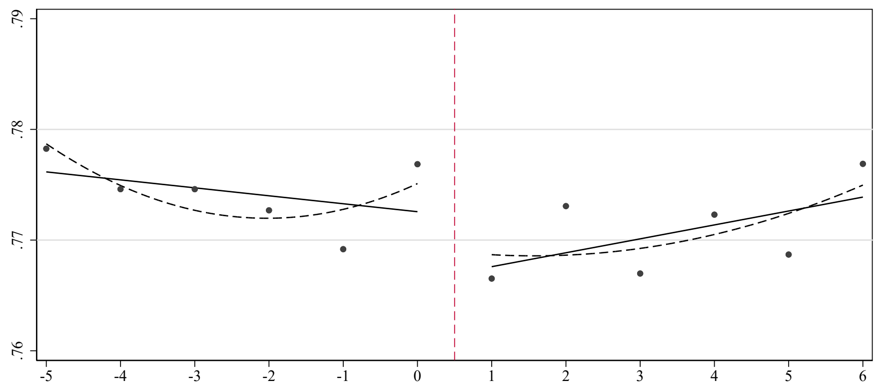
(b) Academic track certificate



(c) University education



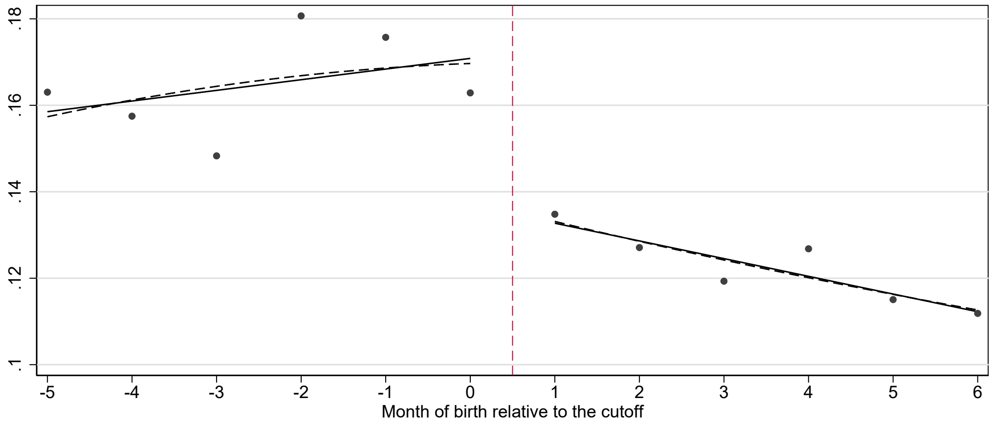
(d) Vocational training



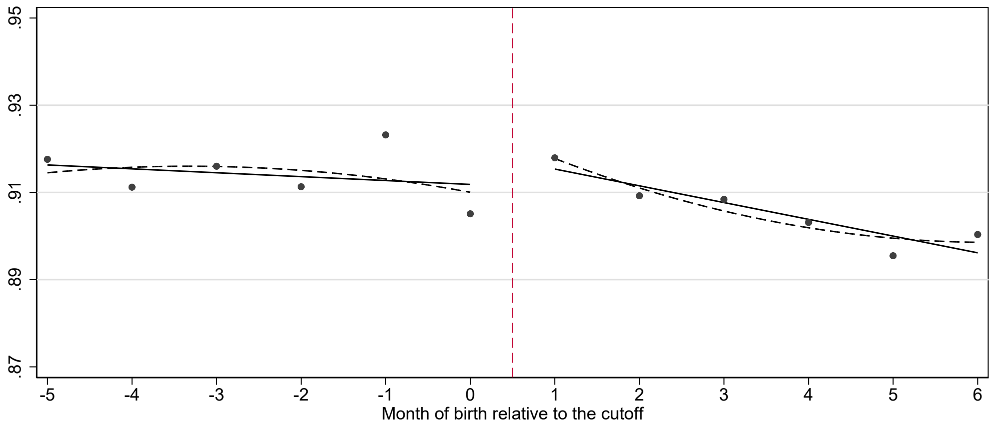
Notes: All panels show outcome trajectories for five months before and six months after the cohort and federal-state-specific school entry cutoff (indicated by the dashed vertical line). The month of birth on the x-axis is normalized so that it takes the value zero for the last birth month before the cutoff and the value one for the first month after the cutoff. Hence, individuals to the left (right) of the cutoff more often belong to the oldest (youngest) students in the class. The dots indicate the average outcome for the corresponding relative month. In contrast, the solid and dashed black lines represent linear and quadratic fits of the points separately for each side of the cutoff. The Figures are based on West German cohorts born between 1945 and 1965.

Figure A.3: Age-specific earnings by the distance to the cutoff

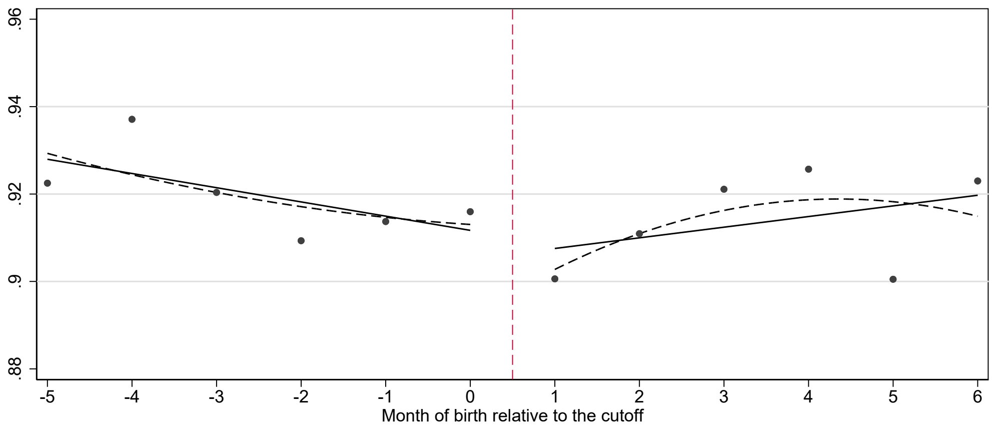
(a) at age 18



(b) at age 35



(c) at age 62



Notes: All panels show outcome trajectories for five months before and six months after the cohort and federal-state-specific school entry cutoff (indicated by the dashed vertical line). The month of birth on the x-axis is normalized so that it takes the value zero for the last birth month before the cutoff and the value one for the first month after the cutoff. Hence, individuals to the left (right) of the cutoff more often belong to the oldest (youngest) students in the class. The dots indicate the average outcome for the corresponding relative month. In contrast, the solid and dashed black lines represent linear and quadratic fits of the points separately for each side of the cutoff. The Figures are based on West German cohorts born between 1945 and 1965. The outcome is annual earnings relative to the cohort-specific reference earnings averaged over prime working ages (ages 30–54).

Figure A.4: Distribution of the number of observations by distance to the cutoff

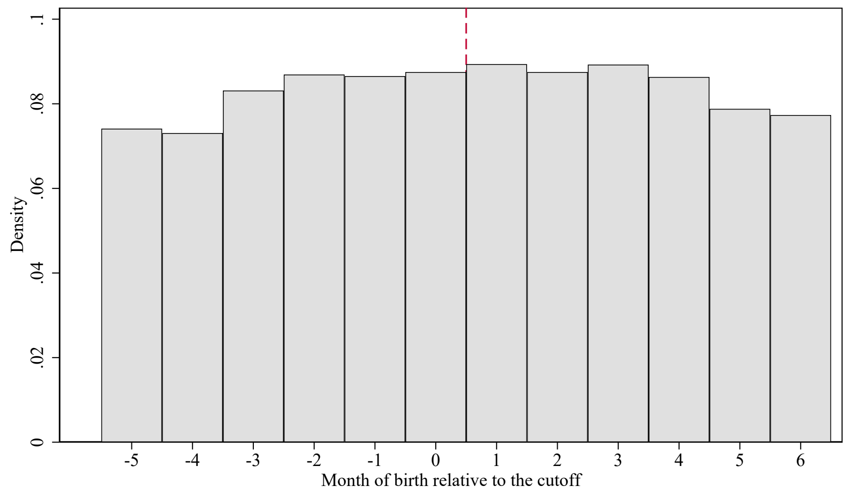
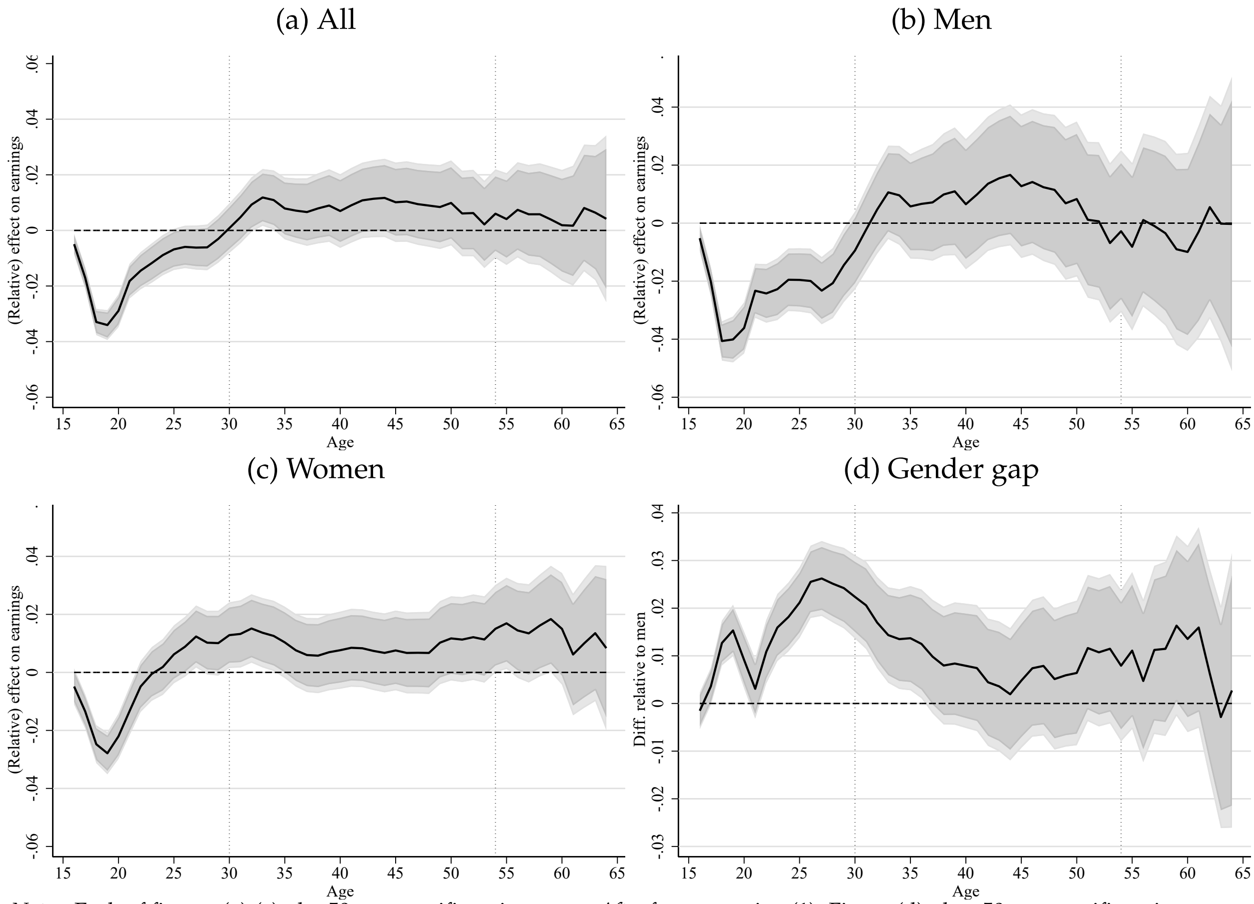
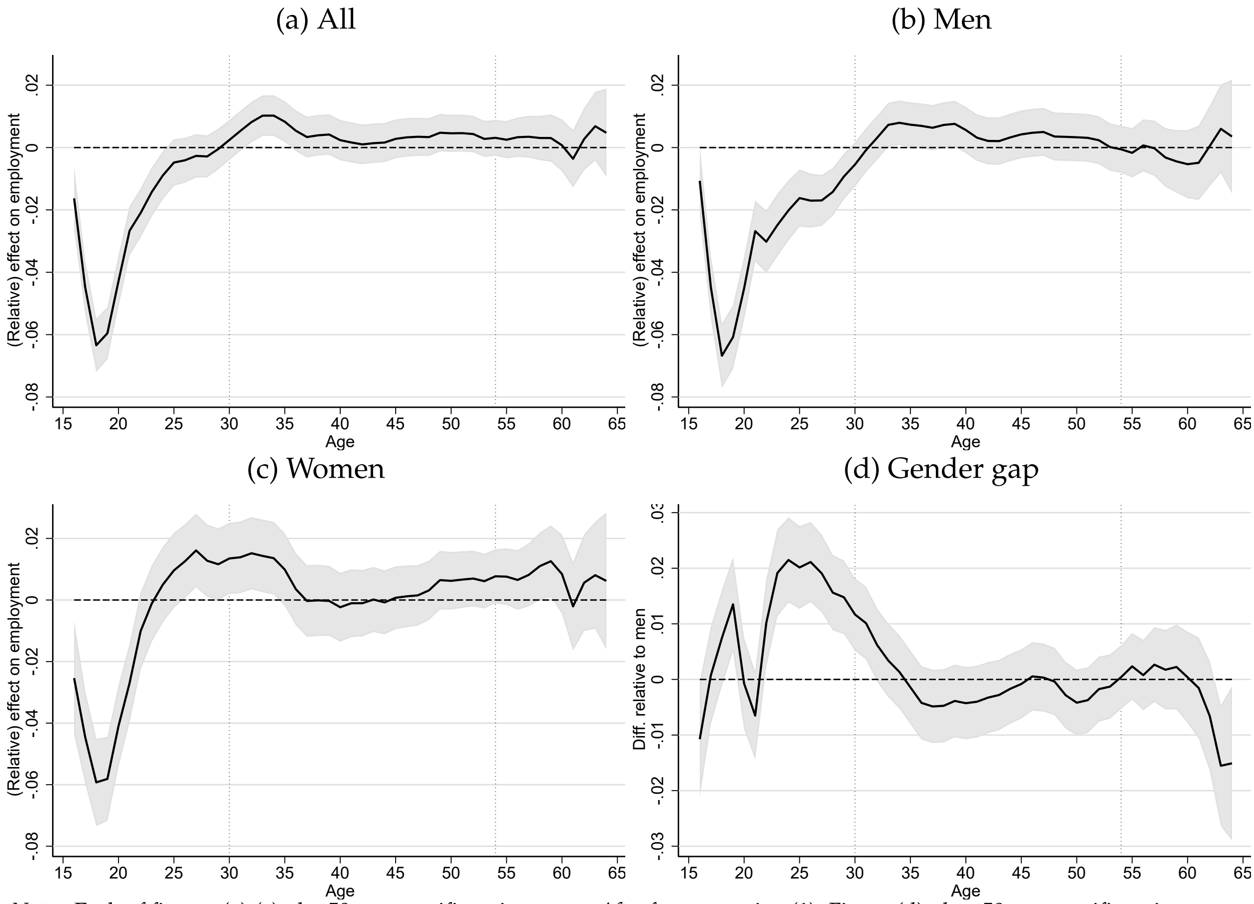


Figure A.5: Age-specific effects of being born after the cutoff on earnings: using the full-sample cohort-specific mean to scale the outcomes in all subfigures



Notes: Each of figures (a)-(c) plot 50 age-specific estimates on *After* from equation (1). Figure (d) plots 50 age-specific estimates on *After* interacted with a female dummy. Each estimate is from a separate linear regression, including cohort fixed effects and a linear trend in the running variable, the slope of which is allowed to vary on each side of the cutoff. Shaded areas show 90% confidence intervals based on robust standard errors. The vertical dashed lines mark the prime-age interval 30-54, where the estimation samples are balanced in birth cohorts. The Figures are based on West German cohorts born between 1945 and 1965. Age-specific earnings are measured relative to average cohort-specific prime-age earnings in the full sample (sum at ages 30–54).

Figure A.6: Age-specific effects of being born after the cutoff on employment



Notes: Each of figures (a)-(c) plot 50 age-specific estimates on *After* from equation (1). Figure (d) plots 50 age-specific estimates on *After* interacted with a female dummy. Each estimate is from a separate linear regression, including cohort fixed effects and a linear trend in the running variable, the slope of which is allowed to vary on each side of the cutoff. Shaded areas show 90% confidence intervals based on robust standard errors. The vertical dashed lines mark the prime-age interval 30-54, where the estimation samples are balanced in birth cohorts. The Figures are based on West German cohorts born between 1945 and 1965. Age-specific employment is measured as the annual sum of working days relative to average cohort-specific (in panels b and c, also gender-specific) prime-age employment (sum at ages 30–54).

B Supplementary evidence from the National Educational Panel Study: Starting Cohort Adults (NEPS-SC6)

B.1 Data and sample

We use data from the National Educational Panel Study: Starting Cohort 6 - Adults (NEPS-SC6, see Blossfeld and Roßbach (2019)) for several auxiliary analyses. The study is carried out by the Leibniz Institute for Educational Trajectories (LIfBi, Germany) in cooperation with a nationwide network (see Roßbach et al., 2023). The NEPS-SC6 started in 2007/2008 as a sample representative of the population born between 1956 and 1986. In 2009/2010 (second wave), the sample was extended to birth cohorts 1944-1955, and since then, the survey has been conducted annually. The key advantage of the data is the availability of detailed information on entire educational trajectories, which is collected retrospectively for each individual during the first interview. The educational spells are updated with more recent information from successive interviews, if applicable.

We exploit the richness of the NEPS-SC6 data for several additional pieces of evidence. First, unlike social security records, the NEPS allows us to test for balance in pre-determined characteristics (see Appendix B.2) because it provides retrospective information on individuals' socio-economic background (such as parental education, migration experience, age at birth, and the number of siblings). Second, we estimate the first-stage effect (see Appendix B.3) of being born after the cutoff on the actual SSA. We compute an individual's age at school entry using the information on the primary school entry date and birthdate.[29] Third, given that we also observe an individual's state of school enrollment, we use the NEPS data to assess the magnitude of the measurement error in our proxy for the state of schooling in the social security records (see Appendix B.4).

As with the SIAB data, we restrict the NEPS sample to the 1945–1965 birth cohorts and focus on individuals born and enrolled in school in West German states (excl. Berlin). Table B.1 shows the descriptive statistics. Our analytical sample includes 6,621 individuals and is gender balanced. The respondents are approximately 52 years old at the time of their first interview. On average, they started school at the age of 6.4. Immediately after elementary school, about 15 percent attend the academic track (slightly more men than women), which is considerably lower than the figure in the SIAB data. This is not implausible, however, since the SIAB data measure the highest level of education attained throughout the life course and not the initial placement in the secondary track.

[29]The computed variable initially contained some implausibly small (incl. negative) and significant values, likely due to measurement error from self-reporting. To deal with the outliers, we exclude observations with SSA values below the 1st and above the 99th percentile. Our results remain virtually identical when we undo this data-cleaning step.

Table B.1: Descriptive statistics of the NEPS sample

	All		Males		Females	
Variable	Mean	SD	Mean	SD	Mean	SD
Individual characteristics:						
Female	0.502	0.500	0.000	0.000	1.000	0.000
Year of birth	1956.552	5.792	1956.296	5.864	1956.806	5.709
Month of birth	6.364	3.431	6.354	3.419	6.374	3.443
Age at first interview	52.367	6.581	52.697	6.656	52.039	6.490
First interview:						
– 2007	0.359	0.48	0.342	0.475	0.376	0.484
– 2009	0.356	0.479	0.355	0.479	0.357	0.479
– 2010 (refreshment)	0.285	0.451	0.303	0.460	0.267	0.443
Federal State:						
– Schleswig-Holstein	0.039	0.194	0.038	0.190	0.041	0.198
– Hamburg	0.025	0.155	0.024	0.153	0.026	0.158
– Lower Saxony	0.139	0.346	0.149	0.356	0.130	0.337
– Bremen	0.013	0.114	0.013	0.112	0.014	0.116
– North Rhine-Westphalia	0.295	0.456	0.288	0.453	0.301	0.459
– Hesse	0.081	0.273	0.080	0.272	0.081	0.273
– Rhineland-Palatinate	0.07	0.255	0.072	0.259	0.067	0.251
– Baden-Württemberg	0.145	0.352	0.138	0.345	0.153	0.360
– Bavaria	0.170	0.376	0.174	0.379	0.166	0.373
– Saarland	0.023	0.149	0.025	0.156	0.020	0.142
School start and educational outcomes:						
School starting age (SSA)	6.427	0.540	6.439	0.555	6.416	0.525
Born after the cutoff	0.507	0.500	0.497	0.500	0.517	0.500
Compliance: Actual = expected entry yr	0.715	0.452	0.704	0.456	0.725	0.446
Calendar month of the cutoff	6.159	2.870	6.091	2.826	6.227	2.912
Expected year of school entry	1963.103	5.777	1962.840	5.852	1963.365	5.691
Parental characteristics:						
Maternal age at birth	28.378	6.163	28.394	6.241	28.362	6.086
Paternal age at birth	31.688	7.247	31.721	7.290	31.656	7.206
Parental years of education (max)	12.483	2.217	12.475	2.190	12.492	2.245
Parental educ.:						
– basic or less	0.661	0.474	0.672	0.469	0.649	0.477
– middle	0.145	0.352	0.141	0.348	0.150	0.357
– high school	0.156	0.363	0.150	0.357	0.162	0.368
– other/miss	0.038	0.191	0.037	0.188	0.039	0.195
German-born parent(s)	0.940	0.238	0.943	0.233	0.937	0.243
No. of older siblings	1.085	2.049	1.028	1.888	1.143	2.198
State of schooling=state of first job	0.852	0.355	0.848	0.359	0.856	0.351
Nine yrs. of compulsory schooling	0.791	0.407	0.772	0.419	0.809	0.393
Exposed to short school yrs	0.301	0.459	0.289	0.453	0.313	0.464
Observations/Individuals	6,621		3,299		3,322	

B.2 Balancing tests

A potential threat to the validity of our RD design would be if children born before and after the administrative cutoff for school enrollment differed on predetermined characteristics such as gender, parental place of birth, age at birth, and the number of older siblings. Such differences could arise if some parents systematically timed their births according to the school enrollment cutoff. Ideally, we would like to examine whether predetermined covariates are correlated with the probability of being born after the cutoff using our main sample from the social security records. However, except for gender, the SIAB data do not contain any predetermined characteristics that we would use for this purpose. Therefore, we mainly use the NEPS data to perform the balancing tests.

The results are shown in Table B.2. In columns 1 to 6, we regress the *After* dummy on individual background characteristics. Columns 1 and 2 show that neither a child's gender nor its family background can predict whether a child was born after the cutoff; all estimates are small and statistically insignificant (individually and jointly in an F-test). In column 3, we show that this conclusion holds when we include cohort fixed effects (as in our main specification). In columns 4 to 6, we additionally control for state-specific effects, fixed effects for the calendar months of the cutoff, and potential exposure to educational reforms such as the extension of compulsory schooling and shortened school years. These tests suggest that the sample is balanced across the cutoff.

This picture is supported by the set of regression results in the last column, where we separately regress each covariate on the *After* dummy using our main model specification (see equation 1). Thus, each estimate tests for a bivariate relationship between a given characteristic and the *After* dummy. Again, none of the coefficients is significant.

A potential concern is that these balancing tests may not be informative because the representative sample in the NEPS does not exactly match the population sampled in the SIAB data, which we use for our main analysis. As mentioned in Section 3, civil servants and the self-employed are not subject to social security and, thus, not included in our main estimation sample. To test for potentially endogenous selection into the SIAB data, we use the information on the linkage success of NEPS respondents with their social security records within a linked version of the NEPS data with social security records (NEPS-SC6-ADIAB).[30] Note that in Germany, any data linkage is legally permitted only with an individual's consent. Among the 6,321 respondents in our NEPS estimation sample, about 83% gave consent for linkage, and 84% of those could be linked to social security records (i.e., 70% of our entire NEPS sample).

[30]Details on the linked data are provided in Bachbauer and Wolf (2022). Although we initially considered using the linked data for this paper, the small sample size prevented any reliable RD analysis for labor market outcomes.

Table B.2: Balancing test on predetermined characteristics in the NEPS sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: *After* (Coeff. / SE / p-value)						Bivariate correlation
Female	0.004	0.004	0.004	0.003	0.004	0.004	0.017
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.024)
	[0.485]	[0.493]	[0.523]	[0.587]	[0.534]	[0.533]	[0.497]
Parental education:							
– Basic or less	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	-0.020
							(0.023)
							[0.385]
– Middle	0.000	0.000	0.001	-0.002	0.000	-0.001	-0.003
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.017)
	[0.971]	[0.968]	[0.955]	[0.830]	[0.988]	[0.911]	[0.863]
– High school	0.014	0.014	0.013	0.013	0.013	0.012	0.029
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.018)
	[0.119]	[0.124]	[0.134]	[0.161]	[0.136]	[0.181]	[0.111]
– Other/missing	-0.008	-0.002	0.000	-0.001	-0.001	0.001	-0.006
	(0.017)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.010)
	[0.624]	[0.930]	[0.987]	[0.970]	[0.956]	[0.970]	[0.558]
German-born parent (s)		-0.008	-0.008	-0.005	-0.008	-0.006	-0.006
		(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.011)
		[0.553]	[0.555]	[0.710]	[0.518]	[0.618]	[0.590]
Maternal age at birth		0.000	0.000	0.000	0.000	0.000	-0.033
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.302)
		[0.763]	[0.741]	[0.759]	[0.732]	[0.768]	[0.913]
Paternal age at birth		0.000	0.000	0.000	0.000	0.000	0.062
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.363)
		[0.824]	[0.834]	[0.836]	[0.820]	[0.743]	[0.865]
No. of older siblings		0.000	0.000	0.000	0.000	0.000	-0.009
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.099)
		[0.991]	[0.976]	[0.981]	[0.974]	[0.897]	[0.925]
F-Statistic	0.829	0.414	0.385	0.334	0.392	0.334	
p-value	0.506	0.913	0.929	0.953	0.925	0.953	
Obs./Ind.				6,621			
Cohort FE			yes	yes	yes	yes	yes
State FE				yes			
Cutoff month FE					yes		
Educ. reforms						yes	

Notes: The results in columns (1) through (6) are from linear regressions of the *After* dummy on individual background characteristics. Each estimate in column (7) comes from a separate regression of the covariate reported in each row on the *After* dummy. All regressions include linear trends in the running variable on both sides of the cutoff. Robust standard errors are reported in parentheses, and the corresponding p-value is in square brackets. The F-statistics (and the associated p-values) are from separate tests of the joint significance of the covariates reported in each column. Stars indicate statistical significance at the 1% (***), 5% (**), and 10% (*) levels. FE = fixed effects. Controls for educational reforms include indicators for the exposure to compulsory schooling extensions and short school years. Source: NEPS-SC6 v11.1.0

Importantly, however, we find no evidence of endogenous sample selection when we regress the linkage consent and success indicators on the treatment dummy within our main model specification. Table reports the results B.3. The point estimates on the *After* dummy are small and statistically insignificant. We complement this evidence by performing similar balancing tests for the female dummy using our main sample from the SIAB data. The results are presented in Table B.4. As with the NEPS data, we do find evidence that this characteristic is imbalanced around the cutoff in our main estimation sample. Furthermore, the point estimates are even closer to zero and more precisely estimated.

While the limited sample size in the NEPS data generally precludes strong conclusions, we believe that overall, the validity tests presented in this Appendix mitigate concerns that a potential imbalance in predetermined characteristics across the cutoff or endogenous sample selection could threaten the internal validity of our main results.

Table B.3: Tests for endogenous selection of NEPS respondents into social security records

	(1)	(2)	(3)
		Dependent variable:	
	Linkage consent in full sample	Linkage success in full sample	Linkage success conditional on consent
After	0.010	0.007	-0.003
	(0.019)	(0.022)	(0.019)
p-value	0.610	0.767	0.877
Mean dependent	0.833	0.689	0.844
Obs./Ind.	6,621	6,621	5,402

Notes: Each estimate comes from a separate regression of the dependent variable reported in each column on the *After* dummy. All regressions include linear trends in the running variable on both sides of the cutoff and cohort fixed effects. Robust standard errors are reported in parentheses. Source: NEPS-SC6 v11.1.0

Table B.4: Balancing test for the female indicator in the SIAB sample

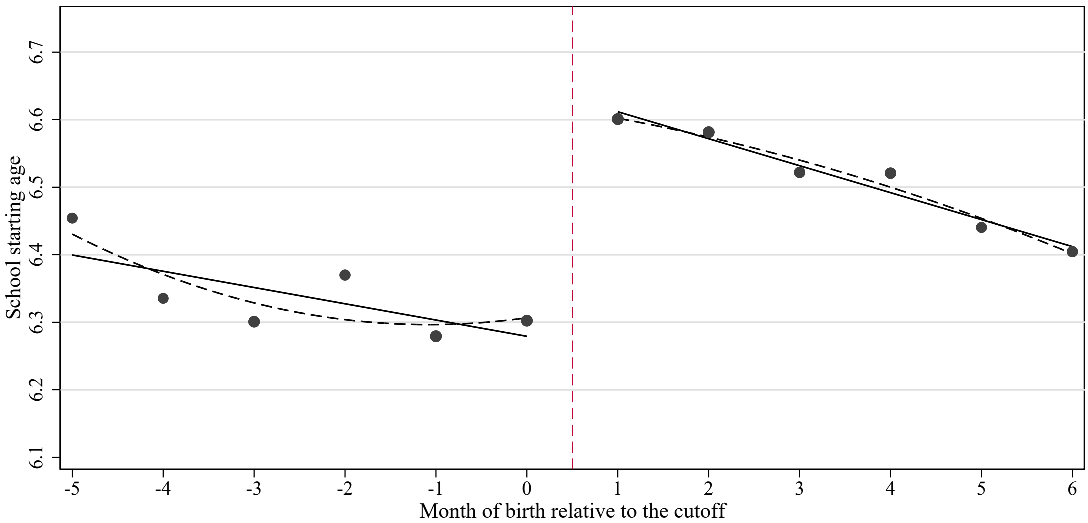
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: *After* (Coeff. / SE / p-value)					Bivariate correlation
Female	0.001	0.001	0.001	0.001	0.001	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
	[0.524]	[0.478]	[0.483]	[0.467]	[0.424]	[0.478]
F-Statistic	0.406	0.503	0.493	0.528	0.640	
p-value	0.524	0.478	0.483	0.467	0.424	
Obs./Ind.			306,145			
Cohort FE		yes	yes	yes	yes	yes
State FE			yes			
Cutoff month FE				yes		
Educ. reforms					yes	

Notes: The results in columns (1) through (6) are from linear regressions of the *After* dummy on individual background characteristics. Each estimate in column (7) comes from a separate regression of the covariate reported in each row on the *After* dummy. All regressions include linear trends in the running variable on both sides of the cutoff. Robust standard errors are reported in parentheses, and the corresponding p-value is in square brackets. The F-statistics (and the associated p-values) are from separate tests of the joint significance of the covariates reported in each column. Stars indicate statistical significance at the 1% (***), 5% (**), and 10% (*) levels. FE = fixed effects. Controls for educational reforms include indicators for the exposure to compulsory schooling extensions and short school years. Source: SIAB 1975-2019

B.3 Compliance with the cutoff: the first-stage relationship

We begin the analysis of the relationship between the administrative school entry cutoff and school starting age (SSA) with a graphical inspection; Figure B.1 plots the evolution of SSA along the running variable m_{ics}. The dots show the sample means at the respective m_{ics} value. The solid lines depict linear trends, and the dashed lines are second-order polynomials fitted separately on each cutoff side. As expected, there is a smooth (nearly linear) downward trend in school starting age along all values of m_{ics} except for the substantial jump at the cutoff. This discontinuity suggests that individuals born just after the cutoff enter school when they are nearly 0.4 years older than those born just before.[31]

Figure B.1: School-entry cutoff and school starting age



Notes: Each dot shows the average school starting age for individuals born in the corresponding month from five months before to six months after the cohort and federal-state-specific school entry cutoff (indicated by the dashed vertical line). The month of birth on the x-axis is normalized to take the value zero for the last birth month before the cutoff and the value one for the first month after the cutoff. Hence, individuals to the right (left) of the cutoff more often belong to the oldest (youngest) students in the class. The solid and dashed black lines represent linear and quadratic fits of the points separately for each side of the cutoff. The Figures is based on West German cohorts born between 1945 and 1965.

In Table B.5, we estimate the first-stage effect using various specifications of the model in equation 1. The dependent variable is the actual SSA, and all regressions include a linear trend in the running variable, which allows the slope to differ on either side of the cutoff. Column 1 shows the estimated coefficient associated with the indicator for being born after the cutoff date from our main specification, which includes birth cohort fixed effects. The point estimate in Panel A implies that being born after the cutoff increases the actual SSA, on average, by 0.375 years. This confirms the graphical evidence in Figure B.1 and is nearly identical to earlier findings for Germany, albeit mostly from samples including

[31] In the case of full compliance, we would expect students born just after the cutoff to be exactly one year older at school entry than those born before. However, compliance is typically lower in the birth months surrounding the cutoff, so the discontinuity is locally less than one.

more recent birth cohorts (e.g., Puhani and Weber, 2008; Mühlenweg and Puhani, 2010; Görlitz et al., 2022).[32]

Table B.5: First-stage effect of being born after the cutoff on school starting age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All (Obs./Ind. 6,621)							
After	0.375***	0.373***	0.377***	0.370***	0.375***	0.377***	0.374***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Panel B: Men (Obs./Ind. 3,299)							
After	0.382***	0.380***	0.383***	0.375***	0.382***	0.381***	0.382***
	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)
Panel C: Women (Obs./Ind. 3,322)							
After	0.367***	0.366***	0.369***	0.361***	0.365***	0.371***	0.364***
	(0.039)	(0.039)	(0.039)	(0.038)	(0.038)	(0.038)	(0.039)
Panel D: All, interacted with gender (Obs./Ind. 6,621)							
After	0.376***	0.372***	0.377***	0.371***	0.376***	0.378***	0.374***
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
After×female	0.000	0.002	0.001	0.000	−0.001	0.000	0.000
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
Cohort FE	yes		yes	yes	yes	yes	yes
Gender & age			yes				
State FE				yes			
Cutoff month FE					yes		
Family controls						yes	
Educ. reforms							yes

Notes: The dependent variable is school starting age calculated as the difference between the enrollment date and the birth date, both expressed as year and month. All regressions include linear trends in the running variable on both sides of the cutoff. Age control in column 3 enters as a linear term. Family controls in column 6 comprise an indicator for at least one foreign-born parent, parents' age at birth, the highest parental education (dummies), the number of older siblings, and indicators for missing information on family background characteristics. All specifications in Panel D include a female dummy. Robust standard errors are in parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively. FE = fixed effects. Source: NEPS-SC6 v11.1.0.

Reassuringly, the results remain remarkably stable when we drop the cohort fixed effects (column 2) or include additional control variables such as gender and age at the first interview (column 3) or state fixed effects (column 4). Given that there were different cutoff dates during the period under study, in column 5, we include fixed effects for the calendar month of the cutoff. In column 6, we control for family background characteristics such as parental education, age at birth, and the number of older siblings. Finally, the last column reports the results when we control for educational reforms such as the extension of compulsory schooling and short school years. The different specifications yield almost identical point estimates. Although not shown, we obtain similar results from other standard sensitivity analyses, including specifications with more flexible functions in the running variable and a donut-hole type of regression.

[32]A similar discontinuity has also been found in Dutch data (0.41, see Oosterbeek et al., 2021). Studies from Norway and Sweden typically estimate a much higher first-stage effect (of about 0.8, see, e.g., Black et al., 2011; Fredriksson and Öckert, 2014) while the average compliance in Denmark seems to be much lower (of about 0.2, see, e.g., Landersø et al., 2017).

In Panels B and C, we split the estimates by gender, which does not lead to substantial differences in the first-stage effect. In Panel D, we use the pooled sample but interact the after dummy with gender. Consistent with the gender-specific results, we find no significant differences in the first-stage effect between men and women. Again, the point estimates are very stable across the columns, which strongly suggests compliance with the cutoff rules is not systematically correlated with background characteristics.

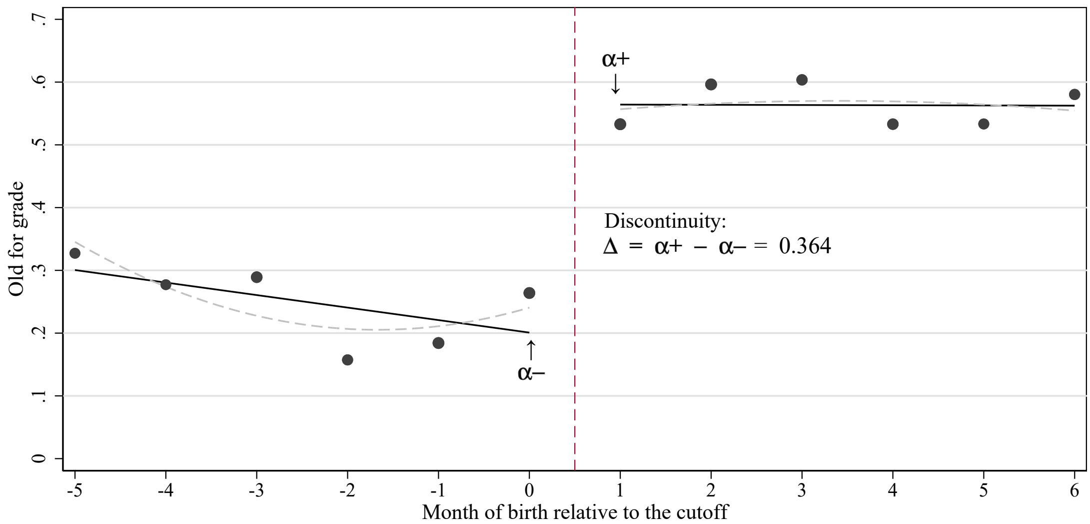
Beyond similar first-stage effects for men and women, similar proportions of compliers and non-compliers across genders are also important for interpreting our gender-specific results for labor market outcomes. Hollenbach et al. (2026) illustrate this issue in the context of gene-environment interactions, demonstrating that a homogeneous first stage is necessary. Applying this argument to our context, we want to compare the intention-to-treat effects while holding the unobserved heterogeneity between genders fixed. Pure descriptively, on average, similar proportions of men and women were enrolled according to the cutoff in our NEPS sample (70% and 72%, respectively). However, the proportions of always-takers and never-takers should also be similar, and this should particularly apply to the local shares at the cutoff.

To assess the local shares of compliers and non-compliers in our setting, we use a binary treatment indicator of school entry in the year of an individual's seventh birthday (as opposed to the sixth birthday) instead of the continuous school starting age. This binary indicator is often referred to in the literature as "old for grade" (e.g., Dhuey et al. (2019)). Figure B.2 and Table B.6 show that the alternative first-stage outcome largely confirms the results for school starting age.[33] Nevertheless, using the binary indicator as the outcome of a first-stage regression reveals the proportion of always-takers and compliers. These correspond to the point estimates for the intercept and the *After* dummy, respectively.

To simplify the discussion, we focus on the unconditional RDD regressions in column 2 of Table B.6, which align with the descriptive evidence in Figure B.2. The full-sample intercept (i.e., the left intercept in the RDD setting) of 0.201 in Panel A corresponds to the local share of always-takers, which we denote as α^- in the following. The RRD estimate of 0.364 on the *After* dummy maps to the local share of compliers, which we denote as $\Delta = \alpha^+ - \alpha^-$, with α^+ being the right intercept. By definition, $\alpha^+ = \alpha^- + \Delta$, i.e., 0.565 in our setting. Thus, under the standard assumption of no defiers, the estimated share of never-takers is $1 - \alpha^+$, i.e., 0.435. We summarize these group-specific shares in column 1 of Table B.7. The remaining columns present the local shares of always-takers, compliers, and never-takers among men and women. These are derived from the corresponding gender-specific regressions and the interacted model specification in Table B.6 (Panel B to

[33] The discontinuity at the cutoff in Figure B.2 implies that being born after the cutoff increases the probability of being "old for grade" at school enrollment by nearly 40 percentage points. This is confirmed by the regression results in Table B.6. Similar to Table B.5, the point estimate on the *After* dummy is very similar across model specifications and gender.

Figure B.2: School-entry cutoff and probability of being ”old for grade”



Notes: ”Old for grade” is a binary indicator for school entry in the year of a child’s seventh birthday (as opposed to the sixth birthday). Each dot shows the average proportion of individuals classified as ”old for grade” among all individuals born in the corresponding month from five months before to six months after the cohort and federal-state-specific school entry cutoff (indicated by the dashed vertical line). The month of birth on the x-axis is normalized to take the value zero for the last birth month before the cutoff and the value one for the first month after the cutoff. The solid and dashed black lines represent linear and quadratic fits of the points separately for each side of the cutoff. The left intercept (α^-) and the discontinuity (Δ) map the estimation results from column 2 of Panel A of Table B.6. The regression includes linear trends in the normalized month of birth on both sides of the cutoff. The Figure is based on West German cohorts born between 1945 and 1965.

D, column 2). The shares are very similar across genders, and any small differences are not statistically significant because the respective confidence intervals overlap.

Overall, we find that the first-stage effects and the proportions of (non-)compliers are very similar for men and women. Thus, any differences in labor market responses to the cutoff rule cannot be attributed to gender differences in (non-)compliance. In general, the fist-stage estimate of approximately 0.37 from Table B.5 implies that one should multiply our reduced-form estimates from the social security records by a factor of 2.7 to interpret them as causal effects of starting school one year later.

Table B.6: First-stage effect of being born after the cutoff on being "old for grade"

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All (Obs./Ind. 6,621)							
After	0.368***	0.364***	0.368***	0.363***	0.367***	0.367***	0.361***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Intercept	0.161***	0.201***	0.666***	0.243***	0.146***	0.208***	0.146***
	(0.045)	(0.013)	(0.240)	(0.052)	(0.047)	(0.060)	(0.045)
Panel B: Men (Obs./Ind. 3,299)							
After	0.390***	0.390***	0.391***	0.382***	0.385***	0.391***	0.384***
	(0.032)	(0.032)	(0.032)	(0.033)	(0.033)	(0.032)	(0.033)
Intercept	0.201***	0.196***	0.760**	0.25***	0.18***	0.247***	0.191***
	(0.059)	(0.018)	(0.342)	(0.072)	(0.063)	(0.082)	(0.058)
Panel C: Women (Obs./Ind. 3,322)							
After	0.345***	0.337***	0.344***	0.343***	0.347***	0.347***	0.339***
	(0.032)	(0.033)	(0.032)	(0.033)	(0.033)	(0.033)	(0.033)
Intercept	0.091	0.206***	0.504***	0.205***	0.081	0.147*	0.069
	(0.068)	(0.018)	(0.338)	(0.075)	(0.071)	(0.088)	(0.067)
Panel D: All, interacted with gender (Obs./Ind. 6,621)							
After	0.372***	0.365***	0.372***	0.366***	0.370***	0.372***	0.365***
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
After×female	−0.008	−0.003	−0.009	−0.006	−0.006	−0.009	−0.007
	(0.023)	(0.023)	(0.023)	(0.022)	(0.022)	(0.023)	(0.023)
female	−0.001	−0.006	−0.002	−0.006	−0.003	−0.001	−0.003
	(0.015)	(0.015)	(0.015)	(0.014)	(0.015)	(0.015)	(0.015)
Intercept	0.161***	0.204***	0.665***	0.245***	0.147***	0.207***	0.147***
	(0.045)	(0.015)	(0.052)	(0.240)	(0.047)	(0.060)	(0.045)
Cohort FE	yes		yes	yes	yes	yes	yes
Gender & age			yes				
State FE				yes			
Cutoff month FE					yes		
Family controls						yes	
Educ. reforms							yes

Notes: The dependent variable is "old for grade", which is a binary indicator for school entry in the year of a child's seventh birthday (as opposed to the sixth birthday). All regressions include linear trends in the running variable on both sides of the cutoff. Age control in column 3 enters as a linear term. Family controls in column 6 comprise an indicator for at least one foreign-born parent, parents' age at birth, the highest parental education (dummies), the number of older siblings, and indicators for missing information on family background characteristics. All specifications in Panel D include a female dummy. Robust standard errors are in parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively. FE = fixed effects. Source: NEPS-SC6 v11.1.0.

Table B.7: Local shares of compliers and non-compliers at the cutoff

	(1)	(2)	(3)	(4)	(5)
	Full sample	Gender-specific samples		Interacted model	
	All	Men	Women	Men	Women
Share of always-takers: α^-	0.201	0.196	0.206	0.204	0.198
	[0.176; 0.226]	[0.161; 0.231]	[0.170; 0.242]	[0.175; 0.233]	[0.169; 0.227]
Share of compliers: $\Delta = \alpha^+ - \alpha^-$	0.364	0.390	0.337	0.365	0.362
	[0.527; 0.602]	[0.533; 0.639]	[0.490; 0.596]	[0.528; 0.610]	[0.519; 0.601]
Share of always-takers & compliers: α^+	0.565	0.586	0.543	0.569	0.560
	[0.319; 0.409]	[0.326; 0.453]	[0.273; 0.402]	[0.315; 0.415]	[0.312; 0.413]
Share of never-takers: $1 - \alpha^+$	0.435	0.414	0.457	0.431	0.440
	[0.398; 0.473]	[0.361; 0.467]	[0.404; 0.510]	[0.39; 0.472]	[0.399; 0.481]

Notes: The shares are based on estimation results from column 2 of Table B.6. All regressions include linear trends in the running variable on both sides of the cutoff. The 95% confidence intervals in square brackets are based on robust standard errors. The standard errors for the constructed quantities (α^+ and $1 - \alpha^+$) are calculated using the delta method. Source: NEPS-SC6 v11.1.0.

B.4 Measurement error due to regional mobility

Since the German social security records do not contain information on where an individual went to school, in our main analysis, we use the first state of residence ever observed for a given individual in his or her labor market biography as a proxy for state of schooling. This introduces a measurement error in the treatment variable. In this Appendix, we provide evidence on the extent of the resulting measurement error and its potential threat to the internal validity of our main results. We do so by using the NEPS, which includes self-reported information on the state of schooling and the state of residence later in life.

Specifically, all NEPS respondents provide retrospective information on their employment histories, including the location of their jobs, upon entering the sample. This unique feature of the data allows us to study the match between an individual's state of schooling and state of residence at labor market entry. The latter is similar to the regional proxy we use in the SIAB for our main analysis. The NEPS data reveal that 85% of individuals from the analyzed cohorts started their careers in the same state where they entered primary school. Thus, the first state ever observed for a given individual in the social security records is potentially a good proxy for the state of schooling.

Although limited, the measurement error in the treatment assignment could be problematic if birth after the cutoff affects cross-state mobility later in life. Table B.8 below examines this issue using the same estimation approach as in our main analysis. The dependent variable is a dummy variable indicating that an individual entered the labor market at the primary school entry. Similar to the first-stage analysis in Appendix B.3, we estimate various model specifications, starting with our baseline model in column 1. Most of the point estimates on *After* in Table B.8 are small in magnitude, and none is statistically significant.

However, we acknowledge that the results require some caution due to limited statistical power because of the small sample sizes. Nevertheless, our findings are consistent with Barabasch et al. (2025) who use the NEPS data to show that school entry cutoffs in Germany do not affect residential mobility across federal states, counties, and labor markets.

Overall, the supplementary evidence from the NEPS data suggests that individuals born before and after the cutoff do not differ in their mobility patterns upon labor market entry. This also holds across gender. Thus, if anything, our main results from social security records potentially suffer from an attenuation bias due to a measurement error in the treatment variable.

Table B.8: Effect of being born after the cutoff on regional mobility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: Indicator that state of labor market entry and birth coincide						
Panel A: All (Obs./Ind. 6,621)							
After	0.007	0.004	0.005	0.002	0.005	0.011	0.010
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Panel B: Men (Obs./Ind. 3,322)							
After	-0.002	-0.006	-0.002	-0.005	-0.004	0.002	0.003
	(0.025)	(0.025)	(0.025)	(0.024)	(0.024)	(0.024)	(0.024)
Panel C: Women (Obs./Ind. 3,322)							
After	0.017	0.015	0.016	0.011	0.016	0.021	0.018
	(0.025)	(0.024)	(0.025)	(0.024)	(0.024)	(0.024)	(0.024)
Panel D: All, interacted with gender (Obs./Ind. 6,621)							
After	0.008	0.005	0.006	0.004	0.006	0.011	0.011
	(0.019)	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)	(0.019)
After×female	-0.002	-0.002	-0.002	-0.004	-0.003	-0.001	-0.003
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Cohort FE	yes		yes	yes	yes	yes	yes
Gender & age			yes				
State FE				yes			
Cutoff month FE					yes		
Family controls						yes	
Educ. reforms							yes

Notes: All regressions include linear trends in the running variable on both sides of the cutoff. Age enters as a linear and quadratic term. Family controls comprise an indicator for at least one foreign-born parent, parents' age at birth, the highest parental education (dummies), the number of older siblings, and indicators for missing information on family background characteristics. All specifications in Panel D include a female dummy. Robust standard errors are in parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively. FE = fixed effects. Source: NEPS-SC6 v11.1.0.