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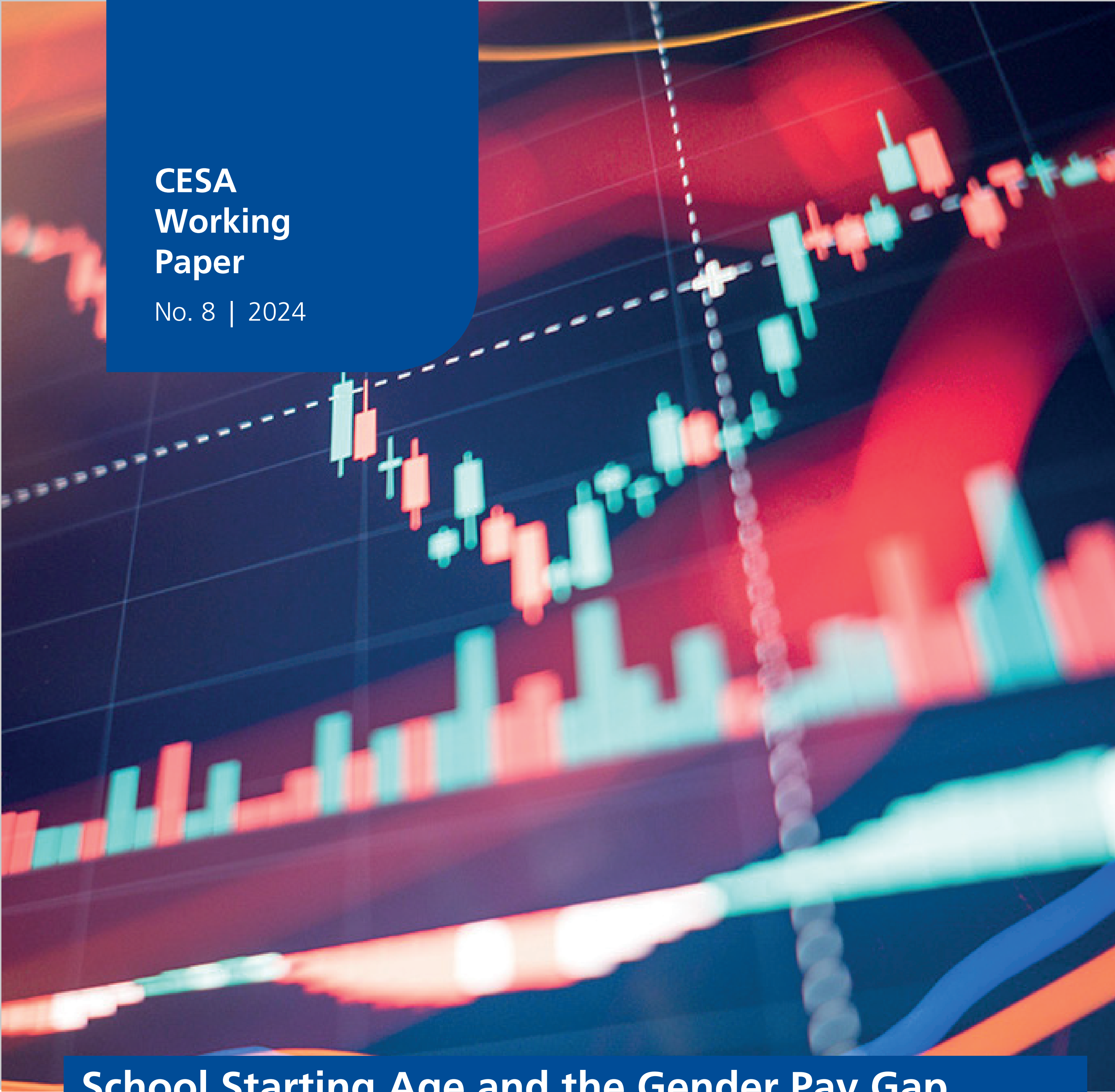

School Starting Age and the Gender Pay Gap over the Life Cycle

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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 and assessing fertility directly, delaying motherhood seems a plausible mechanism behind the results.

Keywords: School starting age, lifetime effects, education, gender gaps

JEL Classification: *I21, I24, I26*

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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).

[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 assess and explain 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 compared to Sweden at the time (e.g., much earlier tracking and limited opportunities for subsequent track changes),

We then extend the analysis beyond the results in FÖ by documenting the implications of SSA for the compression of the gender gap in earnings 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 (e.g., full-time employment, multiple job holding, job complexity, mobility across employers), and fertility decisions. Our estimates suggest that delaying first childbirth and the corresponding gain in experience at career stages when earnings trajectories are steep and returns to experience are relatively high (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 states are responsible for 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 strictly binding, as some states explicitly define exceptions for earlier enrollment.[4] Nevertheless, most parents follow the default rules, with the result 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

[3] Including indicators for the cutoff month does not affect our results (see Appendix Table A.2).

[4] Typically, children born in the first three months after the cutoff are eligible for early enrollment upon application (Kamb and Tamm, 2022; Goerlitz et al., 2024). Other exceptions, e.g., based on lack of intellectual or emotional maturity, are scarce because 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, 202X). Controlling for these policy changes does not change our results (see Appendix Table A.2).

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 from age 15 to 64, which covers the potential working life span in Germany.[9] 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). The payroll information on earnings is generally highly reliable.[10] 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. 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 individual-level data sets to the relevant cutoffs collected by Cygan-Rehm (202X) by using the information on year of birth, month of birth, and a proxy for the state of schooling.[11]

[8] The data cover approximately 80 percent of Germany's workforce because civil servants and the self-employed are not subject to social security.

[9] The time frame of the data (1975-2019) implies that individuals born in 1945 are observed at ages 30-65 and those born in 1965 at ages 15-54, resulting in an unbalanced panel. This is similar to FÖ, who observe individuals from the oldest cohort at ages 25-74 and those from the youngest cohorts at ages 15-54.

[10] Gross earnings are only reported up to the statutory social security contribution ceiling, which is relevant for calculating old-age pensions and unemployment benefits. Earnings above the ceiling are top-coded, which affects about 5 percent of all spells. We impute top-coded earnings using a common procedure suggested in Dauth and Eppelsheimer (2020). Our results change little when we use the top-coded values.

[11] There is no information on state of schooling (or birth) in the SIAB. Thus, we use the first state of residence ever observed for a given individual as a proxy. The resulting measurement error should be limited because interstate mobility was generally low for the cohorts studied. Using the NEPS, we find no significant effects of the cutoff rules on regional mobility for the cohorts under study (see Appendix B.4). Thus, if anything, the measurement error leads to an attenuation bias.

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 labor force participation is, on average, "only" 15 percent (9,418 versus 8,216). The coinciding high earnings gap and the lower participation gap suggest a much lower female labor supply at the 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 regression discontinuity (RD) design. 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. For completeness, we provide evidence on the first-stage effect of being born after the cutoff on SSA in Appendix B.3.[13] In the main analysis, we estimate the following reduced-form equation using the SIAB:

$$Y^a_{ics} = \beta^a \; After + f^a(m_{ics}) + \pi^a_c + x'_{ics}\delta^a + \varepsilon^a_{ics}, \tag{1}$$

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 running variable m_{ics}

[12]The vast majority of West German women in the generation studied here tended to work part-time, which is still common in the West German states and is not limited to mothers of small children. For example, for the 2000s, Dehos and Paul (2023) report that less than 15% (25%) of mothers worked at least 35 hours/week when their youngest child was 7-9 (12-15) years old.

[13]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.

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 m_{ics} runs from -5 to 6. In our preferred specification, we define $f(m_{ics})$ as a linear function of the running variable with different slopes on either side of the cutoff. We also use a quadratic specification in $f(m_{ics})$ for a robustness test.[14] The main regressor of interest is the indicator $\mathit{After} = \mathbb{1}\{m_{ics} > 0\}$. 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.[15]

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, we show in Appendix Figure A.4 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 suggested in Frandsen (2017). We also find that predetermined characteristics are balanced around the cutoff (see Appendix Table 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

[14]Appendix Figure A.3 plots the evolution of mean earnings at selected ages across m_{ics}. Each subfigure also shows linear and quadratic trends fitted separately to the raw data on either side of the cutoff. The trends are fairly linear, which justifies our choice of $f(m_{ics})$ in the main specification.

[15]We also apply alternative inference methods, such as clustering by the running variable and at the state level (given the state-specific cutoffs). Differences across the methods are small with robust standard errors being typically the most conservative method (see Apendix Table A.2).

increases at the cutoff by 0.375 years on average (significant at the 1% level). This is consistent with previous estimates for Germany for more recent birth cohorts (e.g., Puhani and Weber, 2008; Dustmann et al., 2017). The first-stage effects are almost identical for men and women. Thus, any differences in labor market responses to the cutoff rule cannot be attributed to gender differences in 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.[16] For ease of interpretation, we follow FÖ and normalize individual age-specific earnings by the respective mean of total prime-age earnings.

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.5). These patterns largely confirm the findings by FÖ for Sweden and also for 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 fact that there is no 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) reveals that the overall pattern of disappearing effects in the prime working ages is driven by men. Women born after the cutoff, however, experience by 2 to 2.5 percentage points higher earnings until the end of their careers. These effects are particularly pronounced between the ages of 25 and 30. Again, except for

[16]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.

the lack of substantial positive effects around retirement, our gender-specific estimates confirm the findings in FÖ, who documented positive long-run effects for women and virtually no effects for men. Although the life-cycle patterns are fairly similar across countries, the magnitude of the effects for women is much larger in Germany than in Sweden. We return to this issue in Section 6.

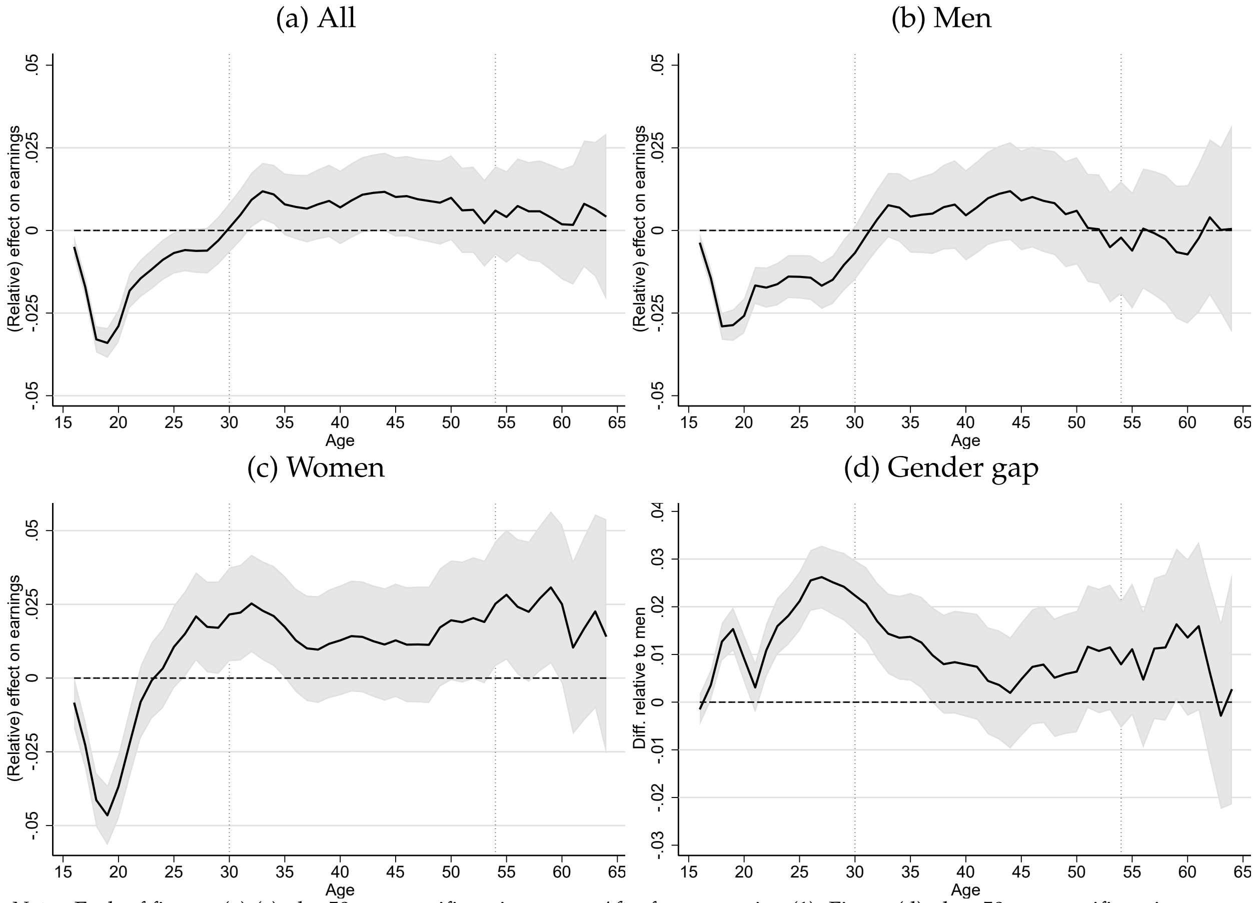
The gender-specific effects suggest further implications for the overall gender pay gap. To shed more light on this issue, we next extend the analysis beyond the results in FÖ. Specifically, in panel (d), we extend the model specification by interacting the *After* indicator with a female dummy and plot the age-specific estimates of this interaction term. The figure shows a substantial effect 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.5).

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 over the entire career (ages 15–64) and during the prime working years (ages 30–54). These effects are measured in percentage points relative to the 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 the foregone earnings due to a later entry into the labor market, as during the prime-age period, the earnings (column 2) and employment (column 4) effects are positive (though small and insignificant).

Panel B shows that men drive the overall zero effects.[17] 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 become more evident in Panel D, where we estimate an interacted model specification, which increases

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

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



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 (in panels b and c, also gender-specific) prime-age earnings (sum at ages 30–54).

precision. 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.[18]

There are several possible explanations for why a later school start may contribute to closing the gender gap in lifetime earnings. First, we consider disproportionate effects on educational attainment in the last four columns of Table 1. Similar to FÖ, we find a significant positive effect on years of schooling, but its magnitude is similar across genders. The effect on academic track completion (column 5) of about one percentage

[18]These results are remarkably robust to the inclusion of additional covariates, quadratic terms in the running variable, trimming the window in the running variable around the cutoff, and alternative inference procedures (see Appendix Table A.2).

Table 1: Lifetime effects of being born after the cutoff

	Earnings (effects in pp)		Employment (effects in pp)		Education			
						Degrees (effects in pp)		
	Ages 15-64 (1)	Ages 30-54 (2)	Ages 15-64 (3)	Ages 30-54 (4)	Years of schooling (5)	Academic track (6)	College/ University (7)	Vocational training (8)
Panel A: All								
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								
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								
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								
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 the parentheses. *, **, and *** statistical significance at the 10, 5, and 1 percent levels, respectively.

point is also comparable for men and women. The better tracking outcomes also appear to have persistent consequences for postsecondary education, with a significant increase in college completion rates (column 6) of 0.6 percentage points, driven by a shift away from vocational education (column 7). Since the shift toward better postsecondary credentials is less pronounced for women, higher returns to education cannot explain the narrowing of the gender wage gap induced by the school entry cutoff.

Second, as noted above, the substantial gender differences in initial earnings effects can largely be attributed to differences in early career labor supply (see Appendix Figure A.5). This pattern likely reflects a smaller loss of experience for women born after the cutoff compared to later-born men, who enjoy somewhat larger effects on postsecondary education (see Table 1), resulting in longer educational spells. While different employment responses cannot explain the differential earnings effects beyond the early 30s (see Appendix Figure A.5), it seems likely that women's earnings may benefit from the relatively higher returns to early-career experience when earnings trajectories are steep (e.g., Aryal et al., 2022), which carries over to benefit earnings in the long run.

Third, we consider the role of job and firm characteristics (see Appendix Table A.3). 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 larger shares of their working lives in jobs with more complex skill requirements (columns 5 and 6), which is 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.[19] Examining this channel (see Appendix Table A.4), we find no effects on the probability of becoming a mother or on the total number of children. However, our estimates imply a significant increase in the age at first birth by about a quarter of a year. This magnitude is remarkably close to the postponement of labor market entry for mothers born after the cutoff, suggesting a mechanical effect on fertility timing.[20] As motherhood is an essential 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. In particular, mothers typically lose experience at career stages when earnings trajectories are steep and returns to experience relatively high (Bhuller et al., 2017). Thus, due to the extended childless period, women born after the cutoff accumulate valuable labor market experience early in their careers, which appears to pay off in the long run.

[19]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).

[20]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.

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, 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, which emerges mostly until the mid-30s, is rather a consequence of postponing first childbirth and thus increased female labor supply in 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.[21] Interestingly, these cross-country differences arise despite remarkably similar effects of SSA on years of schooling.[22] Our results point to the delay in first birth as a potential mechanism behind the positive effect in Germany, which is consistent with motherhood being a key determinant of gender gaps in labor markets (e.g., Olivetti et al., 2024). The fertility channel may be particularly important 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

[21] 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.3. 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.

[22] 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.

potential explanations for cross-gender and cross-country differences in the impact of SSA on earnings.

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Online Appendix – not for publication

A Additional tables and figures

Table A.1: Descriptive statistics

	All		Men		Women	
Variable	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:						
Yearsof 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:	306,145		156,732		149,413	

Table A.2: Robustness checks for the interaction effect

	Earnings (effects in percentage points)		Employment (effects in percentage points)	
	Whole career (ages 15-64)	Prime working ages (30-54)	Whole career (15-64)	Prime working ages (30-54)
Main results (Obs. 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}
Panel A: Adding Fixed Effects for the cutoff month				
$After \times female$	1.808***	1.553**	0.738**	0.529
	(0.583)	(0.628)	(0.352)	(0.378)
Panel B: Adding federal state fixed effects				
$After \times female$	1.769***	1.515**	0.716**	0.509
	(0.582)	(0.627)	(0.351)	(0.378)
Panel C: Adding controls for short school years and compulsory schooling reform exposure				
$After \times female$	1.813***	1.556**	0.737**	0.527
	(0.583)	(0.628)	(0.352)	(0.379)
Panel D: Federal state by birth month fixed effects				
$After \times female$	1.773***	1.517**	0.717**	0.510
	(0.582)	(0.627)	(0.351)	(0.378)
Panel E: Adding federal state by cohort fixed effects				
$After \times female$	1.755***	1.499**	0.714**	0.514
	(0.582)	(0.627)	(0.351)	(0.378)
Panel F: Adding quadratic trends				
$After \times female$	1.816***	1.558**	0.740**	0.530
	(0.583)	(0.628)	(0.352)	(0.379)
Panel G: Donut specification: dropping $-/+$ 1 month around the cutoff (Obs. 251,989)				
$After \times female$	2.350***	2.131***	0.870**	0.693*
	(0.644)	(0.693)	(0.388)	(0.417)
Panel H: Narrowing the bandwidth to 5 months around the cutoff (Obs. 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.3: 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 (306,145 observations)										
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 (156,732 observations)										
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 (149,413 observations)										
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 (306,145 observations)										
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

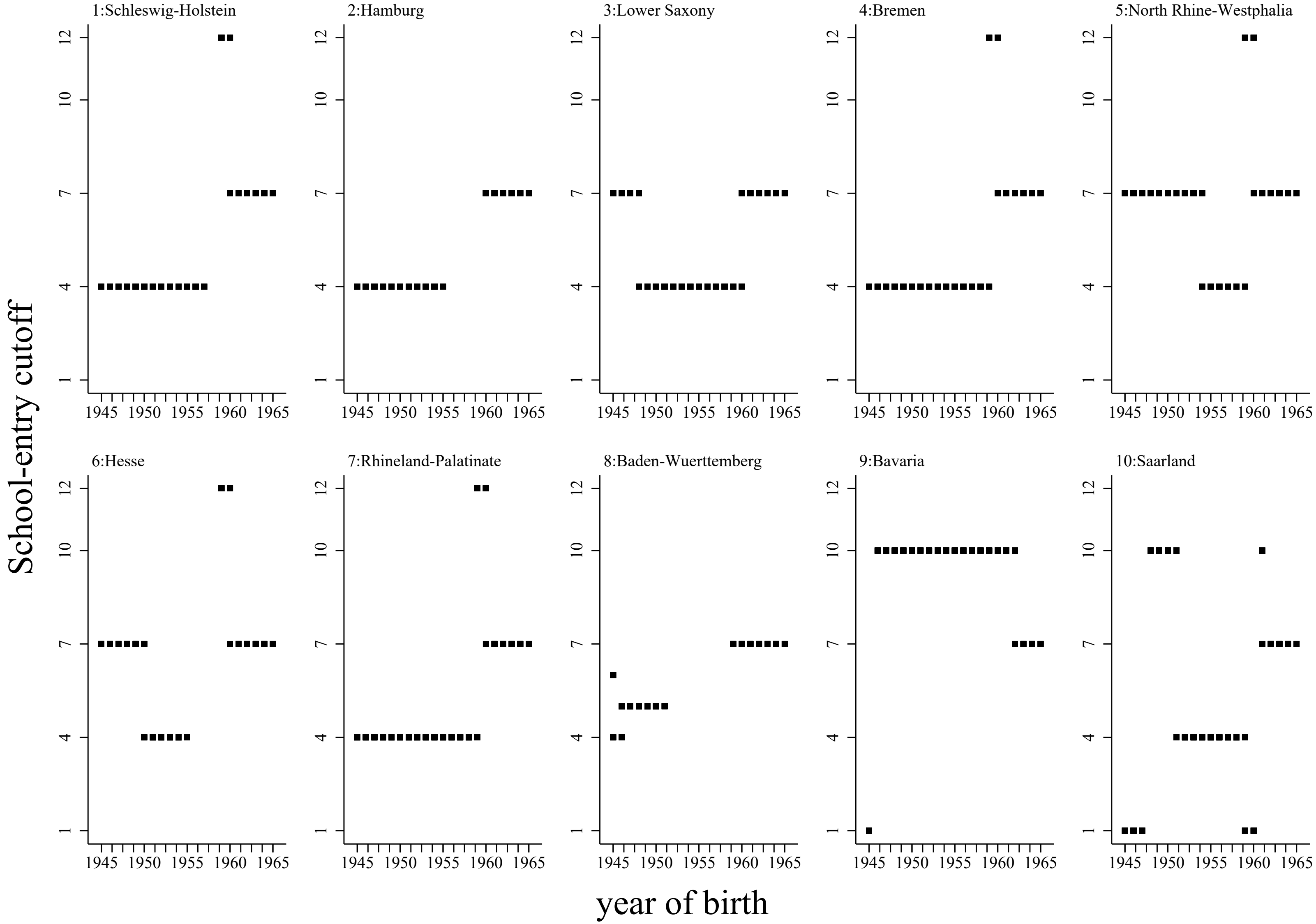
Notes: All reported coefficients are estimates of the discontinuity in the respective outcome (given in the column head) for the school-entry cutoff and/or its interaction. All specifications include cohort fixed effects if not stated differently. Robust standard errors are reported in the parentheses.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

Table A.4: Effects on fertility outcomes for women

	(1) Motherhood indicator (effects in pp)	(2) Number of children	(3) Age at first birth (cond. on motherhood)	(3) Age at labor (market entry)
$After$	-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
Observations:	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.

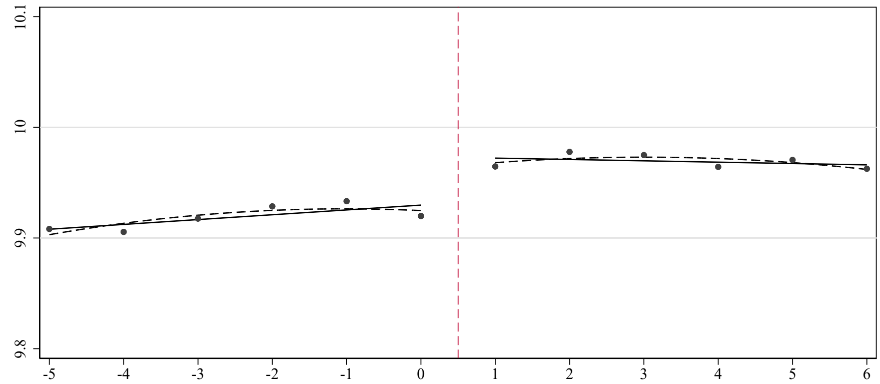
Figure A.1: The month of school entry by birth cohort and federal state



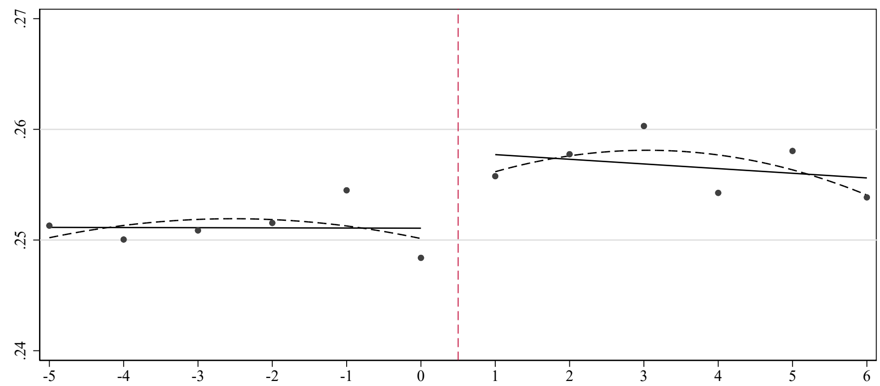
Notes: Based on data collected from primary sources. For details, see Cygan-Rehm (202X). The state-specific cutoff rules vary by birthdate (year and month).

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

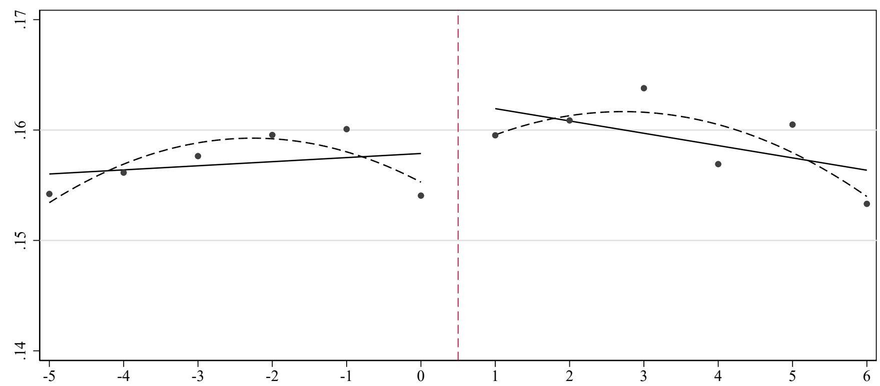
(a) Years of schooling



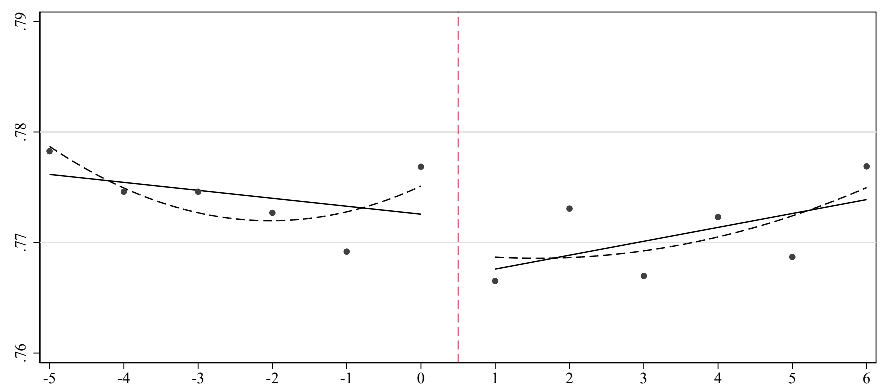
(b) Academic track certificate



(c) University education



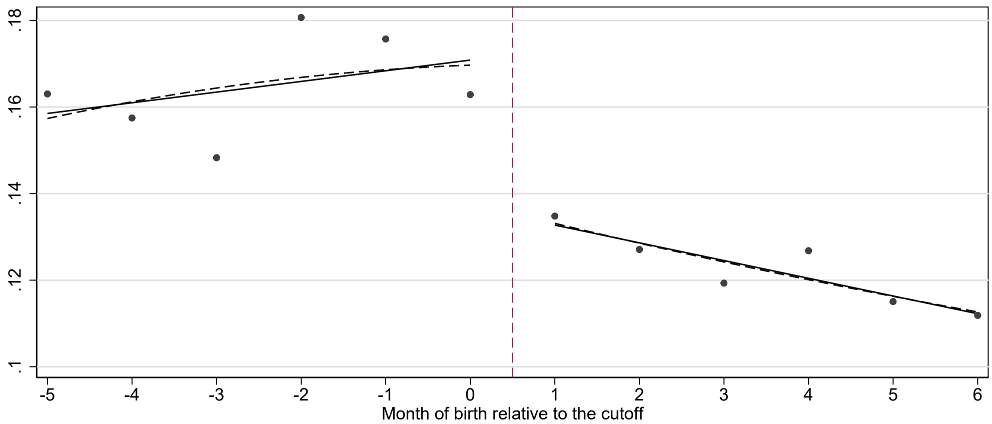
(d) Vocational training



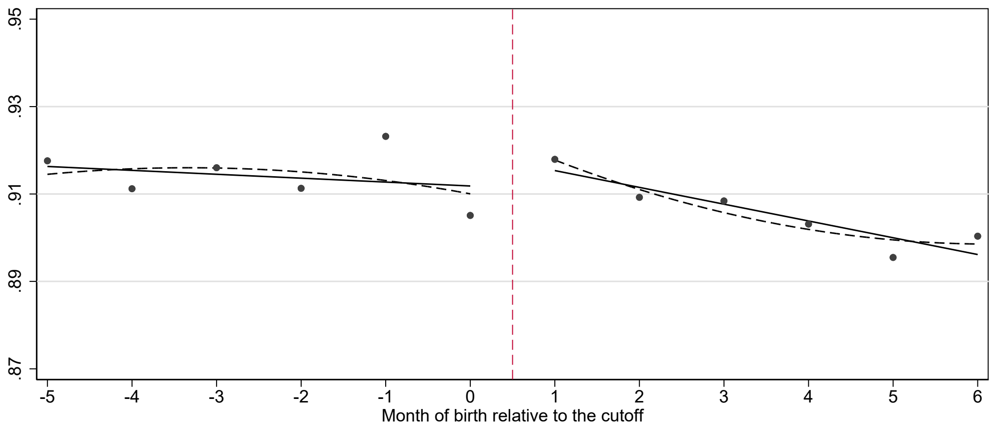
Notes: All panels show outcome trajectories for five months before and six months after the cohort and federal-state-specific school entry cutoff (indicated by the dashed vertical line). Hence, individuals to the left (right) of the cutoff more often belong to the oldest (youngest) students in the class. 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

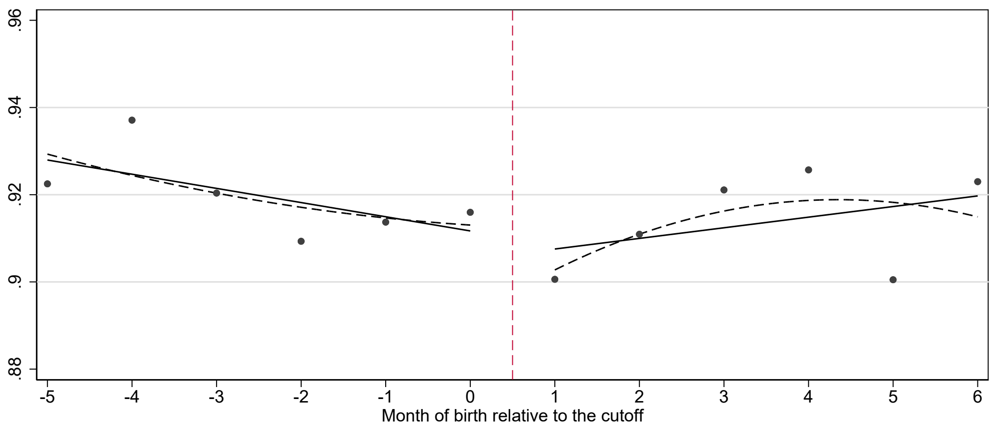
(a) at age 18



(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). 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

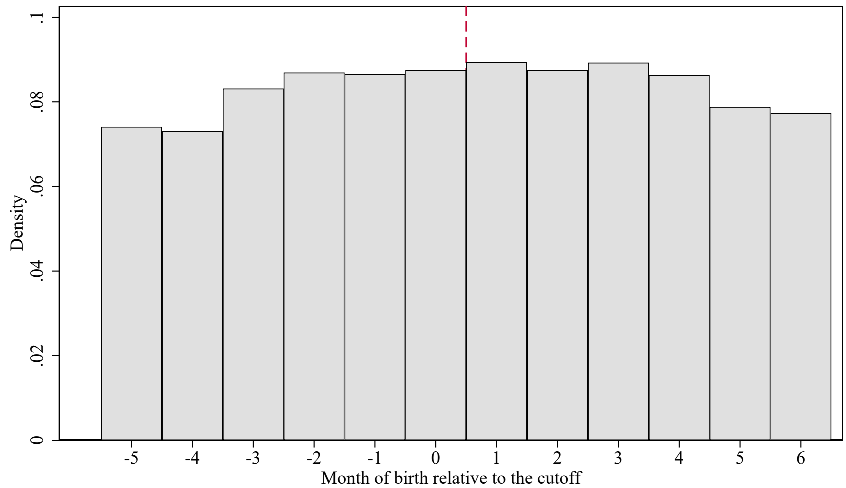
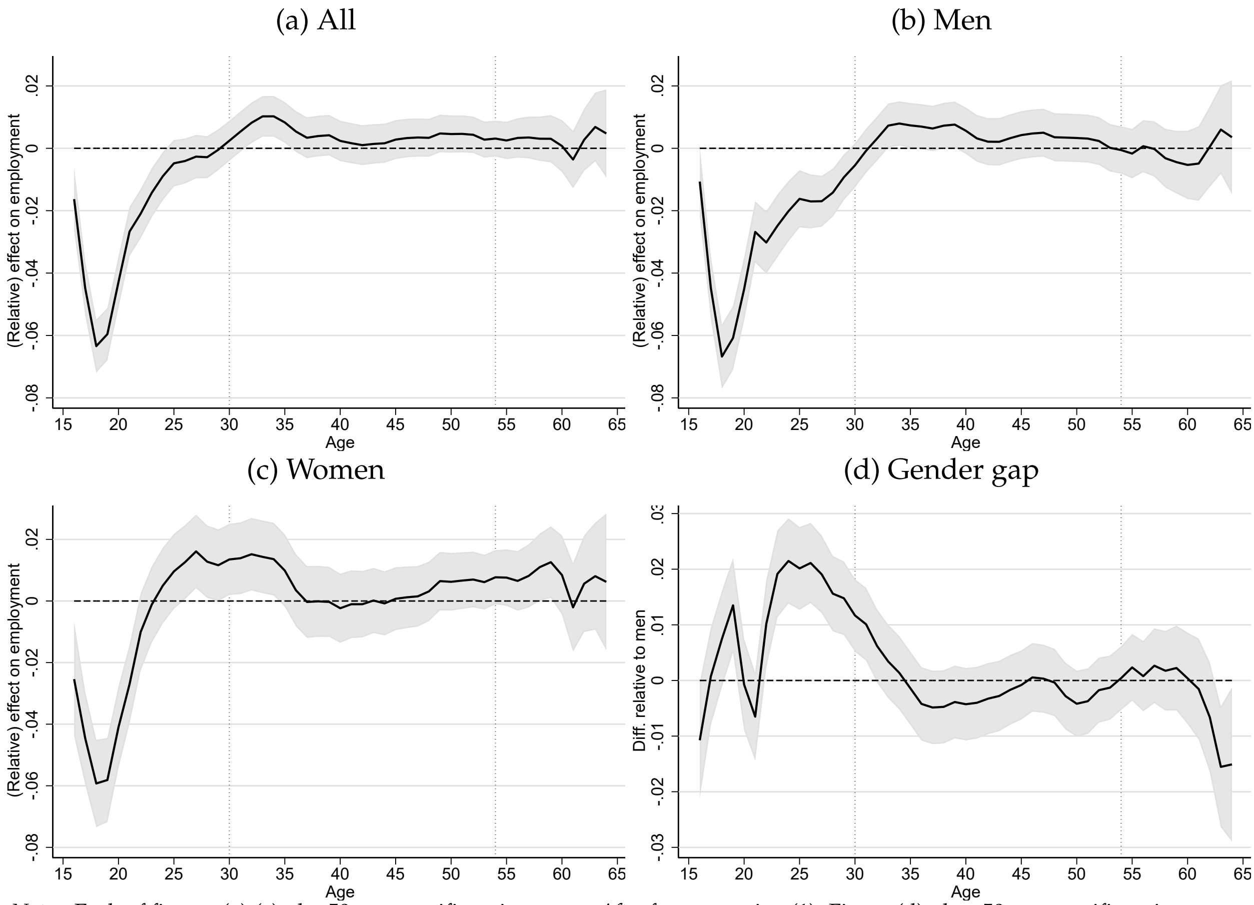


Figure A.5: Age-specific effects of being born after the cutoff on employment



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.[23] 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.

[23] 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

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. Since there is no information on family background in the social security records, in Table B.2, we use the NEPS data to examine whether predetermined covariates are correlated with the probability of being born after the cutoff.

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. We therefore conclude that these tests strongly suggest that the main background characteristics are balanced across the cutoff.

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.[24]

In Table B.3, 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

[24]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.

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.				6,621			
Cohort FE			yes	yes	yes	yes	yes
State FE				yes			
Cutoff month FE					yes		
Educ. reforms						yes	

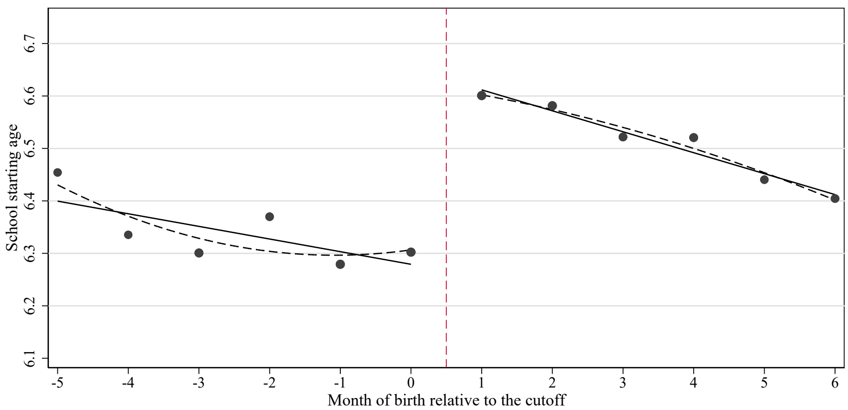
Notes: The results in columns (1) through (6) are from liner regressions of the *After* dummy on individual background characteristics. Each estimate in column (7) comes from a separate regression of the covariate reported in each row on the *After* dummy. All regressions include linear trends in the 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

is nearly identical to earlier findings for Germany, albeit mostly from samples including more recent birth cohorts (e.g., Puhani and Weber, 2008; Mühlenweg and Puhani, 2010; Görlitz et al., 2022).[25]

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.

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. In general, the estimate of approximately 0.37 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.

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



[25]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 fist-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).

Table B.3: First-stage effect of being born after the cutoff on school starting age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: School starting age						
Panel A: All (6,621 observations)							
After	0.375***	0.373***	0.372***	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 (3,299 observations)							
After	0.382***	0.380***	0.380***	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 (3,322 observations)							
After	0.367***	0.366***	0.361***	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 (6,621 observations)							
After	0.376***	0.372***	0.373***	0.371***	0.376***	0.378***	0.374***
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
After$\times$*female*	0.000	0.002	−0.002	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: 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.

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 being born after the cutoff affects cross-state mobility later in life. Table B.4 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 in the state of 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.4 are small in magnitude, and none is statistically significant. We conclude 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.4: 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 (6,621 observations)							
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 (3,322 observations)							
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 (3,322 observations)							
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 (6,621 observations)							
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.

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