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Abstract: This study provides a policy evaluation of laws allowing early school enrollment of children, i.e., enrollment before the official school starting age. It investigates the effects of early enrollment on educational attainment, wages and employment. While the school starting age is usually determined by children's date of birth and legal cutoffs, some German states allowed early enrollment in some years. Exploiting state and cohort variation, the results show that male early enrollees attain fewer years of schooling, enter the labor market earlier and have a larger labor market attachment at around age 16. Positive wage effects persist until approximately age 35. Results for women roughly resemble those for men but they are less convincingly estimated.

Keywords: Early enrollment policy, early school entry, wages, employment, school starting age

JEL classifications: I28, J21, J24

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1. Introduction

Several countries allow parents to enroll their children early, meaning before the official cutoff rules that usually define the compulsory schooling age. For example, this is the case in some states, districts or provinces in the US, Canada, Australia and China. To our knowledge the consequences of these policies have not yet been evaluated empirically. On the one hand, early enrollment has the advantage to lead to lower opportunity costs of schooling through fewer foregone earnings when labor market entry occurs at an earlier age. On the other hand, it can have detrimental effects on learning outcomes through cognitive immaturity or the lack of socioemotional skills.

This study is the first to analyze the consequences of early entry policies. It provides a policy evaluation assessing the effects of early school entry on long-term educational and labor market outcomes. Analyzing the consequences of early entry informs policy-makers on its merits and drawbacks, enabling an evidence-based decision on introducing, maintaining or reforming the corresponding regulations. This is important given that there is no political consensus on allowing or permitting early school entry in many countries yet. For example, Germany's early enrollment policies have been reformed several times in the past. Thus, one important contribution of this study is to provide valuable information for educational policy makers.

The empirical strategy exploits the reforms of the early entry policies in the last decades in Germany. Some federal states switched from non-allowance to allowance, vice versa and back again. Many policies restricted early entrance to children who were born one, two, three, four or as many as six months after the cutoff. These regulations also changed over time and states. To identify the causal policy effects, we exploit this variation and implement a two-sample IV strategy. The analyses rely on German survey data covering school entry information and administrative data with highly accurate wages and employment biographies covering the age span from 16 to 50.

While we are not aware of a study investigating early enrollment, there is a large body of literature analyzing the effects of school starting age (SSA). It illustrates that children who are relatively young at enrollment perform worse than their older classmates on math, reading and writing tests (see, e.g., Bedard and Dhuey 2006, Datar 2006, Puhani and Weber 2007, McEwan and Shapiro 2008, Elder and Lubotsky 2009, Cascio and Schanzenbach 2016). Given the size of this literature, it is surprising that the large majority of studies focuses on outcomes during childhood while longer term outcomes are analyzed less often. Few studies analyze its effects on cognitive ability and educational performance in (early) adulthood (Black et al. 2011, Dhuey et al. 2019, Görlitz et al. 2022). And there are only a handful of studies estimating the long-run effects of SSA on final educational attainment and labor market outcomes such as wages (Dobkin and Ferreira 2010, Kawaguchi 2011, Black et al. 2011, Fredriksson and Öckert 2014, Larsen and Solli 2017).[1] While most (but not all) of these studies find that individuals who belonged to the youngest children of their class have lower educational attainment there is no consensus across studies about wage effects. Our study provides more insights into these effects. It also complements and improves the previous SSA literature by estimating age-earnings-profiles, by controlling for birth months and cohort effects and by eliminating the age-at-test effect at the same time.

Our study also differs from the SSA literature, because the latter focuses on individuals who, as children, were enrolled in accordance with the cutoff rules that exogenously determine SSA and, thus, who became the youngest or the oldest in class. This implies that the effects are measured for “compliers”, i.e. for children whose parents comply with the cutoff rules. Because the empirical procedure exploits laws or school regulations that determine the cutoff dates, the SSA regulations cannot prevent age variation within relative class to occur as long as there is only one school start per year. Put differently, as long as every class has an age variation of one year (which all countries currently have), policies cannot prevent someone from being the youngest in class. In contrast, early enrollment regulations and thus the prevalence of age variation by more than one year can be permitted or banned by law which makes our research question more relevant for the policy practitioners.

Additionally, our study contributes to the SSA literature, because it is also a novel question whether SSA effects for compliers are similar to those of noncompliers, i.e., early enrollees whose parents deviate from the cutoff rules. Compliers and noncompliers could be different for various reasons which could explain different SSA effects. Early enrollees enroll at an even younger age than the youngest compliers in class, thus, increasing the age heterogeneity in class even further. If effects of school starting age are non-linear this might aggravate any negative effects of being young. Early enrollees might also constitute a different selection, e.g., if their parents choose early enrollment only for children who are physically and mentally more mature

[1] Another related study is by Dustmann et al. (2017) who investigate how track choice in middle school affects long-term labor market outcomes in Germany. Because they use the school starting age (approximated by the date of birth) as an instrument for track choice, some of their reduced form estimates are related to our study, even though their research question differs greatly from ours.

than their peers and, thus, likely higher performing students. Or it might be that the characteristics of complying and noncomplying parents differ from another which could affect the size of the SSA effects.[2] Whether the effects for compliers and noncompliers are similar is an empirical question which we will answer as one research question in our study.

Our main analysis is restricted to men, given that Kamb and Tamm (2023) find that early enrollment affects women's fertility. Thus, any potential labor market outcomes for women could either be a direct result of early enrollment or arise due to fertility decisions which makes an overall effect hard to interpret. Furthermore, there is indication that our estimation strategy might not hold for women, which leads to less reliable conclusions. For women the instruments are weak and some of the predetermined characteristics are correlated with the instruments. Focusing on male results also follows the previous literature that restricts labor market outcomes to men (Larsen and Solli 2017, Dustmann et al. 2017). This is why we show the results for women for reasons of completeness but in less detail.

In sum, our study contributes to the literature in various aspects. (i) It is the first study that analyzes early enrollment regulations. Our results can help policy makers to decide on prohibition or allowance of such regulations. (ii) It complements the SSA literature by adding results on long-term labor market effects to the handful of previous studies. (iii) The empirical strategy estimates age-earnings-profiles after controlling for birth months and cohort effects and by eliminating the age-at-test effect. (iv) It also shows how compliers with the cutoff rules and noncompliers who enroll early differ and whether this leads to different SSA effects.

The results for men show that early enrollment laws are highly predictive of early school entrance. The second-stage results show that boys who enroll one year earlier acquire fewer years of schooling than boys who follow the regular cutoff rules for school enrollment. On average, the educational attainment of early enrollees decreases by 0.3 years. Accordingly, they enter the labor market 1.3 years earlier, which raises their labor market participation and wages at the ages of 16 and 17. The positive wage effects persist over several years but diminish in magnitude over time. They remain statistically significant when men are in their twenties, likely because of increased labor market experience. Once men are in their mid-thirties, the wage effect becomes statistically insignificant. Afterwards, the magnitude of this effect decreases further and becomes close to zero at approximately age 50. There is no evidence that regular

[2] We provide evidence that men who were enrolled early have on average younger parents compared to the parents of compliers. This is an additional contribution to the literature.

enrollees outperform early enrollees with regard to wages at any point in time up to age 50. Results for women roughly resemble those for men (exceptions are that effects on years of schooling and age at labor market entry are not significant) but we caution to interpret results for women as causal.

We provide evidence that the necessary assumptions to interpret these two-sample IV effects as causal are not violated for men. First, we show that predetermined parental characteristics are unrelated to the instruments. Second, we show that a violation of the monotonicity assumption is not an issue in our study. Third, because we exploit state-by-cohort variation, we include state fixed-effects that account for time-invariant differences between the states, e.g., in the schooling system and year-of-birth fixed-effects that absorb cohort differences. Furthermore, we include month-of-birth dummies to address concerns raised by Buckles and Hungerman (2013), who show that health outcomes by newborn differ by the months of birth (even for adjacent months within the same season). Fourth, age-at-test effects that matter in the literature on SSA are not an issue in our setting[3] because we are able to measure all outcome variables in the month of birth of each individual.

This study proceeds as follows. The next section describes school enrollment laws in general and early entry regulations. Section 3 outlines the data and the empirical strategy that identifies the causal policy effect of early entry. Section 4 presents the empirical results for men (in subsection 4.1) and a selection of empirical results for women (in subsection 4.2). The last section draws conclusions.

2. Early enrollment regulations

In Germany, state-specific laws determine the age at which children enter school. Usually, children who were born six years before the legal cutoff date had to enroll in school in a given year, while those born after the cutoff had to wait another year. For specific birth cohorts, some states allowed parents whose children were born after the cutoff date to enroll early. To prevent particularly high age heterogeneity within a class, state laws generally limited eligibility for early enrollment to children born within a range of birth months after the cutoff. The most common regulation was to allow early enrollment for children born up to three months after the

[3] Age-age-test effects occur in the SSA literature evaluating test score differentials, because children perform the test at the same date within class, even though they differ in their absolute age (and maturity) when taking it (Smith 2010).

cutoff. However, there were also time periods in which states allowed early enrollment for only those born within one or two months or even as many as six months after the cutoff. State laws prevented early enrollment for children who were born shortly before the cutoff because these children would already be almost one year younger than the oldest in class if they were regularly enrolled. Early enrollment would increase their age difference relative to classmates by one additional year. Importantly, the early enrollment policies changed within states over time but there is also variation in the regulations across states. Table 1 shows how much the regulations differed by state and year.

Table 1. Early enrollment regulations by state and over time

School Year \ State	Baden-Wuert-temberg	Bavaria	Bremen	Hamburg	Hesse	Lower Saxony	North Rhine-Westphalia	Rhineland Palatinate	Saarland	Schleswig-Holstein
1950	no	no	April–June	April–June	no	no	no	no	no	no
1951	no	no	April–June	April–June	June–July	no	no	no	no	no
1952	April–June	no	April–June	April–June	June–July	no	no	April–June	no	no
1953	April–June	no	April–June	April–June	June–July	no	no	April–June	no	no
1954	April–June	no	April–June	April–June	June–July	no	no	April–June	no	no
1955	April–June	no	April–June	April–June	June–July	April–Sept.	no	April–June	no	no
1956	April–June	no	April–June	April–June	June–July	April–Sept.	no	April–June	no	April–June
1957	April–June	no	no	April–June	April–June	April–Sept.	no	April–June	no	April–June
1958	Jan.–March	Oct.–Dec.	no	April–June	April–June	April–Sept.	no	April–June	no	April–June
1959	Jan.–March	Oct.–Dec.	no	April–June	April–June	April–June	no	April–June	no	April–June
1960	Jan.–March	Oct.–Dec.	no	April–June	April–June	April–June	no	April–June	no	April–June
1961	Jan.–March	Oct.–Dec.	no	April–June	April–June	April–June	April-June	April–June	no	April–June
1962	Jan.–March	no	no	Jan.–March	Jan.–March	April–June	April-June	April–June	no	April–June
1963	Jan.–March	no	no	Jan.–March	Jan.–March	April–June	April-June	April–June	no	April–June
1964	Jan.–March	no	no	Jan.–March	Jan.–March	April–June	April-June	April–June	no	Jan.–June
1965	Jan.–March	no	no	Jan.–March	Jan.–March	April–June	April-June	April–June	no	Jan.–June
1966	Jan.–March July–Nov.	no	no	Jan.–March	Dec.	April–June July–Sept.	April–June Dec.–Febr.	April–June Dec.–Jan.	no	Jan.–June Dec.–Jan.
1967	July–Aug.	no	July–Sept.	no	July–Sept.	July–Sept.	July–Sept.	July–Sept.	July–Sept.	July–Oct.
1968	July–Aug.	no	July–Sept.	July–Dec.	July–Dec.	July–Dec.	July–Sept.	July–Dec.	July–Dec.	July–Dec.
1969-1974	July–Aug.	July–Dec.	July–Sept.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.
1975	July–Aug.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.
1976-1994	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.	July–Dec.

Note: The table shows whether the states allowed early enrollment and, if so, the respective birth months allowed to enroll early. Because the policy changed the start of the school year in 1966, there are some states with two cohorts who start school in this year.

3. Data and empirical strategy

3.1 Data

To study the effects of early enrollment on labor market outcomes, we base our analyses on two sets of data. The first dataset contains information on school enrollment, while the second dataset includes data on labor market outcomes. The first dataset consists of information about the adult cohort of the National Educational Panel Study NEPS (doi:10.5157/NEPS:SC6:5.1.0). The NEPS is a survey containing information on educational biographies, including the date of actual school entry for individuals born between 1944 and 1986 (Blossfeld et al. 2011). Information on the date of birth, the cohort- and state-specific cutoff regulations and the information on the actual date of school entry enables us to observe early enrollment. Following Kamb and Tamm (2023), we define early enrollment as occurring when the date of actual school enrollment precedes the date of enrollment that is set by the cutoff regulation by at least 8 months. As already explained in the introduction, we focus our main analysis on men, because Kamb and Tamm (2023) find that early enrollment affects women's fertility, complicating the interpretation of female labor market outcomes. For similar reasons several previous studies limit their analysis to men (e.g., Larsen and Solli 2017 and Dustmann et al. 2017). This is why we focus on the results for men in the following. Section 4.2 illustrates effects for women.

In this main sample, almost one of six boys enrolled early (see Table 2). Table A-1 in the Appendix shows that children enrolled early differ from children complying with the official school entry cutoff. Mothers and fathers of early enrolled boys are significantly younger (at the time of birth of the child) and early enrollees have fewer older siblings. This selectivity in early enrollment is a reason why the effect of age at school entry might differ between compliers with the cutoff and the noncompliers who enroll early, if SSA effects are heterogeneous for children from different parental background. We will test this empirically.

Table 2. Summary statistics for men

Variables	Mean (Std. Dev.)	Observations
	Men	
Early enrollment (NEPS data)	0.156	3,672
Educational Attainment (IAB data)		
Years of schooling	13.90	3,689,000
Years spent in school	10.63	3,689,000
Highest schooling degree: No (0/1)	0.02	3,689,000
Highest schooling degree: Lower track (0/1)	0.28	3,689,000
Highest schooling degree: Medium track (0/1)	0.36	3,689,000
Highest schooling degree: High school (0/1)	0.33	3,689,000
Age at first job and wages (IAB data)		
Age at first employment	22.59	3,746,196
Log daily wages at age 20	3.720 (0.6367)	1,788,138
Log daily wages at age 30	4.459 (0.4618)	2,639,495
Log daily wages at age 40	4.802 (0.5708)	1,833,618
Log daily wages at age 50	4.854 (0.6284)	790,257
Cumulative earnings at age 20	24,902 (25,813.7)	3,746,196
Cumulative earnings at age 30	213,315 (148,067.9)	3,746,196
Cumulative earnings at age 40	583,361 (377,882.9)	2,565,150
Cumulative earnings at age 50	1,001,906 (678,576.2)	1,204,143
Employment (IAB data)		
Labor force participation at age 20	0.484	3,746,196
Labor force participation at age 30	0.707	3,746,196
Labor force participation at age 40	0.717	2,565,150
Labor force participation at age 50	0.659	1,204,143

Our second dataset contains labor market outcomes from highly accurate social security records from the German Federal Employment Agency provided by the Institute for Employment

Research (IAB) for the years 1975 to 2016 (IEB V13.00.00). The IAB data contain all employees covered by social security, excluding the self-employed and civil servants.[4] From these data, we draw a random sample of 40% of workers who were born between 1959 and 1986. The 1959 cohort is the first cohort that we can observe from age 16 onward. The 1986 cohort is the last cohort observed in the NEPS data. We restrict our analyses to individuals who had their first place of residence in West Germany. The data contain detailed labor market information on individuals, such as first labor market entry, employment status and wages. The data also contain several sociodemographic characteristics, such as gender, education, date of birth and place of residence.

We define the age at first employment as the age at which an individual works as a regular full-time worker for more than 6 months. To create indicators of whether an individual was employed at a certain age, we use the employment status as of the 15th of the birth month. Wages represent the log of the real daily wage in 2010 prices on the 15th of an individual's birth month as measured in the main employment spell, which is defined as the employment spell with the highest wage (in case of multiple employments). We define cumulative earnings as the absolute lifetime labor earnings earned up until a specific age. We do not consider transfer payments because they do not stem from labor or one-time payments. Wages above a certain threshold are censored in the data because the German social security system does not include these data. We impute these censored wage data following the standard imputation procedure established by Gartner (2005).

The German social security data also contain educational variables.[5] We create dummy variables for the highest degree achieved at school: no school degree (equivalent to spending seven years in school), lower track degree, which is a lower secondary degree (equivalent to spending nine years in school), medium track degree, which is an intermediate secondary degree (equivalent to ten years) and a high school degree (equivalent to 13 years).[6] We define a quasi-continuous measure of "years spent in school" using the assumed years of schooling. "Years of schooling" is the sum of the years spent in school and the years spent in vocational education or college. An apprenticeship degree[7] is equivalent to three more years of schooling,

[4] The data covers more than 80 percent of the individuals contained in Birth Register Data (Dustmann et al. 2017).
[5] To increase the internal consistency of this information, we apply the standard algorithm suggested by Thomsen et al. (2018).
[6] See Dustmann et al. (2017) for a more detailed description of the German tracking system.
[7] The German apprenticeship combines working in a firm (3-4 days per week) with vocational education acquired in state-financed vocational schools. It lasts two to four years and awards a vocational degree that qualifies the recipient for employment in an occupation as a skilled worker.

and a college or university degree adds five additional years. Table 2 contains the descriptive statistics for men.

As a robustness check, we run our analysis on a data sample restricted to individuals born before 1976. This is important because most of our outcomes are specific to age and not all cohorts reached age 50 at the end of our observation period in the main sample. Thus, the number of cohorts differs between outcomes. Our restriction to cohorts born before 1976 creates a sample where the outcomes are available for all cohorts in the age span from 16 to 40. Another reason for defining this robustness sample is that most of the changes in early enrollment regulations occurred before 1976.

3.2 Empirical strategy

We use a two-sample two-stage IV strategy to estimate the long-term effects of early school entry. The first-stage of the IV estimator is based on the NEPS data, and the second-stage uses the administrative IAB data. We have to rely on different datasets when estimating the first and the second-stage because information on early enrollment, our main explanatory variable, is not included in the administrative data. The first-stage equation is

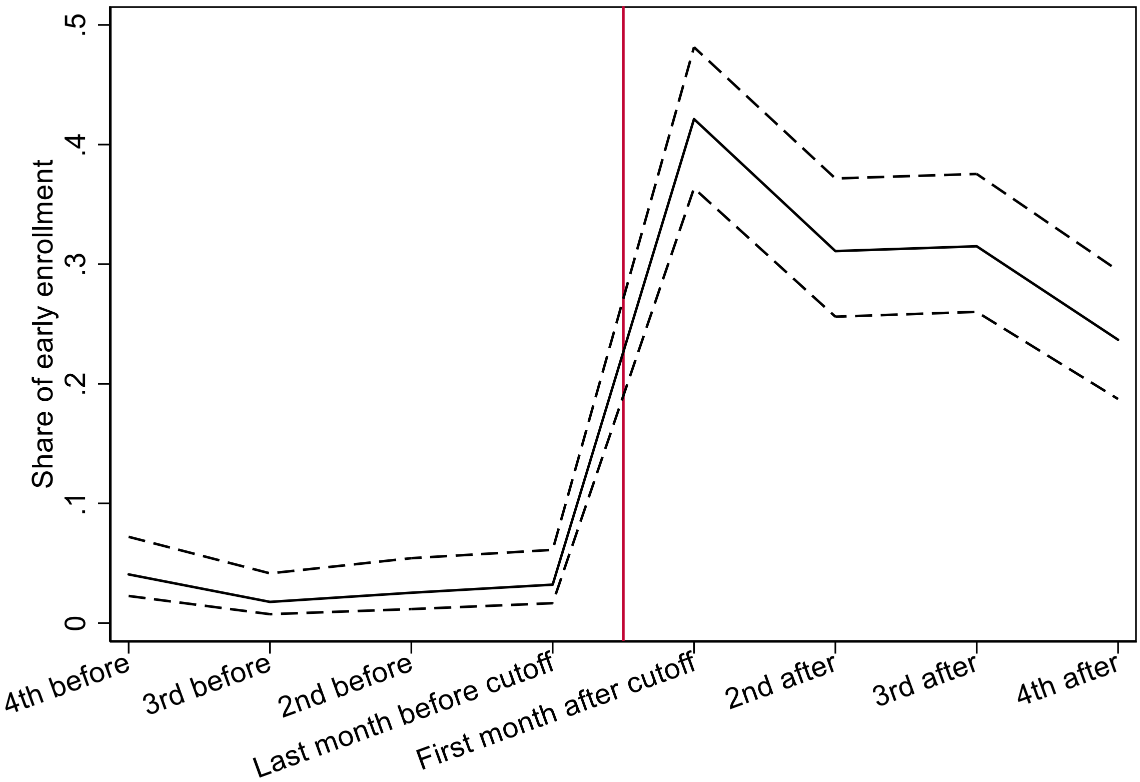
$$Early\ enrollment = \tilde{\alpha} + Z\tilde{\gamma} + X\tilde{\delta} + \tilde{\mu}, \qquad (1)$$

where $Early\ enrollment$ is an indicator variable that is equal to one when an individual enrolled early and zero otherwise. The explanatory variables cover the instruments Z and several covariates X (i.e., dummies for the states of enrollment in primary school, year-of-birth dummies, month-of-birth dummies and dummies for the distance to the cutoff). The state dummies account for differences occurring at the state level, e.g., differences in the schooling system or labor market conditions. The year-of-birth dummies absorb cohort effects which was shown to matter by Fredriksson and Öckert (2014). The month-of-birth dummies take potential differences in health conditions of newborns into consideration that are related to the month of birth (see Buckles and Hungerman 2013). The distance to the cutoff controls for relative age effects. We further discuss the importance of these variables below when discussing the assumptions of the IV strategy. Standard errors are clustered at the level of the states.

The instruments exploit exogeneous variation in the states' early enrollment regulations over time by permitting or restricting early enrollments. Additional variation arises from several changes in the state laws that determined how many months younger than the cutoff a child could be to be allowed to enroll early. As already mentioned in the second section, early enrollment was permitted for children born one, two, three or six months after the regular school enrollment cutoff (see Table 1).

To show that the month of birth matters for enrollment decisions, Figure 1 presents average early enrollment rates for men by distance to the cutoff for individuals in states and birth cohorts with early enrollment permission. Early enrollment is most common and nonnegligible in size among children born shortly after the cutoff. For example, 42% of those born in the first month after the cutoff were enrolled early. If these children were enrolled at the regular time, they would be among the oldest in their class. Their early enrollment made them one month younger than the youngest regularly enrolled child. The share of children who were enrolled early diminishes the further away the birth month is from the cutoff date. However, even four months after the cutoff date, slightly more than 20% of the children were enrolled early. Figure 1 also reveals that early enrollment is less relevant for children born on the other side of the cutoff, e.g., one to two months before the cutoff date.

Figure 1. Share of men with early enrollment by distance to the cutoff



Note: The graph uses NEPS data to illustrate the share of early enrollment for men by birth month relative to the cutoff in states and birth cohorts with early enrollment permission. Dashed lines indicate the bounds of the 95% confidence interval.

Our definition of the instruments reflects the findings from Figure 1. Specifically, Z includes four separate binary instruments. These are dummies indicating whether children were allowed to enroll early and how many months after the cutoff their birth month occurred: one, two, three, or at least four. Thus, the instrument combines the information of the individuals' birth month in relation to their cutoff with early enrollment regulations in their state of school enrollment.

Moving to the **relationship between the instruments and early enrollment**, Table 3 contains the estimation results of the first-stage (see Equation 1). It illustrates that two of the four instruments are statistically significant at the 5% level at least. The results also show that early enrollment is most common among children born in the first or second month after the cutoff. These findings are in line with the graphical evidence from Figure 1. The F-statistic for the joint significance of the instruments is 21.94 and clearly exceeds the rule of thumb given in Staiger and Stock (1997) to rule out weak instruments.

Table 3. First-stage estimates for men

	Early enrollment
Born in the 1st month after the cutoff in a state with early enrollment (y/n)	0.395 *** (0.051)
Born in the 2nd month after the cutoff in a state with early enrollment (y/n)	0.180 ** (0.071)
Born in the 3rd month after the cutoff in a state with early enrollment (y/n)	0.049 (0.092)
Born in the 4th month after the cutoff (or later) in a state with early enrollment (y/n)	0.034 (0.048)
F test of excluded instruments	21.94
Observations	3,672

Note: The table reports the effects of the instruments on early enrollment from a linear probability model based on the NEPS data. Control variables include dummies for the federal states of enrollment in primary school, year-of-birth dummies, month-of-birth dummies and dummies for the distance to the cutoff. Standard errors are shown in parentheses. Statistical significance: p<0.1 *, p<0.05 **, p<0.01 ***.

We identify the causal effects of early enrollment on long-term outcomes by using two-stage least squares (2SLS). The second-stage equation is:

$$Y^k = \alpha^k + \beta^k Early\ \widehat{enrollment} + X\delta^k + \varepsilon^k. \qquad (2)$$

The dependent variable of the second-stage Y^k comprises several long-term outcomes k, such as wages and employment measured at different points in time over an individual's life cycle. $Early\ \widehat{enrollment}$ is the prediction of Equation (1) and X incorporates the same set of covariates as in Equation (1).[8] Thus, these covariates include state dummies, year-of-birth dummies, month-of-birth dummies and dummies for the distance to the cutoff. The estimate β^k

[8] Note that the IAB data does not contain information about the state of school enrollment. This is why we use the first state of residence recorded in the IAB data instead.

is the causal effect of early enrollment on long-term outcome k. ε^k is the error term. To obtain robust standard errors that account for the fact that the second stage uses the prediction of early enrollment as an explanatory variable, we follow Pacini and Windmeijer (2016). Furthermore, all regressions account for clustering at the state level.

Interpreting β^k as the causal effect of early enrollment requires three assumptions to hold. First, the **exclusion restriction** requires early enrollment regulations to have no direct effect on long-term labor market outcomes but only affect them indirectly through the incidence of being enrolled early. This assumption would be violated if eligibility for early enrollment in a state where and during a time when early enrollment is allowed had an effect on wages and employment simply by itself, meaning independently of actual early enrollment. Note that the intention of the early enrollment regulations was to allow early maturing children to enter school earlier and not by considerations about the future labor market outcomes of the children. Thus, we consider it unlikely that the regulation by itself or changes in the early enrollment laws influence outcomes directly. Furthermore, all regressions include state- and year-of-birth effects. As already explained, the state dummies account for state-specific differences in the schooling system. Year-of-birth dummies control for cohort effects and for changes in economic conditions over time. We also tested whether changes to early enrollment regulations were related to economic conditions at the state level at that time. We defined a dummy of early enrollment reforms in a given year and state as one if there was a reform and 0 otherwise. Regressing this dummy on state and year fixed effects in addition to GDP growth (and its lags up to four years), revealed no statistically significant results. The same was true when using the unemployment rate or female employment rates as independent variables instead. These results which are available upon request indicate that reforms were unrelated to general labor market conditions.

Second, the **independence assumption** requires early enrollment regulations to be (conditionally) random with respect to the outcomes. This assumption would be violated if the corresponding regulations and their changes correlate with parents' fertility decisions, which would affect our outcomes. This is unlikely because the regulations changed several times over the years, even within states, so parents could not anticipate stability in the early enrollment regulations when planning childbirth. Figure A-1 in the Appendix shows that there is no bunching of observations to the left or to the right of the cutoff in states that allow early enrollment. Regressing the log number of observations measured at the level of the state, year of birth and months of birth on dummies for the distance to the cutoff and on the instruments in addition to state, year-of-birth and month-of-birth dummies (i.e. similar to Equation 1 for the first-stage) does not indicate that the early enrollment regulations affect the timing when children were born (see Table A-2 in the Appendix).

To provide further evidence that this assumption is valid, Table 4 documents that the instrument is unrelated to predetermined parental characteristics. The NEPS data cover information on parental background such as mother's and father's age at birth, their level of education, dummies for foreign-born mothers and fathers and the number of older siblings. Table 4 presents the outcomes of the predetermined characteristics as second-stage IV estimates, none of which are statistically or economically significant. It is important to note that the independence assumption only has to hold conditional on the covariates. To ensure the validity of our results, we used several controls, including state- and year-of-birth dummies (as already mentioned above). For example, we consider month-of-birth dummies because Buckles and Hungerman (2013) find that parental background differs between children born in different months. We also control for the distance to the cutoff, which absorbs any effects of relative age within class on educational performance and labor market success.

Table 4. Balancing of predetermined characteristics for the sample of men

Predetermined characteristics used as outcome	2SLS estimates	Obser-vations
Mother with college degree (y/n)	-0.066 (0.062)	3,545
Mother's age at birth	-2.128 (3.748)	3,537
Mother foreign born (y/n)	0.078 (0.107)	3,651
Father with college degree (y/n)	0.111 (0.122)	3,527
Father's age at birth	-0.578 (3.526)	3,494
Father foreign born (y/n)	-0.012 (0.070)	3,609
Number of older siblings	0.358 (0.596)	3,256

Note: Based on NEPS data, the table provides IV estimates using the predetermined outcomes listed in the first column as dependent variables in the second stage. Standard errors are shown in parentheses. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Third, the **monotonicity assumption** implies that the instruments have a unidirectional effect on early enrollment. Some studies suggest that this assumption is violated when analyzing SSA because this literature uses the cutoff as an IV, which is positively associated with actual school entry age for children born on one side of the cutoff and negatively associated for children born on the other side of the cutoff (Fiorini and Stevens 2014, Barua and Lang 2016). However, this is not relevant for our setting because, first, Görlitz et al. (2022) test the monotonicity assumption for Germany when analyzing SSA on competencies in adulthood and find no evidence of its violation. Second, in our particular setting with early enrollment, we consider a violation of the monotonicity assumption unlikely because an individual who was entitled to enroll early would either make use of it or not. However, no individual would decide **not** to enroll early (or to even delay entry) **because** it was allowed.[9]

4. Results

4.1 Main results (men)

Table 5 shows how early enrollment affects educational attainment in the two-sample IV estimation strategy. Early enrollment reduces the years of schooling by approximately 0.3 years (see Column 1). Column (2) shows the results when analyzing only the years spent in primary and secondary school, ignoring vocational and college education. Because the corresponding coefficient has a similar magnitude of 0.3 years, we suggest that early enrollment influences the primary and secondary school biography rather than vocational education or college. To provide further evidence in favor of this conclusion, Columns 3 to 6 use binary indicators for an individual's highest schooling degree. The probability of obtaining a high school degree decreases by seven percentage points for early enrollees, while the probability of obtaining a lower track schooling degree increases by seven percentage points. This is in line with the results from the SSA literature using different German datasets. It shows that SSA mainly affects school tracking, through which it influences educational attainment. The last column of Table 5 illustrates that early enrollees enter the labor market 1.3 years earlier. This is plausible given that they are one year younger at school enrollment and that they obtain 0.3 fewer years of schooling. Table A-3 in the Appendix shows that these results are robust when using the robustness sample of individuals born before 1976 for whom we observe all outcomes for all years up to age 40.

[9] Note that we cannot run the corresponding test as Görlitz et al. (2022) did, because it requires that the instrumented variable is continuous as in the case of SSA. It cannot be run on the binary variable of early enrollment.

Table 5. The impact of early enrollment on education and first labor market entry for men

	Years of schooling	Years spent in school	Binary indicators for highest schooling degree: No degree attained	Lower track degree	Medium track degree	High school	Age at first employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
The effect of early enrollment	-0.307 **	-0.291 **	0.002	0.070 ***	-0.001	-0.071 **	-1.311 **
	(0.151)	(0.124)	(0.005)	(0.024)	(0.017)	(0.034)	(0.546)
Obervations	3,689,000	3,689,000	3,689,000	3,689,000	3,689,000	3,689,000	3,746,196

Note: Using the IAB data, the table documents the results of the two-sample IV estimates of early enrollment on men's educational outcomes and first labor market entry. Years of schooling include compulsory and secondary schooling, vocational education and college attendance. Years spent in school only consider compulsory and secondary school attendance, excluding vocational education and college. The highest schooling degree earned is reflected in binary variables distinguishing no school degree, a lower school degree referring to a degree from the lowest track that mainly prepares students for a blue-collar occupation, medium school degree preparing for white-collar occupations and a high school degree that allows university entrance. Standard errors that are based on two-sample-IV-adjusted and clustered standard errors are shown in parentheses. Statistical significance: p<0.1 *, p<0.05 **, p<0.01 ***.

Figure 2 illustrates how early enrollment affects log wages over the lifespan. The upper graph shows the effects for individuals from age 16 to 50. There are statistically significant effects from age 16 to 37. They are largest in magnitude at ages 16 to 21. This is likely because early enrollment affects educational attainment and age at labor market entry. Individuals with early school entry already work full-time when individuals with regular enrollment still attend school and plausibly work only a few hours per year, e.g., during their vacations. Wages do not include zeros because we do not want to confound wage effects with employment effects that are estimated and shown separately below. However, our definition of wages also includes holiday or student jobs that are more likely observed for those who still visit school which are on average more often regularly enrolled students. Early enrollees leave school earlier, so they work more often in a regular job which can explain their higher wages. The fact that the wage effect remains statistically significant until age 37 could be due to higher work experience gained by early enrollees. Dustmann et al. (2017) show that differences in experience completely explain the effect of SSA on wages measured at age 30 in Germany. Our estimated wage effects decrease over the life cycle and become economically insignificant at ages 49 and 50, where the coefficients are -0.002 and 0.005, respectively.

The middle graph of Figure 2 shows that the results are robust when analyzing the sample of individuals born before 1976 for whom we have labor market information for every year from age 16 to 40. The general pattern is the same as in the upper graph but the wage effects become statistically insignificant already by age 32. The lower graph of Figure 2 also shows that the main conclusions are the same when eliminating observations who have small wages, i.e., wages of fewer than 10 Euros per day. Here, wage effects become economically insignificant from age 44 to 50, where they range from -0.004 to 0.009.

Comparing our results to those from the literature on SSA effects shows some similarities. For example, Black et al. (2011) and Fredriksson and Öckert (2014) show that being one year younger at school entry increases wages at an early age, similar to our study. The SSA literature differs slightly from our study with regard to the age at which the positive effects fade out. While Black et al. (2011) and Larsen and Solli (2017) find estimates close to zero in magnitude after age 30, Fredriksson and Öckert (2014) show this to happen when individuals are in their mid-40s. The authors discuss why the size of the wage effects diminish over time. The most likely reason is that the initial benefits from higher experience (due to earlier labor market entry) of the youngest in class fade out over time and are offset by higher earnings that the oldest in class get in the long run (due to their higher education). This can also explain our results.

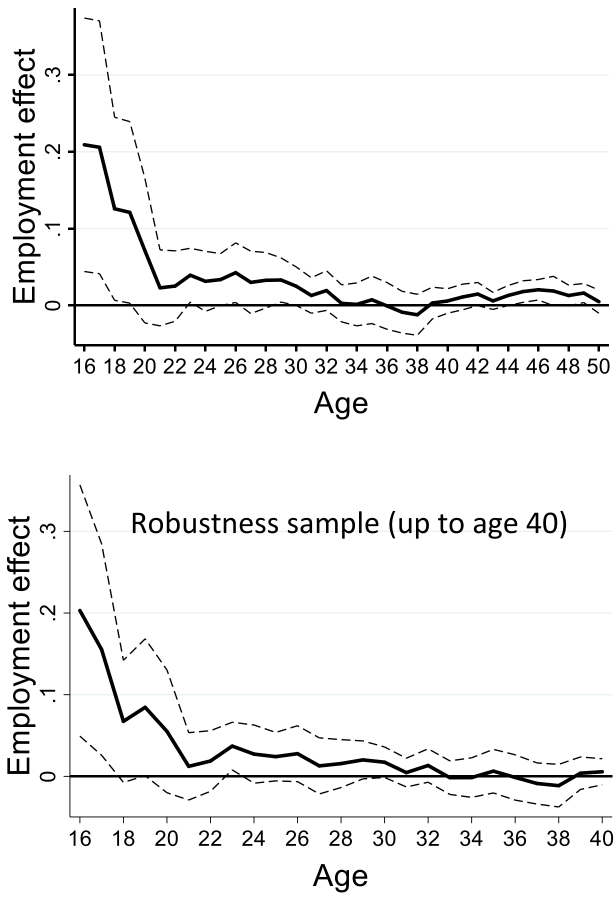
Figure 3 sheds further light on the reasons for positive wage results. Early enrollment increases an individual's employment probability statistically significantly from age 16 to 19 (see upper graph). The size of the employment estimates is especially large at ages 16 and 17. Using the robustness sample of individuals born before 1976 reveals broadly similar findings (see lower graph of Figure 3). In conclusion, our findings suggest that early enrollment leads to higher wages in the years after labor market entry because of higher labor market attachment, especially at ages 16 and 17.

Figure 4 illustrates effects of early enrollment on cumulative earnings measured up to age 50. In contrast to wage results, the cumulative earnings take individuals and periods with zero earnings into account, e.g. due to nonparticipation in the labor market. Effects of early enrollment on cumulative earnings grow up to around age 35 and then start to shrink (even though they are less precisely estimated). This pattern is in line with the wage and employment effects documented in Figures 2 and 3. Note that these effects should not be interpreted as lifetime earnings, because Fredriksson and Öckert (2014) show that considerable changes to the SSA effect on cumulative earnings materialize in the last 10 to 15 years before retirement. In particular, they show that the earnings effects reverse so that individuals being enrolled amongst the oldest gain higher wages starting at around age 55 and continuing afterwards.

Figure 2. The effect of early enrollment on log wages over the life cycle for men

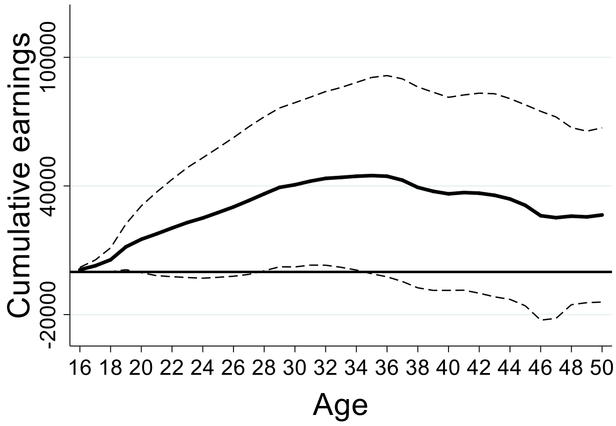
Note: The figures use IAB data to illustrate the two-sample IV estimates of early enrollment on log wages measured at different ages (solid lines). The sample is restricted to men. The upper graph shows the results for the whole sample. The graph in the middle presents the effects for a sample of individuals born before 1976 for whom complete information on labor market biographies is available up to age 40. The lower graph displays the findings when deleting data for those with small daily wages (< 10 Euros). Dashed lines represent 90% confidence bands (based on two-sample-IV-adjusted and clustered standard errors).

Figure 3. The effect of early enrollment on age-specific labor force participation for men



Note: The figures are based on IAB data and illustrate the two-sample IV estimates of early enrollment on the probability of being employed in an individual's birth month by age. The sample is restricted to men. The upper graph contains the results for the whole sample, and the lower graph presents the results for the sample of individuals born before 1976 for whom complete labor market biographies are available up to age 40. Dashed lines represent 90% confidence bands (based on two-sample-IV-adjusted and clustered standard errors).

Figure 4. The effect of early enrollment on cumulative earnings for men



Note: The figures are based on IAB data and illustrate the two-sample IV estimates of early enrollment on cumulative earnings by age. The sample is restricted to men. Dashed lines represent 90% confidence bands (based on two-sample-IV-adjusted and clustered standard errors).

In conclusion, compared to the literature on SSA (Black et al. 2011, Fredriksson and Öckert 2014, Dustmann et al. 2017), the general pattern of our results is similar, even though our estimates have reversed signs because early enrollment is equivalent to a *reduction* of school starting age by one year. If anything, our estimates are slightly larger in magnitude. This indicates that children with early enrollment experience similar long-run effects compared to the youngest children in class whose parents have complied with the cutoff rules. In sum, the effects for individuals whose enrollment complied with the cutoff regulations (as in the SSA literature) are similar to the effects of noncomplying early enrollees. The two different LATE estimates from the SSA literature and for early enrollment do not hint at major heterogeneity in the effect of being one year younger at school entry.

4.2 Results for women

Figure A-2 illustrates average early enrollment rates for women by distance to the cutoff in states and cohorts allowing early enrollment. It shows that in states allowing early enrollment the distance to the cutoff matters for early enrollment decisions of women, similar to men. However, Table 6 shows that, unlike for men, the F-test for joint significance of the instruments has a value of 9.36 which is lower than the value that is recommended by Staiger and Stock (1997). This is evidence of weak instruments which could bias the results in various directions. Furthermore, Table 7 shows that having a mother with college degree is significantly correlated with the instruments which violates the independence assumption. Note that our two-sample IV strategy does not allow to control for mothers' educational degree given that this information is only available in the NEPS data used for the first-stage estimate but not covered in the IAB data used for the second-stage estimates. Both aspects call into question the validity of the findings for women.[10]

Table 6. First-stage estimates for the sample of women

	Early enrollment
Born in the 1st month after the cutoff in a state with early enrollment (y/n)	0.466 *** (0.111)
Born in the 2nd month after the cutoff in a state with early enrollment (y/n)	0.277 ** (0.090)
Born in the 3rd month after the cutoff in a state with early enrollment (y/n)	0.149 * (0.078)
Born in the 4th month after the cutoff (or later) in a state with early enrollment (y/n)	0.039 (0.045)
F test of excluded instruments	9.36
Observations	3,952

Note: The table reports the effects of the instruments on females' early enrollment from a linear probability model based on the NEPS data. Control variables include dummies for the federal states of enrollment in primary school, year-of-birth dummies, month-of-birth dummies and dummies for the distance to the cutoff. Standard errors are shown in parentheses. Statistical significance: p<0.1 *, p<0.05 **, p<0.01 ***.

[10] Note that Kamb and Tamm (2023) use the same reforms and the same data for the first stage of the IV strategy but use different cohorts of women (born between 1944 and 1970), while due to restrictions in the IAB data used for the second stage we focus on cohorts born between 1959 and 1986. This could explain differences in the validity of the IV strategy between their paper and our analysis for women.

Table 7: The effect of early enrollment on predetermined characteristics for women

Predetermined characteristcs used as outcome	2SLS estimates	Obser-vations
	Women	
Mother with college degree (y/n)	-0.115 ***	3834
	(0.037)	
Mother's age at birth	2.749	3,845
	(2.414)	
Mother foreign born (y/n)	-0.164	3,920
	(0.112)	
Father with college degree (y/n)	-0.100	3,769
	(0.061)	
Father's age at birth	0.813	3,798
	(2.929)	
Father foreign born (y/n)	-0.127	3,860
	(0.157)	
Numer of older siblings	0.738	3,528
	(0.748)	

Note: Based on NEPS data, the table provides IV estimates using the predetermined outcomes listed in the first column as dependent variables in the second stage. Standard errors are shown in parentheses. Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-4 presents findings for educational attainment of women, Figure A-3 for log wages, Figure A-4 for employment probabilities and Figure A-5 for cumulative earnings. Overall the wage and employment effects appear roughly similar to those of men. For example, the pattern for log wages in Figure A-3 is similar to the findings of men (cf. Figure 2) where the most severe effect appears shortly after labor market entry, but the magnitude of the female effects is smaller and their statistical significance fades away at an earlier age. Figure A-4 shows that the initially higher wages align with a higher labor market attachment of early enrolled women at ages 16 to 18. However, in contrast to men, early enrollment reduces women’s labor force participation starting from age 29 onwards. This could be due to their higher fertility compared to women who were enrolled at school complying with the cutoff that is shown by Kamb and Tamm (2023). Looking at cumulative earnings, Figure A-5 shows that results for women display a similar pattern as those for men (cf. Figure 4) but are considerably smaller and never statistically significant. Women's estimates become close to zero at around age 40. This is in line with the employment effects that turn negative for early enrolled women from age 29 onwards (as documented in Figure A-4).

When looking at educational attainment in Table A-4 some puzzling results occur. Most of the coefficients are statistical insignificant, except the estimate for lower track degree that is –5.8. This is evidence of a negative effect of early enrollment on the schooling level, although the estimate for years of schooling (which is –0.19) is not significant. The effect on age at first employment is only –0.18 years and not statistically significant. The latter is puzzling, given that early enrollees enter school one year younger, have (if anything) less schooling and have higher employment rates between age 16 and 18 (as shown in Figure A-4). This estimate deviates from the corresponding estimate of men of –1.3 years. This could be due to a weak IV that could bias the results in any direction. In sum, we conclude that women's results are similar to those of men, but generally of smaller size. For women, some findings are puzzling and the validity of the estimation strategy is not always given which is why we warrant caution to put too much weight on women's findings.

5. Conclusion

This paper analyzes how early school enrollment affects education, wages and employment over the lifespan. To identify causal effects, we apply a two-sample IV strategy and instrument early enrollment with its legal allowance and its political design as varying over time and between states. While early enrollment lowers the educational attainment of men, we find positive employment effects at ages 16 and 17 because early enrollees' first labor market entry takes place earlier. These employment effects lead to more work experience and result in higher wages that are most pronounced before an individual reaches age 20. The wage effects decrease in magnitude afterwards and become statistically insignificant between ages 30 and 40. Effects for women roughly resemble those for men but should be interpreted with caution given that the assumptions of our identification strategy might not hold for them.

From a policy perspective, the negative education effect must be weighed against the positive effects induced by earlier labor market entry. At least with respect to wages, there is no indication that the negative effect on education dominates the positive effect on experience at any age up to age 50. The decision to legally permit or prohibit early enrollment finally depends on whether maximizing wages or increasing education is seen as more important. Parents can use our findings to decide upon early enrollment based on their preferences for early labor market entry, higher wages and higher levels of education of their children. Of course, a more sophisticated cost–benefit analysis would require taking all returns and costs into account. Such an analysis would have to consider that education incorporates nonmonetary returns such as reduced crime or higher well-being (e.g., Oreopoulos and Salvanes 2011). Because this is beyond the focus of our study, it remains a topic for future research.

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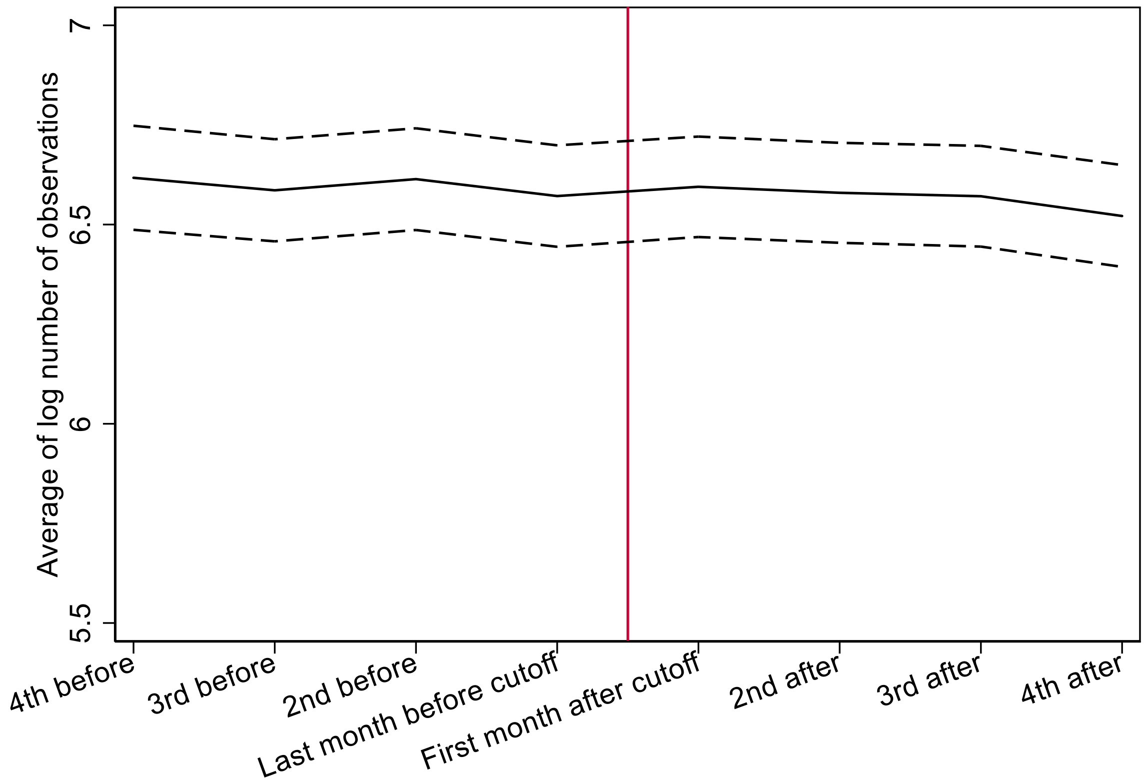
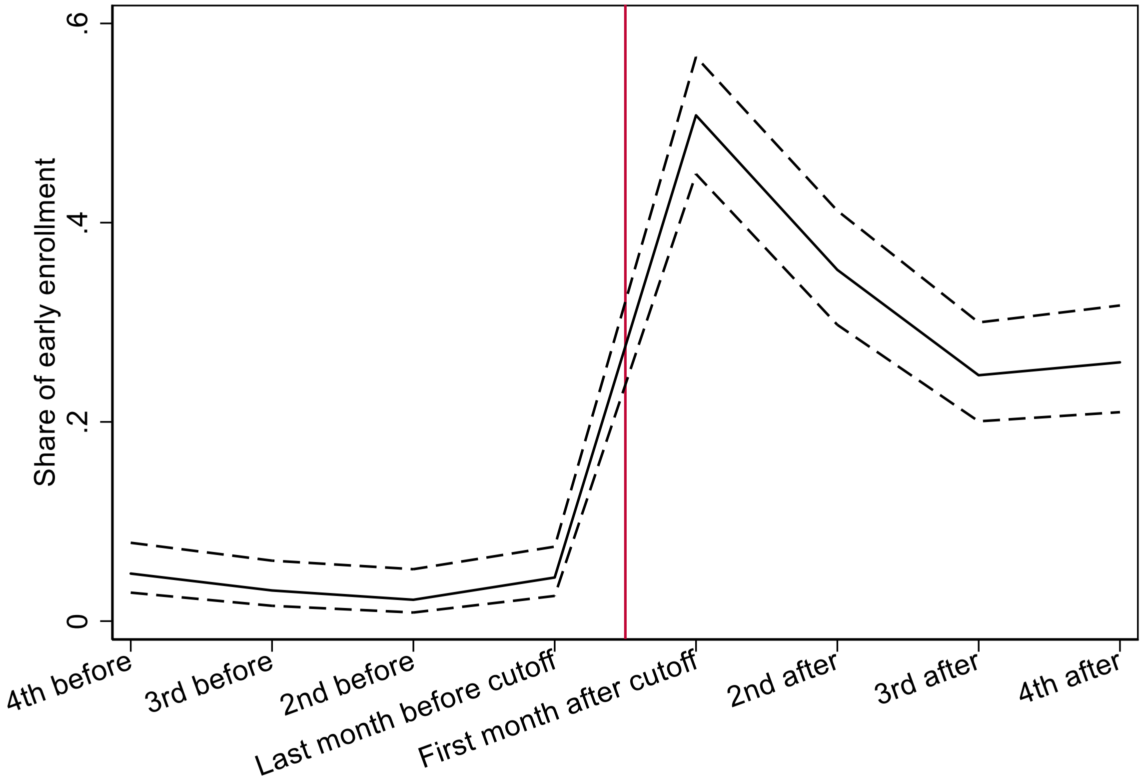
Appendix

Figure A-1. Log number of individuals in the sample by distance to the cutoff (male sample)



Note: The graph uses IAB data to illustrate the log number of male individuals by birth month relative to the cutoff in states and birth cohorts with early enrollment permission. Dashed lines indicate the bounds of the 95% confidence interval.

Figure A-2. Share of women with early enrollment by distance to the cutoff



Note: The graph uses NEPS data to illustrate the share of early enrollment by birth month relative to the cutoff for woman in states and birth cohorts with early enrollment permission. Dashed lines indicate the bounds of the 95% confidence interval.

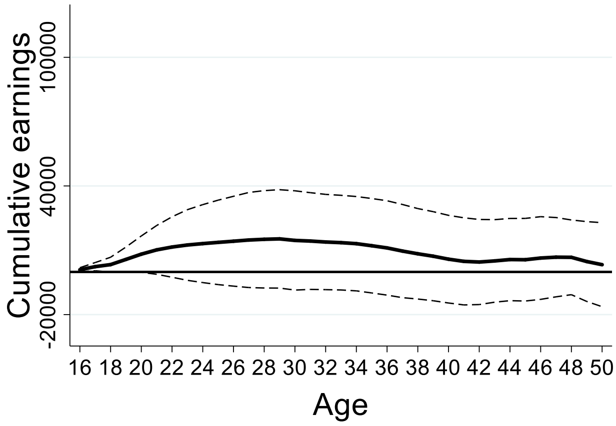
Figure A-3. The effect of early enrollment on log wages over the life cycle for women

Note: The figures use IAB data to illustrate the two-sample IV estimates of early enrollment on log wages measured at different ages (solid lines). The sample is restricted to women. The upper graph shows the results for the whole sample. The graph in the middle presents the effects for a sample of individuals born before 1976 for whom complete information on labor market biographies is available up to age 40. The lower graph displays the findings when deleting data for those with small daily wages (< 10 Euros). Dashed lines represent 90% confidence bands (based on two-sample-IV-adjusted and clustered standard errors).

Figure A-4. The effect of early enrollment on age-specific labor force participation of women

Note: The figures are based on IAB data and illustrate the two-sample IV estimates of early enrollment on the probability of being employed in women's month of birth by age. The sample is restricted to women. The upper graph contains the results for the whole sample, and the lower graph presents the results for the sample of individuals born before 1976 for whom complete labor market biographies are available up to age 40. Dashed lines represent 90% confidence bands (based on two-sample-IV-adjusted and clustered standard errors).

Figure A-5. The effect of early enrollment on cumulative earnings of women



Note: The figures are based on IAB data and illustrate the two-sample IV estimates of early enrollment on cumulative earnings by age. The sample is restricted to women. Dashed lines represent 90% confidence bands (based on two-sample-IV-adjusted and clustered standard errors).

Table A-1. Average characteristics of early enrollees compared to compliers with cutoff rule

Mean of parents' characteristics and siblings	Early enrollees	Compliers with cutoff	t value of difference		Early enrollees	Compliers with cutoff	t value of difference
	Men				Women		
Mother with college degree (y/n)	0.056	0.063	-0.57		0.052	0.055	-0.33
Mother's age at birth	26.844	27.604	-2.85	***	27.316	27.534	-0.88
Mother foreign born (y/n)	0.089	0.089	0.04		0.086	0.089	-0.26
Father with college degree (y/n)	0.142	0.147	-0.32		0.145	0.144	0.06
Father's age at birth	30.032	30.813	-2.59	***	30.457	30.481	-0.08
Father foreign born (y/n)	0.111	0.090	1.51		0.081	0.097	-1.24
Numer of older siblings	0.784	0.910	-2.14	**	0.954	0.998	-0.79

Note: Using the NEPS data, the table shows average characteristics of early enrollees compared to compliers with the cutoff rule. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-2. Estimates for bunching of observations (male sample)

	Log # observations
1st month after the cutoff in a state with early enrollment (y/n)	-0.013 (0.042)
2nd month after the cutoff in a state with early enrollment (y/n)	-0.011 (0.047)
3rd month after the cutoff in a state with early enrollment (y/n)	0.011 (0.029)
4th month after the cutoff (or later) in a state with early enrollment (y/n)	-0.035 (0.029)
Observations (state-year-month combinations)	3,360

Note: Using the IAB data, the table documents whether the log number of observations on the state-year-month level are influenced by early enrollment regulations. Control variables include dummies for states, year-of-birth dummies, month-of-birth dummies and dummies for the distance to the cutoff. Statistical significance: p<0.1 *, p<0.05 **, p<0.01 ***.

Table A-3. Sensitivity analyses of the impact of early enrollment on educational attainment

			Binary indicators for highest schooling degree				
	Years of schooling	Years spent in school	No degree attained	Lower track degree	Medium track degree	High school	Age at first employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
The effect of early enrollment	-0.288 **	-0.238 ***	0.001	0.057 ***	0.001	-0.059 ***	-1.115 **
	(0.123)	(0.088)	(0.005)	(0.021)	(0.010)	(0.022)	(0.489)
Obervations	2,536,011	2,536,011	2,536,011	2,536,011	2,536,011	2,536,011	2,565,150

Note: Using the IAB data, the table documents the results of the two-sample IV estimates of early enrollment on men's educational outcomes for the sample of individuals born before 1976. Years of schooling include compulsory and secondary schooling, vocational education and college attendance. Years spent in school only consider compulsory and secondary school attendance, excluding vocational education and college. The highest schooling degree refers to binary variables distinguishing no school degree, a lower school degree referring to the lowest track that mainly prepares students for blue-collar occupations, medium school degree preparing students for white-collar occupations and a high school degree that allows college entrance. Standard errors that are based on two-sample-IV-adjusted and clustered standard errors are shown in parentheses. Statistical significance: p<0.1 *, p<0.05 **, p<0.01 ***.

Table A-4 The impact of early enrollment on education and first labor market entry for women

			Binary indicators for highest schooling degree				
	Years of schooling	Years spent in school	No degree attained	Lower track degree	Medium track degree	High school	Age at first employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Womens' effect of early enrollment	-0.190	-0.237	0.006	0.058 ***	-0.009	-0.054	-0.175
	(0.134)	(0.147)	(0.005)	(0.021)	(.027)	(0.040)	(0.190)
Obervations	3,624,588	3,624,588	3,624,588	3,624,588	3,624,588	3,624,588	3,712,566

Note: Using the IAB data, the table documents the results of the two-sample IV estimates of early enrollment on women's educational outcomes and first labor market entry. Years of schooling include compulsory and secondary schooling, vocational education and college attendance. Years spent in school only consider compulsory and secondary school attendance, excluding vocational education and college. The highest schooling degree earned is reflected in binary variables distinguishing no school degree, a lower school degree referring to a degree from the lowest track that mainly prepares students for a blue-collar occupation, medium school degree preparing for white-collar occupations and a high school degree that allows university entrance. Standard errors that are based on two-sample-IV-adjusted and clustered standard errors are shown in parentheses. Statistical significance: $p<0.1$ *, $p<0.05$ **, $p<0.01$ ***.