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Abstract
Determining the optimal age at which a child should enter school is a controversial topic in education policy. In particular, German policy makers, pedagogues, parents, and teachers have since long discussed whether the traditional, established age of school entry at 6 years remains appropriate. Policies of encouraging early school entry or increased consideration of a particular child's competency for school ("Schulfähigkeit") have been suggested. Using a dataset capturing children who entered school in the late 1960s through the late 1970s, a time when delaying enrolment was common, we investigate the effect of age at school entry on educational attainment for West and East Germany. Empirical results from linear probability models and matching suggest a qualitatively negative relation between the age at school entry and educational outcomes both in terms of schooling degree and probability of having to repeat a grade. These findings are likely driven by unobserved ability differences between early and late entrants. We therefore use a cut-off date rule and the corresponding age at school entry according to the regulation to instrument the actual age at school entry. The IV estimates suggest there is no effect of age at school entry on educational performance.

JEL-Classification: I21, J13

Keywords: Education, Schooling, Matching, Instrumental Variables

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1. Introduction

Consider two traditional tests aimed at determining whether a child is mature enough to enter school. The "apple-or-coin-test" has its origin in the Middle Ages and goes as follows: A child who is not yet enrolled in school is given the choice between an apple and a coin. If the child chooses the apple, she has to remain in maternal custody. If the child chooses the coin instead, she is considered "worthy of instruction in knightly arts" (Meiers 2002), i.e. she is considered mature enough to engage in formal education. A different test uses the "Philippinian Measure" ("Philippinermaß") to determine a particular child's maturity. The child has to reach over the head with her right arm to see whether she can seize her left earlobe. If so, then the child is considered mature enough for school (Meiers 2002).

While at first glance these two tests appear odd, they are in fact not at all absurd, but instead reflect certain notions about factors that determine a child's aptitude to be formally educated. The apple-or-coin-test intends to determine whether there has been any change in the child's attitudes towards the things surrounding her, i.e. whether she is able to distinguish between things aimed at satisfying immediate, essential desiderata, and things with a more secondary, abstract value. Choosing the coin instead of the apple then indicates a certain degree of mental development presumably being a requirement for education in school.

The second test clearly aims at determining whether the child is mature enough in terms of physical development, at an age when bodily proportions – in particular the size of the head relative to the rest of the body – change significantly. Whereas modern school enrolment tests in Germany – and presumably in other countries as well – are certainly much more refined in many regards, it has been pointed out that their capacity to correctly assess a particular child's "competency for school" ("Schulfähigkeit") frequently remains unclear (Barth 1997; Meiers 2002). This relates to the difficulty in judging whether mental or physical development should be given more relevance, and to what extent e.g. physical development, which is generally easier to measure, simultaneously serves as an indicator of mental development.

Most of us are familiar with an even coarser version of how readiness to enter school can be determined. This coarser version regards simply the "proper" age for a child to enter school, i.e. the idea that age serves as the single indicator for competency for school. This particular issue has since long been a controversial topic in education policy in Germany, and the discussion has involved everybody from politicians to pedagogues, psychologists, parents, and teachers (e.g. Schavan, Wendt 2000). Traditionally, dating back to at least the 17^{th} century in Germany (Rüdiger et al. 1976), there exists the notion that children are ready for formal education around the age of 6. This notion is still reflected in current German schooling laws, which make school enrolment compulsory for children having reached that age. At the same time, German schooling laws allow for both early enrolment and deferment, where since the 1950s there has been a general tendency to increase the average age at school entry in (West) Germany (Rüdiger et al. 1976). This has led to a current perception by the public of a "delayed" school entry (Wendt 2003), reinforcing the controversy regarding the appropriate age for school enrolment, in particular since negative results for the German schooling system from the PISA study have been suggested to be due, in part, to the comparatively high average age at school entry (6 years and 7 months in the mid-1990s).

The crux of the entire discussion, of course, must be the idea that it *does matter* at which age individuals start school, in terms of individual outcomes such as schooling and labor market performance, and therefore also in terms of societal outcomes such as aggregate human capital and aggregate labor market performance relative to other countries. In this regard, the importance of early childhood education, for instance, is well documented (e.g. Heckman 2000; Currie 2001).

Why would we think that a 6-year old child, i.e. a child who is old enough to enter school according to the regulation, should rather wait another year and enroll at the age of 7? The rationale behind deferring a child in this way is the supposition that the deferment will prevent a child considered not mature enough to enroll from failure in school (usually expressed in having to repeat grades), which she would have experienced had she entered at the regular age[1]. In other words, children who enter school at an older age could be expected to be more successful in their educational careers than they (themselves) would have been if they had enrolled at a younger age. Some older studies in the educational psychology literature (reviewed in Angrist, Krueger 1992) find evidence for this "conventional" view that older school entrants fare better, even though the empirical evidence generally stems from small samples of observations and age at school entry is treated as an exogenous variable. Arguing that the years of education that a child attains may be a more appropriate measure of academic success, Angrist/Krueger (1992) examine the effect of age at school entry on the eventual years of schooling attained for the US. They identify this effect using exogenous variation in school starting age – coming from the quarter of the year in which a child is born – and school leaving age regulations. Their results suggest that older entrants tend to attain slightly less education, implying that the case for an effect of school starting age on educational attainment is modest at best.

[1] In Germany, this procedure is in line with a general objective of education policy to equalize students' early endowments when starting formal education (also Currie 2001).

The more recent empirical educational literature on the relation between age at school entry and educational performance (reviewed in Fredriksson, Öckert 2004) finds no conclusive evidence. Some studies compare outcomes of children who entered school regularly, but who differ in birth dates within the year. These studies suggest that the youngest children in the class score slightly below their older peers, though the differences tend to be small and transitory. Some other studies compare outcomes of children with delayed entry to children who entered regularly, typically finding that deferred children perform less well than their same-age peers. Fredriksson/Öckert (2004) suspect that these results may be misleading since they are likely driven by ability bias, i.e. children who are conjectured to display low educational performance are more likely to enter late. Our approach is similar to this type of study, although we intend to control for ability bias using three different identification strategies, in particular an instrumental variable (IV) approach.

This paper provides empirical evidence on the relation between the age at school entry and educational outcomes in Germany. It is written at a time of increased research effort by economists in investigating the effects of compulsory schooling laws, in particular school entry regulations, in various European countries (e.g. Del Bono, Galindo-Rueda 2004; Fredriksson, Öckert 2004; Leuven et al. 2004; Strøm 2003). Evidence from this research is mixed, though there is an indication for some countries that starting school at an older age might be beneficial for educational achievement (Fredriksson, Öckert 2004 for Sweden; Strøm 2003 for Norway).

In our analysis we consider a sample of individuals who entered school between 1966 and 1980, and we therefore distinguish between East and West Germany, where the respective schooling laws were similar at the time, yet differed in some details. As treatment variables, we consider (a) the age at school entry in months as a continuous regressor, and (b) being deferred, i.e. enrolling at age 7 versus enrolling at age 6 as a binary regressor. Our outcome variables capture educational performance in terms of the probability of repeating a grade and the eventual schooling degree attained. We estimate linear probability models and implement a matching estimator. Both methods find a negative relation between the age at school entry and schooling performance, which we attribute to unobserved heterogeneity. We complement these results using an IV approach, instrumenting the actual age at school entry with the age at school entry according to the regulation, both for the continuous and the binary case. Our findings indicate that there is essentially no effect of age at school entry on educational performance, neither in West nor in East Germany.

The paper is organized as follows. Section 2 discusses the historical context and the institutional background regarding school enrolment regulations in

West and East Germany, as well as educational outcomes. Section 3 presents the data and estimation strategy. In section 4 we discuss the empirical results. Section 5 concludes.

2. Age at School Entry and Educational Attainment

Historical context[2]

Historically, the issue of school entry regulations emerges with the idea of a broad, though not yet compulsory, elementary education of all citizens in the German territorial states during the 15th and 16th century. In those days, entry regulations mostly consisted in general requests to the parents, to send their children to school once they start to act "rational". Some school regulations of the 17th century already specify school entry ages of 6 years and 5 years, respectively. Even then the two main criteria for school entry are given – age and level of physical and mental development – and early writers (Comenius 1633) recognize the need for individual flexibility in enrolment decisions. In fact, those children that actually go to school in those days start school between the ages of 5 and 7.

With the age of enlightenment begins the time of state-regulated schooling laws governing now mandatory education. This development calls for more precise entry rules, and indeed regulations increasingly exhibit specifications of the starting point and the duration of compulsory schooling. At the same time, the level of development of a child is largely equated with her age, simplifying enrolment rules and enabling the establishment of grades that bring together age groups. By the early 19th century a general tendency to fix the school entry age at 6 years has emerged. Since then, the basic elements of school entry regulations have been defined: School entry decisions are mainly based on age, while possibilities of deferment and early enrolment exist.

The Elementary Schooling Law ("Grundschulgesetz") of 1920 establishes mandatory attendance at public elementary schools for all children in Germany. In 1922 a cut-off date rule is implemented in Prussia, specifying that all children reaching the age of 6 years on or before June 30th of a given year shall enroll in school during that year, where instruction begins on Easter. A corresponding law adopts this regulation in 1938 ("Reichsschulpflichtgesetz") and adds time limits for application for early enrolment of children displaying sufficient physical and mental maturity for school. The core features of this law constitute the basis of school entry regulations in Germany to date.

However, after 1945 the West German federal states ("Länder") utilize their regained autonomy in cultural and educational matters to enact individual

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2 This overview is based on the detailed exposition in Rüdiger et al. (1976).

compulsory schooling laws, which generally differ only in minor details. The following decades show a tendency to ever increase the average age at school entry, and an expanded practice of deferring children is supposed to add to their "additional maturing" and prevent grade repetition. In 1964, the prime ministers of the West German federal states agree on a uniform regulation specifying that school attendance becomes mandatory on August 1st of a given year for all children who have reached 6 years of age by June 30th of that year. Children who turn 6 between July 1st and December 31st of that year may enroll early given sufficient physical and mental maturity. Children having reached 6 years of age by June 30th who are not considered mature enough for school may be deferred for one year. Being deferred does not reduce the 9-year duration of mandatory school attendance.

In East Germany, very similar regulations were enacted in 1965 as part of the law on the uniform socialist education system ("Gesetz über das einheitliche sozialistische Bildungssystem"). School attendance becomes mandatory on September 1st of a given year for all children who have reached 6 years of age by May 31st of that year. Both early enrolments and deferments are possible, but enrolment decisions are based on the notion that – rather than demanding a certain degree of competency for school at the start of instruction – a particular child's competency for school will develop during the first few weeks or months of school attendance. This is in line with a general tendency to keep deferment numbers low and encourage early enrolment in "unambiguous" cases (Rüdiger et al. 1976: 32). Being deferred does not reduce the 10-year duration of mandatory school attendance.

Institutional background during our sample period

The sample we use contains children who entered school at some point in time during the period 1966–1980 (section 3 for details on the data). First, this implies that – since these are pre-unification data – we will continue to distinguish between West and East Germany. Secondly, this implies that the school enrolment regulations outlined above (i.e. the school entry laws enacted in 1964 and 1965 for West and East Germany, respectively) constitute the relevant institutional background for our study.

In West Germany, the school year technically begins August 1st, with instruction starting sometime in-between early August and mid-September, depending on the federal state. Cut-off date for determining enrolment is June 30th, and for all children having reached 6 years of age on the specific June 30th preceding the start of the school year, school enrolment becomes mandatory. Children born on July 1st or after, but before the beginning of instruction, would be deferred according to the regulation, and would then enroll the subsequent year at the age of 7. Exceptions are possible: Children born before June 30th who are not considered mature enough may be deferred, and chil-

Table 1

Age at School Entry and Month of Birth – West Germany

Month of birth	Age at school entry				
	5	6	7	8	Total
January	2	120	20	0	142
February	1	138	20	0	159
March	2	144	26	0	172
April	0	151	35	0	186
May	2	141	30	0	173
June	0	122	32	0	154
July	6	99	60	1	166
August	10	90	78	1	179
September	15	75	32	1	123
October	13	97	44	0	154
November	7	87	28	0	122
December	9	100	30	1	140
Total	67	1,364	435	4	1,870

dren born after July 1st who are considered mature enough may still enroll. Regulations on determining maturity, and hence enrolment and deferment decisions, are somewhat vague: in some cases parental application is sufficient, in some cases approval by the school and/or a public health officer is required, and sometimes decisions are based on a test. This leads to the fact that there is possible variation in enrolment practices over time and across federal states, and even between neighboring schools (Rüdiger et al. 1976: 27).

The bottom line is that, in principle, the cut-off date rule is unambiguous and would create a natural experiment, since the cut-off date is an exogenous device that would allow comparing educational outcomes of students born in June (who enter at age 6) and students born in July (who enter at age 7) to estimate the causal effect of one additional year outside school on these outcomes. However, in practice various exceptions are possible, and we cannot observe for what reason (parental application, test result, school approval / denial) a child that should have enrolled was deferred, and vice versa.

As Table 1 shows for the West German individuals in our data, both types of exceptions (enrolment of the too young, deferment of the old enough) did occur frequently. The grey cells indicate those children of ages 6 and 7 who enrolled regularly. If the cut-off date rule had been complied with sharply, there would be no observations for "age at school entry=6 and month= July or August" (all of these would have been deferred) and for "age at school entry=7 and month=June, May, April, etc." (all of these would have enrolled at the age of 6). Still, a substantial change in the distribution of age at school entry between the birth months of June and July is clearly visible.

Table 2

Age at School Entry and Month of Birth – East Germany

Month of birth	Age at school entry				
	5	6	7	8	Total
January	0	67	13	0	80
February	1	73	15	1	90
March	0	65	16	0	81
April	0	73	8	0	81
May	0	72	15	0	87
June	0	16	66	0	82
July	0	12	55	0	67
August	0	9	68	1	78
September	0	47	28	0	75
October	0	46	27	0	73
November	0	43	14	0	57
December	0	43	18	1	62
Total	1	566	343	3	913

Table 2 reports the corresponding numbers for East Germany, where the cut-off date was May 31st and school generally started on September 1st, generating a three-month window for regular deferment (June, July, August), rather than the two-month window (July, August) in the West. In general, there appear to be fewer exceptions as regards early enrolment or deferment, and regulations were complied with to a larger extent than in the West. In particular, the change in the distribution of age at school entry from May to June is apparent.

Educational Attainment

Looking at the regulations governing school enrolment the question arises if there is any relation between the particular implementation of these rules and educational outcomes, i.e., specifically, if there is any effect of the age at school entry on educational attainment. This is of particular interest since the practice of deferment is expressly geared towards prevention of failure in school, where "failure" usually means having to repeat a grade. In other words, we would expect deferred children, i.e. those who enter at age 7, to attain better educational outcomes than they (themselves) would have attained if they had started school at age 6. This is precisely the counterfactual question we will try to answer.

We are able to consider two outcome measures capturing school achievement. First, as the more short-term outcome, we take into account whether a child has ever repeated a class. This is the outcome that is usually seen as directly connected with school enrolment decisions. Secondly, we focus on the schooling degree that a child attains, to capture long-term effects. Three categories of schooling degrees are considered: (i) completed secondary degree or less, (ii) intermediary degree, and (iii) upper secondary or technical degree. For West Germany, these categories represent (i) "Hauptschule", i.e. 9 years of schooling, (ii) "Realschule" (10 years), and (iii) "Abitur" (13 years). For East Germany, since the duration of compulsory schooling is 10 years, the bottom category (i) captures drop-outs only, while (ii) represents "Polytechnische Oberschule" (10 years), and (iii) "Erweiterte Oberschule" (12 years).

3. Data and Empirical Strategy

In our empirical application we utilize data from the *Young Adult Longitudinal Survey 1991–1995/1996* ("Junge-Erwachsene-Längsschnitt") conducted among 18-29 year old individuals in East and West Germany in 1991, 1993 and 1995/1996. This survey contains a large set of retrospective questions with the explicit aim to reveal information about the respondents' transition from childhood to adolescence and further on to (young) adulthood. In addition, the dataset provides standard socio-demographic characteristics on the respondent and some core characteristics for her parents. Information on the parent-child relationship is also included. The latter is a unique feature of this dataset and an advantage compared to competing datasets like the German Socio-Economic Panel.

Table 11 in the appendix gives detailed descriptions of the variables we consider. As mentioned above, the two outcome measures of interest are (i) whether the individual has ever repeated a class and (ii) the schooling degree the individual has attained. The three categories on the schooling degree are coded into two dummy variables: The first dummy takes on the value 1 if the individual holds a high schooling degree (completed upper level of secondary education allowing university entrance) and zero otherwise. The second dummy takes on the value 1 if the individual attained a low schooling degree (completed lower level of secondary education or no secondary education degree) and zero otherwise.

Our central variable of interest is the age at which an individual entered school. To provide a comprehensive analysis of the relationship between school entry age and the considered educational outcomes we implement two different models. In the first model, age at school entry is measured in months and utilized as a continuous regressor. In the second model, we explicitly address the practice of deferment and employ a dummy treatment model. In this model school entry age is captured by a binary variable which takes on the value 1 if an individual entered school at the age of 7, and zero for the age of 6. Those individuals who entered school at the age of 5 or 8 are dropped from the sample in this case.

Additionally, the data allow us to control for a set of variables characterizing the individual child, the parental background, and the parent-child relationship during early childhood. The latter is an especially important control variable, as it captures much of the unobserved parent-child interaction that might play a latent role in making deferment/enrolment decisions. Specifically we consider as individual characteristics gender, year of birth, religion, number of siblings (and its square), and an indicator for having contact to peers. Parental background is captured by mother's and father's education. The relationship between parents and child during childhood is expressed in the incidence of joint leisure activities, and parental attitudes towards their child (Table 11).

Table 12 in the Appendix presents summary statistics for all variables. Many of them display a similar distribution for West and East Germany. It is interesting to note, though, that there is much less incidence of grade repetition in East Germany. Also, the low average probability of attaining "low schooling" in the East reflects that this category captures drop-outs only, as outlined above. The same reasoning likely applies to the lower numbers of parents with low education in the East. As expected, most East Germans report no religious denomination, while this is the opposite for West Germans. Looking at our sample, the number of observations is relatively small compared to e.g. the administrative data available to researchers in Sweden (Fredriksson/Öckert 2004), but our data have the advantage that they contain direct information on the age at school entry, and we do not have to construct this from other sources.

In principle, school entry regulations in both East and West generate a natural experiment. If the cut-off date had been strictly complied with, this administrative regulation would act as a quasi-randomization device and there would be no reason to expect that the composition of students entering school in a given year at age 6 is different from the composition of students entering it one year later at age 7. In fact, with respect to observable characteristics this seems to be the case. Tables 3 and 4 report the results from two models in which age at school entry is regressed on the set of observable covariates. The estimation results suggest that almost all observable characteristics do not exhibit an impact on age at school entry at conventional significance levels. For the case of West Germany (Table 3) the F-test on joint significance of all explanatory variables indicates that the model as a whole is not able to explain age at school entry significantly.

In East Germany (Table 4) we observe a tendency for older cohorts to enter school at a younger age. However, only the model for age at school entry in months is significant at the 5% level, whereas all explanatory variables are jointly zero for the case of the dummy outcome model.

However, it is anything but guaranteed that quasi-randomization also holds with respect to unobservable characteristics. Figure 1 indicates that in both

Table 3

Determinants of School Entry Age – West Germany

	Outcome: actual school entry age in months		Outcome: dummy for school entry at age 7	
	Coefficient	t-value	Coefficient	t-value
Female	0.1733	0.56	0.0097	0.47
Year of birth	–0.0721	–1.51	–0.0060	–1.86
Number of siblings	–0.2090	–0.66	0.0044	0.21
Number of siblings, squared	0.0254	0.36	–0.0001	–0.02
Atheist	–0.0839	–0.16	0.0429	1.23
Peers	–0.1830	–0.44	0.0372	1.32
Father low education	–0.3564	–0.83	–0.0034	–0.12
Mother low education	0.1253	0.30	–0.0080	–0.28
Father high education	–0.2213	–0.44	–0.0138	–0.40
Mother high education	–0.4074	–0.71	0.0081	0.20
Joint activities	–0.6947	–1.98	–0.0466	–1.96
Parental attitudes	0.1088	0.35	–0.0206	–0.97
Constant	85.0136	25.59	0.6336	2.82
Number of observations	1,788		1,718	
F-Test[1]	0.81		1.08	

Authors' calculations. – [1]F-Test on hypothesis that all coefficients except the constant are zero.

Table 4

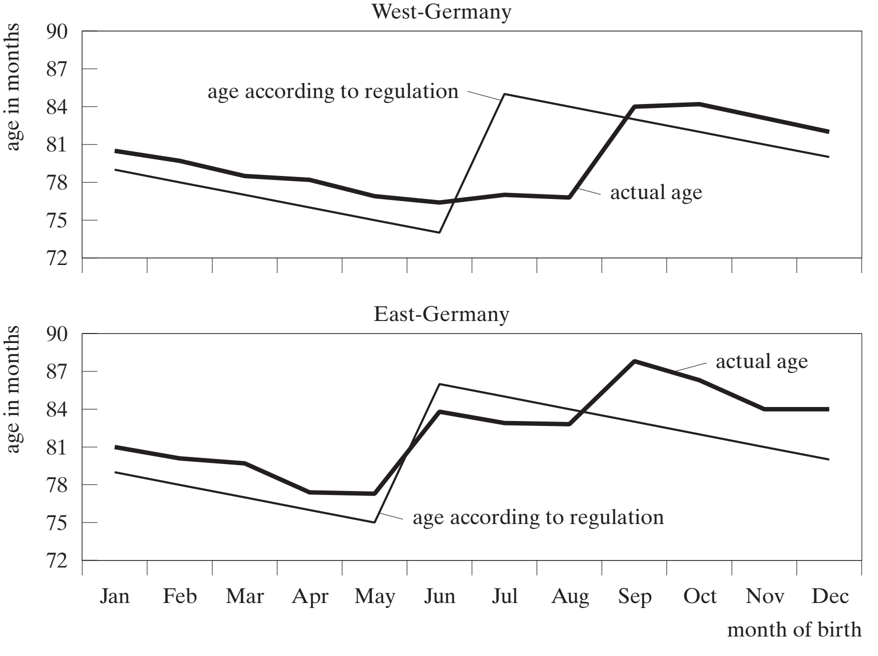
Determinants of School Entry Age – East Germany

	Outcome: actual school entry age in months		Outcome: dummy for school entry at age 7	
	Coefficient	t-value	Coefficient	t-value
Female	–0.1541	–0.38	–0.0478	–1.42
Year of birth	–0.2031	–3.18	–0.0113	–2.11
Number of siblings	–0.5342	–1.15	–0.0019	–0.05
Number of siblings, squared	0.1259	1.16	–0.0022	–0.24
Atheist	–0.9995	–1.97	–0.0351	–0.83
Peers	0.3211	0.65	0.0123	0.30
Father low education	–0.1595	–0.29	0.0113	0.24
Mother low education	0.2414	0.45	–0.0262	–0.58
Father high education	–0.1850	–0.34	–0.0061	–0.13
Mother high education	–0.8268	–1.26	–0.0962	–1.76
Joint activities	–0.4048	–0.87	–0.0168	–0.43
Parental attitudes	–0.0157	–0.04	0.0168	0.47
Constant	96.9564	21.72	1.2148	3.25
Number of observations	859		855	
F-Test[1]	1.77*		1.01	

Authors' calculations. – [1]F-Test on hypothesis that all coefficients except the constant are zero. – *Significant at 5%-level.

parts of the country adherence to the cut-off-date regulation was not perfect, i.e. a considerable share of individuals entered school at an older age than they were supposed to, and vice versa. Thus, deferments and early enrollments were common during our sample period. In fact, compliance with the cut-off-date

Figure 1
Actual age at school entry and age according to regulation



Authors' calculations.

regulation was particularly weak in West Germany during the months determining regular deferment, July and August.

Since the reasons behind deferment and early enrollment decisions are unobservable in the data, and since practices varied over time and on the federal state and school levels, unobserved heterogeneity might be a severe problem. In particular, for children born around the cut-off-date parents could have applied for waiving of deferment or in favor of early enrollment. In consequence, e.g., for individuals who were born shortly after the cut-off-date and entered school at the age of seven one of the following three scenarios applies: (i) parents did not apply for waiving of deferment accidentally; (ii) parents did not apply for waiving of deferment because they considered their child as not mature/able enough for entering school; (iii) parents applied for waiving of deferment but this application was rejected. Thus, for those individuals who entered school at the age of seven because of scenarios (ii) or (iii), it is possible that they were lacking the ability to enter school at age 6.

On the other hand, for those individuals who entered school at a younger age than they were supposed to, it is likely that their ability was higher than that of their peers. Again, unobserved heterogeneity cannot be ruled out. In consequence, ignoring this issue might introduce a severe ability bias into estimation results.

In our empirical analysis we employ three different identification strategies to find an answer to the counterfactual question "What would have happened to those individuals entering school at a given age if they had entered school earlier (or later)?" The three identification strategies are

1. Parametric approach: linear probability model
2. Non-parametric approach: matching on the propensity score
3. Instrumental variable approach

Clearly, these identification strategies vary in their potential to cope with unobserved heterogeneity. We will now discuss these strategies briefly.

Linear probability model[3] *(LPM)*

We begin our empirical investigation utilizing LPMs to assess the relationship between age at school entry and educational outcomes. The central identification assumption of this approach is the exogeneity of all right-hand-side variables. This includes the assumption of no correlation between the explanatory variables and the error term. In the presence of unobserved heterogeneity this assumption is violated and estimates are biased. If those who entered school at an older age exhibit lower ability OLS estimates are biased downward.

Matching[4]

Matching methods mimic a randomized experiment, with the aim of inferring a causal effect of some specific treatment on certain outcome variables. This essentially requires identification of the relevant counterfactual, i.e. what would have happened to the treatment group if it had not been exposed to treatment? Then the causal effect is given by the difference between the factual (=exposed to treatment) and counterfactual (=not exposed to treatment) outcomes, for the population receiving the treatment. In our context the outcomes of interest are (i) educational attainment in terms of schooling degree and (ii) incidence of repeating a class. Treatment is school entrance at age 7 instead of at age 6.

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3 In a previous version of the paper we utilized probit and ordered probit (for the categorical schooling outcome) models. The models yield essentially the same results as the LPMs here. We chose to use LPMs for consistency of the presentation. All results using (ordered) probit models are available from the authors.

4 Matching is applied to the dummy treatment model only.

Thus, consider the following binary treatment: the child either being deferred and entering school late, or enrolling regularly at age 6. The variable $D \in \{0,1\}$ indicates the treatment received, i.e. D=1 if the child enters school late, D=0 if the child enters school regularly at age 6, and we observe the treatment that a specific child is exposed to and the outcome associated with this treatment:

$$Y = Y_0 \text{ if } D = 0,$$
$$Y = Y_1 \text{ if } D = 1,$$

where the variable Y captures post-treatment outcomes of the variable of interest, i.e. educational attainment expressed through schooling degree or incidence of repeating a class.[5] Thus, the unit level causal effect given by $\Delta = Y_1 - Y_0$ is never directly observable. The essential conceptual point is that nonetheless each individual child has two possible outcomes associated with herself, where one realization of the outcome variable can actually be observed for each individual, and the other one is a counterfactual outcome.[6]

Since individual-level effects cannot be observed, the estimand of interest should be a measure that summarizes individual gains from treatment appropriately. Of specific interest is the average treatment effect for the treated population (ATET),

$$E(\Delta | D=1) = E(Y_1 - Y_0 | D=1) = E(Y_1 | D=1) - E(Y_0 | D=1),$$

where the expectations operator $E(.)$ denotes population averages. Still, only the first of the population averages in the ATET parameter is identified from observable data, whereas the second one is not, since the outcome under no-treatment Y_0 is not observed for treated children D=1. This is precisely the counterfactual of interest: What outcome would the treated units have realized if they had not been exposed to the treatment? Since treatment is not randomly assigned, it is necessary to consider a vector of observed pre-treatment variables, or covariates, X, in order to identify the counterfactual. As delineated above, in our application, X consists of covariates characterizing the child, the parents, and the parent-child relationship.

Consider the following *identifying assumption*: The assignment mechanism D is independent of the potential outcomes (Y_0, Y_1) conditional on X. This conditional independence assumption is commonly referred to as *unconfoundedness* (Imbens 2000) or *strong ignorability* (Rosenbaum, Rubin 1983) and con-

5 To keep the notation simple there is no further distinction between Y indicating schooling degrees and Y indicating repeating a class at this point. The empirical analysis assesses effects on both outcomes.

6 This is the reason why this model for causal inference is frequently referred to as "Potential Outcome Model"; Holland (1986), Kluve (2003) for further discussion.

stitutes the counterpart to the exogeneity assumption in regression models. By the unconfoundedness assumption it is possible to replace the no-treatment outcome for the treated population with the no-treatment outcome of the non-treated, i.e. comparison, population[7]:

$$\begin{aligned} E(\Delta | X, D=1) &= E(Y_1 | X, D=1) - E(Y_0 | X, D=1) \\ &= E(Y_1 | X, D=1) - E(Y_0 | X, D=0). \end{aligned}$$

This covariate-adjusted ATET is identified from observable data. Instead of adjusting for the full vector X it is also possible to adjust for the propensity score, i.e. the conditional probability of receiving the treatment, given X (Rosenbaum, Rubin 1983; Rosenbaum 1995 for details).

Matching then proceeds as follows. We estimate propensity scores for full and core samples. Each observation from the treatment group is assigned as matches those observations from the pool of potential comparison observations that fall within a certain caliper distance (oversampling). The caliper is defined as the fraction $1/x$ of the standard deviation of the estimated score, and matching is performed for three different caliper distances (x=100, 200, 400). Matching proceeds with replacement. If in the oversampling technique two or more comparisons fall within the caliper, their outcomes are condensed into one single comparison outcome using the mean outcome of the multiple matches. The matching estimator for the average treatment effect for the treated population is thus given by

$$\hat{\Delta} = \frac{1}{N_t} \sum_{t=1}^{N_t} [Y_1(t) - \tilde{Y}_0(c \,|\, \hat{P}_c \in Cal(\hat{P}_t))]$$

where N_t denotes the number of treated children t, $Y_1(t)$ is the outcome of the treated observation, $\tilde{Y}_0(c|\cdot)$ is the mean outcome of matched comparisons whose estimated propensity score $\hat{P}_c$ falls within the caliper of the estimated propensity score of the treated observation $\hat{P}_t$.

Instrumental Variable Approach

In a third step we implement an IV approach. In this endeavor, we instrument the actual age at school entry in months using the age (in months) at which an individual should have entered school according to school entry regulations.[8]

[7] In fact, all that is required for identification of the ATET is *weak unconfoundedness* or *ignorability*, i.e. the assignment mechanism D is independent of the potential outcome Y_0 conditional on X. Both assumptions, however, do not differ in substantive terms and it has been argued that it is difficult to conceive of settings where weak unconfoundedness holds and unconfoundedness does not (Imbens 2000; Dehejia, Wahba 2002).

[8] Fredriksson/Öckert (2004) implement a similar approach for Sweden.

In the second model, in which entry age is modeled as a dummy variable for those entering school at the age of 7, the instrument is a dummy variable indicating whether an individual should have entered school at the age of 7 according to school entry regulations. In other words, this indicator variable takes on the value of 1 if an individual is born in July/August (West Germany) or June/July/August (East Germany).

The IV approach is able to cope with unobserved heterogeneity and, thus, to deliver unbiased estimates if two criteria are met. First, the instrument has to be correlated with age at school entry. In the case at hand, this means that there has to be sufficient compliance to school entry regulations since our instrument is the age at school entry according to the regulations. From Figure 1 it becomes transparent that a considerable share of individuals in our data did indeed enter school at the age that they were supposed to. Furthermore, first-stage IV results (Tables 13 and 14 in the Appendix) suggest that there is a strong correlation between the instrument and age at school entry.

The second criterion that has to be met for the instrument to be valid requires that it must not exert any direct impact on observed outcomes, i.e. it must not be correlated with students' unobserved ability. While – given the discussion above – it is very likely that the chosen instrument meets the first criterion, it is *a priori* not clear whether it also fulfills the second one. Naturally, it is not possible to test whether the instrument is uncorrelated with students' unobserved ability. In consequence, this choice is an identification assumption which has to be judged upon economic reasoning alone.

In the case at hand, the instrument stems from administrative regulations that were enacted in this exact form in 1964 and 1965 in West and East Germany, respectively, and that were left unchanged during our sample period. Hence, they can be perceived as truly exogenous as long as family planning does not react to them. In other words, the second criterion would likely be violated only if highly able parents have children with high ability and these parents plan the birth of their child so that it can enter school at the age of six. Apart from the fact that the exact planning of birth is anything but trivial, the possibility of waiving of deferment by parental application essentially exempts parents concerned about the age at which their child enters school from any planning endeavor.

4. Results

This section presents estimation results from the three different identification strategies outlined in the last section: (i) LPMs, (ii) matching on the propensity score, (iii) IV estimation. Due to the institutional differences (section 2) and different patterns of compliance with school entry regulations we estimate separate models for West and East Germany.

(i) Linear probability models

Tables 5 and 6 present estimation results of the LPM for West and East Germany, respectively. The outcome “schooling degree” is a categorical variable with three categories (Table 11). LPMs are estimated for two binary outcome measures of “schooling degree”; a “High schooling degree” indicates attainment of an upper secondary or technical schooling degree (=1), and 0 otherwise. A “low schooling degree” indicates attainment of only a completed secondary degree or less (=1), and 0 otherwise. The upper panels of Tables 5 and 6 report results for the first model including age at school entry as a continuous regressor. The bottom panels each report results for the dummy treatment model.

For both parts of the country, estimation results of the LPM indicate a negative association between age at school entry and educational outcomes. That is, an older age at school entry is associated with a higher probability to repeat a class, a lower probability to receive a high schooling degree in West Germany, and a higher probability to attain a low schooling degree or less in the Eastern part of the country, i.e. here to drop out of school. However, since the LPM does not take into account potential unobserved heterogeneity, the group of individuals entering late may contain individuals who differ systematically in unobserved characteristics (such as ability) from those who enter early. Hence, the possibility that these results are contaminated by ability bias cannot be ruled out.

Looking at the set of control variables in Tables 5 and 6 we find that family background variables seem to play a particularly important role for schooling degrees. High educational attainment of both mother and father increase the probability of the child to attain a high schooling degree, though it does not have a significant impact on attaining more than the lowest degree. Low education of parents is an even stronger predictor of the child’s outcomes: children from low-education families are less likely to attain a high schooling degree, and more likely to attain a low degree. However, this intergenerational dependence is less pronounced in East Germany.

With respect to the probability of repeating a class parental education does not play such an important role. Rather, there seem to be systematic gender differences and differences between birth cohorts. Females and earlier birth cohort display a lower probability to repeat a class. For all outcomes positive parental attitudes towards their child also exhibit a qualitatively positive impact in both parts of Germany. Estimation results for the covariates also do not seem to differ much between both models, i.e. it does not seem to make a difference whether age at school entry is modeled as a continuous or categorical variable.

Table 5

Results of Linear Probability Model – West Germany

	Outcome: repeat class		Outcome: high schooling		Outcome: low schooling	
	Coefficient	t-value	Coefficient	t-value	Coefficient	t-value
	Model with continuous regressor					
Actual age at school entry[1]	0.0025	1.74	–0.0037	–2.33	0.0008	0.54
Female	–0.0565	–3.04	–0.0088	–0.43	–0.1028	–5.11
Year of birth	0.0067	2.32	–0.0073	–2.30	–0.0028	–0.88
Number of siblings	0.0251	1.31	0.0365	1.74	–0.0669	–3.22
Number of siblings, squared	–0.0032	–0.76	–0.0103	–2.24	0.0180	3.95
Atheist	0.1253	4.07	–0.0223	–0.66	–0.0461	–1.38
Peers	0.0095	0.38	–0.0363	–1.32	–0.0292	–1.07
Father low education	0.0411	1.59	–0.1723	–6.09	0.2240	7.99
Mother low education	–0.0764	–3.05	–0.1004	–3.65	0.1147	4.21
Father high education	0.0151	0.50	0.1915	5.75	–0.0307	–0.93
Mother high education	–0.0040	–0.11	0.1287	3.38	–0.0202	–0.53
Joint activities	0.0029	0.14	–0.0030	–0.13	–0.0459	–2.00
Parental attitudes	–0.0535	–2.82	0.0603	2.90	–0.0536	–2.60
Constant	–0.4260	–1.81	1.2095	4.71	0.4010	1.57
Number of observations	1,786		1,788		1,788	
F-Test[2]	4.63		35.94		28.08	
	Dummy treatment model					
School entry at age 7	0.0622	2.84	–0.0609	–2.54	0.0268	1.12
Female	–0.0579	–3.07	–0.0112	–0.54	–0.0972	–4.69
Year of birth	0.0069	2.37	–0.0060	–1.87	–0.0029	–0.90
Number of siblings	0.0164	0.85	0.0403	1.90	–0.0684	–3.22
Number of siblings, squared	–0.0015	–0.35	–0.0111	–2.40	0.0183	3.95
Atheist	0.1194	3.77	–0.0325	–0.94	–0.0391	–1.13
Peers	–0.0029	–0.11	–0.0441	–1.58	–0.0259	–0.93
Father low education	0.0404	1.54	–0.1752	–6.11	0.2310	8.03
Mother low education	–0.0739	–2.91	–0.0965	–3.47	0.1131	4.06
Father high education	0.0077	0.25	0.1991	5.87	–0.0245	–0.72
Mother high education	0.0088	0.24	0.1158	2.95	–0.0249	–0.63
Joint activities	0.0081	0.38	–0.0074	–0.32	–0.0455	–1.92
Parental attitudes	–0.0507	–2.63	0.0613	2.90	–0.0508	–2.40
Constant	–0.2458	–1.21	0.8501	3.80	0.4622	2.06
Number of observations	1,716		1,718		1,718	
F-Test[2]	4.74		34.36		26.55	

Authors' calculations. – [1]Age in months. – [2]F-Test on hypothesis that all coefficients except the constant are zero.

(ii) Matching on the propensity score

The second identification strategy – propensity score matching – aims at obtaining an improved counterfactual by matching treated observations (i.e. individuals who entered school at the age of 7) with truly comparable (in terms

Table 6

Results of Linear Probability Model – East Germany

	Outcome: repeat class		Outcome: high schooling		Outcome: low schooling	
	Coefficient	t-value	Coefficient	t-value	Coefficient	t-value
	Model with continuous regressor					
Actual age at school entry[1]	0.0020	2.17	–0.0035	–1.45	0.0043	3.11
Female	0.0058	0.54	0.0263	0.93	–0.0305	–1.87
Year of birth	0.0000	0.00	–0.0149	–3.31	0.0058	2.21
Number of siblings	0.0265	2.14	–0.0048	–0.15	0.0055	0.29
Number of siblings, squared	–0.0039	–1.35	–0.0025	–0.33	0.0047	1.08
Atheist	–0.0006	–0.05	–0.0326	–0.92	–0.0075	–0.36
Peers	–0.0049	–0.37	0.0360	1.04	–0.0167	–0.84
Father low education	0.0144	0.97	–0.0927	–2.37	0.0247	1.09
Mother low education	–0.0172	–1.20	–0.0452	–1.20	0.0135	0.62
Father high education	–0.0136	–0.93	0.2060	5.39	–0.0172	–0.78
Mother high education	–0.0154	–0.88	0.1183	2.57	–0.0294	–1.11
Joint activities	–0.0048	–0.38	0.0796	2.43	–0.0019	–0.10
Parental attitudes	–0.0272	–2.41	0.1218	4.09	–0.0259	–1.50
Constant	–0.1341	–0.90	1.4524	3.72	–0.6598	–2.92
Number of observations	851		859		859	
F-Test[2]	1.96		14.56		3.63	
	Dummy treatment model					
School entry at age 7	0.0094	0.85	–0.0303	–1.05	0.0353	2.12
Female	0.0059	0.54	0.0270	0.95	–0.0317	–1.93
Year of birth	–0.0003	–0.20	–0.0146	–3.23	0.0057	2.17
Number of siblings	0.0251	2.00	–0.0069	–0.21	0.0087	0.46
Number of siblings, squared	–0.0035	–1.19	–0.0021	–0.27	0.0031	0.69
Atheist	–0.0025	–0.18	–0.0276	–0.77	–0.0116	–0.56
Peers	–0.0044	–0.33	0.0342	0.99	–0.0115	–0.57
Father low education	0.0136	0.91	–0.0879	–2.23	0.0290	1.28
Mother low education	–0.0160	–1.11	–0.0497	–1.32	0.0131	0.60
Father high education	–0.0142	–0.97	0.2082	5.43	–0.0155	–0.70
Mother high education	–0.0159	–0.91	0.1183	2.57	–0.0309	–1.16
Joint activities	–0.0052	–0.41	0.0798	2.42	–0.0068	–0.36
Parental attitudes	–0.0276	–2.42	0.1199	4.01	–0.0234	–1.36
Constant	0.0498	0.41	1.1568	3.66	–0.3130	–1.71
Number of observations	847		855		855	
F-Test[2]	1.65		14.34		3.02	

Authors' calculations. – [1]Age in months. – [2]F-Test on hypothesis that all coefficients except the constant are zero.

of observed covariates) comparison observations (i.e. individuals who entered school at the age of 6). Typically, matching approaches aim at getting rid of unobserved heterogeneity by controlling for pre-treatment outcomes. If there are unobserved differences between treatment and comparison groups and these differences are persistent over time, then they should be reflected in the

Table 7

Results of Propensity Score Matching for Dumy Treatment Model – West Germany

Caliper	No. of matches (in % of treated)	ATET	Standard-error	t-value
		Outcome: repeat class		
(i) 1/100* std.dev.	396 (96.6)	0.047	0.023	2.10
(ii) 1/200* std.dev.	375 (91.5)	0.044	0.024	1.83
(iii) 1/400* std.dev.	341 (83.2)	0.039	0.026	1.51
		Outcome: high schooling		
(i) 1/100* std.dev.	397 (96.8)	–0.059	0.024	–2.43
(ii) 1/200* std.dev.	374 (91.2)	–0.053	0.026	–2.01
(iii) 1/400* std.dev.	340 (82.9)	–0.064	0.029	–2.21
		Outcome: low schooling		
(i) 1/100* std.dev.	397 (96.8)	0.041	0.025	1.61
(ii) 1/200* std.dev.	374 (91.2)	0.034	0.028	1.23
(iii) 1/400* std.dev.	340 (82.9)	0.030	0.030	1.00

Authors' calculations. Number of treated observations: 410; number of observations in potential comparison pool: 1,308.

value of the outcome prior to the intervention and, thus, controlling for pre-treatment outcomes captures these unobservable differences. However, in the case at hand this is obviously not possible and, hence, unobserved heterogeneity remains a potential problem. In consequence, following the argument above, it cannot be ruled out that unconfoundedness does not hold for our sample.

Tables 7 and 8 report treatment effect estimates for West and East Germany considering the dummy treatment model and three caliper distances each. We observe that, as the caliper narrows and precision – i.e. comparability – of the match increases, the number of matches found is reduced. Roughly, we move from around 97% matches found for the widest caliper to around 83% for the narrowest caliper in West Germany. In East Germany the share of treated individuals finding a matching partner is somewhat lower (ranging from around 91% to 61%) due to the smaller pool of comparison units.

For West Germany matching results largely resemble the results found in the LPMs for both schooling degrees and the probability of class repetition. That is, we still observe a qualitatively negative relationship between school entry age and educational outcomes. For the Eastern part of the country, the com-

Table 8

Results of Propensity Score Matching – East Germany

Caliper	No. of matches (in % of treated)	ATET	Standard-error	t-value
		Outcome: repeat class		
(i) 1/100* std.dev.	293 (91.0)	0.009	0.011	0.85
(ii) 1/200* std.dev.	245 (76.1)	0.003	0.010	0.30
(iii) 1/400* std.dev.	193 (59.9)	-0.004	0.014	–0.28
		Outcome: high schooling		
(i) 1/100* std.dev.	295 (91.6)	–0.022	0.031	–0.69
(ii) 1/200* std.dev.	258 (80.1)	0.012	0.034	0.35
(iii) 1/400* std.dev.	195 (60.6)	0.015	0.039	0.40
		Outcome: low schooling		
(i) 1/100* std.dev.	295 (91.6)	0.030	0.018	1.62
(ii) 1/200* std.dev.	258 (80.1)	0.022	0.019	1.14
(iii) 1/400* std.dev.	195 (60.6)	0.013	0.023	0.58

Authors' calculations. Number of treated observations: 322; number of observations in potential comparison pool: 525.

parison of truly comparable individuals delivers no significant differences for all considered educational outcomes. However, the question remains whether matching indeed identifies the correct effect.

(iii) Instrumental variable approach

In order to use exogenous variation to identify the causal effect of age at school entry on schooling outcomes, we instrument age at school entry using the age at which an individual should have entered school according to school entry regulations, as delineated in section 3. Tables 13 and 14 in the Appendix report the first-stage IV results for West and East Germany, respectively. The estimates show a strong positive correlation between the instrument and age at school entry, for both models (continuous and binary instrument) and for both parts of the country. That is, we observe a sufficiently high compliance with school entry regulations in our sample.

Tables 9 and 10 report the IV estimates for West and East Germany, respectively, where the upper panels show estimates for the model with the continuous instrument. Regarding the set of control variables it is interesting to note that estimation results are very similar – both in sign and magnitude – to those reported for the LPMs above. Specifically, background information on paren-

Table 9

Instrumental Variable Estimates – West Germany

	Outcome: repeat class		Outcome: high schooling		Outcome: low schooling	
	Coefficient	t-value	Coefficient	t-value	Coefficient	t-value
	Model with continuous regressor					
Actual age at school entry[1]	–0.0097	–0.91	–0.0002	–0.02	–0.0001	–0.01
Female	–0.0546	–2.87	–0.0094	–0.46	–0.1027	–5.08
Year of birth	0.0059	1.92	–0.0070	–2.15	–0.0028	–0.88
Number of siblings	0.0225	1.14	0.0372	1.76	–0.0671	–3.21
Number of siblings, squared	–0.0029	–0.67	–0.0104	–2.25	0.0180	3.94
Atheist	0.1246	3.96	–0.0221	–0.65	–0.0462	–1.38
Peers	0.0074	0.29	–0.0357	–1.29	–0.0294	–1.08
Father low education	0.0369	1.38	–0.1711	–5.98	0.2237	7.89
Mother low education	–0.0748	–2.92	–0.1008	–3.66	0.1148	4.21
Father high education	0.0125	0.40	0.1923	5.74	–0.0310	–0.93
Mother high education	–0.0086	–0.24	0.1301	3.39	–0.0206	–0.54
Joint activities	–0.0055	–0.24	–0.0006	–0.02	–0.0465	–1.92
Parental attitudes	–0.0522	–2.69	0.0599	2.87	–0.0535	–2.59
Constant	0.6061	0.66	0.9158	0.92	0.4848	0.49
Number of observations	1,786		1,788		1,788	
F-Test[2]	4.29		35.34		28.05	
	Dummy treatment model					
School entry at age 7	–0.0851	–0.75	–0.1013	–0.82	0.1634	1.32
Female	–0.0566	–2.96	–0.0108	–0.52	–0.0985	–4.71
Year of birth	0.0061	1.99	–0.0063	–1.90	–0.0021	–0.63
Number of siblings	0.0170	0.87	0.0404	1.91	–0.0690	–3.22
Number of siblings, squared	–0.0015	–0.35	–0.0111	–2.40	0.0184	3.92
Atheist	0.1259	3.88	–0.0308	–0.88	–0.0450	–1.27
Peers	0.0026	0.10	–0.0426	–1.51	–0.0310	–1.08
Father low education	0.0399	1.51	–0.1754	–6.11	0.2314	7.97
Mother low education	–0.0751	–2.92	–0.0968	–3.48	0.1142	4.05
Father high education	0.0057	0.18	0.1986	5.84	–0.0226	–0.66
Mother high education	0.0101	0.28	0.1161	2.96	–0.0260	–0.65
Joint activities	0.0013	0.06	–0.0093	–0.38	–0.0392	–1.59
Parental attitudes	–0.0538	–2.74	0.0604	2.84	–0.0480	–2.23
Constant	–0.1530	–0.70	0.8757	3.71	0.3757	1.57
Number of observations	1,716		1,718		1,718	
F-Test[2]	4.06		33.86		26.10	

Authors' calculations. – [1]Age in months. – [2]F-Test on hypothesis that all coefficients except the constant are zero.

tal education and attitudes retains its importance. The main result is unambiguous: The IV estimates find no significant effect of age at school entry on any of the outcome measures, neither for the model with the continuous regressor nor the dummy treatment model, neither for West nor East Germany.

Table 10

Instrumental Variable Estimates – East Germany

	Outcome: repeat class		Outcome: high schooling		Outcome: low schooling	
	Coefficient	t-value	Coefficient	t-value	Coefficient	t-value
	Model with continuous regressor					
Actual age at school entry[1]	0.0030	1.43	0.0036	0.65	0.0031	0.97
Female	0.0060	0.56	0.0274	0.97	–0.0307	–1.88
Year of birth	0.0002	0.11	–0.0134	–2.90	0.0055	2.07
Number of siblings	0.0270	2.17	–0.0011	–0.03	0.0049	0.26
Number of siblings, squared	–0.0040	–1.39	–0.0034	–0.44	0.0049	1.11
Atheist	0.0004	0.03	–0.0256	–0.71	–0.0087	–0.42
Peers	–0.0053	–0.40	0.0337	0.97	–0.0163	–0.82
Father low education	0.0146	0.98	–0.0915	–2.33	0.0245	1.08
Mother low education	–0.0174	–1.22	–0.0469	–1.24	0.0138	0.63
Father high education	–0.0134	–0.92	0.2073	5.39	–0.0175	–0.79
Mother high education	0.0147	–0.84	0.1241	2.67	–0.0304	–1.14
Joint activities	–0.0043	–0.34	0.0825	2.50	–0.0024	–0.12
Parental attitudes	–0.0271	–2.40	0.1219	4.08	–0.0259	–1.50
Constant	–0.2317	-0.98	0.7656	1.22	–0.5431	–1.51
Number of observations	851		859		859	
F-Test[2]	1.76		14.28		2.96	
	Dummy treatment model					
School entry at age 7	–0.0112	–0.57	0.0457	0.88	0.0004	0.01
Female	0.0049	0.45	0.0306	1.07	–0.0333	–2.03
Year of birth	–0.0006	–0.32	–0.0137	–3.01	0.0053	2.00
Number of siblings	0.0251	2.00	–0.0068	–0.21	0.0086	0.45
Number of siblings, squared	–0.0035	–1.21	–0.0019	–0.25	0.0030	0.67
Atheist	–0.0031	–0.23	–0.0249	–0.70	–0.0128	–0.62
Peers	–0.0040	–0.30	0.0332	0.96	–0.0110	–0.55
Father low education	0.0139	0.93	–0.0887	–2.25	0.0294	1.29
Mother low education	–0.0166	–1.15	–0.0477	–1.26	0.0122	0.56
Father high education	–0.0143	–0.97	0.2087	5.42	–0.0157	–0.71
Mother high education	–0.0179	–1.02	0.1256	2.70	–0.0343	–1.28
Joint activities	–0.0056	–0.44	0.0811	2.45	–0.0074	–0.39
Parental attitudes	–0.0274	–2.40	0.1187	3.95	–0.0228	–1.32
Constant	0.0742	0.60	1.0645	3.30	–0.2706	–1.46
Number of observations	847		855		855	
F-Test[2]	1.61		14.19		2.66	

Authors' calculations. – [1]Age in months. – [2]F-Test on hypothesis that all coefficients except the constant are zero.

In our build-up of three different identification strategies we would argue that the IV estimates are least likely to suffer from bias due to unobserved heterogeneity. Interpreting these estimates, we find there is no indication of an effect of age at school entry on schooling outcomes, i.e. no effect of simply being older at school enrolment on educational attainment or the probability of repeating a class. The negative association between age at school entry and schooling degree and the positive association between age at school entry and probability of repeating a class found in LPM estimates and the matching approach seem to be selection effects, i.e. driven by unobserved heterogeneity, perhaps systematic differences in ability.

5. Conclusions

The question at which age a child should ideally enter school remains a controversial topic in German education policy: Is the established regulation still appropriate? Should deferment be encouraged? Or should the focus be on school enrolment at younger ages? Answering these questions is also of broader interest, since many countries have similar compulsory schooling laws, and an emerging literature in empirical educational economics focuses on studying these regulations.

This paper has aimed at contributing novel empirical evidence regarding the effect of age at school entry on educational attainment. We have discussed school enrolment regulations in West and East Germany, and delineated the policy relevance of the question whether age at school entry has any effect on educational outcomes. To assess this question, we have developed an empirical strategy based on a longitudinal survey of young adults in Germany in the early to mid-1990s. The data focus on the respondents' transition from childhood to adolescence and then on to adulthood during the 1960s through the 1980s, containing, in particular, core information on the child and her parents, and the parent-child relationship. The latter control variable is of special importance, as it captures information likely playing a role in enrolment decisions. Individuals in our sample entered school at some point in time during the period 1966 to 1980, a period during which enrolment regulations were clearly defined and did not change, but did allow for exceptions such as early enrolment and deferment, especially in West Germany.

The two outcomes we consider, probability of repeating a class and eventual schooling degree attained, assess short-term as well as long-term schooling success. As treatment variables we use (a) the age at school entry in months as a continuous regressor, and (b) a binary regressor capturing whether the individual entered at age 7 or at age 6. We have assessed treatment effects using three identification strategies. Linear probability models suggest a qualitatively negative association between deferment and schooling performance, for both West and East Germany. This result, with some refinement, is borne out by propensity score matching estimates. We think that these findings are likely driven by unobserved heterogeneity, i.e. those individuals who entered late did do so since they were conjectured (by their parents or elementary school teachers) to display low educational performance. We have then argued that our third identification strategy, instrumenting the actual age at enrolment with the age at enrolment according to the regulation, is most likely to control for unobserved ability differences and thus to reveal the "true" effect of age at school entry on educational outcomes. The findings suggest that there is no such effect, neither on schooling degrees nor on the probability of repeating a class, neither for West nor for East Germany.

We conclude that holding children back one year does not seem to secure a better schooling performance for this group, i.e. we find no justification for the rationale behind deferring children. If anything, then some of the deferred seem to fare worse, but this appears to be due to negative selection into this group. What policy recommendations could be derived from this result? First, there seems to be no reason to defer a child unless there are indications that she will not be able to follow class, i.e. no child should be deferred who seems perfectly capable of following class at age 6 – this basically invalidates the cut-off date rule. Second, this points to the importance of individual schooling tests, to refine the selection process.

One important issue in this discussion is the question whether children should enroll at even younger ages. Unfortunately, we cannot say much about this, as only few individuals in our data entered school at age 5. It is clearly an interesting research question worth being investigated empirically, but additional data would be needed. Still, we have found that for a particular child it does not seem to matter *in terms of schooling outcomes* whether the child enrolls at age 6 or age 7.

Potentially, this finding has wider implications. Consider the case of Germany, where – in a simplified argument – the regular retirement age is 65. If the same particular child indeed attains the same schooling outcome under enrolment at age 6 or at age 7, she leaves school with the same level of human capital either in a given year or one year later. If her retirement age is a given constant, then her productivity is available to the labor market one year more or less, and she would be paying taxes and be contributing to public pension funds and social security one year more or less. Moreover, if, for instance, German mothers were to return to work only after their child has enrolled in school, it would matter for the length of their productive contribution to the labor market as well if the child enters at age 6 or at age 7.

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Appendix

Table 11

Description of Variables

Variable	Description
	Outcome measures
Highest Schooling Degree	Categorical variable capturing 3 schooling degrees: (A) no schooling degree or completed secondary (*Hauptschule*); (B) intermediary degree (*Realschule*), (C) upper secondary or technical schooling degree (*Abitur*). The indicator variable "low schooling degree"=1 if (A) is the case, 0 otherwise; indicator variable "high schooling degree"=1 if (C) is the case, 0 otherwise
Repeat Class	Indicator variable taking on the value 1 if respondent reported having repeated a school year at some stage during her education; 0 otherwise
	Treatment variables
Actual age at school entry	Age at school entry in month
School entry at age 7	Indicator variable taking on the value 1 if respondent entered school at the age of 7; 0 if individual entered at the age of 6 (all other observations were dropped)
	Control variables
Female	Indicator variable taking on the value 1 if respondent is female; 0 otherwise
Year of birth	Year of birth of the respondent
Number of siblings	Number of siblings of respondent
Atheist	Indicator variable taking on the value 1 if respondent reported no religious denomination; 0 otherwise
Peers	Indicator variable taking on the value 1 if respondent reported having had friends during childhood and adolescence; 0 otherwise
Father low education	Indicator variable taking on the value 1 if respondent's father has no schooling degree or completed secondary schooling degree; 0 otherwise
Mother low education	Indicator variable taking on the value 1 if respondent's mother has no schooling degree or completed secondary schooling degree; 0 otherwise
Father high education	Indicator variable taking on the value 1 if respondent's father has upper secondary or technical schooling degree; 0 otherwise
Mother high education	Indicator variable taking on the value 1 if respondent's mother has upper secondary or technical schooling degree; 0 otherwise
Joint activities	Indicator variable taking on the value 1 if respondent reported having shared at least two of the following four joint activities with her parents during childhood: reading, sports, music and sharing other hobbies; 0 otherwise
Parental attitudes	Indicator variable taking on the value 1 if respondent reported her parents having had at least two of the following four positive attitudes towards her during childhood: to put hope into the child, to believe that the child is highly able, to be ambitious with the child and to have plans with the child; 0 otherwise
	Instrumental variables
Age at school entry according to regulation	(i) Age in months at which an individual should have entered school if school entry regulations had been complied with perfectly. (ii) Dummy variable taking on the value of 1 if an individual is born in June/July (West Germany) or June/July/August (East Germany)

Table 12

Summary Statistics for Variables

	West Germany		East Germany	
	mean	std.dev.	mean	std.dev.
		Outcome measures		
Repeat class	0.19	0.39	0.03	0.16
High schooling	0.35	0.48	0.28	0.45
Low schooling	0.30	0.46	0.06	0.24
		Treatment variables		
Actual age at school entry	79.52	6.42	81.94	5.86
School entry at age 7[a]	0.24	0.43	0.38	0.49
		Control variables		
Female	0.50	0.50	0.48	0.50
Year of birth	67.6	3.24	67.9	3.16
Number of siblings	1.36	1.17	1.32	1.04
Number of siblings, squared	3.22	5.34	2.81	4.46
Atheist	0.11	0.31	0.80	0.40
Peers	0.84	0.37	0.79	0.41
Father low education	0.55	0.50	0.34	0.47
Mother low education	0.61	0.49	0.39	0.49
Father high education	0.23	0.42	0.25	0.44
Mother high education	0.12	0.32	0.14	0.35
Joint activities	0.27	0.45	0.27	0.45
Parental attitudes	0.60	0.49	0.64	0.48
		Instrumental variables		
School entry age in months acc. to regulation	79.39	3.53	80.36	3.57
School entry at age 7 acc. to regulation[a]	0.18	0.39	0.25	0.44
Number of observations	1788		859	

Authors' calculations. – [a]Individuals who entered school at age 5 or age 8 excluded.

Table 13

First-Stage IV Estimates – West Germany

	Sample: repeat class		Sample: schooling degree	
	Coefficient	t-value	Coefficient	t-value
Model with dummy for school entry at age 7				
Female	0.1354	0.44	0.1457	0.48
Year of birth	–0.0350	–0.73	–0.0367	–0.77
Number of siblings	–0.2605	–0.83	–0.2627	–0.84
Number of siblings, squared	0.0376	0.54	0.0380	0.55
Atheist	–0.0461	–0.09	–0.0835	–0.17
Peers	–0.2029	–0.49	–0.2135	–0.52
Father low education	–0.2582	–0.61	–0.2627	–0.62
Mother low education	0.1582	0.38	0.1512	0.37
Father high education	–0.1923	–0.39	–0.1976	–0.40
Mother high education	–0.3503	–0.61	–0.3849	–0.68
Joint activities	–0.5375	–1.54	–0.5377	–1.55
Parental attitudes	–0.0026	–0.01	–0.0013	0.00
Age according to regulation[1]	0.2541	5.86	0.2532	5.85
Constant	62.3444	12.30	62.5379	12.36
Number of observations	1,786		1,788	
F-Test[2]	3.38		3.39	
Model with dummy for school entry at age 7				
Female	0.0132	0.64	0.0137	0.67
Year of birth	–0.0044	–1.38	–0.0045	–1.41
Number of siblings	0.0000	0.00	–0.0002	–0.01
Number of siblings, squared	0.0005	0.10	0.0005	0.11
Atheist	0.0440	1.28	0.0417	1.22
Peers	0.0288	1.04	0.0282	1.02
Father low education	–0.0017	–0.06	–0.0019	–0.07
Mother low education	–0.0052	–0.19	–0.0055	–0.20
Father high education	–0.0061	–0.18	–0.0065	–0.19
Mother high education	0.0095	0.25	0.0075	0.19
Joint activities	–0.0374	–1.60	–0.0373	–1.60
Parental attitudes	–0.0241	–1.15	–0.0241	–1.15
Entry at age 7 according to regulation	0.2170	8.27	0.2157	8.23
Constant	0.4894	2.21	0.4967	2.25
Number of observations	1,716		1,718	
F-Test[2]	6.29		6.24	

Authors' calculations. – [1]Age in months. – [2]F-Test on hypothesis that all coefficients except the constant are zero.

Table 14

First-Stage IV Estimates – East Germany

	Sample: repeat class		Sample: schooling degree	
	Coefficient	t-value	Coefficient	t-value
Model with continuous regressor				
Female	–0.2034	–0.56	–0.1560	–0.43
Year of birth	–0.0760	–1.29	–0.0763	–1.31
Number of siblings	–0.7662	–1.81	–0.8097	–1.93
Number of siblings, squared	0.1835	1.88	0.1950	2.00
Atheist	–1.1037	–2.41	–1.1165	–2.45
Peers	0.0945	0.21	0.0043	0.01
Father low education	–0.0369	–0.07	–0.0714	–0.14
Mother low education	0.2192	0.45	0.2297	0.48
Father high education	–0.4337	–0.88	–0.4175	–0.85
Mother high education	–0.8960	–1.51	–0.9552	–1.61
Joint activities	–0.4122	–0.97	–0.3394	–0.81
Parental attitudes	–0.2432	–0.63	–0.1996	–0.52
Age according to regulation[1]	0.7194	14.02	0.7170	13.98
Constant	31.0922	5.02	31.3870	5.08
Number of observations	851		859	
F-Test[2]	17.18		17.04	
Model with dummy for school entry at age 7				
Female	–0.0284	–1.01	–0.0255	–0.91
Year of birth	–0.0039	–0.86	–0.0040	–0.90
Number of siblings	–0.0331	–1.02	–0.0368	–1.13
Number of siblings squared	0.0059	0.77	0.0068	0.90
Atheist	–0.0615	–1.74	–0.0640	–1.81
Peers	0.0115	0.33	0.0034	0.10
Father low education	0.0154	0.40	0.0113	0.29
Mother low education	–0.0081	–0.22	–0.0057	–0.15
Father high education	–0.0215	–0.56	–0.0229	–0.60
Mother high education	–0.0875	–1.92	–0.0902	–1.98
Joint activities	–0.0418	–1.28	–0.0367	–1.13
Parental attitudes	–0.0038	–0.13	0.0008	0.03
Entry at age 7 according to regulation	0.6251	19.51	0.6236	19.47
Constant	0.5942	1.89	0.6117	1.96
Number of observations	847		855	
F-Test[2]	30.64		30.51	

Authors' calculations. – [1]Age in months. – [2]F-Test on hypothesis that all coefficients except the constant are zero.