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The Impact of a School Entry Cut-off Reform on School Starting Age and School Progression*

Victor Rudakov[1], Jan Marcus[2]

Abstract

School entry age policies aimed at earlier enrolment may accelerate educational trajectories and labour market entry, but their effectiveness depends on compliance with the new school entry rules and on how children progress through school. We study a reform that postponed the school entry cut-off date in half of the German federal states, making children born between the previous and new cut-off dates eligible to enter school one year earlier. Exploiting the staggered adoption of the reform in a difference-in-differences framework using administrative state-level data and individual-level survey data from the German Socio-Economic Panel, we find that the reform reduced average school starting age by less than one month, despite an average postponement of the cut-off date by almost three months. Less than half of directly affected children entered school earlier in response to the reform, reducing their school starting age by 5.5 months. This age advantage largely persists until secondary school entry, with no evidence of adverse effects on academic track attendance, despite a slight increase in grade repetition. A partial reversal of the reform produced effects of similar magnitude in the opposite direction, supporting a causal interpretation. Overall, our findings suggest that limited compliance substantially reduced the reform's effectiveness, while earlier school entry did not come at the cost of subsequent educational opportunities.

Keywords: school starting age, school entry cut-off, compliance, school progression, staggered difference-in-differences, policy evaluation
JEL: I21, I24, I28, J24

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1. Introduction

The age at which individuals begin formal schooling has important implications for both education systems and labour markets. Starting school earlier may enable students to complete their education and enter the workforce at a younger age, potentially enhancing economic productivity and competitiveness (Oosterbeek et al., 2021; Görlitz et al., 2025). However, children who are relatively young at enrolment have lower academic achievement than their older classmates (Bedard and Dhuey, 2006; Puhani and Weber, 2007; McEwan and Shapiro, 2008; Elder and Lubotsky, 2009; Peña, 2020; Sontheim, 2025), so that a later start may improve their readiness for school and their subsequent educational outcomes. As education systems seek to balance an early start against sufficient developmental readiness, school entry cut-off dates have become an important policy instrument.

This study examines the consequences of a reform that postponed these cut-off dates to allow children to enter school at a younger age. In Germany, children who turned six by 30 June were traditionally required to start school in that year. Between 2003 and 2011, eight of the sixteen federal states postponed this cut-off date by between one and six months, so that children born between 1 July and the new cut-off date became eligible for school one year earlier, while the remaining states retained the 30 June cut-off (Standing Conference of the Ministers of Education and Cultural Affairs, 2014). The reform aimed to lower the age at school entry and, ultimately, the age at which young people complete their education and enter the labour market. However, the reform's effectiveness depends on families' enrolment decisions, as they can defer or advance school entry, and on whether earlier enrolment adversely affects academic development, potentially delaying secondary school entry or reducing academic track attendance.

Our focus is on the effects of the reform as a policy, rather than on the effects of school starting age for individual children. Most of the existing literature uses school entry cut-offs to estimate the effects of being relatively young or old at school entry, typically by comparing children born just before and just after a given cut-off date in a regression discontinuity design (see Cavallo et al. (2026) for an overview). These estimates are informative about the consequences of school starting age under a fixed rule, but they do not reveal what happens when the rule itself changes, which is the relevant question for policymakers. A change in the cut-off affects entire cohorts and induces behavioural responses, such as deferring or advancing school entry, which are an integral part of the policy effect. We therefore exploit the staggered adoption of the reform across states and cohorts in a difference-in-differences framework, which allows

us to estimate the effect of the reform on the school entry and school progression of affected cohorts as a whole.

We combine two complementary data sources: administrative state-level data from the Federal Statistical Office of Germany, which cover the entire population of school entrants, and individual-level data from the German Socio-Economic Panel (SOEP), a long-running, nationally representative household survey (Goebel et al., 2019). Our main analysis covers the school eligibility cohorts 2000 to 2016, and we use the cohorts 2012 to 2022 to analyse the partial reversal of the reform from 2017 onwards. While the administrative data provide large sample sizes and objective measures of early and late enrolment and grade repetition, the SOEP data allow us to measure school starting age and the transition to secondary school and, as they contain the month of birth, to identify children whose school eligibility was directly affected by the reform.

Our results show that the reform achieved its objective only to a limited extent. For all children, the reform reduced the average school starting age by less than one month, compared with an average postponement of the cut-off by almost three months in the reform states. At the same time, it substantially changed enrolment behaviour: early enrolment declined by about half (2.8 percentage points) and late enrolment increased by about half (3.1 percentage points) relative to their respective means. These responses offset much of the intended reduction in school starting age, as fewer children entered school early and more children deferred their school entry. In the administrative data, we find a small increase in primary school grade repetition of 0.8 percentage points (about one third of its low baseline rate), whereas the SOEP data show no corresponding increase in time spent in primary school. The reduction in school starting age largely carries over to the transition to secondary school, with no evidence of reduced attendance of the academic track (*Gymnasium*), the highest secondary school track, suggesting that the age advantage did not come at the cost of subsequent educational opportunities.

The analysis of directly affected children explains the moderate average effects. Less than half of the children who became eligible for school one year earlier entered school earlier in response to the reform; the remaining children kept their previous timing of school entry. Accordingly, school starting age decreased on average by 5.5 months among directly affected children, consistent with the share who entered school one year earlier in response to the reform, and these children entered secondary school about six months earlier. For children whose eligibility did not change, the estimated effects are close to zero. The responses to the reform also differ by socio-economic background. Compliance and the reduction in school starting age are largest among children

of parents with low education and children with a migration background, whereas more advantaged families more often retained the previous timing of school entry.

Several pieces of evidence support a causal interpretation of these results. Event-study estimates show no diverging pre-reform trends, and the results are robust to alternative difference-in-differences estimators designed for staggered treatment adoption, to alternative samples, outcome measures and control variables, and to wild cluster bootstrap inference. Moreover, when the reform was partially reversed from 2017 onwards, the effects were symmetric: early enrolment increased, late enrolment decreased, and the school starting age of directly affected children increased by about half a year, mirroring the effects of the reform's introduction.

This paper contributes to two strands of the literature. The first strand examines the effects of school entry age and school entry cut-off rules on educational and labour market outcomes. Most studies in this strand exploit cut-off rules to estimate the effects of relative age at school entry on test scores and grade repetition (Bedard and Dhuey, 2006; Puhani and Weber, 2007; McEwan and Shapiro, 2008; Lubotsky and Kaestner, 2016; Dhuey et al., 2019; Cornelissen and Dustmann, 2019; Sontheim, 2025), educational attainment (Dobkin and Ferreira, 2010; Fredriksson and Öckert, 2014; Oosterbeek et al., 2021; Görlitz et al., 2025), labour market outcomes (Black et al., 2011; Fredriksson and Öckert, 2014; Oosterbeek et al., 2021; Görlitz et al., 2025), and non-economic outcomes such as health, fertility, social relationships and crime (Dhuey and Lipscomb, 2008, 2010; Black et al., 2011; Fumarco et al., 2020; Cook and Kang, 2016); see Cavallo et al. (2026) for a review. In contrast, our study takes a more policy-oriented perspective by examining how reforms of school entry cut-off dates affect actual enrolment behaviour and subsequent educational progression. Studies that directly examine such reforms are scarce and focus on changes that moved the cut-off date *earlier* in the calendar year in the United States (Deming and Dynarski, 2008; Bedard and Dhuey, 2012). We provide evidence on a reform that moved the cut-off *later*, in an institutional context with early tracking, where school leaving is linked to track completion rather than to age, so that earlier school entry shifts the timing of educational careers rather than their length. By distinguishing between directly and not directly affected children and by exploiting the reversal of the reform, we show how limited compliance shapes the effects of cut-off reforms and that these effects are symmetric.

The second strand of the literature evaluates education reforms aimed at facilitating earlier entry into the labour market. While most policies to extend working lives focus on older individuals (Börsch-Supan and Schnabel, 1998; Feldstein and Siebert, 2002), some reforms aim to reduce the age at which individuals enter the labour market, for example by shortening the school year (Pischke, 2007; Cygan-Rehm, 2025),

the duration of degrees (Webbink, 2007; Comi et al., 2022), or the number of years of schooling. Such reforms have often been less effective than anticipated, as affected cohorts lost much of their initial age advantage through grade repetition and delayed university enrolment (Huebener and Marcus, 2017; Marcus and Zambre, 2019). Shifting the school entry cut-off offers a potentially less disruptive alternative, as it advances the entire educational trajectory without shortening the duration of schooling. Our results show that such a reform can indeed lead to an earlier transition to secondary school for children who enter school earlier in response to the new rule, without compromising their subsequent educational opportunities, as evidenced by the absence of adverse effects on academic track attendance. However, limited compliance substantially reduces the reform's effect on average. While most of the affected cohorts have not yet entered the labour market, the earlier transition to secondary school indicates that the reform advances the timing of educational careers, which is a precondition for earlier labour market entry.

The remainder of this paper is structured as follows. Section 2 describes the institutional background and the reform. Section 3 describes the data, and Section 4 the empirical strategy. Section 5 presents the main results, including the effects of the reform on school entry and school progression, the event-study estimates and the effects for directly and not directly affected children. Section 6 reports robustness checks, Section 7 examines effect heterogeneity, while Section 8 focuses on the reversal of the reform, and Section 9 concludes.

2. Institutional Background and the School Entry Cut-off Reform

In the German education system, the school entry cut-off date defines the latest date by which a child must reach the statutory school-entry age to begin compulsory education in a given year. Children who turn six before or on the cut-off date are required to start primary school (*Grundschule*) that year, whereas those who turn six after the cut-off date defer enrolment until the following year (Standing Conference of the Ministers of Education and Cultural Affairs, 2014). Once enrolled, primary school lasts four years in most states.

Traditionally, the school-entry cut-off date in Germany was set uniformly at 30 June at the national level. Under the cut-off date reform, the Standing Conference of the Ministers of Education and Cultural Affairs (Kultusministerkonferenz, KMK) granted federal states the authority to set their own cut-off dates (Berthold, 2008). Several considerations motivated this reform. First, German students are relatively old when they complete compulsory schooling compared to their peers in other OECD countries

(OECD, 2025). Second, German pupils have performed comparatively poorly in international achievement assessments such as the Programme for International Student Assessment (PISA), raising concerns about the effectiveness of the education system and the potential role of a relatively late school start (Berthold, 2008; OECD, 2023). Third, the German education system features early ability tracking with long-lasting consequences, implying that initial performance differences, such as those arising from age at school entry, can have persistent effects on educational trajectories (Hanushek and Woessmann, 2006).

Against this background, between 2003 and 2011, eight of the sixteen German federal states postponed their school-entry cut-off dates, thereby allowing some children to enter school at an earlier age. These reforms were implemented in Baden-Württemberg, Bavaria, Berlin, Brandenburg, Lower Saxony, North Rhine-Westphalia, Rhineland-Palatinate, and Thuringia, while the remaining states retained the 30 June cut-off. Table 1 presents the school entry cut-off reform across federal states during the implementation phase. From 2017 onwards, the reform was partially reversed, with several states moving back toward the original June cut-off. The full reform cycle, including these subsequent adjustments, is documented in Table A1.

Table 1: Implementation of the School Entry Cut-off Reform in the Federal States, 2000–2016

Federal State / Year	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
Baden-Wuerttemberg	6	6	6	6	6	7	8	9	9	9	9	9	9	9	9	9	9
Bavaria	6	6	6	6	6	7	8	9	10	11	9	9	9	9	9	9	9
Berlin	6	6	6	6	6	12	12	12	12	12	12	12	12	12	12	12	12
Brandenburg	6	6	6	6	6	9	9	9	9	9	9	9	9	9	9	9	9
Bremen	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
Hamburg	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
Hesse	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
Mecklenburg-WP	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
Lower Saxony	6	6	6	6	6	6	6	6	6	6	7	8	9	9	9	9	9
North Rhine-Westphalia	6	6	6	6	6	6	6	7	7	8	8	9	9	9	9	9	9
Rhineland-Palatinate	6	6	6	6	6	6	6	6	8	8	8	8	8	8	8	8	8
Saarland	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
Saxony	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
Saxony-Anhalt	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
Schleswig-Holstein	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
Thuringia	6	6	6	7	7	7	7	7	7	7	7	7	7	7	7	7	7

Notes: Entries show the statutory school-entry cut-off month in each state and year. Shading reflects the postponement relative to June (month 6): the darker the blue, the larger the postponement.
Sources: Berthold (2008); Standing Conference of the Ministers of Education and Cultural Affairs (2024).

Reform implementation differed across states along two dimensions. First, the magnitude of the change varied, with cut-off dates shifting by as little as one month in some states and by up to six months in others (for example, Berlin). Second, states differed in the mode of implementation: while some introduced the reform as a one-time change, others postponed the cut-off date in several incremental steps over multiple years (for example, Bavaria). Accordingly, we model the reform not only as a binary variable indicating whether the cut-off was postponed relative to the previous national

cut-off (30 June), but also consider the treatment intensity, expressed in months relative to 30 June.

There is some heterogeneity across states in the strictness and enforcement of school entry cut-off rules. In most states, enforcement is relatively lenient. Prior to the reform, children who were required to start school under the cut-off date regulation could be deferred for one year if it was assumed that they would not be able to participate successfully in primary school. Deferral decisions were based on school readiness criteria, including mental, physical, emotional, and social development, as well as medical examinations or justification. These decisions are usually made by the school principal on the basis of cooperation between the primary school and kindergarten, discussions with parents, involvement of the school medical service or a psychologist, and observations of the child during the registration process (Berthold, 2008).

From 2005, Berlin prohibited all deferrals and introduced the strictest enforcement of on-time school entry (Berthold, 2008). However, this restriction was gradually abolished from 2007 and completely abolished in 2010. As a robustness check, we exclude Berlin and Brandenburg; the results are similar, although the effect on late enrolment in the administrative data loses statistical significance (Section 6). In all other states, deferrals remained permitted during the entire observation period. However, there is some heterogeneity in the acceptable reasons for deferral. In six states (Brandenburg, Bremen, North Rhine-Westphalia, Rhineland-Palatinate, Saarland, and Thuringia), only medical reasons are accepted, whereas in the remaining states deferrals may also depend on discussions with parents and other non-medical considerations. In all federal states, early enrolment is permitted when a child's developmental readiness suggests that they can successfully begin school. In five states (Hamburg, Lower Saxony, North Rhine-Westphalia, Rhineland-Palatinate, and Schleswig-Holstein), no minimum age requirement applies (Berthold, 2008).

After primary school, pupils are allocated to different secondary school tracks. The academic track (*Gymnasium*) leads to the university entrance qualification (*Abitur*), whereas the other tracks primarily prepare for vocational training. Track allocation is based on teachers' recommendations at the end of primary school, which depend strongly on pupils' academic performance, although the extent to which these recommendations are binding differs across states (Osikominu et al., 2025). The timing of the transition to secondary school also differs across states. In most states, primary school comprises four grades, so that children usually enter secondary school after grade 4. In Berlin and Brandenburg, primary school comprises six grades, so that the transition usually takes place after grade 6. Moreover, under the FLEX reform described below, children may spend between one and three years in grades 1 and 2, so that the time

spent in primary school can vary within states, municipalities and schools.

Grade repetition is regulated at the state level in Germany, but follows broadly similar principles across federal states. Repetition is typically based on insufficient acquisition of basic competencies, particularly in reading, writing, and mathematics, and in the early grades also reflects developmental immaturity. Decisions are made by schools in consultation with parents (Standing Conference of the Ministers of Education and Cultural Affairs, 2024).

During our observation period, an additional reform in primary education was implemented. The FLEX reform (*Flexible Schuleingangsphase*), which began in 2003, was gradually introduced across several federal states at different points in time and with varying intensity (i.e., either in selected schools or statewide). The reform aimed to enrol all children in primary school without deferrals and to provide instruction in mixed-age classrooms combining grades 1 and 2. Under FLEX, the school entry phase can last between one and three years, depending on a child's individual learning progress (LISUM, 2007). As the FLEX reform was implemented in a subset of both treatment and control states, we account for it in the robustness checks (Section 6).

3. Data

The study uses two complementary data sources: administrative state-level panel data from the German Federal Statistical Office (2026a) and individual-level microdata from the German Socio-Economic Panel (2025, SOEP version 40.1). The two datasets differ in their level of aggregation and information content but jointly allow us to analyse the effects of the school entry cut-off reform on actual school entry and early school progression. Using two complementary data sources offers several advantages. The administrative data provide a large sample size and are not subject to recall or social desirability bias, ensuring high measurement accuracy. The SOEP, by contrast, contains rich information at the individual and household levels, allowing us to analyse additional outcomes, distinguish between children directly and not directly affected by the reform, and examine heterogeneity in effects across individual and household characteristics. Moreover, because we can construct comparable outcome measures in both datasets, their joint use enables a cross-validation of our findings.

Our main analysis covers cohorts of children scheduled to start school between 2000 and 2016 ("eligibility cohorts"). The observation period starts in 2000 because earlier administrative data are not fully comparable due to a different aggregation procedure, and it ends in 2016 because some federal states began to partially reverse the reform from 2017 onwards. In addition, we use both data sources to examine the

effects of the reform reversal for the cohorts 2012 to 2022 (Section 8).

3.1. Administrative state-level data

The administrative data cover the number of early, on-time, and late school enrolments, as well as grade repetition in primary school, across German federal states. The data are provided only in an aggregated form but include consistent information at the state-year level for all federal states over the period surrounding the cut-off date reforms. Because these data cover the full population of pupils in Germany, sampling error and reporting biases are not a concern. However, the dataset does not contain further background characteristics of pupils. It does, however, report counts separately for boys and girls, which we exploit to analyse effect heterogeneity by gender.

We construct three main outcomes based on the administrative data: *the late enrolment rate, the early enrolment rate, and the grade repetition rate.* In the administrative statistics, early, on-time, and late enrolment, as well as the number of grade repeaters at the state-year-grade level, are reported by schools and compiled by the Federal Statistical Office.[3]

Following the introduction of the FLEX reform (see Section 2), repetition in grades 1 and 2 is no longer reported after 2008. We therefore use grade repetition in grades 3 and 4 as our main outcome, as this measure is consistently available throughout the sample period. For robustness, we additionally report results based on total grade repetition in primary school across grades 1–4 (Table B1, Column (7)).

3.2. SOEP microdata

The individual-level data are drawn from the SOEP, a large-scale longitudinal household survey that has been conducted annually in Germany since 1984 (Goebel et al., 2019). The SOEP follows approximately 30,000 individuals in nearly 20,000 households and is one of the largest and longest-running household panel surveys worldwide. It provides rich information on individuals' socio-economic characteristics and labour market outcomes. It also contains retrospective information on individuals' educational biographies, including the month and year of birth, the year and federal state of first school enrolment, and school starting age.

[3]While all children reported in a given year start school in that year, early enrolees belong to the subsequent school eligibility cohort, late enrolees to the previous cohort, and on-time enrolees to the current cohort. To avoid mixing cohorts, we construct cohort-consistent enrolment rates by combining counts from adjacent reporting years. Specifically, for eligibility cohort t, we define cohort size as $C_t = E_{t-1} + O_t + L_{t+1}$, where E, O, and L denote early, on-time, and late enrolments, respectively. The early and late enrolment rates are then E_{t-1}/C_t and L_{t+1}/C_t. Similarly, letting R_t denote the number of repeaters who entered school in year t, we define the grade repetition rate as R_t divided by the total number of school entrants in year t. See Appendix A.2 for details. Results are similar when using the originally reported enrolment rates (see Section 6).

We restrict the SOEP sample to individuals with valid information on the month and year of birth, as well as the federal state of first school enrolment, which are required to construct school eligibility cohorts under state-specific cut-off rules.[4] In accordance with the administrative data, the main analysis sample is restricted to eligibility cohorts between 2000 and 2016.

We construct six main outcomes based on the SOEP data, three related to school entry and three related to school progression. For school entry, we use binary indicators for (i) early and (ii) late enrolment, as well as (iii) school starting age.[5] We classify actual school entry as early, on-time, or late by comparing the observed enrolment year with the eligibility year according to the applicable cut-off rule, following a logic analogous to that used in the administrative data. School starting age is measured in months and is based on retrospective survey information on the year and age at which the child was first enrolled in school. For school progression, we use (iv) the age at the transition to secondary school, (v) the time spent in primary school, defined as the difference between the age at the transition to secondary school and school starting age, and (vi) an indicator for attending the academic track (Gymnasium) after primary school. These outcomes are available for children for whom both school entry and the transition to secondary school are observed.

The SOEP data also allow us to identify children who are directly affected by the reform: those born between the old cut-off date (30 June) and the new, later cut-off date, who became eligible for school entry one year earlier under the new rule. For these children, we additionally measure compliance with the new rule, defined as an indicator for entering school earlier than allowed under the old cut-off. Furthermore, we use information on parental income and education, as well as the child's migration status and gender, both as control variables and to examine effect heterogeneity.

Both administrative and SOEP data are supplemented with state-level control variables from the Federal Statistical Office of Germany, including the unemployment rate and the share of educational spending in the state budget (German Federal Statistical Office, 2026b).

[4] We define eligibility cohorts as groups of children assigned to the same predicted school-entry year based on their month and year of birth and the applicable state-specific cut-off rule.

[5] Aggregating the binary indicators across the relevant population yields the corresponding enrolment rates used in the administrative data; thus, these measures represent their individual-level sample counterparts and the corresponding effect estimates can be directly compared.

3.3. Descriptive statistics

Summary statistics for key variables are presented in Table 2. Panel A reports descriptive statistics based on the administrative state-level data, and Panel B those based on the SOEP microdata. Within each panel, variables are grouped into outcomes related to school entry and those related to school progression. The administrative data form a balanced panel of 272 state-year observations, covering 16 federal states over 17 years and based on 12.7 million school entrants. The SOEP sample comprises 7,186 individuals for the school-entry outcomes and 4,538 individuals for the school-progression outcomes, again covering 16 states and 17 years. These statistics refer to the period of the reform's introduction (2000–2016); the reversal of the reform after 2016 is analysed separately in Section 8.

According to the administrative data in Panel A, about 5.6% of children enrol early and about 6.3% enrol late. Grade repetition in grades 3–4 of primary school affects 2.2% of pupils. Of the 272 state-year observations, 32.4% are treated under the reform; conditional on treatment, the average reform intensity is 2.8 months.

The incidence of early and late enrolment is higher in the SOEP data (7.9% and 11.1%, respectively), likely due to measurement differences: the administrative data rely on official school statistics, whereas the SOEP outcomes are constructed for the survey sample from reported birth dates and the applicable cut-off rules. In the SOEP sample of 7,186 children at school entry, the average school starting age is 78.4 months (6.5 years). 57.2% of observations are treated, and the average reform intensity, conditional on treatment, is 2.8 months.[6] Among children in reform state-cohorts, 23.7% are directly affected, that is, they became eligible for school entry one year earlier under the new cut-off because they were born between the old and the new cut-off date.

The school-progression outcomes are observed for a smaller sample of 4,538 children, for whom both school entry and the transition to secondary school are observed; the difference mainly reflects panel attrition and missing information on the transition.[7] In this sample, the average age at the transition to secondary school is 128.2 months (10.7 years), and children spend on average 49.9 months (about 4.2 years) in primary

[6]The share of treated observations is higher than in the administrative data because the SOEP was extended by several subsamples in later years, most notably the Families in Germany (FiD) sample and the refugee samples, which disproportionately include children from post-reform cohorts. Our results are robust to this composition: estimates using SOEP survey weights, which account for the sampling design, are very similar to our main unweighted estimates (see Section 6).

[7]Estimates for the school-entry outcomes on this smaller sample and for the school-progression outcomes on the larger sample of all children with an observed transition are similar for school starting age and somewhat larger for secondary school starting age (Table B1, Columns (5)–(6)).

Table 2: Summary statistics

	Mean	SD	Min	Max
Panel A. Administrative state-level data				
School entry				
Early enrolment	0.056	0.042	0.001	0.193
Late enrolment	0.063	0.036	0.000	0.203
School progression				
Grade repetition	0.022	0.008	0.007	0.052
Reform and sample				
Reform (dummy)	0.324	0.469	0	1
Reform intensity (months)	2.85	1.52	1	6
N (state × year)	272			
N (individuals)	12 766 277			
Panel B. SOEP microdata				
School entry				
Early enrolment	0.079	0.270	0	1
Late enrolment	0.111	0.314	0	1
School starting age	78.43	5.67	50	107
Reform (dummy)	0.572	0.495	0	1
Reform intensity (months)	2.78	1.00	1	6
Directly affected (share)	0.237	0.425	0	1
N (individuals)	7 186			
School progression				
Sec. school starting age	128.16	8.21	99	172
Time in primary school	49.87	8.02	36	96
Academic track	0.442	0.497	0	1
Reform (dummy)	0.539	0.499	0	1
Reform intensity (months)	2.75	0.94	1	6
Directly affected (share)	0.235	0.424	0	1
N (individuals)	4 538			

Notes: Eligibility cohorts 2000–2016; statistics refer to the estimation samples of Table 3 and Table 4. Panel A uses administrative state–year data, Panel B - SOEP microdata; school-progression outcomes refer to children with observed school entry and transition. Ages in months. Reform intensity (months by which the cut-off was postponed) and the share of directly affected children (who became eligible for school entry one year earlier under the new cut-off) are computed for reform state-cohorts only. The minimum of late enrolment refers to Berlin in 2005, when deferrals were abolished.
Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

school, reflecting the longer duration of primary school in Berlin and Brandenburg and the flexible school entry phase described in Section 2. On average, 44.2% of pupils attend the academic track (*Gymnasium*).

4. Empirical Strategy

The study exploits the school-entry cut-off reform as a quasi-experiment, leveraging variation in its implementation across federal states and over time within a difference-in-differences framework. For the administrative data, we estimate regressions of the following form:

$$Y_{st} = \beta \, Cut_Off_Reform_{st} + \alpha_s + \theta_t + \varepsilon_{st}, \tag{1}$$

where Y_{st} denotes the aggregated outcome in state s for eligibility cohort t. The variable $Cut_Off_Reform_{st}$ indicates whether, in state s and cohort t, the school-entry cut-off date was later than the previous national cut-off of 30 June. State fixed effects α_s absorb time-invariant differences across states, while cohort fixed effects θ_t account for common shocks and secular trends across cohorts.

For the SOEP data, we estimate the corresponding specification at the individual level:

$$Y_{ismt} = \beta \, Cut_Off_Reform_{st} + \alpha_{sm} + \theta_{tm} + \varepsilon_{ismt}, \tag{2}$$

where i indexes children and m their month of birth. Because the reform changes school eligibility only for children born in particular months, we allow both the state and the cohort fixed effects to vary by birth month: state $\times$ birth-month fixed effects α_{sm} capture time-invariant differences in enrolment behaviour across states for children born in a given month, and cohort $\times$ birth-month fixed effects θ_{tm} capture common changes over time for children born in a given month.

In the main specification, $Cut_Off_Reform_{st}$ is a binary indicator equal to one if the cut-off date was postponed relative to 30 June. This specification captures the average effect of any postponement, irrespective of its magnitude. To account for variation in treatment intensity, we additionally estimate specifications in which treatment is measured as the number of months by which the cut-off date was postponed; for example, a shift from June (month 6) to September (month 9) corresponds to a treatment intensity of three months. In robustness checks, we add time-varying state-level controls (the unemployment rate and the share of education spending in the state budget) and, for the SOEP data, individual-level controls (gender, migration background, parental education, and household income). We apply the same specifications to the

partial reversal of the reform after 2016, with treatment defined as the cut-off being moved back towards 30 June.

The estimates from Equations (1) and (2) refer to all children and thus combine children whose school eligibility changed with children whose eligibility was unaffected. In the SOEP data, we can distinguish these two groups. Children are *directly affected* if they were born between the old cut-off (30 June) and the new, later cut-off, so that they became eligible for school entry one year earlier. We estimate Equation (2) separately for (i) directly affected children in reform state-cohorts and (ii) all other children in reform state-cohorts, each compared with all children in non-reform state-cohorts. For directly affected children, we additionally estimate the effect on compliance, defined as entering school earlier than allowed under the old cut-off. To examine heterogeneity, we estimate these specifications separately by gender, migration background, parental education, and household income, and test the equality of the effects across groups.

We estimate Equations (1) and (2) by ordinary least squares, using a two-way fixed effects (TWFE) approach. Standard errors are clustered at the state level, the level at which the reform was implemented. As Germany comprises only 16 states, we additionally conduct inference using the wild cluster bootstrap. Because states adopted the reform at different points in time, the TWFE estimator may be biased if the treatment effect varies over time or across states. We therefore also apply the estimators proposed by Callaway and Sant'Anna (2021), Borusyak et al. (2024) and Sun and Abraham (2021), which are designed for staggered treatment adoption.

To examine the dynamic effects of the reform and the plausibility of the parallel trends assumption, we replace the single treatment indicator with a series of leads and lags relative to the reform year:

$$Y_{st} = \sum_{k \neq -1} \beta_k \, EventTime_{st}^{k} + \alpha_s + \theta_t + \varepsilon_{st}, \tag{3}$$

where k denotes the number of years relative to the reform year ($k = 0$ for the first reform cohort, negative values for pre-reform cohorts, and positive values for post-reform cohorts). The coefficients β_k trace the dynamic effects of the reform, with $k = -1$ as the reference period. To reduce noise in the tails, all periods $k \leq -4$ and all periods $k \geq 4$ are combined into single bins. For the SOEP data, the event-study specification includes state $\times$ birth-month and cohort $\times$ birth-month fixed effects, as in Equation (2).

5. Results

5.1. Main effects

The TWFE results based on Equations (1) and (2) are presented separately for the effects of the reform on school entry (Table 3) and subsequent school progression (Table 4). Both tables follow the same structure: Panel A reports results using administrative state-level data, while Panel B draws on individual-level SOEP data. For each outcome, we report estimates using both a binary treatment indicator and a treatment-intensity measure capturing the number of months by which the cut-off date was postponed. Standard errors are clustered at the state level.

Using administrative state-level data, Column (1) of Panel A shows that postponing the cut-off date decreases the early enrolment rate by 2.8 percentage points, corresponding to a decline of approximately 50% relative to the mean rate of 5.6%. Estimates based on the treatment-intensity specification (Column (2)) indicate that a one-month postponement in the cut-off date reduces early enrolment by 1.1 percentage points, or approximately 20% of the mean. For late enrolment, the estimates indicate an increase of 3.1 percentage points ($p = 0.059$), corresponding to approximately 50% of the mean late enrolment rate. The treatment-intensity specification (Column (2)) yields an increase of 1.1 percentage points for each additional month of postponement, equivalent to 17.5% of the mean.

Results based on individual-level SOEP data, reported in Panel B, point to a similar pattern. Early enrolment decreases by 6.2 percentage points (Column (1)), while the treatment-intensity specification indicates that a one-month postponement of the cut-off date reduces early enrolment by 2.7 percentage points (Column (2)). For late enrolment, the estimates indicate an increase of 3.9 percentage points, while the treatment-intensity specification yields an increase of 1.6 percentage points for each additional month of postponement (Table 3, Panel B).

Finally, the reform reduced the average age at school entry by 0.8 months (approximately 24 days). This reduction is modest compared with the average postponement of the cut-off date by 2.7–2.8 months in treated states. The treatment-intensity estimates indicate that a one-month postponement of the cut-off date reduces the average age at school entry by approximately 0.4 months (Table 3, Panel B), or about 12 days. These estimates suggest that the postponement of the cut-off date translates only partially into changes in actual school entry timing. For many children affected by the reform, the timing of school entry therefore remains unchanged relative to the previous regime, either because they postpone enrolment or because they no longer enter school early. This interpretation is consistent with the observed changes in early

Table 3: School cut-off reform: Effects on school entry

	(1) DiD		(2) Intensity DiD
Panel A. Administrative state-level data			
Early enrolment	-0.028***		-0.011***
	(0.007)		(0.002)
Mean dep. var.		0.056	
Late enrolment	0.031*		0.011**
	(0.015)		(0.005)
Mean dep. var.		0.063	
State FE	Yes		Yes
Cohort FE	Yes		Yes
N (state × year)	272		272
N (individuals)		12 766 277	
Panel B. SOEP microdata			
Early enrolment	-0.062***		-0.027***
	(0.017)		(0.005)
Mean dep. var.		0.079	
Late enrolment	0.039**		0.016**
	(0.014)		(0.006)
Mean dep. var.		0.111	
School starting age	-0.805**		-0.403***
	(0.319)		(0.126)
Mean dep. var.		78.430	
State × birth-month FE	Yes		Yes
Cohort × birth-month FE	Yes		Yes
N (individuals)	7 186		7 186

Notes: Coefficients indicate the estimated effects of postponing the school entry cut-off date on the respective outcomes, identified using a difference-in-differences design; in the intensity specification (Column (2)), treatment is the number of months by which the cut-off was postponed. Administrative data include state and cohort fixed effects; SOEP models include state × birth-month and cohort × birth-month fixed effects. Ages in months. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

and late enrolment.

Overall, the administrative and SOEP data yield a highly consistent picture of the reform's effects at school entry. Both data sources indicate that the reform substantially reduced early enrolment and increased late enrolment, while the SOEP estimates additionally point to a moderate reduction in the average age at school entry. Although the estimated effects are larger in absolute terms in the SOEP sample, the corresponding sample means are also higher. The consistency in the direction and relative magnitude of the estimates across the two data sources provides reassuring evidence of the robustness of our findings.

Having established how the reform affected the timing of school entry, we next examine whether these changes also translated into differences in children's subsequent educational trajectories. Table 4 reports the estimated effects on grade repetition, the timing of the transition to secondary school, time spent in primary school, and academic track participation. Panel A shows that the reform slightly increased grade repetition in grades 3–4 by 0.8 percentage points, corresponding to approximately 36% of the mean grade repetition rate. The treatment-intensity specification confirms this pattern, indicating that grade repetition increases with the extent of the cut-off postponement.

Panel B reports the results based on SOEP data. The reform also reduced the average age at transition to secondary school by 1.3 months. While this estimate is somewhat larger than the corresponding reduction in school starting age reported in Table 3, the difference between the two estimates is not statistically significant. Consistent with this, we find no statistically significant effect on the time spent in primary school. Moreover, when we restrict the analysis to the same sample of individuals for whom we observe both primary and secondary school entry, the estimated effects on primary and secondary school starting age become even more similar (see Table B1, Column (6)). The treatment-intensity estimate is also of similar magnitude to that for primary school starting age, although it is not statistically significant. This lower statistical precision likely reflects the smaller sample available for the school progression outcomes.

Despite the increase in grade repetition, we find no statistically significant effect on the time spent in primary school. This is not necessarily inconsistent with the grade repetition results: the estimated increase in grade repetition implies that approximately 8 additional students per 1,000 repeat a grade due to the reform. Assuming that each repetition adds one year to primary schooling, this translates into an average increase of only 0.096 months in time spent in primary school. Our estimate of -0.358 months is statistically compatible with such a small positive effect. Another possible explanation is that an increase in grade skipping partly offsets the additional time spent by students

Table 4: School cut-off reform: Effects on school progression

	(1) DiD		(2) Intensity DiD
Panel A. Administrative state-level data			
Grade repetition	0.008***		0.002***
	(0.003)		(0.001)
Mean dep. var.		0.022	
State FE	Yes		Yes
Cohort FE	Yes		Yes
N (state × year)	272		272
Panel B. SOEP microdata			
Sec. school starting age	-1.314**		-0.423
	(0.616)		(0.282)
Mean dep. var.		128.159	
Time in primary school	-0.358		-0.036
	(0.448)		(0.144)
Mean dep. var.		49.870	
Academic track	-0.023		-0.012
	(0.031)		(0.014)
Mean dep. var.		0.442	
State × birth-month FE	Yes		Yes
Cohort × birth-month FE	Yes		Yes
N (individuals)	4 538		4 538

Notes: Coefficients indicate the estimated effects of postponing the school entry cut-off date on the respective outcomes, identified using a difference-in-differences design; in the intensity specification (Column (2)), treatment is the number of months by which the cut-off was postponed. Administrative data include state and cohort fixed effects; SOEP models include state × birth-month and cohort × birth-month fixed effects. Grade repetition refers to grades 3–4. SOEP outcomes are estimated for children for whom both school entry and the transition to secondary school are observed; time in primary school is the difference between secondary school starting age and school starting age. Academic track: attendance of a Gymnasium. Ages in months. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

who repeat a grade.

Finally, we examine whether earlier school entry comes at the cost of subsequent educational opportunities. Children entering school at a younger age may be less prepared academically, potentially reducing their chances of attending the academic track in secondary school. However, we find no evidence that the reform reduced the probability of attending the academic track. This suggests that the reduction in school starting age did not compromise subsequent educational progression, at least as measured by secondary school track choice.

Taken together, these results suggest that, although non-compliance meant that the reform was less effective than intended in lowering the age at school entry, the reduction in school starting age largely carries over to the transition to secondary school without evidence of adverse effects on subsequent educational opportunities.

5.2. Event-study evidence

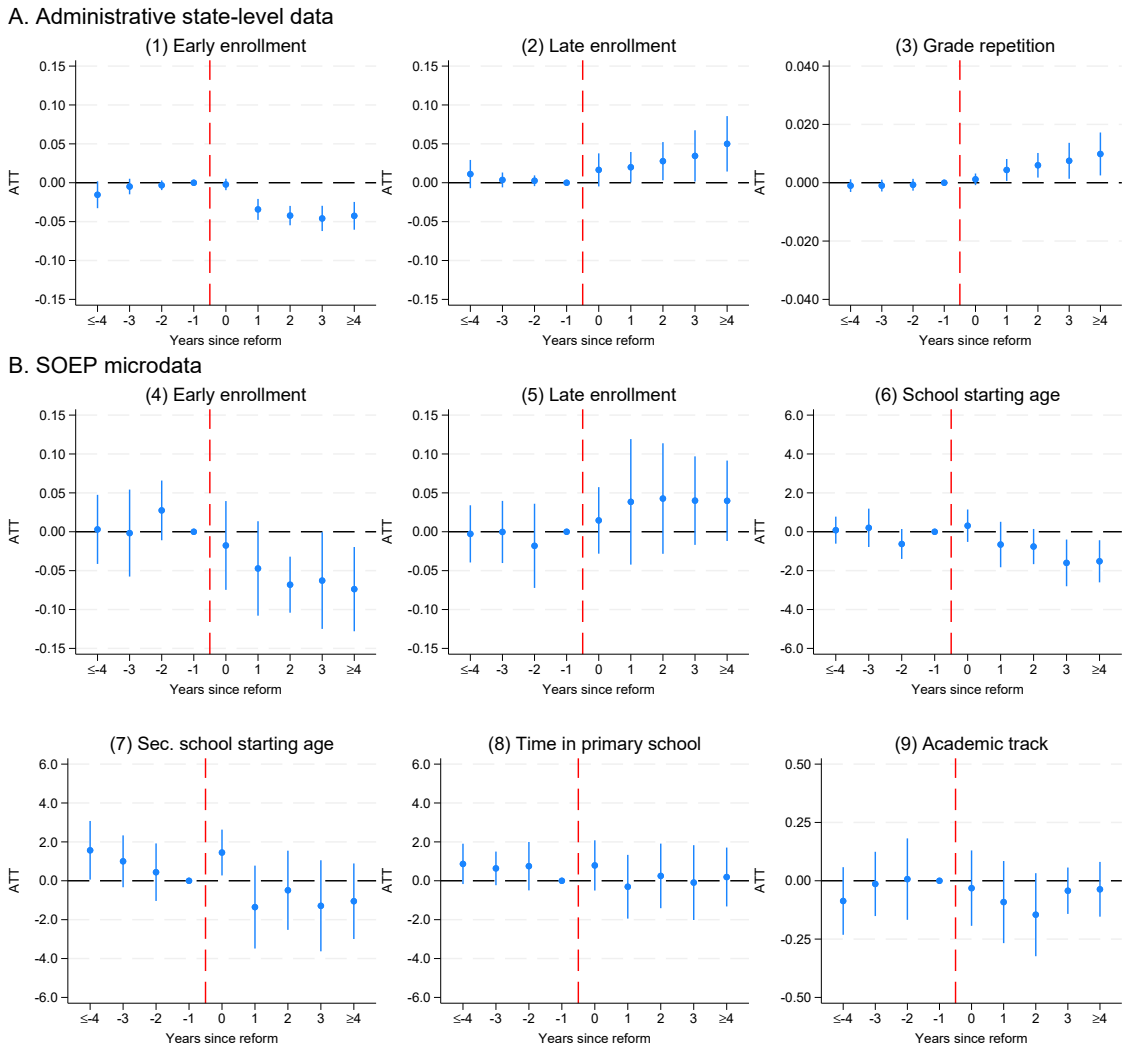
We now turn to the event-study estimates based on Equation (3), reported in Figure 1. These estimates allow us to examine the dynamic effects of the reform and to assess the plausibility of the parallel trends assumption. As before, Panel A reports results using administrative state-level data, while Panel B presents results based on SOEP data.

Across all nine outcomes, none of the pre-reform coefficients is statistically significant, providing support for the parallel trends assumption underlying our causal interpretation. This is also confirmed by visual inspection, as the pre-treatment coefficients show no systematic trends.

Turning to the dynamic effects, the administrative data show a decline in early enrolment due to the reform (subfigure 1), with effects emerging in the first year after implementation and becoming more pronounced in the second and third years.[8] Late enrolment increases almost immediately following the reform, with effects growing over time (subfigure 2). Grade repetition also increases over time, with the effects becoming statistically significant in the first year after implementation (subfigure 3).

[8] An important feature of the design is that early enrolment in the reform year may not yet be affected by the reform, as early enrolment for the first reform cohort takes place in the preceding year, when the old cut-off was still in place. This cohort therefore represents a transition cohort, consistent with the absence of a statistically significant effect on early enrolment in the reform year. We provide complementary evidence using cross-sectional measures of early and late enrolment and by re-estimating the baseline specifications excluding reform-year observations (Table B1, Columns (7) and (2)). The cross-sectional measures reveal a negative and statistically significant effect on early enrolment in the reform year, while the corresponding effect on late enrolment becomes insignificant, reflecting the fact that the two measures refer to different cohorts. Excluding transition cohorts modestly increases the estimated effects across all outcomes (Table B1), suggesting that the baseline estimates may be conservative.

Figure 1: Event-study estimates of the school-entry cut-off reform: Administrative and SOEP data



Notes: Event-study coefficients indicate the estimated effects of the reform on the respective outcomes relative to the year before implementation; event time is binned at ≤ -4 and ≥ 4. Panel A: administrative data with state and cohort fixed effects. Panel B: SOEP microdata with state $\times$ birth-month and cohort $\times$ birth-month fixed effects; second row: school entry, third row: school progression (children for whom both school entry and the transition to secondary school are observed). Ages in months. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the state level; the red dashed line marks the reform.
Source: Own calculations based on data from German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

The SOEP results show a broadly similar pattern for school entry. Early enrolment declines, although the year-specific effects are not statistically significant in the year of implementation or the following year (subfigure 4). For late enrolment, all post-reform point estimates are positive and similar in magnitude to the overall treatment effect, although none is individually statistically significant (subfigure 5). The average age at school entry also declines, with the effects becoming statistically significant only in the later years following implementation (subfigure 6).

Turning to the SOEP-based school progression outcomes, the event-study estimates for secondary school starting age are individually imprecise (subfigure 7). Nevertheless, four of the five post-reform coefficients are negative, while all three pre-reform coefficients are positive, suggesting a decline in secondary school starting age following the reform. For time spent in primary school (subfigure 8) and academic track participation (subfigure 9), none of the pre- or post-reform coefficients is statistically significant, consistent with the absence of detectable average treatment effects on these outcomes.

Overall, the event-study results are broadly consistent with the TWFE estimates, both in terms of the direction and timing of the effects. While the effects are generally more precisely estimated in the administrative data, the SOEP results paint a similar picture, further supporting the robustness of our findings.

5.3. Directly affected children

The estimates presented so far refer to all children, although the reform changed school eligibility only for children born between the old cut-off (30 June) and the post-reform cut-off. As these children account for less than a quarter of children in reform state-cohorts, the estimates for all children might dilute the reform's effect. Distinguishing between directly and not directly affected children shows how the target group responded, as directly affected children became eligible for school entry one year earlier, whereas the eligibility of all other children remained unchanged. Table 5 reports the results for directly affected children in columns (1) and (2) and for not directly affected children in columns (3) and (4).

For directly affected children, the reform had large effects on school entry: early enrolment decreased by 30.1 percentage points and late enrolment increased by 20.3 percentage points (Column (1)). The school starting age of directly affected children decreased by 5.5 months. Compliance, that is, entering school earlier than allowed under the old cut-off, increased by 46.2 percentage points, implying that less than half of the directly affected children responded to the reform by entering school one year earlier. The reduction in school starting age thus corresponds almost exactly to the share of compliers multiplied by twelve months ($0.462 \times 12 \approx 5.5$). The intensity estimates in Column (2) show that these effects increase with the extent of the cut-off postponement.

The reduction in school starting age persists through the transition to secondary school. Directly affected children enter secondary school 6.0 months earlier (Panel B, Column (1)), while the time spent in primary school remains unchanged. We find no effect on academic track participation. Thus, for directly affected children, earlier

Table 5: School cut-off reform: Directly and not directly affected children (SOEP)

	Directly affected		Not directly affected	
	(1) DiD	(2) Intensity DiD	(3) DiD	(4) Intensity DiD
Panel A. School entry				
Early enrolment	-0.301***	-0.064***	0.002	-0.002
	(0.048)	(0.013)	(0.020)	(0.007)
Mean dep. var.		0.091		
Late enrolment	0.203***	0.049***	0.011	0.001
	(0.045)	(0.012)	(0.011)	(0.004)
Mean dep. var.		0.117		
School starting age	-5.527***	-1.320***	0.186	0.065
	(0.962)	(0.308)	(0.257)	(0.111)
Mean dep. var.		78.156		
Compliance	0.462***	0.111***	0.002	-0.002
	(0.072)	(0.023)	(0.020)	(0.007)
Mean dep. var.		0.271		
State × birth-month FE	Yes	Yes	Yes	Yes
Cohort × birth-month FE	Yes	Yes	Yes	Yes
N (individuals)	4 049	4 049	6 213	6 213
Panel B. School progression				
Sec. school starting age	-6.029***	-1.428**	-0.230	-0.044
	(1.415)	(0.525)	(0.674)	(0.288)
Mean dep. var.		127.946		
Time in primary school	-0.335	0.034	-0.212	-0.059
	(0.940)	(0.249)	(0.455)	(0.165)
Mean dep. var.		49.818		
Academic track	-0.000	-0.007	-0.031	-0.010
	(0.061)	(0.024)	(0.032)	(0.012)
Mean dep. var.		0.422		
State × birth-month FE	Yes	Yes	Yes	Yes
Cohort × birth-month FE	Yes	Yes	Yes	Yes
N (individuals)	2 667	2 667	3 963	3 963

Notes: Coefficients indicate the estimated effects of the reform on directly and not directly affected children, using children in non-reform state-cohorts as the comparison group. Directly affected children are born in months for which the new cut-off implies school eligibility one year earlier than under the 30 June cut-off. Columns (1)–(2) compare directly affected children in reform state-cohorts with all children in non-reform state-cohorts; Columns (3)–(4) compare the remaining children in reform state-cohorts with the same group. Columns (1) and (3) use the reform dummy, Columns (2) and (4) the number of months by which the cut-off was postponed. Compliance: school entry earlier than allowed under the old rule. Mean dep. var. refers to the sample in Column (1). All models include state × birth-month and cohort × birth-month fixed effects; school-progression outcomes refer to children with observed school entry and transition. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

school entry translates into an earlier transition to secondary school without reducing the probability of attending the academic track.

For not directly affected children, the estimated effects on all outcomes are small and statistically insignificant (Columns (3) and (4)). The event-study estimates in Figure C1 confirm this pattern. For both groups, the pre-reform coefficients are close to zero. For directly affected children, compliance and the reduction in school starting age emerge with the first reform cohort and remain stable thereafter, whereas the estimates for not directly affected children remain close to zero throughout.

Taken together, these results show that the overall effects of the reform are driven by directly affected children. This pattern strongly supports a causal interpretation of our main estimates: confounding factors at the state level, such as changes in educational policies or economic conditions, would be expected to affect all children in a state rather than only those born in particular months. The results also clarify the moderate average effects for all children. Only a minority of children were directly affected by the reform, and less than half of them changed their school entry in response, so that the reform reduced school starting age by about half a year among directly affected children but by less than one month on average.

6. Robustness Checks

We assess the robustness of our findings in two steps, which mirror the structure of the analysis and of the appendix. We first examine the robustness of the estimates for all children (Appendix B): We (i) apply recently developed difference-in-differences estimators that accommodate staggered treatment adoption, and examine the sensitivity of our results to (ii) the sample definition, (iii) the outcome measures, (iv) the inclusion of control variables, and (v) alternative estimation and inference procedures, including survey weights and wild cluster bootstrap inference to account for the small number of clusters. We then apply these checks to the estimates for directly and not directly affected children (Appendix C).

Staggered treatment timing. Given the staggered implementation of the reform across federal states and years, the TWFE estimator may be biased in the presence of heterogeneous treatment effects (Goodman-Bacon, 2021). We therefore re-estimate the effects using the estimators proposed by Callaway and Sant'Anna (2021), Borusyak et al. (2024), and Sun and Abraham (2021), which avoid comparisons between earlier- and later-treated units by constructing cohort- and time-specific treatment effects. Table 6 reports the results.

Table 6: School cut-off reform: Alternative DiD estimators

	(1) TWFE	(2) CS	(3) BJS	(4) SA
Panel A. Administrative state-level data				
School entry				
Early enrolment	-0.028***	-0.037***	-0.031***	-0.038***
	(0.007)	(0.006)	(0.005)	(0.006)
Late enrolment	0.031*	0.045***	0.044***	0.044***
	(0.015)	(0.013)	(0.008)	(0.009)
School progression				
Grade repetition	0.008***	0.009***	0.009***	0.009***
	(0.003)	(0.003)	(0.002)	(0.003)
State FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
N (state × year)	272	272	272	272
Panel B. SOEP microdata				
School entry				
Early enrolment	-0.062***	-0.070**	-0.084***	-0.074**
	(0.017)	(0.031)	(0.019)	(0.029)
Late enrolment	0.039**	0.088***	0.072***	0.061***
	(0.014)	(0.025)	(0.011)	(0.013)
School starting age	-0.805**	-0.837*	-0.826***	-0.800**
	(0.319)	(0.504)	(0.189)	(0.369)
School progression				
Sec. school starting age	-1.314**	-1.039	-1.463***	-0.669
	(0.616)	(0.783)	(0.519)	(0.697)
Time in primary school	-0.358	0.775	-0.419	0.271
	(0.448)	(0.776)	(0.470)	(0.683)
Academic track	-0.023	-0.041	0.016	-0.066
	(0.031)	(0.058)	(0.032)	(0.052)
State × birth-month FE	Yes	Yes	Yes	Yes
Cohort × birth-month FE	Yes	Yes	Yes	Yes
N (individuals) school entry	7 186	7 186	7 186	7 186
N (individuals) school progression	4 538	4 538	4 538	4 538

Notes: Coefficients indicate the estimated effects of the reform on the respective outcomes for all children, using alternative estimators: TWFE (baseline, Table 3 and Table 4), Callaway and Sant'Anna (2021) (CS), Borusyak et al. (2024) (BJS), and Sun and Abraham (2021) (SA). Administrative data include state and cohort fixed effects. SOEP models include state × birth-month and cohort × birth-month fixed effects. School-progression outcomes refer to children with observed school entry and transition. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

The results for school entry are highly consistent across estimators, with all estimates remaining statistically significant. Using administrative data (Panel A), the reform reduces early enrolment by 2.8 to 3.8 percentage points and increases late enrolment by 3.1 to 4.5 percentage points, while grade repetition rises by 0.8 to 0.9 percentage points. The SOEP estimates (Panel B) show the same pattern, with early enrolment decreasing by 6.2 to 8.4 percentage points and late enrolment increasing by 3.9 to 8.8 percentage points. The estimated effects on school starting age are remarkably similar across estimators, ranging from -0.84 to -0.80 months. For early and late enrolment, the TWFE estimates are the smallest in absolute terms, suggesting that our baseline estimates are rather conservative. For the school-progression outcomes, some of the effects estimated using alternative estimators are less precisely estimated. The reduction in secondary school starting age is confirmed by the BJS estimator (-1.46 months), whereas the CS and SA estimates are smaller and not statistically significant. For time spent in primary school and academic track participation, none of the estimators yields a statistically significant effect, consistent with the TWFE results.

We also estimate event studies using the Callaway and Sant'Anna (2021) estimator (Figure B1), which yield results closely resembling the baseline event-study estimates in Figure 1.

Alternative sample definitions. We first examine whether our results are driven by particular states or years. Excluding one state at a time (Figure B2) or trimming up to three years from the beginning and/or end of the observation period (Figure B3) leaves the estimates largely unchanged. Columns (2)–(6) of Table B1 report additional robustness checks based on alternative sample restrictions: Excluding the reform year, when the first affected cohort may not yet have fully adjusted to the new regulations, increases the magnitude of the estimated effects across all outcomes (Column (2)), suggesting that the baseline estimates may be conservative. Excluding Berlin and Brandenburg, where primary school lasts six years and Berlin temporarily prohibited deferrals, slightly reduces the estimated effects (Column (3)) and the effect on late enrolment in the administrative data also becomes statistically insignificant. Excluding the Families in Germany (FiD) samples, which oversample certain groups of families, yields similar results (Column (4)).

For the school-progression outcomes, the baseline sample is restricted to children for whom both school entry and the transition to secondary school are observed. Including all children with an observed transition, regardless of whether school entry is observed, increases the magnitude of the estimated effect on secondary school starting age to -2.19 months, while the effect on academic track participation remains close

to zero (Column (5)). Conversely, restricting the school-entry analysis to children for whom the transition to secondary school is also observed yields an estimated reduction in school starting age of 0.96 months (Column (6)). This is comparable to the estimated reduction in secondary school starting age of 1.31 months obtained for the same children.

Alternative outcome measures. Column (7) reports results using alternative outcome measures based on the administrative data (Panel A), namely annual cross-sectional shares of early and late enrolment and grade repetition in grades 1–4. The estimates are consistent with the baseline results.

Additional control variables. Columns (8)–(10) examine the sensitivity of our results to the inclusion of additional controls. Adding individual-level controls (Column (8)) leaves the SOEP estimates virtually unchanged. Including time-varying state-level controls (Column (9)) yields similar estimates for the administrative data. In the SOEP data, the effect on late enrolment becomes somewhat larger, while the effects on primary and secondary school starting age become somewhat smaller but remain statistically significant. Controlling for the staggered introduction of the FLEX reform (Column (10)) leaves the results largely unchanged.

Alternative estimation and inference procedures. Using SOEP survey weights yields similar results, with a larger effect on school starting age and a somewhat smaller effect on late enrolment (Column (11)). Given the small number of clusters, conventional cluster-robust standard errors may lead to over-rejection. We therefore also compute wild cluster bootstrap p-values (Table B2). The bootstrap p-values are largely similar to the conventional ones; only the effect on school starting age becomes marginally significant ($p_{\text{WB}} = 0.052$). Our main conclusions remain unchanged.

Robustness for results on directly and not directly affected children. We apply our robustness checks also to the estimates for directly and not directly affected children (Appendix C). The alternative DiD estimators yield very similar effects for directly affected children on late enrolment (0.20 to 0.23), school starting age (-5.2 to -5.5 months), and compliance (0.44 to 0.59), and the BJS and SA estimators confirm the reduction in secondary school starting age (Table C1). The estimates for directly affected children are also stable across the alternative samples, control variables, and the use of survey weights considered in Table B1 (Table C2), while the effects on school entry for not directly affected children remain close to zero across all specifications (Table C3). The main effects for directly affected children remain statistically significant when using wild cluster bootstrap inference (Table C4). Excluding one state at a time or varying the

observation period leaves the estimates for directly affected children largely unchanged (Figure C2 and Figure C3). Finally, restricting the sample to state-cohorts with a 30 June or a 30 September cut-off, in which exactly the children born in July–September are directly affected, allows us to compare directly and not directly affected children in the spirit of a triple-differences estimator. This yields differences of −6.3 months in school starting age and −6.6 months in secondary school starting age (Table C5).

7. Effect Heterogeneity

7.1. Heterogeneity by child and family characteristics

We next examine whether the effects of the reform vary with individual and family characteristics, namely gender, migration background, parental education, and household income. We focus on directly affected children, as only for them did the reform change school eligibility, so that differences in their responses directly reveal which groups were more likely to comply with the new rule. Table 7 reports their effects, estimated separately for each group, together with tests of the equality of the effects within each dimension. Panel A reports the effects on school entry and Panel B those on school progression. Corresponding results for all children are reported in Table B3 and, for the administrative data by gender, in Table B4.

By gender (Columns (1)–(2)), compliance is significantly higher among boys than among girls (54 vs. 41 percentage points), and the reduction in school starting age is correspondingly larger for boys. Early enrolment, by contrast, declines more strongly among girls, whereas the increase in late enrolment is somewhat larger for boys. This gender pattern in early and late enrolment is consistent across all three sets of estimates: the administrative data, the SOEP data for all children (Table B3), and the SOEP data for directly affected children. It reflects the fact that girls are more likely to enter school early and boys are more likely to delay school entry. Under the old rule, many directly affected girls already entered school one year before their regular eligibility, so the reform reclassified their school entry as on time rather than changing its timing. The effects on school progression do not differ between boys and girls.

The results on family heterogeneity point to a socio-economic gradient in compliance with the reform. Compliance is highest among children whose parents have a low level of education, at 70 percentage points, compared with about 45 percentage points in the medium and high education groups (Columns (5)–(7)). Compliance is also higher among children with a migration background than among children without one (63 vs. 44 percentage points; Columns (3)–(4)). Both differences are marginally significant. By household income, compliance declines from 61 percentage points in

the lowest to 46 percentage points in the highest tertile (Columns (8)–(10)), although this difference is not statistically significant. Correspondingly, the reduction in school starting age is largest for children of parents with low education, children with a migration background, and children from low-income households (about 8 months, compared with 5 to 6 months in the respective reference groups). Children from more advantaged families were thus more likely to retain the previous timing of school entry.

Panel B suggests, however, that socially disadvantaged groups lose part of their earlier start by the transition to secondary school. For children with a migration background and children of parents with low education, the effect on secondary school starting age is considerably smaller than the effect on school starting age and not statistically significant. At the same time, the time spent in primary school increases for these groups; the difference from the respective reference group is statistically significant for parental education and marginally significant for migration background. For children from low-income households, the reduction in secondary school starting age is also smaller than the reduction in school starting age, and their probability of attending the academic track declines, although the difference across income groups is only marginally significant. In contrast, among children from more advantaged families, the reduction in school starting age largely carries over to the transition to secondary school. As these estimates rely on small subgroups and are partly imprecise, this evidence should be regarded as suggestive. Nevertheless, it indicates that the children from disadvantaged socio-economic backgrounds who were most likely to enter school earlier under the new rule may have benefited least from the earlier start in their subsequent school careers.

Table 7: School cut-off reform: Heterogeneity of the effects for directly affected children

	Gender		Migration		Parental education			Household income		
	(1) Boys	(2) Girls	(3) Yes	(4) No	(5) Low	(6) Medium	(7) High	(8) Low	(9) Middle	(10) High
Panel A. School entry										
Early enrolment	-0.206***	-0.379***	-0.152	-0.341***	-0.167	-0.336***	-0.322***	-0.162	-0.235***	-0.340***
	(0.057)	(0.054)	(0.100)	(0.038)	(0.133)	(0.078)	(0.074)	(0.093)	(0.069)	(0.094)
p-value	0.003		0.043			0.390			0.296	
Late enrolment	0.210***	0.180***	0.170***	0.187***	0.114	0.195***	0.170***	0.144**	0.217***	0.164**
	(0.060)	(0.036)	(0.052)	(0.045)	(0.065)	(0.051)	(0.052)	(0.068)	(0.049)	(0.064)
p-value	0.443		0.772			0.293			0.378	
School starting age	-6.600***	-4.879***	-7.606***	-5.330***	-7.961***	-5.312***	-5.823***	-7.837***	-6.517***	-5.597***
	(1.279)	(0.822)	(1.551)	(0.841)	(1.705)	(1.306)	(1.327)	(1.017)	(1.038)	(1.673)
p-value	0.062		0.120			0.171			0.298	
Compliance	0.539***	0.408***	0.634***	0.440***	0.697***	0.444***	0.453***	0.608***	0.520***	0.455***
	(0.090)	(0.068)	(0.118)	(0.063)	(0.136)	(0.103)	(0.085)	(0.077)	(0.083)	(0.123)
p-value	0.042		0.082			0.051			0.361	
N (individuals)	2 034	2 015	1 017	3 032	774	1 396	1 862	1 121	1 553	1 305
Panel B. School progression										
Sec. school starting age	-6.672***	-5.960***	-5.157	-6.435***	0.336	-5.908***	-6.395**	-5.350**	-8.297***	-6.895***
	(1.231)	(1.599)	(3.268)	(1.486)	(3.409)	(1.411)	(2.326)	(2.324)	(1.783)	(1.797)
p-value	0.630		0.711			0.098			0.686	
Time in primary school	0.136	-0.926	1.839*	-0.886	5.930*	-0.911	-0.040	1.135	-0.748	-0.237
	(1.308)	(0.989)	(1.036)	(1.014)	(2.814)	(1.129)	(2.026)	(2.645)	(1.333)	(2.465)
p-value	0.511		0.078			0.035			0.817	
Academic track	0.024	-0.003	0.116	0.003	0.026	0.028	-0.036	-0.275**	-0.019	0.139
	(0.083)	(0.081)	(0.133)	(0.074)	(0.120)	(0.158)	(0.113)	(0.100)	(0.080)	(0.179)
p-value	0.827		0.478			0.867			0.062	
N (individuals)	1 363	1 304	621	2 046	458	922	1 281	721	1 021	891

Notes: Coefficients indicate the estimated effects of the reform on directly affected children (who became eligible for school entry one year earlier under the new cut-off), separately by subgroup. Directly affected children in reform state-cohorts are compared with children of the same subgroup in non-reform state-cohorts. Parental education: highest CASMIN level of the parents; household income: tertiles of equivalised household income. *p*-values test the equality of the effects within each dimension in a pooled regression with group-specific effects and fixed effects. All models include state × birth-month and cohort × birth-month fixed effects; school-progression outcomes are estimated for children for whom both school entry and the transition to secondary school are observed. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

7.2. Heterogeneity across states

We next examine heterogeneity in the effects of the reform across federal states. We first focus on directly affected children (Figure C4), as their state-specific estimates reveal how strongly compliance varied across states. Compliance ranges from 16% in Rhineland-Palatinate and about 30% in Bavaria and Thuringia to about 50–60% in Berlin, Lower Saxony, and North Rhine-Westphalia and over 60% in Brandenburg. The state-specific reductions in school starting age closely mirror these differences: across states, the reduction in school starting age of directly affected children corresponds closely to the share of compliers multiplied by twelve months. Compliance is lowest in the states that postponed the cut-off by only one or two months (Thuringia and Rhineland-Palatinate), where many of the directly affected children born in July and August already entered school early before the reform. The state-specific estimates for the school-progression outcomes are imprecise and should be interpreted with caution, in particular for Berlin and Brandenburg, where primary school comprises six grades.

These differences in compliance also help to explain the state-specific estimates for all children, presented in Figure B4 and obtained by interacting the reform indicator with indicators for the eight reform states. In the administrative data, the reform reduced early enrolment in nearly all states, with the largest declines in Berlin, Baden-Württemberg, and Bavaria and negligible effects in Rhineland-Palatinate and North Rhine-Westphalia. Increases in late enrolment are concentrated in Bavaria, Brandenburg, Baden-Württemberg, and Berlin, while late enrolment hardly changed in Lower Saxony, North Rhine-Westphalia, and Rhineland-Palatinate. Where compliance is low, as in Bavaria, more directly affected children keep their previous timing of school entry and are thus classified as late entrants, which explains the large increase in late enrolment in Bavaria. Conversely, in states with high compliance, such as Lower Saxony and North Rhine-Westphalia, the effects on late enrolment are small. In Rhineland-Palatinate, the small share of directly affected children additionally explains the small effects for all children. The estimated effects on grade repetition are positive in all reform states.

The SOEP estimates for all children are less precise but show broadly similar patterns. Early enrolment declines in most states, most strongly in Berlin, and late enrolment increases most in Berlin and Brandenburg. School starting age declines in all states except Thuringia, with the largest reductions in Brandenburg and North Rhine-Westphalia. The state-specific effects on early enrolment (correlation of 0.65) and especially on late enrolment (correlation of 0.85) are highly correlated across the administrative and SOEP data.

8. Reversal of the Reform

Finally, we examine whether the effects are symmetric when the reform is reversed. From 2017 onwards, several states moved their cut-off dates back towards 30 June (see Table A1). We focus on Berlin, which moved its cut-off back from 31 December to 30 September in 2017, and Baden-Württemberg, which moved it back from 30 September to 30 June in three steps from 2020 onwards. Bavaria and Lower Saxony introduced entry corridors, within which parents can choose the year of school entry, and are therefore excluded from this analysis. Using the eligibility cohorts 2012–2022, we estimate specifications analogous to the baseline models, with treatment defined as the cut-off being moved back. Table 8 reports the results.

The results point to broadly symmetric effects. Using administrative data (Panel A), the reversal increased early enrolment by 3.2 percentage points and reduced late enrolment by 4.5 percentage points (Column (1)). These effects are opposite in direction to, and somewhat larger in magnitude than, the effects of the reform's introduction (Table 3). The intensity estimates show that each additional month by which the cut-off was moved back increases early enrolment by 1.2 percentage points and reduces late enrolment by 1.8 percentage points (Column (2)). The effect on grade repetition is small and not statistically significant. The SOEP estimates for all children show the same pattern, with an increase in early enrolment of 4.4 percentage points and a decrease in late enrolment of 4.9 percentage points, while the effect on school starting age is positive but not statistically significant (Panel B, Column (1)).

The estimates for directly affected children provide the clearest evidence of symmetry. For these children, the reversal increased early enrolment by 24.2 percentage points, reduced late enrolment by 22.8 percentage points, and increased school starting age by 6.6 months (Panel B, Column (3)). These effects closely mirror the effects of the reform's introduction for directly affected children (Table 5), which reduced early enrolment by 30.1 percentage points, increased late enrolment by 20.3 percentage points, and reduced school starting age by 5.5 months. For not directly affected children, the effects on early and late enrolment are small and not statistically significant (Column (4)), so that the small effect on school starting age for all children reflects the small share of directly affected children.

Overall, the symmetric response to the reversal provides additional support for a causal interpretation of our baseline results. If the estimated effects were driven by unobserved confounding factors or unrelated trends, one would not expect outcomes to respond in the opposite direction when the policy is reversed, and least of all only for children born in the months affected by the change.

Table 8: Reversal of the school cut-off reform (cohorts 2012–2022)

	All children		Directly affected	Not directly affected
	(1) DiD	(2) Intensity DiD	(3) DiD	(4) DiD
Panel A. Administrative state-level data				
Early enrolment	0.032***	0.012***	–	–
	(0.006)	(0.004)		
Mean dep. var.		0.038		
Late enrolment	-0.045***	-0.018***	–	–
	(0.007)	(0.002)		
Mean dep. var.		0.068		
Grade repetition	0.002	0.001	–	–
	(0.002)	(0.001)		
Mean dep. var.		0.016		
State FE	Yes	Yes	–	–
Cohort FE	Yes	Yes	–	–
N (state × year)	154	154		
N (individuals)		6 061 885	–	–
Panel B. SOEP microdata				
Early enrolment	0.044***	0.024***	0.242***	0.014
	(0.009)	(0.005)	(0.079)	(0.009)
Mean dep. var.		0.078	0.076	0.077
Late enrolment	-0.049*	-0.028***	-0.228***	-0.022
	(0.025)	(0.007)	(0.027)	(0.018)
Mean dep. var.		0.141	0.137	0.141
School starting age	0.407	0.151	6.611***	-0.419*
	(0.514)	(0.234)	(1.102)	(0.232)
Mean dep. var.		78.476	78.492	78.473
State × birth-month FE	Yes	Yes	Yes	Yes
Cohort × birth-month FE	Yes	Yes	Yes	Yes
N (individuals)	4 626	4 626	4 432	4 601

Notes: Coefficients indicate the estimated effects of reversing the school entry cut-off reform on the respective outcomes in Baden-Württemberg (from 2020) and Berlin (from 2017). Bavaria and Lower Saxony, which introduced entry corridors, are excluded. Columns (1)–(2): all children; in Column (2), treatment is the number of months by which the cut-off was moved back relative to 2012. Column (3): directly affected children (eligible one year later than under the 2012 rule) compared with all children in non-reversal state-cohorts; Column (4): the remaining children in reversal state-cohorts compared with the same group. Mean dep. var. refers to the respective estimation sample. Administrative data include state and cohort fixed effects (Columns (3)–(4) not applicable, as birth months are not observed); SOEP models include state × birth-month and cohort × birth-month fixed effects. Standard errors clustered at the state level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

9. Conclusion

School entry age policies are a central instrument through which education systems seek to balance competing objectives. On the one hand, lowering the school starting age has the potential to shorten educational careers and facilitate earlier entry into the labour market, thereby increasing lifetime labour supply and economic productivity. On the other hand, enrolling children at a younger relative age may come at the cost of adverse early school outcomes, particularly if children are not developmentally ready for school. Understanding how these trade-offs play out in practice is therefore crucial for the design of effective and equitable education policy.

This paper provides evidence on these trade-offs by evaluating a reform that postponed the school entry cut-off date in half of the German federal states, allowing some children to enter school one year earlier. Exploiting the staggered adoption of the reform across states and cohorts, and combining administrative state-level data with individual-level survey data, we assess how the reform affected actual school entry, school starting age, and school progression. Our findings show that postponing the cut-off date did not translate mechanically into earlier school entry. The reform reduced early enrolment and increased late enrolment, so that the average school starting age decreased by less than one month, compared with an average postponement of the cut-off by almost three months. The reduction in school starting age largely carried over to the transition to secondary school, and we find no effect on the probability of attending the academic track. Grade repetition in grades three and four of primary school increased slightly in the administrative data, although we do not detect a corresponding increase in the time spent in primary school in the SOEP data. Our findings are supported by the absence of pre-reform trends, a range of robustness checks, and the effects of the partial reversal of the reform, which closely mirror the effects of its introduction.

The analysis of directly affected children shows why the average effects are moderate. Less than half of the children who became eligible for school one year earlier entered school earlier, whereas the others kept their previous timing of school entry. Among directly affected children, school starting age and the age at the transition to secondary school decreased by about half a year, while the effects for children whose eligibility did not change are close to zero. Compliance was highest among children of parents with low education and children with a migration background, whereas more advantaged families more often retained the previous timing of school entry. However, there is suggestive evidence that the initial advantage of disadvantaged children narrows by the transition to secondary school, as they spend more time in primary school.

Overall, our results are consistent with existing evidence highlighting the importance of behavioural responses to education reforms. As shown by Deming and Dynarski (2008), changes in school entry rules do not mechanically translate into corresponding changes in actual enrolment behaviour. Similarly, evidence from the German G8 reform, which shortened secondary schooling by one year, shows that students lost part of their initial age advantage during secondary school (Huebener and Marcus, 2017), as well as during the transition to university and while enrolled in higher education (Marcus and Zambre, 2019). More broadly, our findings contribute to the literature on education reforms aimed at facilitating earlier labour market entry by showing that changes in the timing or duration of education do not necessarily translate into equivalent changes in actual educational trajectories, as behavioural responses may offset part of the intended acceleration.

With respect to the literature on school entry cut-offs, we do not examine the consequences of being born before or after a given cut-off date but focus on the effects of changing the cut-off itself, which is directly relevant for policy design. To our knowledge, this is one of the first studies to analyse the effects of postponing the cut-off, and it provides evidence from a country with an early tracking school system. By distinguishing between directly and not directly affected children, we show that limited compliance explains the moderate average effects of the reform.

One potential reason for the limited compliance is Germany's early school tracking system, in which pupils are sorted into tracks with long-term educational and career implications, only one of which leads directly to higher education. Track placement is based on teachers' recommendations at the end of primary school and depends strongly on pupils' academic performance. Since children who start school at a younger age tend to underperform relative to older classmates, particularly in primary school (Bedard and Dhuey, 2006; McEwan and Shapiro, 2008; Sontheim, 2025), earlier school entry may negatively affect tracking decisions (Puhani and Weber, 2007; Oosterbeek et al., 2021). To mitigate this risk, school entry may be delayed through red-shirting, that is, the intentional postponement of school enrolment despite legal eligibility in order to secure a relative age advantage and improve academic performance, teacher evaluations, and track placement. This explanation is consistent with our finding that more advantaged families were less likely to comply with the new rule. However, we find no evidence that the reform reduced the probability of attending the academic track, neither for all children nor for directly affected children on average; only among directly affected children from low-income households did this probability decline. Concerns about adverse effects on track placement may therefore be larger than the actual consequences of an earlier school start.

This study is subject to several limitations. First, we cannot reliably assess long-term educational or labour market outcomes, such as completed schooling, employment, or wages. Most of the affected cohorts have not yet completed their education, and for the few early cohorts for which such outcomes are observable, the SOEP samples are too small to yield precise estimates. Similarly, our data do not contain measures of academic achievement. Second, we cannot directly observe parental expectations or decision-making processes regarding enrolment. Finally, the German institutional context, in particular early tracking, may limit the external validity of our findings. Future research could examine whether similar reforms have different effects in countries without early tracking and, once suitable data become available, explore the long-term academic and labour market consequences of changes in school entry rules.

Despite these limitations, our findings have important policy implications. First, changes in school entry cut-offs should not be expected to translate one-to-one into changes in school starting age: in a setting in which parents can defer school entry, fewer than half of the children who became eligible one year earlier actually entered school earlier, so that postponing the cut-off by almost three months reduced the average school starting age by less than one month. Second, for directly affected children, the earlier school entry carried over to the transition to secondary school without reducing their probability of attending the academic track, suggesting that an earlier start does not necessarily worsen subsequent educational opportunities. Third, as compliance differs by socio-economic background, such reforms change school starting age most for disadvantaged children, who may need additional support in primary school to retain the benefits of an earlier start. Overall, cut-off reforms can lower school starting age substantially for children who comply, but their average effect depends on how many families, and which families, follow the new rule.

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Appendix A. Institutional Details and Construction of Administrative Outcomes

Appendix A.1. Full reform cycle

Table A1: Full school entry reform cycle (2000–2024)

Federal State / Year													
Panel A: 2000–2012													
	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
Baden-Wuerttemberg	6	6	6	6	6	7	8	9	9	9	9	9	9
Bavaria	6	6	6	6	6	7	8	9	10	11	9	9	9
Berlin	6	6	6	6	6	12	12	12	12	12	12	12	12
Brandenburg	6	6	6	6	6	9	9	9	9	9	9	9	9
Bremen	6	6	6	6	6	6	6	6	6	6	6	6	6
Hamburg	6	6	6	6	6	6	6	6	6	6	6	6	6
Hesse	6	6	6	6	6	6	6	6	6	6	6	6	6
Mecklenburg-WP	6	6	6	6	6	6	6	6	6	6	6	6	6
Lower Saxony	6	6	6	6	6	6	6	6	6	6	7	8	9
North Rhine-Westphalia	6	6	6	6	6	6	6	7	7	8	8	9	9
Rhineland-Palatinate	6	6	6	6	6	6	6	6	8	8	8	8	8
Saarland	6	6	6	6	6	6	6	6	6	6	6	6	6
Saxony	6	6	6	6	6	6	6	6	6	6	6	6	6
Saxony-Anhalt	6	6	6	6	6	6	6	6	6	6	6	6	6
Schleswig-Holstein	6	6	6	6	6	6	6	6	6	6	6	6	6
Thuringia	6	6	6	7	7	7	7	7	7	7	7	7	7

Panel B: 2013–2024												
	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022	2023	2024
Baden-Wuerttemberg	9	9	9	9	9	9	9	8	7	6	6	6
Bavaria	9	9	9	9	9	9	6–9	6–9	6–9	6–9	6–9	6–9
Berlin	12	12	12	12	9	9	9	9	9	9	9	9
Brandenburg	9	9	9	9	9	9	9	9	9	9	9	9
Bremen	6	6	6	6	6	6	6	6	6	6	6	6
Hamburg	6	6	6	6	6	6	6	6	6	6	6	6
Hesse	6	6	6	6	6	6	6	6	6	6	6	6
Mecklenburg-WP	6	6	6	6	6	6	6	6	6	6	6	6
Lower Saxony	9	9	9	9	F	F	F	F	F	F	F	F
North Rhine-Westphalia	9	9	9	9	9	9	9	9	9	9	9	9
Rhineland-Palatinate	8	8	8	8	8	8	8	8	8	8	8	8
Saarland	6	6	6	6	6	6	6	6	6	6	6	6
Saxony	6	6	6	6	6	6	6	6	6	6	6	6
Saxony-Anhalt	6	6	6	6	6	6	6	6	6	6	6	6
Schleswig-Holstein	6	6	6	6	6	6	6	6	6	6	6	6
Thuringia	7	7	7	7	7	7	7	7	7	7	7	7

Notes: Entries show the statutory school-entry cut-off month in each state and year. Shading reflects the postponement relative to June (month 6): the darker the blue, the larger the postponement. "6–9" denotes a three-month entry corridor in Bavaria, in which parents decide whether their child starts school that year or one year later. "F" indicates the flexibilization of school entry in Lower Saxony.
Sources: Berthold (2008); Standing Conference of the Ministers of Education and Cultural Affairs (2024)

Appendix A.2. Cohort-based construction of administrative outcomes

Let r denote the enrolment (report) year. The number of pupils starting school in year r is reported in the German Federal Statistical Office (2026a) data as

$$C_r = L_r + O_r + E_r,$$

where L_r, O_r, and E_r denote late, on-time, and early enrolees reported in year r, respectively. Although all pupils counted in C_r start school in calendar year r, these components correspond to different eligibility cohorts: O_r refers to the cohort scheduled to start in year r, E_r refers to the subsequent eligibility cohort, and L_r refers to the previous eligibility cohort. As a consequence, the reported enrolment shares combine information from different eligibility cohorts in the denominator. When cohort sizes vary over time, this mechanical mixing may distort cohort-specific enrolment behaviour and blur the timing of policy effects.

To obtain cohort-consistent enrolment measures, we realign the administrative counts across adjacent report years. Let t denote an eligibility cohort and let r be the year in which cohort t is scheduled to enrol on time. By construction, children from cohort t who enrol early are recorded in report year $r-1$, those who enrol on time in year r, and those who enrol late in year $r+1$.

We therefore reconstruct the size of eligibility cohort t as

$$C_t = E_{r-1} + O_r + L_{r+1},$$

and define cohort-consistent enrolment rates as

$$\text{LateEnrol}_t = \frac{L_{r+1}}{E_{r-1} + O_r + L_{r+1}}, \qquad \text{EarlyEnrol}_t = \frac{E_{r-1}}{E_{r-1} + O_r + L_{r+1}}.$$

The administrative statistics report the number of grade repeaters at the state-year-grade level. Let r denote the school year and $g \in \{1, 2, 3, 4\}$ the grade level, and let $R_{r,g}$ denote the number of pupils repeating grade g in year r. Since a pupil repeating grade g in year r entered primary school in year $t = r - g$ if no grade was repeated before, we assign repeaters to entry cohort t using this mapping. For each state and entry cohort t, the number of repeaters during primary school is constructed as

$$R_t = \sum_g R_{t+g,\,g},$$

and the grade repetition rate as

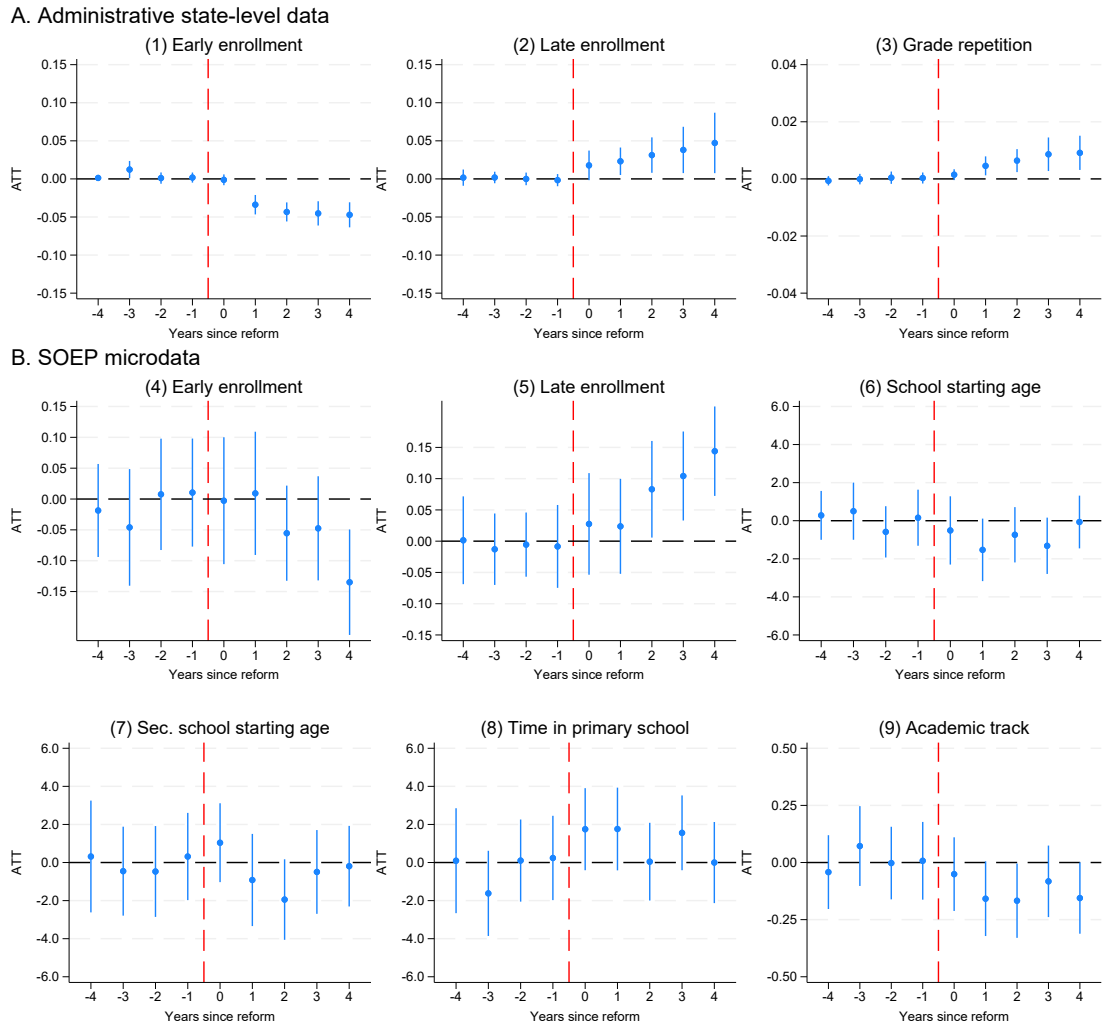
$$\text{GradeRepetition}_t = \frac{R_t}{L_t + O_t + E_t},$$

where the denominator is the total number of pupils who started school in year t, so that numerator and denominator both refer to the year of school entry. In the main analysis, we sum over grades 3 and 4 only ($g \in \{3, 4\}$), as repetition in grades 1 and 2 is not reported consistently after the introduction of FLEX; the measure based on all four grades is used as a robustness check.

This mapping is an approximation, as it assumes that pupils have not repeated an earlier grade and have not spent additional time in the flexible school entry phase. Since repetition rates are low (2.2% on average), multiple repetitions are rare and the resulting misclassification is likely small, mainly blurring the timing of the effects. Moreover, the results are similar when using annual cross-sectional repetition shares, which do not rely on this mapping (Table B1, Column (7)), and when controlling for the FLEX reform (Column (10)).

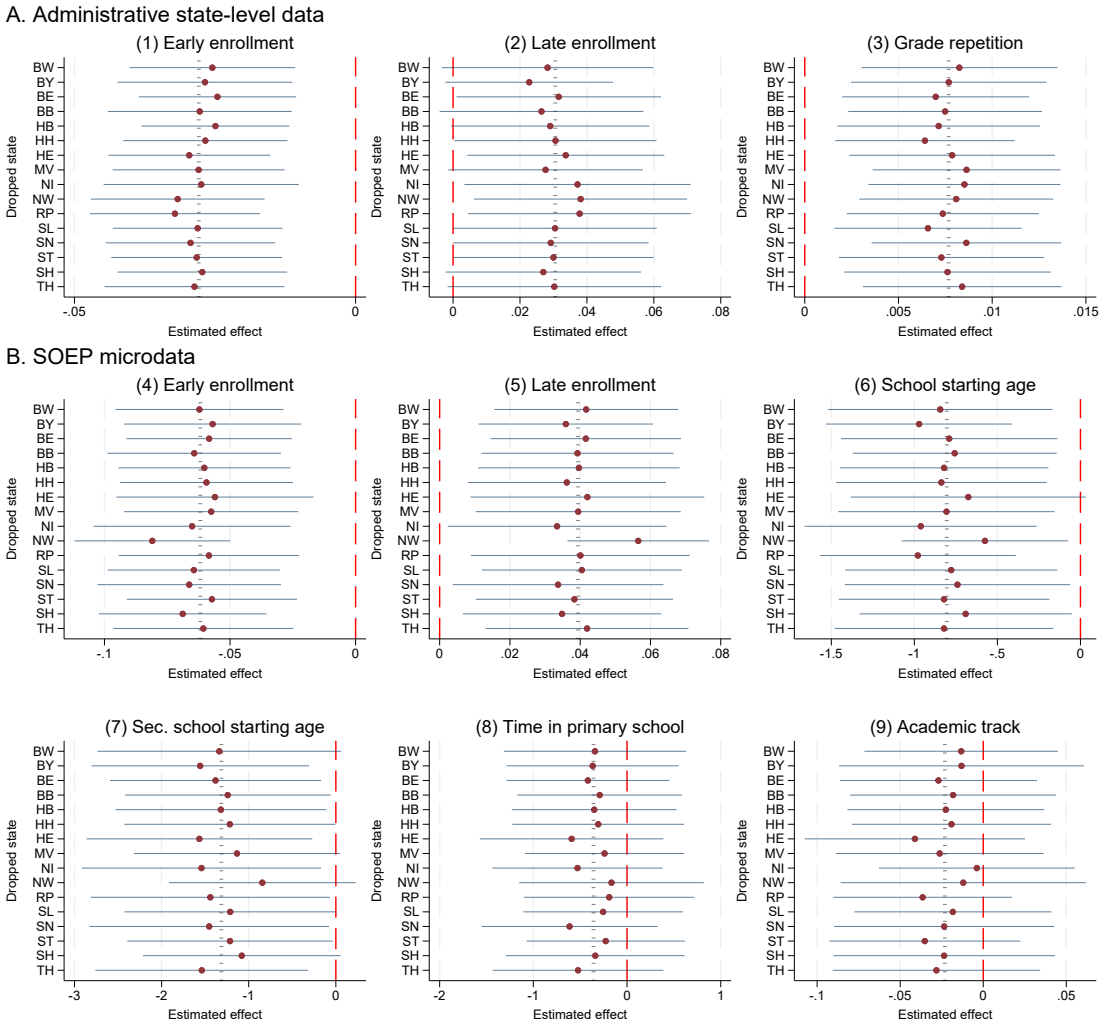
Appendix B. Additional tables and figures: effects for all children

Figure B1: Event-study estimates of the school entry cut-off reform (Callaway and Sant'Anna estimator)



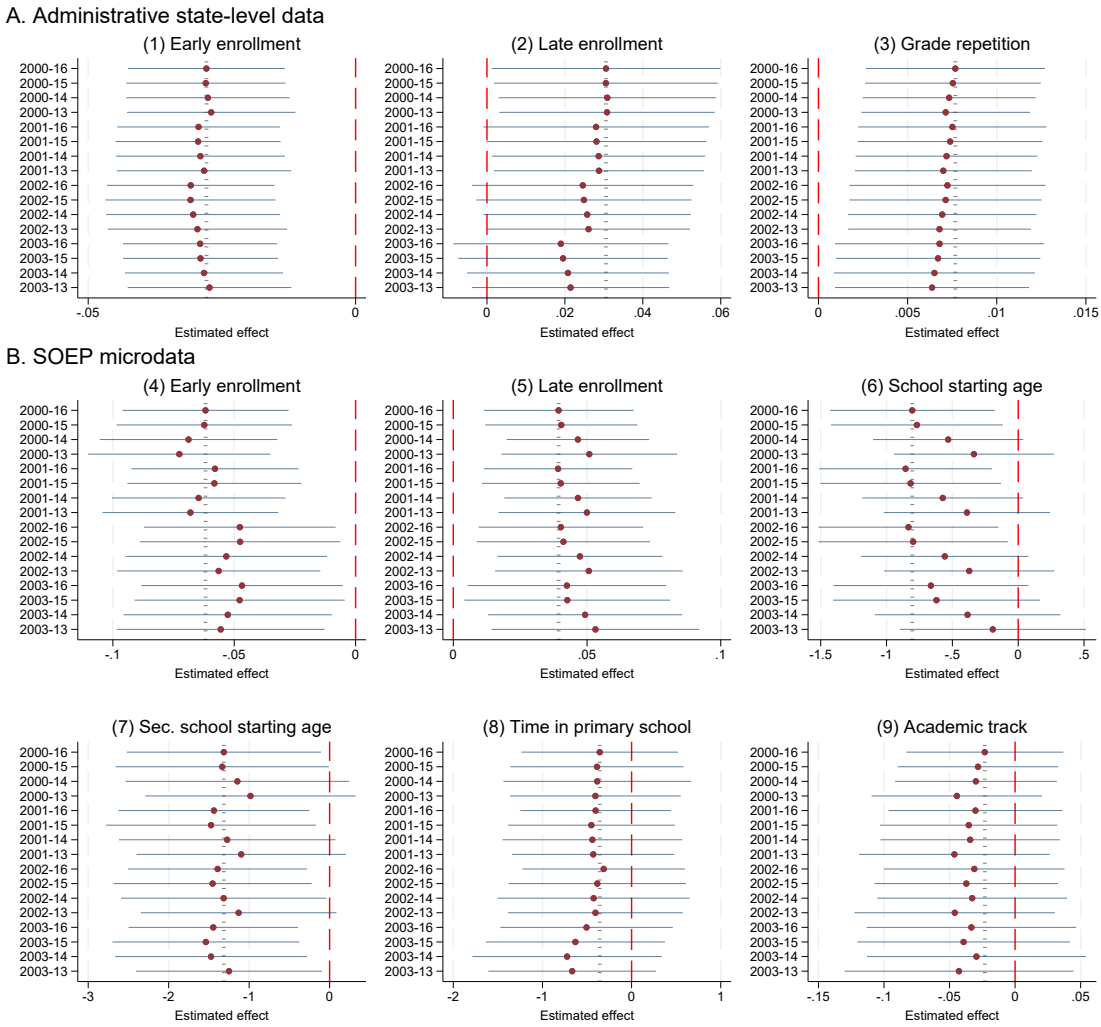
Notes: Event-study estimates from the Callaway and Sant'Anna (2021) estimator (never-treated states as controls, doubly robust estimation). Panel A uses administrative data. Panel B uses unweighted SOEP microdata in repeated cross-section mode with birth-month controls; school-progression outcomes are estimated for children for whom both school entry and the transition to secondary school are observed. Ages in months. Post-reform estimates are relative to the year preceding the reform; pre-reform estimates compare consecutive years. Shaded areas indicate 95% confidence intervals, with standard errors clustered at the state level.
Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

Figure B2: Leave-one-state-out robustness checks



Notes: Estimates show the effects of the reform on the respective outcomes, excluding one state at a time. Administrative data include state and cohort fixed effects. SOEP models include state × birth-month and cohort × birth-month fixed effects; school-progression outcomes are estimated for children for whom both school entry and the transition to secondary school are observed. Ages in months. Horizontal lines denote 95% confidence intervals, with standard errors clustered at the state level. The dotted line marks the full-sample estimate, the red dashed line marks zero. BW = Baden-Württemberg, BY = Bavaria, BE = Berlin, BB = Brandenburg, HB = Bremen, HH = Hamburg, HE = Hessen, MV = Mecklenburg-Western Pomerania, NI = Lower Saxony, NW = North Rhine-Westphalia, RP = Rhineland-Palatinate, SL = Saarland, SN = Saxony, ST = Saxony-Anhalt, SH = Schleswig-Holstein, TH = Thuringia.
Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

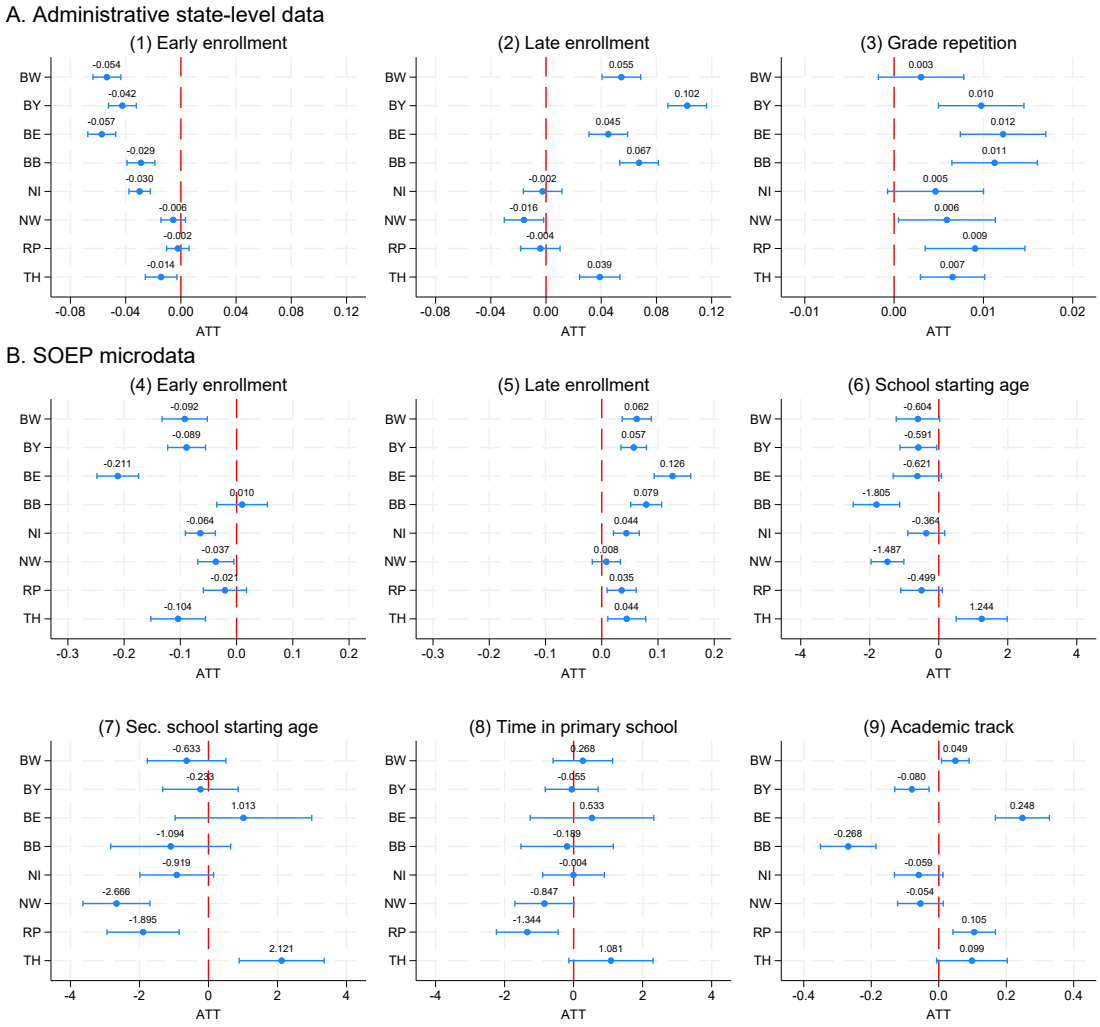
Figure B3: Time-window robustness checks



Notes: Estimates show the effects of the reform on the respective outcomes, over alternative cohort windows, trimming up to three years from the start (2000) and/or the end (2016) of the sample period. Administrative data include state and cohort fixed effects. SOEP models include state × birth-month and cohort × birth-month fixed effects; school-progression outcomes are estimated for children for whom both school entry and the transition to secondary school are observed. Ages in months. Horizontal lines denote 95% confidence intervals, with standard errors clustered at the state level. The dotted line marks the full-sample estimate, the red dashed line marks zero.

Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

Figure B4: State-specific effects of the school entry cut-off reform



Notes: Estimates show the state-specific effects of the reform on the respective outcomes, obtained by interacting the reform indicator with indicators for the eight reform states. Administrative data include state and cohort fixed effects. SOEP models include state × birth-month and cohort × birth-month fixed effects; school-progression outcomes are estimated for children for whom both school entry and the transition to secondary school are observed. Ages in months. Horizontal lines denote 95% confidence intervals, with standard errors clustered at the state level; the red dashed line marks zero. In Berlin and Brandenburg, primary school comprises six grades. BW = Baden-Württemberg, BY = Bavaria, BE = Berlin, BB = Brandenburg, NI = Lower Saxony, NW = North Rhine-Westphalia, RP = Rhineland-Palatinate, TH = Thuringia.
Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

Table B1: School entry cut-off reform: robustness checks

		Sample restrictions					Outcomes	Additional controls			Inference
	(1) Baseline	(2) Excl. reform year	(3) Without BE, BB	(4) Without FiD	(5) Transition observed	(6) Common sample	(7) Annual shares	(8) Individual controls	(9) State controls	(10) FLEX reform	(11) Weighted
Panel A. Administrative state-level data											
School entry											
Early enrolment	-0.028***	-0.034***	-0.024**	–	–	–	-0.038***	–	-0.029***	-0.029***	–
	(0.007)	(0.008)	(0.008)				(0.008)		(0.008)	(0.007)	
Late enrolment	0.031*	0.034**	0.027	–	–	–	0.028*	–	0.033**	0.033*	–
	(0.015)	(0.016)	(0.017)				(0.015)		(0.015)	(0.016)	
School progression											
Grade repetition	0.008***	0.009***	0.007**	–	–	–	0.013***	–	0.008***	0.008***	–
	(0.003)	(0.003)	(0.003)				(0.004)		(0.002)	(0.003)	
N (state × year)	272	264	238				272		272	272	
Panel B. SOEP microdata											
School entry											
Early enrolment	-0.062***	-0.074***	-0.061***	-0.058***	–	-0.049**	–	-0.061***	-0.060***	-0.058***	-0.065**
	(0.017)	(0.018)	(0.017)	(0.019)		(0.018)		(0.018)	(0.019)	(0.017)	(0.025)
Late enrolment	0.039**	0.047***	0.042***	0.030*	–	0.043**	–	0.040**	0.051***	0.040**	0.024*
	(0.014)	(0.015)	(0.013)	(0.015)		(0.016)		(0.016)	(0.011)	(0.015)	(0.013)
School starting age	-0.805**	-1.140***	-0.739**	-0.974**	–	-0.957**	–	-0.808**	-0.573*	-0.842**	-1.110***
	(0.319)	(0.299)	(0.325)	(0.342)		(0.429)		(0.328)	(0.313)	(0.295)	(0.347)
N (individuals)	7 186	6 918	6 741	4 166		4 538		7 186	7 186	7 186	7 085
School progression											
Sec. school starting age	-1.314**	-1.897**	-1.308**	-1.500**	-2.188***	–	–	-1.318*	-0.921**	-1.288**	-1.532**
	(0.616)	(0.793)	(0.604)	(0.550)	(0.412)			(0.651)	(0.421)	(0.513)	(0.690)
Time in primary school	-0.358	-0.506	-0.354	-0.459	–	–	–	-0.350	-0.212	-0.355	-0.296
	(0.448)	(0.552)	(0.441)	(0.384)				(0.458)	(0.291)	(0.483)	(0.398)
Academic track	-0.023	-0.029	-0.022	-0.016	0.027	–	–	-0.024	-0.046	-0.029	0.028
	(0.031)	(0.036)	(0.031)	(0.029)	(0.019)			(0.034)	(0.036)	(0.034)	(0.027)
N (individuals)	4 538	4 369	4 325	2 719	8 016			4 538	4 538	4 538	4 480

Notes: (1) Baseline (Table 3 and Table 4). (2) Excluding the reform year. (3) Without Berlin and Brandenburg. (4) Without FiD samples. (5) All children with an observed transition, irrespective of observed school entry. (6) School-entry outcomes for children with both school entry and the transition observed. (7) Annual cross-sectional shares; grade repetition in grades 1–4. (8) Individual-level controls. (9) State-level controls (unemployment rate, education spending share). (10) FLEX reform controls. (11) SOEP survey weights. Administrative data include state and cohort fixed effects (Columns (4)–(6), (8) and (11) not applicable); SOEP models include state × birth-month and cohort × birth-month fixed effects (Column (7) not applicable). Except in Column (5), school-progression outcomes refer to children with both school entry and the transition observed. Ages in months. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

Table B2: School entry cut-off reform: wild cluster bootstrap inference

Panel A. Administrative state-level data	(1) DiD			(2) Intensity DiD		
	Coef.	p	p_{WB}	Coef.	p	p_{WB}
School entry						
Early enrolment	-0.028	0.002	0.001	-0.011	0.000	0.000
Late enrolment	0.031	0.059	0.050	0.011	0.041	0.006
School progression						
Grade repetition	0.008	0.009	0.008	0.002	0.003	0.021
State FE / Cohort FE		Yes			Yes	
N (state × year)		272			272	
Panel B. SOEP microdata	(1) DiD			(2) Intensity DiD		
	Coef.	p	p_{WB}	Coef.	p	p_{WB}
School entry						
Early enrolment	-0.062	0.003	0.003	-0.027	0.000	0.000
Late enrolment	0.039	0.015	0.031	0.016	0.018	0.038
School starting age	-0.805	0.023	0.052	-0.403	0.006	0.008
School progression						
Sec. school starting age	-1.314	0.049	0.043	-0.423	0.154	0.245
Time in primary school	-0.358	0.437	0.484	-0.036	0.804	0.813
Academic track	-0.023	0.461	0.459	-0.012	0.398	0.494
State/cohort × birth-month FE		Yes			Yes	
N (individuals) school entry		7 186			7 186	
N (individuals) school progression		4 538			4 538	

Notes: Point estimates (Coef.), conventional p-values (p) and wild cluster bootstrap p-values (p_{WB}; 999 replications, Rademacher weights, clustered by state) for the DiD estimates in Table 3 and Table 4. Administrative data include state and cohort fixed effects. SOEP models include state × birth-month and cohort × birth-month fixed effects; school-progression outcomes are estimated for children for whom both school entry and the transition to secondary school are observed. Ages in months.
Source: Own calculations based on data from the German Federal Statistical Office (2026a) and the German Socio-Economic Panel (2025).

Table B3: School entry cut-off reform: heterogeneity of the effects for all children (SOEP)

	Gender		Migration		Parental education			Household income		
	(1) Boys	(2) Girls	(3) Yes	(4) No	(5) Low	(6) Medium	(7) High	(8) Low	(9) Middle	(10) High
Panel A. School entry										
Early enrolment	-0.047*	-0.067***	-0.035	-0.061***	-0.033	-0.091***	-0.049*	-0.065**	-0.044	-0.048
	(0.023)	(0.019)	(0.038)	(0.016)	(0.038)	(0.023)	(0.027)	(0.024)	(0.027)	(0.037)
p-value	0.440		0.401			0.327			0.809	
Late enrolment	0.049**	0.036**	0.022	0.040**	0.065	0.030	0.050***	-0.002	0.062**	0.041**
	(0.023)	(0.013)	(0.024)	(0.016)	(0.037)	(0.018)	(0.013)	(0.028)	(0.023)	(0.017)
p-value	0.430		0.558			0.623			0.042	
School starting age	-0.888*	-0.872***	-1.400	-0.806**	-0.686	-0.808**	-0.634	-1.501***	-0.981**	-0.525
	(0.468)	(0.239)	(0.836)	(0.325)	(0.927)	(0.300)	(0.382)	(0.451)	(0.453)	(0.581)
p-value	0.960		0.456			0.918			0.374	
N (individuals)	3 703	3 483	2 000	5 186	1 397	2 374	3 390	1 824	2 700	2 552
Panel B. School progression										
Sec. school starting age	-1.026	-1.548**	-0.560	-1.670**	0.912	-0.670	-1.280	-1.198	-1.881*	-0.805
	(0.742)	(0.645)	(1.468)	(0.742)	(1.479)	(0.934)	(0.775)	(1.485)	(0.895)	(0.590)
p-value	0.525		0.488			0.344			0.695	
Time in primary school	0.060	-0.726	0.946	-0.686	1.084	0.760	-0.719	0.010	-0.219	-0.627
	(0.730)	(0.479)	(0.719)	(0.576)	(1.682)	(0.643)	(0.422)	(1.459)	(0.565)	(0.665)
p-value	0.403		0.149			0.063			0.866	
Academic track	-0.014	-0.007	-0.035	-0.006	-0.055	-0.084	-0.006	0.035	-0.037	0.004
	(0.025)	(0.066)	(0.074)	(0.032)	(0.061)	(0.072)	(0.031)	(0.045)	(0.058)	(0.050)
p-value	0.921		0.707			0.540			0.649	
N (individuals)	2 329	2 209	1 153	3 385	781	1 502	2 246	1 100	1 718	1 667

Notes: Coefficients indicate the estimated effects of the reform on the respective outcomes for all children, separately by subgroup (as in Table 3 and Table 4), following the same structure as Table 7. Parental education: highest CASMIN level of the parents; household income: tertiles of equivalised household income. *p*-values test the equality of the effects within each dimension in a pooled regression with group-specific effects and fixed effects (joint test across the three groups for parental education and household income). All models include state × birth-month and cohort × birth-month fixed effects; school-progression outcomes are estimated for children for whom both school entry and the transition to secondary school are observed. Ages in months. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

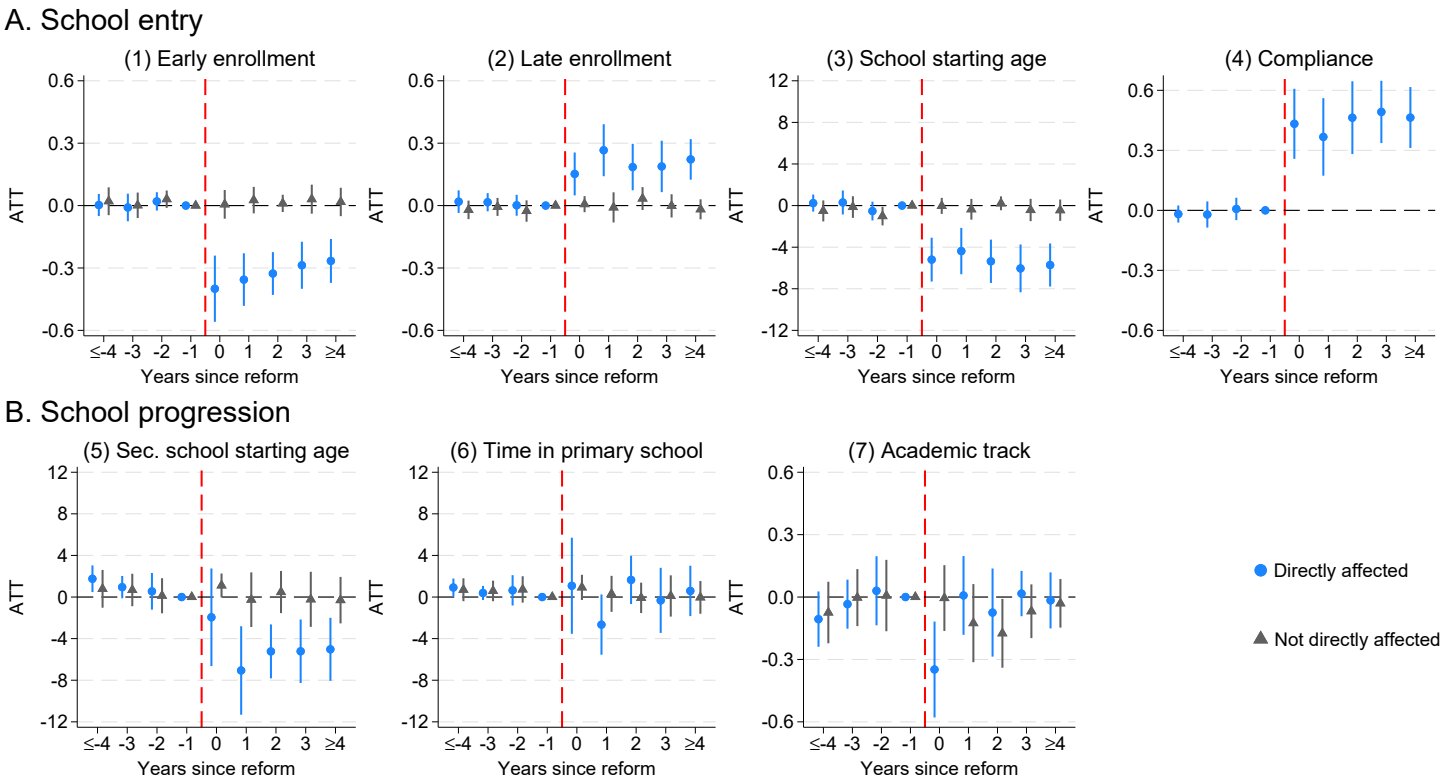
Table B4: School entry cut-off reform: effects by gender (administrative data)

	(1) Boys		(2) Girls
School entry			
Early enrolment	-0.021***		-0.035***
	(0.006)		(0.009)
p-value		0.001	
Late enrolment	0.035*		0.026**
	(0.018)		(0.012)
p-value		0.176	
School progression			
Grade repetition	0.008***		0.007**
	(0.003)		(0.002)
p-value		0.096	
State FE	Yes		Yes
Cohort FE	Yes		Yes
N (state × year)	272		272
N (individuals)	6 563 139		6 203 138

Notes: Coefficients indicate the estimated effects of the reform on the respective outcomes, separately for boys and girls, following the specifications in Table 3 and Table 4. p-values test the equality of the effects in a pooled regression with gender-specific effects and fixed effects. State and cohort fixed effects. Grade repetition refers to grades 3–4. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Federal Statistical Office (2026a).

Appendix C. Additional tables and figures: effects for directly affected children

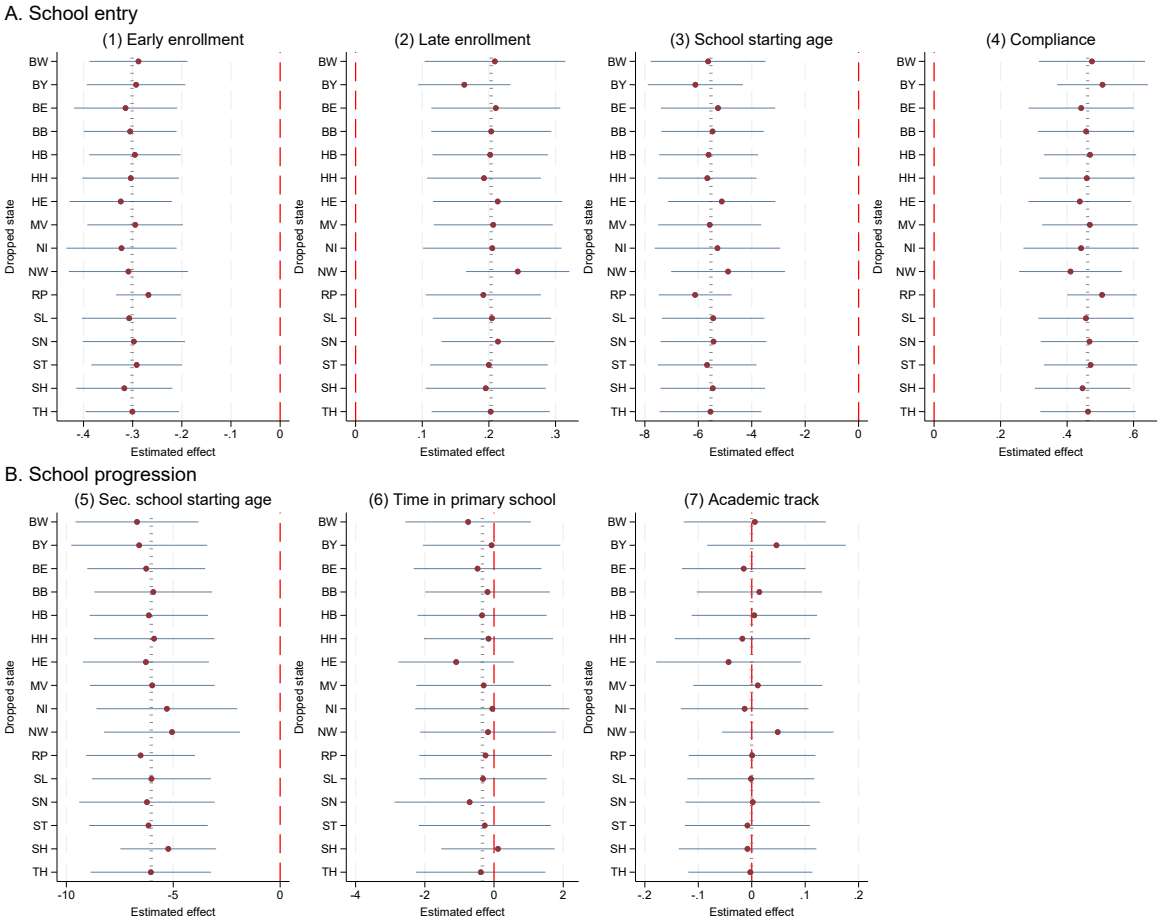
Figure C1: Event-study estimates for directly and not directly affected children (SOEP)



Notes: Event-study estimates for directly affected children (who became eligible for school entry one year earlier under the new cut-off; circles) and not directly affected children (triangles) in reform state-cohorts, each compared with all children in non-reform state-cohorts (as in Table 5, Columns (1) and (3)). Coefficients are relative to the year preceding the reform; event time is binned at ≤ -4 and ≥ 4. State × birth-month and cohort × birth-month fixed effects; school-progression outcomes refer to children with both school entry and the transition observed. Compliance (school entry earlier than allowed under the old rule) is shown for directly affected children only. Ages in months. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the state level; the red dashed line marks the reform.

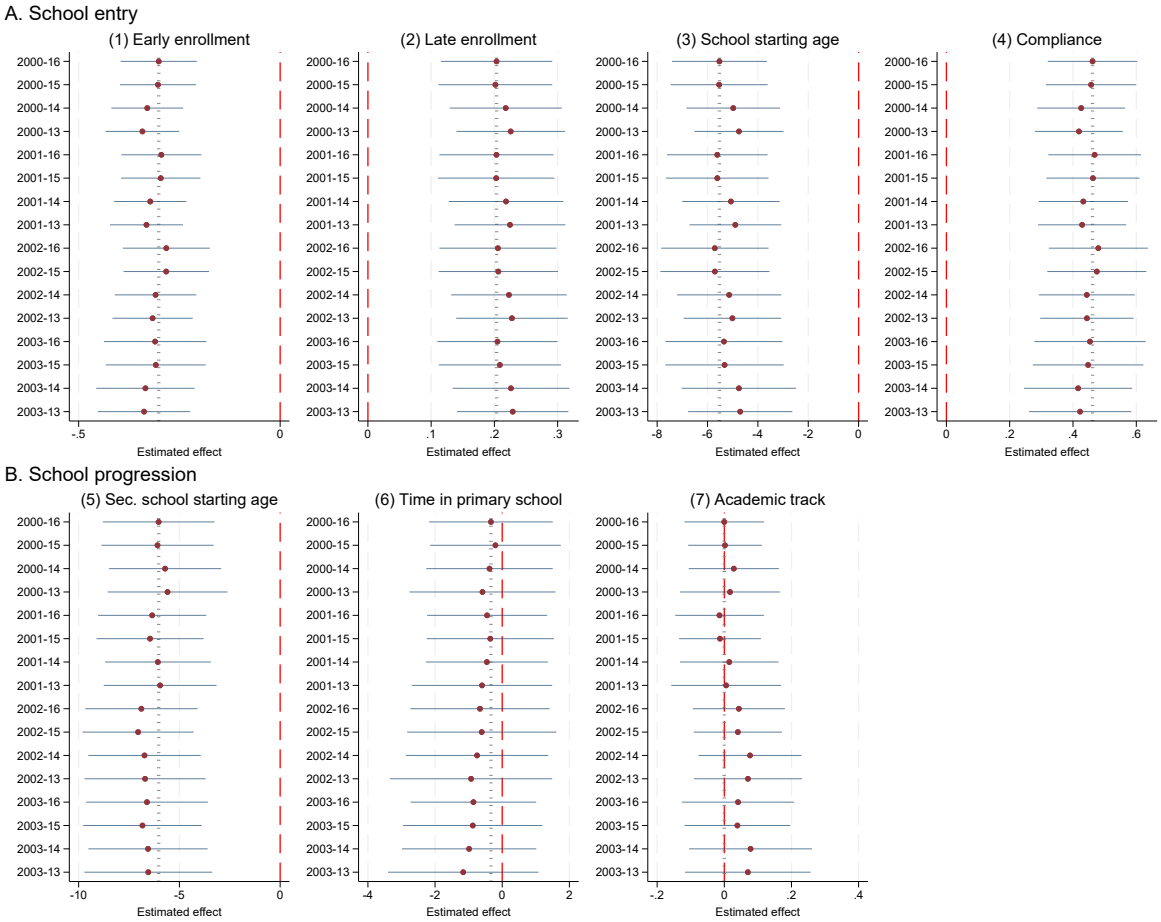
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

Figure C2: Directly affected children: leave-one-state-out robustness checks



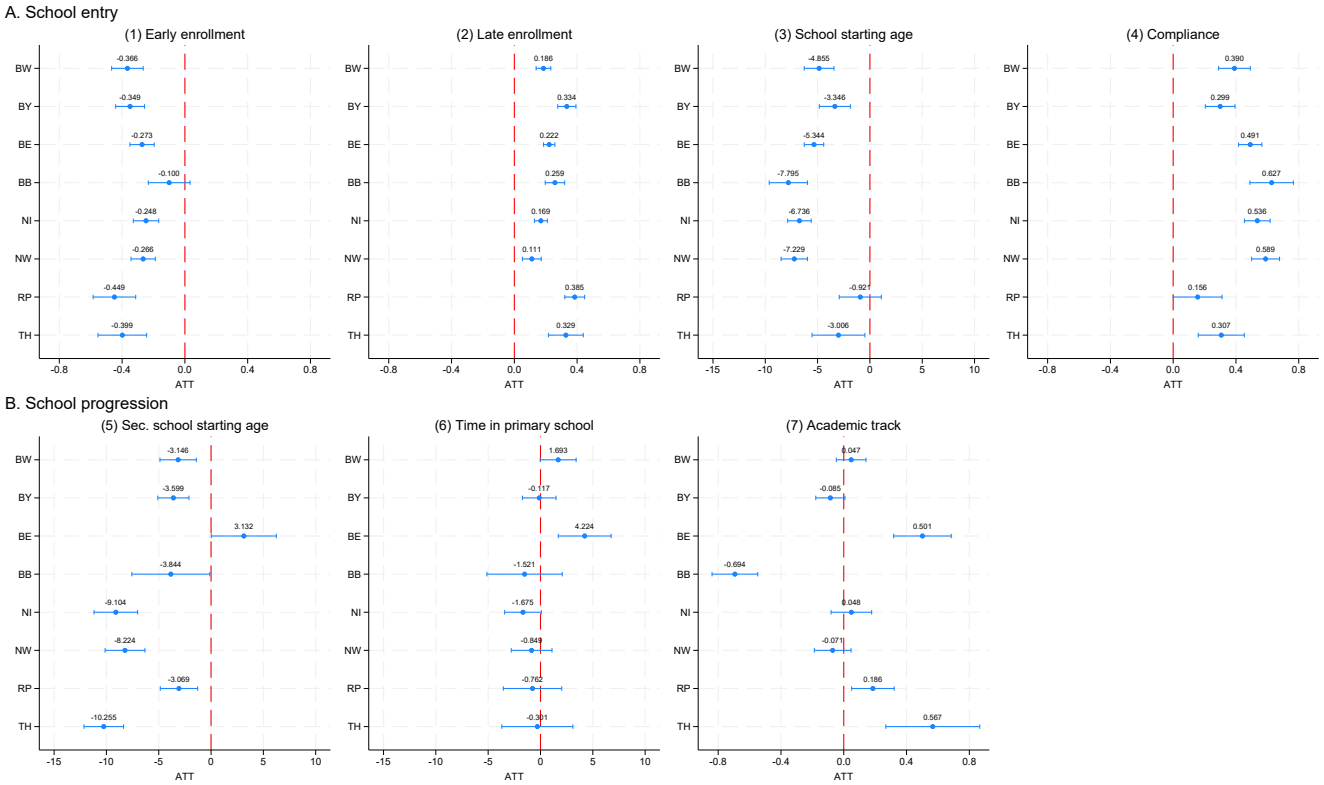
Notes: DiD estimates for directly affected children in reform state-cohorts compared with all children in non-reform state-cohorts (as in Table 5, Column (1)), each excluding one federal state (as in Figure B2). State × birth-month and cohort × birth-month fixed effects; school-progression outcomes refer to children with both school entry and the transition observed. Compliance: school entry earlier than allowed under the old rule. Ages in months. Horizontal lines denote 95% confidence intervals, with standard errors clustered at the state level. The dotted line marks the full-sample estimate, the red dashed line marks zero. State abbreviations as in Figure B4.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

Figure C3: Directly affected children: time-window robustness checks



Notes: Estimates show the effects of the reform on directly affected children in reform state-cohorts, using all children in non-reform state-cohorts as the comparison group (as in Table 5, Column (1)). Estimates are based on alternative observation periods, starting between 2000 and 2003 and ending between 2013 and 2016 (school eligibility cohorts; as in Figure B3). State × birth-month and cohort × birth-month fixed effects; school-progression outcomes refer to children with both school entry and the transition observed. Compliance: school entry earlier than allowed under the old rule. Ages in months. Horizontal lines denote 95% confidence intervals, with standard errors clustered at the state level. The dotted line marks the full-sample estimate, the red dashed line marks zero.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

Figure C4: Directly affected children: state-specific effects



Notes: Estimates show the state-specific effects of the reform on directly affected children in reform state-cohorts, using all children in non-reform state-cohorts as the comparison group (as in Table 5, Column (1)). State-specific effects are obtained by interacting the reform indicator with indicators for the eight reform states. State × birth-month and cohort × birth-month fixed effects; school-progression outcomes refer to children with both school entry and the transition observed. Compliance: school entry earlier than allowed under the old rule. Ages in months. Horizontal lines denote 95% confidence intervals, with standard errors clustered at the state level; as each state-specific effect is identified from a single reform state, these intervals should be interpreted with caution. School-progression estimates rest on small state-specific samples; in Berlin and Brandenburg, primary school comprises six grades, but some children move to the academic track after grade 4. The red dashed line marks zero. State abbreviations as in Figure B4.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

Table C1: Directly affected children: alternative DiD estimators

	(1) TWFE	(2) CS	(3) BJS	(4) SA
School entry				
Early enrolment	-0.301***	-0.156**	-0.300***	-0.307***
	(0.048)	(0.069)	(0.049)	(0.045)
Late enrolment	0.203***	0.199***	0.213***	0.233***
	(0.045)	(0.041)	(0.031)	(0.027)
School starting age	-5.527***	-5.265***	-5.373***	-5.210***
	(0.962)	(1.104)	(0.718)	(0.561)
Compliance	0.462***	0.591***	0.451***	0.439***
	(0.072)	(0.070)	(0.050)	(0.040)
School progression				
Sec. school starting age	-6.029***	0.680	-5.539***	-5.452***
	(1.415)	(1.275)	(0.973)	(1.237)
Time in primary school	-0.335	7.334***	0.338	0.024
	(0.940)	(1.278)	(0.978)	(1.182)
Academic track	-0.000	0.013	0.082*	-0.032
	(0.061)	(0.088)	(0.048)	(0.051)
State × birth-month FE	Yes	Yes	Yes	Yes
Cohort × birth-month FE	Yes	Yes	Yes	Yes
N (individuals) school entry	4 049	4 049	4 049	4 049
N (individuals) school progression	2 667	2 667	2 667	2 667

Notes: Coefficients indicate the estimated effects of the reform using alternative estimators: TWFE (baseline), Callaway and Sant'Anna (2021) (CS), Borusyak et al. (2024) (BJS), and Sun and Abraham (2021) (SA). The analysis compares directly affected children (who became eligible for school entry one year earlier under the new cut-off) in reform state-cohorts with all children in non-reform state-cohorts (as in Table 5, Column (1)). TWFE, BJS and SA include state × birth-month and cohort × birth-month fixed effects, while CS, which cannot accommodate these fixed effects, conditions on birth-month indicators instead. Compliance: school entry earlier than allowed under the old rule. School-progression outcomes refer to children with both school entry and the transition observed. Ages in months. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

Table C2: Directly affected children: robustness checks

		Sample restrictions					Additional controls			Method
	(1) Baseline	(2) Excl. reform year	(3) Without BE, BB	(4) Without FiD	(5) Transition observed	(6) Common sample	(7) Individual controls	(8) State controls	(9) FLEX reform	(10) Weighted
School entry										
Early enrolment	-0.301***	-0.291***	-0.319***	-0.326***	–	-0.337***	-0.300***	-0.299***	-0.302***	-0.332***
	(0.048)	(0.045)	(0.054)	(0.057)		(0.063)	(0.048)	(0.048)	(0.051)	(0.062)
Late enrolment	0.203***	0.211***	0.211***	0.175***	–	0.178***	0.202***	0.216***	0.210***	0.175***
	(0.045)	(0.046)	(0.051)	(0.055)		(0.057)	(0.045)	(0.041)	(0.041)	(0.034)
School starting age	-5.527***	-5.544***	-5.187***	-5.705***	–	-5.693***	-5.572***	-5.391***	-5.426***	-5.713***
	(0.962)	(0.948)	(1.114)	(1.099)		(1.173)	(0.940)	(0.933)	(0.935)	(1.034)
Compliance	0.462***	0.465***	0.437***	0.481***	–	0.458***	0.465***	0.456***	0.458***	0.480***
	(0.072)	(0.070)	(0.082)	(0.086)		(0.088)	(0.072)	(0.069)	(0.071)	(0.080)
School progression										
Sec. school starting age	-6.029***	-6.187***	-6.161***	-5.859***	-6.040***	–	-6.133***	-5.613***	-5.844***	-5.516***
	(1.415)	(1.499)	(1.399)	(1.424)	(1.034)		(1.413)	(1.391)	(1.286)	(1.128)
Time in primary school	-0.335	-0.254	-0.320	-0.532	–	–	-0.385	-0.168	-0.377	-0.024
	(0.940)	(0.984)	(0.919)	(0.896)			(0.936)	(0.960)	(0.910)	(0.756)
Academic track	-0.000	0.034	-0.001	-0.067	0.045	–	0.017	-0.021	-0.001	-0.030
	(0.061)	(0.051)	(0.058)	(0.066)	(0.077)		(0.049)	(0.074)	(0.063)	(0.063)
N (individuals) school entry	4 049	4 027	3 829	2 772		2 667	4 049	4 049	4 049	3 977
N (individuals) school progression	2 667	2 653	2 563	1 902	4 458		2 667	2 667	2 667	2 623

Notes: Robustness check for directly affected children (Table 5, Column (1)). (1) Baseline. (2) Excluding the reform year. (3) Without Berlin and Brandenburg. (4) Without FiD samples. (5) All children with an observed transition, irrespective of observed school entry. (6) School-entry outcomes for children with both school entry and the transition observed. (7) Individual-level controls. (8) State-level controls (unemployment rate, education spending share). (9) FLEX reform controls. (10) SOEP survey weights. Compliance: school entry earlier than allowed under the old rule. All models include state × birth-month and cohort × birth-month fixed effects. Except in Column (5), school-progression outcomes refer to children with both school entry and the transition observed. Ages in months. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

Table C3: Not directly affected children: robustness checks

		Sample restrictions					Additional controls			Method
	(1) Baseline	(2) Excl. reform year	(3) Without BE, BB	(4) Without FiD	(5) Transition observed	(6) Common sample	(7) Individual controls	(8) State controls	(9) FLEX reform	(10) Weighted
School entry										
Early enrolment	0.002	0.010	0.002	0.007	–	0.026	0.002	0.000	0.005	0.007
	(0.020)	(0.023)	(0.019)	(0.017)		(0.026)	(0.020)	(0.020)	(0.018)	(0.027)
Late enrolment	0.011	0.006	0.012	0.007	–	0.022*	0.013	0.017	0.013	-0.006
	(0.011)	(0.013)	(0.011)	(0.012)		(0.012)	(0.012)	(0.011)	(0.011)	(0.011)
School starting age	0.186	0.032	0.195	0.057	–	-0.018	0.210	0.287	0.174	-0.129
	(0.257)	(0.299)	(0.254)	(0.248)		(0.372)	(0.266)	(0.236)	(0.261)	(0.296)
School progression										
Sec. school starting age	-0.230	-0.787	-0.196	-0.330	-1.357***	–	-0.190	0.011	-0.237	-0.591
	(0.674)	(0.964)	(0.669)	(0.589)	(0.338)		(0.688)	(0.523)	(0.628)	(0.740)
Time in primary school	-0.212	-0.570	-0.190	-0.219	–	–	-0.180	-0.048	-0.228	-0.229
	(0.455)	(0.601)	(0.466)	(0.388)			(0.464)	(0.331)	(0.498)	(0.465)
Academic track	-0.031	-0.047	-0.031	-0.006	0.015	–	-0.037	-0.046	-0.034	0.048
	(0.032)	(0.041)	(0.031)	(0.032)	(0.015)		(0.040)	(0.036)	(0.034)	(0.032)
N (individuals) school entry	6 213	5 967	5 915	3 735		3 963	6 213	6 213	6 213	6 121
N (individuals) school progression	3 963	3 808	3 820	2 457	6 976		3 963	3 963	3 963	3 907

Notes: DiD estimates for not directly affected children in reform state-cohorts compared with all children in non-reform state-cohorts (Table 5, Column (3)). Columns as in Table C2. Compliance is not shown, as it coincides with early enrollment for these children.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

Table C4: Directly and not directly affected children: wild cluster bootstrap inference

	(1) Directly affected			(2) Not directly affected		
	Coef.	p	p_{WB}	Coef.	p	p_{WB}
School entry						
Early enrolment	-0.301	0.000	0.000	0.002	0.924	0.922
Late enrolment	0.203	0.000	0.000	0.011	0.327	0.453
School starting age	-5.527	0.000	0.007	0.186	0.481	0.579
Compliance	0.462	0.000	0.005	–	–	–
School progression						
Sec. school starting age	-6.029	0.001	0.013	-0.230	0.737	0.766
Time in primary school	-0.335	0.725	0.736	-0.212	0.647	0.659
Academic track	-0.000	0.998	0.999	-0.031	0.343	0.390
State × birth-month FE		Yes			Yes	
Cohort × birth-month FE		Yes			Yes	
N (individuals) school entry		4 049			6 213	
N (individuals) school progression		2 667			3 963	

Notes: Point estimates (Coef.), conventional p-values (p) and wild cluster bootstrap p-values (p_{WB}; 999 replications, Rademacher weights, clustered by state) for the DiD estimates in Table 5, Columns (1) and (3). Compliance is shown only for directly affected children, as it coincides with early enrollment for the others. All models include state × birth-month and cohort × birth-month fixed effects; school-progression outcomes refer to children with both school entry and the transition observed. Ages in months.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).

Table C5: Directly and not directly affected children: states with a June or September cut-off

	(1) Jul–Sep births (directly affected)	(2) Oct–Jun births (not directly affected)	(3) Difference (1) – (2)
School entry			
Early enrolment	-0.257***	-0.004	-0.253***
	(0.050)	(0.023)	(0.058)
Late enrolment	0.149***	0.016	0.133***
	(0.039)	(0.011)	(0.042)
School starting age	-6.040***	0.286	-6.326***
	(0.872)	(0.273)	(0.952)
Compliance	0.497***	-0.004	0.501***
	(0.063)	(0.023)	(0.067)
School progression			
Sec. school starting age	-6.865***	-0.253	-6.612***
	(1.407)	(0.865)	(1.378)
Time in primary school	-0.201	-0.613	0.412
	(1.013)	(0.680)	(0.988)
Academic track	0.027	-0.052	0.078
	(0.055)	(0.041)	(0.065)
State × birth-month FE	Yes	Yes	Yes
Cohort × birth-month FE	Yes	Yes	Yes
N (individuals) school entry	1 572	4 325	5 897
N (individuals) school progression	1 024	2 768	3 792

Notes: The sample is restricted to cells with a 30 June (non-reform) or 30 September (reform) cut-off, in which exactly the children born in July–September are directly affected. Column (1) compares July–September births in reform and non-reform cells; Column (2) compares October–June births (not directly affected); Column (3) reports the difference between Columns (1) and (2), estimated in a pooled regression with group-specific reform effects. SOEP models include state × birth-month and cohort × birth-month fixed effects; school-progression outcomes refer to children with both school entry and the transition observed. Ages in months. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on data from the German Socio-Economic Panel (2025).