I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 17790

Why Does Starting School Older Harm Schooling? The Role of Youth Employment Laws

Itay Attar
Danny Cohen-Zada

MARCH 2025

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 17790

Why Does Starting School Older Harm Schooling? The Role of Youth Employment Laws

Itay Attar
Ben-Gurion University of the Negev

Danny Cohen-Zada
Ben-Gurion University of the Negev and IZA

MARCH 2025

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Why Does Starting School Older Harm Schooling? The Role of Youth Employment Laws*

Using Israeli data, we establish that the interaction between school entrance age (SEA) policy and youth employment laws increases high school dropout rates among students who start school older—particularly males. This is because these students become eligible for employment at an earlier grade, increasing their likelihood and duration of work, which amplifies dropout rates. Intriguingly, this effect is primarily driven by students who achieved above-average test scores in elementary school. Among males, a higher SEA also reduces participation in and scores on a college entry exam, as well as college enrollment. Unlike most previous estimates, our estimates of the effect of SEA on college entry-exam scores are free from age-at-test effects. In the longer run, a higher SEA reduces educational attainment for both males and females and has a sizable negative, though statistically nonsignificant, effect on their earnings. Our findings suggest that replacing the minimum working age in youth employment laws with a minimum-grade-completion requirement could mitigate the unintended consequence of higher dropout rates among older school entrants.

JEL Classification: I20, I28, J22, J24

Keywords: school entrance age, youth employment, high school dropout, compulsory schooling, returns to education, date of birth, test scores

Corresponding author:
Danny Cohen-Zada
Department of Economics
Ben-Gurion University of the Negev
Beer Sheva 84105
Israel
E-mail: danoran@bgu.ac.il

* We are grateful to Todd Elder, Naomi Gershoni, Moshe Justman, Assaf Kott, Victor Lavy, Yuval Ofek-Shanny, Shirlee Lichtman-Sadot, Mathew Notowidigdo, Analia Schlosser, Ity Shurtz, and Ro'i Zultan for their helpful comments. This study was conducted in the Israel Central Bureau of Statistics research room using de-identified microdata (with direct identifiers removed from data) in files explicitly prepared for this project. We extend special thanks to the Israeli Central Bureau of Statistics staff, especially Yifat Klopstock, for their outstanding assistance in preparing the data set. The paper synthesizes findings from two projects. The first, focusing on the short- and long-term effects of school-entry age, received support from the Spencer Foundation (Grant No. 20180085) in 2018–19. This grant facilitated the acquisition of the research data set and contributed to the key short- and long-term findings presented in this paper. The Maurice Falk Institute for Economic Research in Israel provided additional support for other expenses for this project. The second project, investigating the impact of school entrance age on employment during high school and its influence on dropout rates, as well as the effect of school entrance age on earnings, was funded by the Israel Science Foundation (Grant No. 677/23) in 2023–24.

I INTRODUCTION

The age at which children start school plays a pivotal role in their human capital accumulation, making the issue important to parents, policy makers, and researchers alike. Seminal studies by Angrist and Krueger (1991, 1992) showed that the interaction between school entrance age (SEA) policy and compulsory schooling laws in the United States drives higher dropout rates among students who start school older. This occurs because the compulsory schooling laws mandate a minimum school dropout age, so older school entrants face fewer years of mandatory schooling because they reach the minimum dropout age at an earlier grade. However, this mechanism does not apply in countries where the compulsory schooling law specifies a minimum number of completed grades instead of a minimum dropout age. Thus, while subsequent studies in the United States have consistently found that older school entrants are more prone to dropping out of high school (Dobkin and Ferreira 2010, Cook and Kang 2016), research in other countries has found no effect or even a positive effect of entrance age on educational attainment (Black et al. 2011, Fredriksson and Öckert 2014). Lacking any alternative mechanism to explain why older school entrants are more likely to drop out, these studies generally attribute their findings to the presence or absence of compulsory schooling law mechanism in their respective countries.

However, Angrist and Krueger (1992) demonstrate that compulsory schooling laws prevent only 10% of students in the United States from dropping out of high school. Furthermore, studies have found that child labor laws have a more substantial impact on educational attainment than compulsory schooling laws do (Acemoglu and Angrist 2000, Margo and Finegan 1996). Acemoglu and Angrist (2000) argue that one of the main reasons students leave high school is to enter the workplace, underscoring the need to examine how child labor laws influence dropout rates. Despite this argument, the SEA literature has completely overlooked the impact of SEA on youth employment and how SEA policy may interact with minimum working age to lead older school entrants to drop out of high school at higher rates. This alternative mechanism may explain why students who start school older exhibit higher dropout rates despite presumably being more mature, and why SEA effects vary across countries. The primary purpose of this paper is to fill this gap.

Youth employment laws, common in most developed countries, establish a minimum legal working age. As a result, students who start school a year older reach this age at an earlier grade, making the outside option of working more accessible and thus increasing the risk of dropping out of high school. Indeed, the literature provides direct evidence that stricter child labor laws—requiring more years of schooling before allowing employment or setting a higher minimum working age—lead to increased educational attainment (Acemoglu and Angrist 2000, Lleras-Muney 2002). Further research reinforces this link, showing that greater access to the labor market or better labor market conditions reduce schooling by increasing the opportunity cost of education and encouraging students to work instead (Saha and Steinberg 2017, Charles et al. 2018). Most studies find that this effect is particularly strong among male students (Atkin 2016, Shah and Steinberg 2021, Cascio and Ayushi 2022, Montmarquette et al. 2007, Holford 2020). However, these studies do not examine the full mechanism we propose, as they do not directly address the role of school starting age in shaping access to the labor market.

We explore this mechanism in four steps. First, we establish that a higher SEA significantly increases youth employment—an effect not previously examined in the literature. Second, we provide evidence that this effect is driven to a large extent by the minimum legal working age. Third, we show that a higher SEA also increases the probability of high school dropout, particularly among males. Finally, we use several strategies to demonstrate that the SEA effect on the probability of high school dropout is driven by youth employment. These findings suggest that the well-established positive effect of SEA on high school dropout rates in the United States, previously attributed solely to compulsory schooling laws, may instead be largely explained by the youth employment mechanism we propose.

Our identification strategy leverages a unique setting created by the initial Israeli school-entrance rule, which set a fixed school-entry cutoff date each year based on the Jewish calendar. Since the Jewish lunar year is shorter than the Gregorian year, this cutoff falls on a different Gregorian date in December each year. Consequently, children born on the same Gregorian calendar date in different years may fall on opposite sides of the cutoff. This variation creates a natural experiment in which students born on the same date of the year, who also share the same cultural and institutional environment, start school at different ages solely because of this shifting cutoff date, independent of their socioeconomic background or academic capabilities. To formalize this intuition, we employ a hybrid identification strategy that combines difference-in-differences estimation with a fuzzy regression discontinuity (RD) approach, using an indicator for whether a child was born before or after the relevant cutoff point as an instrumental variable for SEA. This strategy focuses on a narrow bandwidth around the cutoff dates and identifies the impact of SEA by comparing differences in outcome variables across years between children born on different dates.[2]

We use comprehensive administrative records from the Israel Ministry of Education and other Israeli governmental agencies. For two reasons, Israeli data are particularly apt for exploring the effect of SEA on youth employment and that effect's role as a mediator driving the positive effect of SEA on high school dropout rates. First, youth employment in Israel is quite prevalent, with nearly half of Israeli high school students working during the school year. Second, the compulsory schooling law, which mandates a minimum number of completed grades rather than a minimum dropout age, is unlikely to drive a positive SEA effect on dropout rates. This context enables us to assess the significance of the youth-employment mechanism.

Our findings indicate that delaying school entry by one year increases the likelihood of working during high school by 16 percentage points, with this effect being 45% larger for males than for females. It also lengthens the duration of employment by an average of 2.58 months. We provide evidence that this effect is largely driven by the minimum legal working age: Employment rates remain flat before age 16 but rise sharply thereafter. This pattern reveals the underlying mechanism: Starting school a year older allows students to reach the legal working age at an earlier grade, thereby increasing both the likelihood and duration of employment during high school. Furthermore, we find that the SEA effect on high school employment is substantially stronger in the second half of 10th grade than in the first, as only then do older students surpass the minimum working age and gain permission to work, unlike their younger peers.

We find that starting school a year older increases the probability of high school dropout by 4.0 percentage points—a sizable effect that exceeds those observed in various studies in the United States.[3] Notably, the impact is almost three times larger for males (6.6%) compared to females (2.3%). In contrast, a higher SEA does not affect the probability of earning a matriculation diploma. When tracking the same students from elementary school through high school completion, we find that about 90% of the SEA effect on not completing high school is

[1] In a previous study (Attar and Cohen-Zada, 2018), we used this identification strategy to estimate SEA effects on fifth- and eighth-grade test scores. In this study, we significantly broaden our investigation by examining SEA impacts over short-, medium-, and long-term horizons, covering several previously unexplored outcomes.

[3] For comparison, Cook and Kang (2016) find that starting school a year older reduces 11th-grade enrollment by 2.3 percentage points and 12th-grade enrollment by 3.14 percentage points. Dobkin and Ferreira (2010) report even smaller effects, estimating a negative SEA impact on high school graduation of 0.8 and 0.9 percentage points in Texas and California, respectively.

driven by students who performed well in elementary school and achieved above-average fifth-grade test scores.[4]

We use various strategies to provide compelling evidence that the positive SEA impact on high school dropout is mediated by youth employment. First, we decompose the SEA effect on dropout by employment status in the previous grade, demonstrating that the effect is overwhelmingly driven by those who were employed.[5] Second, we estimate SEA effects on both the likelihood of employment during 11th grade and the likelihood of dropping out during 11th or 12th grade across various subgroups, finding a correlation as high as 0.82 between the two. This strong correlation supports our hypothesis that the SEA effect on dropout is driven by prior employment. Third, we estimate heterogeneous effects on employment, dropout, and their interplay, observing consistent patterns. For instance, these SEA effects are weaker among children with more educated parents and stronger among those with more siblings. These findings align well with those of Montmarquette et al. (2007), who show that gender and parental education are key factors in school-to-work decisions.

We also assess the impact of SEA on a range of long-term outcomes.[6] Consistent with our finding that a higher SEA significantly increases the likelihood of high school dropout, particularly among males, we observe corresponding negative SEA effects on subsequent educational outcomes exclusively among them. Specifically, a higher SEA decreases the probability of taking a psychometric college entry exam, achieving high scores on the exam, and enrolling in college. Additionally, a higher SEA reduces the total years of schooling for both males and females. These results suggest that delaying school entry does not enhance human capital accumulation and may even impede it. Notably, the pattern of SEA effects on

[4] However, their higher scores may have reflected their older age at the time of the exam.

[5] Working in grade g may lead to dropping out either during that grade or after completing it without continuing to the next. We decompose the SEA effect on "dropping out in grade g or g+1" into two components: (1) the effect of SEA on "working in grade g and dropping out at grade g or g+1," and (2) the effect of SEA on "not working in grade g and dropping out at grade g or g+1." We find that the latter effect is negligible, while the former is significant and accounts for the vast majority of the overall SEA effect on dropout at these grades. For example, more than 97% of the SEA effect on dropping out during 11th–12th grades is driven by those who worked in 11th grade.

[6] This analysis is valuable because previous studies report mixed results on the effect of SEA on long-term outcomes. For example, Angrist and Krueger (1991) find that SEA reduces earnings, and Black et al. (2011) observe a negative effect until age 30. In contrast, Dobkin and Ferreyra (2010) and Fredriksson and Öckert (2014) find no impact. Results are similarly mixed for higher education outcomes. For instance, Celhay and Gallegos (2022) find that starting school a year older increases the likelihood of taking a college entrance exam and improves test scores, whereas Hurwitz et al. (2015) find no effect on SAT participation. Estimated effects on college enrollment and completion also vary: Dobkin and Ferreira (2010) report a slight negative effect on college enrollment and no effect on completion in California, with no significant results in Texas. By contrast, studies by Hurwitz et al. (2015) in Michigan and Maine, Dhuey et al. (2019) in Florida, and Celhay and Gallegos (2022) in Chile find positive effects on both college enrollment and completion.

high school completion and later educational outcomes is consistent: These negative impacts are largely confined to males and are more pronounced among those with less educated parents and those with more siblings.

Our SEA estimates on college entry-exam scores make a significant contribution to the literature. Unlike in most studies, they measure the impact at a stage of life when cognitive abilities are closely tied to labor market outcomes and are free from age-at-test confounding. Black et al. (2011) is a notable exception, as it disentangles these effects by analyzing IQ scores from military exams taken outside of school around age 18. They find that controlling for age-at-test turns the SEA effect from positive to negative, indicating that younger students would slightly outperform their older peers if tested at the same age.[7] This "reversed effect"—observed only among male students in Norway, where compulsory military service applies exclusively to males—underscores the importance of examining whether this result holds consistently when isolating age-at-test effects in different contexts and among both males and females. In Israel, students who complete high school at an older age face a shorter wait before military service, enabling them to take the psychometric entry exam at approximately the same age as younger graduates. Indeed, our analysis shows that SEA has a negligible and statistically nonsignificant effect on the age at which students take this exam. Consistent with Black et al. (2011), we find that SEA reduces test scores among males. We find no such effect among females.

Our analysis reveals that, in the longer run, SEA reduces years of schooling for both males and females and has a sizable negative, albeit nonsignificant, effect on earnings between ages 28 and 37. At the same time, SEA has no significant effect on the likelihood of employment during these years. Additionally, SEA does not affect the age at which individuals begin or complete a first degree, indicating that for those who pursue higher education, starting school later does not necessarily delay entry into the labor market.

The implications of our findings are significant. First, the interaction between youth employment laws and compulsory schooling laws requires careful consideration when crafting education and labor policies. For example, replacing the minimum age for employment with a minimum grade-completion requirement could mitigate the increased dropout rates among older school entrants. Second, the impact of youth employment on high school dropout rates

[7] Most previous studies focus on test scores in elementary school and find positive SEA effects (Bedard and Dhuey 2006, McEwan and Shapiro 2008, Elder and Lubotsky 2009, Dobkin and Ferreira 2010, Celhay and Gallegos 2022). However, since all children in K–12 schooling take exams at the same time, these estimates are confounded by age-at-test effects because of the intrinsic collinearity between SEA and age at testing.

suggests that interventions to reduce students' motivation to work during high school could lower dropout rates, although the desirability of this goal warrants consideration. Third, there are methodological implications for estimating the returns to education (see Section VI).

The rest of the paper proceeds as follows. The next section briefly provides background on the Israeli compulsory schooling law and youth employment law. Section III presents an overview of the data used in the analysis. In Section IV we outline the empirical strategy. Section V presents the results, and Section VI concludes with a brief summary.

II ISRAELI LAWS ON COMPULSORY SCHOOLING AND YOUTH EMPLOYMENT

Compulsory schooling and youth employment laws are two potential mechanisms that could drive a positive SEA effect on high school dropout rates. In this section, we provide a brief overview of these laws in Israel to contextualize our assessment of their relative importance.

II.A The Israeli Compulsory Schooling Law

Israel's compulsory schooling law, enacted in 1949, initially required children under age 16 at the start of the school year to stay in school until they completed 10th grade. With the passing of Amendment 29 in 2007 and the beginning of its implementation in 2009, this requirement was extended to include all children under 18, and since then, it has mandated completion of 12th grade.[8]

Most children in Israel start school between age 5.7 and 6.7, with only 0.5% in our sample starting after age 7. As a result, they reach 10th grade before turning 16 and 12th grade before turning 18—unless they repeat a grade (3.7% in our sample). Consequently, nearly all students in our sample met the age condition specified in the law and were subject to the same compulsory schooling requirement regardless of whether they started school earlier or later: Students who faced the pre-amendment law were required to complete 10 years of schooling, while those who faced the post-amendment law were required to complete 12 years. This

[8] Under the law, parents are legally responsible for enrolling their children in school and ensuring regular attendance, with penalties such as fines or imprisonment for noncompliance. The law also mandates that the state provide free compulsory schooling, ensuring children have the right to free education until they complete 12th grade or turn 18 at the beginning of a school year. The Ministry of Education, local authorities, and schools are responsible for ensuring students finish high school, with their efforts detailed in the Ministry of Education Director-General's Circular 0303. For example, schools are prohibited from expelling students without providing an alternative educational framework. Truancy officers, known as *bikur sadir*, monitor attendance to ensure compliance. The new amendment extends parents' responsibility for their children's school attendance until they complete 12th grade or turn 18, though legal penalties for parents apply only until the child turns 16.

suggests that the compulsory schooling law is unlikely to explain a positive SEA effect on high school dropout rates in either case. Moreover, the law's *change* is also unlikely to explain it. If, in any given period, students born before the cutoff (who started school younger) were subject to the pre-amendment law while those born after it (who started school older) were subject to the post-amendment law, we would expect SEA to have a negative effect on dropout rates — not a positive one— since the post-amendment law requires more years of schooling. In Section V.D, we provide empirical evidence that the positive effect of SEA on high school dropout is not driven by either the law's *change* or its age requirement.

II.B The Israeli Youth Employment Law

The Israeli Youth Employment Law, enacted in 1953, regulates the employment of minors under 18. According to the law, children under 16 cannot be employed unless exempted from compulsory schooling, while those 16 or older are permitted to work after school hours. This minimum legal working age implies that students born just after the school-entry cutoff date and starting school a year older can begin working as early as the second half of 10th grade. Those born just before the cutoff date and starting school a year younger must wait until the second half of 11th grade. Thus, this law is expected to result in a positive association between SEA and the likelihood and duration of employment during high school.

III DATA

Our database includes administrative records from the Israel Ministry of Education on eight cohorts of students who started school between 1997 and 2004 (hereafter "young cohorts") and five cohorts who started 10th grade between 1991 and 1995 (hereafter "old cohorts").[9] For all cohorts, our raw data set includes information on the entire population of students in public schools living in Jewish localities, totaling 725,852 observations. We excluded 2,405 students (0.33%) born on January 1—because the reported date of birth for these births is unreliable—and 2,524 students (0.35%) with an extreme entrance age of two or more years earlier or later than predicted.[10]

As we employ a fuzzy RD design, we focus on a narrow (28-day) bandwidth around each

[9] The Ministry of Education began documenting student records in 1991, initially tracking students only from 10th grade onward.

[10] To illustrate, the number of reported births for immigrant children from Africa on January 1 is 19.12 times higher than the average number of births on other dates. This pattern appears to be driven by immigrant children from Ethiopia, for whom January 1 may have been recorded as the birth date when the actual date was unknown. While January 1 births appeared reliable for non-immigrants and immigrants from other continents, we excluded all January 1 births as a precaution. Notably, in none of the years in our data set was January 1 the school-entry cutoff date.

school-entrance cutoff, defining as a period the 56 days within that bandwidth. To ensure the validity of our design, we use only periods with data on children on both sides of the cutoff.[11] Our final RD sample includes 70,758 observations for the young cohorts and 37,258 for the old ones.

Appendix Table B1 outlines the data set structure. The first three columns specify the period number, number of observations, and cutoff date. Additional details include location relative to the cutoff (before or after), expected school starting date, expected year of the fifth-grade exam, and age range of students at the end of 2017, the latest year for most outcomes. This last variable demonstrates that students in the same period are roughly the same age, regardless of whether they were born in December of one year or January of the subsequent year or whether they are in different grades during the study period. Thus, when estimating SEA's effect on employment and wage earnings in adulthood (ages 28–37), we compare earnings and employment in the same calendar year, considering any age difference a negligible factor in earning potential.

The table also shows that by the end of 2017, the youngest individuals in the young cohorts were around 20 years old, meaning that all could have completed high school by this age, even if they had repeated a grade. Thus, these cohorts allow us to estimate all short- and medium-run SEA effects on the same students and specifically to examine whether those who experienced short-term benefits from a higher SEA—such as improved fifth-grade test scores and reduced likelihood of grade retention—later faced a higher risk of high school dropout. Similarly, by the end of 2017, the youngest individuals in the old cohorts were around 38 years old—an age by which those intending to acquire higher education would likely have completed it. For example, the National Institute for Testing and Evaluation reports that 98.5% of psychometric college entry-exam takers do so before age 30. Thus, measuring this outcome at age 38 serves as a strong proxy for ever taking this entrance exam, thus minimizing selection bias. This logic extends to several other long-run educational outcomes, which we examine using the old cohorts. Specifically, we estimate the effect of SEA on higher education outcomes, as well as employment and earnings at ages 28–32 and 33–37.

Each record in our data set includes information on parental education and whether the student attended a religious public school (Mamlachti Dati in Hebrew) or a secular public school (Mamlachti). The records of the young cohorts also report the year of school entrance,

[11] Thus, we excluded students after the 1990 cutoff (starting school in 1997) and before the 1998 cutoff (starting school in 2004), since we lack data on the other side. Similarly, for the old cohorts, we excluded periods around the 1975 and 1980 cutoffs.

the number of times the student attended each grade, the total years of schooling completed in K–12, and whether a matriculation diploma was earned. The records for the old cohorts begin tracking students only from 10th grade, preventing us from observing when they started school or whether they were held back or skipped a grade. Thus, we extrapolate the entrance age based on the year they started 10th grade, assuming no grade retention or skipping. These records include long-run educational outcomes, such as educational attainment, college enrollment, bachelor's-degree eligibility, and the ages at which individuals start and complete their first degree. All our outcome variables are measured as of 2017.

To facilitate our estimation of the SEA effects on a wide range of short-, medium-, and long-term outcomes, the Israeli Central Bureau of Statistics merged these administrative records with four additional sources:

1) Information from the Population Registry on the student's date of birth and the following background characteristics: gender, number of siblings, continent of birth, parents' continent of birth, and indicators for whether the students and parents were born in Israel
2) Fifth- and eighth-grade math, Hebrew, English, and science test scores from the Growth and Effectiveness Measures for Schools (Meitzav in Hebrew) for 2002–8. The Meitzav exam in fifth grade was administered to a representative one-in-two sample of schools, with each school participating every two years. In general, all students, except those in special education, were tested. We standardized the raw test scores (1–100 scale) by grade, subject, and year, setting the mean to zero and the standard deviation to one.
3) Information from the National Institute for Testing and Evaluation on whether the student applied for a psychometric college entry exam, the student's age at the time of the exam, the total score, and the subject scores in math, Hebrew, and English
4) Information from the Israel Tax Authority on employment and wage earnings from 2000 to 2017

The employment data are particularly useful for our study, as they are available monthly, allowing us to distinguish whether a student worked during the school year (September–June) or summer vacation (July–August). We define a student as employed during a grade if they worked at least three months during the school year. A student is considered employed during high school if they worked at least one school year between 9th and 12th grades. Independently, employment during a specific grade is defined as working throughout the school year the student is expected to attend, based on their entrance year, regardless of grade retention or skipping. This definition aligns with our proposed mechanism, which suggests that older school entrants work more during high school because they reach the legal working age earlier, with incentives to work driven by age rather than grade level. We also use these employment data to estimate SEA effects on employment and wage earnings in adulthood (ages 28–37).

Table 1 presents summary statistics for the treatment and background variables, with columns 1–3 reporting on the young cohorts and columns 4–6 on the old cohorts. Similarly, Table 2 presents summary statistics of the outcome variables. The top panel covers short- and medium-term outcomes, while the bottom panel reports long-term outcomes. Approximately 8% of students did not complete 12th grade, about half worked during high school, and around a third did not earn a matriculation diploma. On average, a student worked for approximately 6.4 months during high school. Given that about half of the students did not work during high school, those who did worked for an average of approximately one year.

IV EMPIRICAL STRATEGY

Estimating the effects of SEA on outcomes using OLS would likely result in biased estimates, as SEA is endogenously determined by child and parental unobserved characteristics that affect the outcome (see, for example, Bedard and Dhuey 2006, Elder and Lubotsky 2009, Cook and Kang 2016, Depew and Eren 2016, Landersø et al. 2017, 2020). The initial Israeli school-entrance rule, in effect until 2015, provides a rare opportunity to estimate the causal effects of SEA while controlling nonparametrically for date-of-birth effects. This rule set the school-entry cutoff date on the same Jewish calendar date each year. Since the Jewish year is shorter than the Gregorian year, this fixed date shifts each year across different Gregorian dates in December. As a result, children born on the same date in different years may fall on opposite sides of the cutoff date. This creates a natural experiment in which students born on the same date of the year, who also share the same culture and institutional environment, start school at different ages solely because of the shifting cutoff date, independent of their socioeconomic background or academic capabilities. We exploit this exogenous variation in the cutoff dates across years to estimate SEA effects while flexibly controlling for date-of-birth effects.

To formalize this intuition, we employ a hybrid identification strategy that combines difference-in-differences estimation with a fuzzy RD approach. This strategy focuses on a narrow bandwidth of 28 days around the cutoff dates and, within this bandwidth, identifies the impact of SEA by comparing differences in the outcome variable across years between children born on different dates. Specifically, we estimate the following two-stage least squares (TSLS) specification:

$$Y_{idwp} = \alpha_0 + \alpha_1 \cdot SEA_{idwp} + \beta \cdot X_{idwp} + \varphi_d + \tau_p + \sigma_w + \varepsilon_{idwp} \quad (1)$$

Here, Y_{idwp} is the outcome variable for child i born on date d, day of week w, and period p; X_{idwp} is a set of characteristics of the child and family; φ_d , σ_w, and τ_p, are fixed effects for date of year, day of week, and period, respectively.

An indicator for whether a child was born before or after the relevant cutoff date serves as an instrumental variable for SEA. Since there is a sharp positive discontinuity in the SEA around the cutoff (Figure 1), the instrument is strongly correlated with SEA, with the F-statistic on the excluded instrument consistently exceeding the strictest criteria recently suggested by Lee (2022).[12] Our parameter of interest is α_1, which is the local average treatment effect of SEA among compliers. Notably, unlike standard RD specifications—which rely on the assumption that within the narrow bandwidth around the cutoff date, children born on different dates do not systematically differ in unobserved dimensions—our approach instead relies on the weaker assumption that such differences may exist but are constant across periods.

A potential threat to our identification strategy is that parents may time births around the Jewish entrance cutoffs, which could introduce differences in unobserved characteristics affecting outcomes.[13] To validate our design, we employed three strategies. First, we conducted two surveys to assess how often Israeli parents use the Jewish calendar in their daily lives, especially regarding the school cutoff date. The responses indicated that most did not use this calendar and were even unaware that the cutoff date was based on it. Moreover, none knew the exact cutoff date. These results suggest birth timing around cutoff dates is unlikely. The survey details are presented in Appendix A.

Second, Appendix Figure B1 demonstrates that key background characteristics are smooth at the entrance cutoffs. Additionally, columns 3 and 6 of Table 1 indicate that after

[12] Our identification strategy, which holds the date of birth constant, satisfies monotonicity. For compliers, shifting a child from before to after the cutoff increases their school entrance age by one year, while for never-takers and always-takers, it has no effect. Thus, assuming no defiers, the instrumental variable is monotonically related to the school entrance age. For a detailed discussion on monotonicity in various specifications used in the literature, see Attar et al. (2024).

[13] It has been shown that birthdates might not be entirely random, even within a narrow window around school-entry cutoff dates, as parents might shift a child's birth date from one side of the cutoff to the other for reasons that may or may not be related to SEA. For example, Dickert-Conlin and Chandra (1999) show that children are more likely to be born in late December than early January because of tax benefits. Such shifts can invalidate RD approaches relying on Gregorian cutoffs that coincidentally fall at the end of the year. However, our strategy is only compromised if such shifts occur relative to the Jewish calendar date of birth. We next address this concern.

accounting for fixed effects related to date of year, day of week, and period—central elements of our identification strategy—individual traits are balanced around the cutoffs. For both the young and old cohorts, only 3 of 45 covariates show statistically significant differences around the cutoffs, consistent with what one would expect by pure chance.[14]

Third, we test whether birth numbers are continuous at the cutoff, which would indicate that parents do not time births. Appendix Figure B2 presents a McCrary (2008) density test showing with weekly data that the number of births remains continuous around the cutoff date, indicating no manipulation. We use weekly data to smooth fluctuations arising because weekend births are rarer. Notably, even if there are differences between children born on different days of the week, we control for them by including day-of-week fixed effects in all our estimations. Additionally, Appendix Figure B3 presents a second test based on density discontinuity in the daily number of births, using local polynomial density estimations and their 95% confidence intervals (Cattaneo et al. 2018). This test confirms that there is no manipulation around the cutoffs. The overlapping confidence intervals and the p-value of 0.87 for the null hypothesis of continuous density at the cutoff further support the validity of our design. We present further robustness checks in Appendix C.

V RESULTS

V.A Effects on Grade Retention

Appendix Figure B4 plots grade retention against the number of days relative to the relevant cutoff, separated by gender, in three educational phases: early grades (1–3), intermediate grades (4–6), and later grades (7–9). It assesses whether the sharp discontinuity in SEA at the school-entry cutoff, as shown in Figure 1, leads to similar discontinuities in grade retention. The results are consistent across genders. The top panel shows a sharp discontinuity for retention in grades 1–3: Students born just after the cutoff are much less likely to repeat a grade. This effect diminishes in the middle panel (grades 4–6) and disappears entirely in the bottom panel (grades 7–9).

Table 3 presents TSLS estimates from equation (1), showing that starting school older significantly decreases the probability of retention in grades 1–3, with the effect being nearly twice as large for males (0.075 percentage points) compared to females (0.039 percentage points). The impact of SEA on grade retention diminishes in higher grades, becoming smaller

[14] Additional balance checks were conducted for indicators of missing values in mother's education, father's education, number of siblings, and parents' continent of birth. All these variables, which are omitted from the table because of space constraints, were found to be balanced around the entry cutoff dates.

and statistically nonsignificant for grades 4–6 and nearly zero for grades 7–9. This declining impact may indicate that SEA either has less influence on retention as students progress to later grades, or it may reflect a reduced SEA effect on repeating a grade after already having done so in earlier grades. To explore this question further, we estimate the impact of SEA on retention specifically in grades 4–6 and 7–9 without prior retention. In these estimations, the outcome variable takes the value of one only if the student was retained in grades 4–6 without prior retention in grades 1–3, and similarly for grades 7–9 without prior retention in grades 1–6. The results still show no significant effect of SEA on retention in these later stages, suggesting that SEA has less influence on repeating a grade as students progress to higher grades. This finding implies that focusing resources on early grades may be more effective in reducing overall retention, potentially preventing the need for interventions in higher grades.[15]

V.B Effects on Elementary School Test Scores

Appendix Figure B5 plots normalized fifth-grade test scores in math, Hebrew, science, and English against the number of days relative to the relevant cutoff. The figure reveals a sizable positive discontinuity at the cutoff for all subjects and both genders. Table 4 presents the TSLS estimates for fifth and eighth grades, showing that starting school a year older significantly improves normalized fifth-grade test scores across all subjects for both genders. It also increases the likelihood of achieving above-average scores in all subjects combined.[16]

By eighth grade, the effects of SEA vary by subject and gender. For males, the effect on math scores nearly doubles, and the impact on English scores remains stable. In contrast, the effect on Hebrew scores diminishes and becomes nonsignificant, while in science, it nearly disappears. For females, the impact on math scores remains stable, but it decreases and becomes nonsignificant in all other subjects. The tracking system in Israel, which begins in seventh grade only for math and English, potentially explains the persistence of the effects in these subjects, especially for males. This finding is consistent with the literature, which indicates that the impact of SEA tends to last longer when children are assigned to tracks earlier (Mühlenweg and Puhani 2010, Fredriksson and Öckert, 2014). Overall, starting school a year older provides short-term benefits in early grades, including higher test scores and a reduced probability of grade retention, most of which fade by later grades.

[15] Additionally, the last two rows of the table report the effect of SEA on retention in grades 4–6 and 7–9 among students who had not been retained in earlier grades, again showing no impact of SEA. However, these conditional results are suggestive rather than causal, as they condition on no prior retention, which may itself be influenced by SEA.

[16] However, like other estimates for elementary school test scores in the literature, ours are also confounded by age-at-test effects (see footnote 6).

V.C Effects on Youth Employment and Mechanisms

Effects on Youth Employment. Figure 2 shows, for both genders, a sharp positive discontinuity in youth employment at the school-entry cutoff, indicating that students born after the cutoff are more likely to work during high school. Appendix Table B2 provides reduced-form estimates for several key employment variables, showing that being born after the cutoff increases the likelihood of working in each high school grade. Additionally, it increases total employment duration and earnings by approximately 11.6% and 8.5% of the sample mean, respectively.

Panel A of Table 5 presents TSLS estimates for the likelihood and duration of employment during each grade in high school and overall. It shows that for both genders, starting school a year older substantially and significantly increases both the probability and the duration of working during each grade in high school. Specifically, it increases the probability of working during high school by 16 percentage points (31% of the sample mean) and the duration of employment by 2.58 months (40.4% of the sample mean). While the effect on employment duration is similar for both genders, the effect on the likelihood of working is 45% larger for males, despite nearly identical sample averages.

As greater work intensity is linked to a higher likelihood of dropping out of high school, we next focus on measures of work intensity, such as real earnings. Unfortunately, the Central Bureau of Statistics provides only annual earnings data, which do not align with the school year that runs from September to June. To address this issue, we estimate the effect of SEA on children's earnings in each calendar year during high school, noting that each calendar year overlaps two consecutive grades. To mitigate the influence of outliers, we winsorize the top 5% of earnings. The results, presented in Appendix Table B3, show that a higher SEA significantly increases earnings in each calendar year during high school and overall. Starting school a year older raises earnings over the three calendar years beginning in ninth grade by 55% of the sample mean, with similar effects among males and females. Finally, to assess the effect of SEA on varying levels of work intensity, we examine the effects of SEA on having earnings above the median, the 75th percentile, and the 90th percentile, with percentiles calculated among those with a positive income. The results again indicate that a higher SEA has significant positive effects on these outcome variables for both males and females. For example, starting school a year older increases the likelihood of earning above the 75th percentile by about 12.8 percentage points (66% of the sample mean).

Why Do Older School Entrants Work More During High School? There are several possible reasons why older school entrants are more likely to work during high school. First, they reach the minimum legal working age in a lower grade. If students view this legal age as a binding constraint, reaching it earlier is likely to increase both their likelihood and duration of working during high school. Second, being older within their grade makes them more mature, increasing the supply of job opportunities from employers and fostering a stronger intention to work. Third, labor regulations often offer higher wages as students age, giving older school entrants a greater incentive to work in each grade. In this section, we provide evidence that the minimum legal working age plays a key role in driving older school entrants to work more during high school.

In our RD sample, most students began school at around 5.7 or 6.7 years old. Consequently, during the first half of 10th grade, none of these students had reached the legal working age, while only the older students had reached it by the second half. If the legal working age acts as a binding constraint, the SEA effect on employment should be greater in the second half of 10th grade than in the first. The results, presented in Panel B of Table 5, confirm this expectation. They indicate that the effect of SEA on the likelihood of working is more than twice as large in the second half of 10th grade compared to the first. Furthermore, for males, the effect is more than four times greater in the second half. Similarly, the effect of SEA on work duration is almost twice as large in the second half of the 10th grade as in the first half.

To further investigate whether the legal working age acts as a binding constraint, we track students' employment status over time within a 120-day window around the legal working age, provided this window falls within the academic year. Specifically, we include only months from September through June and exclude those during the summer vacation. Students' age is measured at the start of each working month. For example, for a student born on December 3, 2000, employment in December 2016 would be recorded as two days before the cutoff. Figure 3 displays the share of students employed by number of days relative to age 16, with linear trends on either side of the cutoff. Employment rates remain flat before age 16 but sharply increase from 8% to 14% within the 120 days following the cutoff. Appendix Figure B6 presents residuals from a regression of employment status on individual fixed effects, capturing deviations from each student's average employment status over time and reflecting the same trend observed in Figure 3. These findings suggest that reaching the legal working age is a key factor driving older school entrants to work more during high school, since they become eligible at an earlier grade.

Additionally, we estimate equation (2) to formally examine whether there is a structural break at the legal working age of 16:

$$Employment_{ipt} = \gamma_i + \delta_p + \beta_1 \cdot RC_{ipt} + \beta_2 \cdot AC_{ipt} + \beta_3 \cdot AC_{ipt} \cdot RC_{ipt} + \varepsilon_{ipt} \quad (2)$$

Here, $Employment_{ipt}$ is an indicator for whether child i in period p is working at time *t*; RC_{ipt} is the child's age*,* measured in days relative to the cutoff of age 16; and AC_{ipt} is an indicator for whether the child is above age 16. γ_i represents student fixed effects, capturing time-invariant characteristics of child *i*, and δ_p denotes period fixed effects. The results are reported in Appendix Table B4, separately for each gender, both with and without student fixed effects. The findings indicate that the slope of the trend is nearly 0 and nonsignificant before the cutoff but increases 15-fold (to 0.000445) after the cutoff, becoming significant at the 1% level. Finally, at the bottom of the table, we report the F-statistic of a Chow (1960) test for a structural break at age 16. The F-statistic is very large, strongly indicating that we can easily reject the null hypothesis and conclude that a structural break exists. Overall, these results provide direct evidence that the minimum legal working age plays a pivotal role in our youth-employment mechanism.

V.D Effects on High School Dropout and Mechanisms

Effects on High School Dropout. In this section, we provide evidence that although a higher SEA offers the short-term benefits discussed in Sections V.A and V.B, it increases the risk of high school dropout. Figure 4 plots high school outcomes against the number of days relative to the relevant cutoff. Panel A reveals a sizable positive discontinuity in the share of males not completing 12th grade, indicating that students born after the cutoff have a higher probability of not completing high school. No such discontinuity is observed for female students. Panel B shows no evidence, for either males or females, of a discontinuity in the share of students not earning a matriculation diploma.

Panels A and B of Table 6 present reduced-form and TSLS estimates, respectively. Column 1 shows results for the entire population, column 2 focuses on females, and columns 3 and 4 present results for males. The TSLS estimates for males indicate that starting school a year older increases the likelihood of not completing 10th grade by 2.3 percentage points (77% of the sample mean among males), 11th grade by 5.3 percentage points (93%), and 12th grade by 6.6 percentage points (68%). The smaller effect on 10th-grade completion may suggest that students who begin working in the middle of 10th grade are likely to finish the grade but not continue further. In contrast, females experience no significant effect on completing 10th or 11th grade, and the effect on 12th-grade completion is smaller—2.3 percentage points (43.4% of the sample average among females)—and only approaches significance at the 10% level.

Additionally, for both males and females, the results provide no evidence that a higher SEA offers any benefit for earning a matriculation diploma.

Mechanisms. As detailed in Section II.A, the Israeli compulsory schooling law, both before and after the passage of Amendment 29, required children to complete a fixed number of grades regardless of their school-entry age, making it unlikely that the law explains the positive link between higher SEA and high school dropout. However, a small exemption exists: Before the amendment, students were required to complete 10th grade only if they had not turned 16 before the academic year, and after the amendment, they were required to complete 12th grade only if they had not turned 18. These exemptions applied to just the 0.5% of students in our sample who started school above age 7 and the 3.7% who repeated a grade. To further reduce the possibility that the positive SEA effect on high school dropout is driven by the compulsory schooling law, we reestimate the effects of SEA on completing 10th, 11th and 12th grades, this time focusing only on children who started school below age 7 and were thus not exempt. The results, presented in column 4, show that the SEA effects remain largely unchanged. We do not conduct a similar analysis conditioning on not repeating a grade, as grade repetition may itself be an outcome of SEA.

While both the pre- and post-amendment versions of the law are unlikely to account for a positive SEA effect on high school dropout, it could be argued that this effect is influenced by the law's *change*. To address this concern, we estimate equation (3), which allows the SEA effect on dropout to differ before and after the amendment, while also controlling for the law's change. Specifically, we estimate the following TSLS specification:

$$Y_{idwp} = \alpha_0 + \alpha_1 \cdot SEA_{idwp} + \alpha_2 \cdot SEA_{idwp} \cdot NewLaw_{idwp} + \alpha_3 \cdot NewLaw_{idwp} + \beta \cdot X_{idwp} + \varphi_d + \tau_p + \sigma_w + \varepsilon_{idwp} \tag{3}$$

This specification includes two endogenous variables—SEA and its interaction with NewLaw—and thus we use two instrumental variables: an indicator for birth after the entrance cutoff and its interaction with NewLaw. Table 7 reports the results, showing that the effect of SEA on dropout is significant for each grade between 10th and 12th, with no significant difference before and after the amendment. These findings demonstrate that the positive SEA effect on high school dropout is not driven by the law's change.

Another potential mechanism is that students who start school older—and are thus older for a given grade—are more likely to get married or give birth during high school, which could increase their tendency to drop out. However, the low prevalence of marriage (0.36%) and childbirth (0.17%) among high school students in our sample suggests that these factors do not drive this result.

We suggest that employment during high school, which serves as an outside option, drives the positive SEA effect on high school dropout, particularly among males. This mechanism aligns with Holford (2020), which finds that part-time work significantly contributes to high school dropout for males only. [17] Similarly, Montmarquette et al. (2007) estimate a structural model showing that working during high school reduces the likelihood of continuing studies, particularly among males.[18] Several other studies also support this mechanism, finding that better labor market conditions reduce schooling by increasing the opportunity cost of education and encouraging students to work instead, with the effects generally stronger for males (Atkin 2016, Shah and Steinberg 2021, Cascio and Ayushi 2022). Furthermore, our finding that the SEA effect on dropout is stronger for males is consistent with our previous finding that the SEA effect on youth employment is also larger for males.

Next, we employ several strategies to show that the positive SEA effect on high school dropout among males is likely driven by youth employment. We hypothesize that working during a grade may lead students to drop out in that grade or to complete the grade but not progress to the next one, suggesting that dropouts in 10th–11th grades are influenced by 10th-grade employment and dropouts in 11th–12th grade by 11th-grade employment. To test this hypothesis, we decompose the effect of SEA on dropouts in 10th–11th grades, expressing it as the sum of the SEA effects on "working in 10th grade and dropping out" and "not working in 10th grade and dropping out." If the effect is driven by 10th-grade employment, the first component should be significant and account for most of the overall effect, while the second component should be negligible and nonsignificant. We also apply the same decomposition for 11th–12th grade dropouts based on 11th-grade employment.

[17] A potential explanation for this gender difference is that males perceive studying and working as substitutes for their career development, while females view them as complementary. Holford (2020) supports this idea, showing that females with part-time work experience at age 15 are more likely to enter managerial or professional roles by age 25, a pattern not observed among males. Holford also finds that females who work during high school tend to self-select into retail and catering jobs, which develop cognitive skills such as financial literacy, mental arithmetic, and interpersonal skills, yielding long-term educational and labor market returns. In contrast, males are more likely to work in delivery roles, which do not develop these skills. Additional explanations include females' stronger academic performance, which may protect them from challenges that lead to dropout, and females' greater resilience and coping skills, which help them balance work and school.

[18] Their model considers two types of students: those who prioritize work over grades, where part-time employment harms their grades and leads to dropout, and those who prefer staying in school and maintaining strong academic performance, which could lead to employment without causing dropout. Their estimated model indicates that a larger proportion of female students belong to the latter group, suggesting a stronger preference for schooling over work.

Figure 5 presents the reduced form of this decomposition. Panels A and B plot the likelihood of "working in 10th grade and dropping out during 10th–11th grades" and "not working in 10th grade and dropping out during 10th–11th grades" against the number of days relative to the cutoff. As expected, the graphs for males show a sharp discontinuity at the cutoff for "working and dropping out" but not for "not working and dropping out." Similarly, Panels C and D reveal a sharp discontinuity for "working in 11th grade and dropping out during 11th–12th grades," with no such discontinuity for "not working in 11th grade and dropping out during 11th–12th grades." These findings suggest that the SEA effect on high school dropout among males is driven by youth employment. In contrast, no discontinuities appear in any of these variables for females.

Table 8 presents TSLS estimates of this decomposition. For males, column 1 shows that starting school a year older increases dropout during 10th–11th grades by 3.1 percentage points. This overall effect comprises 2.5 percentage points for "working in 10th grade and dropping out" (column 2), representing 81% of the total effect, and a nonsignificant 0.6 percentage points for "not working in 10th grade and dropping out" (column 3). These results again indicate that the SEA effect on dropout among males is largely driven by those who worked in 10th grade. For females, the overall effect and both components are highly nonsignificant, suggesting no similar link to youth employment. Columns 4–6 apply the same decomposition for dropout during 11th–12th grades based on employment status in 11th grade, revealing a similar pattern. For males, the overall effect is 4.3 percentage points, driven almost entirely by a significant 4.2 percentage points increase for "working in 11th grade and dropping out" (column 5), while the "not working" effect (column 6) remains negligible. For females, the overall SEA effect is 2.3 percentage points. Although both components are of similar magnitude, only the effect for "working and dropping out" is significant.[19]

Second, we adopt an analysis similar to that of Angrist et al. (2022) to demonstrate that a specific mechanism drives the effect of a treatment variable on an outcome variable. This

[19] Another approach to examine whether the effect on dropout is driven by youth employment is to estimate SEA effects separately for those who worked and those who did not. While this approach is more intuitive, it does not provide causal estimates, as it conditions on employment, which is itself an outcome of SEA. Figure B7 illustrates the likelihood of dropping out during 10th–11th grades by the number of days relative to the cutoff, with separate panels for those who worked and those who did not work in 10th grade. It also shows the likelihood of dropping out during 11th–12th grades by employment status in 11th grade. The results reveal sharp discontinuities at the cutoff, but only for males who worked in the previous grade. Similarly, Appendix Table B5 presents the TSLS estimates. For males, the SEA effect on 10th- to 11th-grade dropout among those who worked is 0.240 and highly significant, while 0.010 and nonsignificant among those who did not. In 11th–12th grades, the effect is 0.128 for those who worked and 0.014 for those who did not. For females, all coefficients are nonsignificant. These results again indicate that the SEA effect on high school dropout is driven by youth employment.

approach involves estimating the treatment's effects on both the mechanism and outcome variables across various subgroups and examining the correlation between these effects. In our case, we estimate the effect of SEA on "employment during 11th grade" and "dropping out in 11th–12th grades" and assess whether the intensities of these two effects are correlated. Figure 6 shows the effect sizes across the subgroups, where each point represents a subsample, with the x-axis indicating the effect of SEA on employment and the y-axis indicating its effect on dropout. The strong positive correlation (0.82) between these effects provides additional suggestive evidence that the SEA effect on high school dropout is driven by youth employment.

Since our youth-employment mechanism significantly drives high school dropout only for males, we next focus on male students and estimate heterogeneous effects on youth employment, not completing 12th grade, not earning a matriculation diploma, and combinations of youth employment with not completing 12th grade. In these specifications, we use two instruments—an indicator for birth after the entrance cutoff and its interaction with the heterogeneous variable—to instrument for the two endogenous variables: SEA and its interaction with the heterogeneous variable. This analysis is intrinsically valuable because it identifies which students are most affected by our proposed employment mechanism. Additionally, it allows us to assess whether the heterogeneous effects on these outcome variables operate in the same direction. If they do, this provides further suggestive evidence that the effects of SEA on not completing 12th grade or not achieving a diploma are driven by youth employment. The results, presented in Panel A of Table 9, show that the heterogeneous effects on these variables generally align in the same direction. Specifically, all SEA effects are smaller for students with more educated parents, who are less likely to work during high school. In contrast, these effects are stronger among students with more siblings, except for the effect on youth employment per se, which is close to zero and nonsignificant.

A Dynamic Approach: Short-Term Benefits and Long-Term Reversal Effects. In the previous sections, we demonstrated that students who start school older experience short-term benefits, such as a lower probability of grade retention (Table 3) and higher fifth-grade test scores (Table 4). However, in the medium term, they also face a higher probability of high school dropout (Table 6). Fortunately, our data set tracks these students from school entry to high school completion, allowing us to examine for the first time whether a higher SEA increases the likelihood that the same child will benefit in the short term from higher scores or no grade retention and yet later drop out.

To investigate this question, we decompose the overall SEA effect on not completing high school based on whether these students previously achieved above- or below-average fifth-grade test scores. Specifically, we express the overall SEA effect on not completing high school as the sum of the SEA effects on "Above-average scores and not completing high school" and "Below-average scores and not completing high school." If most of the overall effect was driven by the former component, this would suggest that our youth-employment mechanism increases dropout rates not only among traditionally weaker students but also, and even primarily, among students who initially performed well in elementary school.

Figure 7 presents the reduced form of this decomposition. Panels A and B show that, particularly among males, there is a sharp discontinuity at the cutoff in the combined outcome "above-average 5th-grade test scores and not completing 12th grade," while there is no such discontinuity in "below-average 5th-grade test scores and not completing 12th grade." Similarly, there is a sharp positive discontinuity at the cutoff in "above-average 5th-grade test scores and not earning a diploma," whereas there is a sharp negative discontinuity in "below-average 5th-grade test scores and not earning a diploma."

Table 10 presents TSLS estimates of this decomposition for the following outcome variables: high school completion, employment during high school, not completing high school, not earning a matriculation diploma, and combinations of these outcomes. Each entry in the table represents a separate regression: Column 1 shows the overall SEA effect on each outcome variable among children tested in the fifth-grade exams, while columns 2 and 3 display the components for above- and below-average fifth-grade scores, respectively. For example, in the first row, the overall SEA effect on not completing high school is 0.027. Of this figure, the effect on "above-average 5th-grade scores and not completing 12th grade" is 0.024 (89% of the overall effect) and is statistically significant, while the effect on "below-average 5th-grade scores and not completing 12th grade" is only 0.003 and is statistically nonsignificant. This means SEA reduces the likelihood of high school completion by 2.7 percentage points, primarily offset by a 2.4 percentage points increase in the likelihood of "achieving above-average 5th-grade scores and not completing high school," along with an additional negligible and nonsignificant increase of 0.3 percentage points in the likelihood of "achieving below-average 5th-grade score and not completing high school."

Similarly, the results strongly indicate that the SEA effect on employment during high school is driven solely by children who achieved above-average 5th-grade scores. The findings are even more striking for not earning a diploma. While delaying school entry by a year increases by 9.3 percentage points the likelihood of "above-average 5th-grade scores and not earning a diploma," it reduces by 9.2 percentage points the likelihood of "below-average 5th-grade scores and not earning a diploma."

Furthermore, the last two rows of the table show the results of this decomposition for the combined outcomes of "being employed during high school and not completing 12th grade" and "being employed during high school and not earning a matriculation diploma." We find that the SEA effects on these combined outcome variables are driven by students with above-average fifth-grade test scores. For example, the last row shows that SEA significantly increases by 6.1 percentage points the probability that a child will first achieve above-average fifth-grade test scores, then work during high school, and finally not achieve a diploma. In contrast, SEA reduces by 1.8 percentage points the probability that a child will first achieve below-average fifth-grade test scores, then work during high school, and finally not achieve a diploma. Columns 4–6 present a similar decomposition based on whether children retained a grade prior to third grade. The results again reveal that the SEA effects are driven by students who did not retain a grade. In summary, these findings support our hypothesis that most of the SEA effects on not completing high school and not earning a diploma are driven by students who benefited from a higher SEA through higher fifth-grade test scores and a reduced probability of grade retention.

V.E Effects on College Entry-Exam Taking and Scores

From this point onward, we use data on our old cohorts—students who had reached at least 38 years old when the outcome variable was measured—to estimate SEA effects on long-term outcomes. We begin by analyzing the effects of SEA on participation in and scores on the psychometric college entry exam. Admission to most Israeli higher education institutions depends on a weighted average of matriculation diploma scores and psychometric scores. The psychometric test, comparable to the SAT in the United States, is a standardized exam with quantitative, verbal, and English sections. Our data include total scores, ranging from 200 to 800, and section scores, ranging from 50 to 150, with standard deviations of about 100 for total scores and around 20 for section scores.

Figure 8 depicts several higher education outcomes relative to the school-entry cutoff, separated by gender. Panel A shows the psychometric-exam participation rate. Consistent with our finding of a negative discontinuity in high school completion only among males, a sharp negative discontinuity in exam participation is also observed exclusively for males. The TSLS estimates in Table 11 (first row) indicate that for males, starting school a year older reduces the probability of taking the psychometric exam by 13.5 percentage points (about 27% of the sample mean). Among females, the coefficient is also negative but much smaller (−0.031) and statistically nonsignificant.

Notably, individuals take this exam at varying ages, and some may take it more than once. The average age of taking the exam is 21 for males and 19.9 for females, with 27.2% of participants in our sample taking it during high school. For students who took the exam multiple times, we use their initial scores. We do not control for age at exam, as the decision of when to take the exam may itself be influenced by SEA. However, as shown in the second row of Table 11, the SEA effect on age at the time of the exam is minimal and statistically nonsignificant for both genders, with coefficients of only 0.17 years (about two months) for males and 0.32 years (less than four months) for females. This finding is attributable to the practice in Israel whereby students who complete high school younger wait longer until military recruitment, thus taking the psychometric exams after military service at roughly the same age. This unique context provides a rare opportunity to obtain SEA estimates on test scores that are not confounded by age-at-test effects.

Our SEA estimates on test scores contribute to the literature in two key ways. First, they capture the effect of SEA on test scores at a much later stage of students' academic career, when these scores are particularly relevant for acquiring higher education and reflect cognitive skills relevant to labor market performance. Second, unlike in most of the literature, our estimates are free from age-at-test confounding. Black et al. (2011) is a notable exception, having addressed this issue by analyzing military IQ-exam scores in Norway, but unlike Black et al., who study only males, we examine these effects for both males and females.

Estimating the effect of SEA on test scores among test takers is likely to introduce sample-selection bias, as higher SEA reduces the likelihood of taking the exam. The reasoning is that students who take the psychometric exam regardless of their SEA are, on average, stronger than marginal students induced to participate only because they have lower SEA (Krueger and Whitmore, 2001).[20] Thus, within the group of test takers, those with higher SEA are, on average, stronger than those with lower SEA, resulting in an upward-biased estimate of the true SEA effect on test scores.

To address this problem, we follow the approach of Gray-Lobe et al. (2023) and estimate across the entire population of students the effect of SEA on the probability of achieving scores above each quartile threshold. In this analysis, a student is considered as

[20] Krueger and Whitmore (2001) discuss this argument and cite prior studies using state-level data that find average test scores tend to decline when more students take the college entry exam, likely because marginal test takers are weaker than the average student (Dynarski 1987, Card and Payne 2002).

scoring above a given quartile only if they took the exam and exceeded that threshold, while students who did not take the exam are treated as not meeting any of the threshold scores. This approach is valuable for two reasons: First, non-test-takers did not achieve the threshold scores required for higher education, making their status equivalent to taking the exam and not meeting the threshold. Second, it provides a lower-bound estimate of the true SEA effect on test scores by assuming that all non-test-takers fall below the threshold. Additionally, we report conditional estimates among test takers. While these estimates may be upwardly biased, they serve as an upper bound for the true SEA effects, assuming non-test-takers are no stronger than test takers. For consistency in comparing results between the entire sample and test takers, quartile thresholds are calculated solely among test takers who took the exam for the first time. Together, these estimates provide both lower and upper bounds for the true SEA effects on test scores.

Panel B of Figure 8 plots the share of students applying for the exam and scoring in the top quartile against the number of days around the school-entrance cutoff date. The figure reveals a sharp negative discontinuity but only for males: Those born just after the cutoff are less likely to score in the top quartile compared to those born just before. Table 12 presents the TSLS estimates for both the entire sample and test takers. Among males in the entire sample, higher SEA reduces the likelihood of scoring above each quartile threshold for both the total scores and each of the individual subject scores. Moreover, except for the SEA effect on scoring in the top quartile of the English exam—which is negligible and nonsignificant—all coefficients are substantial, ranging between −7.7 and −13.2 percentage points, with six significant at the 5% level and several others either significant or near significance at the 10% level. For example, starting school a year older significantly reduces the probability of scoring above the second and third quartiles in the total score by 13.2 and 10.0 percentage points, respectively, and in the quantitative exam by 12.7 and 9.7 percentage points, respectively. Furthermore, the estimates among test takers, viewed as upper bounds, are never significantly positive. Additionally, a higher SEA shows a negative (though nonsignificant) coefficient on average total, quantitative, and verbal scores. Overall, our findings among males align closely with those of Black et al. (2011), reinforcing the idea that when age-at-test effects are isolated, the positive effect of SEA on test scores turns negative. In essence, if tested at the same age, students starting school younger outperform their older counterparts. In contrast, for females, estimates for both the entire sample and test takers show no significant effect of SEA on the likelihood of achieving scores above a given quartile. Additionally, the SEA coefficients on the average total scores and each of the subject scores are now positive, though still statistically nonsignificant.

As in Black et al. (2011), our setting has the limitation that some students took the psychometric exam during high school. This raises the concern that identifying the pure SEA effect may be confounded if older school entrants consistently had less schooling at the time they took the exam. For instance, Black et al. found that students born before the cutoff date had, on average, 0.8 more years of schooling at test time compared to those born after the cutoff. Consequently, they suggest that the estimated SEA effect should be interpreted as a lower bound of the benefits of starting school at an older age. While we share the same concern, it is less significant in our setting: About three-quarters of our sample took the exam after high school, when all students had completed the same number of schooling years (12 years). Moreover, our data show that the years of schooling at test time for students born before and after the cutoff are very similar—11.8 and 11.78 years, respectively. Specifically, among females, the number of years of schooling is identical on both sides of the cutoff, while among males, those born after the cutoff have only 0.043 fewer years of schooling—a negligible difference.

Nevertheless, to address even this minor concern that our SEA estimates on test scores might be confounded by years of schooling at the time of taking the exam, we re-estimate the effects of SEA on the likelihood of achieving a total score above each quartile, this time excluding students who took the exam during high school. That is, we focus only on those who took the exam after high school—when all these students had the same years of schooling—or those who did not take the exam at all. Notably, since taking the exam after high school may itself be influenced by SEA, we consider this analysis suggestive rather than causal. The results, shown in Appendix Table B6, are even stronger: Only among males does higher SEA have a significant negative effect on the likelihood of achieving scores above each quartile.

V.F Effects on Higher Education Outcomes

Consistent with our findings that male students who enter school at an older age are less likely to apply for the psychometric exam and achieve high test scores, we find they also have a lower probability of entering college (Panel C of Figure 8 shows the discontinuity). Table 11 provides TSLS estimates, indicating that starting school a year older reduces the probability of college entry among males by 13.3 percentage points (23% of the sample mean). For females, the effect is also negative and substantial (8 percentage points) but not statistically significant. Panel D of Figure 8 further shows that children born just after the cutoff complete fewer years of schooling across both genders. TSLS estimates, reported in Table 11, indicate that starting school a year older reduces years of schooling by 0.41 years for males and 0.53 years for females. Additionally, a higher SEA has no impact on the probability of completing a first degree or on the age at which a first degree is started or completed. This indicates that for those who pursue higher education, starting school later does not necessarily delay entry into the labor market.

Examining heterogeneous effects on these long-term educational outcomes reveals a very similar pattern to that observed for high school completion: These effects are significantly negative only for males (except for years of schooling, which is significant for both genders), less pronounced for those with more educated parents, and more pronounced for those with more siblings (Panel B of Table 9).

V.G Effects on Employment and Earnings

Finally, we assess the impact of SEA on employment and earnings at ages 28–32 and 33–37, defining individuals as employed if their income exceeds 10% of the average real wage earnings for the respective age interval. To reduce the influence of outliers, we winsorize the top 5% of earnings. The results, presented in Panel B of Table 11, suggest that the effects of SEA on employment likelihood in both age groups are nonsignificant, though quite sizable (7 percentage points) for ages 27–32 among males.

For earnings, the estimated effects are negative and sizable for both genders and age ranges but not statistically significant because the standard errors are imprecise. Starting school a year older increases real earnings during ages 27–32 and 32–37 by 5.5% and 6.0% of the sample mean, respectively. Dividing these estimates by the SEA effect on years of schooling, which is 0.474 years, yields an implied return to education of about 11%–12%. Although our estimates are imprecise, their magnitudes align with several studies that find a similar return (Ashenfelter and Krueger 1994; Staiger and Stock 1997; Angrist and Krueger 1991; Duflo 2001). However, since these estimates are not statistically significant, they are also consistent with studies suggesting minimal to no educational returns (Pischke and von Wachter 2008, Stephens and Yang 2014, Meghir and Palme 2005).

VI SUMMARY AND CONCLUDING REMARKS

This paper used a unique identification strategy that nonparametrically controlled for date-of-birth effects to estimate the causal impact of SEA on a broad range of short-, medium-, and long-term outcomes. We demonstrated that the interaction between SEA policy and youth employment laws leads older school entrants to drop out of high school at higher rates, as they reach the legal working age in an earlier grade. Notably, this effect is driven primarily by students who initially performed well in elementary school.

We found that higher SEA also reduces the likelihood of taking a college entrance exam, achieving high scores, and enrolling in college. These effects are pronounced among males, particularly those with more siblings and less educated parents. Unlike in most previous studies, our estimates on psychometric test scores are not confounded by age-at-test effects.

In the longer run, higher SEA reduces years of schooling for both genders and has a sizable negative, albeit nonsignificant, effect on earnings between ages 27 and 37. Overall, our findings suggest that while delaying school entry may offer short-term benefits, such as higher elementary school test scores and reduced grade retention, it does not accelerate long-term human capital accumulation and may even hinder it. Thus, parents should carefully consider this trade-off before delaying their child's school entry.

Our results also suggest that policy makers should consider the interaction between youth employment laws and compulsory schooling laws when designing education and labor policies. For instance, had the Israeli Youth Employment Law set a minimum number of completed grades instead of a minimum age, it might have mitigated the higher dropout rates among older school entrants. Moreover, our study highlights that youth employment significantly contributes to dropout rates, suggesting that interventions aiming to reduce students' motivation to work during high school could effectively lower these rates. However, such interventions require careful consideration. Students do not benefit equally from staying in school longer, and for those not pursuing higher education, work experience might improve career prospects. Indeed, the nonsignificant SEA effect we found on earnings suggests that we cannot rule out the possibility that for some students, opting for work over continued schooling might be a rational choice, reflecting largely unchanged earnings potential.

Our findings also help explain how variations in youth employment policies across countries can account for different SEA effects on dropout rates. In Sweden, for example, study allowances reduce incentives to leave school for work, which may help explain the positive SEA effect found there by Fredrickson and Öckert (2014), unlike in the United States. This difference reflects not only Sweden's compulsory schooling law—which sets a minimum number of completed grades rather than a minimum dropout age—but also the reduced incentive Swedish students face to drop out in order to work.

Finally, the study underscores important methodological implications for estimating returns to education. Instrumental variables such as quarter of birth or location relative to school-entry cutoffs may inadvertently capture the combined effect of years of schooling and loss of work experience during high school.[21] Thus, differences in estimated returns to education across studies relying solely on compulsory schooling laws, or their variations across countries and time, may also reflect shifts in youth employment laws.[22] For instance, an increase in the school dropout age that coincides with a change in the legal working age could significantly affect the estimated returns to education. In many countries, youth employment laws prevent children from working before the compulsory schooling age, closely linking the minimum dropout age and minimum working age. Disentangling these effects is essential to understanding the true factors influencing returns to education and provides fertile ground for future research.

[21] Cascio and Lewis (2006) challenge the validity of the quarter-of-birth instrument by showing that it directly affects test scores. Buckels and Hungerman (2013) further demonstrate that the relationship between quarter of birth and outcomes arises from variations in maternal characteristics across births throughout the year. We suggest that even if this concern is addressed by concentrating on a short interval around the school-entry cutoff and controlling for date-of-birth effects, the instrument might still be invalid, as it could influence the outcome variable not only through years of schooling but also through youth employment.

[22] For a comprehensive review of this literature see Card (2001).

References

Acemoglu, Daron, and Joshua D. Angrist. 2000. "How large are human-capital externalities? Evidence from Compulsory Schooling Laws." *NBER macroeconomics annual* 15: 9–59.

Atkin, David. 2016. "Endogenous Skill Acquisition and Export Manufacturing in Mexico." *American Economic Review* 106(8): 2046-2085.

Attar, Itay, and Danny Cohen-Zada. 2018. "The Effect of School Entrance Age on Educational Outcomes: Evidence using Multiple Cutoff Dates and Exact Date of Birth." *Journal of Economic Behavior and Organization* 153: 38–57.

Attar, Itay, Danny Cohen-Zada, and Todd Elder. 2024. "Measuring and Correcting Monotonicity Bias: The Case of School Entrance Age Effects." IZA Discussion paper No. 17088.

Angrist, Joshua D., and Alan B. Krueger. 1991. "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics* 106 (4): 979–1014.

Angrist, Joshua D., and Alan B. Krueger. 1992. "The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples" *Journal of the American Statistical Association* 87 (418): 328–36.

Angrist, Joshua D., David Autor, and Amanda Pallais. 2022. "Marginal Effects of Merit Aid for Low-Income Students." *Quarterly Journal of Economics* 137 (2): 1039–1090.

Ashenfelter, Orley, and Alan B. Krueger. 1994. "Estimates of the Economic Return to Schooling from a New Sample of Twins." *American Economic Review* 84(5): 1157–173.

Bedard, Kelly, and Elizabeth Dhuey. 2006. "The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects." *Quarterly Journal of Economics* 121 (4): 1437–1472.

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. 2011. "Too Young to Leave the Nest? The Effects of School Starting Age." *Review of Economics and Statistics* 93 (2): 455–67.

Buckles, Kasey S., and Daniel M. Hungerman. 2013. "Season of Birth and Later Outcomes: Old Questions, New Answers." *Review of Economics and Statistics* 95 (3): 711–24.

Card, David. 2001. "Estimating the return to schooling: Progress on some persistent econometric problems." *Econometrica* 69 (5): 1127–160.

Card, David, and Abigail A. Payne. 2002. "School finance reform, the distribution of school spending, and the distribution of student test scores." *Journal of Public Economics*, 83(1), 49–82.

Cascio, Elizabeth, and Ethan Lewis. 2006. "Schooling and the armed forces qualifying test: Evidence from school-entry laws." *Journal of Human Resources* 41 (2): 294–318.

Cascio, Elixabeth, and Ahushi Narayean. 2022. "Who Needs a Fracking Education? The Educational Response to Low-Skill-Biased Technological Change." *ILR Review* 75(1): 56-89.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma. 2018. "Manipulation testing based on density discontinuity." *The Stata Journal* 18 (1): 234–61.

Celhay, Pablo, and Sebastian Gallegos. 2022. “Early Skill Effects on Parental Beliefs, Investments and Children Long-Run Outcomes.” *Journal of Human Resources*. doi: 10.3368/jhr.0920–11175R2.

Charles, Kofi K., Erik Hurst, and Matthew J. Notowidigdo. “Housing Booms and Busts, Labor Market Opportunities, and College Attendance.” *American Economic Review* 108(10): 2947-94.

Chow, Gregory C. 1960. “Tests of equality between sets of coefficients in two linear regressions.” *Econometrica* 28(3): 591–605.

Cook, Philip J., and Songman Kang. 2016. “Birthdays, Schooling, and Crime: Regression-Discontinuity Analysis of School Performance, Delinquency, Dropout, and Crime Initiation.” *American Economic Journal: Applied Economics* 8 (1): 33–57.

Depew, Briggs, and Ozkan Eren. 2016. “Born in the wrong day? School entry age and juvenile crime” *Journal of Urban Economics* 96, 73–90.

Dhuey, Elizabeth, David Figlio, Krzysztof Karbownik, and Jeffrey Roth. 2019. “School Starting Age and Cognitive Development.” *Journal of Policy Analysis and Management* 38 (3): 538–78.

Dickert-Conlin, Stacy, and Amitabh Chandra. 1999. ‘‘Taxes and the Timing of Births.’’ *Journal of Political Economy* 107 (1): 161–77.

Dobkin, Carlos, and Fernando Ferreira. 2010. “Do School Entry Laws Affect Educational Attainment and Labor Market Outcomes?” *Economics of Education Review* 29 (1): 40–54.

Duflo, Esther. 2001. “Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment.” *American Economic Review* 91(4): 795–813.

Dynarski, Mark. 1987. “The scholastic aptitude test: participation and performance.” *Economics of Education Review* 6(3): 263–73.

Elder, Todd, and Darren H. Lubotsky. 2009. “Kindergarten Entrance Age and Children’s Achievement: Impacts of State Policies, Family Background, and Peers.” *Journal of Human Resources* 44 (3): 641–83.

Fredriksson, Peter and Björn Öckert. 2014. “Life-cycle Effects of Age at School Start.” *Economic Journal* 124 (579): 977–1004.

Gray-Lobe, Guthrie, Pathak A. Parag, and Christopher R. Walters. 2023. “The long-term effects of universal preschool in Boston.” *The Quarterly Journal of Economics*, 138(1), 363–411.

Holford, Angus. 2020. “Youth Employment, Academic Performance and Labour Market Outcomes: Production Functions and Policy Effects.” *Labour Economics* 19 (2): 211–21.

Hurwitz, Michael, Jonathan Smith, and Jessica S. Howell. 2015. “Student Age and the Collegiate Pathway.” *Journal of Policy Analysis and Management* 34 (1): 59–84.

Krueger, Alan B., and Diane M. Whitmore .2001. “The effect of attending a small class in the early grades on college-test taking and middle school test results: Evidence from Project STAR.” *The Economic Journal* 111(468), 1–28.

Landersø, Rasmus, Helena S. Nielsen, and Marianne Simonsen. 2017. "School starting age and the crime-age profile." *The Economic Journal*, 127(602), 1096–1118.

Landersø, Rasmus, Helena S. Nielsen, and Marianne Simonsen. 2020. "Effects of school starting age on the family." *Journal of Human Resources* 55(4), 1258–1286.

Lee, David, Justin McCrary, Marcelo Morreira, and Jack Porter. 2022. "Valid t-ratio Inference for IV." *American Economic Review*, 112 (10): 3260–3290.

Lleras-Muney, Adriana. 2002. "Were Compulsory Attendance and Child Labor Laws Effective? An Analysis from 1915 to 1939." *The Journal of Law and Economics* 45 (2): 401–35.

McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics*, 142(2), 698–714.

McEwan, Patrick J., and Joseph S. Shapiro. 2008. "The Benefits of Delayed Primary School Enrollment: Discontinuity Estimates Using Exact Birth Dates." *Journal of Human Resources* 43 (1): 1–29.

Margo, Robert A., and Finegan T. Aldrich. 1996. "Compulsory Schooling Legislation and School Attendance in Turn-of-the-Century America: A 'natural Experiment Approach." *Economics Letters* 53 (1): 103–10.

Meghir, Costas, and Mårten Palme. 2005. "Educational Reform, Ability, and Family Background." *American Economic Review* 95 (1): 414–24.

Montmarquette, Claude, Nathalie Viennot-Briot, and Marcel Dagenais. 2007. "Dropout, school performance, and working while in school." *The Review of Economics and Statistics*, 89(4), 752–60.

Mühlenweg, Andrea, and Patrick A. Puhani. 2010. "The evolution of the school-entry age effect in a school tracking system." *The Journal of Human Resources,* 45(2), 407–38.

Pischke, Jörn S., and Von T. Wachter. 2008. "Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation." *The Review of Economics and Statistics* 90 (3): 592–98.

Shah, Manisha, and Bryce M. Steinberg. 2017. "Drought of Opportunities: Contemporaneous and Long-Term Impacts of Rainfall Shocks on Human Capital." *Journal of Political Economy* 125(2): 527-561.

Shah, Manisha, and Bryce M. Steinberg. 2021. "Workfare and Human Capital Investment: Evidence from India." *Journal of Human Resources* 56(2): 380-405.

Staiger, Douglas, and James H. Stock. 1997. "Instrumental Variables Regressions with Weak Instruments." *Econometrica* 65(3): 557–86.

Stephens, Melvin Jr., and Dou-Yan Yang. 2014. "Compulsory Education and the Benefits of Schooling." *American Economic Review* 104 (6): 1777–1792.

Table 1. Summary statistics and balance test of background variables

	Young Cohorts N=70,758			Old Cohorts N=37,258		
	Mean	SD	Balance Test	Mean	SD	Balance Test
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Variables						
After cutoff	0.50	0.50		0.50	0.50	
Actual entrance age	6.44	0.43		6.33	0.49	
Background Variables						
Father's education (0–7 years)	0.023	0.149	-0.002	0.033	0.179	-0.004
Father's education (8–11 years)	0.198	0.399	0.005	0.294	0.456	-0.010
Father's education (12 years)	0.332	0.471	0.012	0.262	0.439	0.004
Father's education (13–16 years)	0.287	0.452	**-0.015**	0.262	0.440	**0.018**
Father's education (17–18 years)	0.093	0.291	0.002	0.070	0.255	-0.009
Father's education (19+ years)	0.066	0.249	-0.001	0.080	0.271	0.001
Mother's education (0–7 years)	0.023	0.150	0.000	0.035	0.184	-0.005
Mother's education (8–11 years)	0.156	0.363	0.003	0.279	0.448	0.011
Mother's education (12 years)	0.370	0.483	-0.008	0.270	0.444	-0.006
Mother's education (13–16 years)	0.293	0.455	0.007	0.289	0.453	-0.001
Mother's education (17–18 years)	0.124	0.330	-0.008	0.088	0.283	-0.001
Mother's education (19+ years)	0.033	0.180	**0.006**	0.040	0.196	0.002
Number of siblings (0)	0.060	0.238	-0.004	0.046	0.210	0.001
Number of siblings (1)	0.209	0.407	-0.008	0.191	0.393	0.008
Number of siblings (2)	0.349	0.477	0.010	0.333	0.471	0.011
Number of siblings (3–5)	0.314	0.464	0.006	0.345	0.475	-0.016
Number of siblings (6+)	0.068	0.252	-0.003	0.086	0.280	-0.003
Male	0.507	0.500	-0.006	0.493	0.500	0.000
Enrolled to a secular school	0.754	0.431	-0.002	0.808	0.394	-0.005
Father born in Asia	0.033	0.178	0.004	0.139	0.346	-0.001
Father born in Africa	0.099	0.299	-0.004	0.196	0.397	0.004
Father born in USSR	0.131	0.338	-0.003	0.114	0.318	0.012
Father born in Europe	0.033	0.178	0.002	0.102	0.303	0.007
Father born in NA/Australia	0.017	0.128	0.001	0.011	0.102	0.001
Father born in South America	0.014	0.118	0.002	0.017	0.129	-0.001
Father born in Israel	0.649	0.477	0.000	0.390	0.488	**-0.022**
Mother born in Asia	0.023	0.151	0.003	0.100	0.300	-0.001
Mother born in Africa	0.079	0.270	-0.003	0.180	0.384	-0.001
Mother born in USSR	0.151	0.358	-0.010	0.129	0.335	0.013
Mother born in Europe	0.034	0.182	-0.002	0.081	0.273	0.004
Mother born in NA/Australia	0.020	0.139	0.000	0.013	0.113	0.002
Mother born in South America	0.016	0.126	0.000	0.018	0.133	0.000
Mother born in Israel	0.676	0.468	0.012	0.469	0.499	-0.017
Student born in Asia	0.002	0.040	-0.001	0.003	0.051	0.000
Student born in Africa	0.008	0.090	0.000	0.009	0.097	-0.004
Student born in USSR	0.051	0.220	-0.003	0.089	0.284	0.010
Student born in Europe	0.008	0.091	0.001	0.013	0.111	**-0.005**
Student born in NA/Australia	0.013	0.112	0.002	0.010	0.098	0.004
Student born in South America	0.004	0.062	**0.002**	0.006	0.077	0.000
Student born in Israel	0.914	0.280	0.000	0.871	0.335	-0.005

Notes: Columns 1–2 and 4–5 report means and standard deviations for the young and old cohorts, respectively. Columns 3 and 6 report the slope from a regression of the background variable listed in the first column on our instrumental variable (an indicator for birth after the cutoff), controlling for date-of-year, day-of-week, and period fixed effects. Numbers in bold are distinguishable from zero at the 5% significance level.

Table 2. Summary statistics of key outcome variables

Variable	Period	Obs	Mean	SD
	(1)	(2)	(3)	(4)
Data Set A: Young Cohort				
Normalized 5th-grade math score	1–7	24,623	0.030	0.989
Normalized 5th-grade Hebrew score	1–7	24,424	0.031	0.990
Normalized 5th-grade science score	1–7	24,464	0.026	0.996
Normalized 5th-grade English score	1–7	24,189	0.036	0.985
Normalized 8th-grade math score	1–7	14,612	0.004	1.002
Normalized 8th-grade Hebrew score	1–7	14,713	0.009	0.997
Normalized 8th-grade science score	1–7	14,370	-0.003	1.003
Normalized 8th-grade English score	1–7	14,333	0.022	0.990
Retained between 1st and 3rd grades	1–7	70,758	0.027	0.163
Retained between 4th and 6th grades	1–7	70,758	0.020	0.139
Retained between 7th and 9th grades	1–7	70,758	0.022	0.146
Did not complete 10 years of schooling	1–7	70,758	0.026	0.159
Did not complete 11 years of schooling	1–7	70,758	0.046	0.209
Did not complete 12 years of schooling	1–7	70,758	0.075	0.264
Completed a matriculation diploma	1–7	70,758	0.675	0.468
Employed during 10th grade	1–7	70,758	0.116	0.320
Employed during 11th grade	1–7	70,758	0.252	0.434
Employed during 12th grade	1–7	70,758	0.455	0.498
Employed during high school	1–7	70,758	0.515	0.500
Months employed during high school	1–7	70,758	6.381	7.631
Real total earnings during high school	1–7	70,758	2.400	3.411
Data Set B: Old Cohort				
Applied for a psychometric exam	8–11	37,258	0.506	0.500
Started a first degree	8–11	37,258	0.583	0.493
Completed a first degree	8–11	37,258	0.451	0.498
Years of schooling	8–11	37,258	14.2	2.4
Psychometric verbal score	8–11	18,857	107.4	19.2
Psychometric math score	8–11	18,857	109.4	18.7
Psychometric English score	8–11	18,857	109.4	22.1
Psychometric total score	8–11	18,857	549.6	98.0
Employed between ages 28 and 32	8–11	37,258	0.867	0.340
Employed between ages 33 and 37	8–11	37,258	0.843	0.364
Real total earnings at ages 28 and 32	8–11	37,258	121.7	95.0
Real total earnings at ages 33 and 37	8–11	37,258	167.7	141.1

Notes: The reported means and standard deviations of the normalized scores in fifth and eighth grades differ from zero and one, respectively, as normalization was applied to the entire population that took the exam, not limited to those within the 28-day interval around the cutoff dates. Real earnings are reported in thousands of NIS.

Table 3. SEA effects on grade retention

	Sample Average	All (1)	Male (2)	Female (3)
Retained during grades 1–3	0.027	-0.055*** (0.010)	-0.075*** (0.016)	-0.039*** (0.010)
Observations		69,103	34,914	34,189
Retained during grades 4–6	0.020	-0.013 (0.008)	-0.025 (0.016)	-0.002 (0.007)
Observations		68,065	34,341	33,724
Retained during grades 7–9	0.022	-0.001 (0.006)	-0.005 (0.014)	0.002 (0.009)
Observations		66,933	33,464	33,469
Retained only during grades 4–6	0.012	-0.008 (0.007)	-0.019 (0.013)	0.000 (0.005)
Observations		67,171	33,812	33,359
Retained only during grades 7—9	0.012	0.005 (0.005)	0.016 (0.010)	-0.005 (0.006)
Observations		65,436	32,621	32,815
Retained during grades 4–6 among children not retained in grades 1–3	0.012	-0.009 (0.007)	-0.021 (0.014)	-0.000 (0.005)
Observations		65,718	32,952	32,766
Retained during grades 7–9 among children not retained in grades 1–6	0.012	0.004 (0.005)	0.015 (0.011)	-0.005 (0.006)
Observations		62,954	31,112	31,842

Notes: The table presents the effect of SEA on grade retention, with each entry derived from a separate regression. The row headers indicate the outcome variables, and the column headers specify the populations included in the regressions. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. The F-statistic on the excluded instrument ranges from 341 to 2,011. Standard errors, clustered by date of year, are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. SEA effects on 5th- and 8th-grade normalized test scores

	5th grade			8th grade		
	All	**Male**	**Female**	**All**	**Male**	**Female**
	(1)	(2)	(3)	(4)	(5)	(6)
Normalized math score	0.284***	0.252*	0.298***	0.361***	0.499**	0.303*
	(0.079)	(0.128)	(0.104)	(0.136)	(0.206)	(0.160)
F-stat on ex. inst.	840	314	603	282	208	169
Observations	24,623	12,141	12,482	14,612	6,923	7,689
Normalized science score	0.417***	0.487***	0.378***	0.051	0.051	0.062
	(0.091)	(0.178)	(0.094)	(0.104)	(0.191)	(0.110)
F-stat on ex. inst.	643	215	432	265	144	158
Observations	24,464	12,018	12,446	14,371	6,961	7,409
Normalized English score	0.361***	0.403**	0.336***	0.206*	0.375*	0.120
	(0.074)	(0.162)	(0.099)	(0.112)	(0.196)	(0.121)
F-stat on ex. inst.	620	199	562	249	139	162
Observations	24,189	11,845	12,344	14,334	6,923	7,410
Normalized Hebrew score	0.349***	0.412**	0.310***	0.200	0.273	0.150
	(0.087)	(0.165)	(0.089)	(0.128)	(0.250)	(0.124)
F-stat on ex. inst.	923	315	640	260	203	162
Observations	24,424	11,969	12,455	14,714	7,013	7,700
Above-average scores across all subjects	0.196***	0.223***	0.177***	0.106**	0.162*	0.071
	(0.041)	(0.065)	(0.054)	(0.052)	(0.086)	(0.056)
F-stat on ex. inst.	967	312	681	345	263	226
Observations	32,505	16,086	16,419	26,012	12,661	13,351

Notes: The table presents the effect of SEA on fifth- and eighth-grade test scores across different subjects, with each entry derived from a separate regression. The row headers indicate the exam subjects, and the column headers specify the populations included in the regressions. Columns 1–3 and 4–6 report the effect on fifth- and eighth-grade test scores, respectively. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. "F-stat ex. inst." refers to the F-statistic on the excluded instrument. Standard errors, clustered by date of year, are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. SEA effects on youth employment

	Sample Average	All (1)	Male (2)	Female (3)
Panel A: SEA effects on the likelihood and duration of employment by grade				
Employed during 10th grade	0.12	0.089*** (0.019)	0.091*** (0.031)	0.088*** (0.020)
Months employed during 10th grade	0.88	0.627*** (0.114)	0.589*** (0.194)	0.653*** (0.118)
Employed during 11th grade	0.25	0.115*** (0.024)	0.131*** (0.032)	0.102*** (0.029)
Months employed during 11th grade	1.88	0.927*** (0.168)	1.011*** (0.223)	0.858*** (0.200)
Employed during 12th grade	0.45	0.119*** (0.029)	0.138*** (0.042)	0.102*** (0.037)
Months employed during 12th grade	3.32	0.811*** (0.184)	0.910*** (0.289)	0.714*** (0.223)
Employed during high school	0.51	0.160*** (0.030)	0.193*** (0.043)	0.133*** (0.035)
Months employed during high school	6.38	2.582*** (0.390)	2.648*** (0.554)	2.506*** (0.481)
Panel B: SEA effects on the likelihood and duration of employment in 10th grade, by first and second halves				
Employed in the 1st half of 10th grade (Sept.–Jan)	0.054	0.032** (0.013)	0.020 (0.021)	0.041*** (0.015)
Employed in the 2nd half of 10th grade (Feb.–June)	0.089	0.074*** (0.016)	0.088*** (0.027)	0.063*** (0.017)
Months employed during the 1st half of 10th grade (Sept. –Jan.)	0.327	0.223*** (0.060)	0.176* (0.095)	0.260*** (0.062)
Months employed during the 2nd half of 10th grade (Feb.–June)	0.549	0.403*** (0.063)	0.413*** (0.114)	0.393*** (0.071)
F-stat on ex. instrument		1,923	642	1,193
Observations		70,758	35,856	34,902

Notes: The table shows the effects on various outcome variables, with each entry derived from a separate regression. Row headers indicate the outcome variables, while column headers specify the populations included in the regressions. Panel A presents SEA effects on employment likelihood and duration by grade, whereas Panel B focuses specifically on 10th grade, displaying results by each half of the grade. An indicator for birth after the entrance cutoff serves as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. "F-stat on ex. instrument" denotes the F-statistic for the excluded instrument. Standard errors clustered by date of year are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. SEA effects on high school dropout

	Sample Avg	All	Female	Males All	Males SEA<7
		(1)	(2)	(3)	(4)
Panel A: Reduced form					
Did not complete 10th grade	0.026	0.003	0.000	0.006*	0.005
		(0.002)	(0.003)	(0.003)	(0.003)
Did not complete 11th grade	0.046	0.007**	0.001	0.013***	0.012**
		(0.003)	(0.004)	(0.005)	(0.005)
Did not complete 12th grade	0.075	0.011**	0.008	0.016**	0.013**
		(0.005)	(0.005)	(0.007)	(0.007)
Did not complete a diploma	0.325	0.004	-0.001	0.010	0.007
		(0.006)	(0.008)	(0.010)	(0.010)
Panel B: TSLS					
Did not complete 10th grade	0.026	0.009	0.000	0.023*	0.021
		(0.008)	(0.009)	(0.013)	(0.014)
Did not complete 11th grade	0.046	0.024**	0.003	0.053***	0.050**
		(0.011)	(0.012)	(0.020)	(0.020)
Did not complete 12th grade	0.075	0.040**	0.023	0.066**	0.057**
		(0.016)	(0.015)	(0.028)	(0.028)
Did not complete a diploma	0.325	0.016	-0.002	0.039	0.028
		(0.023)	(0.026)	(0.041)	(0.042)
F-stat on excluded instrument		1,923	1,193	642	1,217
Observations		70,758	34,902	35,856	35,629

Notes: Panels A and B present reduced-form and TSLS estimates, respectively. The table shows the effects on various outcome variables, with each entry derived from a separate regression. The row headers indicate the outcome variables, and the column headers specify the populations included in the regressions. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. Standard errors, clustered by date of year, are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7. Assessing the impact of Amendment 29 on the SEA effect on dropout (males only)

	Did not complete 10th grade	Did not complete 11th grade	Did not complete 12th grade	Did not complete Matriculation
	(1)	(2)	(3)	(4)
SEA	0.025*	0.059***	0.073**	0.047
	(0.013)	(0.019)	(0.028)	(0.039)
SEA × NewLaw	0.012	0.009	0.010	0.006
	(0.011)	(0.016)	(0.022)	(0.033)
NewLaw	-0.146**	-0.135	-0.155	-0.120
	(0.069)	(0.102)	(0.145)	(0.216)
F-stat on excluded instruments	376	376	376	376
Observations	35,856	35,856	35,856	35,856

Notes: Each column presents results for a different outcome variable, as specified by the column header. SEA is instrumented using an indicator for birth after the entrance cutoff. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. Standard errors, clustered by date of year, are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8. Decomposition of the SEA effects on high school outcomes by youth employment status

	Dropped out during 10th–11th grades			Dropped out during 11th–12th grades				
		and worked 10th grade	and didn't work 10th grade		and worked 11th grade	and didn't work 11th grade	Observations	F-stat on ex. inst.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Entire sample	0.016* (0.009)	0.011*** (0.004)	0.005 (0.008)	0.031** (0.015)	0.024*** (0.021)	0.007 (0.012)	70,758	1923
Male	0.031* (0.018)	0.025*** (0.008)	0.006 (0.016)	0.043* (0.025)	0.042*** (0.014)	0.001 (0.018)	35,856	642
Female	0.005 (0.010)	0.001 (0.003)	0.004 (0.009)	0.023 (0.015)	0.012*** (0.005)	0.011 (0.013)	34,902	1193

Notes: This table presents the effect of SEA on high school dropout, with each entry derived from a separate regression. Column 1 reports the overall effect of SEA on dropping out during 10th–11th grades. Columns 2 and 3 decompose this overall effect into the effects on the combined outcomes of "worked in 10th grade & dropped out during 10th–11th grades" (column 2) and "did not work in 10th grade & dropped out during 10th–11th grades" (column 3). The coefficients in columns 2 and 3 sum to the coefficient in column 1. Similarly, column 4 reports the overall effect of SEA on dropping out during 11th–12th grades, while columns 5 and 6 break down this effect into the combined outcomes of "worked in 11th grade & dropped out during 11th–12th grades" (column 5) and "did not work in 11th grade & dropped out during 11th–12th grades" (column 6). The row headers indicate the populations included in each regression. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All regressions include the full set of control variables listed in Table 1, along with fixed effects for date of year, day of week, and period. "F-stat ex. inst." refers to the F-statistic on the excluded instrument. Standard errors clustered by date of year are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9. Heterogeneous SEA effects on medium- and long-term educational outcomes among male students

	Panel A: Medium-term educational outcomes				Panel B: Long-term educational outcomes			
	Didn't complete 12th grade	**Didn't complete a matriculation diploma**	**Employed during high school**	**Employed and didn't complete 12th grade**	**Applied for a psychometric exam**	**Psycho score in top quartile**	**Started a first degree**	**Years of schooling**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Parent's average education (PE)							
SEA	0.093***	0.065	0.252***	0.081***	-0.199***	-0.141***	-0.218***	-0.892***
	(0.033)	(0.046)	(0.048)	(0.025)	(0.061)	(0.035)	(0.062)	(0.250)
PE × SEA	-0.002**	-0.002**	-0.005***	-0.003***	0.004***	0.003***	0.006***	0.034***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.007)
	Number of siblings (SI)							
SEA	0.060**	0.021	0.197***	0.042*	-0.126*	-0.096**	-0.126**	-0.377
	(0.029)	(0.041)	(0.044)	(0.021)	(0.066)	(0.041)	(0.060)	(0.252)
SI × SEA	0.003*	0.009***	-0.002	0.004***	-0.003**	-0.002*	-0.003*	-0.013
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.008)
Obs.	35,856	35,856	35,856	35,856	18,381	18,381	18,377	18,381

Notes: Each column reports the effect of SEA on a different outcome variable. Panel A presents heterogeneous effects with respect to medium-term educational outcomes, and Panel B with respect to long-term outcomes. Within each panel, the top part of the table reports heterogeneous effects with respect to average parental education, while the bottom part reports heterogeneous effects with respect to the number of siblings. In these estimations, we use two instruments—an indicator for birth after the entrance cutoff and its interaction with the heterogeneous variable—to instrument for the two endogenous variables: SEA and its interaction with the heterogeneous variable. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. The F-statistic on the excluded instrument ranges from 184 to 322. Standard errors, clustered by date of year, are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10. SEA effect's decomposition by achievements in elementary schooling

	SEA effect among Meitzav-tested students	Above-average 5th-grade test scores	Below-average 5th-grade test scores	Overall SEA effect	Didn't repeat a grade before 3rd grade	Repeated a grade before 3rd grade
	(1)	(2)	(3)	(4)	(5)	(6)
Did not complete 12th grade	0.027 (0.017)	0.024** (0.011)	0.003 (0.013)	0.047*** (0.016)	0.047*** (0.014)	-0.001 (0.004)
Did not complete a matriculation diploma	0.001 (0.034)	0.093*** (0.025)	-0.092*** (0.027)	0.022 (0.022)	0.048** (0.022)	-0.025*** (0.008)
Employed during high school	0.181*** (0.037)	0.189*** (0.034)	-0.008 (0.032)	0.156*** (0.033)	0.173*** (0.031)	-0.017** (0.007)
Employed during high school & didn't complete 12th grade	0.022 (0.015)	0.017** (0.007)	0.005 (0.012)	0.029** (0.011)	0.030*** (0.010)	-0.001 (0.003)
Employed during high school & didn't achieve a diploma	0.042 (0.032)	0.061*** (0.018)	-0.018 (0.024)	0.057*** (0.020)	0.067*** (0.020)	-0.010* (0.006)
F-stat on ex. inst.	967	967	967	2022	2022	2022
Observations	32,505	32,505	32,505	69,103	69,103	69,103

Notes: Each entry is derived from a separate regression. Column 1 reports the overall SEA effect on the outcome variables specified in the row headers, among children who took the 5th-grade exam. Columns 2 and 3 decompose this overall SEA effect by whether the student's 5th-grade test scores were above or below average. The coefficients in columns 2 and 3 sum to the coefficient in column 1. For example, in the first row, column 1 reports the overall effect of SEA on not completing 12th grade, and columns 2 and 3 break down this effect into the effects on the combined outcomes of "Above-average 5th-grade test scores & Did not complete 12th grade" (column 2) and "Below-average 5th-grade test scores & Did not complete 12th grade" (column 3). Similarly, column 4 reports the overall SEA effect for students whose grade-retention status is observed. Columns 5 and 6 decompose this effect into the effects on the combined outcomes of "Didn't repeat a grade before 3rd grade & Did not complete 12th grade" (column 5) and "Repeated a grade before 3rd grade & Did not complete 12th grade" (column 6). An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. "F-stat ex. inst." refers to the F-statistic on the excluded instrument. Standard errors, clustered by date of year, are shown in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 11. SEA's effects on long-term outcomes

	Obs	Sample Average	All	Male	Female
			(1)	(2)	(3)
Panel A: Higher education outcomes					
Applied for a psychometric exam	37,258	0.506	-0.079** (0.037)	-0.135** (0.066)	-0.031 (0.050)
Age at psychometric exam	18,857	20.4	0.240 (0.323)	0.170 (0.716)	0.321 (0.259)
Started a first degree	37,258	0.58	-0.106** (0.042)	-0.133** (0.060)	-0.080 (0.058)
Age starting a first degree	21,732	24.3	-0.131 (0.367)	0.231 (0.587)	-0.282 (0.411)
Completed a first degree	37,258	0.45	-0.056 (0.042)	-0.056 (0.067)	-0.061 (0.045)
Age completing a first degree	16,821	28.14	0.166 (0.413)	0.802 (0.804)	-0.227 (0.475)
Years of schooling	37,258	14.17	-0.474*** (0.157)	-0.411 (0.250)	-0.532** (0.241)
Panel B: Employment and earnings					
Employed during ages 27–32	37,258	0.867	0.039 (0.029)	0.070 (0.045)	0.008 (0.037)
Employed during ages 32–37	37,258	0.843	0.002 (0.032)	0.007 (0.045)	-0.004 (0.048)
Annual real wage earnings during ages 27–32	37,258	122.0	-6.697 (7.867)	-9.342 (11.005)	-5.397 (10.889)
Annual real wage earnings during ages 32–37	37,258	167.8	-10.034 (10.828)	-15.614 (19.724)	-6.746 (14.070)

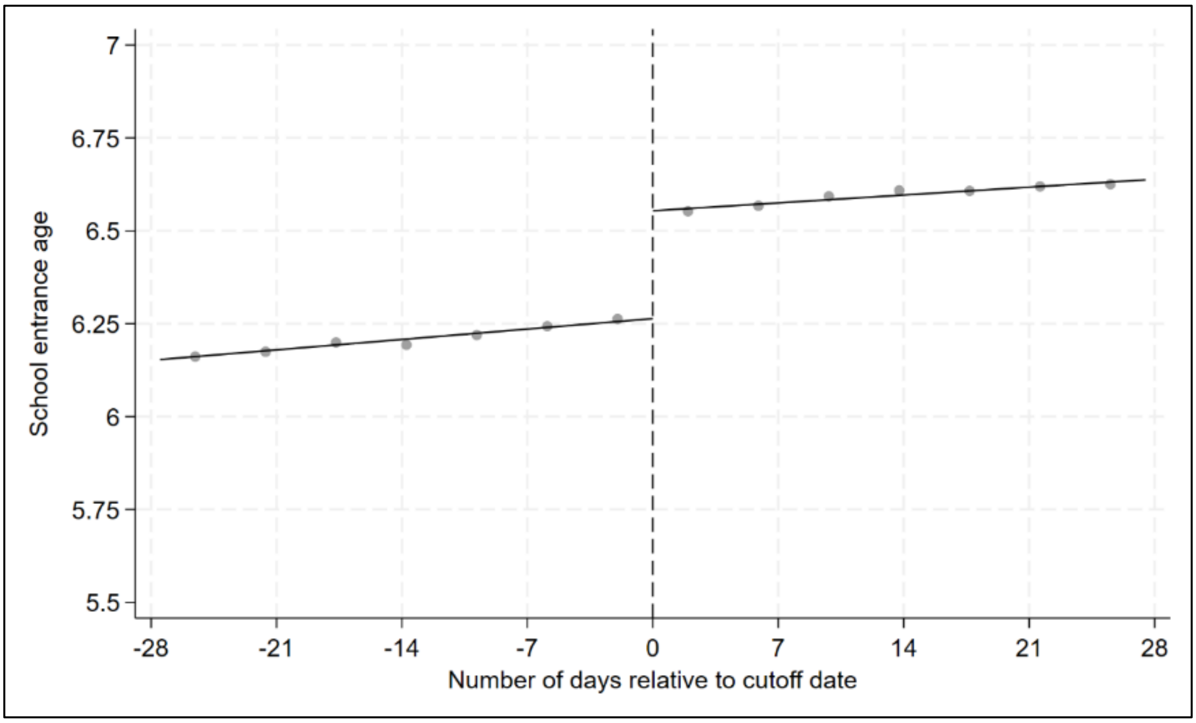
Notes: The table presents the effects of SEA on various outcome variables, with each entry in the table derived from a separate regression. Panel A presents the effects on higher education outcomes, while Panel B focuses on employment and earnings. The row headers indicate the outcome variables, and the column headers specify the populations included in the regressions. Earnings are measured in thousands of shekels in real terms. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. The F-statistic on the excluded instrument ranges from 132 to 627. Standard errors, clustered by date of year, are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 12. SEA estimates on outcomes in a psychometric exam

	Total score		Quantitative score		Verbal score		English score	
	q2>479, q3>553, q4>626		q2>96, q3>110, q4>124		q2>94, q3>108, q4>122		q2>92, q3>111, q4>128	
	All	Test takers	All	Test takers	All	Test takers	All	Test takers
Panel A: Males								
Score above bottom quartile	-0.095 (0.066)	0.048 (0.063)	-0.108 (0.067)	0.024 (0.068)	-0.146** (0.068)	-0.067 (0.082)	-0.122** (0.059)	-0.021 (0.058)
Score above median	-0.132** (0.055)	-0.080 (0.079)	-0.127** (0.059)	-0.070 (0.101)	-0.080 (0.051)	0.009 (0.084)	-0.077 (0.055)	0.014 (0.080)
Score in top quartile	-0.100** (0.040)	-0.088 (0.076)	-0.097** (0.039)	-0.076 (0.074)	-0.098** (0.038)	-0.095 (0.072)	-0.025 (0.043)	0.053 (0.081)
Score (for takers)		-8.026 (14.849)		-1.702 (3.129)		-2.150 (2.924)		0.666 (3.725)
Observations	18,381	8,325	18,381	8,325	18,381	8,325	18,381	8,325
Panel B: Females								
Score above bottom quartile	0.042 (0.045)	0.101* (0.059)	0.009 (0.055)	0.049 (0.076)	0.038 (0.043)	0.088 (0.055)	0.051 (0.037)	0.112 (0.070)
Score above median	0.051 (0.040)	0.097 (0.068)	0.013 (0.038)	0.034 (0.063)	0.034 (0.046)	0.063 (0.075)	-0.002 (0.049)	0.008 (0.078)
Score in top quartile	-0.007 (0.027)	-0.018 (0.046)	0.014 (0.029)	0.025 (0.046)	0.015 (0.029)	0.017 (0.052)	-0.003 (0.028)	-0.003 (0.048)
Score (for takers)		12.075 (10.201)		1.369 (2.312)		2.356 (1.848)		3.261 (2.621)
Observations	18,877	10,532	18,877	10,532	18,877	10,532	18,877	10,532

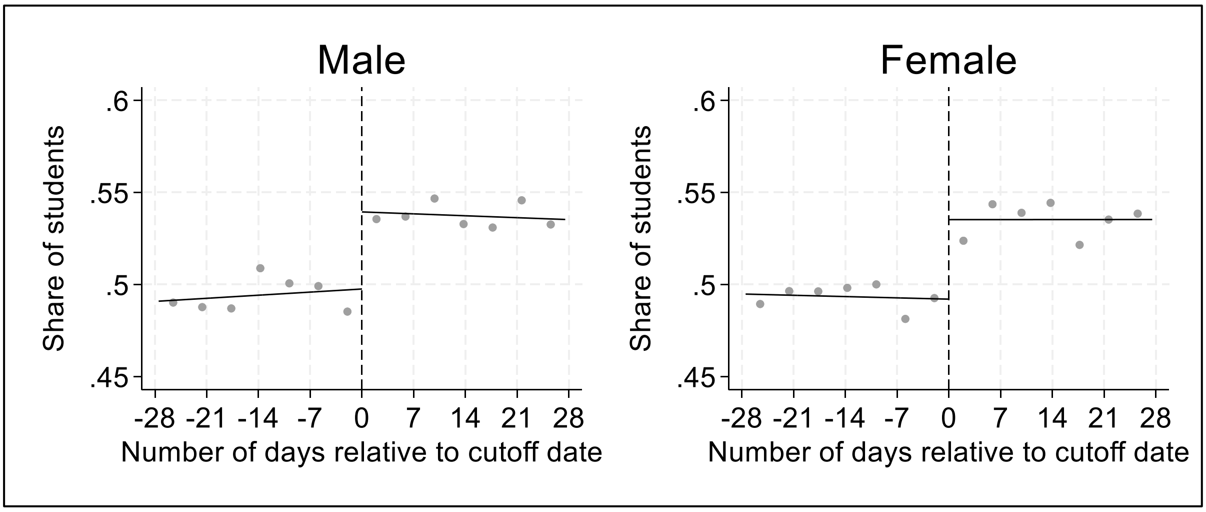
Notes: The table presents TSLS estimates of the effects of SEA on test scores, using both the entire sample and a sample of only those who took the exam. Each entry in the table is derived from a separate regression. For the entire population, outcomes for scoring above a particular quartile are coded as zero for students who did not take the exam. The quartile thresholds, reported in the second row, are calculated only among test takers who took the exam for the first time, ensuring a consistent threshold for comparison between the two samples. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. The F-statistic on the excluded instrument ranges from 177 to 381. Standard errors, clustered by date of year, are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1. School entrance age by days relative to cutoff



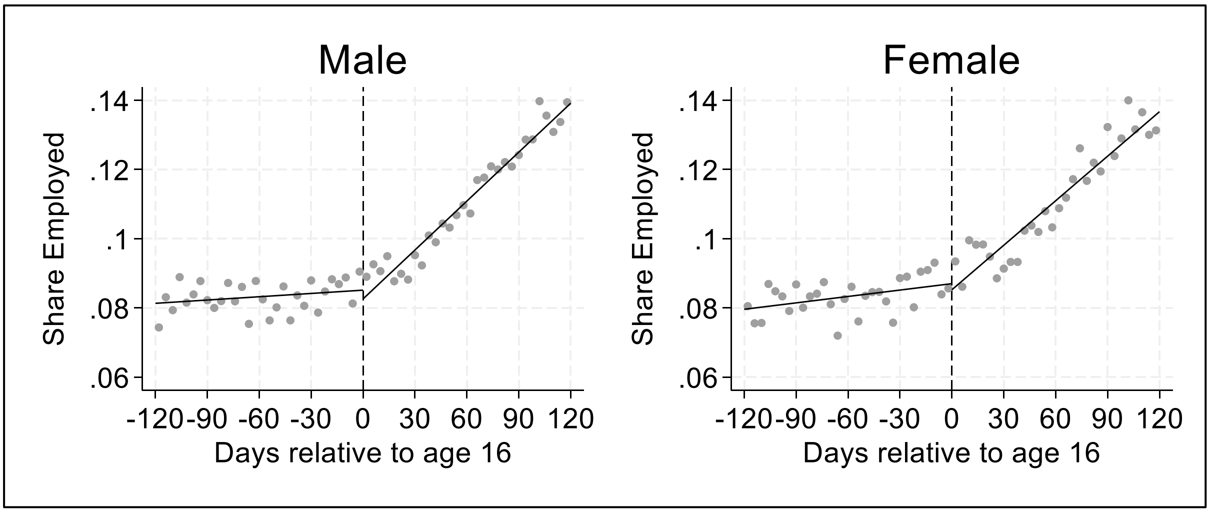
Notes: Dots represent four-day averages of the school entrance age, plotted against the number of days relative to the cutoff date. Solid lines on either side depict fitted values from a piecewise linear specification of the running variable.

Figure 2. Employment rates during high school by days relative to cutoff



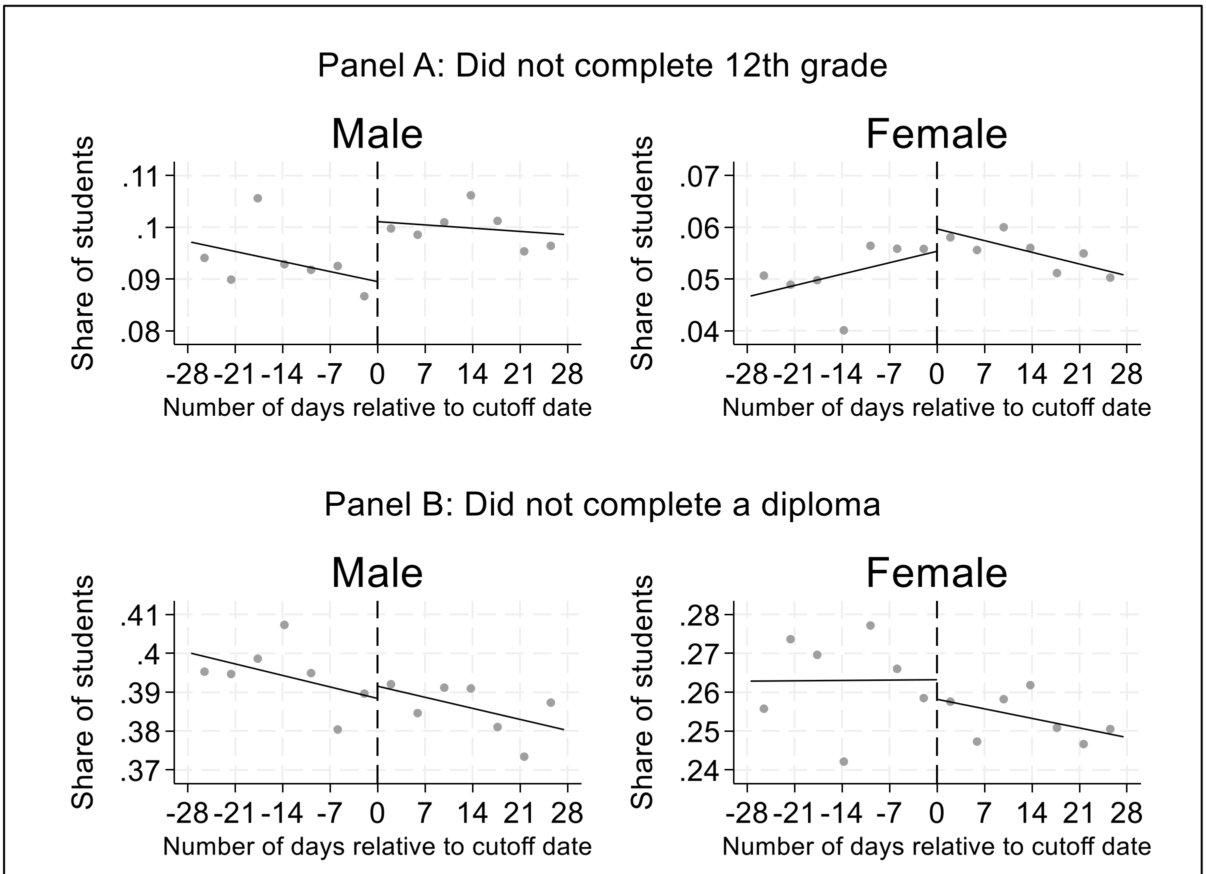
Notes: Dots represent four-day averages of the share of children employed, plotted against the number of days relative to the cutoff date. Solid lines on either side depict fitted values from a piecewise linear specification of the running variable.

Figure 3. Employment rates by days relative to the minimum legal working age (16)



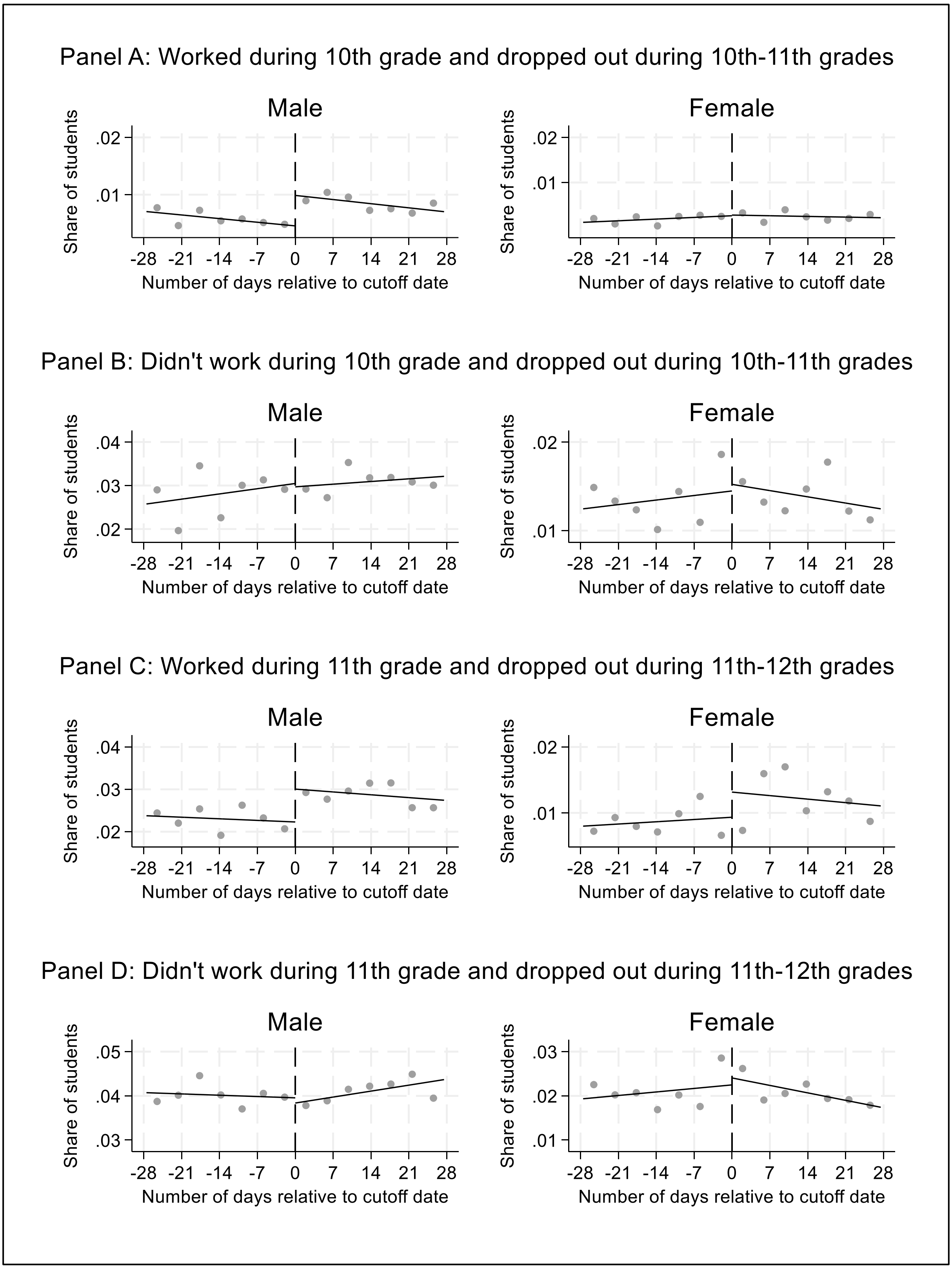
Notes: Dots represent four-day averages of the share of children employed, plotted against the number of days relative to the minimum legal working age of 16. A child's age in any given month is based on their age on the first day of the month. Solid lines on either side depict fitted values from a piecewise linear specification of the running variable.

Figure 4. High school outcomes by days relative to cutoff



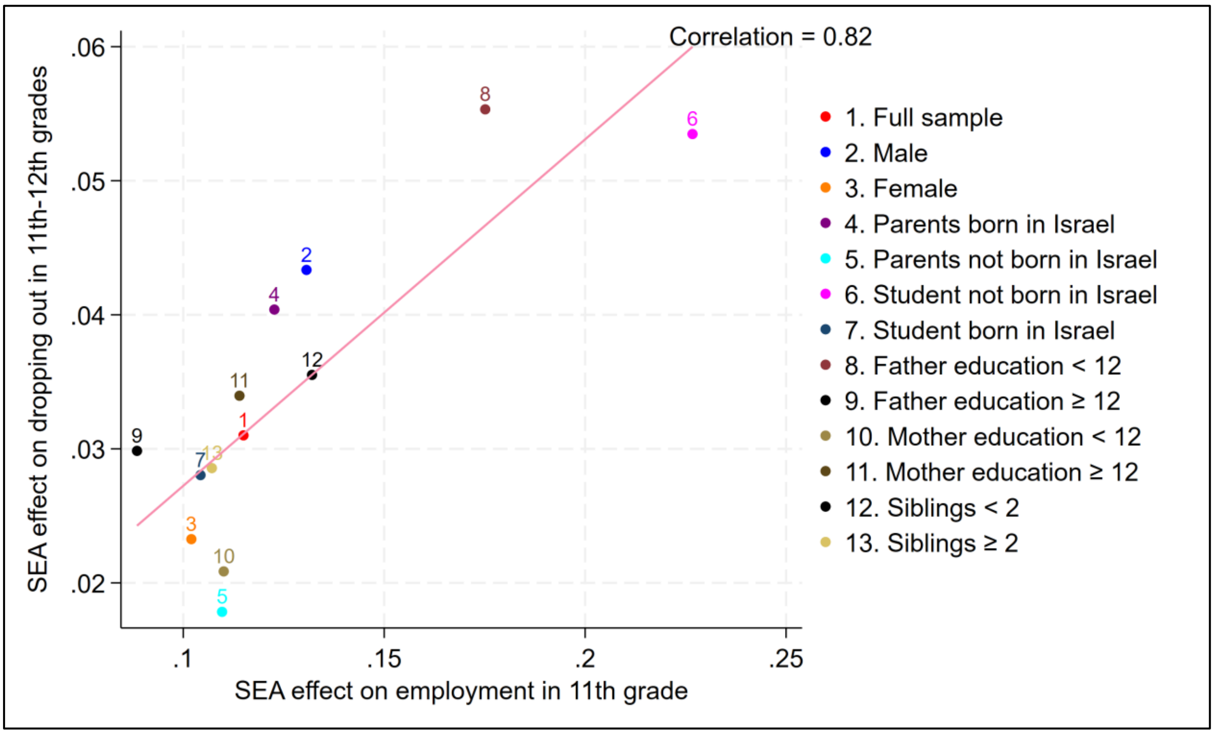
Notes: Each panel plots a different outcome variable against the number of days relative to the cutoff date. Dots represent four-day averages of the outcome variable. Solid lines on either side depict fitted values from a piecewise linear specification of the running variable.

Figure 5. Decomposition of the SEA effect on dropout by youth employment status



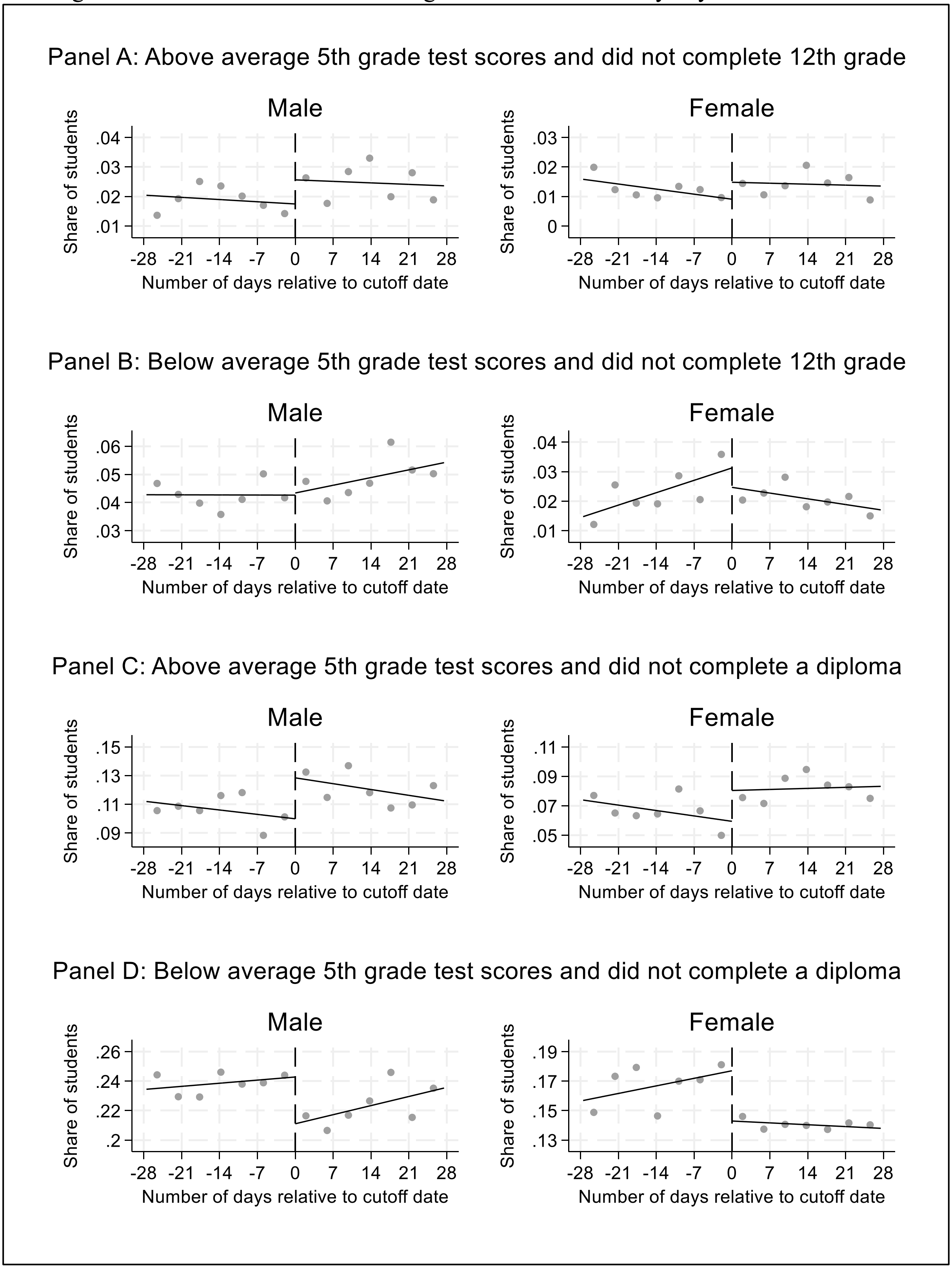
Notes: Each panel plots an outcome variable on the y-axis against the number of days relative to the cutoff date. Dots represent four-day averages of the outcome variable. Solid lines depict fitted values from a piecewise linear specification of the running variable.

Figure 6. Effects of SEA on employment and dropout by subgroups



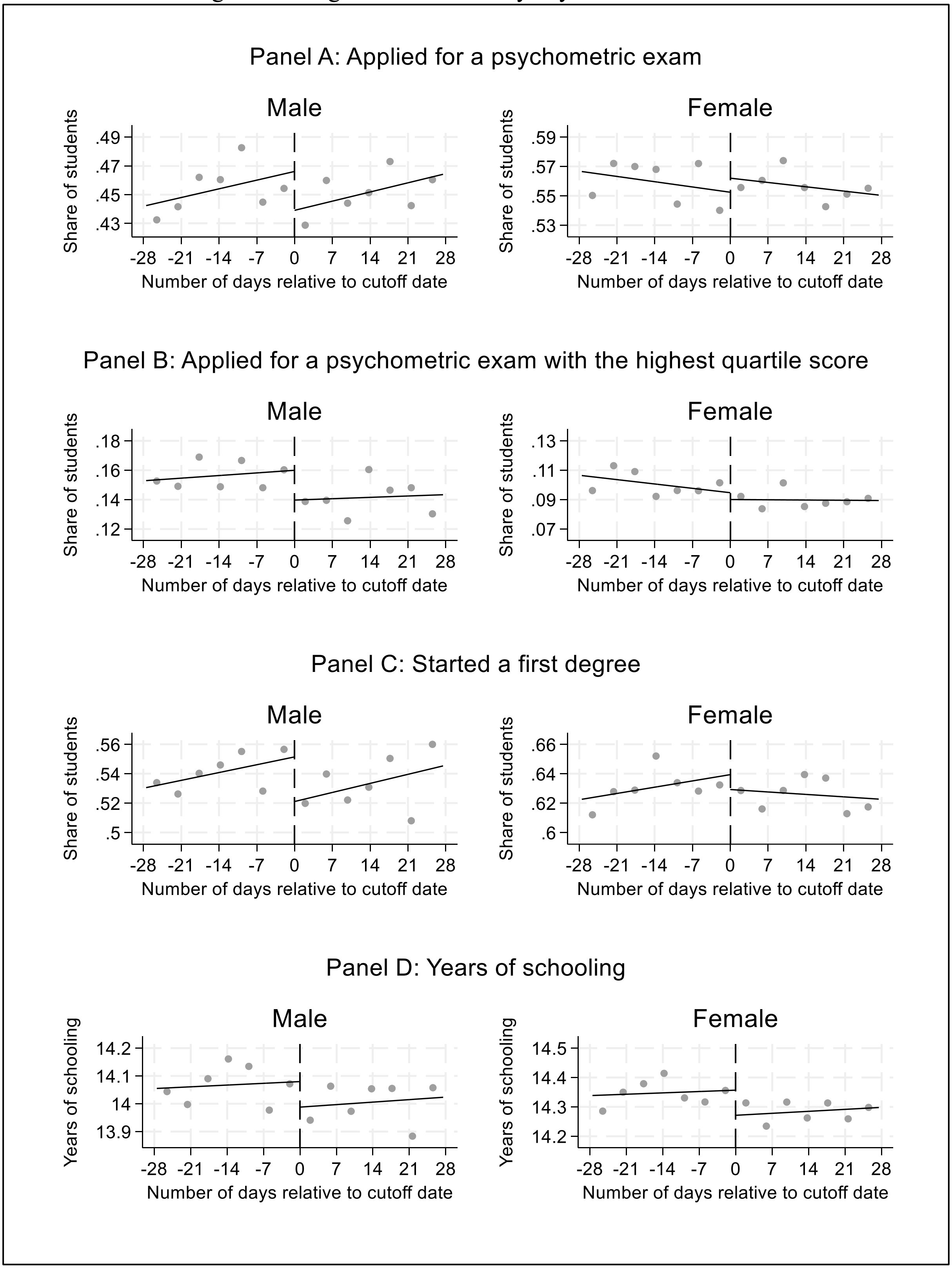
Notes: The figure illustrates the relationship between the effect of SEA on employment during 11th grade and its effect on dropping out in 11th–12th grades. Each point represents a subsample, with the x-axis showing the effect of SEA on employment and the y-axis showing its effect on dropout.

Figure 7. Short-term benefits and long-run reversal effects by days relative to cutoff



Notes: Each panel plots a different outcome variable on the y-axis against the number of days relative to the cutoff date. Dots represent four-day averages of the outcome variable. Solid lines depict fitted values from a piecewise linear specification of the running variable.

Figure 8. Long-term outcomes by days relative to cutoff



Notes: Each panel plots a different long-term outcome variable on the y-axis against the number of days relative to the cutoff date. Dots represent four-day averages of the outcome variable. Solid lines represent fitted values from a piecewise linear specification of the running variable.

Online Appendixes: Why Does Starting School Older Harm Schooling? The Role of Youth Employment Laws

Itay Attar and Danny Cohen-Zada

February 2025

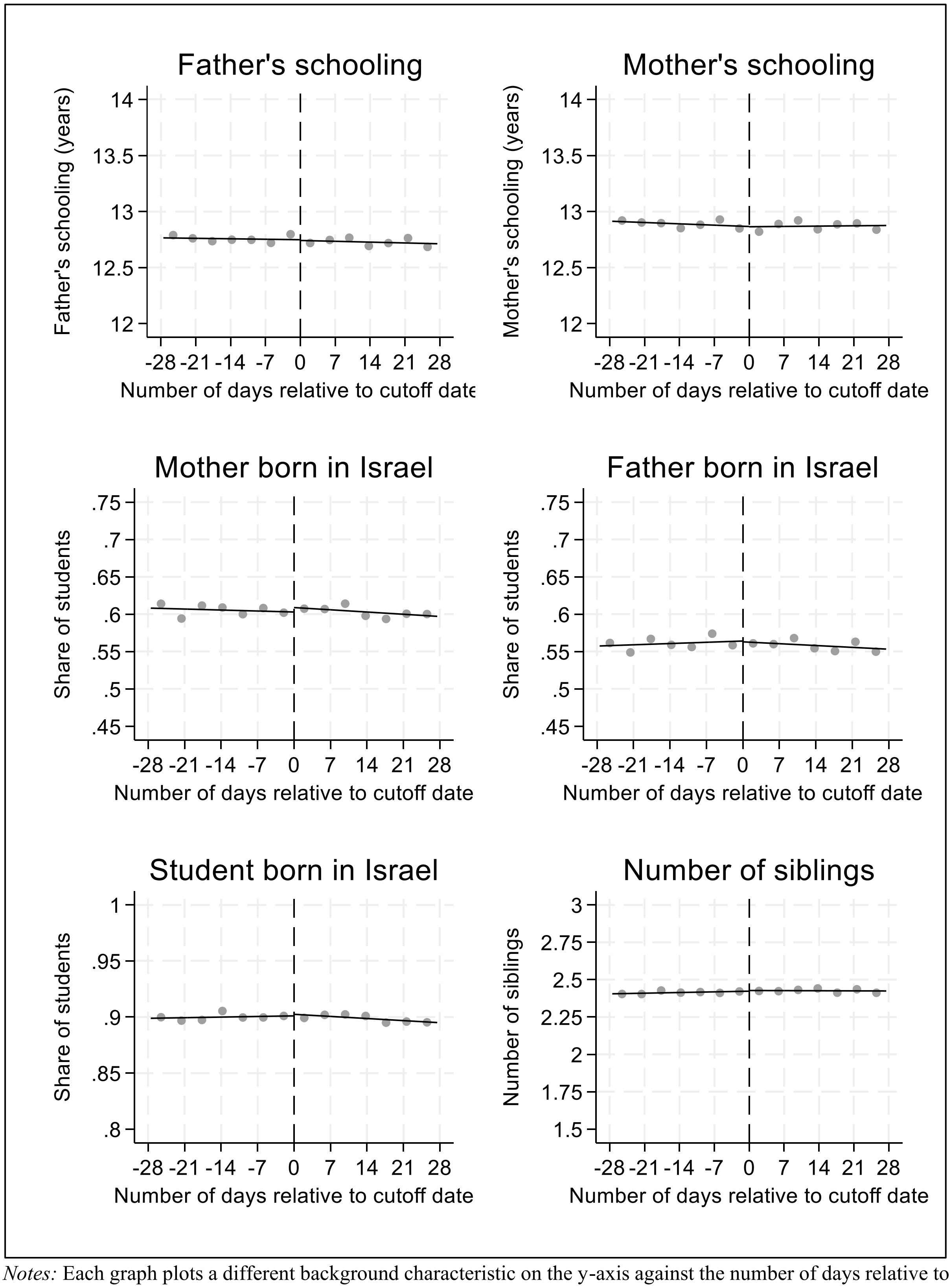
Appendix A: The Jewish Calendar

In this appendix, we present the results of two surveys that demonstrate that Israeli parents do not generally use the Jewish calendar in their everyday lives and are not even aware that the entrance cutoff date is set according to it. Nowadays, its use is primarily limited to determining the dates of Jewish holidays, while its use for civil purposes is quite minimal.

One survey was conducted by Sekernet (a marketing-survey organization) among a representative sample of 200 parents with children in school. The second survey was conducted among 159 economics students at Ben-Gurion University. In the first survey, when asked “What is the date today?” or “What is the birth date of your firstborn child?” all the respondents answered with the Gregorian date. Only 4% of the respondents generally scheduled their meetings according to the Jewish calendar, compared to 85% who used the Gregorian one (the rest use both calendars). To directly assess whether respondents know the Hebrew school-entrance cutoff date, we asked, “What was the school entrance cutoff date of your firstborn child?” No respondent provided the correct Jewish cutoff date, and only 5% provided a mistaken Jewish date, while 61% provided a Gregorian date (20% of which reported December 31, thinking the school-entrance cutoff date would naturally be at the end of the year) and 34% stated that they do not remember. In the second survey, only 52% of the students knew their own Jewish birth date, 6% knew their mother’s Jewish birth date, and 7% knew their father’s Jewish birth date. In contrast, 100% knew their own Gregorian birth date and 95% knew each of their parents’ Gregorian birth date. Taken together, the results of the two surveys clearly indicate that most people primarily use the Gregorian calendar for civil purposes. Furthermore, most parents do not even know what the school-entrance cutoff date is. Therefore, it is very unlikely that parents would manipulate their children’s Jewish birth date in order to influence the school entrance age or for any other motivation.

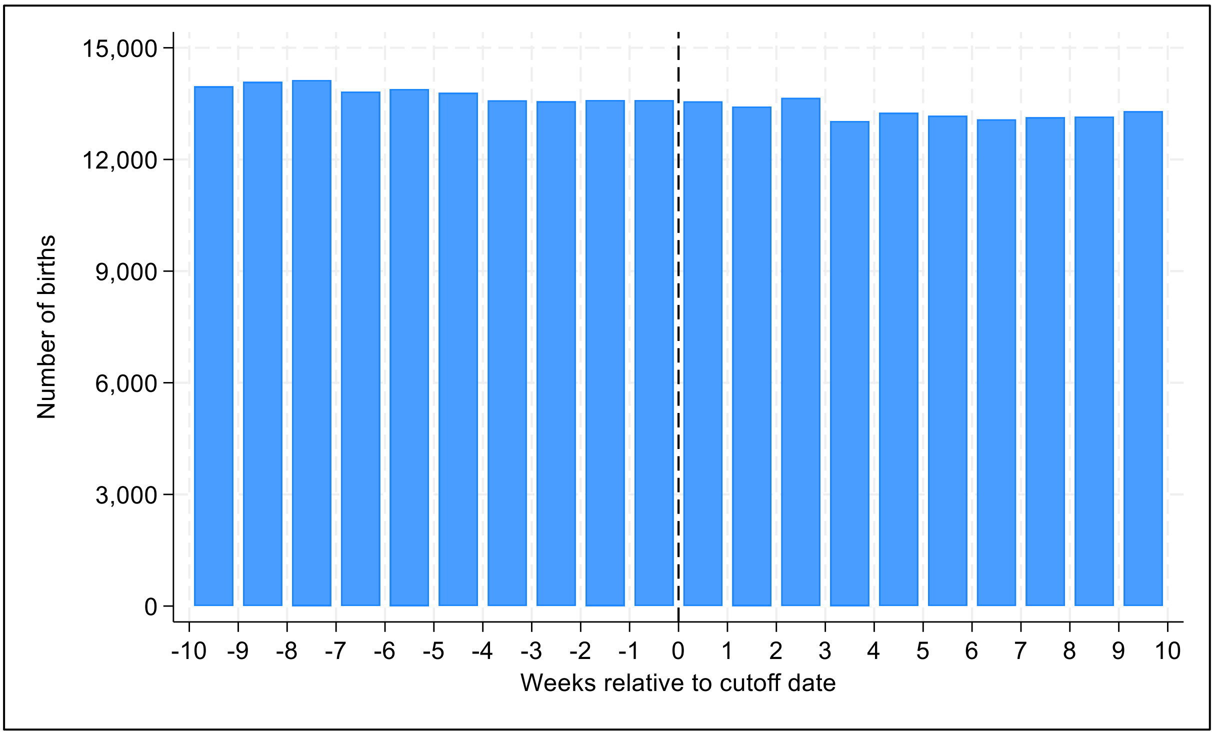
Appendix B: Additional Figures and Tables

Figure B1. Testing smoothness of central background characteristics at the entrance cutoff date



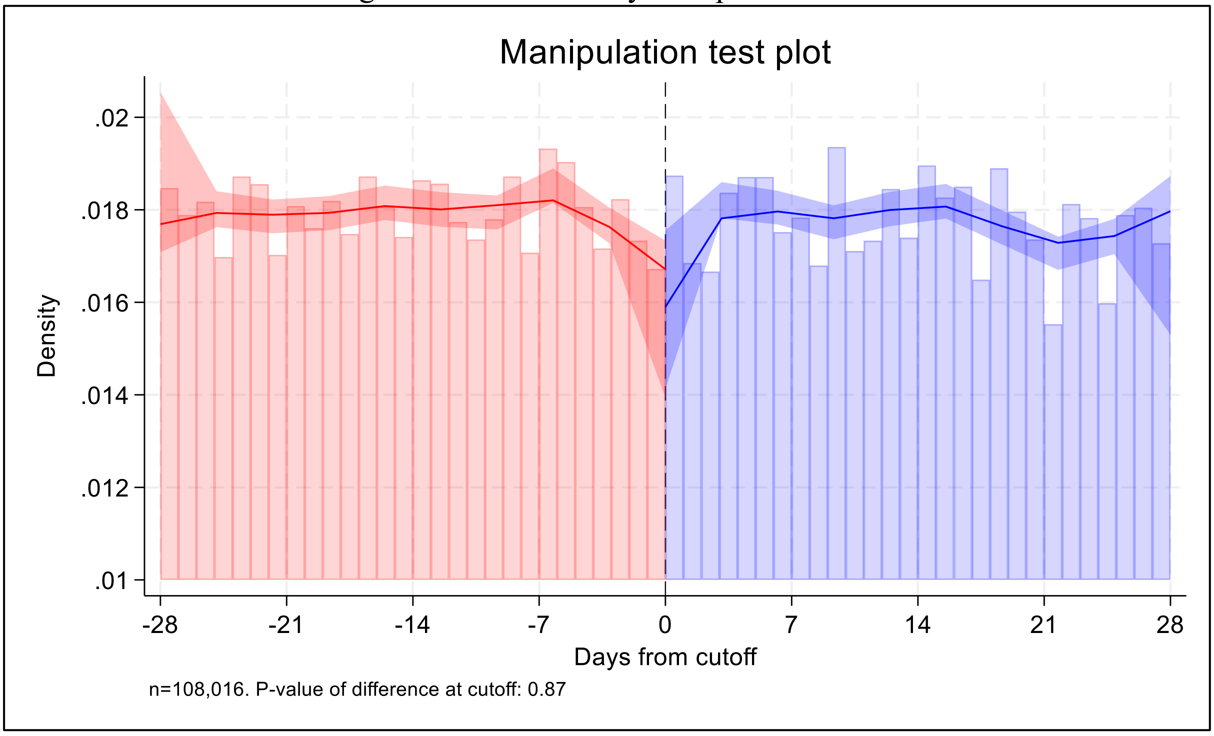
Notes: Each graph plots a different background characteristic on the y-axis against the number of days relative to the cutoff date. Dots represent four-day averages of the variable on the y-axis. Solid lines on either side depict fitted values from a piecewise linear specification of the running variable.

Figure B2. Distribution of births by weeks relative to the cutoff date



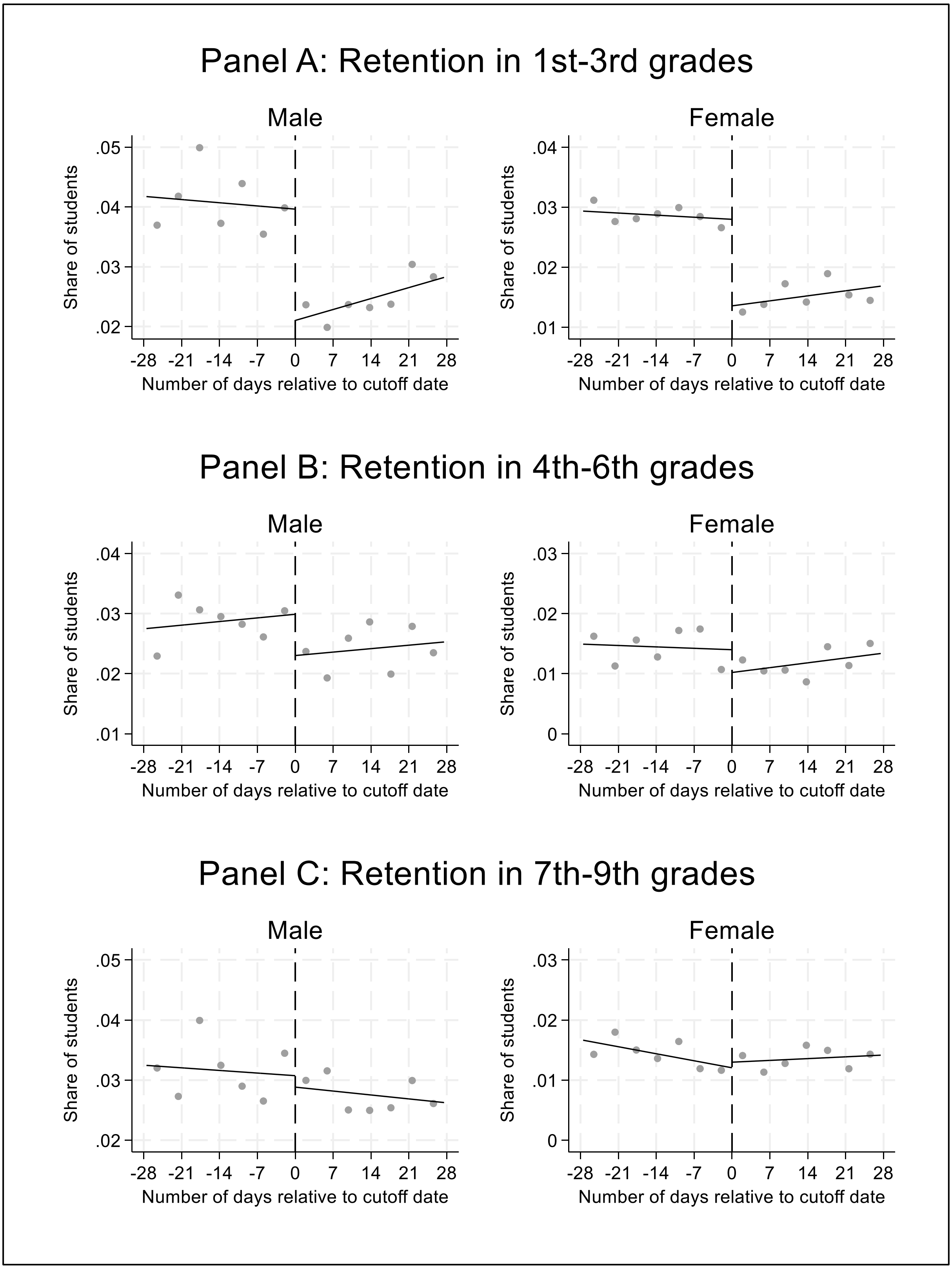
Notes: The figure plots the number of births by weeks relative to the cutoff date. Each bar represents a week.

Figure B3. Birth density manipulation test



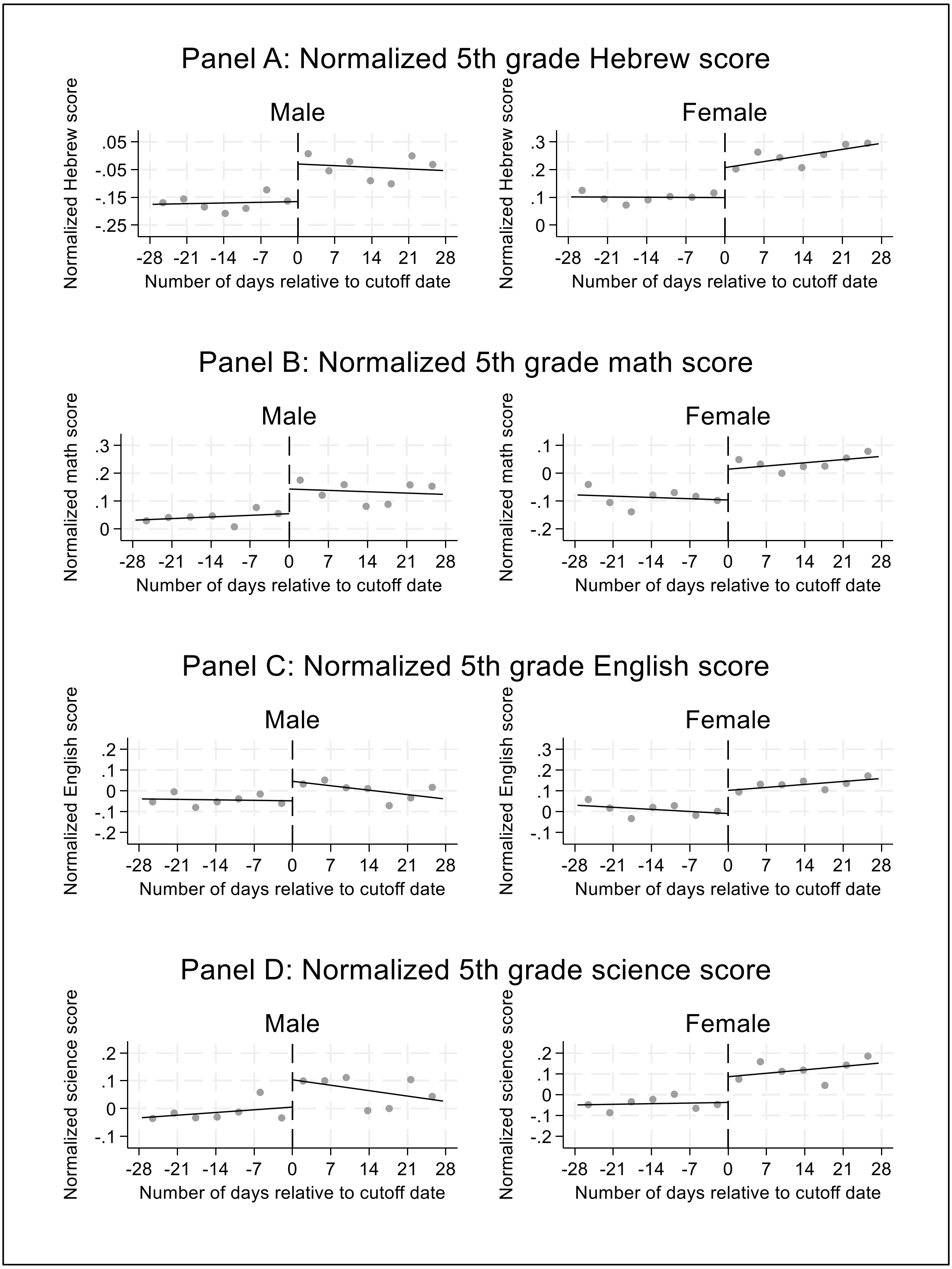
Notes: The figure plots the estimated density of the number of births by days relative to the cutoff date. Each bar represents a day. The vertical solid line represents the cutoff date for school entry.

Figure B4. Grade retention between first and ninth grades by days relative to cutoff



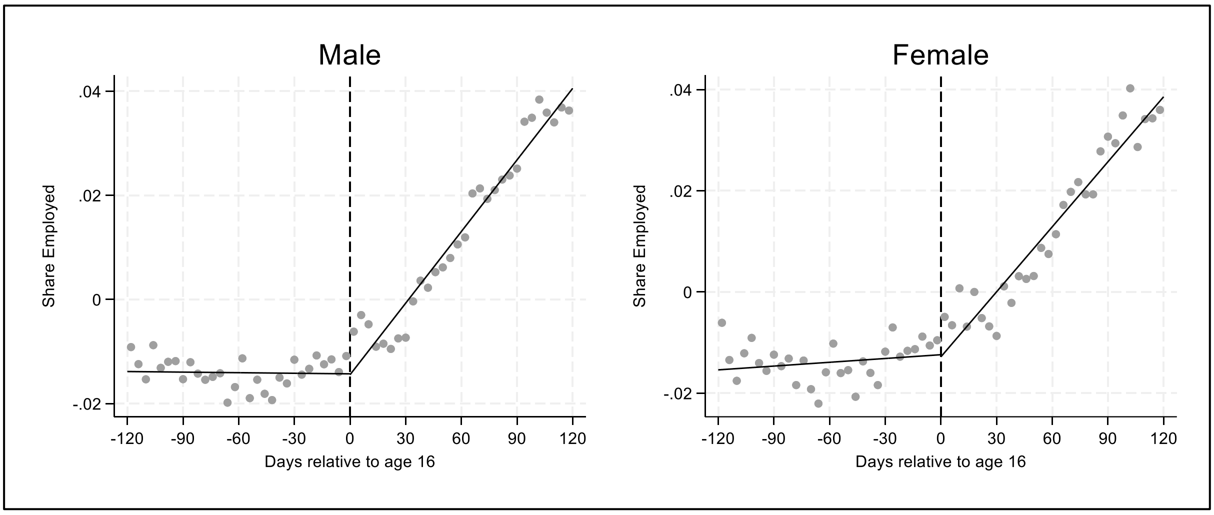
Notes: Dots represent four-day averages of the share of students who retained a grade, plotted against the number of days relative to the cutoff date. The three panels correspond to three educational phases: early grades (1–3), intermediate grades (4–6), and later grades (7–9). Solid lines on either side depict fitted values from a piecewise linear specification of the running variable.

Figure B5. Fifth-grade normalized test scores by days relative to cutoff



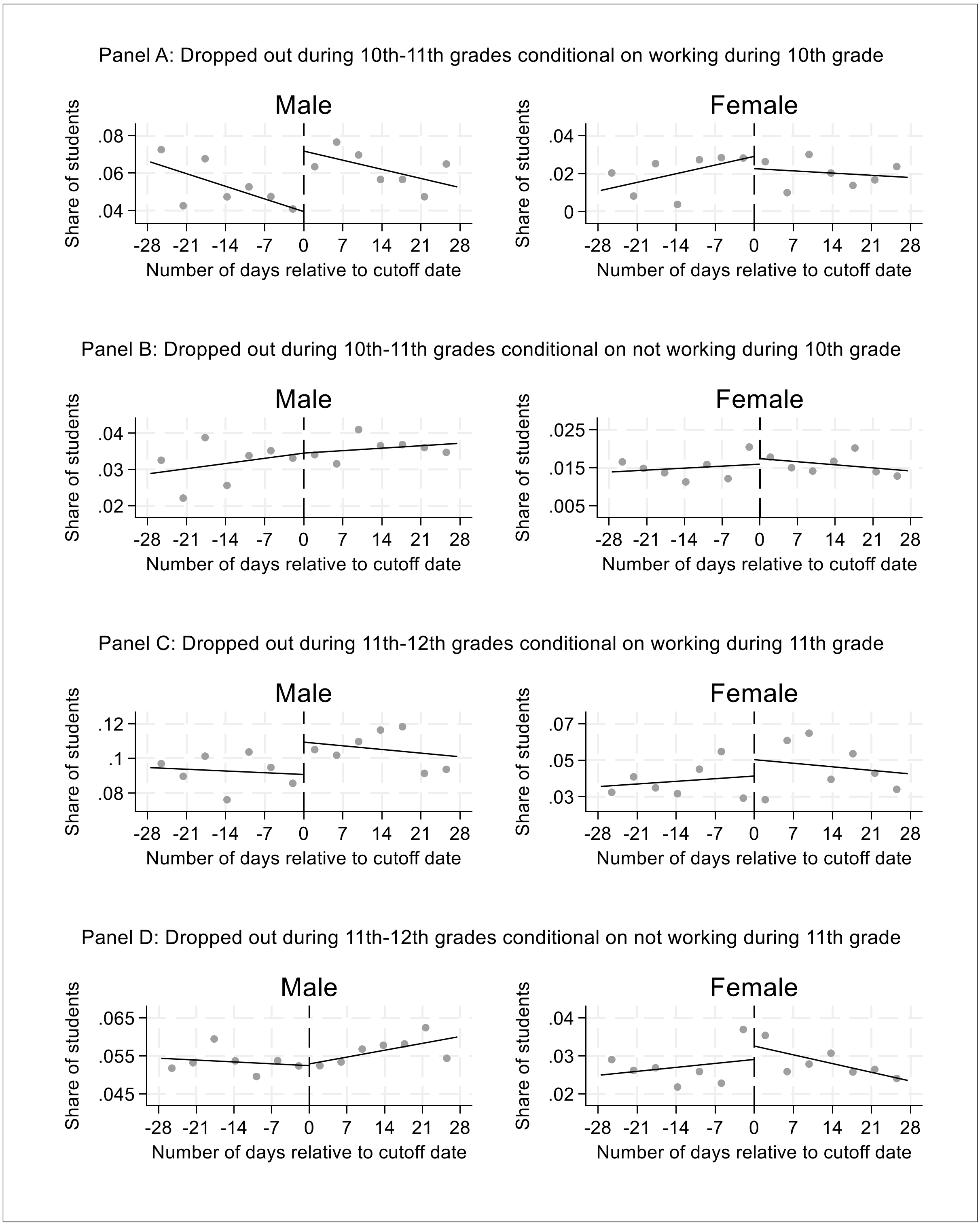
Notes: Dots represent four-day averages of fifth-grade test scores in different subject exams, plotted against the number of days relative to the cutoff date. Solid lines on either side depict fitted values from a piecewise linear specification of the running variable.

Figure B6. Residuals of employment status on student fixed effects



Notes: Dots represent four-day averages of residuals from a regression of employment status on individual fixed effects, plotted against the number of days relative to the minimum legal working age of 16. A child's age in any given month is based on their age on the first day of the month. Solid lines on either side depict fitted values from a piecewise linear specification of the running variable.

Figure B7. High school dropout rates among students who worked and didn't work in the previous grade by days relative to cutoff



Notes: Each panel plots an outcome variable on the y-axis against the number of days relative to the cutoff date. Dots represent four-day averages of the outcome variables. Solid lines on either side depict fitted values from a piecewise linear specification of the running variable.

Table B1. Data structure

Data Set A: Young Cohorts						
Period	**Obs**	**Cutoff date**	**Location relative to cutoff**	**Expected school starting date**	**Expected year of 5th-grade Meitzav**	**Age on 12/31/2017**
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Period 1	10,020	7/12/91	Before	09/1997	2002	26.03–26.11
			After	09/1998	2003	25.95–26.03
Period 2	10,103	24/12/92	Before	09/1998	2003	25.02–25.10
			After	09/1999	2004	24.94–25.02
Period 3	9,841	14/12/93	Before	09/1999	2004	24.05–24.12
			After	09/2000	2005	23.97–24.05
Period 4	10,209	3/12/94	Before	09/2000	2005	23.08–23.15
			After	09/2001	2006	23.00–23.08
Period 5	9,799	23/12/95	Before	09/2001	2006	22.02–22.10
			After	09/2002	2007	21.95–22.02
Period 6	10,236	10/12/96	Before	09/2002	2006	21.06–21.13
			After	09/2003	2007	20.98–21.06
Period 7	10,550	29/12/97	Before	09/2003	2007	20.01–20.08
			After	09/2004	2008	19.93–20.01
Data Set B: Old Cohorts						
Period	**Obs**	**Cutoff date**	**Location relative to cutoff**	**School starting date**	**Year started 10th grade**	**Age on 12/31/2017**
Period 8	9,411	21/12/76	Before	09/1982	1991	41.03–41.10
			After	09/1983	1992	40.95–41.03
Period 9	9,545	10/12/77	Before	09/1983	1992	40.06–40.13
			After	09/1984	1993	39.98–40.06
Period 10	9,278	30/12/78	Before	09/1984	1993	39.00–39.08
			After	09/1985	1994	38.93–39.00
Period 11	9,024	20/12/79	Before	09/1985	1994	38.03–38.11
			After	09/1986	1995	37.96–38.03

Notes: The table presents the data structure by period. Columns 2 and 3 report the number of observations and the relevant cutoff date. For each period and position relative to the cutoff date (before or after), columns 4–6 report the expected school starting date, the expected year of taking the fifth-grade Meitzav, and the range of students' ages at the end of 2017, respectively.

Table B2. Reduced-form estimates on youth employment

	Sample Average	All (1)	Male (2)	Female (3)
Employed during 10th grade	0.12	0.025^{***} (0.005)	0.022^{***} (0.008)	0.029^{***} (0.007)
Employed during 11th grade	0.25	0.033^{***} (0.007)	0.032^{***} (0.008)	0.033^{***} (0.010)
Employed during 12th grade	0.45	0.034^{***} (0.008)	0.034^{***} (0.010)	0.033^{***} (0.012)
Employment duration during high school (months)	6.38	0.737^{***} (0.110)	0.646^{***} (0.135)	0.823^{***} (0.152)
Total real earnings, 9th to 12^{th} grades (in 1,000 shekels)	2.40	0.205^{***} (0.027)	0.157^{***} (0.038)	0.256^{***} (0.030)
Observations		70,758	35,856	34,902

Notes: The table shows the effects on various outcome variables, with each entry derived from a separate regression. The row headers indicate the outcome variables, and the column headers specify the populations included in the regressions. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. Standard errors clustered by date of year are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B3. SEA effects on youth employment earnings

	Sample Average	All (1)	Male (2)	Female (3)
Real yearly earnings, 9th–10th grades	0.261	0.173^{***} (0.039)	0.146^{**} (0.056)	0.196^{***} (0.048)
Real yearly earnings, 10th–11th grades	0.700	0.516^{***} (0.070)	0.459^{***} (0.121)	0.562^{***} (0.068)
Real yearly earnings, 11th–12th grades	1.483	0.671^{***} (0.129)	0.738^{***} (0.201)	0.627^{***} (0.140)
Total real earnings, 9th–12th grade	2.400	1.319^{***} (0.191)	1.362^{***} (0.268)	1.297^{***} (0.200)
Total real earnings above the median	0.375	0.133^{***} (0.023)	0.140^{***} (0.036)	0.126^{***} (0.029)
Total real earnings above the 75th percentile	0.193	0.128^{***} (0.021)	0.124^{***} (0.033)	0.131^{***} (0.023)
Total real earnings above the 90th percentile	0.078	0.087^{***} (0.018)	0.096^{***} (0.025)	0.081^{***} (0.018)
F-stat on ex. instrument		1923	642	1193
Observations		70,758	35,856	34,902

Notes: The table presents the effect of SEA on various outcome variables, with each entry derived from a separate regression. The row headers indicate the outcome variables, and the column headers specify the populations included in the regressions. Earnings are measured in thousands of shekels in real terms. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. The sample averages for variables indicating total earnings above the median, 75th percentile, and 90th percentile differ from 0.5, 0.25, and 0.1 because the distribution is based on children born throughout the entire year, while the reported averages are limited to children born within a 28-day bandwidth around the school-entry cutoff dates. "F-stat on ex. instrument" refers to the F-statistic on the excluded instrument. Standard errors, clustered by date of year, are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B4. A regression discontinuity analysis of the probability of employment around the legal working age

	Male	**Female**	**Male**	**Female**
	(1)	(2)	(3)	(4)
RC/100	0.0029	0.0057***	0.0001	0.0038**
	(0.0018)	(0.0018)	(0.0018)	(0.0018)
Above 16	-0.0026	-0.0018	-0.0017	-0.0019
	(0.0016)	(0.0017)	(0.0014)	(0.0015)
RC/100 × Above 16	0.0445***	0.0374***	0.0461***	0.0402***
	(0.0026)	(0.0026)	(0.0027)	(0.0027)
F-stat (β_2=$\beta_3 = 0$) structural break	149.53	103.79	146.22	109.68
Student FE	No	No	Yes	Yes
Period FE	Yes	Yes	No	No
Controls	Yes	Yes	No	No
Observations	277,434	270,106	277,434	270,106

Notes: Each column presents results from a different regression. RC represents the child's age in days relative to the cutoff of age 16. "Above 16" is an indicator variable for being above age 16. The table reports the F-statistic from a Chow (1960) test for a structural break at age 16. Standard errors clustered at the student level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B5. SEA effects on the probability of high school dropout by youth employment status

	Dropped out during 10th–11th grades			Dropped out during 11th–12th grades		
	All	Among employed in 10th grade	Among non-employed in 10th grade	All	Among employed in 11th grade	Among non-employed in 11th grade
	(1)	(2)	(3)	(4)	(5)	(6)
Entire sample	0.015	0.090**	0.007	0.030*	0.067**	0.017
	(0.009)	(0.037)	(0.009)	(0.016)	(0.030)	(0.016)
Observations	70,758	8,223	62,535	70,758	17,828	52,930
Male	0.028	0.240**	0.010	0.039	0.128*	0.014
	(0.018)	(0.092)	(0.017)	(0.025)	(0.069)	(0.023)
Observations	35,856	4,426	31,430	35,856	9,340	26,516
Female	0.006	-0.007	0.006	0.024	0.032	0.019
	(0.010)	(0.029)	(0.010)	(0.015)	(0.021)	(0.018)
Observations	34,902	3797	31105	34,902	8488	26414

Notes: This table presents the effects of SEA on the likelihood of high school dropout. Columns 1 and 4 report effects for the entire population (including both employed and non-employed students) on dropping out during 10th–11th grades and 11th–12th grades, respectively. Columns 2 and 3 provide separate estimates for students employed in 10th grade (column 2) and those not employed in 10th grade (column 3). Similarly, columns 5 and 6 provide separate estimates for students employed in 11th grade (column 5) and those not employed in 11th grade (column 6). The table also breaks down these effects by gender, with rows for the entire sample, males, and females. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. "F-stat ex. inst." refers to the F-statistic on the excluded instrument. Standard errors clustered by date of year are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B6. SEA estimates on outcomes in a psychometric exam

	Male		Female	
	All	Excluding test takers during high school	All	Excluding test takers during high school
Score above bottom quartile	-0.095 (0.066)	-0.140** (0.064)	0.042 (0.045)	0.008 (0.047)
Score above median	-0.132** (0.055)	-0.143** (0.054)	0.051 (0.040)	0.030 (0.039)
Score in top quartile	-0.100** (0.040)	-0.135*** (0.040)	-0.007 (0.027)	-0.023 (0.025)
Score (for takers)		-5.079 (18.520)		2.900 (10.285)
F-stat on ex. instrument	367.22	345.92	341.64	306.00
Observations	18,381	15,828	18,877	16,293

Notes: The table presents the effect of SEA on various outcome variables, using both the entire sample and a subsample that excludes students who took the exam during high school. Each entry in the table is derived from a separate regression. Outcomes for scoring above a particular quartile are coded to zero for students who did not take the exam. The quartile thresholds are calculated only among test takers who took the exam for the first time. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1 as well as date-of-year, day-of-week, and period fixed effects. "F-stat ex. inst." refers to the F-statistic on the excluded instrument. Standard errors, clustered by date of year, are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C: Robustness Analyses

In this appendix, we select a subset of outcomes from each domain—elementary test scores, grade retention, high school completion, youth employment, psychometric-exam participation and scores, starting a first degree, and years of schooling—and report robustness tests on this selected set. The results are presented in Table C1. First, to address concerns regarding the inclusion of multilevel fixed effects in the estimation (De Chaisemartin and d'Haultfoeuille 2020, Borusyak et al. 2021, Callaway and Sant'Anna 2021), we estimate the effect of SEA on each of our central outcome variables with a piecewise linear trend of the running variable instead of date-of-birth fixed effects. The results, shown in column 2, indicate that this alternative specification has a minimal impact on the short- and medium-term SEA effects that we estimate using our young cohorts. However, the long-term SEA effects that we estimate using our old cohorts are reduced by 20%–30%, though they remain statistically significant for most outcomes. An exception is participation in the psychometric exam, which is now only almost significant at the 10% level.

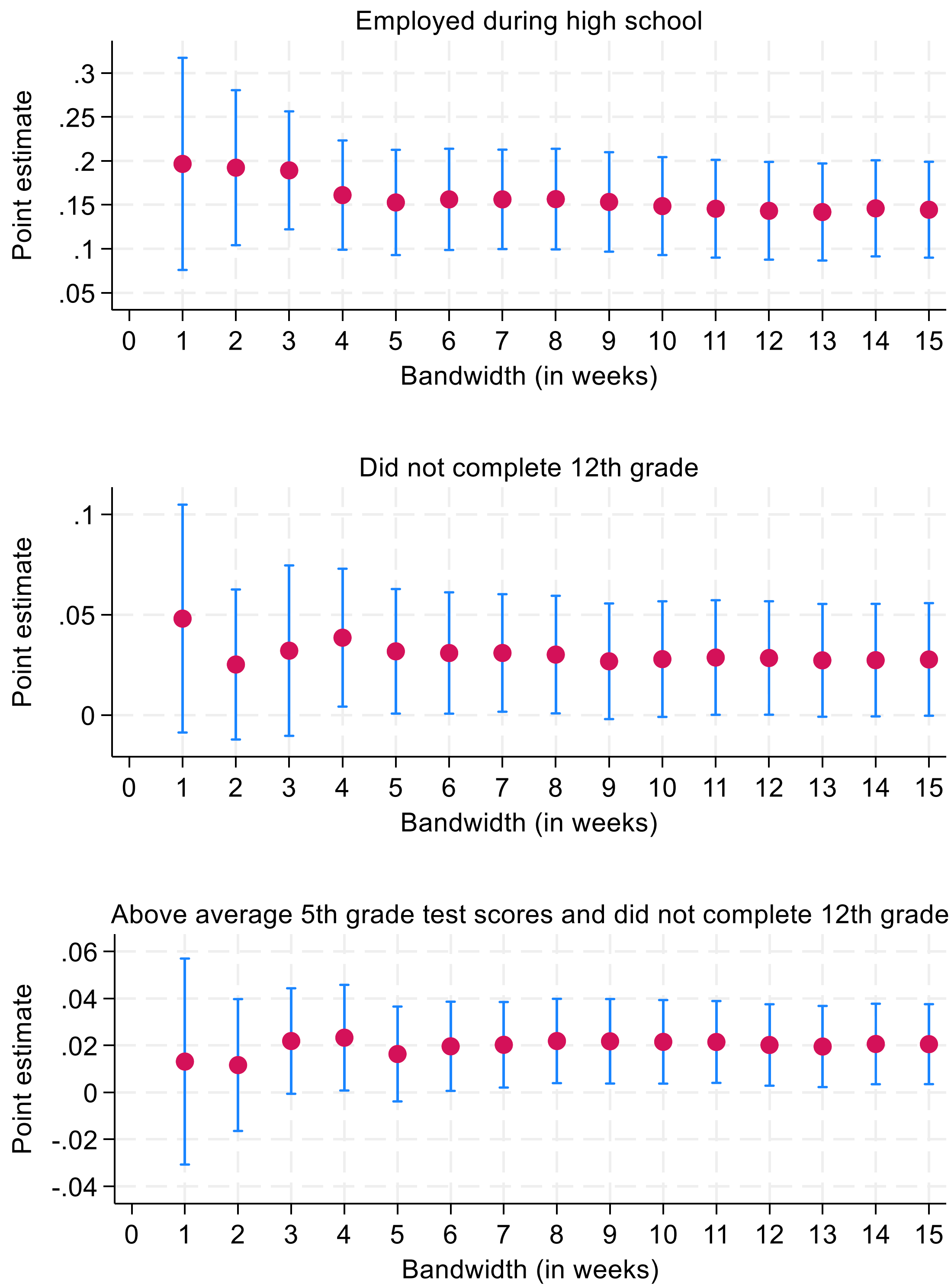
Another possible concern with our RD estimates is their sensitivity to bandwidth choice. To evaluate this, we apply the methods of Imbens and Kalyanaraman (2012) and Calonico et al. (2014, 2017), calculating both coverage error rate optimal bandwidths and mean square error optimal bandwidths for each outcome variable using the "rdrobust" command in Stata. As reported in column 4, the coverage-error-rate optimal bandwidths typically fall within 20–40 days, supporting our choice of a 28-day bandwidth, which aligns closely with the one-month bandwidth commonly used in the SEA literature. In contrast, the mean-square-error optimal bandwidths, shown in column 5, range from 40 to 80 days. Consequently, we test the robustness of our results using an alternative 56-day bandwidth, which lies within the mean-square-error optimal range. The results shown in column 3 are very similar to our main estimates (column 1), confirming the robustness of our findings. Additionally, Appendix Figure C1 illustrates the stability of the SEA estimates across varying bandwidths for our three main outcome variables: youth employment, high school dropout, and the likelihood that a child will both achieve above-average fifth-grade test scores and nevertheless drop out of high school (the "reversal effect").

Table C1. Robustness analysis

	Bandwidth			Optimal Bandwidth	
	28 days	***28 days***	***56 days***	**CER**	**MSE**
	(1)	(2)	(3)	(4)	(5)
Normalized 5th-grade math score	0.284*** (0.079)	0.320*** (0.077)	0.251*** (0.063)	35 (31–41)	64 (55–75)
Retained during 1st–3rd grades	-0.055*** (0.010)	-0.055*** (0.010)	-0.051*** (0.008)	22 (21–32)	41 (41–62)
Employed during high school	0.160*** (0.030)	0.143*** (0.025)	0.154*** (0.028)	34 (33–36)	64 (64–70)
Did not complete 12 years of schooling	0.040** (0.016)	0.029** (0.014)	0.034** (0.014)	40 (37–40)	77 (72–77)
Did not complete 12 years of schooling (male)	0.066** (0.028)	0.053** (0.026)	0.063*** (0.023)	27 (26–36)	50 (50–67)
Participation in the psychometric exam (male)	-0.135** (0.066)	-0.087 (0.055)	-0.095* (0.057)	23 (20–36)	41 (41–66)
Psychometric total score at top quartile (male)	-0.100** (0.040)	-0.067** (0.032)	-0.106*** (0.033)	42 (36–42)	76 (65–76)
Started a first degree (male)	-0.133** (0.061)	-0.105** (0.052)	-0.104* (0.055)	24 (24–34)	43 (39–62)
Years of schooling	-0.474*** (0.157)	-0.344** (0.145)	-0.367*** (0.133)	26 (25–40)	48 (42-75)
Period FE	Yes	Yes	Yes		
Piecewise linear trend	No	Yes	No		
Day-of-week FE	Yes	No	Yes		
Date-of-year FE	Yes	No	Yes		

Notes: Columns 1–3 report SEA effects on the outcome variables, specified by the row headers, with each entry in the table derived from a separate regression. Columns 4 and 5 present the coverage error rate (CER) optimal and mean square error (MSE) optimal bandwidths, using the "rdrobust" command in Stata. Bandwidths derived from the "mserd" and "cerrd" options are reported, while the ranges for each of the bandwidths from all other options are presented in parentheses. An indicator for birth after the entrance cutoff is used as an instrument for SEA. All estimations include the full set of control variables reported in Table 1, along with fixed effects for date of year, day of week, and period. Standard errors, clustered by date of year, are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C1. SEA effects by bandwidth size



Notes: The figure illustrates the sensitivity of our central entrance-age effects to different bandwidths.

References

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2024. "Revisiting event-study designs: robust and efficient estimation." *Review of Economic Studies*. doi:10.1093/restud/rdae007.

Callaway, Brantly, and Pedro H. C. Sant'Anna. 2021. "Difference-in-differences With Multiple Time Periods." *Journal of Econometrics* 225 (2): 200–230.

Calonico, Sebastian, Matias D. Cattaneo, Max H. Farrell, and Rocio Titiunik. 2017. "Rdrobust: Software for Regression-discontinuity." *The Stata Journal* 17 (2): 372–404.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014. "Robust Data-driven Inference in the Regression-Discontinuity Design." *The Stata Journal* 14 (4): 909–46.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma. 2020. "Simple local polynomial density estimators." *Journal of the American Statistical Association* 115 (531): 1449–1455.

De Chaisemartin, Cl´ement and Xavier d'Haultfoeuille. 2020. "Two-way fixed effects estimators with heterogeneous treatment effects." *American Economic Review* 110 (9): 2964–2996.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with Variation in Treatment Timing." *Journal of Econometrics* 225(2): 254–77.

Imbens, Guido, and Karthik Kalyanaraman. 2012. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *The Review of Economic Studies* 79 (3): 933–59.