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The Economics of Age at School Entry: Insights from Evidence and Methods

Mariagrazia Cavallo
Luxembourg Institute of Socio-Economic Research (LISER)

Elizabeth Dhuey
University of Toronto and IZA@LISER

Luca Fumarco
Masaryk University, GLO, J-PAL and IZA@LISER

Levi Halewyck
Maastricht University

Simon ter Meulen
University of Munich and CESifo

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Abstract

This article reviews the growing literature on age at school entry and its effects over the life course. Age at school entry affects a broad range of outcomes, including education, labor-market performance, health, social relationships, and family formation. We synthesize the evidence using a conceptual framework that distinguishes four empirically intertwined components of age at school entry: starting age, age at outcome, relative age, and time in school. Within this framework, we highlight six key channels through which age at school entry operates. While the effects of age at school entry are often substantial and persistent, many studies estimate bundled impacts without isolating specific components or directly measuring underlying mechanisms. We explain how different research designs capture distinct combinations of these components. We also highlight how institutional heterogeneity and behavioral responses can complicate the interpretation of results. We conclude by outlining directions for future research and policy design.

JEL classification

I12, I21, I24, I31, J12, J13, J24, K42

Keywords

age at school entry, starting age, age at outcome, relative age, time in school, institutional mechanisms, quasi-experimental methods

Corresponding author

Luca Fumarco
luca.fumarco@econ.muni.cz

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1 Introduction

How does the age at which children begin formal schooling shape their developmental and long-term outcomes? This question has drawn sustained attention from scholars in education, medicine, psychology, sociology, and sports science. Despite its clear relevance for the accumulation of human capital, it has historically received relatively little attention from economists. For example, between 1990 and 2005, only a handful of economics papers were published on the subject (Allen & Barnsley, 1993; Angrist & Krueger, 1992, 1991). However, following the study by Bedard and Dhuey (2006) on the significant and pervasive impact of age at school entry across a wide range of education systems, economists have become increasingly interested in the topic.

This article provides a guide to the economic literature on age at school entry (ASE).[1] We synthesize findings from recent empirical work and highlight the mechanisms, methodological challenges, and institutional features that shape ASE's impact. Our review contributes to the literature in four main ways. First, it clarifies why economists should care about ASE, not just because it influences test scores and educational attainment, but also because it affects labor market outcomes, mental and physical health, social relationships, crime, and even family formation. Second, it introduces an organizing framework built around four analytically distinct but empirically intertwined ASE components: *Starting-age* (the child's maturity/skill level at school entry), *Age-at-outcome* (the child's age when outcomes are measured),[2] *Relative-age* (the child's position in the classroom age distribution), and *Time-in-school* (days in school at measurement). *Starting-age* is fixed at school entry, while *Age-at-outcome* varies mechanically with the timing of outcome measurement. Together, these components shape both short-run performance and long-run trajectories through a set of interacting developmental and institutional channels. Third, we discuss how different identification strategies capture different combinations of ASE components, thereby helping readers interpret a growing literature. Finally, we assess what is known with confidence, what remains debated, and where promising gaps exist for future research. Throughout the review, we emphasize that most empirical estimates capture bundled effects of age-related components rather than isolated mechanisms, and we interpret findings accordingly.

To illustrate the economic and policy significance of ASE, consider the example of school entry rules in Ontario, Canada. Children begin school in September of the year they turn 4, which means a child born in January may start school at 4 years and 8 months, while a December-born child enters at 3 years and 9 months, nearly a full year younger. This 11-month difference in entry age persists throughout schooling and creates meaningful variation in cognitive development, peer dynamics, and teacher expectations. ASE thus affects students' absolute maturity (*Starting-age*), their relative standing (*Relative-age*) in the classroom, the age at which outcomes are measured (*Age-at-outcome*), and, depending on institutional rules, the total time spent in school (*Time-in-*

[1]There are two dominant but equivalent terms: age at school entry or age at school start, with related derived terms and acronyms, such as ASE and SSA. For the sake of consistency, we adopt the former throughout this work.

[2]In the literature, this is sometimes referred to as "absolute age".

school).

Understanding the effects of different components of ASE is essential for both policymakers and families navigating school-entry decisions. For policymakers, disentangling the effects of ASE components is critical to designing enrollment rules, tracking, curriculum implementation, and dropout policies that optimize developmental outcomes while minimizing inequities across socioeconomic groups. For families, particularly those deciding whether to delay their child's school entry (commonly referred to as "redshirting"), these components frame a complex trade-off between potential maturity advantages and costs such as delayed labor market entry or increased child care burdens. A nuanced understanding of how ASE operates is therefore indispensable for informed, equitable, and context-sensitive decision-making at both the policy and household level.

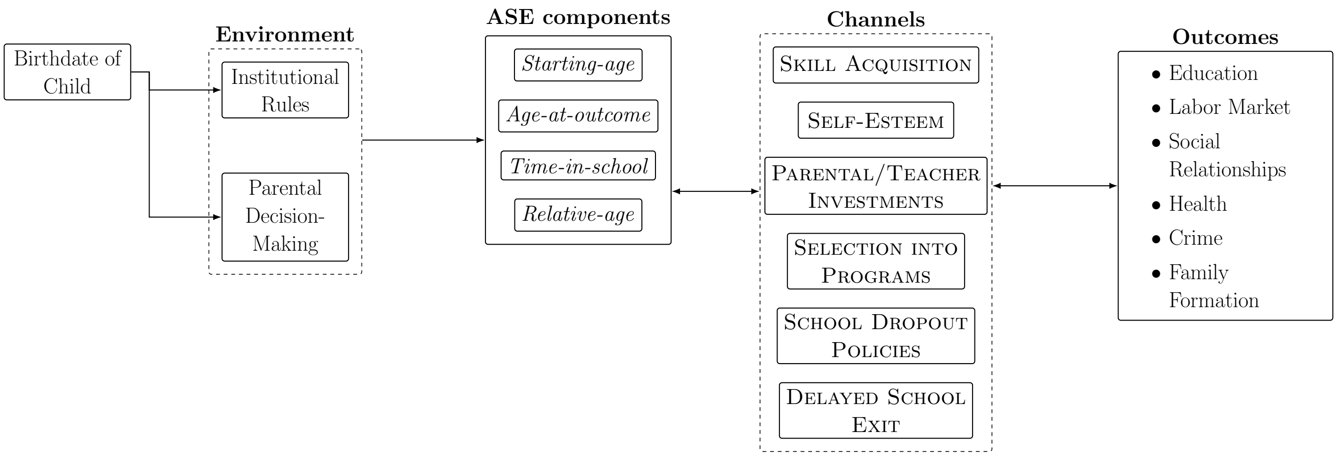
ASE policies are universal, yet their designs vary across jurisdictions and over time. Some systems allow redshirting, while others impose fixed enrollment dates; some have age-based dropout thresholds, while others rely on grade completion. These differences generate within- and cross-country variation in ASE effects and offer important lessons for institutional design. The implications are broad: ASE decisions shape early investments in human capital, interact with educational tracking and behavioral diagnoses, and reinforce or mitigate long-run socioeconomic inequalities.

The remainder of this review is structured as follows. Section 2 lays out the developmental pathways through which ASE may influence a wide range of life outcomes. Section 3 considers methodological challenges and approaches. Section 4 discusses results, channels, literature gaps, and policy implications on education, labor market, social relationships, mental and physical health, crime, and family formation. Section 5 concludes with areas for future research. In addition, there are four supplemental appendixes. Appendix A reports three tables summarizing the effects of individual-level decisions (redshirting), policy-level decisions, and their interaction with ASE components, channels, and outcomes. Appendix B provides a detailed table that, for each article discussed in Section 4 of the paper, reports the outcome studied, country and data used, methodology, main effects, and results from heterogeneity analyses. Appendix C reports school-entry cutoff dates per country and, when applicable, per administrative area. Appendix D provides the complete set of readings, consisting of more than 260 papers, underlying the exposition.

2 Conceptual Framework

Figure 1 presents the conceptual framework that underpins our analysis. It is intended as a heuristic organizing device rather than a causal flowchart. The components and channels it highlights interact dynamically, with feedback loops between institutional settings, parental behavior, and child outcomes. It outlines how a child's age at school entry (ASE) is determined by institutional policies and parental choices, which affect long-run outcomes through multiple developmental and institutional pathways. The framework serves two purposes: it clarifies the mechanisms through which ASE operates, and it provides a map for interpreting the empirical designs reviewed later in the paper. Section 3 employs this framework to discuss identification strategies, while Section 4 employs it to synthesize empirical evidence across key outcome domains.

Figure 1: Conceptual framework linking ASE components to the birthdate of child and outcomes.



Note: This pathway diagram shows the links between ASE components, channels, and outcomes. Dashed boxes highlight environmental components that determine children's school entry and channels, some that act as mediators and some as institutional effect modifiers (that is, SCHOOL DROPOUT POLICIES and DELAYED SCHOOL EXIT).

Figure 1 illustrates how the process begins at birth. A child's ASE is shaped by both parental decision-making and institutional rules. Parental decision-making encompasses whether to enroll on the prescribed date, delay entry (redshirting), enter early (greenshirting), or utilize other pre-primary programs such as preschool or child care. By institutional rules, we refer to the set of regulations and policies that govern primary-school entry and school-leaving decisions.[3]

Section 2.1 examines the parental decision-making process in greater detail, while Section 2.2 focuses on the institutional design of cutoff policies. Together, these forces determine when children enter formal schooling and how age differences translate into heterogeneous educational environments. These two forces jointly determine the child's ASE and, in turn, affect four distinct but interrelated components: *Starting-age*, *Age-at-outcome*, *Relative-age*, and *Time-in-school*.

- *Starting-age* embodies a child's chronological age at school start, as well as the exposure to any (in)formal child care environment before school entry. Institutional cutoffs and parental enrollment timing decisions directly shape this component.

- *Age-at-outcome* captures the child's chronological age at the time outcomes are measured. It varies with both birth date and the timing of assessments or outcome measurement.[4]

[3] In systems with age-based dropout thresholds, older students may be legally allowed to exit before completing secondary school, leading to shorter schooling durations.

[4] We interpret absolute age as a proxy for biological development. However, strictly speaking, there is no one-to-one correspondence between the two because of differences in growth rates and, consequently, in skill development.

- *Relative-age* describes the child's position in the classroom age distribution. As such, it is not a mechanical function of *Starting-age* and cutoff date, because peers who enter school under the same cutoff and at exactly the same age may be assigned to different classrooms.

- *Time-in-school* refers to the nominal number of days of formal education a child has completed by the time of outcome measurement. Here, *nominal* refers to the time elapsed under the formal schooling schedule, abstracting from various forms of grade skipping or repetition. It therefore reflects cumulative exposure to the formal curriculum and the material students are expected to have covered. Depending on the timing of measurement, it may refer to the nominal time spent in school to date (current *Time-in-school*) or to the nominal total time spent in school (total *Time-in-school*), with the latter corresponding to completed years of education.

These components are analytically distinct but empirically intertwined, and disentangling them is central to interpreting the ASE literature. Two of these components are particularly salient in practice. Parents weigh the implications of *Relative-age*, as it influences children's experiences relative to their peers. Moreover, in educational systems where child care is (partly or entirely) paid for by parents, parents care about *Starting-age*, as they must trade off concerns about whether their child is mature enough to start school against the costs of one additional year of child care. Policymakers are primarily concerned with *Starting-age*, which is directly affected by enrollment laws. These considerations are further discussed in Sections 2.1 and 2.2.

These ASE components influence outcomes through six channels, transmitting the effects of ASE to cognitive, non-cognitive (including social and behavioral) skills, and economic domains:

- Skill Acquisition: Greater maturity at school entry (*Starting-age*) may provide an initial advantage in acquiring cognitive and non-cognitive skills.

- Self-Esteem: Older children may benefit from more favorable peer comparisons through *Relative-age* and may find coping with the school environment easier through a higher *Age-at-outcome*, which can build confidence and shape attitudes toward learning and school.

- Parental/Teacher Investments: Parents and teachers may respond to children's maturity or performance by adjusting support, potentially reinforcing or compensating for perceived developmental gaps. Investments can take the form of time, tutoring, grade retention, or other forms of support.

- Selection into Programs: Age-related differences can shape placement into gifted programs, special education, or ability tracks. Younger entrants may be underrepresented in high-track placements and overrepresented among students with behavior-related diagnoses.

- School Dropout Policies: In systems with age-based dropout thresholds, older students may be allowed to exit before completing secondary-school grades, leading to shorter schooling durations. This institutional feature operates as an effect modifier rather than a developmental channel, shaping how ASE components translate into total *Time-in-school*.

- Delayed School Exit: Postponing school entry mechanically shifts the timing of educational progression, delaying graduation and labor market entry. This timing effect is captured through total *Time-in-school* and its downstream consequences for labor market experience.

Some of these components, such as Skill Acquisition and Self-Esteem, can be interpreted as intermediate outcomes within the developmental process. Other channels, such as School Dropout Policies, reflect institutional features that shape the broader environment. Although Figure 1 depicts the pathways as a linear sequence, components, channels, and outcomes likely interact dynamically over time. For example, initial advantages in Skill Acquisition conferred by ASE components may raise Self-Esteem. This, in turn, might alter Parental/Teacher Investments and affect Selection into Programs. Attending these selective academic programs may reinforce the student's Self-Esteem, prompting further responses from teachers, parents, and others.

Some channels dynamically interact with ASE components. For example, having a young ASE increases the likelihood of grade retention, which constitutes an investment through the Parental/Teacher Investments channel. Grade retention, in turn, raises ASE-component *Age-at-outcome* when outcomes are measured at the end of a school grade. Lastly, some outcomes may also serve as intermediate inputs in the production of other outcomes. For example, health is an input into education and labor market outcomes.

Overall, long-run outcomes reflect the feedback loops between the channels themselves, short-run outcomes, ASE components, and their cumulative interactions. Figure 1 presents the dominant pathways identified in the literature. The framework is intentionally stylized: it highlights recurring mechanisms rather than exhaustively cataloging all possible pathways. Additional channels may exist and deserve further empirical investigation. As we will see, some studies directly examine the discussed channels as outcomes, whereas most focus on reduced-form ASE components.

To formalize how the channels link ASE components to outcomes, we introduce a stylized production function that can be written as:

$$y_{it} = g\left(c^1(X_{it}), ..., c^n(X_{it}), X_{it}\right) \tag{1}$$

where y_{it} denotes individual i's outcome at time t, and $g(\cdot)$ is a function of n distinct channels, represented in turn by functions $c^j(\cdot)$, for $j = 1, ..., n$. X_{it} is a vector of ASE components ($Starting\text{-}age_i$, $Age\text{-}at\text{-}outcome_{it}$, $Relative\text{-}age_i$, $Time\text{-}in\text{-}school_{it}$) and other inputs, such as abilities. As discussed, due to their dynamic nature, some channels interact with one another, capturing potential complementarity or substitution effects.

While this stylized production process helps to illustrate the multiple pathways by which ASE components affect outcomes, the function in Equation 1 is rarely estimated directly in empirical work. One reason is that many of the relevant channels, such as Parental/Teacher Investments or Self-Esteem, are unobserved or imperfectly measured. For this reason, most of the literature adopts a reduced-form approach, as will be discussed in Section 3.

2.1 Individual-level effects

In most education systems, a legislated school-entry cutoff forces parents into a binary choice: enroll their child at the mandated age or delay entry by one year.[5] This simple timing decision conceals a complex trade-off. Delaying entry increases three ASE components: *Starting-age*, *Age-at-outcome*, and *Relative-age*. Where compulsory schooling ends at a fixed age rather than upon completing a prescribed number of grades, it may also reduce total *Time-in-school*. For example, in the U.S., students may legally leave school upon reaching a minimum age (often 16), even if they have not completed high school. In contrast, many other countries require completion of a specified number of grades before allowing school exit.

Regardless of institutional design, a delayed school start generally implies a later graduation date and deferred labor-market entry. Additionally, redshirting often entails higher child care costs, especially when formal pre-primary care is limited, which may reduce parental labor-market participation, particularly among mothers.

Parents must navigate these trade-offs under limited information about long-run consequences. Nonetheless, as Black et al. (2011) argue, when considering specific outcomes, parents are more likely to prioritize *Starting-age* and *Relative-age* over *Age-at-outcome* or short-run differences in *Time-in-school* that have limited impact on long-run attainment.[6] This prioritization reflects both developmental concerns and immediate cost considerations. *Starting-age* reflects concerns about developmental readiness: children who begin school too early may struggle with academic and social demands, while starting too late could delay cognitive stimulation or social integration. Moreover, *Starting-age* affects exposure time to (in)formal child care before school entry, which, in turn, can affect educational costs borne by parents and children's readiness.[7] *Relative-age* captures how a child's position in the classroom age distribution affects their peer comparisons and self-confidence. These factors are linked to both academic and behavioral outcomes. In contrast,

[5]While redshirting (delaying school entry by one year) is the dominant behavior in the U.S., delaying school entry might be strictly regulated in other countries, whereas accelerated school entry (greenshirting) might be easier. Greenshirting refers to enrolling a child in school earlier than the standard entry date, typically by requesting an exemption from cutoff rules. The discussion in this section applies to both redshirting and greenshirting.

[6]Table A.1 in the Online Appendix A provides a summary of our hypotheses.

[7]For example, recent evidence from a high-income country (Luxembourg) shows that early childhood care can serve as a compensatory investment, mitigating disadvantages faced by younger children (Albanese & Bousselin, 2025). However, further research is needed on broader programs, such as Head Start in the U.S. (Gibbs, 2026).

Age-at-outcome primarily affects performance on assessments through pure maturity effects, which may not reflect actual learning gains. Total *Time-in-school* of compulsory education is often fixed in systems with grade-based graduation requirements, reducing its relevance to parental decision-making. Even so, these components may matter indirectly by shaping later institutional choices and constraints. Still, both *Age-at-outcome* and *Time-in-school* can shape downstream outcomes, such as selection into secondary school tracks or timing of dropout eligibility. For example, better performance on an end-of-primary-school test due to *Age-at-outcome* may lead to selection into better programs. Similarly, it could shape postsecondary education choices or cause dropout due to age-based compulsory-schooling rules.

Parental choices about school entry are heterogeneous and shaped by both constraints and perceived returns. In the U.S., the typical counterfactual to delayed school entry is enrollment in kindergarten at the standard age (Deming & Dynarski, 2008). The availability of full-day kindergarten programs plays an important role in these decisions by affecting the cost of delayed enrollment. When only part-time programs are available, redshirting often requires additional home-based supervision, which can reduce parental (and especially maternal) labor supply (Dhuey et al., 2021).

These cost considerations interact with socioeconomic status (SES) . High SES families are more likely to redshirt, reflecting their greater ability to absorb the direct and opportunity costs of delayed enrollment. They may also perceive the resulting maturity advantage as a means of accessing selective academic tracks (Berniell & Estrada, 2020; Deming & Dynarski, 2008). In addition, higher-income families appear to select into redshirting when the expected returns are relatively high, whereas lower-SES families exhibit little comparable selection and are more constrained by the immediate costs of delay. As a consequence, the benefits of redshirting are likely to accrue disproportionately to children from more advantaged backgrounds, reinforcing existing educational inequalities (Ricks, 2025).

Gender further interacts with parental school-entry decisions. Boys are more frequently redshirted than girls, consistent with evidence that they, on average, develop self-regulatory and socio-emotional skills more slowly (Dhuey et al., 2019). As a result, observed gender gaps in early academic outcomes and special education referrals may partly reflect differences in school-entry timing rather than underlying differences in ability.

2.2 Policy-level effects

Policymakers influence school-entry timing primarily by setting cutoff dates that determine when children become eligible to start school. In many jurisdictions, these cutoffs have been moved earlier in the calendar year, which raises the minimum age at which children are eligible to start school (Bedard & Dhuey, 2012; Deming & Dynarski, 2008). When establishing such cutoff dates, policymakers must balance the potential benefits of earlier school enrollment, such as skill acquisition, against the risk that younger entrants may lack the developmental readiness needed for effective learning. Delaying entry, in contrast, may increase maturity and early measured performance and can temporarily reduce cohort sizes, thereby generating cost savings. However, it can also defer exposure to formal instruction and create losses in the skills measured by age-adjusted test scores (Peña, 2020, 2017). These trade-offs underscore that cutoff policies shift multiple ASE components simultaneously rather than a single margin.

Understanding how a cutoff change affects outcomes requires recognizing that such a policy does not affect all students uniformly (Bedard & Dhuey, 2012; Aliprantis, 2014). Consider a reform that shifts the cutoff date from December 31^{st} to September 1^{st} of the same year. Suppose first that parents cannot adjust their behavior. This reform increases the minimum school-entry age and delays entry for children born between September and December. Let the full cohort be denoted by S, and partition it into two subsets:

- S_1: students directly affected by the policy (born between September and December), who must now delay entry by one year;
- S_0: students not directly affected (born before September), who continue to enroll at the previously mandated age.

Abstracting from the transitional effects, for children in S_1, the policy increases *Starting-age* and, for outcomes measured in a given grade (*Age-at-outcome*), since they are older than their new peers, their *Relative-age* also increases. The reform also increases the time spent before formal school entry, which may be allocated to formal preschool, informal child care, or home-based care. Current *Time-in-school* is unchanged for outcomes measured at a given grade, but it is lower at a given calendar age. Total *Time-in-school* depends on School Dropout Policies and compulsory schooling rules. Changes in these ASE components may improve outcomes through several channels (for example, Skill Acquisition or Self-Esteem).

For children in S_0, *Starting-age*, *Age-at-outcome*, and current *Time-in-school* are unchanged, while *Relative-age* falls. Students born earlier (who were older within their cohort before the reform) switch to a more middle position in the *Relative-age* distribution, and children born closer to the new cutoff (for example, in August) move from the middle of the within-cohort age distribution to the youngest group. Consequently, even students not directly constrained by the policy may experience substantial changes in peer composition and relative position in the age distribution.

A cutoff change can trigger behavioral responses. Some parents in S_0, although not directly affected by the cutoff change, may now voluntarily delay their child's entry, by redshirting, in response to the altered peer environment. Let us denote this third subset of children as S_2. For children in S_2, both *Starting-age* and *Age-at-outcome* increase relative to the counterfactual. Since they are now older than their new peers, *Relative-age* also increases. The effects for children not directly affected by the policy and who choose to redshirt (S_2) are therefore reinforced. For the remaining children choosing not to redshirt ($S_0 \setminus S_2$), *Starting-age*, *Age-at-outcome*, and *Time-in-school* do not change, while *Relative-age* shifts, because their peer group changes. Behavioral responses, therefore, propagate the policy shock beyond the directly affected group. The combined effects of policy changes and endogenous parental re-optimization generate heterogeneity in ASE components across the cohort.[8]

From a policy perspective, decisions around cutoff timing are often framed in terms of the marginal effect of *Starting-age*, even though such policies simultaneously shift other ASE components. In theory, if older entry ages consistently improve educational or behavioral outcomes, a later cutoff could be beneficial; if the gains are minimal and mostly reflect delayed skill acquisition, an earlier cutoff might be warranted. In practice, policymakers rarely account for the distributional consequences associated with *Relative-age*, even though this component can affect inequality in outcomes. One reason is that any policy that shifts *Relative-age* just reshuffles positions within the distribution. In addition, a focus on average outcomes alone can mask substantial within-cohort heterogeneity.

The government can implement policies to mitigate the long-term effects of ASE. For instance, school dropout policies influence the extent to which ASE affects juvenile crime rates (Section 4.6). Differences in educational tracking due to ASE can be mitigated if track changes are flexible or reversible (Section 4.1.2). These examples illustrate that ASE effects are shaped as much by downstream institutional design as by entry rules themselves. If ASE effects on individual outcomes are both persistent and difficult to offset, then policy levers beyond the cutoff setting deserve closer scrutiny.

3 Methods

The age at school entry (ASE) literature is shaped by two core econometric challenges: omitted variable bias and multicollinearity between ASE components. Both challenges arise because institutions and families jointly determine ASE, and because its components move together mechanically. This section outlines how each challenge affects identification and estimation. We begin by discussing omitted variable bias, which has motivated the widespread use of quasi-experimental designs. We then address multicollinearity, emphasizing that the outcome variable and source of identifying variation determine which combination of ASE components is identified. We conclude by reviewing the empirical strategies most commonly employed in response to these challenges.

As a starting point, consistent with our discussion in Section 2, researchers often specify a reduced-form equation of the following form:

$$y_{it} = f(\textit{Starting-age}_i, \textit{Age-at-outcome}_{it}, \textit{Relative-age}_i, \textit{Time-in-school}_{it}, \textit{Ability}_i) \qquad (2)$$

[8] Table A.2 in Online Appendix A summarizes the impact of a cutoff shift without behavioral responses, whereas Table A.3 adds re-optimization.

where $f(\cdot)$ denotes a well-defined function of the various ASE components and $Ability_i$ captures time-invariant individual endowments that affect outcomes independently of ASE. This formulation makes explicit that empirical designs differ primarily in which arguments of $f(\cdot)$ are changed. Below, we use this baseline equation to illustrate the two core econometric challenges.

3.1 Omitted variables bias

A key takeaway from the ASE literature is that credible estimates require methods that address omitted variable bias rather than relying on observational ordinary least squares (OLS) comparisons. This concern arises because the unobserved $Ability_i$ is typically omitted from the estimating equation and is correlated with any included ASE components and the outcomes.

OLS analyses of age effects in test-score regressions illustrate omitted variable bias in this setting. For example, Bedard and Dhuey (2006) discuss the omitted variable bias that arises when estimating the effect of age on Grade 4 test scores with the following OLS regression:

$$y_i = \alpha_0 + \alpha_1 age_i + X_i' A + e_i \tag{3}$$

where y_i denotes the observed outcome, age_i is the individual's age at observation, and X_i is a vector of control variables. The parameter of interest, α_1, is likely biased because unobserved $Ability_i$ is correlated with both age and the outcome through educational selection and timing decisions. This so-called ability bias can be mitigated by controlling for imperfect measures of ability. However, researchers should be wary of post-treatment bias when such proxies are observed after treatment assignment (Angrist & Pischke, 2009). While post-entry ability measures may reduce overall bias relative to omitting ability controls altogether, they can also exacerbate bias in some settings. By contrast, ability proxies observed prior to treatment assignment do not introduce post-treatment bias. Nonetheless, because school entry decisions are made at a very early age, plausibly pre-determined measures of ability are rarely observed, limiting the usefulness of such controls in practice. As such, without observing $Ability_i$ or the selection process, it is difficult to address omitted variable bias without quasi-experimental strategies, as discussed in Section 3.3.

Redshirting and grade retention are prime examples of educational selection processes that can lead to omitted variable bias. Redshirting alters a child's cohort rank from the youngest to the oldest. As seen in Section 2.1, these decisions are not random; they are more common among high-socioeconomic status (SES) families (Dhuey et al., 2019; Ricks, 2025). Because these families also differ systematically in unobserved inputs, simple comparisons confound age effects with family background.

3.2 Multicollinearity

The other core econometric challenge is the collinearity between the four ASE components ($\textit{Starting-age}_i$, $\textit{Time-in-school}_{it}$, $\textit{Age-at-outcome}_{it}$, and $\textit{Relative-age}_i$). In principle, there is perfect multicollinearity between $\textit{Starting-age}_i$, $\textit{Time-in-school}_{it}$, and $\textit{Age-at-outcome}_{it}$, in the sense that knowing any two of these components is often sufficient to determine the third. For example, for outcomes measured while a student is still in school, knowing the age at which the student started school ($\textit{Starting-age}_i$) and the number of years spent in school (current $\textit{Time-in-school}_{it}$) implies the student's $\textit{Age-at-outcome}_{it}$. By contrast, $\textit{Relative-age}_i$ is highly, but not perfectly, collinear with the other ASE components, as students entering school at the same age under the same cutoff may be assigned to classrooms with different age distributions and thus have different relative-age status.

The interdependence among ASE components implies that, in empirical specifications, at least one component must be omitted to avoid perfect multicollinearity. As a result, the estimated effect of any included component will generally reflect not only its own influence but also the effect of the omitted, mechanically related variables. Put differently, most estimates in the ASE literature should be interpreted as bundled effects rather than isolated causal parameters, even when designs are quasi-experimental. Nonetheless, depending on the outcome variable and the source of variation used for identification, researchers can effectively hold some ASE components constant, allowing them to attribute estimated effects to a smaller subset of components. The next sections explain which components can be held constant when using different types of outcomes (Section 3.2.1) or variation (Section 3.2.2).

3.2.1 Outcome variables and the identification of ASE components

The timing and structure of outcome measurement affect which ASE components in Equation (2) are held constant, and hence determine which bundle of ASE components is identified. Outcome choice, therefore, plays a central role in shaping the interpretation of estimated ASE effects. We distinguish between three common outcome types: (i) grade-based outcomes while a student is still in school, (ii) biological age-based assessments, and (iii) post-schooling adult outcomes.

Grade-based school outcomes measured while a student is still in school hold current $\textit{Time-in-school}_{it}$ constant for children of a specific grade or cohort. For instance, when examining test scores administered at the end of a fixed grade, students will be of different ages but will have completed the same number of grades (current $\textit{Time-in-school}_{it}$). As a result, when grade-level outcomes are used, current $\textit{Time-in-school}_{it}$, defined as nominal days since school entry, is mechanically fixed by construction. In this setting, and absent grade retention or other sources of additional variation, $\textit{Starting-age}_i$ and $\textit{Age-at-outcome}_{it}$ are perfectly collinear, while $\textit{Relative-age}_i$ is highly correlated with both. Grade retention breaks this exact collinearity by allowing children of the same *Starting-age* to differ in age at a given grade-based outcome, but it does not eliminate the strong dependence among these variables. Consequently, $\textit{Starting-age}_i$, $\textit{Relative-age}_i$, and $\textit{Age-at-outcome}_{it}$ cannot be jointly included in an empirical model using grade-level outcomes. Including only one of these variables yields an estimate that conflates its effect with those of the omitted, collinear components. Estimates based on grade-level outcomes should therefore be interpreted as composite effects rather than as isolating any single dimension of age or exposure.

In contrast, biological age-based outcomes hold $\textit{Age-at-outcome}_{it}$ constant. A prominent example is the Program for International Student Assessment (PISA), which assesses students of the same age regardless of grade level. However, students of the same age who started school in different years due to school-entry rules (that is, with different $\textit{Starting-age}_i$ and $\textit{Relative-age}_i$) will have accumulated different amounts of schooling (that is, current $\textit{Time-in-school}_{it}$). This induces high collinearity among $\textit{Starting-age}_i$, $\textit{Time-in-school}_{it}$, and $\textit{Relative-age}_i$. However, the collinearity is not perfect. Grade retention affects nominal $\textit{Time-in-school}_{it}$, as students will have completed fewer grades by the age of measurement. This implies that if one knows the number of completed grades (nominal $\textit{Time-in-school}_{it}$) one does not know $\textit{Starting-age}_i$ with certainty. Moreover, the correlation between $\textit{Relative-age}_i$ and $\textit{Starting-age}_i$ and/or $\textit{Time-in-school}_{it}$ might vary due to peer variation at the school and classroom level. Still, without further variation, if any of these components is included in isolation, the estimated effect will confound the influence of all three. As with grade-based outcomes, interpretation hinges on identifying which components are bundled.

Post-schooling outcomes measured at a fixed age abstract from $\textit{Time-in-school}_{it}$ differences due to starting school at different times. For instance, the likelihood of obtaining a university degree by age 35 is no longer affected by differences in current $\textit{Time-in-school}_{it}$ since hardly anyone is enrolled at this age. This type of outcome similarly benefits from holding $\textit{Age-at-outcome}_{it}$ constant, analogous to the biological age-based outcomes discussed earlier. Thus, the ASE estimate in a regression using a post-schooling age-based outcome will reflect only $\textit{Relative-age}_i$ and $\textit{Starting-age}_i$. These outcomes are therefore particularly informative about longer-run effects that operate beyond schooling duration.

3.2.2 Using quasi-experimental variation to identify ASE components

The source of identifying variation determines which components vary and contribute to the identification. Identification, therefore, depends jointly on the research design and the structure of the outcome variable. In ASE research, there are four primary sources of variation: cutoff-based variation; variation in assessment dates; (quasi-)random assignment to classrooms; and policy-induced changes in school-entry rules. Each interacts with the structure of the outcome variable to determine which bundle of ASE components is identified and which are implicitly held constant.

Cutoff-based variation is the most commonly exploited source of identifying variation. It leverages birthdate-based eligibility rules, which create a sharp discontinuity in the probability of starting school in a given year at the official cutoff date. For example, children born just after the cutoff are typically required to wait an extra year before entering school, while those born just before can start immediately. While this design provides a strong source of quasi-random assignment, it does not allow researchers to identify ASE components separately. Instead, cutoff-based designs recover a composite effect whose interpretation depends on the outcome used (Section 3.2.1).

Variation in assessment dates provides exogenous shifts in the age at which individuals are tested. When assessments are scheduled on different dates for otherwise similar birth cohorts, $\textit{Age-at-outcome}_{it}$ is no longer mechanically determined by school-entry timing. A clear example is the IQ test for Norwegian conscripts analyzed by Black et al. (2011): conscription rules set the expected year and month of testing as a piecewise function of date of birth, while deviations from this schedule generate further variation in actual test age. This allows the authors to use the school-entry cutoff as an instrument for $\textit{Starting-age}_i$ and the conscription schedule as an instrument for $\textit{Age-at-outcome}_{it}$, thereby at least partially separating the two respective effects on IQ scores. In their setting, however, many conscripts are still enrolled in school at the time of testing. Conditional on $\textit{Age-at-outcome}_{it}$, $\textit{Starting-age}_i$ therefore remains highly correlated with current $\textit{Time-in-school}_{it}$.This illustrates that even rich sources of variation may not fully disentangle ASE components when schooling is ongoing. Finally, to separate these two effects, Black et al. (2011) estimate a version using only graduated conscripts who do not continue education.

(Quasi-)random assignment of students to classrooms implies that students with the same $\textit{Age-at-outcome}_{it}$ may have classmates of different ages, generating variation in $\textit{Relative-age}_i$. An example of this approach is Cascio and Schanzenbach (2016), who use data from Project STAR. Within schools, there may be cohort variation in birth months and thus in $\textit{Relative-age}_i$ for a given birth date. For example, Fredriksson and Ockert (2005) use this variation to isolate $\textit{Relative-age}_i$ from the composite effect of absolute maturity. These designs allow researchers to isolate variation primarily associated with $\textit{Relative-age}_i$, holding other components (almost) constant.

A distinct source of identifying variation involves policy-induced changes in school-entry rules. Researchers have studied both (i) reforms that shift the entry cutoff while holding fixed the age (or grade) at which students are expected to complete compulsory schooling, and (ii) reforms that shift the entry cutoff and the expected school-leaving age in parallel. In the first case, moving the eligibility birth date for school entry later in the calendar lowers $\textit{Starting-age}_i$. This also increases (current) $\textit{Time-in-school}_{it}$ at any given age, thereby identifying the combined effect of earlier school entry and extended educational duration.[9] Such reforms, therefore, confound maturity effects with schooling duration effects by design. In the second case, when both the school-entry cutoff and the school-leaving age shift by the same amount, the total duration of schooling remains fixed, but the average *Starting-age* changes, allowing researchers to identify the average effect of delayed (or earlier) school entry, holding total years of schooling constant.

[9]For example, if the school year still begins in September, moving the cutoff date from September 1st to December 1st allows younger children, those born in the fall, to enter school a year earlier. Children who previously would have waited another year can now start at a younger age. By any later age of outcome measurement, these children will have accumulated more days of schooling than under the old policy.

3.3 Identification strategies in the ASE literature

This section discusses the three most common empirical strategies in the ASE literature: two-stage least squares (2SLS), regression discontinuity designs (RDD), and difference-in-differences (DiD) approaches. These methods have been developed and widely applied to address the identification challenges outlined in Section 3.1 and Section 3.2.

Each strategy typically relies on a specific form of identifying variation (Section 3.2.2). RDDs and 2SLS applications commonly use cutoff-based variation, while DiD designs typically leverage policy-induced changes in school-entry rules. Variation in assessment dates and the (quasi-)random assignment of students to classrooms are relatively rare. Still, when used, these sources are often combined with RDDs that exploit cutoff-based assignment rules.

3.3.1 Two-stage least-squares

The 2SLS methodology typically uses cutoff-based variation to address omitted variable bias. Unlike the RDD discussed below, 2SLS is often applied to data from a single cohort, relying on assigned-age variation. A student's assigned age is the age they are expected to have when the outcome is observed, based on their birth date relative to the school-entry cutoff.

Instrumenting age with assigned age isolates variation in age that is unrelated to unobserved factors such as ability. A typical first stage using this approach is:

$$age_i = \gamma_0 + \gamma_1 r_i + X_i'\Gamma + v_i \tag{4}$$

In Equation (4), drawn from Bedard and Dhuey (2006), r_i is assigned age, and age_i is the individual's age. The second stage mirrors Equation (3), but replaces age_i with its instrumented value.

The specific ASE components identified using 2SLS depend on the structure of the outcome variable—whether it is measured relative to grade level or biological age—as discussed in Section 3.2.1. However, in some institutional contexts, total $\textit{Time-in-school}_{it}$ will be included in the ASE-component bundle regardless of the outcome type. This occurs when school entry policies interact with school-leaving policies. In the U.S., students may leave school upon reaching a minimum age, rather than after completing a specific grade. In such settings, students who enter school earlier may remain enrolled longer than those who start later, leading to systematic differences in educational attainment across birth cohorts. This prompted Angrist and Krueger (1991) to use the quarter of birth as an instrument for educational attainment. Referring to Equation (3), this implies that the instrumented coefficient α_1 would include a total $\textit{Time-in-school}_{it}$ effect in addition to any other ASE components. Note that this holds when examining long-run outcomes, such as after schooling completion. By contrast, this would not be the case when considering an individual's educational outcomes while still in school.

The causal interpretation of the instrumented coefficient α_1 relies on the standard 2SLS assumptions: a strong first stage, a valid exclusion restriction (that is, the instrument affects the outcome only through the endogenous regressor), and monotonicity (that is, the instrument has a weakly monotonic effect on treatment assignment).

The relationship between assigned age and actual observed age is often strong enough to ensure a valid first stage. However, the exclusion restriction may be violated if birth month has a direct effect on the outcome of interest. Bound and Jaeger (2000), Buckles and Hungerman (2013), and Currie and Schwandt (2013) document systematic "season-of-birth" effects, showing that individuals born in different months differ in health, cognitive, and educational outcomes for reasons unrelated to schooling. These patterns suggest that birth month may influence outcomes through channels other than schooling, thereby violating the exclusion restriction.

A related concern is that 2SLS designs that rely on a single cohort are susceptible to secular trends in the outcome variable over time. In the presence of a secular trend in an outcome variable, such as steadily improving health or education, individuals later in the school year may appear to have better outcomes simply because they were born later in the calendar year. This correlation between birth month and cohort trends violates the exclusion restriction because the instrument is associated with factors unrelated to treatment status.

Due to these concerns, many researchers prefer to focus on observations near the school-entry cutoff from (at least) two adjacent cohorts, using an RDD. Students close to the cutoff are typically born in the same season.

The use of coarse instruments, such as quarter of birth, may generate monotonicity violations because they aggregate children whose parents exhibit heterogeneous enrollment responses, including redshirting and greenshirting. Redshirting per se does not violate monotonicity. The assumption fails only when the instrument induces both delayed and accelerated entry. Such violations arise when aggregation combines parents who respond by delaying entry (redshirting) with others who respond by accelerating entry (greenshirting), generating offsetting movements in ASE and causing the entry-age distributions to cross, as suggested by Barua and Lang (2016) and Fiorini and Stevens (2021). Such aggregation-induced violations of the monotonicity assumption are less plausible in settings with fine-grained data. For instance, a fuzzy RDD that uses exact date-of-birth data, as discussed below, is also an instrumental variables (IV) design. In this setting, a violation of the monotonicity assumption would require some parents to have enrollment preferences that favor redshirting when their child's date of birth lies just before the cutoff but favor greenshirting when it lies just after, implying a reversal in potential enrollment responses to an arbitrarily small change in the running variable.

Monotonicity can also fail when the school-entry cutoff does not align with the boundaries of the instrument's time interval. In some U.S. states, the cutoff falls several weeks into the fourth quarter (see Online Appendix C). In such cases, using the quarter of birth as an instrument can violate monotonicity, because some children born later in the quarter but before the cutoff may start school earlier than children born earlier in the quarter but after the cutoff (Barua & Lang, 2016). By contrast, in many settings the cutoff aligns with the beginning of a calendar month, and researchers therefore use exact birthdates or calendar months as the instrument or running variable, which makes monotonicity more plausible though not guaranteed in the presence of heterogeneous enrollment responses.

As is standard, the 2SLS estimand captures the causal effect of age on the outcome of interest for compliers, that is, students whose school starting date is influenced by the instrument, such as the school-entry cutoff. This implies that the estimated effect is not the average treatment effect (ATE) for the full population, but rather the local average treatment effect (LATE) for students whose school entry is affected by the policy. Section 3.3.2 discusses a test for whether the potential outcomes of compliers resemble those of always-takers and never-takers.

3.3.2 Regression discontinuity design

RDDs also exploit cutoff-based variation, but they differ from standard school-entry 2SLS designs in their source of identifying variation. Whereas many 2SLS approaches rely on within-cohort differences (for example, quarter- or month-of-birth instruments), RDDs compare children born just on either side of the school-entry cutoff, using observations arbitrarily close to the cutoff in the running variable. Under the continuity assumption, potential outcomes evolve smoothly at the cutoff, so local comparisons mitigate concerns about seasonality in innate ability and smooth trends in outcomes (Lee & Lemieux, 2010).

There are two primary RDD variants: (i) reduced-form RDD and (ii) fuzzy RDD. Reduced-form RDDs estimate the discontinuity in outcomes at the cutoff (an intention-to-treat effect of cutoff eligibility). Fuzzy RDDs additionally use the cutoff as an instrument for a treatment that changes discontinuously at the cutoff, yielding an IV estimand analogous to 2SLS. The distinction matters in school-entry settings because families do not always comply with the prescribed enrollment rule: some cutoff-eligible children delay enrollment (redshirting), while others who are cutoff-eligible for later entry enroll early (greenshirting). As a result, crossing the cutoff only imperfectly determines actual enrollment timing or cohort assignment.

We begin with the fuzzy RDD, which provides a more informative causal interpretation. Let $after_i$ be an indicator equal to one if student i is born after the cutoff date and zero otherwise. Let the treatment D_i capture actual enrollment behavior, for example, an indicator for entering "on time" into the cohort prescribed by the cutoff (or, in alternative specifications below, a continuous measure of *Starting-age*). A common specification uses a local linear control function in the running variable (birthdate relative to the cutoff):

$$y_i = \zeta_0 + \zeta_1 D_i + \zeta_2 \textit{days-to-cutoff}_i + \zeta_3 \textit{days-since-cutoff}_i + e_i, \quad (5)$$

where $\textit{days-to-cutoff}_i$ and $\textit{days-since-cutoff}_i$ are piecewise-linear functions of the signed distance between student i's birthdate and the cutoff. These terms flexibly control for smooth relationships between outcomes and birthdate on either side of the cutoff. Empirical implementations typically restrict attention to a narrow bandwidth of birthdates around the cutoff. Within such bandwidths, the relationship between outcomes and the running variable is often well approximated by a linear function on each side; narrower bandwidths reduce statistical precision, while wider bandwidths increase the risk of misspecification. A systematic evaluation of this trade-off is discussed by Imbens and Kalyanaraman (2012).

The corresponding first stage instruments the treatment with cutoff eligibility:

$$D_i = \eta_0 + \eta_1 after_i + \eta_2 \textit{days-to-cutoff}_i + \eta_3 \textit{days-since-cutoff}_i + u_i. \tag{6}$$

When D_i is a dummy variable, η_1 captures the discontinuity in treatment take-up induced by the cutoff. Under the standard IV assumptions (including monotonicity), this discontinuity corresponds to the share of compliers in the local window. The fuzzy RDD estimand is then the ratio of the reduced-form discontinuity in outcomes to the first-stage discontinuity in treatment, $\zeta_1 = \theta_1/\eta_1$, where θ_1 is defined in the reduced-form equation below.

Data limitations may require estimating the reduced form only. When enrollment data are unavailable, the first stage cannot be directly estimated, and researchers can estimate the discontinuity in outcomes at the cutoff:

$$y_i = \theta_0 + \theta_1 after_i + \theta_2 \textit{days-to-cutoff}_i + \theta_3 \textit{days-since-cutoff}_i + e_i. \tag{7}$$

Here, θ_1 captures the reduced-form discontinuity in the outcome at the cutoff, that is, the intention-to-treat (ITT) effect of cutoff eligibility. If an estimate of the first stage η_1 is available (either from the same dataset or from an external source), one can recover a fuzzy RDD/IV estimate by scaling θ_1 by η_1. When only reduced-form estimates are available, θ_1 should be interpreted as an ITT effect. Under monotonicity and a dichotomous treatment, the reduced-form effect also provides a lower bound in magnitude on the corresponding LATE because $|\theta_1| = |\eta_1 \cdot \text{LATE}| \leq |\text{LATE}|$ when $\eta_1 \in (0, 1]$. Alternatively, one can combine a reduced-form estimate from one dataset with a first-stage estimate from another dataset to obtain a two-sample 2SLS coefficient (Dustmann et al., 2017).

Researchers may alternatively define D_i as an indicator for being on time at the time outcomes are measured. The distinction between defining D_i at school entry and at measurement arises from grade retention: when students repeat a grade, the mapping between cutoff eligibility and current cohort position is no longer one-to-one. This redefinition does not affect the reduced form (Equation 7), which continues to capture the discontinuity in outcomes at the cutoff. Instead, grade retention attenuates the first stage by weakening the relationship between cutoff eligibility and the treatment indicator.

An alternative way to implement the fuzzy RDD is to define treatment as *Starting-age*:

$$y_i = \kappa_0 + \kappa_1 \textit{Starting-age}_i + \kappa_2 \textit{days-to-cutoff}_i + \kappa_3 \textit{days-since-cutoff}_i + e_i, \tag{8}$$

with the corresponding first stage:

$$\textit{Starting-age}_i = \lambda_0 + \lambda_1 after_i + \lambda_2 \textit{days-to-cutoff}_i + \lambda_3 \textit{days-since-cutoff}_i + u_i. \tag{9}$$

This continuous specification identifies the LATE of a one-year increase in starting age at the cutoff. By contrast, the binary specification discussed above that uses D_i identifies the LATE of switching enrollment status at the cutoff (e.g., from on-time to delayed entry). Both rely on the same reduced-form discontinuity at the cutoff but differ in how the first stage scales this discontinuity. The two coincide only if the binary treatment corresponds exactly to the one-year delay margin induced by the cutoff, so that within the local window starting age is an affine transformation of the binary indicator and the first-stage discontinuities are proportional.

Beyond specification choices, the discussion of dropout policies and the identification assumptions relevant to 2SLS also applies to fuzzy RDD. A further requirement in fuzzy RDD applications is to assess whether birthdates are manipulated around the cutoff, which can be tested using the density-discontinuity test of McCrary (2008). Evidence of manipulation is context-specific and may depend on local characteristics, such as tax incentives, child care costs, or the ease with which mothers can request a non-medically-indicated cesarean section (Huang et al., 2020; Dhuey & Lipscomb, 2010; Dickert-Conlin & Elder, 2010). Manipulation may also arise from fertility planning through conception-timing choices (Clarke et al., 2019), although this mechanism is less likely to affect identification when the analysis focuses narrowly on births immediately around the cutoff date.

Another feature of fuzzy RDD is its ability to assess external validity by testing whether treatment effects differ across compliance types, as in Bertanha and Imbens (2020). If complier effects do not differ from the effects for always-takers and never-takers in that framework, the fuzzy RDD estimate can be interpreted as an average treatment effect.

3.3.3 Difference-in-differences

DiD designs exploit policy variation in school-entry cutoffs across regions and cohorts, or use control groups that are not subject to these policy changes. This offers an alternative to RDDs for addressing birth-month effects and secular trends (Görlitz et al., 2022; Bedard & Dhuey, 2012; Cornelissen & Dustmann, 2019; Peña, 2017; Fletcher & Kim, 2016; Sontheim, 2025).

Another important feature of DiDs is that they can distinguish between the $\textit{Starting-age}_i$ and $\textit{Relative-age}_i$ effects. As discussed in Section 2, the $\textit{Starting-age}_i$ effect is of primary relevance for public policy, addressing the age at which students should begin formal education. Consequently, most studies employing DiD approaches emphasize this dimension.

The DiD approach used by Cornelissen and Dustmann (2019) leverages variation in cutoff dates across the U.K., particularly in regions with multiple school-entry cutoffs. In areas with a single cutoff, all students from the same cohort begin school on the same date, typically September 1st. However, in areas with two cutoffs, there is a distinction: students born before March 1st also start on September 1st, but students born after March 1st must wait until January of the same academic year. Nevertheless, these students are eventually assigned to the same academic cohort and take assessments concurrently. Consequently, students born after March 1st face a slightly longer waiting period before starting school than students in regions with a single cutoff. Using this policy variation, Cornelissen and Dustmann (2019) compare students born before and after March 1st in areas with a single cutoff to the corresponding birth-month contrast in areas with two entry dates. In addition to this two-entry regime, the design also exploits areas with three possible entry dates, which generate analogous variation in school exposure.

Abstracting from partial compliance with entry rules, their baseline methodology can be summarized by the following specification:

$$y_{imr} = \nu_0 + \nu_1 exp_{imr} + DAGET'_{imr}\alpha + X'_{imr}N + \xi_m + \pi_r + v_{imr} \tag{10}$$

where i indexes students, m denotes month of birth, r indexes policy areas, and X_{imr} is a vector of covariates. exp_{imr} is measured in months and called "Exposure to schooling" by Cornelissen and Dustmann (2019). exp_{imr} captures the combined variation in current *Time-in-school*$_{it}$ and *Starting-age*$_i$. Students in different areas who have the same birth month enter the same cohort and therefore have the same *Relative-age*$_i$ and the same age on the nationwide test day (same *Age-at-outcome*$_{it}$).[10] However, students in different areas start at different times and therefore have different *Starting-age*$_i$. Moreover, they spend different numbers of months in school and therefore have different *Time-in-school*$_{it}$. Lastly, month-of-birth and policy-area fixed effects, ξ_m and π_r, capture seasonal variation and policy-area heterogeneity, respectively.

An alternative DiD method involves comparing areas in which the school cutoff date changes with those in which it does not. In this case, we would have:

$$y_{imr} = o_0 + o_1 treated_{mri} + X'_{imr}O + \xi_m + \pi_r + v_{imr} \tag{11}$$

where *treated* implies a policy area where the cutoff changes. In Equation (11), the parameter o_1 represents the effect of changing the cutoff date. This estimate encompasses students who are both directly and indirectly affected by the policy change, making it of primary interest to policymakers (Bedard & Dhuey, 2012). There could be further alternative counterfactuals; for example, Peña (2017) uses older cohorts as a counterfactual instead of policy areas.

[10] For some non-cognitive outcomes, the test day differs slightly across students. In these cases, *Age-at-outcome*$_{it}$ is held constant through the inclusion of fine-grained age-at-test dummies ($DAGET_{imr}$).

In the above estimation approach, indirectly affected students play an important role in identifying the effects of *Relative-age*. An illustrative example highlighted by Bedard and Dhuey (2012) pertains to a 1973 policy change in New Mexico that shifted the cutoff date from January 1st to September 1st. As a result, children born in New Mexico between September 1st and December 31st had to wait an additional year before starting school. This change generated various age effects on the outcomes of interest for the affected students. However, for those born in January, the policy adjustment only meant that they would no longer be the oldest in their class. In the untreated areas where the school cutoff date was already January 1st, no changes occurred for January-born students. By focusing exclusively on January-born students and using Equation (11), o_1 solely identifies the *Relative-age$_i$* effect out of the production function in Equation (2).

Policy-induced shifts in school entry age can generate cohort-level dynamics that influence both the educational environment and student composition. When cohorts are older at school entry, schools may adjust instructional practices or the curriculum (Bedard & Dhuey, 2012; Peña, 2017). Additionally, during the transition period following the policy change, cohort sizes may shrink if students are required to start later or increase if students are allowed to start earlier.

4 Outcomes

Building on the conceptual framework in Section 2 and the methodological discussion in Section 3, this section reviews the empirical evidence on outcomes related to age at school entry (ASE) across the life course. We organize the review into six domains: (i) educational performance and attainment, (ii) labor-market outcomes, (iii) social relationships, (iv) mental and physical health, (v) criminal behavior, and (vi) family formation. For each domain, we examine how estimated ASE effects are plausibly related to the four ASE components (*Starting-age*, *Relative-age*, *Age-at-outcome*, and *Time-in-school*) and to the six channels outlined earlier. These components are often mechanically and behaviorally interdependent, and most empirical estimates identify bundled effects rather than isolating individual mechanisms.

Where possible, we identify ASE components and channels most likely to drive the estimated effects and how these relationships vary across institutional settings. We identify the most robust and policy-relevant findings, synthesize mechanisms, and flag open questions and gaps for future research. Other details are discussed in the appendices. Appendix B reports a table summarizing key details for each paper discussed in this section: outcome studied, country and data, methodology, main effects, and heterogeneity results. Appendix C reports school-entry cutoff dates per country and, when applicable, per administrative area. Appendix D provides the complete set of readings that informed the exposition.

While Section 2 distinguishes between individual and policy-level effects (for example, parental redshirting decisions versus changes in cutoff dates), most causal studies in this review do not estimate the effect of redshirting itself. Instead, they exploit school-entry cutoff rules to identify the effects of *Starting-age*, mostly in combination with other ASE components and typically for compliers, through quasi-experimental approaches reviewed in Section 3. Thus, the estimated effects reflect ASE effects on individuals who followed the expected enrollment path, not on those whose parents chose to delay entry. Causal identification of redshirting effects requires exogenous variation, which is rarely available in the literature (Ricks, 2025). Finally, the magnitude of the estimated effects cited in this section generally refers to a one-year difference in ASE.

4.1 Education

Education is one of the most extensively studied domains in the ASE literature. ASE influences educational outcomes through multiple components and channels, including Skill Acquisition, Self-Esteem, Parental/Teacher Investments, and Selection into Programs. These mechanisms shape two key dimensions: (i) academic performance, often measured by standardized test scores (Section 4.1.1); and (ii) schooling progression and attainment, such as grade repetition, tracking, dropout, and highest level of education completed (Section 4.1.2).

4.1.1 Test scores and other measures of cognitive skills

ASE influences educational performance through three channels: Skill Acquisition, Self-Esteem, and Parental/Teacher Investments. These mechanisms are, in turn, shaped by three core ASE components: *Starting-age*, *Relative-age*, and *Age-at-outcome*.

A consistent empirical finding across countries and institutional contexts is that, for a given amount of schooling, older entrants outperform their younger classmates on standardized tests and cognitive assessments. These advantages are consistent with mechanisms related to Skill Acquisition and are more pronounced at earlier stages of schooling. Most studies report economically meaningful effects ranging from 0.1 to 0.8 standard deviations (SD), depending on the grade or age at which outcomes are measured and the institutional context. These effects tend to be larger in early grades, but decline over time. For example, using data from Tennessee's Project STAR, Cascio and Schanzenbach (2016) show that, at the end of kindergarten, a one-year age difference, reflecting the composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*, increases test scores by 0.67 SD in a conventional two-stage least squares (2SLS) estimation. By contrast, the estimate falls to 0.22 SD eight years later. Bedard and Dhuey (2006), using cross-country data and the same empirical approach (estimating the same bundle of ASE components), show that a one-year age difference in grade 4 increases test scores in Math by approximately 0.21-0.47 SD, with the effect narrowing to 0.02-0.39 by grade 8. A similar pattern emerges for science test scores. In contrast, Dhuey et al. (2019), using Florida administrative data, pooling scores across grades and subjects, and exploiting cutoff-based variation in a fuzzy regression discontinuity design (RDD), report more stable effects across grades 3–8 (about 0.20 SD).

The magnitude and persistence of these effects likely depend on how initial maturity advantages (*Starting-age*) interact with cumulative skill-building (SKILL ACQUISITION) and external feedback from educators (PARENTAL/TEACHER INVESTMENTS). While early advantages are well documented, the persistence of these effects remains uncertain. One explanation is that older children, due to their developmental head start, benefit from stronger SKILL ACQUISITION, either through cumulative advantage or enhanced learning efficiency. The first dynamic arises when early maturity leads to better early performance, which in turn encourages higher expectations and engagement (path dependence). The second reflects the idea that older children may be better positioned to absorb instruction, consistent with theories of dynamic complementarity in skill formation (Cunha & Heckman, 2007; Attanasio, 2026).

Yet the long-term implications of these early differences remain contested. A central concern is that many estimated effects at young ages may reflect age-related maturation and development or learning outside of school, rather than a causal impact of *Starting-age* on human capital accumulation. In this case, outcomes would differ simply because older children are further along in normal developmental trajectories at the time of assessment, even if schooling inputs and learning at school were identical. This interpretation implies that early gaps should shrink mechanically as children age and the relative importance of small age differences declines.

Consistent with this view, Elder and Lubotsky (2009) and Lubotsky and Kaestner (2016) argue that initial advantages fade as schools or families adjust investments, and as maturation effects dissipate. Similarly, Cornelissen and Dustmann (2019) find no significant effects on cognitive test scores by age 11, and Nam (2014) reports minimal effects at the upper-secondary level. These patterns are consistent with a "maturity" channel and do not, on their own, establish sustained differences in skill formation attributable to school entry timing.

In contrast, evidence of impacts that persist into late adolescence or adulthood is harder to reconcile with a pure maturation interpretation and is therefore more informative about longer-run human capital consequences. Peña (2020) documents effects persisting beyond age 18, and Görlitz et al. (2022) finds measurable differences in receptive vocabulary (though not in math or text comprehension) among adults aged 23 to 71. The mechanisms emphasized in these studies also point away from a simple age-related developmental model. Peña (2020) highlights investment and self-concept channels: (i) within-class comparisons generate persistent differences in self-concept, (ii) these differences induce endogenous investments even if initial skill differences were small or partly illusory, and (iii) investment gaps become self-reinforcing through feedback effects on confidence and achievement. Görlitz et al. (2022), by contrast, interpret their results as arising primarily through selection into educational programs: early age advantages increase assignment to academic tracks, and tracks differ substantially in curriculum content and language exposure. While basic math and reading comprehension are taught across tracks, receptive vocabulary is especially fostered in academic tracks, which can plausibly generate long-run differences that cannot be attributed to maturation alone.

A growing body of research links these conflicting findings to different bundles of ASE components. For instance, Black et al. (2011) estimate that *Age-at-outcome* increases IQ at age 18 by 0.20 SD per additional year, whereas *Starting-age* has a small negative effect. They use a 2SLS strategy that combines the school-entry cutoff with the conscription testing time: the entry rule instruments *Starting-age*, while the conscription timing instruments *Age-at-outcome*. Because many conscripts are still enrolled at the time of testing, these estimates combine *Starting-age* with current *Time-in-school*. Differently, Crawford et al. (2014) implement an RDD that compares test scores taken in the same grade with scores taken at the same age; this comparison suggests that most ASE differences in achievement are mainly driven by *Age-at-outcome*, with limited additional contribution from *Relative-age* or *Starting-age*. However, this decomposition relies on functional-form and exclusion assumptions to work around the collinearity among components. With data from Project STAR, Cascio and Schanzenbach (2016) find a negative *Relative-age* effect on test scores. The isolated effect of this ASE component is identified by exploiting variation arising from the random assignment of students to classrooms, while holding *Starting-age* and *Age-at-outcome* fixed. Following Section 3, these studies illustrate how different quasi-experimental strategies speak to specific bundles of ASE components, rather than to a specific component.

Increasing schooling by starting earlier (which jointly lowers *Starting-age* and increases current *Time-in-school*), rather than by extending the total duration of schooling through modifying compulsory schooling laws, has been found to increase short-run test scores, measured in the early years of schooling (Leuven et al., 2010; Cornelissen & Dustmann, 2019). However, the long-run results differ (Cascio & Lewis, 2006; Cornelissen & Dustmann, 2019). These long-term findings could potentially be explained by the presence of *Relative-age* effects in Cascio and Lewis (2006), whereas such effects are absent in Cornelissen and Dustmann (2019). Delaying school-start times, while adjusting the end of compulsory schooling to keep total years constant, appears to improve short-run test scores, though the effect in grade 12 is less clear (Fletcher & Kim, 2016).

In addition to SKILL ACQUISITION, a second important channel is PARENTAL/TEACHER INVESTMENTS. Parents contribute both genetically and behaviorally, and their responses to children's maturity levels can amplify or offset initial differences (Biroli et al., 2026). Some parents may increase investment in older children (reinforcing advantage), whereas others may compensate for younger entrants. Empirical evidence is mixed: Celhay and Gallegos (2025) find greater investment in older children, while Fredriksson et al. (2024) document compensatory responses. These patterns vary by socioeconomic status (SES), with high-SES families more likely to redshirt or invest in ways that amplify maturity-based advantages (Berniell & Estrada, 2020; Ricks, 2025). On the school side, teachers may offer differentiated attention, smaller class placements, or enrichment based on observed ability, which can itself be influenced by ASE. Together, these findings underscore the need to disentangle the direct effects of ASE from parental and school responses, as each mechanism carries distinct implications for how educational inequalities emerge and persist.

The third channel, SELF-ESTEEM and other non-cognitive skills, may also reinforce ASE effects on academic performance. For example, Crawford et al. (2014) find that older students are more likely to perceive themselves as academically competent. Relatedly, as documented by Dee and Sievertsen (2018) in Denmark, older entrants exhibit higher self-regulation (measured by inattention/hyperactivity), a non-cognitive trait that strongly predicts academic performance. This is consistent with the literature showing that students who feel more confident tend to exert greater effort and perseverance (Peña & Duckworth, 2018). There is also some evidence that non-cognitive advantages often persist longer than cognitive ones, influencing behavioral traits and educational trajectories well into late childhood, adolescence, and even into adulthood long after schooling is completed (Cornelissen & Dustmann, 2019; Mühlenweg et al., 2012; Barabasch et al., 2026). Cognitive outcomes are easier to catch up to, whereas non-cognitive outcomes are less tied to the curriculum and operate through behavioral feedback loops that reinforce themselves over time.

The role of Self-Esteem is also consistent with models of skill formation, where non-cognitive skills enhance the productivity of cognitive ones (Cunha & Heckman, 2007), and with the inherently multidimensional nature of early childhood development (Attanasio, 2026). Understanding how this channel interacts with ASE components remains an open question for future research. Within this context, there is a strong conceptual link between Self-Esteem and *Relative-age*, which is often reflected in other outcomes, such as social relationships (Section 4.3).

In sum, higher ASE is associated with stronger early academic performance, operating through multiple reinforcing channels. But the persistence, magnitude, and drivers of these effects vary across contexts and cohorts. Disentangling the mechanisms and understanding their interactions with institutional settings and family responses remain critical areas for future research.

4.1.2 School progression and attainment

ASE's short-term effects on test scores often translate into medium- and long-term educational trajectories. These effects operate through multiple channels: Skill Acquisition, Self-Esteem, Parental/Teacher Investments, Selection into Programs, and School Dropout Policies. These channels shape grade progression, track placement, and educational attainment.

The key idea is that early differences in cognitive and non-cognitive development may trigger divergent responses from families and schools, which compound over time. These responses can lead to inefficient or inequitable educational allocations, setting older and younger entrants on systematically different paths. These compounding responses may be driven not only by observed performance but also by internalized signals about ability and belonging, consistent with the role of Self-Esteem in shaping motivation and academic identity.

Extensive research examines the consequences of ASE through tracking. Older children are more likely to be recommended for gifted programs or placed in advanced academic tracks, likely due to both developmental maturity and teacher perceptions of readiness (Dhuey et al., 2019). Conversely, younger children are more likely to be retained, assigned to special education programs, or streamed into vocational or non-academic tracks. These tracking decisions are often influenced by PARENTAL/TEACHER INVESTMENTS, as families and educators respond to maturity-based cues and performance proxies, many of which are shaped by the child's *Relative-age*.

The structure and timing of tracking play a central role in the persistence of early gaps. Where tracking begins early and mobility between tracks is limited, ASE effects are likely to influence final educational attainment and later-life outcomes (Section 4.2). For example, Austria, Germany, and the Netherlands implement early tracking, and ASE effects are initially observed in all three. However, in Germany and the Netherlands, younger entrants can move into higher academic tracks later on, mitigating long-run differences (Oosterbeek et al., 2021; Dustmann et al., 2017; Mühlenweg & Puhani, 2010). In Austria, where track mobility is more restricted, ASE effects persist and are reflected in long-run labor-market earnings disparities (Schneeweis & Zweimuller, 2014; Zweimüller, 2013). In systems without tracking, the impact of ASE on attainment appears minimal when exploiting variation that includes *Starting-age* and *Relative-age* (Fredriksson & Öckert, 2014), or average increases in *Starting-age* due to cutoff shifts (Bedard & Dhuey, 2012).

The relevance of the tracking system is highlighted in Fredriksson and Öckert (2014). This is one of the few studies that estimates how the ASE effects vary with different tracking regimes within the same country (Sweden), comparing early selective systems (with ability tracking in grade 5 or 7) with comprehensive systems (that postpone tracking until age 16). Their evidence shows that ASE effects on educational attainment are stronger under the selective systems and weaker once tracking is postponed. Notably, while younger entrants are disadvantaged by tracking, older entrants can face negative consequences for different reasons. Cotofan et al. (2022) show that older entrants, who are placed into higher tracks due to greater maturity rather than ability, may struggle academically and perform worse than their peers. This highlights a potential cost of maturity-driven placement: when *Starting-age* rather than underlying ability drives SELECTION INTO PROGRAMS, students may be misallocated, raising concerns about both efficiency and equity. The extent of such misallocation depends critically on the rigidity of tracking systems and opportunities for later correction.

Compulsory schooling laws also shape ASE effects by altering dropout incentives. Where students may leave school upon reaching a minimum age, as in the U.S., older entrants are more likely to drop out before completing their final year of secondary education (Angrist & Krueger, 1991). In contrast, in systems requiring completion of a specific grade level, this risk is reduced. These institutional features interact with ASE components, particularly (total) *Time-in-school* and *Starting-age*, to either amplify or dampen long-run educational effects. These laws illustrate how SCHOOL DROPOUT POLICIES, interacting with age-based enrollment timing, can moderate or amplify ASE effects on ultimate educational attainment, particularly when students become eligible to exit before reaching key educational milestones.

4.2 Labor market

ASE can influence labor market outcomes through multiple pathways. The most prominent mechanisms operate via differences in educational attainment, driven by skill accumulation and selection, and through timing effects related to graduation, work experience, and retirement. In addition, ASE may shape occupational preferences, leadership outcomes, and family-level spillovers. Several of these pathways map onto specific ASE channels, particularly SKILL ACQUISITION, DELAYED SCHOOL EXIT, and SELF-ESTEEM, which jointly shape educational and career trajectories.

ASE effects on education-driven labor market outcomes are shaped by two institutional features: (i) timing and rigidity of academic tracking, and (ii) selectivity of tertiary education systems.

Continuing the prior discussion of how early tracking shapes educational attainment, early track assignment amplifies initial ASE advantages and translates them into labor market gaps. In Austria, younger entrants attain lower education levels and are more likely to enter blue-collar occupations, resulting in lower earnings (Zweimüller, 2013). By contrast, in Germany and the Netherlands, no persistent ASE effects on labor market outcomes are observed (Dustmann et al., 2017; Oosterbeek et al., 2021). These comparisons illustrate how the flexibility of tracking systems conditions whether early educational disadvantages propagate into adulthood. However, even when track switching is formally possible, later correction may depend on parental advocacy or teacher recommendations.

Tertiary education systems moderate ASE effects. In Mexico, where university admissions are highly selective, older entrants are more likely to access higher education and earn more (Peña, 2017). In contrast, in countries with comprehensive, non-selective post-secondary systems, such as Norway or Canada, there is little consistent evidence of ASE effects on college entry or labor-market outcomes (Balestra et al., 2020; Black et al., 2011; Dobkin & Ferreira, 2010; Fredriksson & Öckert, 2014). Cross-country variation in earnings effects reflects broader differences in education systems and labor-market returns. First, selective education systems are more prevalent in middle-income countries, where ASE effects on attainment and labor-market sorting are more pronounced. Second, economic returns to education are higher in these contexts: Psacharopoulos and Patrinos (2004) estimate average returns of approximately 10.8% in low- and middle-income countries, compared with 7.4% in high-income countries. Even if ASE effects on education were constant across countries, their earnings implications would vary depending on these structural factors.

Additionally, ASE affects labor-market outcomes through timing mechanisms. Older entrants typically graduate and enter the workforce later than their younger peers, implying at least three effects. First, delayed graduation reduces lifetime earnings by one year of foregone income (Black et al., 2011; Fredriksson & Öckert, 2014; Oosterbeek et al., 2021). Second, older graduates accumulate less work experience over time, which can lead to lower cumulative earnings, particularly in the first decades of working life (Oosterbeek et al., 2021). Third, delayed entry can postpone retirement, especially in systems where eligibility depends on years of service rather than age. Evidence from

Sweden and Norway shows that older school entrants tend to retire later than younger counterparts (Fredriksson & Öckert, 2014; Larsen & Solli, 2017). These effects reflect the operation of the Delayed School Exit channel, whereby a later start shifts educational and labor-market timing forward, with implications for cumulative experience and retirement age.

School Dropout Policies can also moderate these effects. In systems with age-based school-leaving thresholds, older students may become eligible to drop out before completing secondary education, thereby reducing total years of schooling (Larsen & Solli, 2017).

Most labor-market studies rely on cutoff-based designs (Section 3). When school-entry laws are exploited in RDD frameworks (Fredriksson & Öckert, 2014; Dobkin & Ferreira, 2010), the discontinuity simultaneously shifts *Starting-age*, *Relative-age*, and, in systems with age-based School Dropout Policies, total *Time-in-school*, while differences due to *Age-at-outcome* are minimal. These estimates are therefore best viewed as composite ASE effects. Some studies also examine earnings across multiple *Age-at-outcome* groups, such as Black et al. (2011). Here, the instrument moves *Starting-age* as well as earlier *Relative-age*, and the balance between *Time-in-school* and potential labor-market experience, while *Age-at-outcome* is held constant.

Beyond selection and timing, ASE may influence labor-market outcomes through non-cognitive traits and social perceptions. Several studies hint at this channel. Older students are more likely to become CEOs (Du et al., 2012), political representatives (Muller & Page, 2016), or more effective financial decision-makers (Bai et al., 2019). These patterns are often attributed to early advantages in confidence, leadership roles, or risk preferences. However, findings on ASE and risk attitudes are mixed. For example, Page et al. (2019) show that older entrants take fewer risks in ambiguous, externally controlled settings (for example, lab-based tasks), but engage more in self-directed risk behaviors, such as reckless driving. This suggests that ASE may shape domain-specific risk preferences rather than a general propensity for risk-taking.

ASE may also spill over to family members. Landersø et al. (2020) show that mothers of older entrants are more likely to be employed and earn higher wages around the time of school start, with older siblings of these children performing better in school. The opposite pattern is observed among mothers and older siblings of children who start school earlier, suggesting that these children may require more parental attention and resources, which could crowd out investment in their siblings and reduce maternal labor supply.

Limited available evidence suggests that delaying school start for all students can raise earnings in some institutional contexts (Bedard & Dhuey, 2012). It is plausible that long-run benefits operating through improvements in cognitive and non-cognitive skills may outweigh the short-run earnings losses associated with delayed labor-market entry. Nonetheless, no study has tested this hypothesis directly.

4.3 Social relationships

As in earlier domains, most estimates in these areas reflect composite ASE effects rather than isolated channels. Self-perception and *Relative-age* may also serve as mechanisms through which ASE shapes individuals' social relationships; however, additional factors likely contribute as well. Dhuey and Lipscomb (2008) find that the youngest students are less likely to identify as leaders and to develop leadership skills, while Page et al. (2017, 2019) show that older entrants are more competitive. These patterns suggest the presence of Self-Esteem gaps favoring older classmates (Peña, 2020; Peña & Duckworth, 2018). More broadly, individual differences in non-cognitive skills, shaped in part by ASE, likely play a role in shaping peer dynamics and social development.

Studies also document gaps in social outcomes. Younger entrants report fewer and less stable friendships during adolescence (Fumarco & Baert, 2019), and fewer romantic or sexual experiences during early adulthood (Pellizzari & Billari, 2012). These effects can be sizable. In Italy, the youngest students in a cohort report 35% fewer sexual encounters (roughly two versus an average of three) and are 18 percentage points less likely to be in a stable relationship (Pellizzari & Billari, 2012).[11]

Another dimension of social experience is peer victimization. Low-ASE students who are younger and physically less mature than their classmates are more likely to be bullied or excluded. Mühlenweg (2010) and Ballatore et al. (2020) suggest that physical immaturity may increase vulnerability to aggression from peers. These effects are economically meaningful: Mühlenweg (2010) finds that being born just after the school-entry cutoff reduces the likelihood of victimization by eight percentage points from a 55% baseline—equivalent to a 15% relative reduction. These findings highlight an underexplored pathway through which *Relative-age*, operating via both the Self-Esteem and Selection into Programs channels, may shape students' roles in peer hierarchies and disciplinary environments.

Most studies in this section use a 2SLS approach, with the instrument being assigned or expected *Relative-age*, obtained by combining information on birth dates and school-entry cutoffs. For in-school outcomes, this typically generates composite effects of *Starting-age*, *Relative-age*, and *Age-at-outcome*, with *Time-in-school* largely held fixed by grade. By contrast, Peña and Duckworth (2018) and Fumarco and Baert (2019) explicitly distinguish *Relative-age* from *Age-at-outcome* by exploiting classroom-level variation in age composition.

A different approach is taken by Page et al. (2017, 2019), who combine experimental data with RDDs and 2SLS. In their framework, the estimated effect is conventionally interpreted in terms of *Relative-age*. However, Section 3 and above studies highlight that, strictly speaking, a fully "pure" *Relative-age* effect would require leverage on classroom age distribution; when this is not observed, estimates will generally bundle *Relative-age* with, at least, *Starting-age*. More broadly, we hope

[11] Pellizzari and Billari (2012) rule out the effect of *Age-at-outcome* and focus on compensatory behaviors. There is evidence that younger entrants devote more time to studying in tertiary education to close human capital gaps.

that making the four ASE components explicit will help future work describe more precisely which combinations of age-related mechanisms are being identified.

While the existing literature provides valuable insights, several avenues for expansion remain, two of which appear particularly promising. First, no study has examined the long-term consequences of ASE for social networks, which are especially important in aging societies, where social isolation poses a growing concern. Second, most research on peer victimization focuses on victims. Yet perpetrators may also experience long-run consequences, including disciplinary actions or legal involvement, with potential implications for trajectories of juvenile crime, as discussed in Section 4.6.

4.4 Mental health and developmental concerns

There is a broad consensus that higher ASE reduces the likelihood of being diagnosed with Attention Deficit Hyperactivity Disorder (ADHD) (Section 4.4.1). Similar patterns are observed for other mental health outcomes (Section 4.4.2).

The dominant mechanism behind these effects is likely *Relative-age*. Behavioral assessments in school settings are typically benchmarked against classmates, without sufficient adjustment for developmental maturity. As a result, younger entrants, who may appear inattentive, restless, or less socially adjusted, are more likely to be flagged for behavioral issues or referred for special services (Dhuey & Lipscomb, 2010; Nicodemo et al., 2024). These early assessments often have downstream consequences, including diagnosis, medication, and placement in support programs. These processes most clearly reflect the influence of the Selection into Programs channel, whereby behavioral evaluations, often shaped by age-based comparisons, affect the likelihood of receiving diagnostic labels or educational supports.

While Sections 4.1, 4.2, and 4.3 feature a range of econometric approaches that closely reflect the toolbox discussed in Section 3, most of the studies reviewed in this and the following sections on physical health, crime, and family outcomes (Sections 4.5, 4.6, and 4.7) rely on RDDs, sometimes combined with *Age-at-outcome*-specific analyses and/or cross-jurisdiction variation in cutoff dates. As a result, the estimates typically capture a bundle of ASE components rather than isolated effects, a pattern that is also evident in Online Appendix B.

4.4.1 Attention deficit hyperactivity disorder

The association between ASE and ADHD diagnosis is one of the most well-established findings in the ASE literature, including in medical research (Layton et al., 2018). Although the estimated magnitude varies across contexts, the direction of the effect is consistent: younger entrants are significantly more likely to be diagnosed with ADHD.

These effects can be strikingly large. Nicodemo et al. (2024) find that in England, younger entrants have up to twice the ADHD diagnosis rate of their older classmates. Elsewhere, effects are more modest but still meaningful. For example, Schwandt and Wuppermann (2016) document a one percentage point increase in ADHD diagnoses for young German entrants compared to older entrants. This translates into a 22% increase relative to a 5% baseline.

These two studies go beyond standard investigations in two ways. Schwandt and Wuppermann (2016) analyze the role of demand-side and physician-supply-side factors, with the latter being an unexplored "institutional" aspect. They find that the competitive physician market is not a pulling factor for ADHD misdiagnoses. Nicodemo et al. (2024) is one of the few studies on health outcomes to disentangle various ASE components by leveraging different types of variation. First, in England, the cutoff date to determine *Starting-age* is August 31st. Second, *Age-at-outcome* reflects the child's age when ADHD diagnoses are measured. Third, current *Time-in-school* captures, as usual, how much schooling a child has accumulated by a given age. Fourth, they can estimate *Relative-age* with the child's position in the classroom age distribution. Their findings show that while *Starting-age*, *Age-at-outcome*, and *Time-in-school* play minor roles, *Relative-age* is the dominant factor, with younger children in a class significantly more likely to be diagnosed with ADHD due to peer-comparison effects.

These diagnostic differences carry important downstream consequences. First, they are reflected in medical treatment patterns: younger entrants are significantly more likely to be prescribed stimulant medications. Second, these treatments may reinforce educational disparities. Currie et al. (2014) show that medicated students tend to perform worse in school, and may be tracked into less demanding academic programs. On the other hand, the evidence suggests that ADHD is strongly detrimental to educational performance. Dee and Sievertsen (2018) show that inattention/hyperactivity affects test scores more than any other component of the Strengths and Difficulties Questionnaire (a diagnostic tool widely used in clinical practice). Taken together, these findings emphasize the need for more effective targeting of stimulant medications. Third, multiple studies raise concerns about the long-term health effects of these medications, including cardiovascular risks, mood instability, and growth deficits (Currie et al., 2014; Elder, 2010). They also appear to reduce the likelihood of post-secondary education. Together, these outcomes highlight a compounding sequence in which the SELECTION INTO PROGRAMS and PARENTAL/TEACHER INVESTMENTS channels reinforce one another—misdiagnosis can affect both how students are treated and the resources they receive. ASE-driven diagnoses may also generate spillovers beyond the individual child. Persson et al. (2025) find evidence of a "snowball effect," whereby an ADHD diagnosis in one child increases the probability of diagnosis in younger relatives, such as cousins, through family medical history pathways, even when those children are not young for their cohort.

As seen above, the institutional context plays a central role in shaping these effects. Furzer et al. (2022) and Elder (2010) show that in the U.S., teachers are more likely than parents to report behavioral concerns for younger entrants. Because teachers observe a wider within-classroom age range, they may be more susceptible to making age-based comparisons. In contrast, parents may lack this frame of reference and report fewer concerns. In Denmark, where ADHD can only be diagnosed by medical specialists, Dalsgaard et al. (2012) find no significant ASE effect on diagnosis. This suggests that minimizing the role of subjective, school-based assessments and centralizing diagnostic authority may help reduce ASE-related diagnostic bias.

4.4.2 Other mental health and developmental concerns

Younger entrants are more likely to be identified with a range of mental health and developmental concerns beyond ADHD. They are disproportionately referred for special education services (Dhuey & Lipscomb, 2010), more likely to be classified with general emotional or behavioral disorders (Black et al., 2011), and more frequently referred to school psychology services (Balestra et al., 2020). At the same time, they are less likely to be identified as gifted (Dhuey et al., 2019), and more likely to be diagnosed with dyslexia (de Gage et al., 2025). They also tend to receive lower temperament ratings across several dimensions (Mühlenweg et al., 2012).

These effects can be substantial. Dhuey and Lipscomb (2010) find that younger entrants in U.S. cohorts are between 24 and 60% more likely to receive special education services.[12] In France, de Gage et al. (2025) find that younger entrants are 64% more likely to begin speech therapy. The strength of these associations varies by country; for example, in Switzerland, Balestra et al. (2020) find no significant relationship between ASE and referrals to school psychology services, suggesting institutional or cultural factors may moderate diagnostic thresholds.

Self-reported mental health also shows consistent disparities. Younger entrants report lower life satisfaction and more frequent psychosomatic symptoms (Fumarco et al., 2020). These effects may reflect cumulative exposure to social comparison stressors or negative feedback over time, reinforcing perceptions of underperformance or difference. These internalized effects likely operate through the Self-Esteem channel, suggesting that the psychological burden of being younger entrants may accumulate even in the absence of a formal diagnosis.

Despite a broad literature on short-run effects, the long-term mental health consequences of ASE remain underexplored. Only a handful of studies follow students into adulthood. Balestra et al. (2020) examine the likelihood of receiving disability insurance later in life but find inconclusive results. In contrast, Matsubayashi and Ueda (2015) and Thompson et al. (1999) document elevated suicide rates among Japanese and Canadian adults who were among the youngest in their school cohorts. These findings suggest that early maturity disadvantages may persist and manifest as heightened risks of depression and severe mental health outcomes later in life.

4.5 Physical health

Younger entrants report worse physical health during adolescence, and these disparities appear to persist into adulthood (Arnold & Depew, 2018; Fumarco et al., 2020). While the mecha-

[12] This range assumes a linear monthly effect; Dhuey and Lipscomb (2010) estimate that each additional month of age reduces the probability of special education placement by 2 to 5%.

nisms behind this relationship remain poorly understood (Anderson et al., 2011), emerging evidence suggests it may reflect a combination of poorer mental health (Section 4.4), more frequent weight-related issues (Section 4.5.1), and greater engagement in risky health and sexual behaviors (Sections 4.5.2 and 4.5.3). Together, these outcomes are likely shaped by the SELF-ESTEEM and SKILL ACQUISITION channels, as lower confidence and delayed physical development may reduce participation in healthy behaviors or hinder the acquisition of positive health habits.

4.5.1 Weight issues

Younger entrants are more likely to experience weight-related problems, potentially due to less healthy diets and lower levels of physical activity. Compared with their older classmates, they tend to consume fewer fruits and vegetables and more sugary foods and drinks. These dietary disparities appear to stem from social comparison mechanisms and are influenced by socioeconomic status (Carpenter & Churchill, 2025; Fumarco et al., 2026; Levasseur, 2022). Low-ASE students also participate less frequently in physical activity and have more screen time (Fumarco & Schultze, 2020), likely due to their lower *Relative-age*, which can negatively affect perceived performance, confidence, and motivation. These patterns are shaped by both the SELF-ESTEEM channel, where lower maturity diminishes self-confidence, and the SELECTION INTO PROGRAMS channel, through which younger entrants may be excluded from organized physical enrichment opportunities.

These behavioral differences contribute to observable disparities in physical health. For example, Fumarco et al. (2026) and Carpenter and Churchill (2025) find that younger entrants are significantly more likely to be overweight, with an effect size comparable to the gap between students from high- versus low-SES households.

Unlike most studies in Section 4.5, Fumarco and Schultze (2020), Fumarco et al. (2026), and Carpenter and Churchill (2025) use a 2SLS methodology that allows them to identify *Relative-age* in isolation by leveraging variation between individuals and classrooms.

Although short-run associations between ASE and diet, activity, and body weight are well documented, long-run causal evidence in this domain remains limited.

4.5.2 Risky health behaviors

ASE is also associated with a range of risky health behaviors, particularly substance use. Johansen (2021) finds that younger entrants are 1.8 percentage points more likely to experience alcohol poisoning before age 20—an increase of nearly 60% relative to the baseline. The same study also reports higher alcohol consumption among younger entrants. Other work shows that older entrants are less likely to smoke in their mid-30s (Bahrs & Schumann, 2020), and that female younger entrants are more likely to use marijuana (Argys & Rees, 2008).

Despite growing interest, this literature remains underdeveloped. First, only Lopez-Mayan et al. (2024) has explored ASE effects on other addictive behaviors, such as heavy drug use or gambling. Second, few studies examine the interaction between ASE and local laws more broadly. One exception is Routon and Walker (2023), who examine U.S. college students and find that older entrants drink less alcohol, despite becoming legally eligible to drink earlier. This finding suggests that maturity effects may outweigh the effects of legal access. This pattern parallels school dropout behavior, in which eligibility thresholds affect only a subset of the cohort. These findings underscore the potential role of the DELAYED SCHOOL EXIT and SCHOOL DROPOUT POLICIES channels in shaping exposure to unsupervised settings in which risky behaviors emerge.

Although ASE has been linked to poor health behaviors in adolescence and young adulthood, no existing studies examine long-term outcomes such as obesity-related disease, cardiovascular problems, or substance dependence in later life. This represents a key gap in the literature.

4.5.3 Risky sexual behaviors

Early sexual activity and related outcomes are influenced by ASE (Black et al., 2011; Johansen, 2021; McCrary & Royer, 2011; Peña, 2017; Pellizzari & Billari, 2012). Most studies report that younger entrants are more likely to engage in early or unprotected sexual activity and to experience higher teen fertility rates. Moreover, Johansen (2021) shows that women who are younger entrants in their cohort are three percentage points more likely to have an abortion before the age of 20. This corresponds to an increase of more than 35% relative to the baseline rate.

Nonetheless, evidence across contexts remains mixed. This mixed evidence likely reflects institutional variation. For instance, studies from the U.S. and Mexico tend to find stronger effects than those from Scandinavia, where more comprehensive sexual education and welfare policies may mitigate the consequences of early sexual behavior. Sample composition may also contribute. For example, Pellizzari and Billari (2012) focus on Bocconi University students, who tend to come from high-SES backgrounds.

Beyond institutional factors, the channels and components outlined in our framework may help explain age-related variation in sexual behavior. However, the exact mechanisms remain speculative. *Relative-age* likely affects sexual behavior through its influence on social norms and peer dynamics (Johansen, 2021). Moreover, lower academic rank and school engagement of younger entrants may be associated with reduced self-efficacy or riskier behavior patterns (Elsner & Isphording, 2018). While these hypotheses remain largely speculative and warrant further empirical testing, they are conceptually consistent with ASE affecting outcomes working through the SELF-ESTEEM and SELECTION INTO PROGRAMS channels. These channels influence perceived social rank, peer affiliation, and autonomy over health decisions.

4.6 Crime

ASE has been linked to juvenile and adult criminal behavior, though the direction and magnitude of effects vary across settings. In some contexts, younger entrants appear more likely to engage in delinquent activity. In other cases, it is the older entrants who face greater long-run risk. These patterns are consistent with the operation of several channels, including SELF-ESTEEM, SKILL ACQUISITION, and SCHOOL DROPOUT POLICIES, which together influence how adolescents interact with formal institutions and respond to risky opportunities. The relationship between ASE and crime seems sensitive to institutional features such as school-leaving laws and grade progression policies, which can moderate or even reverse the long-run effects.

In early adolescence, younger entrants are more likely to engage in delinquent behavior, potentially due to the "incapacitation effect." Older students tend to be more engaged in school and spend less unsupervised time outside structured environments. Younger entrants may also face lower opportunity costs of crime, weaker institutional attachments, and less optimistic expectations about their future (Cook & Kang, 2016; Depew & Eren, 2016). These mechanisms underscore the role of the SELF-ESTEEM channel, as perceived underperformance among younger entrants can reduce their motivation and connection to school-based norms.

Whether these early disadvantages persist into adulthood depends largely on SCHOOL DROPOUT POLICIES. In systems with age-based eligibility rules, older entrants may drop out before completing their studies, thereby forgoing the protective effects of extended schooling. This may increase long-run criminal involvement, particularly among individuals with lower patience or long-term orientation (Cook & Kang, 2016). In contrast, grade-based exit thresholds, as in Denmark, require all students to complete the same schooling, thereby limiting dropout-driven reversals and preserving disadvantage for younger entrants (Landersø et al., 2017).

Studies on the effects of ASE on crime outcomes are characterized by a particular identification challenge: legal regimes vary with *Age-at-outcome*. In practice, because criminal responsibility, prosecutorial treatment, and even the legal existence of certain offenses depend on age and jurisdiction, identical propensities to engage in criminal behavior may translate into different observed crime rates and legal consequences at different ages. Thus, ASE gaps in criminal behavior may become observable only once individuals cross specific legal thresholds. In extreme cases, it is possible for younger entrants to exhibit higher latent criminal propensity, yet for older entrants to display higher observed crime rates simply because they are legally punishable. In this sense, being older or younger at a given calendar time also alters exposure to legal institutions. To address this issue and sharpen interpretation, some studies conduct RDD analyses by exact age and examine how the crime-age profile shifts across cohorts. This allows them to assess whether ASE operates through delayed onset or the persistence of criminal activity (Landersø et al., 2017).

ASE-related effects on crime also vary across demographic groups. Depew and Eren (2016) find that ASE benefits are strongest for Black females in Louisiana. Peña (2019) explores ASE effects on incarceration among Black males convicted of drug-related offenses, showing that those with higher ASE have significantly lower incarceration rates in their 30s. These long-run differences appear to be shaped by early deficits in non-cognitive skills among low-ASE students, which lower their labor-market prospects and increase the appeal of illicit activity (Peña & Duckworth, 2018).

These effects reinforce the importance of early investments through Skill Acquisition, and highlight how deficits in non-cognitive development may have long-term consequences for social integration.

Researchers have found large ASE effects on crime in both directions. In Denmark, Landersø et al. (2017) find that starting school later reduces the likelihood of any criminal charge by age 18 by 1.5 percentage points—a 30% reduction relative to the sample mean. In North Carolina, Cook and Kang (2016) estimate that 82% of the observed increase in crime among older students is attributable to higher dropout rates.

4.7 Family formation

ASE may influence family formation and fertility through multiple pathways, including risky sexual behavior, assortative matching, and the timing of life transitions linked to school-leaving policies.

While Dobkin and Ferreira (2010) find no significant relationship between ASE and marital status, other studies document more nuanced effects. Johansen (2021) finds that younger-entrant women are significantly more likely to cohabit by early adulthood than their older peers. The effect peaks at age 20, with a five-percentage-point difference, but then fades thereafter. This finding may reflect higher rates of unplanned pregnancies among younger entrants, consistent with earlier engagement in risky sexual behavior (Section 4.5.3). Such dynamics likely reflect the combined influence of Self-Esteem and Parental/Teacher Investments. Lower self-confidence and differential guidance in adolescence may shape decisions around relationships and sexual activity.

Similarly, Fredriksson et al. (2022) show that women born just after the school-entry cutoff, who enter and exit school later, cohabit for the first time roughly six months later than those born just before it. This finding aligns with the Delayed School Exit channel, where older school-entry age delays the entire sequence of educational and life-course transitions, including household formation. Slightly higher maternal age among older entrants may also explain the small observed differences in infant health. For instance, some studies find marginally lower birth weights among children born to mothers who were older school entrants. However, the effects are minimal and unlikely to affect long-term development (Fredriksson et al., 2022; Johansen, 2021). These small shifts in maternal age and infant health may also reflect broader influences of the *Starting-age* component on life timing, even when biological consequences are limited.

Family formation and related risky health behaviors are cumulative and recurrent, which leads to the distinction between extensive and intensive margins and to tracking outcomes over long age spans. This design choice shifts interpretation away from point-in-time ASE effects toward changes in the timing and sequencing of life-course transitions. Consequently, RDD estimates are typically evaluated separately at multiple ages and over long horizons (Johansen, 2021), so that identification conditions on age at measurement and is largely net of the *Age-at-outcome* component.

ASE may also affect family formation indirectly through assortative matching. Using data from Mexico, Peña (2017) finds that older entrants are more likely to partner with more educated spouses by about one percentage point. This likely reflects ASE-driven differences in educational attainment and timing, which shape individuals' opportunities and preferences in the marriage market. These patterns are plausibly shaped by the Skill Acquisition and Selection into Programs channels. Differences in maturity and early school placement can influence educational attainment, which in turn affects partner matching later in life.

5 Areas of future work

This review has synthesized a large and diverse literature showing that age at school entry (ASE) shapes outcomes across education, labor markets, health, social relationships, and family formation. A central conclusion is that most empirical estimates reflect bundled effects of multiple ASE components, which makes it difficult to identify the specific contribution of each. Likewise, key mechanisms such as Skill Acquisition and Self-Esteem are often hypothesized but rarely measured or causally identified. Progress in the literature therefore hinges on improving our ability to disentangle components from mechanisms.

One promising direction for future studies is to leverage experimental data, as in Page et al. (2017, 2019), to more clearly identify ASE mechanisms. Experimental evidence, such as that exploiting randomized classroom assignment (Cascio & Schanzenbach, 2016) or controlled variation in peer composition, has already provided valuable insights into *Relative-age* mechanisms. Randomized controlled trials in early childhood and early schooling environments also offer scope to distinguish developmental effects from institutional responses, particularly when combined with rich measures of intermediate outcomes. Such designs are especially useful for separating maturity-related skill formation from downstream sorting, labeling, or tracking decisions.

Beyond experimentation, there is substantial scope to expand the econometric toolkit applied to ASE questions. Much of the existing literature relies on regression discontinuity designs around school-entry cutoffs, often within a single institutional context. While these designs are powerful, they restrict the set of identifiable components and tend to focus on compliers. Greater use of policy reforms, cross-jurisdictional variation, and difference-in-difference designs would improve external validity and yield estimates that are more directly informative for policymakers. Existing evidence on the effects of shifting school-entry cutoff dates remains limited and highly context-specific, in part because most reforms simultaneously alter *Relative-age*, maturity, and schooling duration, making it difficult to extrapolate results across institutional settings. In parallel, more systematic cross-country comparisons would help clarify how institutional features mediate ASE effects, particularly outside high-income settings where evidence remains sparse (see for example Bor et al. (in press) and Morales (2020)).

Among the ASE components, *Relative-age* stands out as especially important but still incompletely understood. Future research could advance this area in at least two directions. First, by developing new measures of perceived rank or social comparison within classrooms (Ballatore et al., 2020; Murphy & Weinhardt, 2020). Second, by exploring interactions beyond the classroom, including family dynamics (Landersø et al., 2020; Persson et al., 2025). These directions offer insights into psychological and institutional mechanisms through which ASE may shape individuals' lives. Together, these approaches could shed light on how age-based comparisons operate across social contexts and over the life course.

Another underexplored area concerns spillovers within families. Existing evidence suggests that ASE can affect parental labor supply, sibling outcomes, and family decision-making, yet these channels remain peripheral in most studies. Understanding how ASE shapes household behavior and intergenerational trajectories is important both for welfare analysis and for the design of complementary policies that mitigate unintended consequences.

Evidence from the early childhood education field suggests that there may be multiple optimal ages for entering different stages of early learning (Duncan et al., 2023). For policymakers, a key question is when children should begin formal early learning, whether it be in child care, preschool, kindergarten, or formal schooling. These programs are often treated as counterfactuals in evaluation studies, but they may also function as complementary interventions that reinforce developmental outcomes. Evidence on the interaction between ASE and early childhood care and education programs, such as the U.S. Head Start (Gibbs, 2026), nevertheless remains limited. Bridging the ASE and early childhood education literatures remains an important open task.

Finally, most quasi-experimental estimates identify effects for compliers rather than for children whose parents actively choose to delay or accelerate school entry. Importantly, the empirical literature is considerably more informative about the consequences of individual deviations from typical entry age than about the effects of cohort-wide policy changes that shift cutoff dates for all students. Studies aiming to inform parental decisions about redshirting should assess whether the potential outcomes of redshirting children (always-takers) resemble those of compliers, using methods such as those proposed by Bertanha and Imbens (2020). If the outcomes are similar, this would strengthen the validity of their policy advice; if not, the discrepancy should be reported. Moreover, ethically approved randomized controlled trials with informed consent could incorporate lotteries that allocate some children an additional year in a publicly funded pre-school program before kindergarten entry, thereby providing experimental evidence on the consequences of individually delaying formal school entry, rather than implementing cohort-wide policies. Such evidence would be especially informative for parental decision-making.

While ASE effects span multiple life domains, the evidence base for policy-level interventions remains limited. Credible causal evidence on the effects of changing cutoff dates, as opposed to individual-level variation in school entry timing, is largely confined to education and labor market outcomes, and even within these domains, few studies directly inform policy design.

The most direct policy lever is to shift school-entry cutoffs. However, such reforms simultaneously alter *Starting-age*, *Relative-age* distributions, and potentially total *Time-in-school,* which makes it difficult to predict their net effects. Limited available evidence suggests that later mandated school entry, when total years of schooling are held constant, may yield modest positive effects on skill accumulation (Bedard & Dhuey, 2012; Fletcher & Kim, 2016), although this finding comes from a single institutional context and warrants replication. Importantly, shifting cutoffs does not eliminate *Relative-age* effects: it reassigns which children occupy the youngest positions within their cohorts.

This observation may seem to conflict with findings on early childhood education (Duncan et al., 2023). However, the evidence is consistent once we distinguish policy margins: reforms that mandate earlier school entry while keeping the school-leaving age fixed effectively extend total *Time-in-school* and are associated with improvements in both cognitive and non-cognitive skills—conditional on adjusting the program for the first grade, now attended by younger entrants.

For policymakers, the central lesson from this literature is that the consequences of ASE are not governed by a single universal margin, but instead emerge from the interaction between ASE components and institutional context. Policies that shift school-entry timing inevitably redistribute maturity, relative standing, and schooling duration across cohorts, with heterogeneous effects that depend on tracking regimes, assessment practices, and dropout rules. Neither earlier nor later school entry is intrinsically optimal, since welfare comparisons depend critically on how institutional features translate age-based differences into opportunities and constraints over the life course. The most promising policy directions may therefore involve not cutoff changes per se, but complementary interventions such as flexible tracking systems, age-appropriate assessment benchmarks, and teacher training on developmental variation that mitigate the mechanisms through which ASE generates persistent inequality. Mentoring younger entrants could raise their prospects as well, even though such interventions are typically designed for low-socioeconomic status populations (Falk et al., 2026; Kraft & Falken, 2021).

Future research that explicitly links new sources of variation to specific ASE components and channels, rather than treating ASE as a single margin, will be essential for producing evidence that is both internally valid and policy-relevant.

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Online Appendix A — Conceptual Framework

The Economics of Age at School Entry: Insights from Evidence and Methods

Mariagrazia Cavallo, Elizabeth Dhuey, Luca Fumarco, Levi Halewyck, Simon ter Meulen*

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*Mariagrazia Cavallo: University of Lausanne, Research Fellowship at Luxembourg Institute of Socio-Economic Research (LISER).
Elizabeth Dhuey: University of Toronto.
Luca Fumarco: Masaryk University, GLO, HEDG, IZA, J-PAL.
Levi Halewyck: Maastricht University.
Simon ter Meulen: ifo Institute, University of Munich, CESifo.

This appendix reports three tables summarizing our hypotheses on individual-level effects (that is, the effect of redshirting), policy-level effects, and their combination. More practically, each table reports hypothesized effects on age at school entry (ASE) components, channels, and outcomes.

Table A.1: Individual-level effects on ASE components, channels, and outcomes. As in the survey, we focus on redshirting as parental response.

ASE component	**Channel**	**Outcomes**					
		Educ	Labor	Health	Social	Crime	Family
Starting Age ↑	Skill Acquisition ↑	+	+	+	+	–	+
	Self-Esteem ↑	+	+	+	+	–	+
	Dropout ↑	–	–	–	–	+	–
	School Exit ↑	–	–	–	–	+	–
Age at Outcome ↑	Skill Acquisition ↑	+	+	+	+	–	+
	Parental/Teacher Inv. ↑	+	+	+	+	–	+
	Selection into Programs ↑	+	+	+	+	–	+
Relative Age ↑	Skill Acquisition ↓	–	–	–	–	+	–
	Self-Esteem ↑	+	+	+	+	–	+
	Parental/Teacher Inv. ↑	+	+	+	+	–	+
	Selection into Programs ↑	+	+	+	+	–	+
Time in School ↓	Dropout ↑	–	–	–	–	+	–
	School Exit ↑	–	–	–	–	+	–

Note: Arrows indicate the direction of the shift induced by redshirting on the ASE-components, and, in turn, of the ASE-components on the individual channels."+" (Green) and "–" (Red) indicate the sign of the final effect on individual outcomes.

Table A.2: Policy-level effects from moving cutoff earlier on ASE components, channels, and outcomes.

ASE Component	**Channel**	**Outcomes**					
		Educ	Labor	Health	Social	Crime	Family
S_0 **students**							
Starting Age	Skill Acquisition						
	Self-Esteem						
	Dropout						
	School Exit						
Age at Outcome	Skill Acquisition						
	Parental/Teacher Inv.						
	Selection into Programs						
Relative Age ↑,↑↓	Skill Acquisition ↓↑	+/−	+/−	+/−	+/− +	+/−	+/−
	Self-Esteem ↑↓	+/−	+/−	+/−	+/− +	+/−	+/−
	Parental/Teacher Inv. ↑↓	+/−	+/−	+/−	+/− +	+/−	+/−
	Selection into Programs ↑↓	+/−	+/−	+/−	+/− +	+/−	+/−
Time in School	Dropout						
	School Exit						
S_1 **students**							
Starting Age ↑	Skill Acquisition ↑	+	+	+	+	−	+
	Self-Esteem ↑	+	+	+	+	−	+
	Dropout ↑	−	−	−	−	+	−
	School Exit ↑	−	−	−	−	+	−
Age at Outcome ↑	Skill Acquisition ↑	+	+	+	+	−	+
	Parental/Teacher Inv. ↑	+	+	+	+	−	+
	Selection into Programs ↑	+	+	+	+	−	+
Relative Age ↑	Skill Acquisition ↓	−	−	−	−	+	−
	Self-Esteem ↑	+	+	+	+	−	+
	Parental/Teacher Inv. ↑	+	+	+	+	−	+
	Selection into Programs ↑	+	+	+	+	−	+
Time in School ↓	Dropout ↑	−	−	−	−	+	−
	School Exit ↑	−	−	−	−	+	−

Note: Arrows indicate the direction of the shift induced by change in cutoff date on the ASE-components, and, in turn, of the ASE-components on the individual channels. "+" and "–" indicate typical direction of effects; green, red, olive green, and gray shading mark positive, negative, ambiguous, and unaltered associations, respectively, as documented in the literature. Effects reflect impacts across channels of the policy-level change; magnitudes vary by context. *S0:* students not directly affected by the cutoff change, who continue to enroll at the previously mandated age. *S1:* students directly affected by the cutoff change, who must now delay entry by one year; *S2:* students not directly affected (*S0*) whose parents decided to delay their child's school entry voluntarily.

Table A.3: Policy-level effects from moving cutoff earlier, combined with Parental Response, on ASE components, channels, and outcomes.

ASE Component	**Channel**	**Outcomes**					
		Edu	Labor	Health	Social	Crime	Family
$S_0 \setminus S_2$ **students**							
Starting Age	Skill Acquisition						
	Self-Esteem						
	Dropout						
	School Exit						
Age at Outcome	Skill Acquisition						
	Parental/Teacher Inv.						
	Selection into Programs						
Relative Age $\uparrow,\uparrow\downarrow$	Skill Acquisition $\downarrow\uparrow$	+/−	+/−	+/−	+/− +	+/−	+/−
	Self-Esteem $\uparrow\downarrow$	+/−	+/−	+/−	+/− +	+/−	+/−
	Parental/Teacher Inv. $\uparrow\downarrow$	+/−	+/−	+/−	+/− +	+/−	+/−
	Selection into Programs $\uparrow\downarrow$	+/−	+/−	+/−	+/− +	+/−	+/−
Time in School	Dropout						
	School Exit						
S_1 **students**							
Starting Age $\uparrow$	Skill Acquisition $\uparrow$	+	+	+	+	−	+
	Self-Esteem $\uparrow$	+	+	+	+	−	+
	Dropout $\uparrow$	−	−	−	−	+	−
	School Exit $\uparrow$	−	−	−	−	+	−
Age at Outcome $\uparrow$	Skill Acquisition $\uparrow$	+	+	+	+	−	+
	Parental/Teacher Inv. $\uparrow$	+	+	+	+	−	+
	Selection into Programs $\uparrow$	+	+	+	+	−	+
Relative Age $\uparrow,\uparrow\downarrow$	Skill Acquisition $\downarrow\uparrow$	+/−	+/−	+/−	+/− +	+/−	+/−
	Self-Esteem $\uparrow\downarrow$	+/−	+/−	+/−	+/− +	+/−	+/−
	Parental/Teacher Inv. $\uparrow\downarrow$	+/−	+/−	+/−	+/− +	+/−	+/−
	Selection into Programs $\uparrow\downarrow$	+/−	+/−	+/−	+/− +	+/−	+/−
Time in School $\downarrow$	Dropout $\uparrow$	−	−	−	−	+	−
	School Exit $\downarrow$	−	−	−	−	+	−
S_2 **students**							
Starting Age $\uparrow$	Skill Acquisition $\uparrow$	+	+	+	+	−	+
	Self-Esteem $\uparrow$	+	+	+	+	−	+
	Dropout $\uparrow$	−	−	−	−	+	−
	School Exit $\uparrow$	−	−	−	−	+	−
Age at Outcome $\uparrow$	Skill Acquisition $\uparrow$	+	+	+	+	−	+
	Parental/Teacher Inv. $\uparrow$	+	+	+	+	−	+
	Selection into Programs $\uparrow$	+	+	+	+	−	+

Relative Age ↑	Skill Acquisition ↓	−	−	−	−	+	−
	Self-Esteem ↑	+	+	+	+	−	+
	Parental/Teacher Inv. ↑	+	+	+	+	−	+
	Selection into Programs ↑	+	+	+	+	−	+
Time in School ↓	Dropout ↑	−	−	−	−	+	−
	School Exit ↑	−	−	−	−	+	−

Note: Arrows indicate the direction of the shift induced by the combined change in cutoff date and parental response, on the ASE-components, and, in turn, of the ASE-components on the individual channels. "+" and "–" indicate typical direction of effects; green, red, olive green, and gray shading mark positive, negative, ambiguous, and unaltered associations, respectively, as documented in the literature. Effects reflect impacts across channels of the policy-level change and parental responses; magnitudes vary by context. *S0:* students not directly affected by the cutoff change, who continue to enroll at the previously mandated age. *S1:* students directly affected by the cutoff change, who must now delay entry by one year; *S2:* students not directly affected (*S0*) whose parents decided to delay their child's school entry voluntarily.

Online Appendix B — Summaries of all Studies Included in Section 4 of our Manuscript

The Economics of Age at School Entry: Insights from Evidence and Methods

Mariagrazia Cavallo, Elizabeth Dhuey, Luca Fumarco, Levi Halewyck, Simon ter Meulen*

February 24, 2026

*Mariagrazia Cavallo: University of Lausanne, Research Fellowship at Luxembourg Institute of Socio-Economic Research (LISER).
Elizabeth Dhuey: University of Toronto.
Luca Fumarco: Masaryk University, GLO, HEDG, IZA, J-PAL.
Levi Halewyck: Maastricht University.
Simon ter Meulen: ifo Institute, University of Munich, CESifo.

This appendix reports the summaries of all studies included in Section 4 of our survey. For each study, we report:

- authors, year, and outcome
- country and data
- methodology
- main effects
- effects from heterogeneity analyses

The table is followed by the list of complete references.

In the survey, each study could be cited multiple times across subsections. Here, they are reported multiple times only when they refer to different outcomes (for example, study X results on age at school entry (ASE) effects on both scores and attainment, is reported in Education in panels A and B). Differently, they are reported only once when multiple outcome sections discuss the same result; for example, if the ASE effect from a study on outcome O is reported in Physical health, it is not reported in Family formation too to discuss again the result on outcome O, even if its outcome section discusses it.

Table B.1: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Education				
Panel A. Test scores and other measures of cognitive skills				
Bedard and Dhuey (2006). **Grade-based school outcomes**: Composite effect of ASE on test scores in Math and Science at the 4th grade [3rd grade for the US] and the 8th grade	Austria, Canada, Czech Republic, England, Greece, Iceland, Japan, New Zealand, Norway, and Portugal (4th and 8th grades); Belgium, Denmark, Finland, France, Italy, Slovak Republic, Spain, Sweden (8th grade only); United States (3rd and 8th grades). Trends in International Mathematics and Science Study (TIMSS) 1995 and 1999; Early Childhood Longitudinal Study (ECLS); National Education Longitudinal Study (NELS).	two-stage least squares (2SLS) using the assigned age (namely, birth month relative to the school cutoff date) as an instrument for observed age. Reduced-form: test score on assigned age. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. Robustness: Controls for season of birth effects are added in some specifications (data pooled across countries). The cross-country variation in cut-off dates is exploited to separate seasonal effects from the (bundled) age effects. **Key channels**: SKILL ACQUISITION.	In the following, the lower bound corresponds to the smallest estimated effect among the countries in the sample, while the upper bound corresponds to the largest estimated effect. **2SLS effects, grade 4, Math**: 0.190-0.430 test score points for one-month effect, which translates to a one-year effect (11-month difference) of 2.09-4.73 score points. Provided that, Mean (M)=50 and Standard Deviation (SD)=10, this one-year effect is equivalent to: 0.209-0.473 SD. **2SLS effects, grade 4, Science**: 0.181-0.369 test score points for one-month effect, which translates to a one-year effect (11-month difference) of 1.991-4.059 score points. Provided that, M=50 and SD=10, this one-year effect is equivalent to: 0.199-0.406 SD. **2SLS effects, grade 8, Math**: 0.020-0.353 test score points for one-month effect, which translates to a one-year effect (11-month difference) of 0.220-3.883 score points. Provided that, M=50 and SD=10, this one-year effect is equivalent to: 0.022-0.388 SD. **2SLS effects, grade 8, Science**: 0.049-0.380 test score points for one-month effect, which translates to a one-year effect (11-month difference) of 0.539-4.18 score points. Provided that, M=50 and SD=10, this one-year effect is equivalent to: 0.054-0.418 SD.	England, Iceland, Japan, Norway–"low failure rate" countries (namely, countries with little or no evidence of early/late starting or grade retention)–have larger (bundled) age effects than "high failure rate" countries. In 4th grade, older children from low failure rate countries score 11-12 percentiles better than younger children; while, older children from high failure rate countries perform 6-10 percentiles better than younger children. Similar heterogeneous patterns between "low failure rate" and "high failure rate" countries are observed in the 8th grade, except that the magnitude of the (bundled) age effects is lower.
Berniell and Estrada (2020). **Biological age-based assessments**: Composite effect of ASE on Math and Reading test score measured at age 15.	Spain. PISA; Spanish population census; STUS survey (Spanish Time Use Survey); GDA survey (General Diagnostic Assessment); microdata from the Spanish National Statistical Institute (Spanish birth certificates); Living conditions survey.	Reduced-form birth-month design directly comparing January-born vs December-born students. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design. **Key channels**: PARENTAL/TEACHER INVESTMENTS	The study investigates the difference in outcomes between December-born vs January-born students around the Jan 1st cutoff (youngest vs oldest in the same cohort), which implies the estimation of (approximately) one-year effects. **Math score**: December-born students score 13.861 points lower. Provided that in PISA test, M= 500 and SD= 100, this one-year effect is equivalent to 0.139 SD. **Reading score**: December-born students score 11.164 points lower. Provided that in PISA test, M=500 and SD= 100, this one-year effect is equivalent to 0.112 SD.	*Parental socioeconomic status (SES)*. **Math score**: Lower-SES December-born students score 16.439 points lower (one-year effect equivalent to 0.164 SD). The (bundled) age penalty is 10.153 points smaller for high-SES students than for low-SES students. **Reading score**: Lower-SES December-born students score 13.754 points lower (one-year effect equivalent to 0.138 SD). The (bundled) age penalty is 10.195 points smaller for high-SES students than for low-SES students.

Table B.1 Continued: Literature review

Study	**Country & Data**	**Method**	**Effects**	**Heterogeneity**
				Gender differences. **Math score**: Female students score 11.568 points lower than boys (effect equivalent to 0.116 SD). **Reading score**: Female students score 34.421 points higher than boys (effect equivalent to 0.344 SD).
Biroli et al. (2026). **Grade based school outcomes:** Interaction of Gene–environment with the composite effects of ASE on test scores in entry assessment (age 4-5), Key Stage 1 (age 6–7), Key Stage 2 (age 10–11), Key Stage 3 (age 13–14), Key Stage 4/GCSE (age 15–16).	UK. Avon Longitudinal Study of Parents and Children (ALSPAC); National Pupil Database.	Regression discontinuity design (RDD) using September 1 cutoff; ordinary least squares (OLS). **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: PARENTAL/TEACHER INVESTMENTS. Complementarity between genetic endowments and early parental investments at school entry; substitutability between genetic endowments and formal schooling in later grades.	Being born just after the September 1 cutoff increases all available test scores, with effects declining in magnitude for later grades. Using a local linear regression discontinuity (RD) design with a $\pm$3-month bandwidth around the cutoff, the reduced-form discontinuity estimates (comparing students born just after versus just before the cutoff) are: Entry Assessment (ages 4–5): 1.133–1.151 standard deviations (SD); Key Stage 1 (ages 6–7): 0.687–0.698 SD; Key Stage 2 (ages 10–11): 0.379–0.389 SD; Key Stage 3 (ages 13–14): 0.210–0.223 SD; Key Stage 4 (ages 15–16): 0.274–0.281 SD. These coefficients represent a one-year effect (discrete discontinuity effect from comparing just after vs just before September 1). Within-cohort month-of-birth gradients (linear trend among controls) are negative.	Ages 4-5 benefits are larger for high-PGI children (increasing inequality) compared to ages 7-16, where benefits are larger for low-PGI children (reducing inequality). At school entry (Entry Assessment), the interaction between being old-for-grade and PGI is positive and statistically significant: children with higher genetic propensity for education benefit more from being old-for-grade (approximately 0.12 SD per one SD increase in PGI). During formal schooling (Key Stages 1–4), the interaction term becomes negative.
Black et al. (2011). **Biological age-based/Post-schooling adult outcomes**: Composite and Decomposed effects of ASE on IQ scores (measured at 18, military conscription).	Norway. Norwegian Registry Data by Statistics Norway; Norwegian military records (1980-2005).	2SLS using expected age as an instrument. **Identifying variation**: cutoff-based variation, and variation in the scheduling of the test. **Identified bundle of components:** the bundle of *Starting-age*, *Relative-age*, and *Time-in-school* is separated from *Age-at-outcome*. With specification on graduated conscripts who have completed education, the bundle of *Starting-age* and *Relative-age* is separated from *Age-at-outcome* (abstracting from current *Time-in-school*). **Key channels**: SKILL ACQUISITION, *Age-at-outcome*.	IQ is measured in stanines (Standard Nine) units, which is a 9-point standard scale that has a discrete approximation to a normal distribution, with Mean=5, and a Standard Deviation=2. Being one year older when taking the test increases the score by about 0.2; this is one-fifth of a stanine, and about one-tenth of a standard deviation. The bundled effect of *Starting-age*, *Relative-age*, and (partially of) *Time-in-school* is negative: being one year older reduces IQ scores by about 0.06 (equivalent to about one-twentieth of a stanine, and 0.03 SD). The total effect of being one-year older on the IQ score is 0.14-0.16 (approximately, 8% of SD).	Only men; family background.

Table B.1 Continued: Literature review

Study	**Country & Data**	**Method**	**Effects**	**Heterogeneity**
			When restricting the sample to those who have completed education (both early and late starters) by the time of the test, it is found: no statistically significant effect of school *Starting-age* (still bundled with *Relative-age*), and a slightly smaller (but still statistically significant) effect of *Age-at-outcome*.	
Cascio and Lewis (2006). Effect of schooling on the AFQT (Armed Forces Qualifying Test). The instrument used in the paper involves ASE.	U.S. National Longitudinal Survey of Youth (NLSY 79), which includes AFQT results	2SLS exploiting as an instrument variation in the state minimum age requirements for school entry; Reduced-form estimation; OLS for comparison. **Identifying variation**: cutoff based variation. **Bundle of ASE components** captured by the instrument: *Starting-age*, *Time-in-school*, and *Relative-age*.	One additional year of schooling increases AFQT by: 0.131-0.391 SD for Blacks (depending on the specification), 0.085-0.424 SD for Hispanics, 0.123-0.210 SD flipping to a negative effect in some specifications (though estimates are less precise) for Non-Hispanic Whites	Ethnicity
Cascio and Schanzenbach (2016). **Grade based school outcomes:** Composed and Decomposed effect(s) of ASE on test scores (average of Reading and Math) measured at the end of kindergarten through eight years later	U.S. (Tennessee). Project STAR.	2SLS; (OLS showed for comparison). **Identifying variation**: cutoff based variation and random assignment of students to classrooms. **Identified bundle of components:** from standard estimation a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. Exploiting random assignment of students, *Relative-age* is identified in isolation from the remaining bundle of *Starting-age* and *Age-at-outcome*. **Key channels**: SKILL ACQUISITION.	**Composite effect** of *Starting-age*, *Relative-age*, and *Age-at-outcome*: children one year older perform 0.674 SD (or 0.707 SD) better at the end of kindergarten. Test score differences between older and younger students decline through grades. In grade 8, the one-year effect on test scores shrinks to 0.215 SD. **Decomposed effect** of *Relative-age* from the bundle of *Starting-age* and *Age-at-outcome*. **2SLS not accounting for lower ability of older peer**: the one-year effect from bundled *Starting-age* and *Age-at-outcome* at the end of kindergarten is 0.910 SD (or 1.020 SD), while the isolated effect of *Relative-age* is -0.310 SD (-0.233 SD in alternative specification) non significant. Eight years later, the one-year effect from bundled *Starting-age* and *Age-at-outcome* is 0.650 SD, while the isolated effect of *Relative-age* is -0.429 SD (significant at the 5% level). **2SLS estimates accounting for lower ability of older peer**: the one-year effect from bundled *Starting-age* and *Age-at-outcome* at the end of kindergarten is 1.407 SD (or 1.564 SD), while the isolated effect of *Relative-age* is -0.847 SD (or -0.724 SD in alternative specification). Eight years later, the one-year effect from bundled *Starting-age* and *Age-at-outcome* is 0.651 SD, while the isolated effect of *Relative-age* is -0.430 SD.	Gender; whether eligible for free or reduced price lunch.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Celhay and Gallegos (2025). Early skill effects on parental beliefs, investments and children long-run outcomes. This paper explicitly studies the channel of parental investments (Parental/Teacher Investments) in response to children's maturity levels (as captured by ASE)	Chile. Administrative data from the Ministry of Education (e.g., 4th grade exams, college entrance exams). Survey data on parents (e.g., financial and time investments, along with beliefs).	RDD. **Identifying variation**: cutoff-based variation. They exploit exogenous differences in skills generated by variation in school entry (which involves bundled ASE components that, based on their different nature, may differ for the different outcomes considered). **Key channels**: PARENTAL/TEACHER INVESTMENTS (direct measurement). They explicitly apply mediation analysis.	There are positive effects on grades in school and on parents' financial investments, there are marginally significant effects on parents' time investment, there are positive effects on parental beliefs and on college enrollment and grades. Mediator analyses on parental investment and beliefs explain nearly 34% of the causal effect on college enrollment.	Analyses by SES status shows that the effects are systematically larger for students from low SES families.
Cornelissen and Dustmann (2019). **Grade-based school outcomes**: Composite effect of ASE on test scores measured at the end of first grade (age 5), the end of third grade (age 7) and the end of seventh grade (11 age). 1st grade: language, literacy, problem solving, and numeracy. 3rd grade: reading, writing, math, and science. 7th grade: english, math, science. Fields aggregated to "language" and "numeracy" indices.	England. Millennium Cohort Study (MCS); National Pupil Database (NPD).	2SLS, two-sample 2SLS; using expected exposure to school prescribed by the school entry rules as an instrument for actual exposure. **Identifying variation**: cutoff-based variation, including geographical variation in entry rules. SKILL ACQUISITION	An additional month of exposure to early schooling increases test scores in 1st grade by 0.100 SD (language) and 0.073 SD (numeracy); in 3rd grade by 0.026 SD (language) and 0.014 SD (numeracy); in 7th grade effects are not significant.	The effects are slightly larger for girls in the language and numeracy scores and are largely driven by low-SES boys at age 5 and 7. At age 11 a small significant effect remains for girls in language skills.
Crawford et al. (2014). **Grade-based/Biological age-based outcomes**: Composite and Decomposed effects of ASE on different measures of cognitive skills: test scores (National achievement test scores) measured at Key Stage 1; and WISC test; WISC with correlation above 0.3; WISC with correlation above 0.4; WOLD: comprehension; WOLD: expression measured at around age 8	UK. Avon Longitudinal Study of Parents and Children (ALSPAC), Millennium Cohort Study (MCS).	RDD around the September 1 cut-off. **Identifying variation**: cutoff-based variation. The main idea of the paper is to compare the results from the national achievement test scores, taken in the same grade, with those administered in the survey, taken at the same age, to identify an upper bound for the *Age-at-outcome* effect. **Identified bundle of components:** from grade-based test, the identified bundle of components includes *Starting-age*, *Relative-age*, and *Age-at-outcome*; from age-based test, the bundle of components includes *Starting-age*, *Relative-age*, and *Time-in-school*.	RDD estimates from grade-based test (National achievement test): 0.835 SD effect (baseline specification) for a one-year increase (discontinuity effect comparing just after September 1 vs just before September 1). Age-based test estimates are all statistically insignificant (WISC; WISC with correlation above 0.3; WISC with correlation above 0.4; WOLD: comprehension; WOLD: expression)	None.
Dhuey et al. (2019).**Grade-based school outcomes:** Composite effect of ASE on test scores measured in grade 3 to 8 (FCAT Math and Reading)	U.S. (Florida). Administrative data. Birth records from Florida Department of Health; school records from Florida Department of Education.	RDD. **Identifying variation**: cutoff based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: SKILL ACQUISITION	August-born children score 0.197 SD than their September-born counterparts (pooled across six grades and averaged for math and reading). Results are comparable within the same family (no heterogeneity across SES). No differences for maternal education, income, ethnicity, or gender.	Family; maternal education; income; ethnicity; gender.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Elder and Lubotsky (2009). **Grade-based school outcomes:** Composite and decomposed effects of ASE on test scores in Reading and Math measured at fall and spring kindergarten, grades from 1 to 8.	U.S. Early Childhood Longitudinal Study—Kindergarten Class (ECLS-K); National Educational Longitudinal Survey of 1988 (NELS:88).	2SLS using predicted kindergarten entrance age as an instrument (i.e. age if enrolled when first allowed by law). **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: level of SKILL ACQUISITION, accumulated especially before school-entry. The independent effect of classmates' age is identified by the variation in average birth dates across schools, and the variation across schools in the entrance-age cutoffs. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and (current) *time in-school*; with *Age-at-outcome* held fixed by design. In the peer decomposition: they separate own-age effect from peer-age effect (the effect of being surrounded by older classmates).	Being one year older at kindergarten entry increases Reading test scores by 5.28 points (0.53 SD) in the fall of kindergarten and by 2.27 points in the 8th grade. Math test scores increase by 7.41 points in the fall of kindergarten and by 1.34 points in the 8th grade. Conditional on the child's own age, having older classmates tends to increase Reading and Math test scores.	The effect declines across grades, but is more persistent for children from upper-income families.
Fredriksson et al. (2024). Composite effect of ASE. Parental and school responses to student performance using school entry rules as exogenous variation.	34 countries. Progress in International Reading Literacy Study (PIRLS 2006, 2011); U.S.: Early Childhood Longitudinal Study (ECLS-K:1999, 2011).	2SLS using expected age (based on school entry cut-off rules) as an instrument for reading performance. Controls include school fixed effects, child background, and robustness checks with absolute age. **Identifying variation**: cutoff based variation. **Key channels**: PARENTAL/SCHOOL INVESTMENT.	Parents and schools respond to lower performance with compensatory investments. Parents increase homework help by 0.58 SD per 1 SD drop in reading; schools reduce class size and increase remedial tutoring. Effects grow over grade levels.	Parental responses are stronger in less compensatory school systems, suggesting substitutability. Compensatory behavior is consistent across countries and SES groups, but stronger among highly educated parents. No compensatory behavior at kindergarten entry.
Görlitz et al. (2022). **Biological-age/post-schooling adult outcomes**: Composite long-term effect of ASE on adult cognitive competencies (math, text comprehension, receptive vocabulary).	Germany. National Educational Panel Study (NEPS-SC6), adults aged 23–71	2SLS using expected school starting age (ESSA) based on state-level cut-off laws as instrument for actual school starting age. Controls include month and year of birth, state fixed effects, gender, and parental background. RDD used as robustness check. **Identifying variation**: cutoff based variation. a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment).*Age-at-outcome* does not enter the bundle. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	School starting age has no significant long-term effect on math or text comprehension. However, being one year older at school entry increases receptive vocabulary by 0.35 SD. Effect on receptive vocabulary is driven by absolute age, not relative age. SSA increases likelihood of entering higher school track, which is linked to richer language exposure.	No significant heterogeneity by parental background.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Landersø et al. (2020). **Grade-based school outcomes**: Composite effect of focal child's ASE on older siblings' academic performance.	Denmark. Administrative data.	2SLS. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: (ASE is referred to the focal child) a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: PARENTAL/TEACHER INVESTMENTS (Parents redirect resources toward older siblings' exam preparation when younger child's transition is eased)	There is no significant effects when age distance between the siblings is 1-3 years or 4-6 years. When the distance is 7-9 years: a one-year effect of focal child's ASE determines: 1.065 SD (or 0.985 SD) increase in Math, and 0.768 SD (or 0.763 SD) increase in Danish grammar score. However, there is no significant effect on Danish essay, and Danish oral tests.	Parental background
Lubotsky and Kaestner (2016). **Grade-based school outcomes**: Composite effect of ASE (kindergarten entrance age) on the evolution of cognitive and non-cognitive skill gaps from kindergarten through 8th grade	U.S. Early Childhood Longitudinal Study—Kindergarten Class of 1998–99 (ECLS-K); National Longitudinal Survey of Youth 1979 Children and Young Adults (NLSY79-CYA).	2SLS using predicted kindergarten entrance age (based on state cutoff laws) as an instrument for actual entrance age. Controls include child and family characteristics, quarter of birth, and state fixed effects. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: (ASE is referred to the focal child) a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: SKILL ACQUISITION. Dynamic complementarity appears to be limited.	In the ECLS-K data, a one-year increase in kindergarten entrance age increases fall kindergarten math IRT score by 7.58 points (given an SD = 9.09, the one-year effect in SD units is 0.83); the Reading IRT score by 5.16 points (given an SD=10.21, the one-year effect in SD units is 0.51 SD); the Approach-to-Learning score by 0.49 points (given an SD= 0.68, the one-year effect in SD units is 0.72); the score in Interpersonal skills by 0.20 points (given an SD=0.63, the one-year effect in SD units is 0.32); the score in Self-control by 0.13 points (given an SD=0.62, the one-year effect in SD units is 0.21). A one-year increase in kindergarten entrance age reduces internalizing behavior by 0.13 points (given an SD=0.53, the one-year effect in SD units is -0.25 SD); and it reduces externalizing behavior by 0.13 points (given an SD=0.65, the one-year effect in SD units is -0.20 SD). For the NLSY sample, a one-year entrance-age effects in PIAT Math is 2.69 points (which, given an SD=6.19, in SD units it is 0.43), in PIAT Read. Recognition is 2.47 points (which, given an SD=5.92, in SD units it is 0.42), in PIAT Read. Comp. is 2.44 points (which, given an SD=5.25, in SD units it is 0.46), while the effect on internalizing and externalizing behavior is not significant. Cognitive growth advantages are present in kindergarten and first grade but reverse in later grades, leading to convergence. Non-cognitive differences do not exhibit dynamic complementarity and largely fade over time.	No evidence of heterogeneity in non-cognitive effects. Cognitive convergence is not driven by school-level compensatory investments. Effects are consistent across within- and between-school comparisons.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Mühlenweg et al. (2012). **Biological age-based outcomes**: Composite effect of ASE on IQ scores measured at age 8 and age 11.	Germany (Rhine-Neckar region). Mannheim Study of Children at Risk (MARS).	2SLS using as an instrument assigned age at school entry. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design. The authors do not discuss specific channels. They interpret this as evidence that ASE mainly affects non-cognitive skills, but not cognitive skills.	No statistically significant effect is found.	None.
Nam (2014). **Grade based school outcomes:** Composite effect of ASE on test scores (homeroom-teacher percentile ranks converted into z-scores) measured in the third year of middle school; CSAT test: Math, Korean verbal, English (third year of high school).	South Korea. Korean Education and Employment Panel (KEEP), surveyed by the Korea Research Institute for Vocational Education and Training for the main analysis. Additional datasets: TIMSS, The Korean Education Longitudinal Study (KELS), KYPS (Korea Youth Panel Survey). Economically Active Population Survey is additionally used for adult outcomes.	2SLS using assigned age based on birth month relative to March 1 cutoff, as an instrument for observed age. OLS for comparison. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: Parental/Teacher investments (private tutoring and self-study time); adolescent risky behaviors (smoking, alcohol consumption, dating boyfriend/girlfriend) as intermediate channels. However, the paper does not run formal mediation analysis: results are presented as suggestive evidence.	**2SLS estimates** (main results): Test scores at the third year of middle school increase by 0.025 SD as age increases by one month. The equivalent approximate one-year effect (11-month difference) is 0.275 SD. No significant effect is found on high school CSAT scores.	Controls are added for the student gender, parental education level, whether the student was the first child, whether both parents were alive, household earnings, whether the family owned a house.
Oosterbeek et al. (2021). **Grade-based school outcomes**: Composite effect of ASE on exit test from primary school, and recommendation from primary school	The Netherlands. Administrative data available at Statistics Netherlands.	(Sharp) RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: skill acquisition.	Students born just after the threshold perform better at the end of primary school than students born just before it. Reduced-form RD effects: 0.082 SD-units (with bandwidth 14 days), 0.097 SD-units (with bandwidth 30 days), and 0.085 SD-units (with optimal bandwidth 16 days). Given that children just after the cutoff start school 0.41 years later on average than those just before, and assuming linear scaling, these effects are equivalent to: one-year effect of 0.20-0.24 SD. The performance gap translates into a higher probability to be placed in high ability tracks in secondary education (5 percentage points).	None.
Peña (2020). **Grade-based school outcomes**: Composite and Decomposed effect(s) of ASE on test score in Spanish and Math. GPA throughout school.	Mexico (Puebla and Tlaxcala). Administrative data. National academic achievement test ENLACE, for years 2009 to 2013.	DiD; 2SLS. **Identifying variation**: cutoff-based and policy-based variation.	Own age is the driver of age effects in test scores. No relevant differences by gender. Relative age confers an advantage in GPA throughout high school. The advantage decreases but doesn't disappear as students reach age 18.	Gender.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
? (?). **Grade-based school outcomes**: Composite effect of ASE and redshirting effect on math test scores in 3rd grade.	Michigan (U.S.). Administrative data from Michigan Education Data Center (MEDC); Early Childhood Longitudinal Survey-Birth Cohort (ECLS-B).	Fuzzy RDD, with a Heckman–Vytlacil marginal treatment effects (MTE) framework.	Waiting to enter kindergarten raises third-grade math scores by 0.229 SD (0.374 on eager compliers and 0.172 on reluctant compliers).	Family income.
Panel B. Non-cognitive skills				
Barabasch et al. (2026). **Post-schooling adult outcomes**: Composite effect of ASE on personality traits (Openness to experience; Conscientiousness; Extraversion; Agreeableness; Neuroticism) measured for individuals between age 25 and 60 (average age: 43.54).	Germany. German Socio-Economic Panel (SOEP 1984–2019); auxiliary data: NEPS-SC6 (National Educational Panel Study).	RDD exploiting the statutory cutoff rules for school enrollment. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment).*Age-at-outcome* does not enter the bundle.	Being born after the cutoff significantly reduces levels of neuroticism by almost 0.06 SD. The enrollment cutoffs have an economically relevant policy effect of 20% on the compression of the gender gap in neuroticism.	Gender (the effect is significant for women but not for men).
Cornelissen and Dustmann (2019). **Grade-based school outcomes**: Composite effect of ASE on non-cognitive outcomes at 5: physical development, creative development, personal, social, and emotional development; non-cognitive outcomes at 7: academic interest, relationship with teacher, disruptive behavior, self-perception; and non-cognitive outcomes at 11: academic interest, relationship with teacher, disruptive behavior, self-perception.	England. Millennium Cohort Study (MCS); National Pupil Database (NPD).	2SLS using expected exposure to school prescribed by the school entry rules as an instrument for actual exposure. **Identifying variation**: cutoff-based variation, including geographical variation in entry rules. SKILL ACQUISITION.	At 5, the effect size is about 6-7% of an SD for an additional month of relative age on all outcomes. At 7, the effect size on teacher relationship is about 6.4% of an SD for an additional month of relative age. At 11, the effect size on disruptive behavior is about 8.1% of an SD for an additional month of relative age.	At 5, there is no heterogeneous response. Effects are largely driven by low-SES boys. At 7, the effect is slightly larger for boys (10% sd) and insignificant for girls. Additionally, for boys, the effect size on academic interest is 12% SD. For girls, there is a decrease in disruptive behavior of 7.5% of a SD. Effects are largely driven by low-SES boys. At 11, the effect is larger for boys (10.5%) than for girls (6%). For boys, there is an additional effect on academic interest (12.8%), and teacher relationship (10.8%).
Crawford et al. (2014). **Biological age-based outcomes**: Composite effects of ASE on different measures of non-cognitive skills: Scholastic competence (self-perception); liking school very much; locus of control; self-esteem measured at age 8	UK. Avon Longitudinal Study of Parents and Children (ALSPAC)	RDD around the September 1 cut-off. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** the bundle of components includes *Starting-age*, *Relative-age*, and *Time-in-school*, with *Age-at-outcome* held fixed by design.	RDD estimates show an effect of 0.557 SD (baseline specification) and 0.382 SD (conditioning on prior KS1 attainment) for a one-year increase (local discontinuity effect comparing just after September 1 vs just before September 1) on self-perception of scholastic competence; while non-significant effects are observed for liking school very much; locus of control; self-esteem	None.
Peña and Duckworth (2018). **Grade-based outcomes**: grit (perseverance) and grit (consistency) in 9th and 12th grades	Mexico. For 9th grade, data are from the Metropolitan Commission of Public High Schools; for 12th grade the test PLANEA is from the National Institute for the Evaluation of Education; students are identified by the Unique Population Registry Key (CURP)	2SLS, using as instruments expected relative and absolute age **Identifying variation**: Birth-date-based variation exploited through expected-age instruments; variation from repeated testing over time (longitudinal dimension). **Identified bundle of components:** a composite effect of *Age-at-outcome*, *Starting-age*, and differences in *Time-in-school* which may not be completely netted out; *Relative-age* only. **Key channels**: (non-cognitive) skill acquisition, self-esteem, peer comparison	*Relative-age* has no significant effect on the Grit perseverance, and a positive effect on the Grit consistency of 0.22 SD per year. Absolute age (which includes the combined effect of *Age-at-outcome*, *Starting-age*, and differences in *Time-in-school* has a positive effect of 0.09 SD on perseverance of effort and a negative effect of -0.13 SD on consistency of interest	Gender (Results broadly similar by gender). Robust to restricting sample to students born in the State of Mexico (less redshirting)
Panel C. School progression and attainment				
Angrist and Krueger (1991).				

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Bedard and Dhuey (2006). **Grade-based school outcome**: Composite effect of ASE on pre-university behavior.	Canada (British Columbia), U.S. Restricted-use administrative data for British Columbia; second and third waves restricted-use version of NELS data for the U.S.	2SLS using the assigned relative age (namely, birth month relative to the school cutoff date) as an instrument for observed age (in grade 9 for British Columbia, in grade 8 for the U.S.). The specification includes school-fixed effects. **Identifying variation**: cutoff based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	In British Columbia, older students are 12.8 percent more likely to be "university-bound". In the U.S., older students are 11.1 (10.7) percentage points more likely to have taken the SAT or ACT (enrolled in college) than the youngest students.	
Cotofan et al. (2022). **Grade-based school outcome**: Composite effect of ASE on conscientiousness, neuroticism, need for achievement, perseverance, school motivation, self-confidence, educational expectations.	Netherlands. OnderwijsMonitor Limburg (OML).	Difference-in-discontinuities design, fuzzy RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	The authors research the interaction effect between being placed in a higher track and relative age on noncognitive outcomes. Attending the higher track benefits older students more in terms of higher need for achievement, higher perseverance, and lower neuroticism. There is no effect on the other noncognitive outcomes.	None.
Dhuey et al. (2019).**Grade-based school outcome**: Composite effect of ASE on kindergarten readiness, parental holding back behavior (redshirting), school retention behavior (before third grade), middle and high school course selection, and high school graduation.	U.S. (Florida). Birth records from Florida Department of Health; school records from Florida Department of Education.	RDD. **Identifying variation**: cutoff based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	Older children are more kindergarten-ready than their younger counterparts, are less likely to be redshirted, are less likely to be retained in early grades, are more likely to take advanced courses in math and reading and less likely to take remedial courses in middle school. In high school, older students are more likely to take any advanced placement (AP class), but there is no significant effect on computer science. Older students are more likely to obtain a high school diploma. However, they are less likely to stay in high school when they are not graduated. There is no effect on high school dropout rate.	kindergarten-readiness is larger when the mother did not finish high school; for low-income families; for black/Hispanic families; for males. Redshirting is larger when the mother is a college graduate; for high-income families; for white families; for males. The likelihood to be retained in early grades is larger when the mother is a high school dropout; for low-income families; for males. Differences based on SES are more pronounced in middle than high school. For most AP courses, the effects are more pronounced for females. There is no difference for maternal education, income, race/ethnicity, or gender on high school dropout rate.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Dobkin and Ferreira (2010). **Post-schooling outcomes** (completed attainment): Composite effect of ASE on educational attainment (grade completion) by grade from 7 to 12, High School Diploma, Some College, and College Degree measured on adults aged 30–79.	U.S. (California and Texas). Restricted-use data from the Decennial Census Long Form Data (2000).	(reduced-form) RDD exploiting school-entry cutoffs. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school*. *Age-at-outcome* does not enter the bundle. **Key channels**: SCHOOL DROP OUT POLICIES, DELAYED SCHOOL EXIT	In Texas sample, the reduced-form effect of being born just after the cutoff relative to being born just before is -0.0034 (-0.34 pp) in grade 7, -0.0042 (-0.42 pp) in grade 9, -0.0068 (-0.68 pp) in grade 10, -0.0084 (-0.84 pp) in grade 11, -0.008 (-0.80 pp) in grade 12, -0.0077 (-0.77 pp) for high school diploma, not significant for some college and college degree. In California sample, the reduced-form effect of being born just after the cutoff relative to being born just before is not significant in grade 7, -0.0015 (-0.15 pp) in grade 9, -0.0026 (-0.26 pp) in grade 10, -0.0049 (-0.49 pp) in grade 11, -0.0060 (-0.60 pp) in grade 12, -0.0089 (-0.89 pp) for high school diploma, -0.0066 (-0.66 pp) for some college, and not significant for college degree.	Limited heterogeneity
Dustmann et al. (2017). **Post-schooling outcomes**: Composite long-term effects of early track choice on wages, education, and employment	Germany. Data from social security records, Microcensus, School Census, and 1987 Census.	RDD and two-sample 2SLS. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment).*Age-at-outcome* does not enter the bundle. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	No significant long-term effects of attending a more advanced track for marginal students (between the threshold between two tracks) due to up- and downgrading after middle school, which offsets initial track assignment.	None.
Oosterbeek et al. (2021). **Grade-based school outcome**: Composite effect of ASE on school track at 9th grade and enrollment in university.	Netherlands. Administrative data from Statistics Netherlands.	RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	Student born in October are in 9th grade 4 percentage points more likely to be in the college or university track than students born in September.	Differences between SES, ethnicity, and gender are considered.
Mühlenweg and Puhani (2010). **Grade-based school outcome**: Composite effect of ASE on school tracking up to 12-13 years after school entry.	Germany (Hessen). Hessen administrative datasets; German Socio-Economic Panel (GSOEP) and Mikrozensus (MK).	2SLS using assigned school-entry age as an instrument for observed school-entry age. **Identifying variation**: cutoff based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	Younger students are less likely to be assigned to the academic track: entering school at age seven rather than six raises the probability to attend a highest track secondary school by about 13 percentage points. Deferring tracking at age 12 does not reduce this effect, while if the students are facilitated in track modification (i.e. they have a second chance to change) at 16, there are some mitigations.	Gender.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
? (?) **Grade-based school outcome**: Recommendations, self-selection, and redshirting	Michigan (U.S.). Administrative data from the Michigan Education Data Center (MEDC); Early Childhood Longitudinal Survey-Birth Cohort (ECLS-B)	Fuzzy RDD, with a Heckman–Vytlacil marginal treatment effects (MTE) framework, and policy simulations	There is positive selection on gains: children opting into waiting experience 2.4 times larger achievement gains on average (0.44 SD). There is a negative selection on levels (children opting into waiting would be lower achieving in third grade had they started kindergarten at age five). Positive selection on gains only occurs for high-income families. Shifting from requirements to recommendations would widen income-achievement gap by 12%.	Family income
Schneeweis and Zweimuller (2014). **Grade-based school outcomes**: Composite effect of ASE on track choice in grades 5-8 and in grade 9	Austria. Administrative data (from Linz). PISA (2003 and 2006)	2SLS (instrumental variable (IV)-Probit) using the assigned age as an instrument for observed age. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time in-school* held fixed by design. **Key channels**: Selection into Programs.	**Effects measured on Administrative Data (City of Linz)**. In grade 5, being 11 months older (one-year effect) increases probability of high-track attendance by 17.5 percentage points; in grade 6, being 11 months older (one-year effect) increases probability of high-track attendance by 16.5 percentage points; in grade 7, being 11 months older (one-year effect) increases probability of high-track attendance by 16.0 percentage points; in grade 8, being 11 months older (one-year effect) increases probability of high-track attendance by 15.4 percentage points. **Effects measured on PISA sample**. In grade 8, being 11 months older (one-year effect) increases probability of high-track attendance by 12.4 percentage points. In grade 9, being 11 months older (one-year effect) increases probability of high-track attendance by 7.2 percentage points.	**Socio-economic background**. In grade 8, the (bundled) ASE effect is similar across socioeconomic groups: being 11 months older (one-year effect) increases the probability of attending a high-track school by 12.0 percentage points among low-SES students and 12.2 percentage points among high-SES students. After the second tracking in grade 9, the (bundled) ASE effect for high-SES students becomes small and statistically insignificant (3.3 percentage points), indicating that younger high-SES students are able to catch up. In contrast, among students from low-SES backgrounds, the (bundled) ASE effect increases to 21.2 percentage points, implying that younger disadvantaged students fall further behind at the second tracking stage. **Urban vs Rural Areas**. In urban areas, the (bundled) ASE effect is large and persistent. Being 11 months older (one-year effect) increases the probability of attending a high-track school by 24.4 percentage points in grade 8 and by 18.4 percentage points in grade 9. In rural areas, by contrast, the (bundled) ASE effect is small and statistically insignificant: 2.0 percentage points in grade 8 and 2.9 percentage points in grade 9.
Labor market				

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Bai et al. (2019). **Post-schooling adult outcomes**: Composite effect of ASE on mutual fund performance (alpha), portfolio activeness, ability to attract fund flows.	U.S. LexisNexis Public Records (LNPR) database; Morningstar Direct Mutual Fund database; Thomson Reuters Mutual Fund Holdings database; Center for Research in Security Prices (CRSP) database; LinkedIn; Amazon Mechanical Turk survey.	OLS panel regressions (reduced-form) with fund and year fixed effects. **Identifying variation**: Cutoff-based variation (using state-specific kindergarten eligibility rules). **Identified bundle of components**: *Starting-age*, *Relative-age*, accumulated differences in total *Time-in-school* (while no longer affected by differences in current *Time-in-school*). While *Age-at-outcome* varies across individuals, it is not mechanically included in the bundle since it is controlled for. **Key channels**: SELF-ESTEEM (confidence).	Managers in the oldest age quartile outperform those in the youngest quartile (approximately one-year difference effect) by 0.42% per year (raw return difference) and 0.48% per year (Four-factor alpha difference); and increases annual fund flows by about 4%; stock holdings of older managers outperform those of younger managers by roughly 1.6–1.8% per year.	None.
Balestra et al. (2020). **Post-schooling outcomes**: Composite effect of ASE on employment, earnings, and Disability insurance receipt.	Switzerland (St. Gallen canton). Administrative data from School Psychological Service St. Gallen, the Ministry of Education of the canton of St. Gallen, the Stellwerk test service provider, the Swiss Federal Statistical Office, and the Swiss Central Compensation Office.	RDD. Two-Sample IV. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age* and *Relative-age*. The bundle likely abstracts from significant differences in total *Time-in-school* (since there is a limited effect of ASE on educational attainment). Similarly, *Age-at-outcome* differences are largely netted out.	The effect is not statistically significant.	None.
Black et al. (2011). **Post-schooling outcomes**: Composite and Decomposed effect of ASE on earnings.	Norway. Norwegian Registry Data by Statistics Norway.	2SLS using the expected school starting age as an instrument for the actual school starting age. The identification of the effect for longer-term outcomes is less complicated than studying in-school tests because *Age-at-outcome* differences are largely netted out. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment).	There is a negative effect of school starting age.	For both males and females the earnings are lower for individuals with a higher school starting age until the age of 30. From age 30 to 35, there is no effect on wage of relative age. Family background: at age 35, the effect of school starting age is negative for males from a disadvantages background (mother's education, family size, birth order). At age 24, the effect of school starting age is negative for advantaged backgrounds.
Dobkin and Ferreira (2010). **Post-schooling adult outcomes**: Composite effect of ASE on wages, employment status, family income, home ownership, house value measured on adults aged 30–79.	U.S. (California and Texas). Restricted-use data from the Decennial Census Long Form Data (2000).	(reduced-form) RDD exploiting school-entry cutoffs. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment, as suggested by findings in Panel B. School progression and attainment). *Age-at-outcome* differences are largely netted out. **Key channels**: SCHOOL DROP OUT POLICIES, DELAYED SCHOOL EXIT	Non significant.	No consistent heterogeneity by gender, ethnicity or cohort.

Table B.1 Continued: Literature review

Study	**Country & Data**	**Method**	**Effects**	**Heterogeneity**
Du et al. (2012). **Post-schooling outcomes**: Composite effect of ASE on being a CEO.	U.S. Observational data. CEOs of S&P 500 companies between 1992. ExecuComp database. Biography Resource Center.	Statistical test(t-test). The effect is a reduced-form correlation birth-month effect. June and July are the birth months associated with being the youngest in school, given the cutoff in the U.S.	The number of CEOs born in June and July is disproportionately small relative to the number of CEOs born in other months: only 12% (while the expected percentage under uniform distribution would have been 16.7%).	None.
Dustmann et al. (2017). **Post-schooling outcomes**: Composite effect of early track assignment on long-term educational and labor market outcomes.	Germany. Administrative data from social security records, census, and school census covering cohorts born 1961–1976. Includes wages, employment, occupation, and educational attainment.	Two-sample 2SLS using birth month as instrument for track assignment. RDD around July 1 school entry cut-off. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment). *Age-at-outcome* does not enter the bundle. **Key channels**: SELECTION INTO PROGRAMS	No significant long-term effects of attending a more advanced track for marginal students. Track assignment at age 10 does not affect wages, employment, or education completed due to later track reversals. Effects of tracking mitigated by later corrections. Supports importance of system design over initial assignment.	None.
Fredriksson and Öckert (2014). **Post-schooling outcomes**: Composite effect of ASE on earnings, present value of lifetime earnings, employment and employment (full-time).	Sweden. Administrative data from Statistics Sweden, combined with Evaluation Through Follow-up (ETF) study.	RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment). *Age-at-outcome* does not enter the bundle. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	The effect of (an increase in) school starting age on employment during the age of 25-54 is of 0.330 percentage points. There is no global effect of school starting age on full-time employment during the age of 41-45.	The effect on employment for females is 0.677 percentage points increase; for males there is no effect. For individuals with low-educated parents there is a 0.696 percentage points increase; for individuals with high-educated parents there is no effect. For females an increase in school starting age increases employment by 0.733 percentage points; for males there is no effect. For individuals with low-educated parents an increase in school starting age increases full-time employment by 0.715 percentage points; for individuals with high-educated parents there is no effect.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Landersø et al. (2020). **Post-schooling outcomes**: Composite effect of focal child's ASE on maternal employment, likelihood that parents continue their relationship, mothers' wage.	Denmark. Administrative data.	2SLS. **Identifying variation**: cutoff-based variation. **Identified bundle of components** (ASE is referred to the focal child): a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	At 7, being one year older at school start increases maternal employment by 4 percentage points (relative to a mean of just below 80%), while there are no significant effects before. For parental relationship, until child's age 6, the effect is not significant, while it is after age 7. The peak is at child's age 15-17. Being one year older at school start increases the likelihood that the parents continue their relationship by 8 percentage points (relative to a mean of just above 60%). Extensive margin effects are 4-5 pp at 7 and 8, while decrease at 2 percentage points at 9. In terms of intensive margin the effect remains substantial also at 9. There is no effect of the focal child school starting age on the outcomes of siblings in lower or middle primary school. There is an effect for siblings who are seven to nine years older.	Differences by parental background are considered.
Larsen and Solli (2017). **Post-schooling outcomes**: Composite effect of ASE on age-specific earnings and life earnings.	Norway. Administrative data.	OLS. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment).*Age-at-outcome* does not enter the bundle. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	The investigated effect is the birth month effect. Individuals born in January, compared with individuals born in December, at the same social age, have higher earnings during the early years of the career, however, younger individuals catch up in their early 40s. After that, the birth month effect has a reversal sign, with December-born individuals having higher earnings than January-born individuals. For non-discounted life earnings, there is no significant birth month effect. There is no birth month effect when earnings are observed at a given biological age.	None.
Muller and Page (2016). **Post-schooling outcomes**: Composite effect of ASE on political selection.	U.S. They use observational data from the U.S. congress (senators and representatives), on June 2011. For their robustness checks, they additionally use data from 1990 to 2008 from the U.S. Department of Health and Human Services on births.	RDD, McCrary discontinuity test. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment).*Age-at-outcome* does not enter the bundle. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	The effects is much larger than for broader populations. Relative age does not correlate with the quality of the politicians (as measured by educational achievement and age when entering congress). One of the robustness checks verifies whether there is strategic childbearing; for this, they use an RDD with monthly frequencies of birth over the entire U.S. population of parents and over the population of parents where the mother is highly educated. They do not find any evidence of strategic childbearing.	None.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Oosterbeek et al. (2021). **Post-schooling outcomes**: Composite effect of ASE on earnings and labour market experience.	The Netherlands. Administrative data.	RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment). *Age-at-outcome* does not enter the bundle. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	There is a negative effect.	SES, ethnicity, gender.
Oosterbeek et al. (2021). **Post-schooling outcomes**: Composite effect of ASE on labour market experience observed at 30.	The Netherlands. Administrative data from Statistics Netherlands.	RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment). *Age-at-outcome* does not enter the bundle. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	Being born in September instead of October raises experience by 3.6 months (0.3 of a year).	None.
Peña (2017). **Post-schooling outcomes**: Composite effect of ASE on earnings, employment status, having employer provided medical insurance.	Mexico. ENOE (National Survey on Occupation and Employment).	RDD; month-of-birth fixed effects model. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and likely differences in total *Time-in-school* (generated through the effect of ASE on educational attainment); while, *Age-at-outcome* differences are largely netted out.	Regression discontinuity estimates (quadratic specification with covariates) of the impact of an additional year of ASE: earnings increase by 0.0239 pp, percentage of occupied increases by 0.422 pp, percentage with provided medical insurance increases by 0.488 pp. For females, there is an increase by 0.977 pp in the fraction of occupied. There is no significant effect for males. For males, the fraction of individuals with an employer-provided medical insurance increases by 0.922 percentage points (but this is only marginally significant). For females, it increases by 0.129 pp (not significant effect).	Gender.
Zweimüller (2013). **Post-schooling outcomes**: Composite effect of ASE on entry job type (white-collar job).	Austria. Austrian Social Security Database (ASSD); Austrian Tex Register; Austrian Unemployment Register.	2SLS; RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment). *Age-at-outcome* does not enter the bundle. **Key channels**: SELECTION INTO PROGRAMS, SKILL ACQUISITION	Males born after the cut-off date are significantly less likely to work in a blue-collar job, and more likely to work in a white-collar job.	Only males are considered (to avoid complications from female labor supply such as part-time, or childbirth).
Social relationships				
Ballatore et al. (2020). **Grade-based school outcomes**: Composite effect of ASE on school victimization (being beaten, forced to do something against her will, things stolen).	Italy. SNV (National Students' Assessment Survey), carried by INVALSI (National Institute for the Evaluation of the Education System).	2SLS using the theoretical school starting age as an instrument for age rank. The measure of relative age considered in this paper is ordinal, rather than cardinal. Thus, the measured effect is more precisely an age rank effect.	A one-decile increase in the age rank decreases the probability of being bullied by 1.3 percentage points in both grades (against a baseline probability of about 22% for 5th graders and 14% for 6th graders).	Age rank effects are stronger when calculated within groups defined by observable characteristics (gender, socio-economic status, immigrant status, and length of the weekly school schedule).

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Dhuey and Lipscomb (2008). **Grade-based school outcomes**: Composite effect of ASE on leadership (being team captain, club president, self-reported leadership skills; number of times being captain, number of times being president).	U.S. Project Talent (1960); National Longitudinal Study of High School Class (1972); High School and Beyond (1980-82).	Linear probability model(LPM); (Poisson regression model and negative binomial regression model are used to estimate count data models). **Identifying variation**: cutoff-based variation. Assigned-age quartiles dummies, built by considering the state cut-off dates for kindergarten entrance and the student's date of birth are used. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time in-school* held fixed by de sign. **Key channels**: Self-Esteem, maturity mistaken for ability, Selection into Programs (suggested but not formally estimated).	Being approximately one-year older (9-12 month effect, given that the age is measured in quarters) increases the probability of being team captain by 2.0 percentage points; of being club president by 2.7 percentage points; of self-reporting leadership skills by 0.055–0.058 SD (leadership index is standardized such that M=0, and SD=1). Conditional on participation (team membership), being approximately one-year older (9-12 month effect) increases the probability of being team captain by 1.6 percentage points and being club president by 2.7 percentage points. Being approximately one-year older (9-12 month effect) increases the expected number of times serving as team captain by 5.2–5.3% and the expected number of times of being club president by 5.8–5.9%.	Differences across gender are considered. Project Talent dataset: Being approximately one-year older (9-12 month effect) increases the probability of being team captain by 2.4 percentage points for males but only by 1.3 percentage points for females. Being approximately one-year older (9-12 month effect) increases the probability of being club president by 3.5 percentage points for males, but only by 1.9 percentage points for females. NLS-72 dataset: Being approximately one-year older (9-12 month effect) increases the probability of being team captain by 2.2 percentage points for males, but only 0.4 pp for females. No statistically significant effects for being club president. HS&B dataset: for the effect on the probability of being team captain, male estimate is small and statistically insignificant (0.6 percentage points), while female estimate is larger but also imprecise (1.8 percentage points). For the effect on the probability of being club president both estimates are statistically insignificant.
Fumarco and Baert (2019). **Grade-based school outcomes**: Composite effect of ASE on E-communication, quantity of meetings with friends, and quantity of friends measured at between 10.5 and 16.5 years of age.	Europe. Health Behaviour in School Aged Children (HBSC) survey.	2SLS using as an instrument the expected age. **Identifying variation**: cutoff-based variation.	A one-month increase in relative age decreases e-communication by 0.004 SD, increases the quantity of friends by 0.004 SD, and increases the quantity of meetings by 0.003 SD.	ASE changes between educational systems, with different ability grouping, and by state.
Mühlenweg (2010). **Grade-based school outcome**: Composite effect of ASE on school victimization (things stolen, being bullied, being hurt) measured in 4th grade [with the exception of Scotland and England were students are in 5th grade].	Austria, Belgium (Flemish), British Columbia, Nova Scotia, Ontario, Québec, England, France, Iceland, Italy, Luxembourg, Norway, Poland, Scotland, Singapore, Slovakia, South Africa, Spain, Sweden, Taiwan. Progress in International Reading Literacy Study 2006 (PIRLS 2006).	2SLS using the assigned relative age (i.e. birth month relative to the school cut-off date) as an instrument for observed age. **Identifying variation**: cut-off based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	**Estimates on pooled sample of countries**: A one-year increase of age reduces the probability of victimization by 8 pp, the probability that something was stolen by 5 pp, the probability of being bullied by 9 pp, and the probability of being hurt by 6 pp. Effects vary in magnitude across countries.	The bundled age effects on victimization tend to be more severe in countries with a comprehensive system allowing (intra-class) ability grouping (e.g. Canada, England and Scotland), while they tend to be lower for integrative systems with individualized teaching (e.g. northern European countries) and in countries where children are prepared for different ability tracks (e.g. some central European countries).

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Page et al. (2017). **Grade-based school outcomes**: Composite effect of ASE on preference for competition; self-confidence; risk and ambiguity attitudes; behavioral risk-taking (BART) measured while students are enrolled in Grade 8 or Grade 9.	Australia. 38 Australian schools in Queensland, and New South Wales. Experimental data.	(fuzzy) RDD, with bandwidth of approximately 2 months. **Identifying variation**: cutoff based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Time-in-school*; with *Age-at-outcome* approximately held fixed by design. They compare students who are nearly the same biological age, but placed in different grades. **Key channels**: SELF-ESTEEM, *Relative-age*.	The effect of being older across adjacent birth months at the cutoff increases competitiveness by 27 pp. No significant effect on self-confidence, Risk aversion, Ambiguity aversion, Behavioral risk-taking. These are reduced-form discontinuity effects (not per-month or per-year slopes).	The size effect (for competitiveness) in the male subsample ranges from 0.576 to 0.651 (58–65 pp), depending on specification.
Page et al. (2019). **Post-schooling outcomes:** Composite effect of ASE on self-confidence, competitiveness (past), competitiveness (future), risk attitudes lottery (risk and ambiguity), risk attitudes, total number of pumps in a ballon (BART), risk seeking behavior (driving, finance, health, leisure, occupation), trust and patience measured on adults aged 24–60.	Australia. Experimental data from an online experimental survey.	2SLS using assigned relative age as an instrument for observed age. **Identifying variation**: cutoff based variation.	Being older within the school cohort (comparing individuals born at most two months before vs. after the school entry cut-off date) increases confidence by about 9-11 percentile points, and has a smaller effect on competitiveness (0.12 point when significant). There is a positive effect on trust (0.355 points). There is a non significant effect on patience, risk attitudes lottery, risk attitudes, total number of pumps in a ballon (BART), risk and ambiguity seeking.	No clear difference between men and women. Only for risk attitudes lottery (risk and ambiguity) there is a negative effect on women, but not on men.
Pellizzari and Billari (2012). **Grade-based (during university) outcomes**: Composite effect of ASE on relationship status (stable relationship), physical activity (sport), social activities (discos/dancings), monthly sexual intercourses.	Italy. International Survey on Affectivity and Sex (ISAS).	Probit. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	The youngest students are 18 percentage points less likely to be in a stable relationship and have on average 1.2 fewer intercourses than their older classmates. There is no significant effect on physical activity and social activities.	None.
Peña (2020). **Grade-based school outcomes**: Composite and Decomposed effect of ASE on test score for allocation to public high schools and expectations about the result of this test (various outcomes are explored).	Mexico City vs. State of Mexico. COMIPEMS, for years 2006 to 2015.	DiD, 2SLS, fuzzy RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	Younger students seem to anticipate a worse performance in the test. They aim at lesser-quality schools and are more likely to be admitted to less-coveted options than their older classmates.	Differences between gender are considered.
Mental health and developmental concerns				
Dee and Sievertsen (2018). **Biological-age based outcomes**: Composite effect of ASE on mental health (inattention/hyperactivity).	Denmark. Administrative data are matched with the Danish National Birth Cohort Study.	fuzzy RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* approximately held fixed by design.	A 1-year delay in kindergarten entry reduces inattention/ hyperactivity at age 7 by –0.73. The effect persists at age 11.	The effect is identified for girls. Other subgroups are also investigated: low-high income; low-highly educated parents; children with a birth weight above-below 3.500g. However, the estimates across these subgroups are imprecise.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Dhuey and Lipscomb (2010). **Grade-based school outcomes**: Composite effect of ASE on the probability of disability classification (evaluated for and diagnosed with a possible disability) for grades K-10.	U.S. Early Childhood Longitudinal Study, Kindergarten Class of 1998–1999 (ECLS); National Education Longitudinal Study (NELS); Education Longitudinal Study (ELS).	2SLS using assigned age as an instrument for observed age. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	An additional month of age relative to the cutoff date is associated with a 2–5 percent reduction in the probability of receiving special education services.	The effects are larger for boys than for girls. In addition, until grade 3, boys are entirely responsible for the effect. The effect is stronger among whites than other ethnicities. In some years there is some evidence also for Hispanics, but there is no evidence for black students. There are no differences across socioeconomic quartiles.
Elder and Lubotsky (2009). **Grade-based school outcomes**: Composite and Decomposed effect(s) of ASE on learning disabilities (ADHD).	U.S. Early Childhood Longitudinal Study—Kindergarten Class (ECLS-K); National Educational Longitudinal Survey of 1988 (NELS:88).	2SLS. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	Being older at Kindergarten entry reduces the probability to receive a diagnosis by 2.5 percentage points. Relative age effect: conditional on a child's own age, having older classmates increases the probability to receive a diagnosis of learning disability.	SES.
Panel A. Attention deficit hyperactivity disorder				
Dalsgaard et al. (2012). **Biological age-based outcomes**: Composite effect of ASE on ADHD diagnosis.	Denmark. Register data on all children born July 1990–June 2001. ADHD diagnoses from national psychiatric registry; linked to rich socio-demographic data.	Fuzzy RDD around January 1 school entry cutoff. Diagnoses made only by specialists. Controls for child and parental characteristics. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	No significant effect of being young-for-grade on ADHD diagnosis. Estimated effects are small and not robust across specifications. Contrasts with large effects found in U.S. and Canada, suggests specialist-based diagnostic systems may reduce subjectivity and relative age bias in ADHD diagnoses.	None.
Elder (2010). **Grade-based school outcomes**: Composite effect of ASE on ADHD diagnosis, ADHD assessment (teacher), ADHD assessment (parents), and ADHD treatment (using behavior-modifying prescription stimulants).	U.S. Early Childhood Longitudinal Study-Kindergarten (ECLS-K).	RDD, fuzzy RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	The youngest children are 5.4 percentage points more likely to be diagnosed with ADHD (baseline diagnosis rate in survey is 6.4 percentage points). They are also more likely to be assessed with ADHD related symptoms by their teachers. However, age-for-grade is only weakly related to the parental assessment of the symptoms. A one-year increase in relative age reduces the likelihood to use behavior-modifying prescription stimulants by 4.4 percentage points.	Control variables in some specifications include gender, race, ethnicity, family structure, the marital status of the child's primary caregiver, Census region, urbanicity, parental education, log family income, and family size.
Furzer et al. (2022). **Grade-based school outcomes**: Composite effect of ASE on ADHD diagnosis rate (Children's Behavioural Scale (CBS) parent and teacher pre-diagnosis assessments).	Canada. National Longitudinal Survey of Child and Youth (NLSCY).	RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	Results suggest over-assessment of ADHD for the youngest (0.25 SD) in kindergarten and under-assessment of the oldest (0.23 SD). This holds for teacher assessments only.	Gender: under-assessment specifically for older female students (0.56 SD); over-assessment for boys (irrespective of school starting age), but especially for the youngest (0.3 SD). SES: effects are smaller and less precise for schools with more higher-income families.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Layton et al. (2018). **Biological age-based outcomes**: Composite effect of ASE on ADHD diagnosis and treatment.	U.S. Insurance claims data on 407,846 children born 2007–2009, followed through 2015. Focus on states with September 1 kindergarten cutoff.	Multivariable linear regression and RDD. Identification from arbitrary school entry cutoff. Controls for child and parental demographics and comorbidities. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	Children born in August (youngest in grade) were 34% more likely to be diagnosed with ADHD and 32% more likely to be treated than those born in September. No differences in other health conditions or in states without September 1 cutoff. Suggests relative age within grade influences ADHD diagnosis. Highlights potential overdiagnosis due to behavioral comparisons in classroom settings.	Effects significant for boys, and not for girls.
Nicodemo et al. (2024). **Age-based/grade-based outcomes**: Composite effect of ASE on ADHD diagnosis and treatment.	England. Administrative health records from QResearch and hospital data for 96,698 children born 2002–2010. ADHD prescriptions and diagnoses tracked from ages 5 to 15.	LPM;RDD. Identification from September 1 school entry cutoff. Controls for demographics, SES, maternal age, and GP fixed effects.	Early starters (July–August births) are 40–100% more likely to receive ADHD prescriptions than late starters (September–October). Gap driven by first-time prescriptions between ages 5–8; persists due to treatment continuation.	Effects of SSA on ADHD diagnosis are larger for girls, but difference is not statistically significant at all ages. Effects larger for high SES children.
Persson et al. (2025). **Age-based outcomes**: Family spillover effects of marginal ADHD diagnoses.	Sweden. Administrative data on 1.1 million cousin pairs born 1985–2001. Linked birth, outpatient, prescription, education, and earnings records.	RDD around January 1 school entry cutoff. Identification from relative age effects on ADHD diagnosis. Controls for cousin and family characteristics.	Children born just before the cutoff are 17.6% more likely to be diagnosed with ADHD. Their younger cousins are 9.1% more likely to be diagnosed and 5.8% more likely to be treated, even conditional on own relative age. No long-term gains in GPA, graduation, college, or earnings.	Marginal diagnoses propagate via intrafamily communication (cousin pairs). Spillover effects are higher for low SES, Swedish-born mother, and mother with no college education.
Schwandt and Wuppermann (2016). **Biological age-based outcomes** (measured during schooling): Composite effect of ASE on ADHD diagnosis, hay fever, and diabetes.	Germany. Data collected and provided by the Zentralinstitut fuer die Kassenaerztliche Versorgung in Deutschland (ZI), on all children insured in the SHI aged 4 to 14 for the years 2008 through 2011.	Simple non-parametric analysis; RDD, Local linear RD (reduced form). **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and (current) *Time-in-school*; with *Age-at-outcome* held fixed by design (within each age-specific regression, analyses compare children just before and just after the school-entry cutoff within the same age). **Key channels**: maturity; teacher referrals; overdiagnosis; parental demand.	Two margins are analyzed: (a) jumps in ADHD rates by age within imputed grades, and (b) Differences across adjacent birth months at the cutoff. ADHD diagnosis jumps by roughly 1 percentage points at the cutoff (baseline rate of 3–5%). The cutoff jumps in ADHD diagnosis rates translate into comparable jumps in prescription of ADHD-medication of about 0.8 pp around the cutoff (baseline of about 2.5% to 4%). These are reduced-form one-year effects (local discontinuity effect comparing just after vs just before the cutoff). Effects are strongest at ages 9–13 (approximately imputed grades 3–8). Finally, they conducted a study at aggregate level, to understand whether macro characteristics of the district have an effect on the jumps. Hay fever and diabetes serve as placebo outcomes to rule out season-of-birth or general health differences.	Gender differences: Results are weaker for girls.
Panel B. Other mental health and developmental concerns				

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Balestra et al. (2020). **Grade-based school outcomes**: Composite effect of ASE on ADHD, being referred to the school Psychological Service, Special needs diagnosis, behavioral problems, speech impediments, dyslexia/dyscalculia.	Switzerland (St. Gallen canton). Administrative data from School Psychological Service St. Gallen, the Ministry of Education of the canton of St. Gallen, the Stellwerk test service provider, the Swiss Federal Statistical Office, and the Swiss Central Compensation Office.	RDD is the main strategy. Two-sample IV (equivalent to a fuzzy RDD) is also considered. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	No significant effect is found for ADHD and dyslexia/dyscalculia. Instead, a one-year increase in relative age decreases the probability of being referred to the School Psychological Service by their teachers by 5.7 percentage points, decreases the probability of being diagnosed with special needs by 4.2 percentage points (14 percent reduction), and decreases the probability of having a speech impediment including dyslexia by 3.1 percentage points (20 percent) and a speech impediment excluding dyslexia by 2.8 percentage points. Starting school at a younger age increases the probability of developing behavioral problems (socio-emotional problems, disruptive behavior, and violent behavior) by 35%.	None.
Black et al. (2011). **Age-based/Post-schooling outcomes**: Composite and Decomposed effect of ASE on mental health.	Norway. Norwegian Registry Data by Statistics Norway. Norwegian military records (1980-2005).	2SLS using the expected school starting age as an instrument for the actual school starting age. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	A one-year increase in school starting age increases the probability of being classified "without problems" by 0.5 percentage points.	Only males are analyzed. Differences by family background are considered.
Dhuey et al. (2019). **Grade-based outcomes**: Composite effect of ASE on gifted status and disability (cognitive, behavioral, physical).	U.S. (Florida). Birth records from Florida Department of Health; school records from Florida Department of Education.	RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	Older children are 0.021 to 0.029 SD more likely to have a gifted status. Older children are less likely (0.029-0.049 SD) to have any disability (cognitive, behavioral, physical).	The effect of age on gifted status is consistent across different SES, while there is no difference for maternal education, income, race/ethnicity, or gender. The results for a cognitive (0.01 SD) and physical disability (0.02 SD) are largest for singletons (no siblings). While the impact on behavioral disability (0.02-0.03 SD) is robust across different families. The difference between August and September-born students to have any disability is larger (1) when the mother is a high school dropout (vs. high school graduate and college graduate); (2) for low-income families; (3) for males.
de Gage et al. (2025). **Age-based outcomes**: Composite effect of ASE on initiation of speech therapy in children.	France. Data from the French National Health Data System (French National Mother-Child register), includes all children born in France between 2010 and 2016 from September of the year of their 5th birthday until July of the year of their 10th birthday or July 31, 2022 (4.2 million children).	Cox proportional hazards models. Exploited variation from month of birth within school grade, however the estimated effect is mostly correlational. Controls for gestational age, birth weight, exposures, socioeconomic status, and geography.	Children born in December (youngest in grade) were 64% more likely to start speech therapy than those born in January. Effect size similar to ADHD medication; no effect for desmopressin (control). Suggests relative immaturity may be misdiagnosed as learning disorders.	None. Effects largely the same in all subgroups (sex, affiliation with solidarity-based complementary health insurance, disadvantage index of the municipality of residence, birth rank among siblings, and prematurity at birth).

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Fumarco et al. (2020). **Age-based outcomes**: Composite and Decomposed effect of ASE on life-satisfaction, psychosomatic complaints, general health, overweight.	European countries. HBSC.	2SLS.	There is a negative effect of absolute age on life-satisfaction, general health, overweight and a positive effect on psychosomatic complaints. There is a negative relative age effect on psychosomatic complaints and overweight, while a positive effect on life-satisfaction and general health.	None.
Matsubayashi and Ueda (2015). **Biological age-based outcome** (though not fixed age): Composite effect of ASE on suicide among youth in Japan measured at age between 15–25.	Japan. Vital Statistics data on all deaths (1989–2010) and births (1974–1985). Focus on suicides among individuals aged 15–25.	RDD around April 2 school entry cutoff. Suicide rates by date of birth compared within narrow windows. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	Individuals born just before April 2 (youngest in grade) had significantly higher suicide rates than those born just after. Effect strongest at ages 19–21. Suggests ASE disadvantage may lead to academic and career disadvantages, increasing suicide risk.	ASE disadvantage results in lower likelihood to complete high school or college, and higher likelihood to enter blue-collar jobs, which may lead to lower SES. This in turn increases possible suicidal behavior and completed suicide.
Mühlenweg et al. (2012). **Age-based outcomes**: Composite effect of ASE on temperament (activity, approach/withdrawal, soothability, adaptability, emotionality, attention span/persistence, intensity of reaction, rhythmiticy, threshold of responsiveness).	Germany (Rhine-Neckar region). Mannheim Study of Children at Risk (MARS).	2SLS using as an instrument predicted age at school entry. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	At age 8, relatively younger children are significantly more (hyper)active, and their attention span/persistence is significantly lower. At age 11, relatively younger children have a significantly lower adaptability to change, lower attention span/persistence, and are more irritable.	None.
Thompson et al. (1999). **Biological age-based outcome** (though not fixed age): Composite effect of ASE on youth suicide committed while being under age 20.	Canada (Alberta). Administrative data on all suicides under age 20 from 1979–1992 from Alberta Provincial Medical Examiner's Office.	Descriptive analysis comparing suicide rates by ASE within school cohorts. Chi-square tests. **Identifying variation**: cutoff-based variation: a dummy assigned-age variable (older vs younger half) determined by matching birth month to local school entry cutoffs is used. **Identified bundle of components**: the outcome is not measured at a fixed grade, and is not measured at a fixed age (since *Age-at-outcome* can vary). Schooling status may differ across individuals, which implies that *Time in-school* is not fixed, either. Thus, the effect captures the entire bundle of components (*Starting-age*, *Relative-age*, *Time-in-school*, and *Age-at-outcome*). **Key channels**: Self-Esteem; academic performance and mental health (increased depression/hopelessness) are intermediate outcomes. However, there is no formal mediation analysis.	Among youth suicide cases, 55.3% were born in the second half of the school year (younger within cohort) versus 44.7% in the first half ($\chi^2 = 6.38$, $p < 0.01$).	None.
Physical health				

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Anderson et al. (2011). **Grade-based outcomes**: Composite effect of ASE on BMI, overweight/obesity, and underweight.	U.S. Early Childhood Longitudinal Study - Kindergarten Cohort (ECLS - K).	RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design.	The effect is not significant	There is a heterogeneous response for children that experienced the largest "shock" (from little to no childcare (¡25h/week) to full-day kindergarten), an additional year of schooling reduced BMI by 6 percent.
Arnold and Depew (2018). **Age-based/post-schooling adult outcomes**: Composite effect of ASE on (self-reported) health status, excellent or very good health, fair or poor health.	U.S. Survey of Income and Program Participation (SIPP).	reduced form; RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	Higher school starting age increases health status, increases the likelihood of reporting an excellent or very good health and reduces the likelihood of reporting a fair or poor health.	For males, a higher predicted school starting age increases health status by 0.078 points (from a mean of 3.93), increases the likelihood of reporting an excellent or very good health by 0.026 points (from a mean of 0.71), and decreases the likelihood of reporting a fair or poor health status by 0.029 points (from a mean of 0.07). For females, all the effects are not statistically significant.
Panel A. Weight issues				
Carpenter and Churchill (2025). **Age-based/grade-based outcomes**: Composite effect of ASE on self-image, and weight-related health behaviors.	European countries. HBSC.	2SLS. **Identifying variation**: cutoff-based variation.	A one SD increase in relative age increases by 1.7-1.9 percentage points the likelihood that adolescents feel their bodies of the right size, by 1.1 percentage points the likelihood that they describe themselves as "too skinny", by 0.9 percentage points the likelihood that they describe themselves as "too fat", by 1.3 percentage points the likelihood that they report to have no reason to be on a diet, by 1.7 percentage points eating fruit, by 1.2 percentage points eating vegetables, being physically active by 0.12 more days, decreases by 1.3 percentage points eating sweets and by 2.3 percentage points drinking soda, reduces by 1.2 percentage points the likelihood of being classified as overweight, by 0.5 percentage points of being obese, and by a 0.9 percent of the bodyweight.	Gender.
Fumarco et al. (2026). **Age-based/grade-based outcomes**: Composite effect of ASE on dietary behavior.	European countries. HBSC.	2SLS. **Identifying variation**: cutoff-based variation.	There is a negative relative age effect on overweight and being on a diet, a positive effect on eating vegetables and fruits, a negative effect on frequency of consuming sweets and soft drinks, a positive effect on frequency of breakfast on week days.	None.
Fumarco and Schultze (2020). **Age-based/grade-based outcomes**: Composite effect of ASE on sports activity.	European countries. HBSC.	2SLS. **Identifying variation**: cutoff-based variation.	There is a positive effect on time spent doing sports, a negative effect on watching tv and playing videogames.	Analyses by broad age groups suggest these effects might disappear or reverse in time.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Levasseur (2022). **In school outcomes (at different ages/grades)**: Composite effect of ASE on nutritional outcomes (BMI-for-age (z-score), Underweight / Overweight, Height-for-age (z-score), Moderate stunting) measured within a school-based sample at varying biological ages (11–15).	Brazil. PeNSE 2015 survey data on 9,297 middle school students aged 11–15. Includes measured height, weight, birth month/year, and family background.	2SLS strategy using birth month to instrument late vs. early school enrollment. Controls for demographics, maternal education, wealth, and school fixed effects. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, *Time-in-school*, and *Age-at-outcome*. Students differ by ages and grade is not fixed. **Key channels**: School meals access, childcare expenditures.	A one-year delay reduces **BMI-for-age**: by 0.140 SD on the whole sample, by 0.213 SD for boys (statistically significant at 10% level), by 0.167 if born from mother with little education, while being not statistically significant for girls and being born from better educated mother; it reduces **overweight** by 5.9 % points for boys and by 6.1 % points if born from better educated mother (while not significant for girls, the whole sample, and being born from a mother with little education); it reduces **underweight** by 1.7 % points if born from better educated mother, while being not statistically significant effect for whole sample, boys, girls, and being born from little educated mother; it reduces **height-for-age** by 0.30 SD on the whole sample, by 0.474 SD for girls, by 0.186 SD if born from better educated mother, by 0.739 if born from little educated mother, while being not statistically significant for boys; and it increases **stunting** by 1.5 pp on the whole sample, by 2.1 pp for boys, by 4.9 pp if born from mother with little education, while being no statistically significant for girls and being born from better educated mother.	Gender. Effects higher in low SES families.
Panel B. Risky health behaviors				
Argys and Rees (2008). **Age-based outcomes**: Composite effect of ASE on marijuana use, smoking, alcohol use, sexual activity.	U.S. National Longitudinal Survey of Youth (NLSY) 1997 cohort.	Probit. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	For girls, being younger than the average peer is associated with 0.017-0.024 increase in probability of marijuana use, 0.032-0.035 increase in probability of drinking, and 0.024-0.041 increase in probability of smoking. The effect for drinking disappears when controlling for grade.	There is little evidence of an effect for boys.
Bahrs and Schumann (2020). **Age-based outcomes**: Composite effect of ASE on health (mental and physical).	Germany. German Socio-Economic Panel (SOEP).	RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	One month increase is school starting age increases the self-reported health by 0.042 (4.5% of an SD). The probability to report at least good health increases by 1.6 percentage points. These results are driven by physical health (increase of 0.364; 3.6% of an SD). There is no significant impact on mental health.	None.

Table B.1 Continued: Literature review

Study	**Country & Data**	**Method**	**Effects**	**Heterogeneity**
Johansen (2021). **Age-based outcomes**: Composite effect of ASE on alcohol poisoning, abortion, births/fatherhood, contraception, cohabitation, chlamydia, pregnancy, and partner's relative age.	Denmark. Administrative data.	2SLS using assigned age as an instrument for observed age ; fuzzy RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	Being young-for-grade increases by 1.8 % the probability of an alcohol poisoning by age 20. By age 30, the increase reduces to 1.6 % but it is also not significant. Being younger increases also by 3% the probability of an abortion by age 20, and births between ages 19–22. However, there is no significant effect by age 30. Being younger increases by 8.9 % contraceptive use by age 15 and by 8% by age 16. By age 17, the estimate is smaller and insignificant, and by age 18, the estimate is zero. Being young-for-grade leads also to a higher probability of cohabitation until age 22, and increases the probability to be pregnant between ages 19–22, but there is no significant effect by age 30. Women who cohabit younger have also relatively older partners.	No effect for men on most of the outcomes.
Lopez-Mayan et al. (2024). **Grade-based outcomes**: Composite effect of ASE on adolescent risk-taking in Spain.	Spain. 2018 Spanish School Survey on Drug Use (SSSDU), nationally representative sample of 21,156 students enrolled in the last two grades of lower secondary (grades 3 and 4) and of upper secondary education (high school and vocational education).	RDD using month-of-birth cutoff (Jan 1) and 1-month bandwidth. Compares December-born (young-for-grade) vs. January-born (old-for-grade). Controls for demographics, grade, school, and retention. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and *Age-at-outcome*; with (current) *Time-in-school* held fixed by design. **Key channels**: Differences partly explained by maturity and school cycle.	Young-for-grade students are significantly less likely to engage in risky behaviors (e.g., alcohol, tobacco, gambling, risky sex). Effects diminish by age 17–18.	Gender. Public vs. Private schools. For gambling, the effects are larger for boys, while for drinking and smoking tobacco, the negative effects are larger for girls. Some effects are larger for public schools (vs. private schools).
Routon and Walker (2023). **Grade/school-stage based outcomes**: Composite effect of ASE on beer and wine consumption.	U.S. The Freshman Survey (TFS) from Higher Education Research Institute. The College Senior Survey (CSS) from Higher Education Research Institute.	RDD. **Identifying variation**: cutoff-based variation.	During high school, a month increase in ASE results in a 0.1-0.3 percentage points decrease in beer consumption and a 0.1-0.2 percentage points decrease in wine consumption. During the college (18-22), the effect is only significant for males. Older male undergraduates are 6.1 percentage points less likely to report frequent beer consumption and are 6.1 percentage points less likely to report frequent wine consumption.	Gender.
Panel C. Risky sexual behaviors				

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Black et al. (2011). **Age-based outcomes**: Composite and Decomposed effect of ASE on teenage pregnancy.	Norway. Norwegian Registry Data by Statistics Norway.	2SLS using the expected school starting age as an instrument for the actual school starting age. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design.	Three-month increase in school starting age reduces probability of teenage pregnancy by 0.5 percentage points. However, a three-month increase in school starting age increases the probability of birth within the first 12 years of school by 1.2 percentage points (and thus might interrupt schooling).	The effect is calculated for women only, and is more likely to occur in less advantaged families (mother's education, family size, birth order).
McCrary and Royer (2011). **Post-schooling outcomes**: Composite effect of ASE on educational attainment at motherhood (completed education at the time of birth).	U.S. (Texas and California). 1989-2001 Texas and 1989-2002 California natality data of state Department of Health, combined with Natality Detail Files of National Center for Health Statistics.	RDD. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and likely differences in total *Time-in-school* (generated through the effect of ASE on educational attainment); while, *Age-at-outcome* differences are largely netted out.	School entry policies affect education at motherhood for 14 (24) percent of young first-time native mothers in California (Texas); women being born after the school entry date have a year less education.	Only females.
Crime				
Cook and Kang (2016). **Biological age-based/Post-schooling adult outcomes:** Composite effect of ASE on juvenile (committed between ages 13 and 15) and adult crime (at ages 17–19).	U.S. (North Carolina). North Carolina Education Research Data Center (NCERDC) and North Carolina Department of Corrections (NCDOC). Administrative data for six cohorts (1987–1989, 1991–1993).	(Fuzzy) RDD, with running variable: exact birth date, and treatment: being born just after cutoff. Estimated effects are reduced-form (ITT) effects. **Identifying variation:** cutoff-based variation. **Identified bundle of components:** For juvenile crime, the bundle of effects mostly include all components (*Starting-age*, *Relative-age*, *Time-in-school*, and *Age-at-outcome*). Though it is an age-based outcome, *Age-at-outcome* is not fixed (13, 14, 15 are pooled together). For adult crime, the bundle of effects abstract away from differences in current *Time-in-school* (since most individuals are out of school). However, *Age-at-outcome* can vary in the sample of individuals, as well as the accumulated total *Time-in-school*, and those differences are still captured by the bundled effect. **Key channels**: School Dropout Policies, Delayed School Exit, Skill Acquisition.	Delayed-entry students are –2.8 percentage points less likely to commit a juvenile crime (with baseline delinquency at around 8.8%, which implies approximately 30% reduction); however, they are 0.80 percentage points more likely to commit an adult crime between ages 17 and 19 (with baseline conviction rate of around 5.68%, which implies approximately a 14% increase ((0.80/5.68)). Reported effects are one-year local discontinuity effects (from comparing individuals born just after vs just before the cutoff).	Differences for children born to unwed mothers; children whose mothers had less than high school education; children eligible a to free/reduced lunch program at age 11 are considered.
Depew and Eren (2016). **Biological age-based outcome**: Composite effect of ASE on juvenile crime committed by age 17.	U.S. (Louisiana). Department of Education and the Department of Public Safety and Corrections, Youth Services, Office of Juvenile Justice.	(Fuzzy) RDD; (reduced form and IV). **Identifying variation:** cutoff-based variation. **Identified bundle of components:** a composite effect of *Starting-age*, *Relative age*, and (current) *Time in-school*; with *Age-at-outcome* held fixed by design. **Key channels**: Skill Acquisition, education as an intermediate outcome channel.	Increasing school entry reduces the incidence of crime.	Gender; ethnicity.

Table B.1 Continued: Literature review

Study	Country & Data	Method	Effects	Heterogeneity
Landersø et al. (2017). **Age-based outcomes**: Composite effect of ASE on propensity to crime and number of crimes committed.	Denmark. Administrative data (Danish register-based data).	RDD. **Identifying variation**: cutoff-based variation.	A higher school starting age lowers the propensity to commit crime at young age but the effect begins to fade out in the twenties.	For girls: being old-for-grade reduces the propensity to commit crime only at the age of 15 (mainly driven by violent crimes), and the propensity to commit crime at or before a given age until age 19. Effect school starting age on crime is smaller when the mother completed $>$ 12y of education; smaller and insignificant when father is not employed. For boys: being old-for-grade reduces the propensity to commit crime at each age until 19 (mainly driven by property crimes), and the propensity to commit crime at or before a given age until age 22. Being old-for-grade reduces the number of committed crimes by half a crime until their mid-twenties.
Peña (2019). **Age-based outcomes**: Composite effect of ASE on the probability of incarceration in adulthood (until age 30-40).	U.S. (Florida). Birth certificate data kept by the National Bureau of Economic Research, prison records from Offender-Based Information System of the Florida Department of Correction, and cutoff dates for kindergarten. Focus on cohorts born 1978–1988.	RDD; DiD. **Identifying variation**: cutoff-based variation.	Reduction (12–20%) in incarceration for drug trafficking among Black males born after the cutoff. No strong effects for other groups or offense types.	Effect only present for Black males.
Family formation				
Dobkin and Ferreira (2010). **Post-schooling adult outcomes**: Composite effect of ASE on marital status measured on adults aged 30–79.	U.S. (California and Texas). Restricted data from the 2000 Decennial Census Long Form Data.	(reduced-form) RDD exploiting school-entry cutoffs. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and differences in total *Time-in-school* (likely generated through the effect of ASE on educational attainment, as suggested by findings in Panel B. School progression and attainment).*Age-at-outcome* does not enter the bundle. **Key channels**: SCHOOL DROP OUT POLICIES, DELAYED SCHOOL EXIT	Non significant.	No consistent heterogeneity by gender, ethnicity (white, black, Hispanic), or cohort
Fredriksson et al. (2022). **Post-schooling outcomes**: Composite effect of mother's ASE on maternal age at birth, and child outcomes.	Finland. Administrative register data on women born 1950–59 and 1971–78 and their children. Includes birth outcomes, education, crime, and family background.	RDD using Jan 1 school entry cutoff as instrument for maternal age at birth. Controls for cohort, birth order, and day-of-birth polynomials. **Identifying variation**: cutoff-based variation.	Being born after the cutoff delays motherhood by 0.4 years. Slight reduction in birth weight and gestation, but no effect on child long-term outcomes (education, crime).	None.
Peña (2017). **Post-schooling outcomes**: Composite effect of ASE on college attainment of the spouse and number of children.	Mexico. ENOE.	RDD; month-of-birth fixed effects model. **Identifying variation**: cutoff-based variation. **Identified bundle of components**: a composite effect of *Starting-age*, *Relative-age*, and likely differences in total *Time-in-school* (generated through the effect of ASE on educational attainment); while, *Age-at-outcome* differences are largely netted out.	Regression discontinuity estimates of the impact of an additional year of ASE: The fraction of individuals with a spouse with some college education increases by 1.1 percentage points, the average number of children decreases by 0.04 (fertility effect estimated for women only).	Gender

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Online Appendix C — Cutoff Dates Used in Referred Studies

The Economics of Age at School Entry: Insights from Evidence and Methods

Mariagrazia Cavallo, Elizabeth Dhuey, Luca Fumarco, Levi Halewyck, Simon ter Meulen*

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*Mariagrazia Cavallo: University of Lausanne, Research Fellowship at Luxembourg Institute of Socio-Economic Research (LISER).
Elizabeth Dhuey: University of Toronto.
Luca Fumarco: Masaryk University, GLO, HEDG, IZA, J-PAL.
Levi Halewyck: Maastricht University.
Simon ter Meulen: ifo Institute, University of Munich, CESifo.

This appendix reports cutoff dates for illustrative purposes. For further details, we invite the reader to refer to the original studies (see Table C.1), or to the local institutional material for exact dates and years (for example, the exact cutoff date might be the end of the school cohort, December 31st, or its beginning, January 1st; dates might change across time, such as in Germany and the U.S.).

Table C.1: Countries age at school entry documentation.

Country	Source
Argentina	González & Dip (2024)
Australia	https://alt-qed.qed.qld.gov.au/aboutus/rti/DisclosureLogs/disclosure-log-340-5-2044.pdf
Chile	McEwan & Shapiro (2008)
China	Huang et al. (2020)
Estonia	https://www.eesti.ee/eraisik/en/artikkel/education-and-research/general-education/compulsory-school-attendance
Germany	https://handbookgermany.de/en/school-enrolment
Germany	Geißler & Hartmann (2025)
Greenland	Rex et al. (2014)
Isle of Man	https://desc.gov.im/education/education/primary-education/enrolling-for-primary-school
Israel	Attar & Cohen-Zada (2018)
Lesotho	Bor et al. (in press)
Multiple countries, Europe	Borodankova & Coutinho (2011)
Multiple countries, world	https://pirls2021.org/wp-content/uploads/2022/11/Exhibit-2-National-Policies-on-Age-of-School-Entry-and-Promotion.pdf
Multiple countries, world	Fredriksson et al. (2024)
Peru	Morales (2020)
South Africa	https://www.education.gov.za/Informationfor/ParentsandGuardians/SchoolAdmissions.aspx
U.S.	Bedard & Dhuey (2012)

Table C.2: Countries with uniform age at school entry, with associated cutoff date.

Country	Age at school entry, cutoff month
Albania	Age 6 by Sep
Argentina	Age 6 by Jul
Austria	Age 6 by Sep
Bahrain	Age 6 by Sep
Belgium, Flanders	Age 6 by Jan
Belgium, Wallonia	Age 6 by Jan
Bosnia and Herzegovinia	Age 6 by Apr
Brazil	Age 5 by Jan
Bulgaria	Age 7 by Jan
Chinese Taipei	Age 6 by Sep
China	Age 6 by Sep
Croatia	Age 6 by Apr
Cyprus	Age 6 by Sep (age 5 and 8 months prior 2021)
Czechia	Age 6 by Sep
Denmark	Age 5 by Jan
Egypt	Age 6 by Sep
England	Age 4 by Sep (5 prior 2011)
Estonia	Age 7 by Oct
Faroe Islands	Age 6 by Jan
Finland	Age 6 by Jan
France	Age 5 by Jan
Georgia	Age 6 by Sep
Greece	Age 6 by Jan
Greenland	Age 6 by Jan
Hong Kong	Age 5 and 8 months by Sep
Hungary	Age 6 by Aug
Iceland	Age 6 by Jan
Iran	Age 6 by Mehr (about Sep 20^{th})
Ireland	Age 4 to 6 by Jan
Isle of Man	Age 5 by Sep
Israel	Age 6 by Tevet (between mid-December and late January)
Italy	Age 5 by Jan
Japan	Age 6 by Apr
Jordan	Age 5 and 8 months by Sep
Kazakhstan	Age 5 by Jan
Kosovo	Age 5 by Jan
Latvia	Age 6 by Jan
Lesotho	Age 5.5 by Jul
Liechtenstein	Age 6 by Sep
Lithuania	Age 6 by Jan
Luxembourg	Age 7 by Sep
Macao	Age 5 by Jan
Malta	Age 5 by Jan
Montenegro	Age 5 by Jan
Morocco	Age 6 by Mar
Netherlands	Age 6 by Oct
New Zeland	Age 5 by May
North Macedonia	Age 5 by Jan
Northern Ireland	Age 4 by Jul
Norway	Age 5 by Jan
Oman	Age 5 and 8 months by Sep
Perù	Age 6 by Apr since 2011, by Jul in 2009 and 2010, by Aug before 2009
Poland	Age 7 (6 prior 2012) by Jul
Portugal	Age 6 by mid-Sep
Qatar	Age 5 by Jan
Romania	Age 6 by Sep
Saudi Arabia	Age 6, SSY
Scotland	Age 5 by Mar
Serbia	Age 6.5 to 7.5 by Sep
Singapore	Age 6 by Jan
South Africa	Age 5 by Jul
Slovakia	Age 6 by Sep
Slovenia	Age 5 by Jan
South Africa	Age by Jul
Spain	Age 5 by Jan
Sweden	Age 6 by Jan
Switzerland	LEA (both age and cutoff)
Turkey	Age 5.75 by Oct
Uzbekistan	Age 6 by Jan
Wales	Age 5 by Sep

Notes. Israel: the first day of the fourth Jewish month of Tevet, which varies by year, and it is in December. Iran: Mehr typically begins on September 22 or 23. LEA stands for Local Education Authority. SSY stands for Start of School Year. Wording to indicate age at school entry vary; in general, wording such as "in the year when they turn 6" is interpreted as the students have to have turned 5 by January of the year when school starts (for example, students born in January will be 5 years and 9 months old when they start school in September), while "by" means the child has to have turned that age before the beginning of the month.

Table C.3: Australia, Canada, Chile, United Arab Emirates, and Russian Federation age at school entry, with associated cutoff date, by administrative area.

Country	Age at school entry, cutoff month
Australia	
Australian Capital Territory	Age 5 by May
New South Wales	Age 5 by Aug
Northern Territory	Age 5 by Jul
Queensland	Age 5 by Jul
South Australia	Age 5 by May
Tasmania	Age 5 by Jan
Victoria	Age 5 by May
Western Australia	Age 5 by Jul
Canada	
Alberta	Varies locally
British Columbia	Age 6 by Jan
Manitoba	Age 6 by Jan
New Brunswick	Age 6 by Jan
Newfoundland and Labrador	Age 6 by Jan
Nova Scotia	Age 6 by Oct
Ontario	Age 6 by Jan
Quebec	Age 6 by Oct
Saskatchewan	Age 6 by Jan
Chile	
Uniform until 1992	Age 6 by Apr
School-specific since 1992	Age 6 by Apr, May, Jun, Jul
United Arab Emirates	
School-specific	Age 6 by Sep, Apr
Russian Federation	
Moscow City	6.5 by Sep
Rest of Russia	Age 6 by Mar or 6.5 by Sep

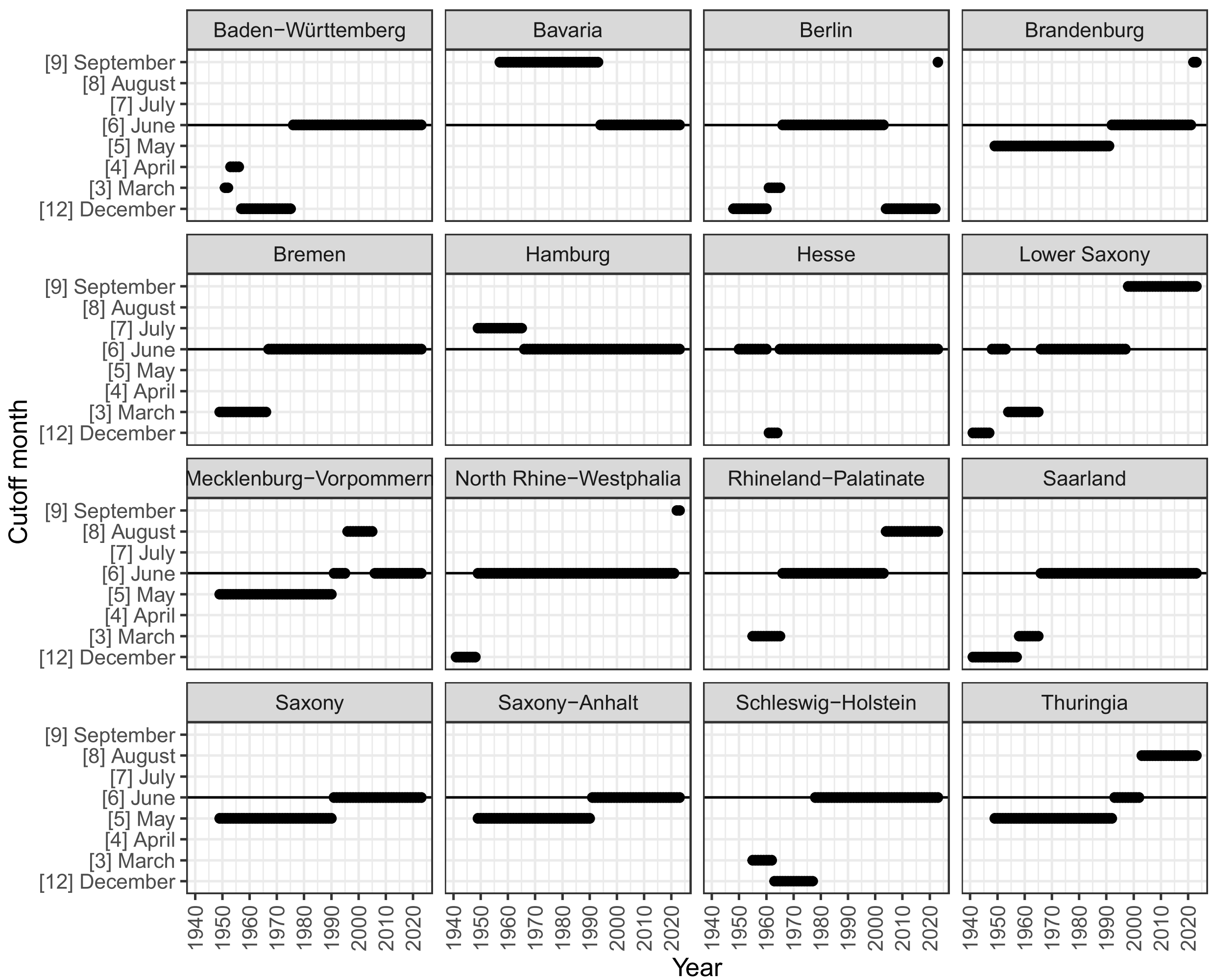
Notes. Wording to indicate age at school entry vary; in general, wording such as "in the year when they turn 6" is interpreted as the students have to have turned 5 by January of the year when school starts (for example, students born in January will be 5 years and 9 months old when they start school in September), while "by" means the child has to have turned that age before the beginning of the month.

Table C.4: U.S. age at school entry, 5, with associated cutoff date, by state.

US	Age at school entry, 5, cutoff month
AL	by Oct
AK	by Nov
AZ	by Jan ($\leq$ 1978), Dec (1979), Nov (1980), Oct (1981), Sep ($\geq$1982)
AR	by Oct
CA	by Dec
CO	LEA
CT	by Jan
DE	Sep ($\leq$1968), by Jan ($\geq$1979)
FL	by Jan ($\leq$1979), by Dec (1980), by Nov (1981), by Oct (1982), by Sep ($\geq$1983)
GA	none ($\leq$1984), by Sep ($\geq$1985)
HI	by Jan
ID	by Oct
IL	by Dec ($\leq$1985), by Nov (1986), by Oct (1987), by Sep ($\geq$1988)
IN	LEA
IA	by Oct 15 ($\leq$1974), Sep 15 ($\geq$1975)
KS	SSY ($\leq$1964), by Sep ($\geq$1965)
KY	by Jan ($\leq$1978), Oct ($\geq$1979)
LA	by Jan
ME	by Oct
MD	Unknown
MA	LEA
MI	by Dec
MN	by Sep
MS	by Jan(1976), Dec (1977), Nov (1978) Oct (1979), Sep ($\geq$1980)
MO	by Oct ($\leq$1985), by Sep (1986), by Sep (1987), by Aug ($\geq$1988)
MT	SSY ($\leq$1979), by Sep 10 ($\geq$1979)
NE	by Oct
NH	by Oct
NJ	LEA
NM	by Jan ($\leq$1972), by Sep ($\geq$1973)
NY	by Dec
NC	by Oct ($\leq$1969), by Oct 15 ($\geq$1970)
ND	by Nov ($\leq$1974), by Oct (1975), by Sep ($\geq$1976)
OH	none ($\leq$1964), by Nov ($\geq$1965), by Oct ($\geq$1969)
OK	by Nov ($\leq$1979), by Sep ($\geq$1980)
OR	by Nov 15 ($\leq$1985), by Sep ($\geq$1986)
PA	by Feb
RI	none ($\leq$1966), by Jan ($\geq$1967)
SC	none ($\leq$1977), by Nov ($\geq$1978)
SD	by Nov ($\leq$1978), by Sep ($\geq$1979)
TN	by Jan ($\leq$1965), by Dec (1966), by Nov (1967), by Oct ($\geq$1968)
TX	by Sep
UT	SSY ($\leq$1987), by Sep 2 ($\geq$1988)
VT	by Jan
VA	by Oct ($\leq$1973), by Nov (1974), by Dec ($\geq$1978), LEA ($\geq$1979)
WA	SSY ($\leq$1976), by Sep ($\geq$1977)
WV	none ($\leq$1971), by Nov ($\geq$1972), by Sep ($\geq$1983)
WI	by Dec ($\leq$1978), Sep ($\geq$1979)
WY	by Sep 15

Notes. Cutoffs change dates are defined as the school year in which the change went into effect. LEA stands for Local Education Authority. SSY stands for Start of School Year. None indicates that the cutoff is not reported in the state statute. Wording to indicate age at school entry vary; in general, wording such as "in the year when they turn 6" is interpreted as the students have to have turned 5 by January of the year when school starts (for example, students born in January will be 5 years and 9 months old when they start school in September), while "by" means the child has to have turned that age before the beginning of the month.
Source. Bedard & Dhuey (2012).

Figure C.1: Germany age at school entry, 6, with associated cutoff date, by state and year, since the end of World War 2.



Notes: This figure is taken from Geißler & Hartmann (2025); for precise dates, see that original paper, currently under review.

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Online Appendix D — Complete Reading List

The Economics of Age at School Entry: Insights from Evidence and Methods

Mariagrazia Cavallo, Elizabeth Dhuey, Luca Fumarco, Levi Halewyck, Simon ter Meulen*

February 24, 2026

*Mariagrazia Cavallo: University of Lausanne, Research Fellowship at Luxembourg Institute of Socio-Economic Research (LISER).
Elizabeth Dhuey: University of Toronto.
Luca Fumarco: Masaryk University, GLO, HEDG, IZA, J-PAL.
Levi Halewyck: Maastricht University.
Simon ter Meulen: ifo Institute, University of Munich, CESifo.

Space limitations constrained the reference list in the main text. To compensate for this limitation and assist researchers new to the economics of age-at-school-entry and closely related topics (such as month-of-birth confounders), this appendix reports the complete set of readings underlying the exposition. While most studies in this list pertain to economics, a few contributions come from other other social sciences, and medicine.

Figure 1 reports the time trend of studies included in this list, excluding the few early contributions before year 2000. The figure shows a steady increase in publications starting in the mid-2000s.

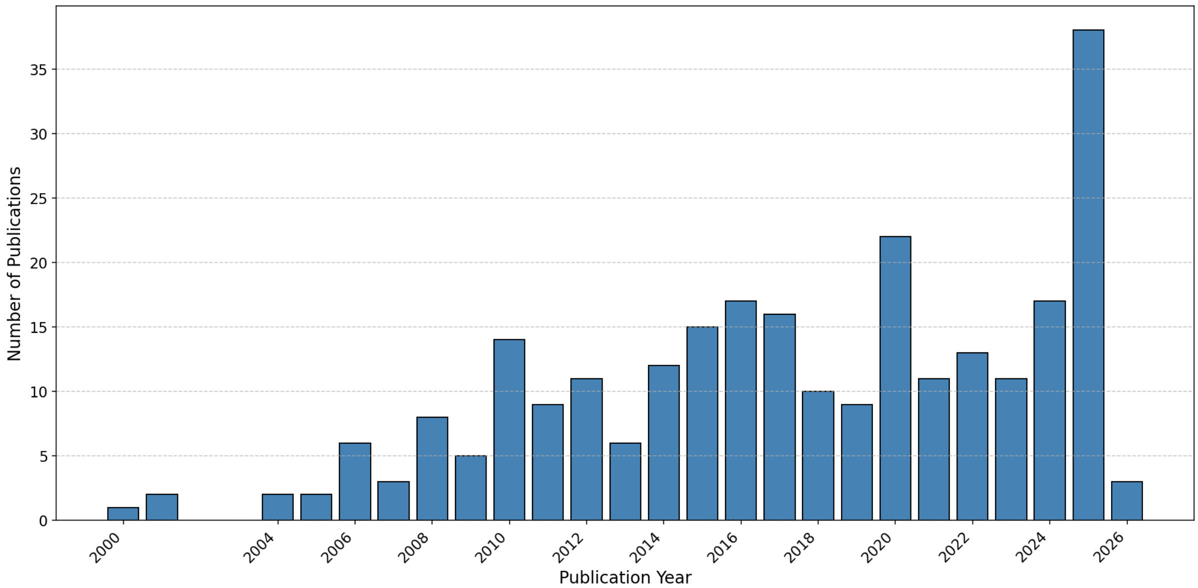

Figure 1: Yearly studies on age at school entry.

To the best of our knowledge, the earliest study on age at school entry dates back to Bigelow (1934). Economics picked up the topic much later, with early influential works by Angrist and Krueger (1991, 1992) as well as Allen and Barnsley (1993). Research activity increased markedly following Bedard and Dhuey (2006), and the number of studies has continued to grow in recent years, reaching a peak in 2025 among the papers reviewed here. Based on what we observe in the list we assembled, the quantity of mimeos, work-in-progress, theses, forthcoming, and recently published papers (more than 40 across these categories, since 2025 included) suggests the upward trend is going to persist.

All entries in this list report authors' full first names to facilitate identification and retrieval, particularly for studies that may be harder to locate, such as working papers, mimeos, or PhD theses.

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