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JEL Classification: I21, I28, J13

The Earlier, the Better? School Entry and Child Maltreatment

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Abstract

Debate over the optimal school starting age has focused largely on children's educational and developmental outcomes, with little attention given to whether starting school earlier may protect vulnerable children from maltreatment. This paper examines that possibility by exploiting a date-of-birth cut-off for school entry in Australia. We find that earlier school entry reduces the probability of a maltreatment notification from a non-education reporter by 4.3 percentage points or 50% relative to the mean. The reduction is driven largely by fewer police notifications and notifications involving emotional abuse. In contrast, notifications from education-sector reporters are unaffected overall, reflecting offsetting changes in notifications from preschool and school-based educators. The protective effects are concentrated among children without younger siblings and extend to older siblings, with the pattern of results suggesting that relaxed childcare constraints and increased maternal employment may be important mechanisms.

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1 Introduction

When should children start school? School starting ages vary widely across countries, and in many systems parents and schools can delay enrolment until a child is deemed "ready".[1] Much of the economics literature and policy debate has focused on the potential benefits of starting later. Older school starters often perform better on tests and cognitive assessments (Attar and Cohen-Zada, 2018; Dhuey et al., 2019; Görlitz et al., 2022), display fewer behavioural problems and stronger self-regulation (Dee and Sievertsen, 2018; Balestra et al., 2020), engage in less risky behaviour and juvenile crime (Depew and Eren, 2016; Landersø et al., 2017), and in some settings experience better adult health outcomes (Bahrs and Schumann, 2020; Arnold and Depew, 2018; De Neve et al., 2026).[2] This evidence has helped support the view that delayed school entry improves children's outcomes. But a later start may also carry an important cost. Delaying enrolment keeps children out of school, and therefore in the home or other care arrangements for longer. For children in vulnerable households, this may increase exposure to abuse or neglect. The issue may be especially important because children who are most likely to have their entry delayed may also be those most in need of the structure and supervision that schools provide.

Child maltreatment is a critical outcome to consider, given its prevalence and severe long-run consequences. A large literature shows that exposure to abuse or neglect has lasting effects on physical and mental health, cognitive and non-cognitive development, educational attainment, labour market outcomes, and criminal behaviour (Gilbert et al., 2009; Currie and Tekin, 2012; Doyle and Aizer, 2018; Schurer et al., 2019; Henkhaus, 2022). These harms also translate into substantial economic costs. For example, Conti et al. (2021) estimate that the discounted lifetime cost of a single non-fatal case of child maltreatment in the UK is £89,390. Yet school-entry policies are rarely discussed through this lens. We examine whether earlier entry into formal schooling reduces children's risk of child protection involvement, distinguishing between reporting effects and changes in underlying maltreatment, and we explore the mechanisms underlying any effects.

We use linked administrative records covering all children in South Australia, including their child

[1] In the United States, for example, the age at first-grade entry has drifted upward over time, largely reflecting parental or school decisions (Deming and Dynarski, 2008).

[2] Though the majority of studies find positive effects of older school entry, several report mixed or opposite results. For example, Black et al. (2011) find small negative effects on earnings until about age 30, with limited impacts on educational attainment. Cook and Kang (2016) show relatively older entrants are more likely to drop out and to commit a felony by age 19 despite better in-school performance. Arnold and Depew (2018) find improved adult health for males but lower high-school completion and no clear labour market gains.

protection and school enrolment histories. To estimate the effect of earlier school entry on child protection involvement, we exploit the date-of-birth cut-off for school-start eligibility in a regression discontinuity design. The linked data allow us to observe actual enrolment and the source of each notification, enabling us to estimate the effect of enrolment itself, rather than eligibility alone, and to distinguish between reporting effects and changes in underlying maltreatment. The data also allow us to link siblings within households and examine spillover effects on other children in the family.

Our first main finding is that school enrolment does not increase child protection notifications from education-sector reporters. Although we find that school entry increases notifications from school-based educators, there is an offsetting reduction in notifications from childcare- and preschool-based educators, resulting in the absence of any overall effect. This suggests that, in our context, earlier school entry does not improve the detection of underlying maltreatment overall. This contrasts with the teacher-reporting effects documented by Benson et al. (2025) in the U.S. context and suggests that the detection effects of school exposure may depend on institutional features such as attendance patterns in childcare and preschool, the intensity of non-school care, and staff reporting practices across settings.

Our second main finding is that earlier school entry reduces child protection notifications from non-education sources. Enrolment in school reduces the probability of receiving one or more notifications from non-education reporters by 4.3 percentage points and reduces the number of such notifications by 50%. The decline is driven largely by police reports and maltreatment relating to emotional abuse, which in our setting often reflect children's exposure to intimate partner violence (IPV). Overall, this pattern suggests a genuine reduction in underlying maltreatment risk, rather than a change in detection. It is also consistent with emerging evidence that greater time in school or formal care can protect children from maltreatment. Dzulkipli et al. (2025) show that raising the school-leaving age in Australia reduced the probability of being reported to CPS, while Sandner et al. (2025) find that expanded public childcare provision in Germany lowered maltreatment cases leading to out-of-home placement.

Several mechanisms could explain why earlier school entry reduces maltreatment risk. First, school attendance reduces time in parental care, and therefore potential exposure to risky home environments. Second, school entry lowers out-of-pocket childcare costs, which can ease financial stress and thereby reduce the parental conflict and mental strain that can contribute to abuse and neglect. Third, by relaxing parental childcare constraints, earlier school entry may increase maternal employment and strengthen mothers' economic position within the household, potentially reducing IPV. These channels imply different patterns across family structure and socioeconomic circumstances, as well as different spillover effects. We examine these channels through heterogeneity analyses and estimates of effects on siblings.

Our third main finding is that the additional results suggest a mechanism operating through increased maternal employment and reduced household conflict. The reduction in maltreatment is concentrated among children who are the youngest in the household, precisely where school entry is most likely to relax childcare constraints and increase mothers' labour supply, but is absent when the focal child is not the youngest.[3] We also find spillover effects on older siblings, which is consistent with school entry affecting broader household conditions rather than operating through the treated child spending less time at home. The effects are also larger among mothers with stronger prior labour market attachment, again suggesting a maternal employment response. Together with the concentration of effects in police reports and emotional abuse, these patterns are consistent with earlier school entry improving mothers' economic position and reducing children's exposure to IPV and other forms of household conflict.

Overall, our findings suggest that later school entry, which is often justified on the grounds of improving academic outcomes, may carry hidden costs by increasing children's exposure to violence in the household. If jurisdictions, schools, and parents continue to pursue delayed entry, greater support for vulnerable families before formal schooling begins may be needed to avoid increasing children's exposure to maltreatment.

The rest of this paper is structured as follows. Section 2 provides an overview of the data used in the paper. Section 3 discusses our empirical strategy and the corresponding assumptions required for identification. In Section 4, we present our main findings on the effect of primary schooling on maltreatment. Section 5 discusses and examines potential mechanisms underlying our main results. Lastly, we conclude in Section 6.

[3] A large literature shows that earlier entry into school or preschool increases maternal labour supply, with effects often concentrated in families where the focal child is the youngest and school entry therefore relaxes the binding childcare constraint (Gelbach, 2002; Cascio, 2009; Bauernschuster and Schlotter, 2015; Zhu and Bradbury, 2015; Gibbs et al., forthcoming).

2 Data

2.1 Administrative Data and Sample Construction

Our data source is linked administrative records on all children born in South Australia from January 1986 to December 2017, accessed through the Impacts of Child Abuse and Neglect (iCAN) data repository (Segal et al., 2019). The linked data draw on the South Australian Births Registry and Perinatal Statistics Collection, child protection records from the Department for Child Protection (DCP), and public school enrolment records from the Department of Education. The study received ethics approval from the Department for Health and Wellbeing Human Research Ethics Committee (2020/HRE00347).

Our analysis focuses on children born between January 2009 and December 2011, who turned five between 2014 and 2016 and were therefore exposed to the school entry rule studied in this paper. We exclude children who died during the observation window and children who are never observed in the public school enrolment records. The resulting analysis sample contains 38,610 children.

2.2 School Enrolment

Since 2014, South Australian public schools have used a 1 May date-of-birth cut-off for school entry: children who turn five before 1 May are eligible to start at the beginning of the school year (in late January) in the year they turn five, whereas children who turn five on or after 1 May are eligible to start at the beginning of the following school year (unless they receive special approval to begin earlier). Non-government schools were not subject to this rule and continued to operate with multiple intakes during the study period. Since all children must be enrolled in school by age six, the policy primarily affects whether children start school in the year they turn five or the year they turn six.

We measure school enrolment using the South Australian public school census, which records enrolment in government schools. Our first-stage treatment variable is a binary indicator equal to one if a child is enrolled in public school during the calendar year of their fifth birthday, and zero otherwise. This captures whether the child starts school as early as permitted under the public-school eligibility rule.

2.3 Notifications to Child Protection Services

We measure children's exposure to maltreatment using administrative records of notifications to the DCP. Notifications are reports communicated to DCP for suspected maltreatment and may come from a variety of sources. South Australia has comprehensive mandatory reporting laws which require workers who interact regularly with children to report suspected familial abuse or neglect. Some examples of mandated reporters include teachers, medical practitioners, police officers, and social workers. Aside from mandated reporters, notifications can also be made by concerned members of the community. During our sample period, notifications were typically communicated via telephone to a centralised call centre. Additional details regarding the child protection system in South Australia are provided in Appendix A.

While children can be notified to DCP at any age up to their 18th birthday, children's probability of being notified tends to be highest in infancy. Appendix Figure B.1 presents the proportion of children in each age group from 0 to 18 who were the subject of a notification in South Australia between 2004 and 2017. The rate of notifications drops after infancy, but then reaches a second peak around ages 4 to 6, which is approximately the ages in which children commence schooling.

Our primary outcome is a binary indicator of whether children experience any maltreatment notification in the calendar year in which they turn 5 years old. We also examine the count of notifications experienced in that calendar year. Appendix Figure B.2 demonstrates that conditional on having at least one notification, approximately 45% of our sample experience more than one notification throughout the calendar year of their fifth birthday.

Our data record several features of each notification, including the reporter type, response level, and alleged maltreatment type. Police are the largest reporter group, accounting for 28.7% of notifications in this age group. Education professionals are the second largest, accounting for 22.5% of notifications, which includes school-based educators (16.0%) and staff in early childhood education and care (6.5%). Other major reporter groups include government employees (12.4%), non-government employees (11.3%), family and friends (11.3%), and health professionals (10.9%). Information on alleged maltreatment type is available for screened-in notifications, that is, notifications assessed by DCP as meeting the threshold for a response. Among screened-in notifications, maltreatment is classified into four broad categories: physical abuse (23.8%), sexual abuse (13.8%), emotional abuse (47.3%), and neglect (46.8%). These categories are not mutually exclusive, as a notification may involve more than one type of maltreatment.

Using administrative data on child protection notifications has both strengths and limitations. A key strength is that notifications capture serious child protection concerns recorded close to the time of the incident, avoiding the recall error, non-response, and social-desirability issues often associated with survey data (Bowling, 2005; Kjellsson et al., 2014; Black et al., 2017), particularly survey-based measures of maltreatment (Hardt and Rutter, 2004; Schurer et al., 2019). This is important for young children, who cannot reliably self-report abuse or neglect. A limitation is that administrative data miss maltreatment that is never reported to authorities and they also reflect reporting behaviours. This issue is especially relevant in our setting, since school entry may itself change who observes and reports concerns. We therefore examine reporting behaviour in our analysis.

3 Empirical Framework

3.1 Regression Discontinuity Design

The school entry rules in South Australia operate such that children born before 1 May are eligible to start school in the calendar year they turn 5 years old, whereas children born on or after 1 May are only eligible to start school the following year. We use this eligibility criteria in a regression discontinuity (RD) design to estimate the impact of schooling on children's exposure to maltreatment. The design compares children born close to either side of the 1 May cut-off, under the assumption that potential outcomes vary smoothly through the threshold apart from the discontinuous change in school-start eligibility.

The reduced-form RD model estimates the effect of school entry eligibility on our outcomes of interest, and can be represented via the following equation:

$$Y_i = \alpha + \delta D_i + f(X_i) + \varepsilon_i \tag{1}$$

where Y_i is the outcome of interest (i.e. CPS notifications in calendar year of fifth birthday) for individual i, D_i is a binary indicator equal to 1 for individuals born before 1 May (i.e. eligible to start school), and X_i is the running variable which is the day of birth from 1 May. The parameter of interest here, δ, represents the intention-to-treat (ITT) effect of the school entry policy.

In our baseline specification, we adopt a local linear RD approach where we use a linear control function of the running variable and a uniform kernel. We also allow for separate slope coefficients of the running variable on either side of the school entry cut-off to capture differential trends across the cut-off. Year-of-birth fixed effects are additionally included in all regressions.

We also estimate the effect of school enrolment, using school entry eligibility as an instrumental variable (IV) in a two-stage least-squares model. The corresponding first-stage and second-stage estimating equations are represented as follows:

$$
\begin{aligned}
Enrol_i &= \alpha + \gamma D_i + f(X_i) + \epsilon_i \\
Y_i &= \lambda + \beta \hat{Enrol_i} + f(X_i) + \mu_i
\end{aligned}
\tag{2}
$$

where $Enrol_i$ is a binary variable indicating whether individual i is enrolled in school in the calendar year of their 5th birthday, and the other variables are as previously defined. The parameter of interest β can be interpreted as the local average treatment effect (LATE) of schooling amongst the compliers of the school entry policy, provided the standard IV assumptions are satisfied.

We perform optimal bandwidth selection using the procedure developed by Calonico et al. (2014). In particular, we use the mean-squared error (MSE) optimal bandwidth, which ranges between 25 and 53 days depending on the outcome and subsample. For robustness, we present estimates across alternative bandwidths, including the MSE-optimal bandwidth calculated by the placebo-zone-based algorithm of Kettlewell and Siminski (2026). Robust (Eicker-Huber-White) standard errors are used for inference, given that running-variable clustering performs poorly in RD models with discrete running variables (Kolesár and Rothe, 2018).

For count outcomes, such as the number of notifications, we use Poisson specifications rather than linear models. These models are well suited to the non-negative and highly skewed distribution of notification counts. We apply the same RD structure as in the baseline specifications, including the same running variable, bandwidths, and set of controls.

3.2 Identifying Assumptions and Diagnostic Checks

This section discusses the identifying assumptions underlying the RD design and presents diagnostic checks for the validity of the empirical strategy. A key assumption is that individuals cannot precisely manipulate the running variable and sort around the treatment threshold (McCrary, 2008). This is unlikely in our setting because the school entry rule came into effect after the children in our analysis sample were born. Parents therefore could not have anticipated the policy and adjusted the timing of childbirth in response. We nevertheless test for manipulation using the density test proposed by Cattaneo et al. (2020). Appendix Figure B.3 plots the distribution of the running variable around the 1 May cut-off, demonstrating no visible discontinuity. The density test returns a p-value of 0.5095, consistent with no evidence of manipulation.[4]

We also test for balance in pre-treatment characteristics at the cut-off. In the absence of manipulation, children born just before and just after the threshold should be comparable in predetermined characteristics. Appendix Figure B.4 plots local averages for selected birth characteristics and shows no clear visual discontinuities at the cut-off. Table 1 reports formal tests of covariate balance using the baseline specification. None of the estimated discontinuities are statistically significant at conventional levels, supporting the comparability of children born close to the school entry threshold.

For the IV estimates to have a local average treatment effect (LATE) interpretation, the standard assumptions of independence, relevance, exclusion, and monotonicity are required (Imbens and Angrist, 1994). The independence assumption is supported by the RD design and the diagnostic evidence above, which indicate that children born close to either side of the cut-off are comparable. Relevance is directly testable, and we show in Section 4 that school entry eligibility strongly predicts enrolment. The exclusion restriction requires that eligibility affects maltreatment outcomes only through its effect on school enrolment, conditional on the running variable. This assumption is not directly testable, but we are not aware of any other institutional changes at the 1 May cut-off that would directly affect maltreatment risk. Finally, monotonicity requires that eligibility weakly increases the probability of enrolment. Recent work suggests that monotonicity concerns in school-entry RD designs are substantially reduced when treatment is defined as a binary enrolment indicator rather than a continuous school-starting-age

[4]Given that our sample consists of children in the public school system, the density test implicitly provides evidence that there is no discontinuity at the 1 May cut-off in the probability that a child appears in the public school census. This suggests that the school entry rule did not generate sorting into or away from the public-school system.

variable (Attar et al., 2024).

4 Main Results

4.1 School Enrolment

We begin by examining the relationship between the school entry rule and actual enrolment. Figure 1 plots the fraction of children enrolled in school in the calendar year of their fifth birthday by date of birth, using two-day bins relative to the 1 May cut-off. The figure shows a large discontinuity in enrolment: most children born before 1 May commence school in the year they turn five, whereas very few children born on or after the cut-off do so. The corresponding RD estimate indicates that school entry eligibility increases the probability of enrolment by 80.6 percentage points (p-value < 0.01), with similar estimates across alternative bandwidths (Appendix Figure B.5). This large first stage confirms that the eligibility rule generates substantial variation in school enrolment, which we use below to estimate the effects of earlier school entry.

4.2 Maltreatment Reports from Educators

We begin by examining notifications from education professionals. This is a key first step because school entry may affect child protection notifications through changes in reporting, even if underlying maltreatment is unchanged. Once children enter school, teachers and other school staff observe them regularly and may identify concerns that would otherwise go unreported. Consistent with this detection channel, Benson et al. (2025) find that school eligibility increases educator reports of maltreatment in the United States. In our setting, however, the effect of school entry on education-sector reports is less clear ex ante, because children who have not yet started school may instead be observed by staff in preschool or childcare settings.

Panel A of Figure 2 plots notifications from all education professionals around the school entry cut-off, including from school-based educators and staff in preschool and childcare settings. We find little visual evidence of a discontinuity in either the probability of a notification or the number of notifications. The corresponding estimates in Panel A of Table 2 are positive but statistically insignificant. Eligibility to start school increases the probability of an education-sector notification by 0.6 percentage points, relative to a control mean of 3.2%, while the fuzzy RD estimate implies a 0.7 percentage point effect of school enrolment. The Poisson estimates for notification counts are also positive but statistically insignificant. Overall, there is no evidence that school entry increases notifications from education professionals as a whole.

This aggregate result masks offsetting changes across types of education reporters. Panels B and C of Figure 2 separately examine reports from school-based educators and from educators in preschool and childcare settings. The visual evidence shows a clear increase in school-based reports at the cut-off, alongside a decline in reports from preschool and childcare educators. The estimates in Table 2 confirm this pattern. Eligibility to start school increases the probability of a notification from school-based educators by 1.7 percentage points, an 80% increase relative to the control mean. The corresponding fuzzy RD estimate implies that school enrolment increases this probability by 2.1 percentage points, or 99%. For notification counts, the Poisson estimates imply even larger proportional effects: school entry eligibility increases the number of school-based educator reports by 104%, while school enrolment increases them by 166%.

At the same time, school entry reduces notifications from preschool and childcare educators. Eligibility to start school reduces the probability of a notification from these reporters by 1.4 percentage points, approximately equal to the control mean, while the fuzzy RD estimate implies a 1.8 percentage point reduction. The Poisson estimates also show large declines in notification counts, with reductions of 83% for eligibility and 87% for school enrolment. These offsetting effects suggest that school entry shifts children between education settings and changes which education professionals are likely to observe and report maltreatment concerns, rather than increasing detection by the education sector overall.[5]

These findings are robust to a range of alternative specifications and inference procedures. Appendix Figure B.6 shows that the point estimates remain stable across alternative bandwidths. Appendix Table B.1 reports similar results using a quadratic control function of the running variable, while Appendix Table B.2 shows robustness to alternative inference procedures, including clustering standard errors by the running variable and using the honest RD approach of Kolesár and Rothe (2018).

[5]This result contrasts with Benson et al. (2025), who find that school eligibility increases educator reports in the United States. The reporting effect of school entry depends on the counterfactual care environment. When children not yet in school are mainly at home or in informal care, school entry may substantially increase contact with mandated reporters. In contrast, when they commonly attend preschool or childcare, and staff in those settings are mandated reporters, school entry may instead shift reporting across education professionals. Differences in preschool attendance, hours of care, reporting obligations, and compulsory staff training will therefore contribute to different patterns across settings.

Appendix Table B.3 provides an additional robustness check by estimating effects on education-sector notifications in the calendar years children turn three, four, and six. Outside the year children turn five, children on either side of the cut-off should generally be in similar care settings: at ages three and four, children are typically not yet in school, while by age six all children should be enrolled.[6] Reassuringly, the estimated discontinuities in the age-three, age-four, and age-six outcomes are all statistically insignificant. This timing pattern supports the interpretation that the main education-sector results reflect the change in care setting induced by school entry, rather than pre-existing differences across the cut-off or broader discontinuities in reporting behaviour.

We also examine whether the offsetting changes in education-sector reporting alter the composition of reported maltreatment. Even if total notifications from education professionals are unchanged, a shift from preschool and childcare educators to school-based educators could affect which types of maltreatment are recorded. Appendix Table B.4 shows that the distribution of reported maltreatment types is broadly similar across the two groups of educators. Consistent with this descriptive evidence, the RD estimates in Appendix Table B.5 provide little evidence that school entry changes the composition of maltreatment types reported by education professionals. Across all education professionals, school-based educators, and preschool and childcare educators, the estimates are small in absolute terms, imprecisely estimated, and not statistically significant at conventional levels.

4.3 Maltreatment Reports from Non-Education Sources

We next estimate the effect of school enrolment on notifications from reporters outside the education sector, including police, health professionals, relatives, and neighbours. Figure 3 plots the probability of any notification and the average number of notifications from non-education reporters around the school entry cut-off. The figure shows a clear decline at the cut-off: children eligible to start school in the year they turn five are less likely to be notified to CPS by non-education reporters, and also receive fewer notifications on average.

Table 3 reports the corresponding reduced-form and fuzzy RD estimates. Column (1) shows that children born before the entry cut-off are 3.5 percentage points less likely to receive a notification

[6] The age-four comparison should be interpreted with some caution because participation in formal preschool programs may also vary around the school-entry cut-off. In particular, children born before the cut-off may be more likely to attend formal preschool in the year before school entry, which could create some differences in exposure to preschool educators even before primary school begins.

from a non-education reporter in the year they turn five. This represents a 40% reduction relative to the control mean. The fuzzy RD estimate in Column (2) implies that school enrolment reduces the probability of a notification from a non-education reporter by 4.3 percentage points, or 50% relative to the mean. The count estimates tell a similar story. The Poisson estimates in Columns (3) and (4) indicate that eligibility reduces the number of notifications from non-education reporters by 44%, while school enrolment reduces the number of notifications by 51%.

We interpret these estimates as evidence of school enrolment causing a genuine reduction in maltreatment, rather than changing maltreatment reporting. Unlike education-sector notifications, reports from non-education sources should not mechanically increase or decrease when children start school. In the absence of a change in underlying maltreatment risk, school entry should have little direct effect on whether children come into contact with police, relatives, neighbours, or other non-education reporters. One possible exception is health professionals, since school entry may affect healthcare use through changes in parental time or income. However, the reporter-specific estimates shown below provide no evidence that the decline is driven by health professionals.

As with the educator results, the estimated effects for non-education reporters are robust to alternative specifications and inference procedures. Appendix Figure B.7 shows that the point estimates remain stable across alternative bandwidths, Appendix Table B.6 reports similar results using a quadratic control function of the running variable, and Appendix Table B.2 demonstrates that the estimates are robust to alternative inference procedures. Appendix Table B.7 repeats the analysis for notifications in the calendar years children turn three, four, and six. The age-six estimates, which are close to zero and statistically insignificant, are particularly informative. The small estimates indicate that the discontinuity disappears once children on both sides of the cut-off have entered school, implying that there is no rebound in notifications among earlier entrants, which might occur if the age-five reduction reflected only a temporary reduction in maltreatment.

To examine which reporters drive the reduction in notifications, Table 4 presents fuzzy RD estimates by reporter category. The estimates are negative across all reporter groups, with effect sizes ranging from 49% to 91% relative to the control mean. The largest reductions are for notifications by police (3.2 ppt, $p < 0.01$) and employees in non-government organisations (1.6 ppt, $p < 0.05$). Police are the largest source of notifications in our sample, accounting for 28.7% of notifications in this age group, and often make reports after attending households for incidents involving family violence or other safety concerns (Mathews et al., 2016). Employees in non-government organisations include shelter workers, family support workers, and social welfare workers, who may come into contact with families experiencing violence, housing instability, financial hardship, or other forms of acute household stress.

To better characterise how children's exposure to maltreatment is changing, we disaggregate notifications by maltreatment type and present the corresponding RD estimates in Table 5.[7] Column (3) shows that school enrolment primarily reduces notifications involving emotional abuse (2.1 ppt, $p < 0.05$). Emotional abuse is a broad maltreatment category that includes children's exposure to threatening, intimidating, or violent household environments (Australian Institute of Health and Welfare, 2022). Police are the most common reporters of emotional abuse in this age group, accounting for 40% of such notifications, consistent with police generating child protection notifications when attending households for domestic and family violence incidents or other safety concerns involving children (Mathews et al., 2016).

In supplementary analyses in Appendix Table B.8, we find that school enrolment reduces children's probability of experiencing a CPS notification that is classified as Tier 1 or 2 (i.e. urgent response required). Despite this, we find no significant effect of school enrolment on investigated or substantiated notifications. Due to heavy caseloads and resource constraints within CPS during this time period, a substantial proportion of notifications were not investigated, even after meeting the risk threshold for investigation (Nyland, 2016). Our data suggests that 70% of children who are assigned a Tier 1 or 2 rating received no investigative response.

5 Mechanisms

The results above show that school entry reduces CPS notifications from non-education sources, with the largest reductions coming from police and from notifications involving emotional abuse. This pattern suggests that school entry affects children's exposure to household circumstances that generate child protection involvement, rather than simply changing which professionals observe and report maltreatment. In this section, we examine possible mechanisms. The evidence is necessarily indirect, because the administrative child protection dataset does not include information on household be-

[7] As noted in Section 2, information on maltreatment type is only available for screened-in notifications, that is, notifications assessed by DCP as meeting the threshold for a response.

haviour, parental employment, or justice-system records. We therefore use heterogeneity and sibling analyses to distinguish between mechanisms that operate through the focal child's own exposure and mechanisms that operate through broader changes in household conditions.

There are three main channels through which earlier school entry could reduce maltreatment. First, school entry may directly reduce the focal child's exposure to risky home environments by reducing the amount of time the child spends at home. Parents are the main perpetrators of child maltreatment (Sedlak et al., 2010; Benson et al., 2025), so reallocating children's time from the home to school may reduce their exposure to abuse and neglect. This mechanism predicts effects for the focal child, but does not predict benefits for older siblings whose own time in the household is not directly changed by the younger child starting school.

Second, school entry may reduce household financial stress by lowering the need for paid childcare. Childcare in Australia represents a substantial household expense, even with public subsidies (Noble and Hurley, 2021; Laß et al., 2025), and so reductions in required childcare may ease household resource constraints. Financial stress is a well-established risk factor for child maltreatment (Berger and Waldfogel, 2011), and this channel is also consistent with evidence that income support and other improvements in household resources reduce child maltreatment (Bullinger et al., 2023; Rittenhouse, 2025). The reduced financial stress mechanism predicts benefits for the focal child, regardless of sibling composition, and also spillover benefits for other children in the household.

Third, school entry may relax parental time constraints, increasing maternal labour supply. Prior evidence shows that earlier school entry can increase maternal employment (Gelbach, 2002; Zhu and Bradbury, 2015), which in turn may strengthen mothers' economic position and reduce exposure to household conflict or IPV.[8] This mechanism is especially relevant in our setting because witnessing IPV can be classified as emotional abuse, and emotional abuse is frequently reported by police. The maternal-employment channel should be strongest when the focal child is the youngest child in the household, because school entry is then most likely to relax the binding childcare time constraint. Consistent with this prediction, Gibbs et al. (forthcoming) find that the significant maternal labour-supply effects of expanded full-day kindergarten are concentrated among mothers whose kindergarten-aged child is their youngest child. The maternal-employment channel should also be stronger among moth-

[8] There is substantial evidence that female economic empowerment can reduce intimate partner violence and family violence more broadly (Aizer, 2010; Bobonis et al., 2013; Anderberg et al., 2015; Sandner et al., 2025).

ers with prior labour-market attachment, who are more likely to return to work. If increased maternal employment improves broader household conditions or reduces household conflict, the benefits may also extend to older siblings.

We first conduct heterogeneity analyses to distinguish between the possible mechanisms described above. We estimate the RD regressions separately by whether the mother was employed or in education at the focal child's birth, and by whether the focal child has a younger sibling at the time of school entry. Appendix Table B.9 presents the corresponding first-stage estimates and shows that eligibility has similar effects on enrolment across these subsamples. This indicates that differences in the estimated notification effects reflect differences in the effect of school enrolment itself, rather than differences in how strongly the eligibility rule changes enrolment. Table 6 reports the fuzzy RD estimates. Columns (1) and (2) show that the effect is concentrated among children whose mothers had stronger prior labour-market attachment. For children whose mothers were not employed or in education at childbirth, the estimated effect of school enrolment is small and statistically insignificant. For children whose mothers were employed or in education, school enrolment reduces the probability of a non-education notification by 4.3 percentage points, or 88% relative to the control mean. Columns (3) and (4) show an even sharper contrast by sibling composition. Among children with no younger sibling, school enrolment reduces the probability of a non-education notification by 6.5 percentage points, or 75% relative to the control mean. In contrast, the estimate for children with younger siblings is small and statistically insignificant.

This pattern is most consistent with a mechanism operating through relaxed childcare constraints and maternal employment. The largest effects occur in the families where school entry is most likely to increase mothers' capacity to work: families in which the mother had stronger labour force attachment, and families in which the focal child is the youngest child. The results are less consistent with a pure direct-exposure mechanism or childcare-cost mechanism, both of which would predict effects for the focal child regardless of whether younger siblings remain in the household.

We next examine whether the effects extend to other children in the household. Siblings commonly share household environments and maltreatment risks (Currie and Tekin, 2012; Witte et al., 2018), so spillover effects are informative about whether school entry changes broader household conditions. If the main mechanism is only that the focal child spends less time at home, then we would not expect benefits for older siblings when the focal child enters school. In contrast, if school entry relaxes household constraints, increases maternal employment, or reduces household conflict, other children in the household may also benefit.

The final columns of Table 6 report RD estimates for sibling outcomes. For younger siblings, the estimates are negative but imprecise and not statistically significant, providing no evidence of adverse spillovers onto younger children. This is consistent with the focal-child heterogeneity results above, which show little effect of school entry when a younger sibling remains in the household. The evidence for older siblings is more informative. We estimate that school enrolment of the focal child reduces older siblings' probability of a non-education notification by 3.9 percentage points, although the estimate is only marginally significant ($p < 0.10$). Because older siblings' own school attendance is not directly changed by the focal child's enrolment, this result is difficult to explain through a pure direct-exposure channel. Instead, it is more consistent with a household-level mechanism, such as increased maternal employment.

6 Conclusion

There is ongoing debate about the optimal age at which children should start school (Dhuey and Koebel, 2022; Cavallo et al., forthcoming). A well-established literature documents benefits of later school entry for test scores, cognitive skills, academic tracking, and other outcomes (Bedard and Dhuey, 2006; Black et al., 2011; Landersø et al., 2017; Balestra et al., 2020; Cavallo et al., forthcoming), contributing to growing interest among parents, school administrators and policymakers in delaying school entry. Much less attention has been paid to a potential cost of delaying school entry, namely whether starting school later increases children's risk of maltreatment. This possibility is consequential given the immediate harms of child abuse and neglect, their severe and persistent effects on children's development and later-life outcomes, and the substantial social costs that result (Gilbert et al., 2009; Currie and Tekin, 2012; Misheva et al., 2017).

Using school entry laws in South Australia and a regression discontinuity framework, we find that earlier school enrolment reduces notifications to Child Protection Services for alleged maltreatment among non-education professionals, most prominently police. In contrast, we find no overall effect on notifications from the education sector, consistent with a redistribution of maltreatment reporting from childcare and preschool educators to school-based educators, across the school entry threshold. As the increase in time spent with educators is unlikely to reduce detection of underlying maltreatment, and may in other contexts increase it (Baron et al., 2020; Benson et al., 2025), we interpret the reduction in non-education notifications as reflecting a genuine reduction in maltreatment risk.

Additional analyses point towards maternal employment as a mechanism underlying our results. In particular, we find that protective effects are concentrated among children who are the youngest in their family, and children whose mothers had stronger prior labour market attachment. These represent households which are likely to experience the largest increase in parental labour supply once the focal child enters school (Gelbach, 2002; Cascio, 2009). We additionally find evidence of spillover benefits to older siblings, which is difficult to reconcile with a mechanism operating solely through the focal child spending less time at home. Together with the concentration of effects in police and emotional abuse notifications, which commonly reflect children's exposure to IPV, these patterns provide evidence of a household-level mechanism in which earlier school entry relaxes childcare constraints, increases maternal employment, and strengthens mothers' economic position within the household, ultimately reducing household conflict.

On balance, our findings indicate that being in the school environment leads to a tangible reduction in children's exposure to maltreatment, and that this protective effect operates primarily through improving household conditions rather than increasing surveillance. As child development is a cumulative process, preventing these children from being exposed to abuse and neglect early in life is likely to translate into meaningful welfare gains across the lifecycle. When advising parents and schools on school readiness and the merits of delayed entry, policymakers should consider not only developmental indicators but also the child's home environment – for children in vulnerable or at-risk households, delaying entry may carry meaningful maltreatment risks that offset the benefits often cited in favour of a later start.

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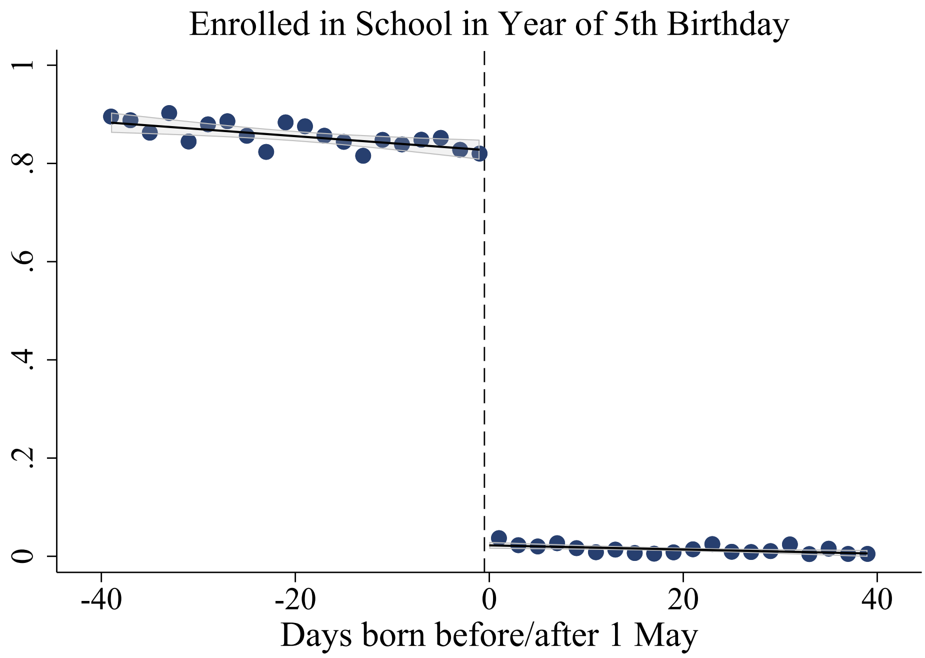
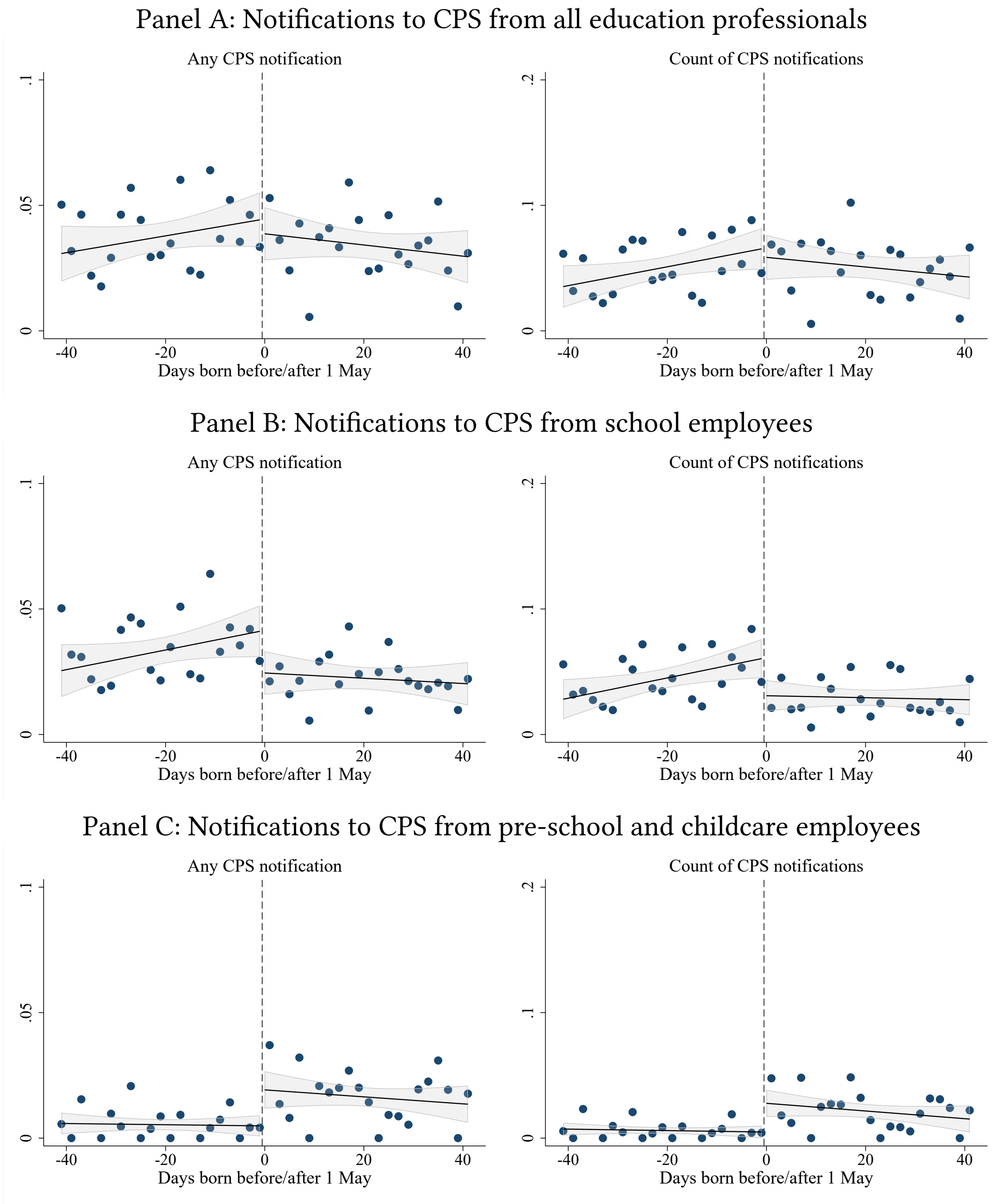
Figures

Figure 1.
School Enrolment Across the School Entry Cut-Off



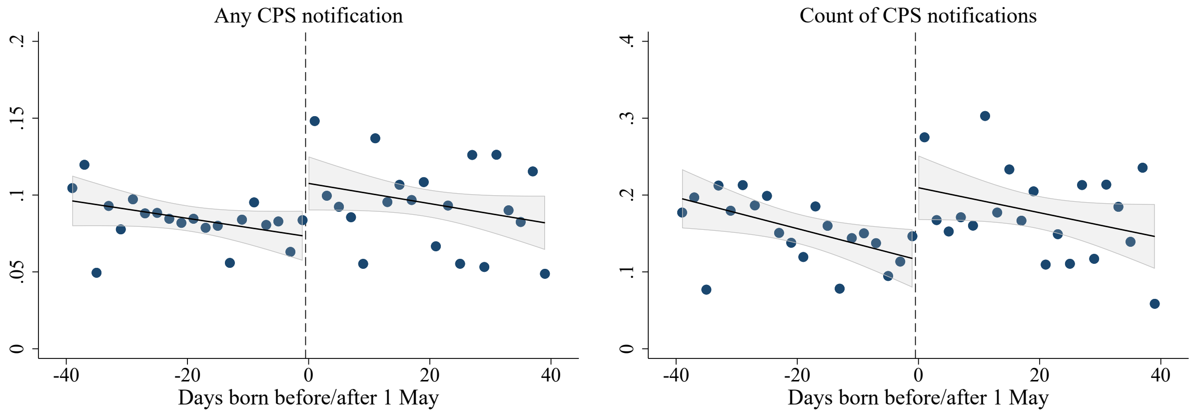
Notes: The above figure plots the proportion of children who were enrolled in public school in the calendar year of their 5th birthday, by day-of-birth (in 2-day bins) relative to the school entry cut-off. Vertical dashed lines depict the school entry cut-off, which is 1 May. Children born before 1 May are eligible to start school the year they turn 5 years old, whereas children born on or after this cut-off are only eligible to start school the following year. Fitted lines are obtained from local linear regressions using a uniform kernel and a bandwidth of 39 days. Grey shaded areas represent 95% confidence intervals. The RD treatment effect estimated using the optimal bandwidth of Calonico et al. (2014) is 0.806 (p-value < 0.01).

Figure 2.
Notifications to CPS by Education Professionals Across the School Entry Cut-Off



Notes: The above figures plot data on notifications to Child Protection Services (CPS) from education professionals (e.g. school teacher, pre-school teacher, childcare worker, principal), based on their workplace setting. The left-hand side graph plots the proportion of children who were the subject of a CPS notification in the calendar year of their 5th birthday, by day-of-birth (in 2-day bins) relative to the school entry cut-off. The right-hand side graph plots the average counts of CPS notifications in this same calendar year. Vertical dashed lines depict the school entry cut-off, which is 1 May. Children born before 1 May are eligible to start school the year they turn 5 years old, whereas children born on or after this cut-off are only eligible to start school the following year. Fitted lines are obtained from local linear regressions using a uniform kernel and a bandwidth of 41 days. Grey shaded areas represent 95% confidence intervals.

Figure 3.
Notifications to CPS by Non-Education Reporters Across the School Entry Cut-Off



Notes: The above figures present data on notifications to Child Protection Services (CPS) from all reporters outside the education sector (e.g. police, health professionals, family and friends, government agency, non-government organisation) in a neighbourhood around the school entry cut-off. The left-hand side graph plots the proportion of children who were the subject of a CPS notification in the calendar year of their 5th birthday, by day-of-birth (in 2-day bins) relative to the school entry cut-off. The right-hand side graph plots the average counts of CPS notifications experienced by children in the same calendar year. Vertical dashed lines depict the school entry cut-off, which is 1 May. Children born before 1 May are eligible to start school the year they turn 5 years old, whereas children born on or after this cut-off are only eligible to start school the following year. Fitted lines are obtained from local linear regressions using a uniform kernel and a bandwidth of 39 days. Grey shaded areas represent 95% confidence intervals.

Tables

Table 1.
Covariate Balance

	Coefficient (1)	(Std. error) (2)	Control mean (3)
Female	-0.031	(0.022)	0.482
Birthweight (in grams)	-14.506	(25.009)	3347.282
Low birthweight ($<$ 2500 g)	0.016	(0.010)	0.066
Gestational age (in weeks)	-0.018	(0.083)	38.744
Low gestational age ($<$ 37 weeks)	0.002	(0.012)	0.086
Any pediatric conditions	0.007	(0.010)	0.059
Mother's age at birth (in years)	-0.022	(0.015)	0.126
Mother's age $<$ 21 years at birth	-0.000	(0.263)	29.246
Mother not married/de facto at birth	-0.003	(0.012)	0.074
Low SES (bottom IRSD quintile)	-0.026	(0.020)	0.313

Notes: This table presents results of covariate balance tests for different birth characteristics of the children in our estimation sample (N = 8421). Each line item represents a separate regression for a given birth characteristic. Column (1) presents the estimated effect of being born before the 1 May cut-off (i.e. children who are eligible to start school in the calendar year of their 5th birthday) on the covariate of interest. Individuals are considered to have low socioeconomic status (SES) if their residential suburb at time of childbirth is in the bottom quintile of the Index of Relative Socioeconomic Disadvantage (IRSD), which is a summary measure of relative disadvantage for an area. Column (3) presents the means of the covariates for children born after the 1 May cut-off (i.e. the control group). All specifications are based on local linear regressions using a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and a bandwidth of 39 days, optimally selected using the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.
RD Estimates of the Impact of Schooling on CPS Notifications from Education Reporters

	Any notification		Count of notifications	
	RF	IV	RF Poisson	IV Poisson
	(1)	(2)	(3)	(4)
Panel A: All education professionals				
Born before 1 May	0.006		0.131	
	(0.008)		(0.244)	
School enrolment		0.007		0.252
		(0.010)		(0.312)
Effect size (%)	17	21	14	29
Mean (born after 1 May)	0.032	0.032	0.045	0.045
Observations	8832	8832	8832	8832
Panel B: Educators in school				
Born before 1 May	0.017^{**}		0.712^{**}	
	(0.007)		(0.277)	
School enrolment		0.021^{**}		0.979^{***}
		(0.009)		(0.362)
Effect size (%)	80	99	104	166
Mean (born after 1 May)	0.021	0.021	0.027	0.027
Observations	8832	8832	8832	8832
Panel C: Educators in pre-school and childcare				
Born before 1 May	-0.014^{***}		-1.763^{***}	
	(0.005)		(0.523)	
School enrolment		-0.018^{***}		-2.017^{***}
		(0.006)		(0.728)
Effect size (%)	-100	-124	-83	-87
Mean (born after 1 May)	0.014	0.014	0.018	0.018
Observations	8832	8832	8832	8832

Notes: This table presents reduced-form (RF) and fuzzy RD estimates of the impact of school enrolment on notifications to Child Protection Services (CPS) from education professionals, based on their workplace. For Columns (1) and (2), the outcome is a binary variable indicating whether the child had any CPS notification in the calendar year of their 5th birthday. For Columns (3) and (4), the outcome is the count of CPS notifications in this same calendar year. Columns (2) and (4) present fuzzy RD estimates of the effect of school enrolment, using school entry eligibility as an instrumental variable (IV). The estimated first-stage discontinuity is 0.803 ($p < 0.01$). Effect sizes for Columns (1) and (2) are calculated as a percent of the control group mean. Effect sizes for Poisson regression coefficients in Columns (3) and (4) are calculated as $[\exp(\hat{\beta}) - 1] \times 100$. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and a bandwidth of 41 days, optimally selected using the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.
RD Estimates of the Impact of Schooling on CPS Notifications from Non-Education Reporters

	Any notification		Count of notifications	
	RF	IV	RF Poisson	IV Poisson
	(1)	(2)	(3)	(4)
Born before 1 May	-0.035***		-0.576***	
	(0.013)		(0.169)	
School enrolment		-0.043***		-0.713***
		(0.016)		(0.212)
Effect size (%)	-40	-50	-44	-51
Mean (born after 1 May)	0.087	0.087	0.166	0.166
Observations	8421	8421	8421	8421

Notes: This table presents reduced-form (RF) and fuzzy RD estimates of the impact of school enrolment on notifications to Child Protection Services (CPS) from non-educators. For Columns (1) and (2), the outcome is a binary variable indicating whether the child had any CPS notification (excluding reports from education professionals) in the calendar year of their 5th birthday. For Columns (3) and (4), the outcome is the count of CPS notifications (excluding reports from education professionals) in this same calendar year. Columns (2) and (4) present fuzzy RD estimates of the effect of school enrolment, using school entry eligibility as an instrumental variable (IV). The estimated first-stage discontinuity is 0.806 ($p < 0.01$). Effect sizes for Columns (1) and (2) are calculated as a percent of the control group mean. Effect sizes for Poisson regression coefficients in Columns (3) and (4) are calculated as $[\exp(\hat{\beta}) - 1] \times 100$. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and a bandwidth of 39 days, optimally selected using the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.
RD Estimates of the Impact of Schooling on Notifications from Different Non-Education Reporters

	Any notification by:				
	Police	Health	Family/friend	Govt.	Non-govt.
	(1)	(2)	(3)	(4)	(5)
School enrolment	-0.032***	-0.009	-0.007	-0.011	-0.016**
	(0.012)	(0.009)	(0.006)	(0.009)	(0.008)
Effect size (%)	-70	-49	-49	-56	-91
Mean (born after 1 May)	0.046	0.019	0.015	0.020	0.018
Bandwidth (days)	34	31	41	32	34
Observations	7369	6718	8832	6931	7369

Notes: This table presents fuzzy RD estimates of the impact of school enrolment on notifications to Child Protection Services (CPS) by different non-education reporters. The fuzzy RD framework uses school entry eligibility (i.e. born before 1 May) as an instrumental variable (IV) for school enrolment in the calendar year of children's 5th birthday. The outcome in Columns (1) to (5) is a binary variable indicating whether the child had any CPS notification in the calendar year of their 5th birthday, reported by either police, health professionals, family and friends, government employees, or employees in non-government organisations respectively. Effect sizes are calculated as a percent of the control group mean. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and an optimal bandwidth selected using the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5.
RD Estimates of the Impact of Schooling on CPS Notifications from Non-Education Sources (by Maltreatment Type)

	Notifications by maltreatment type:			
	Physical	Sexual	Emotional	Neglect
	(1)	(2)	(3)	(4)
School enrolment	-0.002	-0.006	-0.021**	-0.014*
	(0.007)	(0.004)	(0.009)	(0.009)
Effect Size (%)	-15	-77	-83	-63
Mean (born after 1 May)	0.013	0.008	0.025	0.023
Bandwidth (days)	27	47	37	35
Observations	5911	10125	7987	7533

Notes: This table presents fuzzy RD estimates of the impact of schooling on notifications to Child Protection Services (CPS), based on the type of maltreatment associated with that notification. The fuzzy RD framework uses school entry eligibility (i.e. being born before 1 May) as an instrumental variable (IV) for school enrolment in the calendar year of children's 5th birthday. The outcome in Columns (1) to (4) is a binary variable indicating whether the child had any CPS notification in the calendar year of their 5th birthday for physical abuse, sexual abuse, emotional abuse, or neglect, respectively. Effect sizes are calculated as a percent of the control group mean. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and the optimal bandwidth selected using the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.
Subgroup Effects and Sibling Spillovers of Schooling on CPS Notifications

	Mother employed or in education?		Younger sibling?		Sibling spillovers:	
	No	Yes	No	Yes	Younger	Older
	(1)	(2)	(3)	(4)	(5)	(6)
School enrolment	-0.013	-0.043**	-0.065***	-0.012	-0.031	-0.039*
	(0.035)	(0.017)	(0.023)	(0.020)	(0.027)	(0.023)
Effect size (%)	-8	-88	-75	-13	-31	-28
Mean (born after 1 May)	0.153	0.048	0.087	0.087	0.102	0.139
Bandwidth (days)	46	25	33	52	32	35
Observations	2615	3651	3932	5068	3374	5480

Notes: This table presents fuzzy RD estimates of the treatment effects of school enrolment on notifications to Child Protection Services (CPS), across various subgroups. The fuzzy RD framework uses school entry eligibility (i.e. being born before 1 May) as an instrumental variable (IV) for school enrolment in the calendar year of children's 5th birthday. The outcome in Columns (1) to (4) is a binary variable indicating whether a child had any CPS notification (excluding reports from education professionals) in the calendar year of their 5th birthday. The outcome in Columns (5) and (6) is similarly a binary indicator for CPS notifications, but across all reporters. Columns (1) and (2) present treatment effects for children based on whether their mother was in employment or education at the time of childbirth. Columns (3) and (4) present treatment effects for children based on whether they have a younger sibling in the year of enrolment. Lastly, Columns (5) and (6) present treatment effects of the focal child's school enrolment, on younger siblings and older siblings respectively. The estimated first-stage discontinuity for each Column is presented in the Appendix. Effect sizes are calculated as a percent of the control group mean. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and the optimal bandwidth selected using the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendices Intended for Online Publication

A Child Protection System in South Australia

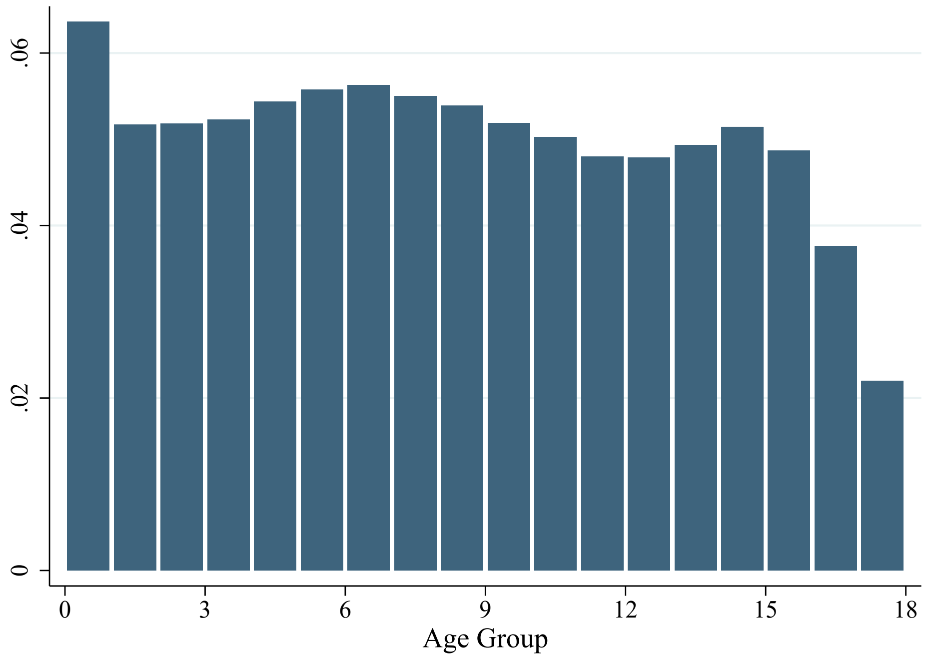
The setting of our study is South Australia, where the CPS agency is known as the Department for Child Protection (DCP). Reports made to the DCP for alleged maltreatment are formally termed *notifications.* Similar to other jurisdictions, South Australia has comprehensive mandatory reporting laws which require professionals and other workers who interact regularly with children to report suspected familial abuse or neglect to DCP. Some examples of mandated reporters include teachers, medical practitioners, police officers, and social workers. Aside from mandated reporters, notifications can also be made by concerned members of the community. Notifications are typically communicated via telephone to a centralised call centre, or more recently, via the internet.

Once received, notifications are screened by the DCP using a decision tool to determine whether a response is required. Screened-in notifications are notifications which meet the threshold for intervention and thus require a response from the DCP. Screened-in notifications may be classified as Tier 1 (highest response priority), Tier 2, or Tier 3. Notifications that are assigned a Tier 1 or 2 rating are classified as high-urgency and designated for investigation by caseworkers. These represent notifications which are assessed as involving a higher risk of potential harm to the child, and where a response is needed within 24 hours (Tier 1), or within 3-10 days depending on the child's age (Tier 2). Tier 3 notifications receive a non-investigative response, typically involving a consultation between the child, their family, and a caseworker.

During the course of an investigation, caseworkers collect information regarding the alleged maltreatment incident by visiting the family and child, inspecting the family home, and conducting interviews. If there is sufficient information demonstrating that the child has been harmed or is at risk of serious harm (meeting DCP thresholds), the notification is *substantiated.* For the most severe cases involving actual or imminent risk of serious harm, a court order can be sought for removal of the maltreated child from the household and placement into *out-of-home care* (OOHC), usually with a relative (often grandparent) or foster carer.

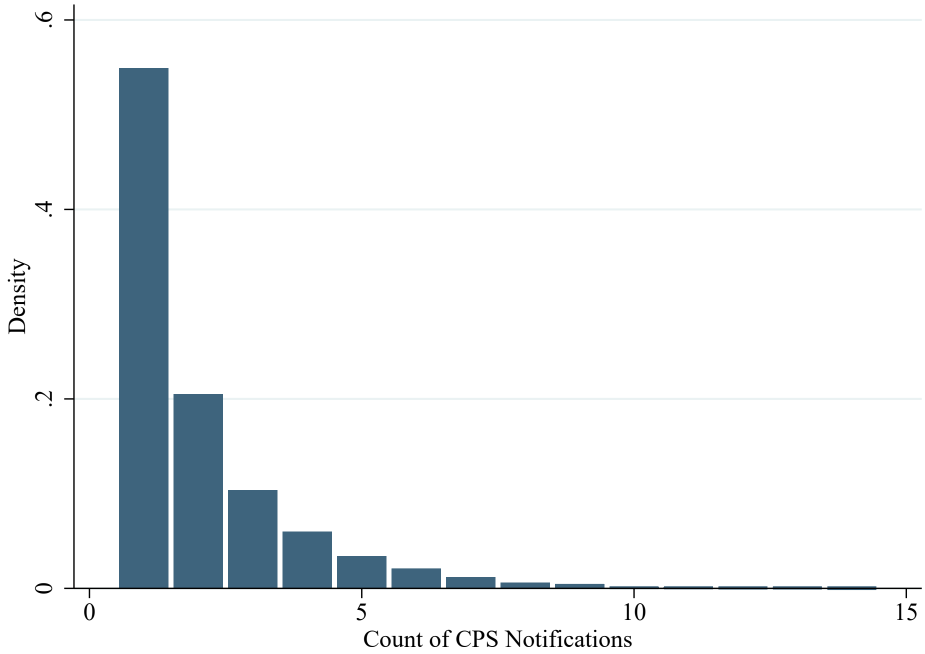
B Additional Figures and Tables

Figure B.1.
Proportion of Children Notified to CPS by Age in South Australia, 2004 to 2017



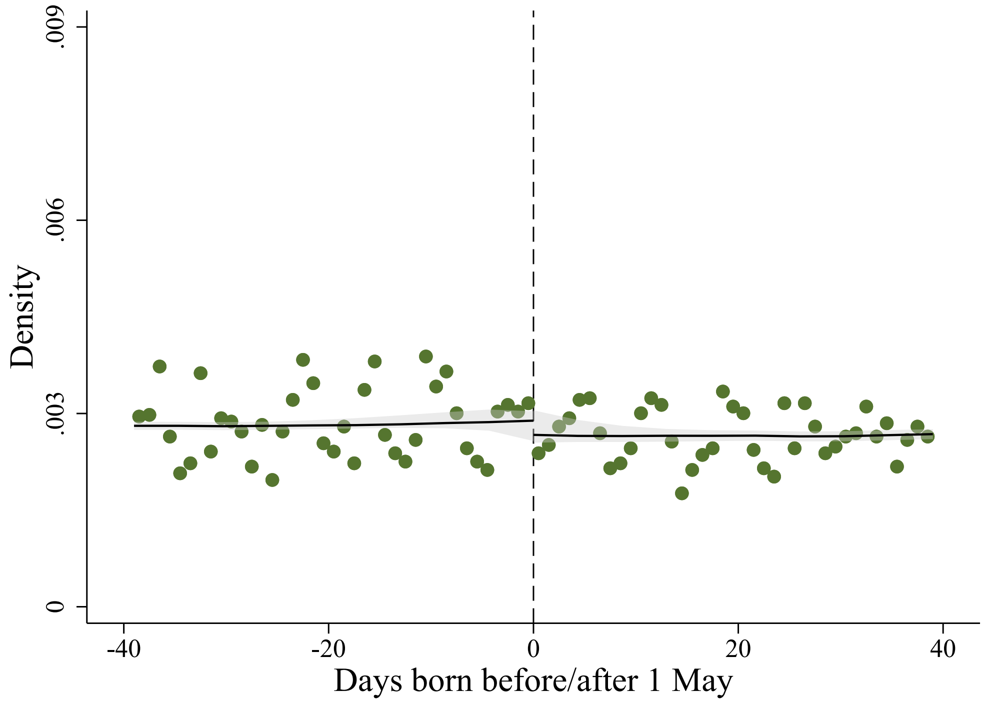
Notes: The above figure plots the proportion of children in each age group who were notified Child Protection Services (CPS) for suspected abuse and neglect in South Australia, for all years between 2004 and 2017.

Figure B.2.
Histogram of Counts of CPS Notifications



Notes: This figure plots the distribution of counts of CPS notifications experienced by children in the calendar year of their 5th birthday – conditional on having at least one notification within this period. The sample includes all children born in South Australia from 2009 to 2011 (N = 38,610) and the corresponding time period for these notifications is 2014 to 2016. The total count of notifications is 8,214.

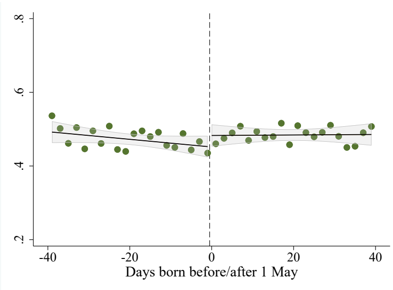
Figure B.3.
Density Test for Manipulation of the Running Variable



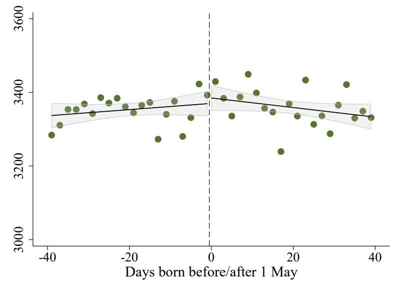
Notes: This figure plots results from the density test of Cattaneo et al. (2020), generated via the `rddensity` command in Stata. The underlying scatter plot represents the distribution of the running variable, which is the day-of-birth from the school entry cut-off. Vertical dashed lines depict the school entry cut-off, which is 1 May. Children born before the cut-off are eligible to start school the year they turn 5 years old, whereas children born on or after the cut-off are only eligible to start school the following year. Fitted lines are generated from local linear regressions using a uniform kernel and the optimal bandwidth selected by the `rddensity` algorithm. Grey shaded areas represent bias-corrected 95% confidence intervals. The density test of Cattaneo et al. (2020) returns a p-value of 0.8579.

FIGURE B.4.
COVARIATE BALANCE

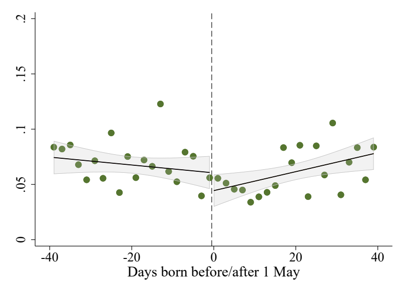
(a) Female



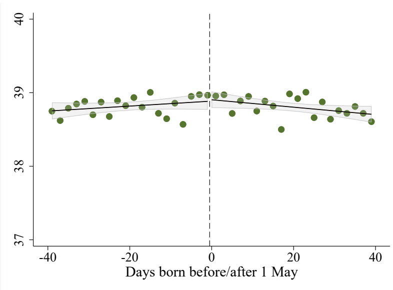
(b) Birthweight (in grams)



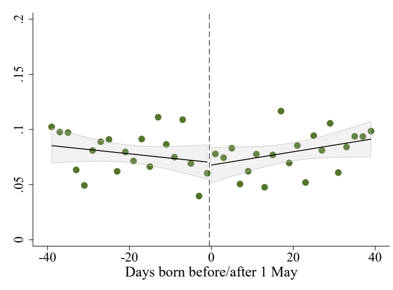
(c) Birthweight $<$ 2500 grams



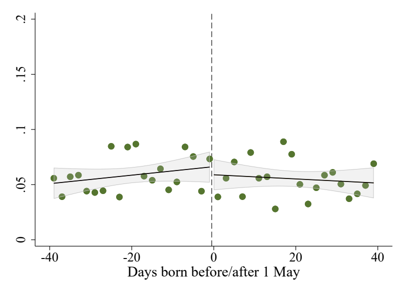
(d) Gestation (in weeks)



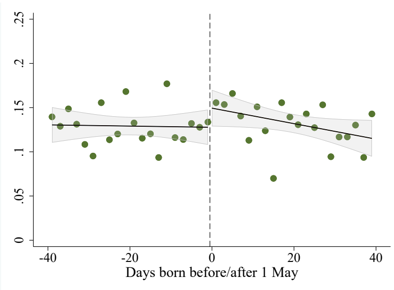
(e) Gestation $<$ 37 weeks



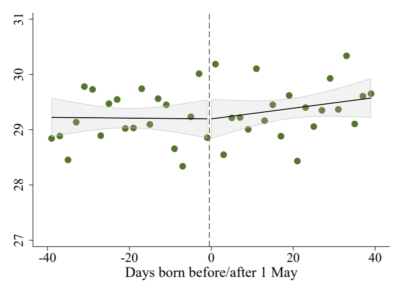
(f) Pediatric Condition



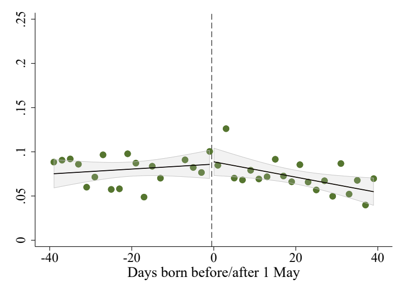
(g) Mother Single at Birth



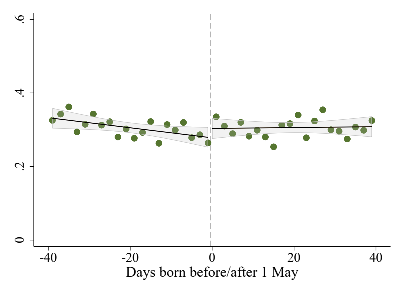
(h) Mother's Age at Birth



(i) Mother's Age $<$ 21 at Birth

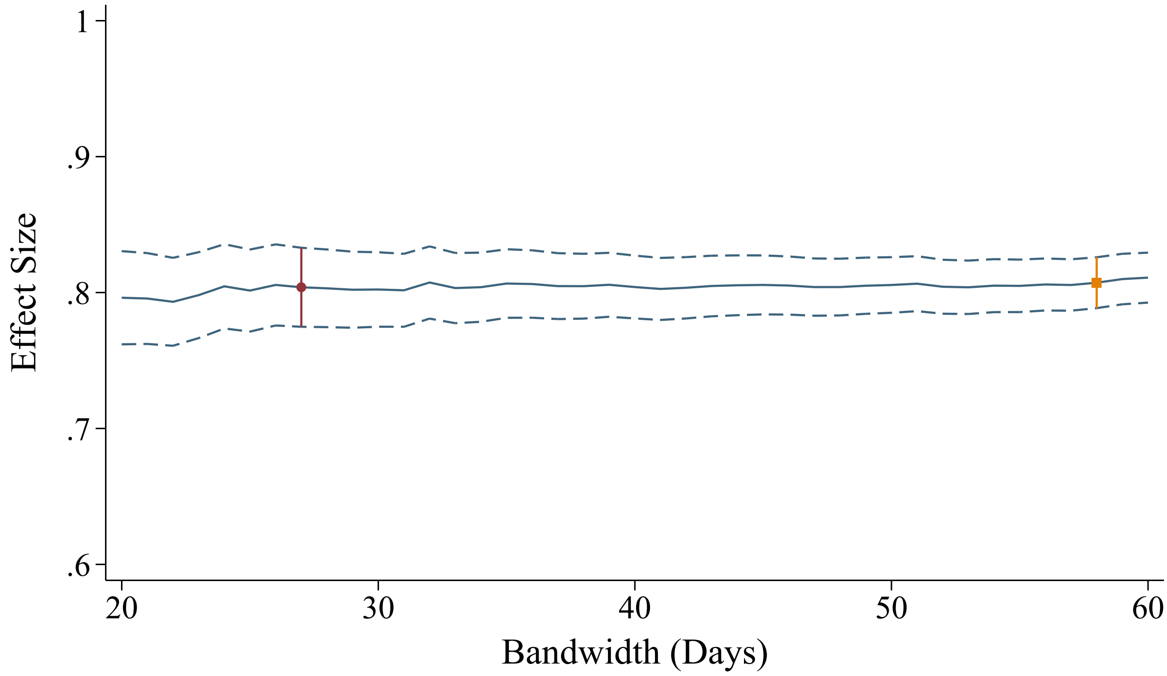


(j) Low SES (bottom quintile)



Notes: The above figures present visual evidence that pre-treatment characteristics are balanced across the school entry cut-off in South Australia. Each figure plots the local averages of a given pre-treatment characteristic by day-of-birth (in 2-day bins) relative to the school entry cut-off. Vertical dashed lines depict the school entry cut-off, which is 1 May. Children born before 1 May are eligible to start school the year they turn 5 years old, whereas children born on or after this cut-off are only eligible to start school the following year. Children are considered to have low socioeconomic status (SES) if their residential suburb at time of birth is in the bottom quintile of the Index of Relative Socioeconomic Disadvantage (IRSD), which is a summary measure of relative disadvantage for an area. Fitted lines are obtained from local linear regressions using a uniform kernel. Grey shaded areas represent 95% confidence intervals.

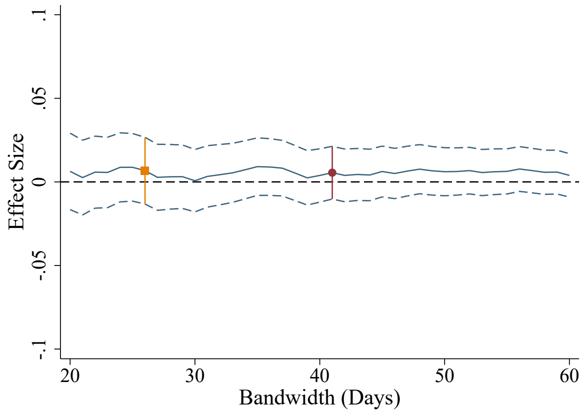
Figure B.5.
RD Estimates for School Enrolment with Varying Bandwidths



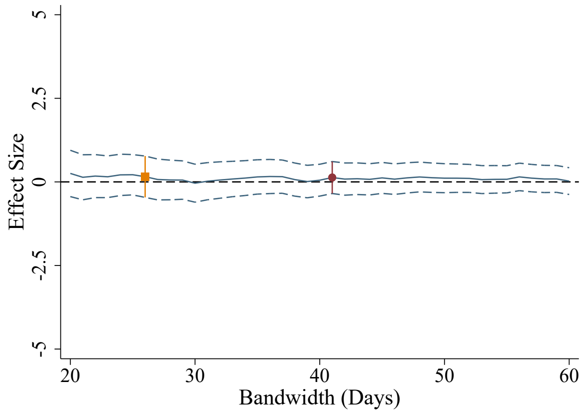
Notes: The above figures plot the point estimates and corresponding 95% confidence intervals of the reduced-form RD treatment effect of school entry eligibility (i.e. being born before 1 May) on school enrolment in the calendar year of the 5th birthday, across a range of bandwidths. The baseline point estimate that is calculated using a bandwidth of 27 days, optimally selected using the procedure developed in Calonico et al. (2014) (CCT), is highlighted in maroon and denoted by a circle. The point estimate calculated using the optimal bandwidth of Kettlewell and Siminski (2026) (KS) (i.e. 58 days) is highlighted in orange and denoted by a square.

Figure B.6.
RD Estimates for CPS Notifications from Education Professionals with Varying Bandwidths

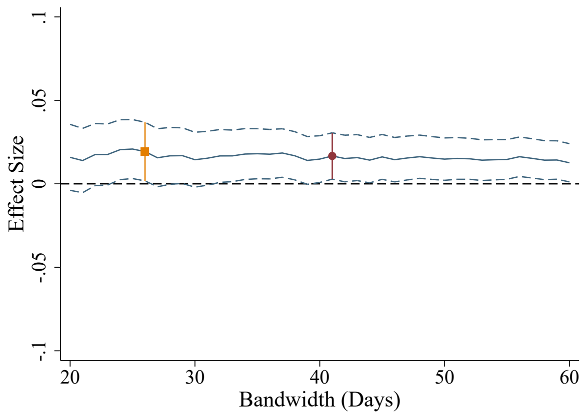
(a) Any notification (all educators)



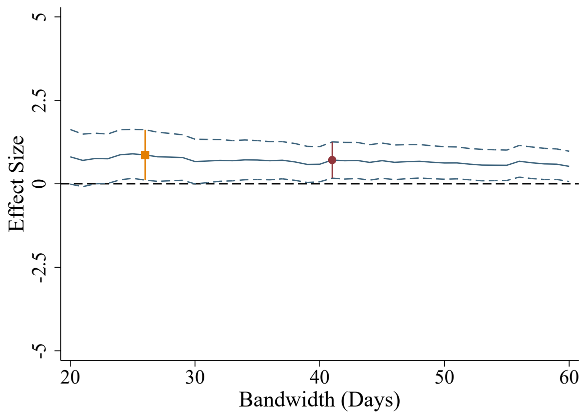
(b) Count of notifications (all educators)



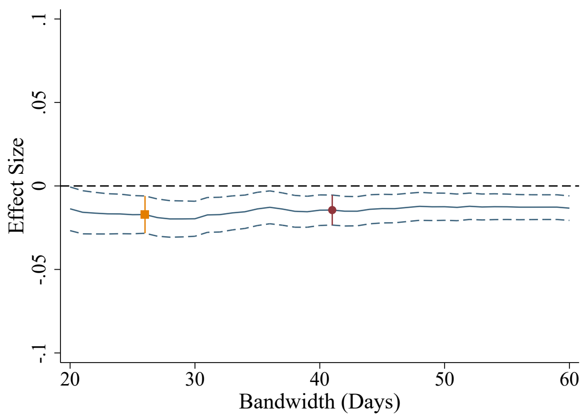
(c) Any notification (school)



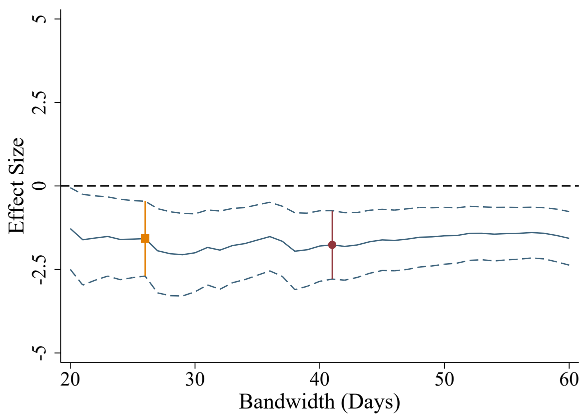
(d) Count of notifications (school)



(e) Any notification (pre-school/childcare)



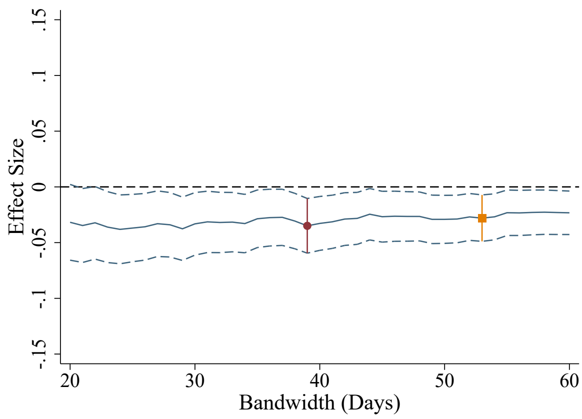
(f) Count of notifications (pre-school/childcare)



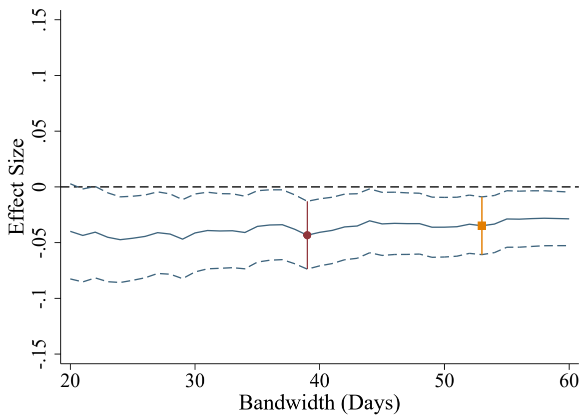
Notes: The above figures plot the point estimates and corresponding 95% confidence intervals of the reduced-form RD treatment effect of school entry eligibility (i.e. being born before 1 May) on notifications to Child Protection Services (CPS) by education professionals across a range of bandwidths. Where the count of CPS notifications is the outcome, the point estimates represent Poisson regression coefficients. The baseline point estimate that is calculated using a bandwidth of 41 days, optimally selected using the procedure developed in Calonico et al. (2014) (CCT), is highlighted in maroon and denoted by a circle. The point estimate calculated using the optimal bandwidth of Kettlewell and Siminski (2026) (KS) (i.e. 26 days) is highlighted in orange and denoted by a square.

Figure B.7.
RD Estimates for CPS Notifications from Education Professionals with Varying Bandwidths

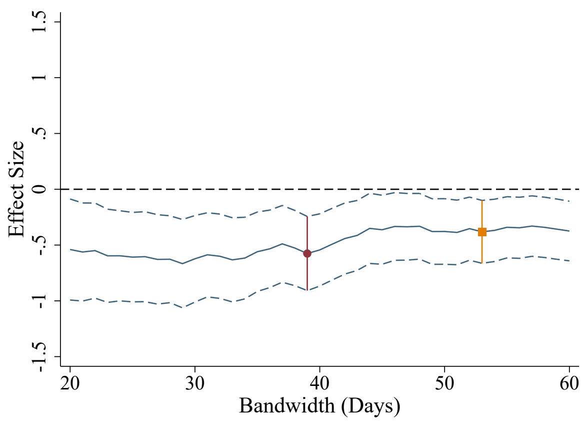
(a) Any CPS notification (reduced-form)



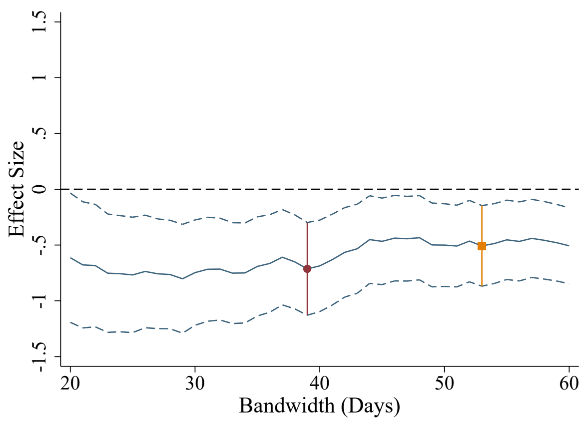
(b) Count of CPS notifications (IV)



(c) Any CPS notification (reduced-form)



(d) Count of CPS notifications (IV)



Notes: The above figures plot the point estimates and corresponding 95% confidence intervals of the reduced-form and fuzzy RD treatment effects of enrolment on notifications to Child Protection Services (CPS) by non-education reporters across a range of bandwidths. Where the count of CPS notifications is the outcome, the point estimates represent Poisson regression coefficients. The baseline point estimate that is calculated using a bandwidth of 39 days, optimally selected using the procedure developed in Calonico et al. (2014) (CCT), is highlighted in maroon and denoted by a circle. The point estimate calculated using the optimal bandwidth of Kettlewell and Siminski (2026) (KS) (i.e. 43 days) is highlighted in orange and denoted by a square.

Table B.1.
Robustness of RD Estimates to using a Quadratic Control Function – CPS Notifications from Education Reporters

	Any notification		Count of notifications	
	RF	IV	RF Poisson	IV Poisson
	(1)	(2)	(3)	(4)
Panel A: All education professionals				
Born before 1 May	0.005		0.126	
	(0.012)		(0.364)	
School enrolment		0.007		0.233
		(0.015)		(0.459)
Effect size (%)	16	20	13	26
Mean (born after 1 May)	0.032	0.032	0.045	0.045
Observations	9022	9022	9022	9022
Panel B: Educators in school				
Born before 1 May	0.018*		0.754*	
	(0.011)		(0.435)	
School enrolment		0.022*		0.971*
		(0.013)		(0.541)
Effect size (%)	85	106	113	164
Mean (born after 1 May)	0.021	0.021	0.027	0.027
Observations	9022	9022	9022	9022
Panel C: Educators in pre-school and childcare				
Born before 1 May	-0.018**		-1.719**	
	(0.007)		(0.701)	
School enrolment		-0.022**		-2.281**
		(0.009)		(1.040)
Effect size (%)	-124	-154	-82	-90
Mean (born after 1 May)	0.014	0.014	0.018	0.018
Observations	9022	9022	9022	9022

Notes: This table presents reduced-form (RF) and fuzzy RD estimates of the impact of school enrolment on notifications to Child Protection Services (CPS) by education professionals, using a quadratic control function of the running variable (i.e. day-of-birth from 1 May). For Columns (1) and (2), the outcome is a binary variable indicating whether the child had any CPS notification in the calendar year of their 5th birthday. For Columns (3) and (4), the outcome is a count variable indicating the number of CPS notifications that children have in the calendar year of their 5th birthday. Columns (2) and (4) present fuzzy RD estimates of the effect of school enrolment on the corresponding outcomes, using school entry eligibility as an instrumental variable (IV). The estimated first-stage discontinuity is 0.801 ($p < 0.01$). Effect sizes for Columns (1) and (2) are calculated as a percent of the control group mean. Effect sizes for Poisson regression coefficients in Columns (3) and (4) are calculated as $[\exp(\hat{\beta}) - 1] \times 100$. All specifications are based on local quadratic regressions with a uniform kernel and a bandwidth of 42 days, optimally selected using the procedure developed in Calonico et al. (2014) (accounting for the local quadratic specification). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2.
Robustness to Alternative Methods of Inference

	Table 3			Table 4	Table 5					Table 6			
	Educ.	School	Preschool	Non-educ.	Police	Health	Family	Govt.	Non-govt	Physical	Sexual	Emotional	Neglect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A: Reduced-form													
Born before 1 May	0.006	0.017	-0.014	-0.035	-0.026	-0.008	-0.006	-0.009	-0.013	-0.002	-0.005	-0.017	-0.012
	(0.009)	(0.008)	(0.005)	(0.015)	(0.012)	(0.007)	(0.005)	(0.008)	(0.006)	(0.005)	(0.003)	(0.008)	(0.006)
	[0.492]	[0.018]	[0.002]	[0.005]	[0.007]	[0.293]	[0.240]	[0.186]	[0.049]	[0.774]	[0.171]	[0.021]	[0.095]
Observations	8832	8832	8832	8421	7369	6718	8832	6931	7369	5911	10125	7987	7533
Panel B: Fuzzy RD													
School enrolment	0.007	0.021	-0.018	-0.043	-0.032	-0.009	-0.007	-0.011	-0.016	-0.002	-0.006	-0.021	-0.014
	(0.011)	(0.010)	(0.006)	(0.019)	(0.014)	(0.008)	(0.007)	(0.009)	(0.008)	(0.007)	(0.004)	(0.010)	(0.008)
	[0.492]	[0.018]	[0.002]	[0.005]	[0.007]	[0.293]	[0.240]	[0.186]	[0.049]	[0.774]	[0.171]	[0.021]	[0.095]
Observations	8832	8832	8832	8421	7369	6718	8832	6931	7369	5911	10125	7987	7533

Notes: This table examines the robustness of our main results to the use of alternative methods of inference. Standard errors that are clustered on the running variable (i.e. day-of-birth from 1 Nat) are presented in parentheses. The *p*-values implied by the 'honest' (i.e. bias-aware) approach of Kolesár and Rothe (2018) for constructing confidence intervals are presented in square brackets. For each column, the outcome is a binary variable indicating whether an individual had any CPS notification in the calendar year of their 5th birthday. Columns (1) to (3) present outcomes analysed in Table 2. Column (4) presents the binary outcome analysed in Table 3. Columns (5) to (9) present outcomes analysed in Table 4. Columns (10) to (13) present outcomes analysed in Table 5. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 January 1993) with a uniform kernel and the optimal bandwidth selected by the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications.

Table B.3.
RD Estimates Across Different Calendar Years (Notifications by Education Professionals)

	Calendar year after year of birth:			
	3rd	4th	5th	6th
	(1)	(2)	(3)	(4)
	I. Educators in school			
Panel I.A: Any CPS notification				
Born before 1 May	0.002	0.003	0.017**	0.000
	(0.004)	(0.006)	(0.007)	(0.009)
Effect size (%)	29	31	80	1
Mean (born after 1 May)	0.008	0.010	0.021	0.032
Bandwidth (days)	32	34	41	34
Observations	6931	7369	8832	7369
Panel I.B: Count of CPS notifications				
Born before 1 May	0.261	0.388	0.712**	0.053
	(0.589)	(0.382)	(0.277)	(0.298)
Effect size (%)	30	47	104	5
Mean (born after 1 May)	0.011	0.012	0.027	0.045
Bandwidth (days)	32	34	41	34
Observations	6931	7369	8832	7369
	II. Educators in pre-school and childcare			
Panel II.A: Any CPS notification				
Born before 1 May	0.000	-0.002	-0.014***	-0.004
	(0.004)	(0.004)	(0.005)	(0.004)
Effect size (%)	8	-27	-100	-79
Mean (born after 1 May)	0.006	0.009	0.014	0.005
Bandwidth (days)	32	34	41	34
Observations	6931	7369	8832	7369
Panel II.B: Count of CPS notifications				
Born before 1 May	0.519	-0.118	-1.763***	-0.848
	(0.745)	(0.428)	(0.523)	(0.664)
Effect size (%)	68	-11	-83	-57
Mean (born after 1 May)	0.007	0.011	0.018	0.006
Bandwidth (days)	32	34	41	34
Observations	6931	7369	8832	7369

Notes: This table presents reduced-form RD estimates of the impact of being born before the school entry cut-off on notifications to Child Protection Services (CPS) by education professionals, across different calendar years after birth. Children born before 1 May are eligible to enrol in school in the calendar year of their 5th birthday. For Columns (1) to (5), the outcome is either a binary variable indicating whether the child had any CPS notification or the count of CPS notifications, in the corresponding calendar year. RD treatment effects are estimated using OLS for binary outcomes and Poisson regressions for count outcomes. Effect sizes for OLS regression coefficients are calculated as a percent of the control group mean. Effect sizes for Poisson regression coefficients are calculated as $[\exp(\hat{\beta}) - 1] \times 100$. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and the optimal bandwidth selected using the procedure developed in Calonico et al. (2014). Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.4.
Composition of Maltreatment Reported by Educators in School vs. Pre-school/Childcare

	Education professionals in:	
	School	Pre-school and childcare
	Percent (%)	Percent (%)
Physical	32.2	34.7
Sexual	15.7	14.8
Emotional	36.2	32.4
Neglect	47.3	46.6

Notes: This table presents the composition of CPS notifications at ages 4 to 5 – by type of maltreatment – reported by education professionals working in schools and early childhood education (pre-school and childcare) respectively. The corresponding time period for these notifications is 2014 to 2016. Maltreatment categories are not mutually exclusive as one notification may involve multiple types of maltreatment. Additionally, information for maltreatment type is only available for notifications that are screened-in by CPS.

Table B.5.
RD Estimates of the Impact of Schooling on CPS Notifications from Educators (by Maltreatment Type)

	Notifications by maltreatment type (binary):			
	Physical	Sexual	Emotional	Neglect
	(1)	(2)	(3)	(4)
Panel A: All education professionals				
School enrolment	0.002	-0.001	0.003	0.006
	(0.004)	(0.002)	(0.004)	(0.005)
Effect size (%)	45	-26	54	97
Mean (born after 1 May)	0.005	0.002	0.005	0.007
Observations	8832	8832	8832	8832
Panel B: Educators in school settings				
School enrolment	0.004	0.001	0.007*	0.008*
	(0.003)	(0.002)	(0.004)	(0.004)
Effect size (%)	120	63	175	181
Mean (born after 1 May)	0.003	0.001	0.004	0.005
Observations	8832	8832	8832	8832
Panel C: Pre-school and childcare professionals				
School enrolment	-0.002	-0.002*	-0.004	-0.001
	(0.002)	(0.001)	(0.002)	(0.002)
Effect size (%)	-80	-229	-192	-39
Mean (born after 1 May)	0.002	0.001	0.002	0.002
Observations	8832	8832	8832	8832

Notes: This table presents fuzzy RD estimates of the impact of school enrolment on notifications to Child Protection Services (CPS) from education professionals, based on the type of maltreatment associated with that notification. The fuzzy RD framework uses school entry eligibility (i.e. being born before 1 May) as an instrumental variable (IV) for school enrolment in the calendar year of children's 5th birthday. The outcome in Columns (1) to (4) is a binary variable indicating whether the child had any CPS notification in the calendar year of their 5th birthday for physical abuse, sexual abuse, emotional abuse, or neglect, respectively. Effect sizes are calculated as a percent of the control group mean. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and a bandwidth of 41 days, optimally selected using the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.6.
Robustness of RD Estimates to using a Quadratic Control Function – CPS Notifications from Non-Education Reporters

	Any notification		Count of notifications	
	RF	IV	RF Poisson	IV Poisson
	(1)	(2)	(3)	(4)
Born before 1 May	-0.035**		-0.617***	
	(0.016)		(0.221)	
School enrolment		-0.044**		-0.747***
		(0.020)		(0.268)
Effect size (%)	-41	-51	-46	-53
Mean (born after 1 May)	0.087	0.087	0.166	0.166
Observations	11618	11618	11618	11618

Notes: This table presents reduced-form (RF) and fuzzy RD estimates of the impact of school enrolment on notifications to Child Protection Services (CPS) from non-education reporters, using a quadratic control function of the running variable (i.e. day-of-birth from 1 May). For Columns (1) and (2), the outcome is a binary variable indicating whether the child had any CPS notification in the calendar year of their 5th birthday. For Columns (3) and (4), the outcome is a count variable indicating the number of CPS notifications that children have in the calendar year of their 5th birthday. Columns (2) and (4) present fuzzy RD estimates of the effect of school enrolment on the corresponding outcomes, using school entry eligibility as an instrumental variable (IV). The estimated first-stage discontinuity is 0.801 ($p < 0.01$). Effect sizes for Columns (1) and (2) are calculated as a percent of the control group mean. Effect sizes for Poisson regression coefficients in Columns (3) and (4) are calculated as $[\exp(\hat{\beta}) - 1] \times 100$. All specifications are based on local quadratic regressions with a uniform kernel and a bandwidth of 54 days, optimally selected using the procedure developed in Calonico et al. (2014) (accounting for the local quadratic specification). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.7.
RD Estimates Across Different Calendar Years (Notifications by Non-Education Reporters)

	Calendar year after year of birth:			
	3rd	4th	5th	6th
	(1)	(2)	(3)	(4)
Panel A: Any CPS notification				
Born before 1 May	-0.006	-0.021	-0.035***	-0.001
	(0.013)	(0.014)	(0.013)	(0.011)
Effect size (%)	-8	-25	-40	-2
Mean (born after 1 May)	0.075	0.083	0.087	0.077
Bandwidth (days)	32	29	39	43
Observations	6931	6288	8421	9195
Panel B: Count of CPS notifications				
Born before 1 May	-0.179	-0.332*	-0.527***	0.054
	(0.214)	(0.199)	(0.172)	(0.181)
Effect size (%)	-16	-28	-41	6
Mean (born after 1 May)	0.132	0.154	0.166	0.137
Bandwidth (days)	32	29	38	43
Observations	6931	6288	8204	9195

Notes: This table presents reduced-form RD estimates of the impact of being born before the school entry cut-off on notifications to Child Protection Services (CPS) by non-education reporters, across different calendar years after birth. Children born before 1 May are eligible to enrol in school in the calendar year of their 5th birthday. For Columns (1) to (5), the outcome is either a binary variable indicating whether the child had any CPS notification or the count of CPS notifications, in the corresponding calendar year. RD treatment effects are estimated using OLS for binary outcomes and Poisson regressions for count outcomes. Effect sizes for OLS regression coefficients are calculated as a percent of the control group mean. Effect sizes for Poisson regression coefficients are calculated as $[\exp(\hat{\beta}) - 1] \times 100$. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and the optimal bandwidth selected using the procedure developed in Calonico et al. (2014). Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.8.
RD Estimates of the Impact of Schooling on CPS Notifications by Response Level

	Response to notification:				
	Screened-out	Screened-in	Tier 1 or 2	Inv.	Sub.
	(1)	(2)	(3)	(4)	(5)
School enrolment	-0.054***	-0.023*	-0.026**	0.001	0.003
	(0.015)	(0.012)	(0.012)	(0.005)	(0.004)
Effect size (%)	-73	-50	-63	12	48
Mean (born after 1 May)	0.073	0.045	0.042	0.012	0.007
Bandwidth (days)	34	32	34	36	31
Observations	7369	6931	7173	7735	6718

Notes: This table presents fuzzy RD estimates of the impact of school enrolment on notifications to Child Protection Services (CPS), based on the level of response provided by CPS to that notification. The fuzzy RD framework uses school entry eligibility (i.e. being born before 1 May) as an instrumental variable (IV) for school enrolment in the calendar year of children's 5th birthday. For Columns (1) to (4), the outcome is a binary variable indicating whether the child had any CPS notification (excluding reports from education professionals) in the calendar year of their 5th birthday that was screened-out, screened-in, assigned a Tier 1 or 2 rating (high-urgency), investigated, or substantiated respectively. Tier ratings are only assigned for screened-in notifications. Effect sizes are calculated as a percent of the control group mean. All specifications are use a linear function of the running variable (i.e. day-of-birth from 1 May) with a uniform kernel and the optimal bandwidth selected using the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.9.
Heterogeneous Effects of the School Entry Cut-Off on School Enrolment

	Mother employed or in education?		Younger sibling?	
	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Born before 1 May	0.828***	0.801***	0.802***	0.816***
	(0.020)	(0.019)	(0.018)	(0.015)
Mean (born after 1 May)	0.005	0.004	0.007	0.004
Bandwidth (days)	46	25	33	52
Observations	2615	3651	3932	5068

Notes: This table presents heterogeneous reduced-form RD estimates of the effect of being born before 1 May (i.e. children who are eligible to start school in the calendar year of their 5th birthday) on school enrolment. For Columns (1) to (4), the outcome is a binary variable indicating whether the child was enrolled in school in the calendar year of their 5th birthday. Columns (1) and (2) present RD estimates separately for children based on whether their mother was in employment or education at the time of childbirth. Columns (3) and (4) present RD estimates separately for children with and without younger siblings at the time of enrolment. All specifications use a linear function of the running variable (i.e. day-of-birth from 1 May) and a uniform kernel for estimation. The optimal bandwidth is calculated separately for each subgroup using the procedure developed in Calonico et al. (2014). Year-of-birth fixed effects are included in all specifications. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$