Small Seeds, Big Returns: Delivering Student Grants Through College Savings Accounts*

William Arbour† Laëtitia Renée‡ Fernando Saltiel§

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Abstract

Student financial aid is typically delivered directly to students or through postsecondary institutions. We study an alternative: the Canada Learning Bond, a grant deposited into college savings accounts during childhood and accessible upon college enrollment. Exploiting a date-of-birth eligibility cutoff and administrative data, we find that eligibility encourages parents to open accounts and contribute their own money, despite no saving requirement. Recipients accumulate $1,470 in additional parental savings by age 17, exceeding the average $1,300 grant received, without crowding out other savings. Effects also spill over to older, ineligible siblings, and we find evidence of increased college enrollment.

Keywords: College savings plans, student financial aid, Canada Learning Bond
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†Department of Economics, University of Montreal (william.arbour@umontreal.ca)
‡Department of Economics, University of Montreal (laetitia.renee@umontreal.ca), corresponding author.
§Department of Economics, McGill University (fernando.saltiel@mcgill.ca)

1 Introduction

Many countries spend heavily on student financial aid to widen access to postsecondary education (OECD, 2020). Traditionally, this aid takes the form of grants and loans, with eligibility and amounts determined at enrollment and payments made directly to the student or the institution. More recently, some governments have adopted a different approach: delivering student grants through children's college savings accounts. Under this model, the government contributes to children's savings accounts during childhood, but the funds can only be withdrawn if the child later enrolls in college. This raises a question: does it matter how financial aid is delivered, or is a dollar of aid equally effective regardless of how it reaches families?

In this paper, we study the Canada Learning Bond (CLB), a Canadian federal program that deposits up to CAD $2,000 into the college savings accounts of children from low-income families. College savings accounts, known as Registered Education Savings Plans (RESPs) in Canada, are tax-advantaged saving vehicles whose withdrawals are restricted to postsecondary expenses.[1] RESPs are opened and managed privately by parents or other relatives rather than by the government, and the CLB was layered onto this existing infrastructure: when an eligible child has an RESP, the government automatically deposits the CLB into the account. These CLB deposits accrue gradually over childhood until they reach the $2,000 maximum. Although the program targets low-income families, approximately 60% of families in our sample meet the income-eligibility criteria at least once during their children's eligibility years, indicating that eligibility extends to a broad segment of families. CLB funds can be withdrawn only upon proof of college enrollment. Thus, as with traditional student aid, students receive the money at college entry. If the child never enrolls, the grant is returned to the government.

Our main finding is that CLB eligibility induces parents to open college savings accounts

[1] RESPs are comparable to 529 plans in the United States, Junior ISAs in the United Kingdom, and Education Bonds in Australia. Appendix Table A2 compares RESPs and 529 plans.

so that their children can claim the grant, and that, in the vast majority of cases, these parents also contribute their own money. This is not mechanical. Parents could have opened an account, left it empty, and still received the full CLB. Their decision to contribute reveals a behavioral response, which we discuss in greater detail below. We also find evidence that CLB eligibility increases college enrollment, by a magnitude consistent with the amplification effect generated by increased parental saving. Together, these findings show that delivering aid through savings accounts can crowd in private savings that amplify the impact of the public transfer. By encouraging parents to save more for their children's education, this policy design can help finance college enrollment at a lower public cost.

For identification, we exploit the fact that only children born on or after January 1, 2004 are eligible for the CLB. Children born only a few weeks earlier are not eligible: we thus compare low-income children born just before and just after the threshold. We remove systematic differences across months of birth using a difference-in-discontinuities design. Our analysis draws on comprehensive administrative data from British Columbia that link the universe of children's school records with parental tax returns, RESP account records, and postsecondary enrollment records.[2] Our estimates identify the incremental effect of CLB eligibility in a setting with existing student aid and RESP saving incentives.

In the first part of our results, we show that CLB eligibility increases the probability that parents open a college savings account by 7.3 percentage points and that, in 72% of these newly opened accounts, parents go on to contribute their own funds. Combining this extensive-margin response with a possible intensive-margin response, parents of eligible children save, on average, $720 more by the time their child reaches age 17. This response exceeds the average CLB transfer of $642, implying a sizable multiplier effect.[3] Moreover, because of this additional parental saving, eligible children also accumulate an average of $170 in additional matching grants from the government, and, assuming annual returns of 3 to 7 percent, additional investment returns, ranging from $400 to $1,200. In total, we

[2]We focus on British Columbia because these linked administrative data are uniquely available there.
[3]All dollar values are expressed in 2024 Canadian dollars unless noted otherwise.

estimate that CLB-eligible children have $1,900 to $2,700 more in RESP assets by age 17. Since only 49% of eligible children actually receive the CLB, this corresponds to an effect of roughly $3,900 to $5,500 for treated children.

The increase in parental saving is notable because the transfer requires no parental contribution and is paid in full regardless of whether, or how much, parents save. As such, standard price incentives cannot explain the increase in savings. We also examine whether reallocation could explain this effect, but find little evidence of reduced savings in other accounts.[4] We thus consider *activation* as a possible mechanism in our setting, as the CLB provides a salient reward for opening an account, and having an account lowers future participation costs while building financial familiarity. If this mechanism were at work, we would expect the effects to extend beyond the eligible child's own account to household saving more broadly. We test for this possibility by using older siblings of children born just before or after the CLB eligibility cutoff. Because these older siblings are never themselves CLB-eligible by birth date, any differences in their outcomes must operate through changes in parental behavior. We find that when the younger sibling is CLB-eligible, older siblings are 3.8 percentage points more likely to have an RESP and to receive parental contributions, and their parents contribute $1,200 more.

In the second part of the paper, we examine the effects on college enrollment. First, CLB eligibility raises enrollment in a bachelor's degree program at age 18 by 4.6 percentage points, which represents a 19 percent increase relative to baseline participation rates. Compared to alternative government programs, this effect is large: it corresponds to 5.7 percentage points per $1,000 of government aid, three to six times the effect of traditional aid, which the literature puts at 1 to 2 percentage points per $1,000 of total aid eligibility (Seftor and Turner, 2002; Stanley, 2003; Dynarski, 2003; Kane, 2003; Abraham and Clark, 2006; Cornwell et al., 2006; Kane, 2007; Dynarski and Scott-Clayton, 2013). We also find that older siblings, who do not directly receive the CLB grant, are 2.4 percentage points more likely to enroll in

[4]All children in our sample, whether or not they are CLB-eligible, qualify for generous federal matching grants on parental RESP contributions, making RESPs widely used even among the control group.

a bachelor's degree. This suggests that increased parental saving may itself raise enrollment and hence account for part of why the effect is so large.

Our findings have implications for the design of financial aid and policies aimed at encouraging household saving. First, they highlight the importance of barriers to participation. Even when generous matching incentives are available, encouraging families to open a savings account can lead them to start saving their own money. This finding is relevant beyond education, particularly for retirement savings programs, where many households do not take advantage of existing incentives. Second, our results speak to ongoing debates about providing children with financial resources early in life, as in children's savings accounts and baby bond initiatives in the United States. These policies aim not only to provide financial support but also to shape families' expectations and investments in their children's futures. Our findings suggest that delivering grants through savings accounts can encourage additional parental investment and increase college enrollment, with benefits that extend beyond the initial government transfer.

This paper contributes to various strands of the literature, standing at the intersection of prior work on college financial aid, household saving behavior, and college savings accounts. First, we add to a broad literature studying the effects of student financial aid on college enrollment (see Herbaut and Geven (2020) and Dynarski et al. (2023) for reviews). Most research in this literature examines grants delivered at the time of enrollment on postsecondary decisions, particularly for students from lower-income backgrounds (Seftor and Turner, 2002; Stanley, 2003; Dynarski, 2003; Kane, 2003; Avery and Hoxby, 2004; Abraham and Clark, 2006; Cornwell et al., 2006; Kane, 2007; Dynarski and Scott-Clayton, 2013; Angrist et al., 2022; Renée, 2025). Recent work further shows that the delivery mechanism of financial aid matters independently of the amount, showing that simplifying the application process (Bettinger et al., 2012) and providing early guarantees of aid (Dynarski et al., 2021; Burland et al., 2023) can generate larger enrollment effects than equivalent aid delivered through standard processes. We study a related but distinct delivery mechanism: depositing grants

into college savings accounts years before enrollment.

Prior work on grants delivered through college savings accounts is rather limited. It includes small-scale randomized experiments conducted on specific populations in the United States and Italy and mostly focuses on intermediary outcomes such as take-up and savings (Kim et al., 2015; Clancy et al., 2016; Long and Bettinger, 2017; Martini et al., 2021).[5] Closer to our context, Messacar and Frenette (2019) study the Canada Education Savings Grant, a federal matching grant available since 1998, and find that higher match rates increase contributions among existing savers but do not induce new families to open accounts or begin saving. They do not, however, investigate the effects of grant eligibility itself, nor do they analyze subsequent impacts on college enrollment. Lastly, previous work on the CLB uses the same eligibility cutoff and finds positive effects on some RESP outcomes in early childhood (Duhaime-Ross, 2015; Imbeau, 2017).

We also contribute to a large literature on policies promoting household saving. Traditional interventions in this literature rely on matching contributions to encourage participation, primarily in the context of retirement savings, and generally find significant but moderate effects on saving behavior (Duflo et al., 2006; Engelhardt and Kumar, 2007; Mitchell et al., 2007; Madrian, 2012; Hinz et al., 2013). In contrast, we study a non-matching financial incentive tied only to account opening and show that it substantially increases savings among low-income families who are unresponsive to generous matching grants. This result adds to a growing body of work, often rooted in behavioral economics, that explores alternative policy designs to encourage saving, including automatic enrollment (Madrian and Shea, 2001; Beshears et al., 2022; Choukhmane, 2025; Chetty et al., 2014), compulsory active choice (Carroll et al., 2009), and information-based interventions (Duflo and Saez, 2003; Saez, 2009).

Lastly, we contribute to the literature studying the role of college savings accounts in shaping inequalities. Prior work in both Canada and the U.S. shows that participation in tax-advantaged college savings accounts is concentrated among higher-income families,

[5]Table A1 provides a detailed comparison of these studies. For each paper, we summarize the intervention design, identification strategy, and outcomes.

raising concerns that these vehicles may exacerbate educational inequalities (Dynarski, 2004; Milligan, 2005; Dynarski and Scott-Clayton, 2016; Frenette, 2017; Bonikowska and Frenette, 2020; Briscese et al., 2025). In addition, recent causal evidence from 529 plans in the U.S. finds that wealth in these accounts increases college enrollment, but that because participation is concentrated among higher-income families, the resulting gains may widen rather than narrow educational inequality (Vasilenko, 2026). In contrast, we show that targeted government grants added to these accounts can address the barriers that have historically limited participation among lower-income households, reducing gaps relative to higher-income households.

The remainder of this paper proceeds as follows. Section 2 provides details about college savings accounts in Canada and the program we study. Section 3 describes the data sources, sample, and measurement of outcomes. Section 4 presents our empirical strategy. Section 5 estimates the effects on RESP savings, while Section 6 examines the mechanisms behind the saving response. Section 7 presents the effects on college enrollment for eligible children and their older siblings and assesses their magnitude. Section 8 concludes.

2 Institutional Background

The program we study in this paper, the Canada Learning Bond (CLB), operates through Registered Education Savings Plans (RESPs). Before describing the CLB program, we first provide background on RESP accounts.

2.1 Registered Education Savings Plans

In Canada, college savings accounts are known as Registered Education Savings Plans (RESPs), which are savings vehicles introduced in 1972 to encourage families to save for postsecondary education. These accounts are tax-advantaged, as investment income accumulates tax-free

while held in the account.[6] RESPs are opened by parents or other individuals with a designated child beneficiary who may later access the accumulated funds upon enrollment in an approved postsecondary institution.[7] RESPs are held through private financial institutions and other authorized promoters, which administer accounts and facilitate access to government grants.

Since 1998, the Canadian federal government has supported participation in college savings accounts through matching grants. The Canada Education Savings Grant (CESG), introduced in 1998, provides a 20% match on the first $2,500 of annual contributions, up to a lifetime maximum of $7,200 per beneficiary. In 2005, the government introduced the Additional Canada Education Savings Grant (A-CESG), which offers an additional 10% or 20% match on the first $500 contributed annually for families with incomes below specified thresholds.[8]

RESP withdrawals are subject to specific rules. In particular, once a child enrolls in postsecondary education, the subscriber can withdraw the original contributions tax-free.[9] By contrast, investment earnings and government grants (CESG and CLB) are paid to the student as Educational Assistance Payments (EAPs), which count as taxable income for the student. EAP withdrawals are initially capped at $8,000 for full-time students and $4,000 for part-time students during the first 13 weeks of enrollment in a qualifying program. After this period, these limits no longer apply.

On the other hand, if the beneficiary does not pursue postsecondary education, the subscriber can still withdraw the original contributions tax-free. However, investment earnings are subject to the subscriber's marginal income tax rate plus an additional 20% tax, and government grants must be repaid.

[6] Contributions to RESPs are not tax-deductible.

[7] Contributors to these accounts, formally termed subscribers, are not required to be related to the beneficiary. In practice, however, parents constitute the vast majority (80%) of subscribers (Employment and Social Development Canada, 2015).

[8] In 2024, families with adjusted incomes up to $55,867 are eligible for the 20% additional match, while those with incomes between $55,867 and $111,733 are eligible for the 10% additional match.

[9] Qualifying programs include full-time and part-time programs at Canadian universities, colleges, and trade schools, as well as at some institutions abroad, as long as they meet minimum requirements for program duration and instructional hours.

RESPs share their tax design with 529 plans in the United States (Table A2). Unlike 529 plans, RESPs come with a federal match, the CESG, for families at all income levels. In 2019, 53 percent of Canadian families with a child under 18 held an RESP, compared with 14 percent of U.S. households with a child under 18 in 2024 (Statistics Canada, 2022b; U.S. Census Bureau, 2026).

2.2 Canada Learning Bond

The Canada Learning Bond (CLB) is an additional grant created by the Government of Canada to help low-income families cover the cost of postsecondary education. The program was announced in the March 2004 federal budget and enacted in 2005. Eligibility applies retroactively to all children born on or after January 1, 2004, which motivates our empirical analysis below. Under the program, the federal government provides direct transfers to the RESPs of children from low-income families without requiring any parental contributions. As with other RESP grants, CLB funds can be withdrawn only if and when the beneficiary enrolls in a qualifying postsecondary education program.

Beyond the date-of-birth restriction, eligibility for the CLB is determined by family income and is assessed for each benefit year from birth through the benefit year in which the child turns 15. For a given benefit year, a child qualifies for a CLB deposit if adjusted family income falls below thresholds set by the Canada Revenue Agency. These thresholds are indexed for inflation and vary with family size. For example, for the 2025–2026 benefit year, families with one to three children must have an adjusted income at or below approximately $57,375 to qualify. Eligibility for a given benefit year also requires that the primary caregiver files a tax return, as income eligibility cannot otherwise be assessed.

Eligible children qualify for an initial $500 CLB deposit in the first year of eligibility, followed by $100 for each subsequent eligible year up to age 15, for a lifetime maximum of $2,000 per child. Thus, children who are eligible for the CLB in at least one year may qualify for total CLB amounts ranging from $500 to $2,000, depending on the number of years in

which their family income falls below the eligibility threshold.

To receive a CLB deposit, a child must be named as a beneficiary on an RESP. Caregivers must also fill out any required CLB consent forms, which need only be completed once. Each year, eligibility is assessed based on date of birth and family income, and if the criteria are met, the CLB payment for that benefit year is deposited into the child's account. If a child does not have an account open during an eligible year, missed deposits can be received retroactively once an account is opened, up until the day before the child's 21st birthday. Children who never have an account opened in their name cannot receive CLB deposits.[10]

3 Data and Sample

3.1 Data Sources and Outcomes

Our analysis uses four large administrative datasets provided by Statistics Canada to determine CLB eligibility and track parental savings and children's outcomes. The core dataset is the British Columbia Kindergarten to Grade 12 (BC K–12) dataset (Statistics Canada, 2025a), which we link to (i) parental tax records from the T1 Family File (T1FF) (Statistics Canada, 2025e), (ii) administrative records from the Canada Education Savings Program (CESP) (Statistics Canada, 2025b), and (iii) postsecondary enrollment records from the Postsecondary Student Information System (PSIS) (Statistics Canada, 2025d). The datasets are linked at the child level using anonymized individual identifiers provided by Statistics Canada through its Education and Labour Market Longitudinal Platform (ELMLP; Statistics Canada, 2024).

Our primary dataset, the BC K–12 dataset, covers the universe of students who attended elementary and secondary schools in British Columbia between 1991 and 2022, excluding federally funded First Nations band schools. These records include each child's month and year of birth, which we use to determine age-based eligibility for the CLB. We link the BC

[10]Between ages 18 and 21, eligible young adults may open an account in their own name and apply for the CLB, receiving the full amount corresponding to all years of eligibility, up to the $2,000 lifetime maximum.

K–12 data to parental tax records from the T1FF, which provide detailed annual information on parental income and allow us to determine income-based CLB eligibility for each child, as described below. Using linkage keys provided by Statistics Canada, we match approximately 80% of children born around the January 1, 2004, cutoff in the BC K–12 data to parental T1FF records. Children who cannot be matched are excluded from our analysis.[11] We further exclude 3% of children who are matched to more than two parents.

We link these data to administrative records from the Canada Education Savings Program (CESP) to study RESP savings outcomes and to the Postsecondary Student Information System (PSIS) to analyze postsecondary enrollment outcomes. Table 1 summarizes the construction of all outcome variables.

The CESP data cover the universe of RESP accounts in Canada between 1998 and 2021 and provide detailed individual-level information on annual contributions and government grants received. From this dataset, we construct several RESP savings outcomes. These outcomes are measured from the year of birth through the end of the calendar year in which the child turns 17, which corresponds to December of the year preceding potential college enrollment. This timing allows us to observe all CLB deposits as well as all parental contributions and matching grants received before college entry.

The PSIS contains annual individual-level records, including program type and institution attended, from the universe of public postsecondary institutions in Canada from 2009 onward. From this dataset, we measure enrollment in bachelor's degree programs in the calendar year in which the child turns 18, corresponding to the typical age of entry into postsecondary education. PSIS records cover public postsecondary institutions only. However, in Canada, nearly all universities (bachelor's degree–granting institutions) are public, so the PSIS provides near-complete coverage of bachelor's degree enrollment.[12] Although the CLB can also be used

[11] Statistics Canada uses information from Canada Child Benefit claims to match children to their parents; see Jones et al. (2024) for details. 76% of unmatched cases correspond to children who entered the British Columbia school system after age 14, in which case Statistics Canada cannot identify parents.

[12] The few exceptions that exist are non-standard institutions, typically religious, specialized, or online universities enrolling small numbers of students (Usher et al., 2025).

Table 1: Outcome Definitions and Measurement

Outcome	Definition and measurement
Panel A: CLB and RESP outcomes (birth to age 17)	
Ever received CLB	Indicator equal to one if any CLB grant was ever deposited into the child's RESP accounts.
Total CLB grants	Cumulative amount of CLB grants deposited into the child's RESP accounts, expressed in 2024 Canadian dollars.
Ever had an RESP account	Indicator equal to one if an RESP account was ever opened for the child (as beneficiary). Accounts that were opened but never received a deposit are not observed and are coded as zero.
Any parental contributions	Indicator equal to one if parents or other relatives ever contributed to an RESP account of the child.
Total parental contributions	Cumulative contributions by parents or other relatives to the child's RESP accounts, in 2024 Canadian dollars.
Total matching (CESG) grants	Cumulative amount of Canada Education Savings Grant (CESG) payments deposited into the child's RESP accounts, in 2024 Canadian dollars. Includes the additional CESG (A-CESG).
Total deposits (contr.+grants)	Sum of parental contributions, CLB grants, and CESG grants deposited into the child's RESP accounts, in 2024 Canadian dollars.
Balance ($), return = 3, 5, 7%	Simulated balance of the child's RESP accounts at age 17, in 2024 Canadian dollars. Because investment income is not observed, we compute the balance by assuming that each deposit earns a constant annual return of 3, 5, or 7 percent from the date it is made until age 17. The simulation ignores withdrawals and fees.
Age at first RESP deposit	Age of the child, in years, at the first observed deposit into an RESP account. Defined only for children with an RESP account.
Panel B: Bachelor's degree enrollment at age 18	
Any enrollment	Indicator equal to one if the individual is enrolled in a bachelor's degree program in the calendar year in which they turn 18.
Full-time enrollment	Indicator equal to one if the individual is enrolled full time in a bachelor's degree program at age 18.
STEM field	Indicator equal to one if the individual is enrolled in a STEM field within a bachelor's degree program at age 18.
Flagship university (U15)	Indicator equal to one if the individual is enrolled in a bachelor's degree program at a flagship university at age 18. Flagship universities are the members of the U15 Group of Canadian Research Universities, which includes the University of British Columbia.

Notes: All Panel A outcomes are cumulative, measured from birth through the end of the calendar year in which the child turns 17, corresponding to December of the year preceding potential college enrollment. Amounts are deposits made into the child's RESP accounts in 2024 Canadian dollars. Enrollment outcomes in Panel B are measured in the calendar year in which the child turns 18, the typical age of entry into postsecondary education.

to finance enrollment in short-cycle or vocational programs, we focus on bachelor's degree enrollment as our primary postsecondary outcome for three reasons. First, bachelor's degree completion yields substantial returns in Canada compared to shorter programs (Boothby and Drewes, 2006; Frenette, 2014). Second, this focus follows a large literature on student financial aid that examines impacts on bachelor's degree enrollment (Dynarski et al., 2023). Third, short-cycle postsecondary institutions can be either public or private and are therefore not fully covered by the PSIS.

3.2 Sample Selection

We define our sample by children's dates of birth, in line with the empirical design described below. The birth window varies across specifications; our baseline sample includes 94,470 children born between July 2002 and June 2004.

Within each birth window, we sort children by whether their parents' income met the CLB eligibility criteria at least once during childhood. For children born in 2004 or later, we apply the actual criteria. For children born before 2004, we check whether they would have been income-eligible had the program applied to their cohort. In all cases, we proceed in two steps. First, we check income eligibility in each year from ages 0 to 15, the ages at which CLB entitlements can accrue, using parental tax records and a data-driven procedure to identify the relevant income cutoffs (see Appendix A.3). Second, we combine these yearly results to place each child in one of three mutually exclusive groups: eligible at least once, never eligible, or ambiguous.

Children who were eligible at least once make up our low-income sample, which includes 56,240 children in the baseline window. Children who were never eligible make up our high-income sample, which includes 24,200 children and serves for placebo tests and a difference-in-difference-in-discontinuities design. We cannot classify the remaining 15% of children with certainty, so we exclude them from the analysis.

Apart from the restrictions that come from data linkages, date of birth, and parental

income, we impose no other restrictions on the main sample.

3.3 Descriptive Statistics

Table 2 presents summary statistics for our main sample of BC K–12 children from low-income families (i.e., at least once eligible for the CLB) born between July 2002 and June 2004. We present these characteristics separately for the four birth-date cells corresponding to the four dimensions of our main empirical design.

The first part of Table 2 summarizes children's socio-demographic characteristics. In our sample, 36% of children speak a language other than English at home, around 14% of them self-declare as Indigenous, and 21% are observed with only one parent. All three shares are higher than in the full population of British Columbia children, reflecting the socio-economic vulnerability of the sample. Average parental income in 2014 is approximately $57,000, consistent with the program's targeting of low-income families.

The table also presents descriptive statistics for savings in RESPs. Focusing on the first three columns, which correspond to birth cohorts unaffected by the introduction of the CLB, approximately 54% of children ever had an RESP account opened in their name. Among children with an RESP, the average age at first account deposit is about 5 years. Conditional on having an RESP, average parental contributions are approximately $18,500, and children receive on average about $3,800 through the Canada Education Savings Grant (CESG) program, for which all children in the sample are eligible. These patterns indicate some engagement with RESPs among low-income families even prior to the policy change, consistent with prior work documenting RESP participation patterns across income groups (Milligan, 2005; Frenette, 2017; Bonikowska and Frenette, 2020). Lastly, 26 percent of students in our sample enroll in a bachelor's program by age 18, with the majority of them enrolling full-time.

The last two columns of Table 2 compare children born in 2003 and 2004 within the main group used in the empirical analysis. Across all socio-demographic characteristics,

Table 2: Descriptive Statistics for the Low-Income Sample

	Child birth date			
	Placebo group		Main group	
Variables	July-Dec 2002	Jan-June 2003	July-Dec 2003	Jan-June 2004
Male child	0.516	0.517	0.514	0.516
Speaks English at home	0.618	0.656	0.637	0.637
Self-declared as Indigenous	0.138	0.149	0.145	0.143
Parental age at child's birth	30.6	31.3	30.8	31.3
Observed with only one parent	0.212	0.218	0.207	0.202
Parental income in 2014 (2014 CA$)	56,700	56,400	56,900	56,900
Ever had an RESP account	0.545	0.546	0.543	0.617
Conditional on having an RESP account:				
Age of child at first RESP deposit	5.1	4.7	5.1	4.8
Total parental contributions ($)	18,500	18,600	18,500	17,600
Total CLB grants ($)	0	0	0	1,000
Total matching (CESG) grants ($)	3,700	3,800	3,800	3,600
Total deposits (contr.+grants) ($)	22,200	22,400	22,200	22,300
Enrolled in a bachelor's degree program	0.258	0.259	0.244	0.259
Conditional on bachelor's degree enrollment:				
Full-time enrollment	0.810	0.814	0.798	0.814
STEM field	0.426	0.426	0.424	0.407
Flagship university (U15)	0.473	0.438	0.412	0.419
Sample size	14,470	13,930	14,040	13,790

Notes: This table reports descriptive statistics for children in the low-income sample defined in Section 3, including socio-demographic characteristics, RESP savings, and bachelor's degree enrollment. Statistics are reported separately for four groups defined by the child's date of birth. The two columns on the left form the placebo group, and the two on the right form the main group, which includes the children born just before and just after the January 1, 2004, CLB cutoff. RESP outcomes are cumulative from birth through age 17, and enrollment outcomes are measured at age 18. Dollar amounts are in 2024 Canadian dollars, except parental income, which is in 2014 Canadian dollars. Sample sizes are reported in the last row and apply to all outcomes except those listed under the two conditional headings. The outcomes below "Conditional on having an RESP account" are computed among children with an RESP account. The outcomes below "Conditional on bachelor's degree enrollment" are computed among children enrolled in a bachelor's degree program.

children born just before and just after the cutoff are highly comparable, with no significant differences in gender, language spoken at home, Indigenous status, parental age at birth, family structure, or parental income. This balance in observable characteristics supports the comparability of children born just before and just after the cutoff.

4 Empirical Strategy

Our empirical strategy exploits the fact that only children born on or after January 1st, 2004 are eligible for the CLB. A simple comparison of outcomes for children born just before versus just after the cutoff would provide a natural regression discontinuity design. However, some features of our setting complicate this approach. First, saving outcomes are measured on a calendar-year basis, creating mechanical differences in outcomes between January and December births. Second, the school entry cutoff in British Columbia falls on the same date, placing children on either side of the threshold in different school cohorts. Third, relative age within cohort may independently affect parental behavior and child outcomes (Bedard and Dhuey, 2006; Black et al., 2011). To address these concerns, we implement a difference-in-discontinuities design (RD-DD) that uses earlier, untreated cohorts to difference out the expected January 1st discontinuity that would arise absent the CLB.

Specifically, we first estimate the discontinuity in outcomes at the January 1, 2004 eligibility cutoff, then subtract the corresponding January 1 discontinuity for an earlier cohort unaffected by the CLB. This second difference accounts for systematic differences between children born in December and January that are unrelated to CLB eligibility, including those arising from calendar-year measurement, relative age effects, or other patterns in parental behavior.

Our main specification compares children born within a six-month window on each side of January 1st, 2004 (the *main group*) to children born within the same window around January 1st, 2003 (the *placebo group*). We restrict our analysis to low-income children who were at least once eligible for the CLB, and include children born before 2004 who would have been eligible had the program existed for their cohort. We estimate the following equation:

$$Y_i = \beta_0 + \beta_1 \,\text{MainGroup}_i \times \text{PostJan1}_i + \beta_2 \,\text{MainGroup}_i + \beta_3 \,\text{PostJan1}_i + \epsilon_i \tag{1}$$

where Y_i denotes the outcome for child i. MainGroup_i is an indicator equal to one for

children in the group centered around January 1st, 2004 (born between July 2003 and June 2004) and zero for children in the 2003 window (born between July 2002 and June 2003). PostJan1_i is an indicator equal to one for children born after January 1st within each group and zero otherwise. The coefficient β_1 is our parameter of interest: it captures the change in the January 1st discontinuity at the 2004 eligibility cutoff relative to the corresponding placebo discontinuity in 2003, which we interpret as the causal effect of CLB eligibility.

The coefficient β_1 can be interpreted as an intent-to-treat (ITT) effect of the policy on eligible children. We report these ITT estimates throughout the paper. When relevant, we also discuss the implied treatment-on-the-treated (TOT) effects, that is, the effect of CLB receipt. These effects can be recovered by scaling the ITT estimates by the inverse of the take-up rate: given that 49% of eligible children in the treated group receive the CLB, the corresponding TOT effects are obtained by multiplying β_1 by approximately two.

Because our design combines RD and DiD elements, standard bandwidth-selection procedures used in single-cutoff RD designs (e.g., Imbens and Kalyanaraman, 2012; Calonico et al., 2014) are not directly applicable. We select a symmetric six-month bandwidth around the January 1st discontinuity as our baseline specification, as it includes enough observations for precise estimation while staying close enough to the cutoff that children on either side are comparable.[13] As a robustness check, we also estimate the effects using a narrower three-month bandwidth. The resulting estimates are qualitatively similar, providing reassurance that our findings are not driven by the specific bandwidth choice. We present heteroskedasticity-robust standard errors throughout.[14]

Finally, our baseline specification deliberately does not include any controls for the running variable (date of birth).[15] This approach does not assume that outcomes are unrelated to

[13]Note that six months is also the maximum symmetric bandwidth that allows us to include a placebo cohort with a corresponding January 1st 2003 cutoff.

[14]With only six monthly bins on each side of the cutoff, clustering at the running variable level, as recommended by Lee and Card (2008), has poor coverage properties (Kolesár and Rothe, 2018).

[15]This approach is similar to that in Dustmann and Schönberg (2012), Danzer and Lavy (2018), Khanna and Mukherjee (2023), and González and Trommlerová (2023), although the terminology used to describe the design varies across papers.

date of birth. Rather, it allows the relationship between outcomes and date of birth to vary arbitrarily as long as it is stable across the main and placebo groups.[16] We also estimate an alternative specification that allows the relationship between outcomes and date of birth to differ across the main and placebo groups by including group-specific linear functions of the running variable (see Bertrand et al. (2021) and Kaplan et al. (2023) for examples of such designs). While this specification relaxes the stability assumption, it requires imposing an additional functional-form restriction. Because our running variable is discrete, measured at the month-of-birth level, the data provide limited support for reliably estimating smooth functions. As a result, this specification yields less precise estimates.

The key identifying assumption of our RD-DD design is that, in the absence of the policy, the January 1st discontinuity in outcomes would be stable over time (Grembi et al., 2016). We assess the plausibility of this assumption in two complementary ways. First, we examine the stability of January 1st discontinuities in the pre-policy period by presenting the evolution of outcomes over time and by conducting placebo tests that re-estimate Equation (1) using fake treatment years prior to policy implementation. Second, we estimate Equation (1) for a sample of children from high-income families whose parental income makes them ineligible for the CLB (or would make them ineligible were the program in place for cohorts born before 2004). Under the identifying assumption, these placebo estimates should be close to zero.

A second assumption is that there is no manipulation of birth timing around the cutoff. This is unlikely in our context because the CLB was announced in the federal budget presented on March 23, 2004, and applied retroactively to all births from January 1, 2004. As such, parents of children born around the cutoff could not have anticipated the policy at the time of conception or birth. Nevertheless, we verify the distribution of births in Figure B1 and confirm that there is no discontinuity in the number of births at the January 1st, 2004 cutoff.

[16]This is a distinctive feature of the RD-DD design relative to a standard RD design. In an RD design, identification typically requires approximating the relationship between the outcome and the running variable locally using linear or higher-order polynomials. In contrast, the RD-DD design allows the relationship between outcomes and the running variable to be purged using a placebo group.

Extension for College Enrollment Outcomes The RD-DD design presented in equation (1) is well-suited for savings outcomes, but faces an additional challenge when applied to college enrollment. Specifically, the main concern is that children born on either side of January 1st belong to different school cohorts and therefore make their enrollment decisions across different years. As a result, cohort-level factors, such as changes in macroeconomic conditions or labor market prospects, affect enrollment rates, and these could confound our estimates even after differencing out the placebo cohort. This concern is particularly salient in our setting, as the enrollment cohorts in our analysis reached college entry age during the COVID-19 pandemic (2020-2022), which differentially affected postsecondary enrollment decisions across cohorts (Bulman and Fairlie, 2022; Statistics Canada, 2022a).[17]

To address school-cohort–specific confounding in college enrollment outcomes, we extend our empirical strategy by introducing an additional control group of children from high-income families whose parental income renders them ineligible for the CLB (or would render them ineligible were the program in place for cohorts born before 2004). These children experience the same cohort-level shocks as their low-income peers, since they enter school in the same year and face the same macroeconomic conditions at college entry. We thus implement a difference-in-difference-in-discontinuities (RD-DDD) design that differences out school-cohort–specific macroeconomic shocks that vary over time but are common across eligibility groups. This extended design allows us to isolate the causal effect of CLB eligibility on college enrollment outcomes under a different stability assumption than that required by the baseline RD-DD specification. We estimate the following equation using a combined

[17]Children born in December 2003 and January 2004 enter school in different years and reach college age in different calendar years. If, say, labor market conditions were unusually weak when the 2004 cohort reached college age, their enrollment rates could rise relative to the 2003 cohort for reasons unrelated to the CLB. Differencing out the January 1st, 2003 placebo discontinuity does not solve this, since the 2003 cohorts faced different macroeconomic conditions at college entry than the 2004 cohorts.

sample of children from low- and high-income families:

$$\begin{aligned} Y_i = \beta_0 &+ \beta_1 \,\text{MainGroup}_i \times \text{PostJan1}_i \times \text{LowInc}_i + \beta_2 \,\text{MainGroup}_i + \beta_3 \,\text{PostJan1}_i \\ &+ \beta_4 \,\text{LowInc}_i + \beta_5 \,\text{MainGroup}_i \times \text{PostJan1}_i + \beta_6 \,\text{PostJan1}_i \times \text{LowInc}_i \\ &+ \beta_7 \,\text{MainGroup}_i \times \text{LowInc}_i + \epsilon_i. \end{aligned} \tag{2}$$

where *LowInc* is an indicator equal to one for children from low-income families (that is, children who are eligible, or would have been eligible, at least once, for the CLB). The remaining variables are defined as in Equation (1). The coefficient β_1 captures the triple difference: how much the change in the January 1st discontinuity between the main and placebo groups differs for low-income versus high-income children. The identifying assumption is that, absent the CLB, the difference in outcomes across the January 1st cutoff would evolve similarly for low- and high-income children across cohorts. This assumption is weaker than the RD-DD requirement of a stable January 1st discontinuity, as it allows for cohort-specific factors that are common across income groups. In Section 7, we discuss placebo tests using fake treatment cutoffs that further support the validity of the RD–DDD design for these college enrollment outcomes.[18]

5 Treatment Effects on RESP Savings

The effect of the CLB on RESP resources available for college depends on four factors: how many eligible families receive the grant (which requires holding an RESP); how much they receive; how parents adjust their own contributions in response; and how much the deposits earn before the child reaches college age. We present these effects in two ways. Figure 1 plots average outcomes by the focal child's month of birth for six RESP variables, allowing us to visually assess the discontinuity at the eligibility cutoff. Table 3 then reports the formal

[18]The RD–DDD design can also be applied to savings outcomes. However, RD–DDD estimates are substantially less precise than RD–DD estimates. Since school-cohort–specific confounding is less of a concern for these outcomes, as they are cumulative and measured over many years, we therefore use the more precise RD–DD specification for savings outcomes as our main specification.

estimates. Column (2) presents our preferred RD-DD estimate: the discontinuity in outcomes at January 1, 2004, net of the corresponding discontinuity at January 1, 2003, which removes any January-versus-December difference unrelated to the CLB. Columns (3) to (5) report placebo tests (high-income children and fake cutoff dates), for which we expect no effect.

We estimate the effect of CLB eligibility rather than of CLB receipt. These are intent-to-treat (ITT) effects, which average over eligible children who receive the grant and those who do not.

5.1 CLB Take-up and RESP Account Ownership

We first examine the share of eligible families that actually receive the CLB. We follow children from birth through age 17, which covers every year in which a CLB deposit can be made before the typical age of college entry. Panels A and B of Figure 1 plot the share of children who ever received the CLB and the average cumulative grant amount, by the focal child's month of birth. The pattern follows the program rules: children born before 2004 receive no CLB transfers, while a substantial share of those born just after the January 2004 cutoff do.

As shown in Table 3, roughly half of eligible families receive the CLB. Take-up is not universal, largely because receiving CLB transfers requires opening an RESP account, and a substantial share of eligible families do not do so. In fact, only about 62% of eligible children have an RESP account, as can be seen from Panel C of Figure 1. Conditional on having an account, however, take-up is high: our estimates indicate that approximately 80% of eligible children with an RESP receive CLB transfers.[19] The fact that take-up remains below 100% likely reflects administrative frictions, such as failure to file annual tax returns (required to verify income eligibility), incomplete application requirements, or the fact that a few financial institutions do not offer the program (Harding et al., 2019).

We can distinguish two types of recipients: *incidental recipients*, who would have opened

[19] 62% of eligible children have an RESP account, whereas 49% receive CLB transfers.

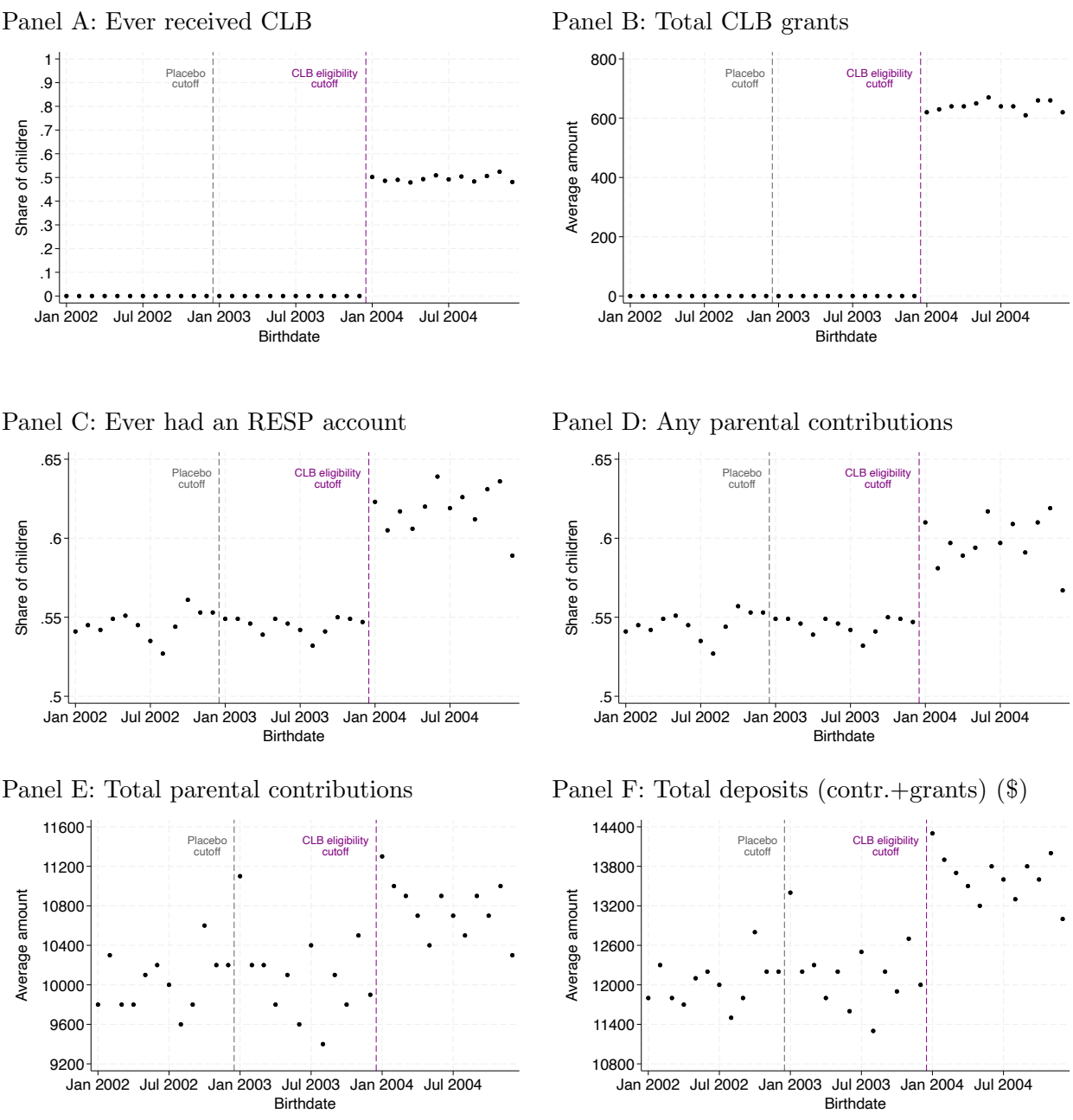


Figure 1: Cumulative RESP Outcomes (Birth to Age 17) by Month of Birth

Notes: This figure plots average RESP outcomes by children's month of birth. The sample is restricted to low-income children, as defined in Section 3. All outcomes are cumulative from birth through age 17. Panels A, C, and D report the share of children for whom the event ever occurs; Panels B, E, and F report average cumulative amounts per child, expressed in 2024 Canadian dollars. The vertical dashed lines mark January 1, 2003 (placebo cutoff) and January 1, 2004 (CLB age eligibility cutoff).

Table 3: Effects of CLB Eligibility on Cumulative RESP Outcomes (Birth to Age 17)

			Placebos		
Outcome	Baseline mean (1)	Main TE (RD–DD) (2)	High-income (3)	Jan. 1, 2002 (4)	Jan. 1, 2003 (5)
Panel A: CLB receipt and RESP account ownership					
Ever received CLB	0.000	0.492***	0.025***	0.000	0.000
		(0.004)	(0.002)	(0.000)	(0.000)
Total CLB grants ($)	0.000	642***	15***	0	0
		(7)	(1)	(0)	(0)
Ever had an RESP account	0.543	0.073***	0.006	0.008	-0.007
		(0.008)	(0.011)	(0.008)	(0.008)
Panel B: Parental contributions					
Any parental contributions	0.543	0.053***	0.002	0.007	-0.007
		(0.008)	(0.011)	(0.008)	(0.008)
Total parental contributions ($)	10,000	720***	-39	297	52
		(242)	(437)	(240)	(241)
Panel C: Additional deposits					
Total matching (CESG) grants ($)	2,100	167***	-18	63	10
		(47)	(79)	(46)	(46)
Panel D: Total deposits and account balance at age 17					
Total deposits (contr.+grants) ($)	12,100	1,528***	-42	360	62
		(289)	(514)	(286)	(286)
Balance ($), return = 3%	14,800	1,943***	-80	387	107
		(359)	(650)	(352)	(354)
Balance ($), return = 5%	17,100	2,294***	-111	404	148
		(419)	(768)	(408)	(412)
Balance ($), return = 7%	19,800	2,718***	-146	420	200
		(494)	(916)	(478)	(484)
Sample size		56,240	24,200	56,890	56,450

Notes: This table reports the effects of CLB eligibility on RESP outcomes. All outcomes are cumulative from birth through age 17: indicators equal one if the event ever occurs, and amounts are cumulative totals per child, expressed in 2024 Canadian dollars. Total deposits are the sum of parental contributions, CLB grants, and Canada Education Savings Grant (CESG) payments. Account balances at age 17 assume a constant annual return of 3, 5, or 7 percent applied from the date of each deposit. The sample is restricted to low-income children as defined in Section 3, except in column (3). Column (1) reports means for low-income children born between July and December 2003, the six months before the CLB eligibility cutoff. Column (2) reports difference-in-discontinuities (RD-DD) estimates: the difference in outcomes between children born just after and just before January 1, 2004, minus the corresponding difference around the placebo cutoff of January 1, 2003, using a six-month bandwidth on each side of each cutoff (Equation (1)). Column (3) reports a placebo test that applies the same specification to high-income (ineligible) children. Columns (4) and (5) report placebo tests for low-income children using fake January 1 eligibility cutoffs in 2002 and 2003, respectively, subtracting the corresponding discontinuity one year earlier and using the same six-month bandwidth. Sample sizes are rounded to the nearest 10. Heteroskedasticity-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

an RESP account regardless but receive CLB transfers as a byproduct, and *active recipients*, who open an account in order to receive the transfer and would not have done so otherwise. The effect of eligibility on RESP account ownership tells us how large the second group is. Among cohorts born before 2004, about 54% of low-income children already have an RESP (Figure 1).[20] Ownership rises sharply at the eligibility cutoff. Our baseline estimate in Table 3 indicates that CLB eligibility raises the probability that a child ever holds an RESP account by 7.3 percentage points. Since 49% of eligible children receive the CLB, and assuming that children induced to open an account receive it, about 7 percentage points of children are active recipients, and the remaining 42 percentage points are incidental recipients.

Table 3 also shows that eligible children receive $642 in cumulative CLB deposits on average. Since only 49% of eligible children receive the CLB, this implies that recipients get about $1,300 each. This is below the program maximum of $2,000, because many families do not remain income-eligible for the full 16 years during which deposits can accrue.

5.2 Parental Contributions

We next analyze whether the CLB changes how much parents save on their own. The program could affect parental savings in three ways. First, it could *crowd out* private savings, as parents may see the government deposit as a substitute for their own money and contribute less (Attanasio and Brugiavini, 2003; Jensen, 2004). Second, it could have no effect, which would occur if active recipients open an account only to claim the grant and incidental recipients keep saving exactly as they would have without it. No effect is plausible because receiving the CLB does not depend on parental contributions in any way. Finally, it could crowd in private savings. Having an account with money already in it may make it easier for families to overcome administrative barriers, become more familiar with saving, or change their expectations about their child's education. This is plausible given evidence that household saving is sensitive to participation costs and program design (Madrian and Shea, 2001; Saez,

[20]This participation rate is relatively high, which likely reflects pre-existing incentives, notably the Canada Education Savings Grant (CESG) introduced in 1998.

2009; Chetty et al., 2014).

Panel B of Table 3 reports the results, which strongly support the third scenario: the CLB crowds in private savings. Specifically, CLB eligibility increases the probability that parents make any voluntary RESP contribution by 5.3 percentage points and raises total parental contributions by $720 on average.

The effect on the share of contributing parents is particularly informative, because it can be compared with the 7.3 percentage point increase in RESP account ownership. In the absence of the CLB, families that open an RESP for their child necessarily contribute to it, so the 5.3 percentage point increase in the share of contributors must come from families induced by the program to open an account for their child.[21] The ratio of the two effects, 5.3/7.3, thus implies that approximately 72% of the families induced to open an RESP for their child also contribute their own money, even though they could receive the CLB without doing so.

Turning to the amount contributed, the $720 increase in total parental contributions represents approximately 7% of the baseline mean of $10,000.[22] This overall increase combines contributions from families induced to start saving by the program (the extensive margin) and any changes in the amounts saved by families that would have contributed anyway (the intensive margin). Our estimates do not separately identify these two components. Scaling the overall effect by the 49% CLB take-up rate yields an implied treatment-on-the-treated effect of approximately $1,470 per recipient.

[21] Without the CLB, an RESP gives no government grant unless someone contributes to it, so there is little reason to open one and leave it empty. Some parents may do so, but an account that never receives a deposit is not observed in our data. For children born before 2004, having an RESP account and having parental contributions therefore coincide.

[22] Figure B2 shows how eligibility shifts the distribution of cumulative parental contributions. The probability of making no contribution falls by 5.3 percentage points, while the probability rises by about 3 percentage points in the lowest positive bin ($1 to $7,500) and by about 1.5 percentage points in the top bin ($30,001 and above). Effects in the middle bins are small and not statistically different from zero.

5.3 Total Deposits and Accumulated Balances

So far, we have shown that CLB eligibility raises RESP deposits through two channels: eligible children receive $642 in CLB grants on average, and parents contribute an additional $720. These additional parental contributions also qualify for matching grants through the Canada Education Savings Grant (CESG) program. We estimate that CLB eligibility increases CESG payments by $167 per child on average (Panel C of Table 3).

Adding these components, CLB eligibility increases total deposits (contributions and grants) by $1,528 by age 17, a 13% increase relative to the baseline mean of $12,100. Scaling by the take-up rate, this implies a treatment-on-the-treated effect of approximately $3,100 for children who actually receive the CLB. This figure is a lower bound on the increase in RESP resources. Our data record contributions and government grants (CLB, CESG, and Additional CESG) but not the investment earnings on them, and these earnings accumulate over the many years between the first deposit and college entry. To gauge their importance, we simulate account balances at age 17 under constant annual returns of 3, 5, and 7 percent, applied from the date of each deposit (Panel D of Table 3). Under these assumptions, CLB eligibility raises the balance by $1,943, $2,294, and $2,718, respectively. The corresponding treatment-on-the-treated effects are roughly $3,900, $4,700, and $5,500.

Figure B3 shows how the two main components accumulate over childhood. Panel A plots the effect of eligibility on cumulative CLB grants, which rises from early childhood and levels off after age 15, consistent with the program design. Panel B plots the corresponding effect on cumulative parental contributions, which also rises gradually, with no discrete jump at any particular age. These patterns are consistent with families opening accounts at different ages and receiving grants and making contributions over many years.

5.4 Placebo Tests and Robustness Checks

The last three columns of Table 3 present various placebo tests. In particular, column (3) applies the main specification to high-income children, who are not eligible for the CLB.

Columns (4) and (5) apply it to low-income children at fake cutoffs on January 1, 2002 and January 1, 2003. If the January 1 discontinuity were not stable over time, or if our estimates reflected something other than CLB eligibility, we would expect to find effects in these columns. We find no statistically significant effects on RESP account ownership, parental contributions, CESG grants, deposits or balances in any of these placebo tests. The high-income result also supports the use of high-income families as a comparison group in the enrollment analysis (Section 7). One exception is CLB receipt among high-income children, which is small but statistically significant. This likely reflects small measurement error in income classification or administrative targeting errors. Reassuringly, this rate is small and does not translate into any detectable behavioral response.

Table B1 shows that the main estimates are robust to alternative specifications. These include a three-month bandwidth, separate linear trends in date of birth, a triple-difference (RD-DDD) specification, and single-cutoff RD estimates. The effects on RESP account ownership (6.7 to 7.6 percentage points) and on the probability of any parental contribution (5.1 to 5.8 percentage points) are stable across specifications. The estimates for contribution amounts are more sensitive to the specification and less precise. They range from $559 with linear trends to $768 with the narrower bandwidth in the RD–DD specifications, and rise to $1,177 in the single-cutoff RD with a one-year window. All estimates are positive, but only some are statistically significant.

5.5 Heterogeneity Across Families

Figure B4 examines heterogeneity by child gender, language spoken at home, parental age at birth, and household structure. The effects on CLB receipt are larger among non-English-speaking households, children of older parents, and children in two-parent families. These differences do not necessarily indicate stronger behavioral responses. They may partly reflect differences in baseline RESP ownership, as families that would have opened an account anyway can receive the grant without changing their behavior. The estimated effects on

account ownership, the probability of contributing, and total deposits are positive across all subgroups. Although their magnitudes vary, the confidence intervals are wide and overlap. Overall, these results suggest that CLB receipt varies with family characteristics, but the saving response is positive across different types of families.

5.6 How Large Are the Effects?

We benchmark the increase in RESP resources in two ways. The first is relative to the cost of college. Depending on the assumed rate of return, children who receive the CLB end up with $3,900 to $5,500 more in their RESP. Average annual university tuition in British Columbia in 2025 was approximately $5,800, or $23,200 for a four-year degree (BC Ministry of Post-Secondary Education and Future Skills, 2026). The effect therefore covers roughly 17 to 24 percent of four-year tuition. By comparison, average student loan balances at repayment consolidation in Canada were around $14,000 in 2010–2011 (Lochner et al., 2021), so the effect equals roughly 28 to 39 percent of a typical balance.

The second benchmark is the gap in college savings between high- and low-income children (Tables 2 and B2). Eligibility closes about 30 percent of the 24 percentage point gap in RESP ownership, about 8 percent of the $8,600 gap in parental contributions, and about 15 percent of the $10,000 gap in total deposits.

Beyond the overall magnitude, it is useful to ask where the increase in deposits comes from. Each dollar of CLB deposits induces approximately $1.12 in parental contributions ($720 divided by $642), so parents' own contributions account for a slightly larger share of the increase in total deposits than the CLB transfer itself. Including the $167 in additional CESG matching grants generated by these contributions, the parental saving response accounts for approximately 58 percent of the increase in total deposits, compared with 42 percent from the direct CLB transfer. Investment returns raise the resources accumulated from both sources.

Taken together, these comparisons show that the increase in RESP resources is economically substantial relative to both college costs and income gaps in college savings. Parental

contributions account for an important share of this increase, amplifying the impact of the government transfer. We next examine why eligibility activates parental saving.

6 Understanding the Saving Response

The parental saving response is both large and surprising. The CLB does not require any parental contribution, so it does not lower the price of saving, and a standard price-incentive explanation therefore cannot account for the response. In addition, all children in our sample, regardless of CLB eligibility, qualify for generous matching grants on parental RESP contributions. The CLB therefore induces additional saving even though the financial incentives to contribute were already in place. So what explains this response?

We investigate this question in three steps. We first examine whether the additional contributions represent new saving or money moved from other household accounts. We then ask whether the response extends to older siblings, who receive no CLB grant. Finally, we discuss the mechanisms most consistent with these findings.

6.1 Do Parents Shift Savings from Other Accounts?

The additional parental contributions could represent new saving or money moved from other household accounts. To distinguish between these possibilities, we use tax-return data that record parents' contributions to two types of tax-advantaged accounts, a general-purpose savings account and a retirement account, as well as their investment income from ordinary taxable accounts. The data come from the Longitudinal Administrative Databank (LAD; Statistics Canada, 2025c), a 20% longitudinal random sample of Canadian tax filers.[23] We

[23]Unlike the rest of the analysis, which relies on British Columbia data, the LAD sample is nationwide. In this dataset, we observe contributions to Tax-Free Savings Accounts, which are general-purpose savings accounts in which investment income accumulates and is withdrawn tax-free, similar to Roth IRAs in the United States. We also observe contributions to Registered Retirement Savings Plans, which are tax-deferred retirement accounts similar to traditional IRAs or 401(k)s. Finally, we observe investment income from non-registered taxable accounts. Our LAD sample follows the same construction procedure as the BC K–12 analysis, with one modification: outcomes are measured at the parental level rather than the child level.

apply the same difference-in-discontinuities design to parents of children born just before and just after January 1, 2004, and compare cumulative contributions from birth through age 17, together with account values and investment income in 2019. If parents financed their RESP contributions by saving less elsewhere, these estimates should be negative. Table B3 shows effects that are small relative to baseline means, and none is statistically significant at the 5% level. The confidence intervals are wide, however, reflecting the larger variance of these outcomes. For the general-purpose tax-free savings account (TFSA), the lower bound of the 95% confidence interval on cumulative contributions is a decrease of about $550, which rules out an offset as large as the $720 increase in RESP contributions. For the retirement account (RRSP), the interval is wide enough that it does not, so we cannot rule out some offset in retirement saving. Overall, we find little evidence that CLB-induced RESP saving crowds out other household saving, which is consistent with the increase reflecting new saving rather than a reshuffling of existing assets. This conclusion aligns with Messacar and Frenette (2019), who show that RESP contributions induced by higher matching grants do not significantly reduce retirement savings.

6.2 Does the Response Extend to Older Siblings?

The results so far suggest that the CLB increases parental saving rather than simply shifting money between accounts. We now ask whether this response is limited to the child who receives the grant or extends to other children in the household. If parents save more because the CLB makes college more affordable for the eligible child, the response may be concentrated in that child's account. If instead the CLB changes household saving behavior more broadly, parents may also save more for their other children. We test this possibility by examining older siblings, who are not eligible for the CLB and receive no CLB grant.

To do so, we use the same January 1, 2004, cutoff, but now as the birth date of the *youngest* sibling in the household. This design is simpler than the one for the main child. Consider two families, each with an older child born in March 2000. In one family the youngest

child is born in December 2003 (just before the cutoff), and in the other in January 2004 (just after). The two older children do not differ in the calendar-year measurement of their outcomes, in school cohort, or in relative age. The only systematic difference between them is whether their younger sibling is eligible for the CLB, so a standard regression discontinuity design is appropriate. Appendix A.4 provides the estimating equation and further details.

Figure 2 plots RESP outcomes for older siblings by the birth month of their youngest sibling. We find that having a CLB-eligible youngest sibling increases the probability that an older sibling has an RESP account by 3.8 percentage points (Table 4). The effect on any parental contribution is identical by construction, because an older sibling's account is observed only if someone contributes to it.[24] Parents contribute an additional $1,184 on average to older siblings' accounts, which raises their simulated balance at age 17 by about $2,000 under a 5 percent return. The estimates are positive and statistically significant across all bandwidths (columns (2) to (5)), though significance levels vary. The high-income placebo estimates in column (6) are small and statistically insignificant.

These findings show that the saving response is not limited to the child who receives the grant. Parents also open accounts and contribute more for older siblings, even though these children receive no CLB. This suggests that the CLB changes household saving behavior more broadly.

6.3 What Explains the Saving Response?

Four explanations could account for the increase in parental saving. The first is *reallocation*: the CLB gives families a reason to open an RESP, and once they have one, parents may move existing college savings into it for convenience. However, RESPs already offered generous matching grants on parental contributions, giving families strong incentives to use these

[24] An RESP account is observed in our data only if it receives a deposit, which can come from the CLB, the CESG, or contributions by parents or other relatives. For CLB-eligible children, some accounts receive only CLB deposits and no contributions, which creates a gap between the share of children with an RESP account and the share with any parental contributions. Older siblings are ineligible for the CLB, and the CESG is paid only on contributions, so their accounts can receive deposits only if parents or other relatives contribute. For them, having an RESP account and having parental contributions therefore coincide.

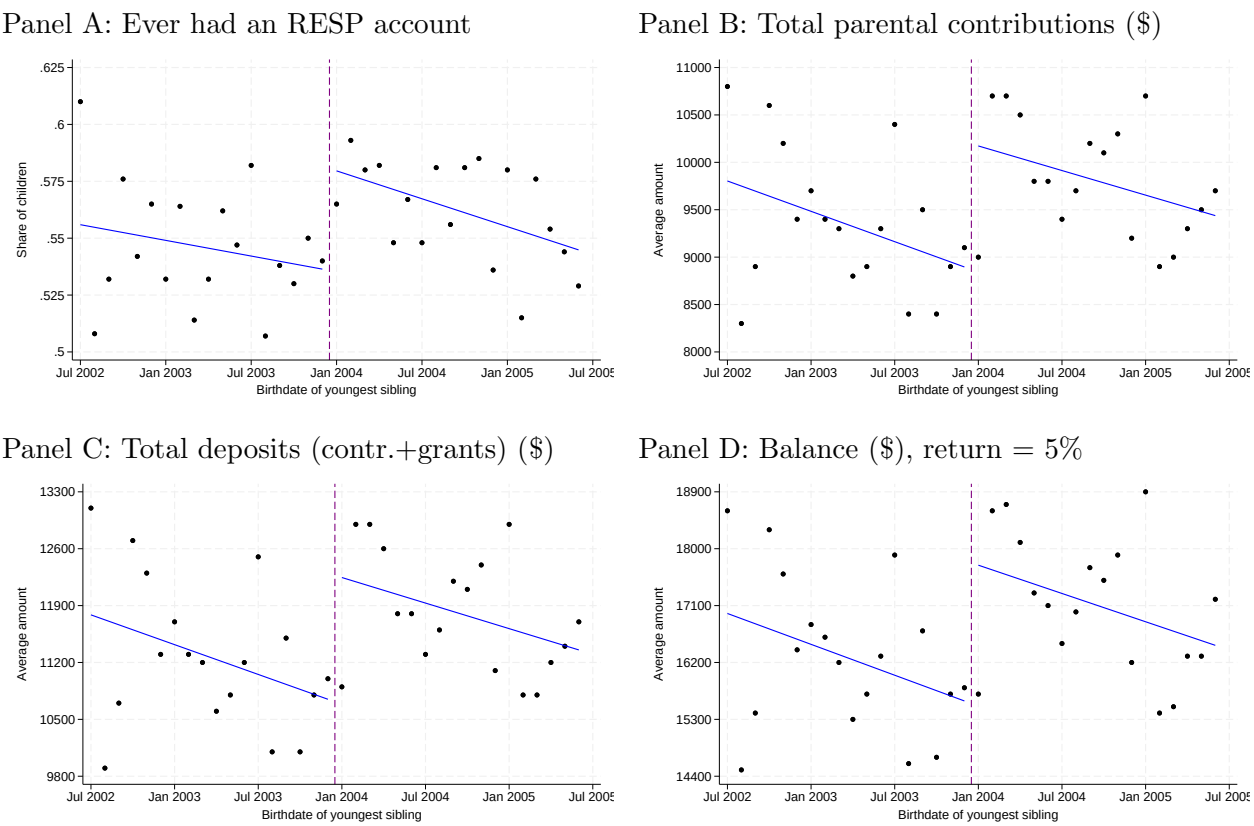


Figure 2: Older Siblings' RESP Outcomes by Youngest Child's Month of Birth

Notes: This figure plots average RESP outcomes for older siblings by the birth month of the youngest child in the family. The sample is restricted to low-income families as defined in Section 3, based on the eligibility history of the youngest child. Older siblings are born before the CLB eligibility cutoff and are therefore not eligible for the CLB themselves. All outcomes are cumulative from birth through age 17: an indicator for ever having an RESP account (Panel A), cumulative parental contributions (Panel B), total deposits into RESP accounts (Panel C), and the total account balance assuming a constant annual return of 5 percent (Panel D). Amounts are in 2024 Canadian dollars. The vertical dashed line marks the January 1, 2004, CLB eligibility cutoff for the youngest child. Solid lines show local linear fits estimated separately on each side of the cutoff.

accounts even without the CLB. Moreover, as shown above, we find little evidence that the additional RESP contributions come at the expense of saving in other accounts. Reallocation therefore seems unlikely to explain the full saving response.

A second possibility is *differential information*. CLB-eligible families may learn more about RESPs than other families, for example through targeted advertising or advice from financial institutions, and this information, rather than the grant, leads them to save. We view this as unlikely. Families on both sides of the cutoff had access to the same CESG matching grants, so financial institutions had no reason to promote RESPs more to one side

Table 4: Spillover Effects of CLB Eligibility on Older Siblings' RESP Outcomes

		Alternative bandwidths				Placebo:
Outcome	Main TE (RD) (1)	12 months (2)	15 months (3)	21 months (4)	24 months (5)	High-income (6)
Ever had an RESP account	0.038***	0.029*	0.037***	0.032***	0.027**	-0.003
	(0.013)	(0.016)	(0.014)	(0.012)	(0.011)	(0.013)
Any parental contributions	0.038***	0.029*	0.037***	0.032***	0.027**	-0.003
	(0.013)	(0.016)	(0.014)	(0.012)	(0.011)	(0.013)
Total parental contributions ($)	1,184***	1,134***	1,380***	1,078***	936***	-426
	(353)	(433)	(386)	(327)	(308)	(508)
Balance ($), return = 5%	2,024***	1,931**	2,383***	1,815***	1,574***	-780
	(614)	(752)	(671)	(569)	(537)	(885)
Bandwidth (months)	18	12	15	21	24	18
Sample size	23,680	15,970	19,850	27,500	30,940	16,680

Notes: This table reports spillover effects of CLB eligibility on older siblings' RESP outcomes. The sample is restricted to low-income families as defined in Section 3, based on the eligibility history of the youngest child. Older siblings are born before the January 1, 2004, CLB eligibility cutoff and are therefore not eligible for the CLB themselves. Estimates compare outcomes for older siblings whose youngest sibling was born just after versus just before January 1, 2004, using a regression discontinuity (RD) specification. All outcomes are cumulative from birth through age 17: indicators equal one if the event ever occurs, and amounts are cumulative totals per child expressed in 2024 Canadian dollars. Balances assume a constant annual return of 5 percent applied from the date of each deposit. Column (1) reports the main specification using an 18-month bandwidth. Columns (2)–(5) vary the bandwidth as robustness checks. Column (6) reports a placebo test that estimates the main specification on older siblings from high-income (ineligible) families. Sample sizes are rounded to the nearest 10. Heteroskedasticity-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

than to the other.

A third possibility is *feasibility*: the CLB may make college more financially attainable, giving parents a reason to save. We think this is unlikely for two reasons. First, the transfer is small: it averages about $1,300 per recipient, with a maximum of $2,000, compared with about $23,200 for four years of tuition in British Columbia. A transfer of this size is unlikely to make college affordable for families who otherwise could not afford it. Second, older siblings receive no CLB grant, so if parents saved more because the grant made college accessible, we would not expect them to increase saving for these siblings. Yet parents contribute an additional $1,200 to older siblings' accounts. This finding is difficult to reconcile with an explanation based solely on the grant making college affordable for the eligible child, and instead points toward a broader change in household saving behavior.

A fourth explanation is *activation*: the incentive to claim the CLB brings families into the RESP system and this encourages subsequent saving. Saving after account opening could be triggered through two channels. The first is lower participation costs. Opening an account takes time, paperwork, and contact with a financial institution. The CLB gives families an immediate, concrete reason to open an account, and once this cost is paid, continuing to save is easier. The second is financial familiarity. Having dealt with a bank and owning an account may make families more comfortable with savings products and more likely to use them. Once an account is open, families may also be exposed to more information about postsecondary education and about why saving early matters for children like theirs. The two channels also apply to older siblings, since a family that has navigated the process, and has come to see saving for college as important, finds it easier to open and fund an account for another child.

This activation mechanism also helps explain why the CLB can encourage saving among families that do not participate when only matching grants exist. A match makes opening an account worthwhile only if parents expect to contribute enough to justify the fixed costs of account opening. Small or uncertain expected contributions may therefore deter entry. Limited trust or financial literacy may also be barriers, as parents need to feel comfortable enough with the system to contribute their own money before benefiting from a match. The CLB requires no such commitment. It gives families a reason to open an account and gain experience with the system, which may build the trust and familiarity needed to begin saving.

7 Treatment Effects on College Enrollment

We now turn to whether the increase in RESP resources documented above translates into higher college enrollment. We focus on enrollment in a bachelor's degree program, for two reasons. First, bachelor's enrollment is the outcome we observe reliably: the PSIS covers nearly all bachelor's-granting institutions, which are almost all public in Canada whereas

short-cycle or vocational programs are only partly covered by our data. Second, a bachelor's degree carries large labor-market returns in Canada (Boothby and Drewes, 2006; Frenette, 2014), so enrollment in these programs is the margin where additional access matters most. CLB funds can also be used to finance short-cycle or vocational programs. If CLB eligibility also encourages enrollment in such programs, our estimates would understate the effect on overall postsecondary enrollment. We measure enrollment at age 18, the typical age of entry into postsecondary education.

7.1 Effects on Main Child

In contrast to the analysis presented above, we adopt a difference-in-difference-in-discontinuities (RD–DDD) design as our baseline specification. As discussed in Section 4, college enrollment may be affected by cohort-specific shocks (e.g., macroeconomic conditions or changes in postsecondary capacity) that differ across birth cohorts. These shocks could bias a standard RD–DD estimate. To address this concern, we add a third difference using high-income families, who are not eligible for the CLB. This allows us to account for cohort-specific changes in enrollment unrelated to the program, provided these changes affect low- and high-income families similarly.

Table 5 presents the results, and column (2) reports our main RD–DDD estimates. CLB eligibility raises bachelor's degree enrollment at age 18 by 4.6 percentage points, a 19% increase relative to the baseline mean. Almost all of this increase is in full-time enrollment, which rises by 4.3 percentage points (22%). Enrollment in STEM fields rises by 2.8 percentage points (27%), and enrollment at flagship universities (U15) rises by 2.2 percentage points (22%).

Columns (3) and (4) report the different components of the RD–DDD estimate. The RD-DDD subtracts the high-income RD–DD estimate from the low-income RD–DD estimate. For instance, for any enrollment, the low-income RD–DD estimate is 1.5 percentage points, compared with −3.1 percentage points for high-income children, who are not eligible for

Table 5: Effects of CLB Eligibility on Bachelor's Degree Enrollment at Age 18

			RD-DDD components	
Outcome	Baseline mean (1)	Main TE (RD-DDD) (2)	Low-income RD-DD (3)	High-income RD-DD (4)
Any enrollment	0.244	0.046***	0.015**	-0.031**
		(0.015)	(0.007)	(0.013)
Full-time enrollment	0.194	0.043***	0.015**	-0.028**
		(0.014)	(0.007)	(0.012)
STEM field	0.103	0.028***	0.003	-0.025***
		(0.011)	(0.005)	(0.009)
Flagship university (U15)	0.100	0.022**	0.018***	-0.004
		(0.011)	(0.005)	(0.009)
Sample size		80,440	56,240	24,200

Notes: This table reports the effects of CLB eligibility on bachelor's degree enrollment outcomes. All outcomes are indicators measured at age 18, the typical age of college entry, and are defined for all children in the sample (unconditional on enrollment). The sample includes children from low-income and high-income families, as defined in Section 3. Column (1) reports baseline means for low-income children born between July and December 2003. Column (2) reports our main estimates, based on a difference-in-difference-in-discontinuities (RD-DDD) specification (Equation (2)). These estimates compare outcomes for children born just after versus just before January 1, 2004, net of the corresponding discontinuity around the placebo cutoff of January 1, 2003, and take the difference between low-income and high-income children, thereby differencing out school-cohort-specific shocks. Columns (3) and (4) report the underlying difference-in-discontinuities (RD-DD) estimates for low-income and high-income children, respectively (Equation (1)). Subtracting column (4) from column (3) yields column (2), up to rounding. All specifications use a six-month bandwidth on each side of the January 1 cutoffs. A flagship university is defined as a member of the U15 Group of Canadian Research Universities (U15), and STEM refers to science, technology, engineering, and mathematics fields. Sample sizes are rounded to the nearest 10. Heteroskedasticity-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the CLB. Subtracting the high-income estimate yields an RD–DDD effect of 4.6 percentage points. The high-income adjustment thus accounts for about two-thirds of the estimated effect on any enrollment. For flagship university enrollment, however, the adjustment is much smaller: the low-income RD–DD estimate (0.018) is already close to the RD–DDD estimate (0.022), while the high-income estimate is near zero (-0.004).

The RD–DDD estimate reflects the effect of CLB eligibility only if, without the program, enrollment would have changed, between children born in 2003 and children born in 2004, by the same amount across the two income groups. We cannot test this assumption directly at the 2004 cutoff, so we test it at earlier cutoffs, when no program existed. Specifically, we repeat the RD–DDD analysis using January 1 of each year from 2000 through 2003 as

placebo cutoffs. If the two income groups would have followed similar patterns without the program, these placebo estimates should be close to zero. Figure 3 shows that all four placebo estimates are statistically indistinguishable from zero and smaller in absolute magnitude than the main estimate. This supports using high-income children as a comparison group.[25]

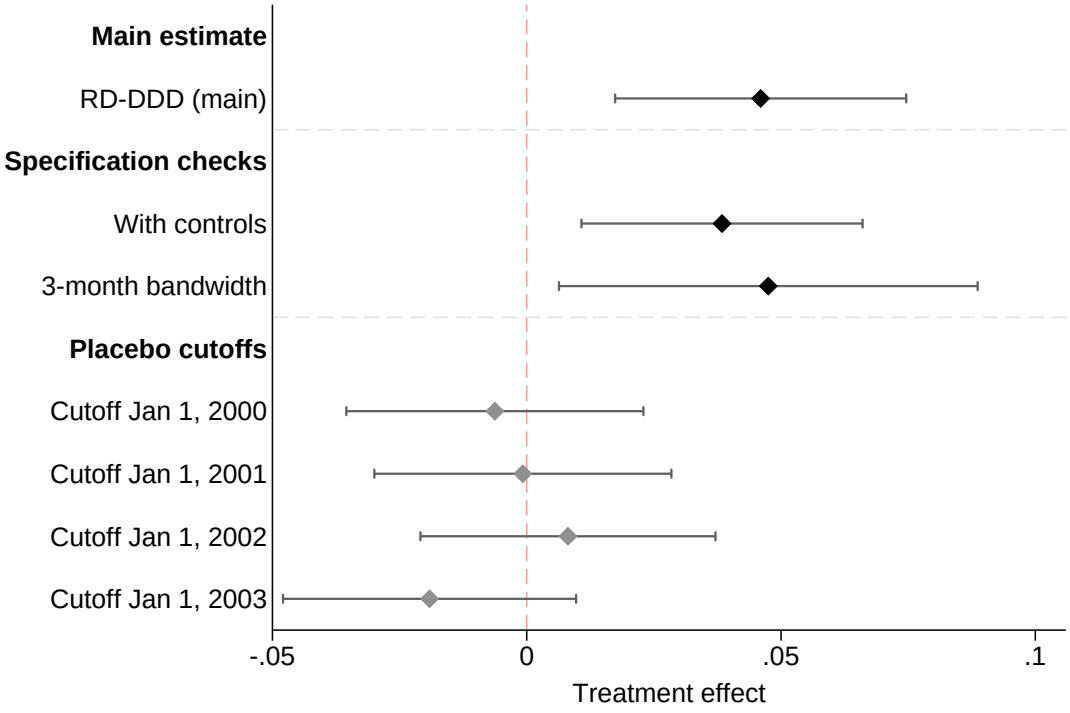


Figure 3: Robustness Checks for Bachelor's Degree Enrollment at Age 18

Notes: This figure reports robustness checks for the effect of CLB eligibility on being enrolled in a bachelor's degree program at age 18. The main estimate uses the difference-in-difference-in-discontinuities (RD–DDD) specification with a six-month bandwidth on each side of the January 1 cutoffs. Specification checks add controls for child gender, parental income category, single-parent status, language spoken at home, and kindergarten observation, or use a narrower three-month bandwidth. Placebo estimates apply the main specification at fake eligibility cutoffs on January 1 of 2000 through 2003, using the preceding year's cutoff as the comparison. Horizontal bars show 95% confidence intervals based on heteroskedasticity-robust standard errors.

Figure 3 also shows that our main enrollment estimate is robust to alternative specifications. Adding controls for child gender, parental income category, single-parent status, language spoken at home, and kindergarten observation lowers the effect on enrollment to 3.8 percentage

[25]In contrast, a simpler RD–DD design does not control for cohort-specific shocks at enrollment. Figure B5 shows that this design produces statistically significant estimates even at the placebo cutoffs before the CLB, which indicates that cohort-specific shocks are important.

points. Using a narrower three-month bandwidth, which halves the sample and widens the confidence intervals, raises it to 4.8 percentage points.

We also assess whether the enrollment results are sensitive to the age at which enrollment is measured. Figure B6 shows that CLB eligibility raises the probability of being enrolled in a bachelor's degree program at age 19 by 3.2 percentage points, and the probability of being enrolled at either age 18 or 19 by 2.9 percentage points. These results suggest that our findings are not specific to enrollment at age 18. Eligibility also raises the probability of being enrolled at both ages by 5.0 percentage points, consistent with persistence across the two years.

Finally, we analyze whether CLB eligibility affects preparation for university while students are still in high school. Specifically, we examine the impacts on the number of university-track courses taken, and their grades in these classes in Table B4. We find that eligibility for the CLB leads students to take an additional 0.15 university-track courses on average, 10 percent of the baseline mean. Moreover, program eligibility significantly increases the probability of receiving an A in at least one such course by 4.6 percentage points, and also the total number of courses in which they received an A. Taken together, these findings suggest that the response to CLB eligibility starts before children reach university. Eligible children take more university-track courses and earn high grades in more of them, which is consistent with greater preparation for university. Although these results do not establish how much of the enrollment effect operates through high school preparation, they suggest that the CLB affects educational decisions before families draw on their RESP funds.

7.2 Effects on Older Siblings

The parental saving response is not limited to the eligible child: parents also save more for older siblings, who receive no CLB grant. We now ask whether older siblings also see higher enrollment rates. We use the same regression discontinuity specification as in Section 6.2, with the youngest sibling's birth date as the running variable, and measure the siblings'

enrollment in a bachelor's degree program at age 18.

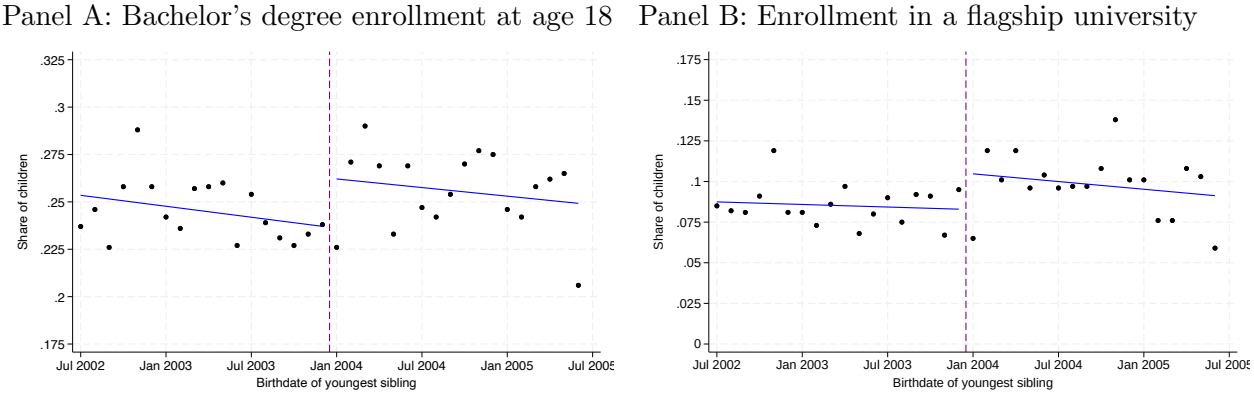


Figure 4: Older Siblings' Bachelor's Degree Enrollment by Youngest Child's Month of Birth

Notes: This figure plots average enrollment outcomes for older siblings by the birth month of the youngest child in the family. The sample is restricted to low-income families as defined in Section 3, based on the eligibility history of the youngest child. Older siblings are born before the CLB eligibility cutoff and are therefore not eligible for the CLB themselves. Outcomes are measured at age 18: enrollment in a bachelor's degree program (Panel A) and enrollment at a flagship university, defined as a member of the U15 Group of Canadian Research Universities (Panel B). The vertical dashed line marks the January 1, 2004, CLB eligibility cutoff for the youngest child. Solid lines show local linear fits estimated separately on each side of the cutoff.

Figure 4 shows a discontinuity at the eligibility cutoff in both bachelor's degree enrollment (Panel A) and enrollment at a flagship university (Panel B). Having a CLB-eligible younger sibling raises the probability that an older sibling enrolls in a bachelor's degree program by 2.4 percentage points, and the effect on full-time enrollment is similar (2.5 percentage points). Moreover, Table 6 shows that the likelihood of enrolling at a flagship university similarly increases by 1.7 percentage points. Importantly, the high-income placebo in column (6) is close to zero for all outcomes. The estimates are positive at every bandwidth, but they are less precise than the effects on savings. Nonetheless, these results show that CLB eligibility changes how parents behave, and that this change is enough to raise enrollment for an older child who does not directly receive the transfer.

7.3 Magnitude of the Effects

Is a 4.6 percentage point increase in bachelor's enrollment large? One reference point is the enrollment gap between low- and high-income children. Before the CLB, high-income

Table 6: Spillover Effects of CLB Eligibility on Older Siblings' Bachelor's Degree Enrollment

		Alternative bandwidths				Placebo:
Outcome	Main TE (RD) (1)	12 months (2)	15 months (3)	21 months (4)	24 months (5)	High-income (6)
Any enrollment	0.024**	0.018	0.027**	0.021**	0.017*	0.005
	(0.011)	(0.014)	(0.012)	(0.011)	(0.010)	(0.015)
Full-time enrollment	0.025**	0.017	0.030***	0.021**	0.017*	0.005
	(0.010)	(0.013)	(0.011)	(0.010)	(0.009)	(0.015)
STEM field	0.009	0.001	0.007	0.007	0.007	-0.003
	(0.008)	(0.010)	(0.009)	(0.007)	(0.007)	(0.011)
Flagship university (U15)	0.017**	0.004	0.017**	0.016**	0.013**	0.003
	(0.008)	(0.009)	(0.008)	(0.007)	(0.007)	(0.011)
Bandwidth (months)	18	12	15	21	24	18
Sample size	23,680	15,970	19,850	27,500	30,940	16,680

Notes: This table reports spillover effects of CLB eligibility on older siblings' bachelor's degree enrollment. The sample is restricted to low-income families as defined in Section 3, based on the eligibility history of the youngest child. Older siblings are born before the January 1, 2004, CLB eligibility cutoff and are therefore not eligible for the CLB themselves. Estimates compare outcomes for older siblings whose youngest sibling was born just after versus just before January 1, 2004, using a regression discontinuity (RD) specification. All outcomes are indicators measured at age 18 and are defined for all older siblings in the sample (unconditional on enrollment): any enrollment in a bachelor's degree program, full-time enrollment, enrollment in a STEM field, and attendance at a flagship university, defined as a member of the U15 Group of Canadian Research Universities. Column (1) reports the main specification using an 18-month bandwidth. Columns (2)–(5) vary the bandwidth as robustness checks. Column (6) reports a placebo test that estimates the main specification on older siblings from high-income (ineligible) families. Sample sizes are rounded to the nearest 10. Heteroskedasticity-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

children enrolled in bachelor's programs at a rate about 14 percentage points higher than low-income children (Tables 2 and B2), so our estimate closes about one-third of this gap. This suggests that delivering student aid through college savings accounts may help close the income gradient in college attendance.

A second question is whether the CLB raises enrollment efficiently per dollar spent. We address it by comparing the CLB effect with the effect of traditional student aid. Eligibility raises the grants deposited in an eligible child's RESP by about $809 on average by age 17 ($642 in CLB grants and $167 in CESG matching grants). As with other student aid, these funds are available only if the child enrolls. The enrollment effect therefore corresponds to 5.7 percentage points per $1,000 of government aid. A large literature, primarily based

on U.S. programs, finds enrollment effects of approximately 3 to 6 percentage points per $1,000 (USD) of *annual* aid eligibility (Seftor and Turner, 2002; Stanley, 2003; Dynarski, 2003; Kane, 2003; Abraham and Clark, 2006; Cornwell et al., 2006; Kane, 2007; Dynarski and Scott-Clayton, 2013). Because the CLB is a single amount available for the whole time in college, the right comparison is with total aid, not annual aid. Students typically receive aid for several years. If we assume three years on average, the literature's estimates translate into 1 to 2 percentage points per $1,000 of total aid eligibility. The CLB effect per dollar of government aid is therefore roughly three to six times larger than that of traditional aid.[26]

Why is the CLB effect per dollar of government aid so much larger than that of traditional aid? One possibility is that parental saving, which eligibility also raises, affects enrollment in its own right. Two pieces of evidence point in this direction. First, older siblings do not receive the CLB grant, yet their enrollment rises by 2.4 percentage points when their youngest sibling is CLB-eligible and their parents save more. Their simulated RESP balances at age 17 rise by about $2,000 under a 5 percent return (Table 4), so the sibling effect corresponds to about 1.2 percentage points per $1,000 of additional savings. Second, Vasilenko (2026) uses variation in the investment returns of 529 plans in the United States and finds that wealth accumulated in college savings accounts raises four-year college attendance by about 0.6 to 0.9 percentage points per $1,000 of account balance. Our sibling estimate is somewhat larger but consistent with this finding. Both are also similar to the effect of grant aid per dollar of total aid. Together, these results suggest that additional parental savings can raise enrollment, and can raise it by roughly as much per dollar as a grant does.

We therefore also scale the CLB enrollment effect by the total increase in RESP resources. CLB eligibility raises the child's simulated balance at age 17 by $1,943 to $2,718, depending on the assumed return. The corresponding enrollment effect is 1.7 to 2.4 percentage points

[26]We do not convert between currencies, because no single adjustment is clearly appropriate. Converting at market exchange rates would raise our estimates per $1,000 (USD), since a Canadian dollar is worth less than a U.S. dollar. Using purchasing power parity would shrink this increase. On the other hand, college costs are lower in Canada than in the United States, so a given amount of aid covers a larger share of tuition; adjusting for the cost of college would therefore lower our estimates per dollar of aid. The net direction of these adjustments is ambiguous.

per $1,000 of additional resources, at or somewhat above the 1 to 2 percentage point range for traditional aid. In other words, accounting for parental savings and accumulated returns yields an effect per dollar broadly consistent with the literature.

Two other explanations could account for why the CLB has a larger effect on enrollment than traditional aid. The first is certainty. The CLB amount is known years before enrollment, and early guarantees of aid have been shown to raise enrollment more than equivalent aid revealed at the time of enrollment (Dynarski et al., 2021; Burland et al., 2023). The second is aspirations. Seeing funds set aside for college from an early age may shape the eligible child's expectations and encourage preparation well before the enrollment decision (Destin and Oyserman, 2009; Elliott, 2009; Elliott et al., 2011). Both would raise the effect of the grant on the eligible child's enrollment even without any increase in parental savings.

8 Conclusion

In this paper, we study the effects of the Canada Learning Bond (CLB) program. To identify the causal effects of CLB eligibility, we implement a difference-in-discontinuities research design that compares children born immediately before and after the January 1, 2004, eligibility cutoff. We show that the program increased college savings account ownership, increased parental savings in these accounts, and increased bachelor's degree enrollment. Our findings yield three key takeaways:

1. *Non-matching grants crowd in private savings.* Despite requiring no parental contributions, 72% of parents induced to open an account to receive the CLB voluntarily choose to contribute their own funds. This suggests that non-matching grants can overcome the behavioral barriers that prevent low-income families from participating in savings programs, even when substantial financial incentives are already available.

2. *CLB increases college enrollment at low cost.* CLB eligibility increases bachelor's degree enrollment by 4.6 percentage points, an effect that is large compared to traditional

financial aid programs.

3. *The parental saving response may amplify the effect on enrollment.* When a younger child is CLB-eligible, parents save more for older siblings, who receive no CLB grant, and these siblings are more likely to enroll in a bachelor's degree program. This spillover suggests that the CLB raises enrollment through the additional parental saving it triggers, in addition to the grant itself, and it helps explain why the enrollment effect for eligible children is large relative to the size of the transfer.

Our results also have implications for inequality in college access. The CLB closes roughly one-third of the 14 percentage point pre-existing gap in bachelor's degree enrollment between high- and low-income children, along with similar reductions in college savings outcomes. These effects are particularly important given longstanding concerns that tax-advantaged college savings accounts primarily benefit higher-income families (Dynarski, 2004; Milligan, 2005; Frenette, 2017; Bonikowska and Frenette, 2020; Briscese et al., 2025).

Two questions remain open for future research. First, our findings emerge from a context where baseline RESP participation is relatively high and where postsecondary education costs are moderate compared to the United States. Whether similar programs would be equally effective in settings with lower baseline savings account participation and substantially higher college costs remains an open question. Second, while we demonstrate that non-matching grants activate saving among families who did not respond to pre-existing matching grants, we cannot directly compare the relative effectiveness of these two policy instruments. Future research could experimentally vary the structure of grants (matching only versus non-matching only versus both) to identify which design most cost-effectively encourages household savings and expands college access.

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Small Seeds, Big Returns: Delivering Student Grants Through College Savings Accounts

Online Appendix

William Arbour Laëtitia Renée Fernando Saltiel

November 10, 2026

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Appendix A: Additional Context and Methods

A.1 Literature Review

Table A1 summarizes the small but growing set of studies that examine the causal effects of government grants delivered through college savings accounts. For each study, we report the institutional context, the intervention design, and the identification strategy employed.

Two randomized experiments in the United States, the SEED for Oklahoma Kids program and the Boston Early College Planning Initiative, show positive effects of financial incentives tied to college savings accounts on account-opening and parental savings (Kim et al., 2015; Clancy et al., 2016; Huang et al., 2021; Long and Bettinger, 2017). Long and Bettinger (2017) also report preliminary marginally significant effects on college enrollment of the Boston Early College Planning Initiative. Positive effects of college savings accounts grants on college enrollment were also found in an Italian randomized experiment (ACHAB) conducted with high school students (Martini et al., 2021).

The only paper studying a large-scale existing grant program is Messacar and Frenette (2019), who analyze the Canada Education Savings Grant, a federal matching grant for college savings targeted to low- and middle-income families. They find that higher matching rates increase parental savings at the intensive margin.

Table A1: Programs Providing Government Grants through College Savings Accounts

Program	Papers	Methodology	Timing	Intervention	Effects
Panel A: Randomized Controlled Trials					
SEED for Oklahoma Kids experiment (SEED OK), United States	Several papers. Main ones: Kim et al. (2015); Clancy et al. (2016); Huang et al. (2021).	RCT. N≈2,700	From birth to age 5	Automatic enrollment + seed deposit ($1,000) + matching grant + information	Account ownership: ↑ Assets: ↑ Parental expectations for college: ↑
Boston Early College Planning Initiative, United States	Long and Bettinger (2017)	RCT. N≈400	Grades 7–10	Information + enrollment assistance + seed grant ($50)	Account opening: ↑ Parental savings: ↑ Assets: ↑ College enrollment: weak ↑
ACHAB (Percorsi program), Italy	Martini et al. (2021)	RCT. N≈700	End of high school	4:1 matching grant (up to €10,000) + financial education	Parental contributions: ↑ Assets: ↑ College enrollment: ↑ Persistence: ↑
Panel B: Quasi-Experimental					
Additional Canada Education Savings Grant (A-CESG). Federal program. Canada.	Messacar and Frenette (2019)	RD: Discontinuous variation in match rate at income cutoffs	From birth to age 17	Additional 10–20% match rate (base = 20% matching grant)	Parental contributions: ↑ Account ownership: — Crowd-out: —
Canada Learning Bond (CLB). Federal program. Canada.	This paper	RD-DD Birth-date cutoff (Jan 1, 2004)	From birth to age 15	Seed grant worth $500 to $2,000	Account ownership: ↑ Parental savings: ↑ Assets: ↑ Crowd-out: — College enrollment: ↑

Notes: RCT stands for Randomized Controlled Trial. For each outcome, an upward arrow (↑) indicates that the program increases that outcome, a downward arrow (↓) indicates that the program decreases it, and a dash (—) indicates that the outcome is not directly affected by the program. See the Appendix text for additional details.

A.2 RESPs and 529 Plans

Table A2: Registered Education Savings Plans and 529 Plans

	Canada: RESP	**United States: 529 plan**
Providers	Banks, investment firms and scholarship plan dealers offer individual, family and group plans.	States sponsor the plans, which private firms manage. Some states also sell prepaid tuition plans.
Contributions	Not tax-deductible. Lifetime limit of C$50,000 per child.	Not deductible from federal income tax. Plans cap the balance per child at US$235,000 to US$675,000.
Federal matching grant	The CESG matches 20% of the first C$2,500 saved each year, up to C$7,200 per child. Lower-income families receive an extra 10% or 20% on the first C$500.	None.
Deposits without a contribution	The CLB deposits C$500, then C$100 a year, up to C$2,000, for low-income children born in 2004 or later. It requires an RESP but no contribution.	Some states deposit US$25 to US$500 into children's 529 accounts.
Provincial or state support	Quebec adds a 10% match. British Columbia deposits C$1,200 for children born in 2006 or later.	More than 30 states offer an income tax deduction or credit. About eight states match the savings of lower-income families.
Taxes	Earnings grow tax-free. Students pay income tax on the grants and earnings they withdraw.	Earnings grow tax-free. Withdrawals for eligible costs pay no federal income tax.
Eligible studies and costs	Full-time and part-time postsecondary programs in Canada and some abroad. Payments can cover tuition and reasonable living costs. The provider pays only after the student shows proof of enrollment.	College, vocational and apprenticeship programs, and K-12 tuition up to US$20,000 a year. Room and board only for students enrolled at least half-time. Plans pay without proof of enrollment, and families keep receipts for the IRS.
Unused funds	The subscriber can change the beneficiary or take back contributions tax-free. Grants go back to the government. Earnings face income tax plus an additional 20% tax, yet up to C$50,000 can move to a retirement plan.	The owner can change the beneficiary to a family member. Other withdrawals of earnings face income tax plus an additional 10% tax. Up to US$35,000 can move to a Roth IRA after 15 years.
Take-up	53% of families with a child under 18 held an RESP in 2019, up from 16% in 1999.	14% of households with a child under 18 held a 529 or Coverdell account in 2024.

Notes: Table A2 presents the main features of Registered Education Savings Plans (RESPs) in Canada and Section 529 plans in the United States in 2026. Some rules differed during our sample period. Before 2007, the CESG matched the first C$2,000 saved each year, and no provincial grant applied to the British Columbia cohorts we study. In the United States, K-12 tuition became an eligible cost in 2018 and the Roth IRA rollover started in 2024. Take-up comes from Statistics Canada (2022b) for Canada and from the Survey of Income and Program Participation for the United States (U.S. Census Bureau, 2026), which counts 529 and Coverdell accounts together. The rules come from Canada Revenue Agency (2026) and Internal Revenue Service (2025).

A.3 Income Eligibility Determination

For each child and each year of potential CLB eligibility (ages 0 to 15), we determine whether the child is income-eligible for the CLB. We then aggregate each child's yearly statuses across childhood and classify children into three mutually exclusive groups: (i) eligible at least once, (ii) never eligible, and (iii) ambiguous. This section describes how we determine the yearly status.

The yearly status requires two inputs: parental income, which we take from tax records, and the CLB income threshold for that year. A child is classified as income-eligible in a given year if total parental income falls below that year's threshold.

CLB income thresholds. We could not obtain a complete series of official CLB income thresholds back to 2004, so we estimate them. For each year, we calculate CLB receipt rates across $100 parental income bins and identify the income level with the largest discrete change in receipt rates as in Porter and Yu (2015). We validate this method against the official thresholds for the years in which they are available, and our estimates are very close to them.

We use these estimated thresholds for all calendar years from 2004 onward. For children born before 2004, we assess whether they would have been income-eligible had the program applied to their cohort. This requires thresholds for 1999 to 2003, before the program existed. For those years, we take the 2004 threshold and adjust it backward for inflation.

Parental income. In some cases, we do not have enough tax information to determine income eligibility with certainty. For example, a parent's tax return may be missing for a given year, or, following a separation, eligibility may depend on the income of a new partner whose tax information we do not observe. We classify eligibility in those years as ambiguous.

A.4 Empirical Strategy for the Effects on Older Siblings

To identify how CLB eligibility for a child affects outcomes for their older, non-eligible siblings, we exploit the same January 1, 2004, eligibility cutoff but now as the birth date of

the *youngest* sibling in the household.

Siblings are identified through parental identifiers in the linked BC K–12 and tax data, and are defined as all children ever claimed by the same parent(s). To ensure completeness of sibling identification, we restrict the sample to older siblings who are continuously observed in the British Columbia public school system, which increases the likelihood that all siblings in the household are captured in our data.

The design here is simpler than for the main child. Consider two families, each with an older child born in March 2000. In one family, the youngest child is born in December 2003 (just before the cutoff), whereas in the other, the youngest is born in January 2004 (just after). The two older children do not differ in calendar-year measurement of their outcomes, in school cohort, or in relative age. The only systematic difference between them is whether their younger sibling is eligible for the CLB. Therefore, a standard regression discontinuity design is appropriate for this analysis.

We estimate the following specification:

$$Y_{i,my} = \alpha + \beta\,\text{PostJan1}_i + \text{Dist}_i(\delta_1 + \delta_2\text{PostJan1}_i) + \gamma_y + \gamma_m + \epsilon_i, \tag{3}$$

where $Y_{i,my}$ denotes the outcome of older sibling i, born in month m and year y. PostJan1_i is an indicator equal to one if the younger sibling of older sibling i is born on or after January 1, 2004, and zero otherwise. Dist_i measures the distance, in months, between the younger sibling's date of birth and January 1, 2004. The terms γ_y and γ_m denote fixed effects for the older sibling's birth year and birth month, respectively. The coefficient β is the parameter of interest and captures the spillover effect of having a younger sibling eligible for the CLB.

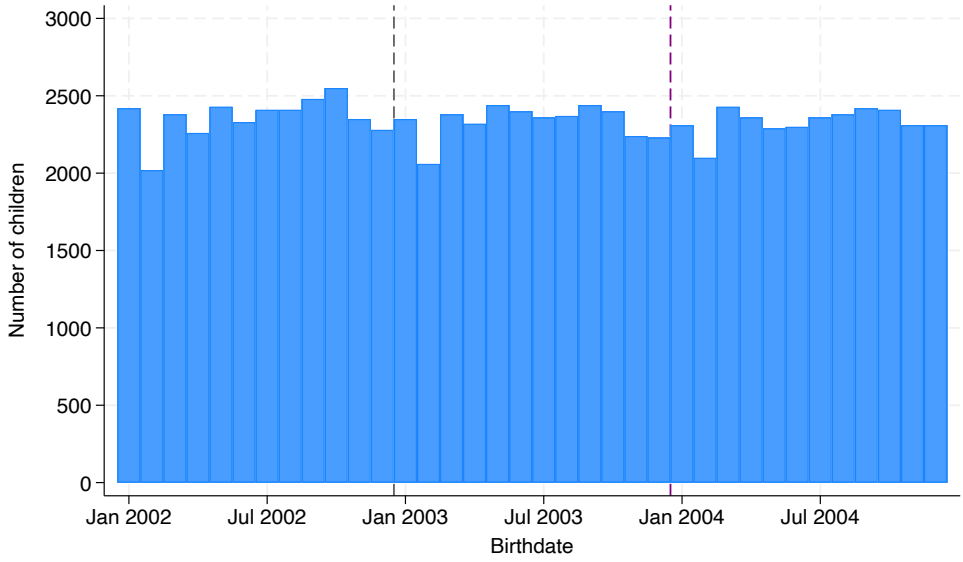
Given the smaller number of older siblings included in this sample, our baseline estimates use a symmetric bandwidth of 18 months around the cutoff. Nonetheless, we assess the sensitivity of our results to alternative bandwidth choices of 12, 15, 21, and 24 months. We

report heteroskedasticity-robust standard errors throughout.

We focus on a sample of older siblings born between 1995 and 2003. The upper bound of 2003 ensures that older siblings are born before the January 1, 2004, CLB eligibility cutoff, making them ineligible for the CLB. The lower bound of 1995 allows us to observe a substantial share of their RESP saving histories, which are available from 1998. Our results are robust to relaxing this lower birth-year bound. As in the main analysis, we restrict attention to families classified as low-income based on the eligibility history of the focal child.

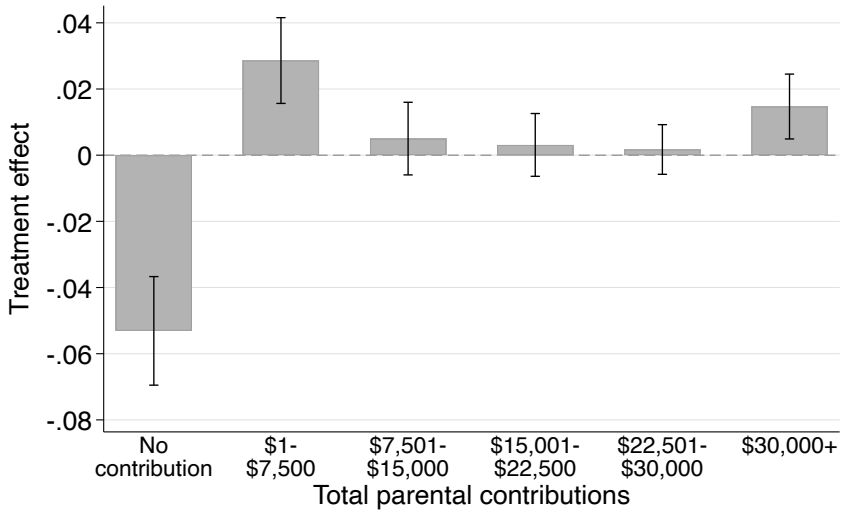
Appendix B: Additional Tables and Figures

Figure B1: Distribution of Date of Birth



Notes: The figure plots the distribution of children's dates of birth in the main sample of low-income families. Each bar reports the number of children born in a given month.

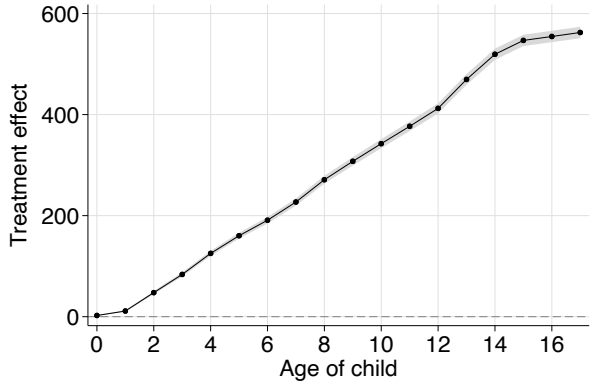
Figure B2: Distribution of the Effect of CLB Eligibility on Parental Contributions



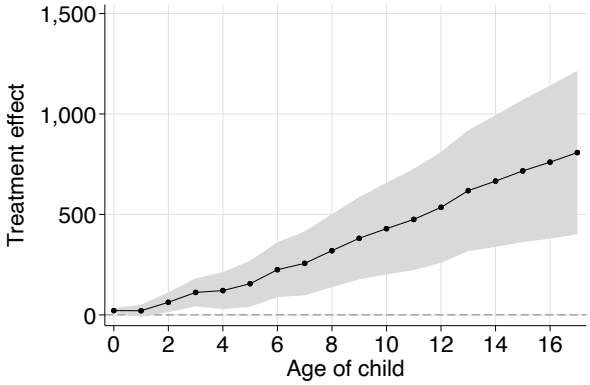
Notes: This figure plots difference-in-discontinuities estimates of the effect of CLB eligibility on the probability of falling into each bucket of cumulative parental contributions, measured from birth through age 17. Each bar reports a separate estimate from Equation (1), where the outcome is an indicator equal to one if total parental contributions fall within the indicated range. Contribution amounts are expressed in 2024 Canadian dollars. The sample is restricted to low-income children as defined in Section 3. Vertical bars represent 95% confidence intervals based on heteroskedasticity-robust standard errors.

Figure B3: Dynamic Treatment Effects

Panel A: Total CLB grants



Panel B: Total parental contributions



Notes: This figure plots difference-in-discontinuities estimates of the effect of CLB eligibility on cumulative RESP outcomes at each age of the child, from birth through age 17. Panel A reports effects on cumulative CLB grants received, and Panel B reports effects on cumulative parental contributions. Amounts are in nominal Canadian dollars, so the estimates at age 17 differ slightly from the totals in 2024 dollars reported in Table 3. The sample is restricted to low-income children as defined in Section 3. Estimates correspond to Equation (1). Shaded bands show 95% confidence intervals based on heteroskedasticity-robust standard errors.

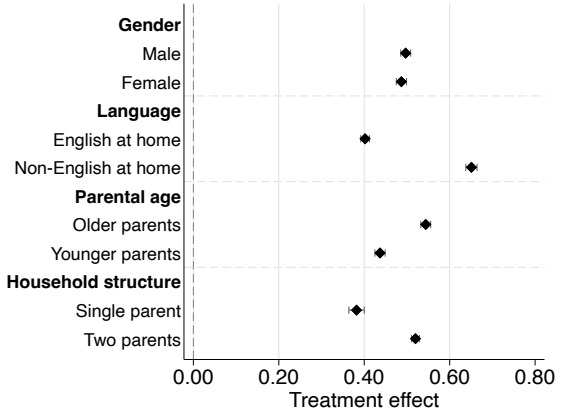
Table B1: Robustness of the Effects of CLB Eligibility on RESP Outcomes

		RD-DD alternatives			Single-cutoff RD	
Outcome	Main TE (RD–DD) (1)	3-month BW (2)	Linear MoB (3)	RD-DDD (4)	BW= 1 year (5)	BW= 2 years (6)
Panel A: CLB receipt and RESP account ownership						
Ever received CLB	0.492***	0.492***	0.488***	0.467***	0.489***	0.489***
	(0.004)	(0.006)	(0.009)	(0.005)	(0.006)	(0.006)
Total CLB grants ($)	642***	631***	617***	626***	636***	636***
	(7)	(9)	(13)	(7)	(9)	(9)
Ever had an RESP account	0.073***	0.076***	0.074***	0.067***	0.074***	0.076***
	(0.008)	(0.012)	(0.017)	(0.013)	(0.008)	(0.010)
Panel B: Parental contributions						
Any parental contributions	0.053***	0.057***	0.058***	0.051***	0.054***	0.056***
	(0.008)	(0.012)	(0.017)	(0.013)	(0.008)	(0.010)
Total parental contributions ($)	720***	768**	559	759	1,177***	764***
	(242)	(348)	(501)	(499)	(246)	(282)
Panel C: Additional deposits						
Total matching (CESG) grants ($)	167***	166**	125	185**	236***	182***
	(47)	(67)	(97)	(92)	(48)	(54)
Panel D: Total deposits and account balance at age 17						
Total deposits (contr.+grants) ($)	1,528***	1,564***	1,302**	1,570***	2,049***	1,582***
	(289)	(416)	(598)	(590)	(296)	(338)
Balance ($), return = 3%	1,943***	1,949***	1,586**	2,024***	2,815***	1,994***
	(359)	(517)	(743)	(743)	(368)	(421)
Balance ($), return = 5%	2,294***	2,270***	1,821**	2,405***	3,493***	2,341***
	(419)	(604)	(867)	(876)	(430)	(492)
Balance ($), return = 7%	2,718***	2,656***	2,098**	2,864***	4,347***	2,760***
	(494)	(711)	(1,021)	(1,040)	(506)	(580)
Sample size	56,240	27,670	56,240	80,440	55,960	84,260

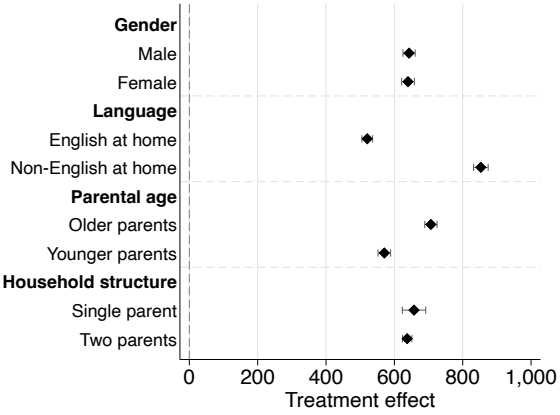
Notes: This table reports the effects of CLB eligibility on RESP outcomes under alternative specifications. The sample is restricted to low-income children as defined in Section 3, with high-income children included as an additional comparison group in column (4). Column (1) reproduces the main difference-in-discontinuities (RD-DD) estimates from Table 3. Column (2) uses a three-month bandwidth. Column (3) allows separate linear trends in date of birth on each side of each cutoff. Column (4) reports difference-in-difference-in-discontinuities (RD-DDD) estimates, which additionally difference out school-cohort-specific shocks using high-income children (Equation (2)). Columns (5) and (6) report regression discontinuity (RD) estimates around January 1, 2004, without subtracting the discontinuity at the January 2003 cutoff. Column (5) uses a one-year window and separate linear trends on either side of the cutoff. Column (6) uses a two-year window and additionally includes month-of-birth fixed effects. Sample sizes are rounded to the nearest 10. Heteroskedasticity-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure B4: Heterogeneous Treatment Effects of CLB Eligibility on RESP Outcomes

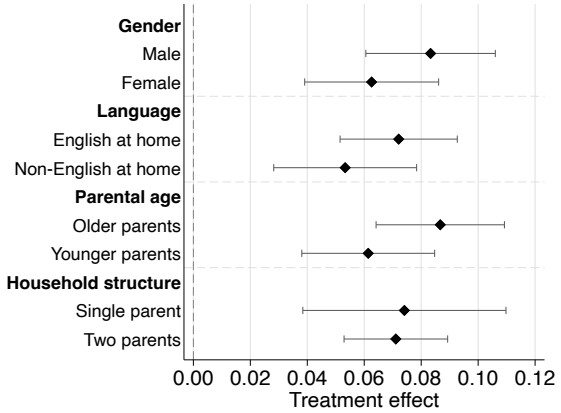
Panel A: Ever received CLB



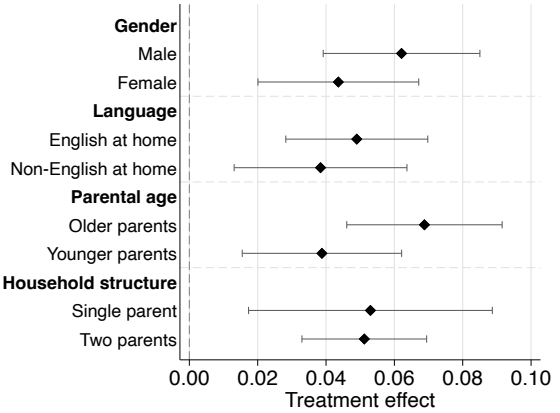
Panel B: Total CLB grants



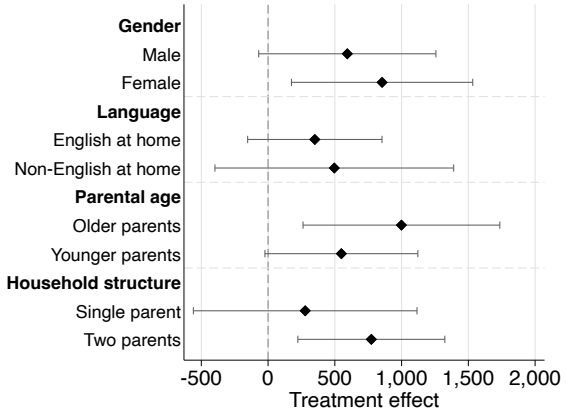
Panel C: Ever had an RESP account



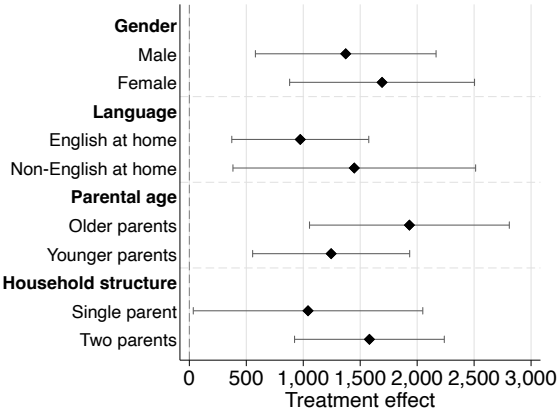
Panel D: Any parental contributions



Panel E: Total parental contributions ($)



Panel F: Total deposits (contr.+grants) ($)



Notes: This figure plots difference-in-discontinuities estimates of the effect of CLB eligibility on RESP outcomes, separately by subgroup. The sample is restricted to low-income children as defined in Section 3. All outcomes are cumulative and measured from birth up to age 17: indicators equal one if the event ever occurs, and amounts are cumulative totals expressed in 2024 Canadian dollars. Estimates correspond to Equation (1). Horizontal bars represent 95% confidence intervals based on heteroskedasticity-robust standard errors.

Table B2: Descriptive Statistics for the High-Income Sample

	Child birth date			
	Placebo group		Main group	
Variables	July-Dec 2002	Jan-June 2003	July-Dec 2003	Jan-June 2004
Male child	0.517	0.511	0.514	0.517
Speaks English at home	0.901	0.915	0.895	0.902
Self-declared as Indigenous	0.055	0.056	0.051	0.060
Parental age at child's birth	33.3	33.9	33.3	34.0
Observed with only one parent	0.005	0.005	0.003	0.005
Parental income in 2014 (2014 CA$)	168,500	162,900	161,800	158,900
Ever had an RESP account	0.773	0.785	0.783	0.799
Conditional on having an RESP account:				
Age of child at first RESP deposit	3.3	2.9	3.2	2.7
Total parental contributions ($)	23,600	24,400	23,800	24,300
Total CLB grants ($)	0	0	0	20
Total matching (CESG) grants ($)	4,500	4,600	4,500	4,600
Total deposits (contr.+grants) ($)	28,100	29,000	28,300	28,900
Enrolled in a bachelor's degree program	0.387	0.417	0.387	0.386
Conditional on bachelor's degree enrollment:				
Full-time enrollment	0.860	0.859	0.850	0.845
STEM field	0.380	0.405	0.401	0.389
Flagship university (U15)	0.398	0.398	0.375	0.396
Sample size	6,040	6,090	6,070	6,010

Notes: This table reports descriptive statistics for children in the high-income (ineligible) sample defined in Section 3, including socio-demographic characteristics, RESP savings, and bachelor's degree enrollment. Statistics are reported separately for four groups defined by the child's date of birth. The two columns on the left form the placebo group, and the two on the right form the main group, which includes the children born just before and just after the January 1, 2004, CLB cutoff. RESP outcomes are cumulative from birth through age 17, and enrollment outcomes are measured at age 18. Dollar amounts are in 2024 Canadian dollars, except parental income, which is in 2014 Canadian dollars. Sample sizes are reported in the last row and apply to all outcomes except those listed under the two conditional headings. The outcomes below "Conditional on having an RESP account" are computed among children with an RESP account. The outcomes below "Conditional on bachelor's degree enrollment" are computed among children enrolled in a bachelor's degree program.

Table B3: Effects of CLB Eligibility on Other (Non-RESP) Savings

			Alternative specifications		
Outcome	Baseline mean (1)	Main TE (RD-DD) (2)	3-month BW (3)	Linear MoB (4)	RD-DDD (5)
Panel A: Savings in TFSAs (tax-advantaged savings accounts)					
Any savings	0.495	-0.006	-0.008	-0.005	-0.002
		(0.006)	(0.008)	(0.012)	(0.009)
Total savings	9,700	104	-28	39	-1,293
		(332)	(476)	(686)	(918)
Market value in 2019	7,800	-206	-343	-84	-218
		(295)	(425)	(608)	(850)
Panel B: Savings in RRSPs (tax-advantaged retirement accounts)					
Any savings	0.554	-0.011*	-0.006	0.001	-0.010
		(0.006)	(0.008)	(0.012)	(0.007)
Total savings	22,100	-278	-581	-714	956
		(704)	(1,020)	(1,456)	(2,829)
Panel C: Savings in other taxable accounts					
Investment income in 2019	470	29	-83	-103	-199
		(86)	(92)	(142)	(246)
Sample size	32,060	126,300	61,670	126,300	191,390

Notes: This table reports the effects of Canada Learning Bond (CLB) eligibility on parents' non-RESP savings. The data come from the Longitudinal Administrative Data Bank (LAD), a 20% longitudinal random sample of Canadian tax filers. Unlike the other tables, which use British Columbia children, the sample here is nationwide: it consists of parents of low-income children born anywhere in Canada between July 2002 and June 2004. All outcomes are measured at the parental level rather than the child level. Panel A reports savings in Tax-Free Savings Accounts (TFSAs), Panel B reports savings in Registered Retirement Savings Plans (RRSPs), and Panel C reports investment income from non-registered taxable accounts. "Any savings" is an indicator equal to one if the parent ever contributed to the account. "Total savings" are cumulative contributions, net of withdrawals, from the child's birth through the year the child turns 17, expressed in 2024 Canadian dollars. TFSA market value and investment income are measured in calendar year 2019, when the children are aged 15 to 17, and are expressed in nominal terms. Column (1) reports baseline means for parents of children born between July and December 2003. Column (2) reports difference-in-discontinuities (RD-DD) estimates. Column (3) uses a three-month bandwidth. Column (4) allows separate linear trends in date of birth on each side of each cutoff. Column (5) reports difference-in-difference-in-discontinuities (RD-DDD) estimates. Sample sizes are rounded to the nearest 10. Heteroskedasticity-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

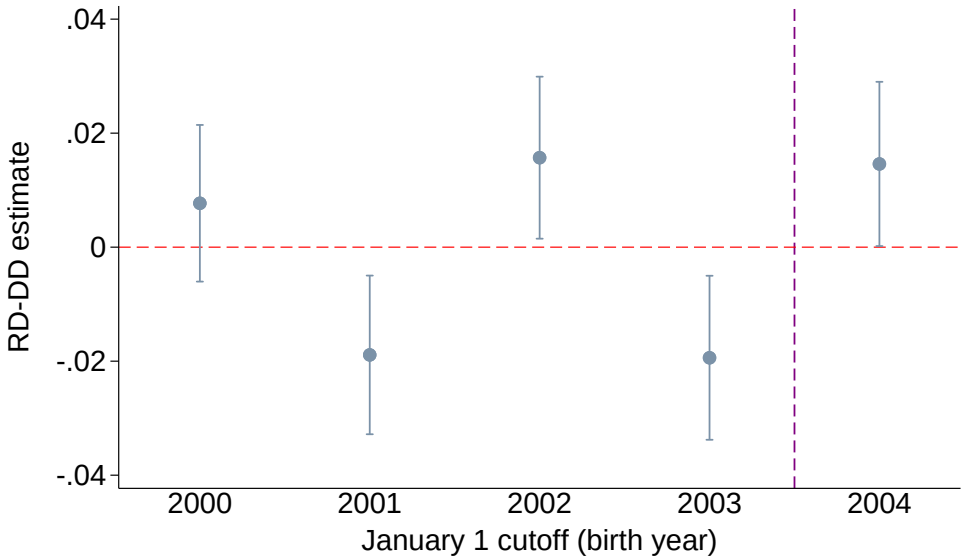


Figure B5: RD–DD Estimates of Bachelor's Enrollment at Age 18 by Birth-Year Cutoff

Notes: This figure reports difference-in-discontinuities (RD–DD) estimates for being enrolled in a bachelor's degree program at age 18. The sample is restricted to children from low-income families, as defined in Section 3. Each estimate compares the enrollment discontinuity at January 1 of the indicated birth year with the corresponding discontinuity one year earlier, using a six-month bandwidth on each side of each cutoff. The 2004 estimate corresponds to the actual CLB eligibility cutoff; estimates for 2000 through 2003 are placebo tests. The vertical dashed line separates the placebo cutoffs from the actual eligibility cutoff. Vertical bars show 95% confidence intervals based on heteroskedasticity-robust standard errors.

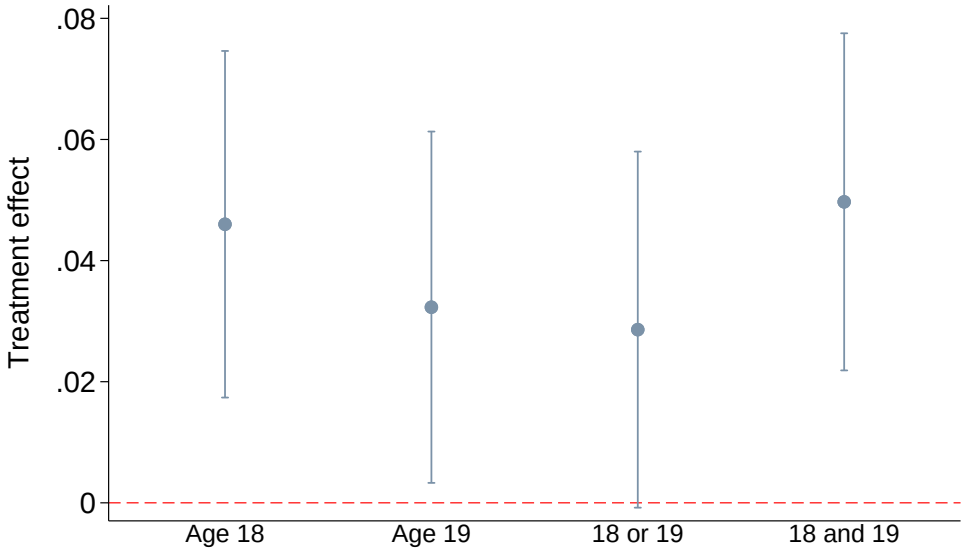


Figure B6: Effects of CLB Eligibility on Bachelor's Degree Enrollment by Age

Notes: This figure plots the effect of CLB eligibility on being enrolled in a bachelor's degree program at age 18, at age 19, at either age, and at both ages. The sample includes children from low-income and high-income families, as defined in Section 3. Coefficients are estimated using the difference-in-difference-in-discontinuities (RD–DDD) specification. Vertical bars show 95% confidence intervals based on heteroskedasticity-robust standard errors.

Table B4: Treatment Effects of CLB Eligibility on High School Outcomes

Outcome	Baseline mean (1)	Main TE (RD-DDD) (2)	With controls (3)	3-month BW (4)
Panel A: Course-taking and performance				
Any Grade-12 university-track class	0.69	0.014	0.010	0.007
		(0.012)	(0.012)	(0.018)
Number of Grade-12 university-track classes	1.5	0.151***	0.131***	0.112*
		(0.042)	(0.042)	(0.061)
Passed any Grade-12 university-track class	0.61	0.014	0.011	0.015
		(0.013)	(0.013)	(0.019)
Number of Grade-12 university-track classes passed	1.1	0.095**	0.080**	0.109**
		(0.037)	(0.037)	(0.054)
Any Grade-12 university-track class with an A	0.27	0.046***	0.041***	0.048**
		(0.015)	(0.015)	(0.021)
Number of Grade-12 university-track classes with an A	0.5	0.118***	0.108***	0.127***
		(0.033)	(0.033)	(0.048)
Panel B: High school graduation				
On-time graduation (by June of age 18)	0.63	0.012	0.007	0.010
		(0.012)	(0.012)	(0.018)
Sample size		80,440	80,440	39,130

Notes: This table reports the effect of CLB eligibility on high school course-taking, performance, and on-time graduation. The sample includes children from low-income and high-income families, as defined in Section 3. University-track courses include Biology 12, Chemistry 12, English 12, Physics 12, and Pre-Calculus 12. A course is considered taken if a final percentage grade is recorded, passed if the grade is at least 50%, and completed with an A if the grade is at least 85%. These outcomes are coded as zero if the student did not take the corresponding course. On-time graduation is defined as being eligible to graduate by June of the year the child turns 18. Column (1) reports baseline means for low-income children born between July and December 2003. Column (2) reports estimates from the main difference-in-difference-in-discontinuities (RD–DDD) specification (Equation (2)), using a six-month bandwidth on each side of the January 1 cutoffs. Column (3) adds controls for child gender, parental income category, single-parent status, language spoken at home, and kindergarten observation. Column (4) uses a narrower three-month bandwidth. Sample sizes are rounded to the nearest 10. Heteroskedasticity-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.