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

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Abstract

We provide the first large-scale evidence on the effects of delivering student grants through college savings accounts. We study the Canada Learning Bond (CLB), a federal program that deposits up to $2,000 into the college savings accounts of children from low-income families, with funds accessible only upon college enrollment. Using administrative microdata and a difference-in-discontinuities design around the date-of-birth eligibility cutoff, we identify the causal effects of CLB eligibility and present four main results. First, half of eligible children receive CLB transfers into a college savings account. Second, CLB eligibility triggers a significant parental saving response, more than doubling the effects on savings relative to the grant transfer alone. Third, CLB eligibility raises bachelor's degree enrollment by 4.6 percentage points. Fourth, bachelor's degree enrollment also rises for older siblings who receive no direct grant but whose parents open accounts and save on their behalf. Together, these findings suggest that the design of financial aid matters: delivering grants through savings accounts can generate responses that amplify program impacts beyond the transfer itself.

Keywords: College savings plans, student grants, Canada Learning Bond
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1 Introduction

Many countries invest substantial resources in student financial aid to support access to postsecondary education (OECD, 2020). Traditionally, such support takes the form of grants and loans disbursed at the time of college enrollment. In recent years, however, governments have begun experimenting with an alternative approach: delivering student grants through children's college savings accounts. Under this model, governments deposit funds in children's college savings accounts during childhood, with the funds only accessible upon postsecondary enrollment. This shift in delivery raises an important question: does it matter *how* financial aid reaches families, or is a dollar of aid equally effective regardless of how it is delivered?

While the effects of traditional student grant aid on college access are well documented, little is known about the effects of delivering student grants through college savings accounts (Dynarski et al., 2023). Theory cuts both ways. On the one hand, this approach may reduce uncertainty about future funding availability, shape expectations about children's educational trajectories, and trigger changes in household financial planning and saving behavior, potentially amplifying the grant's effectiveness beyond its face value. On the other hand, accessing these grants requires that families open and maintain a dedicated college savings account, creating administrative barriers that may limit take-up precisely among the lower-income households they aim to serve. The grant might also crowd out parental savings if families treat government contributions as a substitute for their own. Because these predictions point in opposing directions, the net effect of delivering grants through savings accounts remains an open empirical question.

In this paper, we provide the first large-scale evidence on the effects of delivering grants through college savings accounts. Specifically, we study the Canada Learning Bond (CLB), a federal program introduced in 2005 that deposits up to $2,000 (Canadian dollars) into the Registered Education Savings Plans (RESPs) of children from low-income families.

RESPs are tax-advantaged college savings accounts analogous to 529 plans in the United

States, designed to subsidize post-secondary education costs.[1] The CLB operates on top of this existing infrastructure. Three features of the CLB program are worth emphasizing. First, CLB funds are deposited into RESPs, which requires parents to actively open an account in order to receive the grant. At baseline, roughly 54% of eligible children have such an account. Second, CLB funds are only disbursed upon post-secondary enrollment: if the child never enrolls, the grant is returned to the government. The program thus effectively operates as a student grant, with the key difference that funds are committed years in advance and tied to RESP ownership. Third, unlike pre-existing matching grants that condition government support on families' own contributions, the CLB requires no parental saving beyond opening an RESP, ensuring that funds are available at enrollment independently of household finances.

We identify the effects of CLB eligibility using comprehensive administrative data from British Columbia that link children's school records to parental tax returns, RESP account records, and postsecondary enrollment records.[2] Our empirical strategy exploits the January 1, 2004, eligibility cutoff, comparing low-income children born just before and just after the threshold in a regression discontinuity difference-in-differences framework that removes systematic differences across months of birth.

We first document that roughly half of eligible children receive the CLB transfer into a college savings account. Take-up is incomplete primarily because receiving the grant requires that families open an account, and many eligible children do not have one. At the same time, eligibility increases account ownership by 7 percentage points, indicating that the program induces some families to open accounts specifically in order to receive the payment. These results stress both the administrative frictions inherent in account-based aid and the program's ability to draw new families into (potential) college saving.

Beyond take-up, we uncover a striking parental saving response. CLB eligibility increases

[1] Comparable vehicles exist in other countries, including Junior ISAs in the United Kingdom and Education Bonds in Australia.

[2] Linked school records are not available in other Canadian provinces. These linkages are essential for identifying individual children and measuring responses at the extensive margin, such as whether families open accounts or enroll in college.

the probability that parents make any voluntary RESP contribution by 5.3 percentage points and raises total cumulative parental contributions by \$1,030 on average. The magnitude of this response is economically large: each dollar of CLB transfers generates approximately \$1.40 in additional parental contributions, implying a sizable multiplier effect that more than doubles the direct impact of the program on RESP savings. This parental saving response is particularly notable for two reasons. First, the CLB does not require any parental contributions: families can receive the full transfer without saving at all. Second, these households are households who are unresponsive to pre-existing financial incentives, including matching grants of 20–40%. Together, these patterns suggest that the program activates saving behavior through non-price mechanisms, such as by reducing administrative barriers, increasing the salience of postsecondary education, or improving financial literacy.

We next investigate the effects on college enrollment and find that CLB eligibility increases bachelor's degree enrollment by 4.6 percentage points. To benchmark this magnitude, CLB eligibility increases the amount of grant funding available in a RESP at enrollment by \$974 on average, combining the CLB transfer with additional grants triggered by RESP participation. This implies an enrollment effect of roughly 4.7 percentage points per \$1,000 of aid available for college. The magnitude of this effect compares favorably to existing financial aid programs provided at the time of college entry (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).

To shed light on mechanisms, we next examine spillover effects on older ineligible siblings. Consider two families identical in all respects, each with an older and a younger child. In one family, the younger child is born just after the CLB eligibility cutoff; in the other, just before. Any difference in outcomes for the older siblings, who are themselves never eligible, can therefore be attributed to the causal effect of the younger sibling's eligibility on parental behavior. We show that parents of CLB-eligible children are 4 percentage points more likely to open and contribute to RESP accounts for older, CLB-ineligible siblings. Importantly, these behavioral responses translate into enrollment effects: older siblings who receive no CLB grant experience a 2 percentage point increase in bachelor's degree enrollment. This spillover is particularly interesting because these children receive no direct financial transfer: their gains therefore provide clean evidence that induced changes in parental behavior, and not just the grant itself, play an important role in expanding college access.

Our findings carry important implications for research and policy. First, we demonstrate that non-matching grants can activate saving behavior among low-income families who remain unresponsive to traditional financial incentives such as tax advantages and matching grants. The mechanism likely reflects behavioral frictions in RESP participation. This has implications for other domains, such as retirement accounts, where similar frictions may limit participation among lower-income households. Second, our results show that delivering grants through college savings accounts can generate behavioral responses that amplify program impacts beyond the transfer itself, underscoring the importance of delivery mode in shaping the effectiveness of student financial aid.

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; Abraham and Clark, 2006; Cornwell et al., 2006; Kane, 2007; Dynarski and Scott-Clayton, 2013; Angrist et al., 2022; Renée, 2025). Recent work has further shown that the delivery mechanism of financial aid matters independently of the amount, with early unconditional guarantees producing larger effects than equivalent aid delivered through standard processes (Dynarski et al., 2021; Burland et al., 2023). We address a related but distinct question, studying whether delivering grants through college savings accounts changes the effectiveness of aid relative to delivering aid directly at the point of college entry.

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; Briscese et al., 2025).[3] 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.

We further 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, raising concerns that these vehicles may exacerbate educational inequalities (Dynarski, 2004;

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

Milligan, 2004; 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, 2025). 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 and measurement of outcomes. Section 4 introduces the empirical design. Section 5 presents the results, and Section 6 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 program, we first provide background on these 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 1974 to encourage families to save for postsecondary education. These accounts are tax-advantaged: investment income accumulates tax-free while held in the account.[4] RESPs are opened by parents or other individuals with a designated child beneficiary who may later access the accumulated funds upon enrollment in

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

an approved postsecondary institution.[5]

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.[6]

Withdrawals from RESPs follow specific rules. When funds are withdrawn following postsecondary enrollment, the account balance is divided into two components with different tax treatment: original contributions are returned tax-free to the subscriber, while investment earnings and government grants (CESG and CLB) are paid to the beneficiary as Educational Assistance Payments (EAPs), which are taxed as income in the student's hands. EAP withdrawals are subject to initial caps. During the beneficiary's first 13 weeks of enrollment in a qualifying program, EAPs are limited to $8,000 for full-time students and $4,000 for part-time students. After this initial period, no dollar limit applies to EAP withdrawals.

Eligible institutions include Canadian universities, colleges, trade schools, and certain approved institutions outside Canada.[7] Both full-time and part-time programs qualify, provided they meet minimum requirements in terms of program duration and instructional hours.

If the beneficiary does not pursue postsecondary education, the subscriber may still withdraw the original contributions tax-free. However, investment earnings are then subject to the subscriber's marginal income tax rate plus a 20% surtax, and government grants are

[5] 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 Canada, 2015).

[6] In 2024, families with adjusted incomes up to $49,020 are eligible for the 20% additional match, while those with incomes between $49,020 and $98,044 are eligible for the 10% additional match.

[7] In Quebec, this includes Cégeps (*Collèges d'enseignement général et professionnel*), which are pre-university and technical colleges unique to the province.

repaid to the government.

2.2 Canada Learning Bond

The Canada Learning Bond (CLB) is an additional grant introduced by the Government of Canada in 2005 to help low-income families cover the cost of postsecondary education. 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.

The program is available only to children born in 2004 or later, which motivates our empirical analysis below. Beyond the date-of-birth restriction, eligibility for the CLB is determined by family income and is assessed annually from birth until age 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 a RESP. Caregivers must also file any required CLB consent forms, which generally need to be submitted only 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.[8]

2.3 Potential Effects of the CLB

The CLB may affect children's college enrollment through several channels.

First, the CLB directly increases the resources available for postsecondary education. This additional balance may relax financial constraints at the time of enrollment, similarly to grants provided at the point of college entry (Dynarski and Scott-Clayton, 2013; Dynarski et al., 2023). A distinctive feature of the CLB relative to traditional aid delivered at entry, however, is that the value of the grant is known well in advance of enrollment. Prior work suggests that this certainty about future aid is itself powerful: early unconditional guarantees have been shown to increase enrollment more than equivalent aid that is only revealed at the time of enrollment (Dynarski et al., 2021; Burland et al., 2023). In addition, seeing funds set aside for college from an early age may more strongly shape aspirations and increase effort and preparation well before the enrollment decision is made (Destin and Oyserman, 2009; Elliott, 2009; Elliott et al., 2011).

Second, the CLB may affect enrollment indirectly by changing parental saving behavior. Because parental savings, and particularly savings labeled for college, are strongly associated with college enrollment (Elliott, 2013; Nam and Ansong, 2015; Frenette, 2017; Vasilenko, 2025), any effect of the program on private contributions could amplify or dampen its direct impact on educational outcomes. Whether such an effect exists is theoretically ambiguous. The grant may crowd out private savings if households treat the government transfer as a substitute for their own contributions (Attanasio and Brugiavini, 2003; Jensen, 2004). Alternatively, the CLB may have no effect on private contributions, since receipt of the grant

[8] 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.

is entirely independent of parental saving. Finally, the CLB may crowd in private savings through behavioral channels, such as reducing administrative barriers to account participation or improving financial literacy. Such responses are plausible given evidence that household saving is highly sensitive to program design and participation frictions (Madrian and Shea, 2001; Saez, 2009; Chetty et al., 2014; Briscese et al., 2025).

3 Data and Sample

3.1 Data Sources and Outcomes

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

Our primary dataset, the British Columbia Kindergarten to Grade 12 (BC K–12) dataset, covers the universe of students who attended public or independent schools in British Columbia between 1991 and 2020. 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 K–12 data to parental tax records from the T1 Family File (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 successfully 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.[9] We further exclude 3% of children who are matched to more than two parents.

We further 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, which can be made from birth to age 15, 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.[10] Although the CLB can also be used 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

[9]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.

[10]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: RESP outcomes (ages 0–17)	
Ever had an RESP	Indicator equal to one if an RESP was ever opened in the child's name.
Any CLB	Indicator equal to one if the child ever received the Canada Learning Bond.
CLB grant amount	Cumulative Canada Learning Bond amount received, expressed in 2024 Canadian dollars.
Any parental contribution	Indicator equal to one if parents or other relatives ever contributed to a RESP account in the child's name.
Parental contribution amount	Cumulative parental and other relatives' RESP contributions to the child's RESP accounts, expressed in 2024 Canadian dollars.
CESG grant amount	Cumulative Canada Education Savings Grant amount received, expressed in 2024 Canadian dollars. Includes the A–CESG.
Total contributions and grants	Total cumulative parental contributions and government grants (CLB + CESG) received, expressed in 2024 Canadian dollars. This measure does not capture total account balances, as interest earnings are not observed.
Panel B: Bachelor's degree enrollment outcomes	
Any enrollment	Indicator equal to one if the individual ever enrolled in a bachelor's degree program at age 18.
Full-time enrollment	Indicator equal to one if the individual enrolled full time in a bachelor's degree program at age 18.
STEM field	Indicator equal to one if the individual enrolled in a STEM field within a bachelor's degree program at age 18.
U15 university	Indicator equal to one if the individual enrolled in a U15 university at the bachelor's level at age 18. The U15 is a group of Canada's largest research-intensive universities and includes, in British Columbia, the University of British Columbia.

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

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

Our sample is defined by children's dates of birth, following the empirical design described below. The exact birth-cohort window varies across specifications. In the baseline specification, we focus on children born between July 2002 and June 2004, yielding 94,470 children.

Within this window, we identify children who are eligible for the CLB if born in 2004, or who would have been eligible had the program applied to earlier cohorts. We determine income eligibility as follows.

Income Eligibility Determination. We proceed in two steps. First, for each child and for each year of potential CLB eligibility (ages 0 to 14), we determine whether the child is income-eligible for the CLB. This determination requires two components: parental income, which we obtain from tax records, and the income eligibility cutoff for that year, which we retrieve following a data-driven procedure.[11] A child is classified as income-eligible in a given year if total parental income falls below the applicable cutoff for that year. We apply this procedure for all years of potential eligibility (ages 0 to 14) for each child in our sample.

For children born in 2004 or later, we use the actual CLB eligibility cutoffs defined by the program. For children born before 2004, we assess what their eligibility would have been had the CLB applied to these cohorts as well: we use the actual program cutoffs for years when they existed (2004 onward) and backfill cutoffs for earlier years by taking the first official 2004 cutoff and adjusting it backward for inflation. In some cases, eligibility cannot be determined with certainty, for example, when a parent's tax return is missing for a given year or when parental separation implies that eligibility depends on the income of a new partner whose tax information we do not observe. In such cases, we classify eligibility for that year as ambiguous.

Second, we aggregate each child's yearly eligibility status across childhood and classify

[11] Since exact CLB income thresholds are not available in the administrative data for all years and family sizes, we identify the applicable cutoff for each year by computing CLB receipt rates across $100 income bins and selecting the income level with the largest discrete change in receipt rates (Porter and Yu, 2015).

children into three mutually exclusive groups: (i) at least once eligible, (ii) never eligible, and (iii) ambiguous.[12]

Among children born between July 2002 and June 2004, 60% are classified as at least once eligible for the CLB, 26% as never eligible, and 15% as having ambiguous eligibility status. For simplicity, throughout the remainder of the paper, we refer to children who are at least once eligible for the CLB (or would have been at least once eligible had the program been in place for earlier cohorts) as "low-income children" or the "low-income group." Similarly, we refer to never-eligible children as "high-income children" or the "high-income group."

Main Sample. Our main analysis focuses on the low-income (eligible) group, which includes 56,230 children. The high-income (non-eligible) group is used for placebo tests and for the difference-in-difference-in-discontinuities design. Children with ambiguous eligibility are excluded throughout. Aside from date of birth and parental income, no additional sample restrictions are imposed.

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. Statistics are shown separately for four subsamples corresponding to the four dimensions of our main empirical design.

The first part of Table 2 summarizes children's socio-demographic characteristics. Focusing on a few key dimensions, approximately 36% of children speak a language other than English at home, around 14% 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

[12]Children classified as at least once eligible may have some years with ambiguous eligibility, while children classified as never eligible are never eligible in any year. The ambiguous group consists of the remaining children.

Table 2: Descriptive Statistics for the Low-Income Sample

	Child birth date			
	Placebo group		Main group	
Variabe	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	0.211	0.218	0.207	0.202
Parental income in 2014 (2014 CA$)	56,700	56,300	56,900	56,900
Ever had an RESP account	0.545	0.546	0.543	0.617
Age of child at first RESP opening	5.1	4.7	5.1	4.8
Total parental contributions (2024 CA$)	20,600	20,700	20,500	19,800
Total CLB (2024 CA$)	0	0	0	1,200
Total CESG (2024 CA$)	4,200	4,200	4,200	4,100
Total contributions and grants (2024 CA$)	24,800	25,000	24,700	25,100
Enrolled in a bachelor's degree program	0.258	0.258	0.243	0.258
Full-time enrollment	0.810	0.814	0.798	0.814
STEM field	0.426	0.426	0.424	0.407
U15 university	0.473	0.438	0.412	0.419
Sample size	14,470	13,930	14,040	13,790

Notes: The table reports means for children's socio-demographic characteristics and for the main outcomes related to parental savings in RESPs and college enrollment. Statistics are reported for the main analysis sample, split into four groups defined by the child's date of birth. These groups correspond to those used in the main empirical specification. Sample sizes are reported in the last row and apply to all outcomes, except for outcomes listed below "Ever had an RESP account," which are computed conditional on having an RESP, and for outcomes listed below "Enrolled in a bachelor's degree program," which are computed conditional on enrollment in a bachelor's degree program.

$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 opening is about 5 years. Conditional on having an RESP, average parental contributions are approximately $20,500, and children receive on average about \$4,200 through the Canada Education Savings Grant 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, 2004; Frenette, 2017; Bonikowska and Frenette, 2020). Turning to four-year college enrollment outcomes: approximately 25% of children enroll in a bachelor's degree program at age 18, which is slightly smaller than the 29% enrollment rate in the full British Columbia population (Frenette and Handler, 2025). Conditional on enrollment, roughly 80% enroll full-time, about 42% choose a STEM field, and around 44% attend a U15 research-intensive university.

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, children born just before and just after the cutoff are highly comparable, with no significant differences in gender, language spoken at home, Indigenous status, early grade observation, 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.

The intuition is straightforward: we estimate the discontinuity in outcomes at the January 1st, 2004 eligibility cutoff, then subtract the analogous January 1st discontinuity observed in an earlier cohort unaffected by the CLB. This differencing removes any systematic January-December differences unrelated to CLB eligibility, whether 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, which balances two considerations: including 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 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.

[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.

[16] This is a distinctive feature of the RD-DD design relative to a standard RD design. In a 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.

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. The results of these tests are discussed in Section 5.

A second assumption is that there is no manipulation of birth timing around the cutoff. This is unlikely in our context because the CLB program was announced as part of the 2004 Canadian federal budget, which was presented in March 2004 and applied retroactively to all births starting from January 1st, 2004. As such, parents whose children were 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.

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 (Bird et al., 2022).[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). The key insight is that these children experience the same cohort-level shocks as their low-income peers: 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 weaker stability assumption than that required by the baseline RD-DD specification. We estimate the following equation using a combined 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

[17] To see why this matters, consider the comparison in our RD-DD design. 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.

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 5.2, we discuss placebo tests using fake treatment cutoffs between 2000 and 2003 that further support the validity of the RD–DDD design for these college enrollment outcomes.[18]

5 Results

We present our results in three parts. First, we examine the effects of CLB eligibility on savings held in RESP accounts, including both CLB receipts and parental contributions. We then study the effects on college enrollment. Later, we investigate the same outcomes for older, ineligible siblings, which provides additional insights into the mechanisms underlying these effects.

5.1 RESP Savings

Results on RESP savings are organized in two complementary ways. Figure 1 displays average outcomes by children's month of birth for six key RESP-related variables to visually assess the discontinuity at the eligibility cutoff. Table 3 then provides formal regression estimates: column (2) presents our preferred RD-DD specification, while the remaining columns present a range of robustness and placebo checks.

CLB Take-up. We begin by examining the extent to which eligible families receive the CLB. We measure take-up from birth through age 17, capturing the full period during which children can receive CLB deposits prior to typical college entry. Panels A and B of Figure 1 plot receipt rates and average cumulative grant amounts by children's month of birth.

[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.

Consistent with program rules, children born prior to 2004 receive no CLB transfers, while those born just after the January 2004 cutoff receive transfers at high rates.

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 a RESP account, and a substantial share of eligible families do not do so. In fact, only about 62% of eligible children have a 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 a 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 between two types of families in terms of take-up: *incidental recipients*, who would have opened a 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. To estimate the size of this second group, we examine the treatment effect of CLB eligibility on RESP account ownership. Among cohorts born prior to 2004, RESP participation among low-income families is already substantial, around 55%, as shown in Figure 1. This relatively high baseline likely reflects pre-existing incentives, notably the CESG introduced in 1998. Participation nevertheless increases sharply at the eligibility cutoff. Our baseline estimate in Table 3 indicates that CLB eligibility raises the probability that a child ever holds a RESP account by 7.3 percentage points, suggesting that the program induces entry into the RESP system. In other words, among the 49% of children who receive the CLB, approximately 7 percentage points are *active recipients*, while the remaining 42 percentage points are *incidental recipients*.

Table 3 also shows that, on average, eligible children receive about $740 in cumulative CLB deposits. This implies that recipients receive approximately $1,500 in total CLB transfers.

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

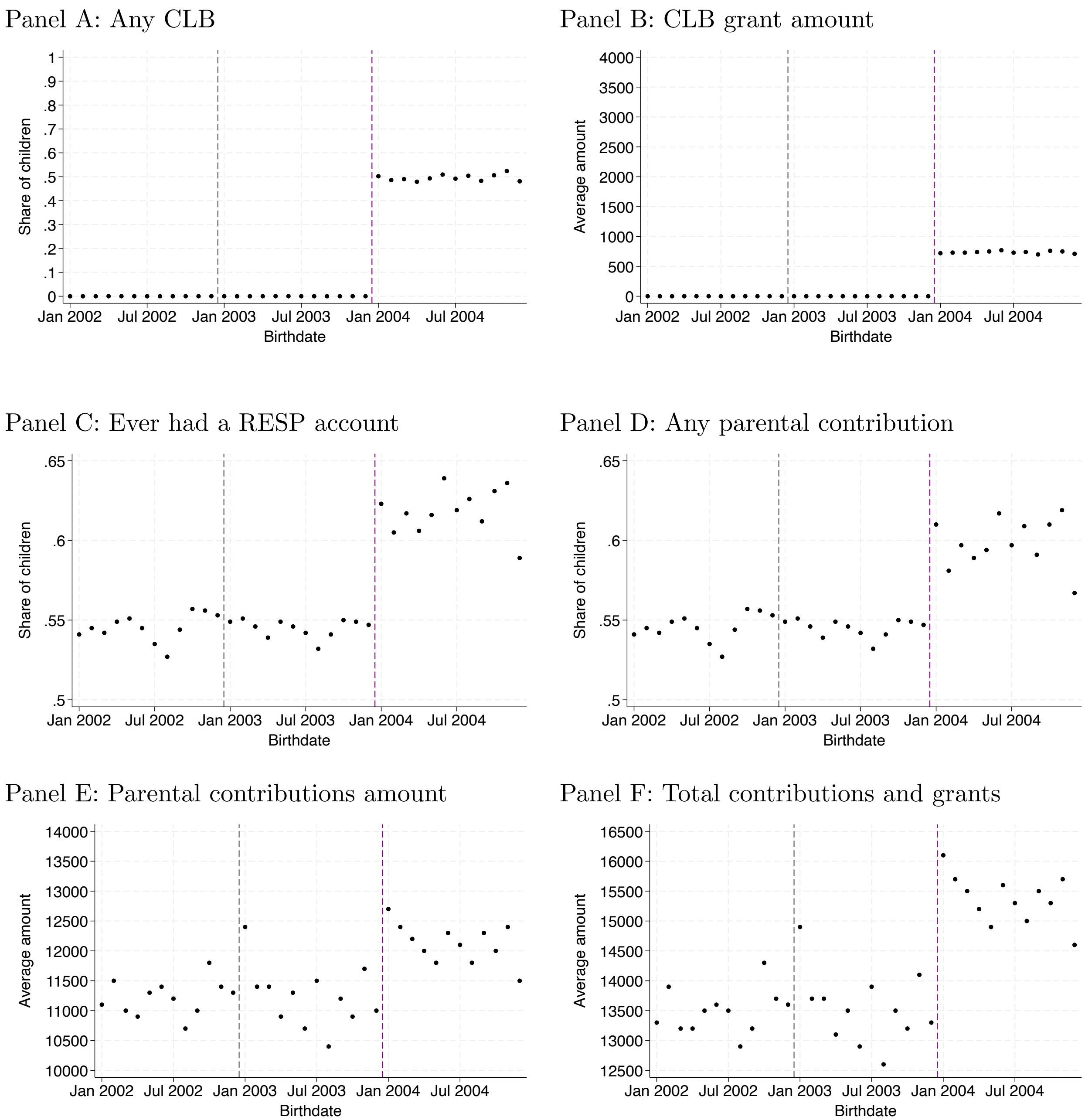

Figure 1: Treatment Effects on RESP Savings

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 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. The vertical dashed lines mark January 1st, 2003 (placebo cutoff) and January 1st, 2004 (CLB eligibility cutoff).

Table 3: Treatment Effects on RESP Savings

			Alternative specifications			
Outcome	Baseline mean (1)	Main RD-DD (2)	3-month BW (3)	Linear MoB (4)	RD-DDD (5)	High-income (6)
Panel A: RESP account ownership and CLB receipt						
Any CLB	0.000	0.492***	0.492***	0.488***	0.467***	0.025***
		(0.004)	(0.006)	(0.009)	(0.005)	(0.002)
CLB grant amount ($)	0.000	738***	727***	713***	721***	17***
		(7.6)	(10.6)	(15.2)	(7.7)	(1.5)
Ever had a RESP account	0.543	0.073***	0.076***	0.074***	0.068***	0.006
		(0.008)	(0.012)	(0.017)	(0.013)	(0.011)
Panel B: Parental contributions						
Any parental contribution	0.543	0.053***	0.057***	0.058***	0.051***	0.002
		(0.008)	(0.012)	(0.017)	(0.013)	(0.011)
Parental contributions amount ($)	11,100	1,030***	1,068***	823	672	358
		(271)	(390)	(561)	(563)	(493)
Panel C: Other RESP amounts						
CESG grant amount ($)	2,300	236***	231***	182*	179*	56
		(53)	(76)	(109)	(104)	(90)
Total contributions and grants ($)	13,400	2,004***	2,026***	1,718**	1,572**	432
		(324)	(467)	(671)	(666)	(582)
Sample size		56,230	27,660	56,230	80,430	24,200

Notes: This table shows the effects of CLB eligibility on RESP outcomes. 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. The sample is restricted to low-income children as defined in Section 3. Column (1) reports baseline means for children born between July and December 2003. Column (2) reports difference-in-discontinuities estimates that compare outcomes for children born just after versus just before January 1, 2004, and subtract the corresponding January 1 discontinuity observed for a placebo group centered around January 1, 2003, using a symmetric six-month bandwidth (Equation (1)). Column (3) uses a narrower three-month bandwidth, while column (4) allows for group-specific linear functions of date of birth. Column (5) reports difference-in-difference-in-discontinuities estimates that additionally difference out school-cohort–specific shocks using children from high-income (ineligible) families as an additional control group (Equation (2)). Column (6) reports a placebo test based on estimating Equation (1) for children from high-income (ineligible) families. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This is somewhat below the program maximum of $2,000, reflecting that not all children remain income-eligible over the full 15-year period during which benefits can accrue.

In Figure B2, we examine the dynamic evolution of CLB transfers over childhood. Panel A shows that CLB amounts accumulate gradually from early childhood through age 15. The increase begins around age 2, consistent with the program's launch in 2005 and its retroactive eligibility for children born in 2004. Transfers then rise steadily over childhood before plateauing after age 15, which reflects the program design, as eligibility for new CLB deposits ends at that age.

Parental Contributions. We next investigate the effects of CLB eligibility on private parental savings in RESPs. Three scenarios are possible. First, the CLB may crowd out private savings if parents treat the government transfer as a substitute for their own contributions and reduce savings accordingly. Second, the CLB may have no effect on private contributions. This will be the case if: (1) families induced to open an RESP (*active recipients*) do so solely to access the grant and (2) inframarginal families (*incidental recipients*) maintain the saving behavior they would have exhibited absent the program. Both conditions are plausible given that CLB receipt is entirely independent of parental contributions. Finally, the CLB may crowd in private savings through behavioral channels (such as inducing households to overcome administrative barriers to participation, improving financial literacy, and shifting parental expectations) even though stimulating parental savings was not an explicit policy objective.

We present the estimated results in the second panel of Table 3. The empirical evidence strongly supports the third scenario, as the CLB clearly crowds in private savings. Specifically, we find that CLB eligibility increases the probability that parents make any voluntary RESP contribution by 5.3 percentage points. Comparing this to the 7.3 percentage point increase in RESP account ownership reported above, it implies that 72% of parents induced to open an RESP by the CLB also voluntarily contribute to it. Panel D of Figure 1 illustrates this pattern graphically, showing a clear jump at the January 2004 cutoff, slightly smaller than the discontinuity in account ownership, and smooth trends on either side, supporting the validity of the research design.

Beyond the extensive margin, we examine effects on contribution amounts. CLB eligibility increases total parental contributions by $1,030 on average, a 9% increase relative to the baseline mean of $11,100. Since 49% of eligible children receive the CLB, the overall saving response implies a treatment-on-the-treated effect of approximately $2,100 for children who actually receive the benefit. This total effect is not necessarily only driven by families who would not have contributed in the absence of the program (the 5.3 percentage point extensive margin response), as adjustments in contribution levels among families who would have contributed regardless (intensive margin) are also plausible.

Panel E of Figure 1 provides graphical evidence of this effect, with average cumulative contributions jumping from approximately $11,000 to $12,000 at the eligibility cutoff. The figure also shows a small discontinuity at the January 2003 placebo cutoff, motivating the RD-DD design. Figure B3 further shows the distribution of treatment effects across contribution bins: most of the increase is concentrated among children with contribution levels up to $7,500, though increases are observed across the distribution, including at higher amounts. Lastly, Figure B2 shows that parental contributions increase progressively throughout childhood. This suggests that the saving response reflects sustained parental engagement over time rather than a one-time adjustment.

Total RESP Savings. So far, we have shown that eligible children receive on average $738 in CLB grants and that CLB eligibility generates $1,030 in additional parental contributions. Because CLB eligibility increases parental savings, it also increases receipt of matching grants through the Canada Education Savings Grant (CESG) program, as can be seen in Panel C of Table 3.

Combining all components, CLB eligibility increases total contributions and grants by $2,004 by age 17, a 15% increase relative to the baseline mean of $13,400. This implies a treatment-on-the-treated effect of approximately $4,000 for children who actually receive the benefit. Note that our data capture contributions and government grants (CLB, CESG, and Additional CESG) but not investment earnings on these balances. This figure therefore provides a lower bound on the total increase in RESP resources, as it excludes any investment returns earned on the accumulated funds.

Validity and Robustness Checks. Columns 3–5 of Table 3 assess the robustness of our estimates across alternative specifications. The estimated impacts on RESP account ownership and parental contribution rates are robust and consistent across these specifications. Effects on contribution amounts are more sensitive, ranging from \$672 in the RD–DDD specification to \$1,068 using the narrower bandwidth, reflecting the greater noise in this outcome. Yet, altogether, the point estimates remain economically significant across specifications.

We also estimate the effects on high-income families as a placebo test (column 6). We find no significant response in RESP account opening or parental contributions among this group, supporting the interpretation that the main results are driven by CLB eligibility and that high-income families provide a valid comparison group for the analysis that follows. That said, we do find a small but statistically significant CLB receipt rate of 2.5 percentage points among ineligible children, which likely reflects measurement error in income classification or administrative targeting errors. Reassuringly, this rate is low small and does not translate into any detectable behavioral response.

Table B2 presents additional placebo tests using fake treatment cutoffs between 2000 and 2003 to further assess the validity of our design. Across all savings-related outcomes, the placebo estimates are small and statistically indistinguishable from zero. One exception is total contributions under the fake January 1, 2001 cutoff, which yields a marginally significant estimate. Given the number of placebo tests conducted, this isolated result is consistent with what would be expected by chance.

Heterogeneity. Figure B4 presents the CLB and savings estimates separately by child gender, language spoken at home, parental age at birth, and household structure. Across all outcomes, the estimates are broadly stable across subgroups. CLB take-up rates and grant amounts are similar for boys and girls, for English- and non-English-speaking households, for older and younger parents, and for single- and two-parent families. The same stability holds for the extensive margin responses on RESP account opening and parental contributions. Point estimates for contribution amounts and total resources show somewhat more variation across subgroups, but the confidence intervals overlap in all cases. Overall, the patterns suggest that the responses to CLB eligibility documented in the previous sections are not concentrated among particular family types.

Source of Additional Parental Savings. A natural question is where these additional savings originate: do they reflect genuinely new savings, or a reallocation from other household accounts? To investigate this, we turn to additional tax-return data, which provide comprehensive information on contributions to tax-advantaged registered accounts as well as investment income from non-registered accounts.[20] Using the same difference-in-discontinuities design, we compare cumulative savings and contributions from birth through age 17, along with 2019 account balances, for parents of children born just before versus just after January 1, 2004. We find effects that are small relative to baseline means and statistically insignificant across all savings vehicles (Table B3), although confidence intervals are wide enough that we cannot rule out small offsets. Taken together, the results provide little evidence that CLB-induced RESP savings crowd out other forms of household saving, suggesting that the increase in RESP savings primarily reflects new saving rather than a reshuffling of existing financial 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.

[20] Specifically, we use the Longitudinal Administrative Data Bank (LAD), a 20% longitudinal random sample of all Canadian tax filers. In this dataset, we observe contributions to Tax-Free Savings Accounts (TFSAs) which are tax-advantaged general savings accounts similar to Roth IRAs in the United States. We also observe contributions in Registered Retirement Savings Plans (RRSPs) 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.

Discussion. The evidence above indicates that CLB eligibility activates saving behavior, not merely account opening. This finding is notable for two reasons. First, the CLB does not require any parental contributions: families can receive the full transfer without saving at all. Second, these are households that are unresponsive to pre-existing financial incentives, including matching grants of 20–40% available through the CESG. Together, these patterns suggest that the program operates through mechanisms beyond simple price incentives.

Although our empirical design does not allow us to isolate the precise mechanisms underlying this effect, the results are consistent with several complementary channels. By offering a transfer conditional only on account opening, the CLB provides an immediate and salient incentive to enter the RESP system. Once accounts are opened, administrative barriers are reduced, potentially facilitating ongoing engagement. Account ownership may also increase exposure to financial products and improve financial literacy. Finally, the presence of a dedicated education savings account with an initial balance may increase the salience of postsecondary education and shift parental expectations, further encouraging saving. [21] This contrasts with matching grants, which require households to contribute in order to benefit and may therefore be less effective among families who are uncertain about their ability to save consistently or have more limited financial knowledge.

The magnitude of the saving response is economically large. Each dollar of CLB transfers generates approximately \$1.40 in additional parental contributions, implying a sizable multiplier effect that more than doubles the direct impact of the program on RESP savings. Because induced contributions also trigger CESG matching grants, and because investment returns accumulate over the child's early years, the total amplification of the program's impact on RESP balances is larger still.

Overall, we estimate that treated children accumulate, on average, \$4,000 more in RESP

[21] A potential alternative channel is differential exposure to information about the CLB, whether through targeted information campaigns or financial advice. We view this explanation as unlikely. A broad information campaign would likely generate a discrete jump in account openings or contributions at a specific point in time, which we do not observe (see Figure B2). Financial institutions, for their part, had no differential incentive to promote RESPs on either side of the eligibility cutoff, since children on both sides qualified for the same CESG matching grants.

assets, before accounting for investment returns. To put this magnitude in context, average annual university tuition in British Columbia in 2025 was approximately $5,800, implying total tuition costs of roughly $23,200 for a four-year degree (BC Ministry of Post-Secondary Education and Future Skills, 2026). The treatment-on-the-treated effect therefore represents an important contribution toward these costs. In addition, average student loan balances at repayment consolidation in Canada were around $14,000 in 2010–2011 (Lochner et al., 2021), indicating that the program offsets a non-trivial share of typical borrowing needs.

Moreover, the CLB significantly narrows the gap in income gap in college savings shown in Tables 2 and B1. For instance, while the difference in RESP ownership for children in the pre-CLB birth cohorts equals 24 percentage points, this gap falls to 16.7 percentage points for children born in 2004. Similarly, we find that the gap in parental savings also falls by 11% (from $9,700 to $8,700), and the total college-resources gap falls by 18%. Taken together, these comparisons indicate that the program generates economically meaningful improvements in the financial resources available to children from low-income families at the point of post-secondary enrollment.

5.2 Effects on College Enrollment

We now turn to examining whether these increases in education savings, and the potentially associated changes in college aspirations, translate into higher college enrollment. Specifically, we focus on bachelor's degree enrollment at age 18, which corresponds to the typical age of college entry. We focus on bachelor's degree enrollment given the large labor-market returns associated with completing a bachelor's degree in Canada (Boothby and Drewes, 2006; Frenette, 2014). While CLB funds can also be used for enrollment in short-cycle programs, we do not reliably observe enrollment in such programs. To the extent that CLB eligibility also encourages enrollment in short-cycle programs, our estimates should be interpreted as a lower bound on the overall effect on postsecondary enrollment.

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 and could confound a standard RD-DD approach. To address this concern, we incorporate a third difference using high-income families, who are never eligible for the CLB, to net out any cohort-specific changes in enrollment unrelated to the program. This approach allows us to isolate the causal effect of CLB eligibility on college enrollment under weaker assumptions about cohort trends.

Table 4 presents the estimated treatment effects. CLB eligibility increases bachelor's degree enrollment by 4.6 percentage points, a 19% increase relative to the baseline. The effect on full-time enrollment is similar in magnitude. We also observe higher enrollment in STEM fields and at research-intensive universities (U15). We find that STEM enrollment increases by 2.7 percentage points (27% relative to baseline), and enrollment in research-intensive institutions increases by 2.2 percentage points, which represents a 22% increase relative to baseline.

Panel B examines outcomes *conditional* on enrollment. Across all program and institution types, the estimated effects are small and statistically insignificant. This pattern indicates that the unconditional increases in full-time, STEM, and U15 enrollment are not accompanied by detectable changes in program choice conditional on enrolling. This is consistent with an interpretation that students induced to enroll by the CLB choose programs similar, on average, to those chosen by other enrollees, suggesting that the CLB primarily affects the likelihood of bachelor's degree entry, with limited observable effects on program composition among enrolled students.

We validate the RD–DDD design by estimating the same specification using fake treatment cutoffs between 2000 and 2003, as presented in Table B4. The resulting estimates are centered near zero, with the number of statistically significant effects close to what would be expected by chance. These placebo estimates are also markedly closer to zero than their RD–DD

Table 4: Treatment Effects on Bachelor's Degree Enrollment

Outcome	Baseline mean (1)	Main RD-DDD (2)	With controls (3)	3-month BW (4)	RD-DD (5)
Panel A: Unconditional enrollment outcomes					
Any enrollment	0.243	0.046***	0.038***	0.048**	0.016**
		(0.015)	(0.014)	(0.021)	(0.007)
Full-time enrollment	0.194	0.043***	0.037***	0.050**	0.016**
		(0.014)	(0.014)	(0.020)	(0.007)
STEM field	0.103	0.027**	0.023**	0.040***	0.003
		(0.011)	(0.011)	(0.015)	(0.005)
U15 university	0.100	0.022**	0.018*	0.017	0.018***
		(0.011)	(0.011)	(0.015)	(0.005)
Sample size		80,430	80,430	39,120	56,230
Panel B: Conditional on bachelor's degree enrollment					
Full-time enrollment	0.800	0.017	0.018	0.033	0.014
		(0.020)	(0.019)	(0.029)	(0.013)
STEM field	0.420	0.017	0.017	0.056	-0.013
		(0.026)	(0.026)	(0.038)	(0.017)
U15 university	0.410	0.025	0.023	0.005	0.046***
		(0.026)	(0.026)	(0.038)	(0.017)
Sample size		80,430	80,430	39,120	56,230

Notes: This table reports the effects of CLB eligibility on bachelor's degree enrollment outcomes for the main sample of children. All outcomes are measured at age 18, the typical age of college entry. The sample is restricted to children from low-income families as defined in Section 3. Column (1) reports baseline means for children born between July and December 2003. Column (2) reports difference-in-difference-in-discontinuities (RD–DDD) estimates corresponding to Equation (2). Column (3) adds controls for child gender, parental income category, single-parent household status, language spoken at home, and an indicator for being observed in kindergarten. Column (4) uses a narrower bandwidth of three months on either side of the January 1 cutoff. Column (5) reports difference-in-discontinuities (RD–DD) estimates. Panel A reports unconditional enrollment outcomes. Panel B reports outcomes conditional on bachelor's degree enrollment. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

counterparts (Table B2), consistent with the view that the additional differencing removes residual cohort-specific trends that may contaminate simpler specifications for enrollment outcomes.

We next examine robustness across alternative specifications. Adding controls for observable characteristics or using a narrower three-month bandwidth produces effects that remain close to the baseline estimates. In particular, the effect on bachelor's degree enrollment ranges from 3.8 to 4.8 percentage points across the RD–DDD specifications, and the estimates for other outcomes show similarly modest variation while remaining qualitatively consistent. For comparison, the last column presents the estimates from the simpler RD–DD model. These estimates are also positive and statistically significant, but substantially smaller in magnitude. As discussed above, they should be interpreted with caution, since the RD–DD design does not account for cohort-specific shocks to college enrollment that may coincide with the eligibility cutoff. Overall, the results consistently indicate that CLB eligibility increases college enrollment among eligible children. While the precise magnitude varies somewhat across specifications and depends on identifying assumptions, the direction and economic significance of the effects are robust.

Importantly, the CLB significantly narrows the persistent income gap in bachelor's degree enrollment. The pre-existing difference in enrollment rates between high- and low-income children amounted to 14 percentage points (see Tables 2 and B1), and our estimates suggest that the CLB closed about one-third of this gap. As such, delivering student aid through college savings accounts may help close the income gradient in college attendance (Dynarski et al., 2023).

Comparison with Traditional Financial Aid. The magnitude of these effects compares favorably to estimates from traditional grant programs. Eligible children in our sample receive, on average, about $974 in CLB and other grant funds deposited in their RESP accounts by age 17, which are accessible only upon enrollment. This implies an enrollment effect of roughly

4.7 percentage points per $1,000 of potential government aid. A large literature, primarily based on U.S. programs, suggests 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). Since students typically receive aid for multiple years, the implied effect per dollar of aid eligibility from these programs is substantially lower. For example, assuming students receive aid for three years on average, the upper end of these estimates (6 percentage points per $1,000 annually) translates to 2 percentage points per $1,000 in total aid eligibility.

However, this comparison uses the government transfer as the denominator. The CLB also induces substantial additional parental saving, which increases the total resources available at enrollment. When we instead scale the enrollment effect by the total increase in savings ($2,004 plus accumulated returns), the implied effect per $1,000 of additional resources is smaller and more closely aligned with estimates from traditional grant programs.

Taken together, these comparisons suggest that the CLB appears particularly effective when evaluated relative to government spending, but its impact relative to total additional college resources is broadly consistent with existing evidence. As we show below, CLB eligibility also increases college enrollment among older siblings who receive no direct transfer but experience higher parental savings, providing further evidence that induced parental saving, and potentially associated changes in aspirations, amplifies the program's impact.

5.3 Treatment Effects on Older Siblings

The savings and enrollment effects documented above show that CLB eligibility does more than simply transfer resources to eligible children: it activates parental saving behavior. If CLB eligibility not only prompts families to open an RESP but also encourages broader engagement with long-term saving, parents may begin setting aside funds for older, ineligible siblings as well, which could in turn affect their college enrollment. In this section, we test for such spillover effects by examining whether having a CLB-eligible younger sibling affects RESP savings and college enrollment outcomes for older siblings who are themselves ineligible for the program.

Estimation. 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.[22]

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 CLB.

[22] 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.

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 2004. The upper bound of 2004 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.

Spillover Effects. We begin by examining whether CLB eligibility for the youngest child spills over into parental saving decisions for older, ineligible siblings. Figure 2 provides clear visual evidence of these spillover effects. Panel A shows that RESP account ownership for older siblings increases discontinuously at the January 1, 2004, cutoff in their youngest sibling's birth date. Prior to the cutoff, account ownership trends smoothly, then jumps discretely at the eligibility cutoff. Panel B shows a corresponding pattern for contribution amounts, with a clear discontinuous increase at the cutoff. The smoothness of trends on either side of the cutoff and the clarity of the discontinuities support our identification strategy.

Panel A of Table 5 quantifies these effects. Having a CLB-eligible youngest sibling increases the probability that parents open an RESP for an older sibling by 3.8 percentage points. The effect on the probability of any parental contribution is identical. Since older siblings are ineligible for the CLB and receive no government grants, there is no incentive to open an empty account; account opening therefore necessarily reflects voluntary contributions. On average, parents contribute an additional $1,300 to older siblings' accounts.

These results indicate that CLB eligibility induces substantial increases in parental saving for older siblings. We next examine whether these changes in saving behavior is associated

Table 5: Spillover Effects of CLB Eligibility on Older Siblings' Savings and Enrollment

Outcome		Varying bandwidths				
	Main RD (1)	12 months (2)	15 months (3)	21 months (4)	24 months (5)	High-income (6)
Panel A: RESP savings outcomes						
Ever had a 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 contribution	0.038***	0.029*	0.037***	0.032***	0.027**	-0.003
	(0.013)	(0.016)	(0.014)	(0.012)	(0.011)	(0.013)
Parental contributions ($)	1,300***	1,246***	1,523***	1,183***	1,027***	-486
	(392)	(480)	(428)	(363)	(342)	(566)
Panel B: College enrollment outcomes						
Any enrollment	0.024**	0.018	0.027**	0.020*	0.017*	0.005
	(0.011)	(0.014)	(0.012)	(0.011)	(0.010)	(0.015)
Full-time enrollment	0.025**	0.016	0.030***	0.021**	0.016*	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)
U15 university	0.017**	0.004	0.017**	0.015**	0.013*	0.003
	(0.008)	(0.009)	(0.008)	(0.007)	(0.007)	(0.011)
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' savings and enrollment 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 ineligible for the CLB themselves. Estimates compare outcomes for older siblings whose youngest sibling was born just after versus just before January 1, 2004, using the regression discontinuity specification in Equation (3). Panel A reports cumulative RESP outcomes measured up to age 17; indicators equal one if the event ever occurs, and contribution amounts are cumulative totals expressed in 2024 Canadian dollars. Panel B reports bachelor's degree enrollment outcomes measured at age 18. 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 based on estimating the same RD for children from high-income (ineligible) families. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: Ever had a RESP account

Panel B: Parental contributions amount

Panel C: Bachelor's degree enrollment

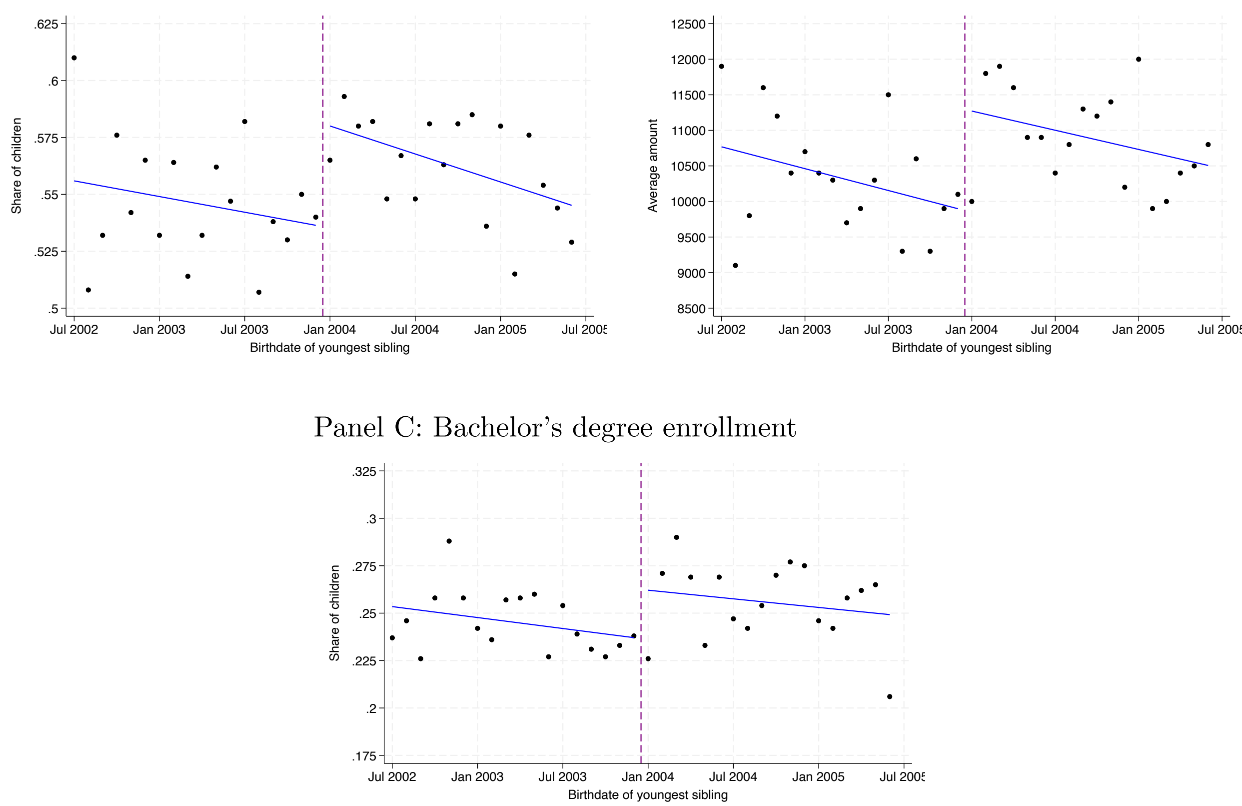

Figure 2: Spillover Effects of CLB Eligibility on Older Siblings' Savings and Enrollment

Notes: This figure plots average outcomes for older siblings as a function of 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. Panels A and B report cumulative RESP outcomes measured up to age 17: an indicator for ever having a RESP account and cumulative parental contributions (in 2024 Canadian dollars). Panel C reports bachelor's degree enrollment at age 18. The vertical dashed line indicates the January 1, 2004, Canada Learning Bond (CLB) eligibility cutoff for the youngest child. Solid lines show local linear fits estimated separately on each side of the cutoff. Older siblings are born before the CLB eligibility cutoff and are therefore ineligible for the CLB themselves.

with higher college enrollment. Panel C of Figure 2 shows a corresponding discontinuity in bachelor's degree enrollment at the eligibility cutoff. Consistent with this visual evidence, having a CLB-eligible younger sibling increases the probability that older siblings enroll in a bachelor's degree program by 2.4 percentage points. The effect on full-time enrollment is similar.

The estimates are stable across bandwidths for both savings and enrollment outcomes (columns 2–5). For savings, the effect on RESP account ownership varies from 2.7 to 3.8 percentage points across bandwidths ranging from 12 to 24 months, and contribution amounts range from $1,027 to $1,523. The enrollment spillovers are more sensitive to specification. While the point estimates remain positive and statistically significant across the 15- and 24-month bandwidths, the narrower 12-month bandwidth yields smaller and no longer statistically significant effects. This greater sensitivity is unsurprising given the smaller effective sample size in the sibling analysis, particularly for enrollment outcomes where the underlying effects are smaller in magnitude. Together, the evidence points to robust positive spillover effects of CLB eligibility on both parental saving and college enrollment for older siblings. Furthermore, the high-income placebo tests in column 6 show estimates that are small and statistically insignificant across both savings and enrollment outcomes, as expected.

Discussion. The effects observed for older siblings help clarify the mechanisms underlying the main-child results in Sections 5.1 and 5.2.

First, we find that parents open RESPs and increase savings for older, ineligible siblings. Because these children do not receive the CLB transfer, this pattern indicates that the program induces a broader shift in household saving behavior. This response is consistent with the mechanisms discussed in Section 5.1, such as reduced administrative barriers, increased financial literacy, or greater salience of education savings.

Second, we observe increases in college enrollment among these ineligible siblings. Since these children do not receive the direct transfer, this implies that changes in household-level parental behavior triggered by CLB eligibility for the younger child are themselves sufficient to affect educational outcomes.[23] This finding suggests that the enrollment effects for the main child reported in Section 5.2 are not driven solely by the financial resources provided by the CLB. This interpretation also helps explain why the enrollment effects for eligible

[23]Whether the enrollment effects we report for siblings operate specifically through increased RESP savings remains uncertain. The magnitude of the enrollment response is broadly consistent with recent evidence linking college savings to higher college enrollment (Vasilenko, 2025), suggesting that increased RESP savings may be an important channel. However, our design does not allow us to disentangle the role of additional savings from broader household-level shifts in aspirations, which could influence enrollment even in the absence of increased savings. We therefore interpret the results as reflecting the combined effect of increased education savings and associated changes in household aspirations, rather than the causal impact of savings alone.

children are large relative to the size of the transfer. If the program operated only through the direct financial resources it provides, more modest impacts would be expected. Instead, the CLB appears to operate not only through direct funding, but also by activating broader household responses that amplify its impact.

6 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. *Household behavioral responses drive the effects.* Parents open accounts and increase saving for older, ineligible siblings when a younger child is CLB-eligible, raising these siblings' bachelor's enrollment rate. This spillover suggests that CLB eligibility reshapes household saving behavior and engagement with the education system in ways that benefit children beyond those directly financially targeted by the program.

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, 2004; 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 the two) 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

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Appendix A: 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.

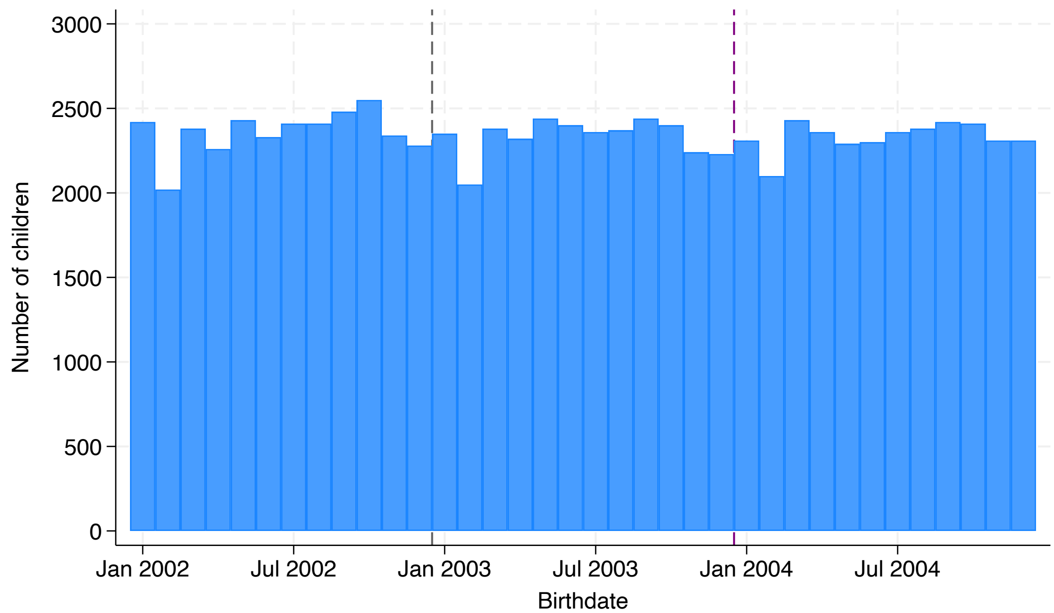
Table A1: Programs Providing Government Grants through College Savings Accounts

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.

Appendix B: Additional Tables and Figures

Figure B1: Distribution of Date of Birth

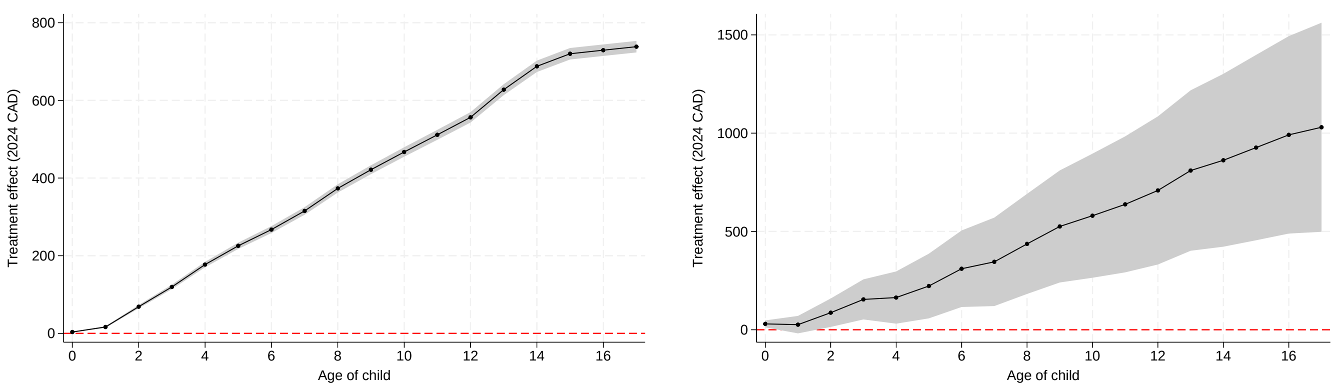


Notes: The figure plots the distribution of children's dates of birth in the main sample of low-income families. Each bar reports the number of children born in a given month.

Figure B2: Dynamic Treatment Effects

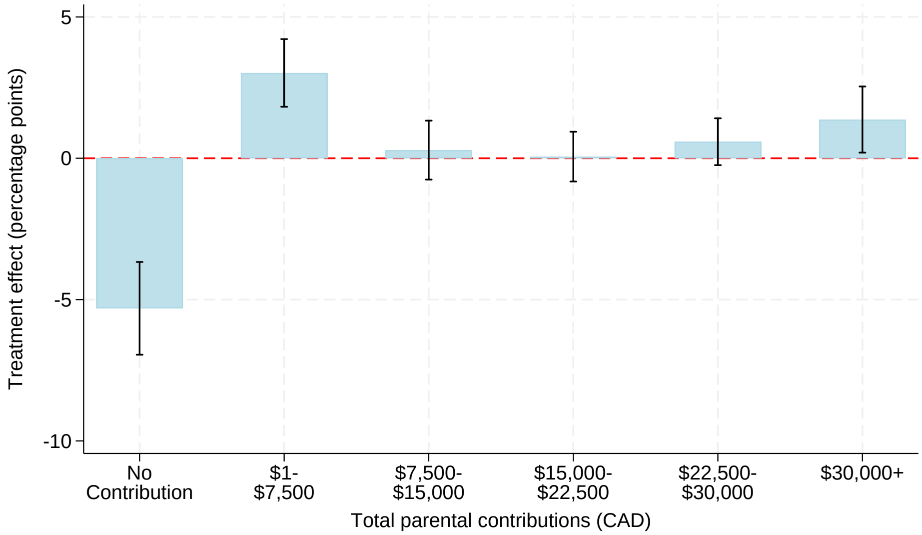
Panel A: CLB grant amount

Panel B: Parental contributions amount



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; Panel B reports effects on cumulative parental contributions. Both outcomes are expressed in 2024 Canadian dollars. The sample is restricted to low-income children as defined in Section 3. Estimates correspond to Equation (1). Grey bands represent 95% confidence intervals based on heteroskedasticity-robust standard errors.

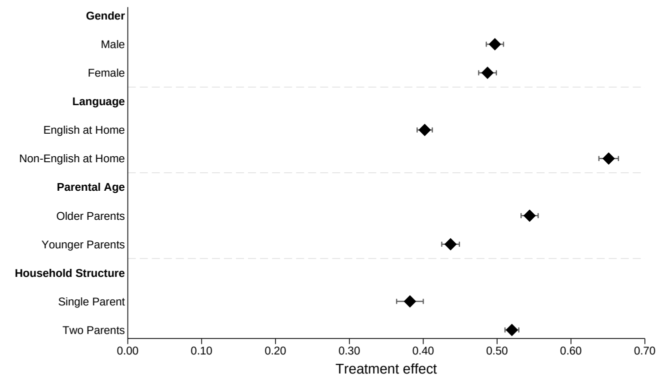
Figure B3: Distribution of Treatment Effects on Parental Contributions



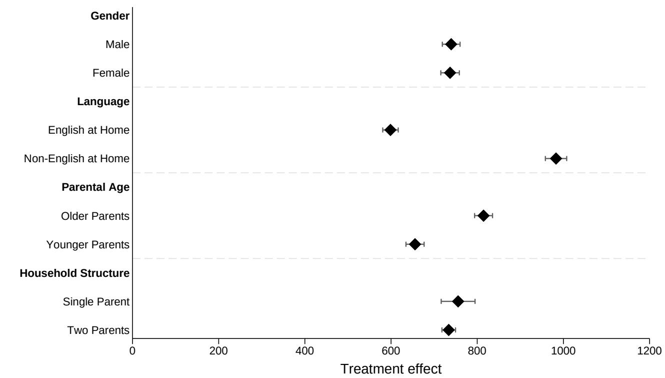
Notes: This figure plots difference-in-discontinuities estimates of the effect of CLB eligibility on the probability of falling into each bucket of cumulative parental contributions, measured from birth through age 17. Each bar reports a separate estimate from Equation (1), where the outcome is an indicator 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 B4: Heterogeneous Treatment Effects on RESP Outcomes

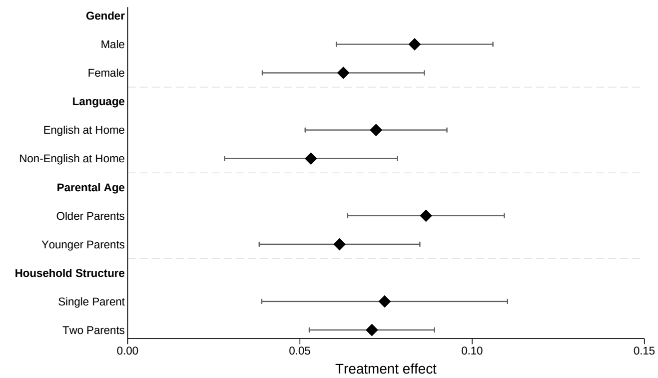
Panel A: Any CLB



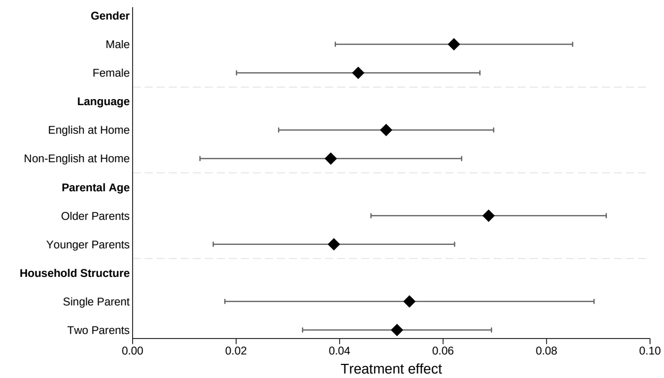
Panel B: CLB grant amount



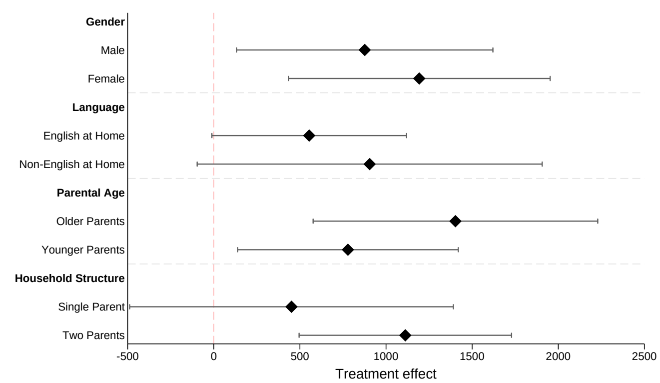
Panel C: Ever had a RESP account



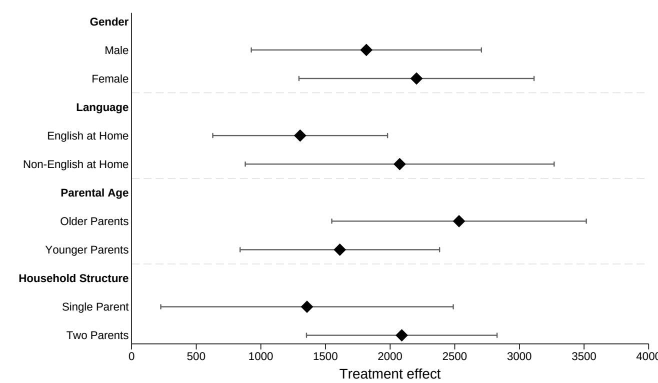
Panel D: Any parental contribution



Panel E: Parental contributions amount



Panel F: Total contributions and 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 B1: Descriptive Statistics for the High-Income Sample

	Child birth date			
	Placebo group		Main group	
Variable	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	0.899	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$)	168,500	162,900	161,800	158,900
Ever had an RESP account	0.773	0.785	0.783	0.799
Age of child at first RESP opening	3.3	2.9	3.2	2.7
Total parental contributions (2024 CA$)	26,600	27,400	26,600	27,600
Total CLB (2024 CA$)	0	0	0	20
Total CESG (2024 CA$)	5,100	5,200	5,100	5,200
Total contributions and grants (2024 CA$)	31,700	32,500	31,700	32,900
Enrolled in a bachelor's degree program	0.386	0.415	0.386	0.383
Full-time enrollment	0.863	0.855	0.850	0.846
STEM field	0.381	0.402	0.396	0.392
U15 university	0.399	0.400	0.370	0.394
Sample size	6,040	6,090	6,070	6,010

Notes: The table reports means for children's socio-demographic characteristics and for the main outcomes related to parental savings in RESPs and college enrollment. Statistics are reported for the high-income (ineligible) sample, split into four groups defined by the child's date of birth. These groups correspond to those used in the main empirical specification. Sample sizes are reported in the last row and apply to all outcomes, except for outcomes listed below "Ever had an RESP account," which are computed conditional on having an RESP, and for outcomes listed below "Enrolled in a bachelor's degree program," which are computed conditional on enrollment in a bachelor's degree program.

Table B2: Placebo Tests Using the RD–DD Design

	Fake treatment cutoff on Jan. 1 of:			
Outcome	2000	2001	2002	2003
Panel A: RESP outcomes				
Ever had a RESP account	-0.002	-0.007	0.008	-0.007
	(0.008)	(0.008)	(0.008)	(0.008)
Any parental contribution	-0.002	-0.007	0.008	-0.007
	(0.008)	(0.008)	(0.008)	(0.008)
Parental contributions amount ($)	274	-659**	264	-94
	(253)	(261)	(267)	(269)
Total contributions and grants ($)	307	-775**	322	-114
	(301)	(310)	(317)	(320)
Panel B: Bachelor's degree enrollment outcomes				
Any enrollment	0.008	-0.019***	0.016**	-0.020***
	(0.007)	(0.007)	(0.007)	(0.007)
Full-time enrollment	0.001	-0.009	0.006	-0.015**
	(0.007)	(0.007)	(0.007)	(0.007)
STEM field	-0.003	-0.005	0.008	-0.008
	(0.005)	(0.005)	(0.005)	(0.005)
U15 university	-0.014***	0.004	0.005	-0.023***
	(0.005)	(0.005)	(0.005)	(0.005)

Notes: Each column reports placebo estimates obtained by re-estimating the RD–DD specification in Equation (1) using a fake January 1 eligibility cutoff in the indicated year. Under the identifying assumption of the RD–DD design, these placebo estimates should be close to zero. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Treatment Effects on Other (Non-RESP) Savings

Outcome	Baseline mean (1)	Main RD-DD (2)	Alternative specifications: 3-month BW (3)	Linear MoB (4)	RD-DDD (5)
Panel A: Savings in TFSAs (tax-advantaged savings accounts)					
Any savings	0.50	-0.005	-0.005	0.001	-0.002
		(0.006)	(0.008)	(0.012)	(0.009)
Total savings	9,900	296	8.43	1.39	-28.4
		(323)	(462)	(662)	(866)
Market value in 2019	7,700	-75.1	-521	-480	-125
		(285)	(408)	(582)	(745)
Panel B: Savings in RRSPs (tax-advantaged retirement accounts)					
Any savings	0.55	-0.008	-0.006	-0.002	-0.010
		(0.006)	(0.008)	(0.012)	(0.007)
Total savings	29,000	-310	-786	-1,005	202
		(742)	(1,058)	(1,514)	(2,550)
Panel C: Savings in other taxable accounts					
Investment income in 2019	280	-10.4	-10.6	-27.6	-76.4
		(21.0)	(29.5)	(43.0)	(55.7)

Notes: This table shows the effects of CLB eligibility on non-RESP savings outcomes. 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. The sample is restricted to low-income children as defined in Section 3. Column (1) reports baseline means for children born between July and December 2003. Column (2) reports difference-in-discontinuities estimates that compare outcomes for children born just after versus just before January 1, 2004, and subtract the corresponding January 1 discontinuity observed for a placebo group centered around January 1, 2003, using a symmetric six-month bandwidth (Equation (1)). Column (3) uses a narrower three-month bandwidth, while column (4) allows for group-specific linear functions of date of birth. Column (5) reports difference-in-difference-in-discontinuities estimates that additionally difference out school-cohort–specific shocks using children from high-income (ineligible) families as an additional control group (Equation (2)). Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: LAD.

Table B4: Placebo Tests Using the RD–DDD Design

	Fake treatment cutoff on Jan. 1 of:			
Outcome	2000	2001	2002	2003
Panel A: RESP outcomes				
Ever had a RESP account	0.013	-0.008	0.014	-0.009
	(0.015)	(0.014)	(0.014)	(0.014)
Any parental contribution	0.013	-0.008	0.014	-0.009
	(0.015)	(0.014)	(0.014)	(0.014)
Parental contributions amount ($)	-895	724	-486	171
	(566)	(571)	(574)	(568)
Total contributions and grants ($)	-1,074	865	-527	170
	(670)	(676)	(678)	(671)
Panel B: Bachelor's degree enrollment outcomes				
Any enrollment	-0.006	-0.002	0.009	-0.020
	(0.015)	(0.015)	(0.015)	(0.015)
Full-time enrollment	-0.009	0.018	0.009	-0.024*
	(0.014)	(0.014)	(0.014)	(0.014)
STEM field	-0.005	0.013	0.001	-0.018*
	(0.011)	(0.011)	(0.011)	(0.011)
U15 university	0.007	0.019*	-0.013	-0.008
	(0.011)	(0.011)	(0.011)	(0.011)

Notes: Each column reports placebo estimates obtained by re-estimating the RD–DDD specification in Equation (2) using a fake January 1 eligibility cutoff in the indicated year. The RD–DDD design differences out time-varying school-cohort shocks common to low- and high-income children; under its identifying assumptions, these placebo estimates should be close to zero. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.