Delivering Student Grants Through College Savings Accounts*

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Abstract

We provide the first large-scale evidence on the effects of delivering postsecondary education grants through college savings accounts on account opening, parental saving behavior, and college enrollment. Specifically, we study the Canada Learning Bond (CLB), a federal program that deposits up to $2,000 into registered college savings accounts for children from low-income families, with funds accessible only upon college enrollment. We identify the effects of CLB eligibility using administrative microdata from British Columbia and a difference-in-discontinuities design around the date-of-birth eligibility cutoff. We find three key results. First, CLB eligibility increases college savings account ownership by 7 percentage points and, despite requiring no parental contributions, 72% of parents induced to open accounts voluntarily choose to contribute their own funds. Second, these behavioral effects extend beyond eligible children: parents also open accounts and increase saving for older, ineligible siblings. Third, CLB eligibility raises bachelor's degree enrollment by 4.6 percentage points for eligible children and 2.4 percentage points for older siblings. This sibling spillover, which occurs absent any direct grant to these older children, provides clean causal evidence that induced parental saving itself expands college access, not merely the direct transfer. Our findings demonstrate that the design of financial aid delivery matters: delivering grants through college savings accounts generates behavioral 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 distributed when students enroll in college. In recent years, however, a newer policy instrument has been introduced: delivering student grants through children's college savings accounts. In practice, governments automatically credit grant amounts to the college savings accounts of eligible children, with withdrawals allowed only if and when the child enrolls in postsecondary education.

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). In theory, these programs may affect college enrollment through several mechanisms that differ from traditional grants. They 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 enhancing their effectiveness. At the same time, accessing these grants requires that families open and maintain a college savings account, which may limit take-up and reduce effectiveness, particularly among lower-income households. The grant might also crowd out parental savings if families treat the government contribution as sufficient or substitute it for their own planned contributions. Because these theoretical predictions point in different directions, understanding how families respond to such incentives remains an open empirical question.

In this paper, we show that student grants delivered through college savings accounts can generate positive behavioral responses in parental saving that amplify program impacts beyond the direct financial transfer. We demonstrate this by studying the causal effects of the Canada Learning Bond (CLB), a federal program introduced in 2005 that deposits up to $2,000 (Canadian dollars) into college savings accounts for children from low-income families, with funds accessible only upon postsecondary enrollment. We exploit the program's sharp date-of-birth eligibility cutoff and administrative microdata from British Columbia

(Canada) to estimate causal effects on account opening, parental saving behavior, and college enrollment.

To receive the CLB, families must open a Registered Education Savings Plan (RESP), a tax-advantaged college savings account, with the child named as beneficiary.[1] RESPs have existed in Canada since 1974, and the federal government has encouraged participation through matching grants on parental contributions since 1998. In our sample of lower-income families from British Columbia, 54% of children born in 2003 (i.e., not eligible for CLB) have an RESP opened in their name.[2] As with all government grants deposited into RESPs, CLB deposits can only be withdrawn if and when the child enrolls in a qualifying postsecondary program; if the child never enrolls, the CLB must be returned to the government. Because CLB funds can only be withdrawn upon postsecondary enrollment, the program is effectively a "postsecondary education grant." The key difference from traditional student grants lies in delivery: the CLB operates through savings accounts opened in childhood rather than direct disbursement at enrollment. While the CLB is a Canadian program, our findings speak directly to ongoing policy debates where governments are considering delivering benefits through child savings accounts, including baby bond programs in the United States and education savings initiatives in other countries.

The CLB differs from preexisting matching grants in a crucial way: it requires no parental contributions whatsoever. Traditional savings incentives, such as tax advantages and matching grants, condition government support on families' own contributions and therefore primarily benefit households that are already able and willing to save. By contrast, the CLB is designed to ensure that financial resources are available at the time of postsecondary enrollment, independently of parental saving decisions.

Eligibility for CLB is determined along two dimensions. First, the program applies only

[1]These accounts are similar to 529 plans in the United States. Other countries have analogous tax-advantaged savings vehicles, such as Junior ISAs in the United Kingdom and Education Bonds in Australia, though these differ in their specific tax treatment and withdrawal rules.

[2]This baseline take-up rate is relatively high compared to the United States, where 529 plan participation among similar populations is typically below 20% (Investment Company Institute, 2022). The higher Canadian rate likely reflects the effect of the generous matching grants.

to children born in 2004 or later, generating a sharp date-of-birth cutoff that forms the basis of our empirical design. Second, conditional on date of birth, eligibility is assessed annually from birth until age 15 based on family income. Children receive deposits in each year their family income falls below the eligibility threshold, with total CLB amounts ranging from $500 to $2,000 depending on the number of eligible years.

Our analysis draws on comprehensive administrative data from British Columbia that link children's school records to parental tax returns, RESP account records, and postsecondary enrollment records.[3] These linked datasets allow us to observe, for each child, whether they are eligible for the CLB based on birth date and family income, how much they actually receive in CLB grants, how much their parents contribute to their college savings accounts, and whether they enroll in college.

We are interested in the causal effects of CLB eligibility on these outcomes. We first identify and restrict our sample to children from low-income families who are eligible for the CLB or who would have been eligible had the program been in place prior to 2004. We then exploit the fact that only children born on or after January 1, 2004 are eligible for the CLB. In principle, we could simply compare low-income children born just before and after this cutoff to isolate the effect of CLB eligibility. However, several features of our setting complicate this approach: outcomes are measured on a calendar-year basis, creating mechanical differences between January and December births; and the school entry cutoff in British Columbia also falls on January 1, creating a discontinuity in relative age within school cohort that may independently affect outcomes. To address these concerns, we use a difference-in-discontinuities (RD-DD) design that differences out the expected January 1 discontinuity that would arise absent the CLB, using children born around January 1, 2003 as a placebo group. In some specifications, we extend this to a difference-difference-in-discontinuities (RD-DDD) design by incorporating high-income families who are never

[3]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.

eligible for the CLB, allowing us to account for school cohort-specific effects that may vary over time.

We begin our analysis by examining take-up of the CLB. Because CLB receipt requires opening an RESP account, CLB take-up is intrinsically linked to RESP ownership. At baseline, 54% of eligible children have RESP accounts opened. Among these families who would have opened accounts regardless of the CLB (inframarginal families), 77% successfully receive the CLB; the remaining 23% may reflect incomplete paperwork, accounts opened by non-primary caregivers, or measurement error. Beyond this inframarginal take-up, we find that CLB eligibility increases RESP account ownership by 7 percentage points, representing induced take-up among families who open accounts specifically because of the CLB. Overall, 49% of eligible children receive the CLB, meaning that half either do not have an RESP opened (39%) or have an account but do not claim the benefit (12%).

Beyond simply receiving the grant, a critical question is whether families adjust their own savings in response. Three scenarios are possible. First, the grant might crowd out private savings if families reduce their contributions in response to receiving these government funds. Second, the grant might have no effect on private contributions: families induced to open accounts to receive the CLB might not contribute since this is not required, while inframarginal families maintain their existing saving patterns. Third, the grant might crowd in private savings if it triggers behavioral changes that increase parental contributions. We find strong evidence in favor of the third scenario: substantial crowding-in of private savings. In particular, among parents induced to open an RESP because of the CLB, 72% voluntarily contribute to the account. This suggests the CLB does more than simply provide funds: it activates saving behavior by lowering administrative barriers, increasing salience, or shifting expectations about postsecondary education. This behavioral response is particularly noteworthy since it occurs despite the presence of generous pre-existing matching grants, which had failed to induce saving among these same families.

While we can directly observe this extensive margin response—new families contributing—

we cannot separately estimate whether inframarginal families (those who would have saved anyway) also changed their saving behavior in response to the CLB. What we can measure is the total effect: combining any responses at both the extensive margin (newly contributing families) and the intensive margin (adjustments among existing savers), CLB eligibility increases total parental contributions by $1,000 (2024 Canadian dollars) on average, a 9% increase relative to baseline.

Using additional data from parental tax returns (the Longitudinal Administrative Data Bank), we further examine whether these induced education savings crowd out other household savings. They do not: retirement account and tax-free savings account contributions remain unchanged, indicating that the CLB generates net new household saving rather than reshuffling existing resources. In total, CLB eligibility increases resources held in education savings accounts at age 17 by approximately $2,000.[4] Roughly one-third of this increase comes from additional parental contributions, one-half from the CLB grant itself, and one-sixth from matching grants earned through increased parental saving. This implies a treatment-on-the-treated effect of approximately $4,000 for children who receive the CLB.

If the program works by changing parental awareness and behavior, do the effects on account openings and contributions extend to other ineligible children in the household? We find that they do. Using the British Columbia school records linked to parental tax records, we identify siblings and show that parents of CLB-eligible children are 4 percentage points more likely both to open and contribute to RESP accounts for older, CLB-ineligible siblings.

The natural next question is whether these changes in take-up and household saving translate into higher college enrollment. We find that CLB eligibility increases four-year college enrollment by 4.6 percentage points among eligible children, a substantial effect given the baseline enrollment rate of 24%. Importantly, these enrollment gains extend beyond the eligible child. Older, ineligible siblings, who receive no CLB grant but benefit from increased parental saving, experience a 2 percentage point increase in four-year college enrollment. This

[4]This estimate excludes investment earnings, which are not observed in our data, and reflects parental contributions and government grant amounts only.

spillover effect is particularly striking: it occurs for children who receive no direct financial benefit from the program, providing clean evidence that induced changes in parental saving behavior, not merely the grant itself, expand college access. Together, these findings indicate that grants delivered through college savings accounts expand college access through both direct financial support and induced changes in household saving behavior.

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 particpation, which the CLB overcomes by covering administrative costs of account opening and highlighting the importance of saving for postsecondary education. This has implications for other domains, such as retirement accounts, where similar behavioral frictions may limit participation among lower-income households.

Second, we show that grants delivered through college savings accounts can effectively expand college enrollment even when provided years before enrollment decisions are made. The magnitude of the effects we find even suggests these grants may even generate larger enrollment effects than traditional aid provided at the time of college entry, as we discuss in Section 6.5. We identify increased parental saving as a key mechanism through which college savings account grants amplify program impacts beyond the direct government financial transfer. Two pieces of evidence support this conclusion: (1) for each dollar the government deposits through the CLB, families contribute $1.4 of their own money, and (2) older siblings who receive no CLB grant but whose parents increase saving in response to a younger child's eligibility experience enrollment gains of their own.

This paper contributes to two strands of literature. First, we add to the broad literature on college financial aid effects on enrollment. Most research focuses on the effects of grants delivered at the time of enrollment on college enrollment decisions, particularly for students from lower-income backgrounds (see Herbaut and Geven (2020) and Dynarski et al. (2023)

for reviews). We address a major gap: understanding whether grants delivered early through college savings accounts are more or less effective than traditional aid. Prior work on this topic includes small-scale randomized experiments (Kim et al., 2015; Clancy et al., 2016; Huang et al., 2021; Long and Bettinger, 2017; Martini et al., 2021) and a study of how matching grant rates affect savings (Messacar and Frenette, 2019); we provide detailed discussion of these papers in Section 2. We provide the first causal evidence from a large-scale universal program on both intermediate outcomes (account opening and saving) and final outcomes (college enrollment), demonstrating that such programs can amplify effectiveness beyond traditional aid by crowding in private saving rather than merely providing direct transfers.

Second, we contribute to the literature on policies promoting household saving. Much of the existing work focuses on pension savings, where traditional interventions rely on matching contributions to encourage participation. These studies generally find significant but moderate effects on saving behavior (see the reviews in Madrian (2012) and Hinz et al. (2013)). In contrast, we examine the effects of a non-matching financial incentive and show that it can activate saving among low-income families who remain unresponsive to traditional matching grants. Our paper therefore adds to a growing set of papers, often rooted in behavioral economics, exploring alternative policy designs to encourage savings, including automatic enrollment (Madrian and Shea, 2001; Beshears et al., 2022; Choukhmane, 2025), compulsory active choice (Carroll et al., 2009), and information-based interventions (Duflo and Saez, 2003; Saez, 2009). To the best of our knowledge, our paper is the first to show that non-matching financial incentives tied to savings accounts can induce new households to save.

The remainder of the article is organized as follows. Section 2 reviews the literature on grants in college savings accounts. Section 3 describes the Canada Learning Bond program. Section 4 presents the data and sample. Section 5 outlines our empirical strategy. Section 6 presents the results: we first report the effects of CLB eligibility on savings-related outcomes, then examine spillovers to older siblings, and finally analyze effects on college enrollment. Section 7 concludes.

2 Prior Literature

Prior to this study, relatively few papers have examined the effects of government grants in college savings accounts, likely in part because these programs have only recently been introduced and have not yet become widespread. We review this nascent literature here, organizing our discussion around the key findings on savings behavior and educational outcomes.

Most existing evidence comes from small-scale randomized experiments in the United States. The SEED for Oklahoma Kids (SEED OK) program, launched in 2007, automatically opened college savings accounts with an initial $1,000 deposit for a treatment group of newborns, with additional incentives for families who opened their own accounts and made deposits. Kim et al. (2015) find that the program increased account ownership and asset accumulation, particularly among lower-income families. Clancy et al. (2016) show that automatic initial deposits combined with investment earnings significantly boosted asset accumulation over time, with the largest effects for low- and moderate-income families who faced the greatest barriers to opening accounts on their own.

The Boston Early College Planning Initiative provides additional experimental evidence. This program combined automatic account opening with initial deposits and matching incentives for families of children entering kindergarten. Long and Bettinger (2017) find that providing initial deposits helps overcome barriers such as complexity and lack of awareness, resulting in increased savings behavior. Importantly, they also report preliminary, marginally significant effects on college enrollment, though these results are based on relatively small samples and short follow-up periods.

Outside the United States, Martini et al. (2021) examine the ACHAB program in Italy, a randomized experiment that provided college savings account grants to high school students. They find positive effects on college enrollment, suggesting that even grants provided later in adolescence can influence educational decisions. While these experiments demonstrate proof of concept, they share important limitations. All were conducted on specific, relatively

small populations, often specific birth cohorts in particular cities or regions. In several cases, the programs included substantial informational components or financial planning assistance alongside the savings incentives, making it difficult to isolate the effect of the grant itself. It therefore remains unclear whether these results would generalize to large-scale universal programs serving broader populations without complementary interventions.

The only paper studying a large-scale existing grant program is Messacar and Frenette (2019), who analyze the Canada Education Savings Grant (CESG), a federal matching grant for college savings that has been available since 1998. The CESG provides matching rates ranging from 20% to 40% depending on family income, with higher rates for lower-income families. Using administrative tax data and a regression discontinuity design based on income thresholds, Messacar and Frenette (2019) find that higher matching rates increase parental savings at the intensive margin: families who already participate in the program save more when facing higher match rates. However, the CESG's design as a matching grant means it only benefits families who are already able and willing to contribute their own funds. Messacar and Frenette (2019) find no effects at the extensive margin: the program does not induce new families to open accounts or begin saving. Moreover, their analysis focuses on variation in matching rates among participants rather than examining grant eligibility per se, and they do not analyze subsequent impacts on college enrollment. This leaves open the question of whether matching grant eligibility per se can increase savings, whether non-matching grants can reach families who do not respond to traditional matching incentives, and whether such grants ultimately affect educational attainment.

3 The Canada Learning Bond

The Canada Learning Bond (CLB) is a monetary transfer program 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 children from low-

income families college savings accounts, without requiring any parental contributions. CLB funds can be withdrawn 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 adjusted income at or below approximately $57,375 to qualify. Eligibility for a given benefit year also requires that the primary caregiver file 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.

The CLB operates through registered college savings accounts. In Canada, these accounts are known as Registered Education Savings Plans (RESPs), which are tax-advantaged savings vehicles introduced in 1974 to encourage families to save for postsecondary education. RESPs are opened by parents or other individuals with a designated child beneficiary who may later access the accumulated funds upon enrollment in an approved postsecondary institution.[5]

To receive a CLB deposit, a child must be named as a beneficiary on a registered college savings account.[6] Each year, eligibility is assessed based on date of birth and family income, and if the criteria are met, the CLB money for that benefit year is deposited into the child's

[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]Caregivers must also file any required CLB consent forms, which generally need to be submitted only once to allow deposits to be made to the account.

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.[7]

Withdrawals of CLB amounts follow the same rules as other government grants held in registered college savings accounts.[8] In particular, CLB funds can be withdrawn only if and when the beneficiary enrolls in a qualifying postsecondary education program at an eligible educational institution. Eligible institutions include Canadian universities, colleges, CEGEPs, trade schools, and certain approved institutions outside Canada. Both full-time and part-time programs qualify, provided they meet minimum requirements in terms of program duration and instructional hours. Withdrawn grant amounts are paid to the beneficiary as Educational Assistance Payments (EAPs) and are taxed as income in the student's hands. Because students typically have low taxable incomes and may claim education-related tax credits, the effective tax rate on EAPs is often minimal (Milligan, 2004). If the beneficiary never enrolls in postsecondary education, CLB amounts must ultimately be repaid to the government when the account is closed. RESPs may remain open until the beneficiary turns 35.

In addition to the CLB, the Canadian federal government supports 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

[7]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.

[8]More generally, when RESP 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, and investment earnings and government grants 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. If the beneficiary does not pursue postsecondary education, the subscriber may still withdraw the original contributions tax-free. Investment earnings may also be withdrawn but under specific conditions (once the beneficiary reaches age 21 and the plan has been open for at least 10 year) and is subject in that case to the subscriber's marginal income tax rate plus a 20% surtax.

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.[9] Importantly for our analysis, these matching grants were similarly available to both treated and control children in our sample.[10]

4 Data and Sample

4.1 Data Sources

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 provided by Statistics Canada, which also supplies 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

[9] 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.

[10] Several provinces have also introduced their own RESP incentive programs. Children in our sample (born 2002–2004 in British Columbia) were eligible only for the federal programs described above.

to parental T1FF records. Children who cannot be matched are excluded from our analysis.[11] We further exclude 3% of children who are matched to more than two parents.

We 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. 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. 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. These records cover public postsecondary institutions only. In Canada, nearly all universities (bachelor degree-granting institutions) are public, so the PSIS provides near-complete coverage of bachelor's degree enrollment. The very few private universities operating in Canada are typically religious, specialized, or online institutions enrolling small numbers of students (Usher et al., 2025). Short-cycle postsecondary institutions, such as applied or vocational colleges, can be either public or private and are therefore not fully covered by the PSIS. For this reason, when analyzing the effects of CLB eligibility on postsecondary education, we focus on bachelor's degree enrollment, for which coverage in the PSIS is nearly complete. Children in the BC K–12 dataset who are not matched to the CESP or PSIS are assumed not to have held an RESP account or not to have enrolled in a bachelor's degree program, respectively.

4.2 Definition of Outcomes

We study the effects of CLB eligibility on two broad sets of outcomes: RESP outcomes derived from the Canada Education Savings Program (CESP) and bachelor's degree enrollment outcomes derived from the Postsecondary Student Information System (PSIS). All RESP

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

savings 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. his 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. College enrollment outcomes are measured in the calendar year in which the child turns 18, corresponding to the typical age of entry into postsecondary education. Table 1 details the definitions of all outcomes.

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 included, in British Columbia, the University of British Columbia.

4.3 Income Eligibility Determination

Our empirical strategy compares children born in 2004 who are eligible for the CLB with children born in 2003 who would have been eligible had the CLB existed for their cohort, and subtracts the same comparison using earlier birth cohorts. Implementing this comparison requires determining income eligibility for all relevant children under a common set of rules. 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. 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 children into three mutually exclusive groups: (i) at least once eligible, (ii) never eligible, and (iii) ambiguous.[12]

[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.

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."

4.4 Sample Selection

Main sample: Our main sample restriction is based on children's dates of birth and reflects the empirical design described below. The exact birth-cohort window varies across specifications. In our baseline specification, we focus on children born between July 2002 and June 2004, which yields a sample of 94,470 children.

Our main analysis sample consists of the low-income group, which includes 56,230 children. We additionally use the high-income group for placebo tests and to implement the difference-in-difference-in-discontinuities design. Children classified as having ambiguous eligibility status are always excluded from the analysis. Beyond date of birth and parental income, we do not impose any further sample restrictions.

Siblings sample: For analyses of sibling spillovers, we organize the data at the family level and identify the youngest observed child in each family as well as all older siblings. Families are identified using parental identifiers in the linked tax data, and siblings are defined as all children ever claimed by the same parent(s). To ensure completeness of sibling identification, we require that older siblings 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. We study outcomes for all siblings of the focal youngest child, exploiting variation in whether this youngest child in the family is born just before or just after the January 1, 2004 eligibility cutoff.

We further restrict older siblings to be 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 beginning in 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 and exclude cases with ambiguous eligibility status.

4.5 Descriptive Statistics

Table 2 reports 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 $57,000, consistent with the program's targeting of low-income families.

The table also reports 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. Turning to four-year

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
Observed in Grade 1 or earlier	0.793	0.816	0.810	0.825
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.

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. 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 meaningful 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.

5 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 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. To address these concerns, we implement a difference-in-discontinuities design that uses earlier, untreated cohorts to difference out the expected January 1st discontinuity that would arise absent the CLB. We first describe this design for the savings outcomes of the main child (Section 5.1), then explain how we adapt our approach for education outcomes (Section 5.2). Finally, we describe our approach to identifying spillover effects on siblings, for whom a standard regression discontinuity is appropriate (Section 5.3).

5.1 Treatment Effects for Savings Outcomes

We implement a difference-in-discontinuities (RD-DD) design that combines elements of regression discontinuity (RD) and difference-in-differences (DiD). The intuition is straightfor-

ward: 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: those at least once eligible for the CLB, including children born before 2004 who would have been eligible had the program existed for their cohort. We estimate the following equation:

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

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 approximately 50% of eligible children in the treated group receive the CLB, the corresponding TOT effects are obtained by multiplying β_1 by two.

Because our design combines RD and DiD elements, standard bandwidth-selection proce-

dures 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. 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.

Similarly, there is no clear guidance tailored to our combined setting regarding inference and, in particular, the choice of how to cluster standard errors.[13] For this reason, we report heteroskedasticity-robust standard errors throughout.

Finally, our baseline specification deliberately does not include any controls for the running variable (date of birth).[14] 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 be fully flexible as long as it is stable across the main and placebo groups.[15] 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

[13]Each component of our design is associated with different recommendations in the literature. See, for example, Lee and Card (2008) and Kolesár and Rothe (2018) for RD designs with discrete running variables, and Bertrand et al. (2004) and Roth et al. (2023) for DiD designs.

[14]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.

[15]This is a distinctive feature of the RD-DD design relative to a standard RD design. In a single-cutoff 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, thereby relaxing the need to model this relationship directly.

functions. As a result, this specification yields less precise estimates. For these reasons, and because the results are qualitatively similar, we adopt the local-constant specification from Equation (1) as our preferred approach.

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. Under the identifying assumption, these placebo estimates should be close to zero. 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). Because these children are unaffected by the policy, any estimated effect in this sample would capture changes in the January 1st discontinuity between the main and placebo groups that are unrelated to CLB eligibility. Finding a null effect among income-ineligible children provides additional support for our design. We present the results of these placebo tests throughout the paper. In the main results section, we report placebo tests based on a fake January 1st, 2003 cutoff and on the high-income sample. We additionally present placebo tests using earlier fake cutoffs (January 1st of 2000, 2001, and 2002) in Appendix Table A.7. Across all savings-related outcomes, these placebo estimates are small and statistically indistinguishable from zero.[16]

A second assumption is that there is no manipulation of birth timing around the cutoff. Manipulation is unlikely in our setting for an institutional reason: 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. Parents whose children

[16]One exception is total contributions under the fake January 1st, 2001 cutoff, which is statistically significant at the 5% level. Given the number of placebo tests conducted, the presence of an isolated significant estimate is consistent with what would be expected by chance.

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 Appendix Figure A.3 and confirm that there is no discontinuity in the number of births at the January 1st, 2004 cutoff.

5.2 Treatment Effects on College Enrollment Outcomes

The RD-DD design described above is well-suited for savings outcomes but faces an additional challenge when applied to college enrollment. The concern is that children born on either side of January 1st belong to different school cohorts and therefore make their enrollment decisions at different points in time. If macroeconomic conditions, labor market prospects, or other cohort-level factors affect enrollment rates, these could confound our estimates even after differencing out the placebo cohort.

To see why this matters, consider the comparison embedded in our RD-DD design. Children born in December 2003 and January 2004 enter school in different years and, consequently, reach college age in different calendar years. If, say, labor market conditions were unusually weak when the January 2004 cohort reached college enrollment, this could raise their enrollment rates relative to the December 2003 cohort for reasons unrelated to the CLB. Importantly, differencing out the January 1st, 2003 discontinuity does not solve this problem: the 2003 placebo cohorts faced different macroeconomic conditions at college entry than the 2004 main cohorts, so the placebo discontinuity does not identify the relevant counterfactual.

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 macro-economic 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 now slightly weaker than before. We no longer require that the January 1st discontinuity in enrollment be stable over time as we now allow cohort-specific factors.

We assess the robustness of the RD–DDD design by conducting placebo tests using fake treatment cutoffs between 2000 and 2003. Under the identifying assumptions of the RD–DDD design, these placebo estimates should be close to zero. Consistent with this prediction, RD–DDD placebo estimates are centered near zero across placebo cutoffs, and the number of statistically significant placebo effects is small and close to what would be expected by chance (Appendix Table A.8). This pattern provides direct support for the validity of the RD–DDD design in the context of college enrollment outcomes.

Relative to RD–DD estimates, RD–DDD estimates are substantially noisier, with standard errors more than twice as large on average. This motivates our outcome-specific choice of empirical design. For savings outcomes, concerns about school-cohort-specific confounding are substantially less relevant than for college enrollment: savings measures are cumulative

and observed over many years, implying that treated and control cohorts are exposed to broadly similar macroeconomic conditions over the accumulation period. Consistent with this interpretation, placebo tests using fake treatment dates reveal no evidence of instability in the January 1 discontinuity over time for savings-related outcomes (Appendix Table A.7). For this reason, we rely on the RD–DD design for savings outcomes, which delivers substantially more precise estimates.

In contrast, college enrollment outcomes are measured at a single point in time and are inherently tied to school-cohort timing. For these outcomes, placebo estimates obtained under the RD–DDD design are markedly closer to zero than those obtained under RD–DD (Appendix Tables A.7 and A.8), indicating that the additional differencing removes residual cohort-specific trends that may contaminate simpler specifications, albeit at the cost of increased noise. Accordingly, we rely on the RD–DDD design for college enrollment outcomes.

5.3 Treatment Effects on Older Siblings

To identify how CLB eligibility for a child affects outcomes for their older, non-eligible siblings, we exploit the same January 1, 2004 eligibility cutoff but now as the birth date of the *youngest* sibling in the household. The question is whether having a CLB-eligible younger sibling changes the outcomes for older, ineligible siblings.

The design here is simpler than for the main child. Consider two older siblings who are identical in their own birth timing—say, both born in March 2000—but who differ in when their youngest sibling was born: one has a younger sibling born in December 2003 (just before the cutoff), the other has a younger sibling born in January 2004 (just after). The older siblings themselves do not differ in calendar-year measurement of their outcomes, in school cohort, or in relative age. The only systematic difference 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.

Our baseline estimates use a symmetric bandwidth of 18 months around the cutoff. 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.

6 Results

We present our results in four parts. First, we examine RESP outcomes for eligible children, including CLB take-up rates, effects on parental savings, and total resources accumulated (Section 6.1). Second, we test whether increased education savings come at the expense of other household savings (Section 6.2). Third, we investigate spillover effects to older siblings' RESP accounts (Section 6.3). Finally, we present effects on bachelor's degree enrollment for both eligible children and their older siblings (Section 6.4).

6.1 RESP Outcomes

Figure 1 displays average outcomes by children's month of birth for six key RESP-related variables, with vertical dashed lines marking the January 1, 2003 placebo cutoff and the January 1, 2004 eligibility cutoff. Panels A and B plot CLB receipt rates and grant amounts;

Panel C shows RESP account ownership rates; Panels D and E show parental contributions (participation rates and average amounts); and Panel F shows total resources (contributions plus grants). The figure provides a visual representation of the discontinuous jump at the eligibility cutoff and helps us assess the validity of the research design.

Table 3 reports the corresponding regression-based treatment effect estimates in three panels: Panel A presents effects on CLB receipt and RESP account ownership; Panel B reports effects on parental contributions; and Panel C shows effects on matching grants and total resources. Column (2) presents our baseline difference-in-discontinuities estimates of the effect of CLB eligibility. Columns (3)–(5) assess robustness to alternative specifications and bandwidth choices, while columns (6) and (7) report placebo tests that support the validity of the research design. Column (1) reports baseline means for children born in the second half of 2003, who constitute the last group unexposed to the CLB.

Note that all outcomes are measured cumulatively from birth through age 17, capturing the full period during which families can open accounts, receive CLB deposits, and accumulate savings before typical college entry.

6.1.1 Take-up

We begin by examining take-up of the CLB. We find that 49% of eligible children receive CLB deposits in an RESP account (Panel A of Table 3, column 2). The pattern depicted in Panels A and B of Figure 1 is stark: among low-income families, no children born before January 2004 receive the CLB, while approximately 49% of those born after the cutoff do. Correspondingly, average cumulative CLB deposits jump discretely from zero to roughly $740 at the January 1, 2004 cutoff (Figure 1, Panel B; Table 3, Panel A, column 2). This implies a treatment-on-the-treated effect of approximately $1,500, which is smaller than the program maximum of $2,000, reflecting the fact that not all eligible children who receive the CLB remain income-eligible for the full 15-year period during which CLB deposits can be made.

Because CLB receipt requires opening a RESP, take-up is intrinsically linked to RESP

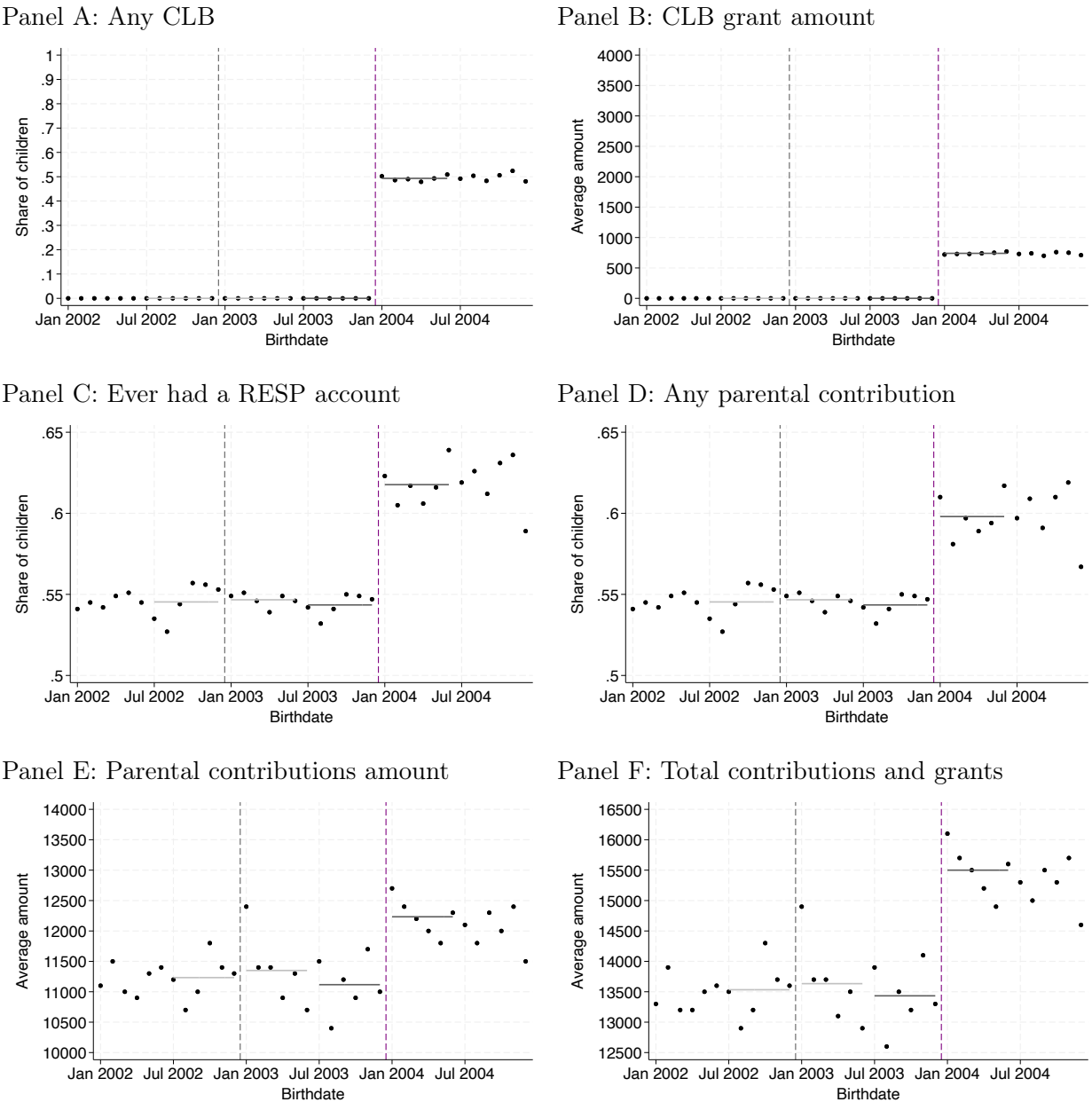


Figure 1: Treatment Effects on RESP Outcomes

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 4. 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). Source: BC K–12 data linked to CESP data.

Table 3: Treatment Effects on RESP Outcomes

			Alternative specifications			Placebos	
Outcome	Baseline mean (1)	Main RD-DD (2)	3-month BW (3)	Linear MoB (4)	RD-DDD (5)	High-income (6)	Jan 1st, 2003 (7)
Panel A: RESP account ownership and CLB receipt							
Any CLB	0.000	0.492***	0.492***	0.488***	0.467***	0.025***	-0.000
		(0.004)	(0.006)	(0.009)	(0.005)	(0.002)	(0.000)
CLB grant amount ($)	0.000	738***	727***	713***	721***	17***	.
		(7.6)	(10.6)	(15.2)	(7.7)	(1.5)	.
Ever had a RESP account	0.543	0.073***	0.076***	0.074***	0.068***	0.006	-0.007
		(0.008)	(0.012)	(0.017)	(0.013)	(0.011)	(0.008)
Panel B: Parental contributions							
Any parental contribution	0.543	0.053***	0.057***	0.058***	0.051***	0.002	-0.007
		(0.008)	(0.012)	(0.017)	(0.013)	(0.011)	(0.008)
Parental contributions amount ($)	11,100	1,030***	1,068***	823	672	358	-94
		(271)	(390)	(561)	(563)	(493)	(269)
Panel C: Other RESP amounts							
CESG grant amount ($)	2,300	236***	231***	182*	179*	56	-20
		(53)	(76)	(109)	(104)	(90)	(52)
Total contributions and grants ($)	13,400	2,004***	2,026***	1,718**	1,572**	432	-114
		(324)	(467)	(671)	(666)	(582)	(320)
Sample size		56,230	27,660	56,230	80,430	24,200	56,440

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 4. 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)). Columns (6) and (7) report placebo tests based on estimating Equation (1) for children from high-income (ineligible) families and using a fake eligibility cutoff at January 1, 2003, respectively. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: BC K–12 data linked to CESP data.

account ownership (Figure 1, Panel C; Table 3, Panel A). For cohorts born prior to 2004, RESP participation among low-income families is already substantial: 54% of children have

an RESP opened in their name even in the absence of the CLB (Table 3, column 1). This relatively high baseline reflects the effect of pre-existing matching grants (the CESG) that have been available since 1998. CLB eligibility nevertheless generates meaningful induced entry into the RESP system: we find that CLB eligibility increases the likelihood that a child ever has an RESP account by 7.3 percentage points (Table 3, Panel A, column 2). This increase reflects families who open an RESP specifically in response to the availability of the CLB.

Conditional on having an RESP account and being eligible based on income, take-up of the CLB is high but not universal. Among low-income children in the 2004 birth cohort with an RESP, approximately 80% (49/(54+7)) receive the CLB. The remaining gap may reflect incomplete paperwork, accounts opened by non-primary caregivers, or measurement error in the administrative records.

Taken together, these results allow us to decompose the 49% CLB take-up rate among eligible children into distinct components. 42% receive the CLB through RESP accounts that would have been opened even in the absence of the program (inframarginal take-up), while an additional 7% receive the CLB because their families open an RESP specifically in response to CLB eligibility (induced take-up). The remaining 51% of eligible children do not receive the CLB: either because no RESP is ever opened (39% of eligible children) or because an RESP is opened but the benefit is not successfully claimed (12% of eligible children).

Column (6) of Table 3 provides a validity check using high-income (ineligible) families. We find a small but statistically significant CLB receipt rate of 2.5% in this group, likely reflecting measurement error in income classification or administrative targeting errors. This rate is reassuringly small. Importantly, we find no significant RESP account opening response among high-income families, confirming that the behavioral responses we document are concentrated among eligible families.

6.1.2 Effects on Parental Savings

We next investigate the effects of CLB eligibility on private parental savings in the RESP account of the eligible child (Figure 1, Panel D; Table 3, Panel B). As discussed in the introduction, 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 saving accordingly. Second, the CLB may have no effect on private contributions if families induced to open an RESP do so solely to access the grant, as CLB receipt does not require parental contributions, while inframarginal families maintain their pre-existing saving behavior. Third, although stimulating parental savings was not an explicit objective of the CLB, the CLB may crowd in private savings if it triggers behavioral responses—such as increased salience of postsecondary education, reduced administrative barriers, or shifts in expectations about children's educational trajectories—that lead parents to increase their own contributions.

The data strongly support the third scenario: the CLB crowds in private savings. 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 implies that 72% (5.3/7.3) of parents induced to open an RESP by the CLB also voluntarily contribute to it. This behavioral response is striking for two reasons. First, the CLB requires no parental contributions whatsoever: parents could simply collect the grant and contribute nothing. That 72% choose to contribute anyway reveals that the program activates saving behavior, not merely account opening. Second, these are the same families who remained unresponsive to generous matching grants (up to 40%) that had been available for all children in our sample. The CLB succeeds in generating parental savings where traditional matching incentives failed, suggesting it works through different mechanisms: reducing administrative barriers, increasing the salience of postsecondary education savings, or reshaping expectations about children's educational trajectories.

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, this implies a treatment-on-the-treated effect of approximately $2,100 ($1,030/0.49) for children who actually receive the benefit. This total effect has two potential sources: newly participating families contributing for the first time (the 5.3 percentage point extensive margin response) and existing savers adjusting their contribution levels (intensive margin). While we cannot precisely disentangle the two sources, the total effect demonstrates substantial crowding-in of private savings.

Panels D and E of Figure 1 illustrate these patterns graphically. Panel D shows a discrete jump at January 2004 in the share of children receiving any voluntary contributions, mirroring the pattern for RESP account ownership but with a slightly smaller magnitude. Panel E reveals a corresponding increase in contribution amounts, with average cumulative contributions jumping from approximately $11,000 to $12,000 at the eligibility cutoff. Notably, both panels show smooth trends on either side of the cutoff, with only a small discontinuity at the January 2003 placebo cutoff, supporting the validity of our research design.

These crowding-in effects are robust across specifications. The 5.3 percentage point increase in parental contribution probability ranges from 5.1 to 5.8 across alternative bandwidths and specifications (Table 3, Panel B, columns 3-5). The effect on contribution amounts ($1,030) is more sensitive to specification choice, ranging from $672 in the RD-DDD specification to $1,068 with a narrower bandwidth, reflecting the fact that it is a noisier outcome. Importantly, the placebo tests show no significant effects. Column (6) finds small and insignificant effects for high-income families on both participation and contribution amounts, while column (7) yields precise zeros when estimating the same RD-DD using the January 2003 fake cutoff.

6.1.3 Total Effect on Resources

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 (driven partly by the

large extensive-margin response). We next examine how these components combine to affect total resources accumulated in RESP accounts by age 17 (Panel C of Table 3).

Because CLB eligibility increases parental savings, it also increases receipt of matching grants through the Canada Education Savings Grant (CESG) program. We find that CLB eligibility increases cumulative CESG receipts by $236 by age 17. This represents an indirect effect: families contribute more, which in turn generates additional matching grants.

Combining all components, CLB eligibility increases total contributions and grants (parental contributions plus CLB deposits plus CESG matching grants) by $2,004 by age 17 (Panel C, column 2), a 15% increase relative to the baseline mean of $13,400. Panel F of Figure 1 illustrates this total effect graphically, showing a clear discontinuous increase at the January 1, 2004 eligibility cutoff. Since 49% of eligible children receive the CLB, this implies a treatment-on-the-treated effect of approximately $4,000 ($2,004/0.49) 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. These estimates therefore provide a lower bound on the total increase in RESP resources, as they exclude any investment returns earned on the accumulated funds. These effects are robust across specifications, with the total resources estimate ranging from $1,572 to $2,026 (Table 3, Panel C, columns 2-5). Placebo tests show no significant effects.

The total effect can be decomposed into three sources: 37% from CLB deposits, 51% from additional parental contributions, and 12% from induced CESG matching grants. In other words, the CLB generates a substantial multiplier effect: each dollar the government deposits through the CLB generates $2.7 in total resources.[17]

[17]The multiplier is computed as follows. For each dollar deposited through the CLB, parents contribute an additional $1.40 in induced savings ($1,030 / $738) and matching grants on these contributions add $0.32 ($236 / $738). The total resources generated per dollar of government spending are therefore $1.00 (CLB) + $1.40 (parental contributions) + $0.32 (matching grants) = $2.72.

6.2 Crowding Out of Other Household Savings

A remaining concern is whether increased education savings come at the expense of other household savings. If parents simply reallocate funds from retirement or other savings accounts to RESPs, the net effect on household wealth and resources available for education would be smaller than the effects presented above suggest.

To assess whether CLB-induced education savings crowd out saving in other household accounts, we turn to administrative tax data from the Longitudinal Administrative Data Bank (LAD), a 20% longitudinal random sample of all Canadian tax filers. Unlike the data used in our main analysis, the LAD provides comprehensive information on savings across all account types.[18] Specifically, we observe contributions to Tax-Free Savings Accounts (TFSAs)—tax-advantaged general savings accounts similar to Roth IRAs in the United States—and Registered Retirement Savings Plans (RRSPs)—tax-deferred retirement accounts similar to traditional IRAs or 401(k)s. We also observe investment income from non-registered taxable accounts. Together, these variables allow us to evaluate the extent of crowding out from other forms of household saving.

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. That is, we compare outcomes for parents who have a child born just after versus just before January 1, 2004, using the same difference-in-discontinuities design. We focus on cumulative savings and contributions measured from the child's birth through age 17, as well as snapshot account balances in 2019 (a pre-pandemic year).

Table 4 presents consistent null results across all savings vehicles and specifications. For TFSAs (Panel A), the point estimates for participation, cumulative contributions, and 2019 market values are all small relative to their standard errors and economically insignificant relative to baseline means. For instance, the main estimate for total TFSA savings is $296

[18]While our main analysis uses BC K–12 school records linked to parental tax data, this linkage provides only a limited subset of tax variables (primarily income measures). Information on household savings is not available in these linked data, necessitating the use of the LAD for this analysis.

(SE=$323) relative to a baseline mean of $9,900, less than 3% of the baseline. Similarly, for RRSPs (Panel B), we find small and insignificant effects on both participation (-0.8pp, SE=0.6pp) and total savings (-$310, SE=$742), relative to a baseline mean of $29,000. Panel C shows similarly null results for investment income from taxable accounts (-$10, SE=$21), relative to a baseline of $280. The consistency of these null results across TFSAs, RRSPs, and taxable investment income provides strong evidence that CLB-induced RESP savings represent net new household wealth rather than reallocation from other accounts.

The absence of crowding out suggests that additional RESP savings come from new sources rather than reallocation of existing financial investments. The most likely sources are reduced consumption, increased earnings, or the mobilization of funds that were previously held in non-interest-bearing accounts unobserved in tax returns. The CLB may have induced these responses through several channels: by making postsecondary education feel more attainable, by increasing the salience of education savings once families open an account, by creating a commitment device through recurring contributions, or by improving financial literacy and encouraging families to invest idle cash rather than leaving it in checking accounts.

Table 4: Treatment Effects on Other (Non-RESP) Savings

			Alternative specifications		
Outcome	Baseline mean (1)	Main RD-DD (2)	3-month BW (3)	Linear MoB (4)	RD-DDD (5)
Panel A: Savings in TFSAs (tax-advantaged savings accounts)					
Any savings	0.50	-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 4. 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.

6.3 Spillover Effects to Siblings

We now examine whether the effects of CLB eligibility extend beyond the eligible child to affect savings for older, ineligible siblings in the same household. As described before, we exploit the same January 1st, 2004 cutoff but now compare older siblings whose youngest sibling was born just before versus just after this date.

Figure 2 provides clear visual evidence of 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.

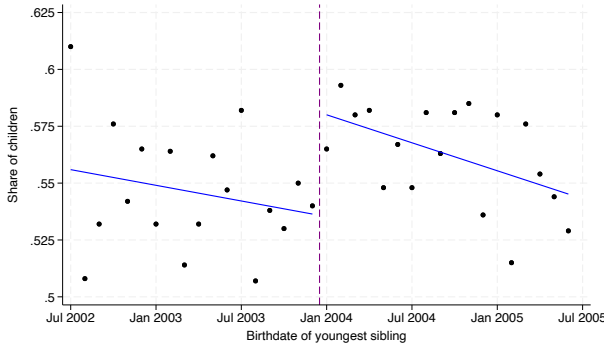
Table 5 quantifies these effects. Column (1) reports our main estimates using an 18-month bandwidth. Having a CLB-eligible youngest sibling increases the probability that parents open an RESP for an older sibling by 3.8 percentage points. Notably, the effect on the probability of any parental contribution is identical. Since older siblings are ineligible for the CLB and receive no government grants, there is no incentive to open an empty account and account opening necessarily implies voluntary contributions. Beyond this extensive margin effect, parents also contribute an additional $1,300 to older siblings' accounts on average, reflecting both contributions by newly participating families and potentially higher contribution amounts among families who would have contributed regardless. Column (6) provides a useful benchmark by reporting the effect of CLB eligibility on the youngest (eligible) child in these same families, using the RD-DD design. The effect differs slightly from our main estimates reported in Section 6.1.

The estimates are stable across bandwidths (Table 5, columns 1-5). Using bandwidths ranging from 12 to 24 months, the effect on RESP account ownership varies from 2.7 to 3.8 percentage points, all statistically significant and economically meaningful. The effect on

contribution amounts similarly ranges from $1,027 to $1,523, clustering around $1,200-$1,300 in most specifications. This stability across bandwidth choices strengthens confidence in the findings.

These spillover effects provide compelling evidence of household-level behavioral responses to the CLB. Since older siblings receive no direct financial benefit—they are ineligible for the CLB by birth date—the only channel through which the program could affect their outcomes is through changes in household awareness, expectations, or financial planning behavior. The fact that spillovers are roughly half the magnitude of direct effects (comparing columns 1 and 6 of Table 5) suggests a mix of child-specific and household-level responses. Parents may treat the CLB as a signal to increase education savings for all children, not just the eligible one, perhaps because it raises the salience of postsecondary education or because opening an RESP for one child reduces the administrative costs of opening accounts for others. We return to the implications of these spillovers when examining effects on college enrollment below.

Panel A: Ever had a RESP account



Panel B: Parental contributions amount

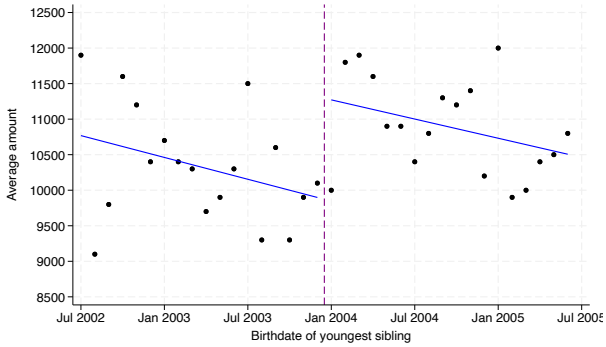


Figure 2: Treatment Effects on Older Siblings' RESP outcomes

Notes: This figure plots average RESP 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 4, based on the eligibility history of the youngest child. 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 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 between 1995 and 2004, before the CLB eligibility cutoff, making them ineligible for the CLB. Heteroskedasticity-robust standard errors are reported in parentheses. Source: BC K–12 data linked to CESP data.

Table 5: Treatment Effects on Older Siblings' RESP Outcomes

	Main RD	Varying bandwidths				Effect on main child (same sample)
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Ever had a RESP account	0.038***	0.029*	0.037***	0.032***	0.027**	0.081***
	(0.013)	(0.016)	(0.014)	(0.012)	(0.011)	(0.018)
Any parental contribution	0.038***	0.029*	0.037***	0.032***	0.027**	0.068***
	(0.013)	(0.016)	(0.014)	(0.012)	(0.011)	(0.018)
Parental contributions amount ($)	1,300***	1,246***	1,523***	1,183***	1,027***	1,581***
	(392)	(480)	(428)	(363)	(342)	(588)
Bandwidth (months)	18	12	15	21	24	.
Sample size	23,680	15,970	19,850	27,500	30,940	12,180

Notes: This table shows the spillover effects of CLB eligibility on RESP outcomes for older siblings. The sample is restricted to low-income families as defined in Section 4, based on the eligibility history of the youngest child. Older siblings are born between 1995 and 2004, before the CLB eligibility cutoff, making them ineligible for the CLB. All outcomes are cumulative and measured from birth up to age 17 for each older sibling: indicators equal one if the event ever occurs, and amounts are cumulative totals expressed in 2024 Canadian dollars. Columns (1) through (5) report regression discontinuity estimates that compare outcomes for older siblings whose youngest sibling was born just after versus just before January 1, 2004, using varying bandwidths around the cutoff (Equation (3)). Column (1) uses an 18-month bandwidth (the main specification), while columns (2) through (5) vary the bandwidth from 12 to 24 months as a robustness check. Column (6) reports the difference-in-discontinuities estimate of the effect of CLB eligibility on the youngest child's RESP outcomes for the same sample of families as column (1), for comparison. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: BC K–12 data linked to CESP data.

6.4 Effects on College Enrollment

Our results so far establish that CLB eligibility substantially increases RESP resources, both for eligible children and their older siblings, without reducing other forms of household savings. We now examine whether these increases in education savings translate into higher college enrollment.

Table 6 presents the estimated effects of CLB eligibility on bachelor's degree enrollment. Panel A reports unconditional enrollment outcomes, measured at age 18, the typical age of college enrollment. Panel B reports outcomes conditional on enrollment, capturing program choices among students who enroll. Column (1) reports baseline means for children born in the second half of 2003. Columns (2) and (3) present RD-DDD estimates for the main (eligible) child using six-month and three-month bandwidths, respectively. Columns (4)-(6) report RD estimates for older siblings using varying bandwidths around the youngest sibling's birth date.

6.4.1 Eligible Children

We begin by examining enrollment effects for eligible children. CLB eligibility increases bachelor's degree enrollment by 4.6 percentage points (Panel A, column 2), a 19% increase relative to the 24% 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): STEM enrollment increases by 2.7 percentage points (27% relative to baseline), and U15 enrollment increases by 2.2 percentage points (22% relative to baseline). Column (3) uses a narrower three-month bandwidth, and the qualitative conclusions remain unchanged.

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 the interpretation that students induced to enroll by the CLB choose programs similar, on

Table 6: Treatment Effects on Bachelor's Degree Enrollment

		Main child: RD–DDD estimate		Older sibling: RD estimate		
Outcome	Baseline mean (1)	Main RD--DDD (2)	3-month BW (3)	Main RD (4)	12-month BW (5)	24-month BW (6)
Panel A: Unconditional enrollment outcomes:						
Any enrollment	0.24	0.046***	0.048**	0.024**	0.018	0.017*
		(0.015)	(0.021)	(0.011)	(0.014)	(0.010)
Full-time enrollment	0.19	0.043***	0.050**	0.025**	0.016	0.016*
		(0.014)	(0.020)	(0.010)	(0.013)	(0.009)
STEM field	0.10	0.027**	0.040***	0.009	0.001	0.007
		(0.011)	(0.015)	(0.008)	(0.010)	(0.007)
U15 university	0.10	0.022**	0.017	0.017**	0.004	0.013*
		(0.011)	(0.015)	(0.008)	(0.009)	(0.007)
Sample size		80,430	39,120	23,680	19,850	27,500
Panel B: Outcomes conditional on bachelor's degree enrollment:						
Full-time enrollment	0.80	0.017	0.033	0.024	0.011	0.012
		(0.020)	(0.029)	(0.022)	(0.026)	(0.019)
STEM field	0.42	0.017	0.056	0.000	-0.021	0.002
		(0.026)	(0.038)	(0.026)	(0.032)	(0.023)
U15 university	0.41	0.025	0.005	0.033	-0.005	0.027
		(0.026)	(0.038)	(0.026)	(0.032)	(0.022)
Sample size		23,790	23,790	5,920	4,000	7,710

Notes: This table reports the effects of CLB eligibility on bachelor's degree enrollment outcomes for the main sample of children, as well as spillover effects on older siblings. 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 4. Column (1) reports baseline means for children born between July and December 2003. Columns (2) and (3) report difference-in-difference-in-discontinuities (RD–DDD) estimates for the main child, corresponding to Equation (2). Column (2) uses a symmetric six-month bandwidth around the January 1 cutoff, while column (3) uses a narrower three-month bandwidth. The specification and sample in column (2) correspond to those used for the RD–DDD estimate reported in column (5) of Table 3. Columns (4) through (6) report regression discontinuity (RD) estimates for older siblings, corresponding to Equation (3). These estimates compare outcomes for older siblings whose youngest sibling was born just after versus just before January 1, 2004. Column (4) uses an 18-month bandwidth (the main specification), while columns (5) and (6) vary the bandwidth to 12 and 24 months as robustness checks. The specification and sample restrictions in columns (4) through (6) mirror those used in Table 5. Panel A reports unconditional enrollment outcomes. Panel B reports outcomes conditional on bachelor's degree enrollment. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: BC K–12 data linked to PSIS data.

average, to those chosen by other enrollees, suggesting that the CLB primarily affects the likelihood of college entry, with limited observable effects on program composition among enrolled students. However, it is also possible that offsetting changes in program choice among inframarginal students and newly induced entrants generate a net zero effect conditional on enrollment.

6.4.2 Older Siblings

Columns (4)-(6) report enrollment effects for older siblings, who are ineligible for the CLB but benefit from increased parental saving induced by their younger sibling's eligibility. These spillover effects provide a particularly clean test of whether induced savings, rather than the CLB grant itself, affect enrollment, since older siblings receive no direct financial transfer from the program.

The results indicate meaningful spillovers. Having a CLB-eligible younger sibling increases older siblings' probability of enrolling in a bachelor's degree program by 2.4 percentage points (column 4), roughly 10% relative to baseline enrollment rates. The effect on full-time enrollment is similar. These estimates are approximately half the magnitude of the direct effect on eligible children, mirroring the pattern observed for savings outcomes in Table 5.

The spillover estimates show some sensitivity to bandwidth choice. The main estimate using an 18-month bandwidth finds statistically significant effects at the 5% level (column 4). With a narrower 12-month bandwidth (column 5), the estimates become smaller and less precise (1.8pp for any enrollment, 1.6pp for full-time), though the point estimates remain positive and similar in magnitude. This sensitivity likely reflects the smaller sample sizes in the sibling analysis, which reduce statistical power, particularly with narrower bandwidths. The 24-month bandwidth (column 6) yields estimates similar to the main specification (1.7pp and 1.6pp, significant at the 10% level). Despite this variation, the overall pattern points toward positive spillover effects on enrollment.

The enrollment effects for older siblings are informative about mechanisms. Since these

siblings receive no CLB grant, the only channel through which their younger sibling's CLB eligibility could affect their outcomes is through changes in household behavior, most plausibly the increased parental savings documented in Section 6.3. The fact that we observe enrollment gains for ineligible siblings suggests that induced savings, not merely the direct transfer, expand college access.

To compare the effects for eligible children and older siblings, we can scale them by the change in total RESP resources (parental contributions plus government grants). For older siblings, CLB eligibility of the youngest child increases parental contributions by approximately $1,300 (Table 5) and grants by approximately $270 (not shown), for a total increase of roughly $1,570 in RESP resources. This is associated with a 2.4 percentage point increase in bachelor's enrollment (Table 6, column 4), implying approximately 1.5 percentage points per $1,000 in additional RESP resources. For eligible children, CLB eligibility increases parental contributions by approximately $1,030 and total grants by approximately $980 (including the CLB amount and additional matching grants), for a total increase of roughly $2,000 in RESP resources (Tables 3 and 5). The estimated enrollment effect is 4.6 percentage points (Table 6, column 2), implying approximately 2.3 percentage points per $1,000. These implied magnitudes are of the same order, though not identical. The difference in magnitude is not surprising: eligible children receive a larger share of the resource increase in the form of grants rather than parental contributions. Unlike parental contributions, grant amounts can only be withdrawn upon postsecondary enrollment, which may strengthen their incentive effect on enrollment decisions. However, the similarity in the enrollment effects per dollar also suggests that increased savings is an important dimension of the increase in enrollment.

6.5 Cost-Benefit Analysis

From the government's perspective, the CLB generates substantial enrollment effects relative to its cost. Eligible children in our sample have, on average, approximately $1,000 in CLB and other grant funds deposited in their RESP accounts (accessible only upon enrollment),

generating an estimated enrollment effect of 4.6 percentage points per $1,000 of aid eligibility.

This compares favorably to existing evidence on traditional grant aid programs in the United States. A large body of research 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). 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 two years on average, the upper end of these estimates (6 percentage points per $1,000 annually) translates to 3 percentage points per $1,000 in total aid eligibility. Our estimate of 4.6 percentage points per $1,000 of aid eligibility thus compares favorably to traditional student grant programs delivered at the time of college entry.

While this comparison is necessarily imperfect since the programs differ greatly in institutional context, it suggests that delivering financial aid through college savings accounts can be a more cost-effective approach to expanding college access. This efficiency likely reflects how the program induces substantial additional parental saving beyond the initial grant, effectively leveraging private contributions with public dollars.

7 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 RESP 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 reveals 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. *Induced parental saving is key.* 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 provides clean evidence that induced parental saving itself expands college access, not merely the direct transfer.

Our results also have implications for inequality. The CLB, which targets children from lower-income families, substantially narrows the gap in college savings and enrollment relative to higher-income families. At baseline, children from high-income families have RESP accounts opened at a rate of 78%, hold $20,800 in parental savings, and enroll in bachelor's degree programs at a rate of 39%. The CLB narrows these gaps considerably: the RESP ownership gap is reduced by 30% (from 24.0 to 16.7 percentage points), the parental savings gap is reduced by 11% (from $9,700 to $8,700), the total resources gap is reduced by 18% (from $11,400 to $9,400), and the bachelor's degree enrollment gap is reduced by 32% (from 14.3 to 10.4 percentage points). These effects are particularly important given longstanding concerns that tax-advantaged college savings accounts primarily benefit higher-income families.

Our study has several limitations that point to important directions for future research. First, our findings emerge from a context where baseline RESP participation is relatively high (54% among low-income families) 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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Supplementary Material

A Additional Tables and Figures

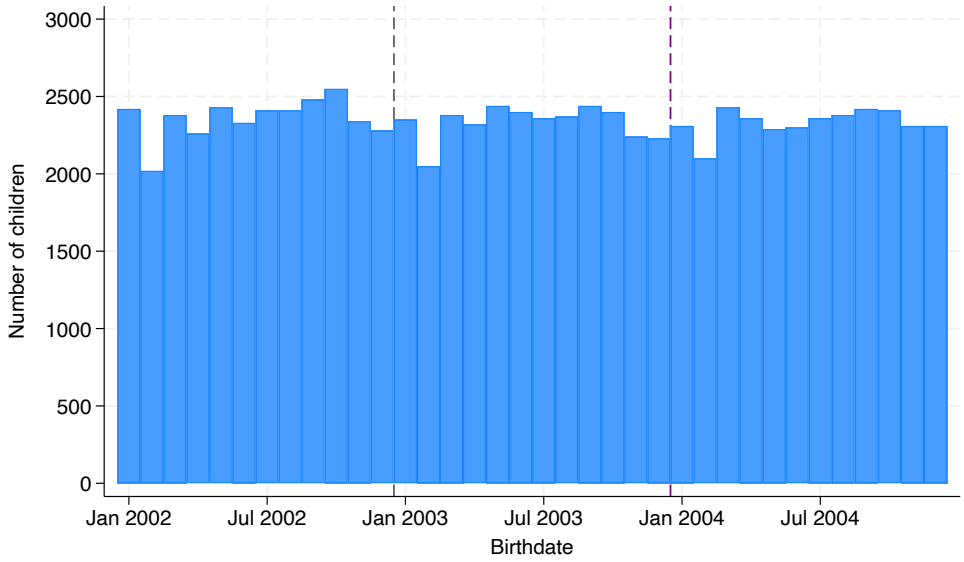


Figure A.3: Distribution of Date of Birth

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.

Table A.7: 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 A.8: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.