Essays in Applied Microeconomics

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Abstract

This dissertation is composed of three essays that strive to understand how public policies can empower disadvantaged groups and help them overcome the barriers they face.

In the first essay, I investigate the effectiveness of two interventions aiming to lower the barriers to college enrollment faced by low-income students. In particular, I study the effects of the Future to Discover Project, a randomized experiment in which Canadian high school students were either invited to participate in career planning workshops or were made eligible for an $8,000 college grant. By matching the experimental data to post-secondary institution records and income tax files, I am able to examine and compare the effects of the interventions on college enrollment, graduation, and earnings in adulthood. This chapter also provides new insights about the drivers of the income gradient in educational attainment.

The second essay studies the effects of a large-scale national conditional cash transfer program—Peru's Juntos program—on women's empowerment and fertility outcomes. To identify the causal effects, we exploit time and geographic variation from the roll-out of the program. We investigate the effects on fertility behavior and explore various mechanisms explaining the effects—including women's empowerment—using data on preferences and decision-making.

The last essay takes a more theoretical approach and investigates parenting style's influence on children's cognitive ability. To do so, we classify a sample of Canadian parents into distinct parenting behavioral types using an unsupervised machine learning algorithm and objective data on how they engage with their children. The classification allows us to study the link between parental socio-economic characteristics, parenting styles, and children's cognitive ability.

Abrégé

Cette thèse est composée de trois essais qui s'efforcent de comprendre comment les politiques publiques peuvent aider les groupes défavorisés à surmonter les obstacles auxquels ils sont confrontés.

Dans le premier chapitre, j'étudie l'efficacité de deux interventions visant à réduire les obstacles à l'inscription à l'université. En particulier, j'étudie les effets du projet Un avenir à découvrir, une expérience randomisée canadienne qui a donné la chance à des élèves du secondaire de participer à des ateliers d'orientation scolaire et/ou de recevoir une bourse universitaire de 8 000 $. En fusionnant les données expérimentales aux dossiers des établissements postsecondaires et aux fichiers d'impôt sur le revenu, je suis en mesure d'examiner et de comparer les effets des interventions sur l'inscription à l'université, l'obtention de diplôme et les revenus à l'âge adulte. Ce chapitre apporte également de nouvelles perspectives sur les inégalités des chances face aux études supérieures.

Le deuxième chapitre étudie les effets d'un programme national de transfert conditionnel de fonds – le programme Juntos du Pérou – sur l'autonomisation des femmes et la fécondité. Pour identifier les effets causaux, nous exploitons les variations temporelles et géographiques du déploiement du programme. Nous étudions les effets sur la prise de contraception et la fécondité et explorons divers mécanismes – y compris l'autonomisation des femmes – en utilisant des données sur les préférences et la prise de décision.

Le dernier chapitre adopte une approche plus théorique et étudie l'importance des pratiques éducatives parentales dans le développement des capacités cognitives des enfants. Pour ce faire, nous classifions un échantillon de parents canadiens entre deux groupes distincts de pratiques éducatives parentales à l'aide d'un algorithme d'apprentissage automatique non supervisé et de données objectives sur la façon dont ces parents interagissent avec leurs enfants. La classification nous permet d'étudier le lien entre les caractéristiques socio-économiques des parents, les pratiques éducatives parentales et les capacités cognitives des enfants.

Acknowledgment

First of all, this thesis would not have been possible without the inspiration and support of my advisors, Fabian Lange and Fernando Saltiel. I am specifically grateful to Fabian Lange for his continued guidance and immense knowledge, and to Fernando Saltiel for his advice, patience, and enthusiasm before and during the job market.

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Many thanks to Laura Lasio, Francesco Amodio, Nicolas Gendron-Carrier, Markus Poschke and Christian Belzil for their insightful comments and suggestions throughout my Ph.D. My gratitude also extends to all the participants at the 2021 annual conferences of the European Society for Population Economics, the Canadian Economics Association, the Society of Labor Economists, the European Economic Association, and the Econometric Society, as well as, to all the participants at the GEEZ Seminars, AMIE Workshop, and RCI Workshop.

I am indebted to the Social Research and Demonstration Corporation team, especially to Reuben Ford, for helping me get access to the Future to Discover project data and for answering my numerous questions on the experiment.

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Statistics Canada.

Special thanks to my fellow graduate students. In particular, I am grateful to Jean-Louis Barnwell and Jean-François Fournel for the many lunches and drinks that were always a great source of encouragement and distraction; and I am grateful to Andrei Munteanu for his considerable support during the job market. These past six years would have been very different without them. I would also like to express my gratitude to all my friends here in Montreal and in Europe for their invaluable moral comfort and inspiration.

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Contribution to Original Knowledge

The second chapter of the thesis contributes to knowledge in three ways. First, it contributes to the scarce literature on the long-term effects of interventions promoting college access. While previous research has shown the effectiveness of career counseling programs in increasing the college enrollment rate of low-income students (e.g., Bettinger et al. (2012); Carrell and Sacerdote (2017); Castleman and Goodman (2018); Cunha, Miller, and Weisburst (2018); Oreopoulos and Ford (2019)), little is known about their long-term effects.[1] I show that career counseling programs are not only effective in increasing low-income students' college enrollment rates but are also powerful in improving students' outcomes in the long run.

The literature on the effects of student grant aid is more extensive. Numerous studies have shown the effectiveness of these grants in increasing both college enrollment and completion (e.g., Fack and Grenet (2015); Castleman and Long (2016), Goldrick-Rab et al. (2016)), and a handful of recent evaluations have shown small but positive effects of grant aid on earnings (Bettinger et al. (2019), Denning, Marx, and Turner (2019); Scott-Clayton and Zafar (2019))[2]. However, the estimates of the treatment effects on earnings are often imprecise and specific to the United States. This chapter shows that providing students with additional financial support, in a country where a number of grants and loans are already available to the students, has no long-term monetary benefits.

Second, it adds to the understanding of the factors contributing to the gap in educational attainment by parental income. To date, there is little consensus about the role played by credit constraints (e.g., Keane and Wolpin (2001); Belley and Lochner (2007); Lochner and Monge-Naranjo (2012)). Moreover, although recent studies have demonstrated the

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2. See Eng and Matsudaira (2021) for an exception.

existence of informational and behavioral barriers for low-income students (e.g., Bettinger et al. (2012); Hoxby and Avery (2013); Dynarski et al. (2021)), little is known on the extent to which they contribute to the gap. This chapter provides new evidence that informational and behavioral barriers explain most of the gap in four-year college enrollment between equally-achieving high- and low-income students.

More generally, this chapter relates to the literature on the fading-out of educational interventions. As Bailey et al. (2020) explains, well-timed interventions introducing the "right" institutional changes are more likely to lead to persistent effects than interventions targeting skills directly. Consistent with this finding, I find that the career education intervention, which tackles informational and behavioral barriers, had large persistent effects on graduation and earnings.

The second chapter adds to two separate strands of the social protection literature. First, it contributes to our understanding of the cumulative effects of cash transfers by exploiting recent advances in staggered treatment effect models. As Cahyadi et al. (2020) discuss, achieving intergenerational poverty reduction is a cumulative process and temporary investments may be of little benefit. Measuring the cumulative effects of cash transfers is challenging. It requires not only that we study longer term effects but do so in a setting where cash transfers have been offered regularly over time and where a pure control group exists for long enough to identify the cumulative effects. Like Cahyadi et al. (2020), we are able to investigate cumulative effects of a national, government-run, CCT program. While their study of Indonesia's CCT program finds strong cumulative effects on child health and education outcomes but limited long-term economic effects for households, we find cumulative effects on fertility. Given the role that lower fertility is believed to have in reducing poverty (Birdsall and Griffin (1988); Sinding (2009)), this result provides new evidence of potentially transformative and long-term effects of anti-poverty programs.

Second, the study adds evidence of the effect of CCTs on secondary outcomes – that is, outcomes that are not directly incentivized by design: since the transfer is conditional on child schooling and pre-natal and infant health check, any effect on fertility and birth control would be indirect. Indeed, recent literature has documented different dimensions in which cash transfers may have unintended effects (Bastagli et al. (2016)), including on adult fertility (Perova and Vakis (2012); Alencastre Medrano and Del Pozo Loayza (2017); Stecklov et al. (2007); Garganta et al. (2017); Nandi and Laxminarayan (2016); Carneiro et al. (2021)). No clear consensus emerges from this literature. For instance, while Stecklov et al. (2007) find positive or nil effects on fertility outcomes from CCTs in Honduras, Mexico and Nicaragua, Todd, Winters, and Stecklov (2012) find an increase in (short-term) birth spacing in Nicaragua between 2000 and 2004. In the Peruvian context, two studies show positive effects of Juntos on contraceptive use, but either do not investigate the effects on fertility outcomes or provide possible explanation for the channels through which birth control use is affected (Perova and Vakis (2012)) or consider only static effects in shorter reference period (Alencastre Medrano and Del Pozo Loayza (2017)). Our study contributes to this literature by looking at a more comprehensive set of fertility outcomes, cumulative effects, and by delving into a broader set of mechanisms including those related to intrahousehold bargaining issues (e.g. Ashraf, Field, and Lee (2014)). Our results suggest that cash transfers can impact welfare in unintended ways by empowering women in taking control over their fertility.

The last chapter of the thesis contributes to the literature concerned about parenting styles.[3] They generally draw the distinction between parenting styles in terms of permissive, authoritarian, or authoritative. The empirical approaches tend to classify parenting styles based on a single binary response to a survey question, such as how important obedience is for a respondent (e.g., Agostinelli et al. (2020)) or latent factor models (e.g., Falk et al. (2021)). The approach undertaken in this fourth chapter allows capturing parenting styles based on many questions with complex interactions. Moreover, an advantage of the data used on parental activities is that they are not self-reported, but are observed and recorded by the enumerator, which should help to reduce systematic measurement error, and are the same set of actions observed across multiple survey waves. The new interpretable measure summarizing the large dimensionality and complexity of parental activities is predictive of human capital above and beyond the predictive power of parental socio-economic characteristics or child fixed effects.

Second, it adds to the rapidly growing use of machine learning in Economics to classify behavioral types. The latent Dirichlet allocation (LDA) was originally developed by computer scientists Blei, Ng, and Jordan (2003). The underlying idea is to classify text documents into a mixture of small number of topics. One key is that the topics are not predefined but are backed out through co-occurrence. I apply the same idea of topics to behavioral types. Other approaches to classifying behavioral types using LDA are Bandiera et al. (2020) who classify CEOs using detailed time-use surveys and find that CEOs distinct

3. One can roughly separate this literature into two strands: First, the literature relating parenting styles to child development (e.g., Cunha (2015), Doepke and Zilibotti (2019), Doepke, Sorrenti, and Zilibotti (2019), Cobb-Clark, Salamanca, and Zhu (2019), Agostinelli et al. (2020)). Second, the literature studying the role of parenting styles in the intergenerational transmission of traits (e.g., Brenøe and Epper (2019Zumbuehl, Dohmen, and Pfann [2021Falk et al. (2021)). Further, Del Boca et al. (2019) propose a model in which parental types are not merely the outcome of utility maximization by the parents but the result of a bargaining process with the children. Kiessling (2020) studies how parents perceive the returns to parenting styles in terms of warmth and control using hypothetical scenarios.

behavior affects firm performance. Draca and Schwarz (2018) use LDA to measure political ideology. We contribute to this literature by using LDA to classify parenting styles and look at its relation to human capital accumulation in very early childhood.

Contribution of Authors

This thesis is composed of three essays. The second essay is co-auhtored with Sonia Laszlo and Farhan Majid. They both came up up with the initial research idea and I contributed by developing and performing the empirical analysis. We edited the manuscript jointly.

The third essay is joint work with Christopher Rauh. He contributed to the research idea and manuscript editing, while I was in charge of coding and calibrating the machine learning algorithm at the Quebec Interuniversity Centre for Social Statistics.

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Chapter 1

General Introduction

Disadvantaged groups, such as women, ethnic minorities or low-income individuals, face a number of barriers—which can be informational, behavioral, or financial in essence—preventing them from making optimal decisions. This thesis investigates the importance of three specific types of barriers—barriers to college enrollment in chapter two, barriers to birth control use in chapter three, and barriers to human capital accumulation in chapter four—through the analysis of different public interventions (in chapters two and three) or the examination of individual behaviors (in chapter four).

In the second chapter "The Long-Term Effects of Financial Aid and Career Education: Evidence from a Randomized Experiment", I investigate the effectiveness of two interventions aiming to lower the barriers to college enrollment faced by low-income students. In particular, I study the effects of the Future to Discover Project, a randomized experiment in which Canadian high school students were either invited to participate in career planning workshops or were made eligible for an $8,000 college grant. By matching the experimental data to post-secondary institution records and income tax files, I am able to examine the effects of the interventions on college enrollment, graduation, and earnings in adulthood. I show that the career education intervention greatly improved students' outcomes in the long run by improving academic matching. In contrast, the college grant had no long-term monetary benefits despite increasing college enrollment, which is consistent with classical models of human capital investment in the absence of credit constraints. My findings suggest that informational frictions and behavioral obstacles——rather than financial constraints——represent the primary barrier to four-year college enrollment faced by low-income students. And that they explain a large part of the gap in four-year college enrollment between high- and low-income students.

In the third chapter, “Conditional Cash Transfers and Women’s Reproductive Choices”, we study potential unintended effects of a large-scale national conditional cash transfer program on women’s empowerment. In particular, we exploit time and geographic (district) variation in administrative data on the roll-out of Peru’s Juntos program to investigate the effects of the program on women’s fertility outcomes and behaviour. We find that Juntos led to a reduction in fertility at the intensive margin (number of children), with no effect on the extensive margin (any childbearing) and that the district rollout is associated with a 5 percentage point increase in the take-up of modern contraception, effects that persist over time. The persistence of these fertility and contraceptive use effects up to six years after the introduction of Juntos suggests that the program may have had long-term, transformational impacts on non-conditioned outcomes. By exploring various mechanisms, we do not find evidence that the program affected fertility preferences and intra-household bargaining power. Our findings rather suggest that Juntos empowered women to take control over their fertility by improving access and affordability of modern forms of birth control.

Finally, the last chapter “How to Measure Parenting Styles?” takes a more theoretical approach and investigates parenting style’s influence on children’s cognitive ability. To do so, we classify a sample of Canadian parents into distinct parenting behavioral types using an unsupervised machine learning algorithm—the latent Dirichlet allocation—and objective data on how the parents engage with their children. Our data is objective in the sense that we do not use parenting behavior self-reported by the parents but rather an evaluation from an external interviewer. The algorithm classifies parents into two distinct behavioral types: positive and negative. Parents of the positive type tend to respond to their children’s expressions in a supportive manner and describe to children features of their environment, while parents of the negative type are less likely to engage with their children in an encouraging manner. In the language of developmental psychology, positive parents exhibit both high warmth and control. We find that parenting styles are systematically related to socio-economic characteristics and positive parenting is more likely amongst educated mothers. Moreover, children of positive parents see their human capital improve relative to children of parents of the negative type—a correlation that hold even after controlling for parental socio-economic characteristics. Overall, the results suggest that parenting style is a possible determinant of human capital accumulation and a driver of socio-economic inequalities in educational attainment.

Chapter 2

The Long-Term Effects of Financial Aid and Career Education: Evidence from a Randomized Experiment

2.1 Introduction

Parental income is, across many countries, a strong predictor of post-secondary education enrollment.[1] In part, this stems from differences in academic preparation between students from high- and low-income families. But large differences remain even after controlling for academic achievement, raising concerns that students from low-income families might make sub-optimal educational choices due to financial, informational, or behavioral barriers (Lochner and Monge-Naranjo (2012); French and Oreopoulos (2017)).

In response to these concerns, governments and other institutions invest large sums in interventions promoting college access. These interventions can be broadly classified into the two following categories: outreach and career counseling interventions aimed at improving students' decision-making regarding post-secondary education; and financial aid interventions designed to help students cover the costs of post-secondary education (Page and Scott-Clayton (2016); Herbaut and Geven (2020)).

Two key questions emerge from the literature: 1) are these interventions effective in improving students' outcomes in the long run? and 2) what type of intervention is

1. See, for example, Bailey and Dynarski (2011) and Chetty et al. (2014) for the US, Frenette (2017) for Canada, and Blossfeld and Shavit (1993) for twelve other countries. See Kinsler and Pavan (2011) and Hoxby and Avery (2013) for the gap in enrollment in selective colleges.

the most successful in doing so? While prior research has shown the effectiveness of career counseling programs as well as financial aid interventions in increasing the college enrollment rate of low-income students, little is known about their long-term effects. Yet, that is not clear that an increase in enrollment will translate into an increase in graduation and earnings. Recent studies have demonstrated that educational interventions tend to "fade out" over time (Bailey et al. (2020)). In the case of interventions promoting college access, the "fade out" can occur because such interventions might induce students with low expected returns to education to enroll in college.[2]

In this paper, I answer these questions by studying the short- and long-run impacts of the Future to Discover Project, a randomized control trial conducted between 2004 and 2008 by the Social Research and Demonstration Corporation (SRDC). The project selected 4,390 students from 30 high schools in New Brunswick (Canada) and randomly assigned them to either a career education intervention, a financial aid intervention, a mixed intervention, or a control group.

Students assigned to the career education intervention were invited to participate in twenty career planning workshops, conducted from Grade 10 through Grade 12. These workshops were designed to help students explore different post-secondary options, formulate their own post-secondary education plans in accordance with their interests and skills, and develop strategies to achieve their goals. An important element of the intervention is that it provided guidance on post-secondary education decision-making and application process, a dimension that has been found effective in raising college enrollment rates (Carrell and Sacerdote (2017)).

The financial aid was only randomized among students from low-income families. Students assigned to the financial aid intervention were eligible for a two-year $4,000 per year grant conditional on post-secondary education enrollment. The grant was substantial as it covered most of the tuition costs of undergraduate studies at that time in New Brunswick. Moreover, compared to existing financial aid programs, the intervention offered an early guarantee of aid with a simple application process—two features that have been shown to enhance application rates (Bettinger et al. (2012); Dynarski et al. (2021)).

To study the long-term effects of the interventions, I match the experimental data to confidential administrative data of post-secondary institution records and income tax files.

2. For example, classical models of human capital investment predict that, by lowering the cost of college, financial aid provision would increase enrollment for students at the margin of enrolling, leading to weak effects on long-term outcomes (Becker (1964)). Moreover, outreach programs can bias students' beliefs about their private returns to college enrollment leading to negative impacts of such interventions on long-term outcomes.

The linked data allow me to investigate the causal impacts of the interventions on students' college enrollment, graduation, and earnings, from the end of high school through age 28.[3] In addition, using the factorial design of the experiment—the fact that two interventions were tested alone and combined—I can compare the relative effectiveness and synergy of career education and financial aid in improving low-income students' outcomes. To the best of my knowledge the Future to Discover Project is the only experiment that allows to do so.

In the second part of the paper, I examine the role that the three interventions had in aligning high- and low-income students' college enrollment and graduation rates. In particular, I estimate the effects of the interventions on the gaps in enrollment and graduation between students with similar academic achievement prior to treatment. Because most studies collect data on the specific group of students they are interested in, there is to date, little evaluation of the effects of such interventions on inequality.

I find that the career education intervention increased the share of low-income students who enrolled in four-year college by 8.3 percentage points. Going further, I find that it raised students' earnings in adulthood. In particular, I estimate that by age 28 low-income students assigned to the career education intervention earned 10% more on average in labor income. It suggests that the intervention effectively improved students' decision-making regarding post-secondary education through the reduction in information frictions and behavioral barriers (e.g., lack of attention and over-reliance on default options) targeted by the program.[4]

In contrast, I do not find evidence that the college grant increased low-income students' earnings, although it substantially increased their community college enrollment and graduation rates. One possible explanation for this finding, consistent with classical models of human capital investment in the absence of credit constraints, is that the aid increased enrollment and graduation for students whose expected benefits from enrolling are slightly smaller than the expected benefits of not doing so, leading to weak effects on long-term outcomes (e.g., Becker (1964)).

Together these findings suggest that informational and behavioral obstacles, rather than financial constraints, represent the primary barrier to four-year college enrollment

3. By matching the experimental data to administrative data I build on previous work conducted by the SRDC (see, for example, Ford et al. (2012) and Hui and Ford (2018)). Specifically, the data used by the SRDC did not allow to accurately identify the impact of the interventions on college dropout and completion, and on earnings beyond age 24.

4. See French and Oreopoulos (2017) for a review of the possible informational and behavioral barriers students face transitioning to college.

faced by low-income students.

I also explore the effects of the career education intervention on high-income students. I find that the intervention also had positive effects on their earnings in adulthood. Suggestive evidence indicates that part of this increase in earnings is driven by the intervention inducing students with a high risk of dropping out from college not to enroll. In fact, the intervention decreased the share of high-income students who enrolled in four-year college by 3.8 percentage points, but this effect is mostly driven by lower-achieving students and I do not observe a similar decline in four-year college graduation.

It suggests that high-income students also suffer from information frictions and behavioral barriers. But while these obstacles lead some low-income students to academically under-match (i.e. some should enroll in a four-year college but do not), they lead some high-income students to academically over-match (i.e. some should not enroll in a four-year college but do.).

The magnitude of the effects—increase in four-year college enrollment for the low-income students and decrease in four-year college enrollment for the high-income students—imply a complete alignment of high- and low-income students' four-year college enrollment behavior. In fact, in the control group, low-income students were 13 percentage points less likely to enroll in a four-year college than similarly-achieving high-income students. In the career education group, the gap is only 1 percentage point wide. It suggests that a large part of the gap in four-year college enrollment between the two types of students is explained by students sub-optimal decisions arising from informational and behavioral frictions.

The extent to which my findings extend to other contexts and countries remains to be seen. There are strong reasons to believe that my main finding, according to which career education programs are efficacious in enhancing students' long-term outcomes, can be extended to other contexts as well. In fact, many US-specific studies demonstrated that career counseling programs, as the program studied in this paper, are effective in increasing students' enrollment in four-year colleges (Avery (2013); Stephan and Rosenbaum (2013); Castleman, Page, and Schooley (2014); Carrell and Sacerdote (2017); Cunha, Miller, and Weisburst (2018); Oreopoulos and Ford (2019)). It is thus natural to think that they would also result in an increase in earnings.

However, my results on the effects of the financial aid intervention are possibly specific to the Canadian context. Two features of the Canadian context make it different from other countries. First, unlike other countries, Canada is characterized by a very high enrollment rate in community and private career colleges, which might make the results on community colleges specific to this country. Second, public colleges and universities are highly subsidized, and a number of grants and loans are already available in Canada, making financial constraints possibly less binding than in countries with weaker financial aid systems.

My paper makes several contributions to the literature. First, I contribute to the scarce literature on the long-term effects of interventions promoting college access. While previous research has shown the effectiveness of career counseling programs in increasing the college enrollment rate of low-income students (e.g., Bettinger et al. (2012); Carrell and Sacerdote (2017); Castleman and Goodman (2018); Cunha, Miller, and Weisburst (2018); Oreopoulos and Ford (2019)), little is known about their long-term effects.[5] I show that career counseling programs are not only effective in increasing low-income students' college enrollment rates but are also powerful in improving students' outcomes in the long run.

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Second, I add to the understanding of the factors contributing to the gap in educational attainment by parental income. To date, there is little consensus about the role played by credit constraints (e.g., Keane and Wolpin (2001); Belley and Lochner (2007); Lochner and Monge-Naranjo (2012)). Moreover, although recent studies have demonstrated the existence of informational and behavioral barriers for low-income students (e.g., Bettinger et al. (2012); Hoxby and Avery (2013); Dynarski et al. (2021)), little is known on the extent to which they contribute to the gap. This paper provides new evidence that informational and behavioral barriers explain most of the gap in four-year college enrollment between

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2.2 The Future to Discover Project

In this section, I draw on Currie et al. (2007) to describe the Future to Discover experiment. Throughout the paper, sample sizes are rounded to the nearest 10 for data confidentiality concerns.

2.2.1 Context and Background

High school in New Brunswick, like in the US, runs from Grades 9 to 12, after which students can decide whether to enroll in post-secondary education or not. Students are typically 14 years old at the beginning of high school and graduate at age 18. Three main options are available to students who want to enroll in post-secondary education in Canada: four-year colleges or universities (hereafter, four-year colleges) offering programs that lead to a bachelor's degree; community colleges, also referred to as colleges of applied arts and technology or institutes of technology or science, which typically grant diplomas for technical studies of two years; and private career colleges that offer career-oriented programs of one year or less.

In Canada, the share of adults with a four-year college degree is nearly equal to 33 percent, which is comparable to most developed countries (Statistics Canada (2020)). However, unlike other countries, Canada is characterized by a very high enrollment rate in community and private career colleges: 26 percent of Canadian adults have a short-cycle tertiary diploma compared to 7 percent of adults in other OECD countries (Statistics Canada (2020)). The high enrollment rate in post-secondary education masks large disparities. A young Canadian adult from a family in the bottom income quintile is 30 percent less likely to attend a post-secondary institution than someone from a family in the top income quintile (Belley, Frenette, and Lochner (2014); Frenette (2017)).

2.2.2 Experimental Design

The Future to Discover project was designed and implemented by the SRDC with the support of Statistics Canada.[7] With the objective of finding out what works best to increase college enrollment, three interventions targeted to high school students were designed and tested in the Canadian province of New Brunswick, namely, a career education intervention, a financial aid intervention, and a mixed intervention.

Figure 2.1 provides an overview of the experimental design. The Future to Discover project was implemented in thirty New Brunswick high schools. The schools were selected to participate in the experiment on the basis of a priority index computed from the size of the school, the number of low-income families in the school, and the number of other schools in the district.

Participants in the experiment were selected from the 2003–07 and 2004–08 high school cohorts during the spring of their ninth grade. A random sample of students—roughly 45% or 5,670 students—was initially chosen among the freshmen cohorts to receive invitations to participate in the experiment. Upon invitation, students along with their parents, were required to give their written consent and answer the baseline survey in order to take part in the experiment. These requirements were fulfilled by about 78 percent of the students invited to participate.

During the baseline interview, the answering parent was asked to provide the annual household income as stated in his previous year's income tax returns. If the amount earned was above the provincial median, the student was classified as a high-income student and as a low-income student otherwise.[8] Low-income students were randomly assigned to the three treatment arms (career education, financial aid, mixed intervention) and the control group. High-income students were not eligible for financial aid and were accordingly only randomized between the career education and control groups. The randomization was conducted at the student level within each school.[9]

7. The SRDC is a non-profit research organization based in Ottawa, Canada. The experiment received financial support from the Canada Millennium Foundation.

8. The threshold varied with family size. Six thresholds were defined, ranging from $40,000 for a single-parent family with one child to $60,000 for a family with two parents and three children or more.

9. Due to budgetary concerns the assignment ratios were adjusted for the second cohort of students, and a small random sample of students was excluded from the data collection. While the differential treatment assignment ratios across cohorts could lead to a complex empirical analysis design, the exclusion of students was conducted so as to equalize the assignment ratios across the two cohorts, allowing for a straightforward pooling of the students in the analysis. Although some administrative data are available for the non-follow-up students, I follow previous studies (Ford et al. (2012), Ford and Kwakye (2016), and Hui and Ford (2018)) and exclude them from my analysis. Table 2.5 presents the distribution of the students across parental income and the four randomization groups.

Tables 2.7 and 2.8 show baseline descriptive statistics (mean and standard deviation) for low- and high-income students in the control group, and report the differences between the control group and the treatment groups. The table shows a balance on almost all baseline characteristics. I find four significant differences out of 56 tests, a number that could have been obtained by chance alone. In addition, I test for whether the baseline characteristics jointly predict treatment status. I find no evidence that the baseline characteristics jointly predict treatment status for both high- and low-income students (p-value from F-test is 0.55 for low-income students and 0.95 for high-income students).

2.2.3 Career Education Program

Students assigned to the career education program were invited to participate in twenty career planning workshops, conducted from Grade 10 through Grade 12. The workshops were split into the following four series:

1. *Career Focusing*—The first workshop series was conducted in Grade 10. It included six workshops that were designed to guide students into the exploration of career options. Besides being taught how to research information on post-secondary education and labor market trends, students were encouraged to explore their post-secondary options through different activities and exercises.

2. *Lasting Gifts*—The second workshop series, which took place in Grade 11, was tailored toward the parents. The four workshops of the series aimed to encourage and assist the parents in getting involved in their children's career planning. Together with their children, parents were exposed to various career-planning approaches and were instructed to test these approaches through interactive activities and reflective exercises.

3. *Future in Focus*—The third workshop series was designed to help Grade 12 students build resilience to overcome unexpected life challenges. The workshops focused on the specific skills and attitudes needed to work through obstacles and on the importance of developing a support network.

4. *Post-secondary Ambassadors*—Six meetings with post-secondary education students from various institutions were organized over Grades 10 to 12. The invited students were asked to share their experiences and advice, providing high school students with peer mentors and role models.

The workshops were held on each school property right after school hours, with the exception of the second workshop series, which took place in the evening to facilitate the participation of the parents. From the randomization, 30 to 35 students were typically invited to the workshops in each school, allowing the meetings to be held in a classroom and facilitating interactions. The workshops were optional. Students were actively reminded about the date and location of the workshops through text messages, mails, and announcements in each school. They were also encouraged to attend through prizes and snacks.

In addition, students were given access to post-secondary and career information via a website and a magazine.[10] The two media shared the same content—a description of post-secondary options, a guide to the financial aid system, labor market trends, and links to other career education resources. The same content was offered across the two media in order to reach more students and parents with different habits and access to the internet.

The career education program can theoretically have several effects on students' college enrollment and earnings. On the one hand, the program can improve students' decision-making regarding post-secondary education by tackling several informational and behavioral barriers students might face. First, by pushing students to look for information on the costs and benefits of each post-secondary option, the program is expected to reduce misinformation. Second, by helping students think about their options and understand the long-lasting effects of their choices, it might minimize students' lack of attention, present bias, and over-reliance on default options—three behavioral barriers that have be found to be important in students' educational decisions (French and Oreopoulos (2017)). An improvement in decision-making can result, in turn, in an increase or to a decrease in college enrollment depending on the direction of the initial mistakes made by the students. It should however lead to an improvement in students' outcomes in the long run.

On the other hand, the program can bias students' beliefs about their private returns to college enrollment, leading to an increase in enrollment but negative effects on students long-run outcomes. That will be the case if, for instance, it pushes students to enroll in four-year college programs regardless of their academic ability.

10. Six issues of the magazine were sent to the students over Grades 10 to 12. To limit spillover, the website was restricted to treated students only via a unique access key. Students received their login information with the magazine's first issue and could access the website anytime from then on.

2.2.4 Financial Aid

Students assigned to the financial aid intervention were eligible for a college grant worth up to $8,000. They could claim $2,000 each academic term they enrolled in post-secondary education, for a period of four terms or two years.[11] They were informed about the grant at the time of recruitment in Grade 9 and reminded about it at the end of Grade 12 and one year after high school.

The financial aid was substantial compared to tuition fees at the time of the experiment. Between 2007 and 2011, when most students from the sample enrolled in post-secondary education, tuition fees in New Brunswick for one year of undergraduate schooling were roughly equal to $5,500 in four-year colleges and $2,300 in community colleges.[12]

The financial intervention can affect students' outcomes in two ways. On the one hand, by reducing the amount students need to borrow to finance their education, the intervention might reduce financial barriers such as credit constraints and debt aversion. In that case, we would expect an increase in enrollment and an improvement in students' long-run outcomes. On the other hand, classical models of human capital investment predict that the aid, by lowering the cost of college, would increase enrollment for students whose expected benefits from enrolling are slightly smaller than the expected benefits of not enrolling, resulting in limited benefits in the long run.

2.3 Empirical Framework

2.3.1 Data

I use data from three main sources.

1. *Experimental Data*—First, I obtained data from the SRDC on (i) students' characteristics collected through the baseline survey conducted in Grade 9 (demographics, family composition, socioeconomic status and aspirations); (ii) students' participation in the workshops and their claims to the financial aid; and (iii) student test scores and high school graduation.

11. To receive the grant, students had to register in a post-secondary program recognized by the Canada Student Loans Program. It includes most four-year and vocational programs as long as they lead to a certificate, diploma, or degree. Students were eligible to receive the payments for three years after high school graduation.

12. Tuition fees from the four main four-year colleges were retrieved from Statistics Canada: *Table 37-10-0045-01 Canadian and international tuition fees by level of study*.

2. *Canadian Post-Secondary Information System*—Second, I matched the experimental data obtained from the SRDC to the Canadian Post-Secondary Information System, which provides information on enrollment and graduation for the universe of students who attended a public post-secondary institution in Canada from the 2000–01 academic year to the 2017–18 academic year, allowing me to observe post-secondary education trajectories in public institutions until age 28 for both cohorts.[13]

3. *Statistics Canada Tax Filer Database*—The experimental data were also matched to earnings data from the Statistics Canada confidential tax filer database. The database provides information on earnings (labor income, total earnings) for all individuals who filed a tax return during a reference year. Because in Canada, individuals need to file a tax return to qualifies for refunds and credits, the majority of adults, including post-secondary students, generally file a tax return every year, regardless of their working status.

These data suffer from two limitations that I address in different ways. First, I cannot estimate the impact of the interventions on enrollment and graduation from private institutions. This is likely a small limitation for the identification of enrollment and graduation from four-year and community colleges as they are nearly all public-funded. Only a few private, most of which are faith-based, four-year colleges exist, and they attract a tiny fraction of students (Jones and Li (2015)). However, a number of small private career colleges, which offer short and career-oriented programs of one year or less, are not captured by the administrative data. To identify the impact of the interventions on enrollment in these types of institutions, I rely on the survey conducted two and a half years after high school graduation. The survey is, however, conducted too soon to provide a reliable view of graduation.

Second, I do not observe earnings data beyond age 24 for the students who never enrolled in a public post-secondary institution or registered as an apprentice (34 percent of the sample). This stems from the fact that the link with earnings data was done in two waves. First, earnings until age 24 were initially acquired by the SRDC for all students in the sample. Second, earnings until age 28 were retrieved for students who enrolled

13. The system aims to cover the universe of public post-secondary institutions. However, only 95 percent of these institutions are indeed covered (even fewer before 2009, when only 80 percent were covered by the system). In New Brunswick specifically, the platform does not cover the New Brunswick Community College—one of the two largest community colleges in New Brunswick—before 2010. This is challenging as I expect most of the impact of the program to happen from 2007 to 2009. To recover data from this institution, I supplement the platform until 2010 with data on enrollment and graduation gathered by the SRDC from the New Brunswick Department of Post-secondary Education, Training, and Labour.

in a public post-secondary institution or registered as an apprentice via the Canadian Education and Labour Market Longitudinal Platform. I address this limitation by imputing the missing data. Section 2.3.3 provides more details on the methodology used.

Appendix Section 2.8 summarizes the timeline of data coverage and details the construction of the outcomes of interest.

2.3.2 Analytic Samples

I exclude 49 students from the sample for whom less than two years of earnings data is available, suggesting that they might have moved out of the country or that their Social Insurance Number used to match the administrative data could not be relied upon. These students account for 1.4 percent of the initial sample. I find no evidence that the attrition rate differs by treatment group, as shown in Table 2.6. In Table 2.9 and 2.10, I also ensure that baseline characteristics remain balanced across treatment groups after excluding these students. In total, my sample is composed of 2,090 low-income students and 1,450 high-income students.

I further restrict my sample when looking at specific secondary outcomes for which data is not available for all students. It is the case for high school graduation and average test scores in Grade 12 that was not provided for all students, most likely because some students dropped out or transferred to another school.[14] This is also true for enrollment in private institutions for which I only have data for students who answered the survey.[15] I test and discuss potential threats to causal identification arising from selective missingness when presenting the results on these outcomes. Moreover, to enable the comparison of the treatment effects measured on the restricted samples with the ones measured from the full sample, I adjust the treatment effects on these outcomes using inverse probability weighting (IPW) (Seaman and White (2013)). This method puts more weight on observations that have, according to observed baseline characteristics, a high probability to be missing for the outcome of interest but are not. In practice, I construct the weights from Probit regressions of the missingness indicators on treatment dummies, baseline characteristics, and cohort and school dummies.

14. Test scores in Grade 12 are available for 80 percent of the low-income students and 91 percent of the high-income students. Graduation data are available for 87 percent of the low-income students and 94 percent of the high-income students.

15. 87 percent of the low-income students and 95 percent of the high-income students answered the survey.

2.3.3 Imputation of Earnings Data

I seek to study the impact of the interventions on earnings at different ages, starting from age 19, when most students have left high school, until the age of 28, the most recent year for which I have data on earnings.

Earnings data may be missing for two reasons (Table 2.11). First, as explained in the data section, earnings data were not collected beyond age 24 for students who did not enroll in a public post-secondary institution. This is the case for roughly 1,210 students, or 34 percent of the entire sample. Second, over the period for which earnings data were collected, earnings records are missing for students who did not file a tax return during the reference year or filed the return too late. It is the case for 6 percent of the records.

To estimate the effects of the interventions on earnings, I need to account for these missing values. I address the issue by imputing the missing records rather than by restricting the sample to complete cases, which would lead to a loss of power and biases. I deal with the two types of missing values in different ways. First, I impute the missing records found over the period data were collected using linear interpolations from the available records of each individual.[16] Second, I forecast the earnings from age 25 to age 28 for each student whose data were only collected until age 24. For this purpose, I estimate a model which takes into account the students' past earnings records, their current level of education and years of experience and whether they are currently enrolled in post-secondary education. The underlying assumption of the forecasting model is that earnings growth rate, conditional on education, is the same for treated and untreated students. To address concerns about the validity of this assumption, I check in Table 2.26 that the prediction errors are not correlated with students' treatment status by comparing the earnings observed at age 24 with their predicted values. I also show how the results vary with alternative models of forecasting in Appendix Table 2.25 .

I formally describe the linear interpolation method and the forecasting procedure in Appendix Section 2.9.

16. The data restriction mentioned above ensure that I observe at least two years of earnings data points for each individual, making sure the interpolation is feasible for each individual.

2.3.4 Empirical Strategy

I first focus on the effect of the three interventions on low-income students. To recover the treatment effects, I estimate the following equation by Ordinary Least Squares (OLS),[17]

$$Y_i = \beta_0 + \beta_1 C_i + \beta_2 F_i + \beta_3 M_i + \boldsymbol{\beta_4 X_i} + \boldsymbol{\beta_5 S_i} + \epsilon_i \tag{2.1}$$

with Y_i is the outcome of interest for student i, C is a binary indicator equal to one if student i was assigned to the career education only group, F is a binary indicator equal to one if student i was assigned to the financial aid only group, and M is a binary indicator equal to one if student i was assigned to the mixed intervention group. $\boldsymbol{X_i}$ is a vector of baseline characteristics for student i and $\boldsymbol{S_i}$ is a vector of school-cohort dummies corresponding to the level of stratification.[18]

I follow the common practice of adjusting the results with the inclusion of baseline characteristics. I explore the sensitivity of the results to the exclusion of covariates in Tables 2.12, 2.13, and 2.14. The results do not significantly change when controls are removed, except for the treatment effects of the career education program on low-income students that are larger. I also report in the same table the results from the regressions where relevant controls are selected using the post-double-selection lasso method developed by Belloni, Chernozhukov, and Hansen (2013).[19] The treatment effects are virtually identical when controls are selected following the post-double-selection lasso method to when they are not.

The β_1 and β_3 coefficients capture the effects of eligibility for the career education program alone and combined with a financial nudge, respectively. Participation in the program was not compulsory—students were neither compelled to attend the workshop or to read the magazine/visit the website. However, note that nearly all students assigned to treatment were exposed to the program if we consider all forms of exposure. Among those assigned to treatment, 85 percent attended at least one workshop, 73 percent declared having read parts of the magazine, and 22 percent visited the website. In what follows, I use

17. I estimate the same linear model for both continuous and binary outcomes. Although a linear probability model can yield fitted values being outside the unit interval, it produces unbiased estimates of the average effects (Wooldridge (2010)).

18. The baseline characteristics included in the regression are all variables in Table 2.7, namely, gender, language spoken at home, whether one parent was born outside Canada, household composition, level of education of parents, whether student wants a four-year college degree and test score dummies.

19. The selection procedure chooses variables from the set of characteristics listed in Table 2.7 and their interactions that are significant predictors of either the outcome of interest Y_i or any of the treatment variables of interest, C_i, F_i, and M_i (Belloni, Chernozhukov, and Hansen (2013)).

the terms "eligibility for the career education program" and "career education intervention" interchangeably.

Since the intervention is randomized at the individual level in each school, I cannot rule out spillover effects. Spillover might have occurred in two ways. First, students from the career education group might have shared information they learned during the workshops or from the website and the magazine. They might have even lent the magazine or shared their login information with other students. Second, by changing the students' enrollment behavior, the program might have influenced students in the control group through peer effects. Assuming that the spillover effects, if any, would play in the same direction as of the direct effects, the impacts I estimate are lower bounds for the true impacts.

I report in all tables Huber–White robust standard errors and standard sampling-based significance levels. In addition, I report in Tables 2.15 and 2.16, randomization-based Fisher-exact p-values for the main outcomes of interest. These p-values do not rely on asymptotic properties but on the random assignment itself (Heß (2017), Young (2019)). I find that the exact p-values are virtually identical to the sampling-based p-values which is explained by the fact that the samples used are not small. I also address in the same table potential concerns arising from multiple hypothesis testing by computing sharpened q-values which control for the False Discovery Rate (Benjamini, Krieger, and Yekutieli (2006), Anderson (2008)). Most of the effects (72 percent) found to be significant using sampling-based significance levels survive multiple hypothesis testing correction. I indicate below when they do not.

In particular, I am interested in the gaps in enrollment and graduation between students with similar academic achievement prior to treatment. I thus estimate the following equation by OLS, on the restricted sample of students assigned to the control or career education groups,

$$Y_i = \gamma_0 + \gamma_1 HI_i + \gamma_2 C_i + \gamma_3 C_i \times HI_i + \boldsymbol{\gamma_4 X_i} + \boldsymbol{\gamma_5 S_i} + v_i, \tag{2.2}$$

where HI_i is a binary indicator equal to one if student i is a high-income student and 0 otherwise. γ_2 measures the treatment effect on low-income students, the sum of γ_2 and γ_3 the treatment effect on high-income students and γ_3 the treatment effect on the gap in outcome between the two types of students.

Finally, I am interested in the effects of the interventions on the gaps in enrollment and graduation between high- and low-income students with similar academic achievement prior to treatment. To this end, I first estimate in each school the relationship between the outcome of interest Y (i.e., enrollment or graduation) and student test scores in Grade 9 for each income group g and treatment arm t. Then I can compute for each treatment arm the average gaps between high- and low-income students across test scores and the thirty schools.

Formally, I estimate the following equation by OLS,

$$Y_{i,gt} = \sum_{k=1}^{30} \gamma_{k,gt} 1[S_i = k] + \beta_{1,gt} TS_i + \beta_{2,gt} TS_i^2 + \epsilon_i, \tag{2.3}$$

where $Y_{i,gt}$ is the outcome of interest for student i, TS_i is student i standardized average test score in Grade 9 and S_i is a categorical variable indicating the school attended by student i in Grade 9.

From the regression, I can compute an approximation of the mean of outcome Y at different points of the test score distribution for each school. Precisely, the predicted outcome mean for a student of type g, with a standardized average test score in Grade 9 ts, attending school s, and belonging to experimental group t is,

$$E(Y_{gt}|TS_i = ts, S_i = k) = \gamma_{k,gt} + \beta_{1,gt} ts + \beta_{2,gt} ts^2. \tag{2.4}$$

And the gap in outcome Y between equally-achieving high- and low-income students belonging to experimental group $t1$ and $t2$, respectively, is,

$$\begin{aligned} E(Y_{ht1} - Y_{lt2}|TS_i = ts, S_i = k) =& \gamma_{k,ht1} - \gamma_{k,ly2} \\ &+ (\beta_{1,ht1} - \beta_{1,lt2}) ts + (\beta_{2,ht1} - \beta_{2,lt2}) ts^2 \end{aligned} \tag{2.5}$$

The average gap across test scores and the thirty schools is then,

$$\frac{1}{30} \sum_{k=1}^{30} (\gamma_{k,ht_h} - \gamma_{k,lt_l}) + \int_{-\infty}^{+\infty} \left((\beta_{1,ht_h} - \beta_{1,lt_l}) ts + (\beta_{2,ht_h} - \beta_{2,lt_l}) ts^2 \right) f(ts), \tag{2.6}$$

with $f(ts)$ the test score distribution.

2.4 Results

2.4.1 Effects of the Career Education and Financial Aid Interventions on Low-Income Students' Outcomes

I first focus on the effects of eligibility for the career education program and eligibility for the college grant on low-income students' outcomes.

High school Graduation and Academic Performance

By changing students' post-secondary education aspirations, the interventions might have influenced students' effort and graduation plans. Therefore, I begin by exploring the effects of the interventions on high school graduation and academic performance in Table 2.17. The two interventions had no meaningful effects on students' academic performance as measured by average test scores in Grade 12, or on the fraction of students who graduated from high school. As a result, any effects observed in the next section on college enrollment are more likely to result from a change in aspiration than from a change in performance.

College Enrollment and Completion

I then explore, in Table 2.1, how low-income students' college enrollment and graduation rates were affected by the two interventions. My first three outcomes of interest are whether a student has ever enrolled in, graduated from, and dropped out of public college within 10 years of high school graduation. As mentioned before, public colleges include nearly all four-year and community colleges but exclude private career colleges that offer programs of one year and less. Column (1) reports the outcomes' means in the control group. I report the treatment effects of eligibility for the career education program in column (2) and eligibility for the financial aid in column (3). I also test whether the treatment effects of the two interventions are significantly different from each other and report the associated p-value in column (4).

In the control group, 52 percent of the low-income students ever enrolled in a public college—of these students, 69 percent graduated. I do not observe a significant change following the career education intervention in the fraction of students who enrolled, graduated, or dropped out of college. In contrast, the college grant significantly increased the fraction of students who enrolled and graduated from college: students assigned to

Table 2.1: Treatment Effects on Low-Income Students' College Enrollment and Graduation

Dependent variable	Control mean (1)	Career education (2)	Financial aid (3)	P-value difference (4)
Ever enrolled in any public college	0.52	0.034	0.080***	0.10
		(0.028)	(0.026)	
Ever graduated from a public college	0.36	-0.007	0.075***	0.00
		(0.028)	(0.026)	
Dropped out of college	0.15	0.032	-0.003	0.15
		(0.024)	(0.021)	
Group size	590	420	530	

Notes: The table reports the treatment effects of eligibility for the career education program and for the financial aid on college enrollment and graduation. Each row represents a separate OLS estimation of equation 2.1. Enrollment and graduation are measured within 10 years of high school graduation. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. For each outcome, I test whether the treatment effects of the two interventions are significantly different from each other and report the associated p-value in column (4). Group sizes are rounded to the nearest 10 for data confidentiality concerns.

the financial aid group were 8.0 percentage points more likely to enroll and 7.5 percentage points more likely to graduate than control group students and the two effects are significant at the 1 percent confidence level.[20]

In addition, relying on the follow-up survey conducted at age 20, I find that two interventions had essentially no impact on enrollment in private career colleges (Table 2.18).

Types of Colleges

I next investigate, in Table 2.2, the impact of the interventions on a set of indicators for the types of colleges students enrolled in and graduated from. The format of the table is identical to Table 2.1.

The insignificant effect of the career education intervention on college enrollment masks strong significant effects on the types of colleges students enrolled in. The fraction of students who ever enrolled in a four-year college rose by 8.3 percentage points following the intervention, which corresponds to a 36 percent increase from the control mean and is

20. The increase in college enrollment occurred between ages 18 and 20 when students were still eligible for the financial aid (Figure 2.6–Panel A3).

Table 2.2: Treatment Effects on the Type of College Low-Income Students Enrolled in and Graduated from

Dependent variable	Control mean (1)	Career education (2)	Financial aid (3)	*P*-value difference (4)
Panel A—Enrollment				
First enrolled in a four-year college	0.21	0.057**	0.030	0.26
		(0.023)	(0.021)	
First enrolled in a community college	0.31	-0.017	0.048*	0.03
		(0.028)	(0.027)	
Switched to a community college	0.06	0.009	0.015	0.72
		(0.016)	(0.014)	
Switched to a four-year college	0.02	0.028**	0.002	0.02
		(0.011)	(0.008)	
Ever enrolled in a four-year college	0.23	0.083***	0.032	0.04
		(0.024)	(0.021)	
Ever enrolled in a community college	0.36	-0.012	0.065**	0.01
		(0.030)	(0.028)	
Panel B—Graduation				
Four-year college degree	0.14	0.037*	-0.003	0.07
		(0.021)	(0.018)	
Community college diploma	0.24	-0.033	0.076***	0.00
		(0.027)	(0.026)	
Panel C—Dropout				
Dropped out of a four-year college	0.08	0.041**	0.026	0.46
		(0.020)	(0.017)	
Dropped out of a community college	0.08	0.019	-0.017	0.09
		(0.021)	(0.018)	
Group size	590	420	530	

Notes: The table reports the treatment effects of eligibility for the career education program and for the financial aid on the type of college students enrolled in and graduated from. Each row represents a separate OLS estimation of equation 2.1. Enrollment and graduation are measured within 10 years of high school graduation. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. For each outcome, I test whether the treatment effects of the two interventions are significantly different from each other and report the associated p-value in column (4). Group sizes are rounded to the nearest 10 for data confidentiality concerns.

significant at the 1 percent level. Note that this increase is not balanced by an equivalent decrease in community college enrollment that would have otherwise been expected from the null effect on any college enrollment.[21]

The increase in four-year college enrollment translated into a significant increase in both four-year college graduation (+3.7 percentage points) and dropout (+4.1 percentage points).[22] Assuming that the *inframarginal* students—those who would still have enrolled in the absence of the intervention—were not affected by the intervention,[23] the results imply that 45 percent of the students induced to enroll in four-year college by the intervention graduated, and that 55 percent did not. This 45 percent success rate is not statistically different from the 61 percent success rate of the inframarginal students, suggesting that the students who were induced to enroll in four-year colleges by the intervention performed similarly to their peers.

In contrast, the effect of the college grant on college enrollment is mostly driven by a 6.5 percentage points increase in community college enrollment. Going further, I observe a strong significant increase in community college graduation (+7.6 percentage points). The fact that the increase in community college graduation is of the same magnitude as the increase in community college enrollment could imply two things. Either all students induced to enroll in a community college successfully graduated from it, which suggests that they are doing (much) better than the *inframarginal* students (Table 2.20). Or some students who would have dropped out from community college in the absence of the intervention were induced to graduate.

As a result of all of the effects combined, the average number of years students spent in college was 0.24 higher in the career education group (p-value=0.08), and 0.15 higher in the financial aid group (p-value=0.19), than in the control group (Table 2.22).

I also explore the effects heterogeneity across skills in Table 2.19. While I observe that the effect of the career education intervention on four-year college enrollment was significantly higher for higher-achieving students, I do not find any major differences in the impact of the financial aid intervention across skills. This suggests that the two types of interventions reached different types of students, which is confirmed by the analysis of the mixed intervention in Section 2.4.2.

To sum up, the two interventions had contrasting effects on the types of colleges students enrolled in and graduated from.[24] On the one hand, the career education inter-

21. In line with these results, I find that eligibility for the career education program increased the fraction of students who enrolled in four-year college at ages 18 and 19, as well as at later ages (Figure 2.5–Panel A1).

22. Note that these effects do not survive multiple hypothesis correction.

23. This is speculative as I cannot exclude that the program also influenced the *inframarginal* students through a change in major choice, effort, and access to financial aid.

24. The treatment effects are different between the two interventions for most outcomes of Table 2.2.

vention increased four-year college graduation by 3.7 percentage points while decreasing community college graduation and increasing dropout. On the other hand, the financial aid intervention increased community college graduation by 7.6 percentage points while having no adverse effect on dropout.

From these results alone, it is unclear what will be the effects of the two interventions on students' long-term outcomes. These effects will ultimately depend on the returns to four-year and community college graduation for the marginal students, and on the magnitude of the adverse effects of college dropout.

Earnings

I finally present the results of the impact of the two interventions on labor income, unconditional on working, in Figure 2.2. The estimated effects capture the combination of the effects on the probability of working as well as on the wage earned when working. The Figure presents the estimated effects against age. Each point is estimated from equation 2.1, and the shaded area indicates the associated 90 percent confidence interval. The impact of the career education intervention and of the financial aid are presented in Panel (a) and (b), respectively. I also report in Table 2.23 the corresponding coefficients and standard errors. The same table presents the effects on total earnings, which include on top of labor income, self-employment earnings, unemployment insurance benefits, and other social benefits. All earnings are expressed in 2020 Canadian dollars.

The career education intervention marginally decreased low-income students' unconditional labor income between the ages of 19 and 21, which is in line with the increase in college attendance induced by the intervention. Conversely, I observe, starting from age 22, a growing positive effect of the intervention on average annual labor income. By the age of age 28, low-income students assigned to the career education group earned on average $3,300 more annually in labor income than control group students, corresponding to a 10 percent increase from the control mean of $31,700, and is statistically significant at the 10 percent confidence level (p-value=0.15 after correcting for multiple hypothesis testing). The effects on total earnings are largely identical (Table 2.23). Moreover, the increase in earnings does not stem from an increase in the likelihood of working but rather by an increase in wages as can be seen from Table 2.24.

Assuming the rise in income is solely driven by the 0.24-year increase in the length of post-secondary education observed in Table 2.22, it can be inferred that the students who were induced to get more schooling after the intervention had very high returns to it. More specifically, students earned on average $14,000 more annually by age 28 for each additional year of schooling students they were induced to get, which is significantly higher than the returns to schooling observed in the control group.[25] However, this is only suggestive as I cannot empirically rule out that other channels drove the increase in earnings, such as changes in major or occupational choices.

These findings imply that the intervention did lower the barriers students encountered in maximizing earnings. Since the intervention only changed students' decision-making process but not the environment they actually faced, it highlights the existence of informational and behavioral barriers for some students.

The decrease in labor income between the ages of 19 and 21 induced by the career education intervention is also observed with the financial aid treatment since they both had similar effects on college enrollment. Surprisingly, the financial aid intervention had no noticeable effect on labor income and total earnings beyond age 22, although it substantially increased graduation from community colleges. The lack of effect suggests that students were induced to enroll in and graduate from programs with on average limited monetary returns.

One possible explanation for this finding is that the aid increased enrollment and graduation for students whose expected benefits from enrolling/graduating are slightly smaller than the expected benefits of not doing so, leading to weak effects on long-term outcomes. This is consistent with classical models of human capital investment (e.g., Becker (1964)). One alternative explanation is that the intervention effectively improved students' long-term outcomes albeit along non-pecuniary dimensions that are not captured by monetary outcomes. In fact, college attendance is often associated with non-pecuniary benefits such as social interactions, improved health, less risky behaviors, and better occupational matching, which are not captured in my analysis (see, for instance, Oreopoulos and Salvanes (2011).).

The identification of the treatment effects on earnings hinges on the validity of the imputation performed. I address these concerns in Table 2.25 by showing how the results vary with alternative models of forecasting for the missing data. The treatment effects remain largely unchanged with different models. More specifically, none of the significant coefficients become insignificant and vice-versa. Moreover, I also check in Table 2.26 that the prediction errors are not correlated with students' treatment status by using the earnings observed at age 24.

25. In the control group, one year of post-secondary education is associated with a $3,000 increase in annual labor income by age 28.

2.4.2 Effects of the Mixed Intervention on Low-Income Students' Outcomes

I next explore the effects of the mixed intervention in Table 2.3 in order to understand the synergy between the two interventions. Column (1) reports the treatment effects of the intervention. For each outcome, I test whether the treatment effect of the mixed intervention is significantly different from the effect of the career education intervention and of the financial aid intervention and report the associated p-value in columns (2) and (3), respectively.

I find that the mixed intervention combined the effects of the career education and financial aid programs but had no additional effect, which suggests (i) a lack of synergy between the two types of interventions and (ii) that they reached different types of students.

Specifically, the mixed intervention increased the college enrollment rate similarly to the financial aid intervention, and increased the four-year college enrollment rate similarly to the career education intervention.[26] The fraction of students who enrolled in a community college was unaffected by the intervention. This is probably explained by the fact that the intervention induced some students who would not have enrolled in any college to enroll in a community college (financial dimension), and some students who would have enrolled in a community college to enroll in a four-year college (career education dimension).[27]

Consistent with these effects on enrollment, students assigned to the mixed intervention group were more likely to graduate from any college and from four-year college than control group counterparts (+5.5 and +2.6 percentage points, respectively). The intervention also increased, as the career education intervention, four-year college dropout (Table 2.21). However, this increase is compensated by a rise in community college graduation. All in all, there was no change in the fraction of students who dropped out of college.[28]

26. As with the career education intervention, the treatment effect of the mixed intervention on four-year college enrollment was significantly higher for higher-achieving students (Table 2.19).

27. This is the case if we assume: (1) that students who would have enrolled in college in the absence of the intervention were not induced not to enroll following the interventions; (2) students who would have enrolled in a four-year college in the absence of the intervention were not induced to enroll in a community college following the interventions; and (3) students who were induced to enroll in a four-year college by the career education intervention were also induced to enroll in a four-year college by the mixed intervention.

28. This is coming from two channels. First, as with the financial aid-only intervention, the dropout rate of students who enrolled in a community college decreased following the intervention from 30 to 23 percent (Table 2.20). Second, students assigned to the mixed intervention were more likely to switch and graduate from a community college after dropping out of a four-year college, compared to students in the career education group (Table 2.21).

Table 2.3: Treatment Effects of the Mixed Intervention

Dependent variable	Treatment effects (1)	*P*-value diff. with career educ. (2)	*P*-value diff. with financial aid (3)
Panel A—Enrollment			
Ever enrolled in any public college	0.059**	0.36	0.42
	(0.028)		
Ever enrolled in a four-year college	0.087***	0.87	0.01
	(0.024)		
Ever enrolled in a community college	0.013	0.42	0.07
	(0.030)		
Panel B—Graduation			
Ever graduated from a public college	0.055**	0.04	0.47
	(0.028)		
Four-year college degree	0.026	0.61	0.13
	(0.021)		
Community college diploma	0.029	0.01	0.07
	(0.025)		
Dropped out of college	-0.003	0.15	0.99
	(0.024)		
Panel C—Earnings			
Labor income at age 28	1,588	0.35	0.19
	(1,688)		
Control group size	590		
Mixed intervention group size	540		

Notes: The table reports the treatment effects of eligibility for the career education program combined with eligibility for the financial aid on the main outcomes of interest. Each row represents a separate OLS estimation of equation 2.1. Enrollment and graduation are measured within 10 years of high school graduation. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. For each outcome, I test whether the treatment effect of the mixed intervention is significantly different from the effect of the career education intervention and of the financial aid intervention and report the associated p-value in column (2) and (3), respectively. Group size is rounded to the nearest 10 for data confidentiality concerns.

Considering these findings, the effects of mixed intervention on earnings would be expected to be similar to those of the career education intervention. Indeed, the only difference observed between the effects of the career education intervention and of the mixed intervention is that unlike the former, the latter prompted some students to enroll in community college. However, the community college programs in which students were induced to enroll did not seem to yield any monetary returns. Consistent with the prediction, I observe a rise in annual labor income following the mixed intervention which is not statistically different from the effects of the career education intervention—although it is smaller and not significantly different from zero.[29]

2.4.3 Effects of the Career Education Intervention on High-Income Students' Outcomes

The design of the experiment allows me to assess the effects of eligibility for the career education program on high-income students. Table 2.4 presents the results on college enrollment and graduation. Column (1) reports the effects for low-income students, column (2) the effects for high-income students, and column (3) the impact differential between the two types of students, as estimated from equation 2.2.[30]

While I showed earlier that eligibility for the career education program substantially increased low-income students' enrollment in four-year colleges, a contrasting effect emerges for high-income students. High-income students assigned to the career education group were 3.8 percentage points less likely to enroll in a four-year college than control group students. It is however empirically unclear whether this decrease is balanced by an increase in the fraction of students who enrolled in a community college or in the fraction of students who did not enroll at all, since none of these effects are significant. I also observe a reduction in the fraction of students who dropped out of four-year college following the intervention, although the effect is not statistically significant (p-value=0.16). It suggests that students who were induced not to enroll in a four-year college by the intervention would not have graduated in the absence of the intervention. Note that the effects on high-income students do not survive multiple hypothesis testing and should accordingly be taken with caution.

In next explore in Figure 2.3 the effects of the career education intervention on unconditional labor income from ages 19 to 28. Each point is estimated from equation 2.2, and

29. Figure 2.7 shows the evolution of the treatment effects on labor income from ages 19 to 28. Table 2.23 reports the coefficients and standard errors plotted in the Figure and the results on total earnings. Table 2.25 presents the results for alternative models of forecasting.

30. The treatment effects reported for low-income students can slightly differ from the ones reported earlier in Tables 2.1 and 2.2 which is explained by a difference in the sample on which the coefficients from the control variables are estimated. See empirical strategy section.

Table 2.4: Comparison of the Treatments Effects on Low- and High-Income Students' College Enrollment and Graduation

Dependent variable	Low-income students (1)	High-income students (2)	Difference high vs. low (3)
Panel A—Enrollment			
Ever enrolled in any public college	0.040	-0.028	-0.068**
	(0.028)	(0.020)	(0.034)
	0.52	*0.78*	*0.26*
Ever enrolled in a four-year college	0.084***	-0.038*	-0.122***
	(0.024)	(0.021)	(0.032)
	0.23	*0.54*	*0.31*
Ever enrolled in a community college	-0.009	0.010	0.020
	(0.030)	(0.026)	(0.040)
	0.36	*0.41*	*0.05*
Panel B—Graduation			
Ever graduated from a public college	0.000	-0.013	-0.013
	(0.028)	(0.023)	(0.036)
	0.36	*0.62*	*0.26*
Ever graduated from a four-year college	0.038*	-0.016	-0.054*
	(0.021)	(0.021)	(0.030)
	0.14	*0.38*	*0.24*
Dropped out of a four-year college	0.041**	-0.025	-0.066**
	(0.019)	(0.018)	(0.026)
	0.08	*0.15*	*0.07*
Control group size	590	850	
Career education group size	420	600	

Notes: The table reports the treatment effects of eligibility for the career education program on low- and high-income students' outcomes. Each row represents a separate OLS estimation of equation 2.2. Enrollment and graduation are measured within 10 years of high school graduation. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. Control means are reported in italic below the standard errors. For each outcome, I report the effect on low-income students (column (1)), the effect on high-income students (column (2)) and the difference in effect between the two types of students (column (3)). Group sizes are rounded to the nearest 10 for data confidentiality concerns.

the shaded area indicates the associated 90 percent confidence interval. The effects of the career education intervention on low-income students are presented in Panel (a) and the effects on high-income students in Panel (b). I also report in Table 2.27 the corresponding coefficients and standard errors together with the effects on total earnings.

Eligibility for the career education program did not have any noticeable impact on labor income between the ages of 19 and 24. Nevertheless, starting from age 25, I observe a growing positive difference in labor income between treated and control students although none of these differences are statistically significant. The effects are, however, substantial and at the margin of significance—high-income students assigned to the career education group earned, on average, $2,558 more annually in labor income by age 28 than control group students (p-value=0.12). As with low-income students, the increase in earnings does not come from an increase in the likelihood of working but rather by an increase in wages (Table 2.24). The treatment effects are found to be similar on total earnings.

The increase in earnings is consistent with the decline in four-year college dropout. It can also be driven by a change in students' major choices or the quality of the colleges they enrolled in. I however do not find evidence that the intervention changed these dimensions of enrollment even though I cannot rule out some small effects (Table 2.28).[31]

Together, these findings are consistent with a model where students lack information and rely on default options (French and Oreopoulos (2017)). For instance, some low-income students might not enroll in four-year colleges because of a lack of attention and information on opportunities. Inversely, some high-income students with low skills and taste for schooling might enroll in four-year colleges because that is the norm among their peers.

2.4.4 Impact on the Gaps in College Enrollment and Graduation Between High- and Low-income Students

By expanding low-income students' enrollment and graduation rates, the three interventions had sizable effects on the gaps in college enrollment and graduation between high- and low-income students—effects that are reinforced by the lower enrollment rates of high-income students assigned to the career education group. In this section, I evaluate the magnitude of the reduction in these gaps. In particular, I am interested in the gaps in enrollment and graduation between students with similar academic achievement prior to treatment. Results for the four-year college enrollment and graduation gaps are illustrated

31. I do observe, conditional on enrollment, a small increase in the fraction of college students who enrolled in a STEM program. However, I do not observe a change in the overall fraction of students who enrolled in a four-year college program. It might be the case that students who were induced not to enroll in a four-year college following the intervention would not have enrolled in a STEM program in the absence of the intervention.

in Figure 2.4 and Table 2.29.[32]

Eligibility for the career education program for both high- and low-income students decreased the four-year college enrollment gap between equally-achieving students almost entirely. As a matter of fact, the four-year college gap among equally-achieving students decreased by 92 percent on average. In a similar manner the four-year college graduation gap decreased, on average, by 74 percent. The effect on graduation is smaller than that on enrollment as the dropout rate of college students was slightly higher in the career education group when compared to the control group.

I cannot attribute all of the decrease in the four-year college enrollment gap to a reduction in behavioral and informational barriers since the intervention might also have biased students' beliefs concerning their private returns to college enrollment. To put it differently, the intervention might have brought closer students' beliefs and actions to what they should do to maximize their utility, or it could, in contrast, have brought their beliefs and actions further to what is optimal, pushing students who should not enroll to enroll.

However, assuming that all students who were induced to graduate from a four-year college by the intervention did derive benefits from it, my findings indicate that behavioral and informational barriers drive at least 74 percent of the observed differences in four-year college graduation between high- and low-income students.

I further test this hypothesis by investigating how systematic the reduction in gaps is across schools (Figures 2.9 and Table 2.30). The idea is as follows. Suppose the career education program did bias students' beliefs concerning their private returns to college enrollment. In that case, we should not see a strong relationship between the size of the gap between high- and low-income students and the magnitude of the effects of the intervention. However, assuming that the size of the gaps in four-year college enrollment and graduation in each school reveals the extent of the barriers faced by the students, if the intervention effectively removed these barriers, we must observe a positive relationship between initial gap size and the treatment effects. I find that the schools where initial differences between high- and low-income students were the largest are those where the absolute reductions in the gaps were the most significant. Put differently, I observe a systematic and homogeneous decline in within-school inequality. The strong correlation between initial gap size and the magnitude of the treatment effects holds even after

32. I focus on the four-year college enrollment and graduation gaps since my previous findings have proven little monetary benefits of community college enrollment/graduation for the marginal students. Figure 2.8 present the results for any college enrollment.

controlling for school characteristics. It suggests that the intervention effectively removed some of the barriers students faced, supporting the hypothesis that strong informational and behavioral barriers exist.

In addition, low-income students' eligibility for financial aid did not significantly affect the four-year college enrollment and graduation gaps, suggesting that financial barriers do not play a major role in the four-year college enrollment and graduation gaps.

Overall, these findings indicate that career education is an important tool for aligning the four-year college enrollment and graduation rates of high- and low-income students with similar academic achievement. However, while the intervention effectively reduced inequality among equally-achieving pupils, differences in academic achievement account for a large part of the observed differences in enrollment and graduation between high- and low-income students (56–63 percent, Table 2.31). To fully decrease the gaps between the two types of students, one might also understand the factors driving the differences in academic achievement.

2.5 Conclusion

This paper investigates the effects of college grant aid, career education in high school, and the combination of the two on students' college enrollment, graduation, and earnings. I show that career education programs have the potential to improve students' long-term outcomes substantially. My results suggest that the reason why these types of programs are so effective stems from the existence of information and behavioral barriers that prevent students from making optimal decisions regarding post-secondary education. Removing these barriers will induce more low-income students and less high-income students to enroll in four-year colleges, resulting in a sharp reduction in the enrollment and graduation gaps between the two types of students.

One limitation of my study is the lack of power, which prevents any clear exploration of treatment effect heterogeneity. As the career education program resulted in increase in both graduation and dropout rates, I suspect heterogeneous benefits of the intervention on students. Further work should aim to understand who benefited from the intervention and who did not. This understanding would facilitate the design of career education programs that are better suited to helping all types of students.

Moreover, a key question remains unanswered: what features of the career education program were the more effective at increasing low- income students' enrollment? Previous studies suggest that the provision of information alone is not helpful is increasing students' enrollment rates (Bird et al. (2021); Kerr et al. (2015); Bonilla, Bottan, and Ham (2015); Hastings et al. (2016); Carrell and Sacerdote (2017)). By contrast, the intervention provided insights into the post-secondary education application process and decision-making, a dimension that has proven to be effective in increasing college enrollment rates (Avery (2013); Stephan and Rosenbaum (2013); Castleman, Page, and Schooley (2014); Carrell and Sacerdote (2017); Cunha, Miller, and Weisburst (2018); Oreopoulos and Ford (2019)). The fact that programs offering guidance are so efficacious in increasing the enrollment of students and (as I show) in enhancing their long-term outcomes is likely to be explained by their lack of attention when it comes to college possibilities (French and Oreopoulos (2017)).

In this paper, I also show that providing students with additional financial aid has no monetary benefits in the long run. This result was surprising given the fact that the intervention led to an increase in the fraction of students who enrolled and graduated from community colleges. It indicates that students were induced to enroll in programs with limited monetary returns. This lack of returns might be specific to the marginal students but might also arise from a general lack of benefits of some community college programs. My findings underscore the importance of understanding the returns to community college attendance.

The extent to which my findings extend to other contexts and countries remains to be seen. There are strong reasons to believe that my main finding, according to which career education programs are efficacious in enhancing students' long-term outcomes, can be extended to other contexts as well. In fact, many US-specific studies demonstrated that career counseling programs, as the program studied in this paper, are effective in increasing students' enrollment in four-year colleges. It is thus natural to think that they would also result in an increase in earnings.

However, my results on the effects of the financial aid intervention are possibly specific to the Canadian context. Two features of the Canadian context make it different from other countries. First, unlike other countries, Canada is characterized by a very high enrollment rate in community and private career colleges, which might make the results on community colleges specific to this country. Second, public colleges and universities are highly subsidized, and a number of grants and loans are already available in Canada, making financial constraints possibly less binding than in countries with weaker financial aid systems.

In the future, I plan to pursue my analysis of the Future to Discover experiment in three ways. First, I will continue to track students to confirm my findings on the long-term effects of the three interventions and assess their overall effects on lifetime earnings. Second, I will build a structural model of college enrollment under imperfect information and Bayesian learning to exactly quantify the extent to which students are affected by informational and behavioral barriers and provide counterfactual estimates of the gain in earnings that removing these barriers would create. Third, I will exploit exogenous variations in the timing of career counseling workshops created by weather conditions in order to identify how the timing of information affects the decisions made by students.

2.6 References

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2.7 Appendix—Additional Tables and Figures

Table 2.5: Assignment to Treatments, by Parental Income

	Low-income students	High-income students	All students
Control group	600 (28%)	850 (58%)	
Career education group	430 (20%)	610 (42%)	
Financial aid group	550 (26%)	.	
Mixed intervention group	550 (26%)	.	
Total	2,130	1,460	3,590

Notes: The table reports the number and fraction of students assigned to each of the treatment and control groups, by parental income. The fractions are reported in parentheses. Students excluded from the data collection are not shown in this table. Sample sizes are rounded to the nearest 10 for data confidentiality concerns.

Table 2.6: Test for Differential Attrition by Treatment Status

	Low-income students (1)	High-income students (2)
Attrition rate in control group	0.018	0.007
Difference between control group and		
Career education group	0.0062	-0.0028
	(0.0093)	(0.0040)
Financial aid group	0.0026	
	(0.0082)	
Mixed intervention group	-0.0062	
	(0.0073)	

Notes: Differences are obtained from an OLS regression of the attrition dummy on treatment dummies, strata dummies and controls. Each column represents a separate regression. Huber–White robust standard errors in parentheses. *** p<0.01 ** p<0.05 * p<0.1.

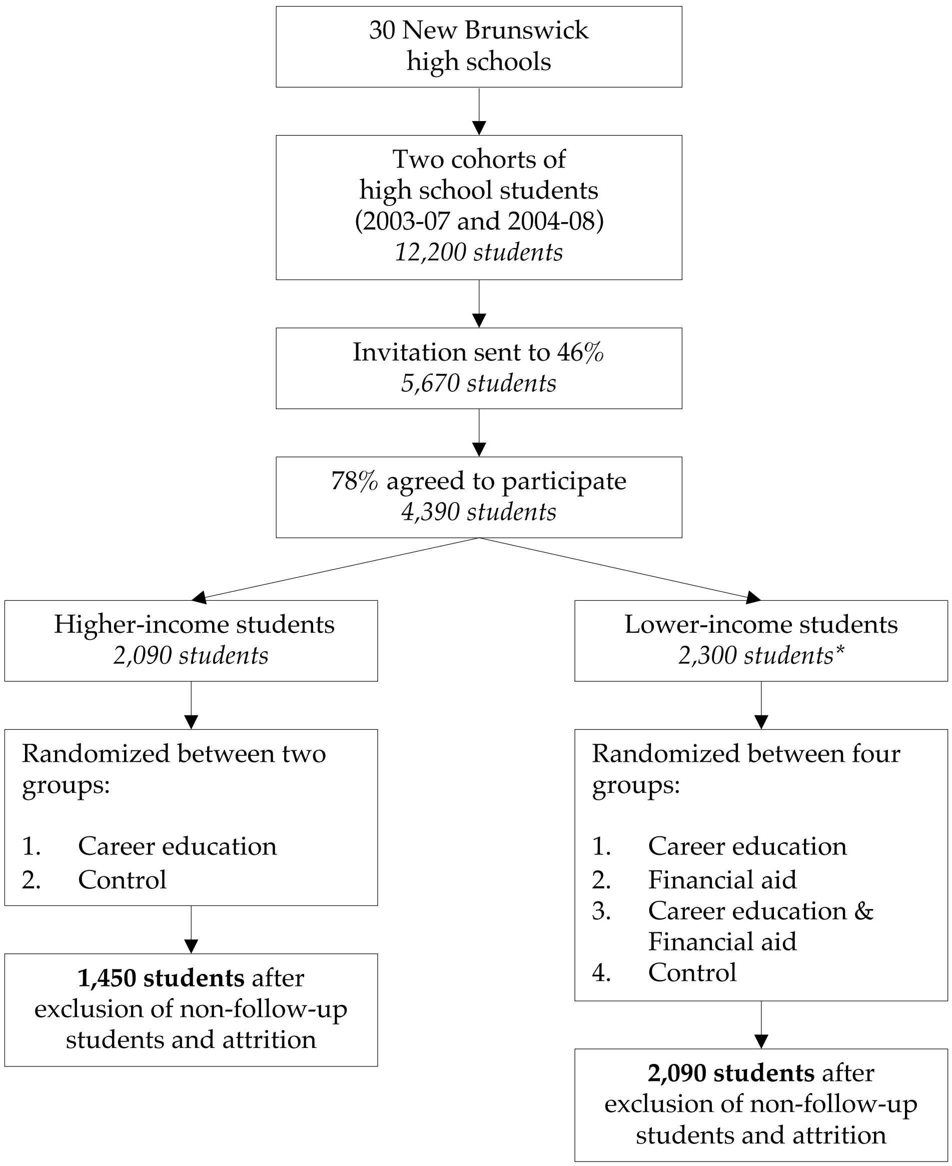


Figure 2.1: Experimental Design

Notes: The figure provides an overview of the experimental design with the number of students at each step of the randomization process. The numbers are derived from Currie et al. (2007).

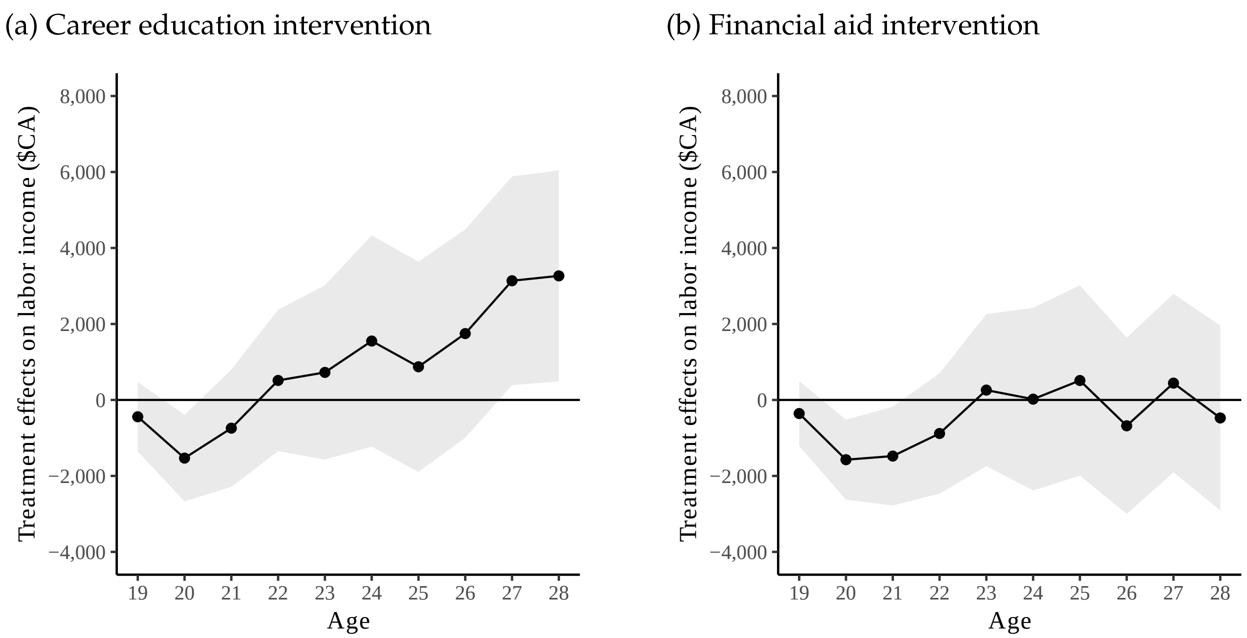


Figure 2.2: Impact on Labor Income Over Time

Notes: The figure plots the effects of eligibility for the career education program and for the financial aid on labor income against age. Point estimates together with the associated 90 percent confidence intervals are reported. Each point is estimated from a separate OLS estimation of equation 2.1. Huber–White robust standard errors are used to compute the confidence intervals. Earnings are expressed in 2020 Canadian dollars.

(a) Treatment effects on low-income students

(b) Treatment effects on high-income students

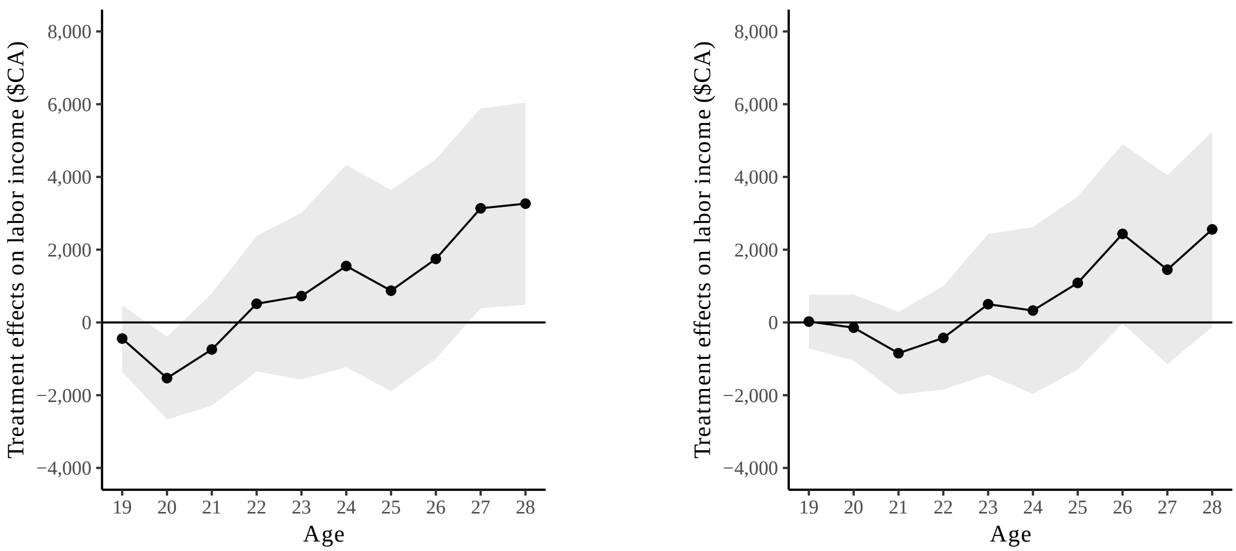


Figure 2.3: Impact on Labor Income Over Time

Notes: The figure plots the effects of eligibility for the career education program on labor income against age, for both low- and high-income students. Point estimates together with the associated 90 percent confidence intervals are reported. Each point is estimated from a OLS regression of the outcome on the treatment dummy, the treatment dummy interacted with the parental income dummy, the parental income dummy and controls for student baseline characteristics listed in Table 2.7. Huber–White robust standard errors are used to compute the confidence intervals. Earnings are expressed in 2020 Canadian dollars.

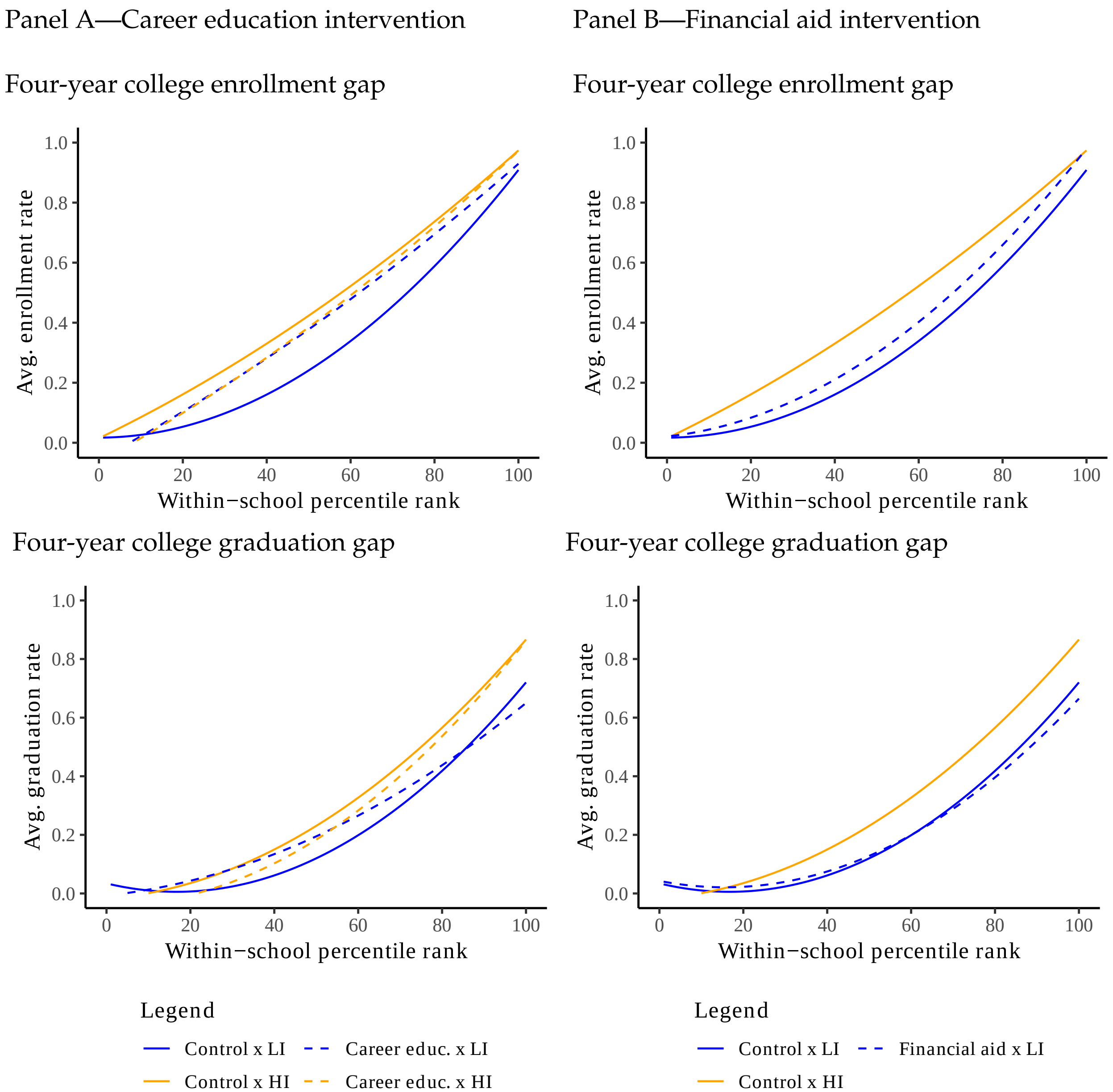


Figure 2.4: Enrollment Rates of High- and Low-Income Students by Percentile Rank and Treatment Arms

Notes: The figure plots, across within-school test scores percentile rank, the four-year college enrollment and graduation rates of high- and low-income students. Panel A plots the rates for students in the career education group and Panel B the rates for students in the financial aid group. The enrollment rates in the control group are also shown for comparability purposes. Each rate is a simple average of the rates across the 30 schools, as estimated from equation 2.5.

Table 2.7: Baseline Characteristics and Differences Between Treatment and Control Groups. Low-income Students.

	Control mean	Career education group difference	Financial aid group difference	Mixed intervention group difference
Demographics:				
Woman	0.54	-0.009	0.003	-0.039
	(0.50)	(0.032)	(0.029)	(0.029)
English speaker	0.54	0.006	-0.006	-0.011
	(0.50)	(0.013)	(0.013)	(0.013)
One parent born outside Canada	0.38	-0.004	-0.007	0.031
	(0.49)	(0.031)	(0.029)	(0.029)
Household composition:				
Single parent	0.34	0.006	-0.006	0.016
	(0.48)	(0.030)	(0.028)	(0.028)
Parental working status:				
Parent(s) not working	0.20	0.001	-0.024	-0.016
	(0.40)	(0.025)	(0.023)	(0.023)
Highest level of education of parents:				
Four-year college degree	0.052	0.013	0.008	0.001
	(0.22)	(0.015)	(0.013)	(0.013)
Community college diploma	0.41	0.016	0.047	0.036
	(0.49)	(0.031)	(0.029)	(0.029)
High school diploma	0.32	-0.014	-0.017	-0.027
	(0.47)	(0.029)	(0.027)	(0.027)
Less than high school	0.22	-0.016	-0.039*	-0.010
	(0.42)	(0.025)	(0.023)	(0.024)
Aspiration in Grade 9:				
Wants a four-year college degree only	0.39	-0.002	-0.022	0.013
	(0.49)	(0.031)	(0.029)	(0.029)
Grade 9 average test score:				
Missing test scores	0.022	-0.001	0.001	0.001
	(0.15)	(0.003)	(0.003)	(0.004)
Bottom quintile	0.28	-0.077***	-0.032	-0.016
	(0.45)	(0.026)	(0.026)	(0.026)
Second quintile	0.24	-0.007	-0.031	-0.003
	(0.43)	(0.026)	(0.025)	(0.025)
Third quintile	0.19	0.004	0.032	-0.005
	(0.39)	(0.025)	(0.024)	(0.023)
Fourth quintile	0.15	0.027	0.035	0.028
	(0.36)	(0.023)	(0.022)	(0.022)
Top quintile	0.12	0.053**	-0.005	-0.005
	(0.32)	(0.022)	(0.019)	(0.019)
P-value F-test of joint significance			0.55	
Sample Size	600	1,030	1,150	1,150

Notes: Differences are based on OLS regressions of each characteristic on treatment and strata dummies. Joint test P-values are computed using a F-test of joint significance from a multinomial regression of treatment assignment on all listed characteristics and strata dummies. The sample is restricted to low-income students. Numbers in parentheses are (i) standard deviations in the control mean column and (ii) Huber–White robust standard errors in the difference with the control group columns. *** p<0.01 ** p<0.05 * p<0.1.

Table 2.8: Baseline Characteristics and Differences Between Treatment and Control Groups. High-income Students.

	Control mean	Career education group difference
Demographics:		
Woman	0.50	0.020
	(0.50)	(0.027)
English speaker	0.52	0.005
	(0.50)	(0.013)
One parent born outside Canada	0.13	0.001
	(0.34)	(0.018)
Household composition:		
Single parent	0.077	-0.012
	(0.27)	(0.014)
Parental working status:		
Parent(s) not working	0.022	-0.006
	(0.15)	(0.007)
Highest level of education of parents:		
Four-year college degree	0.30	-0.007
	(0.46)	(0.023)
Community college diploma	0.51	-0.017
	(0.50)	(0.026)
High school diploma	0.15	0.025
	(0.35)	(0.019)
Less than high school	0.037	-0.001
	(0.19)	(0.010)
Aspiration in Grade 9:		
Wants a four-year college degree only	0.49	-0.001
	(0.50)	(0.026)
Grade 9 average test score:		
Missing test scores in G9	0.020	-0.004*
	(0.14)	(0.002)
Bottom quintile	0.13	0.005
	(0.34)	(0.018)
Second quintile	0.17	-0.012
	(0.37)	(0.019)
Third quintile	0.19	0.005
	(0.39)	(0.021)
Fourth quintile	0.23	-0.017
	(0.42)	(0.022)
Top quintile	0.27	0.024
	(0.44)	(0.023)
P-value *F*-test of joint significance		0.95
Sample Size	850	1,460

Notes: Differences are based on OLS regressions of each characteristic on treatment and strata dummies. Joint test *P*-values are computed using a *F*-test of joint significance from a probit regression of treatment dummy on all listed characteristics and strata dummies. The sample is restricted to high-income students. Numbers in parentheses are (i) standard deviations in the control mean column and (ii) Huber–White robust standard errors in the difference with the control group columns. *** p<0.01 ** p<0.05 * p<0.1.

Table 2.9: Baseline Characteristics and Differences Between Treatment and Control Groups. Low-income Students. Post-Attrition.

	Control mean	Career education group difference	Financial aid group difference	Mixed intervention group difference
Demographics:				
Woman	0.54	-0.004	0.004	-0.035
	(0.50)	(0.032)	(0.030)	(0.030)
English speaker	0.53	0.007	-0.006	-0.010
	(0.50)	(0.013)	(0.013)	(0.013)
One parent born outside Canada	0.38	-0.003	-0.002	0.035
	(0.49)	(0.031)	(0.029)	(0.029)
Household composition:				
Single parent	0.34	0.008	0.000	0.019
	(0.47)	(0.030)	(0.028)	(0.028)
Parental working status:				
Parent(s) not working	0.21	-0.003	-0.027	-0.018
	(0.40)	(0.025)	(0.023)	(0.023)
Highest level of education of parents:				
Four-year college degree	0.053	0.012	0.004	0.001
	(0.22)	(0.015)	(0.013)	(0.013)
Community college diploma	0.41	0.018	0.047	0.029
	(0.49)	(0.031)	(0.029)	(0.029)
High school diploma	0.32	-0.015	-0.013	-0.023
	(0.47)	(0.030)	(0.027)	(0.027)
Less than high school	0.22	-0.015	-0.038	-0.007
	(0.42)	(0.026)	(0.023)	(0.024)
Aspiration in Grade 9:				
Wants a four-year college degree only	0.40	-0.011	-0.027	0.005
	(0.49)	(0.031)	(0.029)	(0.029)
Grade 9 average test score:				
Missing test scores in G9	0.022	-0.001	0.001	-0.001
	(0.15)	(0.003)	(0.004)	(0.003)
Bottom quintile	0.28	-0.080***	-0.032	-0.012
	(0.45)	(0.026)	(0.026)	(0.026)
Second quintile	0.24	-0.013	-0.032	-0.002
	(0.43)	(0.027)	(0.025)	(0.025)
Third quintile	0.19	0.007	0.030	-0.010
	(0.39)	(0.025)	(0.024)	(0.023)
Fourth quintile	0.15	0.028	0.035	0.030
	(0.36)	(0.024)	(0.023)	(0.022)
Top quintile	0.12	0.059***	-0.002	-0.004
	(0.32)	(0.022)	(0.019)	(0.019)
P-value F-test of joint significance			0.45	
Sample Size	590	1,010	1,120	1,130

Notes: Differences are based on OLS regressions of each characteristic on treatment and strata dummies. Joint test P-values are computed using a F-test of joint significance from a multinomial regression of treatment assignment on all listed characteristics and strata dummies. The sample is restricted to low-income students who are part of the final analytical sample. Numbers in parentheses are (i) standard deviations in the control mean column and (ii) Huber–White robust standard errors in the difference with the control group columns. *** p<0.01 ** p<0.05 * p<0.1.

Table 2.10: Baseline Characteristics and Differences Between Treatment and Control Groups. High-income Students. Post-Attrition.

	Control mean	Career education group difference
Demographics:		
Woman	0.50	0.019
	(0.50)	(0.027)
English speaker	0.52	0.005
	(0.50)	(0.013)
One parent born outside Canada	0.13	0.004
	(0.34)	(0.018)
Household composition:		
Single parent	0.073	-0.008
	(0.26)	(0.014)
Parental working status:		
Parent(s) not working	0.023	-0.006
	(0.15)	(0.007)
Highest level of education of parents:		
Four-year college degree	0.30	-0.005
	(0.46)	(0.024)
Community college diploma	0.52	-0.019
	(0.50)	(0.026)
High school diploma	0.15	0.025
	(0.35)	(0.019)
Less than high school	0.037	-0.002
	(0.19)	(0.010)
Aspiration in Grade 9:		
Wants a four-year college degree only	0.49	-0.002
	(0.50)	(0.027)
Grade 9 average test score:		
Missing test scores in G9	0.020	-0.004*
	(0.14)	(0.002)
Bottom quintile	0.13	0.002
	(0.34)	(0.018)
Second quintile	0.16	-0.012
	(0.37)	(0.019)
Third quintile	0.18	0.007
	(0.39)	(0.021)
Fourth quintile	0.23	-0.016
	(0.42)	(0.022)
Top quintile	0.27	0.022
	(0.44)	(0.023)
P-value F-test of joint significance		0.96
Sample Size	850	1,450

Notes: Differences are based on OLS regressions of each characteristic on treatment and strata dummies. Joint test P-values are computed using a F-test of joint significance from a probit regression of treatment dummy on all listed characteristics and strata dummies. The sample is restricted to high-income students who are part of the final analytical sample. Numbers in parentheses are (i) standard deviations in the control mean column and (ii) Huber–White robust standard errors in the difference with the control group columns. *** p<0.01 ** p<0.05 * p<0.1.

Table 2.11: Distribution of missing earnings records

	# of students (1)	# of data points (2)	# missing (3)	% missing (4)
Data collected until age 28	2,320	23,200	1,200	5.2 %
Data collected until age 24	1,210	12,100	5,400	44.6 %
Until age 24	1,210	7,260	560	7.7 %
Between ages 25 and 28	1,210	4,840	4,840	100 %
Total	3,540	35,400	6,600	18.6 %

Notes: The table reports the number of students for which earnings data were collected until age 28 and until age 24 in column (1), the corresponding number of earnings data points theoretically collected in column (2), and the number and fraction of missing records over the total number of data points in column (3) and (4).

Table 2.12: Sensitivity of the Results to the Inclusion and Exclusion of Controls. Career Education Intervention. Low-income Students.

Dependent variable	No control (1)	Controls (2)	LASSO (3)
Panel A—College Enrollment			
Ever enrolled in any public college	0.083***	0.034	0.031
	(0.031)	(0.028)	(0.028)
Ever enrolled in a four-year college	0.136***	0.083***	0.079***
	(0.029)	(0.024)	(0.024)
Ever enrolled in a community college	-0.002	-0.012	-0.012
	(0.030)	(0.030)	(0.030)
Panel B—College Graduation			
Ever graduated from any college	0.034	-0.007	-0.011
	(0.030)	(0.028)	(0.028)
Ever graduated from a four-year college	0.074***	0.037*	0.033
	(0.024)	(0.021)	(0.021)
Ever graduated from a community college	-0.024	-0.033	-0.034
	(0.027)	(0.027)	(0.026)
Panel C—College Dropout			
Dropped out of college	0.039	0.032	0.031
	(0.024)	(0.024)	(0.024)
Panel D—Earnings			
Annual labor income at age 28 (CA$)	5,555***	3,265*	3,786**
	(1,827)	(1,688)	(1,684)
Annual total earnings at age 28 (CA$)	5,668***	3,631**	4,032**
	(1,911)	(1,801)	(1,791)

Notes: The table reports the treatment effects of eligibility for the career education program on the main outcomes of interest, using different sets of controls. Column (1) reports the estimates from separate OLS regressions of the dependent variables on treatment and strata dummies only. Column (2) reports the estimates from separate OLS regressions of the dependent variables on treatment dummies, strata dummies, and controls for student baseline characteristics, as reported in the main tables. Column (3) reports the estimates from separate OLS regressions of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics selected using post-double-selection lasso. The sample is restricted to low-income students. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.13: Sensitivity of the Results to the Inclusion and Exclusion of Controls. Financial Aid Intervention. Low-income Students.

Dependent variable	No control (1)	Controls (2)	LASSO (3)
Panel A—College Enrollment			
Ever enrolled in any public college	0.103***	0.080***	0.080***
	(0.029)	(0.026)	(0.026)
Ever enrolled in a four-year college	0.053**	0.032	0.032
	(0.026)	(0.021)	(0.021)
Ever enrolled in a community college	0.072**	0.065**	0.066**
	(0.029)	(0.028)	(0.028)
Panel B—College Graduation			
Ever graduated from any college	0.093***	0.075***	0.074***
	(0.029)	(0.026)	(0.026)
Ever graduated from a four-year college	0.010	-0.003	-0.003
	(0.021)	(0.018)	(0.018)
Ever graduated from a community college	0.083***	0.076***	0.077***
	(0.027)	(0.026)	(0.026)
Panel C—College Dropout			
Dropped out of college	0.002	-0.003	-0.001
	(0.021)	-0.021	(0.021)
Panel D—Earnings			
Annual labor income at age 28 (CA$)	510	-473	-472
	(1,603)	(1,477)	(1,483)
Annual total earnings at age 28 (CA$)	1,441	530	545
	(1,564)	(1,483)	(1,485)

Notes: The table reports the treatment effects of eligibility for the financial aid on the main outcomes of interest, using different sets of controls. Column (1) reports the estimates from separate OLS regressions of the dependent variables on treatment and strata dummies only. Column (2) reports the estimates from separate OLS regressions of the dependent variables on treatment dummies, strata dummies, and controls for student baseline characteristics, as reported in the main tables. Column (3) reports the estimates from separate OLS regressions of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics selected using post-double-selection lasso. The sample is restricted to low-income students. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.14: Sensitivity of the Results to the Inclusion and Exclusion of Controls. Mixed Intervention. Low-income Students.

Dependent variable	No control (1)	Controls (2)	LASSO (3)
Panel A—College Enrollment			
Ever enrolled in any public college	0.071**	0.059**	0.059**
	(0.029)	(0.025)	(0.025)
Ever enrolled in a four-year college	0.096***	0.087***	0.085***
	(0.027)	(0.021)	(0.021)
Ever enrolled in a community college	0.017	0.013	0.010
	(0.028)	(0.028)	(0.028)
Panel B—College Graduation			
Ever graduated from any college	0.065**	0.055**	0.054**
	(0.029)	(0.026)	(0.026)
Ever graduated from a four-year college	0.034	0.026	0.025
	(0.022)	(0.018)	(0.018)
Ever graduated from a community college	0.031	0.028	0.026
	(0.026)	(0.026)	(0.026)
Panel C—College Dropout			
Dropped out of college	-0.002	-0.003	-0.003
	(0.021)	-0.021	(0.021)
Panel D—Earnings			
Annual labor income at age 28 (CA$)	2,260	1,588	1,522
	(1,596)	(1,506)	(1,494)
Annual total earnings at age 28 (CA$)	1,970	1,415	1,412
	(1,533)	(1,471)	(1,459)

Notes: The table reports the treatment effects of eligibility for both the career education and the financial aid interventions on the main outcomes of interest, using different sets of controls. Column (1) reports the estimates from separate OLS regressions of the dependent variables on treatment and strata dummies only. Column (2) reports the estimates from separate OLS regressions of the dependent variables on treatment dummies, strata dummies, and controls for student baseline characteristics, as reported in the main tables. Column (3) reports the estimates from separate OLS regressions of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics selected using post-double-selection lasso. The sample is restricted to low-income students. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.15: Fisher-Exact P-values and P-values Corrected for Multiple Hypothesis Testing. Low-Income Sample.

	Career education			Financial aid			Mixed intervention		
Dependent variable	Classic *p*-values (1)	Exact *p*-values (2)	FDR *q*-values (3)	Classic *p*-values (4)	Exact *p*-values (5)	FDR *q*-values (6)	Classic *p*-values (7)	Exact *p*-values (8)	FDR *q*-values (9)
Panel A—College Enrollment									
Ever enrolled in any public college	0.225	0.221	0.221	0.002	0.003	0.017	0.021	0.018	0.068
Ever enrolled in a four-year college	0.000	0.000	0.001	0.138	0.127	0.174	0.000	0.000	0.001
Ever enrolled in a community college	0.686	0.681	0.448	0.023	0.021	0.044	0.654	0.657	0.646
Panel B—College Graduation									
Ever graduated from any college	0.811	0.810	0.448	0.005	0.005	0.017	0.037	0.044	0.084
Ever graduated from a four-year college	0.082	0.065	0.171	0.880	0.884	0.686	0.154	0.160	0.275
Ever graduated from a community college	0.213	0.226	0.221	0.004	0.002	0.017	0.275	0.279	0.400
Panel C—College Dropout									
Dropped out of college	0.178	0.166	0.221	0.890	0.895	0.686	0.882	0.880	0.814
Dropped out of four-year college	0.037	0.031	0.153	0.128	0.123	0.174	0.005	0.005	0.026
Dropped out of community college	0.369	0.353	0.327	0.352	0.352	0.336	0.367	0.366	0.400
Panel D—Earnings									
Annual labor income at age 28 (CA$)	0.053	0.059	0.153	0.749	0.740	0.686	0.292	0.290	0.400
Annual total earnings at age 28 (CA$)	0.044	0.036	0.153	0.721	0.716	0.686	0.336	0.332	0.400

Notes: The table reports the p-values associated with the tests of significance of the main treatment effects of eligibility for the career education program, eligibility for the financial aid, and eligibility for both. Columns (1), (4), and (7) present the sampling based unadjusted p-values presented in the main text. Columns (2), (5), and (8) present the Fisher-exact p-values. And Columns (3), (6), and (9) present the sharpened q-values which control for the False Discovery Rate. Sample is restricted to low-income students.

Table 2.16: Fisher-Exact P-values and P-values Corrected for Multiple Hypothesis Testing. High-Income Sample.

	Career education		
Dependent variable	Classic p-values (1)	Exact p-values (2)	FDR q-values (3)
Panel A—College Enrollment			
Ever enrolled in any public college	0.161	0.153	0.549
Ever enrolled in a four-year college	0.065	0.069	0.549
Ever enrolled in a community college	0.684	0.686	0.810
Panel B—College Graduation			
Ever graduated from any college	0.571	0.557	0.750
Ever graduated from a four-year college	0.436	0.439	0.597
Ever graduated from a community college	0.774	0.772	0.810
Panel C—College Dropout			
Dropped out of college	0.896	0.901	0.810
Dropped out of four-year college	0.159	0.168	0.549
Dropped out of community college	0.244	0.246	0.549
Panel D—Earnings			
Annual labor income at age 28 (CA$)	0.116	0.107	0.549
Annual total earnings at age 28 (CA$)	0.096	0.090	0.549

Notes: The table reports the p-values associated with the tests of significance of the main treatment effects of eligibility for the career education program, eligibility for the financial aid, and eligibility for both. Columns (1), (4), and (7) present the sampling based unadjusted p-values presented in the main text. Columns (2), (5), and (8) present the Fisher-exact p-values. And Columns (3), (6), and (9) present the sharpened q-values which control for the False Discovery Rate. Sample is restricted to high-income students.

Table 2.17: Treatments Effects on Low-Income Students' High School Outcomes

	Std. average test score in Grade 12		Ever graduated from high school	
Treatment effect of	(1)	(2)	(3)	(4)
Career education	-0.032	-0.028	0.022	0.024
	(0.058)	(0.061)	(0.019)	(0.019)
P-value selective missingness	0.623		0.752	
Financial aid	-0.007	-0.014	0.000	0.000
	(0.052)	(0.054)	(0.019)	(0.019)
P-value selective missingness	0.454		0.223	
Inverse Probability Weights	N	Y	N	Y
Control mean	-0.20		0.85	
Sample size	1,720		1,860	

Notes: The table reports the treatment effects of eligibility for the career education program and eligibility for the financial aid on academic performance and high school graduation. Average test scores in Grade 12 is standardized with a mean of zero and a standard deviation of one across all students. Each column represents a separate OLS regression of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics listed in Table 2.7 (see equation 2.1). I do not have information on test scores in Grade 12 and high school graduation for all students in the sample; test scores in Grade 12 and graduation data are missing for 20 percent and 13 percent of the low-income students, respectively. I test for selective attrition by regressing the indicator of missingness on the treatment dummies, strata dummies and controls, and report the p-value associated with the test of significance for each of the treatment coefficients. Columns (1) and (3) report the results of the unweighted regressions. To enable the comparison of the treatment effects measured on the restricted sample with the ones measured from the full sample, I also report the effects using IPW in columns (2) and (4). These weights are constructed from a probit regression of an indicator of missingness on treatment dummies, baseline characteristics, cohort and school dummies. The sample is restricted to low-income students. Sample sizes are rounded to the nearest 10 for data confidentiality concerns. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.18: Treatments Effects on Low-Income Students' Enrollment in Private Career Colleges

Treatment effect of	Enrolled in a private career college only (1)		(2)
Career education	0.006		0.008
	(0.024)		(0.024)
P-value selective missingness		0.508	
Financial aid	0.003		0.005
	(0.021)		(0.022)
P-value selective missingness		0.146	
Inverse Probability Weights	N		Y
Control mean		0.11	
Sample size		1,830	

Notes: The table reports the treatment effects of eligibility for the career education program and for the financial aid on private career college enrollment. The outcome takes the value of one if a student has ever enrolled in a private career college according to the survey conducted at age 20 but has never enrolled in a public college. Each column represents a separate OLS regression of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics listed in Table 2.7 (see equation 2.1). 87 percent of low-income students answered the survey. I test for selective missingness by regressing the indicator of missingness on the treatment dummies, strata dummies and controls, and report the *p*-value associated with the test of significance for each of the treatment coefficients. Columns (1) and (3) report the results of the unweighted regressions and columns (2) and (4) of the regressions adjusted with inverse probability weights (IPW). These weights are constructed from a probit regression of an indicator of missingness on treatment dummies, baseline characteristics, cohort and school dummies. The sample is restricted to low-income students. Sample sizes are rounded to the nearest 10 for data confidentiality concerns. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

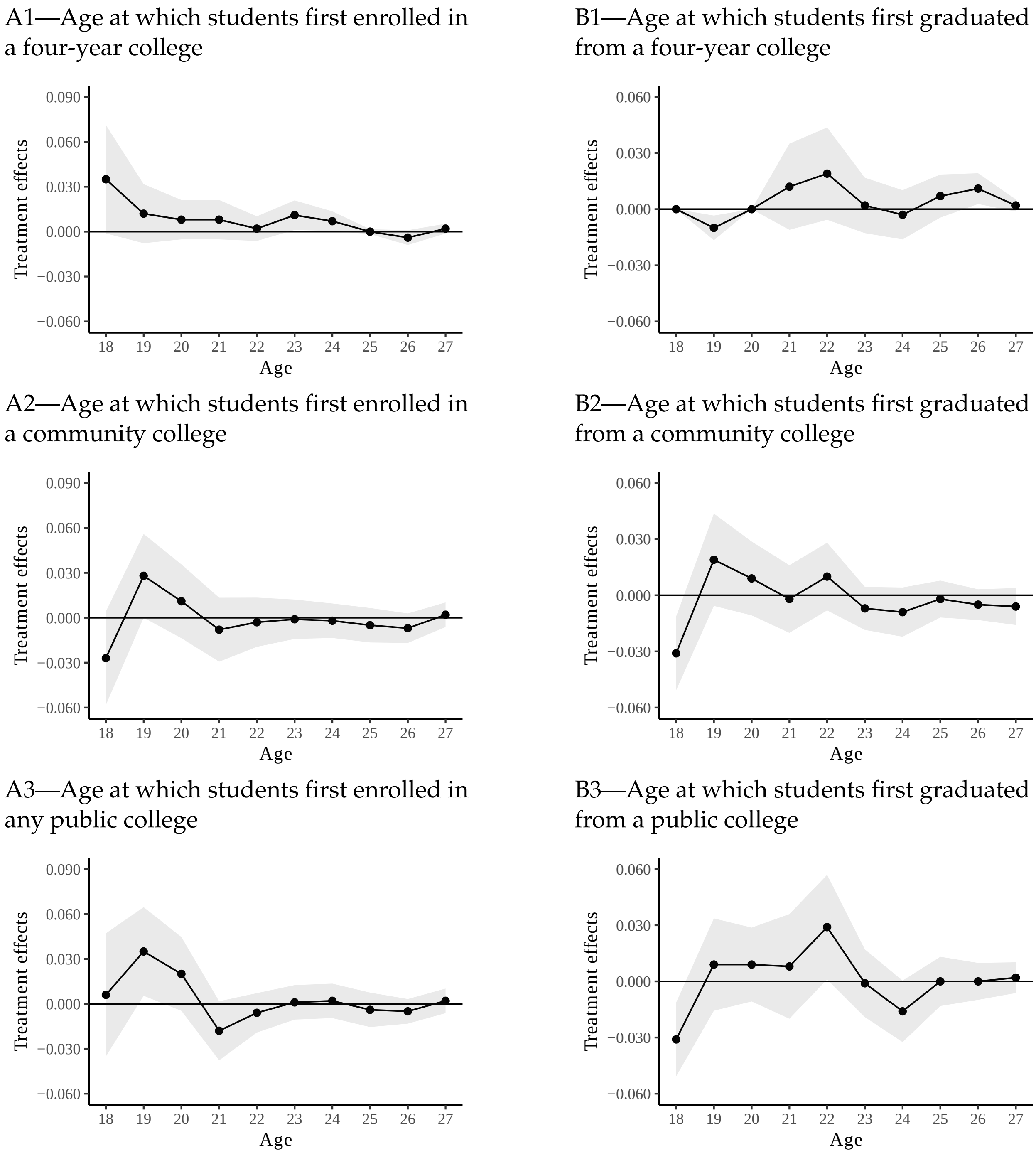


Figure 2.5: Dynamic Treatment Effects of Eligibility for the Career Education Program on College Enrollment and Graduation

Notes: The figure plots the effects of eligibility for the career education program on the fraction of students who first enrolled in college and who first graduated from college from ages 18 to 27. Point estimates together with the associated 90 percent confidence intervals are reported. Each point is estimated from a OLS regression of the outcome on treatment dummies, strata dummies, and controls for student baseline characteristics listed in Table 2.7. Huber–White robust standard errors are used to compute the confidence intervals.

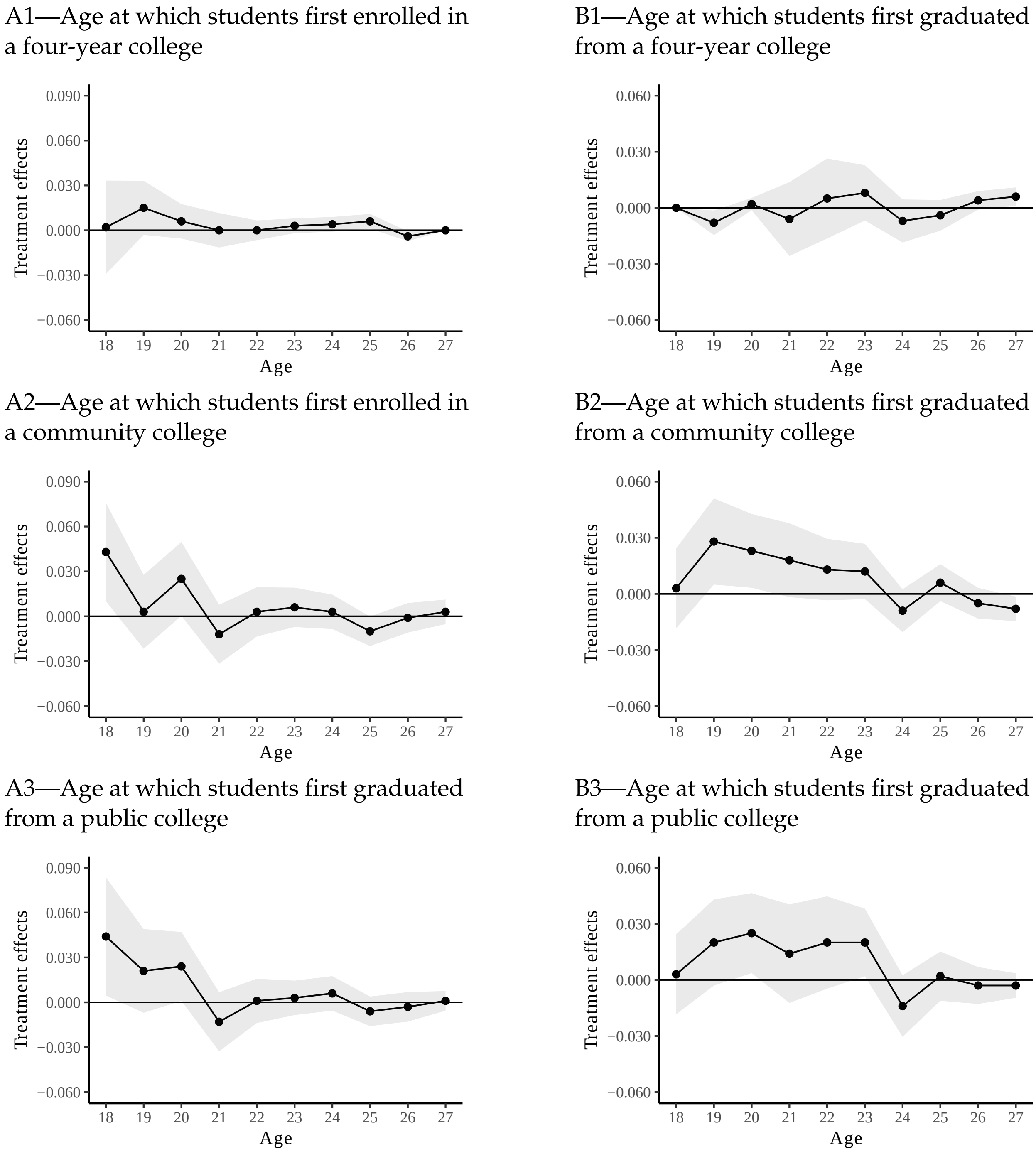


Figure 2.6: Dynamic Treatment Effects of Eligibility for the Financial Aid Intervention on College Enrollment and Graduation

Notes: The figure plots the effects of eligibility for the financial aid on the fraction of students who first enrolled in college and who first graduated from college from ages 18 to 27. Point estimates together with the associated 90 percent confidence intervals are reported. Each point is estimated from a OLS regression of the outcome on treatment dummies, strata dummies, and controls for student baseline characteristics listed in Table 2.7. Huber–White robust standard errors are used to compute the confidence intervals.

Table 2.19: Treatment Effects by Average Test Scores in Grade 9

	Ever enrolled in		Ever graduated from	
Test scores percentile rank (within-school)	Any public college	Four-year college	Any public college	Four-year college
Panel A—Treatment effects of the career education intervention				
25th	0.04	0.07	0.00	0.05
50th	0.02	0.13	0.00	0.07
75th	0.02	0.12	-0.01	0.04
P-value interaction terms	0.86	0.06	0.98	0.33
Panel B—Treatment effects of the financial aid intervention				
25th	0.10	0.02	0.08	0.01
50th	0.10	0.05	0.11	0.00
75th	0.06	0.06	0.09	-0.02
P-value interaction terms	0.54	0.37	0.50	0.88
Panel C—Treatment effects of the mixed intervention				
25th	0.05	0.07	0.04	0.01
50th	0.09	0.12	0.01	0.03
75th	0.10	0.15	-0.02	0.05
P-value interaction terms	0.40	0.03	0.89	0.53

Notes: The table reports the heterogeneity of the treatment effects of the three interventions by average test scores in Grade 9 for the main outcomes of interest. The sample is restricted to low-income students. Average test scores in Grade 9 is standardized within each school. Treatment effects are computed at the 25th, 50th, and 75th within-school percentile of the average test scores distribution. Each column represents a separate OLS regression of the dependent variable on treatment dummies, treatment dummies interacted with average test scores and average test scores squared, strata dummies, and controls for student baseline characteristics listed in Table 2.7, average test scores and average test scores squared. Treatment effects are constructed from the coefficients on the treatment dummies and the interaction terms. Huber–White robust standard errors are estimated. I test whether the two interaction terms a jointly significant and report the associated p-value.

Table 2.20: Treatments Effects on Low-Income Students' College Completion Conditional on Enrollment

Dependent variable	Control mean (1)	Career education (2)	Financial aid (3)	Mixed intervention (4)
Panel A—Conditional on any college enrollment				
Ever graduated for college	0.71	-0.051	0.028	0.022
		(0.039)	(0.035)	(0.036)
Panel B—Conditional on four-year college enrollment				
Highest degree is a four-year college degree	0.61	-0.012	-0.088	-0.051
		(0.058)	(0.059)	(0.057)
Highest degree is a community college diploma	0.12	-0.009	0.050	0.043
		(0.039)	(0.041)	(0.039)
Dropped out	0.27	0.021	0.038	0.008
		(0.054)	(0.052)	(0.052)
Panel C—Conditional on community college enrollment				
Highest degree is a four-year college degree	0.10	0.053	-0.015	0.020
		(0.033)	(0.026)	(0.027)
Highest degree is a community college diploma	0.60	-0.112**	0.085*	0.046
		(0.055)	(0.046)	(0.048)
Dropped out	0.30	0.059	-0.070*	-0.066
		(0.050)	(0.041)	(0.044)
Sample size	590		2,090	

Notes: The table reports the treatment effects of eligibility for the career education program, for the financial aid and for both on college graduation conditional on enrollment. Each row represents a separate OLS regression of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics listed in Table 2.7 (see equation 2.1). Graduation is measured by age 28. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. The sample is restricted to low-income students. Sample sizes are rounded to the nearest 10 for data confidentiality concerns.

Table 2.21: Treatments Effects on Low-Income Students' Post-Secondary Education Trajectories

Dependent variable	Control mean (1)	Career education (2)	Financial aid (3)	Mixed intervention (4)
Panel A—Students who first enrolled in a four-year college				
First enrolled in a four-year college	0.21	0.057**	0.030	0.075***
		(0.023)	(0.021)	(0.021)
Switched to a community college	0.06	0.009	0.015	0.029*
		(0.016)	(0.014)	(0.015)
Highest degree is a four-year college degree	0.11	0.018	-0.004	0.021
		(0.021)	(0.018)	(0.018)
Highest degree is a community college diploma	0.03	0.007	0.024**	0.021**
		(0.010)	(0.010)	(0.010)
Dropped out	0.06	0.027*	0.010	0.026*
		(0.016)	(0.014)	(0.014)
Panel B—Students who first enrolled in a community college				
First enrolled in a community college	0.31	-0.017	0.048*	-0.017
		(0.028)	(0.027)	(0.027)
Switched to a four-year college	0.02	0.028**	0.002	0.012
		(0.011)	(0.008)	(0.009)
Highest degree is a four-year college degree	0.02	0.019**	0.001	0.005
		(0.008)	(0.005)	(0.006)
Highest degree is a community college diploma	0.19	-0.051**	0.053**	0.008
		(0.024)	(0.024)	(0.024)
Dropped out	0.10	0.005	-0.015	-0.029*
		(0.019)	(0.017)	(0.016)
Sample size	590		2,090	

Notes: The table reports the treatment effects of eligibility for the career education program, for the financial aid and for both on college enrollment and completion by type of first enrollment. Each row represents a separate OLS regression of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics listed in Table 2.7 (see equation 2.1). Enrollment is measured by age 27. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. The sample is restricted to low-income students. Sample sizes are rounded to the nearest 10 for data confidentiality concerns.

Table 2.22: Treatment Effects on Low-Income Students' Years of Post-Secondary Education

Dependent variable	Control mean (1)	Career education (2)	Financial aid (3)	Mixed intervention (4)
Total years spent in college	1.76	0.235*	0.150	0.273**
		(0.133)	(0.115)	(0.118)
Sample size	590		2,090	

Notes: The table reports the treatment effects of eligibility for the career education program, for the financial aid, and for both on the numbers of years of post-secondary schooling. Each row represents a separate OLS regression of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics listed in Table 2.7 (see equation 2.1). Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.23: Treatments Effects on Low-Income Students' Earnings

Dependent variable	Control mean (1)	Career education (2)	Financial aid (3)	Mixed intervention (4)
Panel A— Labor income ($CA)				
Labor income at age 19	10,700	-443	-357	-1,119**
		(556)	(519)	(514)
			{0.880}	{0.231}
				{0.148}
Labor income at age 24	25,500	1,551	20	147
		(1,691)	(1,462)	(1,445)
			{0.371}	{0.404}
				{0.930}
Labor income at age 28	31,700	3,265*	-473	1,588
		(1,688)	(1,477)	(1,506)
			{0.035}	{0.346}
				{0.192}
Panel B—Total earnings ($CA)				
Total earnings at age 19	13,300	-523	-567	-1,070*
		(603)	(558)	(558)
			{0.942}	{0.369}
				{0.372}
Total earnings at age 24	31,300	1,980	62	190
		(1,629)	(1,412)	(1,384)
			{0.250}	{0.274}
				{0.927}
Total earnings at age 28	38,200	3,631**	530	1,415
		(1,801)	(1,483)	(1,471)
			{0.110}	{0.251}
				{0.584}
Sample size	590		2,090	

Notes: The table reports the treatment effects of eligibility for the career education program, for the financial aid, and for both, on labor income and total earnings by age. Individuals with no income are included in the regression with an income of zero. Each row represents a separate OLS regression of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics listed in Table 2.7 (see equation 2.1). Earnings are expressed in 2020 Canadian dollars. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. For each outcome, I test whether the treatment effect of each of the two financial aid interventions is significantly different from the effect of the career education only intervention and report the associated p-value below the standard errors in square brackets. I also test whether the treatment effect of the mixed intervention is significantly different from the effect of the financial aid intervention and report the associated p-value below the standard errors in braces. The sample is restricted to low-income students. Sample sizes are rounded to the nearest 10 for data confidentiality concerns.

Table 2.24: Treatment Effects on Students' Probability of Working at Age 28

Treatment group	Low-income students (1)	High-income students (2)
Panel A—Dep. var.: Labor Income > 0		
Control Mean	0.95	0.94
Treatment Effects		
Career education intervention	0.001	0.000
	(0.014)	(0.013)
Financial aid intervention	-0.005	
	(0.014)	
Mixed intervention	0.002	
	(0.013)	
Panel B—Dep. var.: Labor Income > $16,000		
Control Mean	0.72	0.79
Treatment Effects		
Career education intervention	-0.013	0.028
	(0.027)	(0.021)
Financial aid intervention	-0.038	
	(0.026)	
Mixed intervention	-0.015	
	(0.026)	

Notes: The table reports the treatment effects of eligibility for the career education program, for the financial aid, and for both on the likelihood of reporting any labor income and annual labor income greater than $16,000 which roughly corresponds to a full time job at the minimum wage. Each column represents a separate OLS regression of the dependent variable on treatment dummies, strata dummies, and controls for student baseline characteristics (see equation 2.1). Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.25: Robustness of the Effects on Earnings to Alternative Forecasting Models.

	Career education			Financial aid			Mixed intervention		
	Main model	Model with cov.	Simple model	Main model	Model with cov.	Simple model	Main model	Model with cov.	Simple model
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A— Low-income students									
Labor income	3,265*	3,176*	3,190*	-474	-533	-786	1,588	1,512	900
at age 28 ($CA)	(1,645)	(1,688)	(1,506)	(1,478)	(1,447)	(1,474)	(1,507)	(1,473)	(1,494)
Total earnings	3,631**	3,534**	3,556**	530	426	255	1,415	1,331	781
at age 28 ($CA)	(1,801)	(1,766)	(1,802)	(1,483)	(1,462)	(1,480)	(1,471)	(1,445)	(1,458)
Panel B— High-income students									
Labor income	2,558	2,616	2,665						
at age 28 ($CA)	(1,633)	(1,616)	(1,625)						
Total earnings	2,788*	2,827*	2,866*						
at age 28 ($CA)	(1,680)	(1,666)	(1,676)						

Notes: The table reports the treatment effects of eligibility for the career education program, for the financial aid and for both on labor income and total earnings by age, using alternative models of forecasting for the earnings data. The coefficients from Panel A are obtained by estimating equation 2.1 and the ones from Panel B are obtained by estimating equation 2.2. Earnings are expressed in 2020 Canadian dollars. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.26: Prediction Error

Treatment group	Low-income students (1)	High-income students (2)
Panel A—Prediction Error > \$5,000		
Control group mean	0.251	0.273
Difference with control group		
Career education intervention	0.000	-0.007
	(0.028)	(0.024)
Financial aid intervention	0.027	
	(0.026)	
Mixed intervention	0.023	
	(0.026)	
Panel B—Prediction Error < -\$5,000		
Control group mean	0.259	0.332
Difference with control group		
Career education intervention	0.030	0.011
	(0.028)	(0.025)
Financial aid intervention	-0.008	
	(0.026)	
Mixed intervention	0.002	
	(0.026)	

Notes: The table reports the fraction of prediction errors greater than \$5,000 and less than -\$5,000 in the control group together with the differences in these fractions between the control group and each treatment group. The prediction errors are computed by taking the difference between the value at age 24 predicted by the forecasting model and the true observed value, for each student. Each column represents a separate OLS regression of the dependent variable on treatment dummies. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

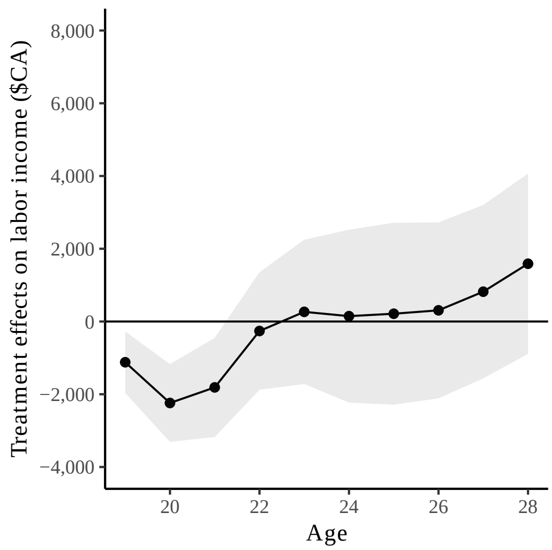


Figure 2.7: Impact of the Mixed Intervention on Labor Income Over Time

Notes: The figure plots the effects of the mixed intervention on labor income against age. Point estimates together with the associated 90 percent confidence intervals are reported. Each point is estimated from a OLS regression of the outcome on treatment dummies, strata dummies, and controls for student baseline characteristics listed in Table 2.7. Huber–White robust standard errors are used to compute the confidence intervals. Earnings are expressed in 2020 Canadian dollars.

Table 2.27: Comparison of the Treatments Effects on Low- and High-Income Students' Earnings

Dependent variable	Low-income students (1)	High-income students (2)	Difference high vs. low (3)
Panel A—Labor income ($CA)			
Labor income at age 19	-411	24	435
	(547)	(449)	(708)
	10,700	*11,000*	*300*
Labor income at age 24	1427	327	-1100
	(1,664)	(1,395)	(2,172)
	25,500	*31,800*	*6,300*
Labor income at age 28	3,050*	2,558	-491
	(1,655)	(1,633)	(,2325)
	31,700	*43,200*	*11,500*
Panel B—Total earnings ($CA)			
Total earnings at age 19	-455	14	469
	(591)	(558)	(813)
	13,300	*13,600*	*300*
Total earnings at age 24	1,906	8	-1,898
	(1,602)	(1,346)	(2,093)
	31,300	*36,500*	*5,200*
Total earnings at age 28	3,563**	2,788*	-774
	(1,746)	(1,680)	(2,423)
	38,200	*49,600*	*11,400*
Sample size		2,460	

Notes: The table reports the treatment effects of eligibility for the career education program on labor income and total earnings by age for both low- and high-income students. Each row represents a separate OLS regression of the dependent variable on the treatment dummy, the treatment dummy interacted with the parental income dummy, the parental income dummy and controls for student baseline characteristics listed in Table 2.7 (see equation 2.2). Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. Control means are reported in italic below the standard errors. For each outcome, I report the effect on low-income students (column (1)), the effect on high-income students (column (2)) and the effect differential between the two types of students (column (3)). The sample is restricted to students who were assigned to the control or career education groups. Sample sizes are rounded to the nearest 10 for data confidentiality concerns.

Table 2.28: Treatments Effects on Low- and High-Income Students' College Enrollment in STEM Programs

	Low-income students (1)	High-income students (2)	Difference high vs. low (3)
Dependent variable: Ever enrolled in a four-year STEM program			
Unconditional on enrollment	0.010	0.012	0.002
	(0.016)	(0.018)	(0.024)
	0.06	*0.15*	*0.09*
Conditional on four-year college enrollment	-0.055	0.061*	0.116**
	(0.046)	(0.034)	(0.058)
	0.27	*0.28*	*0.01*

Notes: The table reports the treatment effects of eligibility for the career education program on low- and high-income students' probability to enroll in a STEM program. STEM stands for science, technology, engineering and mathematics.. Each row represents a separate OLS estimation of equation 2.2. Enrollment is measured within 10 years of high school graduation. Huber–White robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. Control means are reported in italic below the standard errors. For each outcome, I report the effect on low-income students (column (1)), the effect on high-income students (column (2)) and the difference in effect between the two types of students (column (3)). Group sizes are rounded to the nearest 10 for data confidentiality concerns.

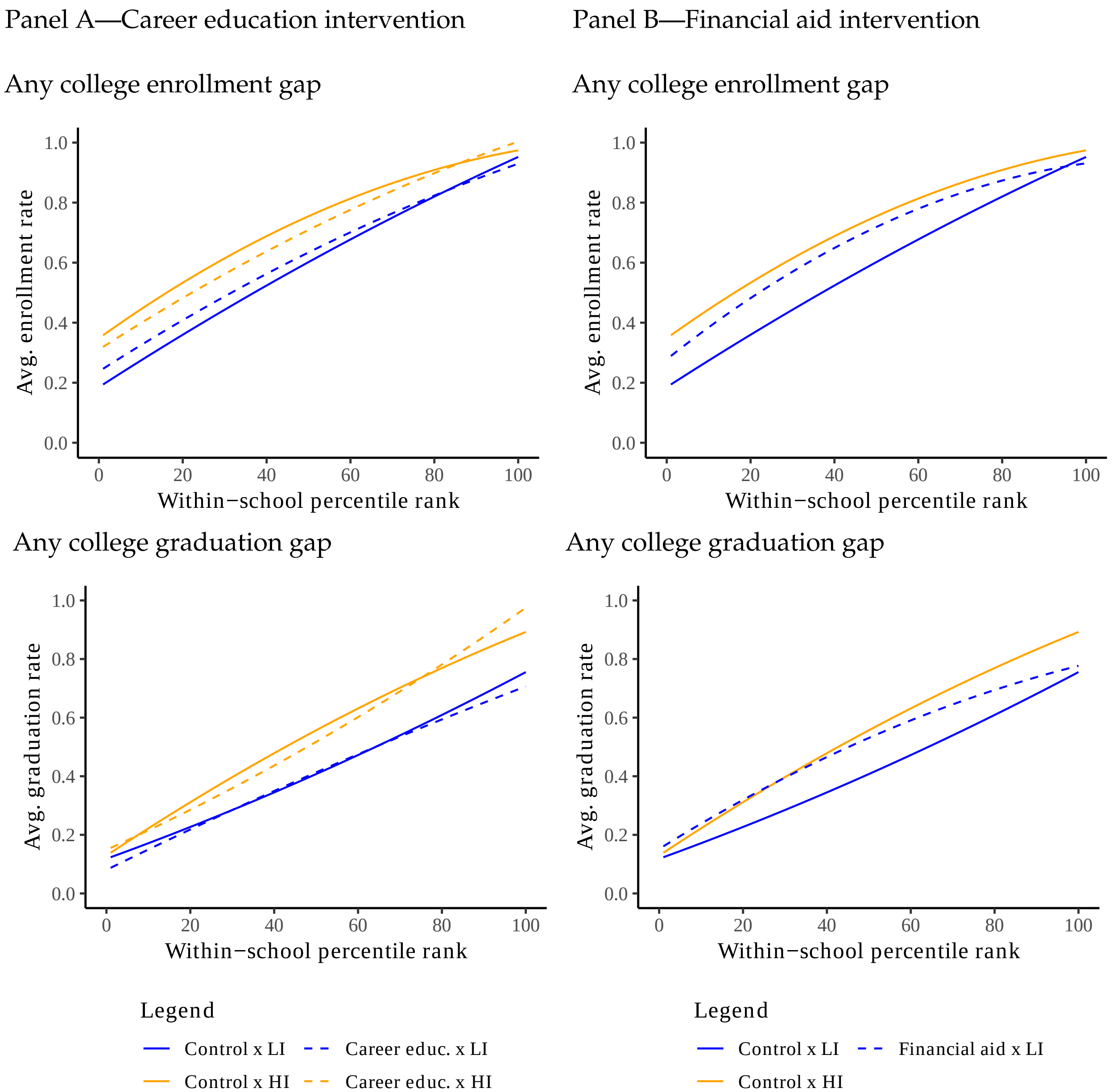


Figure 2.8: Enrollment Rates of High- and Low-Income Students
by Percentile Rank and Treatment Arms

Notes: The figure plots, across within-school test scores percentile rank, the any college enrollment and graduation rates of high- and low-income students. Panel A plots the rates for students in the career education group and Panel B the rates for students in the financial aid group. The enrollment rates in the control group are also shown for comparability purposes. Each rate is a simple average of the rates across the 30 schools, as estimated from equation 2.5.

Table 2.29: Impact on Inequality between High- and Low-Income Students

	Enrollment		Graduation	
	Any public college	Four-year college	Any public college	Four-year college
A—Control group				
Avg. gap among equally-achieving	0.13	0.13	0.12	0.09
B—Career education for both high- and low-income students				
Avg. gap among equally-achieving	0.07	0.01	0.13	0.02
Difference with gap in control group	-44%	-92%	1%	-74%
C—Financial aid targeted to low-income students				
Avg. gap among equally-achieving	0.04	0.08	0.03	0.09
Difference with gap in control group	-68%	-39%	-73%	3%

Notes: The table reports the treatment effects of eligibility for career education for both high- and low-income students and eligibility for the financial aid for low-income students on the gaps in enrollment and graduation between high- and low-income students. Each average gap among equally-achieving students is obtained by estimating the size of the gap in each school at different points of the within-school score distribution in Grade 9, and taking the average.

Panel A—Career education intervention

B—Financial aid intervention

Four-year college enrollment gap

Four-year college enrollment gap

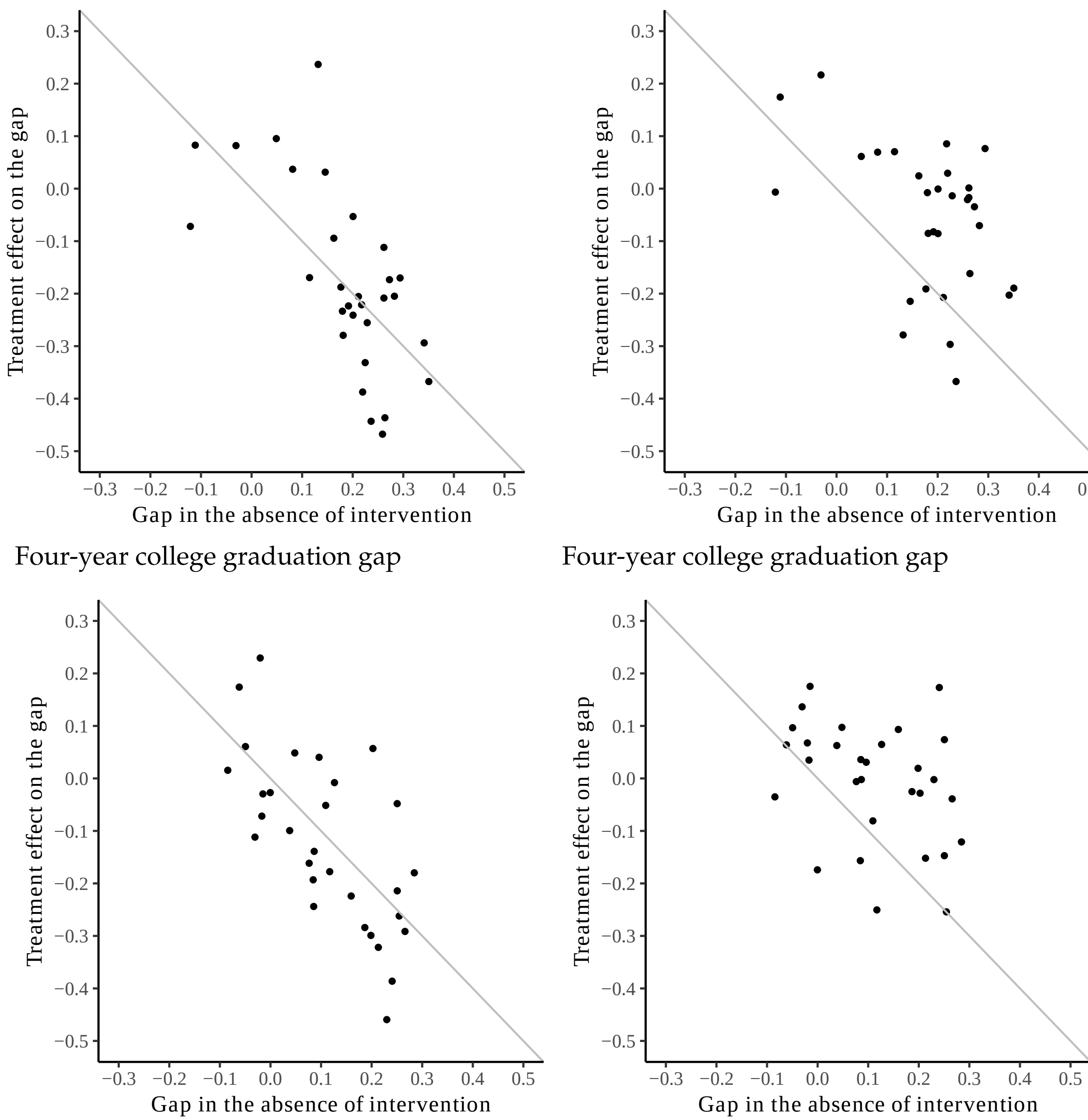


Figure 2.9: Treatment Effects on the Four-Year College Enrollment and Graduation Gaps Against Initial Gap Sizes

Notes: The figure plots, for each school, the magnitude of the reduction in the four-year college enrollment gap between high- and low-income students following each intervention, against the size of the gap in the control group. The gaps are measured at the median of the test score distribution in each school, as estimated from equation 2.5. The grey line indicates the values for which the gap is reduced by 100%.

Table 2.30: School-level Relationship Between the Treatment Effects on the Gap in Four-year College Enrollment and the Initial Gap Size

	School-level treatment effect on the four-year college enrollment gap			
	Career education intervention		Financial aid intervention	
Independent variables	(1)	(2)	(3)	(4)
School-level gap in control group	-0.995***	-0.943***	-0.555**	-0.584**
	(0.217)	(0.272)	(0.205)	(0.226)
Constant	0.006	-0.173	0.044	-0.038
	(0.047)	(0.218)	(0.044)	(0.182)
Additional school-level controls	N	Y	N	Y
Observations	30	30	30	30
R-squared	0.43	0.46	0.21	0.42
Adj. R-squared	0.41	0.33	0.18	0.27

Notes: The table reports the coefficients from school-level OLS regressions of the magnitude of the reduction in the four-year college enrollment gap following the different interventions on the size of gap in the control group. Columns (1) and (2) report the results for the treatment effects of the career education intervention and columns (3) and (4) for the financial aid intervention. The gaps are measured at the median of the test score distribution in each school, as estimated from equation 2.5. Additional school level controls, in columns (2) and (4), include school size, fraction of English speakers, fraction of immigrants, fraction of higher-income students, fraction of students with an unemployed parent, and average test score in Grade 9. Robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%

Table 2.31: Oaxaca-Blinder Decomposition of the Gaps in Enrollment and Graduation between High- and Low-Income Students. Control Group Students.

	Income group			Explained gap		Unexplained gap	
	High-income students	Low-income students	Gap	Size	% of total gap	Size	% of total gap
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ever enrolled in any college	0.782	0.520	0.262*** (0.021)	0.122*** (0.006)	47%	0.139*** (0.022)	53%
Ever enrolled in a four-year college	0.535	0.229	0.306*** (0.020)	0.171*** (0.006)	56%	0.135*** (0.020)	44%
Ever graduated from any college	0.617	0.357	0.260*** (0.023)	0.130*** (0.006)	50%	0.130*** (0.022)	50%
Ever graduated from a four-year college	0.375	0.140	0.235*** (0.019)	0.149*** (0.006)	63%	0.087*** (0.018)	37%
Sample Size	850	590	1,440				

Notes: The table reports the enrollment and graduation rates of high- and low-income students in the control group. Column (3) reports the differences between the two types of students. I decompose the gap between what can be explained by differences in academic preparation in Grade 9 between high- and low-income students and what cannot, following the Oaxaca-Blinder method. Academic preparation is measured as student average test score in Grade 9 and the quality of the school attended through school fixed effects. To decompose the gap I estimate a linear probability model of enrollment/graduation on average test score, average test score squared and school fixed effects, and use the weights from a pooled regression. Robust standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

2.8 Appendix—More on the Data

2.8.1 Data Coverage

Figure 2.10: Timeline of Administrative Data Coverage

- Spring 2004 - Recruitment of the first freshmen cohort
- Spring 2005 - Recruitment of the second freshmen cohort
- Spring 2007 - End of high school for most cohort 1 students
- Spring 2008 - End of high school for most cohort 2 students
- December 2013 - End of tax data coverage for cohort 1 students not in the PSIS/RAIS
- December 2014 - End of tax data coverage for cohort 2 students not in the PSIS/RAIS
- December 2017 - End of tax data coverage for students in the PSIS/RAIS
- Spring 2018 - End of post-secondary education data coverage

Notes: The figure presents the timeline of administrative data coverage. Students in cohort 1, and cohort 2 were in Grade 9 during the 2003-04 and 2004-05 academic year, respectively. RAIS stands for Registered Apprenticeship Information System. PSIS stands for Post-Secondary Information System.

2.8.2 Outcomes of Interest

Standardized average test scores in Grade 12: The student average test scores in Grade 12 standardized with a mean of zero and a standard deviation of one across all students. Data collected by the SRDC from the New Brunswick Department of Education.

Ever graduated from high school Indicator variable which takes the value of one if the student has graduated from high school at any point in time. Data collected by the SRDC from the New Brunswick Department of Education.

Ever enrolled in any public college Indicator variable which takes the value of one if the student has ever enrolled in any public college within 10 years of the theoretical end of high school (i.e., until spring 2017 for the first cohort and until spring 2018 for the second cohort). Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development. Exclude enrollment in private four-year colleges and private career colleges.

Ever enrolled in a four-year college Indicator variable which takes the value of one if the student has ever enrolled in a public four-year college (or university) within 10 years of the theoretical end of high school (i.e., until spring 2017 for the first cohort and until spring 2018 for the second cohort). Author's calculation from the PSIS. Exclude enrollment in private four-year colleges.

Ever enrolled in a community college Indicator variable which takes the value of one if the student has ever enrolled in a community college within 10 years of the theoretical end of high school (i.e., until spring 2017 for the first cohort and until spring 2018 for the second cohort). Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development.

Enrolled in a private career college only Indicator variable which takes the value of one if the student has ever enrolled in a private career college but never in a public college within two and half years of the theoretical end of high school. Author's calculation from the survey conducted by the SRDC two and half years after the students' theoretical end of high school.

First enrolled in a four-year college Indicator variable which takes the value of one if a student's first enrollment in a public college is in a public four-year college (or university). Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development.

First enrolled in a community college Indicator variable which takes the value of one if a student's first enrollment in a public college is in a community college. Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development.

Enrolled in a four-year college after attending a community college Indicator variable which takes the value of one if a student first enrolled in a community college and later in a public four-year college. Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development.

Enrolled in a community college after attending a four-year college Indicator variable which takes the value of one if a student first enrolled in a public four-year college and later in a community college. Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development.

Ever graduated from a public college Indicator variable which takes the value of one if the student has ever graduated from a public college within 10 years of the theoretical end of high school (i.e., until spring 2017 for the first cohort and until spring 2018 for the second cohort). Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development. Exclude graduation from private four-year colleges and private career colleges.

Highest degree earned is a four-year college degree Indicator variable which takes the value of one if the student's highest degree earned is a four-year college degree (i.e., a bachelor's degree). Author's calculation from the PSIS. Exclude graduation from private four-year colleges.

Highest degree earned is a community college diploma Indicator variable which takes the value of one if the student's highest degree earned is a community college diploma or certificate. Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development. Exclude graduation from private four-year colleges and private career colleges.

Dropped out of college Indicator variable which takes the value of one if a student (i) has ever enrolled in a public college, (ii) has never graduated from any public college, and (iii) is not enrolled during the 2016–17 academic year for cohort one and during the 2017–18 academic year for cohort two. Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development. Exclude graduation from private four-year colleges and private career colleges.

Years spent in college It indicates the number of years a student was enrolled in a public college within 10 years of the theoretical end of high school. Author's calculation from the PSIS and the data collected by the SRDC from the New Brunswick Department of Training and Employment Development. Exclude years enrolled in a private institution.

Labor income It indicated the student employment income from T4 Slips. It includes all paid-employment income, i.e. wages, salaries, and commissions, before deductions and excludes self-employment income. Expressed in 2020 Canadian dollars. Data collected from the Statistics Canada taxfiler database.

Total earnings It indicated the student total income before tax from T1 tax form. It includes employment income, self-employment income, investment income, and government transfers (pension, unemployment insurance, child benefits, etc). Expressed in 2020 Canadian dollars. Data collected from the Statistics Canada taxfiler database.

2.9 Appendix—Imputation Procedure for Earnings Data

I proceed in two steps. First, I impute the missing data to ensure that I observe a complete set of earnings over the period data were collected following a linear interpolation. Second, I forecast the earnings until 28 for students whose data are only available until age 24.

Linear Interpolation

Over the period data were collected, 6 percent of the data points are missing. For the missing records occurring between two known records (46 percent of the missing values), I impute y at age x by finding the closest points (x_0, y_0) and (x_1, y_1) on each side of x for which y is observed, and calculating:

$$y = \frac{y_1 - y_0}{x_1 - x_0}(x - x_0) + y_0. \tag{2.7}$$

For the missing points found after two known records (34 percent of the missing values), I use the two closet points on the left side of x. For the missing records found before two known records I assign the value of zero to y (25 percent of the missing values).

Forecasting

I estimate the following equation by OLS on the full sample of students in the experiment[33],

$$\begin{aligned} y_{i,t} =& \beta_0 + \beta_1 y_{i,t-1} + \beta_2 Exp_{i,t} + \beta_3 Enroll_{i,t} + \beta_4 Enroll_{i,t-1} \\ &+ \beta_5 GradFourYear_{i,t} + \beta_6 GradCoColl_{i,t} + \epsilon_{i,t} \end{aligned} \tag{2.8}$$

where $y_{i,t}$ is student i annual earnings at year t, $y_{i,t-1}$ is student i annual earnings at year $t-1$, $Exp_{i,t}$ is student i years of experience accumulated at year t,[34] $GradFourYear_{i,t}$ and $GradCoColl_{i,t}$ are dummies capturing the highest degree earned by student i at year t, and $Enroll_{i,t}$ and $Enroll_{i,t-1}$ are dummies capturing whether student i was enrolled in post-secondary education during any semester of the year. $\epsilon_{i,t}$ is an error term. I also test the robustness of the results to alternative choices of covariates. The estimated forecasting models are presented in Table 2.32.

33. Thus it includes students in the PSIS-RAIS from age 18 to 28 and students who are not in the PSIS-RAIS from age 18 to 24.
34. I use as a proxy for experience the number of years a student has not been observed enrolled in a public post-secondary education from the end of high school until year t.

For each student whose data are only available until age 24, I then forecast, in cascade, earnings from ages 25 to 28 using the estimated coefficients from equation (A2) and the earnings observed at age 24.

Table 2.32: Forecasting Models

	Y_t = Labor income at t			Y_t = Total earnings at t		
	Main model	With covariates	Simple model	Main model	With covariates	Simple model
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Y_{t-1}	0.897***	0.874***	0.927***	0.924***	0.908***	0.950***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Exp_t	-576***	-494***	-151***	-701***	-632***	-254***
	(47)	(47)	(37)	(44)	(44)	(35)
$Enroll_t$	-6,365***	-6,672***		-5,793***	-6,069***	
	(267)	(269)		(249)	(252)	
$Enroll_{t-1}$	52	-369		-612**	-922***	
	(258)	(259)		(241)	(242)	
$GradFourYear_t$	4,987***	4,579***		4,333***	3,923***	
	(230)	(257)		(215)	(241)	
$GradCoColl_t$	3,199***	3,085***		2,490***	2,372***	
	(206)	(209)		(193)	(196)	
Constant	7,728***	11,476***	5,792***	8,830***	12,005***	
	(244)	(818)	(158)	(229)	(766)	
Baseline Characteristics	N	Y	N	N	Y	N
Observations	30,460	30,460	30,460	30,460	30,460	30,460
R-squared	0.723	0.728	0.708	0.760	0.762	0.748

Notes: The table reports in columns (1) and (4) the coefficients obtained from the estimation of equation (2.8) by OLS, in columns (2) and (5) the coefficients obtained from the estimation of equation of (2.8) adding students baseline characteristics and school fixed effects as controls, and in columns (3) and (4) the coefficients obtained from the estimation of equation (2.8) where the independent variables are restricted to Y_{t-1}, Exp_t and a constant. Columns (1), (2) and (3) present the models for labor income, and Columns (4), (5) and (6) for total earnings. Standard errors are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%. Sample sizes are rounded to the nearest 10 for data confidentiality concerns.

Chapter 3

Conditional Cash Transfers and Women's Reproductive Choices

3.1 Introduction

Conditional cash transfers (CCT) are among the most widely adopted social protection programs because of their promise of improved household economic welfare and greater investments in children's human capital—promises that have largely been met (Bastagli et al. (2016)). These gains are driven by the cash transfer and/or the conditions that households must meet in order to receive the income transfer, most often linked to child schooling and health. As interest in cash transfers has increased globally, heightened further by the economic fallout of COVID-19 (Gentilini et al. (2021)), policy makers considering scaling up or expanding their interventions must take into account potential effects on a broader set of outcomes.[1] These effects include how CCT programs might influence secondary outcomes such as those that are *a priori* not directly incentivized. At the same time, policy makers may wish to better understand whether the programs have long-lasting and transformative effects (Cahyadi et al. (2020)), such that they can eventually be phased out once primary objectives of poverty reduction and increased human capital accumulation have been achieved.

One long-standing view among sceptics of anti-poverty income maintenance programs is the worry that poor households will use the cash transfer to finance greater fertility, a view dating back to Malthus' discussion of England's Poor Laws (Huzel (1986)). Such

1. This interest is also manifested by increased discussion of Universal Basic Income schemes (Green, Kesselman, and Tedds (2020); Hoynes and Rothstein (2019); Banerjee, Niehaus, and Suri (2019)).

concerns are especially salient when the cash transfer is explicitly tied to having children, as is the case for most conditional cash transfers from Brazil's Bolsa Familia to Indonesia's Program Keluarga Harapan.[2] Yet, by targeting mothers the cash transfer can empower women (Almås et al. (2018); Das, Do, and Özler (2005); Fiszbein and Schady (2009); Alcázar, Balarin, and Espinoza (2016)), and there is considerable evidence that women's empowerment is linked to a decline in fertility (Upadhyay et al. (2014)). Indeed, if CCTs can sustainably influence women's intrahousehold's bargaining power, they have the potential for transformative effects that could outlive the programs themselves. Meanwhile, existing evidence of the effects of cash transfers on beneficiary fertility provides little consensus, either because of differences in design and conditionalities or because the time horizon of analysis may be too short to detect these effects.

We study the causal effect of Peru's national CCT program Juntos on women's fertility outcomes and birth control use. Juntos was introduced in 2005 and provides a cash transfer to (i) mothers with children under the age of 14 and (ii) pregnant women, on the condition that children attend school, children aged 5 or less attend well-baby checks, and pregnant women attend pre-natal care. We start by looking at whether the cash transfer increased or decreased women's fertility outcomes at the extensive (any childbearing) and intensive (number of children) margins. We study the fertility effects among parents, and not among adolescent children in beneficiary households (as in, for example, Baird, McIntosh, and Özler (2011)). We then investigate the effect of Juntos on women's use of modern forms of birth control, as family planning may play an important role in fertility outcomes.[3] Finally, we look at potential mechanisms by exploring the impact of the program on preferences and intra-household bargaining or on facilitating access to reproductive health information or care.

We answer these questions by combining annual Demographic and Health Survey (DHS) data for Peru from 2004 to 2017 with administrative data on the Juntos rollout, and exploit the staggered implementation of the program in an event study approach. We use dynamic event study models, rather than canonical models, to study the dynamic and longer-term effects of the program. Given recent econometric advances in the differences-in-differences literature (Goodman-Bacon (2021); Chaisemartin and D'Haultfœuille (2020),

2. Cash incentives to increase fertility have indeed been used by pronatalist governments, either implicitly or explicitly (e.g. Quebec's baby bonus (Milligan (2005)) and pro-family policies in Europe (Sobotka, Matysiak, and Brzozowska (2019)).

3. There is some debate between economists and demographers about the link between family planning and fertility. In particular, it is not clear whether recent declines in fertility seen globally is a result of economic growth or access to family planning. See Pritchett (1994), Freedman (1997), Bongaarts and Sinding (2009), Bongaarts (2020), for examples of this debate.

we consider both traditional two-way fixed effects models and more robust estimation techniques that are especially appropriate when we may anticipate heterogenous treatment effects across groups, such as the one suggested by Sun and Abraham (2021). We use district level exposure as treatment (intent-to-treat) because of potential incentive and spillover effects on the non-recipients (Angelucci and De Giorgi (2009); Bastagli et al. (2016)).

We find long-lasting effects of Juntos on women's fertility and reproductive outcomes that persist up to 6 years after the program is rolled out at the district level. We find robust evidence of a decline in fertility in rural and small town districts that receive the cash transfer program compared to similar districts that do not. At the same time, we find no extensive margin effect: women exposed to the program are not more likely to have a child or be pregnant at the time of the survey. These results dispel concerns that anti-poverty policies, especially those targeting families with children, create undesired incentives for families to have more children in the case of Peru.[4] We also find large and robust effects of Juntos on the probability that women in beneficiary districts use birth control, with a disproportionately large share who opt for modern forms. This result is especially noteworthy given the relative importance of traditional forms of birth control in Peru compared to other countries in the region (Ponce de Leon et al. (2019)).

We explore potential mechanisms by investigating whether the effect of Juntos on fertility outcomes and birth control choices is driven by changes to preferences and household bargaining. We find no causal relationship of the Juntos rollout on fertility preferences measured by reported ideal number of children by the respondent, nor do we find any change in discordance between respondents and their spouses in this dimension. We also find little effect of the rollout on responses to measures of women's autonomy in the fertility domain, though we find a small but significant positive effect on the probability that women conceal the use of birth control from their spouse. We take this as evidence of binding barriers to women's empowerment in this domain, but that the program allows them to nevertheless gain some control, albeit covertly, over their reproductive choices. Alternatively, women have better access to reproductive health services once Juntos is rolled in to their district. While few women report cost and access as the reason for which they are not using birth control, the program may expose them to reproductive health information and care through the health conditionalities. Indeed, we find that our results are mostly driven by the sample women with children aged 5 or less, who must regularly attend well-baby checks.

4. Even if it is independent of the number of children, the transfer could lead to increased fertility if children are normal goods or if parents wish to prolong the period in which they might receive the benefit.

We conduct several robustness checks of our identification strategy. First, we check and confirm absence of significant pre-trends, both graphically by inspecting fully dynamic event study specification and with parametric tests on pre-rollout effects. Second, we test our event study identification strategy against newer methods which are more robust to treatment effect heterogeneity (e.g. Sun and Abraham, 2021). Third, we consider treatment effects using alternative sample selection criteria, using only ever treated districts. Our results, by and large, remain robust. To test our strategy further, we conduct two falsification tests by restricting the sample to women who should *a priori* not be affected by the policy: non-poor women and single childless women. Indeed, we find no effect of Juntos on fertility outcomes and reproductive choices for these two groups.

Our study adds to two separate strands of the social protection literature. First, it contributes to our understanding of the cumulative effects of cash transfers by exploiting recent advances in staggered treatment effect models. As Cahyadi et al. (2020) discuss, achieving intergenerational poverty reduction is a cumulative process and temporary investments may be of little benefit. Measuring the cumulative effects of cash transfers is challenging. It requires not only that we study longer term effects but do so in a setting where cash transfers have been offered regularly over time and where a pure control group exists for long enough to identify the cumulative effects. Like Cahyadi et al. (2020), we are able to investigate cumulative effects of a national, government-run, CCT program. While their study of Indonesia's CCT program finds strong cumulative effects on child health and education outcomes but limited long-term economic effects for households, we find cumulative effects on fertility. Given the role that lower fertility is believed to have in reducing poverty (Birdsall and Griffin (1988); Sinding (2009)), this result provides new evidence of potentially transformative and long-term effects of anti-poverty programs.

Second, our study adds evidence of the effect of CCTs on secondary outcomes – that is, outcomes that are not directly incentivized by design: since the transfer is conditional on child schooling and pre-natal and infant health check, any effect on fertility and birth control would be indirect. Indeed, recent literature has documented different dimensions in which cash transfers may have unintended effects (Bastagli et al. (2016)), including on adult fertility (Perova and Vakis (2012); Alencastre Medrano and Del Pozo Loayza (2017); Stecklov et al. (2007); Garganta et al. (2017); Nandi and Laxminarayan (2016); Carneiro et al. (2021)). No clear consensus emerges from this literature. For instance, while Stecklov et al. (2007) find positive or nil effects on fertility outcomes from CCTs in Honduras, Mexico and Nicaragua, Todd, Winters, and Stecklov (2012) find an increase in (short-term) birth spacing in Nicaragua between 2000 and 2004. In the Peruvian context, two studies show positive effects of Juntos on contraceptive use, but either do not investigate the effects on fertility outcomes or provide possible explanation for the channels through which birth control use is affected (Perova and Vakis (2012)) or consider only static effects in shorter reference period (Alencastre Medrano and Del Pozo Loayza (2017)). Our study contributes to this literature by looking at a more comprehensive set of fertility outcomes, cumulative effects, and by delving into a broader set of mechanisms including those related to intrahousehold bargaining issues (e.g. Ashraf, Field, and Lee (2014)). Our results suggest that cash transfers can impact welfare in unintended ways by empowering women in taking control over their fertility.

3.2 Juntos

Juntos (*el Programa Nacional de Apoyo Directo a los más Pobres*) began in 2005 and is currently operated by Peru's Ministry of Development and Social Inclusion (MIDIS) in a bid to reduce rural poverty. Using a two-stage targeting system, first targeting districts and second targeting households within eligible districts, the program offers households who meet the conditionalities 100 soles every month (eventually 200 soles every 2 months).[5] Distributed to households with children aged 14 or less or with a pregnant woman, this transfer is given to the mother/female household head if she is present in the household.[6] The amount received is independent of the number of children eliminating a direct incentive to affect fertility for families who already have at least one child. Figure 3.1 shows the evolution of the program rollout over time in terms of the number of districts. By 2017, the program had been rolled out to 1,305 out of a total of 1,896 districts in Peru. Considering that only rural districts (countryside and small towns) are eligible under the targeting rules, this shows the scope of the regional coverage.[7]

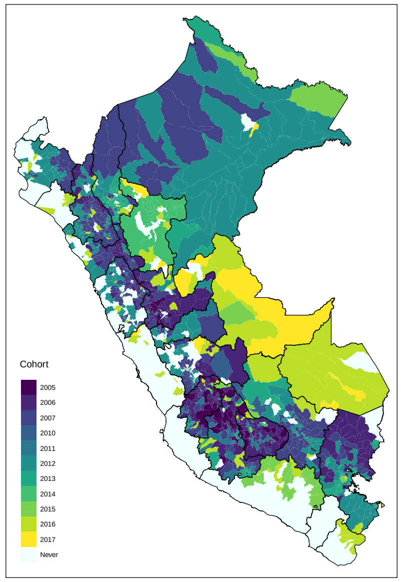
Alcázar (2009), Linares Garcia (2009), and World Bank (2019) spell out the conditionalities of the cash transfer. While the precise details and thresholds for the conditionalities have changed over the course of the program, the key conditions can be summarized as follows. First, children aged 6 and above must register and regularly attend school. Schooling is compulsory in Peru until age 16. According to the national statistical agency,

5. This is approximately equivalent to US$30 per month. In comparison, the 2017 National Rural Extreme Poverty Line is 150 soles per month, roughly 50% above the Cash Transfer amount (INEI (2017)).

6. For more formal details on the Juntos program, see Linares Garcia (2009), Molyneux and Thomson (2011), Escobal and Benites (2012), Díaz and Saldarriaga (2019), MIDIS (2016), Silva Huerta and Stampini (2018), World Bank (2019) and Carpio et al. (2019).

7. According to Robles et al. (2019), using data from 2013, Juntos covered 30% of all poor (48% in rural areas).

Figure 3.1: District Level Rollout by Year 2005-2017



Source: authors' calculations from data on Juntos portal
http://www.Juntos.gob.pe/infoJuntos/indexe.html, last accessed October 22 2018.

primary enrolment rates were already quite high prior to the introduction of the program (over 90%, even in rural areas) (INEI (2019)).[8] Second, most of the health conditionalities related to infant and child health and nutrtion: children under the age of 5 must attend routine well-baby checks and must be up-to-date with child vaccination, and pregnant women must receive monthly pre-natal health checks (Silva Huerta and Stampini (2018); Díaz and Saldarriaga (2019)). Nevertheless, according to the World Bank (2019), pregnant women and mothers must attend nutritional and reproductive health discussions. While we could not find much information about the frequency or content of these discussions, Perova and Vakis (2012) do note in their 5 year evaluation of Juntos that beneficiaries were more likely to attend family planning activities, though participation remains low and the effects dissipate after two years.[9]

8. Angelucci and De Giorgi (2009) make the argument that the similarly high enrolment rates in Mexico made the PROGRESA cash transfers "unconditional for most of the recipients". Secondary school enrolments in rural areas are somewhat lower in Peru than in Mexico at the onset of the introduction of the CCT program at 57% in 2005 for Peru (INEI (2019)) and 60% in 1997 for Mexico (Angelucci and De Giorgi (2009)).

9. Juntos has set up a monitoring unit to verify compliance. According to an early evaluation, only 5% of beneficiaries were rejected for failure to meet the conditions (Presidencia del Consejo de Ministros (2010)), despite low take-up of some of the secondary health conditions such as full immunization, attendance at health discussions.

3.3 Data

3.3.1 Peru's Demographic and Health Surveys

Because of its emphasis on women's health and especially women's reproductive health, Peru's DHS (or ENDES, *Encuesta Demográfica y de Salud Familiar*) is uniquely placed to allow us to investigate the impact that Juntos has had on matters around contraception (use and type) and fertility outcomes (number of children, currently pregnant). In addition, the DHS allows us investigate some dimensions of fertility preferences (ideal family size, spousal discordance in fertility) and intra-household bargaining in this domain (who in the household is the main decision-maker in the use of contraception), which may shed light on mechanisms, as well as a large number of socio-economic variables we use as controls.[10] Contrary to other countries where the DHS was run every four years, the Statistical Agency for Peru (INEI) began running the DHS as a continuous annual survey in 2004. This yields a relatively rare high frequency of nationally representative repeated cross-sections for a single country, allowing us to exploit annual variation in the program rollout and to estimate the dynamic effects of a CCT. For our analysis, we utilize the yearly DHS waves from 2004, the year prior to the introduction of Juntos, to the 2017 wave.

3.3.2 Administrative Data on District-level Rollout of Juntos

Juntos was rolled out in several phases beginning in 2005. We use administrative data on the geographic, district-level, rollout (Figure 3.1). The program originally aimed to target rural districts that disproportionately suffered from the civil conflict. Carpio et al. (2019) document the different phases of the rollout of the program and provide a reconciliation of the administrative data (used here) and the eligibility criteria. While the precise criteria for geographic targeting and data sets employed by the program implementation changed across the expansion periods, they generally all include the following components with some minor variation: the district poverty or extreme poverty rate (total poverty gap, proportion of households with unmet basic needs, percentage of households with chronic malnutrition) and the proportion of population centres ('centros poblados') in the district who were severely affected by violence. We utilize these administrative data to identify districts that are targeted for receipt of Juntos at any given point in time. We merge the DHS with the administrative data on the Juntos rollout at the district-year level. The main

10. The continuous DHS for Peru did not collect data from the spouses/partners.

variable of interest, exposure to Juntos, is constructed as a binary variable equal to 1 if the respondent is in a targeted district at the time of the DHS interview and 0 otherwise. Treatment here refers to district groupings: during each phase of the rollout, numerous districts were rolled in simultaneously, so the identification is made off groups of districts, rather than individual districts.

3.3.3 Sample and Variables of Interest

Pooled, the 2004 to 2017 waves of the DHS yield a sample of 280,248 women respondents aged 15 to 49, living in 1,427 of Peru's 1,874 districts.[11] By 2017, Juntos is rolled out to 1,325 districts, 992 of which are represented in the DHS data for 2004-2017. We exclude women who are menopausal, infertile/sterilized, or in a relationship with an infertile spouse, yielding 233,704 observations. We further restrict our analysis to married or co-habitating women, which produces a sample of 135,807 observations. Of these observations, roughly 50% (68,882 women) live in districts targeted by Juntos in the 2005-2017 period. Our main analytic sample considers only rural districts and small towns, whether they were (952 districts) or were not (275 districts) included in the Juntos rollout (total sample size 64,409). Appendix Tables 3.4 to 3.6 present the descriptive statistics for the outcome variables and the main control variables.

For fertility outcomes we consider whether the respondent has any children at the time of the DHS interview to capture childbearing at the extensive margin, the respondent's number of children to capture overall fertility at the intensive margin, and whether she is currently pregnant. Over 95% of respondents have at least one child. The average respondent has 3 children, while just over 7% are currently pregnant. For family planning outcomes, we generate two outcome variables from survey responses on whether the respondent is using birth control at the time of the interview and if so, we use their response on the type used. Approximately 75% of women in our main sample use some form of birth control, with 43 to 44% using modern forms. We follow the DHS and WHO classification for 'modern method'.[12] Socio-economically, the average woman in the analytic sample is just over 31 years old (with a partner 4 years older), has 7.5 years

11. We exclude observations from 4 surveyed districts because their borders changed in our reference period.

12. This includes female sterilization, male sterilization, pill, DIU, injection, implants or Norplant, condom, foams and jellies, and amenorrhea. (http://www.who.int/news-room/fact-sheets/detail/family-planning-contraception and https://dhsprogram.com/data/Guide-to-DHS-Statistics/Current_Use_of_Contraceptive_Methods.htm). Meanwhile, traditional methods include periodic abstinence and withdrawal.

of schooling (just one year less than her partner). Seventy five percent of respondents are employed, with agricultural self-employment being the most frequently reported occupation (41%) followed by sales (15.5%). About 77% of respondents have wealth positions placing them in the poorest two quintiles, 69.4% live in the countryside, and less than 1/3 report formalizing their relationships into legal unions.

Finally, we also investigate potential mechanisms. First, to capture fertility intentions as main drivers of decision making in this domain (Pritchett (1994)), we consider ideal number of children. The average respondent has an ideal family size of approximately 2.5 children, pointing to an average excess fertility of approximately 0.5 children. Second, and inspired by the work of Ashraf, Field, and Lee (2014) who explore the role of moral hazard effects in intrahousehold decision-making in this domain, we consider spousal discordance in fertility preferences. We define spousal discordance as the case when the respondent reports a different ideal family size than her partner. We categorize households into three types: both the respondent and her spouse have the same preferences over family size (no discordance, accounting for 64% of the sample), respondent wanting more children than her spouse (11%), and respondent wanting fewer children than her spouse (20%). Third, we consider the respondents' answer to a question about who makes the decision about contraceptive use and create the following three variables: whether the respondent makes the decision (herself or jointly with her partner), someone else (her partner or someone else), and whether she conceals the use of contraceptives from her partner, accounting for 93%, 5% and 2% of the sample, respectively. Finally, we also consider women with children under the age of 5 who are required to regularly attend health centers as a condition of receiving Juntos (65% of the sample).

We do not utilize the self-reported Juntos receipt variable in the DHS as it is only collected between 2009 and 2012 and only for women with a child aged 5 or less, which would lead to a considerable reduction in sample size. For example, out of 5747 women in DHS waves for 2010-2012 in our sample in districts targeted before interview (or year of interview) who answered this question (child under the age of five), 44% say they received Juntos. Of those more than half of those who claimed to have received the program were unable to produce their Juntos beneficiary card.

3.4 Identification Strategy

We estimate the treatment effects of Juntos in following a difference-in-difference strategy and the rollout of the program over time (staggered design). In our main specification we estimate dynamic treatment effects using a semi-dynamic model. We also use a fully dynamic specification to establish the validity of the common trends assumption. We describe each in turn after we discuss the sample selection implications of the identification strategy.

Sample Selection Since Juntos was not rolled out experimentally, there is always a question of constructing the appropriate treatment and control groups. Our analytic sample excludes observations in cities ($> 50,000$ inhabitants) as we do not believe these to be appropriate counterfactuals for targeted districts: cities are typically wealthier, with better access to family planning services and less likely to be included in a Juntos rollout.[13] Comparing relatively poor women in relatively poor treated districts to relatively wealthy in relatively wealthy districts leads to a rejection of the common trends hypothesis (results available upon request).

Accordingly, we consider rural districts and small towns as we believe these to be most comparable between treatment and control: these are both urban/rural categories with high rates of wealth poverty (90% and 68% in the bottom two wealth quintiles, and Juntos was rolled out to 88% and 70% respectively). We do this to ensure that counterfactual observations are as comparable to treatment observations. Districts are included regardless of whether they ever received Juntos. This means that the sample includes a group of districts that was never treated—a key element of the methodology in Sun and Abraham (2021).[14]

Econometric Model To estimate the effect of the Juntos rollout on women's reproductive health choices and outcomes, we conduct an event-study analysis that allows for time varying treatment effects (e.g. Hoynes, Schanzenbach, and Almond (2016); Chetty, Friedman, and Saez (2013); Greenstone and Hanna (2014); and Christian, Hensel, and Roth

13. Less than 30% of the DHS sample of married/cohabitating and fecund women in capitals and large cities are in the lower two wealth quintiles (compared to 78% in rural districts), and only 26% the observations in the DHS under this category were included in the rollout for Juntos between 2005 and 2017 (compared to 78% in rural districts).

14. We also conduct a robustness check only using targeted districts, including observations in small cities. In that case, the counterfactuals are the observations in targeted districts prior to their inclusion in the Juntos rollout.

(2019)). We first estimate the following semi-dynamic model:

$$y_{idt} = \lambda_t + \delta_d + \sum_{\tau=0}^{5} \mu_{d\tau}\mathbf{1}\{\tau = t - E_d\} + \mu_{6+}\mathbf{1}\{t - E_d \geq 6\} + \beta X_{idt} + \epsilon_{idt}, \tag{3.1}$$

where y_{idt} is the outcome of interest for respondent i in district d at time t, E_d is equal to the first year Juntos was rolled out in the district where woman i resides, λ_t is a set of dummies to control for year specific effects, δ_d are dummies for district fixed effects, and X_{idt} are respondent i's socio-economic characteristics in district d at time t (age, age squared, years of schooling, occupation, marital status, husband/partner years of schooling and education, wealth index). μ_τ will capture dynamic treatment effects. We bin distant lags from 6 to 12 to increase the sample size.

The model estimates unbiased dynamic treatment effects under three assumptions (Sun and Abraham, 2021): (1) parallel trends in baseline outcomes, (2) no anticipatory behaviour prior to treatment, and (3) treatment effect homogeneity across cohorts. With regards to Assumption (3), we have no a priori reason to believe that women treated in later years would respond differently to women treated in previous years. Nonetheless, we conduct a robustness check by estimating the event study model using the methodology developed by Sun and Abraham (2021) that is robust to treatment effect heterogeneity.[15] Our results are qualitatively and quantitatively similar, suggesting that heterogeneity in treatment effects across cohorts is likely to be minimal. We investigate assumptions (2) and (3) using the following fully dynamic specification with leads and lags:

$$\begin{aligned} y_{idt} = \lambda_t + \delta_d + \sum_{\substack{\tau \geq -5 \\ \tau \neq -1}}^{5} + \mu_{d\tau}\mathbf{1}\{t - E_d = \tau\} + \mu_{6-}\mathbf{1}\{t - E_d \leq -6\}\mu_{6+}\mathbf{1}\{t - E_d \geq 6\} \\ + \beta X_{idt} + \epsilon_{idt}, \end{aligned} \tag{3.2}$$

where μ_τ with $\tau < 0$ are pre-trend coefficients and μ_τ with $\tau \geq 0$ capture the dynamic treatment effects estimated above. We normalize $\tau = -1$ to follow the common practice in the literature. We bin distant leads from -13 to -6 to increase the sample size. Using the fully-dynamic specification, we test the parallel trends and no anticipatory behaviour assumptions in two ways.[16] First, we visually inspect the fully dynamic event studies.

15. We use the Stata package *eventstudyinteract* by Sun and Abraham (2021).

16. We do not believe that individuals were able to anticipate the timing of when Juntos was rolled out into their district. In fact, though Juntos was not explicitly rolled out using a random design, we argue that it would be difficult for individuals to predict the timing of their district's enrolment into the program. We argue that the frequent changes in both the district targeting formulae and the government entity responsible

Second, we conduct two formal tests on the leads. Test 1 reports the p-value associated with the test of significance of the average effect pre-intervention lead coefficients. Test 2 tests the joint significance of all lead coefficients (F-test).

3.5 Results

3.5.1 Main Results

Table 3.1 provides the semi-dynamic results for fertility outcomes (at least one child, number of children and currently pregnant) and for birth control use (modern method, any birth control use). The corresponding fully dynamic estimates are shown graphically in Figure 3.2, and the p-values of two different common trends tests are presented at bottom of Table 3.1. These tests, as well as a visual inspection of the fully dynamic event studies in Figure 3.2 support the parallel trends assumption.

In addition to the estimated effects from the semi-dynamic model, Table 3.1 also reports the average effect computed from the estimated coefficients from lags t1-t6.[17] There are two main results emerging from Table 3.1. First, we find no evidence that Juntos is associated with increases in fertility. If Juntos encouraged some families with no children to have children to take advantage of eligibility criterion, we should expect to see increases in pregnancies or increases in fertility, specially among families who are having their first child. The estimated effects (dynamic and average) of Juntos on fertility along the extensive margin—currently pregnant or having at least one child—are both close to and statistically indistinguishable from zero. Along the intensive margin (number of children) we find evidence of a *decline* in fertility among women living in districts where Juntos has been rolled out. The effects are largest in the longer-term.[18] Second, we find that Juntos rollout is associated with an increase in birth control use, driven by a significant uptake in modern methods. Modern birth control use increased by an average of 5 percentage points in districts where Juntos was rolled out, which is about 11% of the mean of 44%. Furthermore,

for delivery of the program (Carpio et al. (2019)) produces plausible exogenous variation in the rollout and minimizes the chances of anticipatory behaviour on the part of potential beneficiaries.

17. An alternative specification is to run a canonical Difference-in-Difference model, which is now known to be biased when the timing of treatment varies over the analytic period (Goodman-Bacon, 2021). Taking the average over the linear effects estimated using the semi-dynamic model avoids these concerns.

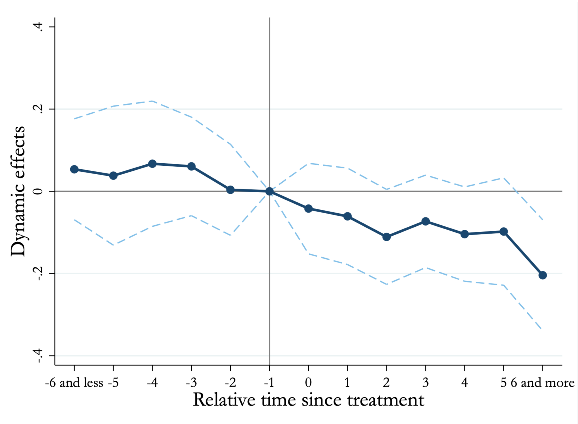
18. We also considered whether the respondent is currently pregnant with an intended pregnancy as an alternative outcome. The yearly effects of the Juntos rollout are also negative, and while some of the linear effects are statistically significantly so, the average imputed effect is statistically insignificant. Just over 60% of current pregnancies are intended. Results available upon request.

Table 3.1: Main Results: Effect of Juntos on Birth Control Use and Fertility Outcomes, Semi-Dynamic Model, Rural Districts and Small Towns (OLS)

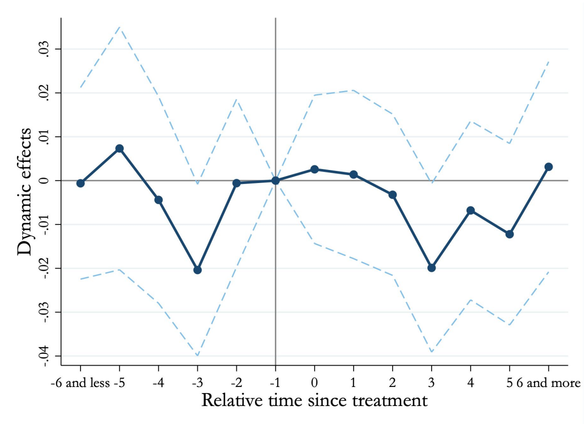
	(1)	(2)	(3)	(4)	(5)
	Fertility outcomes			Birth Control Use	
VARIABLES	At least one child	Number of children	Currently pregnant	Modern Method	Not Using
Year Juntos implemented	0.0034	-0.0649	0.0059	0.0209	-0.0010
	(0.007)	(0.048)	(0.008)	(0.017)	(0.014)
1 year later	-0.0025	-0.0827	0.0055	0.0109	-0.0015
	(0.007)	(0.051)	(0.008)	(0.017)	(0.014)
2 years later	0.0092	-0.1321**	0.0024	0.0430**	-0.0039
	(0.007)	(0.052)	(0.008)	(0.018)	(0.014)
3 years later	0.0072	-0.0889*	-0.0149*	0.0605***	-0.0443***
	(0.007)	(0.051)	(0.009)	(0.018)	(0.014)
4 years later	0.0000	-0.1156**	-0.0022	0.0709***	-0.0442***
	(0.008)	(0.053)	(0.010)	(0.019)	(0.015)
5 years later	0.0155*	-0.1060*	-0.0080	0.0650***	-0.0590***
	(0.008)	(0.062)	(0.010)	(0.020)	(0.015)
6 or more years later	0.0083	-0.2053***	0.0080	0.0491**	-0.0521***
	(0.009)	(0.065)	(0.011)	(0.022)	(0.018)
R-squared	0.173	0.579	0.066	0.115	0.064
Mean of the dep. var.	0.9552	3.0105	0.0766	0.4438	0.2408
Average effect t1-t6	0.0063	-0.1218***	-0.0015	0.0499***	-0.0342***
	(0.006)	(0.044)	(0.007)	(0.015)	(0.012)
			Common trends tests		
P-value test1	0.1631	0.3600	0.6581	0.3949	0.1235
P-value test2	0.6341	0.8699	0.1636	0.6229	0.2402
			Placebo tests (Average effects)		
Non-poor women	-0.0116	-0.0727	0.0097	0.0006	-0.0137
(N=14,279)	(0.017)	(0.063)	(0.015)	(0.032)	(0.026)
Single childless wom.				-0.0159	0.0175
(N=22,127)				(0.010)	(0.012)

Notes: N=64,409. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. Test 1 reports the p-value corresponding to the average effect pre-intervention estimated using the fully dynamic specification (equation (2)). Test 2 tests the joint significance of all lead coefficients estimated by the fully dynamic specification (equation (2)). ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

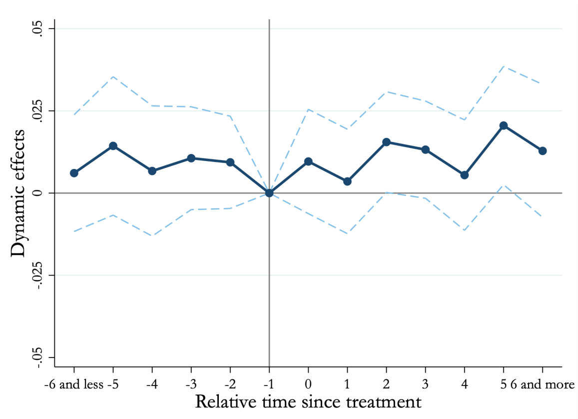
Figure 3.2: Event Study of Juntos' Effect on Fertility and Reproductive Outcomes



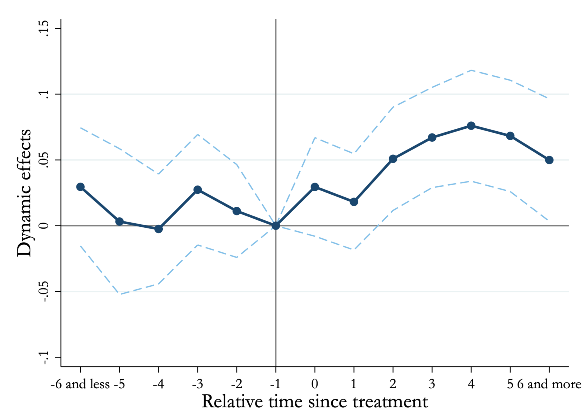
(a) Fertility: Number of children



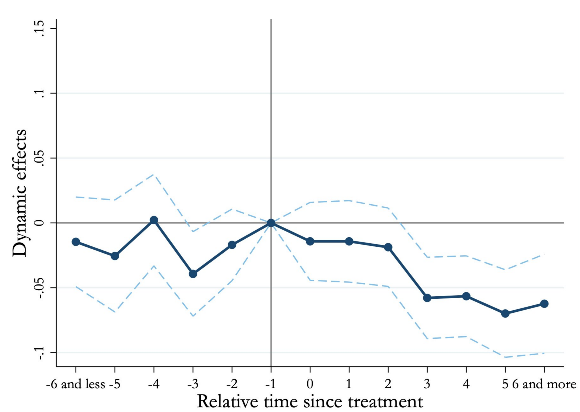
(b) Fertility: Currently pregnant



(c) Fertility: At least one child



(d) Birth control use: Modern methods



(e) Birth control use: Not using

Source: authors' calculations from DHS waves 2004-2017.

these effects are persistent 6 or more years after the first year Juntos is rolled out in a given district.

The persistent effects of Juntos on fertility and birth control use are noteworthy because they speak to the long term effects of conditional cash transfers. As Cahyadi et al. (2020) note, whether CCT programs continue to be effective beyond the 'static' effect of increasing compliance with incentivized behaviors on those entering the program is unclear. Our results adds useful evidence because we show long term dynamic effects on behaviours not explicitly incentivized by the CCT.[19] Given the long lasting effects of having a child, beneficiaries may need more time to change their intentions and behaviors regarding fertility and the related use of contraceptives to shape fertility. In this regard, evaluations of CCTs that only look at immediate effects may miss changes in fertility and contraceptive use that may be most visible over time. The fact that we find longer-term effects on unconditioned outcomes suggest that the program has had deeper, more structural, impacts than would be implied by intended effect on conditioned outcomes (schooling and child health).

We further test our identification strategy by running two falsification tests. First, we consider the sample of non-poor individuals—those in the top three wealth quintiles. Since non-poor women are unlikely to meet the eligibility criteria for Juntos, we should not find statistically significant results. We are not aware of any sharp cutoffs used for determining ineligibility to Juntos and DHS wealth index may not correspond one to one with wealth and income data used to determine eligibility (see Carpio et al. (2019) for more details about eligibility). Nonetheless, at the minimum we expect to see muted effects on the those who belong to richer households relative to those among the poorest households in our DHS sample.[20] In the bottom of Table 3.1, we show our mean estimates for the non-poor women. All the estimates among the non-poor sample are statistically insignificant at the 10% level and generally smaller in magnitude.[21]

19. There are a number of reasons to expect that the 'static' effects of the program may change over time. Interventions may become less effective when implemented by the government at scale than in a smaller pilot stage (e.g., Bold et al. (2018)). Treatment effects could weaken over time as people's initial excitement of being in the program fades, or once beneficiaries learn that the conditions of the CCT were not always perfectly enforced by the government. Furthermore, inflation and improvements in economic conditions could make nominal level of benefit payments less effective in changing household decision making.

20. We do not consider the top two quintiles only because the sample is too small to be meaningful. As a check, in the DHS waves where they asked about Juntos, less than 2% of the individuals in the 3rd wealth quintile said they received the transfer. Meanwhile, 35% of the lowest wealth quintile reported receiving it, and 12.4% for the 2nd poorest. However, only the 2005-2007 waves included self-reported Juntos receipt and this was only asked of women with children aged 5 or less.

21. We also explored a fully dynamic specification for our placebo samples with point estimates of each lag t1 to t6 reported along with tests of common trends. Results remain robust and are available upon request.

Second, we consider single women with no children as another placebo sample and investigate the effect of Juntos on birth control use. These women do not meet the eligibility criteria and are unlikely to have a child in order to meet them. If we find that single childless women in Juntos districts are more likely to use birth control than in non-Juntos districts, we might be concerned about the effects in Table 3.1 picking up increased availability in the community. And while very few women in this sample use any birth control at all (less than 10%), significantly lower than in the main sample (75%) we fail to find any effects on birth control use among sthis group.

We also check for the robustness of our results to different estimation methods to identify causal treatment effects in the presence of varying treatment times. As discussed in section 3.4, we estimated a version of model (1) using the Sun and Abraham (2021) estimator and our results are robust: we find no effect on the extensive margin (on any childbearing), strong and persistent negative effects on the number of children and large positive effects on the uptake in modern forms of contraception (see Appendix Table 3.7). Overall, the results are both quantitatively and qualitatively similar to those from Table 3.1. Similarly, we conducted a robustness check using the sample only of ever targeted (rural + urban) districts, and so does not include a pure control (respondents in similar districts that are not included in the rollout during the 2004-2017 period). The results (Appendix Table 3.8) are similar to those found in Table 3.1 in terms modern contraceptive use, but the results on fertility outcomes become largely statistically insignificant.

In summary, we find robust evidence of persistent effects of Juntos on modern forms of contraception and no evidence of increase in fertility even 6 years after initial rollout. In fact, there is evidence that fertility may have even declined over time in these districts, perhaps mediated through use of modern contraceptives.

3.6 Mechanisms

Since Juntos incentivized school attendance and health check for expecting mothers and children under age 5, but not fertility or reproductive health, the question becomes why we might find such effects. One possibility is that the CCT program has affected fertility preferences or influenced intrahousehold dynamics in response to the increased financial resources accruing to the women. We consider these in turn in Table 3.2. For all these outcomes, we fail to reject the parallel trends assumption. We first investigate (column 1) if Juntos shaped women's fertility decisions through changes in fertility preferences towards fewer children, as measured by ideal number of children, and find no such effect, neither for the main sample nor for the two placebo samples. Next we considered any effect that Juntos has on shifting intra-household bargaining in the fertility domain, as measured by respondent answers to whether the partner would like to have more, fewer, or the same number of children as she does. If a woman's increased share of the household budget affords her more bargaining power in household decisions, we would expect a decrease in spousal disagreement over matters that she values highly. Building on Rasul (2008), Ashraf, Field, and Lee (2014) and Doepke and Kindermann (2019), we would anticipate a reduction in discordance with her partner over fertility related decisions. Again, we find no effect of Juntos on spousal concordance over fertility intentions (columns 2, 3 and 4), in either the main analytic sample nor on the two placebo samples. Finally, we investigate whether Juntos has any effect on women's autonomy in decision-making over fertility, a common measure of women's intra-household bargaining or empowerment (columns 5, 6 and 7). We find little effect of Juntos on whether the woman is the main decision maker (herself or with her partner) or whether someone else makes the decision for her, and the average effects are statistically insignificant. However, we do find a 0.68 percentage point increase in the probability that the respondent conceals her use of birth control. Though small, it is a large effect relative to the mean of this variable, corresponding to a 44.7% increase. Note that this result is not present in the placebo group (non-poor women).

Concealed use of birth control suggests prevalence of moral hazard related concerns in intra-household decision making. In Ashraf, Field, and Lee (2014), moral hazard arises through a psychological cost incurred by a woman who wishes to hide her attempts to control fertility from her spouse. They point out that when there is freedom to hide, women who use contraceptives hide their use through concealable alternatives. Given that Juntos did not lead to greater concordance between spouses, an increase in concealable forms suggest either increased access through conditioned access to health facilities visits and/or greater attempt to assert womens' fertility preferences. Indeed, concealment may be a way to take control in situations where women do not feel they can voice and act on their preferences.[22]

A remaining possibility is that Juntos provides beneficiaries with greater access to re-

22. There exists evidence of women concealing their actions from their spouse in the context of intra-household bargaining (e.g. Fiala and He (2017)). Chang et al. (2020) also discusses a literature on women's agency in which hiding decisions from spouses forms a second best empowerment outcome when household bargaining dynamics are slow to change. Furthermore, cash transfers increase a woman's outside options, this may change her calculus as to whether or not concealing use is optimal even in the presence of psychological costs (Lundberg and Pollak (1996); Eswaran (2002); Rasul (2008); Anderson and Eswaran (2009) and Anderson and Eswaran (2009); and Doepke and Kindermann (2019)). The cash transfer changes the threat point over which women are able to exit a marriage.

Table 3.2: Mechanisms: Semi-dynamic model, Effect on fertility preferences, spousal discordance, and autonomy (OLS) – Rural Districts and Small Towns

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Husbands want			Main decision-maker		
VARIABLES	Ideal nbr. of child.	Fewer	Same	More	Resp.	Someone else	Conceals use
Year Juntos was implemented	-0.0236	-0.0208**	0.0234	-0.0144	-0.0009	-0.0039	0.0039
	(0.049)	(0.010)	(0.016)	(0.012)	(0.015)	(0.006)	(0.003)
1 year later	-0.0047	-0.0101	-0.0025	0.0126	-0.0048	-0.0033	0.0068
	(0.052)	(0.011)	(0.017)	(0.014)	(0.015)	(0.006)	(0.004)
2 years later	0.0105	-0.0184	0.0134	0.0052	-0.0091	0.0040	0.0045
	(0.049)	(0.011)	(0.016)	(0.013)	(0.015)	(0.007)	(0.004)
3 years later	0.0219	-0.0018	-0.0015	-0.0047	0.0292*	0.0042	0.0060
	(0.049)	(0.010)	(0.016)	(0.013)	(0.015)	(0.007)	(0.004)
4 years later	-0.0925*	-0.0206*	0.0198	-0.0006	0.0289*	0.0035	0.0049
	(0.051)	(0.012)	(0.019)	(0.014)	(0.015)	(0.007)	(0.004)
5 years later	-0.0365	-0.0004	0.0076	0.0009	0.0271	0.0185**	0.0081*
	(0.051)	(0.012)	(0.018)	(0.015)	(0.017)	(0.009)	(0.004)
6 or more years later	-0.0322	-0.0031	0.0140	0.0026	0.0150	0.0190*	0.0104**
	(0.060)	(0.013)	(0.022)	(0.017)	(0.019)	(0.010)	(0.005)
R-squared	0.145	0.045	0.042	0.041	0.102	0.049	0.034
Mean of the dep. var.	2.5702	0.1159	0.6378	0.1971	0.6903	0.0395	0.0152
Average effect t1-t6	-0.0223	-0.0091	0.0085	0.0027	0.0144	0.0077	0.0068**
	(0.041)	(0.009)	(0.014)	(0.011)	(0.012)	(0.006)	(0.003)
				Common trends tests			
P-value test1	0.1601	0.3358	0.1267	0.1987	0.2845	0.9683	0.5809
P-value test2	0.6949	0.3857	0.1801	0.3800	0.7278	0.5390	0.5932
				Placebo tests (Average effects)			
Non-poor women	0.0794	-0.0255	0.0144	0.0183	-0.0114	0.0183	0.0058
(N=14,279)	(0.076)	(0.019)	(0.035)	(0.028)	(0.027)	(0.014)	(0.008)
Single childless wom.	-0.0152						
(N=22,127)	(0.041)						

Notes: N=64,409 (N=64,187 for Ideal # children). All regressions include district and year fixed effects, individual characteristics,and DHS weights. Standard errors are clustered at the district. Test 1 reports the p-value corresponding to the average effect pre-intervention estimated using the fully dynamic specification (equation (2)). Test 2 tests the joint significance of all lead coefficients estimated by the fully dynamic specification (equation (2)). ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

productive health information and counselling for pregnant women or those with children under 5 years who must attend health centres. We test this conjecture by re-estimating our main model presented in Table 3.1 on a sample of women who had children under 5. We are aware that fertility itself is affected by Juntos such that restricting the sample to women with children under 5 might lead a selection bias. However, we believe that the bias is minimal as the probability that a woman has a child under 5 only decreased by 2 percentage points following the rollout of Juntos, which is not significant at the 10% level. Moreover, the selection bias is expected to attenuate the effects we find on the sample of women with a child under 5 such as our results can be interpreted as lower-bounds of the true effects. Results are comparably similar to those we found in Table 3.1, if not slightly stronger (Appendix Table 3.9): women in targeted districts have fewer children (on the intensive margin), more likely to be using any form of birth control, and especially modern forms. In contrast, we find null effects for families with no children under the age of 5 (Appendix Table 3.10). This suggests that Juntos may have shaped fertility outcomes through improving resources and the increased frequency of visits to health facilities imposed by the conditionality for mothers of children under the age of 5.[23]

In sum, Juntos is associated with greater fertility control for women in beneficiary districts, facilitated by increased take-up of modern forms of birth control. Since the effect is even more pronounced among women with children under the age of 5—in which case the conditionality of taking children to health checks is noteworthy—we cannot rule our that this is driven by improved access to reproductive health information and care. If so, we view this as empowering women to use modern contraceptives, even if they have to hide the use of birth control from their partners, despite not being able to change fertility preferences or changing spousal attitudes towards fertility

3.7 Conclusion

Using Peru's Juntos program, we investigate the fertility and reproductive health outcomes effects of cash transfers that are conditional on child school attendance and prenatal and infant health checks. We find that women in targeted districts tend to have fewer children for as much as up to 6 years after the program was rolled in, suggesting strong long-term effects at the intensive margin. We do not find any evidence of an extensive margin

23. We do not believe that Juntos would increase availability or reduce the price of birth control in the area, but if it did, we would expect the results to be stronger among this group than among the main results (with the full sample). However, the results on modern use are essentially the same. Regardless, cost or availability are not cited by DHS respondents to be the leading reasons for not using birth control.

effect, contrary to fears among policy-making circles that cash transfers will lead to greater fertility (e.g. Peterman (2021)). The decrease in fertility seems to be driven by an increase in the take-up of modern forms of birth control. This last result is of particular importance for sexual and reproductive health and rights given a disproportionate reliance on traditional methods in Peru (Ponce de Leon et al. (2019)), methods that are neither effective nor necessarily safe. Investigating potential mechanisms, we find that fertility preferences remain stable and that while women in targeted districts do not seem to benefit from increased explicit autonomy in intra-household decision making over fertility, those that are more likely to use contraception may be doing so with concealable forms. We interpret these results as evidence of binding barriers to women's empowerment, and that the cash transfer program may help some women covertly assert their preferences and take control over their fertility.

We also investigate whether the program indirectly exposes women to reproductive health information and care by considering the sub-sample of women with children under the age of 5: to comply with the conditionalities of the program, these children are required to undergo regular health checks. We find the same results as for the larger sample and so we cannot exclude the possibility that mothers are also receiving greater access to reproductive care and technology while they are there with their children. We test our identification strategy by running placebo tests on samples of non-eligible demographic groups: indeed we find no effect of Juntos among women who are non-poor or childless. Finally, our results are robust to new techniques for estimating event studies in the presence of varying treatment times.

The persistence of the effects on fertility and reproductive health outcomes has policy relevant importance for two reasons. First, since these are outcomes that are not explicitly targeted by the program conditionalities, our findings add to the case that evaluating anti-poverty programs ought to consider broader sets of outcomes than those directly targeted by program designers. Second, our results provide encouraging insights against the concern that dynamic effects of social protection programs may wane as beneficiaries' excitement about the program fades, compliance with conditionalities become less strictly enforced, and interventions become less effective when implemented by governments at scale compared to smaller pilot programs run by NGOs (Bold et al. (2018); Cahyadi et al. (2020)). Indeed, the long term, dynamic, effects of a large scale cash transfer program on women's reproductive outcomes suggests potentially transformative effects on the lives of beneficiary families, given the long term expected benefits of reduced fertility (Birdsall and Griffin (1988); Sinding (2009)).

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3.9 Appendix

3.9.1 Additional Tables

Table 3.3: Sample Description

Panel A: Sample restrictions on women's characteristics		
	# of women	
Initial sample size[1]	280,248	
After removing unfecund/menopausal/sterilized women	233,704	
After restricting to married women	135,807	
Panel B: Sample restrictions on type of districts		
	# of women	# of districts[2]
(1) All districts	135,807	1,427
(2) Targeted rural districts and small towns	50,457	952
(3) Never-targeted rural districts and small towns	13,952	275
(4) Targeted districts in small cities[3]	18,425	40
Main sample:		
All rural districts and small towns (2)+(3)	64,409	
Robustness:		
Ever targeted districts (2)+(4)	68,882	

1. 2004-2017 DHS waves, excluding 4 districts whose borders have changed over the period.
2. There are 1,838 districts in Peru. Districts that are not in our sample were either not sampled at all or were affected by a border change during the 2004-2017 period.
3. Urban areas of Huánuco, Moquegua, Piura, Pucallpa.

Table 3.4: Descriptive Statistics (Outcome Variables)

	Main Sample Rural and Towns	Robustness Targeted Only
Fertility Outcomes		
At least one child	0.955	0.952
	(0.207)	(0.212)
Number of children	3.010	3.013
	(2.138)	(2.151)
Currently pregnant	0.077	0.078
	(0.266)	(0.268)
Birth Control Use		
Modern	0.444	0.431
	(0.497)	(0.495)
None	0.241	0.248
	(0.428)	(0.432)
Number of observations	64,409	68,882

Standard deviations in parentheses.

Table 3.5: Descriptive Statistics (Individual Characteristics)

	Main Sample Rural and Towns	Robustness Targeted Only
Age	31.564	31.465
	(8.125)	(8.089)
Partner's age	35.674	35.614
	(9.209)	(9.208)
Education in years	7.436	7.563
	(4.119)	(4.185)
Partner's education in years	8.336	8.482
	(3.510)	(3.598)
Respondent's occupation = not working	0.257	0.252
	(0.437)	(0.434)
Respondent's occupation = Prof., Tech., Manag.	0.045	0.055
	(0.206)	(0.229)
Respondent's occupation = Clerical	0.014	0.018
	(0.116)	(0.131)
Respondent's occupation = Sales	0.152	0.173
	(0.359)	(0.379)
Respondent's occupation = Agric-self employed	0.410	0.373
	(0.492)	(0.484)
Respondent's occupation = Household & domestic	0.035	0.037
	(0.184)	(0.189)
Respondent's occupation = Services	0.016	0.018
	(0.126)	(0.134)
Respondent's occupation = Skilled manual	0.028	0.031
	(0.165)	(0.174)
Respondent's occupation = Unskilled manual	0.014	0.013
	(0.116)	(0.113)
Wealth index = Poorest	0.448	0.437
	(0.497)	(0.496)
Wealth index = Poorer	0.330	0.332
	(0.470)	(0.471)
Wealth index = Middle	0.147	0.150
	(0.354)	(0.357)
Wealth index = Richer	0.057	0.061
	(0.232)	(0.240)
Wealth index = Richest	0.018	0.019
	(0.131)	(0.137)
Lives in a rural area	0.694	0.610
	(0.461)	(0.488)
Cohabiting (vs. married)	0.658	0.678
	(0.474)	(0.467)
Number of observations	64,409	68,882

Notes: Standard deviations in parentheses. Missing individual characteristics were imputed with the outcome mean. Indicators for missing values are added to the regressions.Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

Table 3.6: Descriptive Statistics (Mediating Variables)

	Main Sample Rural and Towns	Robustness Targeted Only
With children under 5	0.652	0.655
	(0.476)	(0.475)
Ideal number of children	2.570	2.558
	(1.294)	(1.284)
Partner wants fewer children than respondent	0.116	0.116
	(0.320)	(0.321)
Partner wants the same number of children than respondent	0.638	0.643
	(0.481)	(0.479)
Partner wants more children than respondent	0.197	0.194
	(0.398)	(0.395)
Respondent makes decision about contraceptives (self & jointly)	0.690	0.686
	(0.462)	(0.464)
Someone else makes decision about contraceptives	0.040	0.037
	(0.195)	(0.190)
Conceals use of contraceptives	0.015	0.016
	(0.122)	(0.124)
Number of observations*	64,409	68,882

Notes: Standard deviations in parentheses. *N for Ideal Number of children: 64,187 and 68,650.Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

Table 3.7: Main Results using Sun and Abraham Estimator, Rural Districts and Small Towns: Effect of Juntos on Birth Control Use and Fertility Outcomes

	(1)	(2)	(3)	(4)	(5)
	Fertility outcomes			Birth Control Use	
VARIABLES	At least one child	Number of children	Currently pregnant	Modern Method	Not Using
Year Juntos implemented	0.0003	-0.0825**	0.0080	0.0193	-0.0089
	(0.006)	(0.039)	(0.007)	(0.013)	(0.011)
1 year later	-0.0029	-0.1022***	0.0075	0.0073	-0.0035
	(0.006)	(0.039)	(0.007)	(0.013)	(0.012)
2 years later	0.0091	-0.1246***	0.0090	0.0312**	-0.0027
	(0.006)	(0.045)	(0.008)	(0.015)	(0.013)
3 years later	0.0048	-0.0789*	-0.0086	0.0569***	-0.0444***
	(0.006)	(0.045)	(0.008)	(0.015)	(0.013)
4 years later	-0.0002	-0.0926**	0.0023	0.0746***	-0.0456***
	(0.007)	(0.047)	(0.009)	(0.016)	(0.014)
5 years later	0.0143*	-0.0778	-0.0015	0.0632***	-0.0685***
	(0.008)	(0.052)	(0.010)	(0.017)	(0.015)
6 or more years later	0.0039	-0.1788***	0.0160	0.0510**	-0.0601***
	(0.010)	(0.068)	(0.013)	(0.022)	(0.020)
R-squared	0.173	0.579	0.066	0.117	0.065

Notes: N=64,409. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

Table 3.8: Main Results: Semi-Dynamic Model, Effect of Juntos on Birth Control Use and Fertility Outcomes, Ever Treated Districts Only (OLS)

	(1)	(2)	(3)	(4)	(5)
	Fertility outcomes			Birth Control Use	
VARIABLES	At least one child	Number of children	Currently pregnant	Modern Method	Not Using
Year Juntos implemented	0.0032	0.0297	-0.0039	0.0239*	0.0000
	(0.006)	(0.034)	(0.006)	(0.014)	(0.011)
1 year later	0.0027	-0.0258	0.0044	0.0088	0.0020
	(0.006)	(0.044)	(0.007)	(0.014)	(0.012)
2 years later	0.0112*	-0.0223	0.0078	0.0300*	0.0111
	(0.007)	(0.047)	(0.009)	(0.016)	(0.013)
3 years later	0.0090	-0.0126	-0.0122	0.0442***	-0.0233*
	(0.007)	(0.045)	(0.009)	(0.017)	(0.014)
4 years later	-0.0005	-0.0157	0.0017	0.0580***	-0.0176
	(0.008)	(0.052)	(0.010)	(0.019)	(0.016)
5 years later	0.0166**	0.0055	-0.0087	0.0551***	-0.0326**
	(0.008)	(0.057)	(0.010)	(0.020)	(0.016)
6 or more years later	0.0104	-0.0471	0.0044	0.0464**	-0.0319*
	(0.010)	(0.069)	(0.012)	(0.023)	(0.019)
R-squared	0.169	0.581	0.060	0.097	0.057
Mean of the dep. var.	0.9527	3.0128	0.0779	0.4309	0.2476
Average effect t1-t6	0.0082	-0.0197	-0.0004	0.0404**	-0.0154
	(0.006)	(0.043)	(0.008)	(0.015)	(0.013)
			Common trends tests		
P-value test1	0.2122	0.2549	0.9254	0.4933	0.4792
P-value test2	0.5211	0.7636	0.0667	0.3265	0.1124
			Placebo tests (Average effects)		
Non-poor women	0.0060	-0.0408	0.0086	0.0053	0.0284
(N=15,857)	(0.015)	(0.058)	(0.017)	(0.030)	(0.028)
Single childless women				-0.0123	0.0189
(N=25,383)				(0.008)	(0.012)

Notes: N=68,882. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. Test 1 reports the p-value corresponding to the average effect pre-intervention estimated using the fully dynamic specification (equation (2)). Test 2 tests the joint significance of all lead coefficients estimated by the fully dynamic specification (equation (2)). ***, **, * are 1%, 5% and 10% respectively.Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

Table 3.9: Mechanisms: Semi-Dynamic Model Effect of Juntos on Birth Control Use and Fertility Outcomes, Women With Children Under 5, Rural Districts and Small Towns (OLS)

	(1)	(2)	(3)	(4)
	Fertility outcomes		Birth Control Use	
VARIABLES	Number of children	Currently pregnant	Modern Method	Not Using
Year Juntos implemented	-0.0313	0.0056	0.0361*	-0.0187
	(0.055)	(0.010)	(0.022)	(0.017)
1 year later	-0.0857	-0.0039	0.0170	-0.0214
	(0.053)	(0.010)	(0.021)	(0.019)
2 years later	-0.1604***	-0.0037	0.0607***	-0.0161
	(0.057)	(0.010)	(0.023)	(0.018)
3 years later	-0.1022*	-0.0205**	0.0709***	-0.0550***
	(0.054)	(0.009)	(0.022)	(0.019)
4 years later	-0.1170**	-0.0107	0.0929***	-0.0553***
	(0.059)	(0.012)	(0.023)	(0.020)
5 years later	-0.0810	-0.0145	0.0879***	-0.0834***
	(0.065)	(0.011)	(0.025)	(0.020)
6 or more years later	-0.2416***	-0.0116	0.0657**	-0.0729***
	(0.069)	(0.013)	(0.027)	(0.022)
R-squared	0.660	0.057	0.148	0.083
Mean of the dep. var.	3.0648	0.0631	0.4893	0.2429
Average effect t1-t6	-0.1313***	-0.0108	0.0658***	-0.0507***
	(0.047)	(0.009)	(0.019)	(0.016)
Common trends tests				
P-value test1	0.1745	0.4372	0.8007	0.8525
P-value test2	0.4557	0.0233	0.2650	0.3202
Placebo tests (Average effects)				
Non-poor women	-0.0513	-0.0010	0.0249	-0.0255
(N=8,302)	(0.071)	(0.019)	(0.040)	(0.035)

Notes: N=42,010. Notes: All regressions include district and year fixed effects, individual characteristics, and DHS weights.Standard errors are clustered at the district level. Test 1 reports the p-value corresponding to the average effect pre-intervention estimated using the fully dynamic model (equation (2)). Test 2 tests the joint significance of all lead coefficients estimated by the fully dynamic model (equation (2)). ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

Table 3.10: Mechanisms: Semi-Dynamic Model, Effect of Juntos on Birth Control Use and Fertility Outcomes, Women With No Children Under 5, Rural Districts and Small Towns (OLS)

	(1)	(2)	(3)	(4)
	Fertility outcomes		Birth Control Use	
VARIABLES	Number of children	Currently pregnant	Modern Method	Not Using
Year Juntos implemented	-0.0979	0.0023	0.0006	0.0163
	(0.069)	(0.015)	(0.024)	(0.021)
1 year later	-0.0629	0.0158	0.0098	0.0207
	(0.070)	(0.017)	(0.027)	(0.024)
2 years later	-0.0789	0.0132	0.0163	0.0138
	(0.074)	(0.016)	(0.028)	(0.022)
3 years later	-0.0327	-0.0075	0.0431*	-0.0317
	(0.076)	(0.017)	(0.026)	(0.023)
4 years later	-0.0102	0.0046	0.0460	-0.0306
	(0.081)	(0.017)	(0.029)	(0.022)
5 years later	-0.0535	-0.0009	0.0265	-0.0147
	(0.084)	(0.019)	(0.029)	(0.025)
6 or more years later	-0.0150	0.0268	0.0199	-0.0130
	(0.100)	(0.022)	(0.034)	(0.030)
R-squared	0.627	0.205	0.139	0.147
Mean of the dep. var.	2.9086	0.1020	0.3585	0.2370
Average effect t1-t6	-0.0422	0.0086	0.0269	-0.0093
	(0.063)	(0.014)	(0.023)	(0.019)
Common trends tests				
P-value test1	0.7265	0.3584	0.2770	0.0530
P-value test2	0.6123	0.7445	0.7440	0.5170
Placebo tests (Average effects)				
Non-poor women	-0.0434	0.0213	-0.0189	0.0067
(N=5,977)	(0.092)	(0.029)	(0.052)	(0.041)

Notes: N=22,399. Notes: All regressions include district and year fixed effects, individual characteristics, and DHS weights.Standard errors are clustered at the district level. Test 1 reports the p-value corresponding to the average effect pre-intervention estimated using the fully dynamic model (equation (2)). Test 2 tests the joint significance of all lead coefficients estimated by the fully dynamic model (equation (2)). ***, **, * are 1%, 5% and 10% respectively.Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

Chapter 4

How to Measure Parenting Styles?

4.1 Introduction

Early childhood investments have been shown to be crucial for children's human capital development (Cunha, Heckman, and Schennach (2010), Del Boca, Flinn, and Wiswall (2014), Attanasio, Meghir, and Nix (2020)), but the measurement of interactions between different dimensions of investment is challenging due to their complexity (Attanasio (2015)). Parental time investments generally are captured through different activities parents engage in with their children, such as visits to museums or the frequency of a parent reading to their children. The number of activities considered is either vast or restricted arbitrarily. When many investments are considered, they generally are combined (log-)linearly in latent factor models or using principal component analysis. More recently, the debate about parenting styles has emerged in economics emphasizing that not only investments but also the style of investing matters (Doepke and Zilibotti (2017)).[1] In developmental psychology, Baumrind (1967) already classified parenting styles and related them to behavioral traits among pre-school children. This approach was extended by McCoby and Martin (1983) to four styles along two dimensions: warmth and control.

Parenting is characterized by a complex set of interactions and decisions. Draca and Schwarz (2018) discuss why linear combinations of features with the highest degrees of variance in the data, as is the case for principal component analysis, may not provide optimal summaries of complex data-generating processes. This paper proposes a new

1. In the earlier economics literature, parenting styles were defined as an index of punitive/aversive parenting using four questions about how the parent responds when the child misbehaves (Burton, Phipps, and Curtis (2002)).

methodology to summarize parenting styles based on parental interactions with their children using unsupervised machine learning.[2] One advantage of this approach is that it allows aggregating any number of granular parental activities in a non-linear fashion. The algorithm learns from the co-occurrence of parental actions. Given that it is a mixed-member model, the same action can be assigned to different types. If, on the one hand, a parent regularly checks on a child, and combines this with hugs and kisses, this could be considered warm control in the words of McCoby and Martin (1983). If, on the other hand, regularly checking on a child co-occurs with yelling at the child, the parenting style could be considered controlling combined with a lack of warmth.

When we restrict the algorithm to identify two styles, we find that parents can be classified into "positive" and "negative" types.[3] Positive parents are more likely to be supportive of their children's progress and speak directly to their child, while negative parents are characterized by hardly interacting with their children in the presence of the interviewer, and if they do, they tend to do so in a negative manner. In line with ad-hoc classifications in developmental psychology, the two parenting styles we discover can be interpreted as high warmth and high control (positive) vs low warmth and low control (negative). Although parenting styles exhibit some persistence over time, we find that parents are more likely to adopt positive parenting styles when the children are younger.

We contribute to two strands of literature. First, we contribute to the literature concerned about parenting styles.[4] They generally draw the distinction between parenting styles in terms of permissive, authoritarian, or authoritative. The empirical approaches tend to classify parenting styles based on a single binary response to a survey question, such as how important obedience is for a respondent (e.g., Agostinelli et al. (2020)) or latent factor models (e.g., Falk et al. (2021)). Our approach allows capturing parenting styles based on many questions with complex interactions. Moreover, an advantage of our data on parental activities is that they are not self-reported, but are observed and recorded by the enumerator, which should help to reduce systematic measurement error, and are the

2. With the availability of the necessary data, the approach could easily be applied to summarize parental investments rather than styles as well.

3. The reason we limit the estimation to two styles is twofold. First, we only observe ten different parental actions, which complicates the identification of more types. Second, two types simplify the exposition.

4. One can roughly separate this literature into two strands: First, the literature relating parenting styles to child development (e.g., Cunha (2015), Doepke and Zilibotti (2019), Doepke, Sorrenti, and Zilibotti (2019), Cobb-Clark, Salamanca, and Zhu (2019), Agostinelli et al. (2020)). Second, the literature studying the role of parenting styles in the intergenerational transmission of traits (e.g., Brenøe and Epper (2019Zumbuehl, Dohmen, and Pfann ■[2021Falk et al. ■(2021■)). Further, Del Boca et al. ■(2019■) propose a model in which parental types are not merely the outcome of utility maximization by the parents but the result of a bargaining process with the children. Kiessling ■(2020■) studies how parents perceive the returns to parenting styles in terms of warmth and control using hypothetical scenarios.

same set of actions observed across multiple survey waves. Our new interpretable measure summarizing the large dimensionality and complexity of parental activities is predictive of human capital above and beyond the predictive power of parental socio-economic characteristics or child fixed effects.[5]

Second, we add to the rapidly growing use of machine learning in Economics to classify behavioral types. The latent Dirichlet allocation (LDA) was originally developed by computer scientists Blei, Ng, and Jordan (2003). The underlying idea is to classify text documents into a mixture of small number of topics. One key is that the topics are not predefined but are backed out through co-occurrence. We apply the same idea of topics to behavioral types. Other approaches to classifying behavioral types using LDA are Bandiera et al. (2020) who classify CEOs using detailed time-use surveys and find that CEOs distinct behavior affects firm performance. Draca and Schwarz (2018) use LDA to measure political ideology. We contribute to this literature by using LDA to classify parenting styles and look at its relation to human capital accumulation in very early childhood.

4.2 Data

We use the Québec Longitudinal Study of Child Development (QLSCD), a detailed panel of a representative sample of families from Québec, a province in Canada, with a baby born between October 1997 and July 1998. More specifically, we focus our work on the 1,985 families who participated in the first three waves of the panel, conducted when the designated baby was 5, 17 and 29 months old.

We rely on the Observations of Family Life (OFL) instrument filled by the enumerator at the end of the annual interview. It includes observations made during the interview about the behaviour of the key respondent—the mother in 99% of the cases—and her interactions with her baby. This has the advantage of not relying on self-reported behavior which is common in the human capital literature and a potential source of bias.

We exclude mother-children pairs for whom the OFL instrument was not completed at child ages 5, 17 or 29 months because the child was sleeping. We end-up with a sample of 1,443 mother-children pairs. Table 4.1 describes the socio-economic characteristics of the families.

We focus our analysis on the ten variables from the OFL instrument that assess the

5. Despite the intuitive results we cannot claim causal effects due to the lack of an exogenous shock to parenting styles. This is a common feature of studies measuring the impact of parenting styles.

behavior of the interviewed mother toward her child. Table 4.2 displays descriptive statistics for these variables. We see that some parental actions are highly dependent on the age of the child. For instance, the share of parents regularly checking on their child decreases from 72% when the child is 5 months old to 32% when the child is 29 months old.

4.3 Discovering Latent Parenting Types

In the next step, the different features of parental behavior are summarized into interpretable behavioral types using a machine learning algorithm based on the latent Dirichlet allocation. This methodology developed by Blei, Ng, and Jordan (2003) is a clustering algorithm for discrete data, which traditionally was meant to reduce the high dimensionality of text into an arbitrary number of topics specified by the user. Each parental action can be featured with difference importance in each type, and each parent can be a mixture of types.

The algorithm learns from the co-occurrence of counts through Bayesian learning. The idea is that if certain variables tend to appear together, they are likely to be linked to each other. In Appendix 4.7.1 we explain the technical details. For the sake of simplicity and interpretation, we settle on two types of parents. The final output of the algorithm is the distribution of actions for each type and the type distributions for each parent. With this information at hand, we can then relate parental types summarized into just two types to human capital accumulation.

4.3.1 Parenting Types

We pool the three waves together and estimate the classification for that sample.[6] In Table 4.3 we display the relative probabilities of actions by the two types. The action that distinguishes the two types most in relative terms are supportive comments made by the parent to the child about its progress. While it is very common for the positive type to make supportive comments about the progress of the child, this is hardly the case for parents of the negative type, i.e. positive parents are 626 times more likely to do so.

6. We could estimate a different classification for each wave separately as some actions might be more pertinent for different ages of the child, as is indicated by the distribution of actions in Table 4.2. However, the parental classification would not be comparable over time, which would pose other challenges for the rest of our analysis.

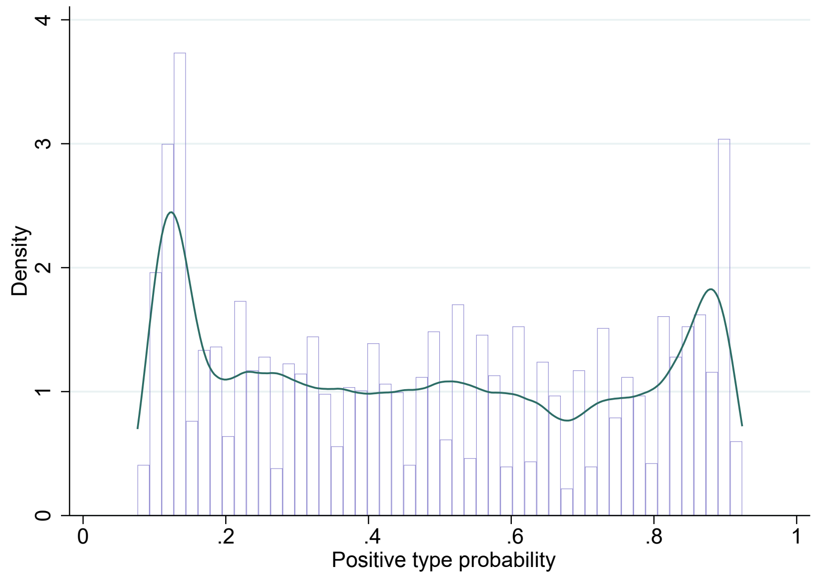
Similarly large differences exist for speaking to the child directly. Actions that are more typical for negative parents are reprimanding the child or expressing annoyance.

The distribution of actions across types suggests that what distinguishes parents is the richness and warmth of action by one type versus the authoritarian and control by the other, hence the labels positive and negative parents.

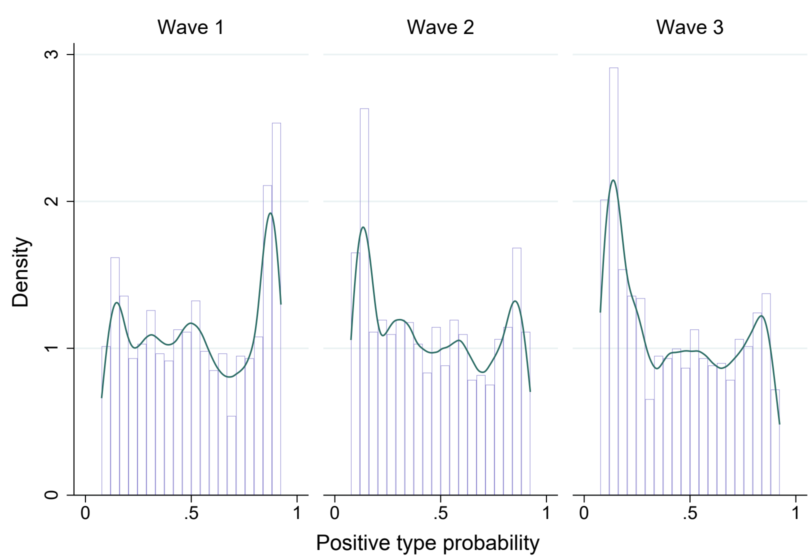
The LDA algorithm assigns to each parent a probability of being of type 1, the positive type (and with the remaining probability they are of type 2, the negative type). The top panel of Figure 4.1 shows the distribution of the positive type probability for the full sample and the bottom panel for each wave separately. We see a concentration of two masses: one with a low probability of being of the positive type (i.e. with a high probability of being of the negative type) and the opposite. Over time, parents tend to move from the positive type to the negative type.

Figure 4.1: Distribution of Types

a) Pooled across all waves



b) For each wave separately



Notes: The transparent bars represent the binned probabilities of the probability of being a positive type rather than a negative type, while the solid line is the kernel density.

Table 4.1: Descriptive Statistics

	N	Prop.
Number of siblings		
No sibling	656	45.5
One	560	38.8
Two or more	227	15.7
Household type		
Two-parent	1,179	81.7
Blended	159	11.0
Single-parent	102	7.1
Missing	3	0.2
Mother's age		
Less than 25	332	23.0
25-29	446	30.9
30-34	469	32.5
35 and more	195	13.5
Missing	1	0.1
Mother born outside Canada		
No	1,301	90.2
Yes	140	9.7
Missing	2	0.1
Language spoken at home		
French	1,176	81.5
Other	265	18.4
Missing	2	0.1
Mother education		
High school degree or less	380	26.3
Some college education	681	47.2
College degree	380	26.3
Missing	2	0.1
Parental working status		
Two-parents: both work	984	68.2
Two-parents: one works	304	21.1
Two-parents: none work	45	3.1
Single-parent: works	38	2.6
Single-parent: does not work	58	4.0
Missing	14	1.0
Below poverty threshold		
No	1,106	76.6
Yes	317	22.0
Missing	20	1.4
N	1,443	100

Notes: The table shows descriptive statistics for families in our sample at the time of the first interview in 1998, when the designated child is 5 months old.

Table 4.2: Parental Behaviour

	Proportion of mothers who ...		
	Wave 1 5 months	Wave 2 17 months	Wave 3 29 months
Regularly checks on her child	71.7	47.8	31.9
Speaks spontaneously to her child	40.6	43.2	46.3
Answers to her child	45.0	46.8	57.4
Kisses and hugs her child	42.6	17.3	13.9
Screams toward her child	< 0.5	4.9	6.8
Is annoyed by her child	1.6	7.1	10.5
Reprimands her child	< 0.5	4.3	5.5
Supports her child's progress	38.0	26.1	25.5
Organises play time	58.5	53.6	43.6
Gives pedagogical toys	68.2	59.0	43.7
Observations	1,443	1,443	1,443

Notes: The table describes the behaviour of the respondents and their interactions with their children during the annual QLSCD interview. Behaviours are evaluated by the enumerator during the interview. Statistics are presented for the three first waves, when the designated child is 5, 17 and 29 months old.

Table 4.3: Classification of Behaviors by Parental Types

	Ratio type 1 to type 2
Supports child's progress	626
Speaks directly to child	454
Answers child	260
Kisses and hugs child	256
Organizes play time	14.4
Gives pedagogical toys	11.0
Regularly checks on child	3.63
Screams at child	1.71
Reprimands child	0.94
Expresses annoyance by child	0.78

Notes: The table describes the occurrence of behaviours for the two types found by the LDA algorithm. Behaviours are ranked from what is the relatively most likely behavior of type 1 relative to type 2. We label these types positive (type 1) and negative (type 2). The second column displays the ratio of the probability for type 1 over the probability for type 2.

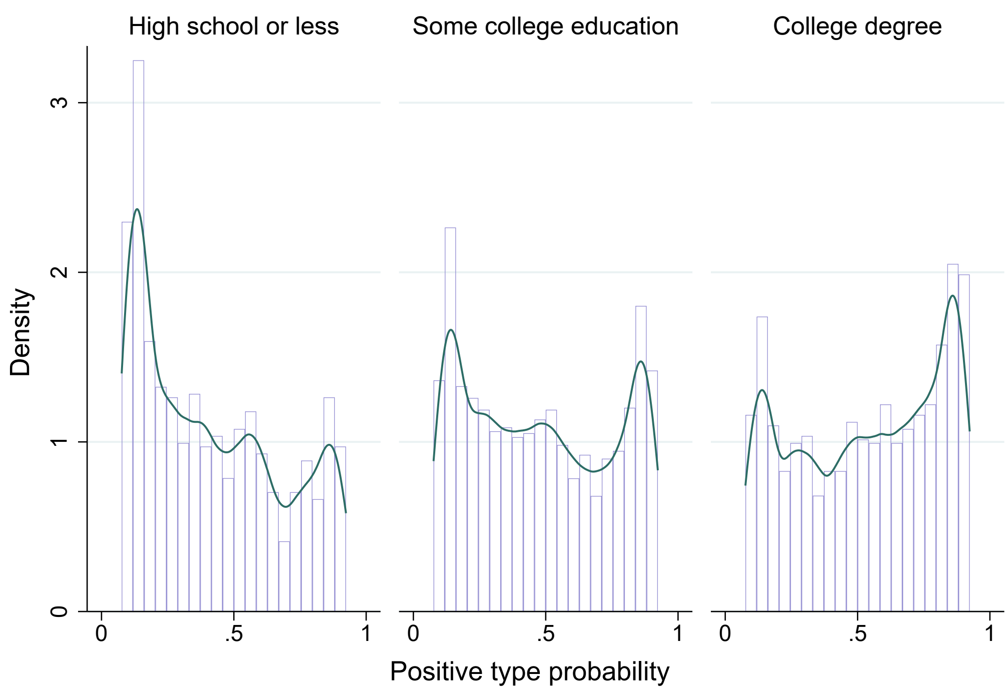
4.3.2 Correlates and Persistence of Parenting Types

In Figure 4.2 we show the distribution of positive types by maternal education. In the left panel we see that mothers with high school or less tend to be of the negative type with an average share of positive types of 41.8%. In the middle panel we see that for mothers with some college education the distribution appears closer to bi-modal with an average probability of positive types of 48.3%. Finally, in the right panel we see that amongst more educated mothers with a college degree, the average likelihood of being of the positive type increases to 53.9%.

While the previous figure suggests that the likelihood of being a positive mother is increasing in education, we take a more systematic look at the relationship between type and individual characteristics by regressing the probability of being a positive type on age, education, poverty level, whether the parent is an immigrant, marital status, employment status, number of siblings, and the gender of child. In the first column of Table 4.4 we see the results for the pooled sample and in the following three columns for each age of the child, separately.

We see that parents with more than one child tend to be less likely to be of the positive type. The probability of being positive appears to be increasing in maternal age and education. While some of the coefficients vary, in general the direction of coefficients is very similar across waves. Maternal types reveal a considerable persistence as suggested by the correlations across waves exhibited in Table 4.5. Between wave 1 and wave 2 the correlation in types is 0.26, and between wave 2 and wave 3 it is 0.36. In fact, regressing individual fixed effects on parenting types achieves an R^2 of 0.52. We further breakdown the persistence in Table 4.6 in which we show the transition matrix between positive types (defined as being of the positive type with a probability above 0.67), an intermediate type (positive type with a probability between 0.33 and 0.67), and the negative type (positive type with a probability of less than 0.33). According to this matrix 38% (51%) of positive (negative) mothers in wave 1 are of the same type in wave 2, and 42% (58%) of positive (negative) mothers in wave 2 are of the same type in wave 3.

Figure 4.2: Distribution of Types by Maternal Education



Notes: The transparent bars represent the binned probabilities of the probability of being a positive type, while the solid line is the kernel density.

Table 4.4: Positive Type Probability and Parental Characteristics

	Probability of being of the positive type			
	Pooled	Wave 1	Wave 2	Wave 3
Number of siblings (reference: no sibling)				
One sibling	-0.043***	-0.037**	-0.057***	-0.035**
	(0.011)	(0.016)	(0.016)	(0.016)
Two or more siblings	-0.060***	-0.039*	-0.105***	-0.035
	(0.017)	(0.023)	(0.023)	(0.023)
Household type (reference: two-parents family)				
Blended family	0.013	-0.005	0.035	0.010
	(0.018)	(0.025)	(0.024)	(0.023)
Single-parent household	-0.146	-0.160	-0.102	-0.176*
	(0.127)	(0.193)	(0.178)	(0.103)
Mother's age (reference: less than 25)				
25-29	0.024	0.053***	0.013	0.006
	(0.015)	(0.021)	(0.021)	(0.021)
30-34	0.055***	0.089***	0.046**	0.029
	(0.016)	(0.021)	(0.023)	(0.021)
35 and more	0.082***	0.100***	0.071***	0.075***
	(0.019)	(0.027)	(0.027)	(0.026)
Mother born outside Canada (reference: no)				
Yes	-0.035*	-0.061**	-0.042	-0.003
	(0.020)	(0.029)	(0.028)	(0.029)
Language spoken at home (reference: French)				
Other	-0.034**	0.034	-0.060***	-0.075***
	(0.014)	(0.021)	(0.020)	(0.021)
Mother education (reference: high school degree or less)				
Some college education	0.050***	0.042**	0.037**	0.073***
	(0.013)	(0.018)	(0.018)	(0.018)
College degree	0.089***	0.084***	0.079***	0.103***
	(0.015)	(0.022)	(0.022)	(0.022)
Parental working status (reference: two-parents: both work)				
Two-parents: one works	-0.001	0.014	-0.005	-0.012
	(0.013)	(0.019)	(0.019)	(0.019)
Two-parents: none work	-0.033	-0.004	-0.088*	-0.006
	(0.034)	(0.045)	(0.047)	(0.045)
Single-parent: works	0.136	0.127	0.091	0.189*
	(0.133)	(0.199)	(0.184)	(0.113)
Single-parent: does not work	0.151	0.147	0.102	0.204*
	(0.132)	(0.198)	(0.183)	(0.113)
Below poverty threshold (reference: no)				
Yes	-0.025	-0.055**	0.003	-0.022
	(0.016)	(0.023)	(0.022)	(0.022)
Constant	0.436***	0.455***	0.455***	0.400***
	(0.0159)	(0.022)	(0.022)	(0.022)
Observations	4,329	1,443	1,443	1,443
R-squared	0.050	0.070	0.060	0.056

Notes: Each column presents the estimates of an OLS regression of positive type probability on parental characteristics. The categories for missing values are also included in the regression but not shown in the table as they only concern a few individuals and are thus hard to interpret. Robust standard errors clustered at the family level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4.5: Correlation Matrix of Positive-Type Probability across Waves

	Wave 1	Wave 2	Wave 3
Wave 1	1.00	.	.
Wave 2	0.26	1.00	.
Wave 3	0.24	0.36	1.00

Notes: The table displays the correlation between the positive type probability variable in wave 1 and the one in wave 2, the positive type probability variable in wave 2 and the one in wave 3, and the positive type probability variable in wave 2 and the one in wave 3.

Table 4.6: Transition Matrix Between Binned Types

(a) Between waves 1 and 2

	Wave 2		
Wave 1	Positive	Intermediate	Negative
Positive	0.38	0.37	0.25
Intermediate	0.27	0.36	0.37
Negative	0.17	0.32	0.51

(b) Between waves 2 and 3

	Wave 3		
Wave 2	Positive	Intermediate	Negative
Positive	0.42	0.36	0.23
Intermediate	0.27	0.35	0.37
Negative	0.14	0.28	0.58

Notes: The first table presents the transition matrix between positive types (defined as being of the positive type with a probability above 0.67), an intermediate type (positive type with a probability between 0.33 and 0.67), and the negative type (positive type with a probability of less than 0.33) between wave 1 and wave 2. The second table presents the same transition matrix between wave 2 and wave 3.

4.4 Relating Parenting Types to Children's Outcomes

To test the relationship between parental type and the accumulation of children's cognitive skills, we use the results from an Imitation Sorting Task (IST) test conducted during each wave.[7] Here the sample size reduces to 1,121 children who took the IST test at 5, 17 and 29 months. Excluded children were sleeping or sick at the time the test was supposed to take place or the test was not fully completed. The test score in each wave is standardized with a mean of 0 and a standard deviation of 1.

In the first column of Table 4.7 we show the results of the pooled sample in which we regress the IST test score at each of the three stages on the probability of being a positive parent and a constant. We find that moving from a negative to a positive parent is associated with an increase in the IST score of 0.223 standard deviations. In the second column we add controls for parental characteristics and still find a highly significant positive association between the probability of being a positive parent and test scores of 0.167 standard deviations. In the third column we control for parental fixed effects, thereby removing any constant heterogeneity across parents and children. Using this specification we find a strengthened association between being a positive type and cognitive development with a highly significant coefficient of 0.338.

7. The task comprises different situations in which the infant must grasp objects placed in front of him/her and place them in given containers. The task used in the ELDEQ is a variation of the Imitation Sorting Task developed by Uzgiris and Hunt ■(1975■).

Table 4.7: Positive Type Probability and Cognitive Development

	Standardized IST score		
	(1)	(2)	(3)
Positive type probability	0.223***	0.167**	0.338***
	(0.069)	(0.069)	(0.108)
Observations	3,363	3,363	3,363
R-squared	0.004	0.020	0.387
Family characteristics	NO	YES	NO
Family FE	NO	NO	YES

Notes: Each column presents the estimates of an OLS regression of the child standardized IST score on her mother positive type probability. Family characteristics (column 2) include family composition (number of siblings, household type), maternal characteristics (age, whether born outside Canada, educational attainment), parental working status, language spoken at home, and whether family is below poverty threshold. They are described in more details in Table 1. Robust standard errors clustered at the family level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

4.5 Conclusion

Human capital accumulation is one of the most important fundamentals of productivity and innovation. However, estimating human capital production functions is riddled with complications including the high dimensionality and potentially non-linear relationships between parental actions. In this paper, we provide a new way to summarize parental styles adopted from computational linguistics. We use an unsupervised machine learning model, the latent Dirichlet allocation, to classify parents into two types. The resulting types can be interpreted as positive parents who encourage their children and express their affection, versus negative parents who do not interact much with their children and are more likely to punish when they do so.

We show that these two types relate systematically to parental characteristics, i.e. mothers with higher education tend to be more likely to be of the positive type. Moreover, we show that children of more positive parents tend to achieve higher levels of human accumulation. While we cannot establish a causal relationship between parenting types and outcomes due to the nature of the data, we are optimistic that future studies including natural experiments or randomized control trials can make use of the proposed methodology to classify parents into types based on their their actions. Another advantage of the approach is that this can be done with an extremely large set of actions or even detailed time use data.

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4.7 Appendix

4.7.1 Latent Dirichlet allocation

Adapting the technical terms from Blei, Ng, and Jordan (2003) for text and applying to our objective, the corpus of behavioral actions D is composed of parents w of actions. A behavioral type is a probability distribution over all actions. The assumed underlying process with which types generate actions is by drawing θ from a Dirichlet distribution with hyperparameter α. Then for each action n of all actions N, one chooses a type from z_n. After that an action w_n is chosen for the corresponding type z_n from a Dirichlet distribution with hyperparameter β.

Written formally, the generative process of actions is expressed as the following joint distribution

$$p(\beta, \theta, z, w_d) = \prod_{i=1}^{k} p(\beta_i) \prod_{d=1}^{D} p(\theta_d) (\prod_{n=1}^{N} p(z_{d,n}|\theta_d) p(w_{d,n}|\beta, z_{d,n})).$$

Given the corpus of actions, the task of the algorithm is to infer the type-specific action distribution and the parent specific type distribution. So the posterior distribution of the latent variables is given by

$$p(\beta, \theta, z|w_d) = \frac{p(\beta, \theta, z, w_d)}{p(w_d)}.$$

In order to infer the marginal distribution $p(w_d)$, which can be done through approximation using Gibbs sampling, or Variational Kalman Filtering and Variational Wavelet Regression, we rely on the Stata implementation developed by Draca and Schwarz (2018). Draca and Schwarz (2018) use the inference algorithm developed by Hoffman, Bach, and Blei (2010) and implemented by Pedregosa et al. (2011). As is the case in Draca and Schwarz (2018), the assumption of the independence of responses does not strictly hold in our approach. If an action has been recorded the same action is not recorded again for the same person. They discuss in detail why the inference of LDA is nonetheless still valid.

4.8 Discussion and Conclusion

The first chapter of my thesis investigates the effects of college grant aid, career education in high school, and the combination of the two on students' college enrollment, graduation, and earnings. I show that career education programs have the potential to improve students' long-term outcomes substantially. My results suggest that the reason why these types of programs are so effective stems from the existence of information and behavioral barriers that prevent students from making optimal decisions regarding post-secondary education. Removing these barriers will induce more low-income students and less high-income students to enroll in four-year colleges, resulting in a sharp reduction in the enrollment and graduation gaps between the two types of students.

One limitation of my study is the lack of power, which prevents any clear exploration of treatment effect heterogeneity. As the career education program resulted in increase in both graduation and dropout rates, I suspect heterogeneous benefits of the intervention on students. Further work should aim to understand who benefited from the intervention and who did not. This understanding would facilitate the design of career education programs that are better suited to helping all types of students.

Moreover, a key question remains unanswered: what features of the career education program were the more effective at increasing low- income students' enrollment? Previous studies suggest that the provision of information alone is not helpful is increasing students' enrollment rates (Bird et al. (2021); Kerr et al. (2015); Bonilla, Bottan, and Ham (2015); Hastings et al. (2016); Carrell and Sacerdote (2017)). By contrast, the intervention provided insights into the post-secondary education application process and decision-making, a dimension that has proven to be effective in increasing college enrollment rates (Avery (2013); Stephan and Rosenbaum (2013); Castleman, Page, and Schooley (2014); Carrell and Sacerdote (2017); Cunha, Miller, and Weisburst (2018); Oreopoulos and Ford (2019)). The fact that programs offering guidance are so efficacious in increasing the enrollment of students and (as I show) in enhancing their long-term outcomes is likely to be explained by their lack of attention when it comes to college possibilities (French and Oreopoulos (2017)).

In this paper, I also show that providing students with additional financial aid has no monetary benefits in the long run. This result was surprising given the fact that the intervention led to an increase in the fraction of students who enrolled and graduated from community colleges. It indicates that students were induced to enroll in programs with limited monetary returns. This lack of returns might be specific to the marginal students but might also arise from a general lack of benefits of some community college programs. My findings underscore the importance of understanding the returns to community college attendance.

The extent to which my findings extend to other contexts and countries remains to be seen. There are strong reasons to believe that my main finding, according to which career education programs are efficacious in enhancing students' long-term outcomes, can be extended to other contexts as well. In fact, many US-specific studies demonstrated that career counseling programs, as the program studied in this paper, are effective in increasing students' enrollment in four-year colleges. It is thus natural to think that they would also result in an increase in earnings.

However, my results on the effects of the financial aid intervention are possibly specific to the Canadian context. Two features of the Canadian context make it different from other countries. First, unlike other countries, Canada is characterized by a very high enrollment rate in community and private career colleges, which might make the results on community colleges specific to this country. Second, public colleges and universities are highly subsidized, and a number of grants and loans are already available in Canada, making financial constraints possibly less binding than in countries with weaker financial aid systems.

In the future, I plan to pursue my analysis of the Future to Discover experiment in three ways. First, I will continue to track students to confirm my findings on the long-term effects of the three interventions and assess their overall effects on lifetime earnings. Second, I will build a structural model of college enrollment under imperfect information and Bayesian learning to exactly quantify the extent to which students are affected by informational and behavioral barriers and provide counterfactual estimates of the gain in earnings that removing these barriers would create. Third, I will exploit exogenous variations in the timing of career counseling workshops created by weather conditions in order to identify how the timing of information affects the decisions made by students.

Second, in the third chapter of my thesis, I investigate the fertility and reproductive health outcomes effects of cash transfers that are conditional on child school attendance and prenatal and infant health checks. We find that women in targeted districts tend to have fewer children for as much as up to 6 years after the program was rolled in, suggesting strong long-term effects at the intensive margin. We do not find any evidence of an extensive margin effect, contrary to fears among policy-making circles that cash transfers will lead to greater fertility (e.g. Peterman (2021)). The decrease in fertility seems to be driven by an increase in the take-up of modern forms of birth control. This last result is of particular importance for sexual and reproductive health and rights given a disproportionate reliance on traditional methods in Peru (Ponce de Leon et al. (2019)), methods that are neither effective nor necessarily safe. Investigating potential mechanisms, we find that fertility preferences remain stable and that while women in targeted districts do not seem to benefit from increased explicit autonomy in intra-household decision making over fertility, those that are more likely to use contraception may be doing so with concealable forms. We interpret these results as evidence of binding barriers to women's empowerment, and that the cash transfer program may help some women covertly assert their preferences and take control over their fertility.

We also investigate whether the program indirectly exposes women to reproductive health information and care by considering the sub-sample of women with children under the age of 5: to comply with the conditionalities of the program, these children are required to undergo regular health checks. We find the same results as for the larger sample and so we cannot exclude the possibility that mothers are also receiving greater access to reproductive care and technology while they are there with their children. We test our identification strategy by running placebo tests on samples of non-eligible demographic groups: indeed we find no effect of Juntos among women who are non-poor or childless. Finally, our results are robust to new techniques for estimating event studies in the presence of varying treatment times.

The persistence of the effects on fertility and reproductive health outcomes has policy relevant importance for two reasons. First, since these are outcomes that are not explicitly targeted by the program conditionalities, our findings add to the case that evaluating anti-poverty programs ought to consider broader sets of outcomes than those directly targeted by program designers. Second, our results provide encouraging insights against the concern that dynamic effects of social protection programs may wane as beneficiaries' excitement about the program fades, compliance with conditionalities become less strictly enforced, and interventions become less effective when implemented by governments at scale compared to smaller pilot programs run by NGOs (Bold et al. (2018); Cahyadi et al. (2020)). Indeed, the long term, dynamic, effects of a large scale cash transfer program on women's reproductive outcomes suggests potentially transformative effects on the lives of beneficiary families, given the long term expected benefits of reduced fertility (Birdsall and Griffin (1988); Sinding (2009)).

Finally, in the last chapter, I provide a new way to summarize parental styles adopted from computational linguistics. We use an unsupervised machine learning model, the latent Dirichlet allocation, to classify parents into two types. The resulting types can be interpreted as positive parents who encourage their children and express their affection, versus negative parents who do not interact much with their children and are more likely to punish when they do so.

We show that these two types relate systematically to parental characteristics, i.e. mothers with higher education tend to be more likely to be of the positive type. Moreover, we show that children of more positive parents tend to achieve higher levels of human accumulation. While we cannot establish a causal relationship between parenting types and outcomes due to the nature of the data, we are optimistic that future studies including natural experiments or randomized control trials can make use of the proposed methodology to classify parents into types based on their their actions. Another advantage of the approach is that this can be done with an extremely large set of actions or even detailed time use data.

In the future, we plan to expand the analysis to a classification involving more than two types and to longer-term children outcomes.

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