ABSTRACT

Title of Dissertation: INTENDED AND UNINTENDED IMPACTS OF GOVERNMENT PROGRAMS IN AGRICULTURE AND EDUCATION

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Agriculture and education are often considered crucial programmatic areas for governments around the globe. In their search for economic growth and social well-being, governments across the developing world implement policies aimed at enhancing human capital formation and increasing agricultural productivity. In this dissertation I study the intended and unintended impacts of three types of government programs commonly used to improve outcomes in agriculture and education.

In countries where land was distributed to collectives or groups rather than to individuals, concerns about how collective ownership may hinder agricultural productivity led to a ”second wave” of land reforms . In my first chapter, I study a land tenure transition from collective to individual land rights, and present evidence on the impacts of the Philippine parcelization program. Contrary to its objective, the implementation of this transitional stage has increased tenure insecurity, albeit without affecting agricultural productivity for most farmers in the short term. In turn, higher tenure insecurity has prompted land leases and a reallocation of labor to the non-farm sector. These unintended effects are likely due to a nontransparent and lengthy

implementation process stemming from governmental capacity constraints.

My second and third chapters are on education. Teacher quality is one of the most relevant factors influencing student learning and affecting human capital formation. Attracting the best candidates to the teaching profession has become central to improving education systems around the world. In my second chapter, I assess the effectiveness of an ability-based scholarship on attracting top-performing students into teaching majors.

My third chapter is joint work with Miguel Sarzosa and Ricardo Espinoza. We study how free college, a policy that has been gaining momentum in Latin America, affects self-selection into teaching majors. We find that free college decreased the relative returns to pursuing a teaching career, making it substantially less popular among relatively poor high-performing students who now self-select into programs with higher returns. We also find that the reform reduced the academic qualifications of the pool of students entering teaching programs, which can negatively affect long-term teacher quality.

Intended and Unintended Impacts of Government Programs in Agriculture and Education

by

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Chapter 1: Transitioning from Collective to Private Land Rights: Experimental Evidence on Tenure Security, Investments, and Agricultural Production

1.1 Abstract

The most recent stages of land reform programs in developing countries have involved the privatization of collective land. This second stage of the land redistribution process has been implemented with the hope that more complete land rights will increase agricultural investment and productivity and spur economic development. I present evidence from a unique randomized controlled trial of a parcelization program that assigns individual titles to parcels belonging to collective landholdings, previously distributed through the Philippines' Agrarian Reform. Specifically, this paper studies an intermediate stage of the parcelization process, namely the physical subdivision and demarcation of the individual parcels, which entails the participation and agreement of all co-owners along with the placement of cornerstones delineating parcel boundaries. I find that the subdivision deteriorates farmers' tenure security as well as their perceptions of the ability of local governments to effectively enforce property rights. Consistent with this finding, farmers in treated parcels decreased the area planted with annual crops. Despite greater tenure insecurity, subdividing also increases land leases. This latter result is driven by plots owned by farmers with relatively fewer years of tilling experience, suggesting a reallocation of labor to the non-farm

sector among individuals with less farming ability.

1.2 Introduction

Land reforms in many developing countries have assigned land rights to collectives rather than individuals. Collective ownership can hinder agricultural investments and cause inefficiencies in the use of land for several reasons. First, inalienable property rights may discourage land-specific investments by making them highly illiquid (Besley, 1995). Second, collective ownership can hamper agricultural productivity by restricting land market transactions, thus limiting the reallocation of land from less productive to more productive users. Lastly, when co-owners do not engage in collective production but rather separately exploit individual parcels, collective ownership may result in allocative inefficiencies if individuals employ resources in an attempt to secure their ownership rights to their individual parcels (Barzel, Y., 1989)).

Although initially most of these first reforms successfully distributed land to significant portions of rural inhabitants, they ultimately led to low agricultural productivity (see, for example, (De Janvry, A. and Gordillo, G. and Sadoulet, E., 1997) for the case of Mexico). Hoping that more complete land rights (i.e., private property rights) will increase agricultural investments, recent stages of land reforms have sought to privatize previously-distributed collective landholdings (see, for example, (Janvry de et al., 2015), (Gáfaro, 2017)). However, it is not clear whether this change will prove socially beneficial. The answer will depend on how the expected gains, which should be realized mainly through productivity increases, fare against the magnitude of common-pool losses, the size of the associated contracting costs necessary to resolve such losses, and the economic costs of defining and enforcing the new set of property rights (see (Ostrom, E., 1990)).

Since the transition between land rights systems is not instantaneous and may span several years or even decades, it is important to examine the intermediate effects of these changes on economic outcomes. By doing so, we can contribute to our understanding of the conditions under which expanding a modern property rights regime enhances agricultural productivity, particularly when there is already a formal land tenure system in place. We can also assess what factors are instrumental for a more rapid realization of expected benefits, potentially even before the actual ownership documentation is issued. For example, many titling programs spend considerable time and resources implementing high-precision boundary surveys that seldomly these equate to a greater security of rights (Burns et al., 2007). Conversely, simple demarcation activities involving all community members may be enough to significantly increase tenure security and land investments (Goldstein et al., 2018). Lastly, studying transitions between property rights regimes can enhance our understanding of both the types of costs that may arise during these changes, such as transaction costs from increased uncertainty or rent-seeking activities, and how we can mitigate these costs.

In this paper, I examine the causal effects of the transitional stage of privatization of collective-owned land on tenure security, land market transactions, investments, and agricultural productivity. I use data from a randomized controlled trial of the Philippines' Department of Agrarian Reform Parcelization Program (DARPP), which assigns private property titles to parcels belonging to collective landholdings that were previously distributed through the country's Agrarian Reform. Specifically, this paper studies an intermediate stage of the parcelization process, namely the physical subdivision and demarcation of the individual parcels, which entails the participation and agreement of all co-owners along with the placement of cornerstones delineating parcel boundaries.

I find that subdivision reduces farmers' tenure security and erodes their trust in the ability of local institutions to protect their land rights during conflict situations. Certain features of the implementation of this transition are likely driving these results. In particular, the cancellation of the previously-issued collective titles, an overall lack of clarity about the process, and uncertainty about if or when the new titles will be received may give farmers the impression that their land rights are fragile. Given that titles are the primary proof of ownership in this context, farmers may fear that without any legal document, local institutions may be less able or even less willing to protect their land rights. In line with the decrease in tenure security, I find that the area cultivated with annual crops decreases in treated parcels. Interestingly, despite greater tenure insecurity, subdivision also leads to more land leasing. This latter result is driven by plots owned by farmers with relatively fewer years of tilling experience, suggesting a reallocation of labor to the non-farm sector among individuals with less farming ability, for whom the opportunity cost of non-agricultural activities increases relatively as tenure security deteriorates.

This paper contributes to the literature studying the effects of the privatization of collectively-owned land on economic outcomes. Closely related to this study is that of (Deininger, Bresciani, 2001), who provide early evidence on the impact of PROCEDE, the Mexican land regularization program that grants individual land certificates to plots within *ejidos* or agrarian communities. In line with my results, their descriptive findings suggest that PROCEDE is associated with increases in land rentals and participation of certified *ejidatarios*[1] in off-farm activities. However, in contrast with my findings, Deininger and Bresciani also report an increase in tenure security and greater social unity within certified *ejidos*, which are likely the drivers of the increase in land rentals in the context of their study. Implementation differences between the Philippine

[1] Farmers receiving land certificates

Parcelization Program and PROCEDE plausibly explain the distinct impact of these two otherwise similar interventions on tenure security.[2] (Janvry de et al., 2015) also study the impact of PROCEDE and find that the privatization of *ejidos* (i.e., communal land) in Mexico induced urban migration while, at the same time, had little effect on the total cultivated area due to the consolidation of larger farm units. In the same vein, there are (Gáfaro, 2017) findings on the privatization of collective land in Peru. She finds that the privatization program led to an increase in land sales and off-farm activities, and a decrease in agricultural labor. Increased migration outflows from rural to urban areas and greater participation in off-farm activities are both consistent with greater transferability of land rights as a mechanism through which property rights affect occupational choices.

This paper complements the literature studying the effects of property rights on land markets. The available empirical evidence is limited. Most studies focus on land transactions after the formalization of private rights, neglecting the transition phase, and do not provide causal evidence.[3] My results provide experimental evidence of the reactivation of land markets during a property rights regime transition fueled by an increase in tenure insecurity.

This chapter proceeds as follows: Section 1.3 presents background information on the Philippines' agrarian reform and its Parcelization Program, through which collective land is privatized. In Section 1.4.1, I describe the experimental design of this study, the data, and my empirical strategy. Main results are presented in Section 1.5, and Section 1.6 provides some

[2]In contrast with DARPP, PROCEDE has a streamlined process with a pre-specified timeline (12-18 months), involves a majority of community members throughout the process - even those who do not hold collective land rights-, as well as local officials, who are in charge of certifying the procedures.

[3](Zegarra et al., 2003) find that land titling in Nicaragua had no effects on land sales and is only correlated with a negligible increase in land rentals. Similar findings are reported by (Boucher et al., 215) when they evaluate titling programs implemented in Nicaragua and Honduras. In contrast, (Jiron et al., 2001) find a positive correlation between property formalization and land transactions in Peru.

concluding remarks.

1.3 Context

1.3.1 Land Reform in the Philippines

In 1988, the government of the Philippines launched the Comprehensive Agrarian Reform Program (CARP) through its enabling law, Republic Act (RA) 6657, also known as the Comprehensive Agrarian Reform Law (CARL). The objectives of the law were to give ”land to the tiller” and achieve a more equitable distribution and ownership of land (CARL, 1988). CARP targeted all agricultural lands, private and public, across all crops and tenurial arrangements, and imposed a 5-hectare ceiling to all private land owners. Furthermore, land was to be redistributed to Agrarian Reform Beneficiaries (ARBs) (recipients of land) at a maximum of 3 hectares per ARB. To qualify as an ARB, a farmer had to be landless or own less than 3 hectares, and be willing to cultivate the land (CARL, 1988).

Land acquisition took place on a compulsory and voluntary basis (sale of excess private land to government or beneficiaries directly) at fair market value. In total, the reform aimed to redistribute nearly nine (9) million hectares, which corresponds to over 80% of the country’s arable land. As of 2016, a total of 7.26 million hectares had been awarded under CARP. The lead implementing agency, the Department of Agrarian Reform (DAR), had redistributed about 4.7 million hectares of previously private and government-owned land, while the remaining 3 million hectares were public land distributed by the Department of Environment and Natural Resources. The government heavily subsidized the reform by covering land transfer fees and titling costs, foregoing compensation from ARBs who were awarded government-owned land,

and providing a credit subsidy through the Land Bank of the Philippines to ARBs who were previously awarded private land that needed to be repaid in a 30-year period. The reform was to be completed within 10 years, but for a number of reasons - including cumbersome and lengthy processes, and lack of institutional capacity ((Adriano, 1994)) - the implementation of CARP had to be extended and acquisition and redistribution of nearly 1 million hectares was still pending as of 2016 ((De Los Reyes, 2016)).

DAR distributes land through Certificates of Land Ownership Award (CLOAs). Of all the land distributed by DAR as of 2019, 53% had been awarded through individual CLOAs, while the remaining 47% was distributed via Collective CLOAs (CCLOA) ((De Los Reyes, 2016)). These collective titles are ”co-ownership titles”[4] given to groups of individual farmers who were typically not engaged in collective agricultural production, with the promise of later providing individual titles through subdivision of the collective lands. Because the provision of individual CLOAs is a costly and lengthy process, CCLOAS were extensively used during the 1990s to expedite redistribution. However, due to lack of incentives[5] and high costs of individual titling, CCLOA subdivision has advanced at a slow pace during the past decades.

The evidence on the impacts of CARP is mixed. The first generation of studies find positive effects of the land reform on farmers' income (Reyes, 2002), greater increase in inter-generational transmission of human capital, and greater household welfare and productivity ((Deininiger et al., 2000)). However, the improvements mentioned were typically true only in Agrarian Reform Communities (ARCs)[6] where farmers have greater access to complementary agriculture support

[4]Based on DAR AO No. 3 Series of 1993, there are three types of collective CLOAs: 1)co-ownership, 2)farmers' cooperatives, and 3) other forms of farmers' collective organizations.

[5]Until recent years, subdivision of CCLOAs did not count as an accomplishment for DAR, whose performance was only evaluated based on the number of hectares distributed per year.

[6]Geographical clusters with the highest concentration of ARBs and distributed lands that have access to additional agriculture support services, such as post harvest facilities, farm-to-market roads, irrigation, and technical extension

services. More recent studies find smaller and indirect impacts on poverty reduction ((Balisacan, Fuwa, 2003)), null impacts on income ((Gordoncillo, 2012)), and negative impacts on agricultural productivity through loss of economies of scale due to instituted land ceilings (Adamopoulos, Restuccia (2020)).

The most rigorous quantitative evidence on CARP's impacts does not distinguish between individual and collective CLOAs; however, there are some observational studies that have highlighted the potential constraints that CCLOAs may place on credit market access and investment. For instance, ((Bresciani, 2008)) finds that the ownership of land increases access to credit from formal institutions for reform beneficiaries with registered and individual titles, but not for those with CCLOAs titles. In addition, several policy papers cite anecdotal evidence on boundary disputes between co-owners as well as conflicts regarding land management since most of the CCLOA holders did not come from organized farmer associations and did not have experience (or interest) in collective land management ((Ballesteros et al., 2017), (Casidsid-Abelinde, 2017), (Galang, 2020)). Overall, the slow pace at which agricultural productivity has grown in the Philippines compared to other countries in the region has turned the attention of national policymakers to the investment and production bottlenecks that may arise within collectively-owned landholdings.

Although the Philippines' government has been implementing a program to subdivide collectively-titled lands and distribute individual titles to the respective co-owners for the past three decades, it is not until recently that such program has gained momentum.[7] This process of providing individual titles to CCLOA owners is known as parcelization (hereafter referred as the DAR Parcelization Program, or DARPP). There are still over 848,000 hectares - more than 50%

programming.

[7]Under the Support to Parcelization of Lands for Individual Titling (SPLIT) program ((Bank, 2008)), the World Bank has lent over US$400 million to the Government with the aim of contributing to expediting the parcelization process.

of the total CCLOAs that have been awarded - awaiting parcelization ((De Los Reyes, 2016)).

1.3.2 The Department of Agrarian Reform Parcelization Program

The DAR is the lead agency of the Parcelization Program, which is implemented in coordination with the Department of Environment and Natural Resources (DENR), the Land Management Service (LMS), and the Land Registration Authority (LRA). DARPP consists of two main stages:

1. **Validation and subdivision survey:** During this first stage, the DAR verifies that ARBs are tilling their respective agricultural parcels within the collective CLOA and requests their approval to proceed with the subdivision of the landholding. During a *pulong-pulong*, or assembly of ARBs under the collective title, the DAR explains the subdivision process and the rights and obligations of farmers once they receive their individual CLOA titles. Through this participatory process, all ARBs must reach a consensus on their respective parcel borders and the DAR may facilitate dispute resolution in case conflict between co-owners arises.

   If all ARBs agree to have the landholding subdivided and co-owners reach consensus regarding their individual parcel boundaries, the DAR conducts a land survey. The land survey consists of the drawing of a detailed map specifying each parcel's boundaries, its exact area, and the placement of ”monuments” or landmarks physically delimiting the individual parcels. The survey plan is then submitted to the LMS for their technical review and approval. In addition, ARBs are expected to relinquish their collective title documents after the survey subdivision, as these documents need to be cancelled in order for the individual CLOAs to be issued.

2. **Title registration and distribution:** Upon approval of the survey plans, the DAR prepares a Deed of Parcelization for all co-owners to sign, which is then registered with the LRA and the local Register of Deeds. Once registered, the ARBs receive their individual CLOA titles.

The individual CLOA title provided to ARBs at the end of the Parcelization Process seeks to clarify and formalize the different legal rights each farmer has over their individual parcel. Table 1.1 presents the legal differences between individual and collective CLOA titles with respect to a number of rights held by ARBs over their individual parcels. In contrast with CCLOA titles, individual CLOA titles confer ARBs the right to unilaterally decide the use patterns of their own land, the right to exclude others from their individual parcel, and the right to sell, lease, or transfer all or some of these and other rights to a third party.

Table 1.1: Legal Differences Between Individual and Collective CLOA Titles With Respect to the Rights ARBs Have Over Their Individual Parcels

Right	Definition	Collective CLOA Title	Indvidual CLOA Title
Access	The right to enter the individual parcel.	Yes	Yes
Use	The right to exploit the parcel and obtain its products.	Yes	Yes
Management	The right to unilaterally regulate internal use patterns and transform resources within the individual parcel.	No	Yes
Exclusion	The right to unilaterally determine who will have an access right to the individual parcel and how that right may be transferred.	No	Yes
Alienation	The right to unilaterally sell, lease, or transfer* any or all of the rights above.	No	Yes. For compensable parcels, this is true once the land has been amortized.**

* It is possible to transfer co-ownership of a CCLOA to one's children. An individual CLOA title expands the right to transfer the parcel to anyone.
** Non-compensable landholdings were typically government-owned lands whereby ARBs do not have to make amortization payments to LBP. Thus, ARBs on these lands can legally sell or use the land as collateral once they receive the individual CLOA title.

The second stage in the process of obtaining an individual CLOA title may take a considerable amount of time as the participation of other governmental agencies may exacerbate the delay due to coordination issues. While there are no statistics on the average wait time, anecdotal evidence suggests that it could take as long as 27 years ((Bank, 2008)). Although these are outlier cases, the process is indeed lengthy. By the time endline data collection activities of this study took place, only 60% of the CCLOAs assigned to treatment had undergone subdivision and just about 4% of treatment parcels had been given individual CLOA titles, after an average wait time of 20 months (see Section 1.4.1).

Lastly, it is worth noting that along with the individual CLOA title, ARBs whose parcel was previously private land (i.e., compensable parcels) also receive a payment schedule, and a 30-year amortization period commences. If the ARB fails to fulfill three consecutive annual payments, they risk foreclosure by the government. Since ARBs in this study had not yet received their individual CLOA titles, they had not yet begun making these amortization payments. However, they could anticipate them.

1.4 Experimental Design, Data, and Econometric Approach

1.4.1 Experimental Design and Implementation of the First Stage of the Parcelization Program

The experimental evaluation of first stage of the parcelization process involved the random assignment of a baseline sample of 475 collective CLOA titles into a treatment and a control group. The treatment CCLOAs were selected for parcelization to receive individual titles while the control group maintained their collective titles during the study's duration. The selection of field sites and respondents was a comprehensive, multi-step process, beginning with examining provincial-level administrative data on collective titles and ending with on-the-ground validation of ARBs ((Castro-Zarzur et al., 2008)). The titles were selected in conjunction with the DAR and are located in five of the 17 regions of the country, namely Bicol, Northern Mindanao, Davao, Soccsksargen, and Caraga, which made up 34% of the CCLOA land that was awaiting subdivision in 2019.[8] All titles in the sample are eligible for subdivision, and had at least one ARB that was an original CCLOA co-owner who was making direct or indirect use of the land at the time of the on-the-ground validation. The research team excluded titles with more than 30 ARBs (e.g., sugarcane lands) and where all of the original ARBs were either deceased, had permanently migrated, had sold the land, or could not be found. While subdivision in these cases is possible, the added administrative requirements would have delayed the parcelization process beyond the study's timeline.

Sample CCLOA titles were first matched into pairs and then randomized into treatment conditions within pairs. Matching was based on the following characteristics: 1) whether the titled landholding is on compensable or non-compensable land, 2) provincial location, 3) whether the title is within an Agrarian Reform Community (ARC) through which DAR channels support services, and 4) the number of ARBs on the collective title qualified for the study. Once randomized, DAR prioritized subdivision of the study's treatment titles while withholding subdivision from control titles for the duration of the evaluation. The study's ARBs were not aware of the randomized experiment and thus the control group ARBs did not know they were randomly selected to be subdivided only after the study ended.

Matching and randomization of CCLOA titles was carried out in four waves that took place between January 2016 and July 2018 (Figure A.1). This was because the selection of field sites with DAR's regional offices was done in a staggered manner. After selecting CCLOA titles and carrying out the baseline survey in one geographic region, we conducted the randomization of

[8] According to DAR's data on backlogs of CCLOA to be subdivided.

the surveyed titles and moved on to the next region. This resulted in baseline data coming from a rolling survey which spanned for about 2 years. The last matching and randomization wave took place in July 2018, 17 months before the start of endline data collection fieldwork (December 2019).

Implementation in a region started only after CCLOAs in that region had been randomized into treatment and control. Although CCLOAs in this study had been prioritize for parcelization by DAR, there were important implementation delays. In particular, less than 25% of the study's CCLOAs in the provinces of Bukidnon (Northern Mindanao) and Davao Occidental (Davao) had not undergone a subdivision survey by December 2019, when endline data collection activities started.[9] Because of the slow pace of implementation in Bukidnon and Davao Occidental, CCLOAs in the study's baseline sample from these two provinces were not included in the endline survey. Of the original 475 CCLOA titles, 181 were lost when we excluded Bukidnon and Davao Occidental. Although dropping these titles from the study's sample reduces the statistical power to detect effects of the intervention, it should not pose a threat to internal validity since the randomization was stratified by province.

The final sample of this study consist of 570 individual parcels within 294 collectively-titled landholdings, which were matched into 147 randomization pairs. In 54% of the 294 CLOAs we gathered information about between two and 10 individual parcels, while in the remaining 46% of CCLOAs we collected information for just one of the individual plots. Although this latter group of CLOAs has more than one individual parcel, the co-owners of the other plots were disqualified from the study for various reasons including the death of the original ARB and the informal sale

[9] Most treatment titles in Bukidnon had not undergone subdivision because the survey equipment usually used by the staff to administer the land surveys was reportedly often broken. Davao Occidental had made little progress due to a reported lack of funding for parcelization.

of the plot, which typically result in longer parcelization times.[10]

Although the pace of the implementation was relatively faster in the 294 CCLOAs left after dropping the field sites in Bukidnon and Davao Occidental, compliance was still imperfect. By the time endline data collection activities had started, just about 60% of the CCLOAs assigned to treatment had undergone a subdivision survey while 11% of the CCLOAs assigned to the control group had been subdivided (Table A.2). The progress of the intervention resembles the slow pace that the DAR's Parcelization Program has had in the country in the last decades. Capacity constraints of the implementing agencies, coordination issues, and lack of funding are the typical factors accounting for the delays. Co-owner conflict during the *pulong-pulong* was not an important driver of the holdup since only nine ARBs across eight different collective titles reported land disputes during this stage, almost all of which involved border disputes that were resolved ((Castro-Zarzur et al., 2008)).

In addition to implementation delays, ARBs in treatment CCLOAs whose land was subdivided reported low levels of information and involvement in the process. Only 55% of farmers whose parcels underwent a subdivision survey reported receiving an invitation to attend the *pulong-pulong*to discuss the parcelization of their landholding. Moreover, while almost all of those who were invited attended the assembly, farmers were not provided with sufficient information about the process and its timeline during the meeting. Among ARBs who attended the *pulong-pulong*, only 13% could recall receiving some information about the duration of the parcelization process, and just a third of them believe they could access more information if they desired.

[10]The parcelization process has additional paperwork and verification requirements whenever the original ARB is not alive or is no longer connected to the land.

1.4.2 Data and Sample Description

Overall, 556 ARBs in 294 CCLOAs and 147 randomization pairs were interviewed at baseline, with information on 570 parcels. At the farmer and household levels, this data contains information on household demographics, food expenditures, assets, income, savings, and credit. The plot modules contain a rich set of data on self-reported parcel characteristics, self-reported perceived likelihoods of confiscation by different types of agents (i.e., Government, Neighbor, Last Owner, Other), information on some investment decisions and parcel leases, and data on the types of crops cultivated - however, I do not have, information on parcel output and agricultural productivity at baseline. During the 2019 endline fieldwork we did collect detailed data on agricultural measures at the parcel level - that is, for each parcel within a CCLOA - allowing me to construct output and productivity estimates. In addition, endline data also contains a richer set of measures on tenure security and trust in the capacity of institutions to enforce property rights and protect farmers' ownership claims when in conflict. This set of outcomes includes perceived likelihoods of parcel confiscation by different types of agents, and perceived efficacy of government institutions in protecting farmers' ownership of their parcels under three hypothetical conflict scenarios: with their neighbors, with the government, and with a private company.

Balance checks at the plot-level confirm baseline balance across treatment and control parcels for a range of key self-reported and observable characteristics prior to the implementation of the program (Table A.8). Roughly 40% of the plots in the sample were acquired through settlements, nearly 40% were acquired through Voluntary Offers of Sale (VOS), 11% were previously Government Owned Lands (GOL), and the remaining proportion corresponds to lands acquired through several other redistribution mechanisms. The average parcel at baseline had an extension of 2.4 Ha, which is below the 3 Ha ceiling imposed by CARP. A little less than 40% of the plots had an irrigation system, and over half of them are located in uplands, where agricultural exploitation can be more challenging and farmers are typically engaged in subsistence food production.

In terms of baseline tenure security, 4.8% of the plots had had an ownership dispute in the last two years. Consistent with this, the perceived likelihoods of arbitrary confiscation of the parcel by different agents were generally low. In about 10% of the parcels, ARBs said that confiscation by the last owner was somewhat or very likely; while confiscation by neighbors was reported to be somewhat or very likely in 13% of parcels. At the same time, in over 90% of the parcels ARBs believe that they were somewhat or very likely going to be able to transfer ownership to their children. Interestingly, however, farmers perceived the government as the biggest potential threat to their security of land tenure, with ARBs declaring that government confiscation was somewhat or very likely in over 30% of the plots.

In contrast with what is commonly assumed about CCLOAs and land tenure security, ARBs in my sample enjoyed relatively high levels of tenure security despite the fact that just 46% of their plots had legally valid ownership documents (Table A.8).[11] Furthermore, although in this context CCLOA titles are the only legally valid proof of ownership, having such documents is not significantly associated with the likelihoods of parcel confiscation by neighbors, previous owner, or other agents (Table A.1). Having a land title is only associated with a decrease in the likelihood of government confiscation (-0.25 units or 13%) and an increase in the likelihood of transferring the land to children (0.19 units or 5%). This suggests that though appropriate

[11] Many farmers anecdotally report losing CCLOA titles over time, or during typhoon floods, which are common throughout the Philippines.

ownership documentation may be important for tenure security, it is mostly helpful when facing confiscation threats by the government, and there must be other factors ensuring recognition of property rights for parcels within Collective CLOAS.

ARBs in my sample were on average 54 years old at baseline, had over 40 years of farming experience, and owned 1.6 plots. (Table A.9). 70% of them are men and almost 50% have some high school education or above. Balance checks at the ARB-level show some moderate imbalances between the experimental groups. Control ARBs are statistically more likely to have secondary or higher education, while treatment farmers have more years of farming experience, though they own slightly fewer plots. The standardized mean differences of these statistical imbalances range between 0.13 and 0.18, and are all below 0.2 standard deviations, suggesting that they are likely not meaningful.

The mean household size is 4.7 members, which is close to the national average (4.2 members (PSA, 2018)). At baseline, 65% of households had at least one individual employed in waged labor, over 30% owned a business, and more than 60% received unearned income such as a pension or remittances. The average per capita household income was 5,394 PHP, similar to the national average in 2015 - 5,238 PHP or USD$ 104 (PSA, 2015). Waged labor was the most important source of earnings and corresponded to about 70% of household income, indicating that revenues from own agricultural exploitation, included in income from own businesses, were not as relevant for the average household as were revenues from other economic activities. This combined evidence suggests that agricultural exploitation of parcels was not the most important source of income for families in this study and about 70% of households may had been using their land for subsistence agriculture.

Average monthly per capita food expenditures were around 1,604 PHP and corresponded to

30% of the average monthly per capita income. In terms of savings and credit, at baseline, 43% of households had savings, though household per capita savings were small XX. About 20% of households periodically borrowed from agricultural traders, though access to more formal sources of credit was not common: less than 10% of households belonged to a credit cooperative and just 6% had applied for a commercial bank loan in the past two years.

Balance checks also show some statistical differences between experimental groups at the household level. Overall, control households are slightly wealthier: they had higher per capita food expenditures and larger per capita income, driven by larger unearned income and higher revenues from own businesses. In addition, control households were also better off in terms of asset ownership. Nonetheless, although these differences are significant they are also small in magnitude. The standardized mean differences of these imbalances range between 0.13 and 0.18, and are all below 0.2 standard deviations, therefore suggesting that they may not be meaningful.

1.4.3 Econometric Approach

I estimate the impact of the CLOA subdivision survey on parcel measures of tenure security, investments and land transfers, and agricultural production through the following specification:

$$y_{icj} = \alpha_1 + \beta_1 T_{cj} + \gamma_1' x_{icj} + \delta_j + \epsilon_{1icj} \tag{1.1}$$

where y_{icj} is the outcome for parcel i in CLOA c, which is part of randomization pair j, T_{cj} is an indicator variable that equals one if CLOA c was randomly selected to be subdivided, x_{icj} is a vector of baseline controls at the household and parcel levels included to increase precision, δ_j are randomization pairs fixed-effects, and ϵ_{icj} is a random error component. The random assignment of the program at the pair-level allows me to identify β_1, the parameter of interest. I exploit the within-pair random assignment of the program to recover the intention-to-treat effect (ITT) of the subdivision survey. All standard errors are clustered at the CLOA level to account for the clustered design of the randomization (i.e., assignment to treatment is perfectly correlated within CLOAs) .

Due to imperfect compliance from the slow pace of the implementation, the treatment estimates resulting from a simple fixed-effects difference estimate like that in Equation (1.1) will likely underestimate the effects for parcels that underwent subdivision survey. Following (Imbens, Angrist, 1994), I estimate the Local Average Treatment Effect (LATE) on those parcels that went through a CLOA subdivision survey- the first stage of the parcelization. To do so, I estimate impacts through an instrumental variables approach:

$$y_{icj} = \alpha_2 + \beta_2 S_{cj} + \gamma_2' x_{icj} + \delta_j + \epsilon_{2icj} \tag{1.2}$$

$$S_{cj} = \phi_1 + \phi_2 T_{cj} + \gamma_3' x_{icj} + \delta_j + \epsilon_{3icj} \tag{1.3}$$

Equation (1.3) is the first stage regression where the indicator variable S_{cj}, which equals one if CLOA c in pair j has undergone subdivision, is run against the treatment indicator T_{cj}, the randomization pairs fixed-effects δ_j, and baseline controls x_{icj}. β_2 is the LATE parameter, which is estimated in the second stage regression (Equation (1.2)).

1.5 Results

1.5.1 Attrition

Because of the pair-wise design of this randomization, attrition in this study can occur directly or indirectly. Direct attrition happens when a parcel present at baseline is missing from the endline sample. Indirect attrition occurs when a parcel present at endline is dropped from the estimation sample because the parcel(s) of its corresponding randomization CLOA pair were lost to direct attrition. Since an important proportion (32%) of the CLOA randomization pairs in this study contain just two parcels, one in the treatment condition and the other in the control group, if one of these two parcels is lost to direct attrition, we also lose the other parcel indirectly because the pair can no longer be included in the analysis sample for the impact estimations.[12] I consider both types of attrition in this analysis, direct and indirect, when looking at attrition rates across treatment groups.

At the parcel level, direct attrition rates vary by the type of outcome (Table 1.2) and are generally moderate, ranging from 14% regarding land transfer outcomes, to 19% for agricultural output, and to almost 24% with respect to tenure security and trust (Column 4, Table 1.2). In particular, perceptions of tenure security and trust were only asked to the owner of the parcel. Therefore, although we may not have information on these outcomes because of the unavailability of some owners at endline, we may still have data on land transfers and agricultural output - for example, if the owner has leased out her parcel and we interviewed the lessee. Similarly, for agricultural output, we have missing data at endline for parcels in which we could not interview

[12] The identification of β_1 and ϕ_2, the parameters of interest in Equations (1.2)and (1.1), entail a comparison of the value of the outcome at the pair-level.

at least one of the land tillers (i.e., the ARB and/or lessees), but we may still know if the parcel was leased out or not. Effective attrition rates at the parcel level, which take into account both direct and indirect attrition are higher and range from 20% for land transfer outcomes to 34% for outcomes related to tenure security and trust (Column 6, Table 1.2). Importantly, however, the differences in direct attrition rates (Column 5) and effective attrition rates (direct plus indirect, Column 6) between treatment and control parcels "T-C Diff" across all types of outcomes are not statistically significant, suggesting that treatment and control parcels left the sample at the same rate.

Table 1.2: Attrition (Parcels)

	Obs Baseline	Obs Endline	Eff. Obs Endline	Endline Sample Attrition (%)	T-C Diff	Eff. Endline Sample Attrition (%)	T-C Diff
Type of Outcomes							
Agricultural Output	570	462	415	18.9	-2.9	27.2	-3.6
Tenure Security and Trust	570	435	375	23.7	0.1	34.2	-0.3
Land Transfers (Lease Out)	570	490	459	14.0	-1.9	19.5	-0.1

Note: CLOA-clustered standard errors. T-C Diff corresponds to the difference between the treatment and control groups. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

It is worth noting that effective attrition rates at the parcel level tend to be mechanically similar between treatment conditions because of the pair-wise randomization.[13] In that sense, direct attrition rates give us more information on whether the propensity to leave the sample was equal for parcels across treatment conditions. I complement this analysis by assessing pair-level attrition rates and testing if the number of pairs lost to direct attrition is statistically equal across treatment groups.

Table (1.3) shows that parcels in the endline sample come from 145 of the 147 different

[13] In other words, regardless of whether we lose a pair due to direct attrition from a treatment parcel or from a control parcel, we would also lose the corresponding paired parcel. Therefore, we do not expect to see differences in the effective attrition rates between treatment conditions.

randomization pairs in this study. The number of effective pairs at endline was lower and corresponded to those pairs for which I have at least one parcel in each treatment condition. Columns 4 and 5 show the number of randomization pairs that were lost due to *direct* attrition of treatment and control parcels, respectively, with several randomization pairs being lost due to direct attrition from both their treatment and control units. Across all types of outcomes, I cannot reject that the number of randomization pairs lost to direct attrition of control parcels is equal to number of pairs lost to direct attrition of treatment parcels. Overall, the evidence on attrition rates suggest that although these are moderate, they are not statistically different between treatment groups, neither at the parcel-level nor at the pair-level.

Table 1.3: Attrition (Randomization Pairs)

	Pairs at Baseline	Pairs at Endline	Eff. Pairs at Endline	Pairs Lost to Treatment Attrition	Pairs Lost to Control Attrition	T-C Diff
Type of Outcomes						
Agricultural Output	147	145	113	20	16	4
Tenure Security and Trust	147	104	104	43	43	0
Land Transfers (Lease Out)	147	146	122	12	14	2

Note: Across all types of outcomes, several pairs were lost due to direct attrition from both the treatment and control units. CLOA-clustered standard errors. T-C Diff corresponds to the difference between the treatment and control groups. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.05$.

I next assess differential attrition by testing whether treatment and control parcels and ARBs in the endline sample were statistically different at baseline across several key variables. Because the number of non-missing observations at endline vary by the type of outcome, I conduct baseline balance tests for three different endline samples: 1) the sample with non-missing data on agricultural output, 2) the sample with non-missing data on land transfers, and 3) the sample with non-missing information on tenure security and trusts outcomes. The evidence presented in Tables (A.10) - (A.12) suggest that the balance between treatment and control parcels has been preserved at endline across all three samples. Out of the 20 variables, only 2 (parcel is on an upland and likelihood of neighbor confiscation) are significantly different between treatment and control parcels across the three samples. Although treatment parcels in the endline sample are less likely to be on uplands and had lower levels of perceived likelihood of neighbor confiscations at baseline, the standardised mean differences between experimental groups are not large (¡ 0.2 SD). Nonetheless, to control for these imbalances, I include the number of years as primary tiller and the number of plots owned in all impact estimation.

I replicate the previous analysis for ARB and household level variables and characteristics. I find that baseline balance of the endline sample at the ARB-level remains. There are no meaningful baseline statistical differences between treatment and control farmers in any of the endline samples considered (Tables (A.13) - (A.15)). At the household level, all but one of the baseline statistical differences between the experimental groups at endline were already present in the baseline sample. Therefore, these differences are not a consequence of differential attrition but rather of initial imbalances. Similar to what was reported for the baseline sample, there is some evidence suggesting that endline households in the control group are slightly wealthier.

Overall, the attrition analysis suggests that while there was moderate attrition at endline, it was balanced across the experimental groups. There are not any statistical differences between treatment and control ARBs in the endline sample. At the parcel-level, there are a few differences (in 2 out of 20 variables) between treatment and control plots across key characteristics and outcomes. Lastly, at the household level, only one of the imbalances between the experimental groups at endline was not already present at baseline. Importantly, all of these differences are small to moderate in magnitude (¡ 0.2 SD), hence they are likely not meaningful. Nonetheless, I adjust all impact estimations by including some of these as controls in order to address any potential concerns. Specifically, I include an indicator for the parcel being located in an upland, an indicator for whether the household has its own business, and per-capita household income.[14]

1.5.2 Effects on Tenure Security

I start by looking at the impact of CLOA subdivision on parcel disputes in the last two years. I assess the effect of the intervention on the likelihoods of having ownership-related disputes, conflicts regarding land use, and disputes arising from encroachments. Although I do not find any Intent-to-Treat impacts, the LATE estimates indicate that the intervention decreased ownership disputes by 8.1 percentage points (pps) or about 95%. Such impact is in line with what the *pulong-pulong* and land demarcation activities aim to achieve: facilitate conflict resolution by reaching a consensus on each parcel's borders. Interestingly, I find null impacts of the intervention on the propensity to have any type of dispute. Such a null effect arises mainly because: 1) the significance of the reduction in ownership disputes is not strong, 2) the intervention increases the likelihood of having encroachment conflicts. Although this latter effect is not significant at conventional levels (p=0.101), the direction of the impact on encroachment disputes is opposite to the effect on ownership conflicts and thus reduces the impact of the intervention when we consider both types of land conflicts together.

[14] I do not control for per capita food expenditures, per capita unearned income, asset index, or the likelihood of neighbor confiscation because they have several missing values at baseline and their inclusion would significantly reduce the analysis sample. However, the pairwise correlations between per capita household income and per capita food expenditures, per capita unearned income, and the asset index conditional on the randomization pairs are 0.59, 0.58, and 0.59 respectively. Therefore, by adjusting impact estimates by per capita household income, I am also partly and indirectly controlling for these other variables.

Table 1.4: Effects of Subdivision Survey on Parcel Disputes in the Last Two Years

			ITT		LATE	
	Obs	Control Mean	Coef.	Std.Err.	Coef.	Std.Err.
Parcel Disputes in the Last 2 Years:						
Any Disputes	374	0.111	-0.026	(0.030)	-0.057	(0.055)
Ownership Dispute	374	0.084	-0.037	(0.023)	-0.081*	(0.044)
Land Use Dispute	374	0.021	-0.001	(0.016)	-0.003	(0.030)
Encroachment Dispute	374	0.021	0.024	(0.017)	0.053	(0.033)

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

I continue by exploring the effects of the intervention on perceived measures of tenure security. This set of outcomes complements the information on land disputes in two ways. First, capturing ARB's expectations of their future tenure security, which could be different from their past land conflicts, is essential to understanding their present land investment decisions. Second, these outcomes provide additional information on various sources of tenure insecurity and allow us to assess whether the intervention changed tenure threats differently. Specifically, I look at ARB's perceptions of their security from eviction, their own ability to restrict access to their parcel, likelihoods of confiscation by different agents (government, neighbors, and others), and their worry about losing ownership of their parcels. Responses to these questions were all given on a 5-point Likert scale. In addition, I created an overall tenure security index combining information from all seven outcomes.

I find that the intervention decreases ARB's perceived ability to restrict access to their parcels, decreases their security from eviction, and decreases the overall index of tenure security (Table 1.5). The LATE estimates indicate that the intervention decreases farmers' perceived ability to restrict access to their parcels by about 0.35 likert points ($\sim 7\%$) and decreases their sense of security from eviction (-0.395 points or over -8%). In addition, although not statistically significant, the point estimate for the perceived likelihood of being able to transfer the parcel to their children decreases after subdivision, while the perceived likelihoods of confiscation by neighbors, the government, or others increase. Furthermore, farmers in treated parcels are more likely to worry about losing ownership of their land. Consistent with the direction of all coefficient estimates, which point towards greater tenure insecurity, the impact of subdivision on the index summarizing these measures is significant and corresponds to an almost a six-fold decrease in tenure security relative to the control mean (0.093).

I also explore impact heterogeneity by farmer's gender (Table (A.3)). The results suggest that tenure security in female-owned plots was affected to a greater extent than in male-owned parcels. In addition to decreasing security from eviction and the ability to restrict access to one's own parcel, the intervention also significantly increases the perceived likelihoods of confiscation by both the government and neighbors in female-owned parcels only. Moreover, subdivision reverses the existing gender gap (i.e., gender gap observed for the control group, which corresponds to the coefficient on the $female$ variable) as threats of confiscation by the government and neighbors are *lower* in control women-owned parcels compared to control male-owned parcels.

I also investigate heterogeneous impacts between compensable and non-compensable parcels. During semi-structured interviews at baseline, some farmers reported concerns over amortization payments when referring to tenure security. As mentioned previously, if farmers fail to fulfill three consecutive annual payments, they risk foreclosure by the Land Bank of the Philippines. In practice, foreclosures of compensable parcels within collective CLOAs are extremely rare as they are hard to implement[15]; however, with subdivision and individual titling, foreclosure evictions would be easier to carry out and farmers of compensable plots could now be anticipating this as

[15] Parcels within a collective CLOA do not have individual titles and their precise areas are usually not known.

a credible threat. The results indicate that collective CLOA subdivision did not make owners of compensable plots significantly less tenure-secure than those of non-compensable plots (Table (A.4)), therefore it is unlikely that these impacts are being solely driven by more credible threats of foreclosure in the event of amortization default. Meanwhile, ARBs in non-compensable plots that underwent subdivision do have statistically higher levels of perceived likelihoods of confiscation by neighbors than owners of compensable plots that were subdivided, although it is unclear to me what may be driving this difference.

Table 1.5: Effects of Subdivision Survey on Tenure Security

			ITT		LATE	
	Obs	Control Mean	Coef.	Std.Err.	Coef.	Std.Err.
Ability to Restrict Access to Parcel	374	4.763	-0.190**	(0.086)	-0.417**	(0.162)
Secure from Eviction	374	4.695	-0.197*	(0.103)	-0.431**	(0.184)
Worried about Losing Ownership	374	3.053	0.146	(0.178)	0.320	(0.331)
Likelihood: Transferring to Children	374	4.717	-0.008	(0.069)	-0.017	(0.127)
Likelihood: Neighbor Confiscation	374	1.874	0.122	(0.138)	0.267	(0.259)
Likelihood: Govt. Confiscation	374	2.058	0.097	(0.145)	0.212	(0.270)
Likelihood: Confiscation by Other	374	1.626	0.120	(0.131)	0.262	(0.242)
Principal Components Index	374	0.042	-0.286**	(0.144)	-0.627**	(0.271)

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.05$.

A critical aspect of tenure security is the trust that individuals have in the enforcing capacity of the institutions in charge of administering property rights. The Barangay Council, equivalent to a village or neighborhood assembly, is the lowest level of elected government in the Philippines and is in charge of local law enforcement and conflict settlement. I asses the impact of the intervention on the perceived efficacy of the Barangay Council in protecting farmers' ownership of their parcels under three hypothetical conflict scenarios: with their neighbors, with the government, and with a private company. CCLOA subdivision significantly decreases individual's trust in the Barangay Council's ability to safeguard parcel property rights with respect to all types of dispute opponents (Table (1.6)). The negative point estimates of these impacts range from around 12% in the case of conflict with a neighbor to 7% for disputes with the government. Overall, the index combining all trust measures shows an even larger negative impact of an approximately fourteen-fold decrease in farmers' confidence in local law enforcement to effectively protect property rights during conflict situations.

Regarding heterogeneity in the impact of subdivision on trust in the Barangay Council's effectiveness to protect parcel property rights, I find that while there are no differential effects between male and female-owned plots, there are significant differences between compensable and non-compensable parcels. Overall, distrust in the Barangay Council's ability to protect land property rights under different conflict scenarios is larger for compensable plots.

Table 1.6: Effects of Subdivision Survey on Trust in Barangay Council Effectiveness in Protecting Property Rights to Parcel under Hypothetical Conflict

			ITT		LATE	
	Obs	Control Mean	Coef.	Std.Err.	Coef.	Std.Err.
When in Dispute with Neighbor	374	4.628	-0.315***	(0.091)	-0.690***	(0.167)
When in Dispute with Govt	374	4.426	-0.228**	(0.113)	-0.500**	(0.207)
When Dispute with Private Company	374	4.665	-0.207***	(0.079)	-0.454***	(0.144)
Principal Components Index	374	0.084	-0.463***	(0.153)	-1.015***	(0.279)

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Overall, the evidence suggests that land subdivision and demarcation activities have contrasting impacts on tenure security. On the one hand, the intervention reduces the incidence of land ownership disputes, possibly through facilitating consensus of parcel boundaries among ARBs within the same CLOA. On the other hand, it worsens perceptions of tenure security across several dimensions. Farmers in treated parcels feel less able to restrict access to their parcels and less secure from eviction. In addition, female farmers have also experienced an increase in their perceived threats of confiscation by the government and neighbors. Lastly, subdivision also results in greater distrust in the ability of local government institutions to effectively protect land property rights, an impact which is mostly driven by compensable parcels.

1.5.3 Effects on Land Transfers

Parcel leases are relatively common at endline, with over 15% of the plots being partially or totally leased out. The intervention increases the likelihood of leasing out the land and the impact is significant regardless of whether we consider intra-family leases (i.e., Relatives=1) or not (i.e., Relatives=0), suggesting that treated farmers are also renting-out their parcels to people who are not in their immediate family (Table (1.7)). In particular, subdivision increases the probability of renting out the parcel to someone in the family by 5.4 pps or about 36% with respect to the control mean (ITT impact). The LATE corresponds to an increase of over 75% in the probability of leasing out the parcel, which is significant regardless of whether we consider land leases between family members or not. Lastly, I also find that subdivision did not prompt land sales, which are relatively rare with less than 1% of parcels being sold by endline.

Table 1.7: Effects of Subdivision Survey on Land Transfers

			ITT		LATE	
	Obs	Control Mean	Coef.	Std.Err.	Coef.	Std.Err.
Parcel Leased Out (Relatives=1)	458	0.150	0.054*	(0.031)	0.118**	(0.060)
Parcel Leased Out (Relatives=0)	458	0.132	0.046	(0.030)	0.099*	(0.056)
Parcel Sold	458	0.009	0.000	(0.008)	0.001	(0.014)

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Increased land leasing transactions are usually interpreted in the literature as indicative of tenure security. Indeed, one of the mechanisms through which economic theory models the impact of well-defined property rights on agricultural productivity is precisely thorough improved transfer rights. Specifically, enhanced transfer rights may make farmers more willing to lease land out to more productive agents if they are more confident they can reclaim it once the lease ends. Because of this, an increase in land renting is usually associated with improvements in tenure security which have taken place through better defined property rights. In this context, even though farmers do not yet have their individual land titles, it is possible that the land survey and subdivision activities facilitated consensus regarding each parcel's borders. Clear border delimitation is usually a pre-requisite for leasing out a plot as 1) it is plausibly easier to rent out a land of known extension, and 2) potential lessees would likely be more willing to rent out a parcel whose limits are clear and unanimously recognized by its immediate neighbors. Therefore, it is possible that the increases in land leases are a consequence of an improvement in the recognition of the individual parcel limits, which is also consistent with the significant reduction of ownership disputes previously documented.

Nonetheless, the overall evidence on the impact of the intervention on tenure security is conflicting at best. On the one hand, subdivision decreases the likelihood of ownership-related disputes; on the other hand, it aggravates the perceptions of tenure insecurity and effective enforcement of land rights. How can farmers be more likely to lease out their land if their perceived tenure security has significantly worsened? Why are farmers more likely to rent out their parcels if they have less trust in the ability of local institutions to protect their property rights in the event of a conflict? To address these questions I first test the robustness of these results and then to proceed to investigate heterogeneous impacts of the intervention on land leasing to examine the potential mechanisms that could be driving these findings.

I test the robustness of this result by considering the following two possibilities:

1. The positive impact on land leases is solely being driven by parcels for which we do not have data on tenure security.

2. The impact estimates of the intervention on the tenure security outcomes are negatively biased (i.e., they overestimate tenure insecurity).

Since the sample of parcels for which we have tenure security data at endline (374 plots) is a subset of the larger sample for which we have information on land transfers (458 plots), it is possible that the impact I find on land leases is not applicable to the smaller tenure security sample. If this is the case, then I should not find a positive effect of the intervention on land leases if I restrict the estimation sample to the tenure security sample. If this assumption is true, then I would simultaneously see mixed impacts of the intervention on tenure security and a non-positive (null or negative) impact on land leases. However, Table A.16 shows this is not the case. Even when I impose this restriction, I find the LATE estimates are positive and significant indicating that subdivision increases the likelihood of leasing. In other words, the intervention increases both land leases and tenure insecurity.

The other possibility I consider is that the impact estimates of the effect of subdivision on tenure security are negatively biased thus resulting in an over-estimation of tenure insecurity.[16] For this to be true, two conditions should jointly hold:

1. **Treatment parcels that have been leased out are more likely to have missing information on tenure security outcomes than control parcels that have been rented out.**

[16]Note that I have already provided some evidence against this possibility. The attrition analysis (Section 1.5.1) indicates that baseline balance between treatment and control parcels has been preserved in the endline tenure security sample. This implies that comparing treatment and control parcels in the tenure security sample at endline is methodologically valid and should provide us with unbiased estimates of the effect of the intervention.

2. **The relationship between the probability of leasing out a parcel and tenure security is non-negative and monotonic** such that as tenure security increases, the likelihood to lease out land never decreases. This means that we would expect that parcels enjoying higher perceived tenure security are also more likely to be leased out.

Using the land transfers sample (458 parcels), I assess whether Condition (1) holds by estimating Equation (1.4) below via OLS and IV.

$$MissingTS_{icj} = \omega_0 + \omega_1 T_{cj} + \omega_2 LeasedOut_{icj} + \omega_3 T_{cj} * LeasedOut_{icj} + \delta_j + \epsilon_{icj} \quad (1.4)$$

where $MissingTS_{ij}$ is an indicator that equals one if we have missing values for the tenure security outcomes of parcel i in CCLOA c and randomization pair j, and zero otherwise. T_{cj} is an indicator variable that equals one if CCLOA c was randomly selected to be subdivided, $LeasedOut_{icj}$ is an indicator that equals one if parcel i was leased out, $T_{cj} * LeasedOut_{icj}$ is the interaction of the two indicator regressors, and ϵ_{icj} is a random error component. I estimate the regression both with and without randomization pair fixed-effects δ_j. In the IV specifications, I instrument having been subdivided with assignment to treatment (i.e., T_{cj}), and the interaction between subdivision and leasing with the interaction between assignment to treatment and leasing (i.e., $T_{cj} * LeasedOut_{icj}$).

I find that leasing out a parcel increases the chances of having missing data on tenure security outcomes (Tables (A.17) and (A.18)). This is expected as the tenure security questions were only asked to the owner herself, hence it was less likely to find her tilling her parcel during the endline data collection if she had rented it out. This is not necessarily problematic provided that the increase in the probability of missing values for tenure security outcomes due to leasing is equal for treatment and control parcels. However, I find some evidence in support of condition 1 suggesting that leased-out parcels in the treatment condition are more likely to have missing data on tenure security outcomes than leased-out parcels in the control group (Column 4 in Table (A.17), and Columns 2 and 4 in Table (A.18)).

I now turn to evaluate whether condition 2 holds. The idea is that if the relationship between the probability of renting out a parcel and tenure security is non-negative and monotonic, then average levels of tenure security in leased parcels cannot be worse and may be better compared to all parcels. If this is the case, then it may be possible that the impact estimates of the intervention on tenure security outcomes would be negatively biased because the missing data on these outcomes would mostly come from treatment parcels plausibly enjoying higher levels of perceived tenure security.

I explore the association between land leasing and tenure security using baseline data and consider the different sources of tenure insecurity - government, neighbors, and others - separately. Descriptive results in Table A.7 indicate that a higher perceived likelihood of confiscation by the government is associated with an increase in the propensity to rent out land, everything else constant. Conversely, increases in the likelihoods of confiscation by neighbors or others are associated with a decrease in the probability to lease out parcels. Although none of the coefficients on the tenure security variables are significant[17] and these associations are not causal,

[17] Although the design of this study does not allow me to disentangle and separately estimate the causal effect that the multiple sources of tenure security have on the probability to rent out a parcel, both theoretical models and empirical evidence have identified several aspect of tenure security as primary determinants of the decision to lease out land.

they imply that threats from different actors affect land leasing decisions distinctly, and that the relationship between land leases and tenure security may not always be non-negative and monotonic. [18]

Although these results need to be taken with a grain of salt, they may provide suggestive evidence indicating that treatment plots that were leased out at endline and for which we do not have tenure security data do not necessarily enjoy higher levels of tenure security across all dimensions. In particular, treatment ARBs who rented out their plots may have higher perceived confiscation threats from the government. A priori, one would think that greater tenure insecurity from the government may increase land leases:

1. Among plots owned by individuals who are relatively less productive in agricultural jobs, for whom the opportunity cost of leasing out their land is relatively lower.

2. If the probability of confiscation by the lessee is low. This is likely to occur in small, close-knit communities with very strong, well-defined formal or informal property rights.

3. If the lessee is in a better position to defend the parcel from government confiscation. Strong lessees could allocate more resources to increasing tenure security by, for example, increasing the amount of labor allocated to the parcel which serves both production and security purposes, making investments to delimit borders (e.g., tree planting, fencing), or leveraging political connections to prevent confiscation.

I explore whether my data supports these three conjectures by examining heterogeneous impacts of subdivision on parcel leases by ARBs' farming experience, gender, and connections

[18] The coefficients on the other variables in this regression are in line with what one would expect: more farming experience (number of years as tiller) is associated with a significant decreases in the probability of leasing out a parcel, while being a farmer with multiple plots or a female ARB is associated with a decrease in the likelihood to rent out land.

with the local barangay council. Consistent with Conjecture 1, I find that the increase in land leases among the treatment group is primarily being driven by parcels owned by farmers with relatively less farming experience (i.e., those with less than the median number of years of farming experience). Treatment parcels owned by ARBs with more than 27 years of farming experience are about 30 pps less likely of being leased out than treatment parcels from relatively less experienced farmers (Table A.19).

I then try to assess whether there is evidence in support of Conjecture 2 by exploring heterogeneous impacts on land leasing by the gender of the ARB, but did not find any significant results. Since treatment parcels owned by female farmers experienced a significant increment in the likelihood of confiscation by neighbors, we may expect land leases to be less common for this group of parcels. However, these plots also saw a significant increase in perceived confiscation threats by the government, which may in turn increase the propensity to rent them out.

Lastly, I examine if there is (indirect) evidence for Conjecture 3 by looking at whether having closer connections with the local barangay council differentially affects the probability to lease out treatment parcels, but do not find any significant results.

1.5.4 Effects on Investment Decisions

I also investigate impacts of subdivision on agricultural investment decisions and find that treated farmers have significantly reduced the area of their parcels devoted to the cultivation of annual crops.[19] The LATE estimate indicates that, on average, treated plots currently have 0.26 fewer hectares planted with annual crops compared to their control counterparts (Table (1.8)), a decrease of nearly 60% in the area devoted to these crops. This decrease in annual

[19] Rice and Corn are the most common annual crops in my sample

crop cultivation has not been accompanied with an increase in the area devoted to tree crops or left to fallow for productivity reasons (i.e., with the intention of planting it later). Compared to control ARBs, treated farmers did not plant significantly more trees in their parcels during the past 12 months and were over one-fold less likely to be fallowing their plots[20] (for productivity reasons) by endline. This combined evidence suggests that treated parcels are presently being less intensively exploited than control plots. Furthermore, the decrease in the total area cultivated with annual crops in subdivided parcels is consistent with a decrease in the likelihood of engaging in agricultural investments resulting from increased tenure insecurity.

Table 1.8: Effects of Subdivision Survey on Investment Decisions

			ITT		LATE	
	Obs	Control Mean	Coef.	Std.Err.	Coef.	Std.Err.
Area Cultivated with Annual Crops	414	0.484	-0.102*	(0.061)	-0.216*	(0.115)
Fallowing (entire or portion of) Parcel	414	0.064	-0.024	(0.019)	-0.052	(0.034)
Number of Trees Plantes Last Year	414	47.724	5.686	(9.789)	12.112	(17.555)

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

1.5.5 Effects on Agricultural Production

Now turning my attention to agricultural production, I find that CLOA subdivision did not affect farmers' decisions to engage in agricultural production (extensive margin) as shown by the insignificant coefficient on the output indicator for *All Crops*. This suggests that the additional land leases in the treatment group are mainly from plots that were already being productive before the intervention. In addition, despite the decrease in the area devoted to annual crops, I find that the reduction in the likelihood of producing these crops due to the intervention is not statistically

[20]Land fallowing is one of most important investments in land quality in a resource-constrained farming system such as this one. Around 50% of the plots in my sample have been fallowed in the past to maintain land productivity.

significant.

To assess the impact of subdivision on the intensive margin, I constructed a measure of total harvest value for all crops and for annual crops using reported quantities and median crop prices. Using median crop prices has the advantage of reducing measurement error since reported prices tend to be noisy – especially when farmers consume most of their harvests and have little information about agricultural market prices. In addition, since the intervention does not aim to directly affect agricultural prices, any differences in reported prices between the experimental groups should be random. Further, if the intervention has an effect, then it must be due to changes in output quantities.

I find that CLOA subdivision reduces the value of total agricultural output from all crops, though this impact seems to be driven by outliers. Total value harvested diminishes by around 24,000 PHP, which corresponds to a decrease of 52% with respect to the control mean. However, this large impact loses significance and shrinks to 15,500 PHP (-35%) when I censor the sample at the 95th percentile (Winsorization at 95th percentile), and further drops to an insignificant -8,100 PHP impact (-21%) if I truncate output values above the 95th percentile. In line with these results, the treatment and control densities of the unexplained variation in output values between treatment and control are similar - except around the right tails (Figure (A.2)).[21] The reduction in total output value from all crops is likely driven by the large and significant decrease in output value from annual crops. While this impact is robust to censoring the sample at the 95th percentile, it is no longer significant if I drop observations above the 95th percentile, though the coefficient estimate is still negative.

[21] Instead of showing the distributions of agricultural output values for the treatment and control groups, I present the distributions of the residuals from a regression in which output value is run against randomization pair fixed effects to take into account the design of the experiment and control for within-pair factors.

Table 1.9: Effects on Agricultural Production

			ITT		LATE	
	Obs	Control Mean	Coef.	Std.Err.	Coef.	Std.Err.
All Crops						
Output Indicator	414	0.857	-0.003	(0.033)	-0.007	(0.059)
Output Value (1000's of PHP)	414	48.852	-11.325**	(5.487)	-24.126**	(10.310)
Output Value (Win. 95th ptile)	414	41.659	-7.275*	(4.270)	-15.498*	(7.912)
Output Value (Truc. 95th ptile)	390	32.683	-3.803	(3.519)	-8.134	(6.405)
Yield (1000's of PHP/Ha)	414	27.697	-2.167	(3.244)	-4.617	(5.846)
Log of Yield	357	9.366	0.005	(0.169)	0.011	(0.278)
Annual Crops						
Output Indicator	414	0.374	-0.042	(0.039)	-0.089	(0.071)
Output Value (1000's of PHP)	414	25.719	-9.366**	(3.997)	-19.952***	(7.664)
Output Value (Win. 95th ptile)	414	20.639	-7.145***	(2.717)	-15.221***	(5.192)
Output Value (Truc. 95th ptile)	391	12.233	-2.352	(1.890)	-4.919	(3.372)
Yield (1000's of PHP/Ha)	414	22.310	10.243	(12.425)	21.821	(22.178)
Log of Yield	154	10.146	-0.408*	(0.227)	-1.179**	(0.510)

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

1.6 Discussion

In this paper, I study how the physical subdivision of collectively-owned landholdings, a typical transitional stage of land reform programs in developing countries, affects tenure security, agricultural investments, and productivity. My paper has three main findings.

First, subdivision of collective landholdings increases tenure insecurity if the transition to formalizing individual land titles is not swift and there is a lack of sufficient information about the process and its timeline. Farmers in treated parcels feel 7% less able to restrict access to their plots, 8% less secure from eviction, and they experience a six-fold decrease in overall tenure security, as measured by an index aggregating seven different outcomes. Fear of parcel confiscation due to amortization default is likely not the main driver behind the increase in tenure insecurity since I do not find differential impacts between compensable and non-compensable parcels. Alternatively, in the transition to obtaining private land titles, specific features such as the cancellation of the CCLOA titles, overall lack of clarity about the process, and uncertainty about if or when the new titles will be received may give ARBs the impression that their land rights are in limbo. Moreover, Barangay officials, who are in charge of settling agrarian conflicts and enforcing land rights at the local level are not involved in the parcelization process, which may cast doubts on their ability to guarantee farmers' rights to their parcels. Consistent with this interpretation, I find a reduction in the trust in local institutions and their ability to protect land rights during conflict situations. Subdivision significantly decreases the perceived efficacy of the Barangay Council in protecting farmers' ownership of their parcels under three hypothetical conflict scenarios: with their neighbors (-15%), with the government (-11%), and with a private company (-10%). Given that titles are the primary proof of ownership in this context, farmers may fear that without any legal document, local institutions may be less able or even less willing to protect ARBs' land rights.

Second, consistent with an increase in tenure insecurity, I find that subdivision leads to a decrease in agricultural investments. The area devoted to annual crops decreased by 0.21 hectares or 43% in treated parcels and was not accompanied by an increase in the probability of a parcel being fallowed, a common method for improving land quality. In line with this decrease in agricultural investments, subdivision reduces the value of total agricultural output, although my estimates are driven by outliers.

Third, this transition increases the likelihood of leasing out individual parcels in spite of the decrease in tenure security. Subdivision raises the probability of renting out land by 12 percentage points, which amounts to 75% increase. This result is driven by plots owned by ARBs with relatively fewer years of farming experience, which I treat as a proxy for farming ability.

This is an interesting finding as land leasing transactions are usually interpreted in the literature as indicative of greater tenure security. However, because of a greater (perceived) likelihood of confiscation, an increase in tenure insecurity lowers the expected returns from directly exploiting one's own land relative to simply renting it out. A reduction in the expected returns from own agricultural production should make leasing more attractive, particularly to individuals who are relatively less productive in agricultural jobs as their opportunity cost of leasing their land is lower.

My findings are important for two reasons. First, they show that a revitalization of land markets, specifically an increase in land rentals, can occur amidst greater tenure insecurity. Therefore, when evaluating land titling programs, it is important to understand the changes in perceived tenure security and, if possible, test whether the increase in land rentals and sales corresponds to an improvement in the functioning of land markets.[22] Second, several policy implications specific to the Philippine Parcelization Program follow from this study's findings. A transparent and streamlined process with a pre-defined timeline and readily available information may help alleviate ARB's uncertainty regarding their land tenure status during the transition. Moreover, in a context where formal written law prevails, the provision of certificates -even if temporary- after the subdivision survey may help alleviate farmers' tenure worries while they wait for their full titles. Even more so if, following the example of PROCEDE (see (Deininger, Bresciani, 2001)1 and (Janvry de et al., 2015)), local law enforcement officials certify these temporary documents and are involved throughout the process as a means to legitimize it. Lastly, additional actions could address other sources of uncertainty, such as amortizations payments for

[22] Unfortunately, given data limitations, I cannot test whether subdivision has affected the functioning of land markets.However, since land ceilings are still in place, land markets should still continue being inefficient. See (Deininger, Bresciani, 2001) for a simple empirical test to assess the performance of the land rental market.

compensable parcels.

Chapter 2: Effectiveness of the BVP in Time

2.1 Abstract

This paper studies the effectiveness of the Beca Vocación de Profesor (BVP), an ability-based teaching scholarship introduced in Chile in 2011, on attracting skilled high-school students into teaching majors. Using a regression discontinuity approach, I find that the scholarship raised the probability of high-performing students applying to and enrolling in teaching programs for the first three cohorts (2011-2013), leading to an increase in the overall proportion of skilled people pursuing a teaching career. However, I also find that the effect of the scholarship is no longer significant for 2014-2016, a period which coincides with a significant expansion of various public scholarship programs and culminates with the introduction of free-tuition college for low-income students in 2016. My findings suggest that the 2014-2016 financial aid expansion changed the relative price of teaching programs, offsetting the incentives previously set out by the BVP and rendering the scholarship ineffective. Policymakers should design financial aid and scholarship programs with complementary incentive schemes if they want to avoid affecting the sorting of students into majors by distorting the equilibrium prices of higher education markets.

2.2 Introduction

Before 2011, Chile was recruiting its future teachers from among the students with the lowest performance on the Prueba de Selección Universitaria (PSU), the national university entrance examination. This practice is the opposite of what countries with the best quality education do (Auguste et al., 2010a) and there is evidence suggesting it is problematic. PSU scores are positively correlated with teacher quality ((Alvarado et al., 2012; Gallegos et al., 2019)), and there is an important branch of economic literature that links teacher quality to student academic performance (Araujo et al., 2016; Bau, Das, 2019; Hanushek et al., 2018; Schacter, Thum, 2004) and future adult outcomes (Chetty et al., 2014).

In order to improve this situation, in 2011 the Chilean Ministry of Education introduced the Beca Vocación de Profesor (BVP), a competitive service scholarship that aims at attracting high-quality students to teaching degree programs. When first introduced, the BVP policy was the best available scholarship for higher education in Chile. Students who score in the top third of the PSU distribution are eligible to study in a quality-accredited education program for free[1]. In a preliminary study commissioned by the Ministry of Education, (Alvarado et al., 2012), using a regression discontinuity (RD) approach in a subsample of Chilean universities, found that for the 2011 cohort the BVP raised the probability of applying to a teaching major by 40%, and of enrolling in a teaching program by 35% around the 600-point threshold. For the same cohort, their RD estimates around the 700-point cutoff suggested that the BVP duplicated the probability of enrolling in a teaching degree major. However, between 2012 and 2016 there was a large expansion of public higher education grant programs and new scholarships became available -

[1]This article uses the terms ”teaching degrees” and ”education degrees” interchangeably, as well as the terms ”major,” ”degree,” and ”program.”

with fewer strings attached than the BVP grant. This change in student opportunities for financing higher education could have countervailed the incentives created by the BVP.

In this paper, I expand Alvarado, Duarte and Nielson's study by first discussing how the expansion of public financing of higher education could have hindered the effect of the BVP policy. Second, I re-estimate the policy's impact using the same RD approach but with a theoretically-based bandwidth size. Third, I provide BVP impact estimations on enrollment using the whole universe of Chilean universities, in addition to our new results drawn from the narrower subsample of higher education institutions used by Alvarado and colleagues. Lastly, I present the first available results on the impact of the BVP grant on the probability of applying to and enrolling in education programs[2] for cohorts between 2012-2016.

I find that around the 600 point-threshold, the BVP raised the probability of students applying to teaching programs by 20%, and of enrolling in a teaching major by between 25 and 30%, for the 2011 cohort. However, when I repeat the analysis for subsequent years the evidence suggests that this positive impact diminished after 2013, and becomes insignificant in the last two years. Furthermore, our impact estimates around the 700-point threshold are only significant for the 2011 cohort, while 2012-2016 freshmen cohorts with PSU scores above and close to 700 points do not seem to consider the additional benefits offered by the BVP grant at all in their college application and enrollment decisions. In addition to the local impact estimates, I also include a descriptive analysis of changes in the composition of BVP education cohorts with regards to PSU scores. This analysis suggests that the BVP policy could have changed the quality of the average education freshman not only by attracting better students (at least in the first years

[2] As I will explain in Section 2.3, the grant is only available for students who enroll in BVP teaching programs. These programs are quality-accredited, require full-time dedication and only admit students with PSU scores ¿= 500.

of implementation) but also by limiting the proportion of ”bad” students who pursue teaching degrees.

This paper contributes to the literature on the effect of scholarships on college applications and enrollment, which has mainly focused on developed countries (especially the US) - leaving us knowing less about the impact of financial aid in other parts of the world where loan and grant programs are less extensive. In addition, this paper also contributes to the yet-to-grow literature on the effectiveness of service scholarships that aim to attract good students to certain professions. Do they work? Can they still be effective in a context where students can get similar (or better) financial aid with no service periods? How do students select in or out of certain majors as a response to changes in tuition costs?

The document is organized as follows. In the first section I describe the Chilean Higher Education system, characterize the demand for teaching majors before the introduction of the BVP policy, and outline the BVP scholarship program, discussing its incentives and how these may have been hindered by the significant growth of public funding for university education that started in Chile in 2012. Then, I describe the data and present enrollment trends in teaching programs after the introduction of the BVP, as well as some descriptive statistics of the BVP program itself. I continue by explaining the empirical strategy and presenting my results. I end the paper with a short discussion.

2.3 Background

2.3.1 The Chilean Higher Education System

Chile's higher education system comprises four types of higher education institutions (HEI): Technical Training Centers (Centros de Formación Técnica or CFT), Professional Institutes (Institutos Profesionales or PI), and Universities. CFT generally offer two- to three-year vocational and technical training degrees, while the IP and universities offer four- to five-year professional degrees. Universities are further divided into two categories: 1) 25 traditional schools, which are part of CRUCH (Consejo de Rectores de Universidades Chilenas) - a consortium that encompasses the most prestigious and selective universities in the country, and 2) non-traditional universities.

Access to higher education has expanded considerably in Chile during the last 25 years. The number of students enrolled in undergraduate degree programs has increased fourfold, from less than 250,000 in 1990 to 1.2 million in 2016 (SIES Public Data 2016). This most recent amount represents around 6% of the total population, which is high relative to other Latin American countries (OECD 2014). Most of this expansion has been through PIs, while enrollment in non-traditional universities and CRUCH schools has increased at a slower rate.

Tuition scholarships, which are the main mechanism through which the State finances higher education, have increased dramatically in recent years (Figure (B.1)). In 2010, about 168,000 million Chilean pesos (CLP) were spent on scholarships allocated through nine public grant programs. In 2011, the BVP grant program was introduced and aid funding increased by 17%. By the end of 2011 and through 2013, there were a series of country-wide student-led protests demanding greater public involvement and financing of higher education. These protests provoked a significant expansion of the public grant programs, which started in 2012 with a relaxation of income eligibility requirements. While in 2011 the vast majority of scholarships were only available to students in the lowest 40% of income distribution, by 2013 those in the bottom 70% became eligible to receive tuition grants. The amount of public funds allocated through tuition scholarships rose 155% between 2011 and 2015, growing from nearly 200,000 million CLP to more than 500,000 million CLP. Furthermore, in December 2015, Congress approved the so-called GRATUIDAD or Free College policy, which allowed individuals in the lower 50% of the income distribution to study for free and obtain any professional degree at 30 universities, including the CRUCH schools. In addition, with the introduction of GRATUIDAD in 2016, and the simultaneous expansion scholarship programs covering tuition for technical and professional degrees at non-traditional institutions were further expanded. As a result, by the end of 2016, the State had spent more than 760,000 million CLP on these programs, allocated through 11 tuition scholarship programs plus GRATUIDAD.

2.3.2 Demand for Teaching Degrees Before the BVP Policy: 2000-2010

Enrollment in teaching degrees during the 2000s increased at a faster rate than total undergraduate enrollment (Figure (B.2)). This increase occurred in spite of the fact that teacher wages had been, for decades, relatively much lower than those of other professions, as well as the fact that - with some exceptions - teaching majors were not considered prestigious. During the decade of 2000-2010, education programs grew at an annual rate of 11.8%, growing from over 43,000 to 136,600 students, while total enrollment increased 8% on average per year, growing from over 435,000 to 940,00 students.

Despite the significant rise in enrollment during the 2000s, teaching majors still had concentrations of students with the lowest scores on the national university entrance examination PSU.[3] For the 2007-2010 period, teaching freshmen had a mean PSU score of 504, which was around 53 points below that of their non-education counterparts. Additionally, while 33% of students in non-teaching degrees scored above the top third of the PSU distribution ($\geq$ 600 points), just 10.1% of those in teaching were above this threshold.

Furthermore, college application data[4] for 2007-2010 shows that there was an inverse relationship between PSU score and preference for teaching programs: individuals who applied to teaching as a first option had lower scores than those who considered it as a second choice or did not consider it at all (Tables (B.1) and (B.2) and Figure (B.3)). For example, in 2010, those who applied to teaching as their first choice had a mean PSU of 530, while those who considered it their lowest option or did not consider it at all scored 569 and 594 on average, respectively. Consequently, those who applied to teaching but ended up enrolling in another field of study had higher PSU scores than those who applied and enrolled in education.

Given this application scenario, it is clear that Chile was recruiting its future teachers from among its worst students - at least in terms of PSU scores. This is the opposite of what is done by the countries with the highest quality education (Singapore, Finland and South Korea) (Auguste et al., 2010a). It is a problematic scenario because there is evidence suggesting that PSU scores are positively correlated with teacher quality. A further concern is that an important branch of economic literature links teacher quality not only to students' academic performance but also to adulthood outcomes.

[3]The PSU is a standardized examination with a mean of 500 and standard deviation of 110.
[4]For 2007-2010 application data is only available for the 25 CRUCH universities.

2.3.3 The BVP Scholarship Program and the Changing Context of Public Funding of Higher Education

The BVP scholarship program was introduced in 2011 with the purpose of recruiting students with PSU scores above 600 points into accredited, high-quality teaching degree programs. Since its creation, it has been the only public scholarship in Chile that does not consider income level among its eligibility criteria. In addition, in 2011, it was the only public scholarship that paid for the real or full tuition costs plus enrollment fees. All other available scholarships paid for the ”reference” tuition, which is always less than the full tuition. In monetary terms, the BVP was the best grant available in Chile when it was first introduced.

The grant is available to prospective freshmen and to students in their senior year of college who want to enroll in a one-year teacher training program. This paper only considers the first type of scholarship, which represents more than 94% of the awards allocated in each year of the policy. Eligibility requirements and corresponding benefits are the following:

1. PSU score ≥ 600: covers full tuition and enrollment fees.
2. PSU score ≥ 700:covers full tuition and enrollment fees, and provides a monthly stipend (CLP $80,000).[5]
3. PSU score ≥ 720: covers full tuition and enrollment fees, provides a monthly stipend (CLP $80,000), and offers the student the opportunity to do a semester abroad (at an international university) for free.

[5]CLP $80,000 corresponded to approximately 44% of Chile’s legal minimum wage (LMW) in 2011, and to 29% LMW in 2016.

In this paper I only estimate the impact of the BVP scholarship on applications and enrollment around the 600-point and 700-point cutoffs since there are not enough observations near the 720-point cutoffs. Most of the analyses are with respect to the 600-point cutoffs since it is precisely around this threshold that the sample sizes are considerable enough to perform solid econometric work. Furthermore, note that the local impact of free tuition and enrollment fees is given by the RD analysis around the 600-point cutoff, as the analyses around the 700- and 720-point thresholds reflect the additional effect(s) of having a semester abroad experience and receiving a stipend, respectively.

In order to receive the scholarship, students must enroll in BVP programs, which are teaching majors that are quality-accredited, require full-time dedication and only admit students with PSU scores¿=500. In return for the scholarship, after graduating from college, BVP grantees must teach for at least 30 hours per week at a public or voucher school for a period of three years. The scholarship is available for the regular length of the program as determined by the university (usually five years). To maintain the aid, the student must pass 60% of their first-year courses and 70% of the courses in subsequent years. Importantly, the student has no obligation to repay any amount of money if she does not graduate, whether she switches to another degree program or drops out. The conditions for both eligible students and eligible majors have remained the same since the introduction of the scholarship program.

Economists assume that rational individuals make decisions, such as attending college or enrolling in a particular major, by comparing the net expected utilities of the available alternatives. Costs and benefits of both monetary and non-monetary nature are taken into account in the calculation of these expected utilities, and the weight assigned to each factor depends on the individuals' preferences, including their preference for risk. In this sense, the BVP grant can affect decisions on whether to apply or not and enroll or not in a BVP teaching program for students with PSU scores ≥ 600 through at least three channels:[6]

1. By changing the relative price of BVP programs, thus making them less costly with respect to all other alternatives - e.g., enrolling in other majors or not attending college.

2. By affecting individual expectations about the quality of the average teaching freshman in BVP programs. This could, for example, take the form of an anticipated (positive) peer effect that attracts students with a vocation for teaching but who, without the policy, would have gone into degree programs with higher-caliber classmates.

3. By positively affecting the respectability of the BVP degrees and thus attracting students interested in teaching but who, in absence of the policy, would have gone to degree programs that are regarded as better-quality.

In a world where all other relevant variables are constant in time, these three channels should non-negatively affect applications and enrollment in teaching degrees; thus, an estimate of the effect of the BVP in 2011 would be enough for understanding the effectiveness of the policy over time. However, the BVP was introduced just one year before the start of the largest expansion of public funding of higher education in recent history in Chile. It is therefore possible that in this changing context, the effectiveness of the BVP incentives also changed.

An example helps illustrate the point: in 2011, a student coming from the lowest 50 percent of the income distribution with a PSU score of 600 and high school GPA of ≥ 5.0[7] is eligible only

[6] Note that while the first channel would only affect the decision-making process of BVP-eligible students, the second and third could be relevant also for non-BVP-eligible students.

[7] The grading system in Chile ranges between 1 and a maximum possible grade of 7.

for the BVP scholarship (Figure (B.4)). By 2016, this same student has five tuition scholarships potentially available (including the BVP grant), of which four allow her to choose a degree in any field of study. This means that if in 2011 she is indifferent about either paying tuition to study with a non-education major or enrolling in a BVP program for free, in 2016 she will be able to “have her cake and eat it too” - enroll in a non-education major for free. In other words, if in 2011 the relative cost of some education degrees diminished significantly for a portion of the applicants (PSU$\geq$600), the public funding expansion that occurred between 2012 and 2016 could have reversed the BVP’s price incentive for a subgroup of students (PSU¿=600 and low-income) by equalizing the relative cost of a wide range of degrees.

For this reason, evaluating the effect of the BVP policy just in 2011 would be insufficient for understanding its actual impact. Indeed, the context in which the BVP was designed and first implemented no longer resembles the current panorama of opportunities for students to finance their higher education, and this scholarship is no longer the best available grant (in monetary terms) for a considerable portion of college freshmen.[8]

2.4 Empirics

2.4.1 Data

I use four different datasets in this study. First, I use college applications and first-year enrollment tables from universities subscribed to the DEMRE centralized admissions system. For 2011 and previous years, DEMRE data contains records for the 25 CRUCH schools. As

[8]For example, GRATUIDAD also covers real tuition and enrollment fees, but does not require the student to enroll in a program in any particular field of study, and does not require a period of service after graduation as does the BVP.

of 2012, DEMRE has applications and freshmen enrollment data on a total of 33 universities.[9] The centralized admissions system works as follows. Prospective students apply to up to ten university-major pairs (henceforth, ”programs”) ranked according to their personal preference. Students do not apply to universities, but rather to specific programs within the universities. For example, ”Economics at University A” or ”Sociology at University B.” Once preferences are collected, a student-proposed deferred acceptance algorithm assigns students to programs according to their score - which is a weighted average of their high school GPA ($\sim 20\%$) and PSU score ($\sim 80\%$), preferences, and the number of seats available. [10] Our data contains the student-level applications to universities in the DEMRE system, and I observe the ranking list submitted by each student and whether the individual enrolled or not in the program she was admitted to.

DEMRE data does not identify the field of study of a given program. Therefore, in order to flag teaching programs, I generated an indicator variable that equals 1 if the name of the program contains the strings ”EDUC” or ”PEDA”[11] and 0 otherwise. Then, I hand-curated the list of potential teaching programs, and encountered very few false positives. In addition, I performed a manual scan of the programs that had been labeled as non-teaching to make sure they did not contain any false negatives. The final list of teaching programs turned out to be very similar to the one originally generated by the string indicator.[12]

[9]The most competitive eight non-traditional universities joined the DEMRE's centralized admission system in the 2012 admissions cycle

[10]The algorithm: 1) defines a score cutoff for each program, taking into account the scores of all the applicants and the seats available and, 2) ends whenever all students have been assigned to a program; or if there are unmatched students, when these have all been rejected by all of their choices. The final result is that students are placed in only one program, which is their most preferred choice among all of the choices for which the student's application score is higher than or equal to the program's cutoff score. Notice that students do not know, a priori, if they are going to be admitted to any of their choices since the cutoffs are endogenously generated during each admissions cycle. Nonetheless, they do have an idea of their probabilities of being accepted given information from past years.

[11]In Spanish, teaching programs are commonly known as ”Pedagogías.”

[12]5 programs that were non-teaching programs were included as teaching majors by the automatic indicator flag.

Second, I used individual records from the PSU exam in years 2011-2016. The data contains demographics such as gender and age, and self-reported socio-economic characteristics such as family income (discrete categories), household size, and parental education, among others. It also has information on the student's high-school GPA and high-school type (public, voucher, private).

Third, I used two BVP datasets from the Ministry of Education. The first dataset corresponds to the public lists of BVP programs for each year. I merged them with our student-level datasets through a unique program identifier. The second dataset contains individual applications to BVP scholarships.

Fourth, I used data from Chile's Higher Education Information System or Sistema de Información de la Educación Superior (SIES), which corresponds to institution-reported student-level enrollment in all programs and for all HEI. While it is true that this dataset could potentially provide a more general picture on enrollment than the DEMRE enrollment data, it does contain a significant amount of reporting error, which made me consider it less reliable.[13] All of our tables across all different sources contain either a common unique individual identifier or a common unique program identifier through which I was able to merge them together

In spite of the reporting error, I used SIES data for the general descriptive statistics but preferred the DEMRE tables for my impact estimations; hoIver, I also report impact estimates from SIES data (when available) in order to avoid representativeness issues. Indeed, DEMRE

I relabeled them correctly.

[13]In cleaning and analyzing the SIES enrollment data, I found many errors and inconsistencies across time in the way some institutions classify their students, particularly first-years. This is possibly because the instructions provided by SIES can be unclear and confusing. Also, when I merged the DEMRE enrollment data with the SIES tables, I found significant inconsistencies between what was reported by the same institution to SIES versus to DEMRE. Again, this is possibly because the SIES reporting instructions can be somewhat obscure. Furthermore, as noted previously, SIES program-level unique identifiers change between years so it is very difficult to obtain, for instance, total enrollment for a given program over time.

institutions correspond to a sample of universities that: 1. encompass the most prestigious and selective universities of Chile, 2. are the only ones that systematically use PSU scores to select their students and, 3. comprise the vast majority of the BVP programs. Therefore, DEMRE data correspond to a sample of universities that are different from the average Chilean higher education institution. This is why, despite the reporting error present in the SIES tables, I also present impact estimates obtained with our cleaned version of this dataset.

Nonetheless, a priori, I should not expect major differences between the impact estimates obtained using DEMRE tables versus those obtained with SIES data. The RD bandwidths utilized in the impact estimations are -at most- approximately 30 points wide (60 points, in total). Consequently, the samples of students used in all regressions have PSU scores between 570 and 630 (and higher for the RD estimations around the 700-point threshold), which are considered good scores, competitive enough to be admitted to many programs at a DEMRE institution. Therefore, it is not surprising that most of the Chilean freshmen with PSU scores in this range are enrolled in DEMRE universities. In particular, as Table (B.6) shows, during 2012-2016 between 82% and 85% of the undergraduate freshmen with test scores between 570 and 630 studied at a DEMRE institution, compared to 14%-15% who went to a non-DEMRE university. This means that the SIES data around the 600- and 700-point thresholds is not greatly different from DEMRE data, and thus impact estimations using both samples should tend to be similar. However, since most of the BVP programs are teaching programs in DEMRE institutions, the differences that may arise between DEMRE and SIES impact estimates should be such that the DEMRE data would tend to overestimate the actual impact of the grant policy.

2.4.2 Enrollment in Teaching Degrees and BVP Program Descriptive Statistics: 2011-2016

In 2011, the year the BVP was introduced, first-year enrollment in teaching degree programs declined for the first time in a decade (Table (2.1), Figure (B.2), and SIES public data) and continued decreasing steadily until 2016, when it slightly grew again. Such decline could have partly been due to a system-wide decrease in first-year enrollment (SIES, mifuturo.cl Enrollment Reports), also present in non-teaching programs in 2012, 2014 and 2015, all of which is consistent with a deceleration in total undergraduate enrollment as pictured in Figure (B.2)). Nonetheless, the decline in freshmen enrollment in teaching programs could have started earlier and been steeper as a consequence of the BVP policy's requirement that BVP programs only admit students with PSU scores above 500 points. Table (B.3) provides some partial descriptive evidence suggesting this was the case. This Table shows that while for BVP programs the change rate in first-year enrollment between 2010 (one year before the policy was implemented) and the years 2011 and 2012 was negative (-10.4% and -17.8%, correspondingly), it was actually the opposite for non-BVP teaching majors (2011 (10.1%) and 2012 (8.9%)). This means that the decline in teaching enrollment was being pulled precisely by BVP programs. Unfortunately, I was unable to replicate this analysis for 2013-2016 due to an untraceable between-years change of the program-level identifiers in the SIES data. [14]

Moreover, as expected, the reduction in enrollment in BVP programs was because no

[14]Many SIES program-level unique identifiers change from one year to another so it is difficult to trace the same program over time, and almost impossible to do so for hundreds of majors. This feature of the SIES data does not affect the other analyses because: 1. the main tables and regressions do not require us to trace a given program over time and, 2. BVP programs can be identified within each year thanks to the BVP data provided by the Ministry of Education.

students with less than 500 points on the PSU test were admitted into these majors. As Table (B.4) shows, while in 2010, majors that became BVP programs in 2011 and 2012 admitted 20% of their students with PSU scores below 500 points, by 2011 and 2012 no students were admitted into these programs with test scores below 500. As a consequence, the student composition in BVP programs changed. For example, between 2010 and 2011, individuals with PSU scores 501¡=PSU¡=599 went from comprising 63% of the student body to 67%. Similarly, for the same years, the percentage of students in BVP programs with PSU¿=600 grew from 17% to 33% (Tables (B.4)).

Table (2.3) shows that BVP programs do indeed select better students; nonetheless, a priori, it is not entirely clear how this is going to affect the overall pool of students undergoing teacher training. A valid concern would be to ask if those programs that do not get certified as a BVP end up admitting more students with PSU$\leq$500 than before, thus negating the potential effect of the policy for the country as a whole. Table (2.2) provides evidence suggesting that this has not been the case. Just after the implementation of the policy, the percent of freshmen in all teaching majors with PSU¡=500 decreased from 43.1% to 37.9%. More importantly, between 2010 and 2016 the percent of teaching freshmen with PSU¡=500 dropped almost 10 percentage points from 43.1% to 33.9%.

Additionally, if I look at proportion of teaching freshmen in the top third of the PSU distribution (i.e., PSU$\geq$600), I see that between 2010 and 2011 it jumps from 10.3% to 16.7%, and then fluctuates from a low of 14.6% in 2016 to a high of 17.3% in 2015. Similarly, the percent of teaching students with test scores between 501 and 599 overall increases between 2010 and 2016, from 46.6% to 51.4%. In general, all of the student composition statistics suggest that the quality of the average teaching freshman has increased since 2011 and are consistent with the increase in the PSU score of the average education freshman, which went up by 15 points between 2010 and 2016 (Table (2.1)).

Table 2.1: First-Year Undergraduate Enrollment in all Higher Education Institutions (HEI): Teaching and Non-Teaching Degrees

Year	Freshmen (Non-T)	Freshmen (T)	Mean PSU (Non-T)	Mean PSU (T)
2007	127,209	26,276	556	503
2008	130,733	27,268	557	503
2009	141,126	28,587	559	506
2010	150,671	30,975	560	507
2011	154,119	29,534	556	516
2012	151,904	27,638	557	514
2013	154,547	24,787	556	516
2014	152,397	21,424	557	521
2015	148,491	19,892	559	525
2016	153,617	19,921	556	522

Sources: SIES, BVP and DEMRE data. Own Calculations: Includes all HEI and students in regular majors. T: teaching; Non-T: Non-Teaching

Table 2.2: Composition of Teaching Freshmen by PSU scores

Year	PSU$\leq$500	501$\leq$PSU$\leq$599	PSU$\geq$600
2007	45.1%	44.7%	10.2%
2008	45.3%	44.6%	10.1%
2009	44.3%	45.7%	10.0%
2010	43.1%	46.6%	10.3%
2011	37.9%	45.4%	16.7%
2012	38.8%	46.5%	14.7%
2013	37.0%	47.6%	15.4%
2014	34.3%	49.0%	16.7%
2015	31.9%	50.8%	17.3%
2016	33.9%	51.4%	14.6%

Sources: BVP and DEMRE data. Own Calculations

Table (2.3) shows the corresponding enrollment statistics for just the BVP teaching programs.

As with enrollment in all teaching majors, the size of freshmen cohorts in BVP programs decreased between 2011 and 2015, and grew again slightly in 2016. Conversely, the percentage of teaching freshmen who are in BVP degree programs presented a moderate increase from 36% to 43% between 2011 and 2016. Nonetheless, the direct impact of the BVP policy could still be limited since a significant proportion of teaching freshmen (around 55%) are enrolled in non-BVP programs.

In terms of student quality, as expected, the PSU score of the average teaching freshman in BVP Programs is significantly higher - by 40 points or more - than the PSU of the average education first-year student (BVP and non-BVP combined). As said before, BVP programs only admit students with test scores above 500; consequently the percentage of freshmen in BVP programs with PSU between 501 and 599, and above 600 points is higher than in teaching programs generally. In particular the proportion of students in the top third of the PSU distribution decreased from a maximum of 33% in 2011 to a minimum in 2016 (27%). Conversely, those with test scores between 501 and 599 points increased from 67% to 72% of the student body in BVP programs.

Column 2 in Table (2.3) shows an important supply-side statistic: an overall declining trend in the number of BVP Programs, dropping from 321 in 2011 to 233 in 2016, giving it a negative growth rate of 27%. A possible explanation for such decrease has to do with the fact that not all teaching programs are BVP-eligible and not all BVP-eligible programs are required to become certified BVP programs. As mentioned in Section (2.3.3) above, to be BVP-eligible, a program must be quality-accredited, require full-time dedication, and only admit students with PSU scores greater than or equal to 500 points. Many majors comply with the first two conditions but still have a significant portion of their prospective applicants/students below the 500-point threshold. Such a high limit could indeed be a tough requirement for some programs considering that during the 2010-2007 period the average education freshman had a PSU score just above this threshold (Table (2.1)). In this sense, when deciding whether or not to become a BVP program[15], eligible programs face a trade-off between losing prospective students with scores below 500 and attracting BVP grantees.

Table 2.3: BVP Teaching Programs & Freshmen Enrollment

Year	BVP Programs	Freshmen in BVP Prog.	% of T. Freshmen in BVP Prog.	Mean PSU in BVP Prog.	% with $501 \leq$PSU ≤ 599	% with PSU ≥ 600
2011	318	10,711	36	574	67%	33%
2012	317	9,991	36	570	69%	30%
2013	303	9,357	38	567	71%	28%
2014	293	8,674	40	570	70%	29%
2015	240	8,291	42	573	70%	29%
2016	233	8,602	43	569	72%	27%

Sources: SIES, BVP and DEMRE data. Own Calculations. T: Teaching

Regarding the number of eligible applications (i.e. PSU$\geq$600), Table(2.4) shows that it reached its highest point the year the BVP was introduced (5,752), and decreased steadily from 2014 through its minimum as yet, in 2016 (3,068). The number and percentage of eligible students who ended up accepting the scholarship were also larger for the first two years, and reached their lowest point in 2016. Specifically, the number of BVP grantees decreased by 56% between 2011 and 2016.[16] In general, these statistics suggest that interest in the BVP grant among prospective grantees has declined over time, and ultimately calls into question the potential of the

[15]BVP-eligible programs decide whether to become a BVP program before each admissions cycle starts.

[16]There are various reasons for why an eligible student may end up not being offered or not accepting the BVP: for example, she may enroll in a BVP program but with another scholarship, she may enroll in a degree program in another field of study, or she may not be admitted to a BVP Program because she was admitted to another of her major choices.

grant to attract high-quality students into teaching majors in the long-run.

Table 2.4: Applications to BVP Scholarships

Year	Eligible Student Applications	Scholarships Accepted (By Students)	% Accepted / Eligible
2011	5,752	3,063	53%
2012	4,088	2,495	61%
2013	4,967	2,238	45%
2014	4,749	2,199	46%
2015	4,584	2,161	47%
2016	3,068	1,340	44%

Sources: BVP and DEMRE (PSU) data. Own Calculations

Table(2.5) presents some descriptive statistics of BVP grantees. I see that between 53% and 56% of them are female, which is interesting considering that the percentage of females in education cohorts rose overall from 66.8% in 2007 to 74% in 2016. This suggests that the BVP helps increase male representation in teaching degrees. Regarding the type of school, more than 50% of the recipients come from voucher schools in all years, while the proportion of students from public schools - who generally come from low-income families - declines steadily over time. In terms of PSU, more than 88% of the grantees have scores between 600 and 700.

Table 2.5: Descriptives Statistics of BVP Grantees

Year	Female	Public	Voucher	Private	PSU $\geq$600 &	PSU $\geq$700 &	PSU$\geq$720
Year	Female	School	School	School	PSU¡700	PSU¡720	
2011	53%	34%	52%	13%	92%	3%	3%
2012	56%	30%	53%	17%	90%	3%	3%
2013	55%	28%	54%	19%	91%	3%	3%
2014	55%	26%	52%	21%	89%	4%	3%
2015	56%	26%	55%	17%	89%	3%	4%
2016	56%	23%	53%	23%	88%	4%	5%

Sources: BVP and DEMRE data. Own Calculations

2.4.3 Empirical Strategy

Estimating the causal effect of financial aid on applications and college enrollment is challenging since there are many unobserved variables that affect both of these outcomes and that are likely to be correlated with scholarship eligibility, thus leading to biased estimates. For example, students who earn a financial award could also have stronger preferences for university education as well as better cognitive and non-cognitive skills that are unobserved by the econometrician, which make them more likely to apply and enroll in college. For this reason, experimental and quasi-experimental settings, or sophisticated estimation techniques are needed for circumventing the endogeneity problems present when estimating the impact of grant aid on student enrollment and applications.

Equation (2.1) characterizes the causal relationship between whether an individual is BVP-eligible and outcome y - which in our case are binary indicators of application to and and enrollment in a teaching degree program.

$$y_i = \beta_0 + \beta_1 X_i + \beta_2 \text{BVP}_i + \beta_3 \text{PSU}_i + \epsilon_i \tag{2.1}$$

Equation(2.1) controls for academic skills through PSU scores and for other relevant observable characteristics X_i that are typically considered outcome determinants, such as high school GPA, household income, parents' education, and future expected wages, among others. Estimating Equation (2.1) via OLS could lead to biased estimates of our parameter of interest β_2 for the reasons discussed above.

In order to overcome this problem, I take advantage of the sharp eligibility rule of the BVP (i.e., PSU$\geq$600) and conduct a regression discontinuity analysis. (Hahn et al., 2001) and (Lee, Lemieux, 2010) state the identifying assumptions of the RD approach. First, if the assignment variable - the PSU score - varies smoothly (continuously) around the cutoff, then the probability of scoring 600 + ϵ or 600 - ϵ is the same, for ϵ sufficiently small. Second, all other outcome-determining characteristics should also vary smoothly (continuously) around the cutoff. This assumption means that individuals should not be able to select themselves in or out of treatment (i.e., BVP-eligibility) based on other characteristics. Third, the outcome should change at the cutoff only because of the BVP policy. This requires, for example, that there is not another outcome-determining policy that uses the 600-point threshold for its eligibility criteria. If the identifying conditions are met, the students just below the cutoff are a good counterfactual for those just above it, because the only difference between these two groups is that the ones above it are BVP-eligible.

Note that for the purpose of this paper, the identifying conditions should be satisfied for every year of the BVP. More specifically, this means that although many things can change from one year to the other, all year-specific characteristics should be continuous around the cutoff.

Let ($y_i(0)$,$y_i(1)$, PSU$_i$), i=1,2,3..,n, be a random sample, where PSU$_i$ has a continuous density, f(PSU). The threshold, PSU$\geq$600, determines whether individual i is assigned to treatment (i.e. is BVP-eligible) or not, and $y_i(0)$, $y_i(1)$ denote the potential outcomes with and without treatment. Then, α_2=E($y_i(1)$-$y_i(0)$—PSU_i=600) is the average treatment effect at the cutoff, or the local average treatment effect. If the identification conditions are met, α_2 can be non-parametrically identified as the difference of the conditional expectations of the outcome at the threshold, that is:

$$\alpha_2 = \lim_{\text{PSU=600}^+} \text{E}(y_i\text{—PSU}_i = \text{PSU}) \quad - \quad \lim_{\text{PSU=600}^-} \text{E}(y_i\text{—PSU}_i = \text{PSU}) \tag{2.2}$$

In this sense, I could estimate the impact of the BVP by comparing the average outcome for individuals in a small vicinity around the threshold. However, it is usually the case that there is not enough data in this close neighborhood, so I need to include individuals farther away. Therefore, the exercise of estimating β_2 requires us to estimate regression functions to the right and left of the threshold. In the RD literature, weighted local linear regressions are commonly employed for this task (Hahn et al., 2001; Porter, 2003).

$$y_i = \alpha_0 + \alpha_1\Delta_i + \alpha 2 * 1(\text{PSU} \geq 600) + \alpha_3\delta_i + \xi_i \tag{2.3}$$

where Δ_i = PSU$_i$ - 600 if PSU$_i$ ¡ 600 and 0 otherwise, and δ_i = PSU$_i$ - 600 if PSU$_i$ $\geq$ 600 and 0 otherwise. The constant α_0 corresponds to the probability of applying to a teaching program for those students who are to the left of the 600-point cutoff (non-BVP-eligible students); α_2[17] is

[17]Note that here I use α_2 instead of β_2 to highlight the fact that RD allows us to identify just local effects, which

our parameter of interest, and corresponds to the additional percentage points that BVP-eligible students have in their probability of applying to a teaching program with respect to non-BVP-eligible students; Δ_i and δ_i allow for different slopes on both sides of the cutoff.

Weights (not shown in Equation(2.3)) are computed by applying a kernel function to the distance between each observation and the cutoff. These kernel functions require a choice of bandwidth and I use the Coverage Error Rate (CER-optimal) data-driven bandwidth selector proposed by (Calonico et al., 2014), which is shorter than the alternative MSE-optimal bandwidth developed by the same authors.[18]

For the sake of clarity, I introduce Equation(2.4), which specifies the local linear regression model I use to estimate the effect of the BPV grant on enrollment in teaching programs. Thus, while y_i in Equation(2.3) is a binary indicator for whether individual i applies or not to a teaching program, Y_i in Equation(2.4) corresponds to a binary indicator that equals 1 if individual i enrolls in a teaching program and 0 otherwise, conditioned on the student enrolling in a program.

$$Y_i = \gamma_0 + \gamma_1 \Delta_i + \gamma_2 * 1(\text{PSU} \geq 600) + \gamma_3 \delta_i + \pi_i \tag{2.4}$$

Equivalently as in Equation(2.3), the constant γ_0 corresponds to the probability of enrolling in a teaching program for those students who are to the left of the 600-point cutoff (non-BVP-eligible students); while γ_2 measures the change in the probability of enrolling in a program for BVP-eligible students who enroll in a university major.

It is important to say that for both of our outcomes I cannot disentangle the "switching effect" from what I call the "pure effect". I will explain the point using enrollment as example,

could be different from average effects.

[18]Calonico, Cattaneo and Titiunik's MSE-optimal bandwidth is an upgraded version of that proposed by (Imbens, Kalyanaraman, 2011)

although the same ideas are valid for application to teaching programs. The total enrollment effect has two components: a "switching effect" and a "pure enrollment effect". The former corresponds to the impact for students who, in the absence of the policy, would have pursued higher education anyways by enrolling in a non-teaching major. The latter is the impact for students who without the policy would not have pursued university education. Our current identification strategy does not allow us to tease out both effects.

Causality in RD Designs and Internal Validity

RD designs - when valid - are basically equivalent to local randomized experiments (Hahn et al., 2001; Lee, Lemieux, 2010). As such: 1) impact estimates around the threshold can be interpreted as local causal relationships and, 2) the tools often used to analyze and test randomized experiments can also be used to check the validity of RD designs.

In order to have a valid RD design, BVP-eligibility should be as if "assigned randomly" around the cutoff. As was stated previously, this means that:

1. The assignment variable - the PSU score - should vary smoothly (continuously) around the cutoff

2. All other outcome-determining characteristics should also vary smoothly (continuously) around the cutoff

3. The outcome should change at the cutoff only because of the BVP policy and not because of other factors.

The literature proposes a variety of tests and data checks to empirically assess the validity of the RD estimates. Regarding the first condition, researchers often implement a manipulation test, which requires the estimation of the density of units on both sides of the cutoff to conduct a hypothesis test about whether the density is discontinuous. The literature offers several manipulation tests for continuous running variables (see (McCrary, 2008), (Otsu et al., 2015), (Cattaneo et al., 2017)). In this paper, I implement the one proposed by (Cattaneo et al., 2017) in which local-polynomial estimates of the density functions on both sides of the cutoff are used to form a t-statistic to test the null hypothesis. Table 2.6 shows that we cannot reject the hypothesis that the density of the PSU score is continuous around the 600-point cutoff in all years, except 2015. Figure B.5 presents visual evidence corresponding to the tests presented in Table 2.6. A priori, it is unclear why there could have been manipulation of the PSU score around the cutoff in 2015 and not in the other the years. While this finding warrants further investigation, it is important to note that most of the evidence supports the continuity of the density of the forcing variable around the 600-point threshold.

Table 2.6: Manipulation Tests Around 600-point Cutoff.

Year	Value of t-Statistic	p-value	N
2011	-0.103	0.918	24,186
2012	0.004	0.997	24,214
2013	-0.423	0.672	24,586
2014	0.335	0.737	24,533
2015	-3.738	0.000	26,033
2016	-1.515	0.130	24,874

Note: Bandwidth= 20 points on both sides of 600-point cutoff.
Sample in each year includes all PSU takers within bandwidth.

Regarding the second condition, Lee and Lemieux state that if variation in the treatment near the threshold is approximately randomized, then it must be that all characteristics determined prior to the PSU score (”baseline characteristics”) should have the same distribution just above and just below the cutoff. That is, their distribution must vary smoothly around the 600-point threshold. In the case of observables, one way to check this assumption is by carrying out balance tests to see if there are any statistical differences in the mean characteristics of students around threshold. Table (2.7) shows that there are no discontinuities in the mean of 16 characteristics for test takers during 2011-2016 who were just above and just below the cutoff. That is, none of the differences between the mean of a given characteristic for non-BVP eligible students and the mean of the same characteristic for BVP-eligible students are statistically significant (Column 6). As a point of reference, Table (2.7) also shows the mean and standard deviation of the characteristics during this period of time.

Table 2.7: Balance of Covariates. Population of PSU Test Takers around 600-point Threshold: 2011-2016

	Mean	SD	Non-BVP Eligible	BVP Eligible	Diff	N
Student Characteristics						
Female	0.523	0.499	0.494	0.494	0.001	213,863
Public School	0.335	0.472	0.213	0.217	0.004	193,215
Voucher School	0.565	0.496	0.588	0.587	-0.002	119,593
Private School	0.100	0.300	0.198	0.197	-0.001	117,113
Isapre	0.204	0.403	0.357	0.364	0.007	152,093
Fonasa	0.715	0.451	0.552	0.551	-0.001	147,060
GPA	557.7	49.2	586.3	586.4	0.031	143,774
Parent's Education						
Mother HS Grad	0.652	0.476	0.842	0.842	0.000	194,566
Mother HE Grad	0.176	0.381	0.311	0.312	0.000	133,104
Father HS Grad	0.648	0.478	0.830	0.830	0.000	204,272
Father HE Grad	0.192	0.394	0.337	0.340	0.003	149,782
Family Background						
Income	2.724	1.077	3.168	3.166	-0.002	173,675
HH Size	4.483	1.645	4.311	4.325	0.014	164,041
Mother Head of HH	0.345	0.476	0.366	0.364	-0.002	208,219
Mother has paid job	0.391	0.488	0.469	0.470	0.000	228,172
Father has paid job	0.629	0.483	0.690	0.692	0.002	198,464

Note: CER-optimal bandwidth selector. Asterisks indicate significance level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table (A7) in the Appendix shows the corresponding balance test analysis for each year between 2011 and 2016, separately. Of the 96 t-tests carried-out, ten are significant. However, there are four elements that may suggest that these statistical differences could have resulted by chance. First, just approximately 10% of the tests report a significant result. Second, half of the discontinuities are significant to the 90% level of confidence and the other half are significant to the 95% level; none of them are significant to higher levels of confidence. Third, there is no systematic statistical difference for any given characteristic across the 2011-2016 period. That is, for any given covariate, the maximum number of years for which I find evidence of a mean discontinuity is 2 (out of 6). Fourth, in the case of **Mother High School Graduate** and **Mother has a paid job**, while in one of the two years in which the difference was significant its sign was positive, in the other year the difference was actually signed negatively. Such inconsistency could suggest that the difference could have occurred entirely by chance.

Furthermore, it is important to note that none of the characteristics directly pertaining to the student herself are statistically different between eligible and non-eligible individuals. Nonetheless, I should point out that 6 of the 10 significant discontinuities correspond to mother covariates, which may provide some evidence suggesting that eligible and ineligible individuals are not statistically equal in at least one dimension. However, these differences are presumably of little economic relevance as for **Mother Higher Education Graduate**, for instance, the difference is 2.5 percentage points, about 0.073 standard deviations - Column 7, Table (A7).

The assumption regarding the continuity of baseline unobservables around the 600-point cutoff is, by definition, untestable. However, if we think about the usual unobservables, innate ability and innate interest in the teaching career, there should not be any concerns regarding the continuity of the distributions of these characteristics around the threshold. A priori, it seems unlikely that teaching vocation or natural ability jump discontinuously at 600 PSU points. Indeed, vocation and innate ability are (plausibly) determined before taking the PSU exam and since the evidence suggests that students (and graders) cannot manipulate test scores, we should not expect unobservably better and/or more motivated students bunching around one side of the cutoff, as we get closer and closer to the threshold.

Finally, the third condition for internal validity of an RD design states that the outcome of interest should change at the cutoff only because of the BVP policy. This means, for instance, that there is not another outcome-determining factor that induces a jump precisely at the 600-point threshold. One example of a factor that would violate this assumption would be another scholarship using the 600-point cutoff as its eligibility criteria. As shown in Table (B.4) this is not the case in any of the years between 2011-2016 (or prior) (Leyes de Reglamentación de Becas de Arancel para Educación Superior en Chile 2011-2016 - Congreso de la República). Moreover, the next highest threshold utilized for scholarship eligibility is 550 points, which is considerably below 600 and does not overlap with the test score bandwidths used for the main results of this paper. It is also worth recalling here that all program admission cutoffs usually change from one year to the next, as they are endogenously generated by an algorithm during each admissions cycle. This means that while applicants may anticipate, for example, higher cutoffs for engineering majors than teaching majors, they cannot precisely predict the final cutoff for any given program in any given year.

One way to investigate the presence of other factors affecting the outcomes of interest around the 600-point threshold is to perform a falsification test for the years prior to 2011. This basically consists of carrying out the same RD analysis for the years before the BVP policy was implemented. If there are not any other outcome-determining elements near the 600-point threshold, we should not find any statistical differences in the probabilities of applying to and enrolling in teaching programs for students on opposite sides of the cutoff for the period 2007-2010. Tables (2.8) and (2.9) confirm that this was indeed the case for the probability of applying to teaching degrees in DEMRE institutions during the years 2007, 2008 and 2009, as well as for the probability of enrolling in a teaching major in a DEMRE institution during the 2007-2010 period. However, for 2009, there is evidence showing a decrease of 1.8** percentage points in the probability of applying to a teaching degree program for students with PSU scores above 600 points.

Table (2.10) presents the same analyses as Tables (2.8) and (2.9) but using SIES data, which takes into account first-year enrollment in **all** higher education institutions - not just DEMRE universities.[19] The results of the falsification test for the 2007-2010 period using the SIES data confirm that there is no statistical difference in the probability of enrolling in a teaching major for students just above and just below the 600-point threshold in the years prior to the implementation of the BVP grant program. This is in line with the majority of the evidence provided by the falsification tests performed on the DEMRE data.

Table 2.8: Falsification Tests. 2007-2010. Probability of Applying (as First Option) to a Teaching Program at a **DEMRE** Institution. RD Analysis around PSU=600.

Year	2007	2008	2009	2010
$\hat{\alpha_2}$	0.004	0.007	-0.018**	-0.008
Robust SE of $\hat{\alpha_2}$	(0.0091)	(0.0090)	(0.0079)	(0.0090)
$\hat{\alpha_0}$	0.110***	0.108***	0.109***	0.119***
Bandwidth - Right of Cutoff	23.3	26.2	31.4	25.4
Bandwidth - Left of Cutoff	32.5	32.0	31.7	30.3
# Obs. - Right of Cutoff	9,179	9,721	12,693	10,650
# Obs. - Left of Cutoff	14,346	13,369	15,254	14,043

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

[19] SIES tables do not contain application data so it is only possible to do a falsification test for applications using DEMRE tables.

Table 2.9: Falsification Tests. 2007-2010. Probability of Enrolling in a Teaching Program at a **DEMRE** Institution. RD Analysis around PSU=600.

Year	2007	2008	2009	2010
$\hat{\gamma_2}$	0.018	0.017	-0.020	-0.022
Robust SE of $\hat{\gamma_2}$	(0.0147)	(0.0155)	(0.0160)	(0.0160)
$\hat{\gamma_0}$	0.168***	0.160***	0.185***	0.197***
Bandwidth - Right of Cutoff	32.6	30.3	31.0	29.8
Bandwidth - Left of Cutoff	25.0	20.7	21.6	19.2
# Obs. - Right of Cutoff	7,872	7,143	7,332	7,588
# Obs. - Left of Cutoff	5,997	5,119	5,548	5,037

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

Table 2.10: Falsification Tests. 2007-2010. Probability of Enrolling in a Teaching Program in a Higher Education Institution. RD Analysis around PSU=600.

Year	2007	2008	2009	2010
$\hat{\gamma_2}$	0.008	0.005	-0.015	-0.006
Robust SE of $\hat{\gamma_2}$	(0.0088)	(0.0092)	(0.0093)	(0.0085)
$\hat{\gamma_0}$	0.121***	0.123***	0.124***	0.128***
Bandwidth - Right of Cutoff	26.5	26.7	20.6	28.1
Bandwidth - Left of Cutoff	40.1	31.0	31.2	31.3
Obs. - Right of Cutoff	10,165	10,378	8,848	12,825
# Obs. - Left of Cutoff	17,712	14,184	15,484	16,351

Data: SIES. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

Falsifications tests around the 700-point threshold for SIES and DEMRE data also indicate that there is no statistical difference in the probabilities of applying to and enrolling in a teaching major for students just above and just below the 700-point cutoff in the years 2007-2010. Tables

(B.11) - (B.13) report these results.

In addition to this classical type of falsification tests presented above, the fact that not all teaching majors are BVP-certified programs creates a unique opportunity for testing that the outcome changes at the cutoff only because of the BVP grant - and not due to other factors. If apart from the BVP scholarship program nothing else in the higher education system affects student incentives precisely at the 600 cutoff, then the probabilities of applying to and enrolling in non-BVP teaching majors should not discontinuously change at that threshold, even during the years following the introduction of the BVP scholarship. Tables (2.11) - (2.13) show the results of RD estimations around the 600-point cutoff for both DEMRE and SIES data.

Tables (2.11) - (2.12) show that the probability of applying to and enrolling in a non-BVP DEMRE teaching programs was statistically equal between both sides of the cutoff for almost all years in the 2011-2016 period. However, for 2015, there is weak evidence (p¡0.1) suggesting local imbalance in the probability of application, and stronger evidence (p¡0.01) indicating that BVP-eligible students were 0.7 percentage point more likely to enroll in non-BVP certified programs than non-eligible students. Nonetheless, when we look at the same falsification tests but using the complete sample of non-BVP programs (SIES data), there is no evidence of statistical difference in the outcomes for the year 2015 (2.13). This seems to suggest that if there were truly other factor affecting the outcomes around the 600-point cutoff in 2015, these could have been specific to DEMRE schools only. Most of the evidence provided by this second type of falsification test argues in favor of the validity of the RD design across the 2011-2016 period, nonetheless it is important to keep in mind that some of it suggest the presence of confounding factors in 2015.

Table 2.11: Falsification Tests. 2007-2010. Probability of Applying (as First Option) to a Non-BVP Teaching Program at a **DEMRE** Institution. RD Analysis around PSU=600.

Year	2011	2012	2013	2014	2015	2016
$\hat{\alpha_2}$	-0.001	0.003	0.003	0.002	0.002*	0.001
Robust SE of $\hat{\alpha_2}$	(0.0010)	(0.0021)	(0.0023)	(0.0013)	(0.0016)	(0.0011)
$\hat{\alpha_0}$	0.002	0.004	0.005	0.002	0.002	0.002
Bandwidth - Right of Cutoff	42.3	33.5	24.1	40.5	29.6	35.1
Bandwidth - Left of Cutoff	31.2	21.0	17.3	29.3	24.5	22.3
# Obs. - Right of Cutoff	16,220	16,302	12,294	18,904	15,124	18,718
# Obs. - Left of Cutoff	14,000	11,327	9,335	16,443	14,714	13,860

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

Table 2.12: Falsification Tests. 2007-2010. Probability of Enrolling in a Non-BVP Teaching Program at a **DEMRE** Institution. RD Analysis around PSU=600.

Year	2011	2012	2013	2014	2015	2016
$\hat{\gamma_2}$	0	-0.004	0	0	0.007***	-0.001
Robust SE of $\hat{\gamma_2}$	(0.0019)	(0.0033)	(0.0033)	(0.0018)	(0.0018)	(0.0023)
$\hat{\gamma_0}$	0.00	0.01	0.01	0.00	0.00	0.01
Bandwidth - Right of Cutoff	36.6	42.0	21.2	31.4	48.2	47.5
Bandwidth - Left of Cutoff	16.2	15.0	19.5	14.6	18.0	21.6
# Obs. - Right of Cutoff	9,209	10,480	5,656	7,971	12,558	12,668
# Obs. - Left of Cutoff	4,113	3,730	5,176	3,894	5,249	6,473

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

Table 2.13: Falsification Tests. 2007-2010. Probability of Enrolling in a Non-BVP Teaching Program in a Higher Education Institution. RD Analysis around PSU=600.

Year	2011	2012	2013	2014	2015	2016
$\hat{\gamma}_2$	-0.001	0.002	0.005*	0.003	0.001	-0.002
Robust SE of $\hat{\gamma}_2$	(0.0041)	(0.0033)	(0.0029)	(0.0028)	(0.0029)	(0.0026)
$\hat{\gamma}_0$	0.018	0.010	0.006	0.007	0.008	0.009
Bandwidth - Right of Cutoff	25.1	29.9	21.8	29.8	26.9	35.1
Bandwidth - Left of Cutoff	17.7	18.8	16.9	18.5	18.3	17.4
# Obs. - Right of Cutoff	11,765	13,310	10,116	13,228	12,616	16,295
# Obs. - Left of Cutoff	8,783	9,321	8,518	9,471	9,903	9,438

Data: SIES. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

2.4.4 Results

In this section I present the main results of the paper. All regressions use a local linear polynomial for students within the CER-optimal bandwidth developed by (Calonico et al., 2014). Inference is based on robust standard errors.

Applying to a Teaching Program

The BVP grant increased the probability of applying to a teaching program (as a first choice) in a DEMRE university around the 600-point cutoff for all years between 2011 and 2013, inclusive; nonetheless, the point estimate of the impact decreased over time and became insignificant in 2014 and 2016, and just barely significant (p¡0.1) in 2015 (Table (2.14)). In 2011, the year of its introduction, the grant increased the probability of applying to a teaching degree by almost 3 percentage points, an estimate that is significant to a 99% level of confidence. By 2016, the point estimate of the impact diminished to 0.9 percentage points and was no longer significant. Figure (B.6) provides graphical evidence of the impact of the BVP grant over time. As expected, the discontinuity at the cutoff for the years 2011-2013 seems to be graphically clearer than the "jump" in more recent years. In addition, the graphs also suggest that the local linear polynomial is a good choice for approximating the relationship between PSU scores and the probability of applying to a teaching program around the 600-point threshold.

The probability of applying to teaching programs for non-BVP-eligible students (α_0) has also decreased over time. In 2011, 14% of prospective students within the optimal bandwidth and to the left of the threshold applied to BVP degrees; in 2016 just 6% did. When testing for difference I find that $\hat{\alpha_0}$ is statistically different to a 99% level of confidence between 2011 and 2016, and even between 2012 and 2016. This is consistent with the decline in freshmen enrollment in teaching programs reported in Table (2.1).[20] [21]

In terms of effect size, Table (2.14) shows the percentage change in the probability of applying to a teaching program. The point estimates of the percentage changes are significant and increase steadily during the first three years of the BVP policy, but tend to decrease after 2013 and are insignificant in 2014 and 2016. Despite the fact that the coefficients are significant for 2011-2013 but not in 2014 and 2016, I cannot reject the null hypothesis that these are statistically equal across years. Specifically, I do not have enough evidence to reject the hypothesis that the

[20] To test for statistical differences between $\hat{\alpha_0}$ across years, I ran a pooled regression using the corresponding CER-optimal bandwidth for each year and performed F tests.

[21] It is important to recall that the 2011 sample only has 25 universities while the sample for all other years contains 33 institutions. In this sense, results for 2011 are only comparable to those of other years if the additional 8 universities are a random sample of the original 25. Since the 8 additional universities are, in fact, the best 8 non-traditional institutions it is very likely that this is the case. However, when testing for statistical difference across years I usually use the 2012-2016 samples.

percent change in 2013 (30.9***%) is statistically equal to that in 2016 (13.5%). This is probably due to the fact that the estimate variances - calculated using the delta method[22] - are very large.

Table 2.14: Effect of the BVP on the Probability of Applying (as First Option) to a Teaching Program at a **DEMRE** institution. RD Analysis around PSU=600

Year	2011	2012	2013	2014	2015	2016
$\hat{\alpha_2}$	0.029***	0.025***	0.027***	0.013	0.015*	0.009
Robust SE of $\hat{\alpha_2}$	(0.0109)	(0.0088)	(0.0085)	(0.0079)	(0.0078)	(0.0072)
$\hat{\alpha_0}$	0.144***	0.096***	0.087***	0.082***	0.081***	0.068***
$(\hat{\alpha 2} \div \hat{\alpha_0})$*100 (% Δ)	20.4**	26.5**	30.9***	15.7	18.1*	13.5
Robust SE of % Δ	(8.54)	(10.58)	(11.45)	(10.54)	(10.61)	(11.23)
P-Value of Test: % Δ in Yr = % Δ in 2013	0.46	0.78		0.33	0.41	0.28
Bandwidth - Right of Cutoff	29.4	27.1	22.1	29.3	23.6	19.2
Bandwidth - Left of Cutoff	21.8	20.0	21.3	20.4	20.8	23.3
# Obs. - Right of Cutoff	11,879	13,489	11,319	14,300	12,473	10,980
# Obs. - Left of Cutoff	9,502	10,856	11,669	11,083	12,219	14,548

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. SE of % Δ are calculated using the Delta Method. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

If we look at the impact of the BVP grant on applications to BVP teaching programs at DEMRE institutions, Table (B.8) shows a very similar pattern to what is reported immediately above, both in terms of point-estimates as well as significance levels. Additionally, Figure (B.7) provides graphical evidence of the impact of the BVP on the probability of applying to BVP teaching programs over time. As when looking at all teaching programs, the discontinuity at

[22]Note that the percentage change $(\alpha 2 \div \alpha_0)$*100 is a non-linear function of the parameters in Equation(2.3). The delta method is one of the available alternatives used when calculating robust standard errors for estimates of non-linear functions.

the cutoff for the years 2011-2013 is graphically clearer than those in more recent years. The similarity between the results for all DEMRE teaching programs and just DEMRE BVP teaching programs is not surprising, since most of the DEMRE teaching programs are BVP-certified.

Regarding the impact of the scholarship program on the probability of applying to a teaching major at a DEMRE institution for students with PSU scores around the 700-point threshold, Table (2.15) shows that the policy seems to have had an effect only in 2011. In the year of its introduction, the BVP scholarship raised the probability of applying to a DEMRE teaching major by 2.9 percentage points, which corresponds to a 103% increase. For 2012-2016, all of the impact point estimates are small in magnitude and insignificant, which suggests that the additional benefit of a stipend for those who were already eligible for a tuition grant had no long-run effect. Among the reasons for why the additional stipend benefit appears to have had no effect after 2011 could be the fact that the total stipend offered to students has been kept constant over time (at CLP $80,000), representing 44% of the country's legal minimum wage in 2011 and as low as 29% five years later in 2016.

Table 2.15: Effect of the BVP on the Probability of Applying (as First Option) to a Teaching Degree at a **DEMRE** Institution. RD Analysis around PSU=700

Year	2011	2012	2013	2014	2015	2016
$\hat{\alpha_2}$	0.029**	-0.001	-0.005	0.002	-0.006	0.014
Robust SE of $\hat{\alpha_2}$	(0.0119)	(0.0086)	(0.0078)	(0.0090)	(0.0077)	(0.0091)
$\hat{\alpha_0}$	0.028***	0.030***	0.031***	0.033***	0.030***	0.025***
Bandwidth - Right of Cutoff	17.1	19.0	19.2	17.9	19.1	17.6
Bandwidth - Left of Cutoff	43.0	27.4	34.6	48.0	35.2	34.6
# Obs. - Right of Cutoff	1,995	2,770	2,796	2,462	2,763	2,749
# Obs. - Left of Cutoff	8,489	5,858	7,694	11,550	7,797	8,219

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

SIES data does not contain information on students so it is impossible to replicate the analysis above for all higher education institutions. However, in the next section documenting the impact of the BVP on enrollment in teaching majors, I will provide results obtained using both DEMRE and SIES samples.

Enrolling in a BVP Teaching Degree

The BVP scholarship increased the probability of enrolling in a teaching program at a DEMRE university around the 600-point cutoff between 2011 and 2015, but appears to have had no effect in 2016. In 2011, the year of its introduction, the grant increased the probability of enrolling in a teaching degree program by 3.6 percentage points. By 2016, the point estimate of the impact diminished to 1.4 percentage points and was no longer significant. These results, obtained from the DEMRE data, are further supported by the SIES data. Table (2.17) shows that the BVP scholarship increased the probability of enrolling in a teaching degree at **any** higher education institutions around the 600-point threshold between 2011-2015, started decreasing after 2013, and became insignificant by 2016. As expected, the point-estimates from both the SIES and DEMRE data are quite similar - especially during the first four years of the policy (2011-2014) - and the point-estimates for 2015-2016 seem to lose magnitude and significance by a greater extent in the regressions using the SIES sample. This last point is not surprising since SIES data contains proportionally more non-BVP certified programs than DEMRE data. Figures (B.8) and (B.9) provide graphical evidence of the impact of the BVP grant over time for both DEMRE and SIES samples. Again, the discontinuities are graphically clearer for the first three years of the BVP than they are for 2014-2016. The graphs also suggest that the local linear polynomial was a good choice for approximating the relationship between PSU scores and the probability of enrolling in a teaching program around the 600-point threshold.

SIES results are also in line with the decline in freshmen enrollment in teaching programs (Table (2.1)) as the probability of enrolling in teaching majors at any university for non-BVP eligible students has decreased over time. In 2011, 11.9% of students admitted to a university program who were within the optimal bandwidth and to the left of the threshold enrolled in teaching majors; in 2016 8.8% did. When testing for difference I found that $\hat{\gamma_0}$ was statistically different (and larger) to more than a 99% level of confidence between 2011 and each of the years in the 2013-2016 period.[23]

In terms of effect size, for both DEMRE and SIES samples, the point estimate of the percent change was significant and increased steadily during the first three years of the BVP policy, but

[23] To test for statistical differences between $\hat{\gamma_0}$ across years, I ran a pooled regression using the corresponding CER-optimal bandwidth for each year and performed F tests. I do not report p-values of F test in the tables to avoid cluttering but this are available to the reader upon request.

tended to decrease, both in magnitude and significance, after 2013 and became insignificant in 2016 (and also in 2015 for SIES). The percent change in 2013 was statistically different (and larger) from that in 2016 (and also in 2015 for SIES). This suggests that not only did the impact diminish over time in absolute terms (as percentage points) but also in relative terms (as a percent).

Table 2.16: Effect of the BVP on the Probability of Enrolling in a Teaching Program at a **DEMRE** Institution. RD Analysis around PSU=600

Year	2011	2012	2013	2014	2015	2016
$\hat{\gamma_2}$	0.036**	0.034***	0.044***	0.022**	0.025**	0.014
Robust SE of $\hat{\gamma_2}$	(0.0150)	(0.0113)	(0.0110)	(0.0107)	(0.0111)	(0.0109)
$\hat{\gamma_0}$	0.142***	0.110***	0.098***	0.097***	0.102***	0.098***
$(\hat{\gamma_2} \div \hat{\gamma_0})$*100 (% Δ)	25.2**	31.2**	45.2***	23.0*	24.7*	14.0
Robust SE of % Δ	(12.29)	(12.42)	(13.88)	(12.54)	(12.64)	(12.08)
P-Value of Test: % Δ in Yr = % Δin 2013	0.28	0.42		0.21	0.26	0.08
Bandwidth - Right of Cutoff	31.8	29.6	18.3	30.2	26.8	21.1
Bandwidth - Left of Cutoff	17.3	16.3	21.2	16.1	14.5	16.0
# Obs. - Right of Cutoff	7,962	11,224	7,339	11,600	10,681	8,962
# Obs. - Left of Cutoff	4,407	6,155	8,514	6,510	6,044	6,942

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. SE of % Δ are calculated using the Delta Method. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

Table 2.17: Effect of the BVP on the Probability of Enrolling in Teaching Program. RD Analysis around PSU=600

Year	2011	2012	2013	2014	2015	2016
$\hat{\gamma_2}$	0.037***	0.032***	0.040***	0.028***	0.014*	0.007
Robust SE of $\hat{\gamma_2}$	(0.0093)	(0.0084)	(0.0094)	(0.0084)	(0.0084)	(0.0074)
$\hat{\gamma_0}$	0.119***	0.110***	0.091***	0.084***	0.095***	0.088***
$(\hat{\gamma_2} \div \hat{\gamma_0})$*100 (% Δ)	31.2***	29.4***	44.2***	33.7***	14.9	8.4
Robust SE of % Δ	(8.995)	(8.995)	(12.497)	(11.939)	(9.487)	(8.790)
P-Value of Test: % Δ in Yr == % Δin 2013	0.40	0.33		0.55	0.06	0.02
Bandwidth - Right of Cutoff	25.2	28.5	19.7	28.6	21.8	24.9
Bandwidth - Left of Cutoff	28.0	32.9	23.3	21.7	25.8	31.1
# Obs. - Right of Cutoff	11,765	12,893	9,345	12,912	10,617	11,967
# Obs. - Left of Cutoff	14,274	17,352	12,024	11,121	13,930	17,461

Data: SIES. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. SE of % Δ are calculated using the Delta Method. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

Tables (B.9) - (B.10) report the impact and effect size estimates of the BVP policy on enrollment in BVP teaching programs in particular, for both DEMRE and SIES samples. Overall, the point estimates of the effect of the grant and the significance levels are quite similar between all teaching programs and just BVP teaching programs, for both DEMRE and SIES samples. In line with previous evidence, these tables report the general trend of an increasing and significant policy impact during the 2011-2013 period, followed by a decreasing and ultimately insignificant effect between 2014-2016.

Finally, with regards to the impact of the scholarship program on the probability of enrolling in a teaching degree for students with PSU scores around the 700-point threshold, Table (2.18) (DEMRE Sample) and Table (2.19) (SIES Sample) show that the policy seems to have had an effect only in 2011. In the year of its introduction, the BVP scholarship raised the probability of applying to a DEMRE teaching major by 4.5 percentage points (which corresponds to a 166% increase), and to **any** teaching program by 2.9 percentage points (or by 103%). For 2012-2016, all of the impact point estimates are small in magnitude and insignificant, which suggests that the additional benefit of a stipend for those who are already eligible for free tuition and enrollment fees had no long-run effect. Here again it is important to note that the total stipend offered to student by the BVP grant has remained constant in time (CLP $80,000), moving from representing 44% to only 29% of the country's legal minimum wage.

Table 2.18: Effect of the BVP on the Probability of Enrolling in a Teaching Degree at a **DEMRE** Institution. RD Analysis around PSU=700

Year	2011	2012	2013	2014	2015	2016
$\hat{\gamma_2}$	0.045***	0.000	-0.009	-0.004	-0.007	0.013
Robust SE of $\hat{\gamma_2}$	(0.0170)	(0.0102)	(0.0101)	(0.0112)	(0.0100)	(0.0108)
$\hat{\gamma_0}$	0.027***	0.031***	0.036***	0.046***	0.033***	0.032***
Bandwidth - Right of Cutoff	17.4	24.4	21.2	26.3	17.5	20.1
Bandwidth - Left of Cutoff	17.5	18.6	24.1	25.6	24.9	33.3
# Obs. - Right of Cutoff	1,547	2,787	2,631	2,894	2,174	2,472
# Obs. - Left of Cutoff	2,047	3,110	4,149	4,402	4,241	6,351

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

Table 2.19: Effect of the BVP on the Probability of Enrolling in a Teaching Degree. RD Analysis around PSU=700

Year	2011	2012	2013	2014	2015	2016
$\hat{\gamma_2}$	0.029**	-0.004	-0.011	0.003	-0.006	0.012
Robust SE of $\hat{\gamma_2}$	(0.0123)	(0.0086)	(0.0090)	(0.0098)	(0.0084)	(0.0103)
$\hat{\gamma_0}$	0.028***	0.035***	0.038***	0.039***	0.028***	0.029***
Bandwidth - Right of Cutoff	15.7	23.9	22.4	21.2	15.2	18.3
Bandwidth - Left of Cutoff	31.6	42.6	31.8	44.7	40.1	37.5
# Obs. - Right of Cutoff	1,973	2,904	2,868	2,598	2,031	2,375
# Obs. - Left of Cutoff	5,914	9,309	6,128	9,443	8,551	8,148

Data: SIES. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. Asterisks indicate significance level: * p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01

2.4.5 Discussion

The evidence suggests that the BVP scholarship policy had a positive local impact on both applications to and enrollment in teaching programs during the initial years of the scholarship; however, point-estimates, effect sizes, and significance levels generally tend to decrease after 2013. With respect to the analysis around the 600-point cutoff, I find that the impact of the BVP grant on the probability of applying to a teaching program is not significant in 2014 and 2016, and barely significant in 2015 (p¡0.1). Similarly, the BVP effect on the probability of enrolling in an education program also starts decreasing as of 2014 and becomes insignificant by 2016, in both the DEMRE sample and the SIES data. In line with the results of the RD analysis around the 600-point threshold, the graphical evidence also appears to indicate that the effect of the scholarship has locally decreased in time. In addition, the impact estimations around the 700-point threshold indicate that the program had a local impact only for the 2011 cohort. Subsequent cohorts with

PSU scores above 700 did not seem to consider the additional benefits offered by the BVP grant in their college application and enrollment decisions.

Additionally, there are several descriptive statistics that are consistent with a decline in the potential effectiveness of the BVP policy in bringing in high-quality students to teaching majors. For example, the number of both eligible applications and accepted BVP scholarships has decreased significantly since 2011. In particular, the number of scholarships won and accepted fell from 3,063 in 2011 to 1,340 in 2016, a decrease of 56%. Furthermore, in terms of supply-side dynamics, the number of BVP programs has also dropped over time by 27%, decreasing from 321 to 233 between 2011 and 2016. This implies that BVP-eligible students have fewer program options where they can enroll and become BVP grantees, thus potentially making the grant program less attractive for some eligible candidates.

One of the reasons behind the reduction in the number of eligible programs could be the fact that as the BVP scholarship's ability to bring in high quality students to teaching degrees diminished over time, the opportunity cost of maintaining the BVP-certified status was too high for some programs - particularly for the least competitive. Indeed, maintaining the BVP-program status implies denying admission to students with test scores below 500, which could represent an important proportion of the pool of applicants to a given program, especially if we take into account that the mean PSU score in teaching majors prior to the BVP policy was just above 500 points.

In spite of the evidence suggesting a decline in the local impact of the BVP policy, it is important to point out that the relative composition of education majors has improved since the BVP's implementation: students with PSU scores above 600 have moved from representing 10.4% of teaching freshmen in 2010 to 14.6% in 2016. More importantly, between 2010 and

2016, the proportion of education freshmen with test scores below 500 has decreased by more than 9 percentage points, dropping from 43.1% to 33.9%. These descriptive statistics seem to suggest that the BVP policy's impact in reducing the proportion of low-scoring students pursuing teaching degree programs could have been as important as its ability to bring good students into teaching careers. Unfortunately, it is not possible to investigate the local causal impact of the BVP policy around the 500-point cutoff via an RD approach, due to the fact that many universities impose this score threshold as a requirement for application to some of their programs; the conditions for RD validity thus are not satisfied.[24]

While the overall increase in proportion of students with PSU scores above 600 between 2010 and 2016 may seem to contradict the RD results around the 600- and 700-cutoffs - which suggest a declining impact of the BVP - it is important to recall that impact estimates of a regression discontinuity analysis are local and do not apply to students who are not around the threshold. An RD analysis is always silent about what happens farther away from the cutoff, in any direction. More importantly, the relative composition of teaching freshmen changes not only because there are more students with exam scores above 600, but because there are significantly fewer with PSU scores below 500.

There are various limitations to this study. The first is that causal estimates of a valid RD approach are always local and do not say anything about what happens at other points along the distribution. Nonetheless, the RD analyses were carried out at two points on the PSU distribution (600 and 700) where we most expect an impact to occur given the design of the scholarship program. Therefore, even if the estimates are local, they are informative of the success or failure

[24]For example, the expectations of being accepted to some programs jump discontinuously at this point for reasons other than the BVP policy.

of the program. The second limitation is that I cannot disentangle the channels through which the BVP impact changes from one year to the next. I find that the impact of the scholarship on the probability of applying to and enrolling in teaching degree programs decreases in time after 2013 and is not significant for 2016. However, I cannot quantify what portion of such decrease is due to, for example, the availability of new and more attractive scholarships. This limitation is mainly because I do not have access to the data that would help parse out these causes.

Research on the effectiveness of the BVP policy should continue, by looking for example at the progression and graduation rates of BVP grantees. Most importantly, the fundamental question regarding what impact these new generations of teachers will have on the academic performance and adult outcomes of students in the future remains an open one.

Chapter 3: Unintended Consequences of Free College: Self-Selection into the Teaching Profession

Joint with Ricardo Espinoza and Miguel Sarzosa

3.1 Abstract

Teacher quality is one of the most relevant factors influencing student learning. However, attracting and retaining skilled people to the teaching profession is challenging. In this paper, we study how making college tuition free affects the pool of students pursuing a teaching career. We exploit the conjunction of two tuition-financing policies implemented in Chile: a scholarship introduced in 2011 for teaching majors, and a massive 2016 reform that made college tuition free for students from households in the bottom 50% of the income distribution. We use the programs' differences in timing and eligibility criteria to study the effects free college had on the self-selection of students into teaching programs. We find that free college decreased the relative returns to pursuing a teaching career, making it substantially less popular among relatively poor high-performing students who now self-select into programs with higher returns. We find that the reform reduced the academic qualifications of the pool of students entering the teaching programs, which can negatively affect long-term teacher quality.

3.2 Introduction

Teachers are a key input in the formation of early human capital. Effective teachers can create fruitful learning environments, inspire and motivate students, compensate for the lack of a favorable home environment, and help level the playing field for disadvantaged students. Research shows not only that teacher performance impacts student learning outcomes and academic achievement at all levels of education (Araujo et al., 2016; Bau, Das, 2019; Hanushek et al., 2018; Schacter, Thum, 2004), but that teacher's effects on students are persistent (Konstantopoulos, 2011). Students who are assigned to high-quality teachers exhibit better long term outcomes, such as a higher probability of attending college, and higher salaries (Chetty et al., 2014). Thus, attracting the best candidates to the teaching profession has become central to improving education systems. But, where and how to find the best teachers? There is convincing evidence that highly effective teachers were once among the best students in school (Alfonso et al., 2010; Glazerman et al., 2005; Xu et al., 2011).[1] For this reason, in countries with highly successful education systems such as Finland, Singapore, and South Korea prospective teachers are selected from among top high school students (Auguste et al., 2010b; Seng Tang, 2015). However, in most countries, universities still struggle to attract the best students to become teachers. Students who pursue teaching programs are disproportionately drawn from the lower end of the academic proficiency distribution (Balcázar, Ñopo, 2014; Eide et al., 2004; Hanushek, Pace, 1995; Santiago, 2002) and tend to perform lower on cognitive tests than students in other fields (Lang, Palacios,

[1]Evidence from Teach for America (TFA), a program that recruits graduates from selective colleges in the US to teach in the most challenging K-12 schools, shows that students lectured by TFA teachers score higher on standardized tests despite the lack of experience of TFA teachers (Glazerman et al., 2005; Xu et al., 2011). Similarly, impact estimates of *Enseña Chile*, the Chilean adaptation of TFA, suggest that placing outstanding college graduates in the most vulnerable schools results in significant student gains in cognitive and non-cognitive abilities (Alfonso et al., 2010).

2018).

To a large extent, the difficulty of attracting top students to the teaching profession can be explained by a combination of low expected labor market returns and low social recognition of the profession (Elacqua et al., 2018; OECD, 2018). For example, teachers are among the lowest-paid college graduates and are paid, on average, 20% less than similarly educated workers in OECD countries (OECD, 2017a).[2] Moreover, recent international evidence shows that the net economic returns of teaching degrees are low on average (Espinoza, Urzua, 2016; Gonzalez-Velosa et al., 2015; Hastings et al., 2013), and that top students are significantly more likely to choose programs with higher economic returns, such as in STEM, business or law. As a result, several countries have implemented policies to make the teaching profession financially more attractive (Bruns, Luque, 2015; OECD, 2005). Such policies include raising teacher salaries, offering more attractive professional development opportunities, making dedicated financial aid available in the form of grants, scholarships or special allowances, among others (Ballou, Podgursky, 1995; Claro et al., 2013; Santiago, 2002). At the same time, there has been increasing pressures to make higher education more affordable. In the US, for example, the idea of making universities tuition-free has been discussed and there are concrete plans for eliminating tuition fees in community colleges across the country. Both types of policies affect the relative return of university degrees and, ultimately, student choices (Bucarey, 2018). For this reason, to continue attracting top students to the teaching profession, it is important to understand the interplay between policies that incentivize qualified students to pursue teaching degree programs and broader efforts to alleviate the financial burden of attending higher education.

[2] Studies in middle-income countries find that this difference is still significant after controlling for observable characteristics typically linked to labor productivity (Mizala, Ñopo, 2016).

In this paper, we study how tuition-free college affects the pool of students pursuing a teaching career. We exploit a major 2016 reform carried out in Chile that made college tuition free for students from households in the lower 50% of the income distribution. We leverage the fact that the introduction of free college affected the application behavior of distinct groups of students depending on their eligibility to the *Beca Vocacion de Profesor* (BVP) tuition grant, a scholarship program introduced five years before, in 2011, that was successful in bringing high-quality students into teaching majors (Castro-Zarzur, 2018). The BVP granted full scholarships to students willing to enroll in teaching majors who scored in the top 30% of the college entrance exam. The subsequent introduction of the free-college policy equalized the relative prices of studying a wide range of different majors, potentially offsetting the incentives set out by the BVP.

Using difference-in-difference and triple-difference strategies on a rich administrative dataset containing test scores, student applications, and enrollment, we compare the application and enrollment behavior of students in cohorts before and after the implementation of the tuition-free policy. Thus, we identify the causal effect of eliminating tuition fees on students' preferences for teaching degrees and the extent to which it affected the academic qualifications of students pursuing a teaching career. Our results suggest that granting tuition-free access to college decreased the demand for teaching programs of top-performing students. The probability of applying to a teaching major among high-performing students who come from relatively poor backgrounds fell by about 15.8%, offsetting the gains obtained by the BVP scholarship during its first years of implementation. In consequence, students admitted to teaching degrees achieve, on average, lower academic performance than before the policy, while the average score of those who were accepted into other majors remained unchanged or even improved. The drop was concentrated on the relatively poor high school graduates whose scores fell by around 10% of a standard deviation.

Our paper provides an important input to the ongoing international debate on free college by analyzing a potential unintended consequence of such policy. In particular, we find that despite the fact that free college may increase total welfare, the distortion of equilibrium prices can affect the sorting of students into different programs and reduce long-term teacher quality.

Our findings are likely to be informative even in contexts lacking previous ability-based scholarships, such as the BVP. Teaching majors tend to be the least costly in many countries—including the United States, where differential tuitions are being increasingly used. Hence, in the absence of tuition-free college, the relatively cheap teaching majors are a pathway to higher education for low-income students. As free college equalizes the tuition cost across all majors, high performers from poorer backgrounds can substitute programs that were relatively cheap with programs that were more expensive and provide better returns. In that sense, the BVP is just a vehicle for identification as it allows us to implement a difference-in-difference strategy while providing important insights on the potential impact of free college on self-selection into the teaching profession in more general settings like the US, where the college tuition-free policy is gaining momentum and several existing programs are already pushing in that direction (e.g., New Mexico state-wide free college policy and the Excelsior program in New York).

The paper is structured as follows. Section 3.3 gives an overview of the institutional background of the teaching profession in Chile and explains the recent reforms of the higher education system. Sections 3.4 and 3.5 present the data and empirical strategy. Section 3.6 and 3.7 present the results and section 3.8 discusses the results and concludes.

3.3 Institutional Background

3.3.1 Tertiary Education in Chile

Access to higher education has expanded considerably in Chile during the last 25 years (Espinoza, Urzua, 2016). The number of undergraduate students has increased fourfold, from less than 250,000 in 1990 to 1.2 million in 2016 (Centro de Estudios MINEDUC, 2017). According to the World Bank Data, the net enrollment rate in Chile is 90.3%, ranking fourth in the world. Higher education is supplied by three types of institutions. Firstly, 59 Universities (*Universidades*), 40 of which are private, offer undergraduate and postgraduate degrees and enroll 58% of higher education students. Universities are further divided into "traditional" or CRUCH (acronym for *Consejo de Rectores de Universidades Chilenas*) and "non-traditional" or non-CRUCH. The former comprises all public universities and private universities founded before a large reform in 1981, receive direct funding from the government and their students are eligible for exclusive scholarships. The second type of institutions are Professional Institutes (PI, *Institutos Profesionales*), which offer 2-5 year non-academic degrees. There is a total of 39 PIs, all of which are private, and enroll 31% of the students in the higher education system. Finally, Technical Training Centers (TTC, *Centros de Formación Técnica*) offer two-year vocational degrees. There is a total of 51 TTCs across the country that enroll 11% of the students in the system (Centro de Estudios MINEDUC, 2017).

Higher education in Chile is financed by private and public sources. Until 2015, all students paid tuition fees and the government supported low-income through scholarships (see Beyer et al., 2015). However, as a result of massive student protests that started in 2011, a large reform in

2015 made universities tuition-free for students from the poorest 50% families (Espinoza, Urzua, 2015).[3]

The admission to universities is primarily based on a nationwide university entrance exam called *Prueba de Selección Universitaria* (PSU) and, to a lesser extent, high school grades. A group of 41 universities, including private and public institutions use a centralized matching system to select students to their programs. Students applying to these universities submit an ordered list containing up to 10 choices. Using a deferred-acceptance matching algorithm, the system matches students with one specific university-degree pair (e.g., teaching in university 1, psychology in university 1, etc.) based on student's PSU scores, high school grades and programs' vacancies (Espinoza et al., 2017). The universities that do not participate in the centralized system, as well as PIs and TTCs, select students independently, although PSU scores are still required.

3.3.2 The Teaching Profession in Chile

As in many other parts of the world, Chile' s teaching programs struggle to attract top-performing students (Alvarado et al., 2012; Balcázar, Ñopo, 2014; Castro-Zarzur, 2018; Eide et al., 2004; Gomez et al., 2019; OECD, 2005). Table 3.1 shows that the average PSU score of students in teaching programs is roughly 30% of a standard deviation lower than students in STEM programs. Similarly, students in non-STEM programs (e.g., liberal arts, social sciences) outperform teaching students in PSU by more than 10% of a standard deviation.[4] Furthermore,

[3]Universities' participation in the tuition-free program was voluntary. All public universities automatically joined the program along with 14 private institutions joined the program. The reform did not include Professional Institutes (PIs) and Technical Training Centers (TTCs), which continued to charge fees.

[4]These gaps are mainly explained by the performance on the math section of the PSU exam. Students admitted in STEM programs score on average 61.6% of a standard deviation higher in math than teaching students. This relates to the fact that, when tested in the TEDS-M—an international study that quantifies the math proficiency of those who

Table 3.1: Characteristics of Admitted Students by Type of Degree

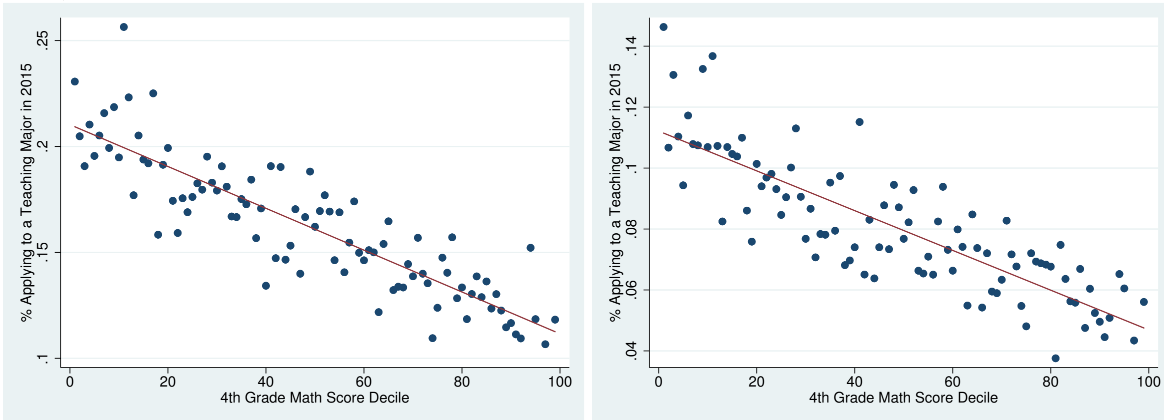
	Type of degree					
	Teaching		STEM		Others	
Overall PSU score	574.88	(0.571)	608.37	(0.474)	596.43	(0.332)
Language PSU score	587.43	(0.722)	586.59	(0.527)	601.65	(0.387)
Math PSU score	562.34	(0.700)	630.14	(0.530)	591.20	(0.373)
% From top 10% PSU score	12.32	(0.362)	32.83	(0.315)	26.89	(0.215)
% Female	62.98	(0.531)	27.47	(0.299)	59.10	(0.239)
Mother's years of schooling	5.99	(0.028)	6.56	(0.018)	6.74	(0.013)
% from public schools	29.32	(0.501)	24.43	(0.288)	22.79	(0.204)
% from voucher schools	58.87	(0.541)	55.10	(0.334)	50.12	(0.243)
% from private schools	11.26	(0.348)	20.02	(0.268)	26.44	(0.214)
% Teaching as 1st choice	20.16	(0.214)	0.09	(0.009)	0.21	(0.010)
% Teaching in top 3 choices	47.90	(0.267)	0.67	(0.025)	1.63	(0.028)
Number of students	8,233		22,187		42,358	

Note: The sample includes all students accepted in university programs under the centralized matching system. Programs considered require at least 8 semesters for completion. We exclude from the sample students enrolling in Professional Institutes and universities running decentralized admission processes. Standard error in parenthesis. STEM programs include majors in the fields of Sciences and Engineering according to the UNESCO classification of degrees. Source: DEMRE and SIES, 2015.

only 12.3% of students admitted to teaching programs perform in the top 10% of the PSU. In contrast, one third of STEM students do. Differences in scholastic achievement against those who sort into teaching majors are deep rooted. Figure 3.1 indicates that students performing in the bottom quintile in 4^{th} grade SIMCE math scores—a nationwide student assessment taken when they were 10 years old—are twice as likely to apply to a teaching program when they finish high school than students in the top quintile.

are studying to become math teachers—future Chilean high school math teachers ranked second to last, while those who would become elementary school teachers ranked last, even behind countries that are vastly less developed, like Botswana and Philippines (Elacqua et al., 2018).

Figure 3.1: Share of Students Applying to Teaching Programs and 4^{th} Grade Math Score (2015 Cohort)



Note: The left figure shows the share of students from the 2015 graduating cohort who listed a teaching major anywhere in their list of preferences and performance in SIMCE 4^{th} grade math SIMCE, by decile. The right figure shows the share of students from the 2015 graduating cohort who listed a teaching major as their top choice by the 4^{th} grade math SIMCE decile. The lines in both figures represent a fitted regression line.

These gaps show that teaching programs are much less attractive and competitive than programs in other fields. Indeed, only 20% of *teaching* students had listed a teaching program as their top choice in the university application process. Students who ended up enrolling in STEM programs did not consider teaching majors as alternative professional paths: less than 1% of them listed a teaching major within their top three choices when applying to college. Table 3.1 also shows that students admitted to teaching programs come from more disadvantaged backgrounds. Compared with students in other fields, a higher share of teaching students attended a public school, and their mothers have completed less years schooling.

The reasons behind the low demand for teaching programs among relatively skilled students and those coming from affluent backgrounds are multiple and intertwined (Ajzenman et al., 2021). First, in Chile there is low social recognition of teachers (Elacqua et al., 2018). For instance, according to related surveys, only one third of parents would like their children to be teachers (Cabezas, Claro, 2011). Similarly, two-thirds of teenagers find that the teaching profession is, along with music and theater, the least prestigious occupations. Second, teachers are underpaid (Mizala, Ñopo, 2016). The average entry level salary of a primary teacher is about 810 USD per month (659,000 CLP), which is just twice the minimum wage and near the average monthly salary in the country (780 USD) (INE, 2021, www.mifuturo.cl). More importantly, salaries of teachers are significantly lower than salaries of workers with similar qualifications. According to the OECD, teacher salaries in Chile are around 20% lower than the earnings of tertiary-educated workers. For example, the entry level salary of business major graduates is about 1.65 times that of teaching graduates and five years after graduation the salary the ratio increases to 2.25. Moreover, despite the relatively low tuition fees of teaching programs (see Table 3.2), the net labor market returns are very low. For example, Gonzalez-Velosa et al. (2015) estimate that the net financial return to teaching degrees in Chile is -1%, on average. What is more, Espinoza, Urzua (2016) find that teachers' income across the life course is similar to income obtained by workers without a college degree. Third, career development and salary raises are mostly based on seniority and not on merit. Despite these downsides, data from the US show that the teaching profession is still attractive to some individuals, particularly to those who are relatively more risk averse (Lang, Palacios, 2018), since teachers enjoy greater job stability and typically have longer holidays.

As a response to this worrying picture, over the last decade the Chilean government has introduced reforms to improve the attractiveness of the teaching profession (OECD, 2017b; Santiago et al., 2013). These reforms include increasing teachers salaries, improving and expanding professional development opportunities, and dedicated financial aid to students pursuing teaching programs. The most recent and well-known is the introduction in 2011 of the "Teaching Calling Scholarship" (*Beca Vocacion de Profesor*) or BVP. The BVP scholarship covers all tuition costs

Table 3.2: Characteristics of Undergraduate Degrees Offered by Type

	Type of degree					
	Teaching		STEM		Others	
Number of programs	256		384		663	
Duration (years)	4.65	(0.03)	5.21	(0.04)	4.97	(0.02)
Annual tuition (2015 USD)	3,273.81	(43.83)	4,602.01	(48.85)	4,843.54	(56.16)
PSU score among enrollees						
Mean	567.61	(2.08)	588.44	(2.42)	581.65	(2.08)
Min	505.20	(1.80)	503.69	(2.31)	502.32	(2.15)
Max	660.31	(3.71)	697.79	(3.03)	685.15	(2.26)

Note: The unit of observation is a degree-university pair. The sample includes all degrees offered in universities in centralized admissions system. We exclude from the sample degrees offered by Professional Institutes and universities not in the centralized admission process. Teaching programs include only primary and secondary teaching programs. Pre-primary and special education majors are categorized as "others". Standard deviation in parentheses. Source: DEMRE and SIES, 2015.

of accredited teaching programs to students scoring 600 or more in the PSU exam (top 30%), irrespective of their income.[5]

Until the tuition-free reform, the BVP was the only public scholarship covering the full tuition costs of a university program in Chile, and had succeeded in attracting students with high PSU score into teaching programs. Alvarado et al. (2012) finds that in its first year of operation, eligible students were significantly mote likely to apply to, and enrolling in teaching programs. Consequently, the share of students from the top 30% increased from 10.7% in the year prior the BVP to 18.1%. Moreover, the evidence shows that, in relative terms, the BVP helped students from poorer backgrounds more than richer students (Claro et al., 2013). Finally, Castro-Zarzur (2018) studies the local impact of the BVP using a regression discontinuity design approach (RDD). She finds that between 2011 and 2015, the BVP increased the probabilities of applying and enrolling in teaching programs around the 600-point threshold on average by 22%

[5]In addition to covering the tuition costs, student scoring above 700 also recieve a monthly stipend of about USD$150. Finally students scoring 720 or more in the PSU, covers the costs of a one-semester exchange program in a foreign university (Bonomelli, 2017).

and 30%, respectively.[6] Our paper complements the findings of Castro-Zarzur (2018) by looking at a different question; we study how making college tuition free affects the pool of students pursuing a teaching career. Although to answer this question we leverage the eligibility of the BVP scholarship, we are not interested in the impact of the BVP alone. Furthermore, our policy recommendations are relevant to both tuition free college and ability-based scholarships (such as the BVP).

3.4 Data

We use four different administrative datasets in this study. First, we use data on student applications to universities in the centralized admission system. These data are managed and maintained by the *Departamento de Evaluación, Medición y Registro Educacional* (DEMRE), an entity that is part of Universidad de Chile, the main public university and one of the 33 higher education institutions affiliated to the centralized admission system as of 2016. The DEMRE designs the PSU test and administers it nationwide. The PSU evaluates students in four subjects: math, language, science, and social sciences. Scores range from 150 to 850 with a mean of 500 and standard deviation of 110. The PSU dataset contains individual information on PSU score, as well as demographics such as gender and age, and self-reported socioeconomic characteristics such as family income (discrete categories), household size, and parental education. It also contains students' high school GPA, their year of graduation and the school they graduated from and its nature (i.e., public, voucher, private). Students take the PSU at the end of high school but it is relatively common for individuals to retake the exam. We restrict our sample to students who

[6]Similar results were found by Gallegos et al. (2019) who also use an RDD approach to estimate the local impact of the BVP across time.

graduated from high school the year before entering higher education.

For those who apply to the centralized admissions system after taking the PSU exam, we also observe the ranking list of up to 10 major-university pairs submitted by each student in each year. Students and programs are matched using a deferred-acceptance algorithm, which takes into account student rankings, programs' preferences (students with higher scores are preferred to student with lower scores), and quotas. Therefore, students may not be matched with their most preferred option. The dataset reports the outcome of the application process. Specifically, we observe the acceptance/rejection decision to each program students applied to.

Second, we use information on the supply of higher education programs, provided by the National Education Council (*Consejo Nacional de Educación*, CNED) and the Ministry of Education. The dataset includes a comprehensive list of programs offered by all higher education institutions in each academic year. The program-level data contain information on tuition fees, field of study, length, geographical location, and application requirements, among others.

Third, we obtain measures of academic proficiency from the SIMCE (*Sistema de Medicion de Calidad de la Educacion*, Education Quality Measurement System), a yearly national test that is part of an information system established by the Chilean government to periodically evaluate learning outcomes across the country. The SIMCE tests are taken by elementary (2nd, 4th, and 6th grades), middle school (8th grade), and high school (10th and 11th grades) students. Their main goal is to provide information about the learning achievements of students in a wide range of knowledge areas that are part of the national curriculum. Currently, the tests measure numeracy and language and communication skills, as well as knowledge of natural sciences, history, geography, social sciences, and English. Since its introduction (1980s) the SIMCE has undergone changes regarding the subjects tested as well as the grades in which the examinations are administered. Each year, the National Agency for the Quality of Education decides which grades will take the SIMCE tests as well as the corresponding areas of knowledge that will be evaluated. It is common for students to take the SIMCE two or three times during their school years. We choose to focus on the high school 2^{nd} grade SIMCE because it is the one that correlates the most with the PSU—which ultimately defines college acceptance—while still being far enough in the past to not influence college/program choice. Importantly, students never learn their own SIMCE scores.

Lastly, we obtain individual income quintiles from the FUAS (*Formulario Unico de Acreditacion Socioeconomica*) form, which is completed by all prospective students applying to any public higher education scholarships or tuition aid, including the BVP and the free-tuition program. Along with the FUAS form, applicants must submit their households' relevant tax and income documentation to the Ministry of Education, which is in charge of assessing these information and determining each student's income quintile. Throughout the income assessment process, the Ministry of Education may cross-validate individual and household data with information from other sources such as the Ministry of Social Protection.

3.5 Empirical Strategy

3.5.1 Application Behavior

A longstanding literature on college enrollment and major choice (see for instance, Altonji et al., 2016; Arcidiacono, 2004; Arcidiacono et al., 2012) documents that students' choices depend on the pecuniary and nonpecuniary cost and benefits of each degree (college-major pair). The utility payoff of pursuing a degree is a function of the expected lifetime profile of earnings, tuition fees as well as nonpecuniary factors that may affect student choices (e.g., job satisfaction, hours worked, etc.). Therefore, in a simple choice model students will pursue a teaching degree if the utility payoff of teaching is greater than the utility payoff of pursuing alternative degrees.

By bringing down and equating the prices of all degrees for eligible students (i.e., low income) to zero, the introduction of tuition-free college changed the potential utility payoffs of all college degrees and also lowered the liquidity constraint to studying expensive degrees. As documented in Gonzalez-Velosa et al. (2015) and Espinoza, Urzua (2016), teaching degrees in Chile have the lowest tuition fees (Table 3.2) and their internal rate of returns are significantly lower relative to other fields. In this context, net of any general equilibrium effects, the relative price drop is larger for non-teaching degrees.

The objective of this paper is to study the impact of the policy on the sorting of students pursuing a teaching program. For such purpose, we use the introduction of the tuition-free college as a natural experiment to analyze the extent to which students may switch from teaching majors to majors in other fields as a response to the policy. Among all groups, we expect that high-ability/low-income students should be the most likely to forego teaching (compliers), as in the absence of the policy, their preference for teaching might have been largely driven by the its low relative price as a result of the BVP scholarship. In contrast, we expect that for some students, the policy should have had marginal to no effects. Among this group are, for example, high-income students who still need to pay for college and low-ability/low-income students who are ineligible for the BVP scholarship (always takers). We also expect a limited impact on students who derive large non-pecuniary utility from teaching (never takers) for whom the relative price drop might have not been large enough to compensate for the non-pecuniary reward to teaching. Similarly, the policy should not have affected the choice of students with a strong preference

Table 3.3: Scholarship Eligibility by Household Income and PSU Score: 2015 - 2016

	$PSU < 500$		$500 \leq PSU < 600$		$PSU \geq 600$	
	2015	2016	2015	2016	2015	2016
Income						
Decile $\geq$8					*BVP*	*BVP*
Decile $= \{6, 7\}$			B&J	B&J	B&J, *BVP*	B&J, *BVP*
Decile $\leq$5		**Free**	B&J	B&J, **Free**	B&J, *BVP*	B&J, *BVP*, **Free**

Note: "Free" stands for the free college policy. "B&J" stands for *Beca Bicentenario (B)* and *Beca Juan Gomez Millas (J)*. The B&J grants provided up to 80% of tuition remission to students who scored more than 500 points in the PSU and whose household income fell in the bottom 70% of the household income distribution. The B&J grants had no field requirement. The *Beca Bicentenario* grant funded students enrolled in CRUCH universities, while the *Beca Juan Gomez Millas* grant also funded students in non-CRUCH institutions that were quality-accredited. The free-college policy had no explicit PSU cutoff. So, even students with $PSU < 500$ were eligible. However, given their low score, they were unlikely to be accepted to any program.

against teaching degrees (always takers) for whom the tuition-free policy decreases the relative payoff to pursuing teaching degrees even more.

We base our identification strategy on the income and ability thresholds that determine the eligibility to the BVP scholarship and the tuition-free benefit. Table 3.3 helps explain how we use the interplay of eligibility criteria of both financial-aid programs to that aim. It shows the scholarships offered in 2015 and 2016 along with their PSU score and income eligibility criteria. Our empirical strategy compares the application behavior of low-income students (i.e., household income decile ≤ 5) who score above and below the BVP's eligibility score (i.e., $PSU \geq 600$), before (2015) and after (2016) the introduction of tuition-free college.

As Table 3.3 suggests, we can use additional variation to strengthen our empirical strategy. Specifically, we use the changes in application behavior from students in the top 50% of the income distribution—who are not eligible for tuition-free benefits—to control for confounding effects that could stem from cohort-specific changes. To this end, we implement a triple difference strategy where the third difference comes from the income eligibility criteria.

Since we observe household income quintiles instead of deciles, we chose to include all the applicants whose income belongs to the third quintile (i.e., fifth and sixth deciles) into the income-eligible pool. Therefore, our estimates of the effect will be conservative as we mix into the treated group some applicants for whom the introduction of the policy would not merit any behavioral change. Let $Y_{i,t}$ be an indicator variable that takes the value of 1 if student i's household income belongs to the bottom 60% of the income distribution and 0 otherwise.[7] Thus, we can write our estimating equation as:

$$\begin{aligned} P_{i,t} = \beta_0 Post_t + \gamma_0 \mathbf{1}[PSU_{i,t} \geq 600] + \gamma_1 Post_t \mathbf{1}[PSU_{i,t} \geq 600] + \beta_Y Y_{i,t} \\ + \beta_g Y_{i,t} Post_t + \gamma_g Y_{i,t} \mathbf{1}[PSU_{i,t} \geq 600] + \delta_g Y_{i,t} Post_t \mathbf{1}[PSU_{i,t} \geq 600] \\ + X_{i,t}\theta + \mu_s + \varepsilon_{i,t} \end{aligned} \quad (3.1)$$

where $P_{i,t}$ is the outcome of interest (e.g., applying to a teaching program or not) of student i at time t, $Post_t$ takes the value of 1 if $t \geq 2016$ the year of the introduction of the tuition-free college and 0 otherwise, $\mathbf{1}[PSU_{i,t} \geq 600]$ is an indicator variable that takes the value of 1 when student i scores more that 600 points in the college entrance exam, $X_{i,t}$ are individual level controls like gender, PSU score and contains a constant, and μ_s is a school-level fixed-effect.[8]

[7] In Appendix **??**, we proxy income eligibility with two long-term household income proxies: school type and mother's education. These proxies do not let themselves directly to the estimation of a triple-difference specification as in equation (3.1). Therefore, we implement a somewhat different specification. We use $Y^g_{i,t}$ as an indicator variable that takes the value of 1 if student i's income belongs to category g and 0 otherwise. By convention, we use $Y^0 = 1$ to account for the base terms that have no income-group interaction (i.e., $\beta_0 Post_t + \gamma_0 \mathbf{1}[PSU_{i,t} \geq 600] + \delta_g Post_t \mathbf{1}[PSU_{i,t} \geq 600]$). Thus we can write our estimating equation as:

$$P_{i,t} = \sum_{g=0}^{G-1} \left(\beta_g Y^g_{i,t} Post_t + \gamma_g Y^g_{i,t} \mathbf{1}[PSU_{i,t} \geq 600] + \delta_g Y^g_{i,t} Post_t \mathbf{1}[PSU_{i,t} \geq 600] \right) + X_{i,t}\theta + \mu_s + \varepsilon_{i,t}$$

By providing an estimate of δ_g for each income group, we can see if the policy affected more students from poorer families than wealthier ones.

[8] We include the PSU score as a control because there is a very strong inframarginal (i.e., away from 600 points) relation between PSU scores and college application outcomes. We believe that partialling-out that heterogeneity is important. Although this can resemble a difference-in-discontinuity approach, it is not because we leverage on

δ_g is thus the parameter that captures the tripe-difference estimator that captures the effect of the tuition-free college on those that would have been eligible for the BVP scholarship.[9]

It is worth noting that throughout the period under study there were additional scholarships available to students. In particular, the *Beca Bicentenario* (*B* in Table 3.3) and the *Beca Juan Gomez Millas* (*J* in Table 3.3) were also available to students scoring over 500 points in the PSU who wanted to pursue a college program in any field of study and came from households with incomes below the 70^{th} percentile. Based on Table 3.3, it is easy to see that our difference-in-difference identification strategy differences-out the effects of the $B\&J$ grants, whose incentives and eligibility criteria did not change. Also, unlike the BVP or the tuition-free college policy, the *B&J* grants only cover up to 80% of the full tuition costs. The remaining 20% was still meaningful for families in the margin. It corresponded to approximately 150% of the yearly legal minimum wage, which is economically significant as per-capita incomes in the fifth decile—the tuition-free college policy eligibility threshold—ranged between 56% and 69% of the legal minimum wage in 2015.[10]

In practice, the introduction of free college changed the relative price of majors for low-income applicants, although it did so differently based on their PSU performance. Low-income students with PSU scores over 500 but less than 600 points went from 80% tuition remission in any major (through *B&J* grants) to 100% free-tuition college. Low-income, high performing students with PSU scores above 600 went from either 80% tuition remission in any major (through *B&J* grants) or free tuition for teaching majors (through BVP) to universal tuition-free college.

differences in outcomes away from the threshold. Our results do not stem from local differences around the cutoff. For that same reason, we believe that our strategy would not resemble an RD, even if we allowed for different coefficients for the PSU before and after the cutoff.

[9]We need to assume that the effect of the BVP is stable across years, which Castro-Zarzur (2018) shows in her paper.

[10]https://portales.inacap.cl/becas-y-financiamiento-old/que-son-los-deciles-y-como-se-calculan

In other words, the introduction of free college meant that for the first group of low-income students (i.e., $500 \leq PSU \leq 600$) any college major become cheaper, while for high performing low-income students any college major—except for teaching—became cheaper.

Unbiased estimation of the effect of tuition-free college on applications to teaching programs requires stability in applicants' preferences for majors before and after the policy change. However, the introduction of free college induced college applications from students that otherwise would not have applied. In fact, the number of applicants jumped between 2015 and 2016 by 12%, more than doubling the growth rate experienced between 2014 and 2015 (4.58%). This expansion might result in a change in the average characteristics of the applicants. In particular, we worry about changes prompted by the fact that the 'new entrants' might be less interested in teaching majors because their likes had not been attracted to teaching majors by the BVP in the past. Thus, the free tuition policy prompts a compositional change that would mechanically bring down the share of applicants to teaching majors, biasing our estimates upward. In Appendix C.3, we analyze in detail this concern. We show that, although theoretically possible, it has little bearing on our results. The main reason for it is that most of the 'new entrants' (8 out of every 9) score less than 600 points in the PSU, which implies that the vast majority of new entrants would not have been eligible for the BVP anyway. Appendix C.3 presents various estimations, including bounding exercises that show that at least 90% of our estimated effects are due to students moving from teaching to other majors and not due to the changes in average preferences prompted by the arrival of 'new entrants'.

3.5.2 Effect on Academic Quality

We are interested in evaluating the effect of the tuition-free college policy not only on the application behavior and preferences between majors of the incoming students, but also on how that sorting changed the distribution of academic quality of students across majors. This stems from the extensive evidence showing that high-achieving students are more likely to become better teachers (Auguste et al., 2010b; Seng Tang, 2015). To do so, we implement versions of (3.1) with measures of academic proficiency as dependent variables and comparing the enrollees to teaching programs with those who enrolled to other fields:

$$
\begin{aligned}
Score_{i,t} = \beta_0 Post_t + \gamma_0 Teach_{i,t} + \delta_g Post_t Teach_{i,t} + \beta_Y Y_{i,t} \\
+ \beta_g Y_{i,t} Post_t + \gamma_g Y_{i,t} Teach_{i,t} + \delta_g Y_{i,t} Post_t Teach_{i,t} + X_{i,t}\theta + \mu_s + \varepsilon_{i,t} \quad (3.2)
\end{aligned}
$$

where $Teach_{i,t}$ takes the value of 1 if the applicant was accepted to a teaching program and 0 otherwise. In this case, δ_g provides a triple-difference estimate of the effect the introduction of tuition-free college had on the average academic proficiency of accepted applicants. By including the comparison between teaching and non-teaching majors as one of the differences, we capture any overall shifts in scores that the free college policy might have caused. As measures of academic proficiency we use PSU score, high school GPA, and scores of the SIMCE standardized tests which students took when they were in their high school sophomore year. We refer to those past SIMCE scores instead of using the PSU for this particular estimation because application decisions may depend on the PSU score obtained. Therefore, there is a feedback process between scores and application behavior that would bias the results. Instead, the 10^{th} grade SIMCE scores, having no bearing on the college acceptance decisions, do not influence students' application behavior.

It is important to note that in order to avoid an increase in the number of vacancies as a response to an expanded financial support (see evidence in (Abraham, Clark, 2006; Dynarski, 2003)), universities were not allowed to expand the supply. Indeed, Table C.1 in the Appendix shows that universities complied with the restriction, as the number of vacancies did not increase after the tuition-free policy.

3.6 Results

3.6.1 Application Behavior

We first show that the introduction of free college had an immediate effect on student application behavior. Table 3.4 compares the field of study of the students' most preferred choice before and after the introduction of the tuition-free college policy. It presents simple mean comparisons between application frequencies to different fields. We split the sample in two. The top panel shows the frequency of applications among students scoring below the 600-point threshold that defines the eligibility to the BVP scholarship. Panel B replicates these statistics for students who are eligible for the BVP, those who score above 600 points. The last two columns show the before-after difference and its statistical significance. The table shows a statistically significant drop in the fraction of students applying to teaching degrees as their most preferred choice among top performers. In the year before the introduction of free college, 4.4% of students scoring more than 600 points applied to a teaching degree as their most preferred choice. After the policy the fraction dropped to 3.6%. That is, among those that are eligible for the BVP

Table 3.4: Change in Application Behavior by PSU Range

Panel A: PSU<600

	Before	After	Diff.	p-value	Diff. (%)
Business	0.102	0.108	0.006	0.001	6.05
Education	0.111	0.109	-0.002	0.293	-1.85
Health	0.288	0.299	0.011	0.000	3.54
Social Sciences/Humanities	0.139	0.145	0.006	0.015	3.83
STEM	0.220	0.199	-0.021	0.000	-9.44
Others	0.140	0.141	0.001	0.621	0.77

Panel B: PSU≥600

	Before	After	Diff.	p-value	Diff. (%)
Business	0.105	0.105	0.000	0.792	0.68
Education	0.044	0.036	-0.008	0.000	-17.78
Health	0.249	0.279	0.030	0.000	12.13
Social Sciences/Humanities	0.105	0.108	0.003	0.325	2.54
STEM	0.350	0.319	-0.031	0.000	-8.69
Others	0.148	0.153	0.005	0.144	3.10

Note: For students applying to degrees in the centralized matching system, we compare the probability of applying to a degree (in each of the fields) as top choice, before and after the implementation of the tuition-free policy. We restrict the sample to students who graduated from high school the year before entering higher education. The last two columns test the statistical significance of these differences.

scholarship, the introduction of free college causes a decline in the probability of applying a teaching degree as top choice of about 17.8%. The drop is largest across all fields of study. Such behavior supports the hypothesis that the introduction of free college decreased the return of pursuing a teaching degree *vis-a-vis* degrees in other degrees that compete for similar students. In contrast, such behavior is not mirrored by students scoring below the 600-point threshold. There is only a slight and non-significant decline from 11.1% to 10.9%.[11]

Next, we use the regression framework detailed in Section 3.5 to estimate the effect of free college on student application behavior. We estimate equation 3.1 using two alternative

[11]Table 3.4 shows that STEM majors also experienced a decline in the fraction of applicants. We analyze this interesting phenomenon in a separate paper (Castro-Zarzur et al., 2018). Importantly, for the purpose of the this paper, the drop in STEM applications is common to both sides of the 600-point threshold. Therefore, it is not a confounding factor in the margin we are interested in.

dependent variables. The first takes the value of 1 if the student applied to at least one teaching degree, independent of the order in which she listed it. In the second one, the dependent variable takes the value of 1 if the student applied to a teaching degree as her top choice. Table 3.5 present our main set of results. We report the changes in applicaiton behavior between 2015 and 2016 (i.e., before and after the introduction of the tuition-free college policy) for each eligibility group. We then present the difference-in-difference estimate for each category of free college eligibility. That is, the comparison of the changes in applicaiton behavior between BVP elibility groups while keeping the free college income eligibility fixed. As indicated in Section 3.5, we can obtain an estimate of the effect of the tuition-free college policy on the applicaiton behavior from the difference-in-difference estimate among those whose household income makes them eligible for free college. Furthermore, Table 3.5 presents triple-difference estimates of the effect, in which we substract the difference-in-difference estimates of those ineligible to free college from the difference-in-difference parameter obtained from free college elible students. The additional difference will capture any potential confounding factors that stem from cohort effect affecting all applicants regardless of their household income.

Table 3.5 shows that the introduction of tuition-free college reduced the applicaiton to teaching programs among students that were eligible to the BVP scholarship. We find that applications to teaching majors among BVP-eligible students that come from poor households—and thus eligible to free college—dropped by 2.57 percentage points, while they did not change among BVP-ineligible or free college ineligible students. The difference-in-difference estimate indicates an 2.3 percentage points drop in the application to teaching majors among students from poor households. That amounts to a drop of about 13.5% in application to teaching majors from one year to the next. As expected, the introduction of the tuition-free college policy did not affect

Table 3.5: Effect of Free College on Applications to Teaching Majors Triple-Diff

	Apply to at least one teaching major			Apply to teaching major as top choice		
	$PSU < 600$	$PSU \geq 600$	Diff-in-Diff	$PSU < 600$	$PSU \geq 600$	Diff-in-Diff
$\Delta(t)$						
Household Income						
Decile ≤ 6	-0.0028	-0.0257***	-0.0230***	-0.0081***	-0.0181***	-0.0100**
	(0.004)	(0.005)	(0.006)	(0.002)	(0.003)	(0.004)
Decile > 6	-0.0014	-0.0029	-0.0015	-0.0034	-0.0028	0.0006
	(0.004)	(0.003)	(0.005)	(0.003)	(0.002)	(0.004)
Triple-Diff			-0.0215***			-0.0106**
			(0.007)			(0.005)
Obs			154,653			154,653
Avg. de. var.			0.171			0.086

Note: We present the size of the effect for each category calculated based on the regression results presented in Table C.11 in the Appendix. The diff-in-diff column represents the difference-in-difference estimate from substracting the change in applications between 2015 and 2016 among those who score more that 600 points in the PSU minus the change in applications between 2015 and 2016 among those who score less that 600 points in the PSU for a given type of income eligibility. The Triple-Diff. coefficient is the difference between the difference-in-difference coefficient show above (the δ_g parameter in Equation 3.1). All regressions include school fixed-effects, gender, and linear PSU score controls. The 600 threshold for the PSU score was chosen based on the minimum score required to apply for BVP scholarship. Standard errors clustered at the school level. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

the applicaiton behavior to teaching programs of those among richer household who would not be eligible. Therefore, the triple-difference estimate is no different from the difference-in-difference estimate among free college eligible students.

When we analize how the introduction of the tuition-free college policy affected the choice of a teaching major as the top preference, we find similar results. We see that the incidence of choosing a teaching major as the top preference fell among free college eligible students only. Interestingly, it fell both among BVP eligible and ineligible applicants, indicating that some poor students might have chosen a teaching major as their top preference in the absence of free college because it is the cheapest program even for those who are not BVP-eligible. However, as expected, the drop was significantly larger among the BVP-eligible applicants. The difference-in-difference estimate indicates a one percentage point drop. Therefore, the introduction of the tuition-free college reduced the incidence of choosing a teaching major as the top preference by 11.6%. Again, the triple-difference estimate is no different from the difference-in-difference estimate among free college eligible students.

3.6.2 Robustness Checks

3.6.2.1 New entrants

The introduction of free college increased the number of applications by 3.4 percentage points, mostly coming from outside the top three deciles of the PSU distribution (Castro-Zarzur et al., 2018). Thus, some of the effects found so far could be due, in part, to "new entrants"—people who would not have applied in the absence of the policy—rather than to a "reshuffle" across majors of people who would have applied anyway. To address that, in Appendix C.3, we perform several robustness tests.

First, we rerun our estimations on a 2016 subsample that is most likely to have applied without the implementation of free college. We define that subsample based on a propensity score estimated from observed characteristics of students and their application decisions in 2015. That is, we model college application decisions with gender, mother's education, grade math and language SIMCE scores, and the school students come from. The model reports an R^2 of 0.91. We take the estimated parameters and predict the propensity of having applied to college based on their observable characteristics for the 2016 sample. Thus, we drop from the 2016 sample those who applied but were less likely to do so according to the propensity score. We drop around 9,000 applicants from 2016 in order to match the number of applicants in 2015. The results excluding the"new entrants" are very similar to the ones obtained using the whole sample, indicating that selection on observables (i.e., scholastic achievement) did not play an important role in determining our estimated effects.

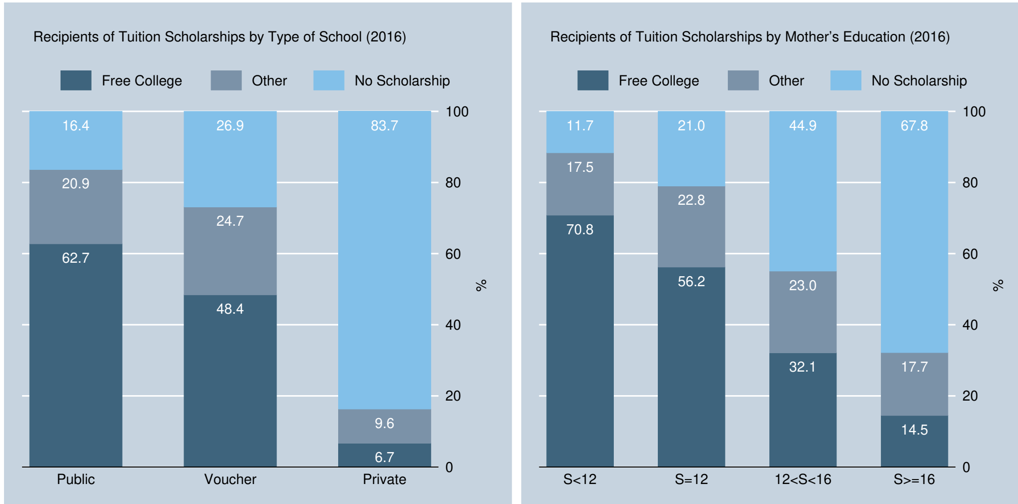
Second, we address the possible selection on unobservables. "New entrants" could have greater distaste for teaching majors because the BVP had already attracted to college those in past cohorts with a strong preference for teaching. Thus, the free tuition policy prompts a compositional change that would mechanically bring down the share of applicants to teaching majors. We find that "new entrants" have overwhelmingly lower PSU-scores. In fact, out of the 9,000 "new entrants", roughly 8,000 score less than 600 points in the PSU and would not have been eligible for the BVP. The BVP should not have generated any selection based on tastes on that group of applicants. In fact, Table C.6 shows that *head count* of applicants to teaching majors *grew* for the students with PSU¡600, while it fell for students with PSU¿600. We test this further by recognizing that the distribution of major *preferences* in 2016 is more complete than in 2015 because financial constraints truncate the latter. Thus, based on the 2016 distribution of revealed preferences, we create a counterfactual applicant population in 2015 (the one that we would have observed without truncation) and compare the application behavior between the two periods. We expand the 2015 sample to include students that would have applied if college was tuition-free, but impose that none of the BVP-eligible "new entrants" would choose a teaching major. That way, we create an extreme scenario that bounds our estimates. We find that our estimates remain almost unchanged.

Finally, we include the estimation of a model that incorporates the selection into applying to college as a first stage. That way, we estimate the triple-difference specifications embedded in a control function approach. We use excluded variation in long-term application rates by *comuna* and school type and the students' high school sophomore-year standardized tests—importantly not the tests used for college applications—to model the selection process. Our results are robust to the introduction of the control function, with the triple-difference estimates of the effects remaining virtually unchanged.

3.6.2.2 Using long-term income proxies

In addition to using household income quintiles for determining the eligibility criteria, we proxy income eligibility with two measures that are known to closely correlate with family income: the education level of the student's mother and the type of high school in which the student finished. These measures indicators of long term socio-economic status, allow us to isolate our estimates from possible strategic behaviors in which families at the margin could reduce their labor supply in order to become eligible for the big financial relief of not having

Figure 3.2: Recipients of Tuition Scholarships in 2016



Note: Both figures show the share of students from the 2016 graduating cohort who enrolled in college and obtain (or not) tuition scholarships. The left panel shows those fractions by school type. The right panel does so by mother's education. $S < 12$: high school dropout, $S = 12$: high school graduate, $12 < S < 16$: technical education or incomplete college, and $S >= 16$: complete college or above.

to pay college tuition. In particular, we split mother education into four categories: high school dropouts (19.4%), high school graduates (36.9%), some tertiary education (22.4%), and college graduates (21.3%). Regarding school type, we use the fact that primary and secondary schools in Chile are classified in three main categories: private, public, and voucher. School types exhibit significant differences between them and are highly correlated with students' socioeconomic background (Correa et al., 2014). Public schools are run by municipalities and publicly funded. Voucher schools, which can be for-profit or not-for-profit, are privately owned and receive a per-student subsidy (voucher) from the state.[12] Finally private schools do not receive any public funds. Wealthier students typically attend private schools, voucher schools are highly demanded by the middle class, and poorer students typically attend public schools (Elacqua, Santos, 2013; Sánchez, 2018).

[12]The fraction of the cost covered by the voucher varies from family to family depending on its socio-economic status and the monthly cost of the school.

Figure 3.2 shows that school type and mother's education are good predictors of access to need-based college scholarships, thus reflecting the incidence of long-term credit constraints. For instance, while 83.6% of applicants coming from public schools have access to a scholarship, only 16.3% of applicants coming from private schools do so. That is almost an exact reversal in the likelihood of need-based accessing scholarships. In that domain, we see that the incidence of access to scholarships among applicants graduating from voucher schools is closer to that of public schools than to that of private schools. Figure 3.2 shows that 88.3% of applicants whose mothers were high school dropouts required some kind of scholarship, while less than a third of students whose mothers completed a college degree applied to college with a scholarship at hand.

Regression results are presented in Tables 3.6. Their structure is similar to that of Table 3.5.

Table 3.6 shows that the probability of listing a teaching major as a choice falls significantly only among those coming from public and voucher schools that scored above the 600-point threshold. That is, with the introduction of the free-college policy, the probability of considering a teaching major falls for relatively poor BVP-eligible students. The drops are not only statistically significant but economically meaningful. The difference-in-difference estimate finds that the likelihood of listing a teaching major among public and voucher school students falls by 2.5 and one percentage points. These drops correspond to a 14.6% and 6% decrease, respectively. In contrast, we find no significant changes in the probability of applying to teaching programs among the relatively wealthy students (i.e., those graduating from private schools) who are not eligible for free college.

Our findings remain overall consistent when we proxy household income with mother's

Table 3.6: Effect of Free College on Application Behavior to Teaching Majors by School Type and Mother's Education

	$\Delta(t)$ in the Prob. of listing at least one teaching major					
	$PSU < 600$		$PSU \geq 600$		Dif-in-Dif	
Household Income Proxy:						
School Type (N=154,277)						
Public	0.002	(0.006)	-0.023***	(0.008)	-0.025***	(0.009)
Voucher	-0.004	(0.003)	-0.014***	(0.005)	-0.010*	(0.019)
Private	0.000	(0.007)	-0.001	(0.003)	-0.001	(0.019)
Mother's Education (N=140,984)						
$S < 12$	-0.005	(0.006)	-0.027**	(0.012)	-0.022*	(0.013)
$S = 12$	-0.004	(0.004)	-0.019***	(0.006)	-0.014**	(0.027)
$12 < S < 16$	-0.003	(0.006)	-0.010*	(0.005)	-0.007	(0.027)
$S \geq 16$	0.005	(0.007)	-0.007*	(0.004)	-0.012	(0.027)
Avg. depend. var.						0.171

Note: We present the size of the effect for each category calculated based on the regression results presented in Tables C.12 and C.13 in the Appendix. S stands for mother's years of schooling. The distribution of mothers' schooling years is as follows: 19.4% have incomplete high school or less ($S < 12$), 36.9% are high school graduates ($S = 12$), 22.4% went beyond high school but did not complete a 4-year tertiary education ($12 < S < 16$), 21.3% have a college degree or more ($S \geq 16$). All regressions include gender, and linear PSU score controls. The model using school type includes *comuna* fixed-effects, while the model using mother's education include school fixed-effects. The 600 threshold for the PSU score was chosen based on the minimum score required to apply for BVP scholarship. Standard errors in parentheses are clustered at the school level. *$p < 0.1$, **$p < 0.05$, ***$p < 0.05$.

years of education.[13] BVP-eligible students from relatively poor socioeconomic backgrounds (i.e. with relatively less educated mothers) respond to the introduction of free college by opting out of the teaching profession. The probability of applying to a teaching program decreases significantly for BVP-eligible students with high school dropout mothers (18%), high school graduate mothers (14.3%). And they did so by significantly more than applicants from equally poor household who

[13]Results in the top and bottom panels of Table 3.6 are not strictly comparable because the ones collected in the former use *comuna*-level fixed-effects, while the ones presented in the latter come from estimations using school-level fixed-effects. The use of school-level fixed-effects may be more desirable as they capture any unobserved school traits that may correlate with college application. However, being the school type a time invariant school characteristics, its effect on a given outcome is not identified in a school-level fixed-effects specification. We consider that the results coming from the models with school types provide relevant insights and thus are worth reporting despite their lack of school-level fixed-effects.

would not have been eligible for the BVP. The difference-in-difference estimate indicates that the tuition-free college policy caused applications to teaching majors from students with high school dropout mothers and high school graduate mothers to drop by 12.8% and 8.2% respectively, while we find non-statistically significant differences among students with the relatively more educated mothers.

3.6.3 Effect on Enrollment

Table 3.7 shows that the free college policy not only altered application behavior to teaching programs, it also ended up changing the pool of students that were accepted to them. It documents that the students who were eligible to the tuition-free college policy were less likely to join a teaching major. Furthermore, that drop is more pronounced among those who also would have been eligible to the BVP scholarship. In fact, out triple-difference estimator indicates that the free college policy caused a 1.17 percentage points drop in the acceptance rate to teaching majors. That amounts to a drop of 18% in acceptance to teaching majors relative to pre-free college policy levels.

The change in application behavior due to the introduction of free college translated into a change in the relative academic proficiency of the students that were offered admission in different programs as measured by the PSU score. In Table 3.8, we present the changes in academic proficiency due to the introduction of free college for teaching programs and non-teaching programs separately. In Panel A, we show that while the introduction of free college did not change the average PSU score of the students accepted to non-teaching programs, the scores among the accepted to teaching ones did deteriorate. Importantly, the deterioration in the quality

Table 3.7: Effect of Free College on Acceptance to Teaching Majors Triple-Diff

	Accepted to a teaching major					
	$PSU < 600$		$PSU \geq 600$		Diff-in-Diff	
$\Delta(t)$						
Household Income						
Decile ≤ 6	0.0069***	(0.002)	-0.0180***	(0.003)	-0.0110***	(0.004)
Decile > 6	0.0083	(0.002)	0.0002	(0.002)	0.0006	(0.003)
Triple-Diff					-0.0117**	(0.005)
Obs						154,653
Avg. de. var.						0.066

Note: We present the size of the effect for each category calculated based on the regression results presented in Table C.11 in the Appendix. The diff-in-diff column represents the difference-in-difference estimate from substracting the change in applications between 2015 and 2016 among those who score more that 600 points in the PSU minus that change among those who score less that 600 points in the PSU for a given type of income eligibility. The Triple-Diff is obtained be substracting the difference-in-difference result for the income-eligible minus the difference-in-difference result for the income-ineligible. All regressions include school fixed-effects, gender, and linear PSU score controls. Standard errors clustered at the school level in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.05$.

of the students admitted to teaching programs as measured by the PSU score is limited to those eligible to tuition-free college. That drop in the score is not mirrored by comparable students that were accepted into other programs. The triple-difference estimate indicates that the PSU score of accepted applicants to teaching majors drop by five points or 7.4% of a standard deviation due to the introduction of the tuition-free college policy.[14]

These findings should be interpreted with caution as students decide which program to apply to based, in part, on their PSU scores. That is why we consider additional measures of academic proficiency that were collected years before the college application process, and thus avoid the feedback process between PSU scores and application behavior that would bias the results. We focus on their cumulative high school GPA, and the math and language SIMCE

[14] While the standard deviation of the PSU among the entire population that took the exam is 110, when we limit the sample to those who ended up being accepted in traditional universities, the standard deviation of the score becomes 68.5.

scores measured when the students were in their high school sophomore year, that is, two years before they consider their tertiary education decision. Note that college admission decisions do not take into account SIMCE scores.

Panel B of Table 3.8 shows further evidence of the sorting caused by the introduction of the tuition-free college policy. It shows that the high school GPA of the accepted low-income applicants to teaching majors dropped, while the high school GPA of accepted applicants to other majors improved. Relative to applicants accepted to other majors, the GPA of free college eligible applicants accepted to teaching majors dropped by 9.2 points or 10.2% of a standard deviation.

Moving on to earlier scores, those measured during their high school sophomore year, Panels C and D show further evidence on how the introduction of the tuition-free college policy caused a deterioration in the academic proficiency of low-income students who were accepted to teaching programs relative to those accepted to other programs. Especially, in terms of math ability, where such deterioration reached 4.3 points or 9.3% of a standard deviation. These stronger negative results in math compared to language could be due to the fact that students with average language and above average quantitative abilities—relative to those who would enroll in teaching programs—are typically more interested in majors where both types of skills are important (e.g, Business) and have higher chances of succeeding at these types of programs. This result is consistent with the evidence presented on Tables 3.4 and C.15, where we see that the introduction of free-tuition college was associated with an increase in applications to and enrollments in Business, Health, and Social Sciences majors among relatively poor students.

These findings provide evidence in favor of a mechanism in which the BVP scholarship had brought relatively high-quality students who could not afford college. Once the relative profitability of the teaching programs changed again due to the free tuition policy that covered the

Table 3.8: Free College and Academic Proficiency of Accepted Applicants

	$\Delta(t)$ in Score		
	Other	Teaching	Diff-in-Diff
Panel A: PSU (*N=114,879*)			
$Decile \leq 6$	-0.411	-4.746***	-4.335***
	(0.562)	(1.157)	(1.306)
$Decile > 6$	-0.430	0.112	0.542
	(0.623)	(1.812)	(1.877)
Triple-Diff			-4.877**
			(2.235)
Panel B: High School PGA (*N=114,872*)			
$Decile \leq 6$	3.454***	-5.730***	-9.184***
	(0.853)	(1.967)	(2.086)
$Decile > 6$	2.515***	-1.928	-4.443
	(0.853)	(2.707)	(2.778)
Triple-Diff			-4.741
			(3.470)
SIMCE (High School Sophomore Year)			
Panel C: Language (*N=103,572*)			
$Decile \leq 6$	-5.900***	-7.129***	-1.229
	(0.499)	(1.139)	(1.134)
$Decile > 6$	-7.100***	-5.891***	1.209
	(0.510)	(1.528)	(1.563)
Triple-Diff			-2.438
			(1.948)
Panel D: Math (*N=103,499*)			
$Decile \leq 6$	1.921***	-2.336**	-4.257***
	(0.495)	(1.139)	(1.156)
$Decile > 6$	1.443***	0.870	-0.573
	(0.471)	(1.426)	(1.421)
Triple-Diff			-3.684**
			(1.837)

Note: We present change for each category calculated based on the regression results presented in Table C.14 in the Appendix. Each panel represents a regression. All regressions include gender controls and school fixed-effects. Column *Diff-in-Diff* presents the difference-in-difference estimate from substracting the change in scores between 2015 and 2016 among those who were accepted to *Teaching* programs minus that change among those who were accepted to *Other* programs. The Triple-Diff is obtained be substracting the difference-in-difference result for the income-eligible minus the difference-in-difference result for the income-ineligible. Standard errors clustered at the school level in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

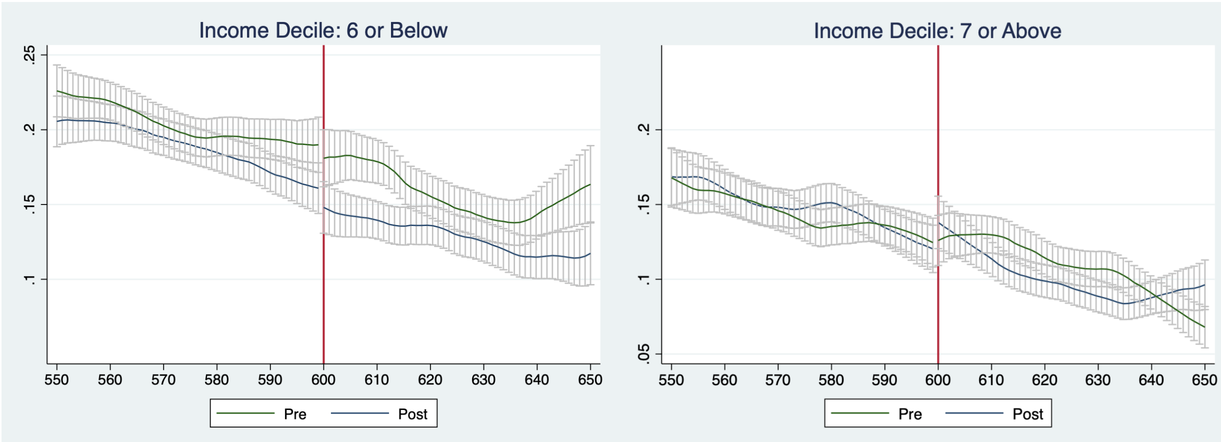
other majors, that subpopulation of high-scoring low-income students shifted to other disciplines, as they no longer saw the teaching majors as the only pathway to a college degree.

3.7 Beyond the Difference-in-Difference

We further use BVP's score eligibility cutoff (i.e., $PSU \geq 600$) to estimate the effects of the introduction of free college on applications to teaching majors. To that effect, we compare the differences in application behavior before and after the introduction of free college. We unpack our difference-in-difference results by identifying heterogeneous effects in application behavior to teaching majors at different PSU levels. We estimate non-parametric functions of PSU scores on application behavior to the left and to the right of the $PSU = 600$ cutoff and before and after the introduction of the free-college policy.

Figures 3.3 present our results. As expected, we find no significant differences in application behavior for those whose PSU scores were less than 600. They simply had the same incentives before and after the introduction of free college as they were never eligible for the BVP scholarship. The interesting results come from the right of the cutoff. First, in line with our parametric difference-in-difference results, we find significant differences in the application behavior of students from poor households who scored more than 600 and were or would have been eligible for the BVP scholarship. We do not find those gaps among students coming from wealthier households. Second, drops in application to teaching programs are not uniform across PSU scores. The biggest declines come from students between 600 and 615 points; those closest to the BVP eligibility threshold. Third, our findings suggest that by 2015 the BVP was not adding (bringing in) extra students to the teaching profession, but avoiding larger shortages of qualified

Figure 3.3: Probability of Listing a Teaching Major Within Choice Set by PSU Score and Income



Note: Local mean smoothing estimated with Epanechnikov kernel. Dependent variable takes the value of 1 of student listed a teaching major anywhere in her choice set and 0 otherwise. Lines labeled *Pre* plot application rates in 2015. Lines labeled *Post* plot application rates in 2016. Capped spikes represent confidence intervals at the 95% level.

applicants to teaching majors.[15] With the introduction of the tuition-free policy, those shortages deepened as it made the BVP scholarship innocuous.

3.8 Discussion

In this paper, we study the extent to which making college tuition free affects the pool of students pursuing a teaching career. We exploit a major reform carried out in Chile in 2016 that eliminated tuition fees of undergraduate careers for students from the poorest 50%. We study how making college tuition-free affects student preferences for teaching programs. We pay particular attention to the behavior of top students who, before the tuition-free policy, were eligible for a generous scholarship- the BVP grant- to study teaching programs. We examine the extent to which the tuition-free policy offset the incentives of the BVP grant, which had shown to be effective in attracting top students to the teaching profession.

[15]This is consistent with findings reported in Castro-Zarzur (2018) where, using a regression discontinuity approach, finds that the positive impacts of the BVP diminished with each incoming cohort.

Our results suggest that the tuition-free policy reduces the demand of top performing from middle to low income families for teaching programs, who were 17% less likely to apply to a teaching programs. In addition, teaching programs became less popular among students. After the tuition-free policy students ranked teaching programs lower when applying to university.

Overall, after the tuition-free policy teaching programs attracted student with lower scores. While the introduction of free college did not change the average PSU score of students accepted to non-education majors, the mean score of those admitted to teaching programs fell by 5% of a standard deviation. This decrease is explained by fewer top students from low socioeconomic backgrounds applying to teaching degrees, as the scores of the wealthier applicants remained unchanged.

Our findings are important for several reasons. First, they highlight the potential unintended consequences of policies distorting equilibrium prices in markets such as higher education. The paper illustrates that the interplay between different financial aid programs is complex and that unintended consequences may arise when new aid become available. Second, our findings illustrate the lack of complementarity between two overlapping benefits: the BVP and the tuition free college reform. Our results suggest that with the tuition-free benefit, the BVP ceased to bring high-performing students into the teaching programs. This is worrisome as there is convincing evidence suggesting that teachers who perform better in high school are more effective than those with lower performance. Third, our findings also provide an important input to the ongoing international debate about making college tuition free college. For example, making college tuition-free policy has emerged as one of the central themes in recent U.S. Presidential Elections where the initiative has shown to be appealing to a numbers of policymakers.[16] This paper shows

[16]Additionally, the free-college movement in the US has continuously gained strength during recent years. As

a potential unintended consequence of such policy acting through a relative price change which affects the sorting of students into programs. This issue in particularly relevant countries with market-oriented higher education systems, as teaching programs tend to be cheaper and thus have usually been an affordable pathway for low-income students to obtain a higher education degree. In particular, our paper shows that in regards to students' preferences over teaching degrees, the free college policy has the potential to negatively affect long-term teacher quality.

This paper only examines the effects of the tuition-free policy in the teacher quality dimension. We show that the reform may affected teacher long-term quality by attracting students with lower scores to the teaching profession. We do not suggest, however, that the tuition-free policy was not beneficial for students. On the contrary, and as we showed in the paper, students tend to change their preferences for programs in areas that have, on average, higher returns.

of 2019, eleven states—Oregon, Nevada, Arkansas, New Jersey, Maryland, Tennessee, New York, Rhode Island, Delaware, Kentucky, and Indiana—have programs that typically offer two years of free tuition in certain colleges for low- and middle-income students. More recently, in September 2019, New Mexico announced a plan to make college tuition-free for state residents regardless of family income. Some examples of widely known tuition-free college programs include the Kalamazoo Promise Program in Michigan and the State of New York's Excelsior Scholarship program.

Appendix A: Chapter 1 Appendix

Figure A.1: Timeline of the Study

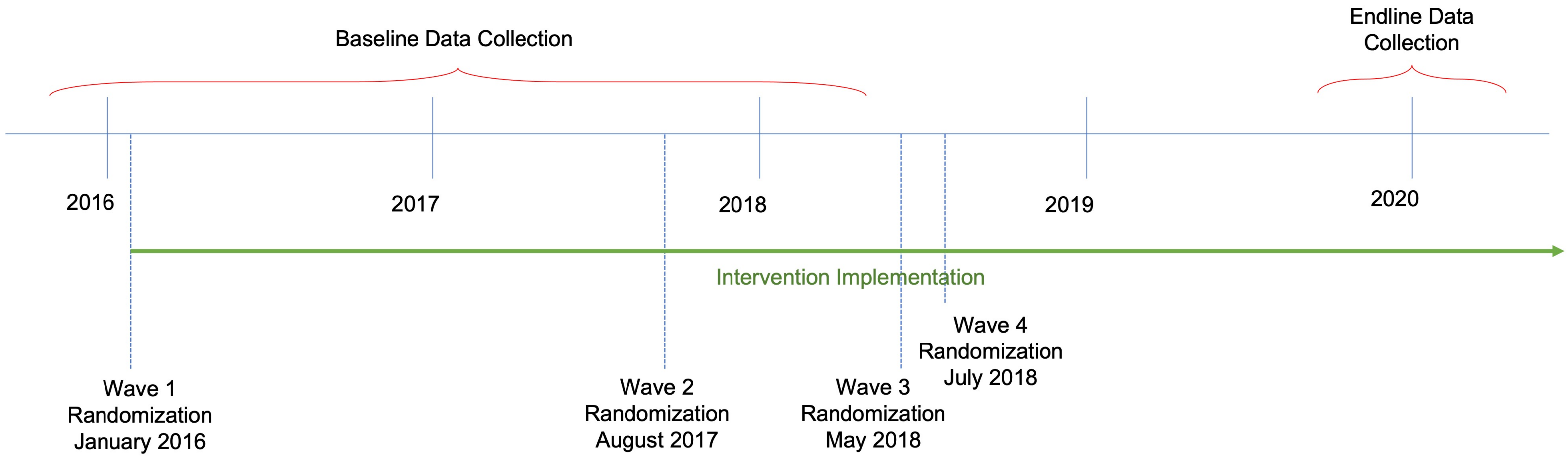
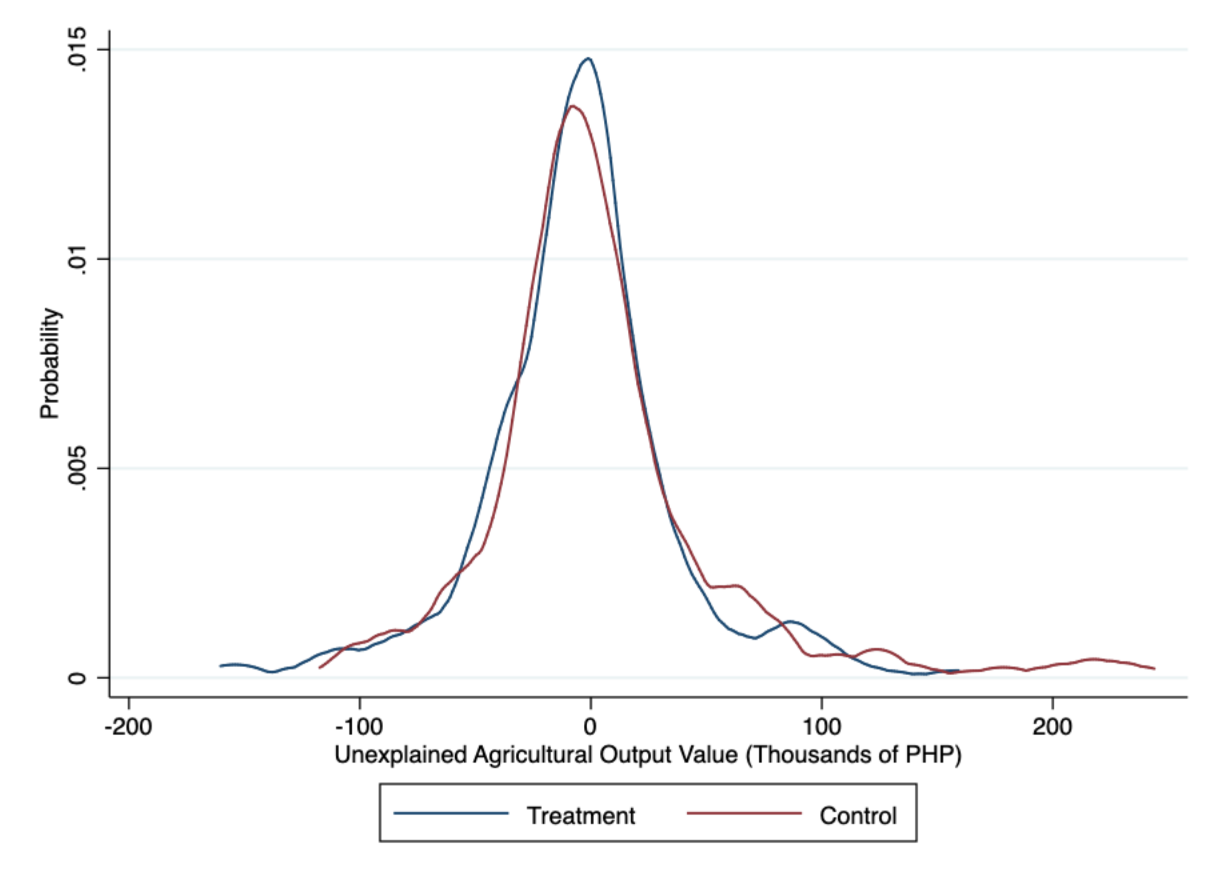


Figure A.2: Density of Residual Agricultural Output Value by Treatment Status



Note: Residual agricultural output values from a regression in which output value is run against randomization-pair fixed effects.

Table A.1: Associations Between Likelihoods of Confiscation and Having a CCLOA Title

	Likelihood: Government Confiscation	Likelihood: Neighbor Confiscation	Likelihood: Last Owner Confiscation	Likelihood: Other Confiscation	Likelihood: Transfer to Children
Parcel has ownership docs	-0.245 **	-0.026	0.021	-0.055	0.194 **
	(0.119)	(0.097)	(0.082)	(0.071)	(0.095)
Mean	1.972	1.439	1.417	1.224	3.581
Obs	533	533	533	533	533

Note: CLOA-clustered standard errors. Randomization-pair fixed effects. Baseline controls included are household size, number of years the ARB has been the primary tiller of the parcel, upland indicator, and per capita household income. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.2: Intervention Compliance

	CCLOA Had Undergone Land Subdivision		Total
	Yes	No	
Treatment	86	58	144
Control	10	139	149

Table A.3: Heterogeneous Effects of Subdivision on Tenure Security. Gender of ARB.

	Restrict Access to Parcel	Secure from Eviction	Worried about Losing Ownership	Likelihood: Transfer to Children	Likelihood: Neighbor Confiscation	Likelihood: Government Confiscation	Likelihood: Confiscation by Others	Tenure Security Index
ITT								
Treat	-0.171*	-0.245*	0.146	-0.052	-0.115	-0.156	0.104	-0.147
	(0.103)	(0.136)	(0.244)	(0.101)	(0.183)	(0.207)	(0.154)	(0.180)
Female	-0.028	0.026	0.139	-0.033	-0.530**	-0.561**	-0.067	0.328
	(0.170)	(0.220)	(0.355)	(0.152)	(0.259)	(0.237)	(0.253)	(0.271)
Treat*Female	0.077	0.157	0.137	0.038	0.637*	0.768**	-0.099	-0.325
	(0.205)	(0.241)	(0.505)	(0.213)	(0.350)	(0.368)	(0.301)	(0.348)
LATE								
Subdivided	-0.358*	-0.515**	0.300	-0.110	-0.257	-0.345	0.219	-0.298
	(0.185)	(0.239)	(0.431)	(0.179)	(0.317)	(0.359)	(0.266)	(0.317)
Female	-0.027	0.021	0.113	-0.034	-0.602***	-0.646***	-0.061	0.377
	(0.151)	(0.194)	(0.319)	(0.137)	(0.232)	(0.203)	(0.230)	(0.236)
Subdivided*Female	0.148	0.306	0.279	0.075	1.271**	1.533**	-0.194	-0.655
	(0.338)	(0.405)	(0.838)	(0.350)	(0.578)	(0.623)	(0.495)	(0.569)
Obs	398	398	398	398	398	398	398	398

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. *$p < 0.1$, **$p < 0.05$, ***$p < 0.05$.

Table A.4: Heterogenous Effects of Subdivision on Tenure Security. Non-Compensable vs. Compensable Plots.

	Restrict Access to Parcel	Secure from Eviction	Worried about Losing Ownership	Likelihood: Transfer to Children	Likelihood: Neighbor Confiscation	Likelihood: Government Confiscation	Likelihood: Confiscation by Others	Tenure Security Index
ITT								
Treat	-0.320**	-0.237	0.265	-0.017	-0.124	-0.100	0.107	-0.230
	(0.147)	(0.164)	(0.261)	(0.112)	(0.240)	(0.227)	(0.219)	(0.232)
Non-Compensable	-0.201	0.015	-0.867	-0.402	-0.828**	-0.704*	-0.134	0.466
	(0.192)	(0.273)	(0.645)	(0.277)	(0.415)	(0.390)	(0.550)	(0.625)
Treat*Non-Compensable	0.252	0.080	-0.267	0.004	0.461	0.368	0.020	-0.096
	(0.187)	(0.231)	(0.366)	(0.152)	(0.309)	(0.304)	(0.276)	(0.295)
LATE								
Subdivided	-0.681**	-0.507*	0.561	-0.036	-0.252	-0.203	0.232	-0.499
	(0.277)	(0.285)	(0.480)	(0.199)	(0.417)	(0.401)	(0.389)	(0.420)
Non-Compensable	-0.176	0.030	-0.889*	-0.401*	-0.806***	-0.687**	-0.138	0.474
	(0.149)	(0.198)	(0.534)	(0.232)	(0.299)	(0.294)	(0.440)	(0.472)
Subdivided*Non-Compensable	0.522	0.156	-0.559	0.008	0.990*	0.789	0.050	-0.223
	(0.345)	(0.403)	(0.667)	(0.270)	(0.561)	(0.550)	(0.491)	(0.543)
Obs	374	374	374	374	374	374	374	374

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.5: Heterogeneous Effect of Subdivision on Farmers' Stated Main Reason for Why Government Could Confiscate Parcel. Non-Compensable vs. Compensable Plots.

	Amortization Default	Lack of Ownership Documentation	Use for Public Good Project	Fail to Pay Land Tax	Left Untilled
ITT					
Treat	0.153**	0.025	0.020	-0.092**	0.025
	(0.062)	(0.023)	(0.047)	(0.042)	(0.037)
Non-Compensable	0.056	0.033	-0.023	0.052	-0.034
	(0.142)	(0.040)	(0.088)	(0.091)	(0.034)
Treat*Non-Compensable	-0.297***	0.005	0.009	0.059	-0.015
	(0.088)	(0.038)	(0.067)	(0.064)	(0.049)
LATE					
Subdivided	0.320***	0.053	0.044	-0.196***	0.053
	(0.108)	(0.040)	(0.085)	(0.071)	(0.065)
Non-Compensable	0.038	0.032	-0.024	0.059	-0.036
	(0.146)	(0.032)	(0.073)	(0.079)	(0.027)
Subdivided*Non-Compensable	-0.633***	0.013	0.021	0.121	-0.031
	(0.153)	(0.068)	(0.123)	(0.114)	(0.086)
Obs	374	374	374	374	374

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.05$.

Table A.6: Heterogeneous Effects of Subdivision on Trust in Barangay Council's Effectiveness in Protecting Property Rights under Hypothetical Conflict.
Non-Compensable vs. Compensable Plots.

	When in Dispute With Neighbor	When in Dispute With Government	When in Dispute With Private Company	Trust Index
ITT				
Treat	-0.550***	-0.420***	-0.290***	-0.770***
	(0.139)	(0.162)	(0.109)	(0.224)
Non-Compensable	-0.130	0.130	0.062	0.030
	(0.364)	(0.367)	(0.302)	(0.582)
Treat*Non-Compensable	0.465**	0.387	0.168	0.613*
	(0.203)	(0.242)	(0.170)	(0.330)
LATE				
Subdivided	-1.169***	-0.891***	-0.620***	-1.638***
	(0.256)	(0.298)	(0.206)	(0.420)
Non-Compensable	-0.086	0.164	0.082	0.089
	(0.320)	(0.297)	(0.266)	(0.503)
Subdivided*Non-Compensable	0.966***	0.808*	0.343	1.272**
	(0.364)	(0.433)	(0.305)	(0.594)
Obs	374	374	374	374

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.05$.

Table A.7: Baseline Associations between Probability to Lease Out a Parcel and Likelihoods of Confiscation

	Parcel Leased Out
Likelihood: Govt Confiscation	0.013
	(0.69)
Likelihood: Neighbor Confiscation	-0.007
	(0.29)
Likelihood: Others Confiscation	-0.025
	(0.86)
Likelihood: Transfer to Children	-0.012
	(0.46)
ARB's Tilling Years	-0.005
	(2.81)***
Total Plots Owned	0.020
	(0.64)
Female	0.063
	(1.47)
Constant	0.378
	(2.85)***
R^2	0.19
N	534
$Outcome\ Mean$	0.25

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Municipality Fixed Effects. CLOA-clustered standard errors.

Table A.8: Plot-level Baseline Balance

	Mean	Control Mean	T-C Diff	SM Diff	Obs
Plot-level variables					
Inside ARC	0.549	0.563	-0.027	-0.055	570
Compensable	0.521	0.532	-0.023	-0.045	570
Acq: Settlement	0.395	0.390	0.009	0.018	570
Acq:VOS	0.384	0.376	0.017	0.036	570
Acq:GOL	0.107	0.107	-0.001	-0.002	570
Leased-out	0.250	0.245	0.010	0.023	565
Area (Ha)	2.399	2.430	-0.063	-0.027	566
Ownership Documents	0.455	0.456	-0.003	-0.007	552
Irrigation	0.374	0.387	-0.026	-0.054	569
Drains quickly	0.754	0.762	-0.016	-0.038	568
Upland	0.543	0.561	-0.037	-0.074	569
Flat slope	0.339	0.358	-0.037	-0.079	569
Steep slope	0.116	0.101	0.030	0.095	569
Perceived Risk Index	0.096	0.162	-0.133	-0.112	549
Disputes	0.048	0.040	0.016	0.076	546
Likelihood:Neighbor Conf.	0.126	0.140	-0.027	-0.080	546
Likelihood:Government Conf.	0.324	0.315	0.018	0.038	544
Likelihood:Last Owner Conf.	0.095	0.084	0.023	0.079	545
Likelihood:Other Conf.	0.048	0.054	-0.014	-0.064	546
Likelihood: Transferring to Children	0.902	0.899	0.007	0.023	543

Note: CLOA-clustered standard errors not reported in table. Mean corresponds to the average value of the variable for both treatment and control plots. T-C Diff corresponds to the difference between the mean value for the treatment group and the mean value for the control group. SM Diff is the Standardized Mean Difference between the treatment and the control groups. All regressions include pair fixed effects. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.9: ARB and Household Levels Baseline Balance

	Mean	Control Mean	T-C Diff	SM Diff	Obs
ARB-level variables					
Female	0.314	0.318	-0.006	-0.014	547
Age	53.996	53.348	1.296	0.093	536
None, Some or Complete Primary Edu.	0.522	0.478	0.089 **	0.179 **	536
Some High School and above Edu.	0.478	0.522	-0.089 **	-0.179 **	536
Number of Years in Farming	40.239	38.976	2.544 **	0.165 **	556
Number of Years as Primary Tiller	25.953	24.929	2.064 *	0.134 *	556
Number of Plots Owned	1.559	1.615	-0.112	-0.140	556
Number of Plots Tilled	1.268	1.327	-0.119 *	-0.147 *	556
Demand for Individual Title of POI	0.925	0.920	0.010	0.040	549
HH-level variables					
Household size	4.686	4.586	0.193	0.081	556
Per Capita Food Exp.	374.161	401.562	-47.252 *	-0.148 *	538
HH Participates in Wage Labor	0.645	0.654	0.009	0.020	556
Per Capita Wage Income	3,694.427	4,199.030	-985.538	-0.111	556
HH Has Own Business	0.316	0.333	-0.029	-0.062	556
Per Capita Income from Own Business	1,318.093	1,690.751	-622.494 *	-0.128 *	556
HH Received Pension or Unearned Income	0.601	0.594	0.010	0.020	556
Per Capita Unearned Income	381.632	461.529	-158.431 *	-0.127 *	556
Total Per Capita HH Income	5,394.152	6,351.310	-1766.464 **	-0.167 **	556
Household Has Savings	0.435	0.452	-0.052	-0.105	551
Per Capita Savings	0.111	0.112	-0.009	-0.051	547
Membership in Credit Co-Op	0.098	0.097	-0.029	-0.106	545
Borrows form Agricultural Traders	0.191	0.205	-0.021	-0.054	546
Applied for Commercial Bank Loan (past 2yrs)	0.063	0.072	-0.015	-0.062	546
Asset Index	0.163	0.327	-0.339 **	-0.176 **	546

Note: CLOA-clustered standard errors not reported in table. Mean corresponds to the average value of the variable for both treatment and control ARBs or households. T-C Diff corresponds to the difference between the mean value for the treatment group and the mean value for the control group. SM Diff is the Standardized Mean Difference between the treatment and the control groups. All regressions include pair fixed effects. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.10: Plot-level Baseline Balance of Endline Sample with Data on Agricultural Output

	Endline Sample					Baseline Sample
	Obs	Mean	Control Mean	T-C Diff	SM Diff	SM Diff
Plot-level variables						
Inside ARC	415	0.552	0.576	-0.046 *	-0.093 *	-0.055
Compensable	415	0.494	0.504	-0.020	-0.040	-0.045
Acq: Settlement	415	0.386	0.377	0.016	0.033	0.018
Acq:VOS	415	0.400	0.398	0.004	0.009	0.036
Acq:GOL	415	0.089	0.090	-0.001	-0.003	-0.002
Leased-out	413	0.245	0.236	0.017	0.039	0.023
Area (Ha)	413	2.388	2.446	-0.113	-0.050	-0.027
Ownership Documents	400	0.455	0.454	0.002	0.004	-0.007
Irrigation	414	0.374	0.375	-0.001	-0.003	-0.054
Drains quickly	414	0.758	0.765	-0.012	-0.029	-0.038
Upland	414	0.553	0.605	-0.101 ***	-0.203 ***	-0.074
Flat slope	414	0.333	0.335	-0.003	-0.007	-0.079
Steep slope	414	0.126	0.110	0.031	0.093	0.095
Perceived Risk Index	401	0.174	0.281	-0.209 *	-0.178 *	-0.112
Disputes	401	0.050	0.044	0.011	0.048	0.076
Likelihood:Neighbor Conf.	401	0.132	0.160	-0.055 **	-0.161 **	-0.080
Likelihood:Government Conf.	400	0.313	0.313	-0.001	-0.003	0.038
Likelihood:Last Owner Conf.	400	0.100	0.094	0.012	0.041	0.079
Likelihood:Other Conf.	401	0.050	0.062	-0.023	-0.104	-0.064
Likelihood: Transferring to Children	398	0.905	0.918	-0.026	-0.089	0.023

Note: CLOA-clustered standard errors not reported in table. Mean corresponds to the average value of the variable for both treatment and control ARBs or households. T-C Diff corresponds to the difference between the mean value for the treatment group and the mean value for the control group. SM Diff is the Standardized Mean Difference between the treatment and the control groups. All regressions include pair fixed effects. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.11: Plot-level Baseline Balance of Endline Sample with Data on Land Transfers (Land Leases)

	Endline Sample					Baseline Sample
	Obs	Mean	Control Mean	T-C Diff	SM Diff	SM Diff
Plot-level variables						
Inside ARC	459	0.549	0.567	-0.036	-0.072	-0.055
Compensable	459	0.499	0.508	-0.018	-0.036	-0.045
Acq: Settlement	459	0.375	0.367	0.015	0.031	0.018
Acq:VOS	459	0.403	0.399	0.009	0.017	0.036
Acq:GOL	459	0.107	0.107	-0.001	-0.003	-0.002
Leased-out	454	0.260	0.248	0.024	0.055	0.023
Area (Ha)	456	2.372	2.427	-0.112	-0.049	-0.027
Ownership Documents	443	0.456	0.454	0.005	0.010	-0.007
Irrigation	458	0.367	0.365	0.005	0.009	-0.054
Drains quickly	458	0.753	0.773	-0.039	-0.091	-0.038
Upland	458	0.546	0.581	-0.071 **	-0.143 **	-0.074
Flat slope	458	0.338	0.340	-0.004	-0.008	-0.079
Steep slope	458	0.118	0.105	0.026	0.080	0.095
Perceived Risk Index	443	0.136	0.226	-0.182	-0.155	-0.112
Disputes	439	0.052	0.049	0.006	0.027	0.076
Likelihood:Neighbor Conf.	439	0.125	0.147	-0.044 *	-0.133 *	-0.080
Likelihood:Government Conf.	438	0.311	0.308	0.006	0.013	0.038
Likelihood:Last Owner Conf.	438	0.094	0.088	0.012	0.041	0.079
Likelihood:Other Conf.	439	0.046	0.055	-0.020	-0.095	-0.064
Likelihood: Transferring to Children	436	0.901	0.907	-0.011	-0.036	0.023

Note: CLOA-clustered standard errors not reported in table. Mean corresponds to the average value of the variable for both treatment and control ARBs or households. T-C Diff corresponds to the difference between the mean value for the treatment group and the mean value for the control group. SM Diff is the Standardized Mean Difference between the treatment and the control groups. All regressions include pair fixed effects. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.12: Plot-level Baseline Balance of Endline Sample with Data on Tenure Security and Trust Outcomes

	Endline Sample					Baseline Sample
	Obs	Mean	Control Mean	T-C Diff	SM Diff	SM Diff
Plot-level variables						
Inside ARC	375	0.560	0.583	-0.046	-0.093	-0.055
Compensable	375	0.488	0.499	-0.023	-0.045	-0.045
Acq: Settlement	375	0.395	0.391	0.007	0.015	0.018
Acq:VOS	375	0.413	0.409	0.009	0.018	0.036
Acq:GOL	375	0.072	0.067	0.010	0.038	-0.002
Leased-out	373	0.220	0.209	0.023	0.054	0.023
Area (Ha)	373	2.375	2.476	-0.204	-0.092	-0.027
Ownership Documents	365	0.436	0.429	0.013	0.027	-0.007
Irrigation	374	0.361	0.368	-0.014	-0.028	-0.054
Drains quickly	374	0.749	0.762	-0.026	-0.061	-0.038
Upland	374	0.556	0.608	-0.105 ***	-0.210 ***	-0.074
Flat slope	374	0.324	0.319	0.010	0.021	-0.079
Steep slope	374	0.131	0.124	0.014	0.043	0.095
Perceived Risk Index	362	0.162	0.265	-0.210 *	-0.179 *	-0.112
Disputes	362	0.052	0.049	0.007	0.033	0.076
Likelihood:Neighbor Conf.	362	0.119	0.147	-0.056 **	-0.173 **	-0.080
Likelihood:Government Conf.	361	0.327	0.333	-0.013	-0.028	0.038
Likelihood:Last Owner Conf.	361	0.091	0.092	0.000	-0.001	0.079
Likelihood:Other Conf.	362	0.044	0.059	-0.030	-0.144	-0.064
Likelihood: Transferring to Children	360	0.894	0.898	-0.008	-0.026	0.023

Note: CLOA-clustered standard errors not reported in table. Mean corresponds to the average value of the variable for both treatment and control ARBs or households. T-C Diff corresponds to the difference between the mean value for the treatment group and the mean value for the control group. SM Diff is the Standardized Mean Difference between the treatment and the control groups. All regressions include pair fixed effects. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.13: ARB and HH-level Baseline Balance of Endline Sample with Data on Agricultural Output

	Endline Sample					Baseline Sample
	Obs	Mean	Control Mean	T-C Diff	SM Diff	SM Diff
ARB-level variables						
Female	413	0.293	0.297	-0.008	-0.018	-0.014
Age	402	54.438	53.962	0.929	0.065	0.093
None, Some or Complete Primary Edu.	402	0.532	0.508	0.048	0.096	0.179 **
Some High School and above Edu.	402	0.468	0.492	-0.048	-0.096	-0.179 **
Number of Years in Farming	415	40.807	39.690	2.187	0.144	0.165 **
Number of Years as Primary Tiller	415	26.306	25.667	1.250	0.080	0.134 *
Number of Plots Owned	415	1.533	1.556	-0.046	-0.059	-0.140
Number of Plots Tilled	415	1.243	1.266	-0.044	-0.056	-0.147 *
Demand for Individual Title of POI	409	0.919	0.909	0.020	0.075	0.040
HH-level variables						
Household size	415	4.752	4.632	0.234	0.094	0.081
Per Capita Food Exp.	403	376.160	411.897	-69.240 **	-0.199 **	-0.148 *
HH Participates in Wage Labor	415	0.665	0.649	0.032	0.067	0.020
Per Capita Wage Income	415	3,626.446	4,130.941	-987.574	-0.104	-0.111
HH Has Own Business	415	0.306	0.347	-0.080 *	-0.174 *	-0.062
Per Capita Income from Own Business	415	1,207.127	1,499.656	-572.639	-0.142	-0.128 *
HH Received Pension or Unearned Income	415	0.619	0.621	-0.003	-0.006	0.020
Per Capita Unearned Income	415	416.432	532.258	-226.735 *	-0.162 *	-0.127 *
Total Per Capita HH Income	415	5,250.005	6,162.856	-1786.948 *	-0.167 *	-0.167 **
Household Has Savings	411	0.426	0.451	-0.050	-0.100	-0.105
Per Capita Savings	409	0.107	0.108	-0.002	-0.011	-0.051
Membership in Credit Co-Op	408	0.081	0.083	-0.005	-0.018	-0.106
Borrows form Agricultural Traders	409	0.191	0.196	-0.010	-0.025	-0.054
Applied for Commercial Bank Loan (past 2yrs)	409	0.066	0.075	-0.018	-0.074	-0.062
Asset Index	409	0.082	0.249	-0.328 *	-0.172 *	-0.176 **

Note: CLOA-clustered standard errors not reported in table. Mean corresponds to the average value of the variable for both treatment and control ARBs or households. T-C Diff corresponds to the difference between the mean value for the treatment group and the mean value for the control group. SM Diff is the Standardized Mean Difference between the treatment and the control groups. All regressions include pair fixed effects. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.14: ARB and HH-level Baseline Balance of Endline Sample with Data on Land Transfers (Land Leases)

	Endline Sample					Baseline Sample
	Obs	Mean	Control Mean	T-C Diff	SM Diff	SM Diff
ARB-level variables						
Female	455	0.303	0.311	-0.015	-0.033	-0.014
Age	444	54.315	53.929	0.787	0.056	0.093
None, Some or Complete Primary Edu.	444	0.520	0.490	0.061	0.122	0.179 **
Some High School and above Edu.	444	0.480	0.510	-0.061	-0.122	-0.179 **
Number of Years in Farming	459	40.712	39.763	1.938	0.127	0.165 **
Number of Years as Primary Tiller	459	26.275	25.675	1.223	0.080	0.134 *
Number of Plots Owned	459	1.551	1.587	-0.073	-0.091	-0.140
Number of Plots Tilled	459	1.246	1.291	-0.092	-0.115	-0.147 *
Demand for Individual Title of POI	453	0.925	0.917	0.017	0.064	0.040
HH-level variables						
Household size	459	4.728	4.596	0.269	0.109	0.081
Per Capita Food Exp.	444	375.666	407.612	-64.766 **	-0.193 **	-0.148 *
HH Participates in Wage Labor	459	0.664	0.662	0.005	0.010	0.020
Per Capita Wage Income	459	3,601.124	4,051.778	-919.334	-0.100	-0.111
HH Has Own Business	459	0.298	0.326	-0.057	-0.124	-0.062
Per Capita Income from Own Business	459	1,194.734	1,406.333	-431.662	-0.106	-0.128 *
HH Received Pension or Unearned Income	459	0.612	0.620	-0.017	-0.035	0.020
Per Capita Unearned Income	459	394.355	496.305	-207.977 *	-0.155 *	-0.127 *
Total Per Capita HH Income	459	5,190.213	5,954.415	-1558.972 *	-0.149 *	-0.167 **
Household Has Savings	455	0.422	0.443	-0.043	-0.087	-0.105
Per Capita Savings	451	0.108	0.111	-0.006	-0.033	-0.051
Membership in Credit Co-Op	449	0.076	0.072	0.008	0.032	-0.106
Borrows form Agricultural Traders	450	0.189	0.194	-0.010	-0.025	-0.054
Applied for Commercial Bank Loan (past 2yrs)	450	0.067	0.070	-0.007	-0.027	-0.062
Asset Index	450	0.103	0.223	-0.245	-0.129	-0.176 **

Note: CLOA-clustered standard errors not reported in table. Mean corresponds to the average value of the variable for both treatment and control ARBs or households. T-C Diff corresponds to the difference between the mean value for the treatment group and the mean value for the control group. SM Diff is the Standardized Mean Difference between the treatment and the control groups. All regressions include pair fixed effects. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.15: ARB- and HH-level Baseline Balance of Endline Sample with Data on Tenure Security and Trust Outcomes

			Endline Sample			**Baseline Sample**
	Obs	Mean	Control Mean	T-C Diff	SM Diff	SM Diff
ARB-level variables						
Female	374	0.291	0.301	-0.019	-0.042	-0.014
Age	367	54.215	53.740	0.969	0.070	0.093
None, Some or Complete Primary Edu.	367	0.526	0.483	0.088	0.175	0.179 **
Some High School and above Edu.	367	0.474	0.517	-0.088	-0.175	-0.179 **
Number of Years in Farming	375	40.557	39.388	2.371 *	0.160 *	0.165 **
Number of Years as Primary Tiller	375	26.677	25.970	1.434	0.096	0.134 *
Number of Plots Owned	375	1.563	1.584	-0.042	-0.052	-0.140
Number of Plots Tilled	375	1.285	1.314	-0.058	-0.072	-0.147 *
Demand for Individual Title of POI	370	0.927	0.924	0.006	0.024	0.040
HH-level variables						
Household size	375	4.795	4.663	0.268	0.108	0.081
Per Capita Food Exp.	362	374.721	414.331	-79.220 **	-0.220 **	-0.148 *
HH Participates in Wage Labor	375	0.672	0.659	0.027	0.057	0.020
Per Capita Wage Income	375	3,576.313	4,140.594	-1143.813	-0.117	-0.111
HH Has Own Business	375	0.299	0.335	-0.075 *	-0.163 *	-0.062
Per Capita Income from Own Business	375	1,198.690	1,303.796	-213.054	-0.050	-0.128 *
HH Received Pension or Unearned Income	375	0.619	0.621	-0.005	-0.010	0.020
Per Capita Unearned Income	375	399.497	517.501	-239.198 *	-0.170 *	-0.127 *
Total Per Capita HH Income	375	5,174.500	5,961.892	-1596.065	-0.143	-0.167 **
Household Has Savings	371	0.426	0.461	-0.071	-0.144	-0.105
Per Capita Savings	368	0.105	0.113	-0.016	-0.100	-0.051
Membership in Credit Co-Op	367	0.079	0.085	-0.013	-0.047	-0.106
Borrows form Agricultural Traders	368	0.198	0.197	0.002	0.005	-0.054
Applied for Commercial Bank Loan (past 2yrs)	368	0.068	0.085	-0.034	-0.135	-0.062
Asset Index	368	0.109	0.307	-0.401 *	-0.205 *	-0.176 **

Note: CLOA-clustered standard errors not reported in table. Mean corresponds to the average value of the variable for both treatment and control ARBs or households. T-C Diff corresponds to the difference between the mean value for the treatment group and the mean value for the control group. SM Diff is the Standardized Mean Difference between the treatment and the control groups. All regressions include pair fixed effects. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.16: Effects of Subdivision Survey on Parcel Leases (Tenure Security Sample)

			ITT		LATE	
	Obs	Control Mean	Coef.	Std.Err.	Coef.	Std.Err.
Land Transfers						
Parcel Leased Out (Relatives=1)	374	0.068	0.039	(0.027)	0.086*	(0.050)
Parcel Leased Out (Relatives=0)	374	0.058	0.039	(0.026)	0.085*	(0.049)
Parcel Sold	374	0.000	0.004	(0.003)	0.009	(0.006)

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.17: Likelihood of Missing Data on Tenure Security Outcomes Among Leased-Out Parcels (Relatives = 1)

	Missing TS = 1	Missing TS = 1	Missing TS = 1	Missing TS = 1
Treat	0.003	-0.019		
	(0.021)	(0.022)		
Parcel Lease Out (Relatives=1)	0.365 ***	0.251 **		
	(0.088)	(0.115)		
Parcel Lease Out (Relatives=1) X Treat	0.142	0.226		
	(0.123)	(0.142)		
Subdivided			0.008	-0.045
			(0.045)	(0.046)
Parcel Lease Out (Relatives=1)			0.351 ***	0.228 **
			(0.101)	(0.115)
Parcel Lease Out (Relatives=1) X Subdivided			0.254	0.402 *
			(0.231)	(0.230)
Obs	459	459	459	459
Estimation	OLS	OLS	IV	IV
Pair Fixed Effects	No	Yes	No	Yes

Note: CLOA-clustered standard errors. TS Missing = 1 is a binary variable that equals one if the parcel has missing data on tenure security and trust outcomes. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table A.18: Likelihood of Missing Data on Tenure Security Outcomes Among Leased-Out Parcels (Relatives = 0)

	Missing TS = 1	Missing TS = 1	Missing TS = 1	Missing TS = 1
Treat	0.010	-0.014		
	(0.025)	(0.026)		
Parcel Lease Out (Relatives=0)	0.343 ***	0.199		
	(0.092)	(0.122)		
Parcel Lease Out (Relatives=0) X Treat	0.128	0.239 *		
	(0.127)	(0.143)		
Subdivided			0.021	-0.031
			(0.055)	(0.052)
Parcel Lease Out (Relatives=0)			0.338 ***	0.193 *
			(0.098)	(0.110)
Parcel Lease Out (Relatives=0) X Subdivided			0.211	0.386 *
			(0.224)	(0.210)
Obs	459	459	459	459
Estimation	OLS	OLS	IV	IV
Pair Fixed Effects	No	Yes	No	Yes

Note: CLOA-clustered standard errors. TS Missing = 1 is a binary variable that equals one if the parcel has missing data on tenure security and trust outcomes. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

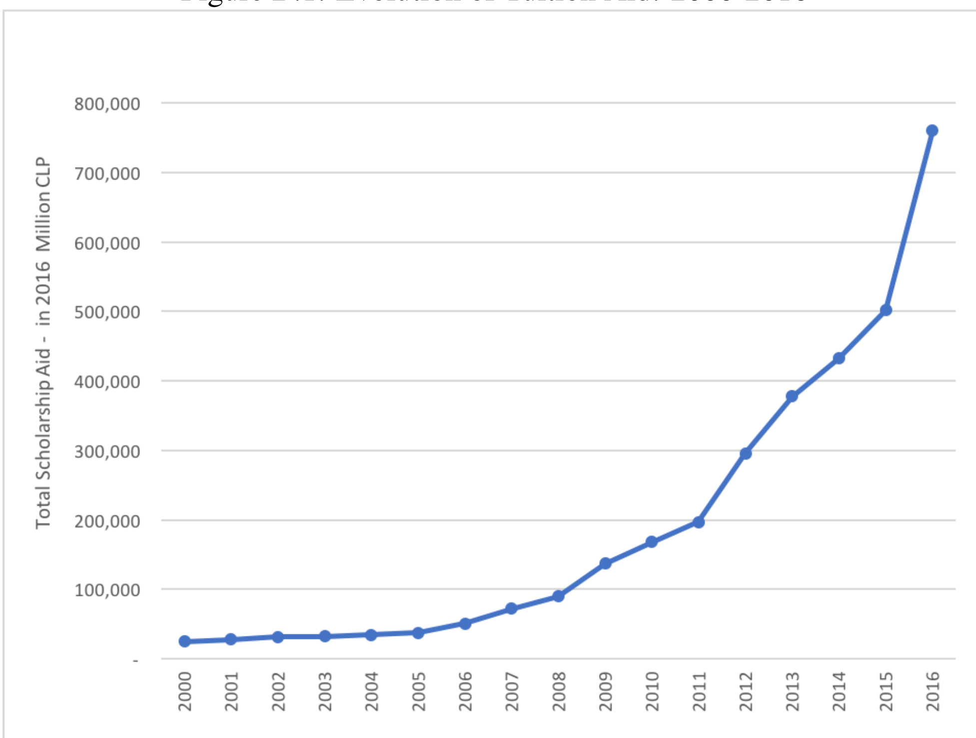
Table A.19: Heterogeneous Effects of Subdivision on Plot Leases by Farmer's Agricultural Ability)

	Parcel Leased Out (Relatives=1)	Parcel Leased Out (Relatives=0)
ITT		
Treat	0.116**	0.107**
	(0.050)	(0.050)
More Farming Experience	0.117	0.087
	(0.085)	(0.080)
Treat*More Farming Experience	-0.131	-0.133
	(0.084)	(0.083)
LATE		
Subdivided	0.252**	0.234**
	(0.100)	(0.102)
More Farming Experience	0.138*	0.109
	(0.082)	(0.079)
Subdivided*More Farming Experience	-0.295*	-0.300*
	(0.171)	(0.176)
Obs	458	458

Note: CLOA-clustered standard errors. Baseline controls included are household size, mode of acquisition of CLOA (Voluntary Offer of Sale, Government-Owned Land, Settlement), linear and quadratic controls for the number of years the ARB has been the primary tiller of the parcel, and total number of parcels owned by ARB. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$..

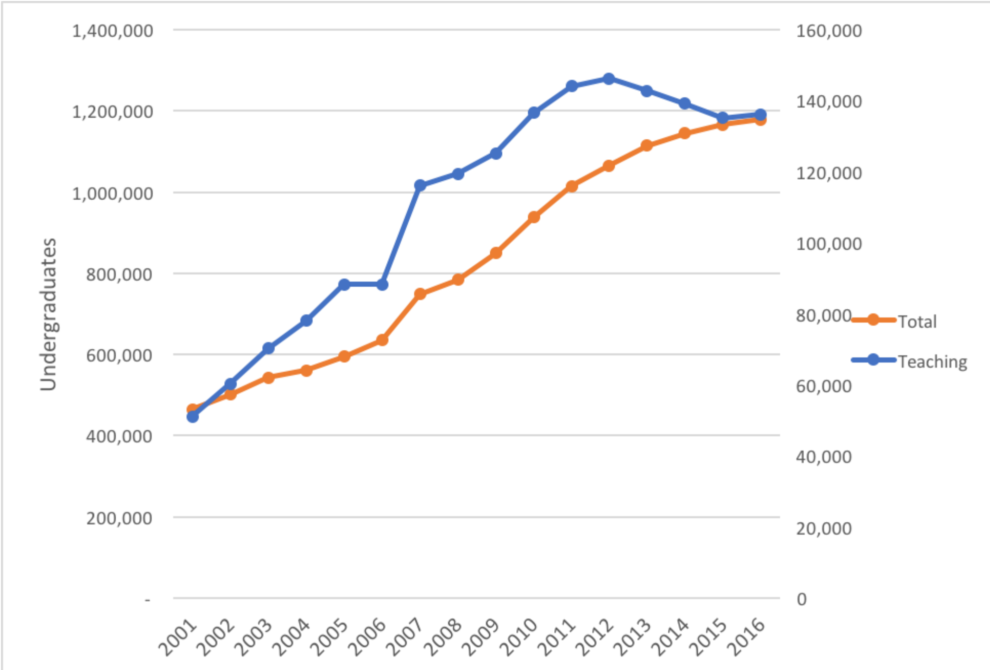
Appendix B: Chapter 2 Appendix

Figure B.1: Evolution of Tuition Aid: 2000-2016



Source: Mi Futuro - Chile. Amount in 2016 includes funds spent through GRATUIDAD. Own Calculations.

Figure B.2: Evolution of Undergraduate Enrollment: 2001-2016



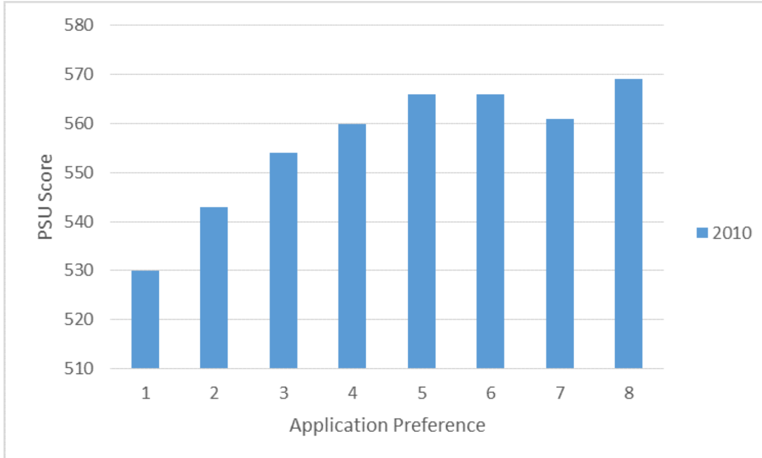
Source: SIES. Includes enrollment in PI and Universities.
Own Calculations.

Table B.1: Demand for Teaching Degrees: Mean PSU Scores of Applicants/Non Applicants and Enrolled/Not Enrolled

A	B	C	D	E	F	G	H	I
Year	Applied	Did Not	C-B	Enrolled in Teaching	Enrolled in Non-Teaching & Did Not Apply	Enrolled in & Non-Teaching & Applied	F-E	G-E
2007	537	587	50	561	610	574	49	12
2008	536	588	52	560	609	573	48	13
2009	537	587	51	562	614	578	52	16
2010	543	594	51	563	616	581	53	18

Source: DEMRE. All differences are significant to the 99% level of confidence.
Own Calculations.

Figure B.3: Mean PSU score of Applicants to Teaching Degrees by Application Preference



Source: DEMRE. Own Calculations.

Table B.2: Mean PSU Score of Applicants to Teaching Degrees by Application Preference

Year	First Option	Second Option	Third Option	Fourth Option	Fifth Option	Sixth Option	Seventh Option	Eight Option
2007	522	535	547	553	562	562	561	560
2008	522	534	548	553	560	563	554	560
2009	523	536	546	556	561	559	557	564
2010	530	543	554	560	566	566	561	569

Source: DEMRE. Own Calculations.

Table B.3: Changes in First-Year Undergraduate Enrollment Teaching Programs 2010 vs. 2011-2012: BVP vs. Non-BVP Majors

Yr (X)	Enrollment 2010 BVP Yr (X)	Enrollment (X) BVP Yr (X)	% Change	Enrollment 2010 Non-BVP Yr (X)	Enrollment (X) Non-BVP Yr (X)	% Change
2011	11,959	10,711	-10.4%	17,096	18,823	10.1%
2012	12,159	9,991	-17.8%	16,208	17,647	8.9%

Source: SIES. Own Calculations. Column 1 shows what was the 2010 total first-year enrollment in programs that were BVP programs in 2011 and 2012. Column 2 shows the corresponding total first-year enrollment of BVP programs for 2011 and 2012 period. Column 3 shows the change rate of the figures reported in Columns 1 and 2. Columns 5-7 present the equivalent variables for non-BVP teaching programs.

Table B.4: Changes in Student Composition (PSU Scores) 2010 vs. 2011-2012: BVP vs. Non-BVP Majors

Yr (X)	PSU≤500 (2010)	501≤PSU≤599 (2010)	PSU≥600 (2010)	PSU≤500 (X)	501≤PSU≤599 (X)	PSU≥600 (X)
2011	20%	63%	17%	0%	67%	33%
2012	20%	62%	18%	0%	69%	30%

Source: SIES and DEMRE. Own Calculations.

Table B.5: Student Composition (PSU Scores) in BVP Programs: 2011-2016

Year	PSU≤500	501≤PSU≤599	PSU≥600
2011	0%	67%	33%
2012	0%	69%	30%
2013	0%	71%	28%
2014	0%	70%	29%
2015	0%	70%	29%
2016	0%	72%	27%

Source: SIES and DEMRE. Own Calculations.

Figure B.4: Main Tuition Scholarship Programs for Higher Education: 2011 and 2016

Scholarship	2011		2016	
	Benefit	Elegibility Criteria	Benefit	Elegibility Criteria
Beca Bicentenario (BBIC)	Tuition* of any degree in a CRUCH universities	40th percentile of income distribution or lower; PSU score of 550 or more	Tuition* of any degree in a CRUCH universities	70th percentile of income distribution or lower; PSU score of 550 or more
Beca Juan Gómez Millas (BJGM)	Up to 1,150,000 CLP for tuition	40th percentile of income distribution or lower; PSU score of 550 or more; High School GPA of 5,0 or more	Up to 1,150,000 CLP for tuition	70th percentile of income distribution or lower; PSU score of 550 or more; High School GPA of 5,0 or more
Beca Nuevo Milenio (BNM)	Up to 500,000 CLP to pay for tuition of technical or professional degeree in Non-CRUCH institutions	40th percentile of income distribution or lower; High School GPA of 5,0 or more	From 600,000 CLP up to 900,000 CLP to pay for tuition of technical or professional degrees in Non-CRUCH institutions	70th percentile of income distribution or lower; High School GPA of 5,0 or more
Beca Vocación de Profesor (BVP)	Tuition of eligible education degrees	PSU score of 600 or more	Tuition of eligible education degrees	PSU score of 600 or more
Beca de Excelencia Académica (BEA)	Up to 1,150,000 CLP of tuition for professional degrees or 500,000 CLP for technical degrees	80th percentile of income distribution or lower; High School GPA in 95 percentile of graduating class or above.	Up to 1,150,000 CLP of tuition for professional degrees or 500,000 CLP for technical degrees	80th percentile of income distribution or lower; High School GPA in 90 percentile of graduating class
Beca Puntaje PSU (BPSU)	Up to 1,150,000 CLP of tuitionfor professional degrees or 500,000 CLP for technical degrees	80th percentile of income distribution or lower; Maximum PSU Score (Puntaje Nacional)	Up to 1,150,000 CLP of tuitionfor professional degrees or 500,000 CLP for technical degrees	80th percentile of income distribution or lower; Maximum PSU Score (Puntaje Nacional)
Beca Hijos de Profesionales de la Educacion (BHPE)	Up to 500,000 CLP of tuition	PSU score of 600 or more; High School GPA of 6,0 or more. Depending on the availability of funds the criteria can be relaxed.	Up to 500,000 CLP of tuition	PSU score of 500 or more; High School GPA of 5,5 or more.
Beca del Articulación (BAR)			Up to 750,000 CLP to pay for tuition of a professional degree	70th percentile of income distribution or lower; High School GPA of 5,0 or more; Individual already has technical degree
Gratuidad			Tuition of any degree in CRUCH Universities + 5 Private Universities	50th percentile of income distribution or lower

Source: Leyes de Reglamentación de Becas de Arancel para Educación Superior en Chile 2011-2016.

Table B.6: Enrolled Freshmen with 570≤PSU≤630: DEMRE vs. Non-DEMRE Institutions

Year	Non-DEMRE	DEMRE	Observations
2011	45%	55%	29,408
2012	18%	82%	29,437
2013	15%	85%	29,460
2014	14%	86%	29,220
2015	15%	85%	30,711
2016	15%	85%	31,383

Source: SIES. Own Calculations.

Figure B.5: Manipulation Tests: 2011 - 2016

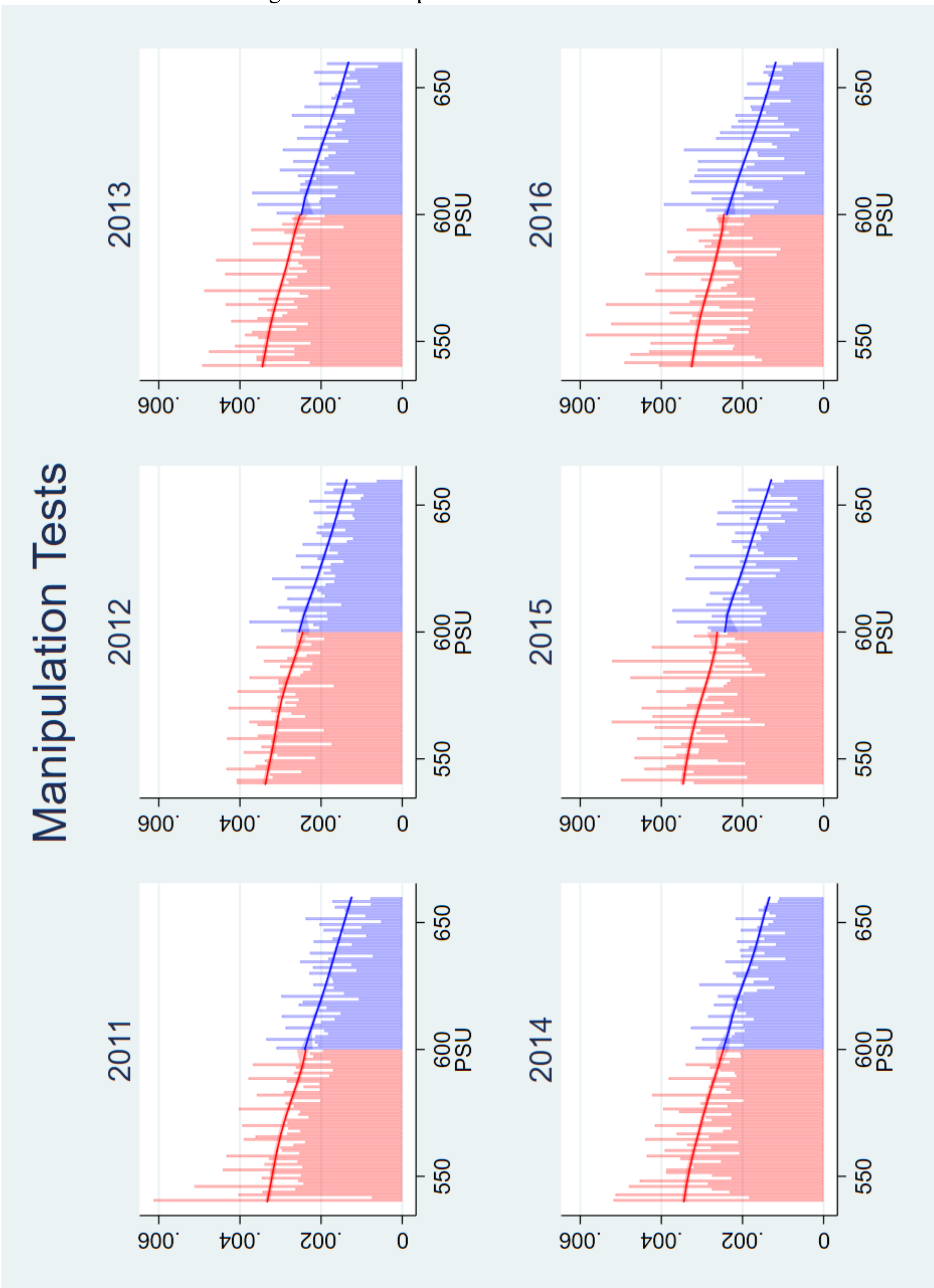


Table B.7: Balance of Covariates. Sample: Entire Population of PSU Test Takers around 600-point Threshold

	Mean	SD	Non-BVP Eligible	BVP Eligible	Diff	Effect Size	N
Female							
2011	0.519	0.500	0.494	0.498	0.004	0.008	59,330
2012	0.525	0.499	0.499	0.497	-0.002	-0.003	52,187
2013	0.524	0.499	0.482	0.492	0.010	0.020	47,944
2014	0.524	0.499	0.478	0.495	0.017	0.035	40,397
2015	0.524	0.499	0.493	0.484	-0.009	-0.018	43,019
2016	0.525	0.499	0.514	0.503	-0.011	-0.023	48,447
Public School							
2011	0.368	0.482	0.239	0.242	0.003	0.007	45,314
2012	0.338	0.473	0.217	0.223	0.006	0.012	43,284
2013	0.330	0.470	0.213	0.207	-0.005	-0.012	45,259
2014	0.326	0.469	0.208	0.206	-0.002	-0.005	53,700
2015	0.325	0.469	0.198	0.209	0.011	0.023	41,928
2016	0.322	0.467	0.211	0.214	0.003	0.006	31,049

Table B.7 – continued from previous page

	Mean	SD	Non-BVP Eligible	BVP Eligible	Diff	Effect Size	N
Voucher School							
2011	0.540	0.498	0.573	0.582	0.009	0.018	32,966
2012	0.561	0.496	0.587	0.583	-0.004	-0.008	30,261
2013	0.570	0.495	0.597	0.594	-0.004	-0.007	27,210
2014	0.571	0.495	0.580	0.586	0.006	0.013	29,116
2015	0.572	0.495	0.603	0.590	-0.013	-0.027	31,993
2016	0.576	0.494	0.603	0.587	-0.016	-0.032	24,458
Private School							
2011	0.092	0.289	0.191	0.176	-0.015	-0.052	30,928
2012	0.101	0.301	0.199	0.194	-0.005	-0.016	29,983
2013	0.100	0.301	0.188	0.203	0.015	0.050	24,669
2014	0.103	0.304	0.209	0.210	0.001	0.004	27,307
2015	0.102	0.303	0.202	0.201	-0.001	-0.004	31,451
2016	0.102	0.303	0.188	0.195	0.007	0.023	31,049
Isapre							
2011	0.190	0.392	0.347	0.332	-0.015	-0.039	32,085

Table B.7 – continued from previous page

	Mean	SD	Non-BVP Eligible	BVP Eligible	Diff	Effect Size	N
2012	0.202	0.401	0.358	0.360	0.001	0.003	33,356
2013	0.201	0.401	0.358	0.377	0.019	0.047	32,357
2014	0.209	0.406	0.372	0.376	0.003	0.008	31,087
2015	0.209	0.406	0.360	0.376	0.016	0.040	40,266
2016	0.212	0.409	0.355	0.366	0.011	0.027	37,485
Fonasa							
2011	0.721	0.449	0.555	0.572	0.016	0.037	36,806
2012	0.714	0.452	0.538	0.544	0.006	0.014	32,549
2013	0.715	0.452	0.548	0.547	-0.001	-0.002	35,365
2014	0.713	0.452	0.547	0.550	0.002	0.005	38,076
2015	0.717	0.451	0.556	0.546	-0.010	-0.022	43,347
2016	0.710	0.454	0.559	0.546	-0.013	-0.029	36,459
GPA							
2011	555.601	49.293	584.077	584.394	0.316	0.006	35,125
2012	558.248	49.390	586.043	585.984	-0.059	-0.001	35,593
2013	557.307	49.270	585.495	584.903	-0.592	-0.012	32,082

Table B.7 – continued from previous page

	Mean	SD	Non-BVP Eligible	BVP Eligible	Diff	Effect Size	N
2014	557.457	49.302	586.933	587.078	0.145	0.003	36,094
2015	557.975	48.959	586.197	586.682	0.485	0.010	36,463
2016	559.818	48.720	588.262	588.849	0.587	0.012	28,960
Mother HS Grad							
2011	0.616	0.486	0.828	0.818	-0.011	-0.022	39,764
2012	0.638	0.481	0.846	0.832	**-0.014***	-0.029	38,361
2013	0.648	0.478	0.844	0.837	-0.007	-0.014	39,478
2014	0.660	0.474	0.832	0.852	**0.02****	0.041	25,865
2015	0.669	0.471	0.847	0.855	0.008	0.017	43,817
2016	0.680	0.467	0.846	0.855	0.009	0.018	37,114
Mother HE Grad							
2011	0.156	0.363	0.290	0.264	**-0.026****	-0.073	28,493
2012	0.168	0.374	0.301	0.297	-0.004	-0.012	31,003
2013	0.173	0.378	0.308	0.314	0.006	0.016	28,022
2014	0.181	0.385	0.320	0.326	0.006	0.016	30,998
2015	0.186	0.389	0.329	0.333	0.004	0.009	33,382

Table B.7 – continued from previous page

	Mean	SD	Non-BVP Eligible	BVP Eligible	Diff	Effect Size	N
2016	0.193	0.395	0.317	0.330	0.013	0.033	33,314
Father HS Grad							
2011	0.623	0.485	0.817	0.819	0.002	0.003	50,503
2012	0.640	0.480	0.830	0.819	-0.010	-0.021	41,327
2013	0.645	0.479	0.833	0.834	0.001	0.002	39,461
2014	0.653	0.476	0.839	0.837	-0.003	-0.006	42,925
2015	0.662	0.473	0.836	0.834	-0.002	-0.005	47,969
2016	0.667	0.471	0.826	0.837	0.011	0.022	34,171
Father HE Grad							
2011	0.177	0.381	0.326	0.303	**-0.023****	-0.061	30,891
2012	0.187	0.390	0.339	0.331	-0.008	-0.021	33,095
2013	0.190	0.392	0.335	0.341	0.006	0.016	30,538
2014	0.196	0.397	0.346	0.348	0.002	0.005	32,834
2015	0.199	0.399	0.345	0.361	0.016	0.041	34,720
2016	0.204	0.403	0.342	0.358	0.016	0.039	31,908
Income							

Table B.7 – continued from previous page

	Mean	SD	Non-BVP Eligible	BVP Eligible	Diff	Effect Size	N
2011	2.476	1.094	3.004	2.963	**-0.041***	-0.038	35,093
2012	2.568	1.090	3.063	3.010	**-0.053****	-0.049	32,522
2013	2.665	1.073	3.132	3.144	0.011	0.011	40,666
2014	2.788	1.057	3.229	3.233	0.003	0.003	43,148
2015	2.866	1.042	3.269	3.296	0.026	0.025	45,629
2016	2.967	1.024	3.316	3.344	0.028	0.027	42,893
Household Size							
2011	4.639	1.675	4.452	4.493	0.041	0.025	45,636
2012	4.566	1.647	4.376	4.381	0.006	0.003	43,459
2013	4.506	1.637	4.374	4.312	**-0.062***	-0.038	30,481
2014	4.438	1.637	4.298	4.294	-0.005	-0.003	32,863
2015	4.404	1.628	4.280	4.275	-0.005	-0.003	39,691
2016	4.354	1.631	4.161	4.201	0.040	0.025	45,575
Mother HH Head							
2011	0.310	0.462	0.329	0.328	-0.001	-0.001	55,535
2012	0.323	0.468	0.341	0.352	0.010	0.022	44,189

Table B.7 – continued from previous page

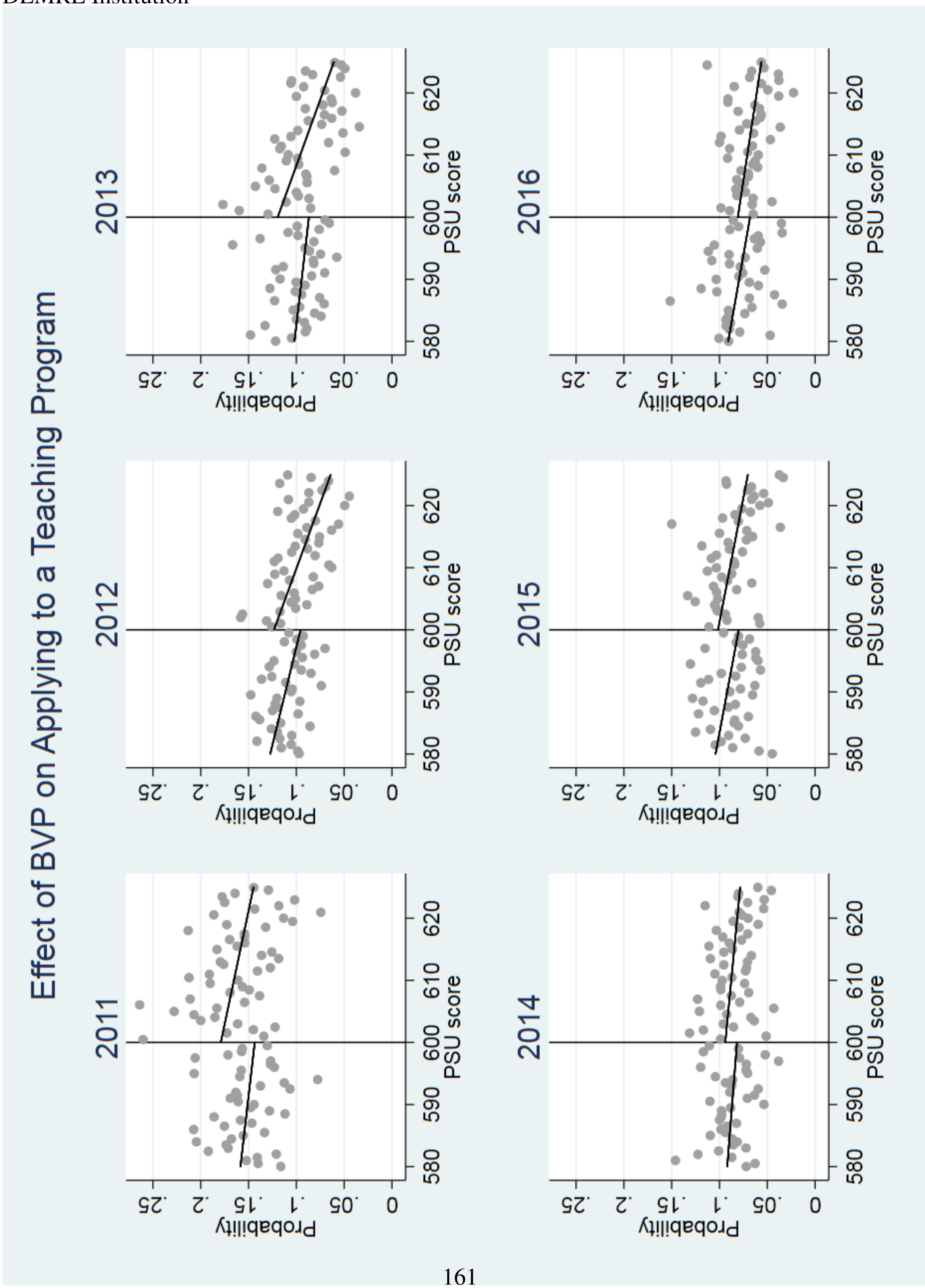
	Mean	SD	Non-BVP Eligible	BVP Eligible	Diff	Effect Size	N
2013	0.336	0.472	0.348	0.364	**0.017***	0.036	48,077
2014	0.352	0.477	0.369	0.361	-0.008	-0.017	48,487
2015	0.364	0.481	0.384	0.384	0.000	0.000	50,210
2016	0.387	0.487	0.407	0.392	-0.014	-0.029	47,578
Mother - paid job							
2011	0.348	0.476	0.437	0.407	**-0.03****	-0.062	41,065
2012	0.363	0.481	0.455	0.451	-0.004	-0.008	36,598
2013	0.385	0.487	0.454	0.475	**0.021***	0.043	51,415
2014	0.406	0.491	0.480	0.474	-0.006	-0.011	41,339
2015	0.416	0.493	0.491	0.495	0.004	0.009	66,465
2016	0.425	0.494	0.497	0.505	0.008	0.036	65,348
Father - paid job							
2011	0.613	0.487	0.680	0.674	-0.006	-0.013	40,184
2012	0.621	0.485	0.677	0.684	0.006	0.013	37,678
2013	0.636	0.481	0.691	0.687	-0.004	-0.009	45,948
2014	0.639	0.480	0.699	0.696	-0.003	-0.007	43,187

Table B.7 – continued from previous page

	Mean	SD	Non-BVP Eligible	BVP Eligible	Diff	Effect Size	N
2015	0.633	0.482	0.694	0.706	0.012	0.024	48,533
2016	0.635	0.481	0.696	0.702	0.005	0.011	43,620

Note: Estimations use CER-optimal bandwidth selector. Asterisks indicate significance level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B.6: RD Plots around PSU=600. Probability of Applying to a Teaching Program at a DEMRE Institution



Data: DEMRE

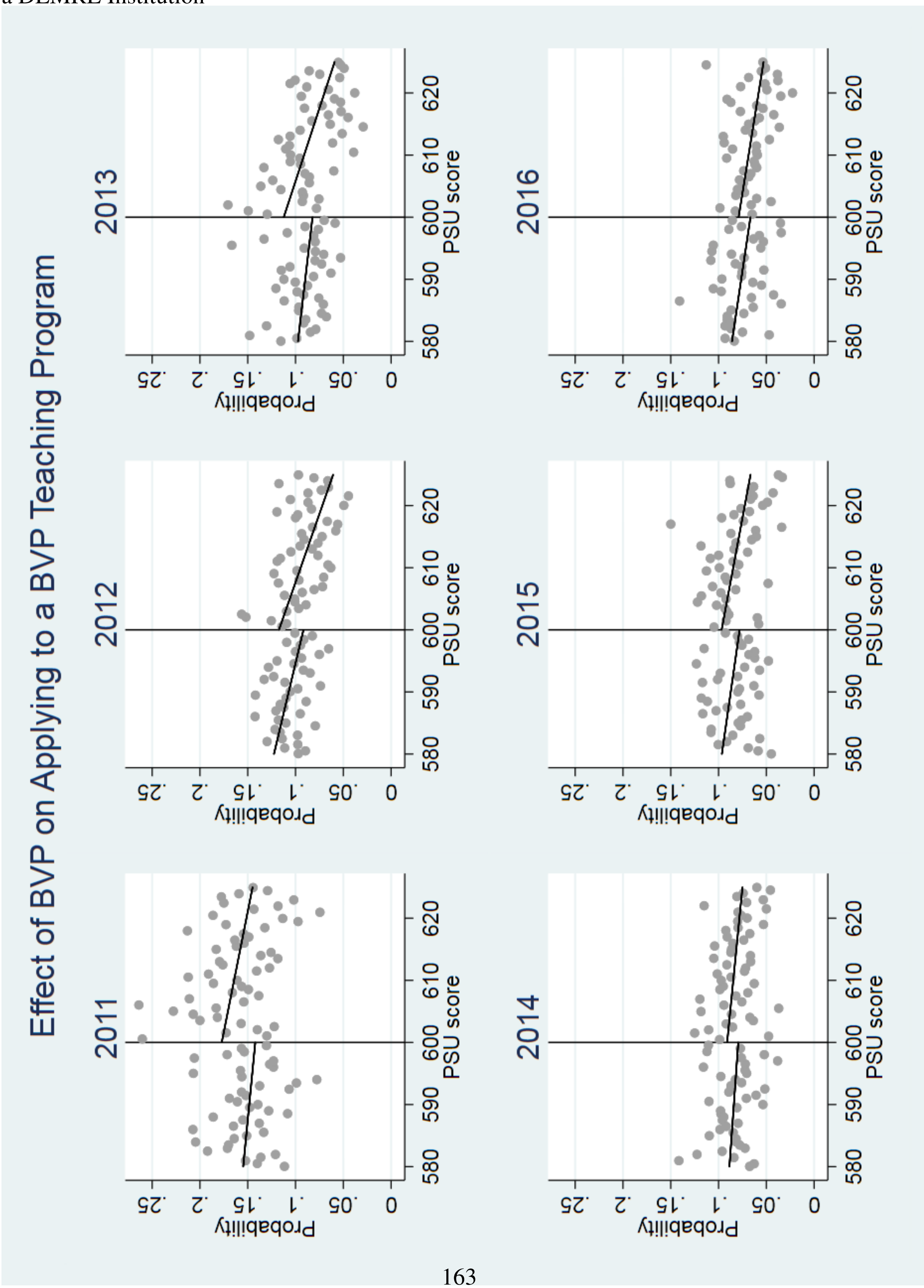
Table B.8: Effect of the BVP on the Probability of Applying (as First Option) to a **BVP** Teaching Program at a **DEMRE** institution. RD Analysis around PSU=600

Year	2011	2012	2013	2014	2015	2016
$\hat{\alpha_2}$	0.029***	0.023***	0.023***	0.012	0.012	0.009
Robust SE of $\hat{\alpha_2}$	(0.0109)	(0.0087)	(0.0085)	(0.0078)	(0.0076)	(0.0073)
$\hat{\alpha_0}$	0.142	0.091	0.082	0.079	0.079	0.067
$(\hat{\alpha 2} \div \hat{\alpha_0})$*100 (% Δ)	20.7**	25.7**	28.4**	14.7	15.4	13.3
Robust SE of % Δ	(8.6)	(11.1)	(12.0)	(10.7)	(10.5)	(11.7)
P-Value of Test: % Δ in Yr = % Δ in 2013	0.60	0.87		0.39	0.41	0.37
Bandwidth - Right of Cutoff	29.4	28.1	24.5	28.9	24.6	18.9
Bandwidth - Left of Cutoff	21.9	18.5	18.2	20.5	20.8	20.8
# Obs. - Right of Cutoff	11,879	13,848	12,294	14,140	12,767	10,662
# Obs. - Left of Cutoff	9,502	10,009	10,026	11,083	12,219	12,992

Data: DEMRE. Own Estimations.

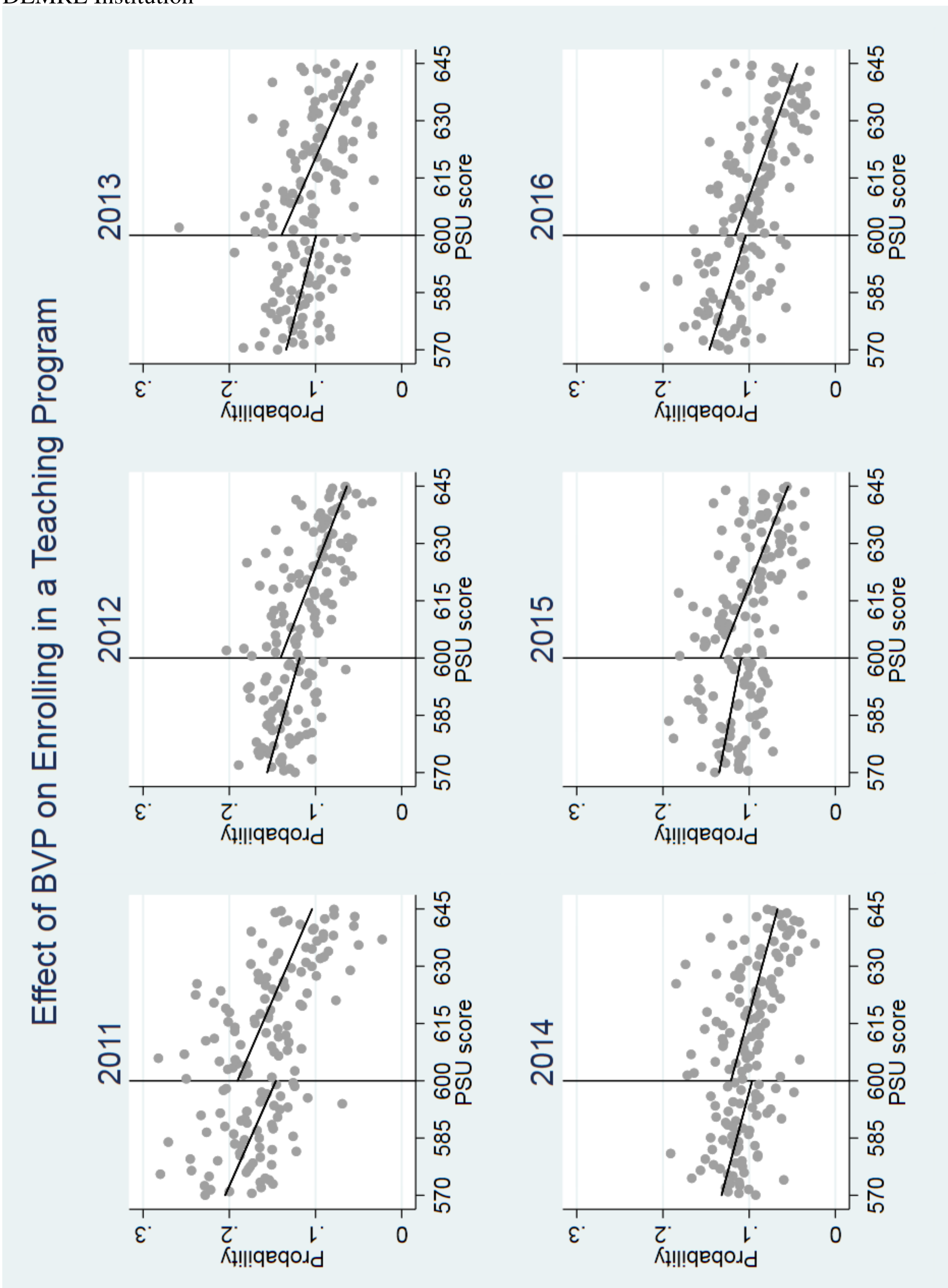
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. SE of % Δ are calculated using the Delta Method. Asterisks indicate significance level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B.7: RD Plots around PSU=600. Probability of Applying to a **BVP** Teaching Program at a DEMRE Institution



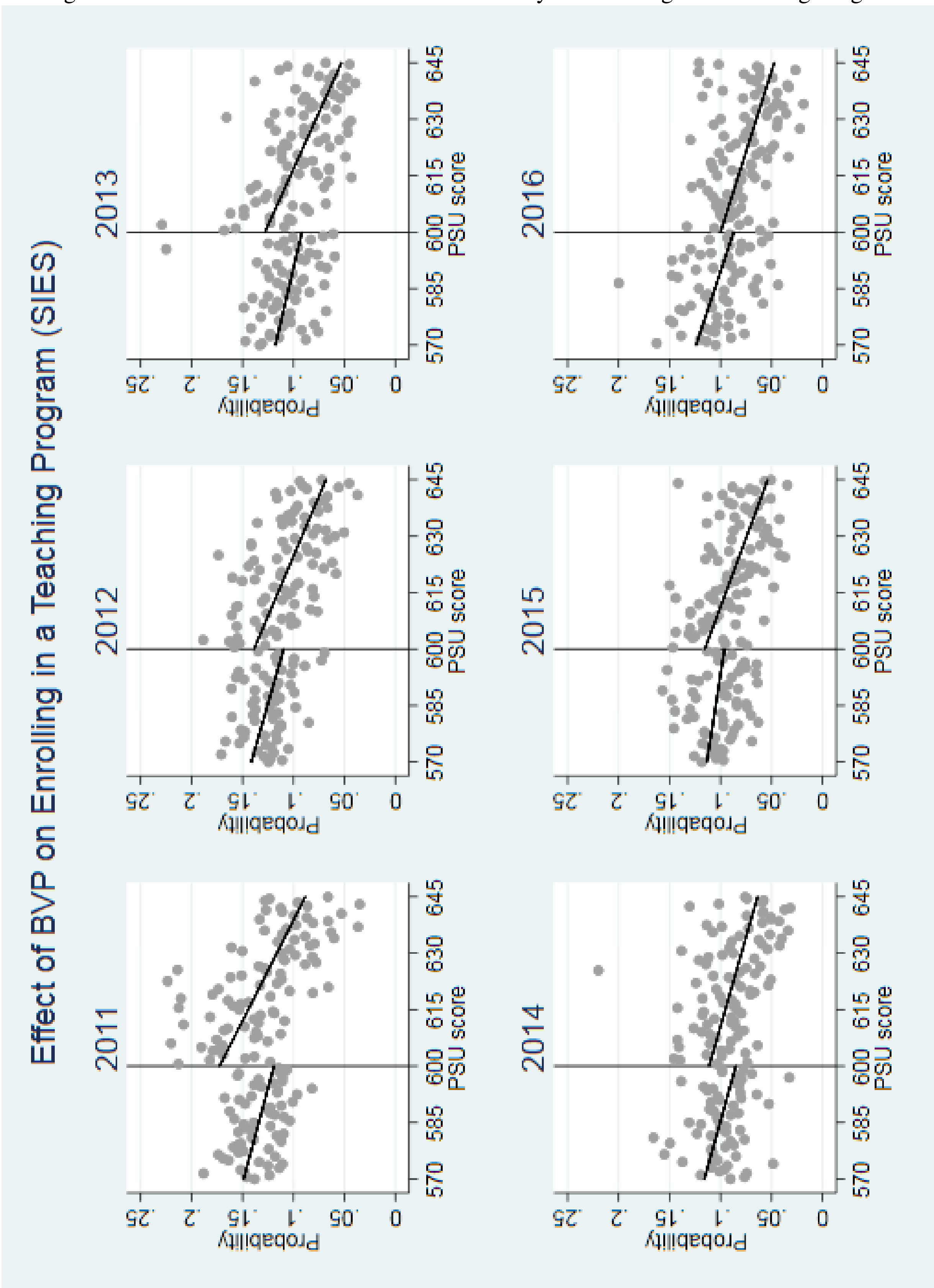
Data: DEMRE

Figure B.8: RD Plots around PSU=600. Probability of Enrolling in a Teaching Program at a DEMRE Institution



Data: DEMRE

Figure B.9: RD Plots around PSU=600. Probability of Enrolling in a Teaching Program



Data: SIES

Table B.9: Effect of the BVP on the Probability of Enrolling in a **BVP** Teaching Program at a **DEMRE** Institution. RD Analysis around PSU=600

Year	2011	2012	2013	2014	2015	2016
$\hat{\gamma_2}$	0.036**	0.032***	0.041***	0.019*	0.019*	0.013
Robust SE of $\hat{\gamma_2}$	(0.0149)	(0.0106)	(0.0104)	(0.0104)	(0.0109)	(0.0107)
$\hat{\gamma_0}$	0.140	0.103	0.091	0.094	0.103	0.094
$(\hat{\gamma_2} \div \hat{\gamma_0})$*100 (% Δ)	25.4**	30.0***	45.0***	20.8	18.3	14.0
Robust SE of % Δ	(12.38)	(12.32)	(14.17)	(12.65)	(11.98)	(12.37)
P-Value of Test: % Δ in Yr == % Δ in 2013	0.316	0.505		0.207	0.152	0.1
Bandwidth - Right of Cutoff	32.2	30.1	21.3	31.3	28.3	21.4
Bandwidth - Left of Cutoff	17.4	20.3	19.6	15.2	14.2	16.0
# Obs. - Right of Cutoff	8,163	11,451	8,634	11,927	11,292	8,962
# Obs. - Left of Cutoff	4,407	7,676	7,998	6,132	6,044	6,997

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. SE of % Δ are calculated using the Delta Method. Asterisks indicate significance level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.10: Effect of the BVP on the Probability of Enrolling in a **BVP** Teaching Program. RD Analysis around PSU=600

Year	2011	2012	2013	2014	2015	2016
$\hat{\gamma_2}$	0.034***	0.035***	0.033***	0.025***	0.016*	0.013
Robust SE of $\hat{\gamma_2}$	(0.0103)	(0.0092)	(0.0095)	(0.0084)	(0.0085)	(0.0084)
$\hat{\gamma_0}$	0.102***	0.095***	0.086***	0.077***	0.085***	0.075***
$(\hat{\gamma_2} \div \hat{\gamma_0})$*100 (% Δ)	33.2***	37.1***	38.8***	32.6**	18.7*	17.5
Robust SE of % Δ	11.798	11.904	13.465	13.071	11.051	12.375
P-Value of Test: % Δ in Yr == % Δ in 2013	0.76	0.92		0.74	0.25	0.24
Bandwidth - Right of Cutoff	19.3	29.6	22.5	30.6	24.1	24.2
Bandwidth - Left of Cutoff	19.1	18.2	17.2	18.4	19.5	16.5
# Obs. - Right of Cutoff	9,310	13,310	10,546	13,552	11,572	11,913
# Obs. - Left of Cutoff	9,648	9,088	8,662	9,178	10,615	8,630

Data: SIES. Own Estimations.

Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector. SE of % Δ are calculated using the Delta Method. Asterisks indicate significance level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.11: Falsification Tests. 2007-2010. Probability of Applying to a Teaching Program at a **DEMRE** Institution. RD Analysis around PSU=700.

Year	2007	2008	2009	2010
$\hat{\alpha_2}$	-0.003	0.004	-0.002	0.001
Robust SE of $\hat{\alpha_2}$	(0.0083)	(0.0084)	(0.0060)	(0.0070)
$\hat{\alpha_0}$	0.013***	0.015***	0.008***	0.015***
Bandwidth - Right of Cutoff	11.7	21.4	11.0	18.8
Bandwidth - Left of Cutoff	26.6	24.5	19.2	25.5
# Obs. - Right of Cutoff	1,354	2,293	1,567	2,444
# Obs. - Left of Cutoff	4,375	3,789	3,247	4,428

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector.

Table B.12: Falsification Tests. 2007-2010. Probability of Enrollling in a Teaching Program at a **DEMRE** Institution. RD Analysis around PSU=700.

Year	2007	2008	2009	2010
$\hat{\gamma_2}$	-0.006	0.007	0.003	0.004
Robust SE of $\hat{\gamma_2}$	(0.0105)	(0.0113)	(0.0085)	(0.0098)
$\hat{\gamma_0}$	0.017***	0.021***	0.010***	0.019***
Bandwidth - Right of Cutoff	11.6	20.2	18.1	17.2
Bandwidth - Left of Cutoff	20.3	23.3	17.7	22.0
# Obs. - Right of Cutoff	1,124	1,732	1,895	1,766
# Obs. - Left of Cutoff	2,591	2,797	2,277	2,928

Data: DEMRE. Own Estimations.
Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector.

Table B.13: Falsification Tests. 2007-2010. Probability of Enrolling in a Teaching Program at any Higher Education Institution. RD Analysis around PSU=700.

Year	2007	2008	2009	2010
$\hat{\gamma}_2$	-0.009	0.007	-0.004	0.006
Robust SE of $\hat{\gamma}_2$	(0.0081)	(0.0094)	(0.0070)	(0.0074)
$\hat{\gamma}_0$	0.016***	0.016***	0.013***	0.014***
Bandwidth - Right of Cutoff	10.1	17.5	12.4	18.7
Bandwidth - Left of Cutoff	26.2	22.7	21.4	24.3
# Obs. - Right of Cutoff	1,115	1,982	1,686	2,456
# Obs. - Left of Cutoff	4,063	3,583	3,583	4,220

Data: SIES. Own Estimations.

Note: Estimations use Triangular Kernel Weights and CER-optimal bandwidth selector.

Appendix C: Chapter 3 Appendix

C.1 Tuition-Free College and the Extensive Margin

Table C.1: Effect of Free College on Program Vacancies and First-Year Enrollment

	(1) Closing Programs	(2) Vacancies	(3) Freshmen Enrollment	(4) Vacancies	(5) Freshmen Enrollment
Post	0.003	0.475	0.530	-0.711	-0.813
	(0.015)	(1.496)	(1.750)	(4.416)	(4.949)
				(3.839)	(4.159)
Post#Agric.				0.839	1.679
				(8.381)	(9.905)
Post#Arts				1.697	2.771
				(7.712)	(8.998)
Post#Scien.				0.965	2.748
				(8.462)	(9.955)
Post#Soc.Scien.				2.547	3.276
				(6.674)	(7.800)
Post#Law				0.802	2.632
				(10.462)	(12.516)
Post#Educ.				0.297	2.483
				(5.343)	(6.108)
Post#Human.				4.748	5.990
				(11.813)	(13.983)
Post#Health				0.698	1.288
				(5.722)	(6.624)
Post#Tech.				1.249	-0.284
				(5.131)	(5.769)
Constant	0.168***	49.931***	54.830***	62.135***	67.389***
	(0.011)	(1.080)	(1.246)	(3.334)	(3.587)
Observations	2,591	2,014	2,152	2,014	2,152

Note: All regressions area of study fixed-effects. Standard errors in parentheses. Vacancies and junior enrollment regressions ran in a sample conditioned on being a non-closing program. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

C.2 Checking the Parallel Trends Assumption

Table C.2: Tripple-Difference Estimates Based on Income Eligibility by Year (2015 Baseline Year)

	Apply to at least one teaching major		Apply to teaching major as top choice	
	Coef.	Std.Err.	Coef.	Std.Err.
Year				
2014	-0.0127	(0.008)	-0.009	(0.006)
2016	-0.0232***	(0.007)	-0.0126**	(0.005)
Obs	224,222		224,222	

Note: We present the triple-difference obtained by substracting the difference-in-difference result for the income-eligible minus the difference-in-difference result for the income-ineligible where the base yaer is 2015. All regressions include school fixed-effects, gender, and linear PSU score controls. The 600 threshold for the PSU score was chosen based on the minimum score required to apply for BVP scholarship. Standard errors clustered at the school level. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table C.3: Difference in Applications to Teaching Majors for Students with $PSU \geq 600$ Relative to Students with $PSU < 600$ by School Type (2015 Baseline Year)

	Teaching as a Choice		Teaching as Top Choice	
	Coef.	Std.Err.	Coef.	Std.Err.
Public				
2014	-0.009	(0.011)	-0.009	(0.007)
2016	-0.024***	(0.009)	-0.010*	(0.006)
Voucher				
2014	-0.004	(0.006)	-0.001	(0.004)
2016	-0.010*	(0.006)	-0.004	(0.004)
Private				
2014	-0.006	(0.007)	-0.007	(0.006)
2016	-0.002	(0.007)	-0.003	(0.005)
Obs.	223,846		223,846	

Note: We present linear combination of the relevant coefficients for each category. All regressions include *comuna* fixed-effects, gender, and linear PSU score controls. The 600 threshold for the PSU score was chosen based on the minimum score required to apply for BVP scholarship. Standard errors clustered at the school level. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

The validity of the difference-in-difference strategy we use in this paper relies on the assumption that the application behavior among students in the treatment and control groups had the same trend before treatment ensued. We test this crucial assumption using data from 2014 college applications, one year before the timeframe of our analysis. Therefore, we expand our specification to include coefficients not only for 2016 (i.e., $Post = 1$ in the original specification) but also 2014. The regression becomes:

$$P_{i,t} = \sum_{g=0}^{G-1} \sum_{t=2014}^{2015} \left(\beta_g Y_{i,t}^g t + \gamma_g Y_{i,t}^g \mathbf{1}[PSU_{i,t} \geq 600] + \delta_g Y_{i,t}^g t \mathbf{1}[PSU_{i,t} \geq 600]\right) + X_{i,t}\theta + \mu_s + \varepsilon_{i,t}$$

If the pre-treatment trends assumption holds, and given that we treat 2015 as the baseline year, the coefficients associated with 2014 should not be statistically different from 0.

Table C.4: Difference in Applications to Teaching Majors for Students with $PSU \geq 600$ Relative to Students with $PSU < 600$ by Mother's Education (2015 Baseline Year)

	Teaching as a Choice		Teaching as Top Choice	
	Coef.	Std.Err.	Coef.	Std.Err.
S¡12				
2014	-0.011	(0.014)	0.001	(0.010)
2016	-0.022*	(0.013)	0.006	(0.008)
S=12				
2014	-0.004	(0.008)	-0.005	(0.006)
2016	-0.014**	(0.007)	-0.010**	(0.005)
12¡S¡16				
2014	0.008	(0.008)	0.000	(0.006)
2016	-0.006	(0.008)	-0.004	(0.006)
$S \geq 16$				
2014	-0.012	(0.008)	-0.004	(0.006)
2016	-0.011	(0.008)	-0.005	(0.005)
Obs.	204,487		204,487	

Note: We present linear combination of the relevant coefficients for each category. All regressions include *school* fixed-effects, gender, and linear PSU score controls. The 600 threshold for the PSU score was chosen based on the minimum score required to apply for BVP scholarship. Standard errors clustered at the school level. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

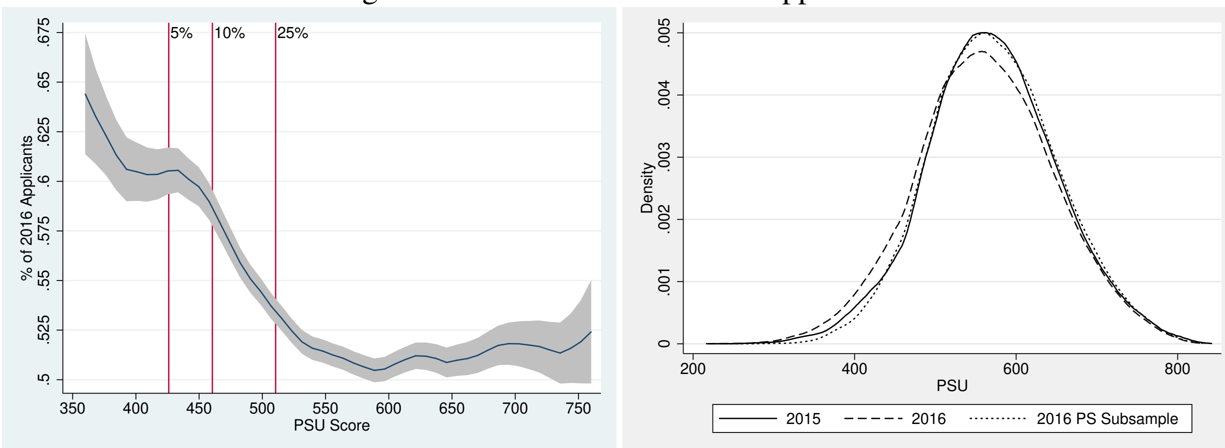
C.3 “Sorting of students” *or* “New Applicants”?

The tuition-free policy, implemented in 2016, encouraged more students to apply for university admission. Even though the number of university applications had been increasing before 2016, the number of applications between 2015 and 2016 rose 12%, more than double the rate from 2014 to 2015 (4.58%). This expansion resulted in a change of the characteristics of applicants which may affect the comparability of the 2015 and 2016 cohorts, and potentially invalidate our empirical strategy if those new applicants are skewed towards being relatively poor, high-achieving and uninterested in teaching programs. In this appendix, we explore whether our results could be driven by the inflow of “new applicants”, or if the tuition-free policy affected student application behavior, and thus the “sorting of students” into teaching programs. Our analysis show that the latter effects dominates and that our results are indeed robust to changes in the pool of applicants.

It is important to note that the gross enrollment in tertiary education in Chile was already high by international standards before the reform (85.3% in 2015). Thus, the room for expansion in the number of applications was limited.

In Figure C.1 we show that when we compare the 2015 and 2016 cohorts by PSU score, post-reform applicants are over-represented in the lowest end of the PSU score distribution. If the distribution of scores had not changed, Figure C.1 would show a flat line close to 50%. However, as the figure shows, 6 out 10 applicants whose scores are at the lowest 5% mark are from 2016. Indeed, low-performing students are over-represented in the bottom 25% of the distribution, after which the curve flattens. Figure C.1 shows that indeed the distribution of 2016 scores shifts to the left with respect to the distribution in 2015.

Figure C.1: PSU Score and New Applicants



Note: The left panel shows a linear polynomial regression where the dependent variable takes the value of 1 if the student is from 2016 and 0 if from 2015. The running variable is the PSU score. The range plot represents the 95% confidence intervals. The vertical lines show the location of the 5^{th}, 10^{th}, and 25^{th} percentiles. The right panel shows the PSU score distributions for different samples, namely, the PSU score distribution for university applicants in 2015, 2016, and the subsample of 2016 after dropping the observations that fall outside the propensity score's common support where "treatment" is having applied in 2016 and the predicting variables are PSU and SIMCE scores.

New entrants might differ from those who would have applied in the absence of the policy in terms of their observable (e.g., PSU) and unobservable characteristics (e.g., taste for majors). In what follows, we explain why we believe that this concern—although theoretically valid—in practice does not invalidate our findings. We provide empirical evidence to support our claim providing robustness checks.

First, in order to assess the extent to which differences based on observables can be factor in our estimates, we use propensity score matching to define a subsample of students in 2016 who are comparable with the applicants in 2015. We use high school sophomore year SIMCE scores to match pre- and post-students using the nearest neighbor matching method with $n = 1$ (same final sample in both years). Thus, we drop around 9,000 students who are the least likely to have applied to college based on their past scores. The resulting distribution, plotted by the dotted line in Figure C.1, keeps the students who most likely would have applied in 2015 when the tuition-free policy was not in place. We use that restricted sample to estimate our model.

Table C.5: Effect of Free College on Applications to Teaching Majors Triple-Diff
Sample in 2016 Matched in Observables

	Apply to at least one teaching major			Apply to teaching major as top choice		
	$PSU < 600$	$PSU \geq 600$	Dif-in-Dif	$PSU < 600$	$PSU \geq 600$	Dif-in-Dif
$\Delta(t)$						
Household Income						
Decil ≤ 6	0.0003	-0.0260***	-0.0263***	-0.0055*	-0.0166***	-0.0111**
	(0.004)	(0.005)	(0.008)	(0.003)	(0.003)	(0.004)
Decil > 6	0.0029	-0.0037	-0.0066	-0.0002	-0.0040*	0.0037
	(0.004)	(0.003)	(0.009)	(0.003)	(0.002)	(0.004)
Triple-Dif			-0.0197**			-0.0088
			(0.008)			(0.005)
Obs			128,956			128,956
Avg. de. var.			0.171			0.086

Note: We present the size of the effect for each category. The diff-in-diff column represents the difference-in-difference estimate from substracting the change in applications between 2015 and 2016 among those who score more that 600 points in the PSU minus the change in applications between 2015 and 2016 among those who score less that 600 points in the PSU for a given type of income eligibility. The Triple-Diff is obtained be substracting the difference-in-difference result for the income-eligible minus that of the income-ineligible. All regressions include school fixed-effects, gender, and linear PSU score controls. The 600 threshold for the PSU score was chosen based on the minimum score required to apply for BVP scholarship. Standard errors clustered at the school level. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

Table C.5 shows the results of the same regression reported in Table 3.5 but using the 2016 PS subsample. The table shows results that are consistent with those reported in Table 3.5. Therefore, our main estimates do not hinge on the fact that the tuition-free policy expanded the pool of university applicants.

Second, we address the possible selection on unobservables. The main concern there is that the new entrants are on average more prone to disliking teaching majors because the BVP had already attracted those with a strong preference for teaching. Thus, the free tuition policy prompts a compositional change that would mechanically bring down the share of applicants to teaching majors, biasing our estimates upward. Overall, we agree with this intuition. However, a closer inspection of the samples for whom this concern is actually valid lessens our concern.

Table C.6: Change in Application Behavior by PSU Range

Panel A: PSU¡600

	Before	After	Diff.	%
Business	4872	6003	1131	23.21
Education	5280	6021	741	14.03
Health	13753	16545	2792	20.30
Social Sciences/Humanities	6644	8015	1371	20.64
STEM	10478	11025	547	5.22
Others	6663	7801	1138	17.08

Panel B: PSU≥600

	Before	After	Diff.	%
Business	2660	2792	132	4.96
Education	1106	948	-158	-14.29
Health	6318	7386	1068	16.90
Social Sciences/Humanities	2666	2850	184	6.90
STEM	8889	8462	-427	-4.80
Others	3769	4051	282	7.48

Note: For students applying to degrees in the centralized matching system, we compare the number of students applying to a degree (in each of the fields), before and after the implementation of the tuition-free policy. We restrict the sample to students who graduated from high school the year before entering higher education. The last two columns quantify the differences in absolute and relative terms respectively.

New entrants have overwhelmingly lower PSU-scores. In fact, out of the 9,000 new entrants, roughly 8,000 score less than 600 points in the PSU and only 1,000 score more than 600 points. This implies that the vast majority of new entrants would not have been eligible for the BVP. For this reason, the BVP should not have generated any selection based on tastes on that group of applicants. Thus, we expect that the group of new entrants includes students with a preference for teaching. In fact, this is evident in Table C.6 where we present the changes in application behavior in *absolute numbers*. It shows that applications to teaching majors *grew* by 741 or 14% year-to-year among low-scoring students. This growth rate is similar to the growth rate of all the applications from students with PSU¡600 (i.e., 16%).

The growth in the number of applications from low-scoring students contrasts with the drop in the absolute number of applicants to teaching majors among students with PSU¿600. After the tuition free policy was implemented, there were actually less high-scoring students applying to teaching majors. In sum, selection on tastes applies to the few high-scoring new entrants for whom the BVP would have been an option. And even there, we observe a drop in the head-count of applicants to teaching majors, which we take as evidence of the fact that the reshuffling between majors prompted by the tuition free policy greatly outweighs any mechanical change in the relative application rates.

Next, we disentangle the effect of the new entrants from the sorting-into-major effect—which is the primary effect we care about. To do so, we assume that the true distribution of preferences is the one we revealed in 2016 when financial constraints were lifted. That distribution of preference differs from the one that we observe 2015 because 2015's observed distribution of preferences is affected by the financial constraints and the effect the BVP could have had on nudging eligible students with some interest in teaching majors.

Based on the 2016 distribution of preferences, we create a counterfactual applicant population in 2015 (the one that we would have observed without truncation) and compare the application behavior between the two periods. Note that this exercise is the opposite to the first one we performed in this Appendix. Instead of trimming down the 2016 sample to match what it would have been without the new entrants, we expand the 2015 sample to include students that would have applied if college was tuition-free. This has the advantage that we are able to exploit the revealed preferences for majors obtained from 2016.

We implement the proposed approach by adding 9,000 observations to our 2015 sample using the following steps:

1. We assume that out of the 1,000 high-scoring (PSU¿600) extra students that would have applied in 2015 if college was free, *none* would have chosen teaching majors. We do so because for them the BVP is binding, and thus all who were interested in teaching were already lured into applying thanks to the BVP.

2. We assign these applicants to non-teaching majors based on the distribution of high-scoring applicants in 2016.

3. We assume that the extra 8,000 low-scoring (PSU¡600) students that would have applied in 2015 if college was free would follow the same major choice distribution we observe in 2016 among the low-scoring applicants. This implies that some of the extra students would have chosen teaching majors because, as explained above, for those with PSU¡600 the BVP was never an option and could not have affected their application preferences.

Table C.7 presents the result of this empirical exercise. Comparing it to Table 3.4 in the main text, we find very small differences in the changes in application behavior. The only noticeable difference is that the change in application behavior to teaching majors among the high-scoring students falls from -17.78% in Table 3.4 to -14.29% in Table C.7. This indicates that even if we assume that none of those extra BVP-eligible students who would have applied to college in 2015 if it was free would have applied to teaching majors, we would still observe a massive drop in applications to teaching majors among the BVP-eligible applicants. Only up to 3.49 percentage points or less than a fifth of that drop could be attributed to the differences in tastes of the new entrants in 2016.

We now take this line of argument to our difference-in-difference and triple-difference specifications. In particular, we want to inquire how robust our estimates are to including the

Table C.7: Change in Application Behavior by PSU Range Simulating 2015 Extra Applicants

Panel A: PSU¡600

	Before	After	Diff. (%)	p-value
Business	0.103	0.108	5.17	0.004
Education	0.110	0.109	-1.60	0.346
Health	0.290	0.299	3.03	0.001
Social Sciences/Humanities	0.140	0.145	3.27	0.029
STEM	0.217	0.199	-8.23	0.000
Others	0.140	0.141	0.66	0.659

Panel B: PSU≥600

	Before	After	Diff. (%)	p-value
Business	0.105	0.105	0.32	0.899
Education	0.042	0.036	-14.29	0.000
Health	0.248	0.279	12.28	0.000
Social Sciences/Humanities	0.105	0.108	2.78	0.277
STEM	0.351	0.319	-8.97	0.000
Others	0.149	0.153	2.48	0.234

Note: For students applying to degrees in the centralized matching system, we compare the number of students applying to a degree (in each of the fields), before and after the implementation of the tuition-free policy. We restrict the sample to students who graduated from high school the year before entering higher education. The last two columns quantify the differences in absolute and relative terms respectively.

unconstrained preferences that we would have observed if college was free in 2015. That way, we provide a lower bound of the effect we analyze by disentangling the possible mechanical effect caused by the differences in major preferences of the new entrants from the reshuffling-into-majors effect we intend to measure. To do so, we extend the three-part procedure described above. In particular, we add 8,000 low-scoring and 1,000 high-scoring 2015 applicants by duplicating existing observations chosen at random. Of course, the 8,000 low-scoring simulated observations are randomly drawn following a stratification by major in order to match the 2016 distribution, and the 1,000 high-scoring simulated observations are randomly drawn from the subsample that *did not apply* to teaching majors. We take such draws 500 times and, for each draw, we estimate the empirical models. Finally, we collect the estimates in each draw and, in Table C.8 we present

Table C.8: Effect of Free College on Applications to Teaching Majors Triple-Diff
Simulating Extra Applicants in 2015 Following Preferences Distributions Revealed in 2016

	Apply to at least one teaching major			Apply to teaching major as top choice		
	$PSU < 600$	$PSU \geq 600$	Diff-in-Diff	$PSU < 600$	$PSU \geq 600$	Diff-in-Diff
$\Delta(t)$						
Household Income						
Decil ≤ 6	-0.0025	-0.0237***	-0.0212***	-0.0077***	-0.0158***	-0.0081**
	(0.004)	(0.004)	(0.006)	(0.003)	(0.003)	(0.004)
Decil > 6	-0.0013	-0.0016	-0.0004	-0.0032	-0.0015	0.0017
	(0.004)	(0.003)	(0.005)	(0.003)	(0.002)	(0.004)
Triple-Diff			-0.0208***			-0.0098*
			(0.007)			(0.005)
Original Obs			154,653			154,653
2015 Obs sim			8,900			8,900
Avg. de. var.			0.171			0.086

Note: We present the size of the effect for each category calculated based on the regression results presented in Table C.11 in the Appendix. The diff-in-diff column represents the difference-in-difference estimate from substracting the change in applications between 2015 and 2016 among those who score more that 600 points in the PSU minus the change in applications between 2015 and 2016 among those who score less that 600 points in the PSU for a given type of income eligibility. The Triple-Dif is obtained be substracting the difference-in-difference result for the income-eligible minus the difference-in-difference result for the income-ineligible. All regressions include school fixed-effects, gender, and linear PSU score controls. The 600 threshold for the PSU score was chosen based on the minimum score required to apply for BVP scholarship. Standard errors clustered at the school level. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

the mean of each parameter.

We should compare Table C.8 with Table 3.5 in the paper. We find that our estimates remain almost unchanged. Thus, they indicate that the differences in major preferences of the new entrants have very limited effect on our results and that most (97%) of the effect we measure is due to the reshuffling of students. Even in the "worst" draws (i.e., those with the 10% smallest effects), the triple-difference estimate of the effect of free tuition on applying to teaching majors is -0.0192, which represents 90% of the effect we report in Table 3.5.

Finally, we include one more empirical tests concerning the potential effect of selection into college application. We estimate a Heckman selection model and run the triple-difference

Table C.9: First Stage: Probability of Applying to College

	Apply to College	
	Coeff.	Std.Err.
Male	-0.307***	(0.005)
Language (Sophomore Year)	0.006***	(0.000)
Math (Sophomore Year)	0.010***	(0.000)
% Applied in Comm-SchoolType	1.794***	(0.014)
Obs	370,900	

Note: We present a probit model with the dependent variable taking the value of one if the student applied to college and zero if she was part of the cohort, but did not apply. The independent variables include the scores on language and math obtained in an standardize test taken during their high school sophomore year and the average *comuna*/school-type college application ratios for the three years prior to 2015 (*% Applied in Comm-SchoolType*). School type includes three categories: public, voucher and private. Robust standard errors in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.05$.

specifications embedded in a control function approach. The goal is to control for the fact that the population of students that apply is a selected sample of all those who graduate from high school. Therefore, in a first step, we predict college application based on two additional data sources: i.) *comuna*-level shares of students applying to college by type of school (i.e., public, voucher and private) for three years prior to 2015, and ii.) the students' high school sophomore-year standardized tests. The former is relevant as it captures variation on the long-term propensity to apply to college as determined by the student's residential characteristics and type of school. The latter (high school sophomore-year test scores) is available for all high school students, including those who we miss in our main empirical sample because they did not apply to college. These scores give us a measure of cognitive skills that will relate to the individual's chances of admission into college, but are not the scores used for college application.

We estimate a selection equation in which we set out to explain college application behavior using these two additional sources of variation. In our Table C.9, we show that our instruments are highly predictive of the selection into college application. With those estimates in hand,

Table C.10: Triple-Difference Estimator With a Heckman Selection Model as Control Function

	Apply to at least one teaching major			Apply to teaching major as top choice		
	< 600	≥ 600	Diff-in-Diff	< 600	≥ 600	Diff-in-Diff
$\Delta(t)$						
Household Income						
Decil ≤ 6	-0.0028	-0.0256***	-0.0228***	0.0080***	-0.018***	-0.0099*
	(0.004)	(0.005)	(0.006)	(0.003)	(0.003)	(0.004)
Decil > 6	-0.0014	-0.0040	-0.026	-0.0040	-0.0040	-0.0002
	(0.004)	(0.003)	(0.005)	(0.003)	(0.002)	(0.004)
Triple-Diff			-0.0203***			-0.0010*
			(0.007)			(0.005)
Inverse Mills Ratio			-0.0176***			-0.0124***
			(0.007)			(0.002)
Obs			153,037			153,037
Avg. de. var.			0.171			0.086

Note: We present the regression estimates including a control function stemming from a Heckman selection model. In a first stage we estimated of a probit model with the dependent variable taking the value of one if the student applied to college and zero if she was part of the cohort, but did not apply. The independent variables include the scores on languange and math obtained in an standardize test taken during their high school sophomore year and the average *comuna*/school-type college application ratios for the three years prior to 2015. Based on those estimates, we calculate each student's inverse Mill's ratio (IMR) to control for the fact that the population of students that apply are a selected sample of all those who graduate from high school. Thus, we run the tripe-difference estimators including the IMR as a version of a control function. Second stage regressions include school fixed-effects. The 600 threshold for the PSU score was chosen based on the minimum score required to apply for BVP scholarship. Standard errors clustered at the school level. $*p < 0.1$, $**p < 0.05$, $***p < 0.05$.

we build the inverse Mills ratio (IMR) which we then introduce as a control function in our triple-difference specifications. We present our results in Table C.10. Despite the IMR being statistically significant, our results are robust to the introduction of the control function, with the triple-difference estimates of the effects remaining virtually unchanged.

C.4 Full Regression Tables

Table C.11: Effect of Free College by Type of School

	(1) Applied to a Teach. Program	(2) Apply to Teach. Program as Top Choice	(3) Accepted to Teach. Program
$Decile \leq 6$	0.017***	0.011***	0.008***
	(0.004)	(0.003)	(0.003)
Post	-0.001	-0.003	-0.001
	(0.004)	(0.003)	(0.003)
$Decile \leq 6$ X Post	-0.001	-0.005	-0.006*
	(0.005)	(0.004)	(0.003)
PSU¿600	-0.007	0.007**	-0.068***
	(0.005)	(0.003)	(0.003)
$Decile \leq 6$ X PSU¿600	-0.001	-0.008*	0.006
	(0.006)	(0.004)	(0.004)
Post X PSU¿600	-0.002	0.001	0.001
	(0.005)	(0.004)	(0.003)
$Decile \leq 6$ X Post X PSU¿600	-0.021***	-0.011**	-0.012**
	(0.007)	(0.005)	(0.005)
psu	-0.001***	-0.000***	0.000***
	(0.000)	(0.000)	(0.000)
Male	-0.037***	-0.026***	-0.025***
	(0.002)	(0.002)	(0.002)
N	154,653	154,653	154,653

Standard errors in parentheses

* p¡0.1, ** p¡0.05, *** p¡0.01

Table C.12: Effect of Free College by Type of School

	(1) Teaching as a Choice	(2) Teaching as Top Choice
Voucher	-0.019***	-0.006
	(0.005)	(0.004)
Private	-0.076***	-0.040***
	(0.008)	(0.005)
Post	0.002	-0.006
	(0.006)	(0.004)
Voucher X Post	-0.006	-0.002
	(0.007)	(0.005)
Private X Post	-0.001	0.008
	(0.009)	(0.006)
PSU¿600	0.003	0.006
	(0.007)	(0.005)
Voucher X PSU¿600	-0.014*	-0.003
	(0.008)	(0.006)
Private X PSU¿600	-0.008	0.007
	(0.009)	(0.006)
Post X PSU¿600	-0.025***	-0.010*
	(0.009)	(0.006)
Voucher X Post X PSU¿600	0.015	0.006
	(0.011)	(0.007)
Private X Post X PSU¿600	0.023**	0.007
	(0.011)	(0.008)
psu	-0.001***	-0.000***
	(0.000)	(0.000)
Male	-0.043***	-0.029***
	(0.002)	(0.002)
N	154,277	154,277

Standard errors in parentheses

* p¡0.1, ** p¡0.05, *** p¡0.01

Table C.13: Effect of Free College by Mother's Schooling

	(1)	(2)
	Teaching as a Choice	Teaching as Top Choice
$S = 12$	0.004	-0.001
	(0.005)	(0.004)
$12 < S < 16$	0.001	-0.006
	(0.006)	(0.005)
$S \geq 16$	-0.016**	-0.020***
	(0.007)	(0.005)
Post	-0.005	-0.016***
	(0.006)	(0.004)
$S = 12$ X Post	0.001	0.010*
	(0.007)	(0.006)
$12 < S < 16$ X Post	0.002	0.013**
	(0.008)	(0.006)
$S \geq 16$ X Post	0.010	0.018***
	(0.009)	(0.007)
PSU¿600	0.002	-0.003
	(0.010)	(0.007)
$S = 12$ X PSU¿600	-0.010	0.006
	(0.011)	(0.008)
$12 < S < 16$ X PSU¿600	-0.015	0.006
	(0.011)	(0.008)
$S \geq 16$ X PSU¿600	-0.003	0.015*
	(0.011)	(0.008)
Post X PSU¿600	-0.022*	0.005
	(0.013)	(0.008)
$S = 12$ X Post X PSU¿600	0.007	-0.015
	(0.015)	(0.010)
$12 < S < 16$ X Post X PSU¿600	0.015	-0.009
	(0.015)	(0.010)
$S \geq 16$ X Post X PSU¿600	0.010	-0.011
	(0.015)	(0.010)
psu	-0.001***	-0.000***
	(0.000)	(0.000)
Male	-0.037***	-0.026***
	(0.003)	(0.002)
N	140,984	140,984

Standard errors in parentheses

* p¡0.1, ** p¡0.05, *** p¡0.01

Table C.14: Effect of Free College on Academic Proficiency

	(1)	(2)	(3)	(4)
		High School	Sophomore Year: SIMCE	
	PSU	GPA	Language	Math
$Decile \leq 6$	-5.174***	-3.494***	-2.151***	-1.763***
	(0.617)	(0.913)	(0.445)	(0.449)
Accepted Educ	-21.282***	-42.058***	-6.277***	-12.326***
	(1.493)	(2.117)	(1.070)	(0.988)
$Decile \leq 6$ X Accepted Educ	12.732***	9.766***	6.062***	4.823***
	(1.785)	(2.523)	(1.333)	(1.248)
Post	-0.430	2.515***	-7.100***	1.443***
	(0.623)	(0.853)	(0.510)	(0.471)
$Decile \leq 6$ X Post	0.019	0.939	1.200**	0.478
	(0.769)	(1.099)	(0.591)	(0.589)
Accepted Educ X Post	0.542	-4.443	1.209	-0.572
	(1.877)	(2.778)	(1.563)	(1.421)
$Decile \leq 6$ X Accepted Educ X Post	-4.877**	-4.741	-2.438	-3.684**
	(2.235)	(3.470)	(1.948)	(1.837)
Male	12.777***	-26.111***	-7.788***	10.071***
	(0.390)	(0.680)	(0.303)	(0.288)
N	114,879	114,872	103,572	103,499

Standard errors in parentheses

* p¡0.1, ** p¡0.05, *** p¡0.01

Table C.15: Change in Enrollment by PSU

Panel A: PSU¡600

	Before	After	Diff. (%)	p-value
Business	0.130	0.134	2.89	0.224
Education	0.119	0.124	5.00	0.047
Health	0.182	0.178	-1.88	0.330
Social Sciences/Humanities	0.117	0.117	0.44	0.860
STEM	0.294	0.295	0.36	0.801
Others	0.159	0.151	-4.93	0.018

Panel B: PSU≥600

	Before	After	Diff. (%)	p-value
Business	0.107	0.113	6.09	0.032
Education	0.053	0.047	-11.46	0.004
Health	0.194	0.204	4.81	0.016
Social Sciences/Humanities	0.110	0.116	5.41	0.053
STEM	0.377	0.356	-5.52	0.000
Others	0.160	0.165	3.14	0.162

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