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# Essays in Labor Economics

by

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*Yo aquí me despido, vuelvo a mi casa, en mis sueños, vuelvo a la Patagonia en donde el viento golpea los establos y salpica hielo el Océano. ....en mi patria encarcelan mineros y los soldados mandan a los jueces. Pero yo amo hasta las raíces de mi pequeño país frío. Si tuviera que morir mil veces allí quiero morir: si tuviera que nacer mil veces allí quiero nacer, cerca de la araucaria salvaje, del vendaval del viento sur, de las campanas recién compradas. Que nadie piense en mí. Pensemos en toda la tierra, golpeando con amor en la mesa. No quiero que vuelva la sangre a empapar el pan, los frijoles, la música: quiero que venga conmigo el minero, la niña, el abogado, el marinero, el fabricante de muñecas, que entremos al cine y salgamos a beber el vino más rojo. Pablo Neruda*

# Declarations

This thesis is submitted to the University of Warwick in support of my application for the degree of Doctor of Philosophy. It has been composed by myself and has not been submitted in any previous application for any degree.

The work presented (including data generated and data analysis) was carried out by the author except in the cases outlined below:

- Chapter 1 was done in collaboration with Pablo Navarrete.

# Abstract

In Chapter 1, we estimate the causal effect of homeownership on employment using a regression discontinuity design that exploits an arbitrary threshold arising from a homeownership program that assigns a house to low-income families in Chile. We establish that homeownership decreases employment by between 3.95 and 5.60 percentage points. These results contrast with previous non-experimental literature, which has often found positive effects. Our findings seem to be driven by children of the heads of households not entering the labor market, rather than workers being motivated to leave their job. We also find that residential stability and neighborhood quality are unlikely to drive the effects, contrary to what has been proposed by previous theoretical papers.

Chapter 2 studies the effect of homeownership on the academic achievements of children in the household, using a regression discontinuity design that exploits an arbitrary threshold arising from a voucher-based homeownership assistance program in Chile. Despite the fact that the homeownership program substantially increases the quality of the homes in which students live, I do not find that it affects their test scores. In a subgroup analysis, I find that homeownership decreases the test scores of elementary school students by 0.16 to 0.18 standard deviations. These effects may be due to the fact that, when receiving a voucher, many families cease to live with a hosting family, who are often close relatives (e.g. grand parents), and begin living in their own house. This seems to suggest that students experience a decrease in learning support that was previously provided to them by those close relatives. My results contrast with previous studies, which have often found positive effects of

homeownership on students' academic achievements.

In Chapter 3, I exploit a plausibly exogenous variation in the characteristics of principals to explore their effectiveness in improving school outcomes. Using a difference-in-differences approach, I find that principals appointed under the reform tend to be younger, less experienced, and more highly educated. Drawing from a panel dataset of teacher responses, I observe that the new principals improve the general climate in their schools by decreasing violence and expanding community engagement. On the other hand, they do not improve teacher-monitoring practices, teachers' pedagogical methods, or students' test results. A plausible explanation for these results is the lack of positive or negative incentives given to principals based on the performance of employees in their schools. Evidence in this paper suggests that, in certain institutional settings, school principals do not seem to be as relevant as is often assumed.

## Chapter 1

# Moving “Away” from Opportunities?: Homeownership and Employment.

## 1.1 Introduction

Homeownership is deeply entrenched within society across the Western world. In the United States, President Bush once stated that “owning a home lies at the heart of the American dream”; on the far corner of the American continent, in Chile, President Bachelet expressed that “... people have organized and realized the dream of homeownership”; and across the ocean, in the United Kingdom, David Cameron claimed: “For years politicians have been talking about building what they call affordable homes... What people want are homes they can actually own”. Consequently, governments have attempted to make this aspiration materialize with extensive homeownership programs. *Good Neighbor Next Door* in the United States,<sup>1</sup> *Affordable Homes* and *Right to Buy* in the United Kingdom,<sup>2,3</sup> the *Home Buyers’ Plan* in Canada,<sup>4</sup> and the *Subsidio Habitacional* in Chile are a few examples of a long list of national government programs that attempt to make houses affordable for families.<sup>5</sup>

Despite the strong public commitment to promoting homeownership, the wider impact that owning a house can have on employment still remains in debate. Economists have proposed various theories to predict the nature of this relationship. On the one hand, McCormick (1983) pointed out that families transiting from renting to outright homeownership experience an increase in their assets and in their unearned income. Following a standard labor supply model, this should increase their reservation wage and decrease their employment levels (Cahuc and Zylberberg, 2004). Also predicting a negative effect, Oswald (1996) argues that homeowners are less likely to be employed than renters, since they are less likely to chase jobs across labor markets.<sup>6</sup> On the other hand, Shroder (2002) suggests that “take it or leave it”-styled housing incentives could increase labor supply,<sup>7</sup> as people may work more to earn additional money and buy a house in a more expensive neighborhood, with better access to health and/or educational services. Goss and Phillips (1997) also predicts a positive effect, arguing that homeowners tend

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<sup>1</sup> [https://portal.hud.gov/hudportal/HUD?src=/program\\_offices/housing/sfh/reo/goodn/gnndabot](https://portal.hud.gov/hudportal/HUD?src=/program_offices/housing/sfh/reo/goodn/gnndabot)

<sup>2</sup> <https://www.gov.uk/topic/housing/funding-programmes>

<sup>3</sup> <https://www.gov.uk/right-to-buy-buying-your-council-home/overview>

<sup>4</sup> [http://www.cmhc-schl.gc.ca/en/co/buho/buho\\_008.cfm](http://www.cmhc-schl.gc.ca/en/co/buho/buho_008.cfm)

<sup>5</sup> [http://www.minvu.cl/opensite\\_20150713124520.aspx](http://www.minvu.cl/opensite_20150713124520.aspx)

<sup>6</sup> Oswald’s theory relies on the assumption that the costs associated with selling and buying a house are higher than the ones of moving from one rental property to another.

<sup>7</sup> “Take it or leave it” interventions are those in which individuals can either use the assistance in the form that is offered (in this case, a voucher to purchase a house, rather than for example money), or receive no assistance at all.



to invest more in social capital and establish new networks in the place where they settle, which can provide them with new labor market opportunities.<sup>8</sup>

In this paper, we estimate the causal effect of homeownership on labor supply by exploiting a Chilean homeownership program in which families are quasi-randomly assigned a house. In this program, families apply to receive a voucher to assist them in buying an already-constructed house in the market;<sup>9</sup> and program restrictions neither allow families to obtain a mortgage to complement their voucher, nor to sell, rent or use the house as a collateral within 5 years. Since the number of applicants far exceeds the number of vouchers, the Chilean government selects voucher recipients by ranking applicant families - based on their poverty level and family composition - and, following the ranking, assigns as many vouchers as the budget permits. This method creates an arbitrary cutoff point. Among households just above the cutoff point, 60 percent redeem their voucher and buy a home. Following this, we use a fuzzy regression discontinuity design to study the effect of homeownership on employment for people of legal working age.<sup>10</sup>

Our results suggest that, four years after applying for the voucher, homeowners' employment rates decrease by between 3.95 and 5.60 percentage points (p.p.).<sup>11</sup> In a further analysis we document that this effect is stronger among those that we suspect to be the children of the head of the household, as well as those who were not working at the moment of application.<sup>12</sup> This implies that the effect arises mainly from young people being disincentivized from entering the labor market, rather than current workers being pushed out of it. A possible explanation for this result is that low-income families often see working-age children as a potential source of "extra income" (Field, 2003; Galiani and Schargrotsky, 2010).<sup>13</sup> Since new homeowner

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<sup>8</sup> For a complete review of the potential effects of homeownership please see: Coulson and Fisher (2002); Dietz and Haurin (2003); DiPasquale and Glaeser (1999); Munch et al. (2008).

<sup>9</sup> This is the same program used in Navarrete (2016) to explore the effect of homeownership on student learning. This program not only provides homeownership, but also a windfall in the form of illiquid housing wealth, as the voucher covers around 96 percent of the cost of the house. Our results study the joint effects of homeownership combined with this windfall.

<sup>10</sup> Legal working age is fifteen years. The legal retirement age is sixty for women and sixty-five for men.

<sup>11</sup> For the average applicant, this corresponds to a point around 3 years after becoming a homeowner.

<sup>12</sup> Unfortunately, we are not able to observe the relationship between each household member. We thus define as the head of the household the family member who submitted the application, and so we do not make any assumptions about the gender or age of this person. We define as a spouse a household member who is within 15 years of age of the head of the household. Finally, a child is any household member who is 15 or more years younger than the head of the household. On average, children were at the end of high school at the moment of application and had been out of high school for 2 years at the point when employment is measured.

<sup>13</sup> These authors point out that poor families with no access to risk diversification, savings bank accounts or insurance, must find alternative solutions. They often rely primarily on networks and family members, and in particular, children of legal working age can provide additional sources

parents receive an “extra source of income”, as they stop “paying rent” and do not have to pay a mortgage, there is less pressure put on their working-age children to join the labor market.

Contradicting some of the theoretical literature, we provide evidence suggesting that spatial mobility is not the main driver of these results. In fact, we observe that new homeowners are neither more nor less likely to move to a different labor market. Furthermore, we document that the quality of the area in which new homeowners live also does not seem to drive the effects. This last result, combined with the fact that voucher recipients are more likely to become homeowners, differentiates this study from the Moving to Opportunity (MTO) experiment, which offered rental vouchers to families with the intention of improving their local environment (Sanbonmatsu et al., 2014; Ludwig et al., 2013). While MTO studies the effects of these improvements while keeping housing tenure constant; here, we are looking at the effect of changes in housing tenure, but keeping residential area characteristics constant.

We also provide evidence that voucher recipients are less likely to have one or more children after applying. A potential explanation for this result is that low-income families can perceive newborns as future insurance (Field, 2003; Galiani and Schargrodsky, 2010). However, for voucher-recipient families studied here, this insurance is provided by their house.

The primary contribution of our paper is to provide what we believe are the first estimates of the effects of homeownership on labor supply that rely on both a clearly exogenous source of identifying variation and a representative sample of applicants to housing assistance programs. This paper also contributes to the literature by providing evidence suggesting that the reduced spatial mobility of homeowners is unlikely to be the main driving factor behind the effects, running counter to the prevailing ideas in current literature.

This paper adds primarily to the literature related to homeownership and employment. Although this literature is wide in scope, to the best of our knowledge there has been no study in which homeownership is randomly or quasi-randomly assigned.<sup>14</sup> Furthermore, this paper could add to literature studying the relationship

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of income to the household by joining the labor market. Additionally, young children can be seen as a type of future insurance, able to provide care or shelter for elderly relatives.

<sup>14</sup> Examples of previous literature are: Battu et al. (2008); Van Leuvensteijn and Koning (2004); Goss and Phillips (1997); Munch et al. (2008); Valletta (2013); Flatau et al. (2003). The main concern with all previous studies has been the endogeneity of homeownership and the potential correlation between omitted variables and labor supply given the non-experimental methodology used. For example: characteristics that are not observable to researchers could impact both the tenure decisions and the outcome variable, such as savings discipline.

between rental vouchers and employment (Jacob and Ludwig, 2012; Mills et al., 2006; Sanbonmatsu et al., 2014), governmental transfers and employment (David et al., 2016; Alzúa et al., 2013; Picchio et al., 2017; Imbens et al., 2001), and property rights and employment (Field, 2007; Galiani and Schargrodsky, 2010).

Section 1.2 presents the program studied here. Section 1.3 then provides an overview of the data used in this paper. Section 1.4 presents the empirical strategy of the paper. In Section 1.5, we discuss the regression discontinuity validity in further depth. Section 1.6 presents the results, Section 1.7 the potential mechanisms, Section 1.8 presents a rationalization of the results and Section 1.9 concludes.

## 1.2 The Homeownership Program

Chile has a long tradition of offering subsidized housing to disadvantaged families. Historical accounts show that Chilean housing policy, established in 1906, was designed with a focus on improving the housing conditions of the poorest Chileans.<sup>15</sup> Current housing policies target the poorest 40 percent of the population, and recently the main focus has moved towards providing disadvantaged families with financial assistance to help them to buy their own house.

In the program analyzed in this paper, called *Adquisición de Vivienda Construída* (AVC), families must submit an application to obtain a homeownership voucher from the government.<sup>16</sup> This voucher must be used to buy a house in the market that has already been built, and the number of applicants far exceeds the number of vouchers offered (for a complete description of the program see Appendix 1.A).

In the AVC, families apply through a regional office of the Ministry of Housing and Urbanism (MINVU, from its Spanish name: “*Ministerio de Vivienda y Urbanismo*”), selecting a particular region in which they would like to purchase a house.<sup>17</sup> To prevent a strategic approach to this process, families are only permitted to apply within one single region and are automatically excluded from the process if they apply in multiple regions. In the application process, families must also provide certificates proving that they belong to the poorest 40 percent of the population, and that they have had savings of at least 400 USD in a bank account for a year; if not, they are also excluded from the process. In the final step before applications are closed, the government identifies whether any member of the household already owns a house; if so, these households are also excluded.

<sup>15</sup> For a complete review of Chilean housing policy, see Rubio (2006); Rodrigo (1999).

<sup>16</sup> In every new offer round, families must submit a new application.

<sup>17</sup> Chile is divided into 15 regions, which are the country’s highest level of administrative division.

Once the application period has expired, the selection process is made according to regional rankings and budgetary restrictions. The MINVU first assigns a score to each application based on two factors - the family composition and their poverty level - and then ranks applications at a regional level according to their score. With these regional rankings, the MINVU then assigns as many vouchers as its regional budget will allow.<sup>18</sup>

Vouchers from the AVC program cover the difference between the market price of the house and the family's accredited savings at the moment of application, up to a value of 25,000 USD. To prevent this money from being used for unrelated expenses, the vouchers are issued in the form of certificates. The voucher's corresponding monetary value is only transferred to the seller of the house after the property contract has been signed.

The AVC housing vouchers are subject to a number of regulations. Recipient families are given a maximum timeframe of two years in which they can use their voucher; after this period, it expires permanently. Second, families cannot buy a house owned by a relative - this is checked against the government's own official family records. Third, families are not allowed to complement their MINVU voucher with financial credits when purchasing a house.<sup>19</sup> Finally, families are prohibited from selling and renting the house, or using it as collateral for credit during the first five years of ownership, and must live in the house over this initial period. The Ministry of Housing pays visits to voucher-bought houses and checks whether the recipient family is indeed living in the house. If families do not to comply with this, they either automatically lose their purchased house or are forced to repay the voucher's monetary value to the MINVU.<sup>20</sup>

In this study, we use data from the 2010 and 2011 AVC offer rounds. Offers from earlier years were not included as MINVU funds were not exhausted and so no cutoff point was generated. In 2012 and 2013 there were no offers as AVC resources were focused on housing reconstruction following the 2010 earthquake. Although the program was reintroduced from 2014 onwards, the short timespan between the offers and the follow-up data results renders these rounds of offers unsuitable for analysis.

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<sup>18</sup> The number of vouchers delivered under this program represents less than 3 percent of total transactions in the Chilean housing market.

<sup>19</sup> This prevents a situation in which a financial institution is able to take over a subsidized property following defaults on payments by voucher recipients.

<sup>20</sup> These features are also common in other housing programs, such as the program discussed in Barnhardt et al. (2017).

## 1.3 Data and Descriptive Statistics

### 1.3.1 Heads of Families and Family Member Dataset

To identify the heads of families, and their corresponding family members, the MINVU has provided us with four different datasets. The first dataset contains the 125,213 heads of families who applied for a voucher in 2010 and 2011. The variables within this dataset are the application score, the family ID, the head of family ID, their town and region of residence when applying, their region of application, the offer round to which they applied, and whether the family received a voucher offer or not.

The second dataset contains information regarding the family members included in the application of each head of family. The variables in this dataset are the family ID, the ID of each member of the family, and the offer round of the application. We link both datasets using the family ID and the offer round. The linked dataset contains 362,698 individuals when counting heads of families and all family members (henceforth referred to as *applicants*).<sup>21</sup>

A third dataset tells us whether or not a voucher was paid out and, if so, the amount paid. A separate dataset tells us the date at which the vouchers were paid. This last dataset is incomplete, and we can only correctly match 93 percent of voucher users with their date of voucher payment.<sup>22</sup> These two datasets are merged with the previous information sources using the family ID.

In a fourth dataset, the MINVU provided us with each applicant's date of birth, gender, any children born after applying, and the town of residence four years after applying. MINVU has also provided us with some housing characteristics four years after applying, such as whether the family lives in an apartment, the number of rooms used as bedrooms by the family, whether the family lives in a house without a concrete floor or potable water (referred to as a rudimentary house), and whether the family lives in another family's house (referred to as a hosting family).<sup>23</sup> We successfully link all applicants with their corresponding variables, excluding 4,915 whose gender could not be identified from MINVU records. Following from the data showing the municipality of residence four years after applying, we collected the

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<sup>21</sup> We use the family ID to cluster standard errors by family, as most of the outcomes are likely to be correlated at the family level. As a check for robustness, we run the analysis clustering standard errors at the municipal level, to allow outcomes to be correlated at this level. This does not change the results (see Appendix C5).

<sup>22</sup> MINVU has tried to centrally keep track of these dates, however on some occasions regional offices do not report it, as it was not a mandatory process in 2010 and 2011.

<sup>23</sup> We do not have access to information regarding the specifics of hosting families, however they tend to be close relatives such as parents (CASEN, 2013).

most recent measure of poverty for each municipality (2013) from the Ministry of Planning.<sup>24</sup>

After combining all sources, we examine each regional voucher assignment process to determine whether some regions did not have any applicants who did *not* receive a voucher. Out of 43 regional assignment processes,<sup>25</sup> we found 3 in which all families were offered a voucher. We further analyzed each applicant's region of application, and identified 161 applicants for whom this variable was missing.<sup>26</sup> We removed from the sample these regional assignment processes that had no non-recipients and all applicants for whom the region of application was missing.

Finally, we centered the cutoff scores from every round of offers and region to zero. We then calculated the centered scores of each application by subtracting the corresponding cutoff from each particular application score. The new score variable ranges from -994 to 986.1 in degrees of 0.1.

### 1.3.2 Unemployment Insurance Dataset

The Ministry of Labor provided us with the wage and date of payment for every applicant with unemployment insurance between January 2010 and December 2015.

As the most recent employment data to which we have access is from December 2015, we can measure labor market outcomes for four years beyond the point at which families applied for a voucher. This means that we use 2014 employment data for offer rounds in 2010 and 2015 employment data for offers in 2011.

Using this dataset, we construct the following variables:

- *Employed*: equal to 1 if the applicant is in the *Unemployment Insurance* dataset in a given month and equal to 0 otherwise.
- *Wage*: equal to the observed wage in the *Unemployment Insurance* dataset (in US dollars) for a given month and equal to 0 if the applicant has no observed wage for that month.
- *White-Collar Job*: equal to 1 if the applicant works in commerce, financial services, hotels and accommodation, international organizations, communications, or health or educational institutions and equal to 0 otherwise.

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<sup>24</sup>[http://observatorio.ministeriodesarrollosocial.gob.cl/indicadores/datos\\_pobreza\\_comunal.php](http://observatorio.ministeriodesarrollosocial.gob.cl/indicadores/datos_pobreza_comunal.php)

<sup>25</sup> There were 2 national offer rounds in 2010 and 1 in 2011. These occurred in all 15 regions, except for the Santiago Metropolitan Region, which participated in neither of the 2010 offer rounds. This accounts for the total of 43 regional assignment processes.

<sup>26</sup> This is most likely to have occurred due to a clerical or typing error at the database level.

We also computed applicants' working status, wage, and 'white-collar' measures 2 months before applying as baseline measures. We combine this with the previous dataset using the applicant IDs.

In the final step, we restricted the sample to those applicants who are of legal working age. According to the Chilean Labor Code, the minimum working age is fifteen years (Chilean National Congress, 2002). To legally claim pension benefits, the minimum retirement age is sixty for women and sixty-five for men (Chilean National Congress, 2009).<sup>27</sup> Since the legal working age limit is dependent on gender, applicants whose genders could not be identified were removed from the sample. This process left us with 231,496 applicants.

It is worth noting that the *unemployment insurance* dataset contains only those workers who have a formal contract, and so excludes the self-employed, independent contractors, civil servants, and members of military forces. We address the implications of this on our results in section 1.6.1. Also, our dataset unfortunately does not provide the number of hours worked by employees. We can thus only investigate the effect of this program on the applicants' decision of whether or not to supply labor (extensive margin), but cannot observe the effect on the number of hours supplied by applicants who work (intensive margin).

### 1.3.3 Descriptive Statistics

Table 1.1 provides summary statistics for applicants of legal working age prior to applying, and shows that the average age across the sample is 32 years. Applicants who go on to receive a voucher are around two years younger than those who do not. A possible explanation for this is that families with young children receive a higher score in the poverty index, thus implying that younger parents will be more likely to receive the voucher (for a complete description of the poverty index see Appendix 1.B). This table also shows that the share of females in the sample remains stable at around 66% across the five groups. This implies that households with a high share of females tend to apply for the voucher in higher numbers, however this does not seem to be correlated with receiving a voucher.

Applicants are on average in the 12th decile of the poorest families of the population. Those that go on to receive a voucher are on average in the poorest 8th decile of families. This is not surprising, given that the single biggest component of applicants' scoring is their poverty level, with poorer families receiving a higher score. In these figures, we can observe also that families in our sample are among

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<sup>27</sup> The Chilean population tends to retire shortly after reaching this retirement age, with the average Chilean woman starting to receive a pension at age sixty-one, and the average man at sixty-five.

**Table 1.1: Characteristics of Voucher Recipients and Non-Recipients Before Applying**

	Sample	Non-Recipients	Recipients	Voucher users	Voucher non-users
Age	32.343 (11.903)	32.557 (11.947)	30.040 (11.162)	30.038 (11.323)	30.042 (11.065)
Female	0.654 (0.476)	0.653 (0.476)	0.668 (0.471)	0.656 (0.475)	0.676 (0.468)
% of Poverty dist	12.233 (7.420)	12.597 (7.429)	8.313 (6.073)	8.588 (6.370)	8.147 (5.880)
Metropolitan Area	0.446 (0.497)	0.447 (0.497)	0.427 (0.495)	0.395 (0.489)	0.446 (0.497)
Disabled Family Member	0.035 (0.185)	0.035 (0.183)	0.041 (0.199)	0.044 (0.206)	0.040 (0.195)
Elderly Family Member	0.035 (0.183)	0.035 (0.185)	0.026 (0.161)	0.033 (0.178)	0.023 (0.149)
App. to Another Region	0.001 (0.030)	0.001 (0.031)	0.001 (0.026)	0.002 (0.042)	0.000 (0.000)
Employment	0.243 (0.429)	0.247 (0.431)	0.201 (0.401)	0.200 (0.400)	0.203 (0.402)
Wage (USD)	109.5434 (245.285)	110.7805 (245.145)	96.20908 (246.402)	93.91537 (239.789)	100.010 (256.959)
2010 Offer Round	0.303 (0.460)	0.305 (0.460)	0.283 (0.451)	0.259 (0.438)	0.298 (0.457)
Voucher Value (USD)					16584.420 (3148.344)
Observations	230247	210699	19548	7358	12190

*Notes:* Data is for applicants of legal working age, four years after applying to the 2010 and 2011 offer rounds. *Age* is the average age of applicants measured in years. *Female* is the share of applicants of legal working age that are females. *% of Poverty dist* is the average decile of applicants' poverty distribution with respect to the Chilean population. *Metropolitan Area* is the share of applicants that apply for a voucher in the Santiago Metropolitan Region. *Disabled Family Member* is the share of applicants in families with a handicapped family member. *Elderly Family Member* is the share of applicants in families with an elderly family member. *App. to Another Region* is the share of applicants that apply to buy a house in a different region to their region of residency. *Employment* is the share of applicants that were employed 2 months before applying. *Wage* is the average observed wage 2 months before applying. *2010 Offer Round* is the share of applicants in the sample that applied for an offer in 2010. *Voucher Value* is the average value of the executed voucher in US dollars.

the poorest of the Chilean population.

Around 44 percent of applicants submitted an application to buy a house in the Santiago Metropolitan Region. This is in line with the share of the Chilean population that lives in this area. Voucher non-users tend to be more concentrated in this area. A potential explanation for this difference is that the housing market is tighter in this area, and therefore the voucher may not provide a realistic sum of money to purchase a house.



The share of families that have a disabled or elderly family member, or that applied to a different region from the one in which they live, is remarkably low in all groups. The employment level of applicants before applying is around 24 percent, and voucher recipients show employment levels that are 4 percent lower.

The characteristics of applicants in this program - women in their early thirties with low employment levels - are remarkably similar to those applying to other programs offering different types of housing assistance such as MTO (Sanbonmatsu et al., 2014), the Section 8 program in the United States (Jacob and Ludwig, 2012), and the small-scale Indian program analyzed in Barnhardt et al. (2017). These types of families are thus the most likely to apply for housing assistance, irrespective of its nature.

The average voucher value in the sample is 16,584 USD. Combining the fact that the average voucher recipient is at the 8th decile of the poverty distribution in the Chilean population with the distribution of wages in Chile, we compute that this value is equivalent to 10 years of wages for the average voucher recipient (CASEN, 2013).

## 1.4 Empirical Strategy

We use a regression discontinuity (RD) approach to estimate the impact of becoming a homeowner under the AVC program on employment. We rely on the fact that an applicant's voucher offer status changes discontinuously at the eligibility threshold. Then, we narrow our focus to the set of applicants close to the cutoff under the following assumption: those applicants who only just missed out on receiving a voucher can serve as a good counterfactual for those who only just received it. The plausibility of these assumptions is covered in the next section, but first we will describe the set of equations used.

### 1.4.1 The Effect of Receiving an Offer for a Voucher

The first step in becoming a homeowner under the AVC program is receiving an offer for a voucher, which can alone have an effect on applicants' behavior. To estimate the causal effect of receiving an offer on a given outcome for applicants around the cutoff, the so-called Intention To Treat (ITT), we perform the following OLS regression:

$$y_{i,r,c,t} = \alpha_0 + \eta_{r,c} + \alpha_1 f(\text{Score}_{i,r,c}) + \beta_{ITT} D_{i,r,c} + \alpha_2 D_{i,r,c} * f(\text{Score}_{i,r,c}) + \mathbf{X} + e_{i,r,c,t} \quad (1.1)$$

where  $y_{i,r,c,t}$  is the outcome of interest for applicant  $i$ , in region of application  $r$ , offer round  $c$ , and at time  $t$ .  $\alpha_0$  is the constant of the equation and  $\eta_{r,c}$  is a region-offer round fixed effect.  $\text{Score}_{i,r,c}$  is the application score for applicant  $i$ , in region of application  $r$ , and offer round  $c$ . We study the robustness to different functional forms of  $\text{Score}_{i,r,c}$  for the RD polynomial, using  $f(\cdot)$ , which is estimated separately on either side of the cutoff.<sup>28</sup>  $\mathbf{X}$  controls for a set of family and individual characteristics including the poverty level, gender, age, working status and wage before applying of applicant  $i$ , as well as dummies for whether their family has a disabled or an elderly member.

$D_{i,r,c}$  is a dummy for receiving an offer for a voucher, which is defined as follows:

$$D_{i,r,c} = \begin{cases} 1 & \text{if } \text{Score}_{i,r,c} \geq 0 \\ 0 & \text{if } \text{Score}_{i,r,c} < 0 \end{cases}$$

The coefficient of interest is  $\beta_{ITT}$ , which estimates the causal effect of receiving an offer for a voucher on the outcome of interest, for applicants around the cutoff score. To select the optimal distance from the cutoff to make voucher recipients and non-recipients “locally” comparable, we use the optimal bandwidth proposed by Calonico et al. (2014). Throughout the analysis, we cluster standard errors by family, as all the different measurements are likely to be correlated at the family level.<sup>29</sup>

#### 1.4.2 The Effect of Using the Homeownership Voucher

If all families that received a voucher were to use it to buy a house, we could use equation 1.1 to estimate the causal effect of homeownership under this program on the outcome of interest. Since not all families redeem their voucher, the parameter of interest in Equation 1.1 -  $\beta_{ITT}$  - will not capture the desired causal effect.

<sup>28</sup> The literature has a long history of using different polynomials. Gelman and Imbens (2014) points out that the polynomial order should not be higher than 2.

<sup>29</sup> As a robustness check, we run the analysis clustering standard errors at the municipal level, to allow outcomes to be correlated at this level (see Appendix C5).

To estimate the desired causal effect, we use the voucher assignment indicator as an instrument for homeownership, and perform a Two-Stage Least Square regression. The set of equations estimated is as follows:

$$\begin{aligned} Homeownership_{i,r,c,t} = & \gamma_0 + \mu_{r,c} + \gamma_1 f(\text{Score}_{i,r,c}) + \beta D_{i,r,c} \\ & + \gamma_2 D_{i,r,c} * f(\text{Score}_{i,r,c}) + \mathbf{\Gamma}X + u_{i,r,c,t} \end{aligned} \quad (1.2)$$

$$\begin{aligned} y_{i,r,c,t} = & \delta_0 + \kappa_{r,c} + \delta_1 f(\text{Score}_{i,r,c}) + \beta_{LATE} Homeownership_{i,r,c,t} \\ & + \delta_2 D_{i,r,c} * f(\text{Score}_{i,r,c}) + \mathbf{\Omega}X + v_{i,r,c,t} \end{aligned} \quad (1.3)$$

where  $Homeownership_{i,r,c,t}$  is a dummy indicator, equal to 1 if the applicant  $i$ , in region of application  $r$ , offer round  $c$ , and at time  $t$  bought a house using the voucher and 0 otherwise. The remaining terms are defined as in the previous section. The optimal distance from the cutoff, to make voucher recipients and non-recipients “locally” comparable, is also computed using the optimal bandwidth proposed by Calonico et al. (2014).

In Equation 1.3,  $\beta_{LATE}$  captures the causal effect of using a voucher to buy a house on the outcome of interest for applicants around the cutoff, the so-called Local Average Treatment Effect (LATE). Our results and interpretations are based on  $\beta_{LATE}$ ; however, for completeness, we report  $\beta_{ITT}$  as well.

## 1.5 Regression Discontinuity Validity

### 1.5.1 The Effect of the Being Above the Eligibility Cutoff on Voucher Utilization and House Characteristics

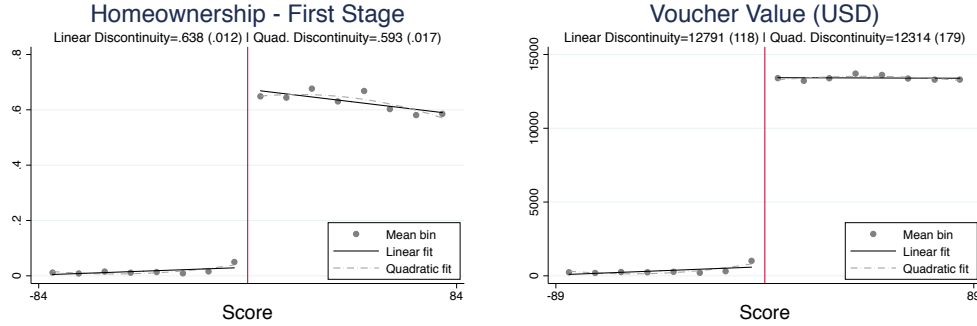
We begin by showing that being above the eligibility cutoff score for receiving an offer for a voucher increases the likelihood of becoming a homeowner, the so-called First Stage. To do this, we estimate Equation 1.1 using as an outcome variable a dummy equal to 1 if the family of the applicant has ever purchased a house using a homeownership voucher after 2010 and 0 otherwise.

The left panel of Figure 1.1 shows that being above the cutoff increases the program take-up by around 60 percent.<sup>30</sup> Point estimates do not vary greatly if we

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<sup>30</sup> To get some sense of the size of the first stage, these point estimates are 20 p.p. higher than the ones in renting programs such as MTO (Sanbonmatsu et al., 2014) and Section 8 (Jacob and Ludwig, 2012), and 30 p.p. higher than in homeownership programs such as the one discussed in Barnhardt et al. (2017).

**Figure 1.1: Effect of Voucher Offer on Utilization and Voucher Value**



*Notes:* Data is for applicants of legal working age, four years after applying to the 2010 and 2011 offer rounds. The left panel uses a dummy variable equal to 1 if the applicant has ever bought a house using a homeownership voucher after 2010 and 0 otherwise. The right panel uses as a variable the value of the voucher redeemed by the applicant after 2010. Each dot represents the mean of the corresponding outcome for the applicants in a particular bin. The vertical line represents the eligibility cutoff score for receiving an offer for a voucher.

control for  $\text{Score}_{i,r,c}$  with polynomials of order 1 or order 2. Table C1 in the appendix confirm these results and shows that in both cases the estimation is significant at the 1 percent level.

This panel also shows that some applicants below the cutoff point use the voucher. This is due to two factors: 1) applicants can appeal to reverse the Ministry of Housing's decision not to offer them a voucher, and 2) applicants who did not receive a voucher in an early round of offers may re-apply and receive a voucher in a later one. Non-recipient applicants could also become homeowners by buying a house in the market without using a voucher. We find it unlikely that families close to the cutoff and who did not receive a voucher have sufficient money to buy a house on the market - particularly when the average value of the voucher is already 10 times the annual salary of a worker at that level of the income distribution. Unfortunately, we do not have data to see whether non-recipients families went on to purchase a house without any assistance.

The left panel of Figure 1.1 also shows that some voucher recipients did not redeem their voucher. Table 1.1 shows that poorer families and families who applied in the Santiago Metropolitan Region are in fact less likely to redeem their voucher. This could reflect two points: first, that poorer families face higher searching costs, perhaps due to their personal networks being more limited in their ability to help them with searching for a house; and second, that the voucher value might not be sufficient for certain areas with a tighter housing market and more expensive houses,

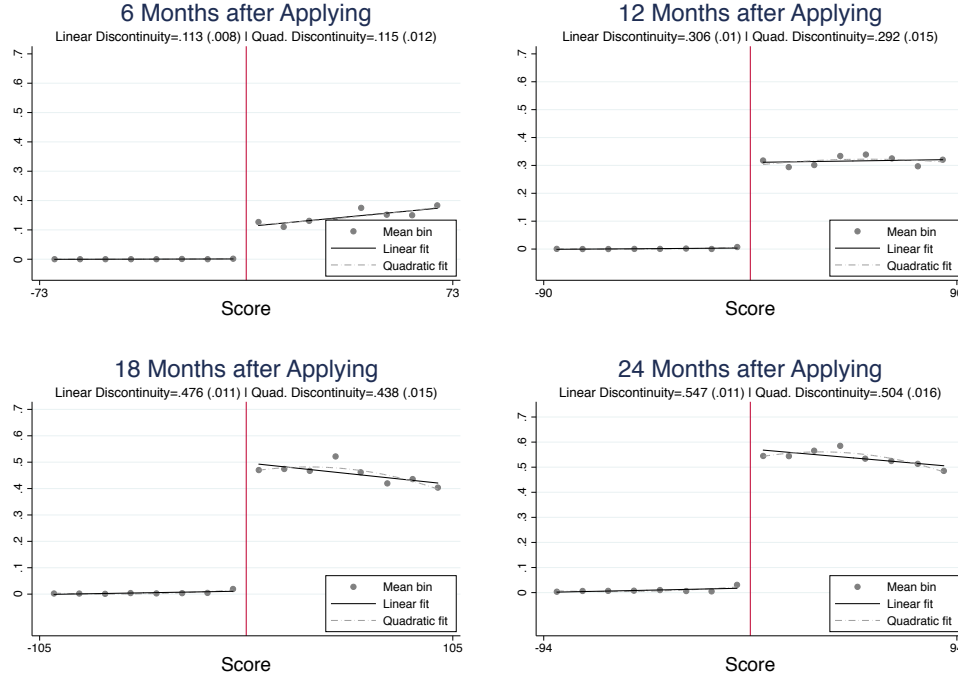
such as the Santiago Metropolitan Region.

The right panel of Figure 1.1 shows that applicants who are only just above the cutoff score receive a large wealth shock in the form of a house. Appendix Table C1 shows that this estimated wealth shock is around 20,000 USD in the IV estimation. Given the restrictions and program conditions described in Section 1.3.1, in that new homeowners cannot sell or rent their house for 5 years, the wealth transferred is extremely illiquid for the period under analysis.

By exploiting the date at which each voucher was paid, we aim to understand how long families take to use their voucher and how long they have been homeowners at different points after their application. Figure 1.2 shows the share of applicants that used their voucher at different points in time, spaced by 6 months. This figure illustrates that around 50 percent of families that use the voucher do so within 12 months of applying, with almost 75 percent doing so within 18 months (see the two lower panels). Appendix Table C2 confirms that voucher utilization is around 30 p.p. 12 months after application and 47 p.p. 18 months after application. The outcomes in Section 1.6 should thus be understood as if the applicants have been homeowners on average for between 2.5 and 3 years.

In exploring the effect that voucher utilization has on house quality, we perform Equation 1.3, but using as an outcome of interest one of the house characteristics described in Section 1.3.1. Figure 1.3 and Appendix Table C3 show that homeownership under this program has a causal effect on reducing the probability of applicants living with a hosting family (e.g. parents) by around 46 p.p., of living in a rudimentary house (e.g. a house without a concrete floor or potable water) by around 7 p.p., and increases the number of rooms that the family use as bedroom by around 0.14 rooms. We do not observe any causal impact on the probability of living in an apartment. Appendix Table C3 is also informative in two further ways: 1) the comparison group for voucher recipients is comprised of roughly 55 percent of applicants who are renters and 45 percent who live with a hosting family,<sup>31</sup> and; 2) living in a rudimentary house is not common for applicants in the absence of this program.<sup>32</sup>

**Figure 1.2: Homeownership Voucher Utilization by Dates After Applying**



*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. The top left panel uses a dummy variable equal to 1 if the applicant used a voucher to buy a house 6 months after applying and 0 otherwise. The top right panel uses a dummy variable equal to 1 if the applicant used a voucher to buy a house 12 months after applying and 0 otherwise. The bottom left panel uses a dummy variable equal to 1 if the applicant used a voucher to buy a house 18 months after applying and 0 otherwise. The bottom right panel uses a dummy variable equal to 1 if the applicant used a voucher to buy a house 24 months after applying and 0 otherwise. Each dot represents the mean of the corresponding outcome variable for the applicants in a particular bin. The vertical line in each panel represents the cutoff score for receiving a voucher offer.

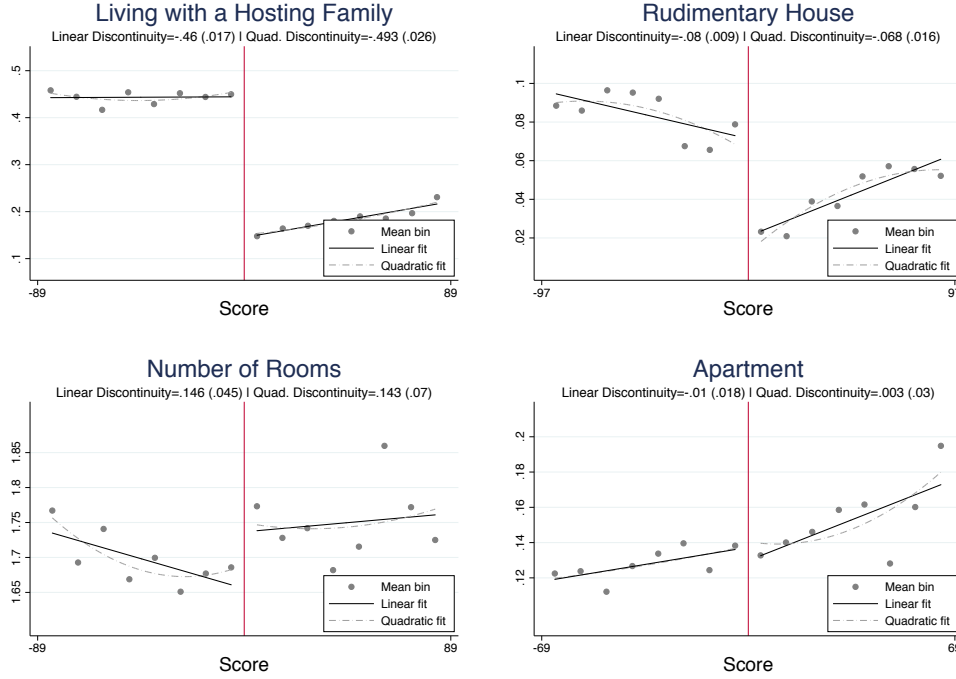
### 1.5.2 The Continuity of the Share of Applicants Around the Cutoff

A key assumption in the regression discontinuity design is that applicants cannot manipulate their application score, so they cannot actively choose to be above the cutoff (McCrary, 2008). Qualitatively, if applicants are able to manipulate the application score, we should see a discontinuity in the mass of applicants below and above the cut-off.

<sup>31</sup> Previous literature in both renting and homeownership programs have remained silent about these proportions, most likely due to a lack of data.

<sup>32</sup> The average share of non-recipient families living in a rudimentary house is around 8 p.p. This differentiates our population from those analyzed in property rights literature, which contains a large share of people without basic services Field (2007).

**Figure 1.3: Effect of Homeownership Voucher Utilization on House Characteristics**



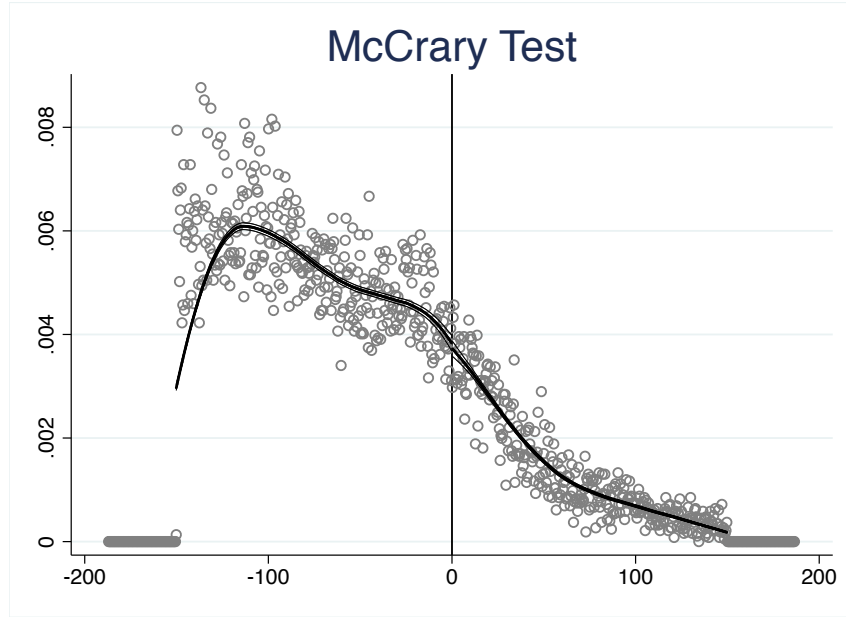
*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Living with a Hosting Family* is a dummy variable equal to 1 if the applicant lives in another family's house and 0 otherwise. *Rudimentary House* is a dummy variable equal to 1 if applicant lives in a house without concrete floor or potable water and 0 otherwise. *Number of Rooms* is the number of rooms that the family use as bedroom in the house. *Apartment* is a dummy variable equal to 1 if the family lives in an apartment and 0 otherwise. Each dot represents the mean of the corresponding outcome variable for the applicants in a particular bin. The vertical line in each panel represents the cutoff score for receiving a voucher offer. Point estimates and standard errors are from the IV regression (Equation 1.3).

When performing the McCrary test to determine whether this discontinuity exists, we reject the manipulation of the score with a t-stat of -0.4068 and a p-value of 0.6842. Figure 1.4 also shows no discontinuity around the cutoff in the number of applicants. This suggests that applicants do not manipulate the application score to be above the cutoff. This is not surprising given that applicants (and authorities) do not know the cutoff ex-ante.

### 1.5.3 The Continuity of Applicant Characteristics Around the Cutoff

Under the identifying assumption of the RD estimator - that treatment and control groups are locally comparable - predetermined covariates should be locally balanced around the threshold (Lee and Lemieux, 2010). To test this, we estimate

Figure 1.4: McCrary Test



Notes: Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. Each circle represents the share of applicants of legal working age in a particular bin.

Equation 1.3 using as an outcome variable one of the predetermined covariates in  $\mathbf{X}$  described in Section 1.4.1.

Figure 1.5 does not show a discontinuity in the mean of any covariate around the threshold. Table 1.2 confirms that there is no discontinuity in any of the predetermined covariates. This evidence suggests that the treatment is as good as locally randomly assigned around the cutoff.

## 1.6 Results

We show that homeownership under this program decreases employment and that the effect is concentrated among those who seem to be the child of the head of the household and those who were not working at the time of application.

### 1.6.1 The Effect of the Homeownership Voucher on Employment and Wages

The left panel of Figure 1.6 displays the causal relationship between employment level and homeownership under this program around the cutoff. This panel shows that applicants who received an offer for a housing voucher - those whose

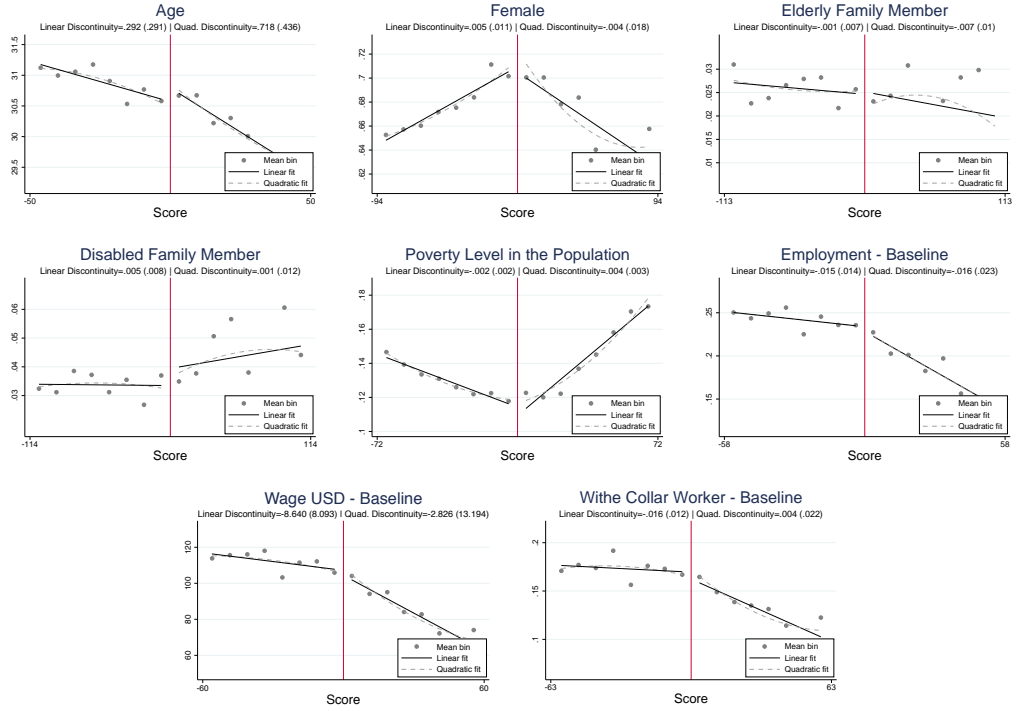


**Table 1.2: Continuity of Covariates Around the Cutoff**

VARIABLES	Age		Female		Elderly Fam. Member		Disabled Fam. Member		Poverty Level in the Population		Employment BL		Wage BL		White Collar Worker	
	(Linear) (1)	(Quad.) (2)	(Linear) (3)	(Quad.) (4)	(Linear) (5)	(Quad.) (6)	(Linear) (7)	(Quad.) (8)	(Linear) (9)	(Quad.) (10)	(Linear) (11)	(Quad.) (12)	(Linear) (13)	(Quad.) (14)	(Linear) (15)	(Quad.) (16)
<i>IV Results</i>																
Homeownership IV	0.292 (0.291)	0.718 (0.436)	0.00532 (0.0111)	-0.00417 (0.0177)	-0.00147 (0.00681)	-0.00728 (0.00982)	0.00498 (0.00829)	0.00148 (0.0123)	-0.00154 (0.00181)	0.00358 (0.00302)	-0.0154 (0.0141)	-0.0159 (0.0229)	-8.640 (8.093)	-2.826 (13.19)	-0.0163 (0.0122)	0.00399 (0.0215)
R-squared	0.010	0.009	0.008	0.005	0.004	0.003	0.005	0.004	0.778	0.775	0.024	0.025	0.029	0.029	0.020	0.019
<i>ITT Results</i>																
Homeownership ITT	0.177 (0.176)	0.432 (0.263)	0.00113 (0.00699)	-0.00170 (0.0103)	-0.00100 (0.00429)	-0.00467 (0.00619)	0.00317 (0.00521)	0.00121 (0.00767)	-0.000783 (0.00112)	0.00108 (0.00164)	-0.00980 (0.00866)	-0.00927 (0.0130)	-5.445 (4.985)	-1.822 (7.511)	-0.0106 (0.00760)	0.00126 (0.0114)
R-squared	0.010	0.010	0.010	0.010	0.004	0.004	0.005	0.005	0.778	0.778	0.025	0.025	0.030	0.030	0.020	0.021
Observations	29,457	29,457	53,040	53,040	63,627	63,627	64,393	64,393	41,037	41,037	33,887	33,887	34,846	34,846	36,118	36,118
BANDWIDTH	49	49	93	93	112	112	113	113	71	71	57	57	59	59	62	62
CONTROLS	NO	NO	NO	NO	NO	NO	YES	YES	YES	YES	YES	YES	NO	NO	NO	NO
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
REGION FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
CONTROL MEAN	30.903	30.903	.675	.675	.026	.026	.034	.034	.13	.13	.243	.243	112.139	112.139	.173	.173

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Age* is the age in years of applicants. *Female* is a dummy variable equal to 1 if the applicant is female and 0 otherwise. *Elderly Fam. Member* is a dummy variable equal to 1 if the family has an elderly family member when applying and 0 otherwise. *Disabled Fam. Member* is a dummy variable equal to 1 if the family has a handicapped family member when applying and 0 otherwise. *Poverty Level in the Population* is the percentile of the applicant's family poverty level with respect to the Chilean population. *Employment BL* is a dummy variable equal to 1 if the applicant was working before applying and 0 otherwise. *Wage BL* is the wage of applicants before applying. *White Collar Worker* is a dummy variable equal to 1 if the applicant was working in a white collar job (as defined in Section 1.3) and 0 otherwise. *Control Mean* is the mean of the corresponding outcome variable for applicants in the bandwidth who are below the cutoff score. Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Figure 1.5: Continuity of Covariates around the Cutoff**

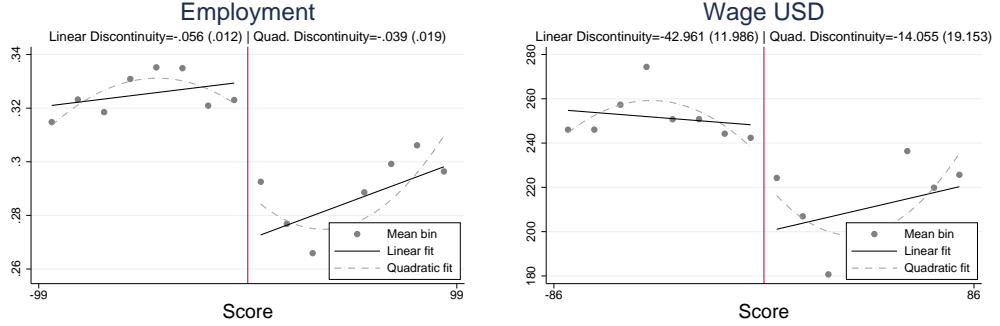


*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Age* is the age in years of applicants. *Female* is a dummy variable equal to 1 if the applicant is female and 0 otherwise. *Elderly Fam. Member* is a dummy variable equal to 1 if the family has an elderly family member when applying and 0 otherwise. *Disabled Fam. Member* is a dummy variable equal to 1 if the family has a handicapped family member when applying and 0 otherwise. *Poverty Level in the Population* is the percentile of the applicant's family poverty level with respect to the Chilean population. *Employment BL* is a dummy variable equal to 1 if the applicant was working before applying and 0 otherwise. *Wage BL* is the wage of applicants before applying. *White Collar Worker* is a dummy variable equal to 1 if the applicant was working in a white collar job (as defined in Section 1.3) and 0 otherwise. Each dot represents the mean of the pre-determined covariates for the applicants in a particular bin. The vertical line in each panel represents the cutoff score for receiving a voucher offer. Point estimates and standard errors are from the IV regression (equation 1.3).

application scores put them just above the eligibility threshold - are on average less employed than applicants who did not receive one (control group). Columns 1 and 2 of Table 1.3 confirm these results and show that homeownership under this program decreases employment by between 3.95 and 5.60 p.p. (by between 12 and 17 percent of the employment level of the control group). The effect is statistically significant in all specifications. This piece of evidence suggests that homeownership under this program has a negative causal effect on employment. This result is robust to different bandwidths as shown in Appendix Figure D2.

It is worth noting that the level of employment is 32.5 p.p. in the control group. Comparable levels of employment are found in studies focusing on similar populations, over similar time spans and using similar datasets such as Moving to Opportunity and Section 8.

**Figure 1.6: The Effect of the Homeownership Voucher on Employment and Wages**



*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Employment* is a dummy variable equal to 1 if the applicant is working four years after applying and 0 otherwise. *Wage USD* is the applicant's observed wage four years after applying. Each dot represents the mean of the corresponding outcome variable for the applicants in a particular bin. The vertical line in each panel represents the cutoff score for receiving a voucher offer. Point estimates and standard errors are from the IV regression (Equation 1.3).

As mentioned in Section 1.3.2, the UI dataset does not contain people who work in the informal sector. We can provide two pieces of information to show that this does not seem to represent a major problem in our estimations. First, the share of the Chilean population of legal working age with unemployment insurance is around 30 p.p., which shows that the population under study in this paper tends to be more formalized than the overall Chilean population. This could potentially reflect an inherent motivation amongst applicants, which may be unsurprising, given that they must collect papers, save money, and go through the application process in order to be in the sample. Second, the share of people of legal working age in the informal sector is around 9.5 p.p.<sup>33</sup> A back of the envelope estimation suggests that the level of informality in the group of people who received the voucher - and are around the cutoff - should be around 45 percent higher than those who did not receive the voucher - and are around the cutoff - in order to cancel out the effects. In light of these two pieces of information, we do not think that our lack of observation of informal workers has a dramatic effect on our results.

Next, we attempt to analyze whether homeownership under this program has an effect on wages. Since homeownership affects the likelihood of being employed, we cannot study its effect on the wages of employed applicants: the populations

<sup>33</sup> The Chilean National Institute of Statistics does not report this figure for the lowest 10 decile of the population.

employed to the left and to the right of the threshold are not comparable. Instead, we conduct the analysis across the whole sample, assigning a wage of 0 to unemployed individuals. The right panel of Figure 1.6 does not show a clear discontinuity around the threshold for salaries of applicants. Table 1.3 suggests that homeownership has a negative impact on wages, which is not robust across our specification (Columns 3 and 4). The results on wages seem ambiguous, not allowing us to draw conclusions about the impact of homeownership on wages.

**Table 1.3: The Effect of the Homeownership Voucher on Employment and Wages**

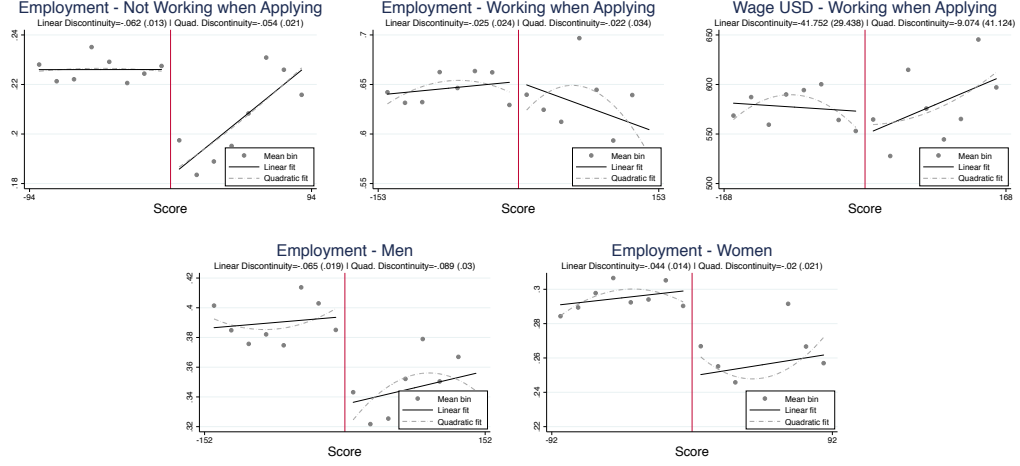
VARIABLES	Employment (Linear) (1)	Employment (Quad.) (2)	Wage USD (Linear) (3)	Wage USD (Quad.) (4)
<i>IV Results</i>				
Homeownership IV	-0.0560*** (0.0115)	-0.0395** (0.0186)	-42.96*** (11.99)	-14.06 (19.15)
R-squared	0.161	0.161	0.213	0.212
<i>ITT Results</i>				
Homeownership ITT	-0.0359*** (0.00726)	-0.0244** (0.0107)	-27.24*** (7.532)	-9.009 (11.19)
R-squared	0.164	0.164	0.215	0.215
Observations	55,916	55,916	48,292	48,292
BANDWIDTH	98	98	85	85
CONTROLS	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES
CONTROL MEAN	.325	.325	251.56	251.56

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Employment* is a dummy variable equal to 1 if the applicant is working four years after applying and 0 otherwise. *Wage USD* is the applicant's observed wage four years after applying. Controls include gender, age, poverty level, elderly family member, disabled family member, employment status before applying and wage before applying. *Control Mean* is the mean of the corresponding outcome variable for applicants in the bandwidth who are below the cutoff score. Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### 1.6.2 The Effect of the Homeownership Voucher on Employment by Working Status When Applying and Gender

Looking at the heterogenous effects of homeownership under this program on employment, Figure 1.7 shows that the effect is stronger for applicants who

**Figure 1.7: The Effect of the Homeownership Voucher on Employment by Working Status when Applying and by Gender**



*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Employment* is a dummy variable equal to 1 if the applicant is working four years after applying and 0 otherwise. *Wage USD* is the applicant's observed wage four years after applying. *Not Working when Applying* is the subgroup of applicants who were not working before applying. *Working when Applying* is the subgroup of applicants who were working before applying. *Men* is the subgroup of applicants who are men. *Women* is the subgroup of applicants who are women. Each dot represents the mean of the corresponding outcome for the applicants in a particular bin. The vertical line in each panel represents the cutoff score for receiving a voucher offer. Point estimates and standard errors are from the IV regression (Equation 1.3).

were not working when they applied for the voucher (top left panel) compared to applicants who were working at the time of application (top center panel). Table 1.5 confirms these results and shows a significant effect on non-working applicants by between -6.18 and -5.35 p.p. (columns 1 and 2). For the group of applicants who were employed when applying, the point estimate on employment - in the linear and quadratic specifications - is around half the size of the previous one and not significant (columns 3 and 4 of Table 1.5). These results suggest that the observed negative impact on employment is stronger in its effect of preventing individuals from joining the labor market than it is in leading those who are already employed away from their work.

As the homeownership voucher seems to have no effect on applicants who were working at the time of application, we analyze the effect on wages for this particular group: the employed populations to the left and to the right of the threshold are comparable. The top right panel of Figure 1.7 does not show any discontinuity in the wages of this particular subgroup. Columns 5 and 6 of Table 1.5 confirm that there is no discontinuity in the wages of this group. This result seems to imply that applicants who were working at the moment of applications either do not move to lower or higher paid jobs or they do not change the number of hours that they

work.<sup>34</sup>

Considering gender, we do not find robust evidence showing that the effect is different for men than for women. The bottom panels of Figure 1.7 suggest a decrease in employment at the cutoff for both groups. Table 1.5 shows that in the linear specification, homeownership under this program has a similar negative point estimate for both groups. These results vary in our quadratic specification, as the decrease in employment for women seems to be smaller. Since the results for gender are not as conclusive as previous results, we cannot draw any conclusions for this particular subgroup at this point. Appendix Figure D1 presents the age distribution, showing that men are more concentrated in the younger age groups. If the age is an important driver of the effect, this could partly explain these ambiguous results. We will return to this discussion in the next section, where we discuss how the impacts vary depending on the relationship of each applicant to the head of the household and age.

### 1.6.3 The Effect of the Homeownership Voucher on Employment Depending on Each Applicant's Relationship to the Head of the Household.

In this section, we attempt to see who in the household is driving our observed effects. Unfortunately we do not have access to the specific relationship of each household member to the main applicant, but we are able to use the age of each person as an indicator of this relationship. We thus define three exclusive groups: 1) the *Head of Household* is the person who submitted the application;<sup>35</sup> 2) the *Spouse* is a family member who is less than fifteen years younger or older than the *Head of Household*; and 3) the *Child* is a family member who is younger than the *Head of Household* by fifteen years or more. These last two groups combined are referred to as *Not Head of Household* throughout the rest of the paper.

Figure 1.8 shows that the effect is not driven by the head of the household (top left panel), but rather by someone who is not the head of the household (top right panel). The bottom two panels show that the child of the household is most likely to be driving the effects. Table 1.5 confirms these results, showing that the head of the household is not affected (columns 1 and 2), while children are the only ones to show a robust effect. For this particular group, employment levels decrease

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<sup>34</sup> This may be due to people simply choosing whether or not they work at all, rather than the number of hours that they work.

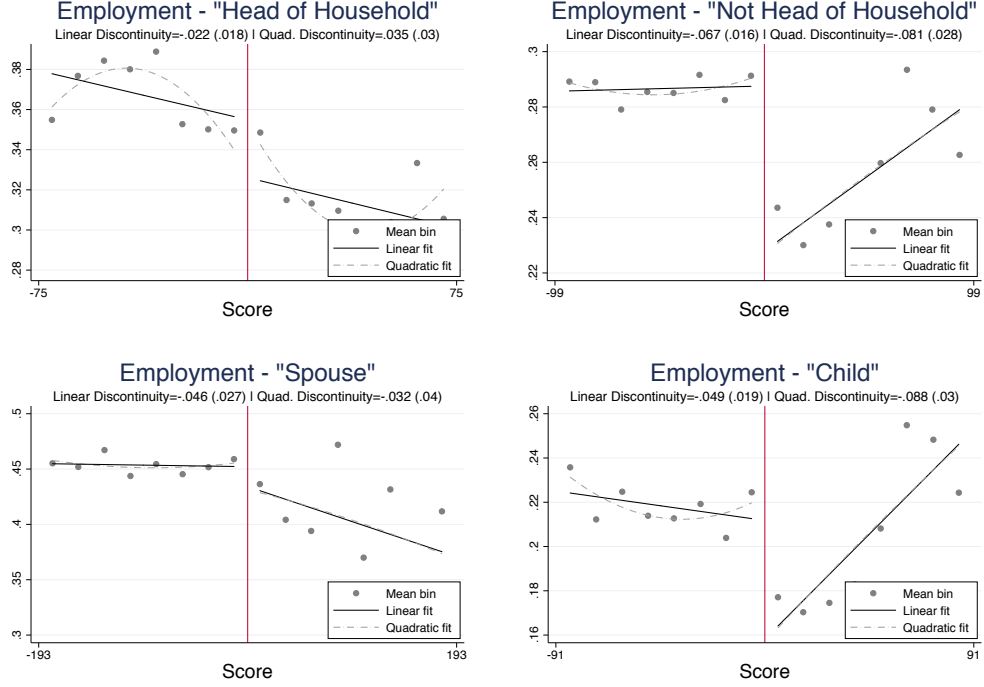
<sup>35</sup> Under this definition, we do not make any assumptions about the age and gender of the head of household.

**Table 1.4: Effect of Homeownership on Employment by Working Status When Applying and by Gender**

VARIABLES	Not Working Before App.			Working Before App.			Men			Female		
	Employment (Linear)	Employment (Quad.)	Employment (Linear)	Employment (Quad.)	Wage USD (Linear)	Wage USD (Quad.)	Employment (Linear)	Employment (Quad.)	Employment (Linear)	Employment (Quad.)	Employment (Linear)	Employment (Quad.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)		
<i>IV Results</i>												
Homeownership IV	-0.0618*** (0.0129)	-0.0535*** (0.0207)	-0.0245 (0.0238)	-0.0219 (0.0336)	-41.75 (29.44)	-9.074 (41.12)	-0.0651*** (0.0186)	-0.0894*** (0.0299)	-0.0445*** (0.0138)	-0.0195 (0.0215)		
R-squared	0.010	0.009	0.039	0.041	0.167	0.166	0.200	0.197	0.129	0.128		
<i>ITT Results</i>												
Homeownership ITT	-0.0393*** (0.00809)	-0.0323*** (0.0120)	-0.0173 (0.0150)	-0.0134 (0.0210)	-27.53 (18.71)	-5.859 (25.95)	-0.0396*** (0.0113)	-0.0508*** (0.0162)	-0.0287*** (0.00885)	-0.0127 (0.0131)		
R-squared	0.011	0.012	0.040	0.041	0.167	0.167	0.201	0.201	0.129	0.129		
Observations	41,052	41,052	18,491	18,491	20,240	20,240	27,794	27,794	35,074	35,074		
CONTROLS	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES		
BANDWIDTH	93	93	152	152	167	167	151	151	91	91		
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES		
CONTROL MEAN	.226	.226	.646	.646	577.232	577.232	.389	.389	.295	.295		

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Employment* is a dummy variable equal to 1 if the applicant is working four years after applying and 0 otherwise. *Wage USD* is the applicant's observed wage four years after applying. *Not Working when Applying* is the subgroup of applicants who were not working before applying to an offer round. *Working when Applying* is the subgroup of applicants who were working before applying to an offer round. *Men* is the subgroup of applicants who are men. *Women* is the subgroup of applicants who are women. Controls include gender, age, poverty level, working status before applying, wage before applying, elderly family member, and disabled family member. *Control Mean* is the mean of the corresponding outcome variable for applicants in the bandwidth who are below the cutoff score. Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Figure 1.8: The Effect of the Homeownership Voucher on Employment by Relationship to the Head of Household**



*Notes:* Data is for applicants of legal working age, four years after they applying to 2010 and 2011 offer rounds. *Employment* a dummy variable equal to 1 if the applicant is working four years after applying and 0 otherwise. *Head of Household* is the person who submitted the application. *Not Head of Household* is any household member who is not the *Head of Household*. *Spouse* is a family member who is less than fifteen years younger or older than the *head of household*. *Child* is a family member who is younger than the *Head of Household* by fifteen years or more. Each dot represents the mean of *Employment* for the corresponding subgroup of applicants in a particular bin. The vertical line in each panel represents the cutoff score for receiving a voucher offer. Point estimates and standard errors are from the IV regression (Equation 1.3).

by between -4.87 and -9.2 p.p. (columns 7 and 8).

Consistent with these results, Appendix Figure D3 presents the effect of homeownership on employment by age and shows that the robust effects are mainly driven by applicants who are younger than 30 years of age. This is in line with our previous findings that the effect is mainly from family members who are at least 15 years younger than the main applicant.

A possible explanation for these result is that low-income families often see working-age children as a potential source of “extra income” (Field, 2003; Galiani



and Schargrodsy, 2010).<sup>36</sup> These working-age children are on average 20 years of age, which means that at the moment of application they were finishing high school, and at the time at which employment is measured, they had been out of school for 2 years. Since new homeowner parents effectively receive an extra source of income, as they stop “paying rent” and do not have to pay a mortgage, there is less pressure put on their working-age children to join the labor market.

**Table 1.5: The Effect of the Homeownership Voucher on Employment by Relationship to the Head of Household**

VARIABLES	Head of Household		Not Head of Household					
			All		Spouse		Children	
	Employment (Linear) (1)	Employment (Quad.) (2)	Employment (Linear) (3)	Employment (Quad.) (4)	Employment (Linear) (5)	Employment (Quad.) (6)	Employment (Linear) (7)	Employment (Quad.) (8)
<i>IV Results</i>								
Homeownership IV	-0.0224 (0.0181)	0.0352 (0.0297)	-0.0667*** (0.0163)	-0.0830*** (0.0281)	-0.0462* (0.0266)	-0.0251 (0.0399)	-0.0487*** (0.0187)	-0.0926*** (0.0302)
R-squared	0.166	0.153	0.162	0.154	0.219	0.199	0.153	0.126
<i>ITT Results</i>								
Homeownership ITT	-0.0144 (0.0117)	0.0183 (0.0173)	-0.0405*** (0.00997)	-0.0470*** (0.0150)	-0.0292* (0.0166)	-0.0154 (0.0239)	-0.0289** (0.0113)	-0.0534*** (0.0170)
R-squared	0.166	0.160	0.164	0.158	0.219	0.199	0.154	0.147
Observations	21,344	21,344	28,243	28,243	14,805	14,805	18,420	18,420
BANDWIDTH	74	74	98	98	192	192	90	90
CONTROLS	NO	YES	NO	YES	NO	YES	NO	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES
CONTROL MEAN	.367	.367	.287	.287	.453	.453	.219	.219
MEAN AGE	37.98	37.98	27.32	27.32	38.90	38.90	20.42	20.42

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Employment* is a dummy variable equal to 1 if the applicant is working four years after applying and 0 otherwise. *Head of Household* is the person who submitted the application. *Not Head of Household* is any household member who is not the *Head of Household*. *Spouse* is a family member who is less than fifteen years younger or older than the *Head of Household*. *Child* is a family member who is younger than the *Head of Household* by fifteen years or more. Controls include gender, age, poverty level, working status before applying, wage before applying, elderly family member, and disabled family member. *Control Mean* is the mean of the corresponding outcome variable for applicants in the bandwidth who are below the cutoff score. Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Returning to the discussion from the previous section, we conduct the analysis by gender, but restricting the age of the applicants to those who are 30 years of age or younger. Even though the estimate becomes less precise due to the reduction in sample size, Figure D4 and Table C4 in the appendix suggest that, if anything, once the sample is restricted by age, female applicants seem to be more affected than male applicants.

## 1.7 Potential Mechanisms

The results indicate that homeownership under this program reduces the labor supply of those who seem to be the children of the main applicant and those

<sup>36</sup> These authors point out that low-income families with no access to risk diversification, savings bank accounts or insurance, must find alternative solutions. They often rely primarily on networks and family members, and in particular, children of legal working-age can provide additional sources of income to the household by joining the labor market. Additionally, Young children can be seen as a type of future insurance, able to provide care or shelter for elderly relatives.

who were not working at the time of application. In trying to explain what may be driving these effects, we show that the mobility of applicants or the quality of the area in which they live does not play any role. On the other hand, we show that voucher recipients tend to have fewer children after the offer round than voucher non-recipients; this suggests that the recipient families see less of a pressing need for future informal insurance.

### 1.7.1 The Effect of the Homeownership Voucher on Residential Stability, Area Quality, and Newborns

As described in the introduction, some scholars have argued that the reduced mobility of homeowners - the so-called lock-in effect - is a mechanism through which homeownership could reduce employment. To test this, we use municipalities as a proxy for a labor market;<sup>37</sup> and estimate Equation 1.3 using as a dependent variable a dummy indicator equal to 1 if the applicant is living in a different municipality from the one in which they were living when applying and 0 otherwise. It is worth noting that, in the metropolitan area (from which 45 percent of applications originate), the average largest distance across a municipality is 5 kilometers (see Appendix Figure D5).

The top left panel of Figure 1.9 and columns 1 and 2 of Table 1.6 provide evidence suggesting that families who did not receive a voucher do not tend to move to another municipality in a higher proportion than those who did receive one. These results imply that, in our context, the reduced mobility of homeowners does not seem to be the main mechanism driving the negative effect of homeownership on employment. This result also provides evidence that, even in an extreme case where homeowners are forced to remain in a fixed location for five years, renters are still not more likely to move to a different municipality in a higher proportion.

Similar results regarding mobility were previously found in other contexts such as those studied in Sanbonmatsu et al. (2014) and Barnhardt et al. (2017). The authors explained these results by arguing that families place a high importance on the social networks established in their place of residency, and that this encourages them to stay in the same area.

The top right panel of Figure 1.9 shows no discontinuity in the poverty level of the municipalities in which applicants live after the offer rounds. Columns 3 and 4 of Table 1.6 also do not show statistically significant results in this dimension. Thus the quality of the area in which homeowners under this program live - and the

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<sup>37</sup> While we do not consider this to be strictly true, we believe that it provides a reasonable approximation.

**Figure 1.9: The Effect of the Homeownership Voucher on Residential Stability, Area Quality, and Newborns**



*Notes:* Data is for applicants of legal working age, four years after they applying to 2010 and 2011 offer rounds. *Applicant Moving to Another Municipality* is a dummy variable equal to 1 if the applicant is living in a different municipality from the one in which they lived when applying and 0 otherwise. *Municipal Poverty level* is the average poverty level of the municipality in which the applicant lives four years after applying. *Child after Applying* is a dummy variable equal to 1 if the household has 1 or more children born into the family after applying and 0 otherwise. Each dot represents the mean of the corresponding outcome for the applicants in a particular bin. The vertical line in each panel represents the cutoff score for receiving a voucher offer. Point estimates and standard errors are from the IV regression (Equation 1.3).

subsequent networks that they form in their new place of residence - are not likely to drive any of the observed effects.

The bottom panel of Figure 1.9 shows that voucher recipients are less likely to have a new child born after applying, and Table 1.6 shows this decrease to be between 5.0 and 6.3 p.p. A plausible explanation for this result is that parents in low-income families tend to perceive young children as a source of future informal insurance (Field, 2003; Galiani and Schargrodsky, 2010). Homeowners under this program thus face less of a pressing need to have more children as they receive a

large transfer of wealth from the government, in the form of a house, which becomes their future insurance.

**Table 1.6: The Effect of the Homeownership Voucher on Residential Stability, Area Quality, and Newborns**

VARIABLES	Applicant Moving to another Municipality		Municipal Poverty Level		Child after Applying	
	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)
	(1)	(2)	(3)	(4)	(5)	(6)
<i>IV Results</i>						
Homeownership IV	-0.0172 (0.0178)	0.00488 (0.0262)	0.00175 (0.00279)	0.00410 (0.00437)	-0.0501*** (0.0177)	-0.0633** (0.0290)
R-squared	0.033	0.028	0.431	0.429	0.009	0.007
<i>ITT Results</i>						
winnerdummy	-0.0116 (0.0112)	0.00217 (0.0165)	0.00126 (0.00176)	0.00238 (0.00256)	-0.0331*** (0.0111)	-0.0371** (0.0164)
R-squared	0.032	0.033	0.432	0.432	0.012	0.012
Observations	55,953	55,953	41,672	41,672	54,693	54,693
BANDWIDTH	114	114	85	85	96	96
CONTROLS	YES	YES	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES
CONTROL MEAN	.212	.212	.151	.151	.214	.214

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Applicant Moving to Another Municipality* is a dummy variable equal to 1 if the applicant is living in a different municipality from the one in which they were living when applying and 0 otherwise. *Municipal Poverty level* is the average poverty level of the municipality in which the applicant lives four years after applying. *Child after Applying* is a dummy variable equal to 1 if the household has 1 or more children born into the family after applying and 0 otherwise. Controls include gender, age, poverty level, elderly family member, and disabled family member. *Control Mean* is the mean of the corresponding outcome variable for applicants in the bandwidth who are below the cutoff score. Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

To summarize, the evidence presented in this section suggests that spatial mobility and the quality of the area of residency are not driving the reduction in employment amongst children of the family. We have also shown that voucher-recipient households tend to have at least one new child in a lower proportion.

## 1.8 A Model of Household Behavior

In this section, I rationalize the results found in this paper, by modeling the behavior of families that purchased a house and thus have ceased to incur in “rental costs”. To this end, I present a simple variation of the model presented in Basu and Van (1998)’s seminal work. Consider a household that has one adult (a) and one child (c); agents can work if they wish, and have the option of either working or not working, but have no say in the number of hours worked. Without loss of generality,

wages of children and adults are the same,  $w_a = w_c = w$ , and given by the market.<sup>38</sup> The potential household earning is thus  $W = 2w$ . The minimum desired level of consumption of a family that has a child in school is  $(1 - \alpha)S$ , with  $0.5 \geq \alpha \geq 0$ . Once the child has completed school and becomes an adult, the minimum household level of consumption rises to  $S$ . This implies that households with a child in school have a desired minimum level of consumption that is  $\alpha S$  units lower than the level once their child has left school.

In this setup, adults choose whether or not their children work. Also, adults benefit from their own non-working time,  $L_a$ , and their child's non-working time,  $L_c$ , and are altruistic in the sense that  $L_c \geq L_a$ . For simplicity, non-working time is discrete,  $L_i = \{0, 1\}$ , and total household non-working time is  $L = L_c + L_a$ . Household preferences are represented by the following Stone-Geary utility function:<sup>39</sup>

$$U(C, E) = \begin{cases} (C - S)(L) & \text{if } C \geq S \\ (C - S) & \text{if } C < S \end{cases} \quad (1.4)$$

subject to the budget constraint:

$$C = (1 - L)W + R \quad (1.5)$$

where  $C$  is the household consumption and  $R$  is some non-monetary *good provided*, such as a house. For simplicity, assume that *good provided* when the household has a student is zero,  $R_s = 0$ . The setup above implies that the household income before the child becomes an adult can satisfy the minimum level of consumption,  $0.5W \geq (1 - \alpha)S$ , and that wealthier families benefit more from leisure. The household maximizes  $U$  with respect to  $C$  and  $L$ . From the first order condition, we get the following results:

<sup>38</sup> I believe this to be a reasonable assumption for this low-skilled income group. Results do not change if  $w_a \geq w_c$ .

<sup>39</sup> Notice that this setup implies that:  $L_c = \{0, 0.5L\}$  and  $L_a = \{0, 0.5L\}$ . Using a more complex form of non-working time, where leisure for each agent is continuous rather than discrete, leads to an interior solution where  $L = \frac{W+R-S}{2W}$ . On the other hand, the amount of household non-working time taken by each agent depends on the functional form of the utility function and the marginal rate of substitution between parent and child non-working time. For instance, if we were to use for the first bracket of the utility the following function:  $(C - S)(\alpha L_a^\beta + (1 - \alpha)L_c^\beta)^{\frac{1}{\beta}}$ ; if  $C \geq S$ , it would lead to a marginal rate of substitution  $\frac{L_a}{L_c} = \frac{\alpha}{1-\alpha}$ .

$$L = \begin{cases} L_c = 0 \text{ and } L_a = 0 & \text{if } 0.5W + R \leq S \\ L_c = 1 \text{ and } L_a = 0 & \text{if } 0.5W \geq S \text{ and } R = 0 \\ L_c = 1 \text{ and } L_a = 0 & \text{if } S > 0.5W \geq (1 - \alpha)S \text{ and } R \geq \alpha S \geq 0 \\ L_c = 1 \text{ and } L_a = 1 & \text{if } R \geq S \end{cases} \quad (1.6)$$

The main takeaway from these results is the following: children will not work if parents make enough money to cover the minimum level of household consumption or if the monetary value of the *goods provided* is large enough to offset the increase in minimum household consumption when a child becomes an adult,  $L_c = 1$  and  $L_a = 0$ . Thus some families who, in the absence of the voucher, would push their children to work ( $R = 0$ ), see an increase in *goods provided* (obtaining a house and ceasing to pay rent) and do not encourage their children to work ( $R \geq \alpha S \geq 0$ ).

## 1.9 Conclusion

The various and at times contradictory theories concerning homeownership and its effect on employment levels mean that these concepts still face much debate. Whilst homeownership continues to be encouraged by governments in most OECD countries, we do not yet have a full understanding of its wider impacts on people's employment levels.

In this paper, we have exploited data from a Chilean government housing program that assigns vouchers for purchasing a home with an unpredicted cutoff point. Using a fuzzy regression discontinuity, we found that homeownership under this program decreases employment by between 3.95 and 5.60 p.p. We further observed that the effect comes mostly from individuals not entering the labor market, rather than exiting it, and from household members who are likely to be the children of the main applicant.

Regarding the mechanism at play, we observe that the reduced spatial mobility and quality of the area of residency are not the main drivers of the decrease in employment. This finding contradicts previous theoretical literature on this topic. We also find that these families tend to have fewer newborn children. Our results imply that new homeowner families are in less pressing need of extra sources of in-

come and future informal insurance; this in turn would put less pressure on current children of legal working age to join the labor market and to have further children themselves.

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## 1.A The Homeownership Assistance Act

Act 174 (Chilean National Congress, 2006), from the Ministry of Housing and Urban Planning (MINVU, from its Spanish name “*Ministerio de Vivienda y Urbanismo*”), regulates all homeownership assistance that the Chilean government provides to the poorest 40 percent of the population.<sup>40</sup> Chapter XV of this Act describes the specific rules regarding the program studied in this paper, “Adquisición de Vivienda Construida” (AVC).

This chapter establishes that people who wish to be considered in a given offer round must submit their supporting documents for the application to their regional MINVU office. The government then ensures that the basic applicant eligibility requirements are met: 1) having had savings of at least 400 USD in a bank account for a year; 2) belonging to the poorest 40 percent of the population; 3) having applied to receive the voucher in only one region; and 4) that no household member already owns a house. If the individual meets all requirements, they are provided with a certificate of application and they then become an applicant.

The homeownership score and all that follows is carried out for applicants and the same rules apply nationwide.

### 1.A.1 The Homeownership Score

In 2010, the score was calculated using the following factors:

1. *Family Poverty Level*: Corresponds to the difference between the underlying *household poverty score* that is used to determine the 40 percent poverty level in the population (maximum score for an application to be valid) and the applicant’s own poverty score, divided by 10. The poverty score assigned to the poorest person in the Chilean population is 2,042 points and the poverty score that determines the 40 percent threshold is 11,734 points.
2. *Family Group*: 10 points is added for each family member accredited in the *household poverty record*. If the applicant has no other accredited family members, he/she will receive a score of zero.
3. *Children in the Family*: The government calculates the share of family members accredited in the *household poverty record* who are 15 years of age or younger, determined by their age in the year of the offer round. The appli-

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<sup>40</sup> Act 174 was later replaced by Act 49 (Chilean National Congress, 2011).

cant receives 1 point for each percentage point of the share calculated in this category.

4. *Disabled Family Members:* The government calculates the share of family members accredited in the *household poverty record* who are disabled. The applicant receives 1 point for each percentage point of the share calculated in this category.
5. *Elderly Family Members:* The government calculates the share of family members accredited in the *household poverty record* who are 60 years of age or older, determined by their age in the year of the offer round. The applicant receives 1 point for each percentage point of the share calculated in this category.

In 2011, MINVU introduced several changes regarding the calculation of these factors. These changes were as follows:

1. *Family Group:* 40 points are given for each family member accredited in the *household poverty record*, excluding the main applicant. If the applicant has no other accredited family members, he/she will no obtain extra points.
2. *Children in the Family:* 30 points for each family member accredited in the *household poverty record* who are 5 years of age or younger, determined by their age in the year of the offer round.
3. *Young Family Members:* 20 points for each family member accredited in the *household poverty record* who is between 6 and 18 years of age, determined by their age in the year of the offer round.
4. *Politically Persecuted* 25 points if any family member accredited in the *household poverty record* is recognized as having been politically persecuted by Pinochet's military regime.
5. *Maximum Score:* The sum of all of the aforementioned scores cannot be higher than 400 points.

In both of the years studied, the score was calculated using the sum of the factors listed above for each offer round. The poverty level score represents around 60 percent of the total score for an average family around the cutoff.

### 1.A.2 The Homeownership Voucher Selection Process

The homeownership voucher selection process is determined using the following steps:

1. Vouchers are assigned to applicant families strictly following the order given by the application score, from the applicant with the highest score to the applicant with the lowest score. In a given offer round, the regional office will provide vouchers until the budget for this round is exhausted in the region.<sup>41</sup>
2. If two or more applicants have the same score in a particular offer round and region, and availability of funds prohibits all from receiving a voucher, the applicant who has the highest score in the poverty level factor will have preference. If the tie still remains, a lottery will be conducted to determine the recipient.
3. Once funds are exhausted, applicants who did not receive the voucher will be removed from MINVU records. They will be able to apply in a future offer round, but will be required to submit an entire new application.

### 1.A.3 Use of the Homeownership Voucher

Once the selection procedure is finished and voucher recipients have been informed, the regional office has 15 days to issue a certificate accrediting their voucher receipt. This certificate will have:

1. The name and the national ID number of the voucher recipient.
2. Date of receipt and expiration date for the voucher.
3. Region of application and value of the voucher.

The voucher must be used in the 21 months following the applicant's notification of the offer round results.<sup>42</sup>

Once the voucher recipient has found a house, the regional MINVU office must "accompany" them in the buying process. Here, government officials have the following duties:

1. Estimating the commercial price of the property.<sup>43</sup> The price of the house set by the seller cannot be 20 percent higher than the price given by the MINVU. If this is the case, the transaction cannot be continued.

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<sup>41</sup> The regional budget is set the preceding year in national budget law.

<sup>42</sup> There is only one exception to this: the MINVU regional office has the power to extend this period only if at the 21st month the voucher recipient has a pre-signed contract with a particular seller and for a particular property. In this case, the voucher recipient can purchase the house referred to in the pre-signed contract or lose the voucher.

<sup>43</sup> This pricing process is determined by Resolution N 347 from the MINVU.

2. Confirming that the house to be purchased satisfied basic habitability requirements (e.g. having a solid concrete floor or potable water supply).
3. Providing advice to the voucher recipient for the legal paperwork required in the buying process.

Once this has been completed, the voucher will cover the difference between the market price of the house and the amount of savings accredited by the family at the time of application, up to a maximum of 25,000 USD. The process here is as follows: the voucher recipient, the seller and the government sign the house purchase contract. After this, the house is transferred, the voucher recipient pays the amount of savings reported in the application, and the government pays the remainder to the seller.

#### 1.A.4 Restrictions

The Act states in its restrictions that this voucher cannot be used to buy a house from relatives up to the second degree - meaning grandparents, grandchildren, aunts, uncles, nephews, nieces or half-siblings.

Furthermore, AVC voucher recipients are not permitted to take out a housing mortgage, nor can they sell or rent the property in the 5 years following the announcement of offer round results. Finally, families are not permitted to use the home as a collateral for credit for a 5 year period.

## 1.B The Poverty Index

The poverty index is the main criteria used by the government to deliver social benefits, and aims to capture the “risk of a family of becoming poor”. Although families may have a grasp of the broad variables used to compute the index, the exact equation is not public knowledge. The Ministry of Planning provides the following sketch of the poverty index equation:

$$2*\text{Poverty Index} = \frac{\text{Ability to Generate Income} + \text{Declared Income} + \text{Permanent Income}}{\text{Household Needs}}$$

where each of the terms in this equation are calculated for all family members in the household. The government provides the following description of the variables:

- *Ability to Generate Income*: Potential labor wage that each household member could receive in the labor market. Certain household members have a potential wage of 0 such as those of retirement age, students under 25 years of age, pregnant women, or disabled family members. Two facts are worth noting: 1) the equation used to input potential labor market wages is not known; and 2) factors that could ultimately increase real wages in the labor market (e.g. further years of education) will increase these potential wages for household members.
- *Declared Income*: Labor income as declared by the family. This particular term is not checked against any official record.

These two measures are combined for all household members and used to create one single factor where *Ability to Generate Income* receives 90 percent of the weighting and *Declared Income* receives 10 percent. The labor income thus does play a part in determining this index, but it is far from being its main component.

- *Permanent Income*: Income received from a pension and any other regular transfers. This is self-reported, but also checked against governmental agencies.
- *Household Financial Needs*: Financial needs associated with disabled family members or terminal illnesses.

These two measures are also combined for all household members. The weighting of each factor in the equation is not provided by the government.

The system is automatized and updated using administrative information. For example, if a household member turns 65 years old or passes away, the score is recalculated to reflect this new information. After all information has been collected, the government computes a score for each family, where a higher score effectively means “richer” and a lower score means “poorer”. The lowest poverty score is 2,042 points, and the two most common thresholds for obtaining social benefits are 8,500 points (poorest 20 percent) and 11,734 points (poorest 40 percent). In the process of applying for a social benefit, families use their score and generally are aware of the maximum scores for applying.



## 1.C Tables

**Table C1: Homeownership Voucher Utilization**

	Voucher Value (USD)					
	Employment		ITT		IV	
	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)
	(1)	(2)	(3)	(4)	(5)	(6)
Homeownership	0.638*** (0.0115)	0.593*** (0.0169)	12,791*** (118.1)	12,315*** (179.8)	20,089*** (231.9)	20,465*** (352.3)
R-squared	0.520	0.520	0.783	0.783	0.736	0.732
Observations	47,307	47,307	50,077	50,077	50,077	50,077
BANDWIDTH	83	83	88	88	88	88
CONTROLS	NO	NO	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES
REGION FE	YES	YES	YES	YES	YES	YES

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Employment* is a dummy variable equal to 1 if the applicant has ever bought a house using a homeownership voucher since 2010 and 0 otherwise. *Voucher Value* is the value of the voucher executed by voucher recipients. Controls include gender, age, poverty level, elderly family member, and disabled family member. Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table C2: Homeownership Voucher Utilization by Dates After Applying**

VARIABLES	6 Months after Applying		12 Months after Applying		18 Months after Applying		24 Months after Applying	
	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homeownership	0.113*** (0.00816)	0.115*** (0.0116)	0.306*** (0.0104)	0.292*** (0.0148)	0.476*** (0.0108)	0.438*** (0.0155)	0.547*** (0.0113)	0.504*** (0.0164)
Observations	41,290	41,290	50,647	50,647	58,872	58,872	52,973	52,973
R-squared	0.123	0.123	0.257	0.257	0.386	0.386	0.443	0.444
BANDWIDTH	72	72	89	89	104	104	93	93
CONTROLS	YES	YES	YES	YES	YES	YES	YES	YES
REGION FE	YES	YES	YES	YES	YES	YES	YES	YES
CONTROL MEAN	0	0	.001	.001	.005	.005	.009	.009

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *6 Months after Applying* is a dummy variable equal to 1 if the applicant used a voucher to buy a house 6 months after applying and 0 otherwise. *12 Months after Applying* is a dummy variable equal to 1 if the applicant used a voucher to buy a house 12 months after applying and 0 otherwise. *18 Months after Applying* is a dummy variable equal to 1 if the applicant used a voucher to buy a house 18 months after applying and 0 otherwise. *24 Months after Applying* is a dummy variable equal to 1 if the applicant used a voucher to buy a house 24 months after applying and 0 otherwise. Controls include gender, age, poverty level, elderly family member, and disabled family member. Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table C3: Homeownership Effect on House Characteristics**

VARIABLES	Living with a Hosting Family		Rudimentary House		Number of Rooms used		Apartment	
	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>IV Results</i>								
Homeownership IV	-0.460*** (0.0166)	-0.493*** (0.0265)	-0.0803*** (0.00947)	-0.0677*** (0.0161)	0.146*** (0.0451)	0.143** (0.0702)	-0.00991 (0.0184)	0.00344 (0.0302)
R-squared	0.205	0.202	0.045	0.036	0.038	0.039	0.045	0.046
F-Stat	3277	1073	3512	1166	2850	905	2249	710
<i>ITT Results</i>								
Homeownership ITT	-0.305*** (0.0122)	-0.310*** (0.0181)	-0.0533*** (0.00641)	-0.0436*** (0.00960)	0.0967*** (0.0285)	0.0874** (0.0414)	-0.00580 (0.0114)	0.00129 (0.0170)
R-squared	0.123	0.124	0.036	0.036	0.038	0.039	0.045	0.045
Observations	44,652	44,652	48,315	48,315	42,959	42,959	34,273	34,273
BANDWIDTH	88	88	96	96	88	88	68	68
CONTROLS	YES	YES	YES	YES	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES
CONTROL MEAN	.444	.444	.084	.084	1.7	1.7	.127	.127

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Living with a Hosting Family* is a dummy variable equal to 1 if the applicant lives in another family's house and 0 otherwise. *Rudimentary House* is a dummy variable equal to 1 if the applicant live in a house without a concrete floor or potable water and 0 otherwise. *Number of Rooms Used* is the number of rooms that the family use as bedrooms in the house. *Apartment* is a dummy variable equal to 1 if the family lives in an apartment and 0 otherwise. Controls include gender, age, poverty level, elderly family member, and disabled family member. Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table C4: Effect of Homeownership on Employment by Gender for Applicants 30 Years of Age or Younger**

VARIABLES	Men		Women	
	Employment (Linear)	Employment (Quad.)	Employment (Linear)	Employment (Quad.)
	(1)	(2)	(3)	(4)
<i>IV Results</i>				
Homeownership IV	-0.0333 (0.0254)	-0.0546 (0.0451)	-0.0725*** (0.0190)	-0.0628** (0.0303)
R-squared	0.242	0.239	0.123	0.123
<i>ITT Results</i>				
Homeownership ITT	-0.0187 (0.0152)	-0.0291 (0.0225)	-0.0461*** (0.0122)	-0.0399** (0.0180)
R-squared	0.243	0.243	0.124	0.124
Observations	11,730	11,730	17,299	17,299
BANDWIDTH	103	103	98	98
CONTROLS	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES
CONTROL MEAN	.298	.298	.258	.258

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Employment* is a dummy variable equal to 1 if the applicant is working four years after applying and 0 otherwise. *Men* is the subgroup of applicants who are men. *Women* is the subgroup of applicants who are women. Controls include gender, age, poverty level, working status before applying, wage before applying, elderly family member, and disabled family member. *Control Mean* is the mean of the corresponding outcome variable for applicants in the bandwidth who are below the cutoff score. Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

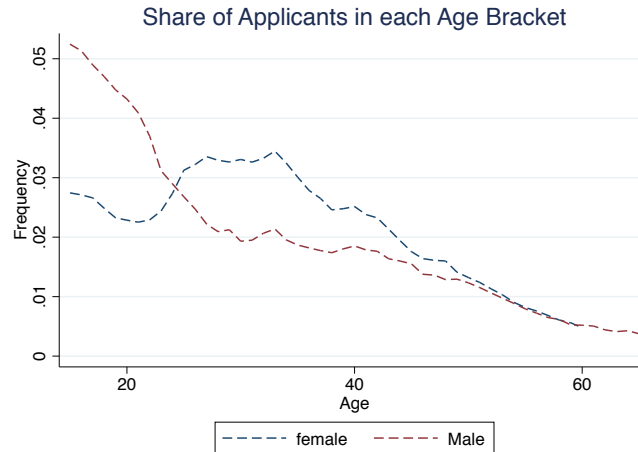
**Table C5: The Effect of the Homeownership Voucher on Employment and Wages - Standard Errors Clustered at the Municipal Level**

VARIABLES	Employment (Linear) (1)	Employment (Quad.) (2)	Wage USD (Linear) (3)	Wage USD (Quad.) (4)
<i>IV Results</i>				
Homeownership IV	-0.0560*** (0.0126)	-0.0395** (0.0202)	-42.96*** (13.09)	-14.06 (20.06)
R-squared	0.161	0.161	0.213	0.212
<i>ITT Results</i>				
Homeownership ITT	-0.0359*** (0.00799)	-0.0244** (0.0116)	-27.24*** (8.24)	-9.009 (12.22)
R-squared	0.164	0.164	0.215	0.215
Observations	55,916	55,916	48,292	48,292
BANDWIDTH	98	98	85	85
CONTROLS	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES
CONTROL MEAN	.325	.325	251.56	251.56

*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Employment* is a dummy variable equal to 1 if the applicant is working four years after applying and 0 otherwise. *Wage USD* is the applicant's observed wage four years after applying. Controls include gender, age, poverty level, elderly family member, disabled family member, employment status before applying and wage before applying. *Control Mean* is the mean of the corresponding outcome variable for applicants in the bandwidth who are below the cutoff score. Standard errors are clustered at the Municipal level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

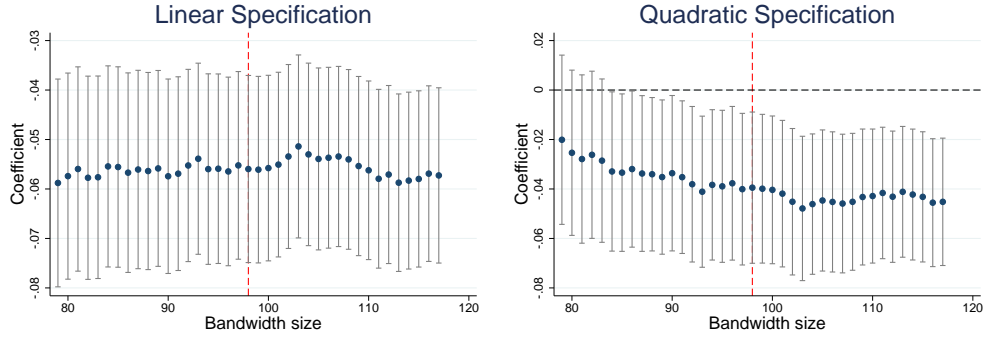
## 1.D Graphs

Figure D1: Age Distribution of Applicants by Gender



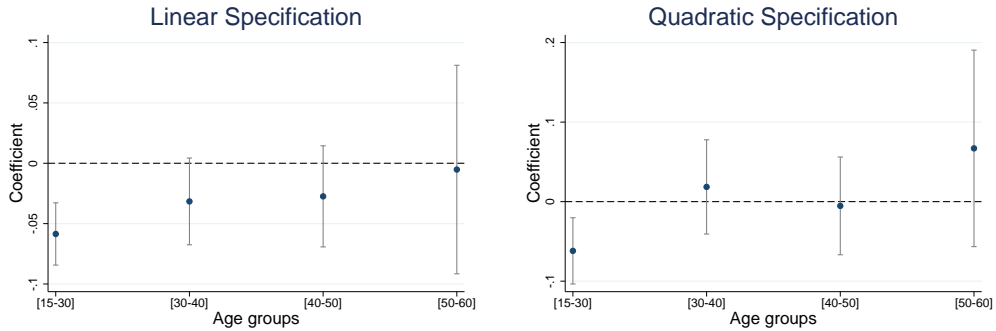
*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. *Men* is the subgroup of applicants who are men. *Women* is the subgroup of applicants who are Women. *Frequency* is the share of people of a particular gender of a given age.

**Figure D2: Effect of Homeownership at Different Bandwidths**



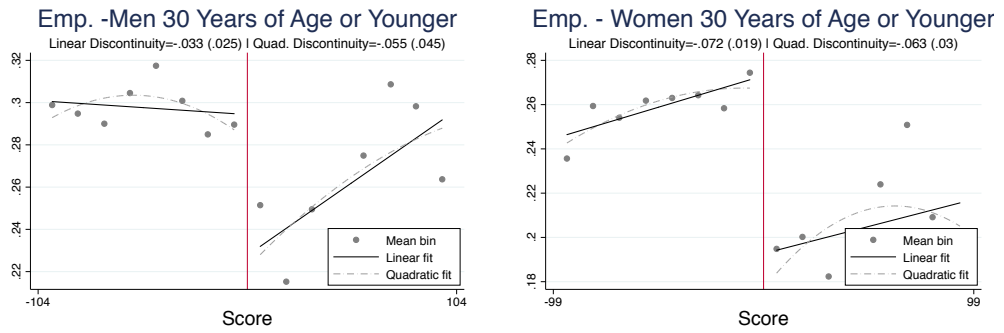
*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. Each dot represents the point estimate of a regression discontinuity using the sample in the bandwidth. The confidence intervals are set at 95% confidence. Controls include gender, age, poverty level, working status before applying, wage before applying, elderly family member, and disabled family member. Standard errors are clustered at the family level. Point estimates and standard errors are from the IV regression (Equation 1.3).

**Figure D3: Effect of Homeownership Voucher on Employment by Age Group**



*Notes:* Data is for applicants of legal working age, four years after applying to 2010 and 2011 offer rounds. We use as an outcome variable a dummy of 1 if the applicant is employed and 0 otherwise. Each dot represents the point estimate of the local linear regression (as in Equation 3) in a bandwidth around the cutoff for applicants who were in each age group four years after applying. The confidence intervals are calculated at a 95% confidence level. Controls include gender, age, poverty level, working status before applying, wage before applying, elderly family member, and disabled family member. Standard errors are clustered at the family level. Point estimates and standard errors are from the IV regression (Equation 1.3).

**Figure D4: Effect of Homeownership on Employment by Gender for Applicants 30 Years of Age or Younger**



*Notes:* Data is for applicants of legal working age who are 30 years of age or younger, four years after applying to 2010 and 2011 offer rounds. *Employment* is a dummy variable equal to 1 if the applicant is working four years after applying and 0 otherwise. *Men* is the subgroup of applicants who are men. *Women* is the subgroup of applicants who are women. Each dot represents the mean of *Employment* for the applicants in a particular bin. The vertical line in each panel represents the cutoff score for receiving a voucher offer. Point estimates and standard errors are from the IV regression (Equation 1.3).

Figure D5: Greater Santiago Metropolitan Area





## Chapter 2

# Homeownership and Human Capital: Evidence from Chile.

## 2.1 Introduction

In 2014 alone, the US government assisted American families with 50 billion USD worth of homeownership plans for low-income families, of which 18 billion were provided as portable vouchers that recipients used to purchase a house in the private market.<sup>1</sup> The government of Chile contributed approximately 10 billion USD towards the same ends, with 1.2 billion being used as portable vouchers.<sup>2</sup> These governmental efforts to promote homeownership rely on the strong belief that owning a house has a positive impact on families. One of the most commonly cited of these impacts is an improvement in the educational development of children (Department of Housing and Urban Development, 2000).<sup>3</sup>

There has however been a lack of settings in which homeownership has been as good as randomly assigned, which has prevented economic scholars from establishing the causal impact of owning a house on student learning. Panel studies have often found that homeownership has large positive effects on academic results (Boehm and Schlottmann, 1999; Green and White, 1997; Mohanty and Raut, 2009; Harkness and Newman, 2002; Haurin et al., 2002). The biggest concern in these studies is the endogeneity of homeownership and its likely correlation with other mediating factors. Some studies have attempted to remove omitted variables such as health or wealth, which are presumably positively correlated with homeownership, in turn finding much smaller effects (Aaronson, 2000; Ma'rof and Redzuan, 2012). The only study of which I am aware that finds no effects of homeownership on students' academic outcomes is Barker and Miller (2009), and I am not aware of any study that has established negative effects.

In this study, I exploit a homeownership program in which families apply to receive a voucher to assist them with the purchase of a house.<sup>4</sup> To select the voucher recipients, the Chilean government ranks applicant families and assigns as many vouchers as the budget permits. This method creates an arbitrary cut-off point. Among households just above the cut-off point, 63% redeem their voucher and buy a house. I use a fuzzy regression discontinuity design, using data drawn from this voucher system, to study the effects of homeownership on the learning outcomes for school-age children (henceforth referred to as *students*) who come from families that applied for the voucher.<sup>5</sup> These effects are measured three years after parents

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<sup>1</sup> <https://www.cbo.gov/publication/50782>

<sup>2</sup> [http://www.dipres.gob.cl/572/articles-72441\\_doc\\_pdf.pdf](http://www.dipres.gob.cl/572/articles-72441_doc_pdf.pdf)

<sup>3</sup> <http://archives.hud.gov/news/2000/pr00-132.html>

<sup>4</sup> This is the same program used in Navarrete and Navarrete (2016) to explore the effect of homeownership on employment.

<sup>5</sup> *School age* in Chile goes from the ages of 6 to 17.

applied for the voucher.

I first analyze the effect of homeownership on student learning across the whole sample, and by gender. Then, I analyze students in elementary school (1st to 4th grades), where students have only one teacher. Finally, I analyze students in middle school and high school (5th to 12th grades), where students have several teachers according to the subject.

Across the whole sample and by gender, I do not find significant effects of homeownership on students' test scores. However, in the sample of students in elementary school, I find a significantly negative effect on students' reading scores of by between 0.16 and 0.18 standard deviations ( $\sigma$ ). In a further analysis, I show that the significant results do not hold in mathematics most likely because 2nd graders - the most strongly affected group of elementary students - do not take the mathematics test.

When exploring potential mechanisms, I find that homeownership decreases the probability of living with a hosting family by between 12 and 20 percentage points (p.p.) for students in elementary school. This effect arises most likely from families that were previously living in another family's house and have made the transition to their own house. The drop in the likelihood of living with a hosting family might lead to a reduction in students' academic achievements due to the loss of learning support provided by members of the hosting family, who are often close relatives (CASEN, 2013).<sup>6,7</sup>

I also observe that homeownership increases the probability of living in a non-rudimentary house - meaning a house with a concrete floor, an indoor bathroom, and access to potable water. However, these improvements do not seem to improve students' test scores.

Results found in this paper are consistent with recent literature studying similar programs, such as Moving to Opportunities, which show that the age of exposure is an important driver of the effects (Chetty et al., 2016).<sup>8</sup> Chetty and his coauthors' results also suggest that the lack of effects across the entire sample seem to be the result of no effects on older students and strong effects on younger students. A similarly pattern is observed in this paper.

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<sup>6</sup> The probability of living with a hosting family does also decrease for students in middle and high school, but do not seem to impact test scores.

<sup>7</sup> The importance of learning support at younger ages and from outside the immediate family has also been documented by Newport (1990); Charles and Piazza (2009).

<sup>8</sup> The Moving to Opportunities program randomly assigned vouchers to families to assist them with rental costs. In the experimental section of the program, families were required to use the voucher in a neighborhood with a lower measurement of poverty in census data. In the non-experimental section of the program, no restrictions were imposed on families.

This paper contributes to the existing literature in several ways. This is the first paper to use a quasi-experimental design to measure the impact of homeownership on student learning. Further to this, I present evidence suggesting that transitions in housing status from living with a hosting family to owning a house have a negative impact on students' academic outcomes, a result that seems to arise from the loss of learning support provided by close family members. Third, my results may point to a broader conclusion that the loss of additional non-parental care tends to have an inherently negative impact on young students' learning.

The paper is organized as follows. In the next section I discuss the behavioral mechanisms through which this program might affect student's outcomes. Section 2.3 presents the voucher-based housing program and the educational context in Chile. Section 2.4 then provides an overview of the data used in this paper. Section 2.5 presents the empirical strategy of the paper. In Section 2.6, I discuss the regression discontinuity validity in further depth. Section 2.7 presents the results, Section 2.8 presents the potential mechanisms, and Section 2.9 concludes.

## 2.2 Previous Evidence

Several scholars have debated the potential effects of homeownership incentives on educational outcomes. The program under analysis in this paper provides an opportunity to study some aspects of these effects. First, this program may carry a wealth-income effect. The value of a voucher represents several years' worth of wages for recipient families (as established in the previous chapter, and discussed further later in this chapter), and these new homeowner families no longer have the need to pay rent (see Sections 1.2). This might encourage parents to invest more heavily in their children's education, for example through opting for more expensive schools or enrolling them in supplementary classes, which could boost students' learning levels. Second, the housing tenure status alone may have an effect. If voucher recipients are more likely to become homeowners, their perception of themselves (e.g. self-esteem) could potentially improve through this "achievement". An increase in self-esteem could then be passed on to students, leading to improvements in their educational outcomes. Third, given the design of the program under study, the conditions of the houses in which these new homeowners now reside may improve, which in turn could increase health outcomes. Improved health conditions can foster learning through several channels, such as increasing school attendance. Fourth, the quality of the area in which students reside might improve (or worsen), which could have an effect on the local networks and services to which these students have access. By being

influenced by more highly achieving peers and improved services, educational outcomes could increase. Fifth, at the moment of application some families lived with a hosting family (e.g. grandparents' house). By design, this program moves these families into independent living, which might reduce the total level of care received by students, given that there are fewer adults at home, and therefore may decrease their learning outcomes.

In theory, an increase in income might motivate families to invest more in their children's education.<sup>9</sup> Dahl and Lochner (2012) exploits non-linear changes in income from the Earned Income Tax Credit, and finds that a 1,000 USD increase in income raises mathematics test scores by 2.1 percent and reading test scores by 3.6 percent of a standard deviation. Currie and Yelowitz (2000) attempt to isolate the effects of income on educational outcomes by exploiting a housing assistance program that provides rental subsidies to families. They also found a complementary relationship between income and educational outcomes.

Theoretically, obtaining a house could increase the levels of confidence and self-esteem of homeowners, as they perceive themselves as being more able to "achieve" outcomes from within the system (Dietz and Haurin, 2003). These perceptions could in turn be passed on to household members, increasing outcomes for other family members, including educational outcomes for students. Empirical correlational studies have found some evidence supporting this claim by showing that homeowners have higher levels of self-esteem, confidence, life satisfaction, and happiness, when compared with households in other living arrangements (Balfour and Smith, 1996; Rakoff, 1977; Rossi and Weber, 1996; Rohe and Stegman, 1994). It thus seems plausible that these changes in housing tenure could have an effect on parental self-perception, and so the effect of this on student outcomes is worth exploring in further depth.

The perception of local networks, from homeowner families, might also affect students' outcomes. First, homeowner families might be more willing to establish strong relationships with their neighbors, given that they have committed for a long period of time to their local area. DiPasquale and Glaeser (1999) propose this theory, and in the empirical section of their paper, their hypothesis is supported by the data. Building on this, Green and White (1997) argue that, given that homeowner families invest more seriously in social capital, they place value on their children acting appropriately within these networks, which in turn might motivate

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<sup>9</sup> There are other ways in which income could be related to educational level. For example, parents from low economic backgrounds have shown low levels of mathematics and literacy skills. I do not believe that these other ways in which permanent low levels of income affect educational outcomes are at play in this context.

improved parental practices and increased time spent with children. Although their paper does not have the data to fully address this concept, their suggestive analysis seems to support their hypothesis.

By design, this program mechanically moves all new homeowner families into houses that are above the minimum conditions of living standards, which could yield increases in health factors.<sup>10</sup> The effect of housing conditions on health has been largely addressed in empirical literature (Krieger and Higgins, 2002; Martin et al., 1987; Haurin et al., 2002). Authors have pointed out that improved housing conditions decrease the probability that children will develop respiratory infections, asthma, mental health problems, and acute sicknesses that keep them housebound. Later studies have established that the prevalence of the aforementioned illnesses has a detrimental impact on educational outcomes (Coffman et al., 2008; Cabana et al., 2014).

It has long been argued that residing in a particular neighborhood can have effects on family and student outcomes. This has been explained using residential sorting models - wealthy (poor) citizens tend to live in wealthy (poor) areas - which suggest that those who do not take into account the effects of their local peers and services might have worse outcomes.<sup>11</sup> This has inspired a growing body of literature, of which the best known experiment is Moving to Opportunities (MTO), where families received a voucher that allowed them to rent a house in a wealthier neighborhood than their current area (Kling et al., 2007). Authors in this study found indistinguishable effects on student learning, which was essentially driven by a zero sum effect between: 1) a positive effect on young women and 2) a negative effect on young men. In a later study, Chetty et al. (2016) re-analyzed MTO using the age of exposure as the relevant factor for the treatment effect. They show that those who were exposed at a young age (less than 13 years old) were positively affected, while those who were exposed at older ages (from 13 to 18 years old) were negatively affected. Another recognized study is that of Oreopoulos (2003). He used as a source of exogenous variation the allocation of public projects into different types of neighborhoods. He also finds little evidence of the effect of the neighborhoods on student learning.

Close relatives, such as grandparents, can complement the support provided by parents to students (Baydar and Brooks-Gunn, 1991; Kontos et al., 1995). New

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<sup>10</sup> Recall that the MINVU verifies that the houses bought using the voucher have potable water, a concrete floor and an indoor bathroom.

<sup>11</sup> Another mechanism through which a neighborhood might affect students is that neighborhoods with a higher share of homeowners could potentially have characteristics that are conducive to creating better students. I do not believe that this program dramatically changes the share of homeowners in different areas, so this mechanism does not seem plausible.

homeowner families under this program who were previously living in the house of a close relative mechanically alter their living arrangement and start living independently. This could decrease the complementary care that the hosting family provides to children, which in turn could have an effect on students' learning outcomes. A large body of studies has documented the importance of supplementary care on learning outcomes (Currie and Almond, 2011; Aassve et al., 2012; Arpino et al., 2010).

The discussion presented suggests that: 1) there is a relationship between income and student learning; 2) the relationship between homeownership status and self-perception seems plausible, and its connection to students' outcomes seems to be worth exploring; 3) the effects of housing conditions on health status, and the consequential effects of this on education, seem well documented; 4) the effect of neighborhood quality on students seems to be ambiguous; and 5) the lost of supplementary care might be an important mechanism at play in this context.

## 2.3 The Homeownership Program and the Chilean Educational System

This is the same program as described in 1.2. Some things that the reader should keep in mind are:

- Families must submit a new application for a homeownership voucher each time the government launches a new offer round. In the application process, families must select a region of application.
- The number of applicants far exceeds the number of vouchers offered.
- Applicants are ranked based on a score that is determined using their level of poverty and several household characteristics.
- The government delivers as many voucher as the budget permits in each region, strictly following the ranking.
- I use data from the 2010 and 2011 AVC offer rounds.

### 2.3.1 The Chilean Educational System

Over the course of an educational life, students pass through three different stages of instruction, each with its own common teaching techniques, grading standards, and often confined within its own physical building. The first stage lasts

from 1st to 4th grades, and is characterized by students having a single teacher for all subjects (henceforth referred to as *elementary school*). The second stage is from 5th to 8th grades (henceforth referred to as *middle school*) and the third is from 9th through 12th grades (henceforth referred to as *high school*). In both middle and high school, students have a different teacher for each subject. The most significant difference between the latter two is that only grades in high school count towards university entrance rankings.

## 2.4 Data

### 2.4.1 Heads of Families and Family Group Datasets

This chapter uses as its main dataset the one described in Section 1.3.1. Some things that the reader should keep in mind are:

- This dataset contains all heads of households, and their household members, who applied to the voucher offer rounds in 2010 and 2011.
- Cutoff scores from every offer round and region are centered to zero. All offer rounds are then pooled.
- Standard errors are clustered at the family level throughout the analysis, as outcomes are likely to be correlated at this level.

### 2.4.2 Student Learning

To explore students' academic outcomes, I use results from the Chilean national education test. Given that 2014 is the most recent year for which I have access to test scores, the latest point in time at which I can measure students' academic achievement is three years after their families applied to the housing voucher. This means that I use 2013 test score results for students in the 2010 voucher offer round, and 2014 results for students in the 2011 round.

Since 2013, this test has taken place in 2nd, 4th, 6th, 8th, and 10th grades. All of these grades take the reading test, and all but 2nd-graders take the mathematics test.<sup>12</sup> In the final dataset, I use reading and mathematics test score results, measured in standard deviations, for the aforementioned grades. I merge this student learning dataset with the previous one using the student ID and year. I

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<sup>12</sup>There are tests in other subjects, but these tests do not take place in every grade and in every year.



successfully merge 39,443 students using test result data taken three years after a given family applied for the housing voucher.

The way in which this dataset is constructed keeps the time span of application constant, and ensures that all students have had at least one year of exposure to the new house when test scores are measured.<sup>13</sup> Moreover, it avoids sample composition bias by keeping constant the compositions of grades taking the test.<sup>14</sup>

### 2.4.3 Mobility, Questionnaires, and House Conditions

I construct this dataset gathering information from several sources. The following data are measured three years after families submitted their application for the housing voucher.

First, I use the student enrolment dataset to identify the school attended by each student, their town of residency, and their gender and date of birth. I then merge this dataset with the previous sources using the student ID and year.

Using the test score and the enrolment datasets, I go on to create a dataset measuring the ‘quality’ of each school, by first taking the test results of all school-age children in Chile, then removing the voucher applicants from this group, and finally computing the average test score results of the school. I link this dataset with the ones mentioned earlier using the school ID and year.

I use responses taken from a student questionnaire, which is completed on the day that students take the test. Among other things, this questionnaire intends to measure students’ self-esteem, using questions such as “How strongly do you agree with the following statement: I am smart?”. It also intends to measure parental engagement in student learning using questions such as “How well do you think that your parents know your grades?”.<sup>15</sup> I use a questionnaire for parents to measure parental expectation towards their children, using the question “What is the highest educational level that you think your child will reach?”.<sup>16</sup> I then standardize each measure by grade and year, leaving the mean at zero and the standard deviation at 1. I merge this source with previous datasets using student ID and the year in which a given student took the test.<sup>17</sup>

The Ministry of Housing also provided variables that allow me to establish the number of rooms in the house of residence, the number of children born since the

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<sup>13</sup> As explained in Section 1.2, families are given two years to use their voucher.

<sup>14</sup> A complete detail of grades taking the test over the years is in Table B1 in the Appendix.

<sup>15</sup> Second-grade students do not respond to the questionnaire.

<sup>16</sup> A complete set of questions used in this paper can be found in Appendix 2.A.

<sup>17</sup> Unsurprisingly, the questionnaire is voluntary; I thus will address potential differential non-responses in the questionnaire in Section 2.6.3.

offer, whether the applicant lives with a hosting family, and whether the family lives in a non-rudimentary house - defined as a house with a concrete floor, an indoor bathroom, and access to potable water - three years after applying for the voucher. By using the head of household ID, I successfully merge all but 4 students from this dataset with previous datasets.

#### 2.4.4 Descriptive Statistics

**Table 2.1: Descriptive Statistics**

	Sample (1)	Non-Recipients (2)	Recipients (3)	Voucher users (4)	Voucher non-users (5)
Age	11.378 (2.894)	11.378 (2.904)	11.372 (2.826)	11.361 (2.829)	11.378 (2.825)
Female	0.500 (0.500)	0.497 (0.500)	0.520 (0.500)	0.519 (0.500)	0.522 (0.500)
Disabled Family Member	0.017 (0.131)	0.015 (0.124)	0.000 (0.170)	0.033 (0.188)	0.025 (0.158)
Elderly Family Member	0.05 (0.037)	0.05 (0.037)	0.04 (0.034)	0.04 (0.038)	0.03 (0.031)
Vulnerability	9.193 (5.489)	9.625 (5.426)	6.266 (4.995)	6.778 (4.818)	5.967 (5.073)
Metropolitan Area	0.314 (0.464)	0.315 (0.465)	0.304 (0.460)	0.290 (0.454)	0.312 (0.463)
2010 Offer Round	0.433 (0.496)	0.436 (0.496)	0.414 (0.493)	0.358 (0.479)	0.447 (0.497)
Voucher Value (USD)				16197 (2959)	
Observations	39443	34367	5076	1874	3202

*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. *Age* is measured in years. *Female* is the share of students who are female. *Disabled Family Member* is the share of students in families that have a family member with a handicap. *Elderly Family Member* is the share of students in families whose have an elderly family member. *Vulnerability* is the average percentile of vulnerability for families containing a student, with respect to the Chilean population. *Metropolitan Area* is the share of students that come from a family applying in the Santiago Metropolitan Region. *2010 Offer Round* is the share of students in the sample that come from a family that applied in the 2010 offer round. *Voucher Value* is the average face value of the executed voucher in US dollars.

Table 2.1 presents the descriptive statistics for students in the sample at the moment of application. This Table shows that the mean age for these students is between 11 and 12 years old. In this sample there is no imbalance between

genders; however, families that received vouchers contain a slightly higher proportion of female students than non-recipients families. This is surprising, given that the students' gender does not factor into the poverty index or the score used to determine voucher recipients. Furthermore, as with the previous chapter, students living in families with a disabled or an elderly family member constitute a small percentage of every group.

The fifth variable of Table 2.1 shows the average percentile in the poverty distribution, relative to the Chilean population, for applicant families with a student in each group. Applicant families containing a student are on average in the poorest 9th decile of families in the population. Students coming from families that received a voucher are on average in the poorest 6th decile of families in the Chilean population. These numbers contrasts with those presented in the previous chapter by showing that families containing a student are poorer than those with an applicant of legal working age. This is unsurprising given that the Chilean government constructs the poverty index using the expected salary of all household members, and family members who are students under 25 years of age are assigned an expected salary of zero. Thus, households with students will have a lower average expected income (for a full description of how the poverty index is calculated, see Appendix 1.B).

The average value of a voucher given across this sample is 16,197 USD, which is similar to the number presented in the previous section. As with the previous chapter, the value of a voucher represents around 12 years' worth of wages for the average voucher recipient family at this poverty level (CASEN, 2013).

## 2.5 Program Take-Up and Empirical Strategy

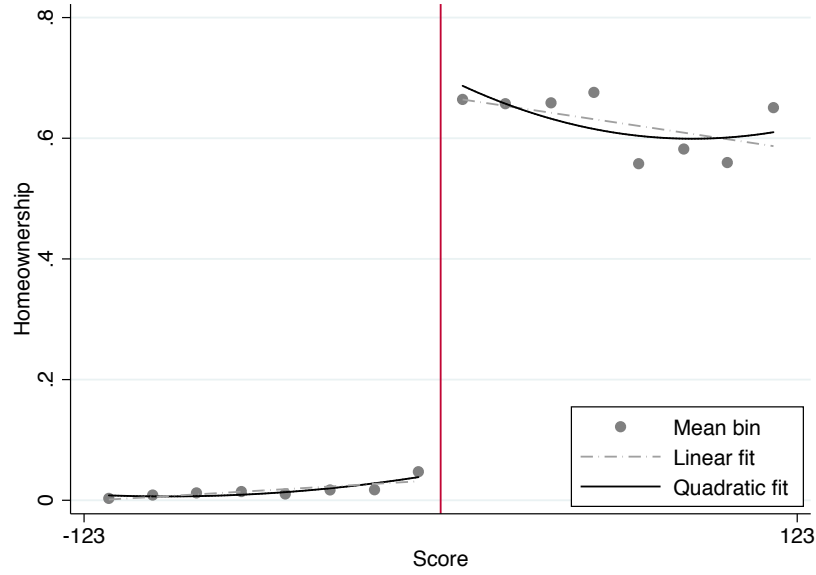
### 2.5.1 Program Take-up

Using the particular subgroup of applicant families containing a student, I will address in this section whether voucher-recipient families, in this particular sample, are more likely to buy a house using the voucher than those who did not receive an offer. To do this, I estimate the following equation:

$$\begin{aligned} \text{Homeownership}_{i,g,r,c,t} = & \alpha_0 + \gamma_{r,c} + \varsigma_g + \alpha_1 f(\text{Score}_{i,r,g,c}) + \beta D_{i,g,r,c} \\ & + \alpha_2 D_{i,g,r,c} * f(\text{Score}_{i,g,r,c}) + \boldsymbol{\varphi} X + e_{i,g,r,c,t} \end{aligned} \quad (2.1)$$

where  $\varsigma_g$  is a fixed effect for school grade, and  $X$  is a vector of controls

**Figure 2.1: First Stage**



*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one person. Each dot represents the share of students in a particular bin that come from a family that purchased a house using the voucher. Benchmark bandwidth estimated using the procedure proposed by Calonico et al. (2014).

containing family characteristics including the poverty level, whether the family has a disabled family member, whether the family has an elderly family member, gender, and the age of student  $i$ , in grade  $g$ , in region of application  $r$ , and offer round  $c$ . The remaining terms are described as in Section 1.4.1. As was the case in previous chapter: 1) this study examines the robustness of different functional forms of  $\text{Score}_{i,g,r,c}$ , using  $f(\cdot)$ , which is estimated separately on either side of the cut-off; and 2) the optimal bandwidth around the cutoff is estimated using the method proposed by Calonico et al. (2014). The coefficient of interest is  $\beta$ , which estimates the differential likelihood of buying a house using the voucher at the cutoff score.

Graph 2.1 shows that receiving a voucher offer increases the probability of becoming a homeowner under this program by around 63%.<sup>18</sup> This point estimate is slightly higher than that found in the previous chapter, which might reflect families with a student being more eager to purchase a house using their voucher.

As previously, this graph also shows: 1) some applicants who are below the cutoff point who used the voucher; and 2) some families who have a student and are

<sup>18</sup> As previously, the point estimate does not vary substantially if I control for  $\text{Score}_{i,g,r,c}$  with polynomials of order 1 or order 2. See Table B2 in the appendix.

above the cutoff that did not redeem their voucher. Explanations for these figures were provided in Section 1.5.1.

### 2.5.2 Fuzzy Regression Discontinuity

To estimate the causal effect of homeownership under this program on student learning and other outcomes of interest, I perform the following equation:

$$\begin{aligned} \text{Outcome}_{i,g,r,c,t} = & \delta_0 + \varepsilon_{r,c} + \varsigma_g + \delta_1 f(\text{Score}_{i,g,r,c}) + \delta_2 D_{i,g,r,c} * f(\text{Score}_{i,g,r,c}) \\ & + \beta_{LATE} \text{Homeownership}_{i,g,r,c,t} + \rho X + e_{i,g,r,c,t} \end{aligned} \quad (2.2)$$

where  $\text{Outcome}_{i,g,r,c,t}$  is the outcome of interest for student  $i$ , region of application  $r$ , offer round  $c$ , in grade  $g$ , and at time  $t$ . In this equation, I instrument  $\text{Homeownership}_{i,g,r,c,t}$  using the first stage equation (Equation 2.1). Then,  $\beta_{LATE}$  captures the effect of homeownership on students around the cutoff. From this point onwards, the results and interpretations are based on  $\beta_{LATE}$ .

## 2.6 Regression Discontinuity Validity

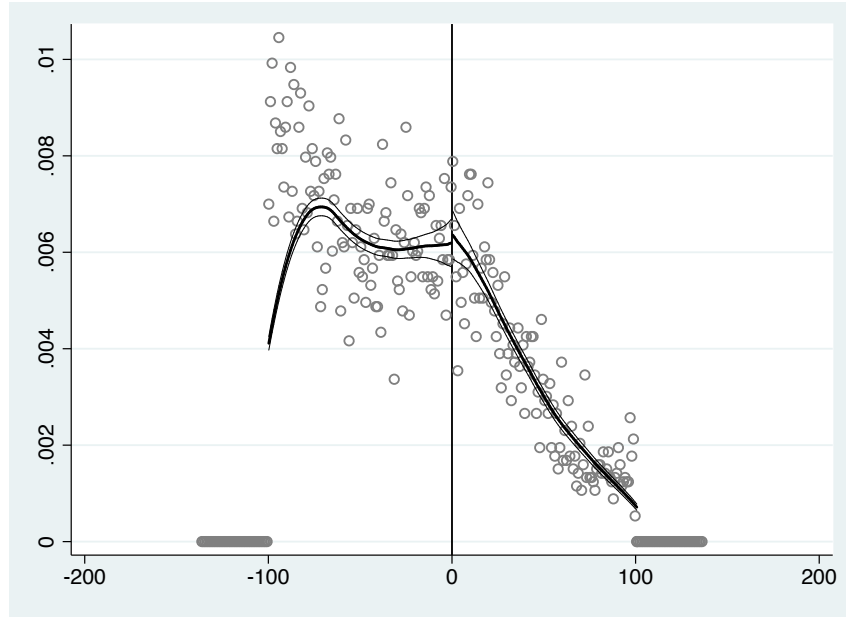
In this section I conduct the validity checks performed in the previous chapter, but for the sub-sample used in this chapter. Students on either side of the cutoff must be locally comparable, in order to interpret the results as the causal effect of owning a house on students' outcomes. On top of this, tests and surveys are voluntary; I show that the treatment does not have any effect on the probability of taking part in them.

### 2.6.1 McCrary Test

Here, I show that the mass of students around the cutoff does not change discontinuously, meaning that families are not able to manipulate the score to be just above the cutoff (McCrary, 2008).

Figure 2.2 shows no discontinuity around the cutoff in the number of students. The formal test rejects the manipulation of the score with a t-stat of -0.9754 and a p-value of 0.3294. This piece of evidence suggests that the score is not manipulated, so families cannot decide to be above the cutoff score.

**Figure 2.2: McCrary Test**



*Notes:* Data are for students who took the standardized test three years after the offer rounds. Each circle represents the share of students in a particular bin. The unit of observation is one student.

### 2.6.2 Balance Tests

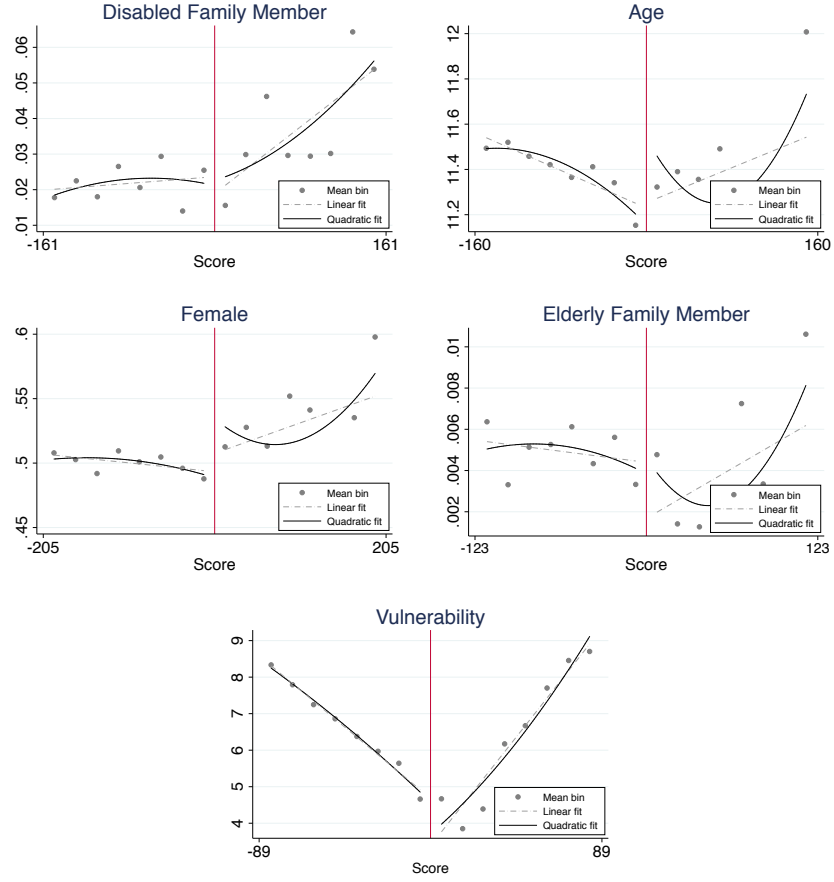
To show that the group of students in families who only just miss out on a voucher offer are locally comparable to the group of students in families who only just receive the voucher, I estimate Equation 2.2 using as an outcome variable one of the pre-determined covariates mentioned in Section 2.4.

Figure 2.3 suggests that the two groups are locally comparable, as it shows no discontinuity in its panels. Table 2.2 confirms these results and shows formally that none of the point estimates are significant. This suggests that students in families that only just miss out on a voucher serve as a good counterfactual to those in families who only just received a voucher.

### 2.6.3 Test And Questionnaire Response Rate

Mimicking the analysis used in randomized control trials, I check whether the treatment has produced some variation in the likelihood of observing a student below and above the cut-off in any of the questionnaires and tests mentioned in Sections 2.4.2 and 2.4.3. Figure 2.4 shows no discontinuity in any of the panels and Table 2.3 shows no differential response rate between the treatment and control

**Figure 2.3: Continuity of Covariates Around the Threshold**



*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. Each dot represents the mean of students for the corresponding pre-determined covariates in a particular bin. *Age* is measured in years. *Female* is a dummy variable equal to 1 if the student is a female and 0 otherwise. *Disabled Family Member* is a dummy variable equal to 1 if the student is in a family that has a disabled family member. *Elderly Family Member* is a dummy variable equal to 1 if the student is in a family that has an elderly family member. *Vulnerability* is the percentile of vulnerability for the student's family with respect to the Chilean population. Benchmark bandwidth estimated using the procedure proposed by Calonico et al. (2014).

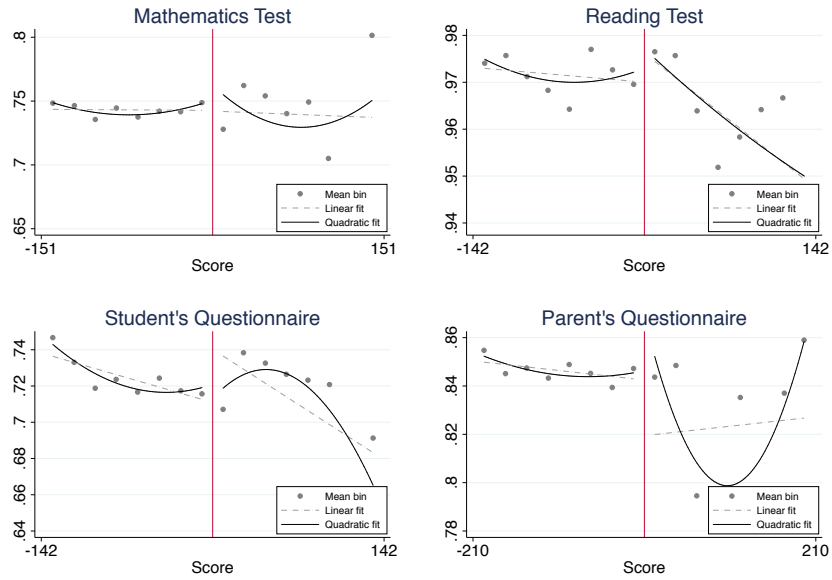
groups for all questionnaires and tests. As a note, the lower response rates for the mathematics test and student questionnaires are due to the fact that students in 2nd grade do not participate in either of these.

This piece of evidence implies that the results presented below are not biased by a differential likelihood of observing a student, in one particular side of the cut-off, for the questionnaire or tests.

**Table 2.2: Balance Test**

VARIABLES	Age		Female		Disabled Family Member		Elderly Family Member		Poverty Level	
	(Linear) (1)	(Quad.) (2)	(Linear) (3)	(Quad.) (4)	(Linear) (5)	(Quad.) (6)	(Linear) (7)	(Quad.) (8)	(Linear) (9)	(Quad.) (10)
Homeownership LATE	0.0152 (0.0345)	-0.0163 (0.0531)	0.0316 (0.0213)	0.0347 (0.0303)	-0.00828 (0.00852)	-0.00482 (0.0126)	-0.00301 (0.00216)	0.000726 (0.00335)	0.0194 (0.151)	-0.297 (0.238)
Observations	18,709	18,709	23,049	23,049	18,814	18,814	14,966	14,966	10,868	10,868
BANDWIDTH	159	159	204	204	160	160	122	122	88	88
GRADE FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
REGION FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. *Age* is measured in years. *emphFemale* is a dummy variable equal to 1 if the student is a female and 0 otherwise. *Disabled Family Member* is a dummy variable equal to 1 if the student is in a family that has a disabled family member. *Elderly Family Member* is a dummy variable equal to 1 if the student is in a family that has an elderly family member. *Vulnerability* is the percentile of vulnerability for the student's family with respect to the Chilean population. Benchmark bandwidth estimated using the procedure proposed by Calonico et al. (2014). Standard errors are clustered at the family level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

**Figure 2.4: Continuity of the Share of Students Responding to Tests and Questionnaires Around the Threshold**

*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. Each dot represents the mean of the outcome in a particular bin. *Mathematics Test* is a dummy equal to 1 if a student took the mathematics test and 0 if he/she did not. *Reading Test* is a dummy equal to 1 if a student took the reading test and 0 if he/she did not. *Student Questionnaire* is a dummy equal to 1 if a student answered the questionnaire and 0 if he/she did not. *Parent Questionnaire* is a dummy equal to 1 if a student's parent answered the questionnaire and 0 if he/she did not. Bandwidth estimated using the procedure proposed by Calonico et al. (2014).



**Table 2.3: Test and Questionnaire Response Rate**

VARIABLES	Student Quest.		Parent Quest.		Math. Test		Reading Test	
	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homeownership LATE	0.00989 (0.00998)	0.00315 (0.0152)	0.0113 (0.0154)	0.0330 (0.0222)	-0.0105 (0.00707)	-0.0146 (0.0113)	0.0120 (0.00789)	0.00924 (0.0118)
Observations	16,952	16,952	23,465	23,465	17,810	17,810	16,938	16,938
BANDWIDTH	141	141	209	209	150	150	141	141
CONTROLS	YES	YES	YES	YES	YES	YES	YES	YES
GRADE FE	YES	YES	YES	YES	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES
REGION FE	YES	YES	YES	YES	YES	YES	YES	YES

*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. *Mathematics Test* is a dummy equal to 1 if a student took the mathematics test and 0 if he/she did not. *Reading Test* is a dummy equal to 1 if a student took the reading test and 0 if he/she did not. *Student Quest.* is a dummy equal to 1 if a student answered the questionnaire and 0 if he/she did not. *Parent Quest.* is a dummy equal to 1 if a student's parent answered the questionnaire and 0 if he/she did not. Bandwidth estimated using the procedure proposed by Calonico et al. (2014). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## 2.7 Overall Results

In this section, I explore whether homeownership has an effect on students' learning results based on reading and mathematics test scores. I start by measuring the effect of homeownership over the whole sample. Then, I measure the effect separating by gender. Finally, I measure the effects separating for students in elementary school and in middle to high school.

Figure 2.5 and Figure 2.6 do not show a clear discontinuity in reading and mathematics test scores for the whole sample of students (top left panel), nor for male students (top right panel), nor for female students (middle left panel). The reading results are confirmed in Panel A of Table 2.4 (columns 1 to 6), where I find no statistically significant results for any of these three groups. Panel B shows some significant results in mathematics - for the whole sample and for female students in the quadratic form - however, none of these results are robust in all specifications; thus, I am unable to draw any conclusions. Results here seem to suggest that homeownership does not have an effect on students' test scores when considering the entire sample, nor when analyzing by gender.

Looking at the third group analyzed - students in elementary school and students in middle to high school - Figure 2.5 shows a clear discontinuity for the younger students (bottom panel), but not for older students (middle right panel). Table 2.4 confirms these results by showing a statistically significant result for students in elementary school (columns 9 and 10, panel A) and a non-statistically significant result for students in middle to high school (columns 7 and 8, Panel A).

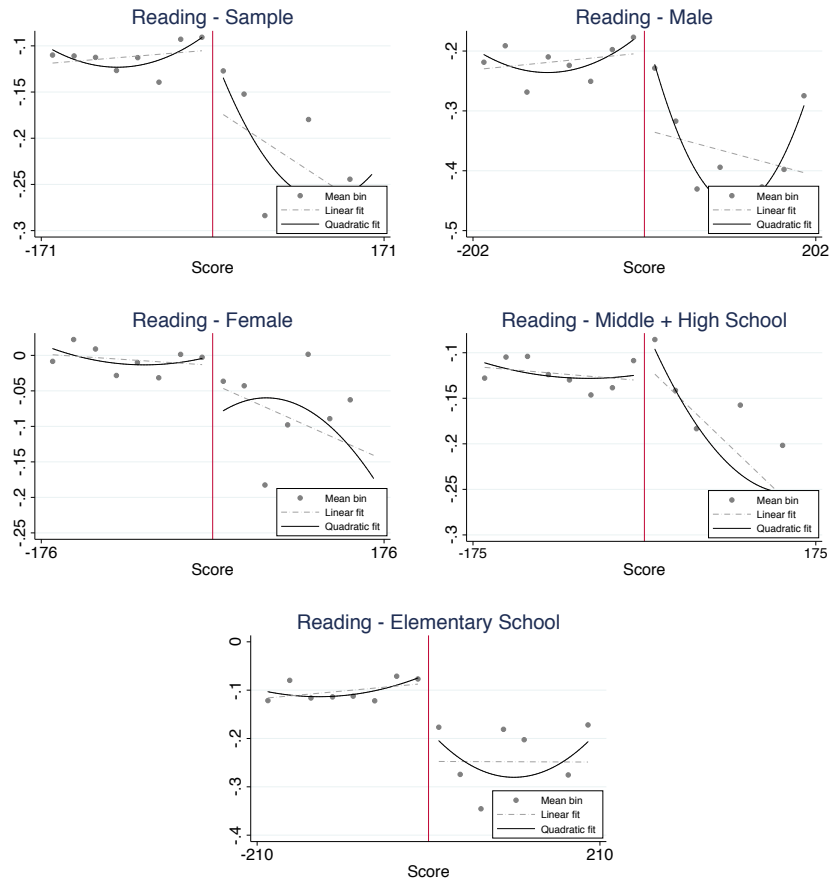
The evidence presented suggests that homeownership decreases reading test scores for students in elementary school by between  $0.162\sigma$  and  $0.183\sigma$ . Moreover, Figure 2.6 does not show a clear discontinuity for mathematics results in either subgroup, which is confirmed in panel B of Table 2.4 (columns 7 to 10).

It may seem surprising that results for students in elementary school are significant and robust in reading but not in mathematics. As mentioned in Section 2.4.2, children in 2nd grade, who are the youngest students in the sample, do not take the mathematics test. If homeownership has a stronger effect at younger ages, the absence of 2nd graders in mathematics may be the cause of the differing results between the two subjects. Figure C2 in the appendix separates the effect of homeownership on students' test score by grade and by subject. This figure shows a similar distribution of point estimates between the two subjects. The results for reading (upper panel) also show that 2nd graders seem to be the group that is most strongly affected by homeownership, which might explain the differential results between the two subjects.

My results are consistent with recent evidence showing the importance of the age of exposure in similar programs such as Moving to Opportunities (Chetty et al., 2016). As in my study, Chetty and his coauthors also show that the lack of effects across the entire sample can mask strong effects on younger students.

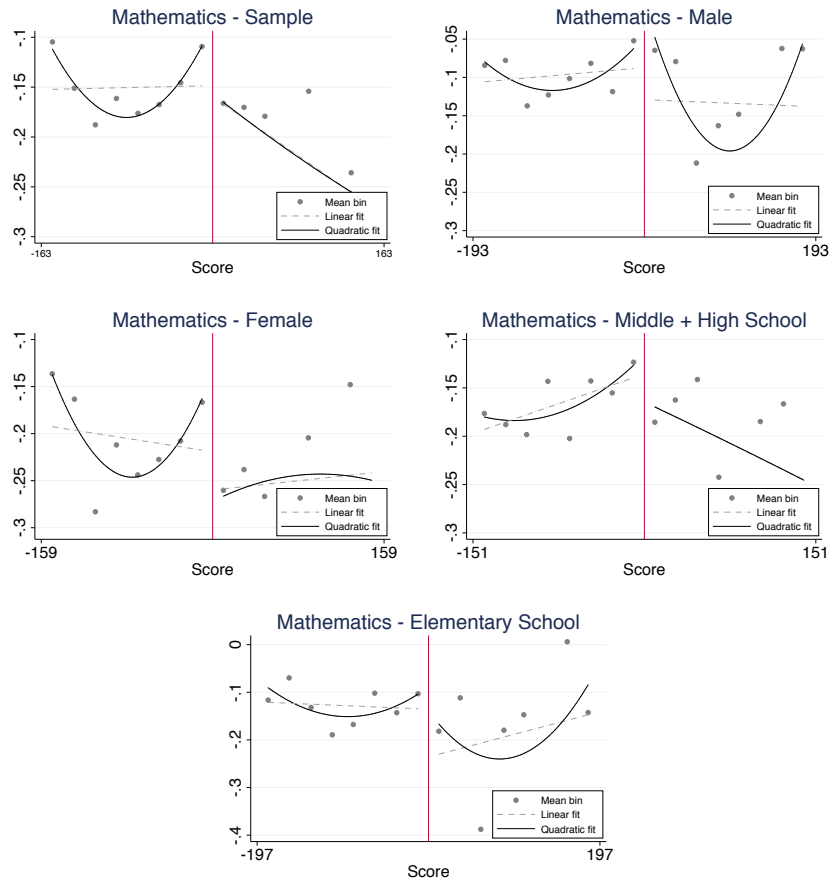
In summary, the evidence presented in this section suggests that homeownership has a causal negative effect on learning for students in elementary school.

**Figure 2.5: Discontinuity Around the Threshold - Reading**



*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. Each dot represents the mean of students for the reading test score in a particular bin. *Reading* test score is measured in standard deviation. Bandwidth estimated using the procedure proposed by Calonico et al. (2014).

**Figure 2.6: Discontinuity Around the Threshold - Mathematics**



*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. Each dot represents the mean of students for the mathematics test score in a particular bin. *Mathematics* test score is measured in standard deviation. Bandwidth estimated using the procedure proposed by Calonico et al. (2014).

**Table 2.4: Effect of Homeownership on Student Learning**

VARIABLES	Sample		Male		Female		Mid + High School.		Elementary School.	
	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)	(Linear)	(Quad.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Reading Outcomes										
Homeownership LATE	-0.0270 (0.0468)	-0.0322 (0.0747)	-0.0392 (0.0619)	0.0244 (0.0908)	-0.0260 (0.0621)	-0.0325 (0.0924)	0.0963 (0.0606)	0.145 (0.0892)	-0.162*** (0.0621)	-0.183* (0.0950)
Observations	19,218	19,218	10,920	10,920	9,927	9,927	10,693	10,693	10,334	10,334
BANDWIDTH	170	170	201	201	175	175	174	174	209	209
Panel B: Mathematics Outcomes										
Homeownership LATE	-0.0104 (0.0517)	-0.144* (0.0805)	0.0320 (0.0691)	0.0666 (0.103)	-0.0829 (0.0697)	-0.250** (0.110)	0.00210 (0.0610)	-0.0882 (0.0967)	-0.0610 (0.0871)	-0.281* (0.151)
Observations	14,140	14,140	8,047	8,047	7,013	7,013	9,496	9,496	4,638	4,638
BANDWIDTH	162	162	192	192	158	158	150	150	196	196
CONTROLS	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
GRADE FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
REGION FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

*Notes:* Data are students who took the standardized test three years after the offer rounds. The unit of observation is one student. *Panel A* Provides the results for reading test scores and *Panel B* for mathematics test scores. *Sample* provides the results for all school-age students. *Male* provides the results for all male school-age students. *Female* provides the results for all female school-age students. *Mid + High School* provides the results for all students above fourth grade. *Elementary School* provides the results for all students in fourth grade or lower. Test score results are measured in standard deviations. Bandwidth estimated using the procedure proposed by Calonico et al. (2014). Standard errors are clustered at the family level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

## 2.8 Potential Mechanisms

In the final part of the analysis I search for possible mechanisms to explain the effects found in the previous sections. Since I found an effect only on elementary school students, I focus on this particular subsample in this section. However, results for the whole sample are reported in Figure C3 and Table B3, and for students in middle to high school in Figure C4 and Table B4 in the Appendix.<sup>19</sup>

Figure 2.7 illustrates the discontinuities for the variables described in Section 2.4.3 for students in elementary school, and shows that the greatest discontinuities exist for the variables measuring whether families live with a hosting family and whether they live in a non-rudimentary house (a house with a concrete floor, an indoor bathroom, and access to potable water).

Table 2.5 shows that homeownership decreases the share of families living with a hosting family by between 12 p.p. and 20 p.p. (Panel A, Columns 1 and 2). This effect arises most likely from families that were previously living in another family's house and have made the transition to their own house, and should not affect families who were already living on their own at the time of application. The most likely link between the decrease in the likelihood of living with a hosting family and students' academic outcomes is the loss of learning support provided by members of the hosting family. These hosting family members are often students' close relatives, typically grandparents (CASEN, 2013). Unfortunately, I do not have access to information detailing which families were living with a hosting family when applying. I am thus unable to estimate the effect of homeownership separately for this subgroup.

Table 2.5 also shows that homeownership increases the probability of a family living in a non-rudimentary house by 14 p.p. (Panel A, columns 3 and 4), for students in elementary school. Better housing conditions are often associated with better health outcomes, which might in turn result in improvements in learning. Therefore, my results could suggest that the presumably positive effect from living in a non-rudimentary house might be outweighed by the loss in learning support described above. The remaining columns of Table 2.5 show no evidence that homeownership has an effect on school quality, moving more frequently to another school or town, students' self-esteem, parental engagement in student learning, parental expectations on students, or more children being born into the family.

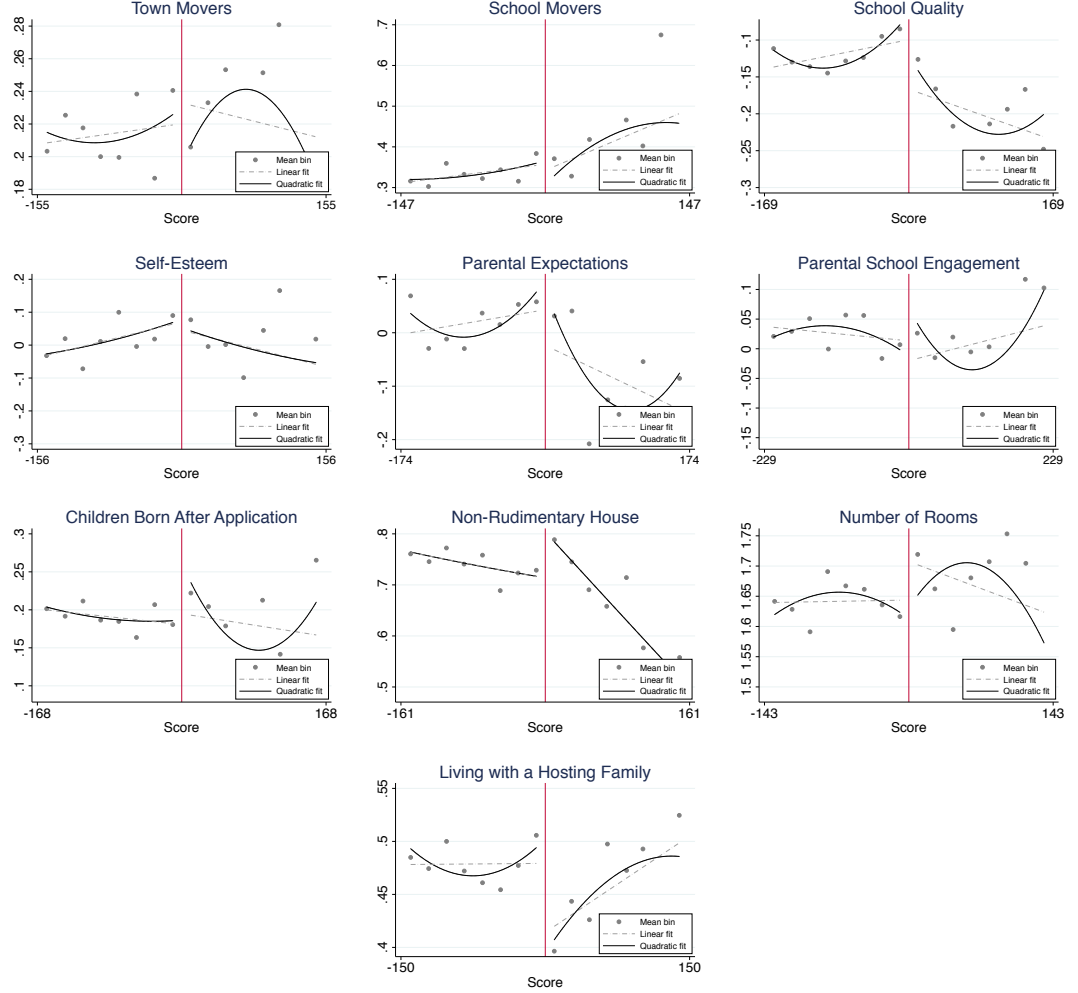
In previous work, Navarrete and Navarrete (2016) found that this same program had a negative impact on employment, which might leave more adult home-

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<sup>19</sup> These results do not differ significantly from what is reported in this section.

owners in the house, and should in turn increase the amount of learning support provided at home. These seemingly contradictory results could be explained by two factors. First, not all families in the sample used by Navarrete and Navarrete (2016) have a child of school age, and the effect of homeownership on labor supply may differ between the whole sample and the families in this sub-sample. Data availability does not allow me to merge the dataset from this paper with the one used in Navarrete and Navarrete (2016); therefore, I am unable to test this potential explanations. Second, the smaller share of adults that do not enter the labor market may not compensate for the sharp drop in learning support provided by hosting families. While Navarrete and Navarrete (2016) find effects on labor market participation ranging from 3 to 5 p.p., here the effect on the probability of living with a hosting family ranges from 12 to 20 p.p.

**Figure 2.7: Mechanisms - Students in Elementary School**



*Notes:* Data are for students in elementary school who took the standardized test three years after the offer rounds. The unit of observation is one student. Each dot represents the mean of students for the corresponding outcome variable in a particular bin. *Hosting Family* is a dummy variable equal to 1 if the family lives in another family's house and 0 if not. *Non-Rudimentary House* is a dummy variable equal to 1 if the family lives in a house with a concrete floor, an indoor bathroom, and access to potable water and 0 if not. *Rooms* is the number of rooms in the house. *Town Movers* is a dummy variable equal to 1 if the family has moved to a different town than the one that they lived in at the time of application and 0 if not. *School Movers* is a dummy variable equal to 1 if the student has changed school since the time of application and 0 if not. *School Quality* is the school's average test score results measured in standard deviations. *Self-Esteem* is the student's self-esteem measured in standard deviations, determined using questions such as "How strongly do you agree with the statement: I am smart?". *Parental Expectations* is the parent's expectations of students measured in standard deviation, determined using the question "What is the highest educational level that you think your child will reach?". *Parental School Engagement* is the parental engagement in students' learning measured in standard deviation, determined using questions such as "How strongly do you feel that your parents help you to study?". Bandwidth estimated using the procedure proposed by Calonico et al. (2014).



**Table 2.5: Mechanisms - Students in Elementary School**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Hosting Family (Linear)	Family (Quad.)	Non-Rudimentary House (Linear)	House (Quad.)	Rooms (Linear)	(Quad.)	Town Mov. (Linear)	(Quad.)	School Mov. (Linear)	(Quad.)
Panel A										
Homeownership LATE	-0.123*** (0.0368)	-0.204*** (0.0628)	0.142*** (0.0327)	0.143*** (0.0521)	0.0713 (0.0608)	0.129 (0.0951)	-0.0219 (0.0290)	-0.0600 (0.0460)	-0.0158 (0.0460)	-0.0210 (0.0735)
Observations	7,953	7,953	8,408	8,408	7,637	7,637	9,341	9,341	5,008	5,008
BANDWIDTH	149	149	160	160	142	142	154	154	146	146
	School Quality (Linear)	Quality (Quad.)	Self-Esteem (Linear)	(Quad.)	Parental Expect. (Linear)	(Quad.)	Parental Eng. (Linear)	(Quad.)	New Children (Linear)	(Quad.)
Panel B										
Homeownership LATE	-0.0457 (0.0299)	-0.0838* (0.0491)	0.0334 (0.101)	0.0443 (0.166)	0.0107 (0.0738)	-0.0801 (0.123)	-0.00521 (0.0667)	0.0957 (0.0959)	0.0417 (0.0354)	0.0790 (0.0548)
Observations	10,066	10,066	4,886	4,886	8,817	8,817	11,115	11,115	8,717	8,717
BANDWIDTH	168	168	155	155	173	173	228	228	167	167
CONTROLS	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
GRADE FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
REGION FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

*Notes:* Data are for students in elementary school who took the standardized test three years after the offer rounds. The unit of observation is one student. *Hosting Family* is a dummy variable equal to 1 if the family lives in another family's house and 0 if not. *Non-Rudimentary House* is a dummy variable equal to 1 if the family lives in a house with a concrete floor, an indoor bathroom, and access to potable water and 0 if not. *Rooms* is the number of rooms in the house. *Town Movers* is a dummy variable equal to 1 if the family has moved to a different town than the one that they lived in at the time of application and 0 if not. *School Movers* is a dummy variable equal to 1 if the student has changed school since the time of application and 0 if not. *School Quality* is the school's average test score results measured in standard deviations. *Self-Esteem* is the student's self-esteem measured in standard deviations, determined using questions such as "How strongly do you agree with the statement: I am smart?". *Parental Expectations* is the parent's expectations of students measured in standard deviation, determined using the question "What is the highest educational level that you think your child will reach?". *Parental School Engagement* is the parental engagement in students' learning measured in standard deviation, determined using questions such as "How strongly do you feel that your parents help you to study?". Bandwidth estimated using the procedure proposed by Calonico et al. (2014). Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## 2.9 Concluding Remarks

Governments continue to allocate large sums of money to promote homeownership. However, the effect of owning a house on many different outcomes, including students' overall learning, still remains unclear.

Using the arbitrary cut-off produced by a Chilean policy that provides poor families with vouchers to purchase a house, I have implemented a fuzzy regression discontinuity strategy to measure the effect of homeownership on students' learning outcomes. When considering the sample as a whole I find no significant results; however, I observe that homeownership does decrease test scores by between  $0.16\sigma$  and  $0.18\sigma$  for students in elementary school. I also show that the families of these students are more likely to live in a higher-quality house and are less likely to live with another hosting family. This latter mechanism is the most likely driver of the effects on student learning, as younger students may face a decrease in the learning support provided by hosting family members - often grandparents - when they move with their parents to their new house.

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## 2.A Appendix: Questionnaires

### 2.A.1 Self-Esteem

**How strongly do you agree with the following statements?**

1. When I grow up I will accomplish all of my goals.
2. I am smart.
3. I am not afraid of talking in front of the class.
4. I remember what I learn in class.

### 2.A.2 Parental Engagement in School

**How strongly do you feel that your parents do the following?**

1. He/She helps me to study.
2. He/She helps me with homework.
3. He/She explains to me what I did not understand from school.
4. He/She knows my grades.
5. He/She congratulates me when I get a good grade.

### 2.A.3 Parental Expectations

1. What is the highest educational level that you think your child will reach?

## 2.B Appendix: Tables

**Table B1: Test Calendar**

Grade	Subject	2007	2008	2009	2010	2011	2012	2013	2014	2015
2nd	Reading						X	X	X	X
4th	Reading	X	X	X	X	X	X	X	X	X
	Math	X	X	X	X	X	X	X	X	X
6th	Reading							X	X	X
	Math							X	X	X
8th	Reading	X		X		X		X	X	X
	Math	X		X		X		X	X	X
10th	Reading		X		X		X	X	X	X
	Math		X		X		X	X	X	X

The X represents years in which the exam takes place for the corresponding grade and subject. Second-grade mathematics is not shown as the test does not take place in any year.

**Table B2: First Stage**

VARIABLES	(1) Homeownership	(2) Homeownership
Winner_Dummy	0.634*** (0.0144)	0.625*** (0.0206)
Constant	-0.186*** (0.0243)	-0.183*** (0.0244)
Observations	14,969	14,969
R-squared	0.529	0.529
BANDWIDTH	122	122
CONTROLS	YES	YES
REGION FE	YES	YES
OFFER ROUND FE	YES	YES

*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. Benchmark bandwidth estimated using the procedure proposed by Calonico et al. (2014). Standard errors are clustered at the family level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1



Table B3: Mechanisms on Students - Sample

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Hosting Family (Linear)	Family (Quad.)	Non-Rudimentary House (Linear)	House (Quad.)	Rooms (Linear)	(Quad.)	Town Mov. (Linear)	(Quad.)	School Mov. (Linear)	(Quad.)
Panel A										
Homeownership LATE	-0.110*** (0.0287)	-0.157*** (0.0449)	0.145*** (0.0256)	0.102*** (0.0385)	0.0912* (0.0547)	0.115 (0.0869)	0.00317 (0.0219)	-0.0196 (0.0354)	0.0486* (0.0277)	0.0502 (0.0405)
Observations	14,370	14,370	14,826	14,826	14,063	14,063	17,935	17,935	13,024	13,024
BANDWIDTH	136	136	141	141	132	132	151	151	140	140
	School Quality (Linear)	Quality (Quad.)	Self-Esteem (Linear)	Self-Esteem (Quad.)	Parental Expect. (Linear)	Parental Expect. (Quad.)	Parental Eng. (Linear)	Parental Eng. (Quad.)	New Children (Linear)	New Children (Quad.)
Panel B										
Homeownership LATE	-0.00201 (0.0241)	-0.0229 (0.0370)	-0.000781 (0.0542)	-0.0818 (0.0810)	0.0106 (0.0535)	-0.0506 (0.0803)	0.0311 (0.0504)	0.0682 (0.0725)	0.00626 (0.0250)	-0.0147 (0.0402)
Observations	19,405	19,405	14,984	14,984	16,858	16,858	18,899	18,899	15,938	15,938
BANDWIDTH	166	166	183	183	177	177	204	204	154	154
CONTROLS	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
GRADE FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
REGION FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. *Hosting Family* is a dummy variable equal to 1 if the family lives in another family's house and 0 if not. *Non-Rudimentary House* is a dummy variable equal to 1 if the family lives in a house with a concrete floor, an indoor bathroom, and access to potable water and 0 if not. *Rooms* is the number of rooms in the house. *Town Movers* is a dummy variable equal to 1 if the family has moved to a different town than the one that they lived in at the time of application and 0 if not. *School Movers* is a dummy variable equal to 1 if the student has changed school since the time of application and 0 if not. *School Quality* is the school's average test score results measured in standard deviations. *Self-Esteem* is the student's self-esteem measured in standard deviations, determined using questions such as "How strongly do you agree with the statement: I am smart?". *Parental Expectations* is the parent's expectations of students measured in standard deviation, determined using the question "What is the highest educational level that you think your child will reach?". *Parental School Engagement* is the parental engagement in students' learning measured in standard deviation, determined using questions such as "How strongly do you feel that your parents help you to study?". Benchmark bandwidth estimated using the procedure proposed by Calonico et al. (2014). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

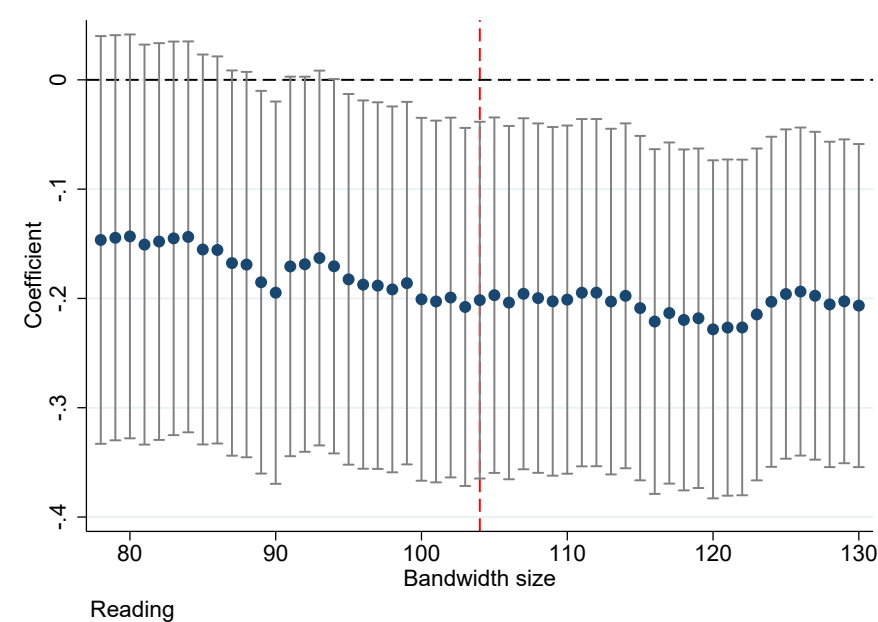
**Table B4: Mechanisms on Students in Middle and High School**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Hosting Family (Linear)	(Quad.)	Non-Rudimentary House (Linear)	(Quad.)	Rooms (Linear)	(Quad.)	Town Mov. (Linear)	(Quad.)	School Mov. (Linear)	(Quad.)
Panel A										
Homeownership LATE	-0.103*** (0.0348)	-0.0923* (0.0541)	0.144*** (0.0325)	0.0709 (0.0498)	0.0862 (0.0759)	0.157 (0.127)	0.0163 (0.0283)	0.0333 (0.0433)	0.0904*** (0.0331)	0.0942* (0.0485)
Observations	7,815	7,815	7,274	7,274	7,493	7,493	9,368	9,368	7,934	7,934
BANDWIDTH	156	156	143	143	148	148	164	164	136	136
	School Quality (Linear)	(Quad.)	Self-Esteem (Linear)	(Quad.)	Parental Expect. (Linear)	(Quad.)	Parent Eng. (Linear)	(Quad.)	New Children (Linear)	(Quad.)
Panel B										
Homeownership LATE	0.0397 (0.0341)	0.0233 (0.0519)	-0.0597 (0.0677)	-0.0951 (0.104)	-0.00969 (0.0709)	-0.0275 (0.102)	0.0511 (0.0688)	0.0418 (0.101)	-0.0416 (0.0298)	-0.0781* (0.0459)
Observations	9,800	9,800	8,404	8,404	8,473	8,473	9,231	9,231	8,099	8,099
BANDWIDTH	173	173	159	159	194	194	217	217	162	162
CONTROLS	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
GRADE FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
OFFER ROUND FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
REGION FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

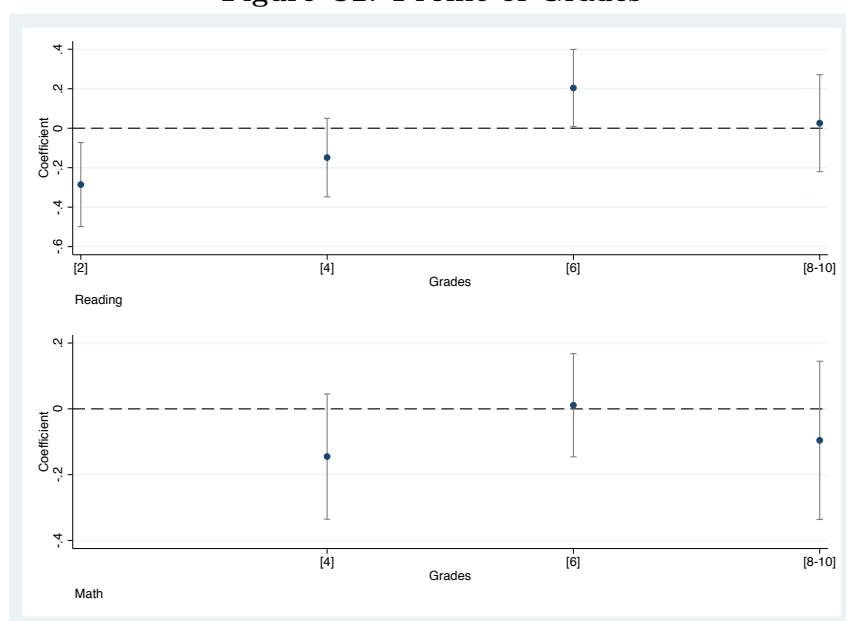
*Notes:* Data are for students in middle and high school who took the standardized test three years after the offer rounds. The unit of observation is one student. *Hosting Family* is a dummy variable equal to 1 if the family lives in another family's house and 0 if not. *Non-Rudimentary House* is a dummy variable equal to 1 if the family lives in a house with a concrete floor, an indoor bathroom, and access to potable water and 0 if not. *Rooms* is the number of rooms in the house. *Town Movers* is a dummy variable equal to 1 if the family has moved to a different town than the one that they lived in at the time of application and 0 if not. *School Movers* is a dummy variable equal to 1 if the student has changed school since the time of application and 0 if not. *School Quality* is the school's average test score results measured in standard deviations. *Self-Esteem* is the student's self-esteem measured in standard deviations, determined using questions such as "How strongly do you agree with the statement: I am smart?". *Parental Expectations* is the parent's expectations of students measured in standard deviation, determined using the question "What is the highest educational level that you think your child will reach?". *Parental School Engagement* is the parental engagement in students' learning measured in standard deviation, determined using questions such as "How strongly do you feel that your parents help you to study?". Benchmark bandwidth estimated using the procedure proposed by Calonico et al. (2014). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

## 2.C Appendix: Figures

Figure C1: Bandwidth Sensitivity

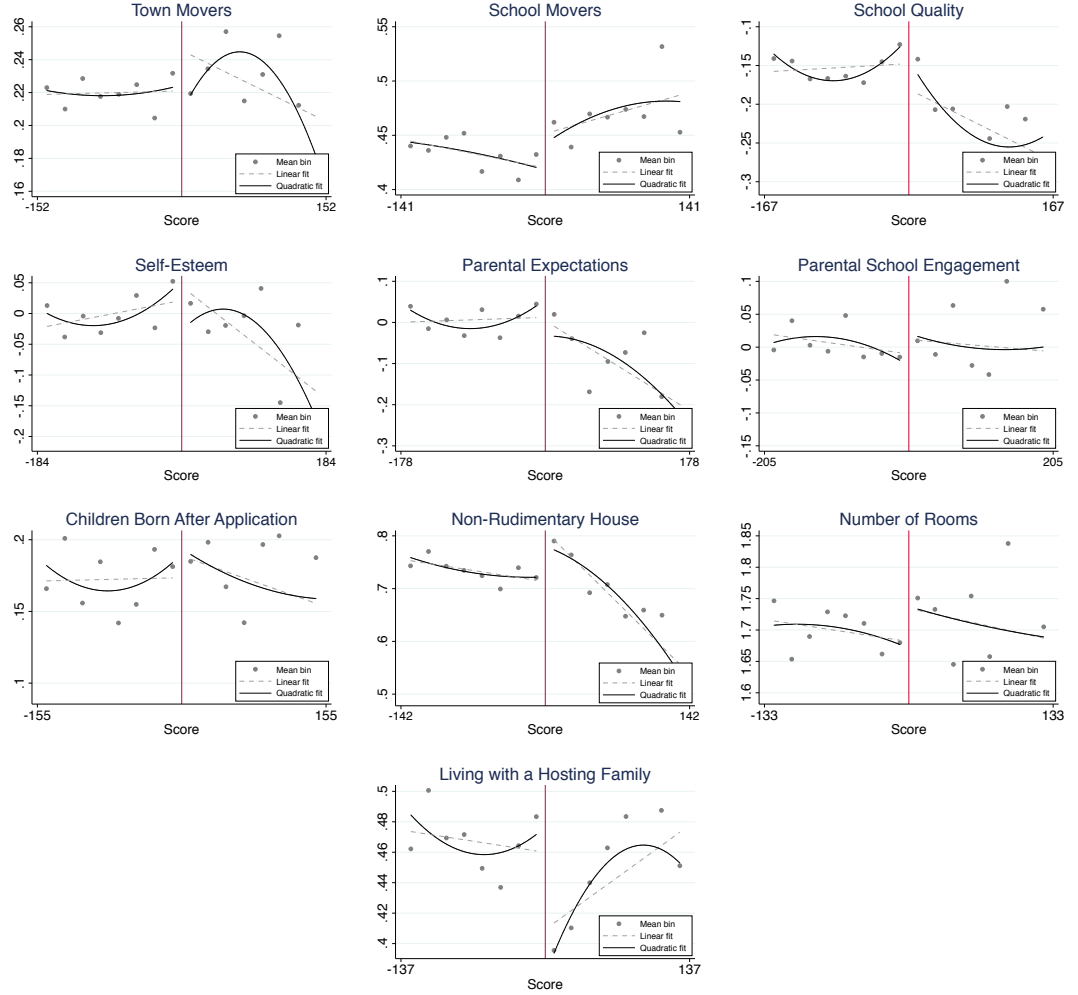


**Figure C2: Profile of Grades**



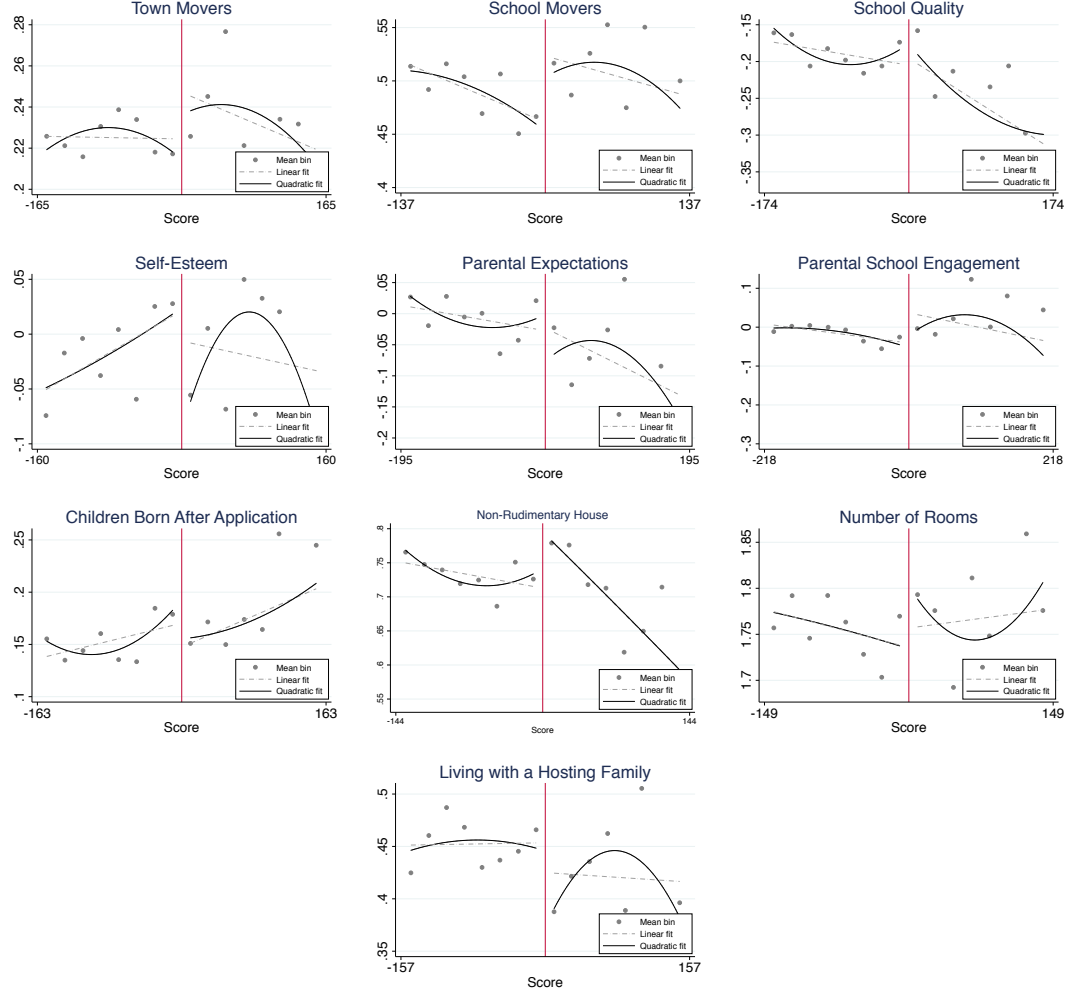
*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. A dot represents the point estimate for the corresponding grade. Confidence intervals are calculated at 95% confidence.

**Figure C3: Mechanisms on Students - Sample**



*Notes:* Data are for students who took the standardized test three years after the offer rounds. The unit of observation is one student. Each dot represents the mean of students for the corresponding outcome variable in a particular bin. *Hosting Family* is a dummy variable equal to 1 if the family lives in another family's house and 0 if not. *Non-Rudimentary House* is a dummy variable equal to 1 if the family lives in a house with a concrete floor, an indoor bathroom, and access to potable water and 0 if not. *Rooms* is the number of rooms in the house. *Town Movers* is a dummy variable equal to 1 if the family has moved to a different town than the one that they lived in at the time of application and 0 if not. *School Movers* is a dummy variable equal to 1 if the student has changed school since the time of application and 0 if not. *School Quality* is the school's average test score results measured in standard deviations. *Self-Esteem* is the student's self-esteem measured in standard deviations, determined using questions such as "How strongly do you agree with the statement: I am smart?". *Parental Expectations* is the parent's expectations of students measured in standard deviation, determined using the question "What is the highest educational level that you think your child will reach?". *Parental School Engagement* is the parental engagement in students' learning measured in standard deviation, determined using questions such as "How strongly do you feel that your parents help you to study?". Benchmark bandwidth estimated using the procedure proposed by Calonico et al. (2014).

**Figure C4: Mechanisms on Students in Middle and High School**



*Notes:* Data are for students in middle or high school who took the standardized test three years after the offer rounds. The unit of observation is one student. Each dot represents the mean of students for the corresponding outcome variable in a particular bin. *Hosting Family* is a dummy variable equal to 1 if the family lives in another family's house and 0 if not. *Non-Rudimentary House* is a dummy variable equal to 1 if the family lives in a house with a concrete floor, an indoor bathroom, and access to potable water and 0 if not. *Rooms* is the number of rooms in the house. *Town Movers* is a dummy variable equal to 1 if the family has moved to a different town than the one that they lived in at the time of application and 0 if not. *School Movers* is a dummy variable equal to 1 if the student has changed school since the time of application and 0 if not. *School Quality* is the school's average test score results measured in standard deviations. *Self-Esteem* is the student's self-esteem measured in standard deviations, determined using questions such as "How strongly do you agree with the statement: I am smart?". *Parental Expectations* is the parent's expectations of students measured in standard deviation, determined using the question "What is the highest educational level that you think your child will reach?". *Parental School Engagement* is the parental engagement in students' learning measured in standard deviation, determined using questions such as "How strongly do you feel that your parents help you to study?". Benchmark bandwidth estimated using the procedure proposed by Calonico et al. (2014).

## Chapter 3

**When do School Principals Matter?: the effects of a reform in the selection procedure of school principals on school outcomes.**

### 3.1 Introduction

Following the ideas presented in literature studying the leaders of firms, scholars have claimed in recent years that those at the head of schools - namely, school principals - constitute a significant input in educational production. Principals can influence students' learning in two ways: 1) by changing the school climate in which students learn, and; 2) by closely monitoring teachers, which influences pedagogical practices and in turn has an effect on students' learning (Di Liberto et al., 2015). However, a fundamental difference exists between the director of a firm and a school principal, namely the incentives that they face in different institutional settings. While a director of a firm risks losing their job if their company does not make high enough profits, school principals are not necessarily made accountable if their students should fail to improve in their performance.

The growing body of literature on school principals has implemented mostly correlational and value-added studies. Correlational studies tend to find that improving principals' practices of monitoring teachers increases students' learning outcomes (Bloom et al., 2015; Di Liberto et al., 2015). Value-added studies have found 0 to 0.15 standard deviation ( $\sigma$ ) increases in student learning when a principal's effectiveness increases by  $1\sigma$  (Branch et al., 2012; Grissom et al., 2014; Laing et al., 2016; Böhlmark et al., 2012; Crawford, 2016; Agasisti et al., 2016). Tavares (2015) is the only quasi-experimental paper focusing on 'principal effects' of which I am aware. Using a regression discontinuity design, she explores a large program in Sao Paulo (Brazil) that teaches managerial practices to principals in low-performing schools and finds positive effects on school climates and teacher practices. Regarding test scores, she finds that mathematics test scores increase by  $0.05\sigma$  for poorly achieving students in eighth grade. The real impact of school principals on schools and the variation throughout different institutional settings thus remains very much in debate.

This paper explores a reform that changes the way in which school principals in Chile are appointed when an incumbent principal comes to the end of their five-year fixed term. This reform provides a plausibly exogenous variation in the characteristics of principals leading public schools, in a setting where school employees are almost impossible to fire or incentivize,<sup>1</sup> and the mayor is ultimately responsible for all hiring decisions.<sup>2</sup>

Prior to the reform, the mayor appointed school principals in public schools

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<sup>1</sup> *School employees* in this paper refers to school principals, the school management team, and teachers.

<sup>2</sup> Hiring decisions in this paper are restricted to school employee.



directly and without external supervision. This system raised concern from the government regarding the quality of principals. In April 2012, a reform was introduced to address these concerns, aiming to increase the number of more highly educated and motivated principals who would be better able to monitor teachers and ensure a less conflictive and a more cooperative school climate (Servicio Civil, 2012). This reform mandated that mayors open applications to the public when school principal positions are renewed. This call for applications must be registered with the government and fixed for a certain time period. Following this, the applicants are judged first by an independent company, and then by a separate committee. Only the final decision is made by the mayor, who chooses from the reduced pool of applicants that passed the previous steps. Furthermore, the government closely monitors this whole process. This reform did not alter any aspect related to the period of time that principals spend in office, nor the terms of school employees' contracts regarding their job security.

Since the reform solely affects this principal renewal process, I implemented a difference-in-differences strategy to explore the effect of principals instated after the reform on various aspects of their schools. Following this, I used schools that renewed their principal under the reform conditions before 2015 as a treated group, and those that had not renewed their principal as a control group.<sup>3</sup> My results show that the reform has brought in principals who are on average 4.5 years younger, have 6.8 years less experience, and are 13% more likely to hold a postgraduate degree. This is consistent with the reform's goal of drawing more motivated and educated principals into the public school system. I also note that, prior to the reform, these changes in principals' characteristics were not typical at the end of a five-year principal term.

I go on to analyze the changes that post-reform principals generate in schools. First, I study the characteristics of other school staff members and find that, in schools with a principal appointed under the new regime, the management team is around 1.5 years younger, has 2 years less experience, and is 4.8% more likely to hold a postgraduate degree.<sup>4</sup> On the other hand, teachers' characteristics do not change much following the arrival of new principals. This is consistent with the high rigidity of the school staff.

Following this, I use a panel dataset of teacher questionnaires to analyze the changes to school climate produced by post-reform principals. Under the new prin-

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<sup>3</sup> The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years.

<sup>4</sup> The management team are those employees who perform managerial tasks but are not the principal. Examples include the vice principal or the director of pedagogical methods.

cipals, school violence decreases by  $0.10\sigma$ ,<sup>5</sup> and community engagement increases by  $0.07\sigma$ .<sup>6</sup> These results are in line with the goal of introducing principals who are able to foster a less conflict-ridden, more cooperative school climate.

I also use this questionnaire to establish whether post-reform principals generate changes in teacher-monitoring practices and teachers' pedagogical methods in the classroom.<sup>7</sup> I found no discernible effect of new principals on these two dimensions, suggesting that the government is failing in its attempt to increase monitoring and change pedagogical approaches. The fact that both principals and teachers face no motivation to improve their own performance - they are almost impossible to fire or incentivize - could explain these unexpected results. Consistent with this explanation, correlational studies have also found that principals can be less effective in a rigid institutional context (Bloom et al., 2015). Finally, I explore the effect of post-reform principals on students' test results, and do not observe any significant change over the period analyzed.

Due to data availability, I am only able to study the short-term effects of school principals. It is possible that, in the long run, an effect on test scores may emerge given that principals might need a longer period of time to produce an impact on student learning, as suggested by Grissom et al. (2014).

This paper contributes to the existing literature in two different ways. First, the results of this paper suggest that the institutional context in which principals operate can be influential, and thus must be taken into account when analyzing the effects of school principals. Second, I provide quasi-experimental evidence showing that principals can indeed improve school climates, and that potential correlations between school climate and monitoring practices should be taken into account in future studies.

The remainder of the paper is organized as follows: Section 3.2 presents the institutional background and explains the reform in detail. Section 3.3 describes the dataset I use in this project. Section 3.4 presents the empirical framework and results. Finally, Section 3.5 concludes.

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<sup>5</sup> School violence is measured using questions such as "How often have fights occurred between students this year?"

<sup>6</sup> Community engagement is measured using questions such as "How strongly do you agree with the statement: The principal involves parents in school activities?"

<sup>7</sup> Teaching-monitoring practices are measured using questions such as "How strongly do you agree with the statement: The principal evaluates teacher performance in terms of students' academic progress?". Teachers' pedagogic practices covers several factors including: 1) Class organization, which is measured using questions such as "How often do you ask students to work in teams?"; 2) Evaluation methods, which is measured using questions such as "How often do you assign a school project to evaluate students?"; and 3) Teaching methods, which is measured using questions such as "How often do you go over test answers during class?"

## 3.2 Institutional Background

### 3.2.1 Educational System

The Chilean educational system is comprised of three school types: private, voucher, and public schools. Private and voucher schools are governed by private institutions, and as such were not affected by the school principal appointment reform. Public schools, however, are administered by municipalities, and the mayor bears the responsibility for all educational decisions that occur within the public system.<sup>8</sup> Students attending public schools tend to come from more disadvantaged families than students at private and voucher schools. The difference in socioeconomic backgrounds is not only due to the higher school fees of private and voucher schools,<sup>9</sup> but is also the result of public schools having no say in who does or does not attend their institution.

On the topic of school violence, the Ministry of Education (2007) finds that 70% of teachers in public schools declare that a violent incident occurs at least once a week in their classroom. Alongside this, 47% of students declare to have been victims of school violence within the last year. Aggressive incidents are more common among students from low socioeconomic backgrounds, while psychological violence (e.g. bullying) is more common amongst those with higher socioeconomic backgrounds.

### 3.2.2 School Employees

Chilean schools contain three types of employees who are relevant for this paper. These three types are: 1) the school principal, who is the head of the school, 2) the management team, meaning employees who perform managerial tasks but are not the principal,<sup>10</sup> and 3) teachers, whose only duty is to teach classes.

From the early 1990s to the present day, the process of hiring and firing employees in public schools has been rigidly set. By law, school principals remain in office for a five-year fixed term, and principals, as well as the management team and teachers, are extremely difficult to dismiss or incentivize, irrespective of their performance. Moreover, since the mayor is ultimately responsible for any educational resolutions in public schools, principals are not directly involved in the hiring

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<sup>8</sup> In legal terms, all school buildings and employees are part of the municipality, and therefore the mayor is ultimately in charge of them.

<sup>9</sup> All private schools charge some fees to students' families, and most voucher schools charge additional fees on the top of governmental subsidies.

<sup>10</sup> For example, the vice principal or the director of pedagogical methods.

process.

### 3.2.3 The School Principal Reform

Prior to the reform, the mayor appointed school principals directly, without any outside supervision over the process. In the late 2000s, the government expressed concern regarding the discretion afforded to mayors in appointing principals. The government felt that this often resulted in mayors appointing principals of low quality due to personal or political connections.

In April 2012, a new regime was implemented in an attempt to increase the quality of principals and improve school conditions. The new regime mandated that mayors open applications to the general public every time that a public school principal position becomes available. This application process follows five steps. First, the municipality undertaking the call for applicants must register the process with the central government agency, the “*Servicio Civil*” (Civil Service), which will supervise the process. During this registration period, the Civil Service and the municipality fix a time schedule for the application process. Second, the Civil Service and the municipality publicly announce the call for applicants and the application process begins.<sup>11</sup> Third, once the application stage has come to a close, a private company selects the twelve most suitable candidates.<sup>12</sup> This company selects candidates by assigning them a score based on an assessment of their curriculum, cover letter, and interviews against the profile required for the position. The final twelve candidates are those with the highest scores. Fourth, a committee is formed from three people: 1) a teacher from the municipality, selected at random, 2) one person from the national office of the Civil Service, and 3) the head of the municipal Department of Education. This committee selects a final shortlist of three to five candidates, again using a scoring system based on the same assessment points used in the previous step. Finally, from this last group, the mayor selects the new school principal.

To ensure that the application process is truly public, the Civil Service takes several measures. The call for applicants is widely advertised in newspapers and social media. If a position receives fewer than five applicants, the process is cancelled and a new call for applications takes place. People can apply to the Civil Service or to the municipality, and this choice has no effect on the likelihood of being appointed. Through this process, the government claims to provide an equal opportunity for every applicant.

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<sup>11</sup> Every individual with a Chilean working permit and experience in education is eligible for the position.

<sup>12</sup> In the small number of schools with more than 1,000 students, the private company may select the fifteen most suitable candidates.

The reform was based around specific goals, for which certain minimum criteria are set for each application and appointment process. One goal of the reform is to promote the appointment of principals who are more highly educated, and are better equipped to monitor teacher and student progress (Servicio Civil, 2012). To achieve this, the external company and the committee assign higher points to candidates who have particularly relevant degrees for the position (for example, pedagogical or management degrees), who have advanced degrees in these areas (for example, a master's or Ph.D. degree), and who have experience in monitoring teachers and developing educational plans (for example, experience as part of a school management team or acting as an advisor to a management team). These points are assigned in a deterministic manner; for instance, applicants with a relevant master's degree in pedagogy receive fifteen points, while an applicant with a relevant bachelor's degree receives five points.

Another governmental goal is to incorporate more motivated school leaders who would be able to ensure a less conflictive and more cooperative school climate. To this end, the Civil Service requires that the selection process assign higher points to applicants who are able to promote a mutually respectful school climate, who can more deeply incorporate the wider school community into the school, and who have the ability to deal with situations of conflict that may arise. Since these characteristics are effectively unobservable, the external company and the committee base their judgment of these attributes on applicants' curriculum, cover letters, and interviews.

The reform did not introduce significant changes in the autonomy of principals nor the job security of school employees. The mayor continues to be ultimately responsible for all educational decisions that occur in a school, and dismissal of school employees remains a difficult process.

### **3.3 Data**

To perform the analysis, I have used five different administrative datasets from two different governmental institutions. The Ministry of Education provided the first four datasets, which allowed me to identify the school that each student attends, students' scores in a standardized test, the teachers' questionnaire responses, and the characteristics of school employees in each school. The Civil Service provided the fifth dataset, identifying which schools renewed their principal under post-reform conditions. I focus on observations from 2009 to 2014 for reasons that I will explain in Section 3.3.3.

### 3.3.1 Student Enrollment

To identify the school attended by each student, I use the *Student Enrollment* (SE) dataset. To build this source, the Ministry of Education collects monthly enrollment reports containing all students in every school throughout Chile. The SE dataset provides information about a student’s gender, grade (year level), GPA, attendance, whether they pass or fail, school type, and school name. The dataset also contains school ID and student ID information that allows me to integrate all the datasets listed below with the SE dataset. I then eliminate all schools that are not public schools, as they are not affected by the reform (Section 3.2.3), and balance the panel, keeping all schools that were active between 2009 and 2014.

### 3.3.2 SIMCE Scores

To measure students’ learning outcomes, I use the Chilean national standardized test in education, SIMCE.<sup>13</sup> For this paper, I have used the results from SIMCE tests for Spanish and mathematics, as they are mandatory in every year.<sup>14</sup> This *SIMCE* dataset contains test scores for the two chosen subjects, student ID, and the school grade of every student who sat the SIMCE.<sup>15</sup> I merge the SIMCE scores with the SE dataset using the student ID, the year of the test, and the student’s school grade.

Given that some schools will start or stop offering certain grades throughout time, I have kept in the panel all schools in which the same school grades took the test between 2009 and 2014. For this purpose, I use test scores from students in fourth, eighth, and tenth grades, as these were the only grades to commence testing in 2009 or earlier.<sup>16</sup>

### 3.3.3 SIMCE Teacher’s Questionnaire

To measure school attributes, I use the SIMCE teacher’s questionnaire. The questionnaire contains answers from all teachers with a student sitting the SIMCE in a given year. The teachers fill in the questionnaire while the students take the test, and the results provide information about the school, its environment, and the particular teacher’s classes.

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<sup>13</sup> From its Spanish name, *Sistema de Medición de la Calidad de la Educación*.

<sup>14</sup> There are also tests for the subjects of social science, English, and natural science; however, students do not take these every year.

<sup>15</sup> This dataset does not contain any demographic variables for the students.

<sup>16</sup> A complete detailing of test schedules and timing can be found in Table B1 in the appendix.

The questionnaire covers topics such as school violence (for example, “How often have fights between students occurred this year?”), evaluation methods (“How often do you assign a test with open questions requiring a detailed response to evaluate students?”), and class organization (“How often do you ask students to give a presentation in front of the class?”), amongst others.<sup>17</sup> Aside from the gender of the respondent, the questionnaire gives no demographic variables. I merged the SIMCE teacher questionnaire with the previous dataset using the school ID, year, and the grade being taught.

I was able to build a balanced panel of teachers’ responses from 2009 onwards because, beyond that year, few questions were removed from the questionnaire.<sup>18</sup> Using Spanish and mathematics teachers’ answers for fourth, eighth and tenth grades, I built a homogeneous panel of teachers’ responses across years, grades, and schools.

### 3.3.4 School Employees

I use this dataset to measure the characteristics of school employees. This is a source collected by the Ministry of Education containing all school employees for every school in Chile. The information is collected in the middle of the academic year (July) for administrative purposes. Given the nature of mid-year data, if a new school employee was assigned to a school in July or earlier, I assigned them to that given year.

School staff data contains a comprehensive set of variables for school employees such as degree held, role in the school, working hours, and teaching hours. Using the detailed set of employee roles, I have classified school employees into three exclusive groups following the classification used in the Chilean school system: school principal, management team, and teachers.<sup>19</sup> This *School Employees* dataset was then combined with the previous datasets using school ID and year.

### 3.3.5 Call for Application

I use this dataset to identify the schools that renewed their principal under the reform conditions. The Civil Service provides this information, which contains the universe of all public calls to appoint school principals. Since the Civil Service

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<sup>17</sup> The complete set of questions used in this paper can be found in Appendix 3.A.

<sup>18</sup> During the early to mid-2000s, the set of questions was consistent, but in 2009 authorities updated the questionnaires, replacing the majority of the questions. Since 2009, the Ministry of Education has kept the questionnaire consistent, and the changes that have been made are generally questions being adding rather than removed.

<sup>19</sup> See Section 3.2.2.

supervises all principal selection processes, they register each call for applications at the moment it begins. This dataset provides the school ID, whether the application call was successful or cancelled, the date of the call, and the date on which the principal started working. I merge the *Call for Application* dataset with all previous datasets using the school ID.

### 3.3.6 Descriptive Statistics

**Table 3.1: Descriptive Statistics of Students**

	Sample	Post-Reform Principal	No Post-Reform Principal	p-value of equality of means test col. 2 & 3
	(1)	(2)	(3)	(4)
Female	0.500 (0.500)	0.516 (0.500)	0.489 (0.500)	0.000
Attendance	91.416 (10.838)	91.229 (8.491)	91.539 (12.131)	0.000
Pass Rate	0.976 (0.152)	0.977 (0.150)	0.976 (0.1530)	0.062
GPA	5.623 (0.586)	5.631 (0.585)	5.617 (0.586)	0.000
Observations	378360	149837	228523	

*Notes* Data taken from years prior to the reform (2009 to 2011). Each observation represents one student taking the SIMCE during this period. Column 1 shows data for students in the sample, column 2 for students in schools that instated a post-reform principal, and column 3 for students in schools that did not instate a post-reform principal. Column 1 is the sum total of columns 2 and 3. Column 4 shows the p-value for the mean difference test between columns 2 and 3. *Female* refers to the percentage of students who are female. *Attendance* refers to the average attendance of the students over the year. *Pass Rate* is the percentage of students who advanced to the next grade. The *GPA* scale is from 1 to 7 points. Standard errors are in parentheses.

Table 3.1 shows the descriptive statistics for data on students collected prior to the reform. The proportion of female students in the sample is 50%, and schools that would go on to instate a post-reform principal have around 2.3% more female students sitting the SIMCE than schools that did not. Attendance is around 91%, the pass rate is around 97%, and the GPA is around 5.6%. For these last three variables, the statistically significant differences shown in column 4 seem to be simply a result of the large number of observations, rather than reflecting some sort of selection in particular types of schools.

Table 3.2 shows descriptive statistics for data on schools collected prior to the reform. On average, schools in the sample group had around 390 enrolled students,<sup>20</sup>

<sup>20</sup> This is the total number of students enrolled in the school across all grades.



**Table 3.2: Descriptive Statistics of Schools**

	Sample	Post-Reform Principal	No Post-Reform Principal	p-value of equality of means test col. 2 & 3
	(1)	(2)	(3)	(4)
School Enrolment	391.112 (348.980)	454.632 (350.769)	358.676 (343.611)	0.000
SIMCE Takers	58.643 (65.497)	67.609 (66.165)	54.065 (64.685)	0.000
Principal Experience	31.743 (8.608)	32.098 (8.498)	31.562 (8.659)	0.030
Principal Age	56.386 (6.509)	56.882 (6.523)	56.133 (6.489)	0.000
Principal Contract Hours	43.899 (1.128)	43.876 (1.322)	43.911 (1.016)	0.319
Female Principal	0.382 (0.486)	0.409 (0.492)	0.368 (0.482)	0.004
Observations	5393	1823	3570	

*Notes* Data taken from years before the reform, from 2009 to 2011. Each observation is at the school level. Column 1 shows data for schools in the sample, column 2 for schools that instated a post-reform principal, and column 3 for schools that did not instate a post-reform principal. Column 1 is the sum total of columns 2 and 3. Column 4 shows the p-value for the mean difference test between columns 2 and 3. *School Enrollment* represents the number of students registered in the school. *SIMCE Takers* represents the combined number of students in fourth, eighth, and tenth grades who sat the SIMCE. *Principal Experience* represents the number of years that a principal has been in the educational system. *Principal Contract Hours* represents the number of contracted hours of work that a principal has in a given school. *Female Principal* represents the percentage of principals who are female. Standard errors are in parentheses.

and the number of students taking the SIMCE was around 58.<sup>21</sup> Columns 2 and 3 show that schools that would go on to instate a post-reform principal had a higher number of enrolled students across the whole school and a higher number of students taking the SIMCE than schools that did not.<sup>22</sup> I have no hypothesis as to why these particular schools are somehow bigger.

Principals of schools that instated post-reform principals had half a year more experience in the educational system. The size of this difference does not seem relevant when compared with the average number of years of experience (32 years). Finally, schools with post-reform principals had 4% more female principals than schools without. This may be simply an effect of timing, suggesting that the male-female division in the educational system, particularly in managerial positions, is shifting throughout the years.

<sup>21</sup> This refers only to students in fourth, eighth, and tenth grades who sat the test.

<sup>22</sup> In Section 3.4.5, I show that this pre-existing difference in school size is not driving the effects found throughout the paper (Section 3.4.5).

## 3.4 Results

### 3.4.1 Principal and School Employee Characteristics

To assess whether the reform changes the characteristics of the principal appointed, I perform a difference-in-differences estimation. The treatment group is made up of the public schools that appointed a principal between the beginning of the reform (April 2012) and 2014, and control group is made up of those that did not.<sup>23</sup> The dependent variable is one of the characteristics of the principals drawn from the *School Employees* dataset. The estimated equation is as follows:

$$\text{Principal Characteristics}_{i,s,t} = \delta + \alpha_s + \gamma_t + \beta * (\text{treated} * \text{post})_{s,t} + e_{i,s,t} \quad (3.1)$$

where  $(\text{treated} * \text{post})_{s,t}$  is a dummy variable equal to 1 for schools in the treatment group, starting from the year in which the post-reform principal is in office,  $\delta$  is the constant of the equation,  $\alpha_s$  is a school fixed effect, and  $\gamma_t$  is a year fixed effect. The coefficient of interest is  $\beta$ , which is the difference-in-differences estimator.

Results in Table 3.3 reveal that the reform brings in principals who have around 6.8 years less experience in the educational system (column 1) and are 4.5 years younger (column 2). One potential explanation of why post-reform principals are younger is that the reform actively searches for more motivated principals, and it is possible that younger, less-experienced principals are more motivated. Column 3 shows that post-reform principals are 13% more likely to hold a postgraduate degree. The 2.3-year gap in the point estimates between experience and age could therefore be the result of post-reform principals spending more time engaged in study, and thus joining the educational system at a later age.

Results in Table 3.3 are consistent with the government's goal of bringing more motivated and educated leaders into the public school system, and are likely to be the mechanical result of the points-based procedure used in the application and hiring process. These results are also consistent with governmental reports, which stated that under the reform, old principals would not be simply reappointed, and new principals would be enticed to join the system. According to the Civil Service, seven out of ten post-reform principals had not previously been principals in the school in which they were assigned (Servicio Civil, 2015).

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<sup>23</sup> The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years.

**Table 3.3: Effect of Reform on Principals' Characteristics**

VARIABLES	(1) Experience	(2) Age	(3) Postgraduate	(4) Degree in education	(5) Contract Hours	(6) Teaching Hours	(7) Female
Treated*Post	-6.876*** (0.619)	-4.533*** (0.446)	0.130*** (0.0238)	-0.00277 (0.00317)	0.0242 (0.0698)	-1.102 (0.682)	0.0102 (0.0248)
Constant	31.47*** (0.336)	56.10*** (0.256)	0.232*** (0.0134)	0.995*** (0.00139)	43.89*** (0.0324)	17.78*** (0.799)	0.375*** (0.0137)
Observations	10,886	10,886	10,886	10,886	10,886	10,886	10,886
R-squared	0.586	0.622	0.722	0.640	0.354	0.470	0.747
YEAR FE	YES	YES	YES	YES	YES	YES	YES
SCHOOL FE	YES	YES	YES	YES	YES	YES	YES

*Notes:* Each observation is at the school level. The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years. *Experience* represents the number of years that the principal has been working in the educational system. *Age* is the age of the principal in years. *Postgraduate* represents whether or not the principal holds a postgraduate degree. *Degree in Education* represents whether or not the principal holds a degree in education. *Contract Hours* represents the number of contracted hours worked by the principal. *Teaching Hours* represents the number of hours that the principal spends teaching classes in school. *Female* is a dummy variable representing whether or not the principal is female. Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

One might be tempted to think that the effects shown in Table 3.3 are a mechanical result of any principal renewal process, rather than being the result of the reform. To address this point, I have estimated Equation 3.1, but considering the two years of principal renewal processes directly prior to the reform (2010 and 2011). Table 3.4 shows that these renewals instated principals who had less than one year less experience (column 1) and were one year younger (column 2). The remaining effects are not significant. This piece of evidence suggests that the effects of a renewal on principals' characteristics prior to the reform were much smaller than those that took place afterwards.

**Table 3.4: Effects of Principal Renewal Pre-Reform on Principals' Characteristics**

VARIABLES	(1) Experience	(2) Age	(3) Postgraduate	(4) Degree in education	(5) Contract Hours	(6) Teaching Hours	(7) Female
Treated*Post	-0.813** (0.377)	-0.983*** (0.247)	0.0107 (0.0155)	-0.000183 (0.00341)	-0.0375 (0.0598)	-1.435 (1.039)	-0.00325 (0.0162)
Constant	31.79*** (0.333)	56.14*** (0.231)	0.215*** (0.0140)	0.996*** (0.000960)	43.91*** (0.0275)	19.72*** (0.953)	0.355*** (0.0150)
Observations	8,999	8,999	8,999	8,999	8,999	8,999	8,999
R-squared	0.665	0.689	0.804	0.680	0.518	0.595	0.773
YEAR FE	YES	YES	YES	YES	YES	YES	YES
SCHOOL FE	YES	YES	YES	YES	YES	YES	YES

*Notes:* Each observation is at the school level. *Experience* represents the number of years that the principal has been working in the educational system. *Age* is the age of the principal in years. *Postgraduate* represents whether or not the principal holds a postgraduate degree. *Degree in Education* represents whether or not the principal holds a degree in education. *Contract Hours* represents the number of contracted hours worked by the principal. *Teaching Hours* represents the number of hours that the principal spends teaching classes in school. *Female* is a dummy variable representing whether or not the principal is female. Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

To see whether changes in principal characteristics translate into changes in

school staff, I performed Equation 3.1, but using the school average of management team characteristics and teacher characteristics as dependent variables. Table 3.5 shows that after the reform, the management team has on average around two years less experience in the educational system (column 1), is 1.5 years younger (column 2), and is 4.8% more likely to hold a postgraduate degree (column 3).<sup>24</sup> The effects of the reform on the characteristics of principals and their management staff are similar, but less pronounced in the latter group. I expected these smaller effects, given the high rigidity of the management team within the school system. One further possible explanation for this correlation is that, when a position for management staff opens, principals are able to partially influence mayors to hire employees with similar characteristics to themselves (younger, less experienced, and more highly educated).

Results in Table 3.6 show that the experience (column 1) and age (column 2) of teachers diminishes by approximately one year. Despite their statistical significance, these effects are much smaller than for principals and their management team, thus reflecting the lack of flexibility in the hiring and firing of teachers.

The evidence discussed in this section suggests that the reform has brought about principals who are more educated, younger, and less experienced (and perhaps more strongly motivated). The reform has had similar albeit weaker effects on the management team, whilst for teachers there is almost no effect.

**Table 3.5: Effect of Post-Reform Principal on Management Team Characteristics**

VARIABLES	(1) Experience	(2) Age	(3) Postgraduate	(4) Degree in education	(5) Contract Hours	(6) Teaching Hours	(7) Female
Treated*Post	-2.140*** (0.487)	-1.423*** (0.371)	0.0480*** (0.0180)	0.00315 (0.00362)	0.243 (0.172)	-0.168 (0.667)	0.0125 (0.0166)
Constant	29.31*** (0.263)	54.32*** (0.210)	0.279*** (0.0100)	0.992*** (0.00193)	42.64*** (0.112)	19.77*** (0.609)	0.571*** (0.0114)
Observations	8,082	8,082	8,082	8,082	8,082	8,082	8,082
R-squared	0.622	0.647	0.784	0.536	0.625	0.517	0.721
YEAR FE	YES	YES	YES	YES	YES	YES	YES
SCHOOL FE	YES	YES	YES	YES	YES	YES	YES

*Notes:* Each observation is the average of the management team's characteristics in the school. The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years. *Experience* represents the average number of years that the management team has been employed in the educational system. *Age* is the average age of the management team in years. *Postgraduate* is the percentage of people in the management team that hold a postgraduate degree. *Degree in Education* is the percentage of people in the management team that hold a degree in education. *Contract Hours* represents the average number of contracted hours worked by the management team. *Teaching Hours* represents the average number hours that the management team spends teaching in the school. *Female* is a dummy variable representing whether or not the principal is female. Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

<sup>24</sup> The slight decrease in the number of observations for the management team is due to some schools having no management team.

**Table 3.6: Effect of Post-Reform Principal on Teacher Characteristics**

VARIABLES	(1) Experience	(2) Age	(3) Postgraduate	(4) Degree in education	(5) Contract Hours	(6) Teaching Hours	(7) Female
Treated*Post	-0.688*** (0.185)	-0.520*** (0.157)	0.0109 (0.00730)	-0.000310 (0.00242)	0.267** (0.125)	-0.136 (0.200)	0.00484 (0.00387)
Constant	20.22*** (0.150)	47.56*** (0.130)	0.247*** (0.00501)	0.944*** (0.00234)	34.32*** (0.107)	28.16*** (0.214)	0.680*** (0.00413)
Observations	10,886	10,886	10,886	10,886	10,886	10,886	10,886
R-squared	0.737	0.738	0.911	0.725	0.762	0.771	0.841
YEAR FE	YES	YES	YES	YES	YES	YES	YES
SCHOOL FE	YES	YES	YES	YES	YES	YES	YES

*Notes:* Each observation is the average of the teachers' characteristics in the school. The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years. *Experience* represents the average number of years that teachers have been employed in the educational system. *Age* is the average age of teachers in years. *Postgraduate* is percentage of teachers that hold a postgraduate degree. *Degree in Education* is the percentage of teachers that hold a degree in education. *Contract Hours* represents the average number of contracted hours worked by teachers. *Teaching Hours* represents the average number hours that the teachers spend teaching in the school. *Female* is a dummy variable representing whether or not the principal is female. Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### 3.4.2 School Environment and Teacher Practices

To estimate the ways in which post-reform principals have affected school climate, teacher-monitoring practices, and teachers' pedagogical practices, I have estimated the following difference-in-differences equation:

$$\text{School Attribute}_{s,t} = \delta + \alpha_s + \gamma_t + \beta * (\text{treated} * \text{post})_{s,t} + e_{s,t} \quad (3.2)$$

where  $\text{School Attribute}_{s,t}$  is the average of a particular characteristic of the school, provided by teachers through the SIMCE questionnaire, for school  $s$  in year  $t$ . The coefficient of interest is  $\beta$  which measures the effect of post-reform principals on these school characteristics.

Table 3.7 shows the ways in which schools change under the direction of post-reform principals. Column 1 shows that community engagement increases by  $0.07\sigma$  and column 2 shows that school violence decreases by  $0.10\sigma$ . These results suggest that the government was able to achieve its goal of introducing principals who will create a less conflictive and a more cooperative school climate.

Column 3 shows no effect of post-reform principals on teaching-monitoring practices. Furthermore, I do not observe any effect on class organization (column 4), evaluation methods (column 5), or teaching methods (column 6). These results suggest that post-reform principals are not implementing better monitoring practices, and therefore the teachers' pedagogical practices in the classroom are not changing. This implies that the reform fails in its goal of introducing principals who are better

able to monitor teacher and student progress.

**Table 3.7: Effect of Post-Reform Principals on School Climate, Monitoring Practices, and Pedagogical Methods**

VARIABLES	School Climate		(3)	Teacher's Pedagogical Methods			(7)
	(1)	(2)		(4)	(5)	(6)	
	Community Engagement	Violence	Monitoring Pract.	Class Organization	Ev. Methods	Teaching Methods	Female
Treated*Post	0.0648* (0.0386)	-0.106*** (0.0363)	0.0461 (0.0394)	0.0415 (0.0338)	0.0106 (0.0345)	0.00870 (0.0305)	-0.00307 (0.0293)
Constant	-0.0934 (0.108)	-0.951*** (0.111)	-0.182* (0.108)	-0.0913 (0.107)	0.213** (0.104)	0.0646 (0.0992)	0.257*** (0.0932)
Observations	9,881	9,896	9,919	9,488	9,470	9,461	9,971
R-squared	0.401	0.484	0.379	0.309	0.296	0.297	0.381
CONTROLS	YES	YES	YES	YES	YES	YES	YES
YEAR FE	YES	YES	YES	YES	YES	YES	YES
SCHOOL FE	YES	YES	YES	YES	YES	YES	YES

*Notes:* Each observation is a standardized measure of the average of the answers given by all teachers in a given school. The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years. Every outcome, excluding *Female*, is measured in standard deviations. *Community Engagement* includes questions such as “How strongly do you agree with the statement: The principal involves parents in school activities?”, *School Violence* includes questions such as “How often have fights between students occurred this year?”, *Teacher-Monitoring Practices* includes questions such as “How strongly do you agree with the statement: The principal often comes to observe classes that I teach?”, *Class Organization* includes questions such as “How often do you ask students to give a presentation in front of the class?”, *Evaluation Methods* includes questions such as “How often do you assign a school project to evaluate students?”, *Teaching Methods* includes questions such as “How often do you go over test answers during class?”. Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

A potential bias of the results in Table 3.7 is that the type of teachers who respond to the questionnaires changes when the post-reform principal is in office. As we observed in Table 3.6, there are some small differences in the observables characteristics of the teachers working under post-reform principals which suggest that the composition of teachers is not constant across time. If a different type of teacher answered the questionnaire when the post-reform principal is in office, the observed effects might be simply an artifact of the composition of the teachers answering the questionnaire, rather than actual changes in the school. There are two pieces of evidence that I can offer to counter this argument. First, changes in the composition of teachers in the school are relatively small (see Table 3.6). Second, the fact that there is no observed change in the gender of teachers answering the questionnaire suggests that the type of teachers answering the questionnaire does not change much (Table 3.7, column 7). However, as there may be other dimensions in which the pool of teachers in treated schools changes, I am not able to fully address this concern.

Overall, the results suggest that more highly educated and younger principals improve school climate but do not change the pedagogical practices implemented by teachers. The low accountability in the Chilean educational system may explain these results.

### 3.4.3 Test Scores

To evaluate the effect of post-reform principals on student test scores, I use the following difference-in-differences estimator:

$$\text{Test Score}_{i,g,s,t} = \delta + \Phi_g + \alpha_s + \gamma_t + \beta * (\text{treated} * \text{post})_{s,t} + \varphi * F_{i,g,s,t} + e_{i,g,s,t} \quad (3.3)$$

where  $\text{Test Score}_{i,g,s,t}$  is the standardized Spanish or mathematics test score for student  $i$ , in grade  $g$ , at school  $s$ , in year  $t$ ,  $\Phi_g$  is a grade fixed effect, and  $F_{i,g,s,t}$  controls for the gender of student  $i$ , in grade  $g$ , at school  $s$ , in year  $t$ . As before,  $\beta$  is the difference-in-differences estimator.

**Table 3.8: Effect of Post-Reform Principals on Test Scores**

VARIABLES	(1) Spanish	(2) Spanish	(3) Math	(4) Math
Treated*Post	-0.0155 (0.0115)	-0.0148 (0.0114)	-0.00896 (0.0117)	-0.00929 (0.0117)
Female		0.197*** (0.00341)		-0.121*** (0.00317)
Constant	-0.228*** (0.0127)	-0.324*** (0.0125)	-0.235*** (0.0115)	-0.176*** (0.0116)
Observations	596,383	596,383	599,196	599,196
R-squared	0.140	0.149	0.187	0.191
CONTROLS	NO	YES	NO	YES
YEAR FE	YES	YES	YES	YES
SCHOOL FE	YES	YES	YES	YES
GRADE FE	YES	YES	YES	YES

*Notes:* Each observation is at the student level. The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years. Spanish and mathematics test scores are measured in standard deviations. Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 3.8 shows the effect of the post-reform principals on test scores. This table shows no statistically significant results. Given the statistical power that this setting provides, I am able to rule out an effect larger than  $0.025\sigma$  on test scores.

One way in which the effects in Table 3.8 might bias is that post-reform principals could induce some change in student composition. To assess the plausibility of this hypothesis, I test whether the number of students in the school, the number of students that sat the SIMCE, and students' genders show some changes when the post-reform principal is in office. Results in Table 3.9 show no significant effect for any of these three variables. This suggests that the effects in Table 3.8 are not driven by changes in student characteristics. As a note, results for student composition are also consistent with the fact that there is no selection process in

place for students in public schools.

**Table 3.9: Effect of Post-Reform Principals on School Composition**

VARIABLES	(1) Female	(2) Enrollment	(3) Students Sitting SIMCE
Treated*Post	-0.00556 (0.00402)	-1.298 (3.871)	-1.278 (0.860)
Constant	0.486*** (0.00393)	410.1*** (3.066)	60.94*** (0.479)
Observations	10,886	10,886	10,886
R-squared	0.670	0.977	0.965
YEAR FE	YES	YES	YES
SCHOOL FE	YES	YES	YES

*Notes:* Each observation is at the school level. The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years. *Female* represents the percentage of female students amongst all students enrolled in the school. *Enrolled Students* represents the total number of students registered in the school. *Students Taking SIMCE* is the combined total number of students in fourth, eighth, and tenth grades who sat the SIMCE test. Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

#### 3.4.4 Leads and Lags

Difference-in-differences identifications can suffer from bias when the effect between the treated and control groups is driven by pre-existing differences in trends. To assess this, I compute leads and lags for schools that appointed a post-reform principal in the years 2013 and 2014,<sup>25</sup> using the following equation:

$$\text{Outcome}_{i,g,s,t} = \delta + \Phi g + \alpha_s + \gamma_t + \sum_{t=-4}^1 \beta_t * \text{treated}_s * \text{year}_t + \varphi * X_{i,g,s,t} + e_{i,g,s,t} \quad (3.4)$$

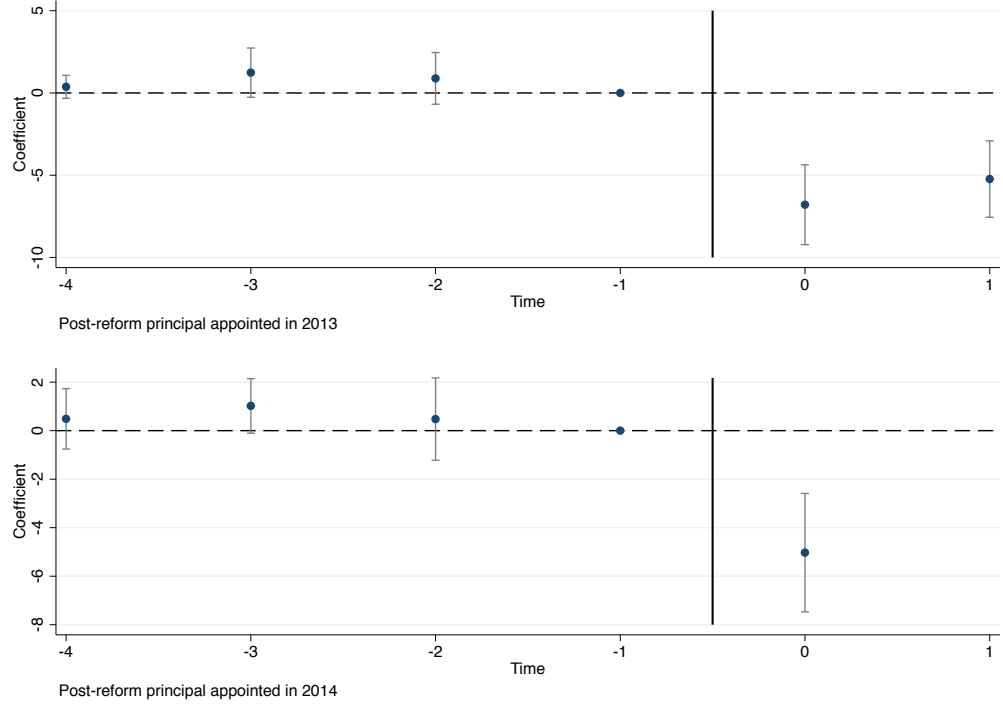
where  $\text{Outcome}_{i,g,s,t}$  is any of the outcome variables presented earlier, and  $\text{year}_t$  represents an indicator variable for each year covering the time period from 4 years prior to the post-reform principal being instated ( $t=-4$ ) to 1 year after their instatement ( $t=1$ ), with the year prior ( $t=-1$ ) being the excluded category.

Figures 3.1 and 3.2 show in their upper panels the dynamic effect for post-

<sup>25</sup> I perform the calculation for each cohort to avoid sample composition bias, and I exclude the cohort from 2012 given the small number of observations.



**Figure 3.1: Principal Experience**

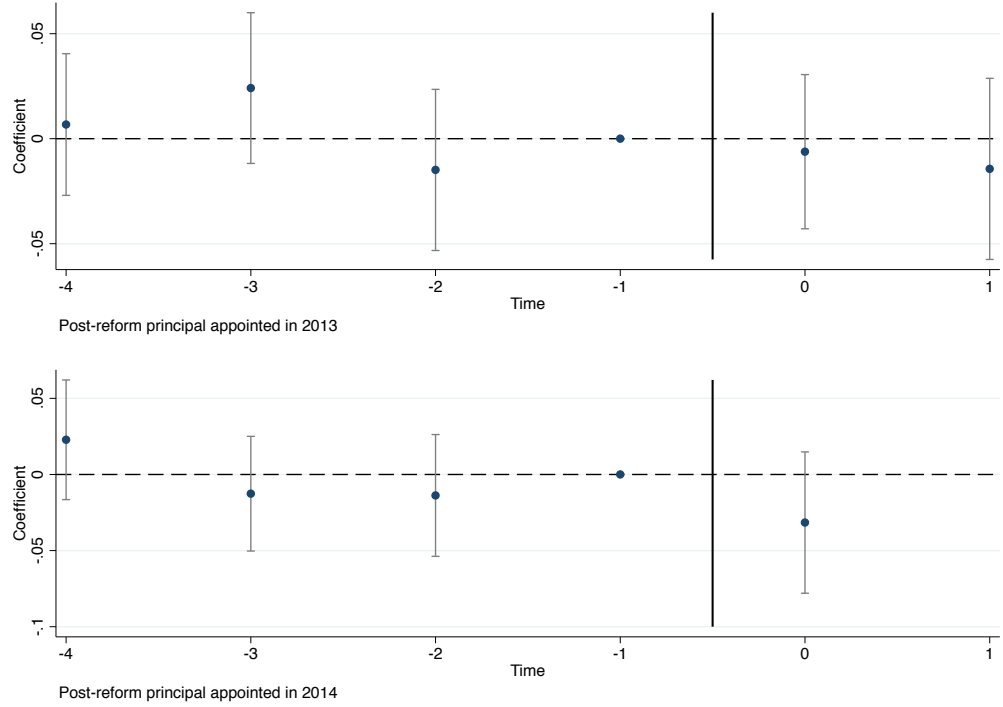


*Notes:* The top panel shows the lead and lag effects for the post-reform principals appointed in 2013, and the lower panel for those appointed in 2014. The excluded category is the year before the post-reform principal was instated in office. Dots represent a point estimate and the confidence interval is calculated at a 95% level.

reform principals in the 2013 cohort and in their lower panels the dynamic effect for the 2014 cohort. Figure 3.1 shows no pre-reform effect on principals' amount of experience, and a persistent significant negative effect after the reform for both cohorts. Figure 3.2 shows neither pre- nor post-reform effect for student test scores in Spanish for both cohorts. The evidence, overall, supports the assumptions underlying my difference-in-differences estimation strategy.

I further computed leads for all 29 outcomes presented earlier - outcomes presented in Tables 3.3, 3.5, 3.6, 3.7, and 3.8. These calculations resulted in 174 lead coefficients ( $29 \text{ outcomes} * 3 \text{ pre-reform periods} * 2 \text{ cohorts}$ ), of which 13 were significant (7.47%). When testing a null hypothesis at the 10% level, it is common to reject it in 10% of the cases, even when it is true. In this case, the percentage of significant leads that reject the null hypothesis is not greater than 10%, and so I do not consider this to be a major identification flaw.

**Figure 3.2: Spanish Test**



*Notes:* The top panel shows the lead and lag effects for the post-reform principals appointed in 2013, and the lower panel for those appointed in 2014. The excluded category is the year before the post-reform principal was instated in office. Dots represent a point estimate and the confidence interval is calculated at a 95% level.

### 3.4.5 Robustness Checks

In the final step of the analysis, I show that results hold when comparing schools of a similar size, and therefore are not explained because schools without post-reform principals are smaller on average (Section 3.3.6). To perform this analysis, I run the same regressions as before, but dropping the 10% of the smallest schools.<sup>26</sup>

Table 3.10 shows that when considering only the larger schools, post-reform principals are still younger, less experienced, and more educated. Table 3.11 shows that post-reform principals in these large schools are also able to decrease violence. The results for community engagement are no longer significant; however, the point estimate yields a result similar to the sample of all schools in the study. This suggests that the loss of significance in this outcome may be due a lack of statistical power

<sup>26</sup> These schools have around 150 students or less.

**Table 3.10: Robustness - Effect of Reform on Principals' Characteristics - Largest 90% of Schools**

VARIABLES	(1) Experience	(2) Age	(3) Specialization	(4) Educational Degree	(5) Contract Hours	(6) Teaching Hours	(7) Female
Treated*Post	-7.827*** (0.684)	-5.013*** (0.491)	0.139*** (0.0271)	-0.00463 (0.00343)	-0.0392 (0.0777)	-0.732 (0.786)	0.0279 (0.0284)
Constant	31.91*** (0.276)	56.60*** (0.198)	0.240*** (0.0127)	0.995*** (0.000589)	43.88*** (0.0342)	15.64*** (0.729)	0.395*** (0.0127)
Observations	7,258	7,258	7,258	7,258	7,258	7,258	7,258
R-squared	0.589	0.628	0.713	0.680	0.407	0.441	0.737
YEAR FE	YES	YES	YES	YES	YES	YES	YES
SCHOOL FE	YES	YES	YES	YES	YES	YES	YES

*Notes:* Each observation is at the school level. The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years. *Experience* represents the number of years that the principal has been working in the educational system. *Age* is the age of the principal in years. *Postgraduate* represents whether or not the principal holds a postgraduate degree. *Degree in Education* represents whether or not the principal holds a degree in education. *Contract Hours* represents the number of contracted hours worked by the principal. *Teaching Hours* represents the number of hours that the principal spends teaching classes in school. *Female* is a dummy variable representing whether or not the principal is female. Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

more than a lack of effect . This Table further shows that post-reform principals seem to do not monitor teachers more closely, and teachers seem to do not change their pedagogical practices. Finally, Table 3.12 suggest that post-reform principals in larger schools do not have an effect on students' test scores, and the minimum detectable size remains more or less the same when compared with the sample of all schools.

**Table 3.11: Robustness - Effect of Post-Reform Principals on School Climate, Monitoring Practices, and Pedagogical Methods - Largest 90% of Schools**

VARIABLES	School Climate		(3) Monitoring Pract.	Teacher's Pedagogical Methods			(7) Female
	(1) Community Engagement	(2) Violence		(4) Class Organization	(5) Ev. Methods	(6) Teaching Methods	
Treated*Post	0.0582 (0.0431)	-0.0940** (0.0417)	0.0332 (0.0419)	0.0335 (0.0374)	0.0327 (0.0378)	0.0168 (0.0336)	-0.0128 (0.0316)
Constant	-0.120 (0.127)	-1.180*** (0.135)	-0.107 (0.121)	-0.117 (0.125)	0.118 (0.117)	0.193* (0.106)	0.253** (0.101)
Observations	6,980	7,001	7,017	6,795	6,781	6,778	7,031
R-squared	0.413	0.473	0.396	0.296	0.279	0.299	0.388
CONTROLS	YES	YES	YES	YES	YES	YES	YES
YEAR FE	YES	YES	YES	YES	YES	YES	YES
SCHOOL FE	YES	YES	YES	YES	YES	YES	YES

*Notes:* Each observation is a standardized measure of the average of the answers given by all teachers in a given school. The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years. Every outcome, excluding *Female*, is measured in standard deviations. *Community Engagement* includes questions such as "How strongly do you agree with the statement: The principal involves parents in school activities?", *School Violence* includes questions such as "How often have fights between students occurred this year?", *Teacher-Monitoring Practices* includes questions such as "How strongly do you agree with the statement: The principal often comes to observe classes that I teach?", *Class Organization* includes questions such as "How often do you ask students to give a presentation in front of the class?", *Evaluation Methods* includes questions such as "How often do you assign a school project to evaluate students?", *Teaching Methods* includes questions such as "How often do you go over test answers during class?". Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 3.12: Robustness - Effect of Post-Reform Principals on Test Scores - Largest 90% of Schools**

VARIABLES	(1) Spanish	(2) Spanish	(3) Math	(4) Math
Treated*Post	-0.0124 (0.0121)	-0.0119 (0.0121)	-0.00580 (0.0123)	-0.00600 (0.0123)
Female		0.190*** (0.00357)		-0.127*** (0.00333)
Constant	-0.224*** (0.0135)	-0.317*** (0.0133)	-0.226*** (0.0121)	-0.164*** (0.0123)
Observations	550,173	550,173	552,999	552,999
R-squared	0.144	0.153	0.193	0.197
CONTROLS	NO	YES	NO	YES
YEAR FE	YES	YES	YES	YES
SCHOOL FE	YES	YES	YES	YES
GRADE FE	YES	YES	YES	YES

*Notes:* Each observation is at the student level. The average length of time that a post-reform principal has been in their position during the time of the study is 1.65 years. Spanish and mathematics test scores are measured in standard deviations. Standard errors are clustered at the school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### 3.5 Concluding Remarks

School principals are typically viewed as an important factor in the educational production function, as they are considered to impact students' learning through their influence on the school climate and teachers' pedagogical methods. By exploiting a reform that changes the characteristics of school principals, this paper has attempted to shed some light on the ways in which a younger, less experienced, and more educated principal can exert an influence on different aspects of their school.

Following the reform targeting the appointment of school principals in Chile, post-reform principals engage more with the school community and reduce the level of school violence. However, post-reform principals do not seem to be able to improve teacher-monitoring practices, teachers' approaches to education and students' test scores. This result may be explained by a lack of incentives to perform better in this school setting, which perhaps counteracts the potential strong positive influence of principals on the educational production function.

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## **3.A Appendix: Teacher Questionnaire**

### **3.A.1 Community Engagement**

**How strongly do you agree with the following statements?**

1. The principal creates situations where teachers, parents, and students can interact.
2. The principal involves parents in school activities.
3. The principal keeps the community informed about the schools academic achievements.
4. The principal regularly informs the community about his/her achievements as a principal.

### **3.A.2 School Violence**

**How often have the following situations occurred this year?**

1. Theft
2. Malicious gossip or exclusion of students.
3. Fights between students.
4. Insults between students.
5. Threats between students.
6. Aggressive incidents involving weapons.
7. Aggressive incidents involving guns being fired.

### **3.A.3 Teacher-Monitoring Practices**

**How strongly do you agree with the following statements?**

1. The principal evaluates teacher performance in terms of students academic progress.
2. The principal gives suitable feedback after evaluating teachers.
3. The principal systematically monitors teachers.
4. The principal often comes to observe the class that I teach.

### 3.A.4 Class Organization

**How often do you do the following during class?**

1. Ask students to work in teams.
2. Ask students to work individually.
3. Ask students to give a presentation in front of the class.
4. Ask students to debate different topics.
5. Go outside the classroom to cover a particular topic.

### 3.A.5 Evaluation Methods

**How often do you do the following when evaluating the students?**

1. Assign a test with true and false questions.
2. Assign a test with open questions requiring a detailed response.
3. Assign a school project.
4. Self-evaluations.
5. Graded homework.

### 3.A.6 Teaching Methods

**How often do you do the following?**

1. Check whether or not students completed their homework.
2. Go over homework answers during class.
3. Re-explain content when not fully understood.
4. Explain until all students fully understand the content.
5. Go over test answers during class.



## 3.B Appendix: Tables

**Table B1: Test Calendar**

Grade	Subject	2007	2008	2009	2010	2011	2012	2013	2014	2015
2nd	Reading						X	X	X	X
4th	Reading	X	X	X	X	X	X	X	X	X
	Math	X	X	X	X	X	X	X	X	X
6th	Reading							X	X	X
	Math							X	X	X
8th	Reading	X		X		X		X	X	X
	Math	X		X		X		X	X	X
10th	Reading		X		X		X	X	X	X
	Math		X		X		X	X	X	X

The X represents years in which the exam take place for the corresponding grade and subject. Second grade mathematics is not shown as the test does not take place in any year.

## Chapter 4

# Discussion and Recommendations

## 4.1 Chapter 1

Chapter 1 studied the effects of homeownership on labor supply. The results show that the program studied seems not to affect the labor supply of heads of households nor of their spouses (i.e. the adults of the household), but does decrease the labor supply of their children. These results seem to imply that those who have already decided whether or not they will join the labor market remain unaffected, while those who are yet to make a decision at the time of receiving assistance are affected.

Several conclusions could be drawn from these results. First, that the incentives presented to heads of households and their spouses are not large enough to alter their labor market preferences. A potential explanation for this result is that, even though the program transfers 10 years worth of wages in the form of a housing asset to this group, new homeowners cannot effectively perform *consumption smoothing* by selling, renting, or using this asset as collateral over the period of time of the study. A behavior worthy of study is what voucher recipients do in terms of their living arrangements and credit constraints after their five-year restrictive period has expired. I hope to address this question in future research. Second, the increase in income that adults face by no longer paying rent is not high enough to change their decisions regarding their labor supply.<sup>1</sup>

Children of working age in families who used the voucher joined the labor market in lower proportions than their non-voucher recipient peers. On average, this group was in their final two years of high school at the time of application, and thus had been out of high school for two years at the end point of our study. A potential explanation for this effect is that low-income families tend to see working-age children as an additional source of income, but as families see an increase in income by not paying rent, they tend to put less pressure on their children to join the labor market. Building on this, these children who do not join the labor market could use their time in two different ways: 1) increased leisure consumption, in which case their unemployment should be seen as a luxury good; or 2) enroll in a tertiary educational institution, in which case the families would be investing in their children's human capital. Unfortunately, data availability does not allow us to disentangle these two mechanisms, and I hope that in future research I will be able to discern which of these, if any, is the more relevant alternative.

These alternative uses of children's time could potentially lead to divergent

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<sup>1</sup> For an average applicant, rent represents around 30 percent of their income, however this may not be big enough to make them stop working.

policy recommendations. On the one hand, if children are effectively increasing their leisure consumption, an incentivization for joining the labor market or studying could be achieved by making the non-repayable nature of the voucher conditional on having all working-age family members studying, job hunting, or working for a certain period of time.<sup>2</sup> We do not know the potential jobs available to these people, and so any recommendations along these lines must take this into account. On the other hand, if these children are enrolling in tertiary education in a higher proportion, this program could represent an opportunity for higher long-term productivity. In this case, the potential effect of this program on degree completion and income paths for this group becomes relevant for future research.

## 4.2 Chapter 2

In this chapter, I showed that the homeownership assistance provided by the Chilean government does not affect students' test scores for those who are in middle and high school, but decreases the test scores of those in primary school. Furthermore, the effect seems to vary with age, as it becomes stronger for younger students. This result seems to be driven by a sharp drop in the probability that a homeowner family under this program lives in the house of a close relative (e.g. grandparents), and the consequential reduction in supplementary care provided by those close relatives.<sup>3</sup>

One recommendation to this program could be that those families who obtain the voucher receive a priority seat in public childcare facilities, and thus try to offset the loss in care provided by close relatives. Complementary childcare assistance must be carefully considered as it could potentially have a displacement effect: the number of places in public childcare is usually not enough to cover the whole population, and so accepting one child implies leaving another one out. Another potential change worth exploring could be increasing the amount of time permitted to action the voucher for families living in the house of a close relative and with young children.<sup>4</sup> In this case, families could have the option of waiting until their children are older

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<sup>2</sup> Alternatively, it could be the case that children increasing their leisure consumption in these voucher recipient families are in fact willing to enroll in further tertiary education, but the family's income shock is not high enough to cover educational fees. I do not believe that this a mechanism at play in this context, as families in these income groups are able to access generous scholarships and educational credits provided by the government.

<sup>3</sup> Recall that around 50 percent of families in the control group live in another family's house, usually that of a close relative, and new homeowner families mechanically move to live on their own.

<sup>4</sup> Currently all families have a two-year window in which to use the voucher, otherwise it expires permanently.

before moving to their new house, and so the drive towards supplementary care would become less strong. It is worth noting that this program actively incentivizes families with young children to receive a voucher by assigning a higher score to those families. In light of the observed effects of voucher use - a decrease in the learning of young children but with a small fraction of families living in a house below minimum living standards - it is not clear whether this is a worthwhile incentive to put in place in this program.

Overall, this chapter provides evidence of this program having no positive effects on students' learning, rather showing that young students tend to be negatively affected. More thought must thus be put in the design of this program in order to offset the potential loss in supplementary care that young students receive.

### 4.3 Chapter 3

In the final chapter, I considered the effects of a change in the way in which the Chilean authorities appoint school principals. The reform granted less discretion to authorities, by selecting principals through implementing the system used for civil servants. Data availability allows me to observe "post-reform principals" 1.65 years after they have been in their position. I observed that these principals are on average younger and more educated, and improved their school climate by reducing school violence and improving community engagement, but were not able to improve teacher practices in the classroom or increase student test scores.

First, improving the school climate is particularly relevant in the context of Chilean education. Over the past 15 years, the proportion of children enrolled in public schools dropped from 58 to 38 percent. The Ministry of Education provides anecdotal evidence showing that school violence is more predominant in public schools, and that school safety is the most relevant criteria for parents deciding where to enroll their children. Although I have not observed a discernable effect on school enrollment, this effect may arise in the long run. Moreover, the potential complementarities of improvements in school climate and student learning could take a longer time to become evident. I expect to address this in future research.

One explanation for the lack of improvement in teacher practices is the rigidity of the institutional system. In Chile, principals have essentially no say on which teachers are hired, and current teachers are near-impossible to fire or incentivize. It could be that, although these principals are more highly educated and presumably more motivated, they cannot bring teachers who share their values and attitude to the school. Moreover, current teachers are aware that their performance will have no

impact on the probability of their contract renewal or their salary, which might put some constraints on the ways in which principals could better incentivize teachers' classroom behavior.

One potential future consideration for authorities is to change the way in which teachers are appointed in Chilean public schools. Currently, teachers are directly appointed by the mayor, with no supervision; therefore, new teachers do not necessarily share the same vision as the principal. A system in which new teachers are appointed in a similar way as principals - through a public offer, where more qualified professionals are more likely to be appointed, with the principal then selecting the successful candidate from a shortlist - could increase teacher quality, and align teachers and principals in their vision for a school. This could reduce the friction between principals and teacher practices and potentially, in the long run, foster improvements in teachers' classroom practices and students' learning.