Assessing the impact of the *CajaVecina* program on SME owner use of financial services in Chile: a natural experiment approach

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Abstracto

En este estudio se analizó el impacto del programa CajaVecina en los microempresarios clientes de BancoEstado que viven en áreas sin presencia física del banco en Chile. Este programa permite que el banco tenga acceso a clientes que viven en zonas aisladas, permitiendo a los clientes a acceder a diversos servicios financieros sin tener que acudir a una sucursal.

Si esta iniciativa es percibida efectivamente como una sustituto cercano a la sucursal y la restricción geográfica es activa, i.e., previene que clientes actuales y potenciales usen ciertos servicios financieros, uno podría esperar que el programa provocara un incremento en el uso de productos financieros ofrecidos por el banco. Sin embargo, la evidencia sugiere lo contrario.

El resultado relevante de este estudio es que el programa no tuvo, en promedio, ningún impacto sobre las distintas medidas de intensidad de uso de servicios financieros. La explicación detrás de este fenómeno no es clara y se requiere más investigación con el fin de entender mejor el impacto del programa.

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Juan Ignacio Elorrieta Maira†

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Abstract

In this study, I analyze how the *CajaVecina* program impacts small and medium enterprises (SME) owners that live in counties without a bank branch in Chile. This program allows the bank to easily reach clients that live in geographically isolated areas by using a remote point of sale technology.

If this initiative is actually perceived as a branch substitute and the distance to the nearest branch is indeed a constraint preventing current and potential clients from using financial services, one could expect that the program would produce an increase in the use of the financial products provided by the bank.

The main finding of this study is that the *CajaVecina* program did not have a significant effect on expanding the use of financial services. The explanation behind this result is not clear and further research is required to understand the impact of the program.

I Introduction

The discussion of whether access to financial markets in rural areas may be a solution to reduce poverty is present in a large body of literature (see Burgess and Pande (2005)). The authors present evidence that suggests the presence of a bank branch in rural areas has helped to reduce poverty among the population that has access to it.

The possible mechanisms on how this may occur are described in several studies. As explained by Burgess and Pande (2005), authors such as Banerjee et al. (2000), Aghion and Bolton (1997) and Levine (1997) provide a theoretical model to show that access to financial markets can induce

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changes in resource allocation, altering production and employment decisions and helping to reduce poverty in the process.

Meanwhile, *BancoEstado*, a Chilean state-owned bank, decided to implement the *CajaVecina* program. This program consists of installing a remote point of sale system (POS) in local stores owned by their clients. This allows local clients to use all the financial services provided by the bank, except for issuing loans. According to the theory, this program could alter the financial behavior of the clients who have the *CajaVecina* program in their counties. The potential benefits of this program are multiple and not solely limited to providing access to the bank's financial services in isolated areas. However, for the purpose of this study, I focus only on this potential benefit.

At first, the bank decided to start a pilot program to test for potential difficulties before expanding the program nationally. The pilot program was launched in 104 counties were no bank branch was present. The financial constraint imposed by the geographical location of the clients was not completely binding for every resident of these counties. The bank had current clients living in these counties and one can expected that by installing *CajaVecina* two things might have happened: (1) new clients will join the bank due to the presence of *CajaVecina* (also known as additionality type I) and (2) existing clients will increase the use of financial services provided by the bank in their county (also known as additionality type II). The key assumption under these claims is that the geographical constrain is binding and is keeping existing clients from using financial services and potential clients from joining the bank. If this assumption is correct, then a program like *CajaVecina* should improve SME owner's access to financial markets.

In this study, I undertake a natural experiment approach for assessing the impact that the *CajaVecina* program had on the use of financial services. Before starting the program, *BancoEstado* decided to install *CajaVecina* in these 104 counties as a trial stage. During this pilot stage, the program was implemented sequentially. This allowed me to identify the installation of *CajaVecina* as a random treatment applied to certain counties. Since the randomization occurred at a county level, one can argue that this may be a source of bias since there might be client characteristics that are correlated with geographic location. I address this issue by testing if the control and treatment groups were statistically different across relevant client's characteristics. I provide evidence that this is not the case, providing evidence that the control and treatment group are balanced and thus making them suitable for the analysis.

I use a difference-in-difference approach for assessing the impact of the *CajaVecina* program. This difference-in-difference strategy allows me to control for the initial situation and the entrepreneurial skill of each client. This identification strategy yields a more robust estimate of the effect of the program than the one obtained from a single difference estimation.

The main finding of this study is that the *CajaVecina* program did not had a significant effect on the use of financial services, nor did it expand the number of new clients. This suggests that, if *CajaVecina* is perceived by the clients as a substitute of the branch, the geographical constraint is

not binding enough to influence, on average, the financial decisions of the SME owners regarding *BancoEstado*'s services.

This result by no means rules out the effectiveness of the program. This program has possible effects that require further research in order to have a more complete idea of the success of the program. Questions such as, did *CajaVecina* help reduce the number of clients using the branch for simple transactions? If so, can the program help reduce the operational costs of a branch? Did the program benefit the store owner in some aspect? remain unanswered and they need to be approached in order to have a broader understanding off the success of the *CajaVecina* initiative.

The study is organized as follows. In section (II) I describe how this program works in a more detailed fashion; in section (III) I elaborate on the identification strategy and present the results and in section (IV) I conclude.

II The *CajaVecina* program

The *CajaVecina* program uses an existing store, owned by a client of the bank, to provide financial services to clients living nearby. The financial services include paying loan installments, depositing money in checking and savings accounts, paying credit card bills, withdrawing money, paying utility bills and checking the balance on all types of accounts.

This is made possible by a tech-gadget known as a remote POS (Point of Sale), that allows the owner of the store to access the bank information system using a phone land line or an internet antenna, to record all kinds of financial transactions. The technology is very straightforward; it takes a week to install and to train the store owner on the use of the system.

BancoEstado decided to implement this program because, allegedly, it would allow them to reach new clients located in remote geographical areas where it would not be profitable to install a bank branch. Also, it would allow them to reduce the waiting time in the branches by providing an alternative where clients can do all their financial transactions in a place nearer than the local branch. Moreover, this programs can create a business alliance between the store owner and the bank. With *CajaVecina*, the store owner is able to add a new service to his store and the bank is able to provide better service to its clients.

The key feature of this program is that it aggregates all the transactions of a geographic area, reducing the waiting time for the clients, and facilitating the task for the bank of providing services to their clients at the nearest branch.

III Empirical analysis

CajaVecina began as a pilot program intended to identify difficulties before expanding the program nationally. At this particular stage, the objective was to install one *CajaVecina* in each of the 104 counties in Chile that do not have any bank branches in their territory. *BancoEstado* committed

to install a *CajaVecina* in each county, regardless of the inherent business characteristics of the county, because as a state-owned bank, one of its goals is to provide all Chilean citizens access to financial markets. This program was viewed by the bank as a possibility of providing an easier access to financial markets to their potential and existing clients.

Since this stage was viewed as a learning process, the installation was performed sequentially. This would give the *CajaVecina* team the chance to address potential problems without compromising the entire project. In Table 1, we can see that the *CajaVecina* team started by installing one POS per month and continued to increase the number of POS installed per month, until reaching a maximum of 19 per month.

In my analysis, I use the fact that the first stage of the program was implemented sequentially and that there were no business interests that lead *BancoEstado* to choose one county over the other. I argue that the decision of implementing *CajaVecina* and the order of implementation was exogenous, therefore, I can undertake an experimental approach to evaluate the impact of installing a *CajaVecina* on a county with no banks in its territory.

In particular, I will analyze the impact over the following variables :

1. Number of clients per county.
2. Number of financial services being used (credit cards, cash-in-advance lines, etc.)
3. Number of clients borrowing money from *BancoEstado Microempresas* and the rest of the financial system.
4. Number of clients paying late the installments of their loans from *BancoEstado Microempresas* and the rest of the financial system.

Variable number 1 allows us to analyze additionality type I, i.e., the effect of the program on encouraging new clients to join the bank. Variables 2 to 4 allow us to analyze additionality type II, that is, the effect of the program on the use of financial services by current clients.

III.i The data

In a best-case-scenario, we will have a sample where half of the counties received the treatment at the same time, while the other half has not. Also, the un-treated counties would not receive the treatment for a reasonable amount of time, allowing us to assess the impact of the program by comparing it with the treated counties. Unfortunately, given that the program was implemented sequentially, this is not the case and a different approach is required to build an adequate sample.

I started by looking at how many *CajaVecinas* were installed by month (see Table A1) and I decided to focus my analysis to October 2006 to December 2006 to obtain data on the counties prior to the treatment. I selected this time period because it was during these months that the three largest amounts of *CajaVecina*s were installed. This allows me to have a treatment group as

representative as possible. I then narrow my analysis to only one month to control for potential seasonal effects. The idea behind this is that the financial situation from October can be different from the one in November due to seasonal reasons.

After focusing on Oct-06 to Dec-06, I add the data of the clients after the treatment. I decided that 11 months after the treatment seemed reasonable. However, the data for Sep-07 and Oct-07 were not available, so I used the Nov-07 data for the analysis. Then, I constructed three different sample groups: Sep-06 to Nov-06, Oct-06 to Nov-07; and Nov-06 to Nov-07.[1]

In addition, I limited the sample to those clients who live in the same region as the ones who received the treatment. This is because the geographic location of one client is correlated with the control variables, particularly with business sector, age and formal education. Therefore, by considering only the same regions, we can prevent this source of bias.

III.i.i Choosing a balanced sub-sample

I needed a criterion for choosing one sample over the other. I decided to choose the one sub-sample that allowed me to have a control and treated group that were not statistically different from one another. In simple, I needed the control and treatment group to be balanced.

First, I chose the control variables following Valenzuela and Venegas (2001) results. The authors argue that SME owners from Chile are heterogeneous across variables such as: age, gender and formal education. Also, they provide evidence showing that business activities are also quite different across sectors, and that they have different business cycles. Therefore, I chose age, gender, formal education, marital status, business sector and years in business as control variables.

Then, I proceeded to compute a confidence interval for each control variable. The problem with this is that the control variables do not appear to behave like a normal distribution, making the standard procedure not as robust as we may want to. Therefore, I obtained the confidence intervals using a bootstrap technique, which yields correct confidence intervals regardless of the distribution being not normal. As we can see in Table 1 the control and treatment group are no different at a 95 percent confidence level.[2] [3]

[1] The data for constructing this panel was obtained directly from *BancoEstado Microempresas* databases.

[2] I used 10,000 iterations for the calculation of the confidence intervals via *bootstrap*

[3] A graph of the distribution of these variables can be found in Figure A1.

TABLE 1: Are the control and treated group statistically not different?

	Treatment Counties ($N_{\text{counties}} = 7$)		Control Counties ($N_{\text{counties}} = 12$)	
	Lower bound	Upper bound	Lower bound	Upper bound
Gender	0.53	0.68	0.60	0.67
Age	46.72	49.96	49.75	51.50
Marital Status	1.76	1.92	1.78	1.85
Formal education	1.51	1.80	1.44	1.57
Business sector	2.69	3.29	2.79	3.12
Years in business	8.34	9.35	8.72	9.21

The reason for comparing the control variables of both groups is due to the "pseudo randomization" method. This randomization occurred at a county level and we may argue that there are several business characteristics correlated with geographical allocation. I argue that the reasonable number of counties in the treated group; along with the fact that I limited the sample to the regions where the treated counties are located, allows me to have a proper control and treatment group. This claim can be tested, and the evidence that both groups are statistically not different is provided in Table 1.

III.i.ii The model: average treatment effect[4]

In order to assess the impact of the *CajaVecina* program, we need to come up with a way of estimating the following expression

$$E[(Y_i|T) - (Y_i|NT)]$$

Where $E[\cdot]$ corresponds to the expected value, Y_i the variable of interest of individual i, and T if that individual received the treatment. The problem with this expression is that is impossible to know the state of the same individual with and without the treatment at the same time. In reality, we observe

$$E[Y_i^T|T] - E[Y_i|NT]$$

In this scenario, we can not know if the difference is due to the treatment or due to inherent characteristics of the individuals. This problem can be expressed by rearranging the latter expression

$$E[(Y_i^T - Y_i^{NT})|T] + E[Y_i^{NT}|T] - E[Y_i^{NT}|NT]$$

Where the term $E[Y_i^{NT}|T] - E[Y_i^{NT}|NT]$ represents the selection bias. Since the pilot stage of the program can be interpreted as a natural experiment, I claim that both groups are on average

[4] Based on Martínez (2007)

statistically not different, therefore the selection bias term is equal to zero. Therefore, I can compare the averages of the control and treatment group in order to obtain an estimate of the average treatment effect.

Given that I have panel data from two periods, I undertake a difference-in-difference approach to estimate the average treatment effect. This approach is ideal for analyzing the actual change in the variable that is being studied, making the results more accurate. This can be expressed as

$$\text{dif-in-dif} = (E[Y_{t_1}|T] - E[Y_{t_0}|T]) - (E[Y_{t_1}|NT] - E[Y_{t_0}|NT])$$

To compute this coefficient, one can simply calculate the averages of the treatment and control group, before and after the treatment. However, one can undertake an ordinary least squares (OLS) approach in order to have a more robust estimation by adding control variables. This yields the following linear equation:

$$\begin{aligned} Y_{ist} = \quad & \beta_1 \cdot \text{time dummy}_{ist} + \\ & \beta_2 \cdot \text{treatment dummy}_{ist} + \\ & \beta_3 \cdot \text{time dummy} \cdot \text{treatment dummy}_{ist} + \\ & \alpha \cdot \text{county fixed effects}_{ist} + \delta \cdot \text{controls}_{ist} + \epsilon_{ist} \end{aligned} \tag{1}$$

Where

$$\text{time dummy}_{ist} = \begin{cases} 1 & \text{if the observation belongs to after the treatment} \\ 0 & \text{otherwise} \end{cases}$$

$$\text{treatment dummy}_{ist} = \begin{cases} 1 & \text{if the client received the treatment} \\ 0 & \text{otherwise} \end{cases}$$

And our average treatment effect coefficient is

$$\beta_3 = (\bar{Y}_{t_1}^T - \bar{Y}_{t_0}^T) - (\bar{Y}_{t_1}^{NT} - \bar{Y}_{t_0}^{NT})$$

Also, some of the dependant variables, Y_{ist}, have a binary form. In these cases, I also used a probit

model with the same linear specification, that is

$$\begin{aligned} Y_{ist} = \quad & \Phi(\beta_1 \cdot \text{time dummy}_{ist} + \\ & \beta_2 \cdot \text{treatment dummy}_{ist} + \\ & \beta_3 \cdot \text{time dummy} \cdot \text{treatment dummy}_{ist} + \\ & \alpha \cdot \text{county fixed effects}_{ist} + \delta \cdot \text{controls}_{ist} + \epsilon_{ist}) \end{aligned} \tag{2}$$

III.ii Results

My results indicate that the hypothesis that the *CajaVecina* program did not have an effect different than zero on the analyzed variables can not be rejected. For estimating the effect of the program, I computed the average treatment effect using a differences-in-differences approach, as described in section (III.i.ii). I present the results in two groups: effects on additionality type I and additionality type II.

III.ii.i Additionality type I

The first result suggests that *CajaVecina* did not have an effect on BancoEsado Microempresas number of clients. To determine this, I computed the average number of clients per county for the treatment and control group before and after the program was implemented. Then, I obtained the difference between the average number of clients before and after *CajaVecina* was installed. Finally, I calculated the difference-in-difference coefficient. The estimated coefficient is positive, but not statistically different from zero. (see Table 2)

TABLE 2: Did *CajaVecina* cause more clients joining the bank?

	Average number of clients per county		
	Sep-06	Nov-07	Difference
Treatment counties	27.29	27.29	0.00
	(2.55)	(2.64)	(2.28)
Control counties	60.67	60.42	-0.25
	(3.26)	(3.16)	(2.53)
		Dif-in-Dif	**0.25**
			(2.19)

III.ii.ii Additionality type II

My second finding suggests that *CajaVecina* did not have a statistically significant effect on the number of financial services used by clients (see Table 3). Moreover, according to the calculations, the effect was actually close to zero.

TABLE 3: Did *CajaVecina* have an effect on the number of financial services used?

	Average number of products used per county		
	Sep-06	Nov-07	Difference
Treatment counties	1.49	1.32	-0.17
	(0.15)	(0.14)	(0.10)
Control counties	1.30	1.12	-0.18
	(0.00)	(0.00)	(0.05)
		Dif-in-Dif	**0.02**
			(0.39)

When analyzing the effect of the program by each financial service, the results suggest that *CajaVecina* did not have a significative effect on the use of any of these products. In fact, all the estimated average treatment effects are close to zero and not statistically significant at even a 90 percent confidence level.

TABLE 4: Did *CajaVecina* have an effect on the use of financial services?

Checking account			
	Sep-06	Nov-07	Difference
Treatment counties	0.14	0.13	-0.01
	(0.05)	(0.05)	(0.06)
Control counties	0.07	0.07	0.00
	(0.00)	(0.00)	(0.03)
		Dif-in-Dif	**-0.01**
			(0.29)
Credit line			
Treatment counties	0.15	0.15	0.01
	(0.05)	(0.05)	(0.06)
Control counties	0.08	0.08	0.00
	(0.00)	(0.00)	(0.03)
		Dif-in-Dif	**0.00**
			(0.30)
Electronic checkbook			
Treatment counties	0.24	0.15	-0.09
	(0.06)	(0.05)	(0.06)
Control counties	0.23	0.12	-0.12
	(0.00)	(0.00)	(0.03)
		Dif-in-Dif	**0.02**

			(0.31)
Savings account			
Treatment counties	0.80	0.72	-0.07
	(0.06)	(0.06)	(0.07)
Control counties	0.74	0.67	-0.06
	(0.00)	(0.00)	(0.04)
		Dif-in-Dif	**-0.01**
			(0.32)
Credit card			
Treatment counties	0.17	0.18	0.01
	(0.05)	(0.05)	(0.06)
Control counties	0.18	0.17	0.00
	(0.00)	(0.00)	(0.03)
		Dif-in-Dif	**0.01**
			(0.31)

Finally, the program does not seem to have an effect on the number of clients taking up commercial loans or on the number of clients paying late on their outstanding debt installments. Again, the estimated coefficients are statistically not significant. Also, three out of the four estimated coefficients are negative, contradicting the hypothesis.

TABLE 5: Did *CajaVecina* have an effect on the loan take-up or late payments?

Debt with *BancoEstado*			
	Sep-06	Nov-07	Difference
Treatment counties	0.38	0.42	0.05
	(0.07)	(0.07)	(0.07)
Control counties	0.35	0.35	0.00
	(0.00)	(0.00)	(0.04)
		Dif-in-Dif	**0.04**
			(0.33)
Debt with rest of the financial system			
Treatment counties	0.57	0.61	0.04
	(0.07)	(0.07)	(0.07)
Control counties	0.51	0.53	0.02
	(0.00)	(0.00)	(0.04)

		Dif-in-Dif	**0.02**
			(0.33)
Past-due debt with *BancoEstado*			
Treatment counties	0.02	0.04	0.02
	(0.02)	(0.03)	(0.04)
Control counties	0.03	0.03	0.00
	(0.00)	(0.00)	(0.02)
		Dif-in-Dif	**0.02**
			(0.25)
Past-due debt with rest of the financial system			
Treatment counties	0.04	0.03	-0.01
	(0.03)	(0.03)	(0.04)
Control counties	0.05	0.03	-0.01
	(0.00)	(0.00)	(0.02)
		Dif-in-Dif	**0.01**
			(0.26)

III.iii Robustness checks

In order to check if the average treatment effect coefficients estimated in the previous section are robust, I compute both linear equation and the probit model. These specifications control for individuals pre-treatment characteristics and for county effects that might be biasing the previous results.

First, in Table 6 I present the results obtained from the linear model (equation 1) estimation using an Ordinary Least Squares technique. I allow the software to perform a White's adjusted heteroscedastic consistent least-squares regression if necessary. This estimation yields a less than or equal difference-in-difference coefficient than the one presented in the results section, after controlling for individual's characteristics and county fixed effects. Also, the estimated average treatment effect remains statistically not significative, even at a 90 percent confidence level.

TABLE 6: Did *CajaVecina* have an effect on the use of financial services? an OLS approach

	Average treatment effect on					
	Number of	Clients using				
	financial services	Checking account	Credit line	Electronic Checkbook	Savings account	Credit card
CajaVecina	-0.06	0	0	0	-0.05	-0.01
	(0.11)	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)
Gender	0.11**	0.03***	0.03**	0.01	0	0.04**
	(0.05)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Age	-0.00**	-0.00***	-0.00***	-0.00**	0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Marital status	0.20***	0.01	0.03***	0.05***	0.08***	0.03*
	(0.04)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)
Formal education	0.21***	0.04***	0.04***	0.02**	0.07***	0.03***
	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Business sector	0.03***	-0.01**	0	0.01***	0.01***	0.02***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Years in Business	0.02***	0.01**	0	0	0.01***	0
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Estimation method	OLS/White	OLS/White	OLS/White	OLS/White	OLS/White	OLS/White
Fixed effects	County	County	County	County	County	County
F statistic	10.84	6.96	5.35	4.83	5.46	6.92
Adjusted R squared	0.06	0.08	0.05	0.04	-0.04	0.07
Observations	1835	1835	1835	1835	1835	1835

I obtain the same results when analyzing the coefficients presented in Table 7. The average treatment effects estimated via the ordinary least square method are less than or equal to the ones presented in the results section. Also, the coefficients remain not statistically significant.

Moreover, the F and the adjusted R-squared statistic of the model's estimation suggest that this specification does not explain the variance of the corresponding dependant variable at a proper confidence level.

TABLE 7: Did *CajaVecina* have an effect on the loan take-up or late payments?

	Average treatment effect on number of clients			
	Taking a loan with		Having a past-due quota with	
	BancoEstado	Financial system	*BancoEstado*	Financial system
CajaVecina	0.02	-0.01	0.01	0
	(0.05)	(0.05)	(0.02)	(0.02)
Gender	0.04	0.04*	0.02**	0.02*
	(0.02)	(0.02)	(0.01)	(0.01)
Age	-0.00***	0	-0.00***	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)
Marital status	0.08***	0.06***	0.02***	0.02***
	(0.02)	(0.02)	(0.01)	(0.01)
Formal education	0.06***	0.08***	0.01	0
	(0.01)	(0.01)	(0.01)	(0.00)
Business sector	0.01**	0.03***	0.01***	0
	(0.01)	(0.01)	(0.00)	(0.00)
Years in Business	0	0.01**	0	0
	(0.00)	(0.00)	(0.00)	(0.00)
Estimation method	OLS/White	OLS/White	OLS/White	OLS/White
Fixed effects	County	County	County	County
F statistic	6.81	7.56	2.77	2.53
Adjusted R squared	0.05	0.04	0.02	0.02
Observations	1835	1835	1835	1835

Second, I estimate the probability model (equation 2) using a probit technique. The evidence presented in Table 8 confirms the findings shown in the results section. Even by controlling for county fixed effects and for individual's characteristics, the estimated coefficient measuring the impact of the initiative remains close to zero and not significant. Moreover, the predictive power of the model, measured by the Likelihood Ratio and the McFadden pseudo R squared, remains low.

TABLE 8: Did *CajaVecina* have an effect on the loan take-up, late payments or use of financial services?

	Average treatment effect on number of clients								
	Clients using					Taking a loan with		Having a past-due quota with	
	Checking account	Credit line	Electronic Checkbook	Savings account	Credit card	*BancoEstado*	Financial system	*BancoEstado*	Financial system
CajaVecina	0.06	0.08	0.1	-0.1	0.03	0.11	0.03	0.4	-0.02
	(0.21)	(0.20)	(0.17)	(0.16)	(0.18)	(0.15)	(0.15)	(0.33)	(0.31)
Gender	0.11	0.08	-0.04	-0.07	0.09	0.04	0.06	0.13	0.06
	(0.09)	(0.09)	(0.07)	(0.07)	(0.08)	(0.06)	(0.06)	(0.14)	(0.12)
Age	-0.03***	-0.03***	-0.02***	0	-0.02***	-0.02***	-0.01***	-0.04***	-0.03***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Marital status	-0.15*	0	-0.01	0.03	-0.09	0.05	0	0.07	-0.02
	(0.08)	(0.08)	(0.07)	(0.06)	(0.07)	(0.06)	(0.06)	(0.12)	(0.11)
Formal education	0.14***	0.15***	0.03	0.14***	0.06*	0.11***	0.16***	0.02	-0.15**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.06)
Business sector	-0.07***	-0.05**	0.03*	0.02	0.04***	0.01	0.05***	0.05*	0
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.03)
Years in Business	0.02*	-0.01	0	0.02**	-0.02*	-0.01	0	-0.03	-0.04**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Estimation method	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Probit
Fixed effects	County	County	County	County	County	County	County	County	County
LR test	91.98	115.35	123.20	51.76	161.98	155.60	157.36	55.86	56.41
McFadden R squared	0.09	0.10	0.07	0.02	0.10	0.06	0.06	0.11	0.09
Observations	1835	1835	1835	1835	1835	1835	1835	1835	1835

IV Conclusions

My findings suggest that the *CajaVecina* program did not on average have a significant effect on the client's decision of using *BancoEstado Microempresas* financial services. This result can be interpreted in various ways. One can conclude that the geographic constraint in these counties is not binding enough to make the clients decide to not use *BancoEstado*'s financial services. Another plausible explanation might be that this program is not perceived by the clients as a close substitute of the branch. One can also interpret these results as a sign that the program lacked proper advertising: perhaps clients did not know what this service was and/or where it was located, etc.[5]

In order to understand the real benefits of this program, we need to know more about how the program is perceived by the clients. First, we need to determine if the geographical constraint is binding. We need to understand if this restriction is making it difficult for clients to access the financial market. Second, we need to know if *CajaVecina* is perceived as a close substitute for a branch. If it is, the program might be a solution for the isolation issue. If it is not, we need to know why and if a different advertising campaign could change this.

Since many questions remain unanswered, it is difficult to understand the benefits of *CajaVecina* and its possibilities as a program for addressing the lack of access to financial markets in rural areas. In conclusion, the results of this study are inconclusive and further research is required.

[5] An internal survey conducted by the bank showed that 76 percent of the clients of the metropolitan area of Chile did not knew about *CajaVecina*. Also, this survey provides evidence that, when informed about *CajaVecina*, 53 percent of these clients say they are willing to use it in the future. (Gerencia de Clientes (2008))

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V Appendix

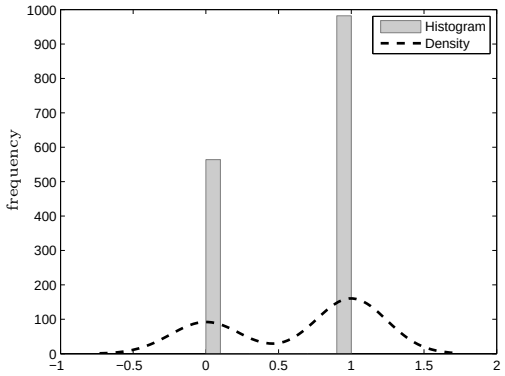
TABLE A1: Why I chose Oct-06 to Dec-06 for the analysis?

Month	Number of *CajaVecinas* installed	Regions where installed
Feb-06	2	VIII
Jul-06	2	X
Aug-06	4	II, IV, V
Oct-06	9	V, VI, VIII, IX
Nov-06	8	VI, VII, XIII
Dec-06	19	V, VI, VII, VIII
Jan-07	4	VIII, IX, XIII
Feb-07	5	III, VIII, IX, XIII
Mar-07	4	VIII, IX, X
Apr-07	4	IV, V, VII, IX
May-07	1	VIII
Jun-07	6	VI, VIII, IX
Jul-07	2	V, X
Aug-07	1	VII
Sep-07	1	VII
Oct-07	2	IX, XIII
Nov-07	4	II, V, VII, VIII
Dec-07	3	VI, VIII, IX
Jan-08	2	VI
Mar-08	1	XI
Apr-08	1	VI
May-08	1	IX
Jun-08	1	XIII
Jul-08	1	IX

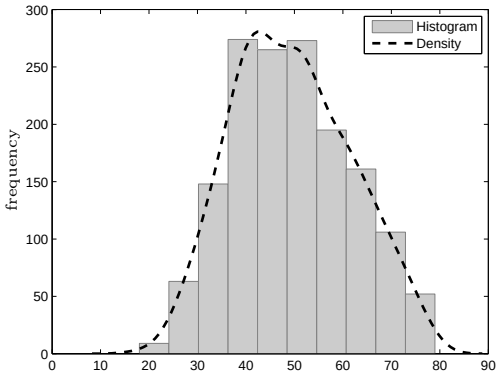
TABLE A2: List of counties in the chosen sub-sample

County name	Group
Olivar	Control
Pumanque	Control
Palmilla	Control
Navidad	Control
La Estrella	Treatment
Marchihue	Treatment
Hualañe	Control
Pinto	Control
El Carmen	Control
Pemuco	Treatment
Hualqui	Control
Renaico	Control
Lumaco	Treatment
Los Sauces	Treatment
Perquenco	Treatment
Vilcún	Control
Melipeuco	Treatment
Toltén	Control
Chillán Viejo	Control

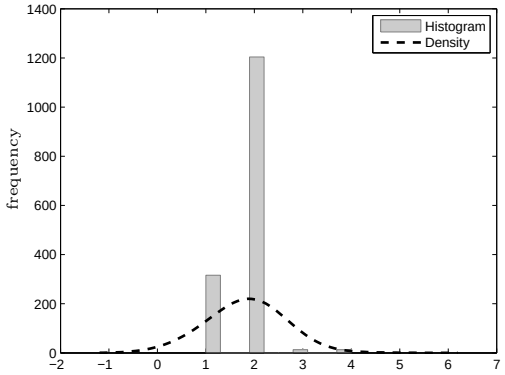
FIGURE A1: Do the control variables seem to behave like a normal distribution?



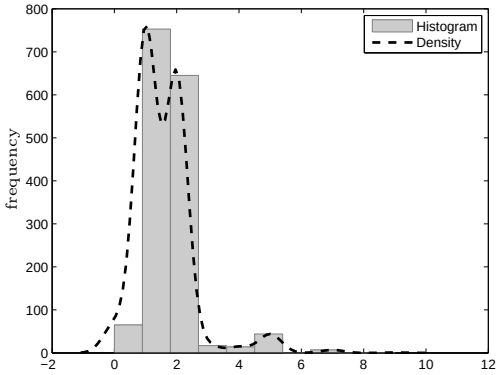
(A) Gender



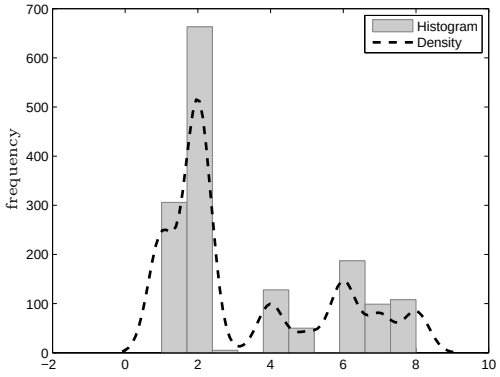
(B) Age



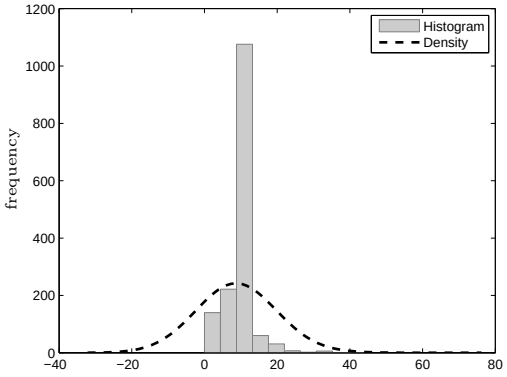
(C) Marital status



(D) Formal education



(E) Business sector



(F) Years in Business

TABLE A3: Did *CajaVecina* have an effect on the use of financial services? an OLS approach

	Average treatment effect on					
	Number of	Clients using				
	financial services	Checking account	Credit line	Electronic Checkbook	Savings account	Credit card
CajaVecina	0.01	-0.01	0	0.02	-0.01	0.01
	(0.11)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
Gender	0.02	0.04***	0.02	-0.01	-0.03	0.02
	(0.04)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Age	-0.01***	-0.00**	-0.00***	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Marital status	0	0.02	0.02	0	-0.03	-0.01
	(0.04)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Formal education	0.14***	0.04***	0.04***	0.01	0.03***	0.02**
	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Business sector	0.02*	0	-0.01	0.01**	0	0.02***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Years in Business	0.01	0.01**	0	0	0	0
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	1.72***	-0.05	0.14***	0.42***	0.88***	0.34***
	(0.15)	(0.05)	(0.05)	(0.06)	(0.07)	(0.06)
Estimation method	OLS/White	OLS/White	OLS/White	OLS/White	OLS/White	OLS/White
Fixed effects	No	No	No	No	No	No
F statistic	17.66	9.06	7.60	9.91	4.43	7.56
Adjusted R squared	0.08	0.04	0.04	0.05	0.02	0.04
Observations	1835	1835	1835	1835	1835	1835

TABLE A4: Did *CajaVecina* have an effect on the loan take-up or late payments?

	Average treatment effect on number of clients			
	Taking a loan with		Having a past-due quota with	
	BancoEstado	Financial system	*BancoEstado*	Financial system
CajaVecina	0.04	0.02	0.02	0
	(0.06)	(0.05)	(0.02)	(0.02)
Gender	0.01	0.01	0.01*	0.01
	(0.02)	(0.02)	(0.01)	(0.01)
Age	-0.01***	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
Marital status	0.02	-0.01	0.01*	0.01
	(0.02)	(0.02)	(0.01)	(0.01)
Formal education	0.04***	0.06***	0	-0.01
	(0.01)	(0.01)	(0.01)	(0.00)
Business sector	0.01*	0.02***	0.01***	0
	(0.01)	(0.01)	(0.00)	(0.00)
Years in Business	0	0	0	0
	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.53***	0.63***	0.07**	0.13***
	(0.08)	(0.08)	(0.03)	(0.03)
Estimation method	OLS/White	OLS/White	OLS/White	OLS/White
Fixed effects	No	No	No	No
F statistic	9.10	10.67	4.65	3.00
Adjusted R squared	0.03	0.04	0.02	0.01
Observations	2462	2462	2462	2462

TABLE A6: Why I did not use the Nov-06 to Nov-07 sub-sample?

	Treatment Counties ($N_{\text{counties}} = 19$)		Control Counties ($N_{\text{counties}} = 14$)	
	Lower bound	Upper bound	Lower bound	Upper bound
Gender	**0.63**	**0.67**	**0.54**	**0.58**
Age	49.20	50.02	49.56	50.61
Marital Status	1.79	1.83	1.79	1.83
Formal education	**1.39**	**1.46**	**1.77**	**1.88**
Business sector	3.36	3.53	3.44	3.65
Years in business	9.18	9.45	8.85	9.24

TABLE A5: Did *CajaVecina* have an effect on the loan take-up, late payments or use of financial services?

	Average treatment effect on number of clients								
	Clients using					Taking a loan with		Having a past-due quota with	
	Checking account	Credit line	Electronic Checkbook	Savings account	Credit card	*BancoEstado*	Financial system	*BancoEstado*	Financial system
CajaVecina	-0.08	0	0.11	-0.04	0.03	0.11	0.05	0.36	0.03
	(0.20)	(0.19)	(0.17)	(0.16)	(0.17)	(0.15)	(0.15)	(0.32)	(0.28)
Gender	0.24***	0.09	-0.06	-0.1	0.06	0.02	0.03	0.16	0.07
	(0.09)	(0.09)	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)	(0.13)	(0.12)
Age	-0.01**	-0.02***	-0.02***	-0.01***	-0.02***	-0.02***	-0.01***	-0.03***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Marital status	0.14	0.11	0	-0.11	-0.02	0.05	-0.03	0.22*	0.09
	(0.09)	(0.09)	(0.07)	(0.07)	(0.08)	(0.06)	(0.06)	(0.12)	(0.12)
Formal education	0.21***	0.17***	0.04	0.10***	0.07**	0.12***	0.17***	0.04	-0.09
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.06)
Business sector	-0.02	-0.03	0.04***	0	0.06***	0.03*	0.06***	0.07***	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)
Years in Business	0.04***	0	0	0.01	-0.02	-0.01	0	-0.01	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Constant	-2.03***	-0.91***	0	1.08***	-0.26	0.12	0.29	-1.20***	-0.67*
	(0.28)	(0.27)	(0.23)	(0.22)	(0.23)	(0.20)	(0.20)	(0.39)	(0.37)
Estimation method	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Probit
Fixed effects	No	No	No	No	No	No	No	No	No
LR test	70.96	67.60	97.26	45.21	71.58	78.15	110.20	45.90	26.47
McFadden R squared	0.07	0.06	0.06	0.02	0.04	0.03	0.04	0.09	0.04
Observations	1835	1835	1835	1835	1835	1835	1835	1835	1835