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Does Centralization Imply Better Targeting?: Evaluating Emergency Employment Programs in Chile

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Does Centralization Imply Better Targeting?: Evaluating Emergency Employment Programs in Chile[1]

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Abstract

This paper develops direct tests for evaluating the performance of two types of emergency employment programs put in place in Chile since 1999. Our results suggest: First, decentralized and "market-driven" programs (subsidies for hiring and training) are more efficient in terms of productivity, but are targeted to people that are less vulnerable to unemployment. Second, direct employment programs result in moderate increases of the income of the households of the participants. This increase may be outweighted by the costs (in present value) associated with higher school drop-out and participation rates. Third, if analyzed at a municipality level (*comuna*), centralized programs do not target municipalities with higher unemployment, increased vulnerability to unemployment, or even lower median income levels, but are strongly correlated with the political affiliation of its major. Finally, our results suggest that the population targeted in direct employment programs is not more vulnerable to unemployment than the actually unemployed.

Key Words: Employment Programs, Targeting, Propensity Score Matching, Chile.

JEL Classification: C25, J45, J64, H31, H32.

1 Introduction

Emergency employment programs (EEPs) are tools aimed at lessening the effects of the economic crisis on consumption and human wealth accumulation (Maloney, 2001). However, most EEPs' practical experiences show that their objectives and target populations are not clearly defined. Furthermore, these programs are only one way of alleviating the consequences of the drop in income and consumption in the face of a crisis, and are not conceived as a way of structurally approaching the problem of poverty and exclusion.[1]

Chile's experience with EEPs began in the crisis of 1982 when the unemployment rate exceeded 20%. It was innovative and, to a certain extent, pioneering in matters of design and ex-post evaluation. These programs and other more recent experiences in Latin America, taught lessons on how a bad design can induce people who do not belong to the target population to participate in them. More precisely, the Chilean experience with EEPs showed that differences in design, in entry requirements, and in wages paid, severely affected the targeted groups and results.

The strong criticism wielded against EEPs and the lesser need to implement them prompted, in 1986, to discard them. However, this situation changed towards the end of 1998, when the unemployment rate measured by the University of Chile jumped from 6.9% in June 1998 to 11.1% in September of the same year. The problem worsened in the year

[1] For a more exhaustive battery of programs, see for example, Khadiagala (1995), Lustig (1997) Klugman (1999) and Wodon and Hicks (2002).

2000, when the open unemployment was close to 15%, and the percentage of those who had been unemployed for more than 6 months reached 43%.

One of the most striking aspects of the labor markets diagnosis is that even though Chile had overcome the world economic crisis relatively well, and that GDP grew at about 3% per annum between 1998 and 2002, according to the National Bureau of Statistics (INE), this was not associated with increases in employment. Different answers have been put forth to solve this riddle. Even though many diagnoses could explain the increased and persistent unemployment, few of them called for the implementation of EEPs.[2]

If the nature of the economic crisis were understood by the policy maker, a clear profile of the most affected could be made, and if necessary, policies designed. In 1999, without a clear diagnosis of the nature of the crisis, the government launched several EEPs, that had different designs and resources involved. Their importance was such that by October 2002, the ratio of beneficiaries to the unemployed reached almost 30%.[3]

The purpose of this paper is to evaluate and compare the performance of the two main types of EEPs initiated in Chile by the end of the 1990s. One is based on direct employment provided by municipalities (*comunas*) and the other subsidizes private sector hiring.[4] The paper has three sections besides this introduction. Section 2 briefly describes the pro-

[2]For instance, Bergoeing and Morandé (2002) suggest that increases in the minimum wage and several proposals for labor reform played an important role in increasing the unemployment. Meanwhile, Chumacero (2000) and Paredes (2001) focus on the methodology to estimate unemployment and provide explanations that are more consistent with macroeconomic factors underlying the increased unemployment.

[3]Despite this huge increase in the number of beneficiaries of EEPs, the unemployment rate has remained virtually unchanged.

[4]Chumacero and Paredes (2002) present a more detailed and less technical description of the programs.

grams and their expected impacts. Section 3 provides an empirical evaluation of the EEPs considered. Finally, Section 4 concludes.

2 The Programs

The EEPs initiated in the crisis of 1982 reached their peak in the mid-eighties, presenting high heterogeneity of results in terms of targeting and ended in 1986. Public employment programs resurfaced timidly in 1992, when the new democratic government initiated a program of employment generation through municipal placement offices and which, it was argued, was an experience quite different from the previous one. At any rate, it did not employ more than 5,000 people.

As a direct consequence of the increased unemployment in September 1998, a fresh impulse to employment programs took place in April 1999. As discussed above, in designing the programs, a diagnosis of the nature of the crisis is not present in the official documents, but the proximity of the presidential election, helped the government of the period to encourage them. Thus, President Frei's government launched an employment program in April 1999 that began with a system of direct employment through municipalities with slightly more than 10,000 jobs, and reached its peak in November 1999, when this program exceeded 100,000 participants (Figure 1).

Once the governing coalition was reelected in the year 2000, the new government drastically reduced the employment programs to a point that, according to official information,

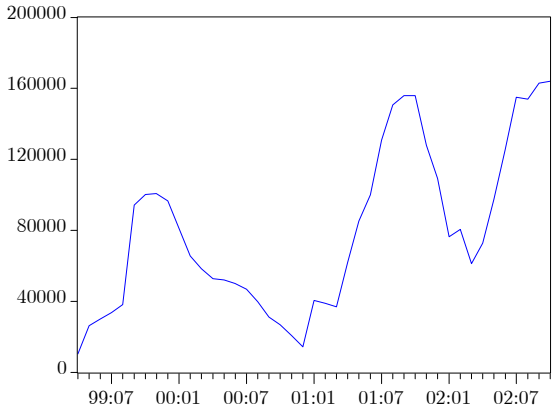

Figure 1: Beneficiaries of EEPs: 1999-2002

in December 2000 it did not exceed 15,000 jobs.[5] In the year 2001, a new qualitative and quantitative change was in place, when the government promoted a new system of programs: subsidizing hiring. EEPs reached 155,000 beneficiaries in October 2001 and reached a new peak of 164,000 jobs one year later.

2.1 Design and Objectives

Since 1999, following a trend that, in part, is a consequence of looking for a more suitable design, but which also reflects the absence of clarity, at least nine employment programs were developed in Chile, each with different designs and institution in charge. Broadly speaking, the programs may be classified under the categories of direct employment (through

[5] However, according to the CASEN survey, in November 2000, more than 54,000 people reported themselves as beneficiaries of such programs.

municipalities), programs subsidizing private sector hiring, and a mixture of both, using subsidies and a centralized allocation of resources.

2.1.1 Direct Employment Programs

The set of direct employment programs (e.g., the Program of Labor and Employment Reinsertion) states prioritizing the incorporation of unemployed in municipal districts that feature especially high unemployment. These programs share the characteristic that hiring is made by a public institution (municipal or central) and focuses on providing social or public infrastructure. Operationally, the targeted population of the programs are sought through the official unemployment information (INE), on the basis of the last moving quarter available.

2.1.2 Subsidy to New Hiring

The subsidy program is conceived as means to facilitate hiring unemployed workers, preferably household heads, who attest to be the only source of family income. The program subsidizes firms with up to 40% of the monthly minimum wage of the salary paid to workers for a period of up to four months (with hiring lasting at least four months). The worker's salary cannot be lower than the monthly minimum wage (approximately US$ 150 per month). The program also finances training costs of up to Ch$ 50,000 (US$ 73) per apprentice worker, which are reimbursed only once, when the company certifies having completed and paid for the learning process to a registered firm in the central training office.

The process of allocation is by contest, in which the best work offers submitted are

privileged. As from the year 2000, among the election factors, the program privileges small and medium sized companies. More specifically, the contests establish that only firms that pay taxes and make social security payments are eligible. For small and medium sized companies (with invoice of less than US$ 0.5 million dollars per year) as well as for new companies, the maximum number of workers to be subsidized is limited to up to 6. In the case of larger companies, they may hire up to 20% of the permanent head-count during the previous 12 months.[6] The greater the length of the contracts (with a minimum is 4 months), the greater the salary offered Over the minimum wage), are a criteria of election, but the . Likewise, over the minimum wage agreed on, and if the beneficiaries are unemployed registered with the municipal placement office, are all criteria that favor granting a subsidy.

A mixture of both direct employments and subsidy programs is FOSIS. This is a centralized institution which proposes the Regional Governors to allocate more resources to municipalities having the highest unemployment rates, identifies firms or municipalities that would hire workers, and also provides a subsidy for hiring.

In addition to the different ways to select applicants and the wages they are paid, both lines of programs differ in two other dimensions: First, direct employment programs select candidates directly from municipalities while in the subsidy program they induce private firms to increase their level of employment. Second, the capacity to monitor and enforce the fulfillment of the designs differ; a program that subsidizes new hiring by private firms should

[6] When a company's life is shorter, the head-count considers the people employed during the period of operation.

also monitor if the firm is actually hiring new candidates or simply formalizing already hired workers. The latter is particularly important as the incentives of the companies and workers, depending on the amounts involved and the monitoring, may lead to serious distortions of the data. Even when a subsidy that reduces the cost for new hiring will encourage additional employment, to the extent that it is not possible to accurately keep track of who are new workers, firms may try to deceive the system and receive a subsidy for all of them. If it is not possible to deceive the system with the workers already employed, there is an incentive to artificially create new employment, without this being the case. Such incentive, which is magnified when the firms are not adjusted to their optimum level of employment, arises if the subsidy is higher than the taxes that must be paid.[7]

2.2 What to Expect from Each Program

Here we derive several empirical implications that the design of each type of employment program should have on the kind of participants it attracts.

Let $\overline{w}$ denote a wage offer and a_i be a variable that reflects individual's i marketable skills.[8] The individual decides to take an offer ($s_i = 1$) if the wage offer exceeds his reservation

[7] In the case of Chile, taxes associated with health and social security are considerably lower than the subsidy, and therefore monitoring is crucial.

[8] Here a is defined broadly. In the next section we discuss the particular way in which it is measured in this study.

wage $c(\cdot)$ (which is an increasing function of a). More compactly, we have that:

$$s_i = \begin{cases} 1 & \text{if } \overline{w} \geq c(a_i) \\ 0 & \text{otherwise.} \end{cases}$$

On the other hand, given a wage rate $\overline{w}$, a firm would choose to make an offer to an individual if his productivity exceeds $\overline{w}$. If productivity, $p(\cdot)$, is increasing in a, and a is observable by the firm, it places an offer if:

$$d_i = \begin{cases} 1 & \text{if } \overline{w} \leq p(a_i) \\ 0 & \text{otherwise.} \end{cases}$$

In equilibrium, given a wage rate $\overline{w}$ there is a level of a consistent with it,

$$\overline{a} = c^{-1}(\overline{w}) = p^{-1}(\overline{w}). \tag{1}$$

Thus, if supply and demand were to freely determine the level of marketable skills that is consistent with a given wage, the reservation wage and the productivity functions would determine a level of a with zero excess demand.

This is (theoretically) the characteristic of the employment program based on direct subsidies given that if the skills of the individuals are well estimated by the firms, they will hire candidates whose productivity at least compensates the wage offer.

On the other hand, direct employment programs are not intended to deal with the demand of labor directly linked with productivity. It generally operates as a system in which, for a given budget and wage rate, the individuals interested in working, will. In such case, the only function that operates is the one that concerns the supply of labor by individuals. Thus for a given wage $\overline{w}$, the individuals that will work on the program will satisfy that $\overline{w} \geq c(a_i)$. But if $\overline{a} = c^{-1}(\overline{w})$ and $c(\cdot)$ is an increasing function of a, then, for the same wage rate, the level of a of the participants of subsidy programs must exceed the average of a for the participants of direct employment programs.

This simple structure shows that from the get go, direct employment programs should attract, on average, individuals of lower reservation wage and lower productivity than participants of subsidy programs. Furthermore, for the same wage rate, the average individual that participates on direct employment programs would not be hired in subsidy programs.

There are several empirical implications that can be drawn from these observations: First, participants of subsidy programs should posses higher levels of marketable skills. Second, direct employment programs should attract individuals that, in the absence of the program, would not have participated in the labor market. This is so because firms would not have made an offer to the typical applicant for direct employment programs. Third, given that on average, employees of subsidy programs should have better qualifications than employees of direct employment programs, the former should (on average) be less vulnerable to unemployment, while the latter should be more prone towards sharing the characteristics of

otherwise inactive population (people out of the labor force). The following section evaluates the empirical support for these and other regularities that should be observed on the data.

3 Empirical Evaluation

This section presents an empirical evaluation of the employment programs put in place by the government since 1999. To do so we rely on three sources of information: First, the CASEN (Socio Economic National Survey) survey of the year 2000 elicited information of the employed population asking if their were employed by any of the direct employment programs. Roughly 0.1% of the employed population (54,244 persons) answered affirmatively. The advantage of having this information is that CASEN is rich in information of the characteristics of the individuals and households. Nevertheless, due to its timing, the survey has information only of individuals that participated in direct employment programs and not subsidy programs which were not in place at the time. A second data base provided by FOSIS has information of the beneficiaries of their programs up to the year 2002 (18,557 beneficiaries). This database has information on some characteristics of the beneficiaries (wage, age, gender, and schooling) but does not have information of the household or the specific type of employment in the program. Finally, a third database consists on information of the beneficiaries of the subsidy program in the year 2002 (56,290 beneficiaries). It also contains information of some personal characteristics, but as FOSIS it lacks of information of the household.

We use these databases and consider three dimensions for evaluating the performance of the different employment programs. First, we evaluate whether the beneficiaries of direct employment and subsidy programs come from different populations in terms of marketable skills and vulnerability to unemployment. Second, we quantify the effects of direct employment programs on variables such as the income of households, participation rates, and school drop-outs. Finally, we evaluate whether or not the objective of focusing on municipalities with high unemployment rates was achieved by any of the programs.

3.1 Characteristics of the Beneficiaries

One of the empirical implications that can be derived from the characteristics of each of the employment programs described on Section 2, is that the beneficiaries of direct employment programs should (on average) have lower reservation wages than beneficiaries of programs based on subsidies. This is so because, given the level of salaries offered by these programs, if a firm were to hire an individual using the subsidy program, it would only do so if the characteristics of the candidate are such that its productivity is at least equivalent to the wage offer. As the direct employment programs do not take this factor under consideration, the average ability of the beneficiaries of the direct employment programs should be lower.

To assess this, let $z_{j,i}$ be the sample average of characteristic j of the beneficiaries of

program i and let $s_{j,i}$ be an estimate of the variance of $z_{j,i}$. The variable $t_{j,i-k}$ defined as

$$t_{j,i-k} = \frac{z_{j,i} - z_{j,k}}{\sqrt{s_{j,i} + s_{j,k}}}$$

is asymptotically normal.[9] This simple test of equality of means tells us if a given characteristic differs among programs. Table 1 presents the equality test of all the variables for which there is information for the FOSIS and Subsidy programs. As expected, the beneficiaries of the subsidy programs have (on average) one more year of schooling, are younger, mostly males and have better monthly wages; which appear to be evidence that the participants in subsidy programs have better marketable skills. To check this, a natural way to proceed is to evaluate what would be the market wage of the beneficiaries of each program and evaluate if the distribution of this variable for the beneficiaries of one program stochastically dominates the distribution for the beneficiaries of another program.

	FOSIS	Subsidy	P-Value
Years of Schooling	9.55	10.54	0.00
Age	35.9	35.0	0.00
Percentage of Males	0.55	0.74	0.00
Wage	116,139	146,687	0.00

Table 1: Descriptive Statistics of Characteristics of Beneficiaries of Different Programs. P-value = P-value of the test of equality of means.

Figure 2 presents the empirical cumulative distribution function (e.d.f.) of the fitted (log) wages for beneficiaries of the Subsidy and FOSIS programs. It was obtained by first

[9] This statistic assumes that the populations of program i and k are independent.

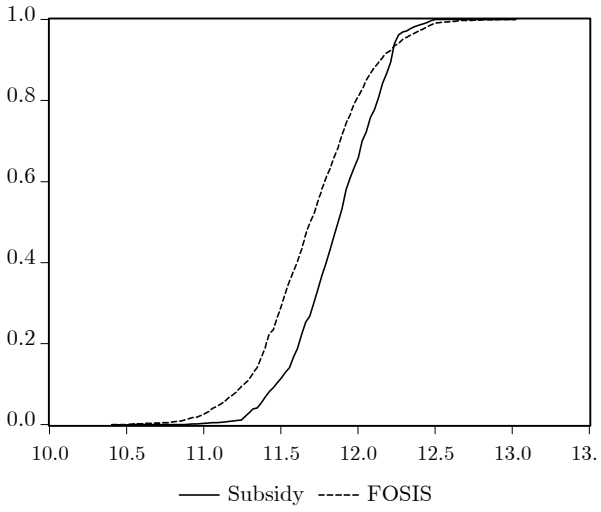

Figure 2: Cummulative Distribution Function of (log of) Wages

estimating a Mincer equation considering the population of CASEN and using the estimates to forecast the wage rate of the beneficiaries of each program considering their observed characteristics (see Table 2).

The results show overwhelming evidence that the individuals of the subsidy program have "better" characteristics than that of the beneficiaries of the direct employment programs. The empirical distribution function of the (log) wage rate for the beneficiaries of the subsidy program is always to the right of the distribution for the beneficiaries of the direct employment program. First order stochastic dominance does not occur because of a few extreme observations on the right of the distribution.[10] However, it is important to recall that due

[10] A formal test of equality of the empirical distribution functions was conducted and the null hypothesis was strongly rejected for all the relevant levels of the (log) wage rate.

to the characteristics of the programs it is unlikely that beneficiaries of the subsidy program could be found there, as there are limits with respect to the wage rate that can be paid.

Thus, the empirical implications of the rationale of each program are in line with the evidence. The beneficiaries of direct employment and subsidy programs come from different populations. The former is constituted by individuals that (on average) have lower education and lower marketable skills.

3.2 Vulnerability

An explicitly stated objective of the EEPs is to provide employment to people that would otherwise be unemployed. Implicitly, it is assumed that the characteristics of the employment and the program should be able to successfully target this population. Next, we consider if the programs in place were able to achieve this objective.

To do so we use the CASEN survey to estimate the probabilities of an individual being employed, unemployed, or inactive (not participating in the labor force) conditional on a vector x of characteristics. Multinomial logit models were estimated for the years 1996 and 2000. The reason for having two estimates is that the year 1996 will be considered as a normal year and the year 2000 as a year of crisis. Once the estimates of the coefficients for each year were obtained, they were used to estimate the conditional probabilities given the x vector observed in the year 2000 (see Table 3). Thus, if the unemployment rate had uniformly increased in the year 2000 one could claim that all the individuals were equally

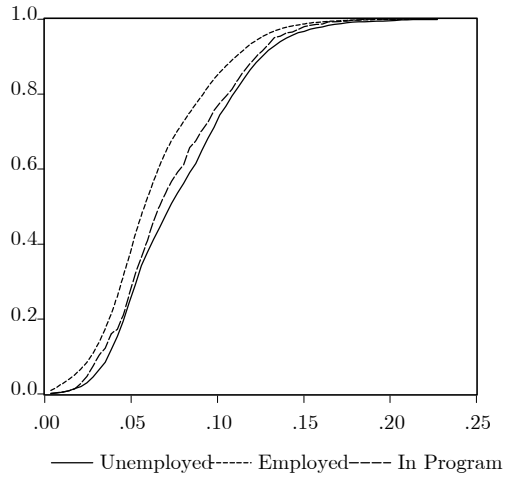

Figure 3: Cummulative Distribution Function of the Probability of Being Unemployed (CASEN)

vulnerable to the referred phenomena. However, if there are some characteristics of the individuals that make them more vulnerable to the unemployment, different implications with respect to how to target potential participants could be devised.

Figure 3 presents the results of constructing the empirical distribution function of the fitted probability of being unemployed in the year 2000 for several groups of individuals. As the figure makes evident, there is first order stochastic dominance of the empirical distribution of the unemployed with respect to that of the employed population. This is expected, given that the multinomial model was estimated considering the characteristics of these individuals. If the population of each group is not chosen randomly, it is expected that the population actually unemployed is more vulnerable to the unemployment that the employed population. However, the interesting result arises when estimating the empirical distribution

function of the estimate of the probability of being unemployed for the people that said that were participating in the direct employment programs of the time. As the distribution function of this population dominates that of the employed population that did not participate in these programs, we conclude that they were more vulnerable to unemployment. Thus, employment programs were able to attract participants that were more vulnerable to unemployment than the people employed. However, notice that there is first order stochastic dominance of the empirical distribution function of the actually unemployed over the e.d.f. of the participants of the employment programs. This means that the programs did not attract its targeted population, but did attract people less vulnerable to unemployment. In all likelihood, this is so because the population attracted to the program were, according to the model, more prone to be out of the labor market.

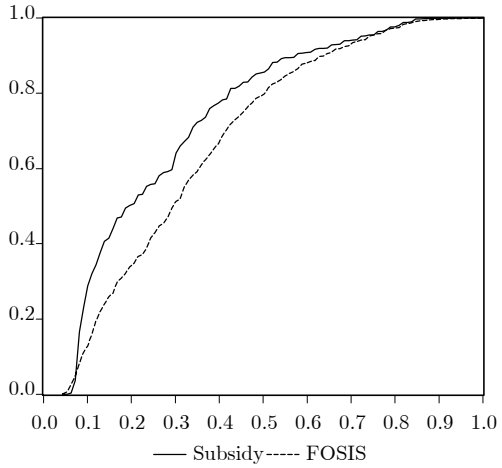

Figure 4: Cummulative Distribution Function of the Probability of Being Inactive

Figure 4 confirms this claim by presenting the e.d.f. of the probability of being inactive (out of the labor force) for the individuals that participated in FOSIS and subsidy programs. The figure eloquently shows that the FOSIS participants stochastically dominate the subsidy programs participants; thus it is more likely for a FOSIS participant to be inactive.[11] Thus, given that the FOSIS participants have more in common with the direct employment participants, the programs were more likely of having attracted people that would have, in the absence of the program, choose to remain out of the labor force. The next section presents further evidence in this regard.

3.3 Impacts

Typically, employment programs are evaluated using methodological devices that are intended to capture what would have been the dynamics of a given variable in the absence of the program. Evaluating the effects of such counterfactuals is not an easy task. A methodology that appears to be popular is that of the propensity score matching (the references present a list of documents that deal with the theoretical and empirical implementation of the technique).

This technique follows a two step procedure: First, a discrete choice model for participating in the program is estimated and forecasted probabilities are assigned to each individual (Table 4). Second, using the information of the individuals that participated in the program,

[11] Given that not all the x variables used on theestimation of the multinomial model were observed for the FOSIS and subsidy programs participants, we imposed the avergae values of these variables for both groups.

one selects individuals in the control group (that did not participate in the treatment) and compares the variable(s) of interest for both populations. For this practice to be successful one needs the discrete choice model to provide enough information to actually capture the determinants of the decision to participate in the program. Once this is determined, it is important to carefully choose the population in the control group. Two strategies have been advanced in the literature. The nearest neighbor method chooses the individual in the control group that has the closest distance between its forecasted probability of participating in the program and that of the individual that actually participated. Other methods do not rely on an individual observation but weight their closeness using kernels.

In our case, as often occurs when evaluating direct employment programs, the binary choice model is not able to do a very good job (in the sense that the Pseudo-R^2 is 0.05 at the individual level and 0.035 at the household level), however, it does better than models that have been used in other studies (e.g. Aedo and Nuñez, 2001).

At any rate, using the nearest neighbor method, we estimated the impact of the program on three variables: income, the household's participation rate, and the household's school drop out rate (for the population in age of assisting to school). The first two variables traditionally appear when evaluating employment programs and typically show that the net benefits of the programs are much lower than the wage that the participants received. This is due to the fact that in the counter-factual scenario there is a chance that persons that were going to participate on the labor market may have found a job and that the presence of the

program may have actually changed their choices with respect to participating all together.

Thus, we also included two variables that measure the effects of the program on the participation rate and drop-out rate. Table 5 shows that the average gain in income for the participants in employment programs was roughly of 1/10 of the wage reported on Table 1. At the same time, the participation rate of the individuals with less than 21 years increased by 5.6% and that of individuals of more than 60 years by 5.4% as a result of the program. Finally, the drop-out of high school and school (for individuals of less than 18 years) increased by 1.1%. These results suggest that the benefits of these program may be overstated, given that the empirical evidence shows that once an individual leaves school, it is highly unlikely that he will return. Furthermore, even with conservative estimates to the return of schooling (say 3% per year), the present value of foregone income due to deserting school may very well be comparable with the modest gains estimated.[12]

3.4 Other Factors

An objective, explicitly stated by the public employment programs, is that it should focus its attention in places (counties) of higher unemployment, or in places where the unemployment rate is high or has increased the most as a consequence of the economic crisis.[13] We can test

[12] Of course, these results do not take into account the distortionary costs that have to be incurred to finance these programs. As is usual in these studies, the general equilibrium implications of financing these programs is ignored. Thus, at best, these results overestimate the benefits (underestimate the costs).

[13] It is possible that some places may have a level of unemployment that is usually high and relatively stable. In such places, a high unemployment rate would be coupled with low levels of vulnerability of its population to a crisis; given that the unemployment rates in periods of booms and crises are the same. Thus, vulnerability to unemployment in periods of crisis can be proxied by using the change in the unemployment

whether these objectives explicitly stated has been achieved and if there are other variables that may be behind how beneficiaries of the employment programs are chosen.

A simple way to do so is to consider the following model:

$$y_i = \alpha + \beta d_i + \gamma v_i + \delta z_i + \theta p_i + u_i \tag{2}$$

where y_i is the ratio between the number of beneficiaries of the program and the economically active population in district i,[14] d is the unemployment rate, v is the ratio between the unemployment rates of the years 2000 and 1996, z is the median per capita income of district i, and p is a dummy variables that takes the value of 1 if the major of the district is from the ruling coalition and 0 otherwise.

The last two variables control for other factors that may be behind the way in which the beneficiaries of the programs are chosen. The first is used as a proxy of another function that these programs may have which is to be considered as aids to the income of poor households. Thus, if the programs had a redistributive purpose attached to it, the number of beneficiaries should be negatively correlated with the median per capita income of the district. Finally, the last variable captures possible determinants that have to do with political factors rather than those explicitly stated in the programs. Table 6 presents the results of the estimation of (2) using OLS.

rate between periods of boom and crisis.

[14] In Chile, electoral and administrative district are called 'comunas' and vary in size.

The results clearly suggest that the way in which each program 'chooses' its beneficiaries is different. Both, CASEN and FOSIS data-bases consider direct public employment programs, thus their results are qualitatively and quantitatively similar. That is, direct public employment programs did not seem to choose their beneficiaries considering either the magnitude or vulnerability of the inhabitants of each district to the unemployment. Rather, these programs seem to have targeted districts with lower income but not necessarily lower unemployment. Finally, a variable that is robustly and positively correlated with the ratio between beneficiaries and labor force in the district is the political affiliation of the major of the district, which accounts for between 0.1% and 0.4% of the difference (in favor of districts where the major is affiliated to the ruling coalition).[15] Although still statistically significant, that the coefficient associated with p in FOSIS is 4 times smaller than that with information from CASEN, is probably due to the fact that the beneficiaries of FOSIS present a mixture of individuals engaged in typical direct employment programs and participants of subsidy programs.

Compared with the results for the subsidy program, the other two are strikingly different. The variables that are statistically significant determinants of the beneficiaries in direct employment programs (median income and political affiliation of the major) are not relevant for the case of the subsidy programs. The only relevant variable appears to be the unemployment level of the district. Nevertheless, its coefficient has the incorrect sign if we

[15] These magnitudes are not only statistically but also economically significant given that they account for 50% of the mean of the dependent variable.

want the program to focus on districts of high unemployment. In particular, beneficiaries and unemployment are negatively and robustly correlated, signaling that when the decision to hire is left to private firms, they seem to choose candidates of low unemployment districts, which presumably have higher human capital or marketable skills.

The results of these regression do not change significantly if different estimation strategies are chosen. For example, given that the dependent variable is a proportion, the same specification was estimated considering this characteristic and obtained similar results. The same is true if Tobit models are considered. Furthermore, there is one district that is influential in the sense of having almost 5% of its economically active population as part of the direct employment program (Tocopilla); if robust methods (as least absolute deviation) are used, the conclusions remain. That is, beneficiaries of direct employment programs (as a fraction of the economically active population) are inversely related with the median income of the district and only appear to be correlated with the political affiliation of the major. On the other hand, programs that subsidize hiring are not related with any of these variables, but are negatively correlated with the unemployment rate of the district; thus, signaling that firms do not focus on choosing candidates from districts of high unemployment, but on the contrary. These aggregate results suggest that different types of employment programs have different impacts on the types of beneficiaries they choose or are chosen by them.

It may be argued that p is statistically significant because it is capturing something else; in particular, it may be the case that districts with a major of the ruling coalition tend to

have poorer population where unemployment may be more important. This argument is not valid because the results are obtained after controlling for variables that pretend to capture the median income and the unemployment rate of the district. On the other hand, these results may be consistent with majors in the ruling coalition that are more pro-active and thus ask for more resources regardless of the particular situation of the district. Finally, it may be argued that the results are contaminated by simultaneity (endogeneity). That is, the outcome of the elections were due (in part) to the 'investments' made on the direct employment programs. This observation may have some merit, but only strengthens the argument that direct employment programs may have a political component.

4 Concluding Remarks

Social programs have been evaluated in Chile since the middle 1980s. This helped to correct a number of problems through the change of the design, and particularly, the requisites for participating in them. The recent employment programs presented several designs that changed over time, reflecting not only the intention of the authority to improve the targeting, but also, the lack of knowledge about the effects each program.

In this paper we evaluated employment programs in their extreme versions: direct employment (with the information of CASEN and parts of FOSIS) and subsidies to private firms. When comparing the characteristics of the populations in each program we found, not surprisingly, that the more decentralized market-driven program is more efficient in employ-

ing relatively more productive workers, but they are less focused in the neediest. Second, we found that vulnerability, defined as the probability of being unemployed, is lower in the population belonging to the employment programs than in the unemployed population. That is to say, the employment programs have not targeted the most vulnerable. Third, we evaluated the impact of the programs on households, building pseudo control groups. We found that even though direct employment programs increased the participants family income, they are associated with an increase in the school drop-out and labor participation rates of the young and old. Finally, we also found that the ratio of beneficiaries to labor force across municipalities (for direct employment programs) cannot be associated with the economic factors such as unemployment and in cases poverty but appears to be correlated with the political affiliation of the major.

We can derive a number of policy implications from this diagnosis. To avoid poor targeting, the market driven program should be more focused allowing beneficiaries, for instance through a voucher system allowing them to choose the firm where they work. Likewise, at the municipality level, changing the way resources are allocated is simple, since the data on unemployment, increases in unemployment and poverty by municipalities is available, it can objectively be used to this end. A new design can also introduce requirements to avoid and limit schooling dropouts. Finally, the introduction of several goals in the government programs reduced their capacity to achieve better performance on what should be their main objective. In particular, privileging small and micro firms reduces the capacity of monitor-

ing the performance of the program, and specifically, to distinguish increases in employment from the formalization of already hired workers.

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Wage equation	Coefficient	Standard error
Schooling	0.1327	0.000107
Experience	0.0123	0.00011
Experience^2	0.0001	0.000002
Male	0.1055	0.000976
I region	-0.1886	0.002047
II region	0.0906	0.002012
III region	-0.1575	0.002615
IV region	-0.211	0.001842
V region	-0.2069	0.001129
VI region	-0.2112	0.001493
VII region	-0.2334	0.001464
VIII region	-0.2633	0.001089
IX region	-0.2753	0.001604
X region	-0.1935	0.001364
XI region	-0.0319	0.004165
XII region	0.188	0.003201
Constant	6.6277	0.003137
Selection equation		
Schooling	0.0536931	0.000127
Experience	0.0772612	0.000087
Experience^2	-0.0015037	0.000001
Male	0.4510174	0.001091
Children $<$ 15 years old	-0.0474323	0.000371
Male married	0.3275119	0.001524
Head house	0.6682929	0.00127
Constant	-1.569186	0.002113
Lambda	-0.2239196	0.0013356

Table 2: Mincer Equation Results

	1996	**2000**
Unemployed		
Schooling	0.0804 (0.0005)	0.0615 (0.00039)
Age	0.3015 (0.00079)	0.3093 (0.00061)
Age^2	-0.0037 (0.00001)	-0.0038 (0.00001)
Male	1.3458 (0.00597)	1.3268 (0.00733)
Children $<$ 15 years	-0.1415 (0.00259)	0.0191 (0.00103)
Male + children $<$ 15 years	0.0593 (0.0034)	0.0452 (0.00136)
Married	-0.9908 (0.00461)	-0.9173 (0.00338)
Per capita income	-0.2627 (0.0018)	-0.1672 (0.00139)
Constant	-8.115 (0.01488)	-8.0527 (0.01326)
Employed		
Schooling	0.094 (0.00022)	0.1028 (0.0022)
Age	0.418 (0.00036)	0.3906 (0.00035)
Age^2	-0.005 (0.000)	-0.0044 (0.000)
Male	1.805 (0.0024)	1.7043 (0.00407)
Children $<$ 15 years	-0.181 (0.00099)	-0.0691 (0.00059)
Male + children $<$ 15 years	0.278 (0.00129)	0.0513 (0.00082)
Married	-0.433 (0.00189)	-0.3701 (0.0018)
Per capita income	-0.017 (0.00035)	-0.0399 (0.00033)
Constant	-8.963 (0.00684)	-8.4907 (0.00762)
# observations	10,997,088	10,804,408
Pseudo R2	0.2891	0.2355
P value Chi2	0.000	0.000

Table 3: Multinomial Logit Results. Standard errors in parenthesis.

	Individual	Household
I region	0.366 (0.024)	0.461 (0.025)
II region	0.345 (0.024)	0.181 (0.027)
III region	0.684 (0.025)	0.790 (0.025)
IV region	0.090 (0.023)	0.134 (0.023)
V region	0.410 (0.014)	0.408 (0.014)
VI region	-0.535 (0.026)	-0.604 (0.027)
VII region	0.017 (0.019)	-0.055 (0.020)
VIII region	-0.183 (0.015)	-0.180 (0.016)
IX region	0.043 (0.020)	0.029 (0.020)
X region	0.250 (0.016)	0.194 (0.017)
XI region	0.085 (0.053)	-0.149 (0.063)
XII region	0.507 (0.038)	0.584 (0.038)
Schooling [Head of household]	-0.067 (0.001)	-0.077 (0.001)
Male	0.591 (0.014)	
Age [Head of household]	0.194 (0.002)	0.022 (0.002)
Age^2 [Head of household]	-0.002 (0.000)	-0.0004 (0.00002)
Household size	0.082 (0.003)	0.244 (0.003)
Married	-0.471 (0.010)	
Head house	0.701 (0.012)	
Children $<$ 15 years	0.011 (0.007)	-0.186 (0.005)
Male + children $<$ 15 years	-0.073 (0.007)	
Imputable rent	-4.7E-6 (4.4E-7)	-1.1E-5 (5.3E-7)
Constant	-9.043 (0.044)	-4.620 (0.054)
# observations	10,804,408	3,832,615
Pseudo R2	0.054	0.035
P value Chi2	0.000	0.000

Table 4: Propensity Scores. Notes: [Head of household] means that the variable used at the household level corresponds to that of the head of the household. Standard errors in parenthesis.

Variable	Impact	Confidence interval	
Income	14,156	11,098	17,213
Participation rate (20 years old or less)	0.056	0.048	0.064
Participation rate (61 years old or more)	0.054	0.037	0.072
Drop out rate (17 years old or less)	0.011	0.006	0.017

Table 5: Propensity Score Matching Results

	CASEN 2000	FOSIS	Subsidy
α	0.004 (0.002)	0.004 (0.001)	0.007 (0.002)
β	-0.006 (0.020)	-0.001 (0.005)	-0.029 (0.014)
γ	0.001 (0.001)	-0.001 (0.003)	0.001 (0.001)
δ	-1.7E-8 (7.7E-9)	-2.4E-8 (8.6E-9)	-1.6E-10 (6.9E-9)
θ	0.004 (0.001)	0.001 (0.0005)	-0.001 (0.001)
R^2 [N]	0.111 [123]	0.127 [101]	0.061 [119]

Table 6: Estimation of Equation (1). Notes: R^2 = Adjusted R^2. [N] = Number of observations. Heteroskedasticity robust standard errors in parenthesis.