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TESIS DOCTORAL

Measuring the effectiveness of two public policies on the sectors of labor markets and urban infraestructure. Experimental and quasi-experimental evidence from developing countries

Midiendo la efectividad de dos políticas públicas en los sectores de mercado laboral e infraestructura urbana. Evidencia experimental y cuasi-experimental de países en desarrollo

MEMORIA PARA OPTAR AL GRADO DE DOCTOR

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INTRODUCTION

This dissertation brings new evidence on the effectiveness of public policies in the areas of youth unemployment and urban development. On the first hand it uses experimental data to study what are the short and long term impacts of a training program for vulnerable youth on labor outcomes and, for the first time, it disentangles the effect of the technical skills offered at the training on labor outcomes. It also brings new evidence on how the program changes job search methods, and, therefore, the entry in the labor markets. On the other hand it uses quasi-experimental methods to estimate how interventions of urban infrastructure – more specifically revitalization of urban spaces and urban transport- affect social wellbeing.

Youth unemployment and underemployment are major problems nowadays. Policy makers around the World are putting in place different Active Labor Markets (ALM) programs to increase youth employability. One of the most widespread programs in the developing World is the provision of vocational training to unskilled youth[1]. These programs aim to improve employability by providing youth with the set of abilities and knowledge that markets demand. Despite recent studies have found that ALMP have modest impacts in employment (Attanasio 2011, Card 2011, Ibarraran 2012, Hirshleifer 2014), they are consistent finding large effects of these programs on the quality of jobs. Card (2011) uses evidence from a vocational training in Dominican Republic and finds little indication of a positive effect on employment outcomes but some evidence of a modest effect on earnings, conditional on working. Ibarraran (2012) studies the same program for a different cohort and, consistently, finds that the program has a positive

[1] A comprehensive review of these programs can be found in Almeida et al., 2012.

impact on job formality for men of about 17 percent and there is also a seven percent increase in monthly earnings among those employed. However, there are no overall impacts on employment rates. Attanasio (2011) finds that a vocational training program in Colombia has significant impacts for women, raising their earnings by 19.6% and having a 0.068 higher probability of paid employment. Hirshleifer (2014) studies a vocational training in Turkey and find that the impact of training on employment is positive, but close to zero and statistically insignificant. However, they find statistically significant effects on the quality of employment during the first year of the program, although these impacts disappear in the long run.

This dissertation contributes to the literature of effectiveness of active labor programs in different ways: on the one hand, it brings long term evidence of employment outcomes from survey data. As opposed to previous efforts to do that (as Hirsheifer 2014), this is the first time that formal jobs are observed. On the second hand, it contributes to understand how these programs work: first, it disentangles the effect of the technical skills offered at the trainings from the soft-skills and from on-the-job experience offered. Secondly, it explores how these programs change the networks of contacts of the youth and how that changes the way in which youth search jobs.

Results show that the program has positive effects on labor outcomes for women and negative for men in the short run. The study also finds that impacts on employment outcomes attenuate in the long run, suggesting that these programs have a modest impact on labor outcomes after a few years. Regarding the technical skills offered by the training program, the study concludes that technical training has very little value added to a generic soft-skills training and on-the-job training in terms of employment outcomes. Given the high cost of the technical training, this result has relevant implications in terms of cost-effectiveness for policy makers. In the same line, the study also finds that training program changes job search methods towards the use of professional networks (especially those developed in internships.) Jobs found through professional contacts are associated with higher levels of formality, suggesting that contacts made during the program may play an important role in increasing job quality.

Besides labor programs, the dissertation study the effectiveness of a different, but increasingly important, development problem: urban development. Emerging economies are experiencing a fast growth of their cities in recent years. This is bringing to scene new problems such as urban transportation saturation and abandonment of public spaces. Despite the amount of resources dedicated to these interventions, little is known about their effectiveness. The main reason for the lack of evidence on the effectiveness of urban infrastructure projects is that they presents specific challenges from the point of view of generating exogenous variations that allow to infer causality. The most important are: first, it is very common that infrastructure projects affect single units and, second, those are pre-assigned based in strategic criteria. (For instance, there is only one reasonable place where a bridge can be built; there is only one city center in the city, etc.…) The main challenge here comes from the requirement of generating comparison groups and from the fact that the statistics techniques need big samples. However, literature has dealt with these challenges by generating exogenous variation at the user level or taking advantage of time and geographic limits or the implementation in phases of the project. Some of the rigorous impact evaluations in this field are Cerdá et al. (2012) and Gonzalez-Navarro et al. (2010). The first one uses a natural experiment to study how a public transit system intervention to connect isolated low-income neighborhoods to the city's urban center affects violence. They find that the homicide rate declined by 66 percent more in treated neighborhoods than in the control. The second one uses a randomized control trial to study how street paving in Mexico raises housing values. Using expert's appraisals, they find that paving streets increases housing prices by 16 percent and land values by 54 percent. This dissertation contributes to that base of evidence by studding the effectiveness of two urban infrastructure interventions in Brazil. The study uses a hedonic prices and a quasi-experimental approach to infer how these interventions change the quality of life of the citizens. Results show that whereas transportation interventions show positive effects on the wellness of the population, no effect was found in the interventions of rehabilitation of public spaces.

The results of these studies contribute to generate a body of rigorous evidence on what is effective to foster development. This dissertation relies in experimental and quasi-experimental methodologies to identify the *causal* impact of the intervention on their intended *outcomes*.

The best method to ensure causality is running an experiment. Whereas experimental methods have been widely used in other disciplines, -such as medicine to study the effects of a new drug-, its use is relatively new in social sciences. Roughly, this methodology consists of assigning the program randomly to a group of eligible people, and leave a comparison group (or control group) of eligible people aside from it. Statistical laws ensure that, with big enough samples, a simple comparison of means of the variable of interest between the two groups is a good estimation of the impact of the program. For instance, in the example of the tablets, a group of students (or schools) randomly selected receive the tablets, whereas another group of students (or schools) do not. After the program is implemented the learning level of the two sets of students is measured (for instance using test scores). The difference on the average learning level of the two groups of students will be a good estimator of how much delivering tablets in school increases students' learning.

However, it is not always possible to randomize the treatment for ethical, political, or practical reasons. In those cases there are other methodologies that can be still used to identify causal impacts of the programs. These are the so-called quasi-experimental methods. They are weaker because they need to use more sophisticated statistical tools and rely on assumptions to identify a reliable comparison group that can be used to isolate the impact of the program.

Impact Evaluation has implied a substantial contribution to the *results-based policy framework*[2] that has been promoted in the past decades[3]. Firstly, it increases effectiveness of public policies. As a key part of the public cycle, impact evaluations influence and guide policy decisions providing information on what interventions are more effective to achieve the intended results. Secondly it improves accountability, since it is able to generate robust and credible evidence on performance. Thirdly, it allows learning from experience by generating a base of knowledge on what works and what doesn't based on solid and rigorous evidence. Finally it allows identifying and correcting failed policies in the public sphere, where there are no other kind of regulatory mechanisms (such as market would do in the private sphere). This is even more important in the case of international aid, since the beneficiaries of the policies are different from those who execute it[4].

However, this kind of evaluation also has limitations. For experimental methods some of them are the ability to generalize and replicate the results in different contexts, the inability to provide results of general equilibrium, and the possibility of obtaining biased results just because individuals are conscious that they are observed and that can affect their behavior. In the case of

[2] "A results framework is an explicit articulation (graphic display, matrix, or summary) of the different levels, or chains, of results expected from a particular intervention—project, program, or development strategy" http://siteresources.worldbank.org/EXTEVACAPDEV/Resources/designing_results_framework.pdf

[3] https://undg.org/wp-content/uploads/2015/01/UNDG-RBM-Handbook-2012.pdf

[4] Alonso (2014).

quasi-experimental methods the biggest limitations come from the fact that they need to pose (sometimes strong) assumptions that cannot be tested. All these limitations impose some caveats to the interpretation of the results, and it needs to be taken into account for policy makers when the evidence generated is used to redesign, escalate, or replicate the program that has been evaluated. Other practical limitations are the amount of time and monetary resources that are necessary to conduct some of these evaluations. However, when the outcomes respond quickly to the program and there are existing data, these limitations are well overcome.

Notice that despite traditional monitoring and evaluation, which has been majorly focused on the delivery of the inputs, outputs or intermediary results, impact evaluation focuses on the final outcomes, and uses quantitative methods to generate hard evidence and demonstrate causality. Although this implies a major contribution to the area of evaluation of public policies in many aspects, it should be acknowledge that impact evaluation is not a substitute for other kind of evaluations, but rather complement. Whereas the majority of the traditional evaluations aim to respond whether the money of the intervention was spent correctly or whether the program was properly implemented, impact evaluation responds to whether the program achieved the expected result on the outcome of interest demonstrating causality.

The dissertation is organized as follows: the first part is devoted to review the methodological framework of impact evaluations. The intention of the first part is to contextualize the methodologies that will be used in the empirical work; the second part of the dissertation is focused on the empirical work. This part includes three papers that constitute the main contributions of the dissertation. Chapter 1 studies the impact of a training program on labor outcomes in the short and long run and disentangles the contribution of the technical skills provided in the training. Chapter 2 studies the same program, but for a different cohort of students, focusing in how the program changed the contacts of the students and therefore the job seeking practices. Chapter 3 studies the impact of urban infrastructure interventions on social wellbeing.; the third part of this dissertation concludes.

PART I. REVIEW OF THE METHODOLOGICAL FRAMEWORK

Causal Inference:

One of the biggest challenges of the Social Science has been to identify causality. Policy makers of social programs aim to identify whether the policies that they are implementing are having the expected results over the outcomes of interest. The policy questions that they want to solve are of the type: Does the program "P" improve the outcome "Y"? This question can be transferred to many fields of social policy: For instance Does providing tablets to students improve their knowledge? Are programs that train unemployed effective to reduce unemployment? Do urban projects (such as creation of parks) improve the quality of life of the neighbors? The challenge here is to capture the impact of the program controlling for all the necessary factors in order to isolate the impact. The typical correlation analysis can help us to explain whether there is a relation between the program and the outcome, but this is not enough to demonstrate causality, i.e. that the observed effect is only caused by the program itself and no by other factors.

In recent years evaluation of public policies have used methodologies that try to capture the pure effect of social programs in the final outcomes. They have also been called Impact Evaluation Methodologies. They consist in isolating in the most rigorous possible way the effect of the program from any other phenomenon that can be affecting the final outcome. In order to do that, Impact Evaluation methodologies compare the situation under the public policy with its counterfactual, that is, how the situation would have been if the policy were not had been implemented. For instance, let's think of a program that provides training (T) to individuals to improve their salaries (Y). The policy question that arises to policy makers is What is the impact of the training in the salary of the beneficiaries?. Using impact evaluation methodologies, the answer to that question in formal notation for a given individual "i" would be given by:

$$Y_i^T - Y_i^C$$

Where Y_i^T is the salary of the individual when she takes the training and Y_i^C is the salary of the same individual when she doesn't take the training. These terms are the two potential outcomes of the individual, however, only one of them is observable: either the individual takes the training or she doesn't[5]. This makes that this problem is impossible to solve empirically at the individual level. However, it is possible to generate two groups of individuals that are statistically identical on average. In practice, impact evaluation compares two groups that are statistically indistinguishable. The question that the impact evaluation will answer is: What is the impact of the training in the average salary of the beneficiaries?.

$$E\left[Y_i^T - Y_i^C\right]$$

Since it is impossible to observe the counterfactual situation (because once the policy is implemented it is not possible to observe the outcome without the policy), Impact Evaluation Methodologies uses *control groups* to estimate the counterfactual.

Under the Central Limit Theorem and the Law of Large Numbers we know (elaborate), using a large number of sample the last expression converges to:

$$D = E[Y_i^T / Attending\ training\ ] - E\left[Y_i^C / No\ Attending\ training\right]$$
$$= E[Y_i^T / T\ ] - E\left[Y_i^C / \ C\ \right]$$

If we subtract and add $E\left[Y_i^C / T\ \right]$, that is the expected outcome for a subject in the treatment group had she not been treated, we obtain the following expression:

[5] This problem has been formally exposed in Rubin, 1974.

$$D = E[Y_i^T / T] - E[Y_i^C / T] - E[Y_i^C / C] + E[Y_i^C / T]$$

$$= E[Y_i^T - Y_i^C / T] + E[Y_i^C / T] - E[Y_i^C / C]$$

The first term $E[Y_i^T - Y_i^C / T]$ is the Treatment Effect that we are trying to isolate: what is the effect that the program had on those who were treated. This is also called the effect of Treatment on the Treated (ToT). The second term is the difference between those who are in the treatment group had they not been treated and those who are in the control group $E[Y_i^C / T] - E[Y_i^C / C]$. It is also called the *selection bias*. In our example it captures differences in potential salary of the treatment group had not been treated and the control group. If this term is different from zero, it means that the treatment group and the control group would have behaved differently in the absence of the treatment. For example, if it is greater than zero could be the case that the treatment group is more motivated than the control group and would have obtained a greater salary, even if they wouldn't have attended the training. On the other hand if the difference is negative could be because the training was offered to people that was less motivated and in the absence of the program would have earned lower salaries.

The problem that remains is that $E[Y_i^C / T]$ is not observable, therefore, it is not possible to calculate the selection bias, so it is not possible to know until what extent the result of the differences between the treatment and the control group is biased. In this sense, impact evaluation technics aim to generate a control group that is comparable to the treatment group, so that the selection bias disappears.

The better the control group is selected, the more best will be our calculation of D. The following sections describe two methods to select control groups.

Selecting Control Groups

In order to generate a valid control group (that is with no selection bias), at least three conditions need to be met. Firstly, the treatment and the comparison group must be identical in the absence of the treatment on average. Secondly, both groups should react to the program in the same way. Finally, the treatment and the comparison groups have to share the same environment during the treatment, for instance, the control group cannot be exposed to other interventions during the evaluation period. If these tree conditions are met, both groups will be comparable and the only difference between the two groups would be the exposure to the program. The methodologies that allow creating a valid control group can be categorized as experimental (random assignment) and quasi-experimental (differences-in-differences, regression discontinuity, matching, instrumental variables, and synthetic controls among others). In the following sections we will elaborate only random assignment and differences-in-differences, because those are the methods that will be used in the dissertation[6].

Random Assignment

The best way to ensure that the conditions mentioned above are met is by assigning the program randomly among the population of interest (also called eligible population). For instance, if we want evaluate a program that foster employment among unemployed youth, the eligible population will be people that is within certain age range (let say between 15 and 30 years old) and that are unemployed. Then we can run a lottery to assign the treatment within the pool of the eligible population and determine randomly who, out of the pool of eligible population, belongs to the treatment group and who belongs to the control group. Under this random assignment

[6] More detail on other methodologies can be found in Angrist and Imbens (1994), Card (1999), Imbens (2004), Todd (2006), Ravallion (2006).

process, with a large enough sample, the Central Limit Theorem[7] states that the average of the two groups is distributed normally (regardless of the distribution of the population), and the Law of Large Numbers[8] states that with a big enough sample, the average of the two groups are statistically equal to the expected value of the population. Therefore, if the treatment group is exposed to the intervention and the control group is not, any change in the average can be attributed to the treatment. If we can observe the samples of the two groups, we can estimate their averages and make the difference to calculate the impact of the program.

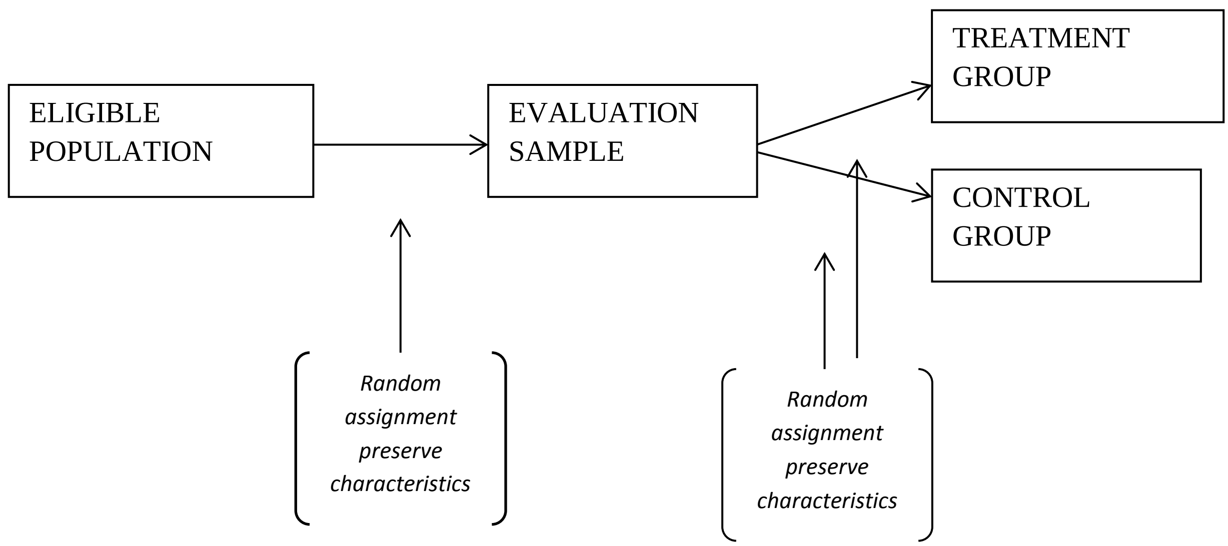


Figure 1. Random Assignment Outline

[7] The Central Limit Theorem says that if the sample size is sufficiently large, then the mean of a random sample from a population has a sampling distribution that is approximately normal, regardless of the shape of the distribution of the population. As the sample size increases, the better the approximation will be.

[8] The Law of Large Numbers states that as the number of identically distributed, randomly generated, variables increases, their sample mean (average) approaches their theoretical mean.

Let's use our example of the training program that tries to increase earnings. Imagine that out of the pool of eligible population we draw a random sample for our study of size N. If N is large enough we know that the average of any variable our sample, for instance earnings ($\bar{Y}_N$), is statistically equivalent to the expected value of the earnings of the eligible population (E[Y]). This experimental sample is then assigned randomly in two groups: N_T individuals will be assigned to the treatment group and N_C individuals will be assigned to the control (or comparison) group. The treatment group is then exposed to the training while the comparison group is not. Then we calculate the average of the earnings is of the two groups. Then, the average treatment effect of the training on the earnings can be estimated as the difference in the sample means of the earnings between the two groups.

Let's formalize it:

$$\widehat{D} = \hat{E}[Y_i/T] - \hat{E}[Y_i/C]$$

Where $\widehat{D}$ represents the average treatment effect on the population, Ê represents the average of the variable (outcome) Y on the sample. By the Law of Large Numbers, as the sample sizes increases, the difference converges to

$$\widehat{D} = E[Y_i^T/T] - E[Y_i^C/C]$$

As we said before, since the treatment has been randomly assigned, the expected value of the outcomes of the two groups only differs through their exposure to the treatment. That is, had neither received the treatment, their outcomes would have been in expectations the same. This means that the selection bias is equal to zero. Additionally, if the potential outcomes of the individuals are unrelated to the treatment status of any other individual[9], and this happens if we randomize treatment, then we have the causal parameter of interest for treatment T:

$$E[Y_i/T] - E[Y_i/C] = E[Y_i^T - Y_i^C/T] = E[Y_i^T - Y_i^C]$$

We can express this as a regression:

$$Y_i = \alpha + \beta T + \varepsilon_i,$$

Where Y_i is the value of the outcome for individual i, T is a dummy variable that determines the treatment status of the individual, α and β are the parameters to estimate, and ε_i is the error term for individual i.

If we estimate the equation using ordinary least squares, the estimated coefficient $\hat{\beta}$ will determine the difference in the outcome variable between the treatment and the control group, that is, the average treatment effect:

$$\hat{\beta}_{OLS} = \hat{E}[Y_i/T] - \hat{E}[Y_i/C].$$

Notice that what we are estimating is the overall impact of the treatment/program on a specific outcome, allowing everything to change in response to the treatment/program. This may be different from the impact of the program on an outcome keeping everything else contact.

Let's consider that the production function for the outcome of interest Y is $Y= f(I)$, where $I=(i_1, i_2, \ldots i_n)$ is a vector of n inputs. Consider a change in one element of the vector, for instance i_t. We

[9] This is the "Stable Unit Treatment Value Assumption (SUTVA) described in Anfrest, Imbens, and Rubin (1996)

can estimate how i_t affects Y when all other explanatory variables remain constant (that is the partial derivative of Y with respect to i_t), but also, we can estimate the change in Y including changes in other inputs that may change in response to the original change i_t (that is the total derivative of Y with respect to i_t). The latter estimation is interesting form the policy point of view because it shows what happens "in reality" when we change an input exogenously, allowing agents to re-optimize. However, the total derivative may not provide an estimate of the overall welfare effects on the outcome of interest. In order to see them, we need to do the partial derivative. However, partial derivatives can only be calculated if there is a model that links various incomes to the outcome of interest and collect data on these intermediary inputs. This implies that randomization needs to be combined with theory.

It is important to keep in mind that results from randomized evaluations (and from other internally valid program evaluations) provide reduced form estimates of the impacts of the treatment, and these reduced form parameters are total derivatives.

Limitations of Random Assignment

Despite the powerful results that these evaluations can provide, there are some limitations. In first place, despite it may be very useful for determining the impact of a specific program on certain population in a specific context, these results may not always be generalized to other populations or contexts. This can happen because (i) different populations may respond differently to the program (because of personal, cultural or cyclical reasons): for instance, youth in the Caribbean may not react to the program in the same way that youth in Africa, or people during a recession may not react to the program in the same way as people during an economic boom; (ii) the program has some differences in design: for instance, sometimes programs need to be adapted to the context and deviations of the design can cause differences in the final outcomes; or (iii) the program is implemented differently from the design: for instance, sometimes impact evaluations are conducted at the pilot stage of the program, and usually pilots are implemented with special care. If the program is escalated, it is likely that there are deviations from the original design that may affect the results on the outcomes of interest. In second place, these evaluations measure only partial effects of the program on recipients and don't measure the general equilibrium effects on the population[10]. For instance, in our example of the tablets, the evaluation cannot estimate what is the impact of providing tablets in the teaching method or education system. On the one hand we could think that the insertion of tablets in schools may foster the use of other technologies, whereas on the other hand it could be argued that education will focus on the use of tablets leaving other technologies aside. In third place, mostly for prospective evaluations, the fact that an impact evaluation is being conducted may affect the behavior of the individuals causing a different result to the one that would have happened without the evaluation. This can happen because individuals know that they are being observed and that can affect their behavior. These effects are known as the Hawthorne and John Henry[11] effects. All these factors impose caveats for the interpretation of the results and policy makers need to be thoughtful when they use this evidence to escalate or replicate their policies. Other limitations on a more practical side are the amount of resources in terms of time and money that is requires conducting some of them. The time constraint comes from the fact that sometimes it is necessary to wait sometime for the program to show results, and sometimes the policy response may be needed sooner (for instance in the example of the tables it is reasonable

[10] Heckman, Lochner, and Taber (1998).

[11] The Hawthorne effect refers to the treatment group and the John Henry effect refers to the comparison group.

to think that effects on learning are not going to happen immediately but after a few months). On the cost side, the most expensive part of these evaluations comes from the necessity to generate data when it doesn't exist, however, the cost decrease dramatically when administrative data are used.[12]

Difference in Differences

Although random assignment is the most reliable way to create a valid comparison group (because it avoid the selection bias), it is not always possible to randomize in practice. In these situations we can use other methodologies that allow creating comparison groups[13]. The main difference between randomization and other methodologies is that the latter are only valid under identifying assumptions, and those assumptions are not always testable, what make these methodologies weaker than randomization.

Sometimes there is no possibility to randomize for political or practical reasons. Other times a program is already implemented by the time that the evaluation has to be conducted. Even more, sometimes, the assignation rules to the treatment are not even known. In those cases it is still possible to create a good comparison group under certain conditions.

In those cases, if we count with data of the treated population, as well as a population, not necessarily similar to the treated population, but that evolves similarly over time, i.e. that is exposed and reacts equal than the treated population to the factors that changes over time, we can estimate the impact of the treatment using the methodology of differences in differences (DD). This methodology use pre-period differences in outcomes between treatment and control group

[12] Coalition for Evidence (2012).

[13] More detail on other methodologies can be found in Angrist and Imbens (1994), Card (1999), Imbens (2004), Todd (2006), Ravallion (2006).

for control pre-existing differences between the groups. In order to do that it is crucial to count with data of the outcome of interest before and after the treatment is implemented for the two groups.

Let's denote Y_j^T the outcome for the treated and Y_j^C the outcome for the control (never treated) for periods j=0, 1, where 0 is the period before the treatment is implemented and 1 is the period after the treatment is implemented in the treatment group (notice the control group never receives the treatment).

The key assumption is that without the treatment, the two groups would have followed parallel trends, that is:

$$\hat{E}[Y_1^C/T] - \hat{E}[Y_0^C/T] = \hat{E}[Y_1^C/C] - \hat{E}[Y_0^C/C]$$

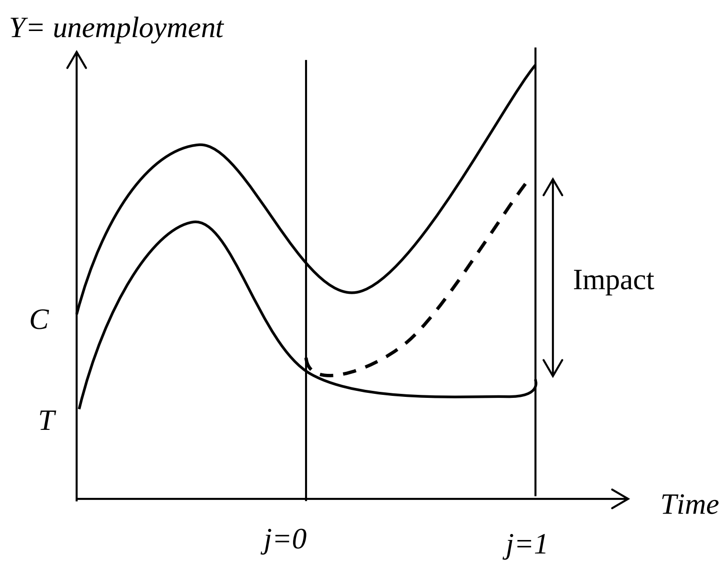
Then, the DD estimator is[14]:

$$\widehat{DD} = [\hat{E}[Y_1^T/T] - \hat{E}[Y_0^C/T]] - [[\hat{E}[Y_1^C/C] - \hat{E}[Y_0^C/C]].$$

Figure 1 below illustrates how the estimator works. Imagine we are trying to evaluate a training program that aims to reduce unemployment for youth. Our variable of interest Y is unemployment. Imagine we can observe unemployment rates of the youth and of older people in the population. Unemployment rates are traditionally larger for elder people than for youth, but

[14] Notice that the fixed effect estimator is a generalization of DD when there is more than one time period or more than one treatment group. The fixed effects estimates are obtained by regressing the outcome on the control variable, after controlling for year and group dummies.

both rates are exposed to the same economic shocks and we can observe that they evolve in parallel. We observe that after the treatment the unemployment rate for the treated decrease dramatically. If we simply compare the unemployment rate of the elder with the unemployment rate of the youth, we wouldn't be taking into consideration that unemployment is always larger for the old and we would be overestimating the impact. On the other hand, if we only compare the unemployment rate of the youth after and before the treatment, we would be underestimating the results because we won't be taking into account that other factors during the time would have made the unemployment rate even bigger than it was at moment 0 (as seen in the trend of the control). The key assumption here is that after the treatment, they unemployment rate of the treatment group (in this case the youth) would have followed the same trend than the unemployment rate of the elders. Figure 1 illustrates how the impact is calculated by projecting the trend of the control group on the treated, and using the



Source: Author

Figure 2. Graphic Representation of Difference in Difference

Note that the counterfactual being estimated here is the change in outcomes for the comparison group. The treatment and comparison group don’t necessarily need to be equal before the treatment, but they need to evolve with same trends.

Limitations of Differences in Differences

The key assumption here is that the treated group would have evolved in the same way than the comparison group in the absence of the treatment. The biggest limitation of this methodology is that this assumption is not testable. What is usually done - if there are enough historical data-- is to test that both trends evolve with same trends in the past. However notice that it doesn’t imply that this is going to happen in the future.

In a more pragmatic side, another practical limitation is the existence and accessibility of data. Despite these evaluations usually use administrative data, sometimes it doesn’t exist in the form or the frequency that is required, and other times there are challenges to access the data from the owners.

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PART II: EMPIRICAL RESULTS

CHAPTER 1: Soft Skills and Hard Skills in Youth Training Programs. Long Term Experimental Evidence from the Dominican Republic

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[15] This document is based on the impact evaluation of the 2008-2009 cohorts of the Programa Juventud y Empleo - República Dominicana, "Youth and Employment Program, Dominican Republic. Impact Evaluation Report for the 2008-2009 Cohorts," prepared by Evelyn Vezza, Brígida García, Guillermo Cruces and Julián Amendolaggine. Juan Martín Moreno provided comments throughout the whole project, and Brigida García provided crucial inputs for the preparation of this document. Funding for this evaluation was provided by BNPP, GAP and SIEF. The report and this paper build on the work of professionals from the Dominican Republic's Ministry of Labor and staff and consultants from the World Bank. These teams designed and carried out the intervention, its evaluation strategy and the data collection tools, and produced documentation that has been partially incorporated to this document. The members of these teams from the Ministerio de Trabajo, República Dominicana are: José Luis Polanco (PJyE), Douglas Hasbún (PJyE), Brígida García (PJyE), Isabel Tavernas (INFOTEP). From the World Bank: Paul Gertler, Co-Principal Investigator, Sebastian Martinez - Co-Principal Investigator, Juan Martín Moreno - Team Task Leader, Cornelia Tesliuc - Original Task Team Leader, Paloma Acevedo - Impact Evaluation Manager, Rodrigo Muñoz - World Bank Consultant for survey and data quality control, Juan Muñoz - World Bank Consultant on baseline survey and data quiality control, Carlos Asenjo - Coordinated Life Skills survey module, Myra Brea - World Bank Consultant on Life Skills survey module, Sigrid Vivo - World Bank Consultant for initial experimental design, Gustavo Bobonis - World Bank Consultant for initial experimental design, Mary Claux - World Bank Consultant on Life Skills survey module, María Isabel de la Rosa - World Bank Consultant on Life Skills survey module. G. Cruces acknowledges financial support from the research projects "Fostering capacities in Impact Evaluation in Latin America" and "Enhancing Women's Economic Empowerment through Better Policies in Latin America" (both projects by CEDLAS with the support of the International Development Research Centre-IDRC, Canada) for a visiting period at UC Berkeley's Center for Effective Global Action. The Center's support for this visit is also gratefully acknowledged.
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Abstract

This paper presents the short term and long term impact the Dominican Republic's "Programa Juventud y Empleo," a job training program for youth at risk. This paper makes two main contributions. On the one hand, the experimental evaluation was designed to gauge the effect of different components of the program. Participants were randomly assigned to two types of training: one included classroom-based vocational training and life-skills elements, while a second group was offered only the life-skills component (both types also included internships with private employers). On the other hand, we study impacts in the short term (from 12 to 18 months after the program, the typical time frame for the evaluation of programs of this type) but also, for the long term (3.5-4 years after the program). The results indicate sizable employment and wage gains for women, and employment losses effects for men in the short term. These effects dissipate in the longer run. In fact, male participants exhibit significantly lower levels of formal employment and higher levels of on the job search in the long run. Female beneficiaries who work exhibit higher levels of job satisfaction in the short run and all females seem more optimistic about job and life prospects in long run, been specially optimistic those who received the vocational training. We find negative effects on self-esteem for men in the long term and no effect for women. Female participants' basic skills and personal characteristics, which were objectives of the life skills training, increased substantially in the long run. The short run labor market effects are similar for the two treatment groups, suggesting that these effects can be attributed to the combination of the life skills module and the internship, which were common to the two versions of the program. The hard skills classroom training module induced few differential effects of the program in the short run, although it induced a larger increase in women's expectations in the long run compared to those who took life-skills training only.

JEL Classification: J08, J24, J31, J68.
Keywords: job training, field experiment, youth employment, cognitive and non-cognitive skills.

1. Introduction

Over the last two decades, young people have become one of the groups most targeted in innovative social policy, with a rise in global and regional active labor market programs that aim to improve employment among this group. In both developed and developing countries, the challenges youth face in their transition to employment have been recognized, and have inspired action from the public sector. In developing countries, policy interventions have centered on youth at-risk, including low income youth who have not completed their education, are poor or have experienced poverty, and are either unemployed or working under precarious conditions (see Almeida 2012, for an overview of these initiatives). However, evidence regarding the effectiveness of these programs in developing countries is still relatively scarce. A notable exception is the "Programa Juventud y Empleo" (Youth Employment Program, hereafter, PJyE), part of a series of initiatives created by the government of the Dominican Republic that have attempted to mitigate youth unemployment. The PJyE figures as one of the region's pioneer programs aimed at addressing to the problems faced by youth at-risk. The program was implemented by the Ministry of Labor with funding from international institutions (IDB and World Bank). It began in 2001, and since that time, has undergone several revisions and modifications, although it remains focused on the same demographic target. Other early programs include Chile Joven, Jóvenes en Acción in Colombia (Attanasio et al., 2011), and PROJOVEN in Peru among others. Unlike most programs of its kind, PJyE has incorporated an experimental evaluation design for several of its different editions, allowing for a precise identification of its causal effects (Card et al., 2011). The program has also included several innovations, for instance the incorporation of "soft" skills as a complement of the more traditional vocational training features of programs of its type.

The PJyE main objective is to improve the employment opportunities of at-risk youth by building their technical skills, work experience and life-skills. To do this, the program enrolls participants in training and internships in the private sector. The target population is individuals between 16 and 29 years old who have not completed secondary school and are unemployed, under-employed or inactive, and who come from the poorest 40% of households (according to the government's information system for social assistance, SIUBEN). Since its beginnings in 2001, the program had several designs. In the 2008-2009 editions on which this paper focuses, training activities address both theoretical and practical module (TTP) dedicated to the development of specific technical and vocational skills and a module of basic skills (DCB) to strengthen non-cognitive abilities (values, attitudes and basic interpersonal skills). The vocational (or technical/"hard") skills module included 150 hours of training in occupations such as sales, beauty salons, tourism and hospitality, carpentry, electricity and others. The life skills (or "soft" skills) component included a shorter module of 75 hours and focused on promoting self-esteem and self-realization, communication skills, conflict resolution resources, life planning, time management, team work, decision making, hygiene and health, and coaching on risky behaviors. All participants were also assigned to 240 hours apprenticeships in private companies, for which they received a daily stipend of about US$2 and basic insurance.

Evidence obtained from previous PJyE cohorts suggests that the program generates greater effects among certain sectors and specific groups, and that impact is concentrated on job quality and salary for those employed, but the program does not affect significantly the employment status of beneficiaries (Card et al., 2011; Ibarraran et al., 2014). The objective of this paper is to evaluate the effects of PJyE on the 2008-9 cohorts, particularly to identify the differential effect of the main innovation of that edition, the incorporation of two components of the training program (technical and vocational skills and basic skills) in a way that allows us to gauge their relative contribution to the beneficiaries on labor market and other outcomes. The main feature of the 2008-2009 two cohorts of the program was the implementation of two types of training: one included the TTP classroom-based vocational training and the DCB life-skills elements, while a second group was offered only the DCB life-skills component (both types also included internships with private employers). The experimental impact evaluation relies on the random assignment of participants to a control group or to one of the two types of training. Another notable feature of the program and its evaluation is that we study impacts in the short term (from 12 to 18 months after the program) but also, exceptionally for a developing country using survey data, also for the long term (3.5-4 years after the program).[16] We examine both medium and short term effects over a broad set of outcome variables.

The results indicate sizable employment and wage gains for women, and employment losses effects for men in the short term. These effects dissipate in the longer run. In fact, male participants exhibit significantly lower levels of formal employment and higher levels of on the job search in the long run. Female beneficiaries who work exhibit higher levels of job satisfaction in the short run and all females seem more optimistic about job and life prospects in long run, been specially optimistic those who received the vocational training. We find negative effects on self-esteem for men in the long term and no effect for women. Female participants' basic skills and personal characteristics, which were objectives of the life skills training, increased substantially in the long run. The short run labor market effects are similar for the two

[16] Card et al.'s (2011) meta-review classifies the timeframe of programs of this type as "short-term impact estimate measuring the effect on participant outcomes approximately one year after the completion of the programme", a "medium-term estimate giving the effect approximately 2 years after completion", and "longer-term (3year) impacts".

treatment groups, suggesting that these effects can be attributed to the combination of the life skills module and the internship, which were common to the two versions of the program. The hard skills classroom training module induced few differential effects of the program in the short run, although it induced a larger increase in women's expectations in the long run compared to those who took life-skills training only.

These results allow us to draw some conclusions. The impact evaluation of training programs should explicitly attempt to follow beneficiaries over a broader horizon. Our results in the short term coincide with other similar programs evaluated over similar time periods in the region (for instance, employment gains in the short run). However, most of these employment gains dissipate in the longer run, which contrasts with the available evidence for developed countries, which typically finds positive medium-term impacts of training programs that often appear ineffective in the short term (Card et al., 2010).

The evaluation design also allows us to establish that, at least in these PJyE editions, the classroom- based vocational training was not very effective, even when (as in the PJyE) it was discussed and developed jointly with private sector employers. The program seems to have been more successful in raising expectations and basic skills rather than on changing labor market outcomes in the longer run. The positive impact of the program on soft skills indicates that private sector training providers might be more effective in transmitting general cognitive skills and developing non-cognitive abilities than in fostering specific vocational competences. Moreover, the positive employment gains in the short term are compatible with a setting in which the effects on employment are due to the program's implicit labor intermediation (through the internship) rather than on the training components.

This document is structured as follows. Section 2 presents a summary of the PJyE program, including a description of its previous versions, of the specific aspects of the cohorts that will be evaluated here, and of the outcomes of interest for these cohorts. Section 3 describes the evaluation design, including the random assignment procedure, sample selection, and further details about modifications made in the 2008-9 cohorts. Section 4 describes the data sources and the estimation strategy. Section 5 details both the short and long term empirical results. The final section presents some conclusions from the analysis.

2. The Program

2.1. The Original Program Design and Previous Evaluations

The PJyE was created in 2001, at which time it fell under the auspices of an initiative funded by the Inter-American Development Bank (IDB) called the "Programa de Capacitación y Modernización Laboral." The PJyE functioned as a job-training component of this program. Although the original loan that funded this initiative concluded in 2006, the PJyE had a second phase of financing by the IDB in 2007-8.[17]

The motivation for the program was the relatively high level of unemployment for youth. The aggregate unemployment rate was relatively low at 4.7% in 2000 and 5.5 in 2001, but the respective rates were substantially higher for youth: it was 9.2% in 2000 and 11.4% in 2001 for those aged 15 to 24, while it was 3.6% and 4.1% for the same respective years for those aged between 25 and 65 (SEDLAC-CEDLAS and World Bank, 2014). The primary goal of this early

[17] This second initiative was carried out as part of the Programa de Mercados Laborales y Transferencias Sociales.

job training initiative was to address problems surrounding labor insertion by offering training in specific skills that were considered in demand by the private sector.

The original PJyE program targeted low-income youth between the ages of 16 and 29 who experienced difficulty finding employment, and who had not completed secondary education. A special effort was made to target women[18]. The program funded training in two phases: an in-classroom training phase and an internship phase, and also financed participants' transportation, a stipend, medical and accident insurance. The first courses were held in 2002. During the 2002-8 periods, the IDB financed the program for 27,500 beneficiaries, of which 57.7% were women (IDB, 2006). From 2008 to 2013, the program has been financed by the World Bank and has conducted an additional 1,924 courses. In total, the program has conducted 3,627 courses since 2002. The courses and internships were administered by private providers, Centers for Operating System (COS) - see the following subsection for more details.

One of the most innovative aspects of the original PJyE structure and of several of the subsequent versions was the inclusion from the onset of an experimental impact evaluation design based on the random allocation of potential beneficiaries to treatment and control groups. While this has been a feature of several training programs in developed countries, such as the Job Training Partnership Act and the Job Corps in the United States, this type of experimental design was relatively uncommon in active labor market programs in Latin America. Individuals applied to receive benefits by filling out an application form, which was in turn used to check applicants' socioeconomic and work background and confirm they met all program requirements. Following this initial screening, participants were selected randomly and two groups were generated: the

[18] At least 45% of beneficiaries would be women, and this ratio would be also applied for the randomized selection of applicants.

first group was composed of individuals enrolled in the program and the second group was composed of those who qualified, but were not selected to participate. The impact evaluations of previous versions of the program relied on representative sample from both groups, which were polled in several follow-up surveys to measure the program's effect on outcomes of interest.

Experimental evidence for previous editions of the PJyE is available for both the 2004 and the 2008 cohorts. Results for the 2004 cohort were obtained by comparing the results of baseline surveys and the follow-up surveys conducted between 10 and 14 months after the end of the course. The 2004 program had statistically significant but modest effects on the salaries of those youth who were employed and had been selected for the program, as compared to those who were employed and had not been selected. Analysis also shows improved quality of work for program participants as compared to non-participants with similar levels of education, (here provision of health insurance was used as the measure of work quality). However, there was no statistically significant effect on employment indicators (Card et al., 2011).

The results from the first evaluation informed the design of future versions. In keeping with its innovative tradition, the new version of the PJyE incorporated explicitly the results from the literature that stressed the importance of non-cognitive ability and life-skills in the labor market (Heckman, Stixrud and Urzua, 2006). A second evaluation was conducted for the 2008 cohort. This cohort experienced a modified version of the program in which there was a more substantial focus on basic non-cognitive skills as compared to employer-recommended training. The baseline survey and the household survey taken 18 to 24 months following the completion of training were compared using a representative sample of selected participants and non-selected applicants. Analysis showed that PJyE had significant positive effects on job quality among men (defined as a job with health benefits) and in salaries among those individuals who were already employed. Compared to the control group, selected participants also demonstrated improved perceptions and expectations of the future, as well as improved non-cognitive abilities. Studies of this cohort also found a reduction in teen pregnancy among participants (Ibarrarán et al., 2014). However, there were no significant effects on overall employment, as in Card et al. (2011).

2.2. The 2008-2009 Cohorts: Specificity and Outcomes of Interest

The PJyE follows what Card et al. (2011) call the "Chilean model" of job training programs in Latin America, were private institutions (rather than employers) provide classroom training and arrange for internships for beneficiaries. The COSs are private institutions authorized by the National Institute for Professional Training (INFOTEP for Instituto Nacional de Formación Profesional). In addition to certifying the COS, INFOTEP controlled curriculum content of courses offered in the PJyE. The Ministry of Labor (MT), particularly the Program Coordination Unit (UCP for Unidad Coordinadora de Programas), oversaw the program and the COS conducted the courses and ensured they met the supervising agency's standards. The COS not only oversaw instruction but also coordinated with companies where internships were arranged and adjusted training contents to suit the needs of the private sector. COS also promoted the program in the targeted priority areas, maintained the applicant registries, and evaluated applicant eligibility, ensuring that each individual in the lottery complied with the program's basic requirements. The UCP further complemented these actions by providing a second review of the applicant registry and examining each candidate's application for inconsistencies.

Previous versions of the program incorporated specific elements to develop life skills and other general cognitive and non-cognitive abilities, and these have been incorporated as additional outcomes of interest (beyond the typical labor market outcomes) in evaluations of previous versions of the program, as detailed in Ibarraran et al. (2014). The main innovation of the 2008-2009 cohorts of the program under World Bank funding was its evaluation strategy, built into the program. This strategy consisted in offering a group of participants both vocational and soft skills, and only soft skills to another group (both of them included an apprenticeship). This design allows separating the differential effects of the traditional hard skills elements from those of the relatively newer soft skills components of the program, as detailed in the following section.

As a job training program, labor market outcomes, such as employment, labor force participation, type of employment, type of contract, and wages (among others), constitute the first set of outcomes of interest. The program emphasized “soft skills”: in fact, one of the groups of beneficiaries did not receive any vocational training, which has usually been considered a cornerstone of job training programs. For this reason, a second set of outcomes is related to perceptions and expectations in the labor market, such as job satisfaction and expectations about work prospects, and to indirect effects of any potential impact on labor force participation (for instance, fertility outcomes). A third set of outcomes of interest is more directly related to the life skills component of the program. On the one hand, we will evaluate its effect on life satisfaction, self-esteem and expectations in general, as well as participation in organizations, satisfaction with interpersonal relations, and non-cognitive skills. On the other hand, given the content of the specific modules carried out in the 2008-2009 cohorts, we will also analyze any potential impact of the program on attitudes and risk behavior.

3. Evaluation Strategy: Program Characteristics and the Random Assignment Process

3.1. Eligibility and Program Characteristics of the 2008-2009 Cohorts

Young persons registered to the program were considered eligible if they met the following qualifications: participants must be between the ages of 16 and 29, found to be at-risk, and were Dominican Republic citizens in possession of a personal identification card. At-risk was defined as either unemployed, under-employed or inactive, or not having completed either secondary school or "adult education."[19] Moreover, applicants must have income levels and place of residency categorized as below the poverty line. These measures and restrictions were put in place in order to guarantee that the PJyE accurately targets the poorest sectors of the population.

3.2. The Random Assignment Process

As in previous editions of the program, there were more applicants than vacancies available for each cohort. This situation facilitated the random selection of beneficiaries from the pool of applicants, since a lottery is an inherently fair way to allocate limited places. The main difference of the 2008-2009 WB cohorts was the evaluation strategy, which was designed to identify the differential effect of the two components of the training program (technical and vocational skills and basic skills only) and to gauge their relative contribution to the beneficiaries' labor market and other outcomes. The applicants were not simply divided into a control group and a treatment group: once determined to be eligible, the beneficiaries were also randomly assigned to one of two possible versions of the program: one included the TTP classroom-based vocational training and the DCB life-skills elements, while a second group was offered only the DCB life-skills component (both types also included internships with private employers). The experimental

[19] The education level of the beneficiaries had to be below the overall average level. A maximum of 30% of young persons registered could be composed of youth that fit the other requirements but were attending adult education courses or distance learning.

impact evaluation relies on the random assignment of participants to a control group or to one of the two types of training.

The random assignment process was accomplished by means of a lottery under the coordination of the UCP. Its implementation was delegated among various actors with shared roles and responsibilities. The lottery was conducted in two stages. The COS needed to obtain 35 applicants for each of the 520 courses that were organized in the 2008-2009 cohorts. Applicants' names and ID numbers were released to the UCP to be entered in a computerized random lottery. In the first stage, out of the 35 applicants per course, the program selected 20 individuals at random that were offered the TTP+DCB treatment. Gender rates were maintained among the participants[20]. Therefore, at the first stage, twenty individuals were informed that they would participate in the TTP+DCB course and 15 individuals were temporarily placed on the waiting list, from which they could be called in as a replacement if slots opened up over the course of the first ten days of instruction.

Once courses with two modules (TTP and DCB) were formed, a second lottery was conducted. From the pool of 15 applicants that were not selected in the first lottery, 5 participants were randomly selected for the second version of the coursework phase[21]. This group only received DCB instruction. These 5 participants were joined to participants from other courses (usually from the same COS) making up a total of 20 individuals per DCB course[22]. Once the courses started, there was a 10 days grace period to replace participants that drop out. Only five

[20] In other words, if a third of the applicants are male, then a third of the spots would be randomly assigned among male applicants and two thirds would be randomly assigned among female applicants.

[21] This second lottery was conducted only in COSs that were sufficiently large.

[22] In some cases, when COS were smaller, DCB courses were formed by integrating three courses, which is to say with 15 individuals. Thus integrated DCB courses were made up of individuals of different COS when it was considered operationally convenient.

replacements were permitted per course in total, making up the waiting list. The remaining five participants made up the control group. Figure 1 illustrates the process.

Thus, a group of applicants were randomly selected to participate in the PJyE's first mode of training (TTP+DCB) and the internship; and another group of applicants were selected to participate in the second mode of training (DCB) and the internship. This stratified treatment plan allowed for the measurement of potential effects of the DCB as compared to the TTP and enabled the possibility of studying the cost-effectiveness of each module. Finally, the participants that were not assigned to either program became part of a control group. Of the more than 20,000 young people that applied for the 2008-2009 cohorts of the PJyE, 16,373 fulfilled the eligibility requirements and were selected by their respective COS to be part of the random lottery assignment. Of this group, by means of random selection, 10,397 were selected for the first model (TTP+DCB). Of the 5,976 applicants that were not selected, 1,604 were randomly selected for the second model (DCB) and the remaining 4,372 remained part of the control group.

4. Data Sources and Estimation

4.1. Data Sources

The data used in this study come from three separate instances of data collection, as illustrated in Figure 2. First, upon applying to PJyE, prospective participants had to complete an enrollment application form, which was akin to a survey of basic individual and household socioeconomic characteristics, and contained also some information on labor market outcomes. This source was created when the Ministry of Labor began the program lottery and the COS began the process of applicant registration. Each COS conducted a preliminary screening of candidates who expressed interest in enrolling in the courses, to ensure that they met the program's target criteria.

Eligibility screening included a crosscheck of the applicant's identity card with the official national identity database, as well as other sources of auxiliary information. The UCP also intervened on occasion to help confirm an applicant's eligibility and supervised promotion of the program and pre-selection of youth by crosschecking each of the courses' participants with other available data, prior to enrollment. Information gathered in the application form created the baseline dataset against which program effects were measured. The form was a necessary condition for applying, and it requested information about employment and educational history, participation in social networks, motivation, characteristics of other household members and housing conditions.

Following this initial screening, COS selected the application forms from qualified candidates. 16,373 applicants filled out the application form and passed eligibility requirements. Applicants enrolled in 2009 composed the majority (64%). From this baseline group, treatment and control groups were selected.

The second and third data sources consist of telephone and household surveys conducted after the program's selection process, and targeted to a random sample of individuals in both treatment and control group from all individuals of the baseline group. The UCP and the COS created a representative sample from the eligible applicant pool, divided into a treatment group and a control group, which would be monitored and followed after the program. The size of the evaluation sample was calculated by considering the distribution of applicants among the 520 courses and establishing a minimum of applicants per course to maximize the ability to observe changes in the main outcomes of interest (labor market outcomes and cognitive and non-cognitive abilities), considering the budgetary restrictions for the surveys at the individual level[23]. Finally the selected sample included 4,700 young people, of whom 1,638 applicants conformed to the TTP+DCB treatment, 1,613 to the DCB treatment and 1,449 applicants belonged to the control group (see Figure 1)[24].

Based on this random sample of the control and treatment groups, three rounds of a telephone survey were implemented shortly after the start of the program (see Figure 2). The purpose of these telephone surveys was to keep in touch with participants, and to evaluate the short term results from the program. The three rounds were conducted with the same short questionnaire which included only a minimum set of questions on labor market outcomes and more general perceptions and expectations. In most cases, individuals in the sample were reached using a Computer Assisted Telephone Interview (CATI), which was supplemented by personal interviews for a sub-sample of young people who could not be reached by telephone[25]. The main purpose of conducting three rounds of this longitudinal survey was to maintain contact information, and, thus, only a few questions on employment outcomes and risk behaviors were included. With regard to labor market outcomes, the surveys asked if the individual was employed, actively seeking work, and, if employed, the number of hours they worked, their

[23] The statistical analysis program Optimal Design Software was used to determine the minimum number of individuals per course that would be required to find the effect of indicators of interest at 80% (90% in the case of employment rates).

[24] Over the course of implementation of the randomized design, the groups that were initially defined became revised due to difficulties in the treatment group formulation. During the first days of the course it was permitted to replace students who were chronically absent or who dropped-out. The Information System of the PJyE (SIPJyE) only kept registration of selected applicants in treatment or control once replacements had been made. Thus, the lottery used is not strictly the original lottery, but rather the selection in place 10 days following training beginnings. During this period of corrections (guided by the UCP) following the initial lottery, there was an increase in the number persons that went through the lottery process for treatment and, conversely a reduction of the size of the control group.

[25] The size of this sub-sample was 10% of the total sample.

wages and job satisfaction. Participants were also asked how many children they had, about risky behaviors and their expectations for the future.

These rounds of surveying were conducted at different intervals between November 2009 and February 2011. The first round of surveys covered the period from November 2009 to March 2010, the second covered the period from May to July 2010 and the third covered the period from November 2010 to February 2011. The response rate improved to 90% when both telephone and personal interviews were used. Most beneficiaries from the 2008 and 2009 cohorts were participating in the program during the first round of the telephone survey, and a significant number were also enrolled at the time of the second round. For this reason, the analysis of short term impacts in Section 5 is based on data from the third round of the telephone survey.

Finally, between October 2012 and March 2013, the Ministry of Labor conducted a household survey to the same sample of individuals. The survey, called "The Encuesta de Hogares para la Evaluación de Impacto de Programa Juventud y Empleo", included a long and detailed questionnaire covering several dimensions where the program could be expected to have an impact. This household survey covered outcomes reflecting the long term impact of the program, since it was implemented 3.5-4 years after the program. The questionnaire asked individuals about the level and quality of employment and sought to assess the impact of training on aspects of life beyond job opportunities, including risk behaviors, attitudes about personal development and health, participation in social networks, and life skills, in general. It was not possible to contact the entire sample and thus, the response rate was lower than in the first case, although it still exceeded 80%. Contrasting the final measurements with the baseline data illustrates that data loss in this study stayed at acceptable levels, and as detailed below, the attrition patterns were similar for the treatment and control groups and was not related to the treatment.

The three datasets (the baseline registries, the short term telephone survey and the long term household survey) made up the data for evaluating the differences in outcomes of interest between the treatment groups and the control group.

4.2. Evaluation Sample: Characteristics and Experimental Balance

The analysis of the characteristics of the individuals applying to the 2008-2009 cohorts of the program indicates that, as in previous editions, the selection process was successful in reaching the program's target population: the program focuses on young Dominicans with low education levels who are unemployed or underemployed, and from poor households.

A detailed analysis of the baseline administrative registry information[26] indicates that the program drew applicants from the lower range of the eligible age range: on average, applicants were 21 years old, and 50% were aged between 16 and 20. The baseline PJyE population is also characterized by a higher proportion of young women - 61% (Table 2). These women represent the majority of applicants with children: of the 38.5% of applicants with children, 33 percentage points correspond to women and only 5.5 to men. Moreover, 45.8% of women with children had more than one child, and 55% were single. The predominant marital status for applicants was being single, with 78.5%, followed by an 18.6% in non-married couples. Finally, only 2.3% were married.

About 95.7% of applicants declared to be unemployed during the week before their application[27]. The level of underemployment is similar among applicants to the program with respect to the general population of the same age range. Applicants with temporary or occasional employment represented 71.6% of those employed in the baseline, compared to 68.5% for those in the 16-29 age range. Finally, only 25% were students—a number which complies with the participation quota for students; 70% declared to have not completed secondary school, a reflection of the

[26] For more detail on baseline see Acevedo (2011).

[27] 24% of those from the same age group were unemployed according to the national household survey, the Encuesta Nacional de Fuerza de Trabajo (ENFT), during the first semester of 2009.

program's focus on youth that have either dropped out or who have put off completion of their secondary education.

An important issue for the evaluation strategy is the balance in observable characteristics between the control group and the treatment group before the treatment. Measures of initial characteristics or outcomes of the respective groups reveal that the lottery assignment among the groups was adequate and that the groups are comparable. The individual characteristics and the differences by experimental group, presented in Table 3, indicate the presence of only a few statistically significant differences between the groups, and no economically meaningful differences. The only exception is the relatively low level of those in the DCB treatment group for Santo Domingo - 25.3% compared to 29.9% in the TTP+DCB and 28.6% in the control group, but this isolated difference can be attributed to chance. Moreover, an analysis (not reported) of the attrition patterns for the telephone and household surveys indicates that there was no correlation between treatment status and participation in the follow-up surveys.

4.3. Estimation

The main analysis is based on a measurement of the difference in results between individuals assigned to the treatment and the control groups, irrespective of whether the individuals assigned to the program actually completed the training and internship phases. This type of causal effects are known in the experimental literature as intention to treat (ITT hereafter) effects, because they capture the difference between offering participation in a program and not offering it - they only provide a lower bound for the causal effects of actually completing all the stages of the program. It can be argued, however, that these ITT effects capture the policy relevant parameter, since policy makers in most cases can only offer programs - as job training programs are entirely voluntary (even though participants have applied and been selected they are not obligated to take the courses), the information that is of most interest in policy design is the effect and outcomes produced by making the courses available to youth.

Previous evaluations of the PJyE relied on the random assignment of applicants to the program or to the control group, comparing then post-treatment outcomes of interest between the two groups - see Card et al. (2011), and Ibarrarán et al. (2014). The evaluation strategy for the 2008-2009 cohorts (described in Section 3) provides a richer mechanism experiment setup beyond the simple treatment and control dichotomy. There are two treatment groups, one with TTP+DCB training and the other with DCB training only. This design allows us to make the usual impact evaluation comparison between any of the treatment categories and the control group, but also to gauge the potential value added of the TTP vocational training component by comparing the differences in outcomes between the two treatment groups. For the cohorts evaluated in this paper, we distinguish between individuals who were initially assigned to the group offered both the TTP and DCB courses (225 hours of instruction as well as an internship), the group only offered the DCB course (75 hours of coursework and an internship), or, finally, the group that was not offered any courses, the control group.

We present the results on the effect of the two PJyE versions on the outcomes of interest by means of OLS regressions of these outcomes against binary variables representing each of the two treatment groups. Since there is substantial heterogeneity in the program's impact according to the gender of beneficiaries, we present all regressions separately for men and women. We include a minimum set of controls[28] with the purpose of improving estimate precision[29]. In terms of inference, we cluster standard errors by COS (i.e., the institution in charge of the training) and treatment group. In the tables below, we also include a t-test for the equality of coefficients for the two treatment group dummies, which allows us to recover the difference in outcomes between the TTP+DCB and the DCB versions, which in effect captures the differential impact of the TTP component over and above the (common) DCB component. For the analysis below, we work with a balanced sample of individuals who responded to the telephone survey and to the final household survey for a better comparability between short and long term results. Finally, we restrict the sample to the training centers that offered both the vocational and the life skills training. Since, in practice, DCB training was only offered in large centers , we exclude small centers without DCB from the analysis because they could be different from the others, and do not allow us to distinguish the effect of the two types of training from that of the differences in training centers (see Vezza et al., 2014, for a discussion).

[28] The variables included are: a variable to identify the cohort to which the individual belonged at the time of application, a set of variables to identify the COS at which the individual registered, and controls for the sector of the course.

[29] Although the point value for the coefficients of interest should not change significantly, the inclusion of these controls decreases the variance of the estimates, particularly if a lagged value of the result variable, or an observable feature that explains a significant part of its variability, is included. See Duflo et al. (2008).14

5. Results

5.1. Labor Market Outcomes

This section evaluates the short and long term effects of the PJyE on individuals' basic employment outcomes, expectations, and personal skills, among other outcomes.

5.1.1. Short Term Effects (1-1.5 years after the training)

The short term effects are based on the data collected from the third round of the telephone follow-up survey, which collected information for a period of about 12 to 18 months after enrollment into the program. As described in Section 4, some beneficiaries were still enrolled at the time of the first and second rounds of the telephone survey, and thus only the third round collected data for the post-treatment period for the treatment and control groups (see Figure 2)[30].

Table 4, panel A, presents the short term effects of PJyE on basic labor market outcomes. The coefficient on the treatment variables indicates that, overall, the program had a positive and significant effect on the probability of working for women of about 7 and 5.2 percentage points (TTP+DCB training and DCB only training, respectively), which represent increases of about 32% and 23.6% with respect to the employment rate of for the control group. This sizable effect is stronger for the combined hard and soft skilled training, although the two are not different in statistical terms. The results for men are substantially different: the combined hard and soft skilled training induced a negative and strongly significant effect on employment (-0.11, about -

[30] For completeness, Appendix 1 presents the estimates similar to those in the tables presented in this section for each of the rounds of the telephone survey, and for a pool of data from the three rounds.

20% with respect to the control group), whereas men in the life-skills only (DCB) group experienced a lower reduction in their employment levels of about 3.1 percentage points (not statistically significant). We can reject the equality of coefficients at the 5% level, which indicates that the negative effect on employment for men was mostly due to the combined hard and soft skills training treatment.

While there do not seem to have significant effects on on-the-job search and hours worked (Table 4, Panel A, columns 3 to 6), the results in columns 7 indicate that the program had a large positive effect on women's salaries (for those working) of about 17%, with very similar effects for the two treatment arms. The coefficient for log monthly salaries for the combined hard and soft skills training treatment for men is positive (0.067), whereas that of the life-skills only is negative (-0.039), although they are not significantly different from 0. Finally, the results in column 9 indicate that the program had a large positive effect on job satisfaction for working women, with large effects for the combined hard and soft skills training than for the life-skills only training, although the difference between the two coefficients is again not statistically significant at standard levels. The results for men indicate a positive but (marginally) non-significant effect for men for the combined hard and soft skills training treatment, and a virtually null effect for the life-skills only group.

Taken together, these results indicate that, in the short run, the two versions of the program successfully and substantially increased employment, salaries and job satisfaction for women but not for men, and that the hard skills TTP module had at most a negligible positive effect on employment for women, and a non-significant but large negative effect for men[31].

[31] A possible explanation for the surprising negative effect on employment for male for the technical training could be that the training increased the reservation wage of males, so they may be rejecting low paid jobs.

5.1.2. Long Term Effects (3.5-4 years after the treatment)

For these estimations we rely on the programs' impact evaluation household survey (described in Section 4.1), carried out from October 2012 to March 2013, about 3.5-4 years after the program. To the knowledge of the authors this is the longest post-treatment period survey of the existing experimental evaluations of job training programs in developing countries. For instance, Card et al.'s (2011) evaluation of the 2004 cohort of the PJyE program is based on a follow-up survey conducted 10 to 14 months after the program; Ibarraran et al.'s (2014) evaluation of an earlier 2008 PJyE cohort is based on a follow-up survey conducted 18 to 24 months after most trainees had finished their initial course work; and Attanasio et al.'s (2011) study of a similar program in Colombia is based on data collected between 19 and 21 months after the beginning of the program. The only experimental evaluation of a job training program with a comparable time frame are Hirshleifer et al.'s (2014) study of Turkey, and Alzua et al.'s (2015) study of the Entra21 program in Argentina, which relies on data from up to 36 months after completion of the training, although both studies rely on administrative data rather than surveys and thus cannot distinguish non-employment from informal employment.

The program evaluation household survey was substantially more comprehensive than the short term telephone surveys, and this allows us to examine the long term effects of the PJyE on more detailed labor market outcomes. Table 4, Panel B presents the effects of PJyE on the main employment outcomes for the long run. In contrast with the results for the short term in Panel A, there are no statistically significant effects of the program on the probability of working for neither women (small positive coefficients of 0.016 and 0.013) nor men (small negative coefficients of -0.009 for both treatments) (columns 1 and 2). Moreover, the program did not seem to have any substantial effect on hours worked or monthly earnings (columns 5 to 8), in contrast to the strong positive effects on salaries for women in the short run.

There are, however, some effects of the program on other dimensions of employment for those who are working. The results in column 4 (Table 4, Panel B) indicate that the program had a large positive and significant effect on the probability of on-the-job search for men (13.6 and 9.2 percentage points for the combined hard and soft skills training and life skills only), which was even larger for those who were selected for the combined hard and soft skills training (although the difference between the two coefficients is not statistically significant). On the job search might be considered a positive outcome in the sense that beneficiaries seem to be more open to new opportunities, but at the same time it can reflect dissatisfaction with the current employment conditions. For women, the program effect on on-the-job search is negative, substantially smaller than for men and not statistically significant.

The results in column 10 of Table 4, Panel B, indicate in turn that men who were selected to participate in the program have a 6.1 and 9.6 percentage points lower probability of working formally (i.e., with access to contract, or any social insurance benefit) for the combined hard and soft skills training and the life-skills only training, respectively. While only the coefficient for the latter is statistically significant, we cannot reject the equality of the two coefficients, which implies that men in both treatment groups who worked in the long term did so in lower quality jobs than working men in the control group.

Taken together, these results indicate that the large gains in employment for women that we found in the short term seem to dissipate in the longer term, and that the program seems to have induced worse employment conditions for working men in the long term. Male beneficiaries of the PJyE who were employed in the long term had worse labor market conditions than those in the control group (i.e., working informally) and they were less satisfied with their jobs, as reflected by their higher propensity to search while employed. At the same time, however, women participants, while not exhibiting higher employment rates as in the short run, seemed to be more satisfied with their jobs than those in the control group, as manifested by their lower desire to change job. All these effects were similar for the TTP+DCB and the DCB only treatment groups. Since all beneficiaries were exposed to the DCB life skills component, the TTP vocational training module does not appear to have added much both in terms of the positive or the negative impacts of PJyE.

5.2. Impact on Expectations

5.2.1. Short Term Effects

Table 5 presents the estimates of the short term effects of the program on expectations based on the third round of the telephone survey. One of the objectives of the DCB life skills components was to help beneficiaries reduce negative expectations and negativity in general. The program seems to have been successful in increasing expectations in the short run. The results in columns 1 and 2 indicate a positive and significant effect on the expectation of improving employment conditions for both treatments and for both women and men (the coefficient for men is only significant for the combined hard and soft skills training treatment, but we cannot reject the null that the coefficients for both treatments are equal). Moreover, further results (not shown) indicate that this positive and significant effect in expectations was present from the very onset of the program, with larger impact in the first round of the telephone survey which then fell over time. While we find a positive short term effect on employment expectations for both men and women, the program also induced a more general positive effect on expectations of improving life conditions, but only for women (roughly equal for the two treatments). The effects for men are small and not statistically significant.

5.2.2. Long Term Effects

The results in Table 6 present the effects of the program on a richer set of expectations collected in the longer term household survey. The results in Columns 1 and 2 in Panel B indicate that the program had a positive effect on salary expectations for women, with higher effects for the combined hard and soft skills training (about 6 percent) than for life-skills only (about 3 percent). At the same time, however, and probably linked to the negative employment effects found in the short term, the combined hard and soft skills training component seems to have substantially reduced salary expectations by about 8.5%, with smaller and non-significant effects for the life-skills only treatment. The results are similar for a broader set of expectations: the program had a positive effect on the expectation of own children having a better life than beneficiary parents (Table 6, Panel A, columns 5 and 6) and of moving to a better neighborhood (Table 6, Panel B, columns 11 and 12) for women (with a stronger effect for the combined hard and soft skilled training, but negative for men (again, more negative for the combined hard and soft skills training treatment, although the coefficients are not statistically distinguishable).[32] On the other hand, the effects are positive for all treatments and for both men and women in terms of current and expected future relative position in terms of wealth (Table 6, Panel A, columns 7 to 10), although these effects are only statistically significant for women in the combined hard and soft skills training treatment.

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[32] The program had a positive effect on the expectation of having a better life than parents for women, but negative for men, especially for the combined hard and soft skills training treatment, although none of these coefficients are statistically significant (Table 6, Panel A, columns 3 and 4).

The consistent pattern of positive effects on expectations for women in the longer term is repeated for those related to the labor market: young women state higher expectations of having their own business, of having their desired job, and of general aspirations in their professional, with higher levels overall and a substantially higher effect for those in the TTP+DCB group (Table 6, Panel B, columns 13 to 18). The corresponding coefficients for men are close to zero and not statistically significant, with the exception of a positive effect for men in the life-skills only group in the expectation of having one's own business.

The DCB module also included extensive work on risk behavior awareness. The evaluation survey included questions to capture the effects of the program on attitudes and awareness on risk behaviors. The program does not seem to have had an impact on being involved in a traffic accident or in a fight in the previous year (not reported). It does not appear either to have affected the probability of being diagnosed with a sexually transmitted disease (again, with very low levels for this variable) nor on the expectation of contracting AIDS (note reported). However, the program seems to have substantially increased the HIV-AIDS awareness among men (Table 6, Panel B, column 20).

5.3. Impact on Skills and Self Esteem

Finally, Table 7 presents the long term effects of the program on other measures of soft or life-skills in the long term (these measures were not collected in the short term telephone survey). The Table presents the results on self-esteem, basic skills and personal characteristics[33]. The first column indicates that the program did not have a significant impact on a self-reported measure of self-esteem based on 10 questions from the Rosenberg scale for women, but it reduced significantly this measure for men in the combined hard and soft skills training group. The results in columns 3 to 6 are based

[33] [17] The first three outcomes analyzed contain information related to self-esteem, basic skills, and personal qualifications. Each variable results in the standardization of the sum of a series of questions in which individuals responded by indicating if they are, or are not in agreement, or if the question relates or does not relate to their personality. Scales were created so that questions that obtained a higher rating had a positive connotation.

on two survey modules designed to measure participants' basic skills and personal characteristics, which were also explicit objectives of the DCB training. The evaluation survey included a module called *Escala de Competencias Personales y Sociales*, which includes information on basic qualities like leadership, capacity to relate to others, order, and empathy. For both basic skills and personal characteristics, we find a positive and significant average effect of the program on these standardized measures for women, although only the combined hard and soft skills training induced a higher impact on the personal characteristics measure.

In the long term, thus, the program seems to have been more successful in raising expectations and basic skills rather than on changing labor market outcomes.

6. Concluding remarks

Taken together, these results indicate that the large gains in employment for women that we found in the short term seem to dissipate in the longer term, and that the program seems to have induced worse employment conditions for working men in the long term. Male beneficiaries of the PJyE who were employed in the long term had worse labor market conditions than those in the control group (i.e., working informally) and they were less satisfied with their jobs, as reflected by their higher propensity to search while employed. At the same time, however, women participants, while not exhibiting higher employment rates as in the short run, seemed to be more satisfied with their jobs than those in the control group, as manifested by their lower desire to change job. All these effects were similar for the TTP+DCB and the DCB only treatment groups. Since all beneficiaries were exposed to the DCB life skills component, the TTP vocational training module does not appear to have added much both in terms of the positive or the negative impacts of PJyE. In the long term, thus, the program seems to have been more successful in raising expectations and basic skills rather than on changing labor market outcomes.

These results allow us to draw some conclusions. The impact evaluation of training programs should explicitly attempt to follow beneficiaries over a broader horizon. Our results in the short term coincide with other similar programs evaluated over similar time periods in the region (for instance, employment gains concentrated in earnings and quality of the job). However, most of these employment gains dissipate in the longer run, which contrasts with the available evidence for developed countries, which typically finds positive medium-term impacts of training programs that often appear ineffective in the short term (Card et al., 2010). The evaluation design also allows us to establish that, at least in these PJyE editions, the classroom- based vocational training was not very effective, even when (as in the PJyE) it was discussed and developed jointly with private sector employers. The program seems to have been more successful in raising expectations and basic skills rather than on changing labor market outcomes in the longer run. The positive impact of the program on soft skills indicates that private sector training providers might be more effective in transmitting general cognitive skills and developing non-cognitive abilities than in fostering specific vocational competences. Moreover, the positive employment gains in the short term are compatible with a setting in which the effects on employment are due to the program's implicit labor intermediation (through the internship) rather than on the training component.

Further research could concentrate on the mechanisms through which these programs seem to be more effective for women than for men, and attempt to derive conditions under which male youth could also benefit from training in both their hard and soft skills and their employment outcomes in the longer run.

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8. Figures

Figure 1: Random assignment process, 2008-2009 program cohorts.

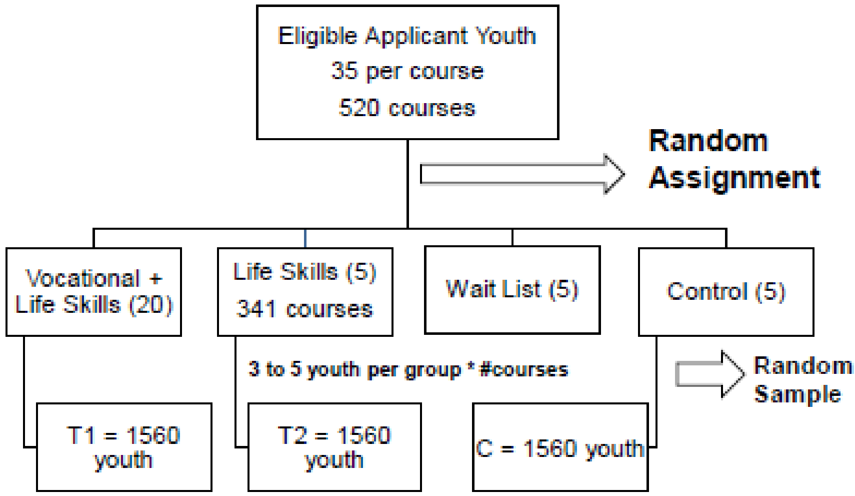
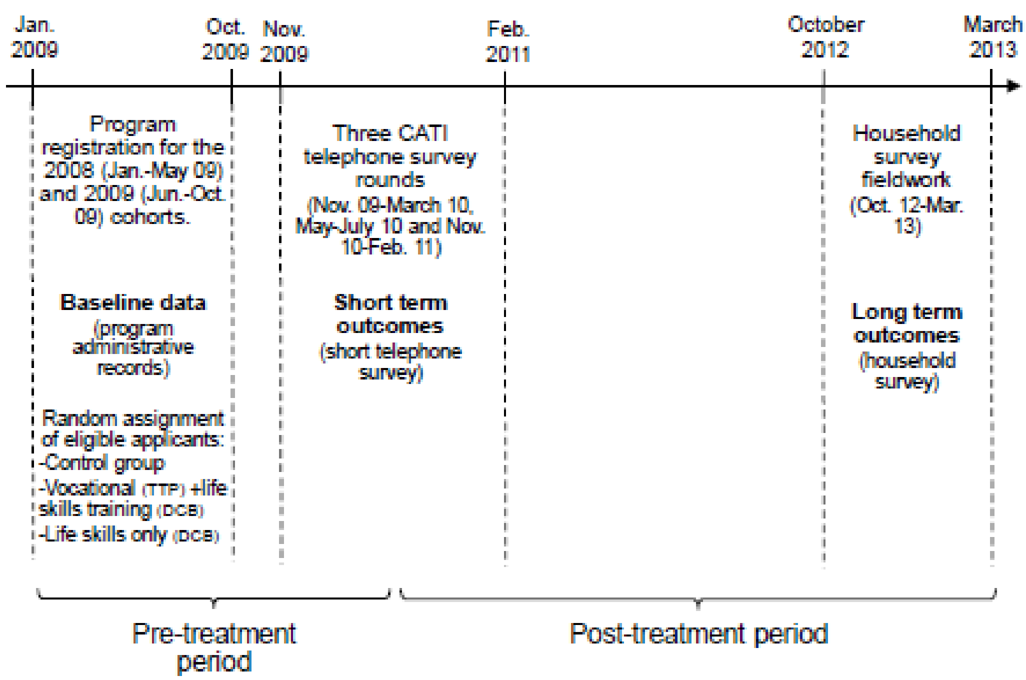


Figure 2: 2008-2009 program cohorts and impact evaluation data-collection timeline



9. Tables

Table 1: Data sources and sample sizes

	Baseline	Short term follow-up			Medium term follow-up
	Application form	CATI 1	CATI 2	CATI 3	Household survey
Observations	**4,700**	**4,115**	**4,238**	**4,221**	**3,873**
Treatment	**3,251**	**2,856**	**2,940**	**2,935**	**2,697**
TTP+DCB	1,638	1,419	1,481	1,470	1,366
DCB	1,613	1,437	1,459	1,465	1,331
Control	**1,449**	**1,259**	**1,298**	**1,286**	**1,176**

Source: Baseline registry data, 2008-2009 cohorts; CATI 1 (2009-10), CATI 2 (2010) and CATI 3 (2011); and household survey for the Impact Evaluation of PJyE (2012-13).

Table 2: Applicants socio-economic characteristics compared to the general population in the 16-29 age group

	Mean PJyE applicants	Mean 16-29 population, ENFT
Male	39%	50%
Age	21.6	20.9
HH members	3.8	4.7
Education (maximum level attained, not necessarily completed)		
Elemetary	27%	31%
Secondary	70%	49%
College	0%	17%
Graduate	0%	3%
Do not know	3%	0%
Marital Status		
Single	79%	69%
Unido	18%	22%
Married	2%	3%
Divorced	0%	6%
Widower	0%	0%
Worked previous week	16%	39%

Source: PJyE baseline registry data and ENFT 2009.

Table 3: Experimental balance: Basic characteristics in the baseline, by treatment and control groups

	Baseline			p-value		
	TTP+DCB	DCB	Control	TTP+DCB vs Control	DCB vs Control	TTP+DCB vs DCB
Age	20.9	21.0	21.1	0.044	0.310	0.365
Male =1	39.5	37.7	41.5	0.218	0.029	0.306
Urban =1	81.4	82.4	80.7	0.541	0.203	0.458
Santo Domingo =1	29.9	25.3	28.6	0.255	0.057	0.003
Studying =1	25.7	26.2	25.6	0.947	0.684	0.723
Single =1	80.0	78.4	78.3	0.248	0.956	0.308
Has children =1	37.5	37.7	38.4	0.620	0.712	0.892
Unemployed =1	62.1	61.0	60.0	0.231	0.615	0.507
Previous work =1	15.3	15.2	15.3	0.951	0.969	0.917
Related experience =1	12.3	12.0	13.5	0.264	0.206	0.831
Participation =1	98.2	98.0	97.6	0.253	0.442	0.759
Remittances =1	6.0	6.0	7.7	0.073	0.074	0.919

Source: Baseline for the 2008-2009 cohorts.

Table 4: Program impact on labor market outcomes, short term and long term

Panel A. Short term outcomes (telephone survey, 1 year after the intervention)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Working=1		Working and looking for another job =1		Hours per week (working=1)		Log of monthly salary (working=1)		Satisfied with job=1	
	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male
TTP+DCB=1 (original assignment)	0.070**	-0.111***	-0.102	0.040	1.814	1.867	0.174*	0.067	0.197***	0.090
	(0.027)	(0.040)	(0.062)	(0.053)	(2.451)	(2.051)	(0.103)	(0.076)	(0.072)	(0.061)
DCB=1 (original assignment)	0.052**	-0.031	-0.064	0.071	1.532	-1.228	0.179*	-0.039	0.143**	0.010
	(0.025)	(0.038)	(0.061)	(0.046)	(2.201)	(1.634)	(0.098)	(0.064)	(0.067)	(0.052)
Observations	1,728	1,051	451	522	448	519	445	512	451	522
Control Mean:	0.220	0.541	0.307	0.229	39.30	45.46	8.431	8.775	0.416	0.547
P-value t1=t2:	0.487	0.0328	0.426	0.527	0.885	0.137	0.944	0.145	0.345	0.171
Sample Conditioned on Working=1	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B. Long term outcomes (household survey, 4 years after the intervention)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Working=1		Working and looking for another job =1		Hours per week (working=1)		Log of monthly salary (working=1)		Formal=1 (working=1)	
	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male
TTP+DCB=1 (original assignment)	0.016	-0.009	-0.042	0.136***	0.600	-1.019	0.012	-0.099	0.012	-0.061
	(0.033)	(0.032)	(0.043)	(0.041)	(1.863)	(1.811)	(0.092)	(0.075)	(0.042)	(0.044)
DCB=1 (original assignment)	0.013	-0.009	-0.006	0.092***	0.334	0.103	-0.027	-0.039	0.034	-0.096**
	(0.029)	(0.030)	(0.039)	(0.033)	(1.741)	(1.706)	(0.085)	(0.058)	(0.038)	(0.040)
Observations	1,728	1,051	844	848	844	849	747	806	843	849
Control Mean:	0.490	0.822	0.306	0.203	35.38	45.22	8.259	8.746	0.320	0.492
P-value t1=t2:	0.928	0.995	0.318	0.258	0.875	0.505	0.634	0.414	0.569	0.400
Sample Conditioned on Working=1	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors clustered at the course and treatment group level in parentheses. All regressions include controls for the educational institution, the sector of the course, and the PJyE cohort. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Program impact on expectations, short term (telephone survey, 1 year after the intervention)

VARIABLES	(1)	(2)	(3)	(4)
	Expectation of improving employment conditions=1		Expectation of improving life conditions=1	
	Female	Male	Female	Male
TTP+DCB=1 (original assignment)	0.033**	0.045***	0.028**	0.007
	(0.016)	(0.017)	(0.013)	(0.016)
DCB=1 (original assignment)	0.037**	0.029	0.032***	0.006
	(0.014)	(0.018)	(0.012)	(0.015)
Observations	1,728	1,051	1,728	1,051
Control Mean:	0.917	0.924	0.943	0.955
P-value t1=t2:	0.753	0.285	0.665	0.993

Standard errors clustered at the course and treatment group level in parentheses. All regressions include controls for the educational institution, the sector of the course, and the PJyE cohort. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Program impact on expectations, long term (household survey, 4 years after the intervention)

Panel A. Long term outcomes (household survey)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log of expected salary for next job		Possibility of having a better life than parents		Possibility that their children have a better life		Position in terms of wealth		Position expected (10 years) in terms of wealth	
	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male
TTP+DCB=1 (original assignment)	0.060**	-0.085**	0.063	-0.070	0.075**	-0.094*	0.084*	0.030	0.115**	0.051
	(0.029)	(0.039)	(0.044)	(0.053)	(0.038)	(0.049)	(0.048)	(0.063)	(0.050)	(0.064)
DCB=1 (original assignment)	0.030	-0.017	0.055	-0.003	0.054	-0.067	0.061	0.073	0.010	0.082
	(0.026)	(0.031)	(0.038)	(0.047)	(0.034)	(0.045)	(0.046)	(0.060)	(0.049)	(0.059)
Observations	1,659	1,003	1,694	1,032	1,693	1,032	1,694	1,032	1,693	1,032
Control Mean:	9.208	9.534	4.428	4.476	4.532	4.550	2.305	2.337	3.947	3.903
P-value t1=t2:	0.205	0.0557	0.826	0.179	0.536	0.545	0.592	0.476	0.0183	0.614

Panel B. Long term outcomes, continued (household survey)

	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	Possibility - Better neighborhood, home, car		Possibility - Having own business		Possibility - Having the desired job		Possibility - Aspirations professional life		Possibility - Relative has AIDS	
	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male
TTP+DCB=1 (original assignment)	0.100*	-0.060	0.154**	0.012	0.129***	0.004	0.083*	-0.033	-0.029	0.078
	(0.059)	(0.069)	(0.071)	(0.082)	(0.050)	(0.064)	(0.047)	(0.059)	(0.053)	(0.066)
DCB=1 (original assignment)	0.011	-0.007	0.052	0.138*	0.075	-0.030	0.007	0.055	0.017	0.143**
	(0.055)	(0.066)	(0.063)	(0.076)	(0.047)	(0.062)	(0.042)	(0.060)	(0.049)	(0.064)
Observations	1,694	1,032	1,694	1,032	1,693	1,032	1,694	1,032	1,693	1,031
Control Mean:	3.822	3.906	3.635	3.728	4.089	4.172	4.218	4.175	1.590	1.557
P-value t1=t2:	0.0759	0.390	0.101	0.0991	0.218	0.570	0.0715	0.101	0.322	0.329

Standard errors clustered at the course and treatment group level in parentheses. All regressions include controls for the educational institution, the sector of the course, and the PJyE cohort. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Program impact on skills, personal characteristics and self-esteem, long term (4 years after the intervention)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Self Esteem		Basic skills		Personal characteristics	
	Female	Male	Female	Male	Female	Male
TTP+DCB=1 (original assignment)	0.207	-0.896*	2.821**	-0.727	1.325***	-0.063
	(0.351)	(0.528)	(1.139)	(1.416)	(0.377)	(0.453)
DCB=1 (original assignment)	-0.019	-0.230	1.867*	0.139	0.569	-0.011
	(0.303)	(0.490)	(0.995)	(1.271)	(0.349)	(0.425)
Observations	1,686	1,029	1,687	1,029	1,684	1,029
Control Mean:	0.0492	-0.0484	-2.298	1.911	-0.780	0.0433
P-value t1=t2:	0.481	0.186	0.338	0.546	0.0167	0.901

Standard errors clustered at the course and treatment group level in parentheses. All regressions include controls for the educational institution, the sector of the course, and the PJyE cohort. *** p<0.01, ** p<0.05, * p<0.1

CHAPTER 2: Job Search and Networking in Training Programs. Experimental Evidence from Dominican Republic.

Paloma Acevedo

ABSTRACT

Most unemployed youth in developing countries rely on informal job search methods to find a job. Active Labor Market Programs (ALMP) are generally found to have modest impacts on employment (Attanasio 2011, Card 2011, Ibarraran 2012, Hirshleifer 2014), but larger effects on the quality of employment. Using data from a large scale experiment in Dominican Republic I find that ALMPs changes job search methods towards the use of professional networks (especially those developed in internships.) Jobs found through professional contacts are associated with higher levels of formality. This suggests that contacts made during the program may play an important role in increasing job quality.

Keywords: job search, networks, vocational training program, training programs, unemployed youth, impact evaluation.

1. Introduction

Despite recent improvements in the labor market indicators in recent years, youth continue to be the age group with the most serious employment problems in the World. The youth unemployment rate in 2014 was of 13.0% and it is expected to remain in that percentage for the next four years. This is almost three times higher than the unemployment rate for adults (ILO 2015). In addition, among the population that is working, more than half, is doing it in informal conditions, not having access to any type of social protection coverage in terms of health and pensions (ILO 2015.)

This situation is concerning for several reasons (Gonzalez 2012): (i) youngsters represents a remarkable share of the working age population and this share might increase in the future, (ii) poor insertion in the labor markets not only has immediate consequences on the quality of life, but it also may have negative persistent effects on individuals´ careers, and (iii) inactivity and lack of opportunities may lead to socially undesirable behaviors, such as drug consumption and delinquency.

Youth face special challenges when entering the labor markets, including lack of the right skills for labor markets, lack of work experience, lack of information about vacancies, and lack of networks. Governments around the world have put in place different Active Labor Market Programs (ALMPs) to overcome these problems. One of the most widespread programs is the provision of vocational training to unskilled youth[34]. These programs aim to improve

[34] A comprehensive review of these programs can be found in Almeida et al., 2012.

employability by providing youth with the set of abilities and knowledge that markets demand. They usually offer a broad selection of disciplines and train youth during several weeks and provide them with on the job experience. Despite recent studies have found that ALMP have modest impacts in employment (Attanasio 2011, Card 2011, Ibarraran 2012, Hirshleifer 2014), they are consistent finding large effects of these programs on the quality of jobs. For instance Card uses evidence from a vocational training in Dominican Republic and finds little indication of a positive effect on employment outcomes but some evidence of a modest effect on earnings, conditional on working. Ibarraran studies the same program for a different cohort and, consistently, finds that the program has a positive impact on job formality for men of about 17 percent and there is also a seven percent increase in monthly earnings among those employed. However, there are no overall impacts on employment rates. Attanasio finds that a vocational training program in Colombia has significant impact for women, raising their earnings by 19.6% and having a 0.068 higher probability of paid employment. Hirshleifer studies a vocational training in Turkey and finds that the impact of training on employment is positive, but close to zero and statistically insignificant. They find statistically significant effects on the quality of employment during the first year of the program although these impacts disappear in the long run. Yet, little is known about what are the pathways in which these programs work. This paper studies how these programs affect jobs search methods, and how those changes may affect labor outcomes.

During the last decades economists have paid substantial attention to the role of contacts in labor outcomes. On the theoretical side, Montgomery (1989) develops a model and shows that workers with more contacts have better work conditions. In a different model Topa (2001) shows that agents are more likely to be employed if their social contacts are also employed. Ioannides

(2006) shows that, on average, workers who are better connected socially experience lower unemployment rates and receive higher wages. Finally Dustman (2011) uses a model that predicts that workers who have obtained their job through a referral have better employment conditions. On the empirical side, there is also substantial evidence on the importance of the role of contacts in job outcomes. Corcoran (1980), shows that about half of all workers heard about their current job through a friend or a relative in the US. Hozler (1988), using data from NLS in 1981 finds that in the US, the methods of search used most frequently and most intensively by unemployed youth are checking with friends and relatives. This was also the most productive in terms of generating job offers and acceptances, conditional on use. Truman Bewley 1999 summarized the results of 24 studies and estimated that 30-60% of jobs were found through personal contacts. Madruger (2010) using data from South Africa finds that present fathers's utility as network connections may be responsible for a one-third increase in their son´s employment rates. Galeanianos (2012) finds that a 10 percentage point increase in the prevalence of referrals is associated with a 28% increase in the matching efficiency of the average industry. There is also evidence that contacts are more important for less educated and lower income population: James Elliot (1999) found that less-well educated workers in high poverty neighborhoods were more likely to use informal contacts and that these contacts were also the main avenue by which these individuals found work: about 73% of jobs in neighborhoods with poverty rates of 40% or more were found through contacts, compared to the 52% of jobs in neighborhoods with poverty rates less than 20%. Ioannides (2004) finds in the US CPS 1993 that more educated job-seekers are less likely to use friends and relatives to search jobs. Kramarz (2007) using Swedish data finds that family ties are especially important for low educated males that tend to follow their parents. Hellerstein (2008) using US data finds that labor market networks play an important role in hiring, more so for minorities and the less-skilled. This evidence shows the importance that a good network of contacts has to search for jobs and find them, and that this may be especially important for vulnerable population.

Despite the importance of contacts in labor outcomes is widely recognized, there are some discrepancies on the sign of the impact on labor outcomes such as wages and formality. A line of theoretical work (Montgomery 1991; Saloner 1985; Simon and Warner 1992) associates contacts with better matches and higher wages since they increase the set of information leading to better quality matches. Evidence on this front comes mostly from developed countries. Corcoran et al.1980; Fernandez et al. 2001; Granovetter 1974; Kugler 2002; Datcher 1983; Marmaros and Sacerdote 2002; Simon and Warner 1992; Staiger 1990 use data from US, UK, and other european countries, and find a positive relation on the use of contacts and labor outcomes. On the other hand other works (Bentolila 2010) point towards the opposite direction arguing that jobs found through contacts induce workers to sacrifice their productive advantage, leading to poorer matches. Empirical evidence (also from developed countries) of this angle can be found in Antoninis 2006, Bentolila et al. 2010, Pistaferri 1999, Mouw 2003, and Datcher Loury 2006.

This study somehow reconciles these results by disentangling two kinds of contacts: professional contacts and personal (or non-professional) contacts. Professional contacts are those who belong to the professional sphere of the individual and know his professional skills, whereas non-professional contacts are those from the personal sphere of the individual (relatives, neighbors and friends) and don´t necessarily know the professional skills of the individual. Whereas professional contacts may lead to better labor outcomes (since they provide a better match), non-professional contacts may lead to jobs that are not aligned with the professional profile of the individual, and therefore, provide worse labor conditions.

Despite the broad evidence of the importance of contacts in developed countries, evidence from developing countries is scarce. The only paper that uses data from developing countries to the knowledge of the author is Marquez et al. (2004). In their paper they use panel data for Venezuela from 1994 to 2002 to analyze the choice of different search methods, their effectiveness, and the relative weight of search method and previous job status in the likelihood of landing on a job or dropping out of the labor force. They conclude that previous job status is a primary determinant of success in moving to employment, and that the use of employment agencies increases the likelihood of that move.

Taking these facts in mind, the contributions of this paper to the existent literature is threefold:

Firstly, this paper tries to shed some light on the mechanisms that operates behind labor market programs. Whereas they are focused in providing the youth with skills, they also provide them with new contacts that may affect their search methods and ultimately, labor outcomes.

Secondly, it contributes to the existent literature on job search and labor outcomes by disentangling between two kinds of networks that can lead to jobs: firstly the network of contacts that are related to their professional profile -these can include colleagues and teachers from school, training courses, and colleagues from jobs and apprenticeships-; and secondly, the network of contacts that are not directly related to your professional skills, such as family, friends, etc... The idea behind this is that the first network is more likely to lead you to better quality jobs.

Finally, it contributes to the base of evidence on formality and job search methods for vulnerable youths bringing new evidence from developing countries. I use data from a job training program in Dominican Republic that targeted unskilled unemployed youth with low socio economic profiles.

Taking advantage of the experimental design of an impact evaluation of a vocational training program that offered the training randomly within the pool of applicants, I find that youths in the treatment program tend to search (and find) jobs more through professional networks compared to the control group. More precisely, they use more contacts found in former jobs and apprenticeships. Furthermore, I find a positive correlation between jobs found through professional networks and formality of jobs, suggesting that professional contacts developed in the program may be leading to improvements in formality.

The paper is organized as follows. Section 2 sets the theoretical framework of the analysis. Section 3 brings the empirical evidence, describe the dataset and study the impact of the program on job search methods, existence of networks, and relation of job search methods to labor outcomes. Finally, section 4 concludes bringing some policy recommendations and suggesting new lines of research.

2. Logical Framework

Active Labor Market Programs (ALMPs) aim to improve employability by providing unemployed individuals with the set of abilities and knowledge that markets demand. They usually offer a broad selection of disciplines and train individuals during several weeks and provide them with on-the-job experience. The logic behind that is that by providing individuals with the right skills and job experience they will improve their employability. Recent studies have found that ALMP have modest impacts in employment (Attanasio 2011, Card 2011, Ibarraran 2012, Hirshleifer 2014), but they find large effects of these programs on the quality of jobs.

Despite the consensus on *what* is the impact of these programs on labor outcomes, there is still little known on *how* these programs work. This implies to open the black box of the program and see the mechanism through which these programs operate. Most of these programs offer technical skills, basic skills, and apprenticeships. What of these components are most effective to improve employability? A recent paper by Acevedo et al. (2015) uses an experiment to disentangle the impact of the hard-skill training from the soft-skills and apprenticeship. Results show that the technical skills received at the training are not significant in improving labor outcomes. If hard skills are not important, then what is leading students to better labor outcomes? This paper digs in this question and provides a possible answer that has not been explored yet. It states that the program not only provides students with skills and experience, but also with a new set of contacts that will change the way in which students search jobs and, as a consequence, will lead them to better jobs. Figure 1 illustrates this logic: programs provide training for hard skills, soft skills, and apprenticeships. This will provide students with new technical and soft skills, and with job experience. However, these programs also provide students with a new set of contacts (those people met in the program) that can be used to search for jobs. Contacts serve as sources of information about vacancies and as referrals. The better your network of contacts is, the better the information and referrals that can provide to you, and the greater the probabilities to find a better job.

In certain settings having a good network of contacts can be of crucial importance. For instance poor environments are characterized by high levels of unemployment and informality.

Introducing a new set of contacts that are linked to the formal sector and to their technical vocation can change the way in which you search for job and the way you entry in the labor market. In the case of the intervention of this study, the training program was offered to unemployed youth between 16-29 years old, with uncompleted secondary education, and living in low income areas. The youngsters that attended the training were exposed to the program for 6 months. The training consisted in two modules that were held in a classroom (thecnical training consisting in 150 hours, and life-skills training consisting in 75 hours), and an apprenticeship in a local firm (consisting in 240 hours.) During that time the program broads the set of contacts the students have through the time expend in the classroom through the apprenticeship.

For the purpose of simplicity I categorize job search methods in three big types:

> S*elf-directed search*: are all the traditional job seeking activities that don´t require the use of contacts -such as the use of employment agencies, internet job-seeking, direct application to firms, etc. Literature has catalogued them as formal methods.
>
> Through p*rofessional contacts*: are contacts related to the academic or professional background of the individual (including teachers, employers, and former co-workers.)
>
> Through p*ersonal contacts*: are contacts from the personal sphere of the individual (including friends, relatives, and neighbors.) They don't necessarily know about the professional characteristics of the individual.

Taking advantage of an experiment, I demonstrate the first causal relation that shows figure 1: training programs change contacts and job search methods towards professional contacts. Even more, I find that the program makes that individuals not only search, but also find jobs more often through professional contacts, suggesting that it may be a more effective method. Given the experimental set up I cannot demonstrate the second causal relation: jobs found through professional contacts are of better quality. However, I explore the correlation between job finding method and labor outcomes and I observe that jobs found through professional contacts are correlated with better labor outcomes, suggesting that contacts made during the program may play an important role in improving job quality.

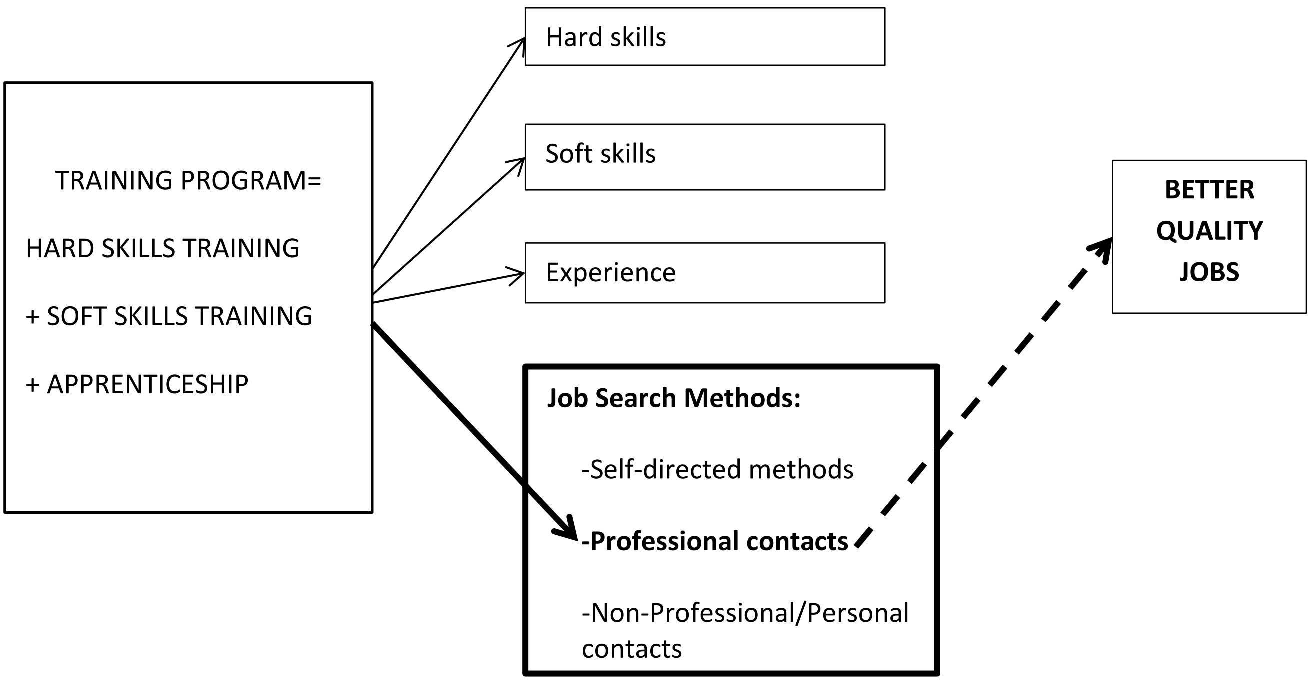


Figure 1. Diagram of the suggested logic of the impacts of training program through contacts.

3. Empirical Evidence

In this section I use data from a large scale experiment in Dominican Republic to see how ALMP´s affect job seeking practices and how jobs found through different job seeking practices might affect labor outcomes.

Dominican Republic is a middle-income country that has experienced high economic growth in the last years. However, unemployment rates remain in two digit figures (14,9% in 2009)[35] being even higher for the youth (30%)[36]. In this context, the Ministry of Labor set up a project in 1999 with the objective to increase the skills and the opportunities in the labor markets of vulnerable youth.

3.1. Program "Juventud y Empleo"

The program "Juventud y Empleo" was created to foster the probabilities og getting a job for the youth by strenghthening their professional and personal skills and by offering a first work experience. It was aimed to unemployed youth between 16-29 years old, with uncompleted secondary education, and living in low income areas. The program was led by the Ministry of Labor in cooperation with local service providers, who conduct the actual training. The professional areas offered were selected by the Ministry after making an assesment of the demands of the labor force at the local level. The resulting areas of training offered were very diverse including barman, secretary, beauty saloon assistant, comercial assistnat, accountant, etc.

[35] Encuesta Nacional de la Fuerza de Trabajo 2009.

[36] Encuesta Nacional de la Fuerza de Trabajo 2009.

The total length of the training was 6 months, divided in three different modules. The first two modules were held in a classroom and included thecnical training (module 1, consisting in 150 hours), that aimed to improve the hard skills of the participants, and life-skills training (module 2, consisting in 75 hours), designed by psicologyts to strenghthen the basic skills (or soft skills) of the participants.

The third module of the program was conducted during the last months and consisted in an apprenticeship in a local firm (240 hours.) The apprenticeship was intended to put in practice the skills acquired in the first two months, to gain some experience in labor markets, and to create a professional network.

A total of 10,309 eligible individuals applied[37] for training at the moment of the registration. Due to an excess of demand of the training, the government, along with international organizations[38], conducted a lottery to assign the training. This had two main advantages: on the first hand it assigned all the eligible populationthe the same probability to participate, and on the second hand, it alloed to conduct an impact evaluation that allowed to measure the effectiveness of the program. I will take advantage of the exogeneity of the assignation rule to the program to study how an exogenous shock affects the job search mechanisms.

3.2. Experimental Design and Data

The randomization process was conducted as follows. The youth who met the eligibility criteria applied for the program of their preference at the local training provider center. Training centers

[37] The characteristics of the applicants were rigorously screened at the moment of the registration to ensure that they complied with the eligibility criteria.

[38] The Inter-American Development Bank and the World Bank.

were distributed along the national territory offering a wide range of disciplines such as administrative assistant, baker, hair stylist, clerk, auto mechanic, bartender, and so on. The training centers screened and recruited 35 applicants per course. Once the list of 35 applicants is conformed, it is sent to the Ministry of Labor. Using an informatic system, 20 individuals were randomly selected within the applicants' list and were offered the program (they are the Intended To Treat group, ITT nowon). The remaining 15 individuals were not offered the program. Within the first ten days of the courses, the training center reported if there were dropouts, and in this case, the Ministry of Labor selected randomly up to five more people within the list of 15 non beneficiaries to cover the vacancies (replacements). The remaining individuals that were not offered the program will conform our comparison group (control group.)

In a more formal way, lets represent the result of the lottery for individual i with the stochastic dicotomic variable Zi. If Zi equals 1 means that individual i won the lottery (was intended to treat). If Zi equals 0 means individual i didn´t win the lottery. In each group of 35 individuals there will be 20 individuals for whom Zi=1 and 15 individuals for whom Zi=0. Lets now represent attendancy to the course of individual i with the stochastic dicotomic variable Di. Di equal 1 means that the individual attended the course, whereas Di equal 0 means that the individual dropped out of the course. Lets assume that in a group of 35 applicants, there are *x* drop outs. Up to five dropouts can be replaced with individuals from the group Zi=0. Figure 2 shows this scheeme.

		Participated in the Lottery - *35 individuals*		
		Won the Lottery		
		Yes *(20 individuals)* $Zi = 1$	No *(15 individuals)* $Zi = 0$	
			In case there are x drop-outs... Won replacement	
			Yes - *(x individuals)*	No - *(15 - x individuals)*
Participated in the Program	Yes $Di = 1$	*ITT - compliers*	*Replacements - compliers*	*Control-Always Takers*
	No $Di = 0$	*ITT-dropouts (x individuals)*	*Replacements-dropouts*	*Control*
		ITT Group	Replacement Group	Control Group

Figure 2. Outline of the randomization of the program

Out of the pool of 10,309 applicants at baseline, 57% of them were originally assigned to the treatment (or ITT) group (5,914) and 43% were assigned to the control group (4,395).

The study uses two main sources of data. Firstly, it uses a baseline database was created using the registration forms of the eligible individuals that applied to the program, adding up to 10,309 individuals. The registration form included basic information such as personal characteristics, socio-economic background, and personal skills[39]. Secondly, it uses a follow-up household survey conducted between November 2010 and February 2011, that is 18-24 months after graduation of the students. Power calculations were conducted to determine the necessary number of observations in the follow up sample that will permit to detect an 8 percent increase in income with a power of 0.8 and an attrition of 30% of the sample with the sampsi Stata command. The resultant sample size was 5,000 individuals. The household survey includes 15 modules that collect data on household composition and socioeconomic characteristics, labor

[39] Annex B shows the main variables.

force participation, labor history, composition of the network of contacts of the individual, assets, time use, courses and internship, consumption, health status, risk aversion, future expectations, pregnancy history, dwelling m materials, and basic skills, including non-cognitive skills and self-esteem.

Out of the 5,000 individuals of the sample, 3,412 individuals were found in the final survey (representing an attrition of 31,8%). Annex A studies attrition and show that it is neither correlated with the treatment nor with the personal characteristics of the individuals at baseline, providing evidence that attrition is not a problem for this analysis.

3.3. Identification and Specification of the Model

Identification relies on the experimental variation in program treatment.

Equation one estimates whether the program affects job search methods:

$$SM_{ij} = \alpha_j + \beta_j \, ITT_i + \gamma_j \, X_i + \varepsilon_{ji} \qquad (1)$$

Where SM_{ij} is a binary variable that takes the value 1 if the individual i uses search method j and 0 otherwise. ITT_i is a dicotomic variable that takes the value 1 if the individual belongs to the treatment group (intended to treat) and takes the value 0 if the individual belongs to the control group[40]. The variable X_i is a vector of personal characteristics for each individual[41], and ε_{ji} is the error term for each of the search methods. The posible categories under search methods (j) are: use of professional contacts (SMpc), use of non-professional contacts (SMnpc) or use of self-directed search methods (SMss).

Related to this, I explore whether the training had any impact in the composition of the network of contacts of the youth that helped them to search for jobs. Thus, I estimate the following regression:

$$C_{ij} = \alpha_j + \beta_j \, ITT_{ij} + \gamma_j \, X_{ij} + \varepsilon_{ji} \qquad (2)$$

Where Cij values 1 if a contact of type j helped the individual to look for a job in the last two years (since they finished the training). ITT_i is a dicotomic variable that takes the value 1 if the individual belongs to the treatment group (intended to treat) and takes the value 0 if the individual belongs to the control group. The variable X_i is a vector of personal characteristics for each individual, and ε_{ji} is the error term for each of the search methods.

Search methods may be influenced by a change in the composition of the network, but there may be other aspects that are affected by the program and can be also affecting the job search

[40] Notice, the reference group of the regressions estimated in the Results Section, will be the control group, that is, those who were not offered the treatment.

[41] Although random assignment ensures comparability within treatment and control groups, some control variables are included to correct for potential biases. These control variables are majorly selected based on significant differences between control and treatment group at baseline (before the intervention). As presented in the Results sections below, the variables selected are: gender, age, marital status, experience in the course of application, poverty index, and education of the head of the household.

methods. Ibarrarán (2012) shows that the program changes some of the personality skills of the youth; particularly they find changes of standard deviations on leadership, behavior in situations of conflict, self-esteem and order, and self-organization[42]. Although we cannot study the causal effect of those characteristics on the search method, I explore whether there is some kind of relation among them. In order to do that and avoid endogeneity effects of the program, I will exclude from the estimation the treated population[43]. Equation 3 shows the relation between life skills and job search methods:

$$SM_{ij} = \alpha_j + \beta_j \; LS_i + \gamma_j \, X_i + \varepsilon_{ij} \qquad (3)$$

Where SM_{ij} is a binary variable that takes the value 1 if the individual searched for job using method j, and 0 otherwise, LS_i is a vector of life skills indexes for individual i[44], X_i are individual characteristics, and ε_{ij} is the error term.

Finally, I explore whether there is a relationship in the way individuals found their jobs and labor outcomes. As in the previous case, in order to avoid endogeneity coming from the program, I will run the regression only for the individuals of the control group.

[42] No impacts were found on abilities to relate with others and empathy and communication skills.

[43] The relation of live skills and job search methods could not be studied because the sample size for that subset of people was extremely low.

[44] As presented in the Results section below, these variables include life skills characteristics such as leadership, socialization, organization, communication, and other indexes based on personality scales such as Rosemberg or Gritt.

$$LO_{ij} = \alpha_j + \beta_j \; FM_{ij} + \gamma_j X_i + \varepsilon_{ij} \qquad (4)$$

Where LO_{ij} are labor outcomes[45], FM_{ij} equals one if th individual found his job through method j (job finding methods are the same as job search methods), X_i is a vector of individual characteristics, and ε_{ij} is the error term.

3.4. Descriptive Statistics

40% of the sample was looking for job in the last four weeks. Figure 3 shows what method they were using to look for job. In line with the existent literature (Hozler 1986), self-directed search methods are the most extensively used by a 75% of the sample, followed by the use of non-professional contacts (family, friends and neighbors) with 17%, and the use of professional contacts, that was reported in a 6% of the sample. Only 2% of the sample tried to open their own business.

[45] Labor outcomes include hourly salary (in logarithm), having a contract, having a permanent position, having life insurance, having health insurance, number of days worked, number of hours worked, and want to change job among others.

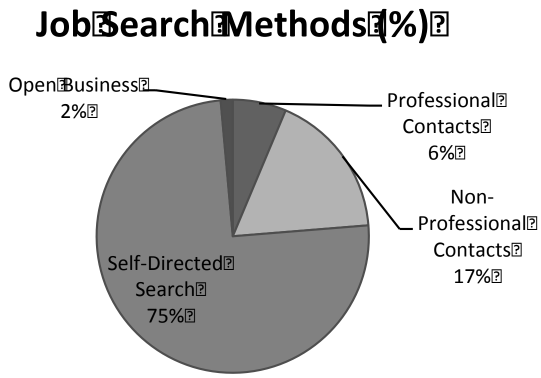


Figure 3. What did you do first[46] to look for a job in the last four weeks?

Figure 4 shows how the individuals found their current jobs (for the sample of individuals who are working). In this occasion the most extensively method to find a job is non-professional contacts (74% of the sample). It is interesting to notice that in this context of vulnerable youth from developing countries, the use of informal contacts is used in a larger proportion than in developed countries: whereas in the developed countries it accounts for approximately 30% and 50% respectively[47], in our sample 74% of the youth found their jobs through informal contacts. This is not surprising since informal economy is more extended in developing countries and, even more in the subsample of the population of socially vulnerable youth we are looking at. Indeed, empirical work shows that less educated workers living in low income neighborhoods tend to use more informal contacts (Elliott, 1999).

[46] Individuals were asked "How did you looked for job?" and they could give up to three answers. The variable is created with the first response that they provided. Although various methods can be used at the same time, I will assume that the method that they report in the first place is the prevalent method. This will also simplify the interpretation of the results, since the variable is exclusive. Other versions of the variables taking into account all the responses of the individuals were created and results were similar. However, the interpretation of those variables is not straightforward, therefore, I decided to discard them for the paper for the sake of simplicity.

[47] See the introductory section.

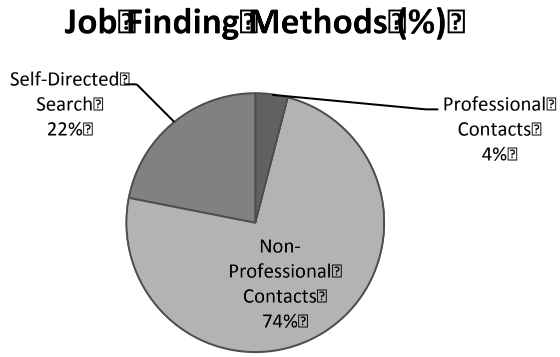


Figure 4. How did you find your current employment?[48]

3.5. Results[49]

Impact of the Program on the Job Search Methods

Table 1 shows the results of estimating equation (1) using Ordinary Least Squares (OLS). On the one hand, as column 1 shows, the program is increasing job search rates among the youth. On the other hand, the program is changing the methods that individuals use. Columns 2 and 4 show that individuals from the treatment group use more professional contacts and self-directed methods than those from the control group. The use of professional contacts may be given by the fact that the treatment group was exposed to more professional contacts (acquaintances made in academic, vocational or professional spheres) than the control group. The difference in self-directed search may be explained by the fact that part of the curricula of the program trained the students in self-directed search methods (creation of a curriculum vitae, use of agencies,

[48] This variable didn't include "open a business" as an option.

[49] Results are shown for Ordinary Least Squares, but Annex C shows other estimations including Probit, Logit, Multinomial Probit and Multinomial Logit. Notice that the results are robust to the specification used.

websites, etc.…). These results are in line with our logical framework: the program is changing job search methods towards more professional and formal methods since they are the ones that maximize the expected utility of the individuals.

Table 1. What was the first method that you used to look for job? (OLS)

		Searched: Methods			
VARIABLES	Didn´t Search	Professional Contacts	Non-Professional Contacts	Self-Directed	Open Business
	(1)	(2)	(3)	(4)	(5)
=1 Intended To Treat	-0.058***	0.012**	0.003	0.040**	0.002
	(0.016)	(0.005)	(0.009)	(0.015)	(0.002)
Observations	3,283	3,283	3,283	3,283	3,283
Control Mean:	0.653	0.0146	0.0657	0.263	0.00417
Number of curso_num	41	41	41	41	41

*** p<0.01, ** p<0.05, * p<0.1. S tandard errors in parentheses clustered at course level. Fixed effects at the course level. Includes the following controls variables at baseline: gender, age, marital status, experiences in the course of application, poverty index, education of the head of the household.

Impact of the Program on Job Finding Methods

Table 2 shows the results of regression 1 for the sample of employees, but in this occasion the dependent variable is the method through which employees found their current job (or if they didn´t find). This gives us some information about whether the program had any impact on job finding methods. As in Ibarraran 2012 the program didn´t have impacts on employment rates (column 1). However, it did have impacts on the way that individuals found their jobs. The treatment group found jobs using professional contacts to a greater extent than the control group. On the other hand, individuals of the treatment group found their job through self-directed search to a lesser extent. This can be seen as a measure of effectiveness of search methods, whereas professional contacts are more used and more effective for the treatment group, self-directed search methods are more used but less effective compared to the control group. However, there is a caveat that should be taken into account and is that, given that the results are conditioned on having found a job, this implies that there may be a self-selection bias that may be biasing the result.

Table 2. How did you find your current job? (Conditional on been working) (OLS)

		Found a Job: Methods		
VARIABLES	Didn´t Find a Job	Professional Contacts	Non-Professional Contacts	Self-Directed
	(1)	(2)	(3)	(4)
=1 Intended To Treat	0.004	0.026***	-0.005	-0.024**
	(0.014)	(0.006)	(0.017)	(0.011)
Observations	3,285	3,285	3,285	3,285
Control Mean:	0.386	0.00626	0.456	0.151
Number of curso_num	41	41	41	41

*** p<0.01, ** p<0.05, * p<0.1. S tandard errors in parentheses clustered at course level. Fixed effects at the course level. Includes the following controls variables at baseline: gender, age, marital status, experiences in the course of application, poverty index, education of the head of the household.

The results so far suggest that the program is shifting the search methods towards more professional or more formal methods (the use of professional contacts and self-directed search), and that the program makes professional contacts to be more effective job finding methods. It is also remarkable that the program doesn´t have any impact on non-professional contacts as a job search and job finding method.

Impact of the program on the networks of contacts

Table 3 shows the results of the estimation of equation 2, where the dependent variable is whether a contact helped to search job in the last two years (since after the program). This variable is a proxy of the composition of the network of the individual. The results reinforce the idea that the program strengthens the network of professional contacts of the youth. On the one hand, column 1 shows that they were helped to a greater extent by contacts than the control group. On the other hand, column 2 shows that the kind of contact that helped them was a contact from the professional sphere. Table 3.B. digs in the specific type of contact used and shows that the difference is driven by an increase of contacts met in *former jobs and apprenticeships*. This suggests that the change in job search methods may be led by contacts met in the apprenticeship. Notice that other type of contacts, such as those met in non-academic courses, seem not be impacted by the program.

Table 3.A. Did a contact of type [X] help you to find a job in the last two years? (OLS)

VARIABLES	A Contact Helped Job Search in Last 2 Years	What kind of contact?	
		Professional	Non-Professional
	(1)	(2)	(3)
=1 Intended To Treat	0.035***	0.055***	0.019
	(0.012)	(0.015)	(0.014)
Observations	3,282	3,282	3,282
Control Mean:	0.696	0.182	0.660
Number of curso_num	41	41	41

*** p<0.01, ** p<0.05, * p<0.1. S tandard errors in parentheses clustered at course level. Fixed effects at the course level. Includes the following controls variables at baseline: gender, age, marital status, experiences in the course of application, poverty index, education of the head of the household.

Table 3.B. Did a contact of type [X] help you to find a job in the last two years? (OLS)

VARIABLES	Where did you meet your contact?							
	Job or Apprenticeship	Non-Academic Course	Academic Course	Family	Social Organization	Political Organization	Friends	Other
	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
=1 Intended To Treat	0.047***	0.010	0.019	0.029	-0.004	0.004	-0.018	0.000
	(0.011)	(0.009)	(0.012)	(0.018)	(0.010)	(0.011)	(0.016)	(0.002)
Observations	3,282	3,282	3,282	3,282	3,282	3,282	3,282	3,282
Control Mean:	0.0880	0.0649	0.0817	0.440	0.158	0.0869	0.282	0.00628
Number of curso_num	41	41	41	41	41	41	41	41

*** p<0.01, ** p<0.05, * p<0.1. S tandard errors in parentheses clustered at course level. Fixed effects at the course level. Includes the following controls variables at baseline: gender, age, marital status, experiences in the course of application, poverty index, education of the head of the household.

Other features of the program that can affect the job search methods

It should be acknowledged that besides the contacts made in the apprenticeship, there may be other factors of the program affecting the change in job search methods. For instance, Ibarran 2012 found that the program has significant impacts on life-skills such as persistency of effort and ambition. It is reasonable to think that a change in life skills may change their behavior when looking for a job. To explore the relation between life-skills and job search methods I estimate equation 3. To avoid the endogeneity the program I use the sample of controls. Table 4 shows that there is not a clear relation between job search methods and soft-skills influenced by the program, suggesting that this is not the pathway in which the search methods are being affected.

Table 4. Relation between Job Search Methods and Life Skills for the Sample of Controls (OLS)

		Search Methods			
	Didn´t Search	Professional Contacts	Non-Professional Contacts	Self-Directed	Open Business
VARIABLES	(1)	(2)	(3)	(4)	(5)
Rossemberg Scale	0.006	-0.001	-0.000	-0.004	-0.001
	(0.003)	(0.002)	(0.001)	(0.003)	(0.001)
Total CPS	0.011	-0.003	0.005	-0.011	-0.002
	(0.014)	(0.004)	(0.004)	(0.013)	(0.002)
Leadership	-0.016	0.003	-0.009*	0.020	0.002
	(0.020)	(0.005)	(0.005)	(0.018)	(0.002)
Conflict Resolution	-0.012	0.003	-0.007	0.015	0.001
	(0.013)	(0.003)	(0.006)	(0.014)	(0.002)
Socialization	-0.008	0.003	-0.001	0.001	0.005
	(0.017)	(0.005)	(0.006)	(0.015)	(0.003)
Organization	-0.020	0.004	-0.006	0.019	0.003
	(0.021)	(0.005)	(0.005)	(0.017)	(0.002)
Communication	-0.009	0.003	-0.009*	0.012	0.002
	(0.012)	(0.004)	(0.005)	(0.013)	(0.002)
GRIT	-0.013*	0.002	0.002	0.009	0.000
	(0.008)	(0.002)	(0.004)	(0.007)	(0.001)
Consistency	0.006	-0.002	0.002	-0.005	-0.000
	(0.008)	(0.002)	(0.003)	(0.007)	(0.000)
Perseverance	-0.000	0.007*	0.003	-0.010	0.001
	(0.015)	(0.004)	(0.007)	(0.017)	(0.002)
GRIT (Revised)	0.015	-0.006*	-0.006	-0.002	-0.001
	(0.011)	(0.003)	(0.007)	(0.011)	(0.002)
Ambition	-0.003	-0.002	0.007	-0.002	0.000
	(0.009)	(0.003)	(0.005)	(0.010)	(0.001)
Observations	1,010	1,010	1,010	1,010	1,010
Control Mean:	0.649	0.0168	0.0644	0.266	0.00396

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Inc ludes controls for gender and age.

3.6. Implications in Labor Outcomes

Disentangling the impact of the search method in the labor markets attributing causality entails a more in depth analysis and escapes to the scope of this experiment. However, I try to explore the relation between job search and job finding methods with labor outcomes to the extent possible. Table 5 shows the OLS regressions of equation 4 for the subsample of controls (to avoid the endogeneity of the program). The job finding methods are a dummy variable and the omitted option is non-professional contacts, so a positive and significant coefficient in a job finding method implies that it is positive compared to the non-professional contacts. Column 1 shows that there is no difference in salary whereas you found your job using professional contacts or self-directed search methods compared to non-professional contacts. In line with Ibarraran 2012, I don't find impact on salaries for the treatment group. However, there are significant effects of finding job through professional contacts on some formality measures, such as having a contract, having a permanent position or the number of hours worked. There are also positive effects for those who found their jobs through self-directed search in having life and health insurance. In both cases the individuals who found their jobs through these methods are less willing to change their jobs.

Table 5. Job Finding Methods and Job Characteristics for the Sample of Controls (OLS)

VARIABLES	Ln Hourly Salary (1)	Contract (2)	Permanent Position (3)	Life Insurance (4)	Health Insurance (5)	Days Worked (6)	Hours Worked (7)	Wants Change Job (8)
=1 Found Job Using Professional Contacts	0.38	0.53***	0.36***	0.17	0.29	0.91**	1.07	-0.45**
	(0.47)	(0.18)	(0.03)	(0.18)	(0.20)	(0.37)	(6.86)	(0.19)
=1 Found Job Using Self-Directed Methods	0.09	0.10	0.10*	0.06*	0.06*	0.29***	3.31**	-0.09**
	(0.09)	(0.06)	(0.06)	(0.04)	(0.03)	(0.10)	(1.24)	(0.04)
Observations	562	637	637	637	637	637	637	637
Dependant Var Mean:	3.406	0.206	0.680	0.111	0.149	5.068	38.20	0.827

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Inc ludes controls for gender and age.

This evidence suggests that part of the impact of the program may be driven by the better job seeking practices triggered by the program. Moreover, the program is enabling students to increase their professional network, therefore, finding jobs that match better their professional profile, and providing with better labor outcomes in terms of formality.

4. Conclusions, Policy Recommendations and Further Research

This paper provides with some contributions to the literature of job search in the theoretical and empirical fronts.

In the theoretical side it provides with a new categorization of job search methods differentiating between the use of professional contacts, non-professional contacts, and self-directed search methods. To the knowledge of the author this is the first time that a clear distinction between the role of professional and non-professional is done for job search models. The empirical part uses original data from a sample of youth in Dominican Republic in 2009. The individuals were socially vulnerable youths that applied to a vocational training program offered by the Ministry of Labor in 2007. In line with previous empirical results, descriptive statistics show that the main search methods used by unemployed are self-directed search methods (75%) followed by the use of non-professional contacts (7%). The main job finding methods are non-professional contacts (74%) and self-directed search methods (22%). Compared to job search rates in more developed contexts this evidence suggests that in socially vulnerable settings individuals tend to use more informal methods to search for jobs. The use of professional contacts only account for 6 % of the search methods and 4% of job finding methods.

Finally, taking advantage of the experimental design of an impact evaluation conducted for the training program, I show that the program has an impact on job search methods. On the one hand the treatment group is more active searching for jobs, and on the other hand they use more professional contacts and self-directed search methods. Also, the treatment group found their current jobs through professional contacts more than the control group. Subsequently, I explore in more detail the networks of the individuals and I observe that the professional network of the treatment group (more precisely because of the contacts met in former apprenticeships and jobs) has been more active for the treatment group. This suggests that the program enlarges the number of professional contacts of the individuals through the apprenticeship. It is also explored the method through which job finding methods can affect labor outcomes, and I see that jobs found through professional contacts (and self-directed search) are associated with higher levels of formality compared to the use of personal contacts. Although this relation is not causal, this suggests that contacts made during the program may play an important role in increasing job quality.

The main policy recommendation that arises from these results is to strengthen the ties with professional networks met during the apprenticeship. This could be done by extending the duration of the apprenticeship, providing with apprenticeships in more than one firm, or celebrating meetings or events between the alumni and the employers and workers of the firms among others.

There are several lines in which this study can be extended. On the analytical front, a theoretical model could be developed to explain the results from the theoretical side. On the empirical side it would be interesting to conduct a heterogeneity analysis differentiating by gender since labor outcomes are considerably different for male and females. It would be also interesting to extend the analysis in the long run. Additionally, this line of research would also benefit from an experimental design that allows disentangling the impact of the life skills module from the apprenticeship to find out what part of the training is more effective, and an experiment that allows creating exogenous variance to estimate the causal relation of professional contacts on labor outcomes. Finally, it would be interesting to see whether the impacts of the program in the job search methods throughout professional contacts in the short run prevail in the long run.

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Annexes

A. Attrition

Attrition is the problem of individuals preferring no longer to be interviewed, and dropping out, or moving and being unable to be traced. Attrition over 5% can be a problem if those who drop out of the sample share common characteristics (e.g. low income, low basic skills), since that can bias our results.

In order to make sure that the results of the study are not led by attrition, I check that attrition is not correlated with the treatment nor with personal characteristics. Table 6 shows the results of the following regression:

$$\text{Attrited} = a + b\ \text{ITT} + c\ X + e$$

Where Attrited equals 1 if the person was not found in the final survey, ITT equals 1 if the person was Intended To Treat (won the lottery of the courses), and X is a vector of characteristics of the individuals at baseline (therefore exogenous). Results show that there is a non significant relation between the attrition, the treatment and most of the characteristics of the individual[50]. This "validates" the results of the study.

Table 6. Attrition of the sample

[50] It is generally accepted that up to 10% of the variables may show a significant relation just by chance.

VARIABLES	Attrited (1)	Attrited (2)
=1 if Intended To Treat (ITT)	-0.009	-0.015
	(0.013)	(0.014)
=1 si joven es hombre		0.001
		(0.016)
edad del postulante en baseline		-0.000
		(0.002)
=1 si joven estudia actualmente en baseline		-0.018
		(0.020)
bl_trabaja		-0.051
		(0.040)
# children at baseline		-0.004
		(0.010)
=1 si joven soltero/a en baseline		-0.233
		(0.163)
=1 si joven en union libre en baseline		-0.230
		(0.162)
=1 si joven casado en baseline		-0.240
		(0.177)
=1 si joven divorciado/a en baseline		-0.028
		(0.181)
=1 si joven viudo/a en baseline		
=1 si en los dos ultimos a_os (antes de bl) ha tenido algun trabajo de al menos		-0.044
		(0.040)
numero de trabajos de >= mes de duraci_n en los dos a_os antes a la lb		0.035
		(0.037)
=1 si tiene experiencia en el curso que solicita		0.010
		(0.016)
ln_num_personas		
bl_personas		0.261***
		(0.054)
bl_num_mujeres		-0.260***
		(0.053)
bl_activecon		0.011
		(0.014)
bl_remesas		0.014
		(0.020)
% ninios menores de 5 anios		0.009
		(0.009)
escala de pobreza 0-100		0.005***
		(0.001)
bl_casa_numhabi		-0.043***
		(0.014)
bl_hacin		-0.019*
		(0.010)
bl_sexojefe		0.012
		(0.015)
bl_educjef1		0.002
		(0.002)
el jefe del hogar tiene bachiller		-0.059***
		(0.020)
bl_estufa		-0.085**
		(0.036)
bl_nevera		-0.019
		(0.019)
bl_televisor		-0.011
		(0.023)
bl_lavadora		-0.017
		(0.012)
bl_vehpriv		-0.016
		(0.016)
bl_aireacond		-0.003
		(0.037)
bl_computa		0.004
		(0.019)
bl_razonparticipa_trab		0.234***
		(0.051)
bl_razonparticipa_estud		0.218***
		(0.078)
bl_razonparticipa_superar		0.211***
		(0.054)
bl_razonparticipa_familia		0.236***
		(0.064)
bl_razonparticipa_emigrar		0.119
		(0.103)
bl_razonparticipa_otro		

bl_org_recre		0.049
		(0.050)
bl_org_depor		0.045
		(0.042)
bl_org_polit		0.072
		(0.050)
bl_org_reli		0.063*
		(0.034)
bl_org_otra		0.116**
		(0.045)
bl_org_ninguna		0.080**
		(0.034)
bl_autoes_satis_muydeacuer		0.028
		(0.044)
bl_autoes_satis_algodeacuer		0.047
		(0.051)
bl_autoes_satis_pocodeacuer		0.055
		(0.049)
bl_autoes_satis_nodeacuer		0.048
		(0.057)
bl_autoes_nobueno_muydeacuer		-0.011
		(0.042)
bl_autoes_nobueno_algodeacuer		0.028
		(0.049)
bl_autoes_nobueno_pocodeacuer		-0.007
		(0.051)
bl_autoes_nobueno_nodeacuer		-0.013
		(0.044)
bl_autoes_cualidades_muydeacuer		0.112*
		(0.057)
bl_autoes_cualidades_algodeacuer		0.102*
		(0.059)
bl_autoes_cualidades_pocodeacuer		0.126*
		(0.071)
bl_autoes_cualidades_nodeacuer		0.141*
		(0.070)
bl_autoes_nobien_muydeacuer		-0.059
		(0.061)
bl_autoes_nobien_algodeacuer		-0.071
		(0.066)
bl_autoes_nobien_pocodeacuer		-0.039
		(0.096)
bl_autoes_nobien_nodeacuer		-0.082
		(0.088)
bl_autoes_noorgull_muydeacuer		-0.069
		(0.051)
bl_autoes_noorgull_algodeacuer		-0.075
		(0.051)
bl_autoes_noorgull_pocodeacuer		-0.035
		(0.051)
bl_autoes_noorgull_nodeacuer		-0.067
		(0.052)
bl_autoes_inutil_muydeacuer		0.066
		(0.068)
bl_autoes_inutil_algodeacuer		0.009
		(0.070)
bl_autoes_inutil_pocodeacuer		0.037
		(0.072)
bl_autoes_inutil_nodeacuer		0.046
		(0.071)
bl_autoes_igualvalor_muydeacuer		-0.102
		(0.062)
bl_autoes_igualvalor_algodeacuer		-0.114
		(0.068)
bl_autoes_igualvalor_pocodeacuer		-0.158**
		(0.063)
bl_autoes_igualvalor_nodeacuer		-0.153*
		(0.082)
bl_autoes_respeto_muydeacuer		0.050
		(0.059)
bl_autoes_respeto_algodeacuer		0.029
		(0.058)
bl_autoes_respeto_pocodeacuer		-0.005
		(0.058)
bl_autoes_respeto_nodeacuer		0.026
		(0.054)
bl_autoes_fracaso_muydeacuer		-0.036
		(0.062)
bl_autoes_fracaso_algodeacuer		-0.047
		(0.079)
bl_autoes_fracaso_pocodeacuer		0.003
		(0.078)
bl_autoes_fracaso_nodeacuer		-0.001
		(0.072)
bl_autoes_posit_muydeacuer		-0.063
		(0.136)
bl_autoes_posit_algodeacuer		-0.080
		(0.141)
bl_autoes_posit_pocodeacuer		-0.023
		(0.144)
bl_autoes_posit_nodeacuer		-0.102
		(0.156)
=1 si joven no respondio		-0.264
		(0.166)
bl_num_hombres		-0.254***
		(0.048)
bl_razonparticipa_amigos		0.110
		(0.094)
Constant	0.200***	0.154
	(0.012)	(0.210)
Observations	4,623	4,087
R-squared		0.030
Number of curso_num	41	41

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

B. Balance at Baseline

According to the Law of Large Numbers and Central Limit Theorem, a large enough sample assigned randomly to two groups should generate two equivalent distributions. In other words, if the lottery of the program worked well, the treatment group and the control group should be equivalent at basement. In order to check this, I will check whether the means of the observable characteristics of the two groups are equivalent at baseline.

I conduct the baseline analysis using the registration form that the youth filled out at the moment of the application. This form included some personal characteristics (including gender, age, civil status, number of children), information about housing (kind of house they live in, tenancy status, ownership of appliances), basic information about the household members (number of people in the house, educational level, labor status), labor history of the youth (how many former jobs have they had to the date of registration, how much they earned, some formality measures), and a self-esteem module. Table 7 shows the average values of the main characteristics of the applicants at baseline. Column (1) shows the average value of all the observations of our sample. Column (2) shows the average values of the treatment group, column (3) shows the average values of the control group, column (4) shows the difference between the two groups, and column (5) shows the T-stat of the difference of averages. If the value of the T-stat falls below 1.96 in absolute values we will consider that the averages are statistically equivalent with a confidence interval of 95%. As Table 7 shows, the majority of the characteristics between the two groups are balanced, so we can validate the randomization and, therefore, assume that any change in the averages of the two groups after the program can be attributed to the program.

Table 7. Test of Equality of Means at Baseline

	No. Obs.	Average All	Average ITT	Average Control	(ITT-Control)	T-Stat	
Panel A. Individual characteristics							
Gender (male=1)	3,411	0.37	0.37	0.36	0.01	0.50	
Age	3,411	21.52	21.54	21.43	0.11	0.79	
Currently studing =1	3,411	0.24	0.24	0.25	-0.01	-0.50	
Currently working =1	3,400	0.26	0.27	0.26	0.01	1.00	
Marital Status							
Single =1	3,411	0.76	0.75	0.77	-0.02	-1.00	
Partner =1	3,411	0.21	0.22	0.18	0.04	2.00	**
Married =1	3,411	0.03	0.03	0.04	-0.01	-1.00	
Divorced =1	3,411	0.00	0.00	0.00	0.00	0.00	
Widow =1	3,411	0.00	0.00	0.00	0.00	0.00	
Number of Children	3,411	0.71	0.70	0.72	-0.02	-0.67	
Panel B. Work experience							
Ever worked =1	3,411	0.21	0.20	0.21	-0.01	-0.50	
Number of previous jobs	3,411	0.23	0.23	0.23	0.00	0.00	
Experience in the field of the course =1	3,390	0.17	0.18	0.15	0.03	3.00	**
Panel C. Household characteristics							
Number of inhabitants	3,411	4.50	4.47	4.57	-0.10	-1.11	
Number of males	3,411	2.12	2.12	2.11	0.01	0.20	
Gender Head of Household (male=1)	3,411	0.44	0.45	0.43	0.02	1.00	
Tertiary Education Head of the Household	3,175	0.16	0.16	0.15	0.01	1.00	
Number of Children under 5	3,411	1.43	1.44	1.41	0.03	1.50	
Economic activity in the house =1	3,411	0.11	0.11	0.11	0.00	0.00	
Receive remmitances =1	3,411	0.11	0.10	0.10	0.00	0.00	
Poverty Index	3,409	61.09	61.23	60.62	0.61	1.91	*
Poor level 1	3,411	0.06	0.06	0.07	-0.01	-1.00	
Poor level 2	3,411	0.37	0.37	0.39	-0.02	-1.00	
Number of rooms	3,411	2.24	2.24	2.26	-0.02	-0.50	
Panel D. Motivation and personality							
Reason for registering in the training							
To find a job=1	3,385	0.45	0.44	0.44	0.00	0.00	
For self development =1	3,385	0.47	0.47	0.46	0.01	0.50	
I feel satisfied with myself							
Strongly Agree =1	3,411	0.84	0.84	0.84	0.00	0.00	
I feel I am not good enough							
Strongly Disagree	3,411	0.64	0.65	0.62	0.03	1.50	

Notes: t-stats of differences in means computed clustering standard errors at the course level.

** Significant at 5% level; * Significant at 10% level.

CHAPTER 3. The Impact of Upgrading Municipal Infrastructure. Evidence from Brazil

Paloma Acevedo

Abstract: This paper studies the impact of a public infrastructure intervention on social wellbeing. It uses the hedonic prices approach to estimate social wellbeing and a differences-in-differences methodology to estimate the impact of the intervention. Results show a modest impact of the intervention overall, however, when it is decomposed by components it finds a positive impact on the transportation component and ambiguous impacts in the revitalization component. Notwithstanding, the analysis has some limitations, principally, because of lack of data.

1. Introduction

54% of the population lives in urban areas today, and it is expected that in the next 15 years the number of people living in cities increases by 50%, from 4 to 6 billion people[51]. This speed and scale of urbanization brings challenges to meet the demands of urban infrastructure in the sectors of transport, housing, and basic services. Meeting these challenges requires intensive policy coordination and investment choices: what interventions are more effective on improving quality of life in the cities? Despite the increasing importance of this question, there is little evidence on the effectiveness of urban infrastructure projects. The main reason for that is that the nature of urban infrastructure projects presents specific challenges from the point of view of the methodologies used in impact evaluations. The most important are: first, it is very common that infrastructure projects affect single units and, second, those are pre-assigned based in strategic criteria. (For instance, there is only one reasonable place where a bridge can be built; there is only one city center in the city, etc.…) The main challenge here comes from the requirement of generating comparison groups and from the fact that the statistics techniques need big samples. However, literature has dealt with these challenges by generating exogenous variation at the user level or taking advantage of time and geographic limits or the implementation in phases of the project.

Some of the rigorous impact evaluations in this field are Cerdá et al. (2012) and Gonzalez-Navarro et al. (2010). The first one uses a natural experiment to study how a public transit system intervention to connect isolated low-income neighborhoods to the city's urban center affects violence. They find that the homicide rate declined by 66 percent more in treated

[51] World Bank (2013).

neighborhoods than in the control neighborhoods. The second one uses a randomized control trial to study how street paving in Mexico raises housing values. Using expert's appraisals, they find that paving streets increases housing prices by 16 percent and land values by 54 percent.

In line with these investigations, this paper shed new light on the impact of urban infrastructure investments on the living standards of the individuals.

The study takes place in the Municipalities of Campo Grande in Brazil. This Municipality received large amount of investments during the last years in the context of a program called "Procidades" that was financed, among other institutions by the IADB. The program invested in several items including rehabilitation of public spaces, and urban roads. Taking advantage of a historical administrative database on property prices and property characteristics, I estimate impacts using a difference in difference approach.

This research is important for two reasons: on the first hand, it increases the scare literature on urban infrastructure evidence, and on the other hand, it exploits a rich administrative database on house prices. The advantages of the use of administrative data are threefold: In first place, it overcomes the noise generated by individual appraisals on property prices[52], setting an objective indicator. In second and third place, from a more practical point of view, it reduces considerably the cost of the impact evaluation (since most of the times the bulk of the expenses comes from data collection), and it allows to give a timely response to policy makers.

The results are mixed. On the one hand I find that the transportation component in Campo Grande had a positive impact, but I don't find significant positive impacts for the revitalization component.

[52] For instance, Gonzales, 2012, uses appraisals of experts that are subjective and therefore, noisy.

From the cost-effectiveness perspective, I find that the transport intervention increased the value of housing in almost 100million dollars.

Notwithstanding, I find some limitations in our analysis that should be taken into consideration. The main one is the lack of information of prices for longer periods of time pre and post interventions. Also, in order to control for other interventions, better information on the other interventions conducted in the municipalities during the time of the analysis would be needed. Finally, the fact that some interventions are conducted in very unique neighborhoods (such as the city center), makes it difficult to find a proper comparison group.

2. Context

Brazil has experienced a high degree of economic growth in the last decade. As a result some cities have received a big amount of population in a relative short period of time. Favorable economic conditions experienced by the cities in recent decades have led to significant advances in infrastructure and housing services. However, some towns still present basic infrastructure and social problems such as: deficiencies in their road system (which prevents a balanced development between different parts of the cities); increasing deterioration of its urban center (threatening economic and social sustainability); and lack of public spaces for recreational use, (such as practice of sports and allow for community livings.)

Several infrastructure investments have been devoted to overcome these problems. One of these programs has been “Procidades” that devoted part of its investment to urban infrastructure interventions in Campo Grande.

The Municipality of Campo Grande, with an area of 8,096 km2 and a population of 796.252 inhabitants, is located in west-central Brazil region. The municipality has a high degree of urbanization (99%) and in recent decades has experienced significant population growth (between 1970 and 2000 its population increased by five). In addition, the economic indicators of the municipality during the last year show characteristic patterns of a process of sustained development, benefiting from a growing trade and service industry. These economic developments are also reflected in significant improvements in the living conditions of its inhabitants, achieving high indicators of health, education and security. According to the UNDP classification, Campo Grande is among the municipalities considered of high human development (HDI greater than 0.8), ranking 11th in the ranking of the capital cities and the number 307 in the national classification comprising 5,507 municipalities.

Despite the favorable economic conditions, some parts of the town still show basic infrastructure and social problems:

One of the main problems facing the municipality is a process of increasing deterioration of its urban center, threatening economic and social sustainability. Regarded as the commercial, residential and cultural heart of the city and headquarters buildings with heritage value, the center began in the early 80s to lose its diversity of use to suffer displacement of housing and trade to other areas resulting in the weakening of the local economy, degradation and waste of infrastructure, creating conditions of insecurity, and physical deterioration of buildings - some historical value and risk of total loss.

Another major problem the town faces are marked deficiencies in its road system, which prevents a balanced development between different parts of the city. The road system has peculiar characteristics with the existence of high capacity roads (up to five lanes) promptly affected by natural barriers (valley bottoms) and by railway lines, which significantly limits their effectiveness.

3. Intervention

The program of sustainable socioeconomic development of Campo Grande aimed to provide solutions to the main problems of the municipality, contributing to the development of a more balanced and equitable city. The main components of the program are: (i) revitalization of the downtown area; and (ii) mobility and transportation.

Component I: Revitalization of the City Center. The first component aims to promote improvements in the urban environment of the historic center of Campo Grande and reverse the loss of economic and social dynamics. The main intervention consists of the renovation of desolated areas into a recreational park called "*Orla Ferroviaria".* This component financed the remodeling of an old railway environment which crosses the city center. Both the station and its environs have become a deserted area, which not only hinders the integration between the east and west zones, but is a source of insecurity for the population living around. The intervention consisted in the creation of a linear park called Orla Railway devoted to contemplation and leisure with a walkway, places of relaxation and conviviality, a playground, a gym, garden spaces, and street furniture and lighting. Also there were conducted cultural activities and exhibitions for local products, crafts and cuisine. There have also remained parts of the old rails, resulting in an aesthetically pleasing composition that refers to the memory of the railroad. The component also includes the renovation and restoration of two traditional houses situated on the park rescuing an important part of the historical and cultural memory of Campo Grande.

Component II: Urban Mobility. The second component aims to address the problems of the road system, especially in the western sector connectivity with the city center. It includes activities related to improve the urban mobility system by promoting connectivity highways. The main investment into this component is the *improvement of road connectivity* between the west and the central area through the expansion and improvement of the main arteries. Routes that received the intervention were: (i) Via Morena[53], (ii) Avenida Julio de Castilho and (iii) Orla Morena[54]. Note that the intervention also included Orla Morena enabling spaces adjacent to the road turning them into linear parks. In addition to that the component also included the *modernization of road safety system* by implementing a new traffic light system for a total of 180 intersections.

Importantly, despite the intervention of Orla Morena is formally included in the transportation component, the intervention has a strong focus on the recovery of public spaces, which makes it a little different from other transport interventions. In this sense the intervention of Orla Morena will be treated as part of the revitalization component.

[53] Section of Campo Grande International Airport to Avenida Julio de Castilho.

[54] From Avenida Castilho July 14 July.

Table 1 shows the major investments made in these components, the amount and date of commencement and completion of works.

Table 1. Main Infrastructure Interventions of Procidades at Campo Grande (2009-2013)

Infrastructure Work	Amount (US$)	Starting Date	Finalizing Date
1. Revitalization of the City Center	**14,098,000**	**Mar-09**	**Jun-13**
Obra Orla Ferroviária	*2,600,000*	*Feb-11*	*Apr-13*
Orla Morena 1st Stage	*6,498,000*	*Mar-09*	*Dic-10*
Orla Morena 2nd Stage	*5,000,000*	*Feb-11*	*Jun-13*
2. Transport and Mobility	**20,906,000**	**Nov-09**	**Jul-13**
Via Morena	*10,071,000*	*Nov-09*	*Dic-12*
Avenida Júlio de Castilho	*10,835,000*	*Ago-11*	*Jul-13*

Source: Inter-American Development Bank

4. Evaluation

The impact evaluation seeks to measure the effect of investments in public infrastructures on the quality of life and socio-economic development of the municipality. To identify the causal effects of an intervention, impact evaluations compare the results of the treated population with an estimate of the situation that would have prevailed in the absence of such intervention (called counterfactual). To estimate the counterfactual situation it is used a “control” group, that should be statistically identical to the treatment group but they won’t receive the intervention.

4.1 Identification and Methodology

Given the retrospective character of the evaluation and the availability data, I will use a differences in differences approach. This methodology compares the changes in outcomes over time between a population that is enrolled in a program (the treatment group) and a population that is not (the comparison group). By comparing the enrolled before and after, we are controlling for factors that are constants over time (since we are comparing the same group to itself). And, by comparing the enrolled group with a group that is exposed to the same conditions, we are controlling for time-varying factors. In this way we are controlling for the most worrisome sources of bias (self-selection and time-varying factors).

With the available data, the smallest unit of assignment available is the property level, so that will be the unit of analysis. To determine which properties belong to the treatment group and to the control group I will use the criterion of geographical proximity to the interventions. Thus, I will assume that those properties that are closest to the interventions are the ones that will benefit from the investment and, therefore, will belong to the treatment group, whereas those that are further away from the works will belong to the control group. To determine the location of the property in respect to the infrastructure works I will use the most accurate possible measures. Since there are no GPS coordinates for the properties, the smallest geographical unit that allows us to allocate accurately the property is the variable "neighborhood". Therefore, the properties belonging to adjacent neighborhoods of the interventions will be considered the treatment group, and the properties of the more remote areas will be used as a comparison group.

Although the properties that are located in different areas of the municipalities may have different values (e.g. property in the center versus properties on the periphery), all properties are simultaneously exposed to economic conditions within the municipality, so we would expect that changes or macroeconomic "shocks“ on municipal properties will affect both, the treatment and control. Therefore, the control group will control for time variations.

There is also information on the dates when the interventions started and finalized. I will assume that individuals anticipate the change in the values as soon as the works start to be implemented, therefore, I will consider the starting date of the works as the beginning of the treatment period.

Finally, since the objective of the program is to improve the quality of life of the population of the municipality and contribute to economic competitiveness, in my analysis I will discard territorial properties, as these are in outlying areas of the city center and selling price is governed by different criteria from the rest of the properties. Even more, to ensure comparability of the observations in the sample, the analysis will focus on residential properties.

Hedonic Prices

To quantify the benefits of the infrastructure works I will use the hedonic price approach using the variation in housing prices. The logic behind that is that, if the interventions have improved quality of life of the citizens, the desirability in terms of demand will be reflected in an increase in housing prices in the neighborhood. Thus, the outcome indicator that I use to conduct the impact analysis is the price of real estate (houses, buildings).

According to the hedonic price model (Griliches, 1979) the price of a good is determined by the implicit price of each of its components. In this case, the price of real estate would be formed by the implicit prices of attributes such as number of rooms, quality of materials and provision of urban infrastructure services, among others. In a competitive market, price is determined by the equilibrium in which the functions of demand and supply of buyers and sellers are equal.

According to the theory of hedonic prices, changes in real estate prices by varying one of its attributes (and keeping everything else constant) determine the valuation of individuals of that attribute. In our case, the change in housing prices by providing them with new urban infrastructure reflects what must be paid to the individual to maintain their standard of living. The marginal willingness to pay for each of the attributes available can be used to infer the welfare effects of a marginal change in one of the attributes for individuals.

In this context, it may be concluded that the intervention of urban infrastructure has a positive impact if the price of housing in the treatment group (those who have benefited from urban investments) is greater than it had been in the absence of investment (estimated by the control group or those of similar homes that have not been beneficiaries of investments).

Importantly, there are some limitations when using prices as an indicator of impact. Specifically we are building on the assumption that markets work well, however, in some countries or regions may dominate certain degree of informality or allocation of land and buildings that are not commercially available or can be barriers to mobility that make prices do not collect welfare. On the other hand, there are advantages to study property prices versus other variables. For example, prices account for the effects faster than other outcome variables such as reallocation of businesses, employment rates, etc... Also, in areas with certain degree of development, data on real estate prices are typically available in administrative records of fiscal agencies or agencies buying and selling real estate.

Identification

Figure 1 shows a map of the area of interventions and the name of the nearby neighborhoods. The names of the neighborhoods that are circled are the ones that are assigned to the treatment group. Those of the left-hand part of the map circled in black and red belong to the transportation component; whereas those at the right-hand side of the map circled in green belong to the revitalization component.

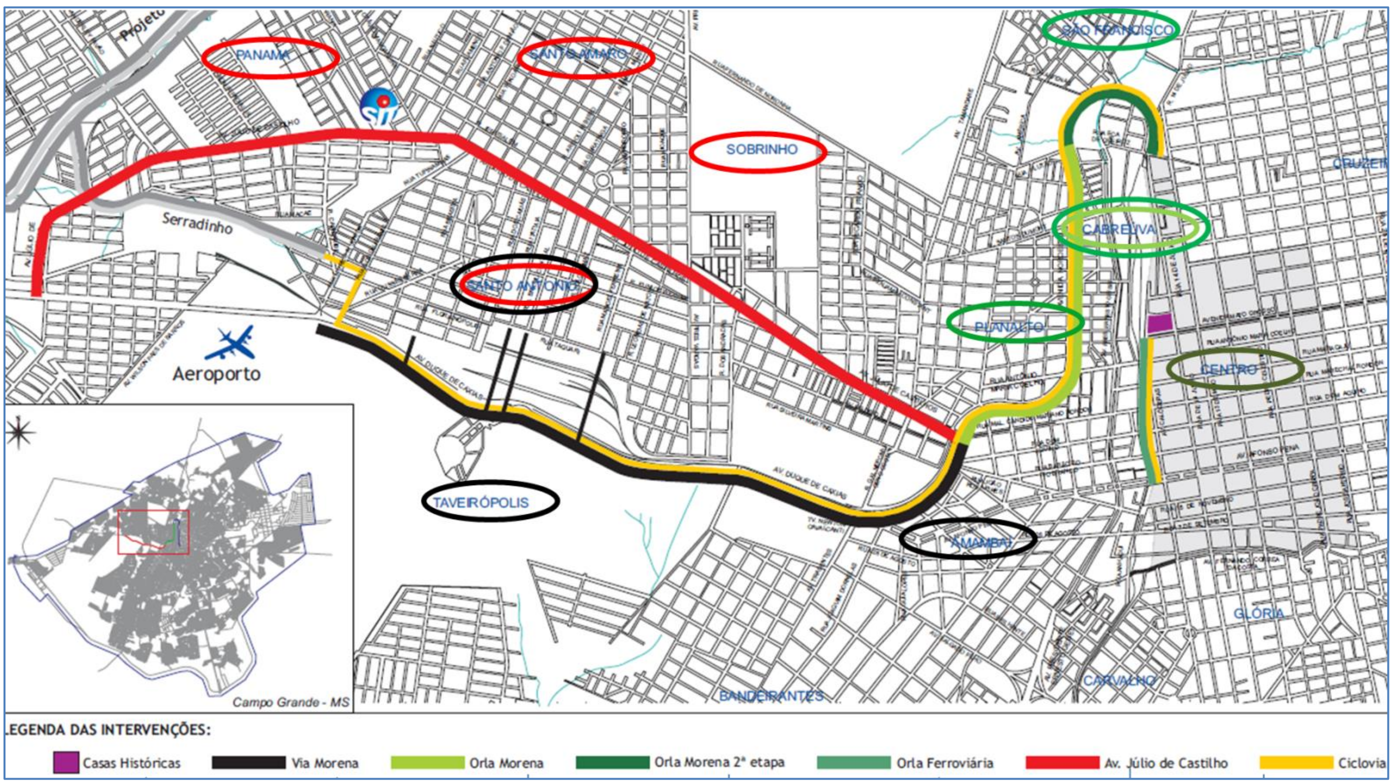


Figure 1. Map of the Investments of Procidades in Campo Grande.

Table 2 shows when the components of the analysis start and finalize (details are specified in Table 1).

Table 2. Timeline of interventions of the urban revitalization and transportation of Procidades in Campo Grande.

Infrastructure Work	Starting Date	Finalizing Date
1. Revitalization of the City Center	Mar-09	Jun-13
2. Transport and Mobility	Nov-09	Jul-13

Specification

The following model is specified to identify impact:

$$P_{ist} = \alpha T_{st} + \mu_s + \pi_t + \beta X_{ist} + \gamma K_s + \varepsilon_{ist} \qquad (1)$$

Where P_{ist} is the logarithm of price per square meter of property i located in neighborhood s in semester t, Tst is a variable that takes the value 1 for the treatment neighborhood from the time when begins treatment and 0 otherwise, μ_s is a dummy variable per neighborhood, π_t is a dummy variable for period of time t, X_{ist} are observable characteristics of the property, K_s is a dummy variable that equals 1 for neighborhoods with other interventions of urban infrastructure and ε_{ist} is the error term, that contains the unobservable characteristics of the property price. Standard errors are clustered at the neighborhood level. I will assume that the property price adjusts instantaneously to changes in the expected value. The coefficient α captures the aggregate impact of the intervention from the time of the start of the intervention.

We also analyze the differential effects before, during or after the period of implementation of the works. For this model 2 is estimated:

$$P_{ist} = \sum_{j=1}^{j=n} \alpha_j T_{stj} + \mu_s + \pi_t + \beta X_{ist} + \gamma K_s + \varepsilon_{ist} \qquad (2)$$

Where T_{stj} is a dummy variable that equals 1 for treatment neighborhoods during the treatment and 0 in the remaining periods of time. The other variables are interpreted as in equation (1).

5. Data

The main sources of information about property prices come from two administrative databases managed by the municipality:

(i) "Tax on Transmission of Real Property" database (ITBI for its Portuguese acronym[55]) contains information on property prices of transactions in real property. It includes variables such as date of registration of the property, type of transaction, property prices and payment date of the transaction, and variable location of housing (including address, area, district, area, subdividing).

(ii) Urban Building and Land Tax (IPTU for its Portuguese acronym[56]) database contains historical data on the basic characteristics of the properties such as area, number of rooms, water service provision, paving, lighting, telephone, urban, materials of the walls, floor, ceiling, roof, and other characteristics of the construction.

There may be other investments in the municipality during the time of the intervention that may be biasing our estimates. For instance, if they are constructing urban infrastructure assets in a

[55] Imposto sobre a transmissão de bens imoveis

[56] Imposto Predial e Territorial Urbano

neighborhood that belongs to the treatment group, we will be counting it as part of our program and therefore we will be overestimating the impact. On the other hand, if they are constructing in the control group we may be underestimating the impact. Therefore, data on other interventions during the period of analysis were also used to control for those bias.

According to ITBI 21,355 properties were sold between February 2008 and November 2013. Table 3 shows its distribution along the different kind of properties. For the reasons mentioned above, we will focus in residential properties.

Table 3 Frequency Chart type of properties sold in Campo Grande (2008-2013)

USOIMOVEL	Freq.	Percent	Cum.
COMERCIAL	110	0.52	0.52
FINALID ESSENCIAIS	2	0.01	0.52
INDUSTRIAL	2	0.01	0.53
MISTO	105	0.49	1.03
RELIGIOSO	6	0.03	1.05
RESIDENCIAL	12,634	59.16	60.22
SERVICOS	309	1.45	61.66
TERRITORIAL	8,187	38.34	100
Total	21,355	100	

Source: administrative data ITBI Campo Grande

The database of IPTU has data on all the properties in Campo Grande from the years 2005 to 2013. The database contains information on basic characteristics such as area, water utilities, paving, lighting, telephone, urban planning, and other.

Figure 2 below shows other interventions conducted in the municipality besides the program that we want to estimate and the neighborhoods that are affected.

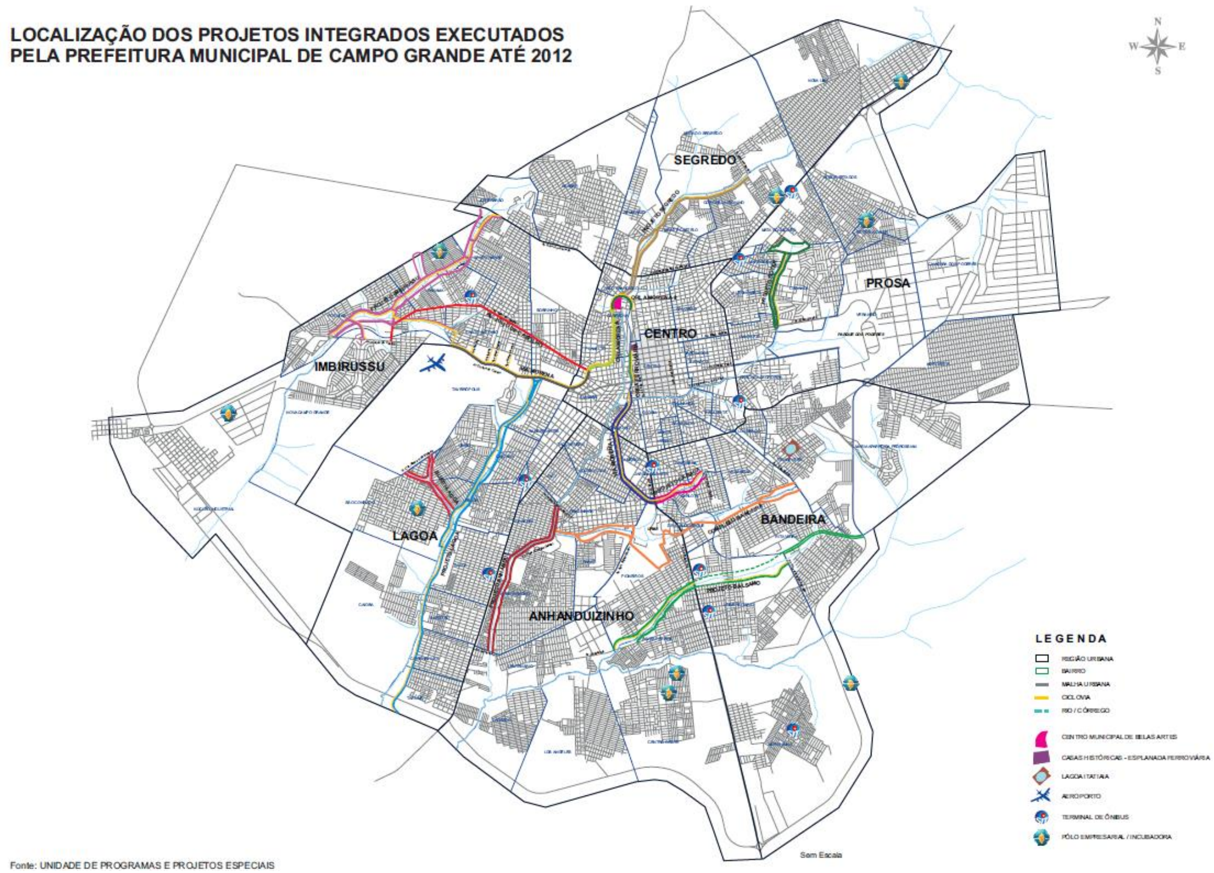


Figure 2. Map of all the urban infrastructure interventions carried out in the municipality of Campo Grande between 2008 and 2012[57]

Therefore, the database used for the analysis of Campo Grande consists of 12,634 observations of the residential properties sold in Campo Grande in the available period February 2008 to November 2013. For each residential property sold, we have the price, date of sale, neighborhood, basic observable characteristics, and on the other hand we count with information on other interventions in the municipality.

Table 4 shows the distribution of observations in the treatment group and control throughout the semester for the overall program and for each of its components.

[57] Notice we don't have data of new interventions started in 2013.

Table 4. Sample distribution by semester for treatment and control groups for different interventions

A. Overall Program

Periodo	Control	Tratamiento	Total
2008-1	873	0	873
2008-2	1,469	0	1,469
2009-1	790	420	1,210
2009-2	768	417	1,185
2010-1	729	372	1,101
2010-2	748	366	1,114
2011-1	683	336	1,019
2011-2	768	337	1,105
2012-1	760	300	1,060
2012-2	691	306	997
2013-1	688	255	943
2013-2	412	146	558
Total	9,379	3,255	12,634

B. Revitalization*

Periodo	Control	Tratamiento	Total
2008-1	876	0	876
2008-2	1,479	0	1,479
2009-1	1,170	43	1,213
2009-2	1,158	34	1,192
2010-1	1,097	16	1,113
2010-2	1,084	43	1,127
2011-1	935	98	1,033
2011-2	985	134	1,119
2012-1	985	82	1,067
2012-2	911	99	1,010
2013-1	874	79	953
2013-2	511	51	562
Total	12,065	679	12,744

C. Transport and Mobility

Periodo	Control	Tratamiento	Total
2008-1	873	0	873
2008-2	1,469	0	1,469
2009-1	1,210	0	1,210
2009-2	908	277	1,185
2010-1	832	269	1,101
2010-2	856	258	1,114
2011-1	772	247	1,019
2011-2	901	204	1,105
2012-1	839	221	1,060
2012-2	788	209	997
2013-1	766	177	943
2013-2	462	96	558
Total	10,676	1,958	12,634

*Includes 110 commercial properties that will be used in the final estimations of the revitalization component only.

Fuente: Municipal administrative records from IPTU and ITBI

6. Results

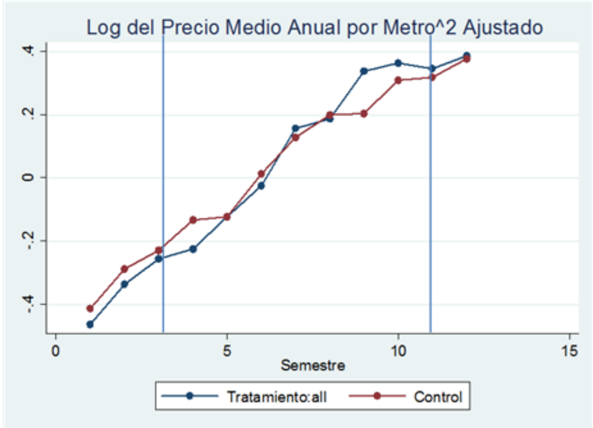
In the following sections we study the impact of the program as a whole and separating the revitalization and transportation components.

Graphic Analysis

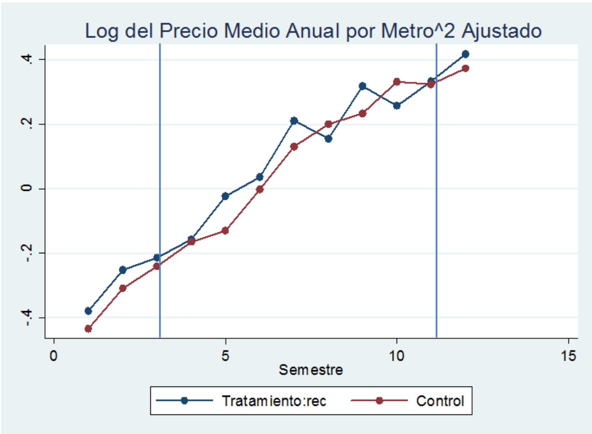
Figure 3 shows the evolution of the variable of interest (the logarithm of price per square meter) for the treatment group and the control group. The results are adjusted for observable characteristics of the properties and control various other program interventions have been carried out in the town. The vertical lines show the beginning and end of the period of implementation of the interventions. The horizontal axis includes the periods of time being 1 the first semester 2008, 2 the second semester of 2008 and so on until the period 12 corresponding to the second semester of 2013.

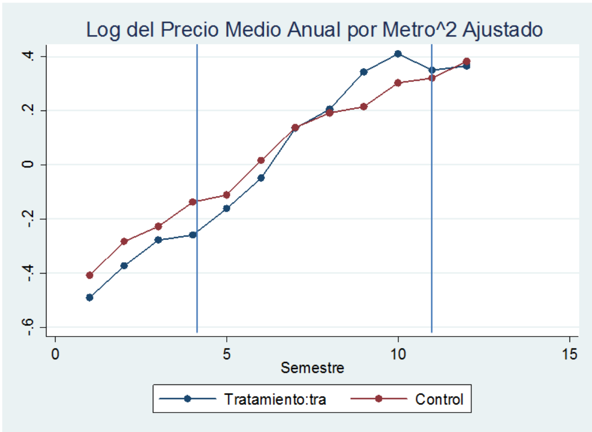
According to the graphs, it seems that the pre-intervention period evolves similarly in all the situations. Although the equality of trends needs will be tested more rigorously, it provides credibility to the comparability of treatment and control groups defined in the study.

Figure 3. Evolution of semi log price per square meter of property[1] set in the town of Campo Grande between 2008 and 2013



A. Overall Intervention





B. Intervention of Revitalization **C. Intervention of Transportation**

[1]Adjusted by observable characteristics of the properties and for other interventions conducted in the municipality.

Source: administrative data from ITBI, ITPU and information of other works in the municipality of Campo Grande.

Pre-intervention equal trends test

This section formally analyzes the pre-program treatment group and the comparison group trends. Table 5 shows the p-value for the results of a joint significance F test that compares the value of the slopes between the treatment group and the comparison group in the pre-intervention period.

Table 5. Joint significance F-test of equal pre-trends

Interventions	p-value of the F-test of equal trend for the pre-intervention periods [1]
Overall Program	0.9402
Intervention of Revitalization	0.7958
Intervention of Transportation	0.6623

[1]Adjusted by observable characteristics of the properties and for other interventions conducted in the municipality.

Source: administrative data from ITBI, ITPU and information of other works in the municipality of Campo Grande

As we see, in all cases we cannot reject the null hypothesis that the trend before the intervention is equal to 95% probability.

Impact of the program

Table 6 shows the result of equations (1) and (2) for different types of interventions.

Column 1 shows the result of equation (1) for the overall intervention. In column 2 the effect is decomposed by period of time. According to these results we would conclude that there are no significant impacts of the intervention (only a positive and significant impact of the program is observed in the first half of 2011.)

Models 3 and 4 show the analogous results but for the revitalization component. As shown, it seems that there is a significant negative impact of 5.3%. Decomposing the effect by periods we see that the negative impact comes from the period the second half of 2012 and first half 2013. Since this component is concentrated in the city center and these areas usually present special characteristics, we explore this result in more detail below.

Finally, columns 5 and 6 show the results for the interventions on transportation. The average impact of these interventions is 6.7% and significant at the 95% level. Decomposing the effect by semesters, we see that there is a negative (although non-significant) impact at the beginning. It could be explained by the inconveniences of the construction works that could have been offsetting the anticipation of the positive effects of the new roads. At the first half of 2011 the intervention starts to show a positive and significant effect that lasts until the second half of 2013. According to the time of implementation of works this increments match with the construction phase of the Avenida Julio Castilho, suggesting that the construction of this road had an immediate impact on prices of nearby properties.

Table 6. Impact of Procidades in the logarithm of the prices by squared meter in the Municipality in Campo Grande (2009-2013)[1]

VARIABLES	Overall Program		Intervention of Revitalization		Intervention of Transportation	
	(1)	(2)	(3)	(4)	(5)	(6)
Total Impact period 2009-2013	0.045		-0.053**		0.067***	
	(0.033)		(0.023)		(0.018)	
Mg impact period 2009-1		0.025		-0.133		
		(0.057)		(0.080)		
Mg impact period 2009-2		-0.044		0.032		-0.049
		(0.040)		(0.055)		(0.036)
Mg impact period 2010-1		0.032		0.030		0.021
		(0.078)		(0.041)		(0.086)
Mg impact period 2010-2		0.003		-0.025		-0.006
		(0.035)		(0.087)		(0.032)
Mg impact period 2011-1		0.080		0.000		0.087*
		(0.049)		(0.030)		(0.047)
Mg impact period 2011-2		0.029		-0.098*		0.085**
		(0.049)		(0.052)		(0.032)
Mg impact period 2012-1		0.178***		0.012		0.206***
		(0.052)		(0.044)		(0.038)
Mg impact period 2012-2		0.083		-0.114**		0.153***
		(0.054)		(0.044)		(0.032)
Mg impact period 2013-1		0.056		-0.084***		0.100*
		(0.052)		(0.025)		(0.050)
Mg impact period 2013-2		0.071		0.003		0.075
		(0.069)		(0.110)		(0.063)
Constant	6.111***	6.132***	6.733***	6.742***	6.618***	6.639***
	(0.464)	(0.463)	(0.445)	(0.444)	(0.466)	(0.463)
Observations	12,631	12,631	12,631	12,631	12,631	12,631
R-squared	0.522	0.523	0.522	0.522	0.522	0.523
Control Mean:	6.416	6.416	6.411	6.411	6.445	6.445
Muestra: Todo el municipio	Yes	Yes	Yes	Yes	Yes	Yes
Muestra Solo region Centro	No	No	No	No	No	No

Robust standard erros in parenthesis
*** p<0.01, ** p<0.05, * p<0.1

[1]NOTES:

1. Inlcudes fixed effects at the neighborhood level, at the period level, and controls for the characteristics of the propoerties and neighborhoods affected by other interventions. The property controls include: whether it is an appartment, the area of the lot, the area of the swimming pool, whether it was constructed before 2000, whether it has access to public transportation, whether it has access to municipal cleaning servicies, whether it has any of the following: water, garbage service, sewage, illumination, curb, paving, electricity, telephone, sidewalks. Also by the characteristics of the materials of the interior and exterior finish of the walls of the building, the roof, ceiling, window frames, structure of the building, floor, installation of electrical and sanitary installation, state of preservation, whether there is a lift and if it is in a regular or irregular situation.

2. Requalification Interventions include Orla Ferroviaria, Orla Morena, and Transportation Interventions includes Via Morena and Julio de Castilho.

Alternative Estimates of the Impact of the Revitalization Component

Revitalization interventions are concentrated in the city center, the historic area of the municipality. Due to the special and unique characteristics presented in the city center and the bordering areas, it is especially difficult to find comparable neighborhoods. Thus, in this section I explore alternative definitions of the comparison group.

Other limitation of this part of the study is that there are a relatively small number of residential properties sold (see table 7). To try to increase the number of observations, I take a broader definition of properties and include properties dedicated to commerce and services. However, only 110 of these property sales were recorded during the study period, so the results do not change much. Columns 1 and 2 of table 6 show the result of the estimation of the new sample.

Once this is done, we address the limitation of the particularities of the city center. The main limitation that city centers present is that it usually has unique features that make it difficult to compare with other areas of the cities. In particular, if the neighborhoods of the municipality included in the comparison group have growth rates that are not comparable with the city center in the absence of the program, our estimation could be biased. To control for this potential bias, I selected as controls only the sub-sample of neighborhoods adjacent to the treatment areas, but where no intervention was performed. Columns 3 and 4 of Table 7 show the result of this regression. As we see, using the new comparison group no significant differences were found between the treatment group and control[58]. One possible explanation for this could be that given the geographical proximity of the neighborhoods there is no effect because the control group is also benefiting from the impact of the construction of urban upgrading interventions.

[58] The p-value of the pre-treatment equal test is 0.1659, so we cannot reject equal trends at an 80% of probability.

Table 7. Estimating the impact of interventions Revitalization component in the log prices per square meter of properties in Campo Grande (2008-2013)[1]

VARIABLES	(1)	(2)	(3)	(4)
Total Impact period 2009-2013	-0.050**		-0.032	
	(0.023)		(0.022)	
Mg impact period 2009-1		-0.127		-0.192***
		(0.081)		(0.039)
Mg impact period 2009-2		0.007		0.022
		(0.063)		(0.073)
Mg impact period 2010-1		0.036		-0.056
		(0.041)		(0.048)
Mg impact period 2010-2		-0.020		-0.033
		(0.085)		(0.097)
Mg impact period 2011-1		0.012		-0.008
		(0.032)		(0.049)
Mg impact period 2011-2		-0.100*		-0.040
		(0.051)		(0.051)
Mg impact period 2012-1		0.021		0.024
		(0.046)		(0.045)
Mg impact period 2012-2		-0.125***		-0.078
		(0.042)		(0.045)
Mg impact period 2013-1		-0.072***		-0.043*
		(0.024)		(0.022)
Mg impact period 2013-2		0.001		0.013
		(0.110)		(0.127)
Constant	5.748***	5.750***	6.912***	7.098***
	(0.350)	(0.349)	(0.739)	(0.832)
Observations	12,741	12,741	5,210	5,210
R-squared	0.521	0.522	0.525	0.526
Control Mean:	6.412	6.412	6.741	6.741
Incluye inmuebles comerciales y de servicios	Si	Si	Si	Si
Sectores de Control	Todos	Todos	Aledaños	Aledaños

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[1]NOTES:
1. Inlcudes fixed effects at the sector level, at the period level, and controls for the characteristics of the propoerties and neighborhoods affected by other interventions. The property controls include: whether it is an appartment, the area of the lot, the area of the swimming pool, whether it was constructed before 2000, whether it has access to public transportation, whether it has access to municipal cleaning servicies, whether it has any of the following: water, garbage service, sewage, illumination, curb, paving, electricity, telephone, sidewalks. Also by the characteristics of the materials of the interior and exterior finish of the walls of the building, the roof, ceiling, window frames, structure of the building, floor, installation of electrical and sanitary installation, state of preservation, whether there is a lift and if it is in a regular or irregular situation.

7. Cost-Effectiveness

According ITBI database, the average price of real estate in the areas affected by transportation investments in the period of the study (2008-2013) was 73,448.79 Reais. Multiplying this value by the price increase attributable to this intervention (6.7%) we get a price increase of 4,921.1 Reais on average for each property. Multiplying this value by the number of properties in the areas affected by the construction of transport (39,691 properties), we could obtain an approximation to the monetary benefits generated by investment in transport, which would be of 195,323,380.1 Reais[59].

8. Limitations of the Study and Possible Extensions

The impact analysis presented above takes into account the data available at the time of the study. The following paragraphs propose some methodological refinements if greater accuracy in geographic information and additional data were available.

1) Level of assignment of the treatment: The analysis was performed using the neighborhood as the level of assignment, so we would not be considering the possible variability within neighborhoods. If information were available at the level of smaller units such as a district or street level, we could exploit more precise information on the properties affected by the different interventions.

[59] That is approximately US$ 97,661,690 using an exchange rate of 2 Reais per Dollar, which is an estimated average of the exchange rate during the period of analysis.

2) Few pre-intervention observations: a strong assumption when using the difference-in-different methodology is that trends between the treatment and the control group were equal in the absence of intervention. Although this assumption cannot be tested, we are more confident that it is true if we look that it is fulfilled in the pre-intervention periods. For the case of Campo Grande we have few periods prior to the intervention. If we had more periods prior to the intervention, we could be more certain that the control group selected is actually a good counterfactual.

3) Few post-intervention observations: having more observations after the interventions were finished would allow us to see if any of the intervention could have had impact over time. In addition we could differentiate between the effect of treatment in anticipation of the works and the effect of treatment with works completed.

4) Information on other interventions in the municipality: So far we only have information about additional infrastructure interventions in Campo Grande from 2008 until 2012. If we could include the information of the beginning and the end of the works, as well as information on works for the subsequent years, we could control for these interventions more precisely in our estimations and exploit further the temporal dimensions of the database.

5) A few observations in the areas of interventions: there is very little real estate activity in some of the areas where the interventions happened, which leaves us with a very small sample size for this intervention and, therefore, estimates may be underpowered.

6) Lack of good counterfactuals. This limitation seems hard to beat. However, if we had more accurate information on which neighborhoods or streets were affected by the intervention, we could distinguish more accurately which properties are affected by the

interventions within neighborhoods, creating a more accurate treatment and control group that would control better for unobservable characteristics.

9. Conclusions

This paper presents the results of the impact assessment of an urban infrastructure intervention in Campo Grande, Brazil. The question I seek to answer is: What is the impact of the urban infrastructure intervention in the quality of life and welfare of the citizens of the municipality?

Using a hedonic price approach, I use property values as a proxy for quality of life. Looking at the overall intervention, I don't detect a significant impact of the urban infrastructure works on the property values. However, when I analyze the interventions of transportation and revitalization separately, the results suggest that transport interventions, which focus on improving mobility routes linking the west with the city center, had a positive and significant effect on the municipality in the short term. This impact is measured as an average 6.7% increase in the price per square meter of real estate in the vicinity of the intervention. The estimation of the impact of the revitalization component in the center of the municipality shows mixed results. On the one hand, using the same criteria of analysis that in the transport component, a negative impact is identified. However, this result could be explained by the difficulty of finding a good counterfactual to the downtown area from the other areas of the city. Once we consider the specific characteristics of the sample and the downtown area where the intervention is performed, the effect in the short term is not significant. However, this may be a lower limit of the true effect of interventions, because the properties located near the intervention could be benefiting from it.

The main limitations of the study are related to lack of information. On the one hand there is no information of a smaller level of assignment, thus we cannot exploit the variability within each neighborhood; also for the case of Campo Grande we have very few observations before the intervention (not allowing us to test the hypothesis behind the differences in differences methodology for a longer period) and after the intervention (making not possible to detect effects in the longer term) which could be biasing our estimations to a lower bound. Other limitations are having few observations at the city center (since few properties were sold), and the lack of a good counterfactual for the city center in Campo Grande. If new information to the study is available, estimates could be made to reflect more accurately the true impact of the program.

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PART III: GENERAL CONCLUSSIONS

GENERAL CONCLUSSIONS

This dissertation use impact evaluation methodologies (randomized controlled trials and quasi-experimental methods) to provide rigorous evidence on the effectiveness of two innovative social programs on promoting social welfare. In the first and second paper it tackles how unemployment can be reduced by studding the impact of employment programs on labor outcomes and, for the first time to the author knowledge, providing causal evidence on the pathways that operate in the program and lead to the observed results . On the third paper it deals with urban development problems, a rising preoccupation given the fast urbanization of developing countries. The results of these studies contribute to generate a body of rigorous evidence on what is effective to foster development.

Youth unemployment is one of the more concerning problems of developing world. Governments around the world struggle to foster employability using active labor programs; however, there is still scarce rigorous evidence about their efficacy.

Chapter 1 brings new evidence on the effectiveness of training programs for unemployed by studding a vocational training program targeted to vulnerable youth in Dominican Republic. Even more, it is the first evaluation to the date that provides causal evidence on the contribution of the technical skills acquired in the training using exogenous variation. It is also one of the very few that looks to the long term outcomes and the only one that does that using survey data.

Results indicate that the program has large gains in employment for women in the short term, however they dissipate in the longer term. For men, the program seems to have induced worse employment conditions long term: male beneficiaries of the program who were employed in the long term had worse labor market conditions than those in the control group (i.e., working informally) and they were less satisfied with their jobs, as reflected by their higher propensity to search while employed. At the same time, however, women participants, while not exhibiting higher employment rates as in the short run, seemed to be more satisfied with their jobs than those in the control group, as manifested by their lower desire to change job. All these effects were similar for the technical and the soft-skills treatment groups showing that technical training has very little value added to a generic soft-skills training and on-the-job training in terms of employment outcomes. Given the high cost of the technical training, this result has relevant implications in terms of cost-effectiveness for policy makers. In the long term, thus, the program seems to have been more successful in raising expectations and basic skills rather than on changing labor market outcomes.

The results allow drawing some conclusions: The impact evaluation of training programs should explicitly attempt to follow beneficiaries over a broader horizon. Our results in the short term coincide with other similar programs evaluated over similar time periods in the region (for instance, employment gains concentrated in earnings and quality of the job). However, most of these employment gains dissipate in the longer run, which contrasts with the available evidence for developed countries, which typically finds positive medium-term impacts of training programs that often appear ineffective in the short term (Card et al., 2010). The evaluation design also allows us to establish that the classroom- based vocational training was not very effective, even when the curricula was discussed and developed jointly with private sector employers. The program seems to have been more successful in raising expectations and basic skills rather than on changing labor market outcomes in the longer run. The positive impact of the program on soft skills indicates that private sector training providers might be more effective in transmitting general cognitive skills and developing non-cognitive abilities than in fostering specific vocational competences. Moreover, the positive employment gains in the short term are compatible with a setting in which the effects on employment are due to the program's implicit labor intermediation (through the internship) rather than on the training component.

Further research could concentrate on the mechanisms through which these programs seem to be more effective for women than for men, and attempt to derive conditions under which male youth could also benefit from training in both their hard and soft skills and their employment outcomes in the longer run. Also it is necessary to bring more evidence of long term results, and to explore further the pathways that operate in the project that lead to the observed results. For instance it would be interesting to isolate the impact of the on-the-job-training from the in-class-training.

Chapter 2 uses a different cohort of individuals of the same training program to open the black box of the program and explore what are the pathways of training programs in the short run. Exploiting the random assignment, the evaluation explores whether training programs affects how individuals search for job and whether they make contacts during the training that may help them to find better jobs. The contributions of the paper to the existent literature on active labor programs and job search is threefold: In the first place, in the theoretical side it provides with a new categorization of job search methods differentiating between the use of professional contacts, non-professional (personal) contacts, and self-directed search methods. To the knowledge of the author this is the first time that a clear distinction between the role of professional and non-professional (personal) is done for job search models. In second place, the empirical part brings new descriptive evidence of vulnerable youth in developing countries. In line with previous empirical evidence, descriptive statistics show that the main search methods used by unemployed are self-directed search methods (75%) followed by the use of non-professional contacts (7%). The main job finding methods are non-professional contacts (74%) and self-directed search methods (22%). Compared to job search rates in more developed contexts this evidence suggests that in socially vulnerable settings individuals tend to use more informal methods to search for jobs. The use of professional contacts only account for 6 % of the search methods and 4% of job finding methods. Finally, taking advantage of the experimental design of an impact evaluation conducted for the training program, the study shows that the program has an impact on job search methods. On the one hand the treatment group is more active searching for jobs, and on the other hand they use more professional contacts and self-directed search methods. Also, the treatment group found their current jobs through professional contacts more than the control group. Subsequently, the paper explores in more detail the networks of the individuals and it shows that the professional network of the treatment group has been more active for the treatment group (more precisely because of the contacts met in former apprenticeships and jobs). This suggests that the program enlarges the number of professional contacts of the individuals through the apprenticeship. It is also explored whether job finding methods could affect labor outcomes, and it is shown that jobs found through professional contacts (and self-directed search) are associated with higher levels of formality compared to the use of personal contacts. Although this relation is not causal, this suggests that contacts made during the program may play an important role in increasing job quality.

The main policy recommendation that arises from these results is that more effort should be made to strengthen the ties with professional networks met during the apprenticeship. Some ways to conduct this could be by extending the duration of the apprenticeship, providing with apprenticeships in more than one firm, or celebrating meetings or events between the alumni and the employers and workers of the firms.

There are several lines in which this study can be extended. On the analytical front, a theoretical model could be developed to explain the results. On the empirical side it would be interesting to conduct a heterogeneity analysis differentiating by gender since labor outcomes may be considerably different for male and females. Additionally, this line of research would also benefit from an experiment that creates exogenous variation to estimate the causal relation of professional contacts on labor outcomes. Finally, it would be interesting to extend the analysis in the long run.

Chapter 3 uses a quasi-experimental methodology to study the effectiveness of a different, but increasingly important, development problem: fast urban development. Emerging economies are experiencing a fast growth of their cities in recent years that is bringing to scene new problems such as urban transportation saturation and abandonment of public spaces.

Using a hedonic price approach and using property values as a proxy for quality of life, the paper estimates the effects of a transport component and a revitalization component on the well-being of the citizens, measured by property values. Looking at the overall intervention, it is not detected a significant impact. However, when the interventions of transportation and revitalization are analyzed separately, the results suggest that transport interventions, which focus on improving mobility routes, had a positive and significant effect on the municipality in the short term. This impact is measured as an average 6.7% increase in the price per square meter of real estate in the vicinity of the intervention. The estimation of the impact of the revitalization component in the center of the municipality shows mixed results. On the one hand, using the same criteria of analysis that in the transport component, a negative impact is identified. However, this result could be explained by the difficulty of finding a good counterfactual to the downtown area from the other areas of the city. Once we consider the specific characteristics of the sample and the downtown area where the intervention is performed, the effect in the short term is not significant. However, this may be a lower limit of the true effect of interventions, because the properties located near the intervention could be benefiting from it. The main limitations of the study are related to lack of information. On the one hand there is no information of a smaller level of assignment, thus we cannot exploit the variability within each neighborhood; also for the case of Campo Grande we have very few observations before the intervention (not allowing us to test the hypothesis behind the differences in differences methodology for a longer period) and after the intervention (making not possible to detect effects in the longer term) which could be biasing our estimations to a lower bound. Other limitations are having few observations at the city center (since few properties were sold), and the lack of a good counterfactual for the city center in Campo Grande. If new information were available, estimates could be made to reflect more accurately the true impact of the program.

SUMMARY:

The thesis presents rigorous evidence of the effectiveness of public policies using experimental and quasi-experimental methodologies. It starts with a comprehensive introduction and a rigorous overview of the methodologies that will be used in the subsequent data analysis.

The first chapter, “Soft Skills and Hard Skills in Youth Training Programs. Long

Term Experimental Evidence from the Dominican Republic” evaluates the impact of a youth employment program on a number of outcomes. The program provides training to young adults at risk of social exclusion on vocational skills and on non-cognitive skills. Remarkably, the methodology used to evaluate the program is a randomized control trial, which provides solid evidence of the causal effect of the program. While previous studies analyzed the impact of related youth programs, no previous study had evaluated the effects 4 years after the program was implemented. This represents an important contribution because the short-term gains of several development programs have been shown not to be sustained in time. This is also what this study finds for labor market outcomes: while the program generates a short-term improvement of employment outcomes for women, this effect dissipates in the long run. However, the program seems to lead to persistent changes on labor market expectations of women: women that attended the training report a more optimistic view of their labor market prospects even 4 years after the program. A second contribution of this chapter is to measure the relative contribution of two different types of training programs: vocational training versus enhancing non-cognitive skills. By randomly assigning participants to two different treatment groups the study finds that the vocational training has little value added in addition to the coaching on non-cognitive skills. This is remarkable because most standard youth employment

programs focus on vocational training. Instead, the results of this study suggest that focusing on enhancing non-cognitive skills would be a more cost-effective training for youth.

The second chapter, “Job Search and Networking in Training Programs. Experimental Evidence from Dominican Republic”, examines how the same youth training program affects the methods of searching for employment. The study finds that the treatment makes individuals more likely to be actively searching for jobs. Furthermore, it is more likely that the search is being done through professional contacts rather than through non-professional contracts. This chapter also provides evidence that the results seem to be driven by the fact that the program increases the size of the professional network of individuals. Furthermore, the study provides evidence that jobs found through the professional network are more likely to be in the formal sector.

This chapter constitutes a relevant contribution to the literature since little research has been done on the evaluation of training programs on the method of search, using randomized control trials.

The third and last chapter, “Impact Evaluation of Urban Infrastructure Interventions.

Evidence from Campo Grande, Brazil”, studies the impact of an infrastructure program in Brazil on property prices. The study implements a differences-indifferences methodology exploiting the variation emerging from the timing of the program and the geographic distance of the property to the particular infrastructure program. The results suggest that infrastructure programs targeted to improve transportation have a positive impact on property prices, while the program to revitalize the city center did not have a positive effect. Data limitations prevent her from studying other potential interesting outcomes.

RESUMEN:

La tesis presenta evidencia rigurosa de la efectividad de las políticas públicas utilizando metodologías experimentales y cuasi-experimentales. La tesis comienza con una introducción completa y una revisión rigurosa de las metodologías que se utilizarán en el análisis posterior de los datos.

El primer capítulo, "Habilidades personales y habilidades técnicas en programas de formación de jóvenes. Evidencia Experimental de Largo Plazo de República Dominicana ", evalúa el impacto de un programa de empleo de los jóvenes en una serie de variables de interés. El programa ofrece capacitación en las habilidades vocacionales y en las habilidades no cognitivas a jóvenes en riesgo de exclusión social. Cabe destacar que la metodología utilizada para evaluar el programa es un ensayo controlado aleatorio, que proporciona evidencia robusta del efecto causal del programa. Mientras que estudios previos analizaron el impacto de los programas para jóvenes relacionados, ningún estudio anterior había evaluado los efectos de 4 años después de la implementación del programa. Esto representa una contribución importante debido a que las ganancias a corto plazo de varios programas de desarrollo han demostrado no ser sostenida en el tiempo. Esto es también lo que este estudio encuentra para los resultados del mercado de trabajo: mientras que el programa genera una mejora a corto plazo de los resultados de empleo para las mujeres, este efecto se disipa en el largo plazo. Sin embargo, el programa parece conducir a cambios persistentes en las expectativas del mercado de trabajo de las mujeres: las mujeres que asistieron al entrenamiento de informar una visión más optimista de las perspectivas del mercado de trabajo hasta 4 años después del programa. Una segunda contribución de este capítulo es medir la contribución relativa de dos tipos diferentes de programas de formación: formación profesional técnica frente a la mejora de las habilidades no cognitivas (básicas). Mediante la

asignación aleatoria a los participantes a dos grupos diferentes de tratamiento, el estudio encuentra que la formación profesional tiene poco valor añadido sobre la de habilidades no cognitivas. Esto es notable porque la mayoría de los programas de empleo juvenil estándar se centran en la formación profesional técnica. En cambio, los resultados de este estudio sugieren que se centra en la mejora de las habilidades no cognitivas (básicas) sería una formación más rentable desde el punto de vista programático.

El segundo capítulo, "Búsqueda de empleo y contactos en los programas de capacitación. Evidencia experimental de República Dominicana ", examina cómo el mismo programa de entrenamiento juvenil afecta a los métodos de búsqueda de empleo. El estudio revela que el tratamiento hace que los individuos sean más propensos a buscar activamente empleo. Además, es más probable que la búsqueda se realice a través de contactos profesionales en lugar de a través de contratos personales. Este capítulo también proporciona evidencia de que los resultados parecen ser impulsado por el hecho de que el programa aumenta el tamaño de la red profesional de los jóvenes. Además, el estudio proporciona evidencia de que los trabajos que se encuentran a través de la red de contactos profesionales tienen más probabilidades de estar en el sector formal. Este capítulo constituye una contribución relevante a la literatura ya que la investigación sobre la evaluación de los programas de formación en el método de búsqueda mediante ensayos controlados aleatorios es escasa.

El tercer y último capítulo, "Evaluación de impacto de las intervenciones de infraestructura urbana. Evidencia de Campo Grande, Brasil ", estudia el impacto de un programa de infraestructura en Brasil en el bienestar de los ciudadanos medido mediante el precio de los inmuebles. El estudio implementa una metodología diferencias en diferencias para explotar la variación temporal del programa y la distancia geográfica de los inmuebles a las intervenciones de infraestructura. Los resultados sugieren que los programas de infraestructura orientados a mejorar el transporte tienen un impacto positivo en los precios de los inmuebles, mientras que el programa para revitalizar el centro de la ciudad no muestra un efecto positivo. Limitaciones de los datos le impiden estudiar otros potenciales resultados interesantes.