



IDB WORKING PAPER SERIES No. IDB-WP-271

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Risks and Labor Market Policies

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December 2011

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Inter-American Development Bank

2011

Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library

Hernani-Limarino, Werner L.

Unemployment in Bolivia : risks and labor market policies / Werner L. Hernani-Limarino, María
Villegas, Ernesto Yáñez.

p. cm. (IDB working paper series ; 271)

Includes bibliographical references.

1. Labor policy—Bolivia. 2. Labor market—Bolivia. 3. Unemployment—Bolivia. I. Villegas, María. II.
Yáñez, Ernesto. III. Inter-American Development Bank. Research Dept. IV. Title. V. Series.

<http://www.iadb.org>

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Abstract¹

This paper attempts to evaluate the effectiveness of Bolivia's labor market institutions, particularly the Plan Nacional de Empleo de Emergencia (PLANE). It is found that unemployment as conventionally defined may not be the most important problem in Bolivia's labor market, as the non-salaried market is always an alternative. While unemployment durations and unemployment scarring consequences are relatively low, labor market regulations and labor market programs do not help to increase the size of the formal market, apparently as a result of Bolivia's rigid labor markets and labor policies based mainly on temporary employment programs. Such programs, however, may have helped to smooth consumption. Given the country's high level of informality, protection policies are second best to active policies specifically designed to increase the productivity/employability of vulnerable populations.

JEL Classification: J64, J08, J21

Keywords: Bolivia, Unemployment, Labor Policies, Impact Evaluation.

¹This document was prepared for the Latin-American Research Network project **Protecting Workers Against Unemployment in Latin America and the Caribbean** sponsored by the Inter-American Development Bank. All the views expressed here are those of the authors and do not necessarily reflect those of the IADB or the institutions to which they are affiliated. Hernani-Limarino and Villegas are affiliated to *Fundacion ARU*. Yañez is affiliated to the Central Bank of Bolivia and to *Fundacion ARU*. We thank Robert Lalonde, Jeffrey Wooldridge, Carmen Pages, Consuelo Ricart, Veronica Alaimo, Jacqueline Mazza and the participants of the IADB Discussion Seminar in Washington, D.C.; and of the Employment Workshop of the Central Bank of Bolivia in La Paz for their useful comments and suggestions. We are also grateful to Paul Villaruel for superb research assistance. The usual disclaimer applies. Additional comments are welcome at whl@aru.org.bo.

1 Introduction

Unemployment is one of the most important concerns for societies in general and policymakers in particular. In fact, recent data from *Latinobarómetro* reveal that two out of three people identify unemployment as *the* most important problem in Bolivia. This paper attempts to evaluate whether Bolivia's labor market institutions and past and current programs were/are good alternatives to the observed extent, nature and consequences of the country's unemployment problem.

The document is organized as follows. Section 2 presents a diagnostic of the extent, nature and consequences of unemployment. Section 3 presents a review of labor market regulation and programs. Section 4 evaluates the impact on consumption smoothing and employability of Bolivia's most comprehensive public temporary employment scheme, the *Plan Nacional de Empleo de Emergencia* (PLANE). Section 5 concludes. Appendix A describes the set of databases used in the unemployment diagnostic and impact evaluation. Appendix B presents the methods to estimate unemployment scarring effects on both subsequent employment and labor earnings. Appendix C describes the methods used for our impact evaluation.

2 Unemployment in Bolivia: Extent, Nature and Consequences

What are the extent, nature, and consequences of unemployment in Bolivia? This section attempts to answer this question in order to understand not only the magnitude of the unemployment problem but also which type of labor market policies may work better to improve the performance of the labor market. First, we analyze Bolivia's labor market structure. Next, we analyze the extent composition and duration of unemployment. Third, we present a dynamic analysis of worker entry and exit flows into and out of unemployment. Finally, we present an analysis of unemployment scarring effects on subsequent employment and labor wages.

2.1 Labor Market Structure

Unemployment is, by definition, the lack of employment. Therefore, a first step toward understanding the unemployment nature is to understand the employment structure, i.e., the process that allocates different people to different types of work.

2.1.1 Salaried vs. Non-Salaried Jobs

First, it is important to distinguish between jobs generated by the *labor market* and *non-market jobs*. In a modern economy, employers perform tasks for employees in return for money, employees exchanges the products of employees' labor with consumers, and employees and consumers do not exchange directly with one another. The "labor market" refers to the first set of exchanges; the "product market" refers to the second set of exchanges. Therefore, in an strict sense, the labor market refers only to *salaried workers*, i.e., those who have a relationship of subordination to an employer/firm and are paid a wage. The labor market does not include *non-salaried workers* who

Table 1. Distinction between Formal and Informal Employment

		(1) Aplicable Salaried	(2) Not aplicable Non-salaried
(A)	Comply	Formal	Informal
(B)	Do not comply	Informal	Informal

Source: Authors' compilation.

exchange their labor directly with the consumer (i.e., self-employed), or consume what they produce (i.e., subsistence farmers), or have non-subordinated relationships with firms.² It is crucial to notice that, in Bolivia, as in other developing countries, most jobs are not generated by the labor market.

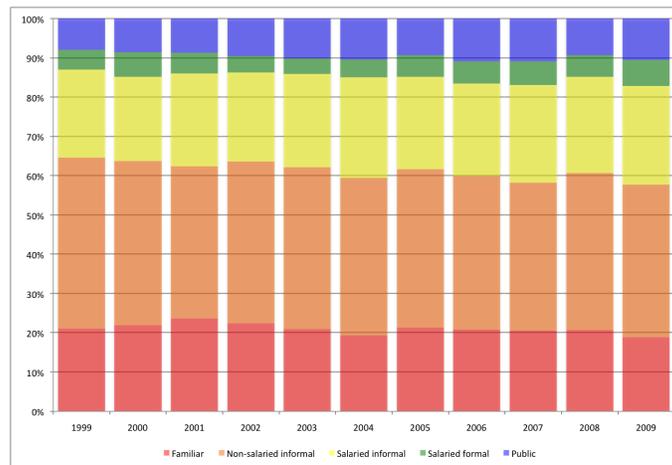
2.2 *Formal vs. Informal Jobs*

Second, it is important to distinguish between *formal* and *informal* jobs. Unfortunately, informality means different things to different people. As stated by Kanbur (2009), “informality is a term that has the dubious distinction of combining maximum policy importance and political salience **with** minimal conceptual clarity and coherence in the analytical literature.” We follow Levy (2009) and define formality with respect to “a” regulation, where the relevant regulation is **coverage of social security**. As illustrated by Table 1, the distinction between formal and informal jobs result from the intersection of the range of application of “a” regulation, coverage of social security, and its enforcement. Columns identify those workers for whom the regulation is applicable, i.e., *salaried workers*; and those for whom the regulation is not applicable, i.e., *non-salaried workers*. Rows identify those who comply and those who does not comply with the regulation. On the one hand, salaried workers enrolled by firms in social security are considered **formal**, and those not enrolled by firms in social security are considered **informal**. On the other hand, both non-salaried workers independent on whether they adjust to the regulation are considered informal, since the regulation is not applicable to their case.

Notice that, under this definition, informality is not equated with **non-salaried work** (since there may be informal workers who are salaried), **the size of firms** (since some micro and small

²Non-subordinated relationships may include contracts to elicit effort or share risk, with commissions, profit-sharing or other pay structures.

Figure 1. Bolivia - Evolution of Employment Structure



Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

firms may hire salaried workers and enroll them in social security), **illegality** (since not all informal workers are salaried), or **low earnings** (since there may be informal workers with high earnings, and formal workers with low earnings).

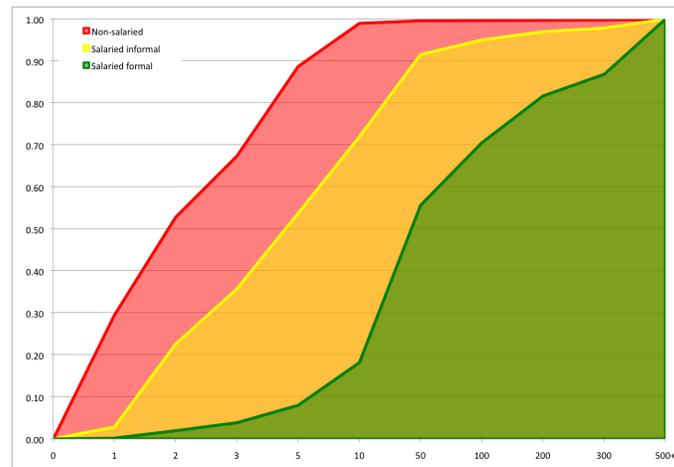
2.2.1 Public vs. Private Jobs

Finally, it is also useful to distinguish between salaried workers in the *public sector*, where the employer is the government (central, departamental or municipal) and those in the *private sector*, where the employer is a private firm. Notice that this distinction may be important because since 2006 Bolivia has been undertaking a series of nationalizations of private firms.

2.2.2 Bolivia's Labor Market Structure

In Bolivia, most jobs are generated by the informal sector, mainly in non-salaried activities. Figure 1 presents the average percentage of total employment accounted for by the non-salaried informal sector (ni), the salaried informal sector (si), the salaried formal sector (sf) and the public sector (p) for the 1999-2009 period. At the national level, 87 percent of jobs were generated by the informal sector, 67 percent by the non-salaried informal sector and 21 percent by the salaried informal sector. The salaried formal sector accounted for only 5 percent of total jobs and the public sector for 8 percent of total jobs. By area of residence, we observe that in urban areas the informal sector accounted for 80 percent of total jobs (49 percent as non-salaried and 31 percent as salaried workers), the formal salaried sector for 8 percent and the public sector for the remaining 12 percent, while in rural areas the informal sector accounted for 96 percent of total jobs (88 percent as non-salaried and 8 percent as salaried workers) and the public sector for the remaining 4 percent. There is no formal sector in rural areas. Table 2 on page 6 presents the evolution of the percentage of

Figure 2. Bolivia - Cumulative Distribution function of size of firm or "business"



Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

total employment accounted for by each sector. Notice that there no significant changes in the employment structure.³

As pointed out before, informality is not related to **the size of firms**. In Bolivia, at least some micro and small firms hire salaried workers and enroll them in social security. Figure 2 presents the average percentage of total employment accounted by each sector and size of firm or family "business." Informality is also not equivalent to **low earnings**. There are many informal workers with high labor earnings as well as formal workers with low labor earnings. Figures 3, 4, 5 and 6 (beginning on page 7) present the kernel estimates of the probability density and cumulative distribution functions of the log of monthly earnings at 2009 prices.⁴

2.3 Extent, Composition and Duration of Unemployment

In an economy with a segmented employment structure such as Bolivia a person is considered unemployed if he lacks a job and is actively seeking one, **whether as a salaried or a non-salaried worker**. This notion is clear in survey questions used to define the unemployed in Bolivia. Both LSMS and employment surveys consider a person *unemployed* if he did not work for at least one hour the week before the interview date *and* he has been actively looking for a salaried work or has been making arrangements to establish any type of "business" during the four weeks before

³Notice that in 2003 and 2004 there is a significant decline in non-salaried informal work relative to other years. This change may be explained by the fact that the 2003-2004 survey was a continuous survey. Seasonality in agricultural activities, which represent the majority of non-salaried informal work, may explain the observed changes.

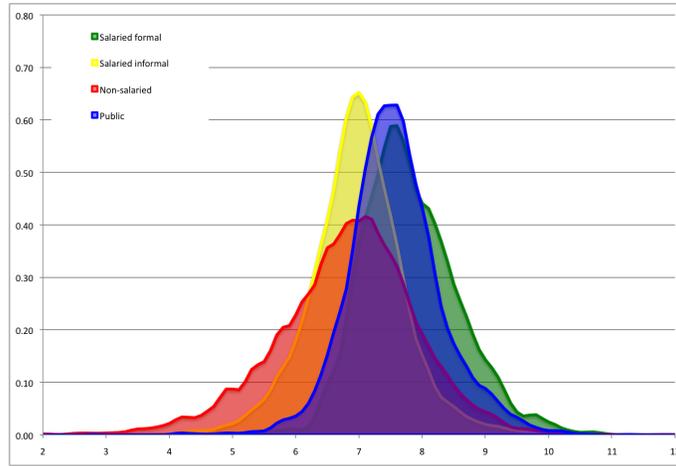
⁴For salaried workers monthly earnings include in-kind earnings and social benefits, while for non-salaried workers monthly earnings include net income once costs of production are accounted for.

Table 2. Employment Structure

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	Total
Bolivia												
Familiar Worker	22.13	21.87	24.41	22.92	22.06	20.33	22.26	21.48	21.42	21.91	19.71	21.79
Non-salaried informal	43.63	43.95	39.15	41.69	40.92	39.87	40.28	39.75	37.8	39.46	38.53	40.28
Salaried informal	21.88	20.39	23.08	22.19	23.31	25.27	23.15	22.94	24.42	24.15	24.92	23.37
Salaried formal	4.8	5.81	4.99	4	3.88	4.36	5.29	5.41	5.77	5.39	6.54	5.17
Public	7.56	7.98	8.37	9.2	9.82	10.17	9.02	10.42	10.59	9.08	10.29	9.4
Urbano												
Familiar Worker	7.5	6.56	11.04	8.83	11.04	8.96	8.98	8.3	6.83	9.38	7.89	8.65
Non-salaried informal	42.06	42.59	36.64	40.64	38.39	38.43	39.49	37.9	36.35	38.23	35.98	38.57
Salaried informal	31.58	30.23	32.18	31.45	30.71	33.18	31.74	32.23	33.31	33.03	33.61	32.24
Salaried formal	7.99	9.48	8.18	6.45	6.44	6.84	8.43	8.27	9.03	7.89	9.61	8.11
Public	10.88	11.14	11.96	12.62	13.42	12.6	11.36	13.29	14.48	11.47	12.9	12.44
Rural												
Familiar Worker	42.25	43.67	43.91	44.02	38.35	38.88	44.35	42.83	44.63	42.71	40.99	42.42
Non-salaried informal	45.79	45.88	42.82	43.25	44.67	42.21	41.6	42.74	40.12	41.5	43.13	42.97
Salaried informal	8.56	6.37	9.8	8.32	12.4	12.38	8.86	7.88	10.27	9.41	9.27	9.44
Salaried formal	0.4	0.58	0.33	0.34	0.1	0.32	0.06	0.78	0.58	1.25	1.02	0.55
Public	3	3.49	3.13	4.07	4.49	6.2	5.13	5.77	4.4	5.13	5.59	4.63

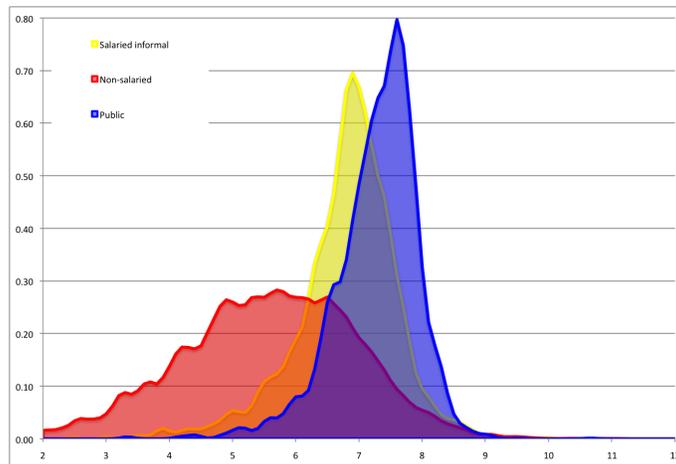
Source: Author's calculation using Fundación ARU Harmonized Household Surveys.

Figure 3. Urban - Monthly labor earnings by sectors of activity



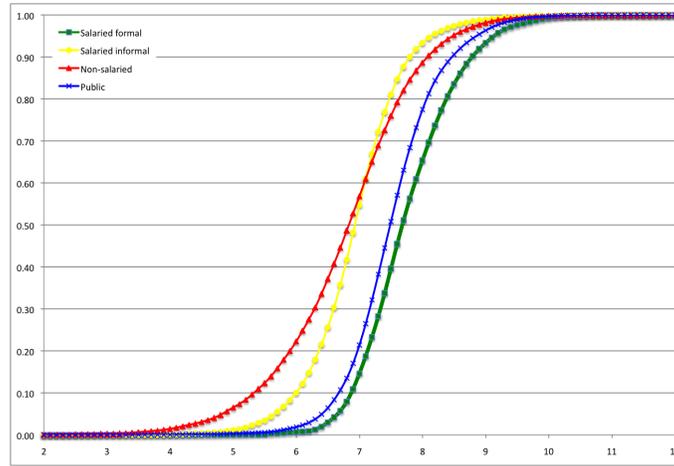
Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

Figure 4. Rural - Monthly labor earnings by sectors of activity



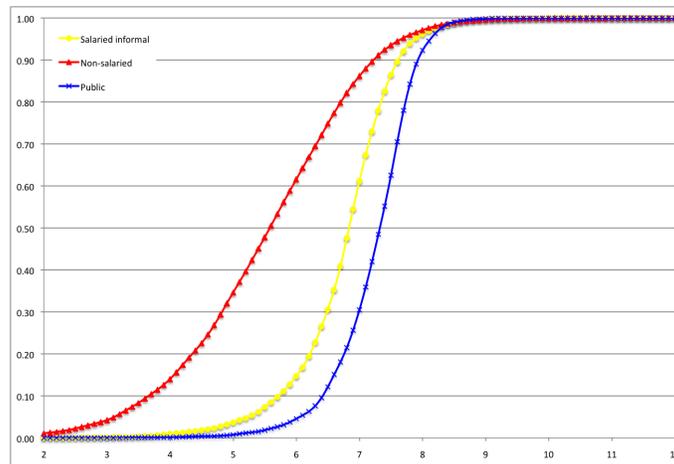
Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

Figure 5. Urban - Monthly labor earnings by sectors of activity



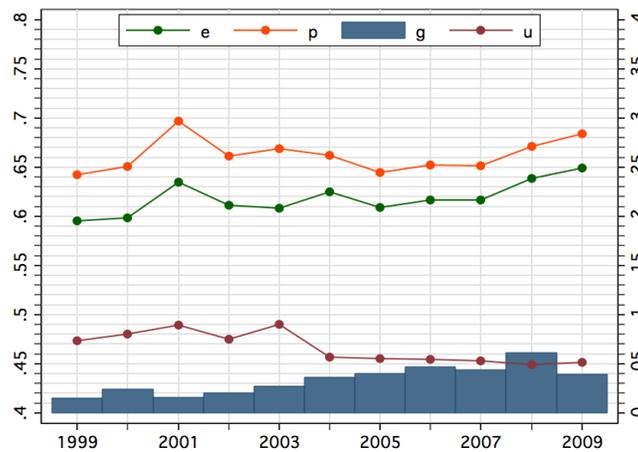
Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

Figure 6. Rural - Monthly labor earnings by sectors of activity



Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

Figure 7. Urban Unemployment and Participation



Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

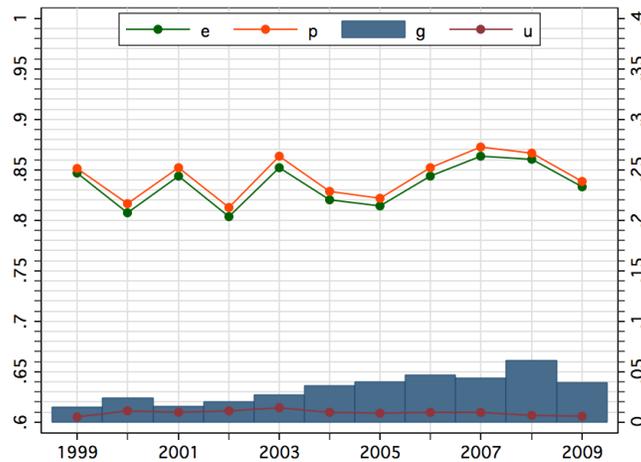
the interview date.⁵ It is important to note that the definition of unemployment includes those who did not work and did not search for work because they already gotten a job or are waiting for a decision on a possible job.

Since anyone who have worked in any type of productive activity at least one hour a month in or out the labor market is considered employed, it is possible that unemployment numbers do not reflect the total number of people who usually lack jobs. *Underemployment*, defined as employed people who not only want and are able to work more hours but also work less than full time (where full time is defined as an schedule of 36 hours per week or more) might be a better measurement of the lack of absorption of the labor force. The *discouragement rate* (the proportion of non-participants who did not search for work because they believe they will not find work, they became tired of looking for work or they are waiting for a period of more economic activity to search for work) might also capture absorption problems of the labor force.

Figures 7 and 8 present the evolution of growth rates, unemployment, employment and participation rates for urban and rural areas, respectively. Regarding unemployment rates, it is important to make a few remarks. First, there is no unemployment problem in rural areas; unemployment rates are usually below one percent. Second, fluctuations in urban unemployment are countercyclical, going up in times of slowdowns and recession (1999-2003), and going down in

⁵It is important to note two issues regarding this definition. First, people who worked at least one hour in agricultural or livestock activities, helping the household “business,” selling in the street, preparing meals for sale, or doing the laundry for a third party are considered employed. Second, people who had a job but did not work due to vacation, sickness, lack of inputs or clients, strikes or labor conflicts, bad weather, or personal problems are also considered employed.

Figure 8. Rural Unemployment and Participation

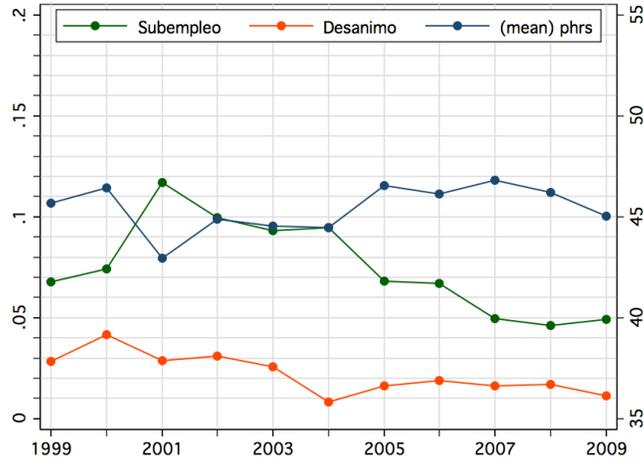


Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

times of economic recovery and growth (2004-2009). Third, notice that the two peaks in urban unemployment rates, in 2001 and 2003, coincide with the years of the lowest growth rate and the year with the greatest social unrest. Fourth, notice that the year with the lowest growth rate shows not only higher urban unemployment rates but also higher urban employment rates (the ratio of employed workers to total population); this fact indicates that unemployment rates increased mainly of an increase in participation rates. Finally, it is important to note that urban unemployment rates in Bolivia are relatively low, at least when compared to unemployment rates in other countries in the region.

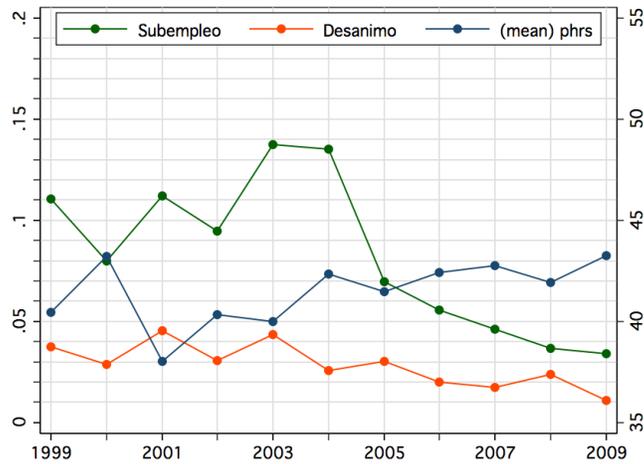
Figures 9 and 10 on the following page present the evolution of average hours worked, underemployment and discouragement rates for urban and rural areas, respectively. Again, underemployment and discouragement rates are countercyclical, with rates going up in times of slowdowns and depression and going down in times of economic recovery and growth. On the one hand, urban underemployment have peaked at 13 percent in 2001 and declined since then to levels around 5 percent of the employed population, while rural underemployment fluctuated around 10 percent between 1999 and 2002, increased above 13 percent in 2003 and 2004 and declined since then to levels below 5 percent of the employed population. On the other hand, urban (rural) discouragement has fluctuated above 3 percent in times of slowdowns and declined below that level in times of economic recovery and growth.

Figure 9. Urban Underemployment and Discouragement



Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

Figure 10. Rural Underemployment and Discouragement



Source: Author's calculation using Fundacion ARU Harmonized Households Surveys.

2.4 *Unemployment Scarring*

Unemployment may have not only short-term consequences (people without employment receive no labor earnings), but also long-term scars through future incidence of unemployment and lower labor earnings.

2.4.1 *On Subsequent Unemployment*

A key question in the study of individuals' transitions between employment and unemployment is whether high persistence in employment and in unemployment states are due to *heterogeneity* or *true state dependence*. In the case of heterogeneity in the productive characteristics of employed and unemployed individuals (e.g., in their levels of human capital), it is clear that high persistence will be observed. However, there are other economic factors that may also make the transition between states difficult or costly. For instance, employers might have high costs of firing workers because of firm-specific human capital or severance payments; likewise, the unemployed might have high costs of searching for a job because of problems of asymmetric information, stigma of unemployed workers, etc. In such cases of *true state dependence* we will also observe high persistence in employment and unemployment states. Since these two explanations have very different economic and policy implications, we attempt to identify their relative contribution to persistence using the Chamberlain (1985) and Honore-Kyriazidou (2000) approaches. A detailed description of their approaches is presented in Appendix C.1.

2.4.2 *On Subsequent Labor Earnings*

Different economic theories predict different effects of unemployment on subsequent wages and earnings. On the one hand, the theory of human capital predicts that unemployment spells negatively affect not only current earnings but also future earnings. Unemployment prevents accumulation of firm-specific (non-transferable) human capital through on-the-job training and experience and may result in deterioration of general skills. Therefore, unemployment spells causes future wages and earnings in employment to be lower than if unemployment had not occurred. The signaling theory also predicts a negative effect of unemployment spells on future earnings. As productivity is unobserved by the firms, a lower wage may be offered if employers take past history of unemployment as signal of low productivity; however, the initial wage penalty may be eroded over time if the worker proves to be of higher productivity than the employer initially infers from unemployment history. On the other hand, the job matching theory predicts both higher and lower subsequent wages and earnings. If a job is terminated by either the worker or the firm because it was a poor match, then higher earnings may result if unemployment spells allows the sorting of workers into better-matched jobs. But if a good match is terminated, then lower subsequent wages and earnings are possible. We follow Jacobson, LaLonde, and Sullivan (1993) and attempt to es-

estimate the effects of an employment interruption on wages and, in case they exist, whether these effects are temporary. The econometric methods used are described in Appendix C.2.

3 Labor Market Regulation and Policies

This section reviews Bolivia's labor market regulation and past and present labor market policies and speculates on how they affect the size and performance of the labor market. First, we describe the most important aspects of labor market regulation, i.e., hiring rules, working conditions (including mandated benefits) and dismissal rules (including job security provisions). Next, we describe the most important labor market policies that have been implemented during the last decade. The analysis of Bolivia's most comprehensive employment program, the *Plan Nacional de Empleo de Emergencia* (PLANE), is left for the next section.

3.1 Labor Market Regulation

Bolivia, like most Latin American countries, includes in its civil law system a General Labor Code (GLC) that regulates permissible types and durations of labor contracts and the conditions for their termination. Although there have been some changes in important aspects, most of the regulations of firm-employee arrangements are based on Law No. 224 approved in December 1942.

3.1.1 Hiring Rules

According to the GLC, hiring a worker has to be established by a written or verbal employment contract. Contracts may be full-time indefinite employment, part-time indefinite employment, or fixed-term temporary employment. In cases of ambiguity, the GLC mandates the assumption of a full-time indefinite employment offer. Employment contracts can be signed by men and women 21 years old or older. Men and women between 18 and 21 years old can also sign contracts unless their parents or legal guardians are opposed. Boys and girls between 14 and 17 years old can only sign contracts with the written authorization of their parents or legal guardians. Children younger than 14 years old are strictly prohibited to work (GLC, Title II, Chapter I, Art. 8.). The GLC recognizes four **types of employment contracts** (GLC, Title II, Chapter II-IV):

1. *Individual employment agreements*, those that determine obligations and working conditions between a firm and an individual (or legal) person. Individual contracts must be explicit about the nature of the services agreed (duration, task, etc.), the working place, the amounts, forms and periods of payment, and the duration of the contract, among other issues.
2. *Collective employment agreements*, those that determine obligations and working conditions between a firm and affiliates of a labor union or group of labor unions (federation or confederation). Collective agreements have to be written, registered in the Ministry of Labor and signed by legally recognized labor unions (GLL, Title II, Chapter I, Art. 5; Chapter 2, Art. 23-29).

3. *Apprenticeship contracts*, those under which an individual works under the responsibility of the employer with the main objective of learning. It is important to note that apprenticeships should not interfere with school attendance. In case of termination by the firm, wages must be paid for time already worked.
4. *Engagement contracts*, those by which an individual is recruited to work by a third party, not by the employee. By Presidential Decree 288 of April 1945 only governmental employment services are allowed to sign contracts of this type.

3.1.2 Working Conditions

The GLC also regulates four important aspects of working conditions: i) working hours, ii) mandated benefits, iii) minimum wages and iv) the right to organize and join a labor union.

Working Hours. Workers can work up to 8 hours per day (7 in case of night-time schedules) and up to 48 hours per week.⁶ Two days of rest per week are mandatory. Exceptions to the general regulations are youth work, work in health services/air navigation and work in beauty shops. The maximum number of work hours for youth (those between 14 and 18 years old) is restricted to a daytime schedule. Work in health services and air navigation can be extended to 12 hours, and employees of beauty shops can work during the weekends and on holidays (GLL, Title III, Chapter III, Art. 46-49).

Mandated Benefits. All workers are entitled to receive health and professional risk insurance from their employers. The mandatory health premium and professional risk premium per worker are equivalent to 10 percent and 1.71 percent, respectively, of the monthly wage. Additionally, employers need to contribute 2 percent and 3 percent of the worker's monthly wage to the Dwelling Fund and the Solidarity Pension Fund, respectively.

Minimum wage. No worker can earn a monthly wage below the minimum wage in a full-time job. Minimum wage is determined by the government each year, usually based on the IPC⁷ inflation rate. Table 3 on the next page presents a summary of the minimum wage regulations. Figures 11 and 12 on page 16 present the evolution of the minimum wage in nominal and real terms, respectively.

Unionization. The right to organize and join a labor union is fully recognized by the GLC. Labor unions can be organized at different levels including firm, sector, industry and professional levels. A labor union needs at least 20 members and, in the case of firm and professional unions, participation by at least 50 percent of all workers must be established. Workers in the public sector are not allowed to be organized in a labor union. Workers elected as representatives for a labor

⁶Night-time schedules comprise work carried out between 8:00 p.m and 6:00 a.m.

⁷Indice de Precios al Consumidor.

Table 3. Bolivia: Minimum Wage (1991-2010)

Year	Supreme Decree/Law/ Ministry Resolution	Date	Minimum Wage (Current Bs.)
1991	D.S. 22739	01-marzo-1991	120
1992	D.S. 23093	03-marzo-1992	135
1993	D.S. 23410	16-febrero-1993	160
1994	D.S. 23791	30-mayo-1995	190
1995	D.S. 24067	10-julio-1995	205
1996	D.S. 24280	20-abril-1996	223
1997	D.S. 24468	14-enero-1997	240
1998	Ley 1286 (PGN), DS 25051	23-mayo-1998	300
1999	RM145/99, DS 25318	18-marzo-1999	330
2000	D.S. 25679	25-febrero-2000	355
2001	Ley 2158 PGN, DS 26047	12-enero-2001	400
2002	D.S. 26547	14-marzo-2002	430
2003	D.S. 27049	26-mayo-2003	440
2004	D.S. 27049	26-mayo-2003	440
2005	D.S. 27049	26-mayo-2003	440
2006	D.S. 28700	01-mayo-2006	500
2007	D.S. 29116	01-mayo-2007	525
2008	D.S. 29473	05-marzo-2008	577,5
2009	D.S. 0016	19-febrero-2009	647
2010	D.S. 497	01-mayo-2010	679,5

Source: Author's elaboration.

Figure 11. Evolution of minimum wage in nominal terms

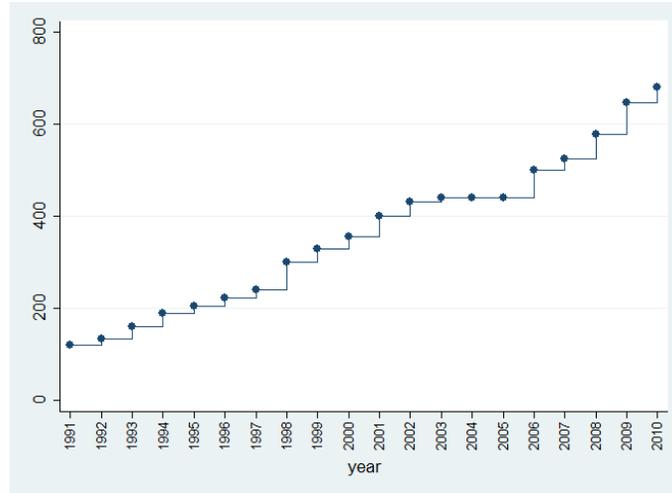
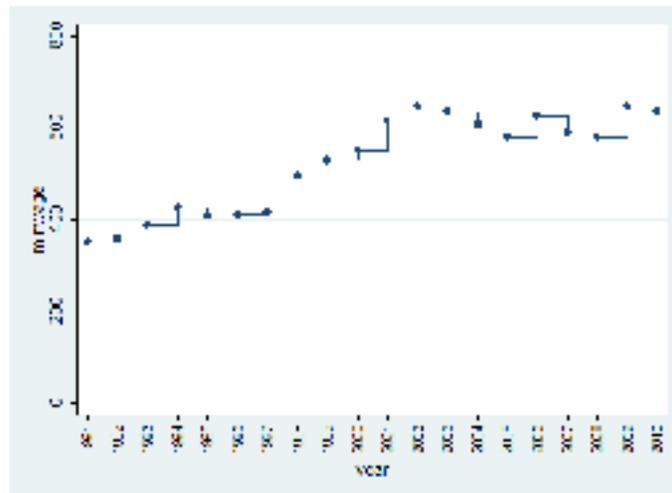


Figure 12. Evolution of minimum wage in real terms



Source: Author's elaboration.

union cannot be dismissed or transferred from one job to another without their consent or a proper trial.

3.1.3 Dismissal Rules

The GLC mandates firms to incur three types of costs in the event of termination of a contract: i) advance notification, ii) foregone wages during any trial in which the worker contest dismissal, and iii) severance payments. Notice that there are no limitations for **collective dismissal**.

Advanced Notification. The GLC mandates firms to provide workers an advance notice of their dismissal. The advance notification period varies from seven days after continuous work of at least one month, 15 days after continuous work of at least six months, and one month after continuous time of at least one year for blue-collar workers (“*obreros*”); and three months for white collar workers (“*empleados*”) (GLC, Title II, Chapter I, Art. 12). Notice that advance notification should be considered as a dismissal cost since productivity may decline substantially after notice. Furthermore, the GLC allows firms to choose between providing advance notice or paying a compensation equivalent to the wage corresponding to that period.

Appeal mechanism. Whenever the worker believes his dismissal is “unfair” he is entitled to begin a trial where the firm must prove any of the causes for fair dismissal.⁸ If fair dismissal is not proven, the firm is not only obligated to pay all foregone wages during the trial period but also obligated to either pay seniority premiums or reinstate the worker to his job, as the worker chooses (Presidential Decree 28699. May 1st 2006, Chapter II, Art. 10). Legal services for labor disputes are freely provided by the regional offices of the Ministry of Labor.

Severance Payment. All workers with at least 90 days of continuous work are entitled to a severance payment equivalent to one monthly wage per year worked unless they resign or were fired with fair cause. Severance payments must be paid within 15 days; otherwise, the amount has to be updated to current value using the percentual variation of the *Unidad de Fomento a la Vivienda* (UFV) as a discount rate. Non-compliance would imply fines equivalent to 30 percent of the total amount to be paid to the benefit of the worker (Presidential Decree No. 28699 May 1st 2006, Chapter III, Art. 2-4,9).⁹ Workers who resigned or were fired with fair cause receive a seniority premium equivalent to one monthly wage per year worked that is cumulative after five years of continuous work.

⁸Causes for fair dismissal are regulated by the GLC, Title II, Chapter I, Art. 9-16. They include: i) intentional damage to firm’s machines, products or merchandise; ii) disclosure of firm’s secrets; iii) non-compliance with industrial hygiene and safety rules; iv) non-compliance with firm’s internal rules; v) abuse of trust or theft; vi) immoral conduct; and vii) continuous absence.

⁹It should be noted that before Presidential Decree No.28699 severance payments were mandatory only after five years of continuous work.

3.2 Labor Market Programs

Here, we briefly review the objectives, design and implementation of past and current labor market programs. Whenever possible we analyze their coverage by size, type of beneficiary, and amount and nature of benefits. One of the most important labor policies, the *Plan Nacional de Empleos de Emergencia* (PLANE), is analyzed extensively in the next section.

3.2.1 PROPAIS

The *Programa Contra la Pobreza y Apoyo a la Inversión Social* (PROPAIS) was a temporary employment program created **“to contribute to the creation of employment opportunities, the reduction of social tension and the economic improvement of the most vulnerable sectors of the population through the intensive use of community labor for the construction of local infrastructure.”** Phase one of the program began in 2004 as part of the Social Protection Network and had an overall investment of US\$ 29 million. Phase two of the program began in 2005 with an overall investment of US\$ 21 million. The initiative targeted municipalities with at least 70 percent of the population characterized as poor according to the Unsatisfied Basic Needs Index, i.e., 228 municipalities out of 314. Eligible projects had to: i) be public goods such as irrigation systems, school infrastructure, roads and streets repairs, among others;¹⁰ ii) cost no more than US\$ 20,000; and iii) capable of being executing in 90 days or fewer. Projects were identified, proposed and executed by local organizations such as neighborhood organizations, territorial organizations, school committees and labor unions. As in the case of PLANE and EDIMO, PROPAIS was administered by the Bolivian Unique Directory of Funds (DUF).

Coverage and reciprocity PROPAIS generated 20,266 employment opportunities (83.5 percent for men and 16.5 percent for women). Notice that, since participants were allowed to participate in the program more than once after a waiting period, the number of employment opportunities is not equal to the number of participants. Unfortunately, there are no available records regarding the number of participants and their characteristics (including whether they had participated in the program more than once). Table 4 on the next page presents the number of employment opportunities created by the program by sex and region from August 2007 to December 2008.

3.2.2 EDIMO

The *Programa de Empleo Digno Intensivo en Mano de Obra* (EDIMO) was an initiative aimed at **providing temporary employment opportunities in municipalities affected by or vulnerable to natural disasters.** The program was implemented in two phases during the 2007-2008 period with an overall investment of US\$ 1 million. Like PLANE and PROPAIS, EDIMO was part

¹⁰According Monje (2007), PROPAIS projects were mainly concentrated in the construction and repair of sports facilities and road improvement.

Table 4. PROPAIS: Employment opportunities created

	Directly created employment-opportunities	Women	%	Men	%
Cochabamba	2434	245	10.1	2189	89.9
Beni	2813	117	4.2	2696	95.8
Oruro	404	91	22.5	313	77.5
Chuquisaca	4567	1107	24.2	3460	75.8
Santa Cruz	3965	375	9.5	3590	90.5
La Paz	2551	696	27.3	1855	72.7
Tarija	276	26	9.4	250	90.6
Pando	289	92	31.8	197	68.2
Potosi	2967	591	19.9	2376	80.1
TOTALES	20266	3340	16.5	16926	83.5

Source: SIREPROS

of the Social Protection Network administrated by the DUF. Again, infrastructure projects were identified, proposed and executed by local organizations through municipal governments.

Coverage and reciprocity EDIMO generated 8,399 employment opportunities in 51 municipalities where the program was implemented in 2007. Unfortunately, there is no information available for 2008. Table 5 on the following page presents the number of employment opportunities generated by EDIMO in 2007 disaggregated by area.

3.2.3 Tarija's PLANE

Tarija's *Plan Nacional de Empleo de Emergencia* (PLANE) was a regional initiative for the creation of short-term employment opportunities. The program was implemented from 2006 to 2010 by the Regional Government of Tarija, under the administration of the UNDP country office, with an overall investment of more than US\$ 19 million. At first the program followed the design of PLANE and PROPAIS, providing temporary employment for the development of local infrastructure in both urban and rural areas of Tarija. Beginning in 2007 Tarija's PLANE incorporated the provision of temporary employment opportunities, not only training activities but also long-term employment opportunities at local firms participating in the program.

Coverage and reciprocity According to Machicado (2008), Tarija's PLANE generated a total of 72,963 employment opportunities, trained 5,095 workers and placed 313 workers. Table 6 on page 21 presents a summary of the number of infrastructure projects, employment opportunities, trained workers and placed workers during the three phases of the program. It is important to note that wages were set slightly above the minimum wage and differed between urban and rural areas.

Table 5. EDIMO: Employment opportunities created

	Number of Municipalities	Employment Opportunities Created
Beni	8	1450
Potosi	8	1060
Santa Cruz	6	700
Tarija	6	865
Cochabamba	11	1654
Chuquisaca	4	1150
La Paz	7	1220
Pando	1	300
TOTAL	51	8399

3.2.4 MPED

Mi Primer Empleo Digno (MPED) is a job training program aimed at increase the employability of youth in the labor market through training and practical internship programs. The program promotes labor market insertion for economically disadvantaged youth who have left the conventional education system and either do not work or work at low-quality jobs. The program was launched in January 2009 in the cities of La Paz, El Alto, Cochabamba, Santa Cruz and Montero, and in September of 2009 it was expanded to the cities of Oruro, Potosi, Sucre, Tarija and Cobija.

MPED provide trainees with two types of training. The first is classroom training, which is provided by competitively selected and pre-qualified private and public training centers in specific areas for a period of three months. During this period the trainee receives a stipend of Bs. 10 per day of training to cover basic lunch and transportation expenses. A second type consists of practical training internships in structured firms directly linked to the specific job training for a period of three months. During this period the trainee receives a stipend of Bs. 550 per month of training, of which the program covers Bs. 300 and the firm cover Bs. 250. The program also covers the cost of accident insurance during both phases of training.

Coverage and recipiency Eligible of potential beneficiaries are restricted to those who live in a city where the program is being implemented;¹¹ are 18 to 24 years old if living in the cities covered by the first phase, or 16 to 29 years old if living in the cities covered by the second

¹¹The first phase of the program was lunch on January 2009 and covers the cities of La Paz, El Alto, Cochabamba, Santa Cruz and Montero. The second phase of the program was lunch on September 2009 and add the cities of Oruro, Potosi, Cobija, Tarija y Sucre.

Table 6. "URGENT EMPLOYMENT PROGRAM" in Tarija: Employment opportunities created

		Employment Opportunities	Infrastructure Projects	Workers Trained	Workers with Job-contracts
PHASE I	May-December 2006	14463	412	0	0
PHASE II	January-December 2007	39500	908	3895	313
PHASE III	January-December 2008	19000	422	1200	n/a
TOTAL		72963	1742	5095	313

Source: Machicao (2008).

Table 7. FAUTAPO: Applicants, Trained Youth and Employment opportunities created

Applicants to the program (2006 to 2010)			
Municipality	Men	Women	Total
Cochabamba	536	845	1381
El Alto	2637	3321	5958
La Paz	1638	3229	4867
Oruro	336	513	849
Santa Cruz	2011	2583	4594
Sucre	1491	1731	3222
Total	8649	12222	20871

Youth trained in the program			
Category			
Women		5098	5098
with children less than 7 years		1598	1598
with discapacity		17	17
Men	5901		5901
with discapacity	23		23
Total	5924	6713	12637

Youth into the workforce		
Type		
With contract		6708
Self-employed		142
Continuation of higher education		571
without employability		1086
Total		8507

phase; have at least secondary complete if living in the cities covered by the first phase, or at least primary complete if living in the cities covered by the second phase; and are classified as poor.¹²

Table 8 on the following page presents the distribution of applicants and trainees up to June 2010. In the pilot phase, the program have registered 2,562 people for participation, of whom 2,016 have finished classroom training, 1,841 have finished practical training internships, and 1,507 have been placed in a job. In the expansion phase, the program have registered 429 people for participation, of whom 36 have finished classroom training, 271 have finished practical training internships and 234 have been placed in a job.

3.2.5 *Public Labor Exchange*

Public labor exchange or labor intermediation services are probably the oldest labor market programs in Bolivia. Public labor exchange services have been provided by the Ministry of Labor since 1996. At first, the services were provided only in the city of La Paz; however, since 2008 the services has been expanded to organized Units for the Promotion of Employment (UPEs) in seven out of nine departments' capital cities.¹³ Currently, public labor exchanges are restricted to the provision of information to both potential employers and job seekers. There are no follow-up activities that allow us to assess the impact of labor intermediation. Furthermore, no records are kept on either successful matches or complaints by either side.

Coverage and Reciprocity. Unfortunately, not enough information has been systematically recorded. Available information includes only records since 2005. According to SPE registries, the coverage of labor intermediation services is between 50 and 150 successful matches per month.

4 **Temporary Employment Programs Consumption Smoothing and Employability**

This section presents an impact evaluation of the most comprehensive public works program implemented in Bolivia, the *Plan Nacional de Empleo de Emergencia* (PLANE), on consumption smoothing and employability. Evaluating PLANE's results as a consumption-smoothing scheme and as way to increase the employability of vulnerable populations is crucial not only because workfare programs were "the" most important type of labor policy implemented in Bolivia during the current decade, but also because, encouraged by findings of positive and significant effects of PLANE on post-program labor earnings and employment probabilities, similar workfare programs were and are still being implemented in the country; these include PROPAIS, EDIMO and Tarija's

¹²Potential beneficiaries are classified as poor by meeting at least two of three criteria: i) having studied in a public school; ii) having declared a monthly household per capita income below the poverty line, Bs. 678.90 per month; and iii) having a per capita electricity consumption receipt below 70 kilowatts per hour, the cutoff point that identifies beneficiaries of the program *Tarifa Solidaria*.

¹³It is important to note that private labor intermediation initiatives also exist. Yet little is known about their performance, number of beneficiaries and covered sectors of employment.

Table 8. MPED(Mi Primer Empleo Digno: Registered, Trained and Employment opportunities created)

City	Registered	Finished Training I	Finished Training II	Currently working	as % of registered)
Pilot's phase (Participation Rates 2009-2010)					
Total Pilot's phase	2562	2016	1841	1507	58.8
La Paz	439	388	343	303	69
Cochabamba	759	527	472	342	45.1
Santa Cruz	625	488	475	393	62.9
El Alto	539	476	434	375	69.6
Montero	200	137	117	94	47
Expansion's phase (Participation Rates 2009-2010)					
Total Expansion's phase	429	362	371	234	54.5
Potosi	125	111	33	33	26.4
Sucre	101	94	91	87	86.1
Oruro	73	61	56	36	49.3
Tarija	83	55	50	37	44.6
Cobija	47	41	41	41	87.2

Source: "Mi Primer Empleo Digno, Annual Report" (2010).

PLANE. First, we describe the program’s objectives, intervention and selection of beneficiaries. Second, we discuss the outcomes, data and methods used in our impact evaluation. Finally, we present our results and compare them with those of other impact evaluation studies.

4.1 Objective, Intervention and Selection

4.1.1 Objective

PLANE was basically a public employment program. It was created in 2001 after the government declared a “national emergency because of rising unemployment and underemployment” (Supreme Decree No. 26317). The program’s objective was to “mitigate the negative impacts of the economic slowdown on employment by providing short-term employment opportunities to poor unemployed individuals” (Supreme Decree No.26318). Its first phase, PLANE-I, operated for a period of 14 months, from October 2001 through December 2002, with an overall investment of US\$ 32 million. Despite economic recovery, the program was extended in 2003 (DS No. 26849) and in 2004 (DS No. 27294). These extensions are known as Plane-II and PLANE-III. PLANE-II operated from April 2003 through May 2004 with an overall investment of US\$ 27 million and an execution of 4,283 projects. Plane-III operated from May 2004 through December 2005 with an overall investment of US\$ 18 million and an execution of 4,832 projects. Although both extensions retained the original objective of “promoting economic recovery for the country’s poorest sectors through the generation of temporary jobs,” a new objective of “creating conditions that reduce social tensions and strengthen governance” was added.

4.1.2 Intervention

Like most workfare programs, the PLANE was design to provide short term work to unemployed poor people repairing or developing local infrastructure. Although neither household income nor “means”/ asset tests were explicitly used to determine eligibility or select beneficiaries, the working schedule, salary and duration of contract were designed so that mostly unemployed poor people self-select into the program.

- **Working schedule.** The program required participants to be available for work for 35 hours per week (seven hours per day, five days per week).
- **Salary and benefits.** Participants were offered a lump-sum payment of Bs. 120 per week for the duration of the project.¹⁴ No other benefits were provided.
- **Duration of contracts.** Contracts last from one week to three months. Once the period of the contract was completed, people were able to re-apply and wait for the next drawing of beneficiaries if they had complied with minimum performance standards (e.g., not leaving the job).

¹⁴In Plane-I salaries were paid weekly. In the last two phases salaries were paid every two weeks.

4.1.3 Eligibility and Selection

Selection of beneficiaries of the PLANE was conducted in two stages. In the first stage, applicants who met the eligibility criteria had to register at wholesaler offices. At the second stage, the administration unit randomly selected beneficiaries, ensuring a minimum of 30 percent of women. The eligibility criteria were based on current employment status and age of the potential beneficiaries.

- **Employment status.** Applicants had to be jobless.
- **Age.** Applicants had to be between 25 and 50 years old.

Although the rationale for the first criterion was clear, it was virtually impossible to enforce. Given the extent of informality in Bolivia, it is difficult to verify the labor market status of an individual, i.e., to limit an employment program to the unemployed excluding the inactive or the employed in the informal sectors. The rationality for the age criteria was to “*maximizing the probability that household’s heads with children in school age are the ones who join the program,*” although past evaluations noted that, at least in PLANE-I and PLANE-II, there were people who have been hired despite non-compliance with the age eligibility criteria (see Landa and Lizarraga (2007)).

4.2 Outcomes, Data and Methods

4.2.1 Outcomes

Public employment programs are common in times of crisis, usually due to macroeconomic or agro-climatic shocks in which a large number of poor become unemployed. Since they only require participants to work in order to obtain benefits, their main aim is to raise current incomes and to help smooth the consumption of poor households hurt by crises. We evaluate the usefulness of PLANE as a consumption-smoothing scheme using two outcomes: per capita household food expenditure and per capita household calorie intake. Although public employment programs may be relevant in times of recession, it is not clear whether it is efficient to choose labor-intensive technologies over capital intensive-technologies to build public facilities in times of economic recovery or expansion. However, as mentioned above, government-sponsored evaluations have found positive and significant effects of PLANE on both post-program employment probabilities and post-program monthly labor earnings. Those findings have not only encouraged a series of new workfare programs but have also suggested that temporary employment programs may help smooth transitions to better employment in the long run. Therefore, we also evaluate the impact of PLANE on participants’ post-program employability.

4.2.2 Data and Sample

To evaluate the effects of PLANE on consumption smoothing and post-program employability we use the 2002 Living Standards Measurement Survey (LSMS) and the 2003-2004 Income and

Expenditure Survey (IES). Both surveys allow us to identify PLANE's beneficiaries since they asked employed and non-employed with working experience the following questions:¹⁵

- *During the last six months, Have you worked at the PLANE?*, and
- *During this period of time, How long have you worked at the PLANE?*

It is important to bear in mind that the sample of PLANE's beneficiaries has two important qualifications. First, people who declare that *they had worked at PLANE* may still be working at the program. Including such people may cause a large bias in the estimation of program effects on post-program employment probabilities and labor earnings since people who were still working at PLANE at the time of the survey were, by definition, employed and earning program salaries. More specifically, an impact evaluation of PLANE's effect on in-program employability will answer how much employment displacement is caused by the program (since it will compare the in-program employment status (employed) with the counterfactual status without the program) and the costs of such employment displacement (since it will compare in-program earnings (*Bs.* 120 per week) with the counterfactual earnings program participants would have received in the absence of a program). Clearly, impact evaluations that include current participants will bias the results toward positive and significant effects on both prospects of employment and future wages. To avoid this problem we use a question on the name of the business, firm or establishment where people were working to exclude current participants from our 2002 LSMS sample and the disaggregated occupational code and sector of activity to exclude current participants from our 2003-2004 IES sample. Second, it is important to notice that, since we have a *stock* sample of people who have worked at PLANE, it is more likely to sample participants with longer durations in the program. Again, since we are interested in post-program effects of PLANE's we exclude from our treatment sample those beneficiaries who have worked at PLANE for less than 4 weeks. Finally, it is important to mention that we impose some restrictions on the sample of both treatment and control units. Since we want to evaluate the effects of the PLANE for non-professionals, we only retain individuals meeting the program's eligibility criteria, i.e., individuals between 25 to 50 years old with at most secondary complete education.

4.2.3 *Methods*

To evaluate PLANE's effect on calorie intake and future employability we use methods of estimation and inference that assume unconfoundedness. Unconfoundedness refers to the situation where adjusting treatment and control groups for differences in observed covariates is enough to remove all biases in comparisons between treatment and control units, i.e., conditional on observed covariates there are no unobserved factors that are associated with the assignment variable or with

¹⁵Non-employed without working experience were excluded from the flow of questions.

potential outcomes. This assumption is at least controversial; however, since we only have cross-sectional observational data there is no clear superior alternative. More formally, we attempt to estimate

$$\tau = E[y_{1i}|X_i, W_i = 1] - E[y_{0i}|X_i, W_i = 0]$$

where τ is the *sample* average treatment effect on the treated (SATT), y_{1i} and y_{0i} are the counterfactual outcomes for individual i with and without the program, X_i is the set of observable covariates, and W_i is the assignment mechanism. The fundamental problem of program evaluation is that we either observe y_{1i} (for those who participate in the program) or y_{0i} (for those who do not participate in the program), but never both. Therefore, in order to identify the SATT we have to assume not only **unconfoundedness**, i.e., assume that beyond observed covariates there are no unobserved characteristics associated with both potential outcomes and treatment;

$$(y_i(0), y_i(1))W_i|X_i;$$

but also **overlapping in the distribution of covariates** in both treated and control samples, i.e., that for all possible values of the covariates we have that

$$0 < Pr(W_i = 1|X_i = x) < 1, \quad \forall x.$$

Under unconfoundedness and overlapping it is possible to show that the SATT can be identified as¹⁶

$$\tau = E[y_i|X_i, W_i = 1] - E[y_i|X_i, W_i = 0].$$

Estimation and inference of the SATT under unconfoundedness can be done based on regression, propensity score, matching methods and their combination. Appendix B, at the end of the paper, describes the eight different estimators we used to explore the sensitivity of our results to the choice of the estimator.

4.3 Results

4.3.1 Assessing of Overlapping

A major concern of using impact evaluation methods under the assumption of unconfoundedness is the lack of overlap in the distribution of covariates. Therefore, before presenting our results on PLANE's impact on consumption smoothing and employability, we present an assessment of the overlapping between treatment and control samples for both surveys.

One way to assess overlap in the distribution of covariates between treatment and control samples is calculating differences in average covariates values by treatment status normalized by

¹⁶See Imbens and Imbens and Wooldridge (2008) for further details.

the square root of the sum of the within-treatment group variances.¹⁷ Formally,

$$\frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_{X_0}^2 + S_{X_1}^2}}$$

where $\bar{X}_w = \frac{1}{N_w} \sum_{i:W_i=w} X_i$ and $S_{X_w}^2 = \frac{1}{N_w-1} \sum_{i:W_i=w} (X_i - \bar{X}_w)^2$. Table 9 on the next page and Table 10 on page 31 present summary statistics of the set of covariates used to adjust differences in treatment in the 2002 and the 2003-2004 surveys, respectively. In terms of means and standard deviations, the comparison between the *treatment sample* (Panel A) and the *full control sample* (Panel B) reveals significant differences in the distribution of at least half of the covariates in both, the 2002 and the 2003-2004 surveys; note that **normalized differences** in averages are above two standard deviations for almost half of included covariates. The large normalized differences in both surveys suggests that there will be serious issues in obtaining credible estimates of the sample average treatment effect of the treatment using the full set of controls.

Although inspecting normalized differences in covariates by treatment status is a sensible starting point, it is not generally sufficient. Even if the marginal distribution of covariates is similar, there may still be regions in the covariates space where the multivariate density of covariates in the treatment group is zero while the multivariate density of covariates in the control group is not, or vice-versa. A complementary approach to assessing overlap in the covariates distributions is to inspect histograms of the estimated propensity score by treatment status. Figures 13, 14, 15 and 16 (beginning on page 13) present the histogram estimates of the distribution of the propensity score for the treatment and the full set of potential control units. Note that for one set of treatment units (those with estimated propensity scores above 0.1 in the 2002 survey and those with propensity scores above 0.2 in the 2003-2004 survey) there is considerable lack of overlap with the full control sample.

4.3.2 Improving Overlap

To improve the overlap in the distribution of covariates we construct a *matched sample* using the Rubin (2006) procedure. First, we estimate a propensity score using a backward step-wise method where increasingly flexible specifications are selected until the specification is deemed adequate. The initial vector of covariates includes a set of dummies for year of the survey, place of residence, sex, age group, ethnicity, years of education, household head condition, household size, non-labor income and household assets. The treated observations are subsequently ordered by decreasing values of the estimated propensity score.¹⁸ Then, the first treated unit (i.e., the one with the highest

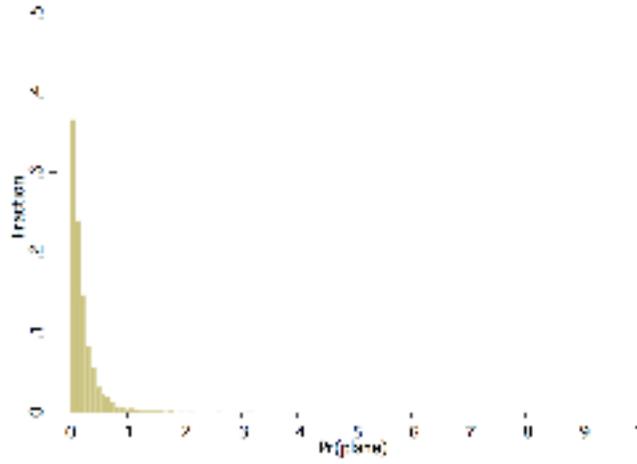
¹⁷Note that t-statistics depend on the sample size so that larger t-statistics only indicate larger sample sizes.

¹⁸The rationale for this is that among units with high values of the propensity score there are relatively more treated than control units, and therefore treated observations with high values of the propensity score are relatively more difficult to match.

Table 9. Summary Statistics for the 2002 LSMS Survey

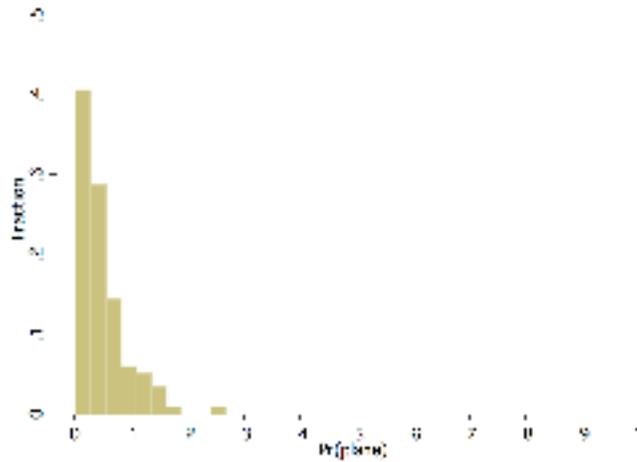
Variable	Treatment sample=103		Full control sample=5687		Matched control sample=103	
	Mean	s.e.	Mean	s.e.	Mean	nor.diff.
employment rate	0.83	0.38	0.80	0.40	0.87	0.33
participation rate	0.86	0.34	0.83	0.38	0.88	0.32
income from primary job	448.50	596.88	846.73	3440.24	771.90	1871.58
asset index	-0.37	1.52	0.01	2.00	-0.45	1.77
dwelling index	-0.23	1.91	0.00	2.14	-0.40	2.28
rural	0.53	0.50	0.44	0.50	0.53	0.50
female	0.41	0.49	0.53	0.50	0.41	0.49
age	36.65	7.54	36.65	7.44	38.12	7.63
years of education	5.20	3.50	6.01	3.87	5.52	3.38
indigenous	0.73	0.45	0.49	0.50	0.71	0.46
household head	0.64	0.48	0.47	0.50	0.69	0.47
household size	5.53	2.54	5.43	2.24	5.21	2.68
TV	0.45	0.50	0.56	0.50	0.43	0.50
bicycle	0.59	0.49	0.48	0.50	0.59	0.49
living furniture	0.02	0.14	0.10	0.30	0.02	0.14
stove	0.78	0.42	0.71	0.45	0.69	0.47
sewing machine	0.36	0.48	0.28	0.45	0.43	0.50
finished inner walls	0.78	0.42	0.72	0.45	0.74	0.44
shingle roof	0.49	0.50	0.48	0.50	0.49	0.50
ceramic tiles roof	0.20	0.40	0.28	0.45	0.17	0.38
concrete roof	0.01	0.10	0.03	0.17	0.00	0.00
exclusive bathroom	0.09	0.28	0.23	0.42	0.11	0.31
cooking with charcoal	0.40	0.49	0.38	0.48	0.43	0.50
cooking with gas	0.59	0.49	0.59	0.49	0.54	0.50
exclusive kitchen	0.64	0.48	0.76	0.43	0.63	0.48
number of rooms square	8.00	10.91	7.63	9.25	9.49	12.61
own dwelling	0.79	0.41	0.67	0.47	0.80	0.40

Figure 13. 2002 Propensity Scores Full Sample. Controls



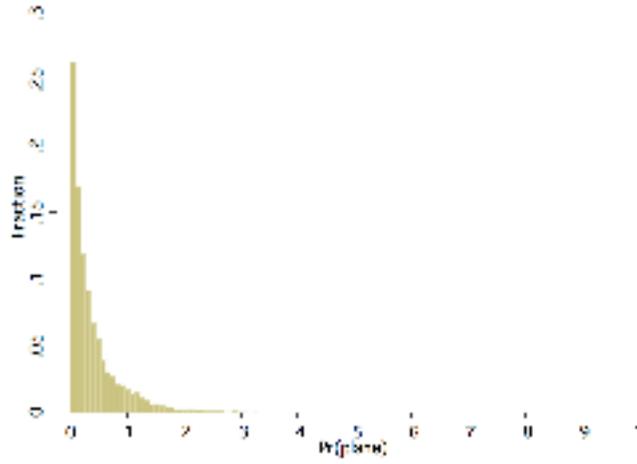
Source: Author's calculation using 2002 harmonized household survey.

Figure 14. 2002 Propensity Scores Full Sample. Treatment



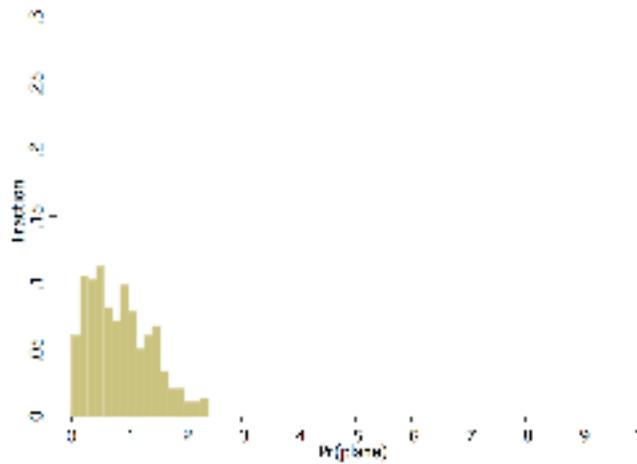
Source: Author's calculation using 2002 harmonized household survey.

Figure 15. 2003-2004 Propensity Scores Full Sample. Controls



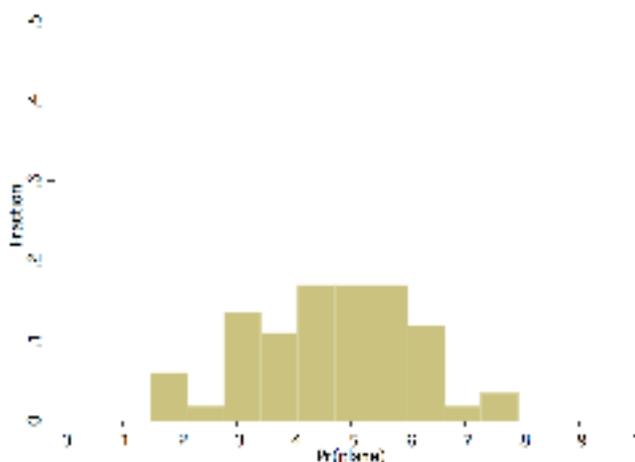
Source: Author's calculation using 2003-2004 harmonized household survey.

Figure 16. 2003-2004 Propensity Scores Full Sample. Treatment



Source: Author's calculation using 2003-2004 household survey.

Figure 17. 2002 Propensity Scores Matched Sample. Controls



Source: Author's calculation using 2002 harmonized household survey.

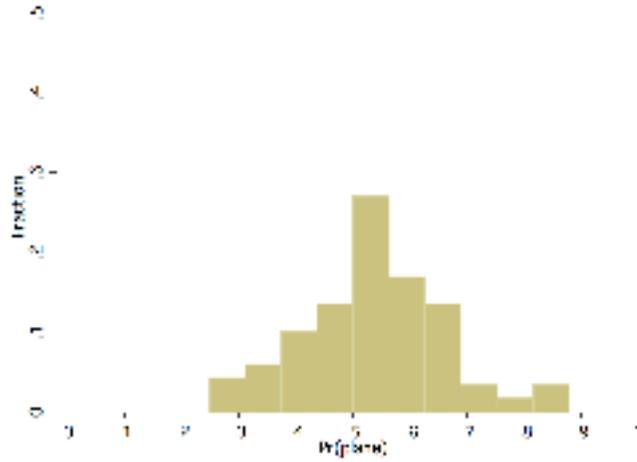
value for the estimated propensity score), is matched to the nearest control unit. Next, the second treated unit is matched to the nearest control unit, including the control unit that was used as a match for the first treated unit. We continue matching with replacement all treated units until we obtain a matched sample of $2N_1$ units (where N_1 is the size of the original treated sample), half of them treated and half of them control units. It is important to note that the Rubin's procedure was used *not* to estimate ATT effects by averaging differences within pairs, but to improve the overlap of the matched sample.

The comparison of the distribution of covariates between the treatment sample (Panel A) and the matched control sample (Panel C) show large improvements in the overlapping of covariates. Note that now, normalized differences are below two standard deviations for all covariates and below 0.50 standard deviations for around half of the included covariates. Figures 17, 18, 19 and 20 (beginning on page 17) present the histograms of the re-estimated propensity scores for the treatment and control matched sample. Although there is still variation in the propensity score, the variation is not so great that econometric analysis could not adjust. This suggests not only that the matched samples are well balanced but also that, given unconfoundedness, the matched samples are more likely to lead to robust estimates.

4.3.3 *PLANE's impact on calorie intake and employability*

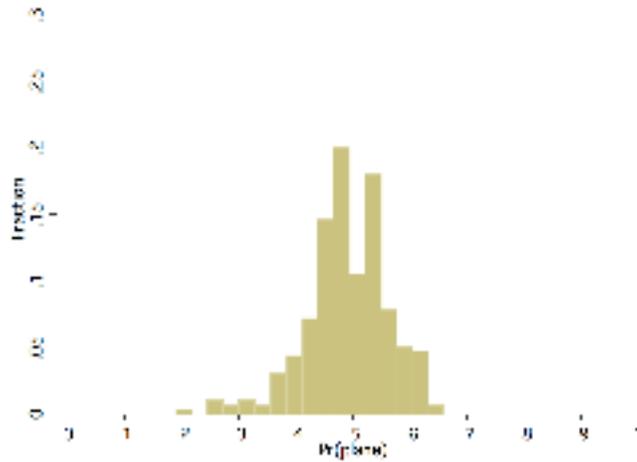
Table 11 on page 37 and Table 12 on page 38 presents the impacts of PLANE on monthly labor earnings (Panel A), the probability of being employed (Panel B), the probability of participating (Panel C) and per capita calorie intake (Panel D), for the 2002 and 2003-2004 samples, respectively. For each survey, four sets of estimates are presented using both the unmatched and matched

Figure 18. 2002 Propensity Scores Matched Sample. Treatment



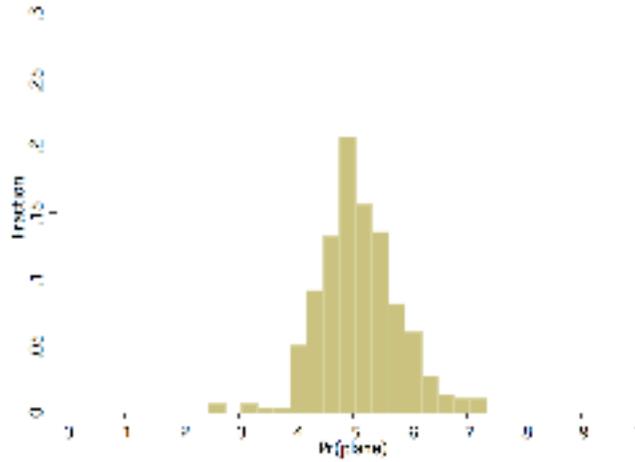
Source: Author's calculation using 2002 harmonized household survey.

Figure 19. 2003-2004 Propensity Scores Matched Sample. Controls



Source: Author's calculation using 2003-2004 harmonized household survey.

Figure 20. 2003-2004 Propensity Scores Matched Sample. Treatment



Source: Author's calculation 2003-2004 harmonized household survey.

samples and using a basic set of covariates (age, sex, ethnicity, years of education, place of residence, relationship to the household head and household size) and an extended set of covariates that includes household assets and dwelling characteristics. The estimates of the SATT of PLANE on monthly labor earnings in all cases are negative and significant. Note that standard errors are always lower when we use the extended set of covariates for adjusting for confounding factors. Furthermore, standard errors are always lower whenever the matched sample is used. The estimates of the SATT of PLANE on both post-program employment and participation rates are all negative, but their significance depends on the sample (unmatched or matched) and the set of covariates used for adjusting confounding factors. Finally, the estimates of the SATT of PLANE on per capita calorie intake are all positive and significant when the extended set of covariates and the matched sample are used.

4.3.4 Assessing Unconfoundedness

Our results of the effect of PLANE on per capita household income and monthly labor earnings can be interpreted in different ways depending on whether we believe we have solved the selection problem. In order to see this point more clearly, notice that our estimates of the SATT can be decomposed into two parts, a true effect of the program on the treated and a sample selection term,

$$E[y_1|X_iW_i = 1] - E[y_0|X_iW_i = 0] = \underbrace{(E[y_1|X_iW_i = 1] - E[y_0|X_iW_i = 1])}_{\text{true SATT}} + \underbrace{(E[y_0|X_iW_i = 1] - E[y_0|X_iW_i = 0])}_{\text{sample selection}}$$

Table 12. PLANE's Sample Average Treatment Effect on the Treated 2003-2004

Estimator	Panel A income from primary job		Panel B employment rate		Panel C participation rate		Panel D pc calorie intake			
	SATT	t	SATT	t	SATT	t	SATT	t		
	s.e.		s.e.		s.e.		s.e.			
	2003-2004 Unmatched Sample with basic set of covariates									
1 Simple Difference	-513.93	-12.75	-0.073	0.027	-0.037	0.024	-0.534	-249.62	108.48	-2.301
2 Regression (Separate)	-281.72	-8.16	-0.062	0.024	-0.027	0.021	-1.266	42.51	96.97	0.438
3 Weighing	-283.30	-7.16	-0.067	0.028	-0.032	0.024	-1.306	54.93	108.32	0.507
4 Blocking	-307.87	-7.78	-0.065	0.027	-0.030	0.024	-1.248	24.62	108.41	0.227
5 Matching	-278.04	-5.06	-0.062	0.026	-0.031	0.024	-1.315	94.64	105.23	0.899
6 Weighting and Regression	-284.09	-8.12	-0.064	0.024	-0.029	0.022	-1.358	58.85	98.35	0.598
7 Blocking and Regression	-272.72	-7.34	-0.066	0.025	-0.028	0.022	-1.260	51.62	99.68	0.518
8 Matching and Regression	-279.57	-5.08	-0.062	0.026	-0.031	0.024	-1.298	82.38	103.68	0.795
	2003-2004 Matched Sample with basic set of covariates									
1 Simple Difference	-199.97	-2.68	-0.067	0.036	-0.021	0.032	-0.650	127.85	136.33	0.938
2 Regression (Separate)	-239.83	-3.06	-0.070	0.033	-0.021	0.030	-0.701	101.95	126.70	0.805
3 Weighing	-246.78	-2.64	-0.069	0.039	-0.021	0.036	-0.586	90.79	147.87	0.614
4 Blocking	-244.40	-2.89	-0.065	0.036	-0.018	0.033	-0.535	121.47	139.56	0.870
5 Matching	-188.45	-2.62	-0.054	0.037	-0.009	0.034	-0.249	198.95	136.66	1.456
6 Weighting and Regression	-243.22	-3.02	-0.071	0.033	-0.022	0.031	-0.717	100.30	125.89	0.797
7 Blocking and Regression	-260.16	-2.74	-0.061	0.036	-0.016	0.033	-0.482	86.18	137.17	0.628
8 Matching and Regression	-201.66	-2.81	-0.065	0.037	-0.017	0.034	-0.485	164.84	132.48	1.244
	2003-2004 Unmatched Sample with extended set of covariates									
1 Simple Difference	-513.86	-12.75	-0.073	0.027	-0.037	0.024	-1.535	-249.329	108.481	-2.298
2 Regression (Separate)	-152.44	-4.60	-0.057	0.023	-0.025	0.021	-1.206	123.877	89.961	1.377
3 Weighing	-185.49	-39.38	-0.057	0.028	-0.023	0.024	-0.937	149.473	108.186	1.382
4 Blocking	-246.17	-6.25	-0.059	0.028	-0.026	0.024	-1.060	97.733	108.403	0.902
5 Matching	-201.67	-55.24	-0.044	0.027	-0.009	0.024	-0.360	127.504	107.005	1.192
6 Weighting and Regression	-178.42	-33.57	-0.055	0.024	-0.020	0.021	-0.954	171.325	95.115	1.801
7 Blocking and Regression	-165.44	-36.24	-0.062	0.024	-0.026	0.022	-1.198	146.843	96.066	1.529
8 Matching and Regression	-181.88	-3.39	-0.035	0.026	-0.003	0.024	-0.109	129.912	104.658	1.241
	2003-2004 Matched Sample with extended set of covariates									
1 Simple Difference	-101.32	-53.26	-0.034	0.037	-0.004	0.033	-0.127	229.38	134.19	1.71
2 Regression (Separate)	-94.37	-44.32	-0.041	0.034	-0.012	0.030	-0.401	246.56	116.91	2.11
3 Weighing	-94.99	-53.47	-0.046	0.043	-0.016	0.040	-0.403	239.78	138.04	1.74
4 Blocking	-99.24	-54.26	-0.041	0.038	-0.012	0.035	-0.349	219.96	132.34	1.66
5 Matching	-48.07	-48.54	-0.016	0.037	0.019	0.034	0.548	360.90	139.91	2.58
6 Weighting and Regression	-91.69	-43.43	-0.045	0.034	-0.016	0.030	-0.518	243.23	119.74	2.03
7 Blocking and Regression	-82.15	-47.87	-0.043	0.035	-0.016	0.032	-0.505	243.55	128.11	1.90
8 Matching and Regression	-83.86	-45.77	-0.019	0.036	0.015	0.034	0.441	303.00	136.04	2.23

Source: Author's calculation.

On the one hand, if the self-selection mechanism of PLANE was successful (in the sense that PLANE attracted only those people with the lowest income generation capabilities) it is likely that, even after controlling for available covariates, there might still be selection issues. In this case we would expect a negative selection bias since people with lower income-generation capabilities are likely to have lower per capita calorie intake, lower monthly earnings and maybe lower probabilities of being employed in the absence of the program. On the other hand, if the self-selection mechanism attracted people with lower generation capabilities, but not only those with the lowest income generation capabilities, we would be more confident that controlling for observed covariates have solved the selection issue and, therefore, the selection term will be close to zero.

In order to test the self-selection mechanism we construct an asset index and a dwelling index using a principal components analysis to estimate a pseudo-causal effect. Notice that, since both household assets and dwelling characteristics are determined prior to the treatment itself, the estimated causal effect of the treatment variable on such variables is expected to be zero. Any non-zero effect would imply that the treated observations are different from the controls in terms of this particular covariate given others and would be evidence of sample selection. Of course, being able to reject the null of no effect does not directly reflect on the hypothesis of interest, unconfoundedness. Table 13 on the next page presents our SATT estimates on both pseudo outcomes. For the 2002, all but the simple difference coefficients are non-significant; however, for the 2003-2004 sample, the results suggest that the self-selection mechanism used by PLANE was successful in recruiting those people with the lowest income-generating potential.

Notice that, even in the case of self-selection, the positive and significant effects of PLANE on daily per capita calorie intake imply that the program was successful as a consumption-smoothing scheme for participants. In fact, our estimates may be interpreted as lower-bound estimates of the true SATT. Now, the negative and significant effects of PLANE on monthly earnings may be attributed either to a negative impact of PLANE, or negative self-selection, the second term. In both scenarios we can conclude that the PLANE has had no effect on either future labor earnings or future employment probabilities.

4.3.5 Comparison with other evaluations

Our impact evaluation estimates are very different from the estimates of other impact evaluations of PLANE. In particular, our results are very different from those of government-sponsored evaluations that have found a positive and significant impact of PLANE on post-program employment probabilities and labor earnings. Landa (2003) and Rivero (2003) find that PLANE-I had increased the post-program probability of being employed for both men and woman and had increased women's post-program salaries. According to both studies, the average impact on the probability of being employed *after* PLANE was at least 3.5 percentage points for men and 6.5

Table 13. PLANE's Sample Average Treatment Effect on the Treated 2002 and 2003-2004

Estimator	2002			2003-2004		
	Panel D asset index		Panel E dwelling index	Panel D asset index		Panel E dwelling index
	SATT	s.e.	t	SATT	s.e.	t
	Unmatched Sample with basic set of covariates					
	SATT	s.e.	t	SATT	s.e.	t
1 Simple Difference	-0.378	0.152	-2.491	-0.235	0.191	-1.232
2 Regression (Separate)	0.073	0.123	0.593	0.109	0.122	0.897
3 Weighing	0.008	0.151	0.050	0.077	0.191	0.403
4 Blocking	-0.054	0.146	-0.372	0.041	0.190	0.216
5 Matching	0.001	0.158	0.004	0.005	0.161	0.030
6 Weighting and Regression	0.052	0.133	0.392	0.115	0.130	0.886
7 Blocking and Regression	0.069	0.140	0.492	0.122	0.139	0.880
8 Matching and Regression	0.013	0.156	0.086	-0.001	0.159	-0.005
	Matched sample with basic set of covariates					
	SATT	s.e.	t	SATT	s.e.	t
1 Simple Difference	0.141	0.226	0.625	0.022	0.276	0.081
2 Regression (Separate)	0.196	0.183	1.068	-0.061	0.190	-0.323
3 Weighing	0.126	0.217	0.581	-0.198	0.277	-0.713
4 Blocking	0.169	0.220	0.767	-0.116	0.271	-0.429
5 Matching	0.131	0.214	0.611	-0.111	0.228	-0.485
6 Weighting and Regression	0.168	0.184	0.915	-0.062	0.189	-0.326
7 Blocking and Regression	0.196	0.246	0.799	-0.196	0.232	-0.845
8 Matching and Regression	0.058	0.210	0.276	-0.148	0.195	-0.756
	Unmatched Sample with extended set of covariates					
	SATT	s.e.	t	SATT	s.e.	t
1 Simple Difference	-0.378	0.152	-2.491	-0.235	0.191	-1.232
2 Regression (Separate)	-0.059	0.056	-1.054	0.046	0.054	0.851
3 Weighing	-0.042	0.151	-0.279	0.038	0.192	0.200
4 Blocking	-0.137	0.153	-0.896	-0.030	0.194	-0.153
5 Matching	-0.004	0.087	-0.046	0.032	0.079	0.398
6 Weighting and Regression	-0.037	0.062	-0.589	0.046	0.059	0.784
7 Blocking and Regression	-0.048	0.074	-0.650	0.056	0.065	0.855
8 Matching and Regression	-0.051	0.076	-0.666	0.036	0.066	0.537
	Matched Sample with extended set of covariates					
	SATT	s.e.	t	SATT	s.e.	t
1 Simple Difference	0.081	0.230	0.352	0.167	0.293	0.569
2 Regression (Separate)	0.074	0.119	0.628	0.046	0.087	0.522
3 Weighing	0.144	0.234	0.613	0.154	0.290	0.533
4 Blocking	0.129	0.223	0.578	0.124	0.289	0.428
5 Matching	0.131	0.158	0.827	0.068	0.142	0.481
6 Weighting and Regression	0.067	0.113	0.598	0.072	0.093	0.776
7 Blocking and Regression	0.216	0.131	1.648	-0.024	0.130	-0.185
8 Matching and Regression	0.113	0.136	0.834	0.071	0.095	0.743

Source: Author's calculation.

percentage points for women, while the average impact on future labor earnings was between 138 and 167 bolivianos of 2002. Landa and Lizarraga (2007) also find that PLANE-III had increased the post-program probability of being employed for both men and woman and had increased men's post-program salaries. In this study, the average effect on the probability of being employed *after* PLANE was at least 39 percentage points for men and 24 percentage points for women, while the average effect on future labor earnings was between 126 and 154 bolivianos of 2003 for men. From our point of view, some of the ATT magnitudes are too high to be true. Furthermore, it seems unlikely that a temporary work experience program such as PLANE would have long-term effects on earnings and employability. Since PLANE did not include any training component and most PLANE projects were intensive in unskilled work, the only channel through which PLANE would increase productivity was labor market experience. However, since PLANE was of a temporary nature and the contracts were explicitly designed not to last more than three months, it is difficult to believe it had increased post-program earnings and employability.

5 Conclusions and Policy Recommendations

Unemployment is one of the most important concerns for societies in general and policymakers, in particular. Recent data from *Latinobarómetro* reveals that two out of three people identify unemployment as *the* most important problem in Bolivia. However, we find that unemployment, defined as measured in a conventional way, may not be *the* most important problem in Bolivia's labor market.

Labor market regulations and labor market programs may be crucial instruments for increasing the size of the "labor market" in which employees perform tasks for employers in return for money. Nonetheless, we find that Bolivia's labor market regulations are among the most rigid in the region. Furthermore, we document that Bolivia's labor policies have been based on temporary employment programs, which usually do not have any impact on the productivity/employability of beneficiaries.

In order to investigate the effects of temporary employment programs on employability we conduct an impact evaluation of one of Bolivia's most comprehensive employment program, the *Plan Nacional de Empleo de Emergencia* (PLANE). We find that, although PLANE was successful as a consumption-smoothing scheme, increasing per capita calorie intake in households where at least one member had participated in the program, it did not have any effects on either post-program probabilities of being employed or post-program wages. This evidence suggests that, although public employment programs might be useful as social protection policies in times of recession, smoothing consumption of poor households with unskilled breadwinners, such programs are not good alternatives for improving the employability of vulnerable populations.

Therefore, we conclude that, given the country's level of informality, protection policies (such as social transfers, unemployment insurance schemes, severance payments, and regulations regarding involuntary dismissal) are second best to active policies specifically designed to increase the productivity/employability of vulnerable populations.

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A Data Sources

All the analysis presented in this document is based on two datasources: *Fundación ARU*'s set of harmonized household surveys and 8 quarters of the *Quarterly Employment Survey*.

A.1 Fundacion ARU's set of harmonized household Surveys

We use Fundación ARU (2010) set of harmonized household surveys with national coverage, i.e., those for the period from 1999 to 2009. The surveys comprise the Living Standard Measurement Surveys (LSMS) from 1999 to 2002, the Income and Expenditure survey of years 2003 and 2004, the LSMS from 2005 to 2007, and the Social Stratification and Mobility Surveys (EMES) of years 2008 and 2009. All but the last two surveys were designed and implemented by the National Institute of Statistics of Bolivia. The EMES was design and implemented by the *Human Development Report* Project of the United Nations Development Programme (UNPD). It is important to note that most surveys, even those implemented by the INE, have had not only different content but also different sample design. Fundación ARU (2010) has not only refrained from the use of imputation procedures, applied modern and homogeneous cleaning procedures, and used post-stratification methods to construct new sample weights that solve most under- and over-representation problems. Instead, to the greatest extent possible, similar definitions were used for the construction of variables and indicators.

B Impact Evaluation Estimators

To evaluate PLANE's effect on calorie intake and future employability of those who participate in the program we use eight different estimators to explore the sensitivity of our results to the choice of the estimator.

1. **Simple differences.** First, it is usually informative to calculate the simple difference of average outcomes between treatment units, \bar{Y}_1 , and control units, \bar{Y}_0 .

$$\hat{\tau} = \bar{Y}_1 - \bar{Y}_0 \quad (1)$$

2. **Regression.** The regression approach for the estimation of ATT define regression functions for each potential outcome. In the simplest case, conditional means are assumed to be linear in parameters, i.e.,

$$\mu_w(x) = E[Y_i(w)|X_i = x] = \alpha_w + \beta'_w(x - \phi X) \quad \text{for } w \in \{0, 1\}$$

Given consistent estimators, $\hat{\mu}_0(x)$ and $\hat{\mu}_1(x)$, a consistent estimator of the ATT will be given by:

$$\hat{\mu} = \frac{1}{N_1} \sum_{i=1}^{N_1} (\hat{\mu}_1(x) - \hat{\mu}_0(x)) \quad (2)$$

Notice that the least squares regression,

$$Y_i = \alpha + \tau W_i + \beta X_i + \gamma(X_i - \bar{X}_1)W_i + \epsilon_i$$

gives a consistent estimator of the ATT, $\hat{\tau}$; and its variance, $\hat{\sigma}_\tau$. Notice that the interaction of the covariates with the treatment indicator is based on deviation from the average covariates values *for the treated* so that $\hat{\tau}$ estimates the SATT, not the sample average treatment effect (SATE).

3. **Weighting.** Estimates of the propensity score, $\hat{e}(X_i)$, can be used in several ways. One method, proposed by Horvitz and Thompson (1952), use the propensity score to weight observations in order to estimate the ATT,

$$\hat{\tau} = \frac{1}{N} \sum_{i=1}^N \frac{[W_i - \hat{e}(X_i)]}{\hat{\rho}[1 - \hat{e}(X_i)]} Y_i = \frac{1}{N} \sum_{i=1}^N k_i \quad (3)$$

where $\hat{\rho} = (N_1/N)$ is the fraction of the treated in the sample. Following Wooldridge (2007), the asymptotic standard deviation, $\hat{\sigma}_\tau$, will be given by:

$$\hat{\sigma}_\tau = \left(\frac{1}{N} \sum_{i=1}^N \hat{u}_i^2 \right)^{1/2}$$

where \hat{u}_i are the OLS residuals from the regression of \hat{k}_i on 1 and the score of \hat{e}_i .

4. **Blocking.** Another way to use the estimated propensity score is to create blocks or strata, estimate the ATT within the strata as the difference in average outcomes between treated and controls, and estimate the overall ATT as the weighted average of the within-stratum estimates - with weights equal to the percentage of treated units in each strata. Formally, let $0 = c_0 < c_1 < \dots < c_J = 1$ be boundary values that define the blocks,

$$B_{ij} = \begin{cases} 1 & \text{if } c_{j-1} \leq e(x_i) < c_j \\ 0 & \text{otherwise} \end{cases}$$

Within-stratum estimates of the ATT effect are given by the simple differences in average outcomes.

$$\hat{\tau}_j = \bar{Y}_{j1} - \bar{Y}_{j0}$$

The rationality behind this strategy is that, if J is sufficiently large, so that the differences $c_j - c_{j-1}$ are small, there will be little variation in the propensity score within a stratum. Therefore, we can analyze the data as if, within a stratum, the data were generated by a completely randomized experiment with assignment probabilities constant within a stratum but varying between strata. The average treatment effect is estimated as the weighted average of the within-stratum estimates with weights equal to the fraction of treatment units in each strata.

$$\hat{\tau} = \sum_{j=1}^J \hat{\tau}_j \frac{N_{j1}}{N_1}$$

The standard deviation is then estimated as

$$\sigma_\tau = \left(\sum_{j=1}^J \left(\frac{N_{j1}}{N_1} \right)^2 \left(\hat{V}_{0j} + \hat{V}_{1j} \right) \right)^{1/2}$$

5. **Matching.** The matching method matches all treatment units to their closest controls. We follow AImbens (2008) and conduct matching, with replacement, on all covariates, weighted

by the diagonal matrix with the inverse of the variances on the diagonal. More formally, let $\ell_m(i)$ be the index of the m -th closest unit in the control group to unit i in the treatment group, in terms of the distance measure based on the norm $\|\cdot\|$. Formally, $\ell_m(i)$ satisfies:

$$\sum_{l:W_l \neq W_i} \{ \|X_l - X_i\| \leq \|X_{\ell_m(i)} - X_i\| \} = m \quad (4)$$

Let $\mathcal{J}_M i = \{\ell_1(i), \ell_2(i), \dots, \ell_M(i)\} \subset \{1, 2, \dots, N_0\}$ denote the set of indices of the M matches for unit i . Then the matching estimator of the ATT effect will be given by:

$$\hat{\tau} = \frac{1}{N_1} \sum_{i:W_i=1} [Y_i(1) - \hat{Y}_i(0)]$$

where:

$$\hat{Y}_i(0) = \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} Y_j \quad \text{for each } W_i = 1$$

6. Weighting and Regression

Based on the weighted least squares regression,

$$Y_i = \alpha + \tau W_i + \beta X_i + \epsilon_i$$

with weights $\lambda_i = \sqrt{W_i + (1 - W_i) \frac{\hat{e}(X_i)}{1 - \hat{e}(X_i)}}$.

7. Blocking and Regression. Based on the same strata (blocks) as the fifth estimator where linear regression is used to estimate the within-blocks average effect.

Imbens and Wooldridge (2008) argue that, with a modest number of strata, this estimator is considerably more flexible and robust than either blocking alone or regression alone.

8. Matching and Regression. AImbens (2008) show that whenever the matching is not exact the matching estimator will be bias in finite samples.¹⁹ As Rubin (1973) and AImbens (2008) have shown, it is possible to reduce the bias by using regression methods. Given the estimated regression function $\hat{\mu}_w(x) = \hat{\alpha} + \hat{\beta}'_w x$, the bias corrected matching estimator will be given by:

¹⁹In particular, AImbens (2008) show that with k continuous covariates the estimator will have a bias corresponding to the matching discrepancies that will be of the order $O_p(N^{-1/k})$.

$$\hat{\tau} = \frac{1}{N_1} \sum_{i:W_i=1} [Y_i(1) - \hat{Y}_i(0)]$$

where:

$$\hat{Y}_i(0) = \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} (Y_j + \hat{\mu}_0(X_i)) - \hat{\mu}_0(X_j) \quad \text{for each } W_i = 1.$$

C Unemployment Scarring

C.1 On Subsequent Unemployment

C.1.1 The Chamberlain Method

To identify the contribution of the past employment state $y_{i,t-1}$ to the present state y_{it} , Chamberlain (1985) proposes estimating the following autoregressive logit model without exogenous covariates x

$$y_{it} = I(\alpha y_{i,t-1} + \eta_i + u_{it} > 0)$$

where η_i and u_{it} are time-invariant and time-variant unobserved workers' characteristics. With at least $T=4$ histories, it is possible to define two set of histories:

$$\begin{aligned} A &= y_1, 0, 1, y_4 \\ B &= y_1, 1, 0, y_4 \end{aligned} \tag{5}$$

where y_1 and y_4 can be 0 or 1 but they are the same in A and B. Chamberlain shows that (for the Logit Model) the probabilities $Pr(A|\eta, A \cup B)$ and $Pr(B|\eta, A \cup B)$ do not depend on η_i .²⁰ So that to estimate α it is possible to maximize the following conditional (log)likelihood function for the subsample of individuals with $y_{i2} + y_{i3} = 1$

²⁰In particular,

$$\begin{aligned} Pr(A|\eta, A \cup B) &= \frac{1}{1 + \exp(\alpha(y_1 - y_4))} \\ Pr(B|\eta, A \cup B) &= \frac{\exp(\alpha(y_1 - y_4))}{1 + \exp(\alpha(y_1 - y_4))} \end{aligned} \tag{6}$$

$$l^C(\alpha) = \sum_i y_{i2} \ln \left[\frac{\exp(\alpha(y_1 - y_4))}{1 + \exp(\alpha(y_1 - y_4))} \right] + (1 - y_{i2}) \ln \left[\frac{1}{1 + \exp(\alpha(y_1 - y_4))} \right] \quad (7)$$

which is \sqrt{N} consistent and asymptotically normal.

C.1.2 The Honore-Kyriazidou Method

Although the Chamberlain Method allows us to obtain a consistent estimate of the true state dependence effect it cannot identify the heterogeneity factors that matter most for persistence in employment and in unemployment states. In order to do this we will follow Honore-Kyriazidou (2000) and extend the C-logit approach to models with exogenous covariates x in addition to $y_{i,t-1}$.

$$y_{it} = I(\alpha y_{i,t-1} + \beta x_{it} \eta_i + u_{it} > 0)$$

where x_{it} is strictly exogenous with respect to u_{it} . Honore-Kyriazidou (2000) note that it is possible to obtain a C-logit estimate of α and β using the subsample of observations with $x_{i2} = x_{i3}$, however, this approach has obvious limitations. Instead they propose a C-logit estimator that exploits all histories with $y_{i2} + y_{i3} = 1$ but where the weight of each individual in the likelihood function depends on a measure of the distance between x_{i2} and x_{i3} . That is,

$$l^C(\beta, \alpha) = \sum_i K(x_{i2} - x_{i3}) y_{i2} \ln \left[\frac{\exp(\alpha(y_1 - y_4))}{1 + \exp(\alpha(y_1 - y_4))} \right] + K(x_{i2} - x_{i3}) (1 - y_{i2}) \ln \left[\frac{1}{1 + \exp(\alpha(y_1 - y_4))} \right] \quad (8)$$

where $K(x_{i2} - x_{i3})$ is a kernel function. Note that this estimator is consistent and asymptotically normal although the rate of convergen is slower than \sqrt{N} .

C.2 On Subsequent Wages

In order to estimate the effect of unemployment spells on subsequent wages (earnings) we use the following model,

$$\ln(w_{i,t}) = x_{i,t} \beta + \sum_{s \in \{1,3,4\}} d_{i,t-s} \gamma + \alpha_i + u_{i,t}.$$

where $w_{i,t}$ is the natural log of hourly wage (or monthly earnings) for individual i at time t , $x_{i,t}$ a vector of observable individual characteristics, $d_{i,t-s}$ a dummy variable if individual entered

employment via spell of unemployment at time $t - s$, α_i a time invariant individual specific error term, $u_{i,t}$ a time-variant individual error term, and β, γ the parameters of interest.

Notice that the above equation can be estimated by ordinary least squares only if there is no correlation between the observed characteristics and the unobserved individual effects. In order to test for this correlation and account for unobserved individual heterogeneity we use three alternative estimators: OLS, random and fixed effects estimators.