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AN EVALUATION OF TRAINING FOR THE UNEMPLOYED IN MEXICO

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An Evaluation of Training for the Unemployed in Mexico

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This working paper is one of several background reports to OVE's Labour Training Thematic Evaluation carried out during the 2005-2006 Ex-Post Evaluation Cycle.

The team of this Thematic Study was comprised of Pablo Ibarraran, David Rosas-Shady, and coordinated by Inder J. Ruprah. The findings and interpretations of the authors do not necessarily represent the views of the Inter-American Development Bank. The usual disclaimer applies. Correspondence to: Pablo Ibarraran, e-mail: pibarraran@iadb.org, Office of Evaluation and Oversight, Inter-American Development Bank, Stop B-750, 1300 New York Avenue, NW, Washington, D.C. 20577.

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INTRODUCTION

This paper summarizes the findings of an impact evaluation of the Mexican training programs PROBECAT_SICAT for the period 1999-2004. It is a study commissioned by the Office of Evaluation and Oversight of the Inter-American Development Bank in accordance to the Bank's policy of ex-post evaluation of operations.

The paper has five additional sections. Section 2 provides an account of the Mexican public policy towards the unemployed and a description of the institutional and operative capacity of this policy for the last two decades. In this section we explain the evolution of pro-cyclical behavior of unemployment and informal employment with respect to economic growth in Mexico. Moreover, we describe the origins and organization of labor public policy in Mexico and describe the evolution of the training for the unemployed programs PROBECAT/SICAT. Section three reviews the previous impact evaluations of the training programs for the unemployed. Section four includes a discussion of the methods we use for program evaluation and our main results. Section five, presents a succint cost-benefit analysis of the program, making use of the results of the impact evaluation. Finally, section six concludes with a summary of results.

I. INSTITUTIONAL ANALYSIS

A. Economic and Political Background

For a few years, before 1982, the Mexican economy and federal government benefited from high oil prices and the discovery of new oil reserves. The improvement in the economy was considered permanent by the government and thus run large deficits financed by foreign savings. Deficits were further encouraged by the low interest rates and abundance of capital in international financial markets. By 1981-82 there is a striking, and for the government, unexpected reversal of the international prices: there is a sharp fall in oil prices and a sharp increase in international interest rate. The impact on the Mexican public deficit is large. Mexico has at the time a fixed exchange rate system, which is highly inconsistent with large public deficits. Initially the government tries to sort the crisis out by nationalizing the banking system; this, however, did not avoid a terrible devaluation of the Mexican peso, and the capital flights typically associated with devaluation.

During the 80s Mexico was going to have its poorest performance of the 20th century in terms of economic growth. But the crisis did something good to the Mexican public policy: it changes it permanently for the better. Mexican economic policy would rely from that time until the present on four basic tenets - less government intervention in the economy, openness to international trade, a flexible exchange regime, and macroeconomic stability, as the mean to recover international credibility and economic growth.

The process by which this policy stance was adopted was gradual and painful. The first years were not easy at all for the population. The Constitution had to be modified in order to be able to adopt the new policy stance.[1] A way to generate consensus was to achieve political deals with the opposition political and social forces, the so-called "pactos". The *Pacto de Solidaridad Económica* of December 1987 and the *Pacto para la Estabilidad y el Crecimiento Económico*, of January 1989 are two good examples. These "pactos" were ratified many times during the following decade.

Despite the deep reforms of the previous decade, Mexico's macroeconomic performance could not be spared the shocks that hit many emerging markets economies in the second part of the 1990s. Actually the Mexican crisis of December 1994 was the first in a series that shocked the world economy for more

[1] Articles 25 and 26, which stablish the degree of government intervention in the economy and the role of the federal government in Mexico's development strategy.

than seven years. Perhaps the Mexican crisis was the least predictable of all them, because the economy's fundamentals seemed sound. A deep recession followed in 1995 from which the country recovered however very fast in 1996. GDP growth rates went back to the normal level of 4-6% between 1996 and 2000. From 2001 to 2005 economic growth slowed down considerably, however. The GDP growth rate halved. Inflation has been declining since then, as the monetary policy has become more predictable, and it is fully under control now. By the end of the period the biggest problem consists of regaining the GDP growth rate of 4-6 %.

1. GDP growth, Unemployment in Informality

Despite de inflexibility of labor contractual arrangements in Mexico, the unemployment rate is very sensitive to changes in the level of economic activity, as the following graphs show. Graph 1 shows the more recent evolution of the unemployment rate and the (Log) GDP of Mexico between 1996 and 2003. Between 1996 and 2000 the unemployment rate and GDP moved in opposite directions, as expected. After 2000, growth slowed down considerably and unemployment rate started to grow again. Therefore, it seems crucial for the level of employment that the economy grows steadily.

Graph 2 shows the annual rate of growth of GDP together with the absolute annual change in the unemployment rate for the period 1980 and 2005. The correlation coefficient between both series is -0.7, very significant. Although unemployment remains low on average, it changed quite dramatically in response to changes in the level of economic activity.

Graph 3 shows the schooling attainment of the Mexican labor force. In 2001, one quarter of working-age population was illiterate or had dropped-out from elementary school; half of the working-age population had elementary or junior high education; the other quarter had senior high school or college level education. In particular about 40 % of the working-age population had only elementary education or had dropped-out from elementary school. IN contrast, an increasing share of the unemployed has attained senior high school and university degrees. Only about a quarter of the unemployed have elementary or lower education, while these groups represent 45 % of the working-age population. So the groups with senior high school and university degrees are the ones more represented among the unemployed (see Graph 4).

Regarding the age distribution of the unemployed for the period 1999-2003, the age groups in the ranges of 12-19, 20-24, and 45 years-old or older have increased their participation in about 1, 4 and 2 percentage points, respectively.

These groups represent 60% of the unemployed in 2004. The groups in age groups 25-34 and 35-44 have reduced their participation in about 3.5 percentage points each. So, most of job seekers are either very young or already mature workers.

An idea of the dynamics of the labor market can be gained from the study of the reasons for which workers lose their jobs. We find that during the economic expansion of 1997-2000 about 40% of the unemployed left their jobs voluntarily in order to find a better one elsewhere; about 30 % were dismissed and another 20 % had their temporary jobs terminated. As economic growth slowed down in the early 2000s, the share of unemployed who had voluntarily quitted halved, but the share of those who had been dismissed or had their temporary job terminated remained stable. The duration of unemployment has remained stable for the period 2000-2004. About 60 % of job seekers remain unemployed for less than 4 weeks; 25 % have to wait between 1 and two months to find a job; the rest have to wait more than two months.

The previous analysis suggests that unemployment in itself does not seem to be a big problem in Mexico, at least no more than it is a problem in the rest of modern economies. The Mexican economy can grow and create employment opportunities; a high share of workers change jobs voluntarily during an expansion of economic activity; most do not remained unemployed for more than one or two months.

Mexican labor markets suffer, however, from informality and the lack of employment opportunities for the youth. A large share of the labor force works in the informal sector; the swings in the unemployment rate are associated with changes in the size of informal employment; most of job seekers are 25 years old or younger. This suggests that the main problem in the Mexican labor market is the problem to create formal jobs for the youth.

Informality in labor relations and arrangements affects between 40 % and 60% of the labor force employed in Mexico, depending on the way we measure informality. Economists tend to think that informality has to do with ill-conceived firm and labor legislation regarding the regulation and taxation of economic activity that ultimately hurts small and medium size firms and their workers' welfare.[2] Graph 5 shows the recent evolution of informality as measured by the share of the employed labor force not registered at the IMSS (*Instituto Mexicano de Seguro Social*). The graph reveals a small decline in

[2] Others think the informal sector is the consequence of lack of growth and supportive social policies. For more on informal labor markets in Latin America see, just two among the myriad of references, Loayza (1997) and IBERGOP (2005)

informality from 65 % to 60% of employment between 1996 and 2003 for all economic sectors.

Informality seems to decline with expansion in economic activity. Graph 6 shows the relation between the evolution of the GDP and the evolution of informality. The Mexican economy grew about 22 % between 1996 and 2000 and informality fell to 61% from 65 % in 1996. So there is a positive effect of economic growth on informality but this effect is small. Graph 7 graph shows that informality and unemployment dynamics are closely related to each other as well. The dynamics of informality are similar to those of the unemployment rate. Informality declines with employment. Since the unemployment rate is small, however, there is an obvious limit to use employment policies to curb the huge informality we observe in Mexican labor market.

In short, informality may involve between 40% and 60% of the Mexican labor force. When the economy is booming and the unemployment rate decreases informality also decreases, but the change in informality is very small. PROBECAT/SICAT by-laws require that firms involved in the mixed modality hire at last 70%-80% of workers at the end of the training period. Since SNE monitors and enforces this requirement, participating firms are most likely to belong to the formal sector of the economy. Therefore, firms in the informal sector are very unlikely to participate in the program.

2. Reforming the Mexican Labor Market

Reforming the Mexican labor market institutions and laws has been probably the most difficult part of the pro-market reform process started by the Mexican federal government in the mid 1980s.

The current labor legislation has been incapable of fostering labor productivity and the creation of enough formal employment opportunities. Among the obstacles to achieve these objectives are: the high costs of taking in and firing employees; a pro-worker paternalist legal framework; lack of alternative wage setting mechanisms, in particular mechanisms that take into account productivity gains; and excessive intervention of labor unions in wage setting mechanism, labor contracts, and firms' decisions regarding the role of human resources in production.

The Mexican labor unions, employers, and government representatives have indeed been discussing the need for a reform since the beginning of the 1990s; the discussions and negotiations have achieved, however, a small progress so far.

In 1995 a formal negotiation started between CTM (*Confederación de Trabajadores Mexicanos*) and COPARMEX (*Confederación Patronal de la República Mexicana*) under the arbitrage of STPS (*Secretaría de Trabajo y Previsión Social*). The parts involved recognized the need to reform labor markets and labor relations in order to "achieve full employment and promote workers productivity and firms competitiveness", and thus established a *Comisión Central* and a *Comité Técnico* that would institutionalized these negotiations and make sure that they yield tangible outcomes.

After a decade, however, nothing like a plan for a labor reform had emerged from these negotiations. Not only the Mexican labor legislation had become increasingly outdated relative to those of other Latin American countries, but also it had contributed to the growth of informal employment, mainly through of its narrow interpretation of job protection and stability.

With the aim of producing a breakthrough in the negotiations the Secretary of Labor established a Roundtable on the Labor Reform (*Mesa Central de Decisión sobre la Reforma Laboral*) with representatives from workers and employers in 2001. The Secretary of Labor established the Roundtable with the objective of fostering the discussion of keys aspects of the reform: 1) new contractual arrangements, that would allow for temporary positions and training; 2) hours worked, the number of which would result from a negotiation between the firm and each worker individually 3) incentives, that allow firms to promote labor productivity gains.

Well before the first year of talks had ended the workers' representatives abandoned the negotiations, and unilaterally announced a proposal, which they sought to get approved at the Congress with the support of some politicians. This initiative failed and workers' representatives eventually resumed their discussion activities at the table. By the end of year 2002 the groups represented reached an agreement that the political parties PRI, PAN and PVEM took to Congress for its discussion. The agreement, which contained proposals for a modification of the Federal Labor Law (*Ley Federal de Trabajo*) but left all constitutional provisions (in particular, article 123) unchanged, included provisions for temporary labor contracts, workers' choice of labor union, and special considerations to labor relations in Medium and Small Firms. Unfortunately the agreement did not reach the stage of a reform proposal due to the opposition of other political parties with seats at the Congress, and efforts to achieve a reform have been abandoned since then.

Meanwhile, the National Labor Policy Program for 2001-2006 (*Programa Nacional de Política Laboral 2001-2006*) has established five basic principles that would guide labor policies making: All interest groups should be included in the discussion of labor policies; reform should be gradual so that workers and firms can adjust to the changes; reform or innovation should be achieved through a consensus; actions by interest groups should be taken within the limits of the law; and labor relations should be framed within a peaceful environment.

Gradualism, consensus, and inclusion of all interest groups may indeed produce a peaceful and cooperative environment in which reforms and innovations can be discussed without jeopardizing labor relations; but it must be recognized that these labor policy guidelines might also delay the reform indefinitely.

The National Labor Policy Program for 2001-2006 has also set five objectives for their labor policies and five strategies through which these objectives would be achieved. The five declared objectives are: 1. To establish a labor culture in society; 2. To reform labor and other regulatory laws; 3. To modernize labor market institutions and government's role within them; 4. To modernize and democratize labor unions; 5. To help workers find a response to the challenges of globalization. The five declared strategies to achieve them are: I. Promotion of formal employment; II. Training and retraining of the labor force; III. Growth of firms' and workers' productivity; IV. Increase in the competitiveness of the Mexican economy; V.Increase in the welfare of workers and their families.

From our point of view, this National Labor Policy Program retains the paralyzing confusion between objectives and strategies that characterized previous national programs. From our perspective, of the five objectives listed above, items 2, 3 and 4 should be viewed as strategies rather than objectives; and of the five strategies listed above, perhaps only item II should be viewed as a strategy, the rest should clearly be viewed as objectives.

Thus, the establishment of a labor culture; the promotion of formal employment; the growth of firms' productivity and competitiveness; and the improvement in the living standard of workers and their families should be the objectives of the labor policy. The strategies to achieve them should be the reform of labor and other regulatory laws; the modernization and liberalization of labor and other market institutions; and the modernization and democratization of labor unions; and probably the design of new taxation schemes for business.

It is not clear at all whether the training and retraining of workers should be an objective or a strategy. In any case we believe it could be an strategy whose scope, timing, and financing should be entirely left to firms themselves, rather than a strategy orchestrated at large scale by the government as a surrogate for a better education system and labor market reform.

B. Institutional Capacity

Since at least the mid 1970s Mexico's federal government has followed active labor market policies and has consistently built institutional capacity to implement those policies.

The *Servicio Nacional de Empleo, Capacitación y Adiestramiento* (SNE) is established in 1978 as part of a reform to the Federal Labor Law (*Ley Federal del Trabajo)*. Its main objectives were to improve the matching between job seekers and potential employers, to improve the chances of the unemployed of finding a job, and to study the labor market in order to improve labor market policies.

During the years, which followed the sovereign debt, crisis of 1982 workers saw their real wages declined sharply due to the higher inflation rate and the fall in the demand for labor. Informal labor started to grow fast. In order to curb informality and improve the matching between job seekers and vacancies the government adopted an even more active labor market policy stance, which consisted of a strengthening of the SNE and the SNE policies and resources.

In 1984 Probecat, the training program for the unemployed is started and the SNE is the institution chosen to implement it. This was a logical consequence of the 1978 Federal Labor Law, which established workers training as an obligation for firms while simultaneously established the SNE.

In 1988 this policy is further strengthen with the launching of the *Programa de Calidad Integral para la Modernización* (CIMO) which provided training to employed workers in their own -small and medium sized- firms. Further innovation to the policy were introduced in 1993 with the launching of the *Sistema Normalizado y de Certificación de Comptencias Laborales* which sought to clearly establish the competencies of workers so that the training programs Probecat and CIMO could focus more efficiently on the abilities and knowledge that firms demanded from the workers.

The SNE is in charge of CIMO and it plays an important role in defining workers competencies. The SNE decides the way these programs are going to be implemented, the federal government sets the normative framework and provides the resources; the programs are then implemented in each Mexican state by the *Servicios Estatales de Empleo* (SEE), with the help of additional state funds.

The scope of activities and processes that the SNE must implement and monitor has thus grown considerably; SNE's infrastructure and resources have grown as well. The SNE started with a headquarters in Mexico City and only 5 branches across Mexico in 1978, by 2002 the number of offices has reached 139. This administrative organization is additionally supported by 77 units run by the SEE. About 2100 employees run the whole system, 920 at the Federal level and about 1180 at the state level.

Its budget has been growing as well. In 2002 the SNE spent 110 million pesos in programs associated with the matching of job seekers and potential employers and other activities of intermediation between workers and firms. It also spent more than 700 million pesos in the implementation of Probecat.

The SNE runs all the different types of training available for the unemployed through Probecat: the one offered through regular courses at technical and other schools (school training, now discontinued); the training offered at the firm (firm training, also called –"mix" training); the training aiming at helping those self-employed (self-employed training); the training for those involved in local initiatives of employment (local employment training)

We conclude that the institutional capacity to implement Probecat has, at least formally, been consistently built and sustained over the years. The question remains to whether a public institution like SNE, with a country- and economy-wide scale of operations is efficient at all. In particular, taking into account the mandatory nature of the training at firms and the need to regulate it and monitor it, it is difficult to determine whether the growth of SNE's institutional capacity is just inertial and a by-product of the mandate to train workers or it is the result of a carefully planned strategy.

C. Description of the Program

Probecat, an abbreviation for *Programa de Becas de Capacitación para Trabajadores Desempleados*, was launched in 1984 with the objective of providing assistance and training to the unemployed. In 2001 its name was changed to SICAT (*Sistema de Capacitación para el Trabajo*) and more recently changed again to Bécate (*Becas a la Capacitación para el Trabajo*).

The beneficiaries of the program receive a *scholarship* equivalent to a minimum salary while they take part in a three-month training course; about 4.75 million workers have been trained between 1984 and 2005. Graph 8 shows the evolution of the number of participants or *trainees*.

In the first 10 years of the program, 71 thousand workers were trained on average every year. The scale of operations increased dramatically after 1994; from 1995 to 2000, 530 thousand workers were trained on average every year. During the years 1999 and 2000, nearly 20% of unemployed workers received training in this program. The numbers of trainees has decreased steadily since then and the figures for 2005 are similar to those of the pre-1994 period.

The SNE (*Sistema Nacional de Empleo*) is the institution in charge of organizing and implementing the program with the aid of the regional offices of SEE (*Servicios Estatales de Empleo*). While the SEE decide the type of training activities to be offered as well as the capabilities and abilities that the trainees should developed during their training, the SNE is in charge of providing the funding for these activities. Funding channeled by the SNE covers the workers' scholarships and all the costs associated with the training activities.

The total amount of resources allocated to the program is shown in Graph 9. The evolution of resources allocated has had an evolution similar to that of the number of trainees, but the real expenditure per trainee has a negative trend, as Graph 10 shows.

In the beginning Probecat offered just one type of training program called "escolarizada"; that is – school-based training. The training consisted basically on spending the three months of training attending classes at a public school – sometimes the SEE would also hire ONGs to provide this type of training. When the training was over, workers would look for a job using the placement services available at the SNE and the SEE.

A few years later an on-the-job training modality was introduced. This type of training –the so-called "mixta" (that is "mixed") consisted on training done at the firm plant or workshop. The SNE paid for the workers scholarships; the SEE paid for the operative costs and the firm financed the training itself. After the training, 70 % or so of the trainees would be hired by the firm, the rest would try to find a job through the SNE placement offices.

There is a large difference between both types of training activities; while the "escolarizada" offered a general type of education, the "mixta" offered a specific type of training. It is not clear whether unemployed workers could choose between one of these two activities or if they were just assigned to them by SEE clerks. There is some evidence, however, that the SEE distinguished between workers with and without previous experience, between qualified and unqualified workers, and between temporary unemployed workers and self-employed informal workers.

The "escolarizada" type of training was dominant until 1998 when the "mixta" started to receive a larger share of the trainees. In 1994, other types of training were also established; the most important of them being the so-called training for the self-employed ("autoempleo"). Therefore, after 1994 the share of trainees allocated to the "escolarizada" type of training started to decline; the program was terminated in 2001. Since 2002 the "mixta" and "autoempleo" types dominate the training activities accounting for about 60 % and 30 % of the trainees, respectively (see Graph 11). For the period 1998-2005, 45 % of trainees in the "mixta" type received training at medium and large firms and 55 % in small firms.

As it was explained in the former section, the SNE mission is twofold: to manage Probecat and to serve as placement office for the unemployed. The training effort done in 1999 and 2000 is impressive but this was achieved at the expense of placement efficacy. Placement efficacy, measured by the ratio of vacancies filled by the SNE with unemployed workers to the number of unemployed workers trained by the SNE through Probecat, declines from 1997 to 1999 when it reaches its lowest value. As training effort decreases after 2000 placement efficacy starts to increase again. After 2002, both SNE's placement efficacy and training effort show a negative trend, however.

D. Operative Capacity

In several official documents we find that the purpose of Probecat, Mexico's training program for the unemployed, was to improve the matching between the suppliers of labor and their potential employers, to increase the employment probabilities and future wages of the unemployed and to improve the productivity and competitiveness of firms. Thus inefficient matching, high unemployment, informality, low wages and low productivity were considered a consequence of the low level of human capital in the Mexican labor force.

From these official documents it is clear that the program's targeted populations were those characterized by low levels of schooling, low wages, high unemployment, low share of qualified labor, high level of informality in the labor markets, and that Mexican states where the labor market indicators looked worse would required, all else constant, relatively more resources.

We thus conjecture that in order for Probecat to achieve its objectives the resources allocated to Servicios Estatales de Empleo (SEE) in each state should be in principle higher the worse the situation of the labor markets there.

By operative capacity of the SEE we mean the availability of enough resources to achieve the objectives of Probecat. That is, to reverse a particularly troublesome combination of low education, high unemployment and low income among the labor force -in particular among the unemployed. The operative capacity of the

SEE should be higher, that is, more resources should be allocated to it, the worse the situation in the state in terms of the indicators mentioned above.

In this section we explore whether the states' SEE were granted the operative capacity that they needed by analyzing the correlation between the resources they received per trainee and indicators of the labor force and labor market. As we shall see from this analysis, the budgets allocated to the SEE were unrelated or even worse, negatively related to the degree of deterioration of the labor force and labor markets in the state.

For instance, was the operative capacity associated with the level of unemployment across states? The budget allocated per trainee to each SSE seems at best weakly associated with the unemployment rate. Graph 13 shows that spending per trainee increased with the rate of unemployment across states in every year, but the correlation was negligible or very weak. The correlation improved, however, in the years 2001-2003.

Regarding the average years of education of the working-age population, Graph 14 shows that they are not associated with the budget per trainee allocated to each state in years 1998-2000, but are positively though weakly associated with those resources in the period 2001-2003. That is, contrary to the objectives of the program, states with higher average years of education received more resources per trainee than states with lower educated labor force.

Summarizing, operative capacity allocated to the SEE has been either unrelated or negatively related with the size of their needs. [3]

If the degree of backwardness in the labor markets facing the SEE was not the criterion followed to allocate the resources, what does explain their distribution across states? Graph 15 shows for the years 1998-2001 that spending per trainee has been driven basically by spending per training activity, that is, per training course; from which it follows that the average number of trainees taking each course has varied widely across states. The average spending per training activity ranged from 30 thousand pesos to 65 thousand in 1998 and 2000, and from 40 thousand to more than hundred thousand in 2001. This means that spending per trainee reflects differences in the quality of training courses offered across states and, as mentioned above, this quality in unrelated to the degree of backwardness of labor markets facing the SEE.

[3] Similar conclusions were found when relating spending to wages, informal employment and share of skilled worker per state.

The operative capacity could also be measured in terms of the total number of courses offered. As Graph 16 shows, this number increases with the total budget allocated to each SEE across states. The large heterogeneity across states in the total, per trainee and per course budgets and the way they related, or rather unrelated, to the degree of backwardness observed in the states' labor markets allow us to reconstruct or hypothesize the way in which the operative capacity of the SEE was determined.

Of course, a main reason behind the change in the sign of the relation between resources spent per trainee and the labor market conditions across Mexican states after 2001 is that some types of training have been discontinued. In 2001 the training through formal schooling was terminated and the so-called mixed was encouraged. The "mixed" training consists on training done at firms with the objective of training the unemployed in the specific abilities and knowledge useful to the firm. As the resources for Probecat were channeled to that purpose the relation across states between the operative capacity of the SEE and the degree of backwardness in the labor markets and labor force weakened even more, as states with better labor market indicators (that is, where most of the firms are located) received a larger budget per trainee and per course.

II. A REVIEW OF PREVIOUS EVALUATIONS

There has been a series of impact evaluations of the Mexican training program for the unemployed. Each study adopts different methods, databases and evaluates different outcomes. In this review, we emphasize only those issues that are comparable to our study.

The first analysis is by Revenga, Riboud and Tan (1994) who make use a retrospective database for beneficiaries of the 1992 cohort. They estimate a probit model in which the probability of employment three months after training depends on age, education, experience, unemployment duration, seasonal dummies and an indicator variable of program participation.[4] The authors find that participants have an 8-percentage points higher probability of finding a job than non-participants. Besides, they estimate an earnings equation corrected for selectivity and find that monthly earnings of male trainees are around 17% higher than male non-trainees, but not significantly different for females.[5]

The Mexican Ministry of Labor finishes a similar study shortly after Revenga's. STPS (1995) makes use of a similar database but for the 1993 cohort. They also estimate earnings equations corrected for selectivity and find positive effects of around 200 pesos a month for males, but no effect for females, with large benefits for those with experience and taking on-the-job training. They also find a positive impact on the probability of finding employment of around 20 percentage points, both for males and females, for those taking on-the-job training.[6]

Five years later, Wodon and Minowa (1999) criticize the previous studies on several grounds. They notice that using as controls a sample from ENEU with high probability of participating in the program induces contamination bias: that is, there may be observations in the control group that actually took the training. Also, the earnings equations correct for selectivity in taking the program but not for selectivity in participating in the labor market. Wodon and Minowa address these two issues by estimating a probability model of participating on one of the two modalities of training (i.e., on-the-job and school-based) using the ENEU

[4] This probit model has a selectivity correction not fully explained in the text. See Revenga, Riboud and Tan (1994), pages 262-266.
[5] The earnings equation in this case has experience, education and its interactions as explanatory variables. The program participation equation, not shown in the paper, controls for marital status, number of children, education and duration of unemployment.
[6] See STPS tables V.7 and V.10bis. The employment effects, in this case, were derived from a Cox hazard duration model.

and the ENCOPE surveys for the 1993 cohort.[7] Then they use the fitted index (not the fitted probability) as an instrument for program participation in a duration model and an earnings equation corrected by labor market participation. Their program participation models have an explicit exclusion restriction: number of program participants as a proportion of state population. They find a negative effect on wages for men who had school-based training, and no effect on women or other modality. They also find a positive effect on employment for women who had school based training.[8]

More recently, Calderón and Trejo (2001) also make use of the data for the 1993 cohort for a study that, for the first time, adopts propensity score matching for the evaluation.[9] The authors compute difference-in-difference for wages before and after training between controls and treatments selected according to a sort of nearest neighbor matching. They find that the program had a negative effect on hourly wages for men under every modality (around 35 cents/hour, that is less than 10%) and a positive effect for women under some modalities (similar size). They are also the first to estimate a model that assumes selection on unobservables, following the procedure proposed by Heckman, Tobias and Vytlacil (2003). With this methodology they find a larger negative effect on wages of 24%.

Finally, Navarro-Lozano (2002) uses the same 1993 cohort data and explores the Heckman et al. (2003, 2001) methods further. This author is the only one that contrasts different methods and parameters of interest. He compares the estimates of the treatment effect on the treated (ATT) from a non-parametric estimation using propensity score matching to a parametric estimation using selection correction methods. However, only wage effects for males are gauged in this study. He finds a positive wage effect of 10% when using the selection correction methods but a negative effect of –15% when using matching. In addition, Navarro-Lozano estimates the marginal treatment effect (MTE) and finds it indicates a positive selection (that is, those who benefit the most from the program are more likely to participate in it).

The most recent study is by Calderon-Madrid (2005) and is the only one that makes use of a more recent database. He computes the impact of the program on the probability of employment transitions (from unemployment to formal and

[7] We also make use of these samples, but for more recent years. A thorough explanation of these samples is in section 0

[8] No actual size of the effects in pesos or percentage points was provided in this paper.

[9] There is the study by Aportela (1999) but it only estimates the impact on unemployment duration. Since we are interested in comparing results in terms of probability of employment and wages, we do not comment this report.

informal employment) as well as on wages making use of data for year 2004. He finds that the beneficiaries of the SICAT program have higher probabilities of finding formal employment but lower probabilities of finding an informal job than comparable control individuals. On the other hand, he finds no robust evidence of a positive impact on wages. Making use of several matching procedures as well as panel and cross sectional data, he finds either no significant effect or significant effects that differ by method of estimation.

This literature review has two common strands. First, all the studies (with the exception of Calderón-Madrid, 2005) make use of a database more than ten years old and make use of a single year database. Second, results depend critically on methods used. Third, most studies, with the exception of Navarro-Lozano (2002) only measure the effect with the parameter known as ATT, that is average treatment effect on the treated. Our study aims at releasing the evaluation of PROBECAT-SICAT from these constraints. We make use of several databases spanning a five-year period (2000-2004), so a story of the evolution of the program impact can be obtained. Besides, we adopt two different methods of impact evaluation and compute several parameters of interest, which allows the study to report not only the robustness of average effects by different methods, but also to describe the selection mechanisms that underlie the program. As will be explained later, our methods allow us to discuss the existence of "hidden bias" in the estimates. Finally, we will report both the average treatment effect (ATE) and the average treatment effect on the treated (ATT), which allow us to discuss the selection mechanism of the program and infer whether the program attracts individuals that benefit the most from it.

Given the debate sparked in the literature on the methods for program evaluation, and the complaint of several authors on the methodological ambiguity of some studies, we proceed with a detailed explanation of the methods used in this study, and its justification. Furthermore, we add a **METHODOLOGICAL ANNEX**, where a detailed explanation of the procedures adopted is provided.

III. IMPACT EVALUATION

The outcome of a program or policy intervention upon individuals from a given population of size n, can be denoted by the variables:

$$Y_i^j \quad j = 0,1 \quad i = 1,2,\ldots,n$$

Here, Y_i^j stands for the outcome of a variable of interest for individual *"i"* under treatment (j=1) and not under treatment (j=0). However, individuals are not observed with and without treatment. Actually, individuals are either under treatment or not. The selection into the treatment can follow many diverse mechanisms (e.g. randomization, selection design, self-selection, etc). Hence, there is an indicator variable (D) that signals whether the observation corresponds to an individual under treatment or not:

$$D_i = \begin{cases} 0 & if \quad Y_i = Y_i^0 \\ 1 & if \quad Y_i = Y_i^1 \end{cases}$$

The evaluation of the impact of a program or policy intervention can be measured in different ways. We could be interested in knowing whether the difference between the outcome under treatment and the outcome without treatment is positive for every individual (formally, $Y_i^1 - Y_i^0 > 0 \quad \forall i$). Alternatively, we could be interested in comparing the distribution of these two variables or some statistical moment of their difference. The most common measure in program evaluations is the average difference in the variable of interest for the individuals with and without the program. This is known as the "average treatment effect" (ATE):

$$ATE = E\left[Y^1 - Y^0\right]$$

Another popular measure is the average treatment effect restricting the population to only those who were subject to the policy intervention under analysis. This is called the "average treatment effect on the treated" (ATT)[10]:

$$ATT = E\left[Y^1 - Y^0 \middle| D = 1\right]$$

[10] There are other parameters of interest for program evaluation such as the marginal treatment effect and the local treatment effect. See Heckman, Tobias and Vytlacil (2001) or Wooldridge (2002).

The comparison between the ATE and the ATT provides interesting information on the selection mechanism of the program. Notice that the ATE can be also defined as the weighted average of the effect on the treated and the effect on the untreated:

$$ATE = E_D\left[Y^1 - Y^0 \middle| D\right] = P(D=1)E\left[Y^1 - Y^0 \middle| D=1\right] + P(D=0)E\left[Y^1 - Y^0 \middle| D=0\right]$$

The difference between ATT and ATE is:

$$\begin{aligned} ATT - ATE &= \left[1 - P(D=1)\right]E\left[Y^1 - Y^0 \middle| D=1\right] - P(D=0)E\left[Y^1 - Y^0 \middle| D=0\right] \\ &= P(D=0)\left\{E\left[Y^1 - Y^0 \middle| D=1\right] - E\left[Y^1 - Y^0 \middle| D=0\right]\right\} \end{aligned}$$

Hence, since $P(D=0) \geq 0$,

$$ATT > ATE \quad \rightarrow \quad E\left[Y^1 - Y^0 \middle| D=1\right] > E\left[Y^1 - Y^0 \middle| D=0\right]$$

In other words, if the ATT is higher than the ATE that means that the impact among the treated is higher than the impact among the untreated, which can be called positive selection because the program is attracting those who benefit more from it. In the opposite case, i.e. ATT lower than ATE, we have negative selection because the program would have had a larger impact among the untreated than among the treated.

Intuitively, the difference between the average effect on the population and the average effect on the treated is a simple comparison between means. If the mean of a group is higher (lower) than the mean of the whole population, then such a group has a higher (lower) mean than the rest of the population.

However, since we observe neither the variable without the treatment for those participating nor the variable with treatment for those who did not participate (i.e., the counterfactuals), none of these parameters can be computed directly. Alternatively, what can be computed with the available data is:

$$E\left[Y^1 \middle| D=1\right] - E\left[Y^0 \middle| D=0\right]$$

that is, the difference between the average outcome among the treated minus the average outcome among the non-treated. The natural question to ask is whether this difference comes close to the ATE.[11]

[11] We concentrate in what follows on the ATE, but a similar argument can be made for the other parameters of interest.

If, given the joint distribution of Y^1, Y^0, and D, we assume that:

$$f\left(Y^j \middle| D\right) = f\left(Y^j\right) \quad j = 0,1$$

then the expectation of the outcomes conditional on participation to the program equals the unconditional expectation. This is known as the assumption of *"ignorability of treatment"* and it is a valid assumption when the program intervention is randomly assigned among the population. In this situation we have that:

$$\begin{aligned} E\left[Y^1 \middle| D=1\right] - E\left[Y^0 \middle| D=0\right] &= E\left[Y^1\right] - E\left[Y^0\right] + \left\{E\left[Y^1 \middle| D=1\right] - E\left[Y^1\right]\right\} - \left\{E\left[Y^0 \middle| D=0\right] - E\left[Y^0\right]\right\} \\ &= E\left[Y^1\right] - E\left[Y^0\right] = E\left[Y^1 - Y^0\right] = ATE \end{aligned}$$

Namely, the difference in the observed averages equals the average treatment effect. However, in most cases, treatment is not randomly assigned but there exist a selection mechanism. Let us further assume that there are other observable variables (vector X) and an unobservable variable (ε) that explain the selection mechanism and are jointly distributed with the variables Y^1, Y^0 and D. We may then assume that either there is selection on observables:

$$\begin{aligned} &f\left(Y^j \middle| D\right) \neq f\left(Y^j\right) \quad j = 0,1 \\ &\textit{but} \\ &f\left(Y^j \middle| D\right) = f\left(Y^j \middle| X\right) \quad j = 0,1 \end{aligned}$$

or selection on un-observables:

$$\begin{aligned} &f\left(Y^j \middle| D\right) \neq f\left(Y^j\right) \quad j = 0,1 \\ &\textit{but} \\ &f\left(Y^j \middle| D\right) = f\left(Y^j \middle| X, \varepsilon\right) \quad j = 0,1 \end{aligned}$$

Then, it can be shown that the difference in the observable averages is biased. Formally:

$$\begin{aligned} E\left[Y^1 \middle| D=1\right] - E\left[Y^0 \middle| D=0\right] = {} & E\left[Y^1\right] - E\left[Y^0\right] \\ & + \left\{\left(E\left[Y^1 \middle| X\right] - E\left[Y^1\right]\right) - \left(E\left[Y^0 \middle| X\right] - E\left[Y^0\right]\right)\right\} \\ & + \left\{\left(E\left[Y^1 \middle| X, \varepsilon\right] - E\left[Y^1 \middle| X\right]\right) - \left(E\left[Y^0 \middle| X, \varepsilon\right] - E\left[Y^0 \middle| X\right]\right)\right\} \end{aligned}$$

where the second term to the right is called the “overt bias” and the third term is the “hidden bias”. The former is due to the existence of a selection mechanism that can be explained with observable variables whereas the second needs unobservable variables to fully explain the participation in the program.

If we assume that there is selection only on observables (so there is no “hidden bias”) then the average treatment effect can be obtained from the average of conditional treatment effects. Formally:

$$ATE = E_X\left[E\left[Y^1 \middle| X\right] - E\left[Y^0 \middle| X\right]\right] = E_X\left[E\left[Y^1 - Y^0 \middle| X\right]\right] = E_X\left[ATE(X)\right]$$

On the other hand, if we assume that there is selection on unobservables then the ATE has to be obtained from average treatment effects conditioned both on observable and un-observable variables. Formally:

$$ATE = E_{X,\varepsilon}\left[E\left[Y^1 \middle| X,\varepsilon\right] - E\left[Y^0 \middle| X,\varepsilon\right]\right] = E_{X,\varepsilon}\left[E\left[Y^1 - Y^0 \middle| X,\varepsilon\right]\right] = E_{X,\varepsilon}\left[ATE(X,\varepsilon)\right]$$

Finally, the comparison between the estimates assuming selection on observables and the estimates assuming selection on unobservables provides information on the existence of hidden bias. Formally:

$$ATE(X,\varepsilon) - ATE(X) = E\left[Y^1 - Y^0 \middle| X,\varepsilon\right] - E\left[Y^1 - Y^0 \middle| X\right]$$

and hence

$$ATE(X,\varepsilon) \neq ATE(\varepsilon) \rightarrow E\left[Y^1 - Y^0 \middle| X,\varepsilon\right] \neq E\left[Y^1 - Y^0 \middle| X\right]$$

In other words, if the average effects differ between methods there is evidence of hidden bias (that is, unobservable variables are not ignorable). This is important because the size and sign of the hidden bias may alter the results of the impact evaluation as well as the assessment of the direction of the selection mechanism.

Intuitively, the presence of hidden bias warns about the existence of unobservable variables that influence the impact of the program. For instance, if participation in the program is affected by age, which is observable, and by work ethics, which is not observable, then computing the differential impact by age group is not enough for controlling the systematic differences between participants and non-participants. In this case, the evaluation might mistakenly report a weak impact of the program that is not because the program is ineffective but is the consequence of participants having a weaker work drive than non-participants.

A. Methods of Impact Evaluation

The exposition of the foregoing section makes it clear that a correct impact evaluation has to take into consideration the existence of selection bias and its components: overt and hidden bias. Methods of impact evaluation cling to assuming either one or both biases. Hence, methods can be divided into two categories: methods assuming selection-on-observables and methods assuming selection-on-unobservables.

Furthermore, since the parameters of interest are conditional expected values, two approaches can be adopted for estimation. First, a non-parametric approach that computes sample averages of the form:

$$\frac{\sum_i \left[Y_i^1 - \sum_j w(i,j) Y_{ji}^0 \right]}{N}$$

where w(i,j) is a function that assigns weights to each control observation j with respect to the treatment observation i, and N is the relevant number of observations.

Second, a parametric approach that assumes that conditional expectations can be modeled as functions (linear or non-linear) of the form:

$$Y_i^j = f\left(X_i, \beta, u_i\right) \quad j = 0,1$$

so

$$E\left[Y^j \middle| X\right] = f\left(X, \beta\right) \qquad j = 0,1$$

Therefore, the methods for impact evaluation can be classified into four categories, depending on assumptions about hidden and overt biases, and on the method for computing expectations. For this study we have chosen two opposite methods: first, propensity matching score with nearest neighbor controls, which is a non-parametric method assuming selection on observables and, second, selection correction, which is a parametric procedure assuming selection on unobservables. For the former we have adopted the methodology developed by Becker and Ichino (2002) based in the seminal work of Rosenbaum and Rubin (1983). For the latter we follow the methodology proposed by Heckman, Tobias and Vytlacil (2003).[12] We have chosen these methods for the sake of robustness

[12] The procedures implemented are extensively explained in the Methodological Annex, page 1.

and, as will be seen below, because comparing these two methods provides additional insights on the performance of the program under evaluation.[13]

B. Available Data

We make use of three different surveys in this study: the ENCOPE (Spanish acronym for Employment survey of PROBECAT/SICAT beneficiaries), the ENECE (Spanish acronym for National Training and Education Survey) and the ENEU (Spanish acronym for Urban Employment Survey). All of them are produced, with varying periodicity by the Mexican statistics bureau (INEGI).

The ENEU is a survey that provides information on human capital and labor force characteristics for the population aged 12 and more in cities with no less than 100.000 inhabitants. This survey is done every quarter since 1988. It has a rotation mechanism that allows identifying individuals for five consecutive quarters. It is important to clarify that each individual in the rotating panel is interviewed at a fixed span of 13 weeks. This is to say, for instance, that for a given year, if one individual was interviewed in the first week of January it will be re-interviewed in the first week of April, again in the first week of July, again in the first week of October and then, for the last time, in the first week of January of the following year. Every week of each quarter an approximately fixed number of individuals is interviewed until completion of the sample size for that quarter. This characteristic of the ENEU will become important since the data for the treatment group do not follow the same pattern.

The ENECE is a special module introduced in the ENEU every second year from 1991 to 1999, and every year since 2001. It provides socio-demographic information for individuals aged 12 and more as well as information on formal schooling and training. It provides individual data on number of courses, type of training, duration, place and sponsoring of training. Since the ENECE is just a module ENEU, information of training can be matched with all human capital and labor participation characteristics for sampled individuals.

Finally, ENCOPE is a survey that interviews a sample of PROBECAT-SICAT beneficiaries between three and six months after finishing their training. Although it has detailed information on type of course taken, socio-demographic characteristics and labor participation at the moment of the interview, it has

[13] For an extensive account of methods of program evaluation see Lee (2005), Cameron and Trivedi (2005) and Wooldridge (2001). For a discussion on evaluation methods applied to anti-poverty program see Ravallion (2005)

limited information on labor conditions during or before training.[14] It is important to mention that ENCOPE captures information of individuals at a point in time and ask the informant to recall information on several issues, which could be distant in time.

ENCOPE contains information on the labor market participation of the interviewed that is analogous to information collected from ENEU, which allows us to select individuals for the treatment and control groups with similar information. From ENCOPE we took as treatment observations those individuals that were unemployed at the moment of starting the program and completed the training course. From ENEU we took as control observations those individuals that were unemployed two weeks or less at the moment the treatment group was starting the training course.

The starting of the training program is a critical moment that what we call time *"T_o"*. We explicitly assume that the labor market experience of individuals in the treatment group before the starting of the program is the same to the experience of individuals in the control group. We call this experience *"clock 1"*. What we measure is the impact of PROBECAT using a second labor market experience clock that starts at *"T_o"*, what we call *"clock 2"*, by pairing recent unemployed from ENEU with those who take training from ENCOPE. Graph 17 shows how these two clocks work. On the horizontal axis we have time in weeks. On the vertical axis we have one measure of the expected impact on an outcome variable (for instance, probability of finding a job). At time *"T_o"* we have people in the treatment group starting the course and people in the control group just becoming unemployed (or with less than two weeks of unemployment). Our evaluation consists in measuring what happened to the treatment and control groups in *"T_o"*+13 weeks and/or "T_o"+26 weeks. In this illustration, training increased the probability of finding a job for those in the treatment group whereas those in the control group also experience a change in their probability of finding a job, seemingly lower.

This timing implies that unemployed individuals decide either to take training or to stay unemployed and search for a job. In this sense, the evaluation tries to measure which of these two strategies renders a higher benefit, in terms of employment and wages, for the unemployed. Other studies have gauged the impact of the program in terms of unemployment duration after training, but it is important to understand that taking a training course is a job-search strategy that may, or may not, be more successful than simply keep looking for a job as an

[14] Currently, this survey is quite different in terms of scope and available information from the surveys used for the previous evaluations, such as Revenga, Riboud and Tan (1994), Wodon and Minowa (1997), Calderán and Trejo (2001) and Navarro-Lozano(2002).

unemployed. Hence, comparing individuals with training and individuals without training, counting weeks of unemployment after the end of training is not the most correct comparison. Instead, we compare the probability of finding a job 13 or 26 weeks after a moment of unemployment (the moment *"T_o"*) between individuals who take training after that moment and individuals who do not.

As indicated above, ENCOPE provides information regarding the span of time between the date of the interview and *"T_o"* when the course was initiated. Table 1 shows for years 1999 to 2004 the number of observation selected from the ENCOPE. For instance, for year 1999, 613 individuals indicated that 26 weeks elapsed between the beginning of the course and the time of the interview. This table shows that the bulk of interviews were done 20-26 weeks after beginning the course. This information will be individual specific. For instance, for a person whose gap is 39 weeks, we would have information on its employment and wages for this whole period (particularly at 13, 26 and 39 weeks). However, for an individual whose interview is 13 weeks after beginning the training we will have labor market information only for that moment. As we will see, this issue is important because for the corresponding control we will have information depending on the rotation mechanism of the ENEU.

We will select control observations from ENEU, but need to deal before with two issues. First, ENEU contains individuals that may have taken a training course. This issue would contaminate the control group. In order to clean ENEU from this problem, we use data from ENECE to estimate the probability, for the unemployed, of participating in a training course. Those individuals with a probability higher than 0.5 were discarded from the control group (see estimates of this probit model in Annex 1). Second, the structure of ENEU implies re-interviews in a fixed period of time (13,26,39 and 52 weeks after the first interview). Consequently we will have labor market information for the controls at regular periods of time: 13, 26, 39, and 52 weeks.

Table 1 also shows for years 1999 to 2004 the number of individuals in the control and treatment groups. How do we use these observations? For year 2000, for instance, we have labor market information for 14685 persons interviewed in ENCOPE. From ENEU, however, we have labor market information for 3122 and 1839 individuals that were re-interviewed 13 and 26 weeks after training, respectively. Thus, we can use these 3122 and compare to the 14487 observations from ENCOPE that have information for their situation 13 weeks after beginning of training. Notice we dropped from the sample of treatments 198 individuals that were interviewed 12 weeks or less after beginning their training. For these we do not have labor market information at 13 weeks so these cannot be compared to the controls from ENU at 13 weeks. By the same token, we will compare the 1839 observations re-interviewed by ENEU 26 after their first interview, with 11384 from ENCOPE with at least 26 weeks after beginning their training.[15] A similar exercise was done for all the years. .[16]

Finally, the combination of treatments from ENCOPE and controls from matched ENEUs provides several datasets. The characteristics of these working databases are presented in Table 2. These datasets are then processed according to the program evaluation techniques explained in section 0 so as to make the treatments and controls fully comparable and the results are summarized in the next section.

C. Results

We apply the abovementioned methods to data for different years and groups. This allows us to test the robustness of the hypotheses on selection mechanisms. It also makes it possible to examine the evolution of the program impact over time. Finally it enables us to verify whether the program has differential impacts on various groups of beneficiaries.

It is important to highlight that we evaluate the impact on the probability of being employed 13 or 26 weeks after starting a training program. In addition, we evaluate the impact on the wage for those who actually have a job either 13 or 26 weeks after starting training. However, given the duality of the Mexican labor market, it is not reasonable to think that employment in the informal sector is an outcome equivalent to employment in the formal sector.[17] Furthermore, the PROBECAT/SICAT program has different modalities of training for those who seek a formal job and those who want to be self-employed. Consequently, our impact evaluation distinguishes employment and wage effects for those who took the mixed and school based modalities, on one side, and for those who took the other modalities, on the other side. For the former group, finding a job in the formal sector is a success, whereas being unemployed or having an informal job is a failure. For the latter group, having a job, either formal or informal, is a success. This separation allows us to take into consideration the quality of the job as well as the type of training for a stricter impact evaluation

[15] That is 14531 total observations from ENCOPE, less 3286 that were interviewed less than 26 weeks after the beginning of training.

[16] In addition, and in order to deal with the possible problem of measurement error in wages and hours, we drop the two lower and upper centiles for the monthly wage and keep only those individuals that worked between 35 and 61 hours per week.

[17] For more on the Mexican informal sector see section 0

Given the large array of results, we first explain the findings according to the non-parametric method that assumes selection on observables (section 1). Then, we explain the outcomes according to the parametric method that assumes selection on un-observables (section 2). We compare the results between methods in section 3 and derive insights on which method appears to give a better account of the evolution of the program. Finally, we add a section that specifically deals with the impact of the program for different program modalities and population groups.

1. According to non-parametric method assuming selection on observables

When the program to be evaluated was not implemented in a randomized way, one can resort to *quasi-experimental* methods to describe the impact of the program. In quasi-experimental designs, targets receiving the intervention are compared to a control group of potential targets that do not receive the intervention. To the extent that the latter resemble the intervention group on relevant characteristics and experiences, or can be statistically adjusted to resemble it, then program effects can be assessed with a reasonable degree of confidence[18].

One way to select *ex-post* the control group is by using the matching method. This technique is commonly applied in evaluation research and basically consists in finding a "twin" or "partner" to each one of the treated individuals. In matching design, the intervention group has already been specified. It is the evaluator's task to construct a control group by selecting targets unexposed to the intervention that match those in the intervention group on selected characteristics. The logic of this design requires that the groups be matched on any characteristics that would cause them to differ on the outcome of interest under conditions when neither of them received the intervention. To the extent that the matching falls short of equating the groups on characteristics that will influence the outcome, selection bias will be introduced into the resulting program effect estimate. For instance, if age is a key factor in affecting a given outcome--e.g., finding a job in three months for an unemployed person—to avoid bias, the matching of people receiving the treatment and not receiving it should be done considering, among other factors, the age of the person.[19]

Once the matching is done, we can then calculate the estimated gain from the program, following Becker and Ichino's (2002) protocol. Table 3 shows the

[18] However, the presence of unobserved characteristics that could be related to the outcome could posit a restriction to the usefulness of these methods.
[19] For a more detailed explanation of the procedure see our Methodological Annex in page 1.

estimated ATE and ATT for the probability of having a job after 13 and 26 weeks of starting training (i.e., after *"T_o"* in Graph 17) for years 1999-2004.[20] The first column shows the year analyzed, whereas columns 2 and 3 show the impact on the probability of having a job after 13 and 26 weeks after starting training respectively for the general case (i.e., without distinguishing between modalities of the program). Columns 4 and 5 show the same but for those who attended the classes for salaried positions, whereas columns 6 and 7 show the same for those that attended classes for self-employment positions. The upper panel shows the ATE and the lower panel the ATT.

When we analyze the impact without distinguishing between program modalities we observe that there is a somewhat positive trend. For ATE (i.e., the expected impact for a person selected at random from the population) the estimated impact after 13 weeks of finishing training changed from a negative –13.8 percentage points in 1999 to a positive impact of 3.5 in 2003 (see column 1, upper panel, Table 3), but a negative –6.3 in 2004. For the case of 26 weeks, the figures are 2.8 and -6.4 respectively (see column 2). For this latter case, the impact was in general higher than that for 13 weeks. Similar results were found when estimating the ATT (i.e., the estimated impact for a person that actually decided to take the training) as it is shown in the lower panel of the table. It should be pointed out that the results for ATE and for ATT at 13 weeks were positive in years 2002 and 2003 only (although not significantly different from zero) and at 26 weeks for all years (also not significantly positive either) but 2001.

We have also estimated the impact taking into account that there are different job qualities and modalities for the training. For the case of training the unemployed that seek a job as employee, following columns 3 and 4 we find that the ATE is positive and significant from 2002 onwards. The ATT for 13 weeks and 26 weeks is similar. For the modality of training for self-employment (see columns 5 and 6), results are in general positive both for ATE as well as for ATT, but significant only in some years with an irregular trend.

Table 4 shows the impact on monthly wages after 13 and 26 weeks of starting training for years 1999-2004. One striking result from this table is that all numbers that are statistically significant are negative. This means that, if an average person from the population took the training his expected wage would have been lower than if that average person had not taken the course (ATE results). Results are the same for those individuals that have actually taken the training (ATT results). The negative impact for ATE ranges from –291 pesos per month after 13 weeks of finishing training in 2003, to -1345 pesos per month

[20] The standard errors were estimated following the option *bootstrap r(att)* and *bootstrap r(ate)* from *psmatch2*.

after 26 weeks of finishing training for self-employment in year 2001. The negative impact for ATT ranges from -174 (statistically non-significant) to –1550 pesos for those that took training for self-employment in 2000. To put these numbers into context, the average impact of –232 pesos for 2001 (column 1 in Table 4) represents about 8% of the average monthly salary of a person in the respective control group. In turn, the highest expected loss of -1250 pesos for year 2000 (column 6) represents about 57% of the average monthly salary of a person in the relevant control group.

Obviously, these results of lower probabilities to find a job, for some years, and lower wages for trainees, almost always, are so contrary to what one would expect that they beg for an explanation. Before entering into it, the next section presents another way of calculating the impact of the PROBECAT-SICAT (control for un-observables) that will provide an important piece for this puzzle.

2. According to parametric method assuming selection on unobservables

As we explained in section 0, assuming selection on observables may lead to erroneous conclusions if there are unobservable variables that are important in explaining program participation and treatment effects. Following our previous example, if work ethics is important in explaining participation in the program such that those who participate have, on average, a higher work drive than those who do not participate, and such a work motivation also leads to a higher probability of finding a job, then not controlling for this unobserved variable may ascribe to the program effects that really correspond to the work effort of participants and not to the training. The problem then is how to control for un-observed variables.

Heckman, Tobias and Vytlacil (2001, 2003), propose a parametric method for dealing with the problem of selection on unobservables. Basically, it consists in running an econometric model for explaining the variable of interest (in our case, employment and wages) controlling for the usual observable variables (the same vector X of the previous section) and adding a variable that controls for the distribution of the unobservable variables. This distribution is assumed a priori and the validity of the procedure hinges on this assumption to be correct.

In general, the procedure follows four stages. First, obtain the parameters of a probit model on the decision to take the treatment; second, compute the appropriate correction for unobservables term; third, run separate outcome-specific regressions for the treatment and control groups with appropriate unobservables-correction terms obtained from the previous step; and fourth, given the parameters of these regressions, obtain point estimates for each observation and compute the ATE and ATT parameters according to specific formulas.[21]

Table 5 and Table 6 summarize the employment and wage effects, respectively, according to the parametric method assuming selection on un-observables. The employment effects for the treated (ATT) show a kind of an inverted-U trend for general employment both at 13 and 26 weeks after starting training. These trend, with mostly positive and significant values can be seen both for salaried and for self-employment. This inverted-U trend means that employment effects for years 2002 and/or 2003 are significantly positive and larger effects than previous and subsequent years.. With some exceptions, the employment effects according to selection on unobservables are larger than according to selection on observables.

The wage effects for salaried workers are mostly positive and significant. Oddly, years 2000 and 2004 show sizeable positive effects for salaried worker that do not recur but look very large (more than 1000 pesos): these would represent nearly two thirds of the monthly wage of a person in the respective control group. Wage effects for self-employed workers are usually positive and significant. The size of the positive and significant effects is also quite large (between 50% and 100% of the monthly wage of a person in the respective control group).

3. Comparison of results between methods

As it was explained in 0, comparing the ATE and the ATT provides information on the selection mechanism of the program. Actually, it provides information on whether the program is attracting those who benefit the most from it or whether it does the opposite. Second, comparing the ATE or the ATT between methods hints on whether there is a problem of hidden bias. Finally comparing results for each method over time, allows us to ascertain whether there is an impact of the program robust to methods of evaluation and data collection period.

From Table 7 to Table 10 we summarize the results of our estimations. These tables compile the employment and wage ATE and ATT, distinguishing two methods, for two types of workers: salaried and self-employed. One main conclusion can be derived from each table. The employment effect of the program on the treated (ATT) is significantly positive, according to both methods, for salaried as well as self-employed workers in most years considered (see Table 7). On the other hand, wage effects vary radically by method, as well as by period and type of worker (Table 8).

[21] Heckman, Tobias and Vytlacl (2001) develop the specific formulas for ATE and ATT under their procedure. For more on this, see our Methodological Annex in page 1.

With respect to employment effects, there are several regularities we want to highlight. First, it should be noted that, for salaried workers, the difference of estimates assuming selection on observables and assuming selection on unobservables declines from large and positive in 2000 to small and negative in 2004 (see Table 9 the row entitled “hidden bias”). This means that up to year 2002, there was an important “hidden bias” and, hence, assuming selection on observables could be misleading. An interpretation of this “hidden bias” could be that individuals who participate in the program exert, on average, a lower effort in looking for a job than individuals who do not participate. Therefore, when not controlling for this unobservable variable, the matching method is not taking into account that participants apply less effort (or some other unobservable variable that is associated with lower employment rates). Again, this hidden bias appears to decline from 2002 onwards and both methods show similar results in 2003 and 2004.

Second, the difference between the ATT and the ATE for salaried workers, according to both methods is mostly negative, but is usually larger in year 2001 or 2002 than in other years. These years represent important changes in the program. Particularly, the school-based modality was phased out and the mixed modality was enhanced (see Graph 11). Since a positive difference between ATT and ATE mean a positive selection mechanism (i.e., those with larger expected benefits from the program are also those with higher probability of entering the program), then it seems that the decline in the negative selection (observed under both methods) portrays and indication that modifications of the program induce a better targeting in the use of it. This is because the concentration of the program in the mixed modality (with its requirement that the firms hire 80% of the trainees) ought to be associated with an increasing employment impact (what we see in both methods) and better selection (i.e., those who would benefit most from it are more likely to select to it). The larger impacts of the program in years 2002, 2003 and 2004 can also be associated with the concentration on the mixed modality.

With respect to the self-employed, the effect on the treated (ATT) according to selection on unobservables varies from negative in years 2000 and 2001, to positive in 2002-2003 and negative again in 2004. These results are accompanied by a negative selection mechanism (see Table 9, lower panel). This seems to indicate that the self-employment and productive project modalities attract individuals who benefit less from the program (perhaps, those who find it very difficult to become self-employed by themselves) but occasionally helps them. A similar trend is observed according to selection on observables, but with mostly positive results. The trend of the hidden bias and the selection effect differs across methods and over time, so no clear pattern can be recognized.

The wage effects, as mentioned earlier, differ by method of estimation. Table 4 shows that wage effects on the treated (ATT) are negative for all workers every year when assuming selection on observables. On the other hand, these effects are usually positive if assuming otherwise (see Table 6). Table 10 shows that, in the case of salaried workers, there is a positive and large hidden bias. This hidden bias is often as large as the negative wage effect reported by selection on observables. Consequently, the wage effect on the treated for the salaried is generally positive and small (this is less than 100 pesos a month).[22] In the case of self-employed workers, the hidden bias varies in sign and size. Notwithstanding this, the wage effect on the treated is always positive but fluctuates in size wildly. Given the instability of results, it appears that the program does not have a robust and steady impact so its wage effects upon those with salaried employment or with self-employment are somehow haphazard.

4. Results by program modality and population group

We now proceed to describe the impact by training modality. Table 11 show the results for employment effects while and Table 12 do the same for wage effects.[23] The most important regularity with respect to employment effects is that on-the-job training programs in firms with more than 30 employees (known as "programa mixto" until 2003 and as "formación laboral en competencias" in 2004) always have the largest positive effects among all programs. On the other hand, the on-the-job training programs in small firms (less than 30 employees) have registered increasing effects, with negative effects until 2001 and positive effects since. The training programs for self-employment (known as "capacitación para el autoemplo") have had both positive (years 1999, 2003 and 2004) and negative effects (years 2002 to 2002). The once important school-based program was phased-out in 2002 after a declining performance that went from positive effects in 1999 to negative effects in 2001. These figures agree with our previous comments of the growing employment effect on salaried workers and an irregular effect on self-employment. The wage effects from selection on-observables show negative effects for every program for any modality all over the period. When assuming selection on unobservables, (see Table 12) all the wage effects turn to positive values (as was already documented in section 3).

Finally, Table 13 and Table 14, show the employment and wage effects on the treated by population groups according to gender, age, education, region and year

[22] Important exceptions to this are the bizarre positive wage effects of nearly 2,000 pesos a month for year 2000 (at 13 and 26 weeks) and 1,000 pesos a month for ear 2004 (at 26 weeks).

[23] All these effects are computed assuming selection on un-observables. Tables with effects assuming selection on unobservables are available upon request.

quarter. For salaried workers (Table 13) no regular pattern emerges for the whole period. However, if we concentrate in the years after 2001/2002, the groups of women, of those with more than junior high school and of those taking the course during the first quarter register always positive and larger employment effects. With respect to wages (Table 14), similar regularities are repeated for women and those with junior high school, but not for the other groups. For the self-employed, due to insufficient observations, many subgroups cannot be evaluated and no clear pattern can be described either for employment or wages.

IV. A COST BENEFIT ANALYSIS

For the cost-benefit analysis we use the estimated ATT from previous sections. As mentioned there, ATT figures may come from a very strict test on the program (i.e., having attained a job after training is considered a success only if that job is in the formal sector) or a less stringent measure of success would be to consider that *any* job gotten after training is a measure of success. A rationale for doing this is that the training program could provide general skills that help people in the job market.

The upper panel of Table 15 shows calculations taking into consideration results for 26 weeks considering unobservable characteristics of trainees. The expected wage before training is calculated as the probability of finding a job times the expected wage (column e=a*b), whereas the expected wage after training is calculated as the probability of finding a job after training times the expected wage (column f=c*e). Column g shows the estimated gain (+) or loss (-) per person/month attributed to the program. In column i we calculated the total per month gain (+) or loss(-) by multiplying the *per person* figure by the total number of participants (column i=g*h). Column j shows the numbers in millions of 2004 $.

All numbers in column j are positive, with the exception of year 2004 (due to the fall in the employment effect) which means that the program reports positive expected benefits to the beneficiaries. To calculate the total monthly gain/loss attributed to the program we subtract from them the average monthly budget of the program for each year (column l=j-k). It can be seen that the net estimated benefit of the program is positive only for years 2000 and 2002.. For 2002 the cost benefit analysis shows a gain of about 28 millions of 2004 pesos (about 2.5 million of US dollars *per month*). For 2003 and 2004, losses were in the order of 28 and 113 millions of 2004 pesos respectively (about 2.5 and 10.5 millions of US dollars *per month* respectively). Since it is difficult to assess any lasting effect of the program on the trainees without further information, we only state a monthly equivalent of the expected performance of the program.

Finally, the lower panel of the table shows cost benefit calculation after 26 weeks of the program by considering ATT figures from *observables* characteristics and for any type of employment. We have chosen these estimates to contrast results from the first panel and provide a range of results to look at. It can be seen that, for this case, the expected gross gain from the program was always negative (see column j in the lower panel), which in turns translate in losses between 9 and 19 millions of US dollars per month, this is because the wage effects under selection for observables are always negative, as it was explained in section 1.

In summary, we can see that, at the aggregate level, our calculations for the cost-benefit analysis of the program on any type of employment show negative net results almost for all years, with positive results was for 2000 and 2002 using our favorite method of selection on unobservables.

V. CONCLUSIONS

This report summarizes an impact evaluation of the PROBECAT-SICAT training program for the unemployed in Mexico. The study refers to the recent performance of the program because it makes use of several databases spanning the period 2000-2004. Besides, it adopts two renowned methods for impact evaluation. First, propensity score matching for non-parametric measures of average effects, following Becker and Ichino (2002). Second, parametric measures of average effects correcting for selectivity, following Heckman, Tobias and Vytlacil (2003). Hence, the study checks for robustness of the estimated parameters to the assumptions of selection on observables and selection on unobservables. It also contrasts the use of either parametric or non-parametric measures of the parameters of interest. Our results give credence to the existence of an important hidden bias but we show the estimates from both methods so that only robust results are reported.

The study provides evidence of a positive effect for salaried employment for most years and an irregular self-employment effect (sometimes positive, sometimes negative) according to both methods. It also finds evidence of small positive wage effects for salaried workers and positive (but of varying size) for self-employed workers according to the selection method. This effect contrasts with always negative wage effects according to the method of propensity score matching.

These effects (ATT) are accompanied by an important change in the selection mechanism of the program, due to the institutional changes adopted in year 2002. Since then, when the school-based modality was phased-out and on-the-job training in large firms required an even larger percentage of hires, the general and the salaried employment effects of the program became larger than in previous years. The self-employment effect, however, kept its negative selection character. This means that the participants in the program have a smaller or equal employment probability advantage than the non-participants. The employment effect for the self-employed has both positive and negative impacts depending on the method and the year of analysis.

All these methodological elements lead us to conclude that the program has a robust positive employment effect, particularly since 2002, under both methods and for all types of employment. However, because of the existence of an important hidden bias, the effects measured by methods that assume selection on unobservables are different (usually larger) than if measured by methods that assume selection on observables. Furthermore, also because of hidden bias, wage effects according to selection on unobservables are small and positive, which seems more likely than the negative effects usually reported by methods that assume selection on observables.

Our results confirm the positive salaried employment effect found by Calderón and Trejo (2002) as well as by Navarro-Lozano (2001). Besides, our wage effects are sometimes much smaller and sometimes much larger that Sanchez-Navarro's perhaps because we separate between salaried and self-employed workers. The former have small positive effects but the latter have both large and small positive effects. Our results also coincide with Calderón (2005) findings of positive effects on salaried employment but we find also a positive effect on self-employment, which he does not.

In contrast with all the previous literature, we perform an inter-period analysis using two alternative methods and evaluate the impact for all modalities of the program and different population groups. Several conclusions can be driven from this effort. First there is evidence of an important hidden bias so selection on observables alone can be a misleading assumption for gauging treatment effects. Second there is also evidence that the program underwent important changes in 2002 that affected its selection mechanisms. This led to make on-the-job training modality in large firms the most effective program, almost by construction. Third, women, those with junior high school and those taking courses during the first quarter of the year appear to be the groups most benefited by the program, particularly since 2002.

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TABLES

Table 1: Weekly distribution of observations by group

GAP*	1999			2000			2001			2002			2003			2004		
	Treatment	Control		Treatment	Control		Treatment	Control		Treatment	Control		Treatment	Control		Treatment	Control	
		13 weeks	26 weeks		13 weeks	26 weeks		13 weeks	26 weeks		13 weeks	26 weeks		13 weeks	26 weeks		13 weeks	26 weeks
10 week or less	1	0	0	183	0	0	4	0	0	0	5	0	0	4	0	0	0	0
11	0	0	0	5	0	0	0	0	0	0	2	0	0	0	0	0	0	0
12	5	0	0	10	0	0	0	0	0	0	5	0	0	1	0	0	0	0
13	0	2863	0	50	3122	0	0	3053	0	0	1,805	0	1	2,369	0	0	2,036	0
14	1	0	0	5	0	0	0	0	0	0	3	0	0	0	0	0	1	0
15	0	0	0	2	0	0	0	0	0	0	0	0	0	0	1	0	0	0
16	1	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0
17	0	0	0	40	0	0	0	0	0	0	0	0	9	0	0	0	0	0
18	8	0	0	116	0	0	0	0	0	0	0	0	6	0	0	0	0	0
19	36	0	0	116	0	0	2	0	0	0	0	0	11	0	0	17	0	0
20	62	0	0	162	0	0	3	0	0	0	0	4	7	0	3	4	0	0
21	91	0	0	211	0	0	17	0	0	0	0	0	50	0	6	26	0	0
22	145	0	0	380	0	0	17	0	0	0	0	0	34	0	3	38	0	0
23	231	0	0	466	0	0	33	0	0	0	0	2	39	0	0	32	0	0
24	285	0	0	659	0	0	46	0	0	0	0	2	41	0	0	110	0	0
25	470	0	0	896	0	0	49	0	0	0	0	5	81	0	0	19	0	0
26	613	0	1854	812	0	1839	81	0	2104	88	0	1,240	142	0	946	182	0	892
27	573	0	0	858	0	0	83	0	0	63	0	1	196	0	0	210	0	0
28	617	0	0	998	0	0	127	0	0	22	0	0	84	0	0	223	0	0
29	656	0	0	1008	0	0	173	0	0	38	0	0	150	0	0	162	0	0
30	709	0	0	1200	0	0	194	0	0	42	0	0	105	0	0	82	0	0
31	630	0	0	1247	0	0	158	0	0	122	0	0	75	0	0	107	0	0
32	702	0	0	999	0	0	250	0	0	138	0	0	108	0	0	227	0	0
33	501	0	0	874	0	0	238	0	0	203	0	0	198	0	0	30	0	0
34	517	0	0	754	0	0	266	0	0	37	0	0	165	0	0	61	0	0
35	350	0	0	620	0	0	139	0	0	96	0	0	272	0	0	45	0	0
36	338	0	0	487	0	0	128	0	0	204	0	0	172	0	0	70	0	0
37	211	0	0	362	0	0	263	0	0	228	0	0	161	0	0	320	0	0
38	147	0	0	329	0	0	123	0	0	149	0	0	67	0	0	290	0	0
39	153	0	0	239	0	0	65	0	0	109	0	0	242	0	0	75	0	0
40	130	0	0	212	0	0	31	0	0	83	0	0	42	0	0	31	0	0
41	35	0	0	127	0	0	31	0	0	41	0	0	66	0	0	58	0	0
42	75	0	0	59	0	0	11	0	0	86	0	0	49	0	0	8	0	0
43	41	0	0	49	0	0	0	0	0	71	0	0	63	0	0	27	0	0
44	10	0	0	36	0	0	4	0	0	67	0	0	17	0	0	1	0	0
45	0	0	0	10	0	0	0	0	0	31	0	0	23	0	0	3	0	0
46	5	0	0	5	0	0	0	0	0	13	0	0	33	0	0	9	0	0
47	0	0	0	25	0	0	0	0	0	0	0	0	19	0	0	0	0	0
48	6	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0
49	0	0	0	34	0	0	0	0	0	0	0	0	0	0	0	0	0	0
50	0	0	0	13	0	0	0	0	0	0	0	0	0	0	0	0	0	0
51	0	0	0	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0
more than 52 weeks	4	0	0	23	0	0	2546	0	0	0	0	0	0	0	0	0	0	0
Total	**8359**	**2863**	**1854**	**14685**	**3122**	**1839**	**5083**	**3053**	**2104**	**1931**	**1820**	**1254**	**2728**	**2374**	**959**	**2467**	**2037**	**892**

Table 2: Descriptive statistics from selected observations from ENEU and ENCOPE

	1999		2000		2001		2002		2003		2004	
	T	C	T	C	T	C	T	C	T	C	T	C
Gender												
Man	36.5	60.4	29.8	58.9	31.0	64.3	32.8	67.3	37.2	62.0	29.1	60.1
Woman	63.5	39.6	70.2	41.1	69.0	35.7	67.2	32.7	62.8	38.0	70.9	39.9
Kinship												
Household head	18.1	23.6	12.7	23.7	16.3	25.8	16.1	25.8	18.9	25.2	16.8	24.1
Spouse	31.6	8.8	39.0	9.6	31.7	6.5	28.9	7.4	21.4	8.9	26.3	9.0
Son/Daughter	46.5	58.5	45.1	57.4	44.2	59.5	44.2	58.7	49.9	58.4	47.5	58.9
Other	3.8	9.1	3.2	9.3	7.7	8.2	10.7	8.1	9.8	7.5	9.4	8.1
Marital Status												
Married	46.0	33.1	48.1	32.9	46.6	33.3	45.9	33.4	38.4	33.8	44.1	32.8
Single	50.0	62.2	48.1	60.9	48.9	62.1	50.3	61.3	57.0	62.5	51.2	63.0
Without couple	3.5	4.7	3.8	6.2	4.5	4.6	3.8	5.3	4.7	3.8	4.7	4.1
Age												
12 to 15	0.1	2.8	0.2	2.6	0.1	2.3	0.1	3.6	0.2	3.5	0.0	4.0
16 to 25	53.8	53.6	51.0	54.3	53.5	52.7	58.4	53.8	53.1	55.0	48.7	54.4
26 to 35	25.2	23.5	26.2	22.9	24.7	24.6	23.9	21.5	29.0	21.3	30.6	20.7
36 to 45	14.1	12.2	16.1	11.8	13.6	11.7	11.0	12.4	12.3	12.7	13.0	12.7
46 to 55	5.5	4.8	5.9	6.0	6.0	6.2	5.1	5.9	3.9	4.9	5.5	5.9
56 or more	1.2	3.1	0.6	2.4	2.2	2.6	1.5	2.9	1.5	2.6	2.2	2.2
Schooling												
No schooling	2.0	2.3	3.1	2.1	1.3	1.8	1.0	1.8	1.1	1.9	1.4	2.2
Elementary School	28.4	22.3	27.3	19.6	23.8	19.8	24.7	21.4	14.7	21.1	17.3	19.7
Junio High School	40.9	30.7	43.7	34.1	38.6	31.4	39.0	33.8	31.3	34.9	26.4	32.8
High School	22.3	23.4	21.0	23.6	27.3	24.9	25.8	24.0	28.1	22.3	26.8	26.5
Graduate School	3.8	21.0	0.8	20.2	8.7	21.8	9.5	18.7	23.8	19.6	28.1	18.6
Region												
North	13.2	32.0	30.6	31.4	20.4	32.1	18.4	34.1	21.1	24.5	17.6	18.4
Gulf	22.3	13.2	19.7	14.9	15.5	15.0	14.6	15.6	9.4	14.3	10.4	11.5
Pacific	11.6	12.3	2.7	13.5	14.1	10.6	14.7	11.1	13.9	14.5	11.1	15.5
South	13.3	7.0	5.1	6.8	10.6	6.3	15.5	6.0	11.0	5.2	7.1	7.2
Center-North	17.2	17.7	24.0	18.6	20.3	22.0	22.3	16.7	14.3	22.6	20.7	28.6
Center	12.8	9.0	5.2	8.6	11.7	7.4	7.8	10.0	10.0	9.6	8.4	8.0
Capital	9.7	8.8	12.6	6.2	7.3	6.7	6.8	6.5	20.2	9.3	24.7	10.9
Labor market												
Prior Experience	54.7	86.7	44.9	86.3	48.1	89.0	58.9	88.7	71.7	87.8	62.6	86.8
Employed (+26)	58.0	60.2	39.6	58.8	44.0	60.7	58.4	62.8	49.9	60.3	40.7	57.8
Formal Employed (+26)	0.0	27.0	19.2	28.6	15.9	28.8	34.6	27.4	31.1	26.0	20.6	19.6

Source:
selected observations from ENEU and ENCOPE databases (see section 4.2)
Notes:
T refers to treatment observations (from ENCOPE) and C refers to control observations (from ENEU)

Table 3: Employment effects according to selection on observables

PROBECAT-SICAT impact on probability of employment, in percentage points
Matching selection on observables

Year	All cases		salaried		Non-salaried	
	13 weeks (1)	26 Weks (2)	13 weeks (3)	26 Weks (4)	13 weeks (5)	26 Weks (6)
	Average Treatment effect (ATE)					
1999	-13.8 ***	2.8			7.5	4.9
2000	-10.6 ***	-0.9	-1.2 ***	0.2	6.1 ***	8.5 ***
2001	-14.8 ***	-12.9 ***	-6.1	-4.2	5.5 *	4.6 *
2002	3.5	3.4	29.6 ***	21.9 ***	19.9 **	24.1 ***
2003	3.5	2.0	26.8 ***	15.3 ***	10.9	6.9
2004	-6.3 **	-6.4	16.5 ***	17.6 ***	-4.3 ***	-1.4
	Average Treatment effect on the treated (ATT)					
1999	-18.1 ***	0.2			6.5 **	6.6
2000	-6.6 **	2.1	-0.5	0.0	6.1 **	10.4 ***
2001	-14.8 ***	-11.7 **	3.1	-3.7	7.8 **	-1.8
2002	0.8	-3.1	24.5 ***	11.0	13.6 **	16.1 **
2003	0.8	0.1	17.2 ***	9.8 *	3.9	14.7 ***
2004	-8.7 *	-10.2	4.7	14.9 ***	1.6	1.6

Source:
Authors' calculations using PROBECAT, ENEU and ENECE databases

Table 4: Wage effects according to selection on observables

PROBECAT-SICAT impact on probability of employment, in pesos per month
Matching selection on observables

Year	All cases		salaried		Non-salaried	
	13 weeks (1)	26 Weks (2)	13 weeks (3)	26 Weks (4)	13 weeks (5)	26 Weks (6)
	Average Treatment effect (ATE)					
1999						
2000	-515 ***	-717 ***	-613 ***	-663 ***	-1250 ***	-900 ***
2001	-232 ***	-226 **	-361 ***	-463 ***	-755 *	-1345 ***
2002	-347 ***	-432 ***	-359 ***	-458 ***	-689	-1075 *
2003	-291 ***	-317 **	-853 ***	-761 ***	-1197 **	NA
2004	-266 *	-348	-943 ***	-464 **	NA	NA
	Average Treatment effect on the treated (ATT)					
1999						
2000	-427 ***	-744 ***	-446 ***	-660 ***	-1550 ***	-913 ***
2001	-339 ***	-392 ***	-546 ***	-552 **	-671	-444
2002	-341 ***	-622 ***	-323 **	-523 **	-1514	-925
2003	-174	-308	-1027 ***	-808 **	-648	NA
2004	-230	-580 *	-1188 ***	-557 *	NA	NA

Source:
Authors' calculations using PROBECAT, ENEU and ENECE databases

Table 5: Employment effects according to selection on un-observables

PROBECAT-SICAT impact on probability of employment, in percentage points
Regression Controlling for unobservables

	All cases		salaried		Non-salaried	
Year	13 weeks (1)	26 Weks (2)	13 weeks (3)	26 Weks (4)	13 weeks (5)	26 Weks (6)
	Average Treatment effect (ATE)					
1999	13.6 ***	6.7 ***			9.1 ***	11.2 ***
2000	-18.5 ***	6.7 ***	11.4 ***	25.7 ***	-2.4 ***	-10.7 ***
2001	6.7 ***	22.6 ***	8.1 ***	23.3 ***	0.2	8.2 ***
2002	19.6 ***	27.6 ***	24.5 ***	39.6 ***	27.6 ***	36.0 ***
2003	2.5 ***	3.6 ***	25.8 ***	7.2 ***	67.1 ***	49.7 ***
2004	-14.9 ***	-15.4 ***	2.9 ***	12.1 ***	9.3 ***	6.2 ***
	Average Treatment effect on the treated (ATT)					
1999	6.0 ***	2.6 ***			6.3 ***	10.0 ***
2000	-18.8 ***	6.4 ***	3.2 ***	18.2 ***	-5.9 ***	-14.8 ***
2001	-8.7 ***	9.8 ***	-6.4 ***	6.4 ***	-10.9 ***	-1.9 ***
2002	12.6 ***	16.6 ***	20.1 ***	23.9 ***	12.3 ***	16.4 ***
2003	-0.3	0.9 ***	14.5 ***	3.2 ***	8.3 ***	11.9 ***
2004	-13.0 ***	-13.5 ***	3.1 ***	11.4 ***	-1.8 ***	-4.0 ***

Source:

Authors' calculations using PROBECAT, ENEU and ENECE databases

Table 6: Wage effects according to selection on un-observables

PROBECAT-SICAT impact on probability of employment, in pesos per month
Regression Controlling for unobservables

	All cases		salaried		Non-salaried	
Year	13 weeks (1)	26 Weks (2)	13 weeks (3)	26 Weks (4)	13 weeks (5)	26 Weks (6)
	Average Treatment effect (ATE)					
1999						
2000	131 ***	395 ***	1124 ***	1684 ***	506 ***	142 ***
2001	-72 ***	-125 ***	-45 ***	-50 ***	-499	35
2002	54 ***	-13	-14	-13	690 ***	-1741 ***
2003	2	151 ***	25	250 ***	-1225 ***	-315
2004	-21	-16	-54 *	1304 ***	251	-627
	Average Treatment effect on the treated (ATT)					
1999						
2000	223 ***	487 ***	1323 ***	2105 ***	299 ***	132 ***
2001	-55 ***	-43 ***	11	21 *	58	285 ***
2002	107 ***	33 **	27	53 **	1116 ***	87
2003	59 ***	204 ***	66 ***	291 ***	519 ***	588 ***
2004	114 ***	47 *	89 ***	1218 ***	441 ***	424 ***

Source:

Authors' calculations using PROBECAT, ENEU and ENECE databases

Table 7: Summary of Employment effects by type of employment

EMPLOYMENT EFFECTS BY DIFFERENT METHODS
percentage points (bootstrapped standard errors in parentheses)

	1999		2000		2001		2002		2003		2004	
weeks after starting training:	**13**	**26**	**13**	**26**	**13**	**26**	**13**	**26**	**13**	**26**	**13**	**26**
AVERAGE TREATMENT EFFECT (ATE)												
Assuming selection-on-observables *(non-parametric)*												
salaried	NA	NA	-1.2	0.2	-6.1 ***	-4.2	29.6 ***	21.9 ***	26.8 ***	15.3 ***	16.5 ***	17.6 ***
			(1.6)	(2.2)	(1.7)	(2.9)	(3.5)	(4.5)	(2.8)	(3.8)	(3.6)	(3.8)
self-employed	7.5	4.9	6.1 ***	8.5 ***	5.5 *	4.6 *	19.9 **	24.1 ***	10.9	6.9	-4.3 ***	-1.4
	(5.2)	(4.8)	(1.9)	(1.4)	(3.1)	(2.8)	(8.5)	(7.0)	(11.2)	(5.7)	(1.6)	(3.8)
Assuming selection-on-unobservables *(parametric)*												
salaried	NA	NA	11.4 ***	25.7 ***	8.1 ***	23.3 ***	24.5 ***	39.6 ***	25.8 ***	7.2 ***	2.9 ***	12.1 ***
			(0.1)	(0.1)	(0.4)	(0.3)	(0.2)	(0.3)	(0.3)	(0.3)	(0.2)	(0.3)
self-employed	9.1 ***	11.2 ***	-2.4 ***	-10.7 ***	0.2	8.2 ***	27.6 ***	36.0 ***	67.1 ***	49.7 ***	9.3 ***	6.2 ***
	(0.2)	(0.2)	(0.2)	(0.3)	(0.5)	(0.5)	(0.4)	(0.6)	(0.5)	(0.8)	(0.4)	(0.6)
TREATMENT EFFECT ON THE TREATED (TT)												
Assuming selection-on-observables *(non-parametric)*												
salaried	NA	NA	-0.5	0.04	3.1	-3.7	24.53 ***	11.02	17.2 ***	9.8 *	4.7	14.9 ***
			(2.3)	(2.6)	(3.2)	(3.7)	(5.5)	(6.9)	(3.4)	(5.1)	(5.3)	(5.7)
self-employed	6.5 **	6.6	6.1 **	10.4 ***	7.8 **	-1.8	13.6 **	16.1 **	3.9	14.7 ***	1.6	1.6
	(3.0)	(4.1)	(3.0)	(1.6)	(3.9)	(2.3)	(5.6)	(6.6)	(6.5)	(5.1)	(3.8)	(7.9)
Assuming selection-on-unobservables *(parametric)*												
salaried	NA	NA	3.2 ***	18.2 ***	-6.4 ***	6.4 ***	20.1 ***	23.9 ***	14.5 ***	3.2 ***	3.1 ***	11.4 ***
			(0.1)	(0.1)	(0.3)	(0.4)	(0.3)	(0.3)	(0.4)	(0.4)	(0.4)	(0.3)
self-employed	6.3 ***	10.0 ***	-5.9 ***	-14.8 ***	-10.9 ***	-1.9 ***	12.3 ***	16.4 ***	8.3 ***	11.9 ***	-1.8 ***	-4.0 ***
	(0.2)	(0.3)	(0.2)	(0.3)	(0.5)	(0.6)	(0.6)	(0.7)	(0.8)	(0.7)	(0.4)	(0.7)

Source: Authors' calculations using PROBECAT, ENEU and ENECE databases

Table 8: Summary of Wage effects by type of employment

WAGE EFFECTS BY DIFFERENT
monthly current pesos (bootstrapped standard errors in

	1999		2000		2001		2002		2003		2004	
weeks after starting	13	26	13	26	13	26	13	26	13	26	13	26
AVERAGE TREATMENT EFFECT												
Assuming selection-on-												
(non-parametric)												
salaried	NA	NA	-613 ***	-663 ***	-361 ***	-463 ***	-359 ***	-458 ***	-853 ***	-761 ***	-943 ***	-464
			(112)	(133)	(122)	(164)	(107)	(160)	(195)	(245)	(223)	(212)
self-employed	NA	NA	-1250 ***	-900 ***	-755 *	-1345 ***	-689	-1075 *	-1197 **	NA	NA	N
			(238)	(219)	402	(420)	(785)	(594)	(520)			
Assuming selection-on-												
(parametric)												
salaried	NA	NA	1124 ***	1684 ***	-45 ***	-50 ***	-14	-13	25	250 ***	-54 *	1304
			(24)	(28)	(9)	(13)	(16)	(19)	(19)	(28)	(31)	(74)
self-employed	NA	NA	-51	-112 ***	-824 ***	-782 ***	690 ***	-1741 ***	-1225 ***	-315 **	251 **	-627
			(39)	(26)	(65)	(75)	(93)	(98)	(133)	(129)	(125)	(142)
TREATMENT EFFECT ON THE TREATED												
Assuming selection-on-												
(non-parametric)												
salaried	NA	NA	-446 ***	-660 ***	-546 ***	-552 **	-323 **	-523 **	-1027 ***	-808 **	-1188 ***	-557
			(121)	(149)	(170)	(222)	(154)	(243)	(248)	(373)	(338)	(307)
self-employed	NA	NA	-1550 ***	-913 ***	-671	-444	-1514	-925	-648	NA	NA	N
			(355)	(330)	(657)	(685)	(1048)	(901)	(832)			
Assuming selection-on-												
(parametric)												
salaried	NA	NA	1323 ***	2105 ***	11	21 *	27	53 **	66 ***	291 ***	89 ***	1218
			(24)	(29)	(10)	(12)	(17)	(21)	(19)	(30)	(28)	(88)
self-employed	NA	NA	299 ***	132 ***	58	285 ***	1116 ***	87	519 ***	588 ***	441 ***	424
			(23)	(17)	(58)	(52)	(118)	(98)	(92)	(123)	(75)	(75)

Source: Authors' calculations using PROBECAT, ENEU and ENECE

Table 9: Evolution of hidden bias and sign of selection for employment effects

Salaried workers

employment effect (26

	2000			2001			2002			2003			2004		
	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)
Obs	0.2	0.0	-0.2	-4.2	-3.7	0.5	22.0	11.0	-10.9	15.3	9.8	-5.5	17.6	14.9	-2.7
Non-obs	25.7	18.2	-7.6	23.3	6.4	-16.9	39.6	23.9	-15.7	7.2	3.2	-4.0	12.1	11.4	-0.7
hidden bias(1)	25.5	18.1	-7.4	27.4	10.1	-17.3	17.7	12.9	-4.8	-8.1	-6.6	1.5	-5.5	-3.5	2.0

employment effect (13

	2000			2001			2002			2003			2004		
	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)
Obs	-1.2	-0.5	0.7	-6.1	3.1	9.2	29.6	24.5	-5.0	26.8	17.2	-9.6	16.5	4.7	-11.8
Non-obs	11.4	3.2	-8.2	8.1	-6.4	-14.5	24.5	20.1	-4.4	25.8	14.5	-11.3	2.9	3.1	0.2
hidden bias(1)	12.6	3.7	-8.9	14.2	-9.5	-23.7	-5.1	-4.4	0.6	-1.0	-2.7	-1.7	-13.6	-1.6	12.0

Self-employed workers

employment effect (26

	2000			2001			2002			2003			2004		
	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)
Obs	8.5	10.4	1.9	4.6	-1.8	-6.4	24.1	16.1	-8.0	6.9	14.7	7.8	-1.4	1.6	3.0
Non-obs	-10.7	-14.8	-4.2	8.2	-1.9	-10.1	36.0	16.4	-19.6	49.7	11.9	-37.8	6.2	-4.0	-10.2
hidden bias(1)	-19.2	-25.3	-6.1	3.5	-0.1	-3.6	11.9	0.3	-11.6	42.8	-2.8	-45.6	7.6	-5.6	-13.2

employment effect (13

	2000			2001			2002			2003			2004		
	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)	ATE	ATT	Selection(2)
Obs	6.1	6.1	0.0	5.5	7.8	2.3	19.9	13.6	-6.3	10.9	3.9	-7.0	-4.3	1.6	5.9
Non-obs	-2.4	-5.9	-3.5	0.2	-10.9	-11.1	27.6	12.3	-15.3	67.1	8.3	-58.8	9.3	-1.8	-11.1
hidden bias(1)	-8.5	-12.0	-3.6	-5.3	-18.7	-13.4	7.7	-1.3	-9.0	56.2	4.4	-51.8	13.6	-3.4	-17.0

Notes:

(1) hidden bias is the difference between the estimates assuming selection on unobservables minus the estimates assuming selection on

(2) selection is the difference between ATT and

Source: Authors' calculations using PROBECAT, ENEU and ENECE databases

Table 10: Evolution of hidden bias and sign of selection for wage effects

Salaried Workers

wage effect (26 weeks)

	2000			2001			2002			2003			2004		
	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]
Obs	-663	-660	3	-463	-552	-89	-458	-523	-65	-761	-808	-47	-464	-557	-93
Non-obs	1684	2105	422	-50	21	70	-13	53	66	250	291	41	1304	1218	-86
hidden bias [1]	2347	2765	419	413	572	159	445	576	131	1011	1099	88	1768	1775	7

wage effect (13 weeks)

	2000			2001			2002			2003			2004		
	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]
Obs	-613	-446	167	-361	-546	-185	-359	-323	36	-853	-1027	-174	-943	-1188	-245
Non-obs	1124	1323	199	-45	11	56	-14	27	41	25	66	41	-54	89	143
hidden bias [1]	1737	1769	32	316	557	240	345	350	5	878	1093	215	889	1277	388

Self-Employed workers

wage effect (26 weeks)

	2000			2001			2002			2003			2004		
	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]
Obs	-900	-913	-13	-1345	-444	901	-1075	-925	150						
Non-obs	-112	132	244	285	-782	-1067	-1741	87	1828	-315	588	903	-627	424	-1051
hidden bias [1]	788	1045	257	1630	-338	-1968	-666	1012	1678						

wage effect (13 weeks)

	2000			2001			2002			2003			2004		
	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]	ATE	ATT	Selection [2]
Obs	-1250	-1550	-300	-755	-671		-689	-1514	-825	-1197	-648	549			
Non-obs	-51	299	351	58	-824	-882	690	1116	426	-1225	519	1744	251	441	-190
hidden bias [1]	1199	1849	651	812	-153	-882	1379	2630	1251	-28	1167	1195			

Notes:

(1) hidden bias is the difference between the estimates assuming selection on unobservables minus the estimates assuming selection on observables

(2) selection is the difference between ATT and ATE

Source: Authors' calculations using PROBECAT, ENEU and ENECE databases

Table 11: Employment ATT by modality

TREATMENT ON THE TREATED EFFECT (TT) BY MODALITIES
ASSUMING SELECTION-ON-UNOBSERVABLES: PARAMETRIC METHOD [(1)]
bootstrapped standard errors in parentheses [(2)]

Modality	1999		2000		2001		2002		2003		2004	
	13	26	13	26	13	26	13	26	13	26	13	26
School-based	-6.2 ***	-32.4 ***	-3.8 ***	-26.5 ***	-1.4	-36.4 ***						
	(0.4)	(0.5)	(0.1)	(0.2)	(3.4)	(2.8)						
Mixed	15.1 ***	11.9 ***	22.5 ***	23.4 ***	29.7 ***	26.0 ***	20.8 ***	15.1 ***	22.5 ***	16.9 ***		
	(0.7)	(0.7)	(0.5)	(0.5)	(1.9)	(1.9)	(0.7)	(0.6)	(0.5)	(0.6)		
MyPEs [(3)]	1.4 **	-8.7 ***	0.4	-4.4 ***	-3.4 *	-14.5 ***	2.9 ***	6.4 ***	3.8 ***	9.9 ***		
	(0.6)	(0.6)	(0.3)	(0.4)	(1.8)	(1.7)	(0.7)	(0.8)	(0.6)	(0.8)		
Self-employment [(4)]	-8.6 ***	-16.2 ***	-10.2 ***	-22.4 ***	-4.7 **	-15.7 ***	-2.0 ***	-0.2	-18.6 ***	-13.7 ***	-17.5 ***	-10.7 ***
	(0.5)	(0.5)	(0.2)	(0.2)	(2.3)	(1.7)	(0.7)	(0.8)	(1.0)	(0.7)	(1.0)	(1.4)
ILE [(5)]	NC	NC	NC	NC	NA	NA						
Basic skills	5.3 ***	-34.2 ***	-6.9 ***	-17.9 ***	NA	NA						
	(1.7)	(1.5)	(0.5)	(0.8)								
Sinorcom [(6)]	-6.9 ***	-30.9 ***	-14.0 ***	-25.8 ***	NA	NA						
	(1.4)	(1.34)	(0.3)	(0.3)								
Health sector	19.4 ***	14.1 ***	-20.2 ***	0.4	NA	NA						
	(2.0)	(2.2)	(3.8)	(2.9)								
Vales de capacitación									-29.0 ***	-18.1 ***	-27.5 ***	-19.2 ***
									(1.0)	(1.0)	(0.6)	(0.7)
Profesionistas y técnicos desempleados									-18.9 ***	16.5 ***	-19.6 ***	-20.8 ***
									(2.0)	(1.4)	(0.6)	(0.7)
Basada en normas técnicas de competencia laboral									10.5 ***	14.2 ***		
									(0.9)	(1.1)		
Paro técnico									-39.5 ***	-28.2 **		
									(9.4)	(11.4)		
Formación laboral en competencias											28.2 ***	21.7 ***
											(1.4)	(0.9)
Formación laboral en la práctica											-0.1	3.4 ***
											(0.9)	(0.9)
Formación productiva											-26.4 ***	-17.9 ***
											(0.9)	(1.3)

Source:
Authors' calculations using PROBECAT, ENEU and ENECE databases

Notes:
(1) propensity score matching according to Becker and Ichino (2002)
(2) non-parametric bootstrapping
(3) MyPEs training, consists of courses offered in medium and small enterprises.
(4) Self-employment modality is aimed to offer knowledge and skills to develop a job
(5) ILE modality trains to members of mutual organizations to improve the productivity.
(6) Sinorcom modality offers courses to workers to obtain a labor certification.

Table 12: Wage ATT by modality (cont.)

TREATMENT ON THE TREATED EFFECT (TT) BY MODALITIES
ASSUMING SELECTION-ON-UNOBSERVABLES: PARAMETRIC METHOD [1]
in monthly current pesos: bootstrapped confidence interval in parentheses [2]

Modality	2000		2001		2002		2003		2004	
	13	26	13	26	13	26	13	26	13	26
School-based	108 ***	168 ***	16	-5						
	(16)	(21)	(87)	(136)						
Mixed	49 ***	80 ***	60 *	45	81 ***	82 ***	77 ***	183 ***		
	(15)	(17)	(33)	(32)	(19)	(21)	(27)	(34)		
MyPEs [3]	443 ***	47	85	58	41	52 *	63 **	90 ***		
	(20)	(14)	(53)	(42)	(29)	(31)	(25)	(33)		
Self-employment [4]	2	16 *	124	12	-31	-89	107	161 **	-7	16
	(9)	(9)	(129)	(62)	(67)	(56)	(87)	(79)	(153)	(96)
ILE [5]	NC	NC	NA	NA						
Basic skills	2741 ***	74	NA	NA						
	(201)	(39)								
Sinorcom [6]	59 ***	28	NA	NA						
	(23)	(23)								
Health sector	6	42	NA	NA						
	(76)	(82)								
Vales de capacitación							147	151	1	621 ***
							(221)	(122)	(221)	(167)
Profesionistas y técnicos desempleados							563	421 **	342	204
							(345)	(206)	(236)	(141)
Basada en normas técnicas de competencia laboral							177 ***	215 ***		
							(42)	(48)		
Paro técnico										
Formación laboral en competencias									130 ***	154 ***
									(43)	(47)
Formación laboral en la práctica									46	226 ***
									(42)	(61)
Formación productiva									-36	-43
									(219)	(149)

Source:
Authors' calculations using PROBECAT, ENEU and ENECE databases

Notes:
(1) selection correction according to Heckman, Tobias and Vytlacil (2003)
(2) non-paramtric bootstrapping
(3) MyPEs training, consists of courses offered in medium and small enterprises.
(4) Self-employment modality is aimed to offer knowledge and skills to develop a job
(5) ILE modality trains to members of mutual organizations to improve the productivity.
(6) Sinorcom modality offers courses to workers to obtain a labor certification.
(a) Las ENCOPES no cuentan con informacion de las prestaciones laborales y de los salarios (1998 y 1999)
NA No Available
NC Not Calculated (Insuficient observations)

Table 13: Employment ATT on salaried workers

TREATMENT EFFECT ON THE TREATED (TT) BY POPULATION GROUP
ASSUMING SELECTION-ON-UNOBSERVABLES: PARAMETRIC METHOD (1)
bootstrapped confidence interval in parentheses (2)

	2000		2001		2002		2003		2004	
	13	26	13	26	13	26	13	26	13	26
TOTAL										
Gender										
men	-5.2 ***	18.5 ***	-4.5 ***	6.0 ***	16.6 ***	22.3 ***	17.0 ***	3.7 ***	3.4 ***	8.8 ***
	(0.1)	(0.1)	(0.5)	(0.7)	(0.7)	(0.6)	(0.5)	(0.7)	(0.7)	(0.8)
women	8.8 ***	13.4 ***	-25.5 ***	-23.7 ***	24.4 ***	25.7 ***	13.9 ***	23.1 ***	5.4 ***	16.1 ***
	(0.1)	(0.1)	(0.4)	(0.5)	(0.4)	(0.5)	(0.5)	(0.4)	(0.4)	(0.4)
Age group										
15-25	2.2 ***	21.7 ***	-0.6	8.8 ***	17.5 ***	28.3 ***	22.7 ***	2.7 ***	8.2 ***	15.0 ***
	(0.1)	(0.1)	(0.4)	(0.4)	(0.4)	(0.5)	(0.4)	(0.5)	(0.5)	(0.5)
26-35	4.9 ***	-11.7 ***	-0.2	8.5 ***	19.7 ***	27.0 ***	-6.7 ***	21.6 ***	-16.1 ***	-3.0 ***
	(0.2)	(0.2)	(0.7)	(0.9)	(1.1)	(1.1)	(1.2)	(0.8)	(1.1)	(1.0)
more than 36	7.2 ***	13.2 ***	2.6 **	1.0	38.2 ***	28.4 ***	4.4 ***	11.7 ***	-10.9 ***	1.9
	(0.2)	(0.3)	(1.0)	(1.6)	(3.4)	(2.9)	(1.4)	(1.5)	(3.6)	(2.6)
Schooling:										
primary	-0.2	18.6 ***	12.0 ***	-1.8 *	21.1 ***	21.6 ***	27.7 ***	10.7 ***	2.5	-5.9
	(0.2)	(0.3)	(1.0)	(1.0)	(1.7)	(1.7)	(1.5)	(1.8)	(3.7)	(5.1)
junior high school	-1.3 ***	10.3 ***	2.9 ***	10.2 ***	26.7 ***	32.6 ***	20.4 ***	23.0 ***	11.4 ***	-5.4 ***
	(0.1)	(0.1)	(0.6)	(0.6)	(0.6)	(0.6)	(0.6)	(0.7)	(1.3)	(1.8)
high school	5.7 ***	0.4 **	-0.4	10.6 ***	18.7 ***	9.1 ***	NC	NC	0.7	17.1 ***
	(0.2)	(0.2)	(0.5)	(0.6)	(0.6)	(0.8)			(0.9)	(0.8)
university	-6.9 **	-9.0 ***	-19.7 ***	-9.0 ***	17.0 ***	16.1 ***	20.1 ***	14.4 ***	-2.9 ***	16.1 ***
	(2.8)	(3.4)	(1.3)	(0.7)	(1.9)	(1.8)	(0.5)	(0.8)	(0.4)	(0.5)
Region										
D.F.	-75.6 ***	8.1 ***	-16.3 ***	-67.4 ***	16.4 ***	38.3 ***	-38.6 ***	5.1 ***	NC	NC
	(0.8)	(0.5)	(1.0)	(2.9)	(4.3)	(8.6)	(1.4)	(0.5)		
Center	10.3 ***	12.9 ***	-22.9 ***	9.3 ***	24.3 ***	10.3 ***	12.8 ***	-32.7 ***	16.1 ***	21.6 ***
	(0.5)	(0.5)	(1.4)	(0.8)	(3.4)	(2.9)	(0.9)	(2.7)	(2.0)	(3.9)
Center-north	0.2	2.2 ***	-5.7 ***	2.8 **	20.9 ***	28.6 ***	29.7 ***	10.5 ***	2.5	-4.5 *
	(0.2)	(0.2)	(0.8)	(1.1)	(1.4)	(0.9)	(0.8)	(1.6)	(1.8)	(2.4)
north	-12.5 ***	11.8 ***	-7.6 ***	4.0 ***	24.4 ***	21.7 ***	5.2 ***	19.3 ***	-4.5 ***	-13.4 ***
	(0.2)	(0.2)	(0.7)	(0.6)	(1.3)	(1.3)	(0.7)	(0.8)	(1.2)	(1.7)
gulf	-59.3 ***	-5.5 ***	-19.0 ***	-8.4 ***	30.6 ***	27.7 ***	31.2 ***	25.5 ***	15.0 ***	18.9 ***
	(0.6)	(0.4)	(0.9)	(1.2)	(1.5)	(1.7)	(1.4)	(1.4)	(3.4)	(3.1)
pacific	-2.2 *	12.4 ***	-0.7	7.8 ***	14.4 ***	12.6 ***	33.6 ***	28.7 ***	40.7 ***	26.2 ***
	(1.2)	(1.1)	(1.0)	(0.9)	(3.2)	(2.8)	(1.8)	(2.2)	(2.3)	(3.0)
south	10.6 ***	17.1 ***	-29.3 ***	-19.4 ***	11.3 ***	15.3 ***	NC	NC	-61.3 ***	-5.3 **
	(0.5)	(0.6)	(2.8)	(2.2)	(2.1)	(1.5)			(3.4)	(2.7)
Quarter										
First	6.7 ***	13.7 ***	6.2 ***	27.2 ***	9.2 ***	24.8 ***	19.2 ***	13.1 ***	11.7 ***	20.2 ***
	(0.3)	(0.3)	(0.9)	(0.7)	(0.6)	(0.5)	(0.6)	(0.6)	(0.8)	(0.6)
Second	-23.9 ***	21.6 ***	3.5 ***	-1.8 *	25.4 ***	21.9 ***	8.5 ***	0.1	-10.6 ***	-4.0 ***
	(0.2)	(0.2)	(0.6)	(0.9)	(0.6)	(0.6)	(0.3)	(0.5)	(0.6)	(0.6)
Third	17.5 ***	-3.3 ***	-17.7 ***	-4.0 ***	NA	NA	NC	NC	NA	NA
	(0.7)	(1.2)	(0.3)	(0.3)						
Fourth	-16.1 ***	11.9 ***	NC	NC	NA	NA	NA	NA	NA	NA
	(0.2)	(0.1)								

Notes:
(1) selection correction according to Heckman, Tobias and Vytlacil (2003)
(2) non-paramtric bootstrapping
(a) Las ENCOPES no cuentan con informacion de las prestaciones laborales y de los salarios (1998 y 1999)
NA No Available
NC Not Calculated (Insuficient observations)

Table 14: Wage ATT on salaried workers

TREATMENT EFFECT ON THE TREATED (TT) BY POPULATION GROUP
ASSUMING SELECTION-ON-UNOBSERVABLES: PARAMETRIC METHOD (1)
in monthly current pesos: bootstrapped confidence interval in parentheses (2)

		Non- Salaried		2000		2001		2002		2003		2004	
		1998	1999	13	26	13	26	13	26	13	26	13	26
TOTAL													
Gender													
	men	NA	NA	670 ***	33 ***	43 *	38	26	46	238 ***	1664 ***	65	171 **
				(29)	(8)	(26)	(24)	(30)	(37)	(47)	(82)	(51)	(70)
	women	NA	NA	NC	NC	68 ***	61 ***	316 ***	95 ***	50 **	293 ***	170 ***	227 ***
						(22)	(20)	(44)	(25)	(21)	(40)	(59)	(67)
Age group													
	15-25	NA	NA	NC	NC	15	25 *	171 ***	53 **	42 **	90 ***	34	89 **
						(11)	(13)	(30)	(23)	(21)	(24)	(31)	(43)
	26-35	NA	NA	434 ***	403 ***	83 *	117 **	56	192 ***	258 ***	631 ***	152	845 ***
				(38)	(38)	(50)	(47)	(51)	(70)	(59)	(91)	(149)	(170)
	more than 36	NA	NA	256 ***	1185 ***	47	201 **	408 ***	1247 ***	NC	NC	NC	NC
				(43)	(63)	(104)	(83)	(135)	(269)				
Schooling:													
	primary	NA	NA	NC	NC	102 **	133 ***	105 **	319 ***	NC	NC	NC	NC
						(43)	(38)	(52)	(95)				
	junior high school	NA	NA	NC	NC	85 ***	108 ***	86 ***	26	67 *	207 ***	73	556 ***
						(23)	(24)	(29)	(23)	(38)	(49)	(59)	(110)
	high school	NA	NA	377 ***	1061 ***	32	88 **	567 ***	141 **	NC	NC	116	100
				(31)	(33)	(27)	(35)	(84)	(65)			(75)	(70)
	university	NA	NA	NC	NC	NC	NC	NC	NC	NC	NC	NC	NC
Region													
	D.F.	NA	NA	206 ***	480 ***	NC	NC	NC	NC	NC	NC	NC	NC
				(50)	(57)								
	Center	NA	NA	1052 ***	374 ***	111	872 ***	NC	NC	NC	NC	NC	NC
				(48)	(44)	(71)	(106)						
	Center-north	NA	NA	NC	NC	19	34	125 ***	187 ***	264 ***	245 ***	302 ***	176 *
						(26)	(31)	(39)	(45)	(54)	(57)	(57)	(101)
	north	NA	NA	364 ***	59 ***	4	28	38	51	NC	NC	-60	331 **
				(27)	(14)	(23)	(25)	(35)	(48)			(201)	(135)
	gulf	NA	NA	54 ***	295 ***	82 *	210 ***	757 ***	1280 ***	NC	NC	NC	NC
				(15)	(33)	(49)	(51)	(160)	(173)				
	pacific	NA	NA	102 *	102 **	167 **	401 ***	216 ***	247 **	727 ***	2164 ***	NC	NC
				(59)	(40)	(83)	(105)	(77)	(98)	(102)	(69)		
	south	NA	NA	NC	NC	151 **	1016 ***	NC	NC	NC	NC	NC	NC
						(65)	(127)						
Quarter													
	First	NA	NA	NC	NC	53 *	34	30	46 *	265 ***	901 ***	147 ***	122 **
						(31)	(23)	(22)	(28)	(40)	(61)	(56)	(56)
	Second	NA	NA	208 ***	93 ***	38 *	126 ***	97 ***	400 ***	1147 ***	74 **	172 **	892 ***
				(21)	(19)	(21)	(33)	(29)	(54)	(76)	(37)	(75)	(123)
	Third	NA	NA	NC	NC	6	74	NA	NA	NC	NC	NA	NA
						(38)	(46)						
	Fourth	NA	NA	524 ***	167 ***	NC	NC	NA	NA	NA	NA	NA	NA
				(22)	(16)								

Notes:
(1) selection correction according to Heckman, Tobias and Vytlacil (2003)
(2) non-paramtric bootstrapping
(a) ENCOPE surveys (1998 and 1999) do not have information on wages and other payments
NA No Available
NC Not Calculated (Insuficient observations)

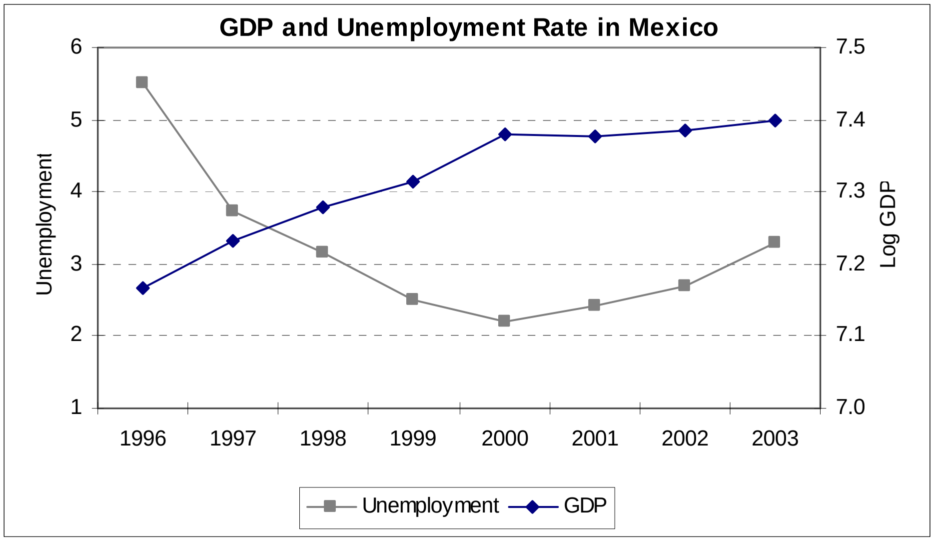
Table 15: Cost benefit analysis

year	Probability finding a job before training	Estimated wage before training in pesos/month	Probability finding a job after training	Estimated wage after training, in pesos month	Expected wage before training, in pesos/month	Expected wage after training in pesos/month	Estimated gain(+)/loss(-) in expected wages, in pesos/month	number of participants	Total monthly gain(+)/loss(-), in millions of pesos	Total monthly gain(+)/loss(-), in millions of 2004 pesos	Budget, in millions of 2004 pesos per month	Total monthly gain(+)/loss(-), in millions of 2004 pesos
	a	b	c	d	e=a*b	f=c*d	g=f-e	h	i=g*h	j	k	l=j-k
Cost Benefit Analysis for PROBECAT- general ATT controlling for Non-observables												
2000	0.328	1938	0.392	2425	636	950	314	593,175	186	228	127	101
2001	0.339	2031	0.437	1988	689	869	180	396,974	72	82	113	-31
2002	0.418	2135	0.584	2168	893	1265	373	230,185	86	94	66	28
2003	0.491	2508	0.500	2712	1231	1355	124	214,931	27	28	55	-28
2004	0.542	2696	0.407	2743	1463	1116	-346	207,239	-72	-72	42	-113
Cost Benefit Analysis for PROBECAT- general ATT controlling for observables												
2000	0.294	2185	0.315	1441	642	454	-189	593175	-112	-137	127	-264
2001	0.373	2378	0.256	1986	888	508	-380	396974	-151	-173	113	-286
2002	0.669	2856	0.637	2234	1909	1423	-486	230185	-112	-122	66	-188
2003	0.516	2853	0.518	2545	1473	1317	-156	214931	-33	-35	55	-90
2004	0.541	3390	0.439	2810	1835	1234	-602	207239	-125	-125	42	-166

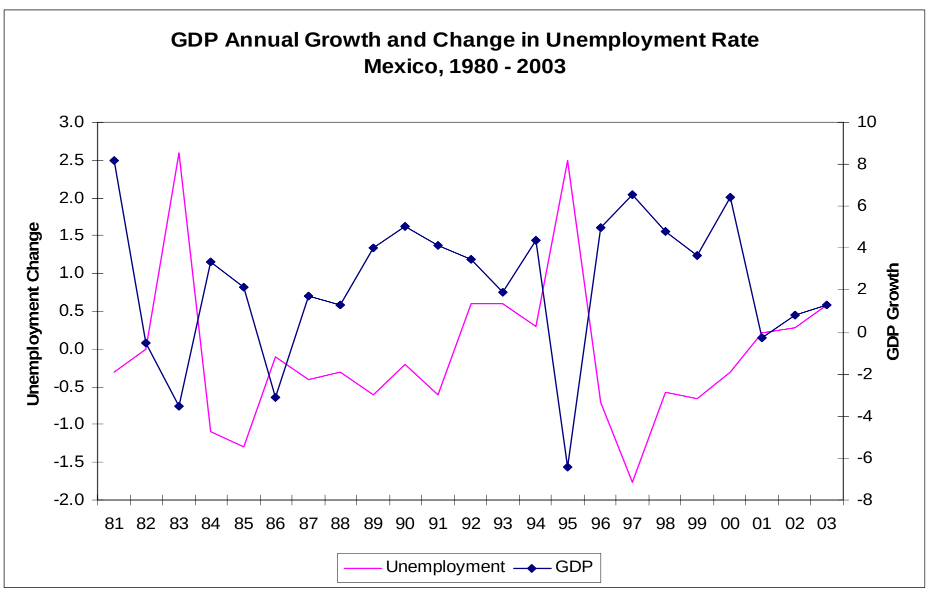
Source: Own estimates and STPS

GRAPHS

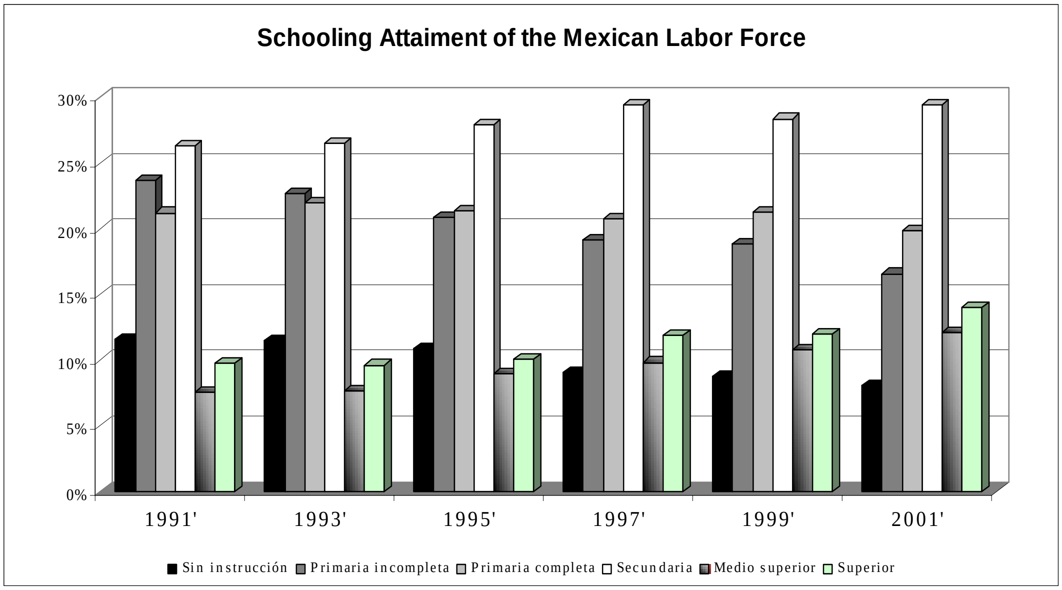
Graph 1



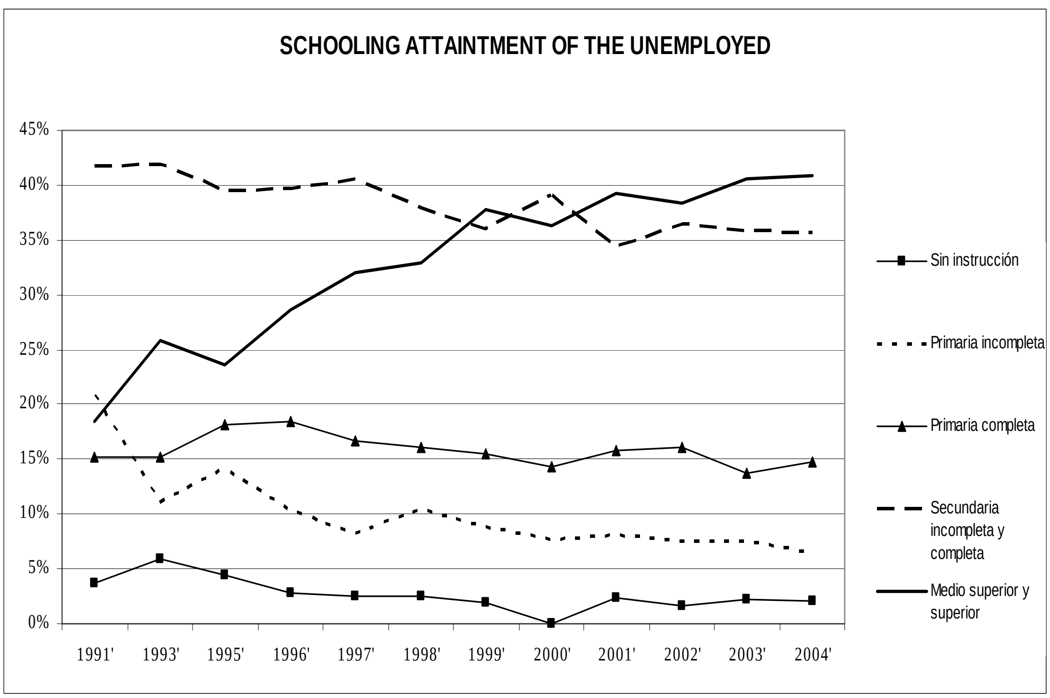
Graph 2



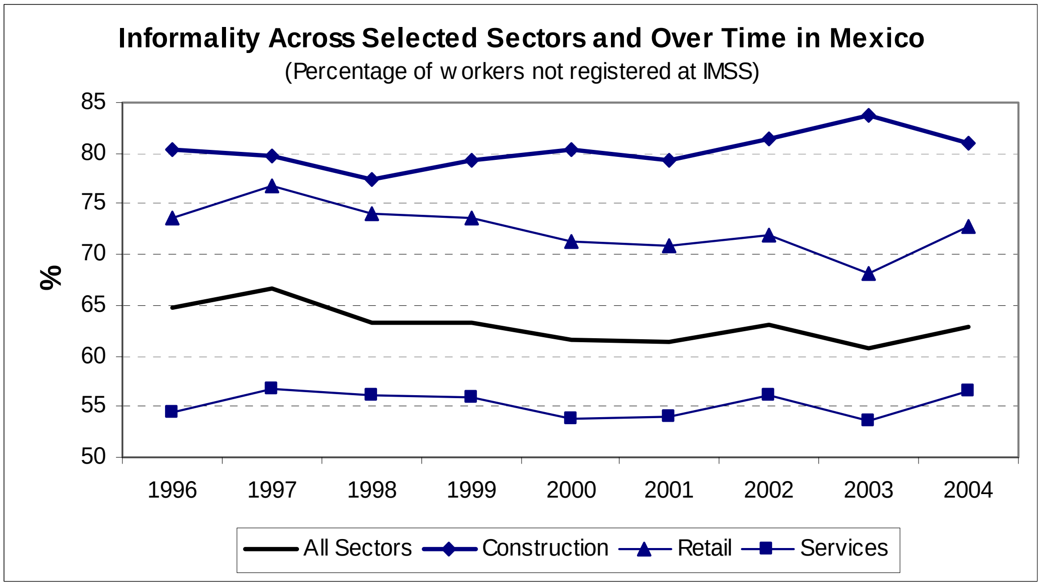
Graph 3



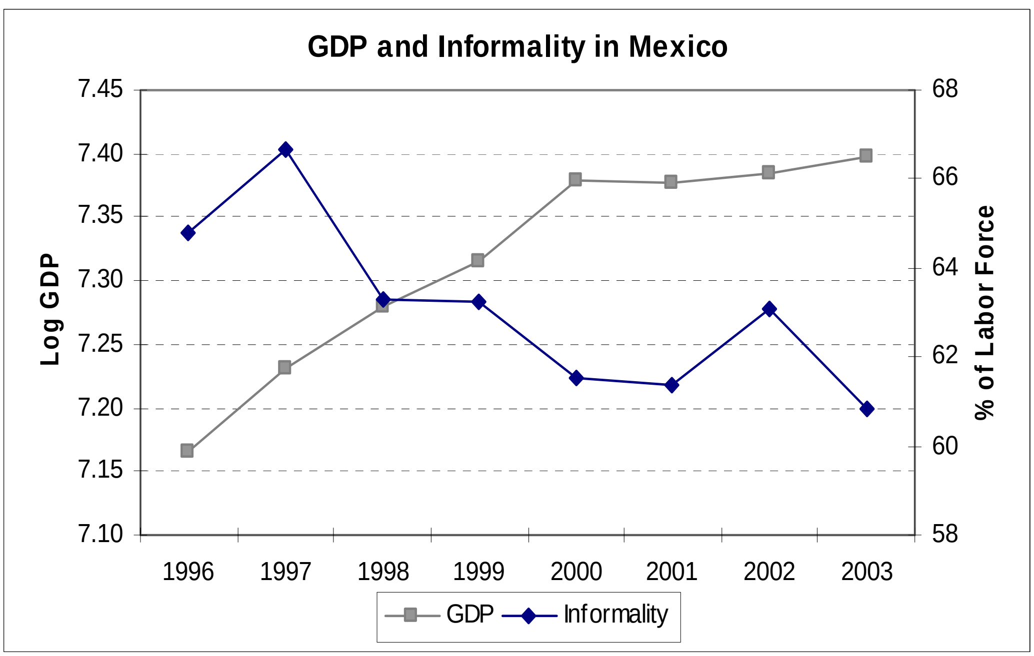
Graph 4



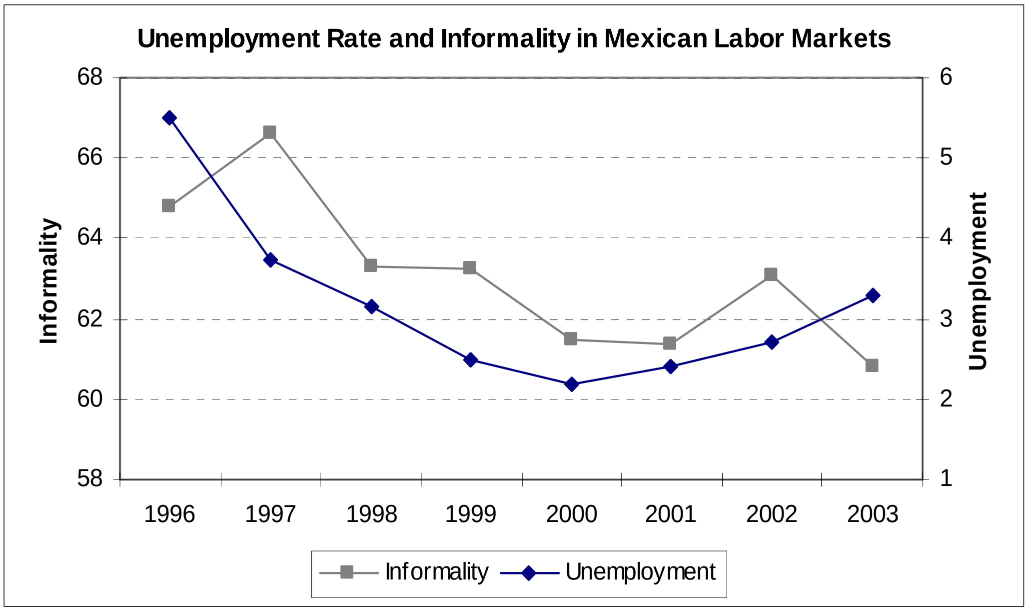
Graph 5



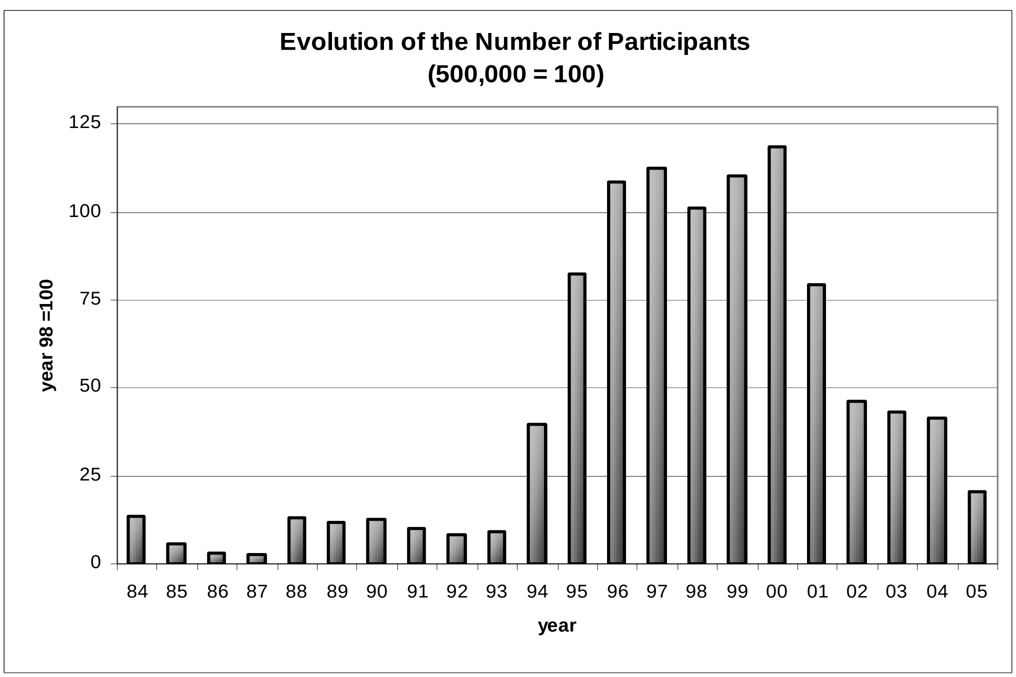
Graph 6



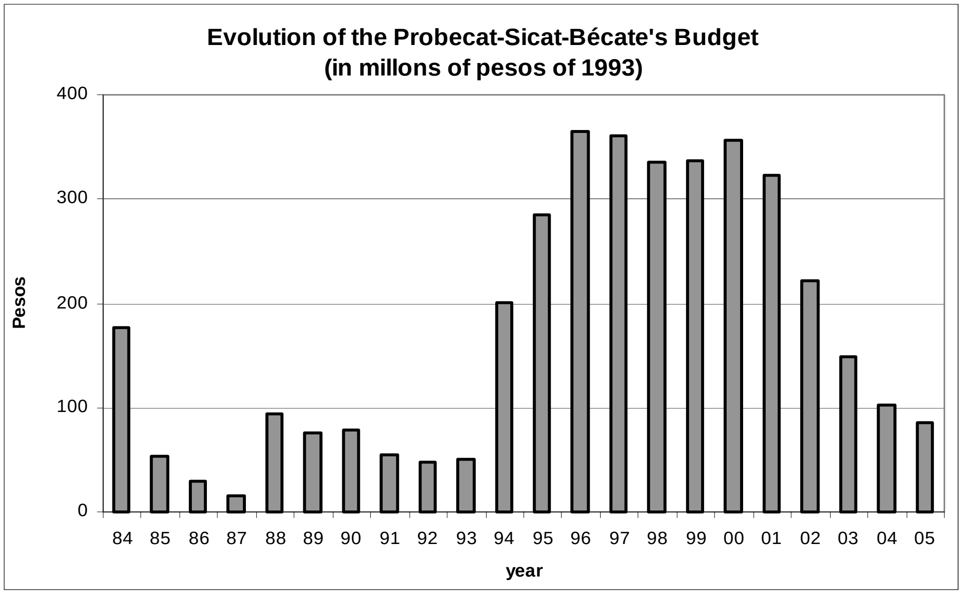
Graph 7



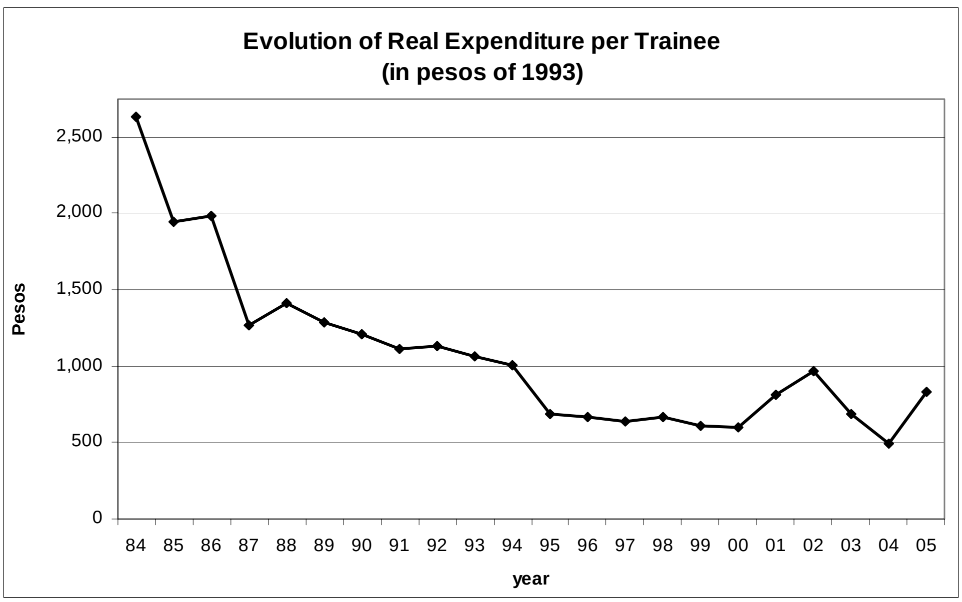
Graph 8



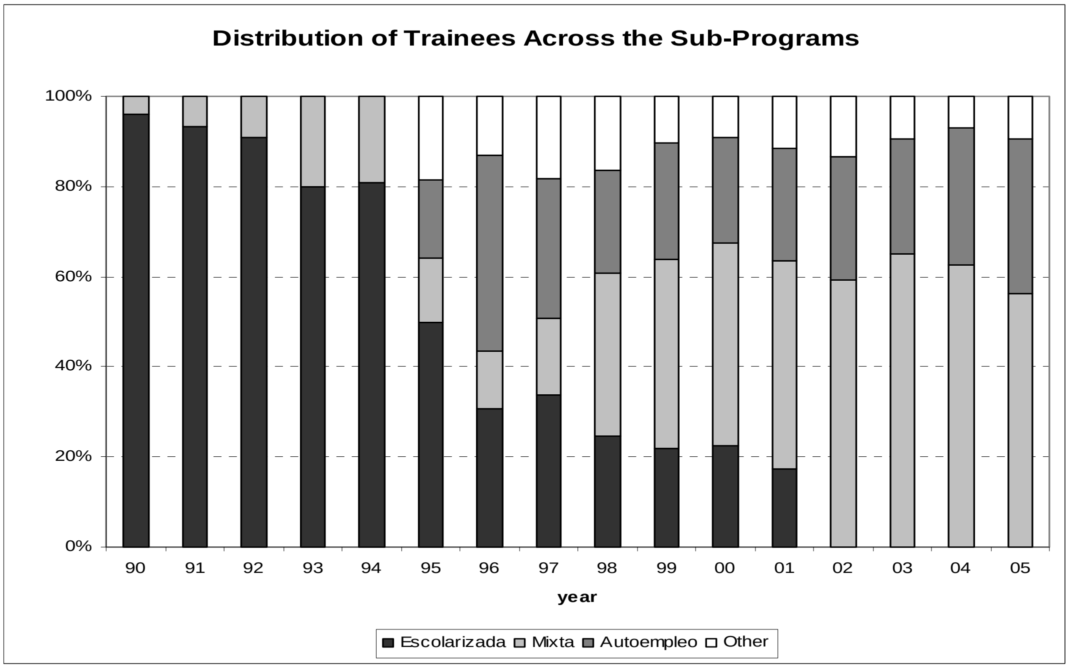
Graph 9



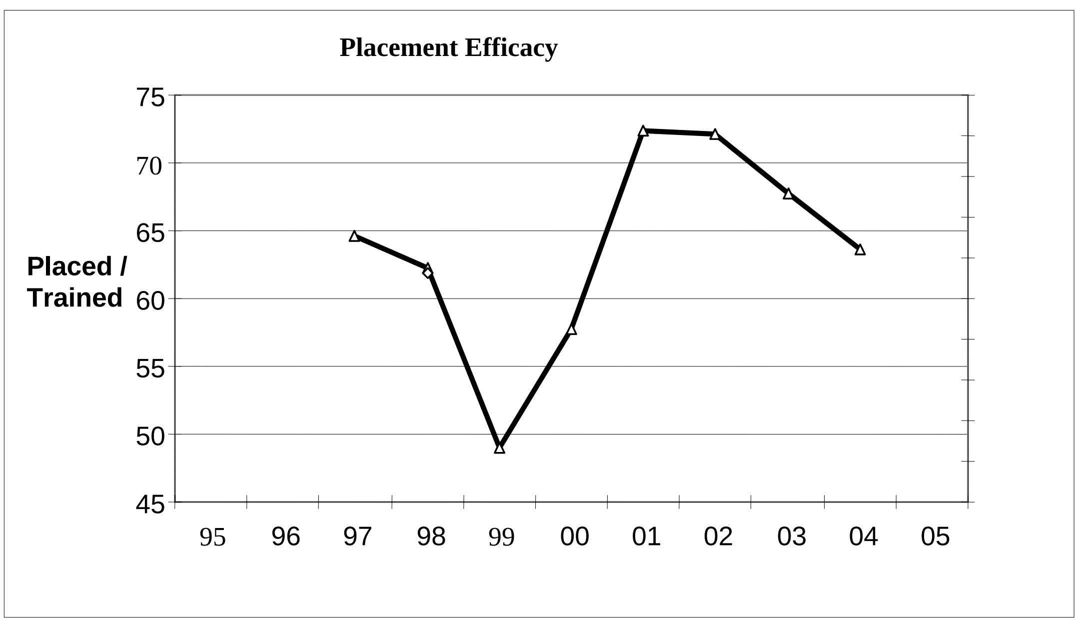
Graph 10



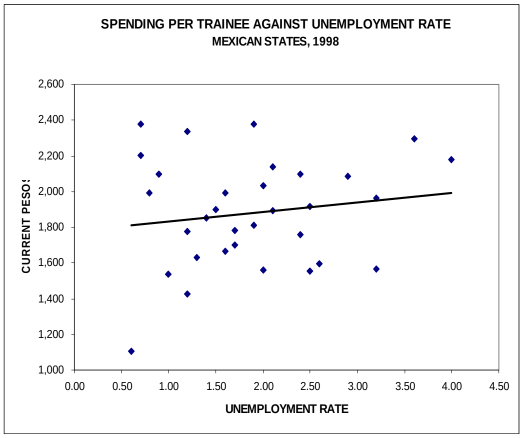
Graph 11

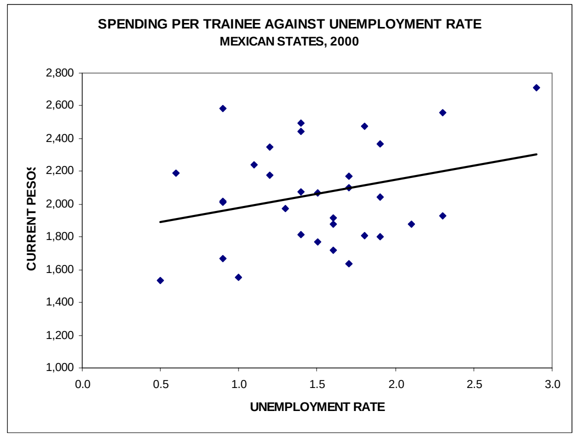


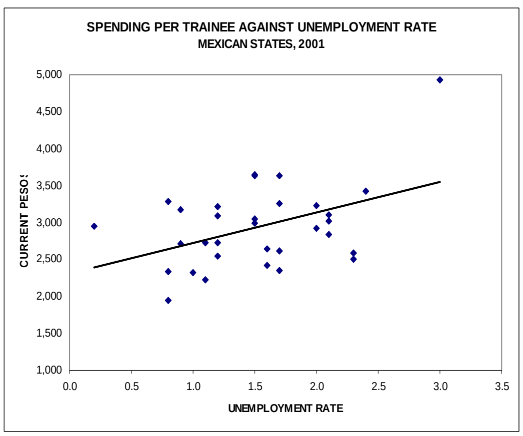
Graph 12

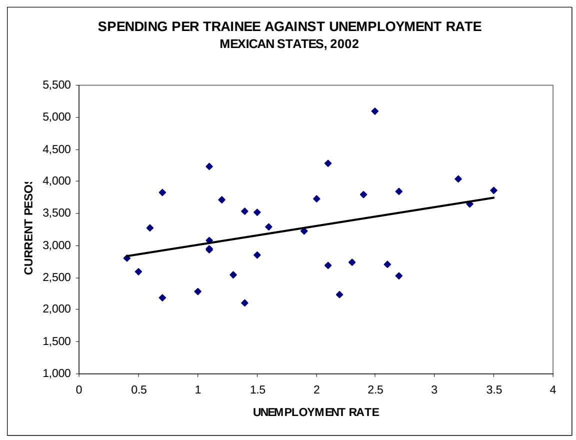


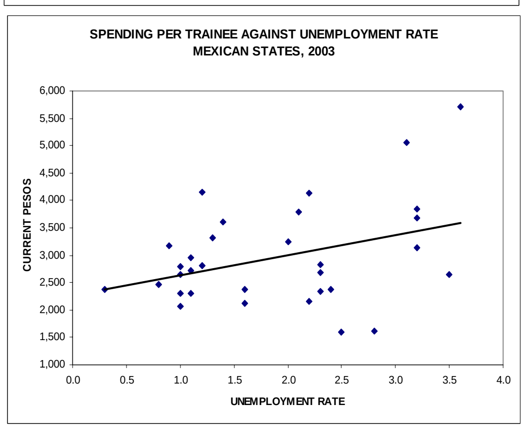
Graph 13







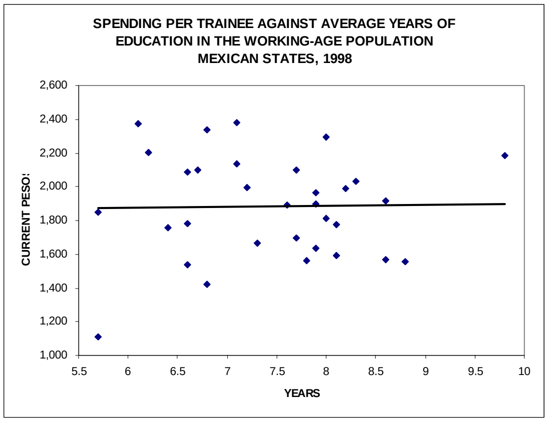


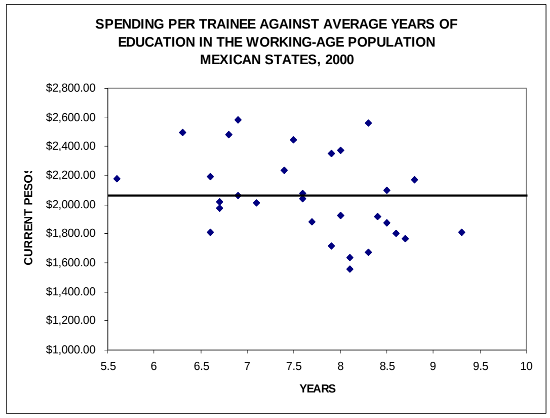


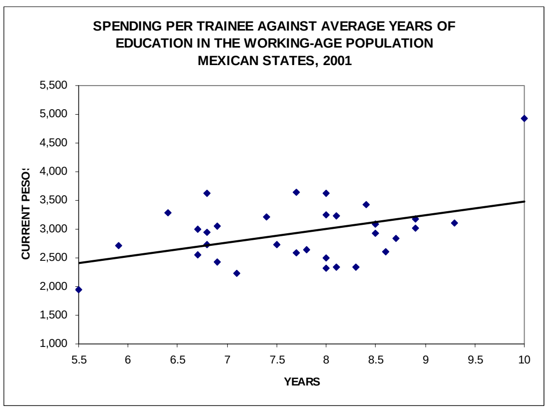
CORRELATIONS

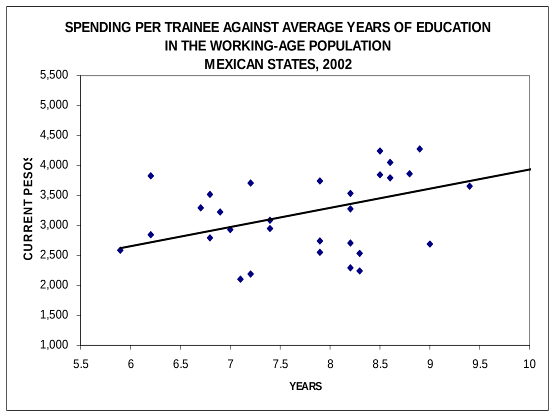
1998	0.16
2000	0.28
2001	0.44
2002	0.35
2003	0.37

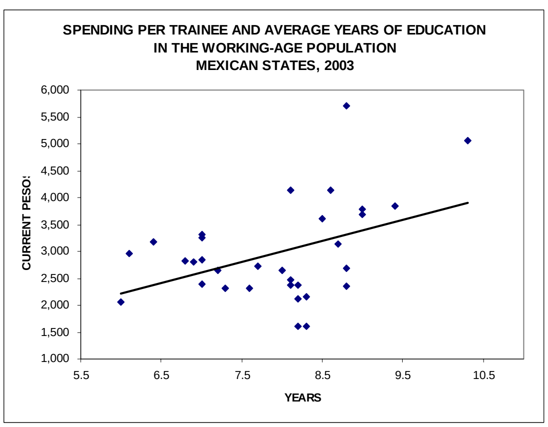
Graph 14





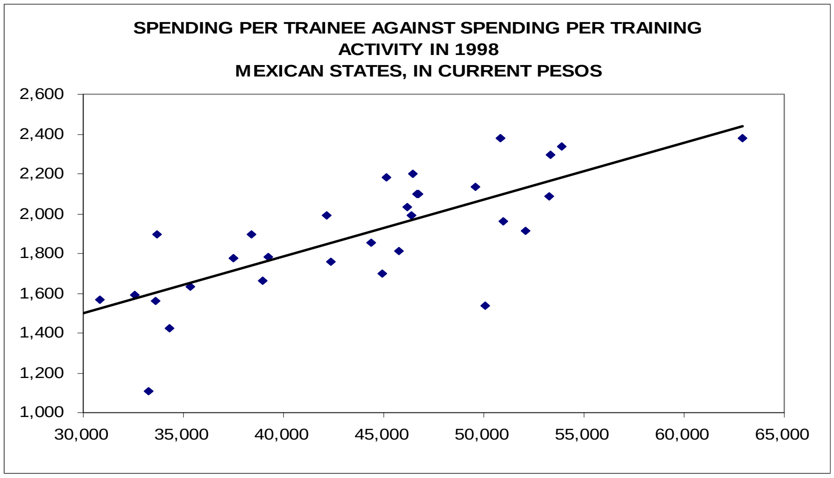


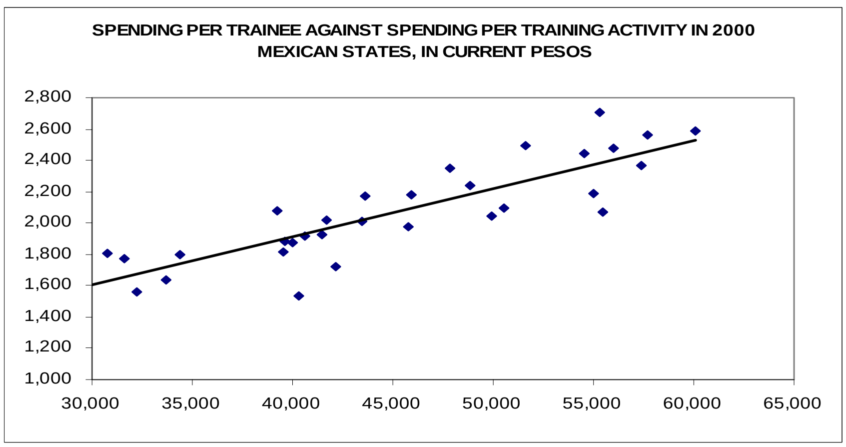


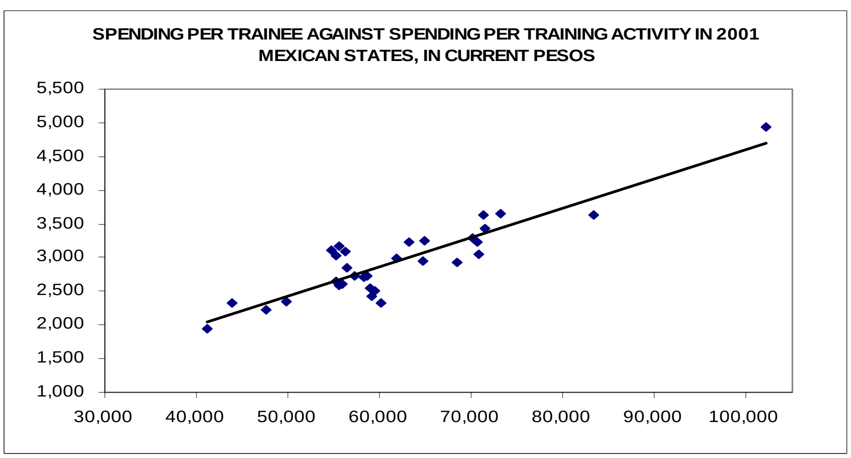


CORRELATIONS	
1998	0.02
2000	0.00
2001	0.43
2002	0.44
2003	0.42

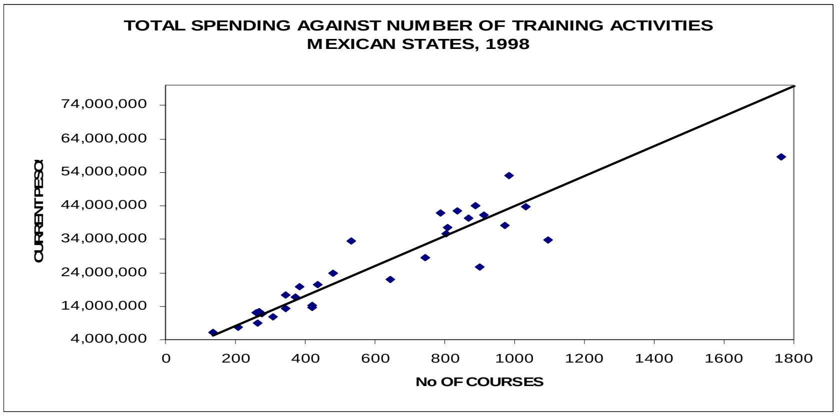
Graph 15

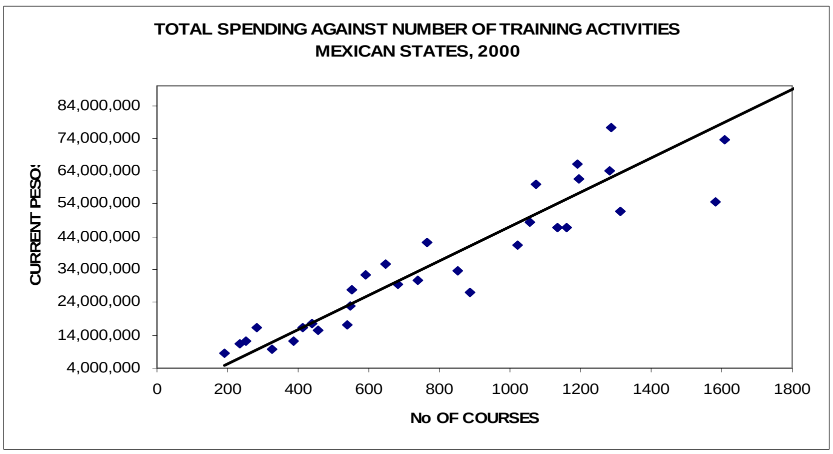

SPENDING PER TRAINEE AGAINST SPENDING PER TRAINING ACTIVITY IN 1998
MEXICAN STATES, IN CURRENT PESOS
2,600
2,400
2,200
2,000
1,800
1,600
1,400
1,200
1,000
30,000
35,000
40,000
45,000
50,000
55,000
60,000
65,000

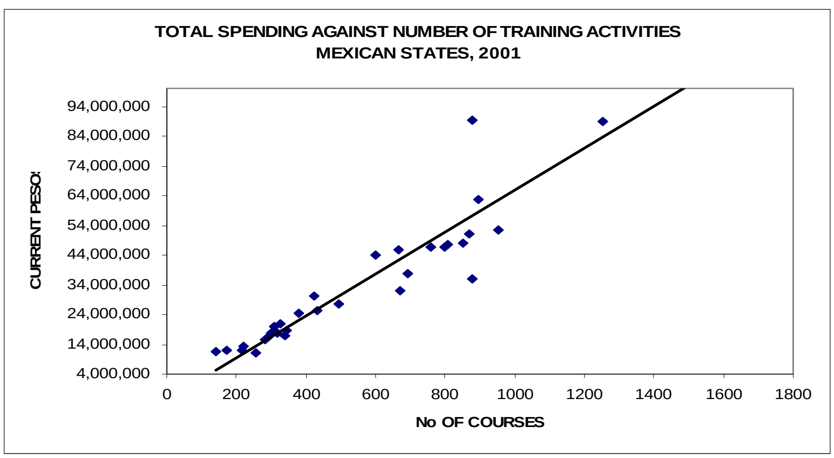

SPENDING PER TRAINEE AGAINST SPENDING PER TRAINING ACTIVITY IN 2000
MEXICAN STATES, IN CURRENT PESOS
2,800
2,600
2,400
2,200
2,000
1,800
1,600
1,400
1,200
1,000
30,000
35,000
40,000
45,000
50,000
55,000
60,000
65,000


SPENDING PER TRAINEE AGAINST SPENDING PER TRAINING ACTIVITY IN 2001
MEXICAN STATES, IN CURRENT PESOS
5,500
5,000
4,500
4,000
3,500
3,000
2,500
2,000
1,500
1,000
30,000
40,000
50,000
60,000
70,000
80,000
90,000
100,000

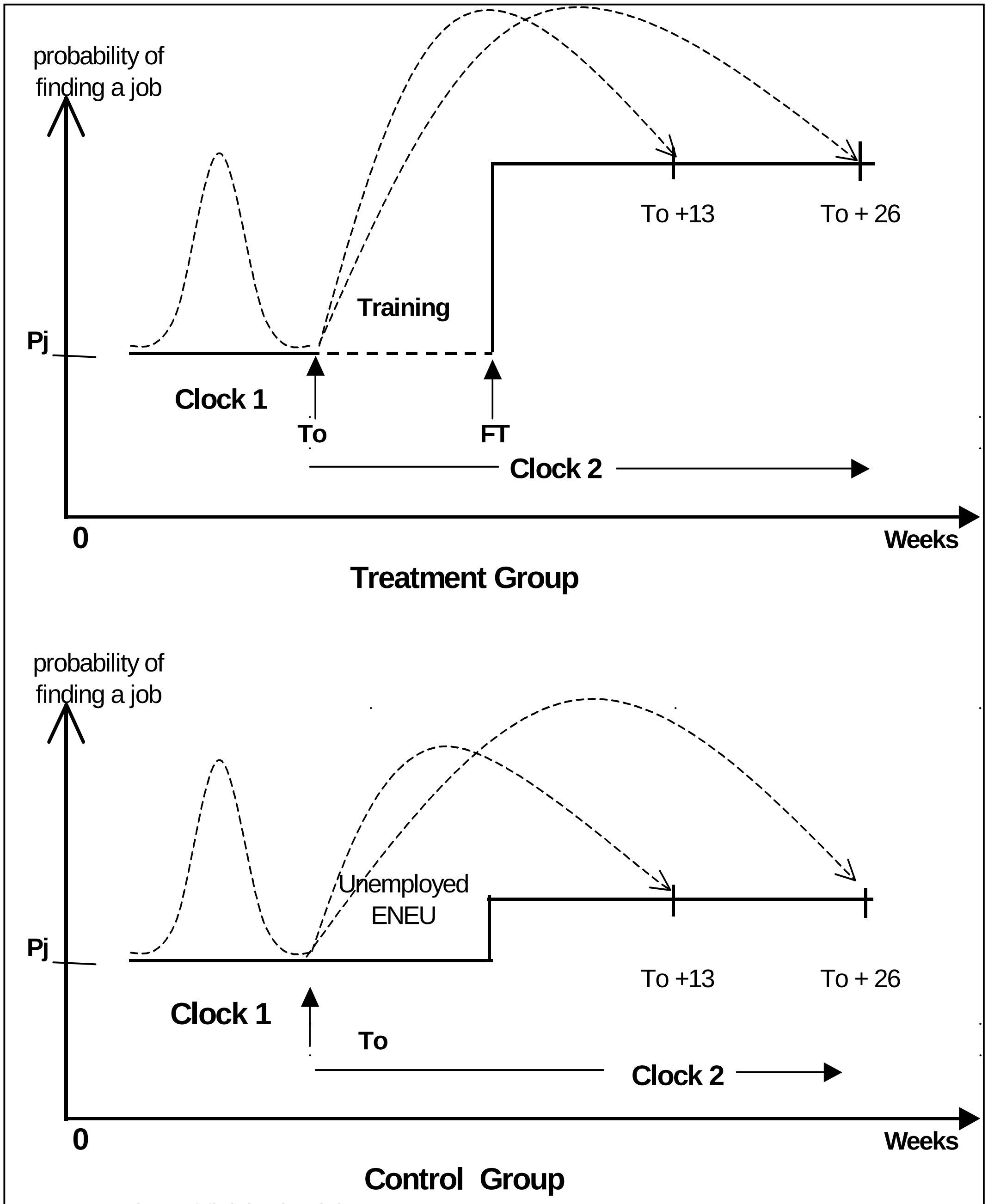
Graph 16


TOTAL SPENDING AGAINST NUMBER OF TRAINING ACTIVITIES
MEXICAN STATES, 1998
74,000,000
64,000,000
54,000,000
44,000,000
34,000,000
24,000,000
14,000,000
4,000,000
0
200
400
600
800
1000
1200
1400
1600
1800
No OF COURSES


TOTAL SPENDING AGAINST NUMBER OF TRAINING ACTIVITIES
MEXICAN STATES, 2000
84,000,000
74,000,000
64,000,000
54,000,000
44,000,000
34,000,000
24,000,000
14,000,000
4,000,000
0
200
400
600
800
1000
1200
1400
1600
1800
No OF COURSES


TOTAL SPENDING AGAINST NUMBER OF TRAINING ACTIVITIES
MEXICAN STATES, 2001
94,000,000
84,000,000
74,000,000
64,000,000
54,000,000
44,000,000
34,000,000
24,000,000
14,000,000
4,000,000
0
200
400
600
800
1000
1200
1400
1600
1800
No OF COURSES

Graph 17: Search and training for treatment and control groups



FT = date of finished training

METHODOLOGICAL ANNEX

A. Non-parametric estimation under selection on observables

For non-parametric estimates of treatment effects, it is necessary that treatment and control observations share the same observable characteristics, so that comparable groups can be differenced based on common characteristics. Otherwise, the groups could be systematically different. This is known as the support problem. Furthermore, if there are several characteristics, it may be difficult to create comparable groups. This is known as the dimension problem.

In propensity score matching, these two problems are addressed by estimating a probability model, which provides a fitted probability of participating for each individual with a vector of characteristics (X). This addresses the dimension problem. Then, observations are kept only if their fitted probabilities fall within the range of the intersection of the propensity scores of treatments and controls. This addresses the common support problem.

To assess the impact of PROBECAT-SICAT, controls were constructed for each person that received the intervention by drawing a "twin" from the pool of individuals who are potential targets of the program but ultimately were unexposed to the program (i.e., the dataset of controls from ENEU). A way to select a comparison group, given the existing data, is to use as a control for each participant a non-participant with the same observed characteristics. Namely, a match for individual "*i"* that participated in PROBECAT-SICAT (i.e., each observation from ENCOPE) and whose age, formal education, etc. could be described by a vector of variables "*Xi*" should be an individual *"j"* from the general population that did not participate in PROBECAT-SICAT (i.e. a selected observation from ENEU) and whose age, formal education, etc., described by a vector of variables *"Xj"* is such that $Xj \equiv Xi$.

Instead of aiming to ensure that the control for each participant has exactly the same value of X (a difficult task when we have many X's), the same result can be achieved by matching on the predicted value of outcome, denoted by "P", given X, which is called the propensity score of X. Rosembaun and Rubin (1983) show that if (for the case of unemployment here) employment outcome without PROBECAT-SICAT is independent of participation given X, then participants are also independent of participation given the propensity score for X: P(X). The propensity score is just the probability for a given event to happen, given de values of X. It is a single number and is easier to handle than the whole set of X variables for each individual. If there is conditional independence of the outcome given X, propensity score eliminates all bias.

The use of matching methods requires making decisions along the way. One wants the comparison group to be as similar as possible to the treatment group in terms of the observable characteristics of individuals, as summarized by the propensity score. Nonetheless, we might find that some of the non-participant sample has a lower propensity score that any of those in the treatment sample. This is known as the problem of lacking a "common support". Thus, in doing the matching, one should eliminate those observations from the set of non-participants to ensure that only gains over the same range of propensity scores are being compared. One should also exclude those non-participants for whom the probability of participating is zero.

Thus, one way to draw a "twin" is first to get P. This can be done by mean of an statistical model, usually a *probit* or a *logit* equation, that relates the probability of an outcome to several explanatory variables: P=G(X). In this research we apply a *probit* model to the probability of participating in a training program and relate this probability to two groups of variables. The first group corresponds to individuals' characteristics, such as: gender, marital status, kinship, number of dependents, age, and formal education. The second group corresponds to variables that control for the economic environment in general (GDP per head, seven Mexican regions), and for variables that control for availability of the program (number of beneficiaries per unemployed in the region) and for the quality of the program available (program budget per beneficiary as a proxy)[24].

The *probit* equation provides estimated probabilities for all persons in the dataset of participating in a job-training program. Thus, it allows us to search for a close equivalent person without the program in the light of these variables in order to locate the closest equivalent person for pairing with each given person in the intervention group. In applying this procedure, "closeness" may be adjusted to make matching possible. There are several possibilities for this adjustment (closest neighbor, kernel, etc.) rendering in general similar results. For our calculations we have selected the method of closest neighbor. The procedure is as follows. First, one regress P on G(X) and get the predicted value of P for each possible value of X, which is then estimated for the whole sample (participant and non-participant). For each participant one then finds a non-participant with the closest value of this predicted probability.

This procedure was done for the case of PROBECAT-SICAT. To save space, we do not present here all the procedures done for each year. Nonetheless, for the sake of clarity, in what follows we present as an example our estimates for year 2002 and for the impact on individuals after 13 weeks of finishing training. The

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[24] Table Annex 1, shows the results of this probit model in detail.

pairing of data shows (according to Table 1) 1805 individuals in the treatment group and 1931 in the control group. Of these, 3670 are used in the regression for creating the propensity score.[25] As indicated above, the propensity score matching methodology implies running a *probit* model where the dependent variable is the probability of a person participating in a training course (see Table Annex 2, page 1). From this *probit*, we get the estimated probability for each individual: *phat(X)* (i.e., its propensity score). Table Annex 3 shows the distribution of these *phat(X)* values. We used a statistical program to calculate the optimal number of blocks to ensure that the mean propensity score is not different for treated and controls in each block.[26] For this particular case, the number of blocks turned out to be 7. Then, we test whether the means of each characteristic do not differ between treated and control units within each block (this process is known as checking for the balancing property of the propensity score). If we require that the common support option be applied, this would mean discard the observations for which the balancing property is not fulfilled. We can see that for this particular case that has 7 blocks, at the lower end of the *phat(X)* there are 275 individuals in the control group and 47 in the treated. At the higher end of *phat(X)* there are 68 individuals in the control group and 487 in the treated[27]. Similar tables for the other years are available from the authors upon request.

Once the group of treatments and controls are chosen according to the propesit score matching defined above, the parameters of interest are estimated according to the following formulas. The formula for the ATT is:

$$ATT = \frac{1}{N^T} \sum_{i \in T} \left[Y_i^1 - \sum_{j \in C(i)} w(i,j) Y_j^0 \right]$$

where T is the set of treatments and C the set of controls. In our study, we make use of nearest neighbor matching, so for each observation *i* in the set of treatments, the control is the observation j that follows:

$$C(i) = \underset{j}{Min} \left| p_i - p_j \right|$$

[25] There is a difference in number of observations due to some missing data among the explanatory variables.
[26] Namely, *psmatch28* for STATA 8.
[27] For sensitivity analysis to check for robustness of results we can use only those blocks with relatively similar number of individuals in each group and discard those with either a low or a high phat(X).

where p stands for the fitted propensity score derived from a probit model of participation in the program. In this case, the weights are:

$$w(i,j)=\begin{cases} 1/N^{C(i)} & if \quad j\in C(i) \\ 0 & otherwise \end{cases}$$

and N^T and $N^C=\sum_i N^{C(i)}$ are the number of observations in the treatment and control sets.

The formula for the ATE is:

$$ATE=\frac{N^T}{N}ATT+\frac{N^C}{N}ATU$$

where the average treatment on the untreated is:

$$ATU=\frac{1}{N^C}\sum_{j\in C}\left[\sum_{i\in T}w(i,j)Y_i^1-Y_j^0\right]$$

For the estimation of these parameters, we make use of the program by Becker and Ichino (2002).

B. Parametric estimation under selection on unobservables

The assumption of selection on observables may be untenable if we have a limited database or if we believe that there are important variables for the program participation and outcomes that are hardly ever observable for the researcher (e.g. talent, beauty, work ethics, genetic predisposition).[28] Heckman, Tobias y Vytlacil (2003, 2001) offer a parametric procedure for this.[29] They assume that program outcomes can be formalized with a linear model of as follows:

$$Y^1=X'\beta^1+U^1$$

$$Y^0=X'\beta^0+U^0$$

[28] Given our available dataset, with limited common questions between the ENEU and the ENCOPE, we think it is sensible to adopt a selection-on-unobservables assumption.
[29] There are also some non-parametric methods for selection on unobservables. For a review see Lee (2005).

Since most programs under evaluation are not randomly assigned, the selection mechanism is modeled as follows

$$D^{*} = Z'\theta + U^{D}$$
$$D(Z) = 1\left(Z'\theta + U^{D} \geq 0\right)$$

where D* is a latent variable that represents the participation decision by each individual and D is a dichotomous variable that indicates whether the individual is observed as participant (also known as treatment, D=1) or as non-participant (control, D=0). Vectors X and Z contain observable characteristics that are assumed to influence the variable under study and the participation in the program. Actually, Z usually includes all the variables in X plus some additional variables.[30] Finally, U^{1}, U^{0} and U^{D} represent unobservable factors that also affect the variable under study and the participation in the program.

Assuming that the errors in the linear models are jointly distributed as a trivariate normal:

$$\begin{pmatrix} U^{D} \\ U^{1} \\ U^{0} \end{pmatrix} \sim N\left(0, \begin{bmatrix} 1 & \sigma_{1D} & \sigma_{0D} \\ \sigma_{1D} & \sigma_{1}^{2} & \sigma_{10} \\ \sigma_{0D} & \sigma_{10} & \sigma_{2}^{2} \end{bmatrix}\right)$$

then, Heckman, Tobias and Vytlacil (2003) show that the average treatment effect (ATE) can be estimated using the following formula:

$$ATE(x) = E\left[Y^{1} - Y^{0} \middle| X = x\right] = x'(\hat{\beta}^{1} - \hat{\beta}^{0})$$

where $\hat{\beta}^{j=0,1}$ are the estimated coefficients from separate regressions of the outcome equations corrected by selection (i.e., after including the selection correction term, the inverse Mill’s ratio, as an explanatory variable). The average treatment effect on the treated (ATT) can be estimated with:

$$ATT(x, z, D[z] = 1) = x'(\hat{\beta}^{1} - \hat{\beta}^{0}) + \left(\hat{\lambda}^{1} - \hat{\lambda}^{0}\right)\frac{\phi\left(z\hat{\theta}\right)}{\Phi\left(z\hat{\theta}\right)}$$

where $\phi(.)$ and $\Phi(.)$ stand for the density and cumulative standardized normal distributions, and $\hat{\lambda}^{j=0,1}$ are the estimated coefficients from the selection

[30] These additional variables are known in the literature as “exclusion restrictions” because they are interpreted as valid instruments or allow for identification of a selection correction model.

correction term in the regressions of outcomes Y^1 and Y^0. Notice that if there were no selection bias, $\hat{\lambda}^{j=0,1}$ would be not significantly different from zero and then ATE would equal the ATT.

Then, according to Heckman, Tobias and Vytlacil (2003), one can consistently estimate these parameters as follows:

i. Obtain $\hat{\theta}$ from a probit model (using Z as explanatory variables) on the decision to take the treatment.

ii. Compute the appropriate selection correction terms evaluated at $\hat{\theta}$. That is:

$$\frac{\phi(Z_i{}'\hat{\theta})}{\Phi(Z_i{}'\hat{\theta})} \quad if \quad D_i = 1$$

$$\frac{\phi(Z_i{}'\hat{\theta})}{1-\Phi(Z_i{}'\hat{\theta})} \quad if \quad D_i = 0$$

iii. Run treatment-outcome-specific regressions from the groups {i:Di(Z)=1} and {i:Di(Z)=0} with inclusion of the appropriate selection-correction terms obtained from the previous step.

iv. Given $\hat{\beta}^1$, $\hat{\beta}^0$, $\hat{\lambda}^1$ and $\hat{\lambda}^0$ from step iii and $\hat{\theta}$ from step i, use these parameter estimates to obtain point estimates of the treatment parameters explained above for given X, Z and Z'.

Since we are estimating the program impact upon both employment and wages, we stipulate the following protocol for estimating the parameters of interest.

C. A model for program participation

Assume individuals choose to participate into the PROBECAT/SICAT program if they find a higher utility from participating than from not participating. Then a linear random utility model:

$$U_i^p = Z_i\theta^p + \varepsilon_i^p \quad if \quad participating$$

$$U_i^{np} = Z_i\theta^{np} + \varepsilon_i^{np} \quad if \quad not \quad participating$$

Utilities are not observed, but we do observe participation according to the following rule:

$$v_i = \begin{cases} 1 & if \quad U_i^p > U_i^{np} \\ 0 & if \quad U_i^p \le U_i^{np} \end{cases}$$

Then, the probability of participating can be formalized as follows:

$$\begin{aligned} \Pr ob[v_i = 1] &= \Pr ob[U_i^p > U_i^{np}] \\ &= \Pr ob[Z_i(\theta^p - \theta^{np}) + (\varepsilon_i^p - \varepsilon_i^{np}) > 0] \\ &= \Pr ob[Z_i\theta + \varepsilon_i > 0] \\ &= \Pr ob[-\varepsilon_i < Z_i\theta] \\ &= F[-\varepsilon_i < Z_i\theta] \end{aligned}$$

where F(.) is a cumulative distribution function. Hence, the conditional expectation of the probability of participating is:

$$\begin{aligned} E[v|Z] &= 0[1 - F(Z\theta)] + 1[F(Z\theta)] \\ &= F(Z\theta) \end{aligned}$$

Assuming the errors of the random utility model are standard normal distributed, we get a probit model for participation:

$$E[v|Z] = \Phi(Z'\theta)$$

We run this model with the following specification:

$$E[v|Z] = \Phi(X'\theta_X + \theta_z z)$$

X is a N*k matrix of data including the following k variables: gender, age, kinship, school level, geographic region, state GDP per head, state demographic density, state program expenditures per beneficiary. The N*1 vector z includes our exclusion restriction: state beneficiaries per unemployed and quarter. An example of this model is shown in Annex 4.[31]

[31] We run separate models for individuals interviewed 13 and 26 weeks after beginning of training or unemployment. Besides, separate models are run for participation into program modalities for salaried employment (i.e. school-based or mixed) as well as for participation into program

With the estimated parameters from these probit models, we get the selection correction terms for those participating and not participating. That is:

$$\begin{cases} o_i^1 = \dfrac{\phi\left(X_i{}' \hat{\theta}_X + \hat{\theta}_z z_i \right)}{\Phi\left(X_i{}' \hat{\theta}_X + \hat{\theta}_z z_i \right)} \quad if \quad v_i = 1 \\ \\ o_i^0 = \dfrac{\phi\left(X_i{}' \hat{\theta}_X + \hat{\theta}_z z_i \right)}{1 - \Phi\left(X_i{}' \hat{\theta}_X + \hat{\theta}_z z_i \right)} \quad if \quad v_i = 0 \end{cases}$$

D. A model for employment effects of the program

Following an analogous argument to the one presented above, the conditional expectation of the probability of having a job, controlling for program participation, for both those who participated ($v_i=1$) and those who did not ($v_i=0$) can be formalized as follows:

$$E\left[y \middle| X^v, o^v \right] = \Phi\left(X'\gamma_X^v + \gamma_o^v o^v \right) \qquad v = 0,1$$

where:

$$\begin{cases} y_i = 1 \quad if \quad employed \quad after \quad 13/26 \quad weeks \\ \\ y_i = 0 \quad if \quad unemployed \quad after \quad 13/26 \quad weeks \end{cases}$$

The explanatory variables include the same variables than the model for participation without the exclusion restriction (i.e., vector X) plus the selection correction terms (i.e., $o^{v=0,1}$). In other words we run two selection corrected probit models[32]: one for participants and another for non-participants. With the estimated parameters from these probit models, we can proceed to compute the

modalities for self-employment (i.e. local emplomnt intiativs, ILE and self-emploment). All models are avaliblable from the authors upon request.

[32] This model was first suggested by Van de Ven and Van Pragg (1981)

employment treatment effects of the program. An example of these models is shown in Annex 5.

From the previous estimation we obtain the selection correction term for those who have a wage job, and distinguish between those who participated in the program and those who did not. Formally:

$$\begin{cases} p_i^1 = \dfrac{\phi\left(X_i' \hat{\gamma}_X^1 + \hat{\gamma}_o^1 o_i^1 \right)}{\Phi\left(X_i' \hat{\gamma}_X^1 + \hat{\gamma}_o^1 o_i^1 \right)} & if \quad y_i = 1 \\ \\ p_i^0 = \dfrac{\phi\left(X_i' \hat{\gamma}_X^0 + \hat{\gamma}_o^0 o_i^0 \right)}{\Phi\left(X_i' \hat{\gamma}_X^0 + \hat{\gamma}_o^0 o_i^0 \right)} & if \quad y_i = 0 \end{cases}$$

E. A model for the wage effect of the program

Finally, we model the conditional expectation of the logarithmic wage, running two selection corrected models: one for participants and one for non-participants.

$$E\left[w \middle| M^y \right] = M^y \varphi^y = M\beta^y + p^y \lambda^y \qquad y = 0,1$$

where w stands for the logarithm of real monthly wage and M^y=[M p^y], y=0,1, is a matrix including the same variables than the model for participation (i.e., matrix X) and additional controls affecting wages (e.g. hours of work, firm size, economic activity, employment function and formal/informal sector) plus the selection correction terms p^y.

Notice that in the model for wages, we correct for participation in the labor force as is usually done in order to prevent the coefficients of the wage equation to be inconsistent due to selection bias. In addition, and in accordance with the methodology suggested by Heckman, Tobias y Vytlacil (2003, 2001) we use a selection correction term for those who participated in the program and another for those who did not participate in the program. That is, both p^1 and p^0, are selection correction terms for participating in the labor market, but the former corresponds to those who participated in PROBECAT-SICAT, and the latter for those who did not participate in the program. Therefore, we control for both program selection and labor market participation.[33]. An example of these models is shown in Annex 6.

F. Estimation of unconditional treatment effects

We compute the treatment effects according to the formulas from Heckman, Tobias and Vytlacil (2003). Since we are measuring the effect of the program upon two variables (i.e. the probability of having a job 13/26 weeks after finishing the training and the wage gap for those who have a have a job 13/26 weeks after finishing the training), we compute the treatment effects for each variable separately.

The average treatment effect (ATE) for the probability of having a job will be:

$$ATE = \frac{1}{N}\sum_{i=1}^{N} ATE(X_i, o_i) = \frac{1}{N}\sum_{i=1}^{N}\left[\Phi\left(X_i{}'\hat{\gamma}_X^1\right) - \Phi\left(X_i{}'\hat{\gamma}_X^0\right)\right]$$

The average treatment effect on the treated (ATT) for the probability of having a job will be:

$$ATT = \frac{\sum_{i=1}^{N} y_i ATT(Z_i, o_i)}{\sum_{i=1}^{N} y_i} = \frac{1}{\sum_{i=1}^{N} y_i}\sum_{i=1}^{N} y_i\left\{\Phi\left(X_i{}'\hat{\gamma}_X^1 + \hat{\gamma}_o^1 o_i^1\right) - \Phi\left(X_i{}'\hat{\gamma}_X^0 + \hat{\gamma}_o^0 o_i^0\right)\right\}$$

The average treatment effect (ATE) for the wage effect will be:

$$ATE = \frac{1}{N}\sum_{i=1}^{N} ATE(M_i) = \frac{1}{N}\sum_{i=1}^{N} M_i\left(\hat{\beta}^1 - \hat{\beta}^0\right) = \overline{M}\left(\hat{\beta}^1 - \hat{\beta}^0\right)$$

The average treatment effect on the treated (ATT) for the wage effect will be:

$$ATT = \frac{\sum_{i=1}^{N} y_i ATT(M_i, p_i^y)}{\sum_{i=1}^{N} y_i} = \frac{1}{\sum_{i=1}^{N} y_i}\sum_{i=1}^{N} y_i\left\{M_i\left(\hat{\beta}^1 - \hat{\beta}^0\right) + \left(\hat{\lambda}^1 - \hat{\lambda}^0\right)p_i^1\right\}$$

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[33] The problem of this double selection mechanism affecting the evaluation of PROBECAT-SICAT was first noted by Wodon and Minowa (1997). See section 0, page 13.

Annex 1: Probability model for taking a training course

(independent variable:1= training course during last year, 0=no course

		number of observations		629211
		Wald chi2(28)		70007.3
		Prob>chi2		0
Log pseud likelihood	-143336.1	Pseudo R2		0.195

	dF/dx	Robust Std. Err.	z	P>|z|
Gender				
female	0.035	0.001	57.480	0.000
Age group				
15 or less	-0.048	0.000	-42.680	0.000
16 to 25 (omit)				
26 to 35	0.144	0.002	88.470	0.000
36 to 45	0.122	0.004	40.530	0.000
46 to 55	0.289	0.010	43.110	0.000
56 and more	0.059	0.015	5.300	0.000
Marital status				
single (omit)				
married	0.000	0.001	0.580	0.562
without couple	-0.015	0.001	-10.460	0.000
Kinship				
head (omit)				
spouse	0.030	0.001	26.140	0.000
son-daughter	-0.005	0.001	-5.490	0.000
next of kin	-0.019	0.001	-16.750	0.000
no kinship	-0.002	0.004	-0.490	0.623
Schooling				
a_dprim*	0.027	0.001	22.990	0.000
a_dprepa*	0.063	0.001	63.990	0.000
a_dsup~r*	0.056	0.001	51.600	0.000
Region				
D.F. (omit)				
golfo*	0.062	0.002	55.350	0.000
norte*	0.098	0.002	89.850	0.000
pacifico*	0.078	0.002	60.070	0.000
sur*	0.013	0.001	10.010	0.000
centro~e*	0.061	0.002	49.160	0.000
centro*	-0.017	0.001	-15.320	0.000
experience	-0.011	0.000	-76.670	0.000
experience squared	0.000	0.000	43.040	0.000
part_t~e*	-0.030	0.001	-42.190	0.000
razon_~o*	0.017	0.001	29.990	0.000
durm_1*	0.076	0.002	38.820	0.000
dur1_2*	0.065	0.002	37.330	0.000
dur3_6*	0.027	0.002	13.540	0.000
obs. P	0.081733			
pred. P	0.046907 (at x-bar)			

Source: author's calculation using ENECE 1999

Annex 2: Probit model for taking the program, selected observations, year 2002

Probit regression					Number of obs = 3670	
					LR chi2(25) = 969.46	
					Prob > chi2= 0	
Log likelihood = -2057.7566					Pseudo R2 = 19%	
capacita	**Coef.**	**Std. Err.**	**z**	**P>z**	**[95% Conf. Interval]**	
Hombre (Base= Mujer)	-0.7773	0.0530	-14.67	0	-0.8812	-0.6734
Conyuje (Base=Jefe del Hogar)	0.4462	0.0872	5.12	0	0.2753	0.6170
Hijo	-0.2295	0.0859	-2.67	0.008	-0.3978	-0.0611
Pariente	-0.1588	0.1043	-1.52	0.128	-0.3632	0.0456
No Pariente	1.0356	0.2127	4.87	0	0.6187	1.4525
Soltero (Base= Casado)	-0.1686	0.0795	-2.12	0.034	-0.3245	-0.0128
Sin Pareja	-0.4281	0.1141	-3.75	0	-0.6518	-0.2044
De 12 a 15 años (Base= de 16 a 25)	-2.0375	0.3712	-5.49	0	-2.7650	-1.3100
De 25 a 35 años	0.0162	0.0964	0.17	0.866	-0.1727	0.2051
De 36 a 45 años	-0.0297	0.1695	-0.18	0.861	-0.3620	0.3026
De 46 a 55 años	0.0765	0.2512	0.3	0.761	-0.4159	0.5689
De 56 y mas	-0.0772	0.4191	-0.18	0.854	-0.8985	0.7441
Sin Educacion (Base= Secundaria)	-0.5715	0.2358	-2.42	0.015	-1.0336	-0.1094
Primaria	-0.0602	0.0705	-0.85	0.393	-0.1983	0.0780
Preparatoria	-0.0014	0.0661	-0.02	0.983	-0.1310	0.1282
Educacion Tecnica	-0.2654	0.0831	-3.19	0.001	-0.4283	-0.1025
Profesional	-0.9008	0.0953	-9.45	0	-1.0876	-0.7140
Norte (Base= Capital)	-0.4373	0.0957	-4.57	0	-0.6249	-0.2498
Golfo	-0.1029	0.1042	-0.99	0.323	-0.3071	0.1012
Pacifico	-0.0943	0.1050	-0.9	0.369	-0.3001	0.1116
Sur	0.5325	0.1127	4.73	0	0.3117	0.7533
Centro-Norte	0.0970	0.0991	0.98	0.327	-0.0971	0.2912
Centro	-0.2132	0.1126	-1.89	0.058	-0.4339	0.0076
Años de Experiencia	-0.0401	0.0113	-3.55	0	-0.0623	-0.0180
Experiencia al cuadrado	0.0005	0.0002	2.43	0.015	0.0001	0.0010
Constante	1.1495	0.1431	8.03	0	0.8691	1.4299

Annex 3

Inferior of block of pscore	Treatment	Control	Total
0	275	47	322
0.2	354	105	459
0.3	372	182	554
0.4	451	415	866
0.6	127	289	416
0.7	108	360	468
0.8	68	487	555
Total	**1,755**	**1,885**	**3,640**

Annex 4

Participation Equation into program modalities for salaried employment among those interviewed 26 weeks after beginning of training (independent variable: (1) participant, (0) non-participant)

	2000		2001		2002		2003		2004	
	dF/dx	p-value	dF/dx	p-value	dF/dx	p-value	dF/dx	p-value	dF/dx	p-value
Gender:										
woman (omit)										
man	-0.057	0.000	-0.117	0.000	-0.200	0.000	-0.124	0.000	-0.139	0.000
Marital status:										
married (omit)										
single	0.057	0.000	-0.082	0.002	-0.194	0.000	-0.165	0.000	-0.192	0.000
without couple										
Kinship:										
son / daughter	-0.040	0.008	-0.137	0.000	-0.129	0.002	-0.042	0.175	-0.038	0.392
spouse	0.090	0.000	0.118	0.000	-0.018	0.712	-0.066	0.065	0.044	0.258
other	-0.168	0.000	-0.134	0.001	0.050	0.329	-0.004	0.923	0.067	0.157
household head (omit)										
Number of household dependents			-0.122	0.000	-0.185	0.000	-0.163	0.000	-0.194	0.000
Age:										
12 to 15	-0.636	0.000	-0.616	0.000	-0.527	0.000	-0.407	0.001		
16 to 25 (omit)										
26 to 35	0.009	0.357	0.006	0.793	-0.001	0.981	0.067	0.003	0.107	0.000
36 to 45	-0.009	0.502	-0.003	0.937	-0.181	0.000	0.053	0.085	0.055	0.161
46 to 55	-0.058	0.007	-0.205	0.000	-0.382	0.000	-0.014	0.791	-0.057	0.358
56 or more	-0.194	0.000	-0.218	0.005	-0.546	0.000	-0.413	0.000	-0.250	0.033
Schooling:										
no schooling	0.053	0.012	-0.138	0.113	-0.364	0.015	-0.274	0.010	-0.337	0.009
primary school	-0.021	0.031	-0.005	0.845	-0.008	0.827	-0.055	0.060	-0.095	0.019
junior high school (omit)										
high school	-0.054	0.000	-0.067	0.001	-0.054	0.083	0.023	0.319	-0.021	0.472
graduate school	-0.713	0.000	-0.398	0.000	-0.224	0.000	0.034	0.096	0.023	0.445
Region:										
Capital (omit)										
North	-0.036	0.078	-0.002	0.954	-0.162	0.005	-0.111	0.001	-0.226	0.000
Gulf	-0.020	0.313	-0.058	0.198	-0.110	0.119	-0.358	0.000	-0.272	0.000
Pacific	-0.262	0.000	0.057	0.126	-0.017	0.807	-0.313	0.000	-0.223	0.000
South	-0.094	0.001	-0.073	0.155	0.209	0.003	-0.304	0.000	-0.114	0.031
Center-North	-0.009	0.662	-0.004	0.933	0.014	0.846	-0.229	0.000	-0.285	0.000
Center	-0.190	0.000	0.021	0.669	-0.053	0.495	-0.179	0.000	-0.187	0.001
Labor market										
experienced	0.170	0.000	0.287	0.000	-0.283	0.000	-0.088	0.000	-0.134	0.000
not experienced (omit)										
Beneficiary per unemployed [3]	0.000	0.325	0.005	0.000	0.008	0.000	0.008	0.000	0.007	0.000
Program budget per beneficiary [1]	0.007	0.620	0.019	0.363	-0.033	0.087	-0.024	0.059	0.087	0.000
GDP per head [2]	0.228	0.168	0.393	0.388	3.272	0.000	-0.363	0.520		
Quarters										
First (omit)										
Second	0.034	0.000	-0.070	0.003	-0.153	0.000	0.045	0.019	0.021	0.410
Third	-0.117	0.000	-0.042	0.068						
Fourth	0.074	0.000	-0.386	0.000						
Number of Observations	9402		5118		2665		3175		2452	
Obs-P	0.832		0.659		0.529		0.698		0.651	
Pred-P	0.912		0.741		0.541		0.765		0.717	
Pseudo R-squared	0.396		0.448		0.423		0.390		0.411	
P>Chi-Squared	0.000		0.000		0.000		0.000		0.000	

Source:
authors´calculations using PROBECATand ENEU databases

Notes:
1 state average (in million pesos per month)
2 state average (in billion pesos a year)
3 state average (number of program beneficiaries divided by number of unemployed in the state)

Annex 5

Employment Equation for Salaried workers 26 weeks since beginning of training and controling for participation in the Program (independent variable: (1) employed, (0) not-employed)

	2000				2001				2002				2003				2004			
	treatment		control		treatment		control		treatment		control		treatment		control		treatment		control	
	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value
Gender:																				
woman (omit)																				
man	0.11	0.01	0.36	0.00	0.26	0.00	0.28	0.00	0.31	0.00	0.24	0.03	0.20	0.01	0.19	0.09	0.17	0.09	0.30	0.03
Marital status:																				
married (omit)																				
single	0.06	0.47	-0.12	0.31	0.17	0.09	-0.45	0.00	0.02	0.90	-0.40	0.01	0.12	0.26	0.05	0.77	-0.01	0.97	-0.06	0.75
without couple	-0.01	0.95	-0.01	0.97	0.36	0.02	-0.24	0.15	0.22	0.32	-0.22	0.30	-0.26	0.10	0.39	0.13	-0.03	0.89	0.11	0.67
Kinship:																				
household head (omit)																				
son / daughter	-0.28	0.00	0.02	0.86	0.16	0.18	0.19	0.16	-0.39	0.01	-0.82	0.00	-0.28	0.02	-0.53	0.01	-0.38	0.01	-0.06	0.80
spouse	-0.36	0.00	-0.42	0.01	-0.37	0.00	-0.73	0.00	-0.29	0.03	-0.13	0.41	-0.27	0.01	-0.52	0.01	-0.28	0.03	-0.19	0.35
other	0.01	0.94	0.08	0.63	0.39	0.01	0.03	0.86	-0.30	0.04	-0.12	0.52	-0.10	0.41	-0.31	0.16	-0.28	0.08	-0.44	0.08
Number of household dependents	Na	Na	Na	Na	0.21	0.00	-0.06	0.10	0.12	0.06	0.00	0.93	0.13	0.04	-0.01	0.83	0.04	0.59	0.03	0.54
Age:																				
12 to 15	-0.34	0.51	0.07	0.85	Na	Na	-0.63	0.06	Na	Na	-0.65	0.05	-0.75	0.27	-0.60	0.09	Na	Na	Na	Na
16 to 25 (omit)																				
26 to 35	-0.15	0.00	0.07	0.44	0.18	0.01	-0.12	0.17	-0.02	0.86	0.00	0.99	-0.22	0.01	-0.07	0.59	-0.20	0.05	-0.05	0.71
36 to 45	-0.31	0.00	-0.04	0.75	-0.07	0.53	-0.21	0.10	-0.16	0.32	-0.27	0.09	-0.35	0.00	0.06	0.72	-0.23	0.14	-0.07	0.72
46 to 55	-0.54	0.00	-0.35	0.05	-0.45	0.03	-0.69	0.00	-0.48	0.10	-0.22	0.29	-0.24	0.22	-0.32	0.20	-1.30	0.00	-0.34	0.20
56 or more	-0.23	0.42	-0.19	0.51	Na	Na	-0.43	0.09	Na	Na	-0.83	0.01	-0.72	0.12	-1.42	0.01	-0.69	0.21		
Schooling:																				
no schooling	-0.40	0.03	-0.36	0.20	0.56	0.13	-0.71	0.05	Na	Na	Na	Na	-0.18	0.72	-1.02	0.06	0.51	0.34	-0.19	0.63
primary school	-0.04	0.41	-0.31	0.01	0.02	0.81	-0.25	0.01	-0.19	0.06	-0.72	0.09	0.08	0.46	-0.21	0.12	0.13	0.43	-0.31	0.06
junior high school (omit)	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na	-0.32	0.01	Na	Na	Na	Na	Na	Na	Na	Na
high school	0.05	0.23	0.16	0.07	-0.17	0.01	0.06	0.44	-0.05	0.55	0.16	0.13	0.05	0.51	0.27	0.03	-0.11	0.25	0.06	0.64
graduate school	0.42	0.06	0.68	0.00	-0.06	0.58	0.16	0.19	-0.12	0.39	0.06	0.64	-0.02	0.78	0.38	0.00	-0.12	0.23	0.12	0.40
Region:																				
Capital (omit)																				
North	0.19	0.04	0.13	0.53	0.64	0.00	0.79	0.00	0.28	0.15	0.30	0.11	0.62	0.00	0.25	0.17	0.61	0.00	0.73	0.00
Gulf	0.02	0.75	-0.06	0.76	0.24	0.18	0.38	0.04	0.32	0.14	0.33	0.13	0.97	0.00	-0.02	0.93	1.04	0.00	0.56	0.03
Pacific	0.30	0.01	0.40	0.06	0.65	0.00	0.34	0.04	-0.07	0.75	0.21	0.33	0.99	0.00	-0.09	0.67	0.84	0.00	0.40	0.11
South	-0.20	0.03	-0.17	0.44	0.09	0.65	-0.16	0.50	-0.73	0.00	-0.39	0.16	-0.13	0.41	-0.31	0.28	-0.08	0.63	0.33	0.25
Center-North	0.03	0.71	0.12	0.55	0.46	0.01	0.65	0.00	0.06	0.77	0.14	0.53	1.09	0.00	0.10	0.66	0.65	0.00	0.67	0.00
Center	0.02	0.83	0.03	0.89	0.04	0.85	0.25	0.30	-0.20	0.40	0.02	0.95	0.30	0.04	-0.21	0.37	0.39	0.04	0.08	0.79
Labor market																				
not-experienced (omit)																				
experienced	0.55	0.00	0.84	0.00	0.33	0.00	0.81	0.00	0.42	0.00	0.51	0.00	0.21	0.01	0.42	0.02	0.25	0.03	0.29	0.15
Program budget per beneficiary [1]	-0.30	0.00	-0.31	0.04	-0.03	0.68	0.03	0.73	-0.08	0.17	-0.02	0.78	0.03	0.51	-0.01	0.83	-0.13	0.06	-0.01	0.86
GDP per head [2]									1.03	0.65	0.32	0.89	5.75	0.00	-2.84	0.31	Na	Na	Na	Na
First (omit)																				
Second	-0.25	0.00	-0.29	0.00	-0.48	0.00	-0.20	0.05	0.12	0.13	-0.06	0.46	-0.32	0.00	-0.09	0.40	-0.30	0.01	-0.11	0.35
Third	0.17	0.03	0.18	0.10	-1.37	0.00	-0.19	0.14	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na
Fourth	-0.32	0.00	-0.47	0.00	-0.63	0.00	-0.08	0.37	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na
Inverse Mills Ratio	-0.49	0.00	0.63	0.00	-0.63	0.00	0.50	0.03	-0.04	0.85	0.50	0.04	-0.06	0.76	-0.08	0.75	0.22	0.37	0.14	0.61
Constant	0.23	0.32	-1.40	0.04	-0.72	0.07	-1.81	0.01	-0.10	0.80	-0.99	0.05	-1.13	0.00	-0.68	0.26	-0.53	0.06	-1.60	0.01
Number of Observations	7964		1839		3240		2104		1405		1254		2216		959		1596		836	
Pseudo R-squared	0.06		0.08		0.17		0.10		0.13		0.08		0.14		0.08		0.14		0.05	
Chi-Squared	548		162		596		252		245		118		389		87		263		45	

Source:
authors´calculations using PROBECAT, ENEU and ENECE databases

Notes:
1 state average (in million pesos per month)
2 state average (in billion pesos a year)

Annex 6

Wage Equation for Salaried workers 26 weeks since beginning of training and controling for selection into labor force (independent variable: log monthly wage in pesos)

	2000				2001				2002				2003				2004			
	treatment		control		treatment		control		treatment		control		treatment		control		treatment		control	
	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value	coef.	p-value
Gender:																				
woman (omit)																				
man	-0.02	0.50	0.19	0.00	0.10	0.00	0.12	0.00	0.13	0.00	0.18	0.00	0.06	0.05	0.05	0.79	0.14	0.00	-0.35	0.08
Marital status:																				
married (omit)																				
single	-0.01	0.93	-0.07	0.24	0.02	0.67	-0.08	0.11	0.00	0.90	-0.01	0.94	-0.02	0.55	-0.14	0.11	-0.07	0.11	-0.04	0.67
without couple	-0.04	0.58	0.05	0.59	-0.17	0.01	-0.06	0.39	0.02	0.79	-0.04	0.68	-0.05	0.46	-0.31	0.35	0.07	0.42	-0.33	0.03
Kinship:																				
household head (omit)																				
son / daughter	-0.02	0.83	-0.09	0.22	-0.12	0.02	-0.09	0.07	-0.13	0.00	0.09	0.15	-0.03	0.44	0.10	0.82	-0.03	0.57	0.30	0.06
spouse	-0.01	0.88	-0.14	0.16	0.02	0.66	-0.12	0.18	-0.06	0.28	0.22	0.08	-0.08	0.13	0.10	0.83	-0.06	0.44	-0.07	0.53
other	-0.09	0.22	-0.13	0.13	-0.13	0.02	-0.04	0.53	-0.13	0.01	0.04	0.60	0.01	0.83	0.08	0.75	-0.03	0.63	0.66	0.05
Age:																				
12 to 15	-0.25	0.58	-0.17	0.36	Na	Na	-0.20	0.10	Na	Na	-0.01	0.94	-0.58	0.03	-0.08	0.89	Na	Na	Na	Na
16 to 25 (omit)																				
26 to 35	0.10	0.03	0.16	0.00	0.02	0.47	0.09	0.02	0.02	0.52	0.07	0.09	0.04	0.17	0.17	0.06	0.18	0.00	0.16	0.03
36 to 45	0.16	0.06	0.11	0.11	0.09	0.06	-0.01	0.86	-0.05	0.28	0.28	0.00	0.06	0.17	0.14	0.14	0.11	0.07	0.24	0.01
46 to 55	0.42	0.01	0.10	0.45	0.30	0.00	0.04	0.70	-0.14	0.26	0.27	0.00	0.10	0.17	0.17	0.56	0.33	0.05	0.53	0.06
56 or more	0.17	0.45	0.15	0.46	Na	Na	-0.07	0.52	Na	Na	0.15	0.42	-0.70	0.00	0.55	0.67	0.23	0.30	Na	Na
Schooling:																				
no schooling	0.01	0.97	-0.27	0.14	-0.24	0.07	-0.14	0.31	Na	Na	0.13	0.49	0.04	0.80	-0.06	0.95	-0.14	0.56	-0.08	0.72
primary school	0.06	0.12	-0.16	0.14	-0.09	0.01	-0.01	0.86	-0.05	0.19	-0.03	0.61	0.00	0.96	-0.02	0.94	-0.08	0.21	0.53	0.03
junior high school (omit)																				
high school	0.07	0.01	0.10	0.05	0.08	0.01	0.08	0.04	0.10	0.00	0.08	0.09	0.06	0.03	-0.02	0.94	0.11	0.01	-0.11	0.12
graduate school	0.03	0.76	0.32	0.00	0.22	0.00	0.27	0.00	0.10	0.01	0.30	0.00	0.10	0.00	0.07	0.81	0.39	0.00	0.02	0.84
Region:																				
Capital (omit)																				
North	0.07	0.37	0.02	0.84	0.14	0.11	-0.16	0.05	0.01	0.91	0.11	0.20	-0.17	0.01	0.05	0.84	-0.07	0.44	-1.02	0.05
Gulf	-0.16	0.01	-0.09	0.36	-0.09	0.29	-0.32	0.00	-0.18	0.02	-0.07	0.46	-0.34	0.00	-0.04	0.71	-0.13	0.34	-0.88	0.03
Pacific	-0.08	0.31	-0.04	0.67	0.07	0.45	-0.20	0.01	-0.20	0.01	0.07	0.49	-0.38	0.00	0.00	0.99	-0.16	0.18	-0.49	0.09
South	0.96	0.00	-0.32	0.01	0.06	0.54	-0.40	0.00	-0.46	0.00	-0.04	0.73	-0.45	0.00	0.12	0.68	-0.16	0.02	-0.59	0.02
Center-North	0.07	0.31	-0.10	0.42	0.10	0.30	-0.25	0.00	-0.16	0.03	0.13	0.14	-0.35	0.00	-0.06	0.66	-0.19	0.05	-1.16	0.02
Center	-0.19	0.01	-0.18	0.10	0.04	0.67	-0.37	0.00	-0.18	0.03	-0.01	0.93	-0.38	0.00	-0.04	0.86	-0.21	0.03	-0.13	0.31
Economic Activity:																				
agriculture (omit)																				
mining	-1.14	0.05	0.21	0.51	0.38	0.26	Na	Na	0.43	0.23	0.27	0.29	0.38	0.00	-0.07	0.31	Na	Na	0.05	0.56
manufacture	0.00	0.96	0.13	0.38	0.19	0.01	0.12	0.35	0.16	0.28	0.27	0.04	0.11	0.76	0.43	0.33	0.04	0.59	0.37	0.14
building	0.00	0.98	0.17	0.29	0.28	0.01	0.17	0.18	0.37	0.06	0.38	0.00	-0.04	0.16	-0.22	0.00	0.19	0.03	-0.10	0.10
water, gas and electricity	0.09	0.45	0.54	0.28	0.41	0.01	0.59	0.06	0.25	0.36	0.98	0.00	0.27	0.00	0.07	0.50	Na	Na	0.00	0.97
restaurants and hotels	0.00	0.99	-0.01	0.94	0.09	0.28	0.02	0.87	0.18	0.22	0.25	0.05	0.02	0.61	-0.15	0.02	0.01	0.79	-0.09	0.15
transport and storage	0.19	0.04	0.22	0.19	0.22	0.04	0.25	0.07	0.61	0.00	0.32	0.03	Na	Na	Na	Na	Na	Na	Na	Na
financial services	0.25	0.01	0.34	0.13	0.37	0.02	-0.02	0.93	0.44	0.03	0.57	0.02	Na	Na	Na	Na	Na	Na	Na	Na
communal services	-0.02	0.77	0.01	0.97	0.18	0.02	0.10	0.44	0.11	0.46	0.21	0.11	Na	Na	Na	Na	Na	Na	Na	Na
Labor market																				
not-experienced (omit)																				
experienced	-0.02	0.85	0.02	0.87	0.06	0.04	-0.02	0.77	0.07	0.09	-0.04	0.60	0.08	0.01	-0.16	0.67	0.15	0.00	-0.46	0.02
Program budget per beneficiary [1]	0.16	0.04	-0.23	0.01	0.11	0.00	-0.09	0.04	0.00	0.95	0.00	0.97	0.03	0.06	0.03	0.30	-0.01	0.00	-0.01	0.78
GDP per head [2]	1.95	0.00	0.39	0.72	0.64	0.39	0.65	0.47	-0.68	0.28	0.80	0.42	0.35	0.68	-0.43	0.87	Na	Na	Na	Na
Quarters																				
First (omit)																				
Second	-0.13	0.01	0.11	0.05	0.14	0.00	0.01	0.75	-0.04	0.17	0.03	0.41	0.01	0.73	-0.02	0.81	0.00	0.88	0.25	0.00
Third	-0.28	0.00	0.04	0.37	0.31	0.00	-0.05	0.17	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na
Fourth	-0.10	0.09	0.10	0.11	Na	Na	0.02	0.60	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na	Na
Inverse Mills Ratio	-0.50	-0.12	0.10	-0.74	-0.29	0.00	-0.06	-0.45	0.09	0.46	-0.31	0.07	-0.10	0.41	-0.39	0.73	-0.09	0.55	-2.30	0.01
Constant	7.70	0.00	7.96	0.00	7.16	0.00	8.27	0.00	7.56	0.00	7.60	0.00	7.92	0.00	8.48	0.00	7.56	0.00	12.12	0.00
Number of Observations	2903		817		821		951		853		598		1087		427		688		359	
Chi-Squared	0.18		0.23		0.29		0.18		0.28		0.26		0.27		0.23		0.35		0.27	
R-squared	0.17		0.19		0.26		0.15		0.25		0.22		0.22		0.17		0.32		0.21	

Source:
authors´calculations using PROBECAT, ENEU and ENECE databases

Notes:
1 state average (in million pesos per month)
2 state average (in billion pesos a year)

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