EVALUATING ENTERPRISE SUPPORT PROGRAMS USING PANEL FIRM DATA[1]

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Abstract

This paper evaluates enterprise support programs in Mexico using ten-year panel firm data (1994-2005), which allows for addressing selectivity bias from observed and unobserved firm heterogeneity by applying fixed effects models. The analysis finds evidence that participation in some enterprise programs such as training, tax breaks, and environmental certification is associated with improvements in key variables such as value added, gross production, and wages. Furthermore, the paper finds evidence that some of the positive effects can take several years to realize.

Keywords: fixed effect model, program impact evaluation, enterprise support programs

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1. INTRODUCTION

In most countries, small and medium size enterprises (SMEs) make up the vast majority of firms, and account for a substantial share of gross domestic product (GDP) and the workforce. However, SMEs often lag behind larger firms in many dimensions of performance. This is widely believed to result from constraints SMEs face, including access to finance, weak managerial and workforce skills, inability to exploit scale economies in production, and imperfect information about market opportunities, new technologies and methods of organization.

In Mexico, microenterprises and SMEs make up 99 percent of firms, employ about 64 percent of the workforce, and account for over 40 percent of GDP. Given the importance of SMEs in the economy, governments in Mexico over the last 20 years have put in place a wide variety of SME support programs. How effective these SME programs have been in achieving their objectives is unclear. In Mexico, impact evaluations of SME programs are rare—most are qualitative in nature and narrow in scope, either measuring beneficiary satisfaction with support services or program coverage.

This paper evaluates SME support programs in Mexico using a panel of firm-level data for two groups of firms—a treatment group that participated in SME programs and a control group that did not. The panel data is created by linking SME program participation information to a large panel of annual industrial surveys (1994-2005) maintained by Mexico's National Statistics Office (*Instituto Nacional de Estadística y Geografía*—INEGI).

The panel data provide an opportunity to address several issues that have plagued impact evaluations of SME programs in most countries, including Mexico. First, program participation questions in the firm survey allow us to identify a control group of firms that have never participated in any programs. Second, the availability of multiple years of information on the characteristics and performance of treated firms, both before and after program participation, allows us to estimate the impacts of support programs addressing selection biases arising from differences between the treatment and control groups in observable attributes and in unobserved heterogeneity. Third, unlike most evaluation studies that track participants for only a year or two after program completion, in our data some treatment firms are observed for as long as 10 years after program participation.

The paper finds that program participation in certain types of support programs is associated with higher value added, sales, exports, and employment. The positive impact associated with firm participation is strongest in the Sector Promotions Program (PROSEC) of the Ministry of Economy, the Fiscal Incentives and Technological Innovation of the National Science and Technology Council, the National Environmental Audit Program (PNAA), and in SME programs supported by state governments. These programs showed positive and statistically significant impacts on firm performance (value added, gross production, sales, hours worked), ranging from 4 to 17 percent. The results also indicate that some outcomes, such as employment and fixed assets, showed positive effects only after the third or fourth year following program participation, and the effect increases as time goes on.

The paper is organized as follows. Section 2 begins with an overview of SME programs in Mexico. Section 3 discusses approaches used to evaluate SME program, and Section 4 presents the data used in the analysis. Section 5 discusses the methodology employed in the paper, and Section 6 reviews the results. Section 7 offers conclusions and future lines of work.

2. SME PROGRAMS

The Mexican federal government supports the development and competitiveness of Mexican firms through improving the overall business environment, and through interventions to support individual firms or groups of firms, especially SMEs. Between 2001 and 2006, the Mexican government invested US$13 billion in about 3.7 million SMEs.[3]

Several agencies in Mexico provide SME support. The Ministry of Economy (ME) was first in terms of coverage, with more than one million firms benefited during 2001-2006, but fourth in terms of resources channeled to SMEs. The National Development Bank's (NAFINSA's) participation in the total budget of SME programs grew more than threefold from 2001 to 2006—from 23 percent to 70 percent—and the number of firms supported grew almost ten times—from 90,000 to 877,000. Banco Mexicano de Comercio Exterior (Bancomext) was second in terms of resources with 43 percent of the total, but fourth place in terms of the number of firms supported. Ministry of Labor and Social Protection (STPS) programs benefited 6 percent of total firms but with a small budget, while the National Council of Science and Technology (CONACyT) had a sizeable budget but reached relatively few firms.

Almost all SME programs explicitly or implicitly require that participating firms be located in Mexican territory and have majority or 100 percent Mexican capital. Most programs are targeted at formal sector enterprises, via the requirement that firms be legally incorporated in Mexico and registered with the tax authorities. Several programs have explicit sectoral criteria for participation (manufacturing, commerce, agriculture, etc.), while others have no sectoral preference. Most programs appear not to have merit-based criteria for participation, although a few require demonstrated solvency and a minimum of one to three years of operation, criteria that are not particularly binding for most formal sector SMEs. If program eligibility requirements are easily satisfied, it follows that most programs will fund every firm or group of firms that is eligible as long as funding lasts. Few programs require progress reports or impact assessments as part of their formal operating procedures, meaning few incentives for systematic measurements by participating SMEs are built into the operating procedures that govern provision of support.

The focus of SME programs in this paper is limited to the programs that yielded the larger samples in our panel data of the firm surveys, as described in Section 4. These programs—Program for Training the Industrial Workforce (CIMO-PAC), Fiscal Incentives and Technological Innovation, PROSEC, and PNAA—are described in some detail below. These programs vary in objectives, selection criteria (including eligibility criteria as well as size and sector of firms targeted), program sizes and operating procedures. Despite their differences,

[3] "Evaluating Small and Medium Enterprise Support Programs in Latin America," World Bank 2010, report No. 52668-LAC.

these programs share the common goal of subsidizing support services for SMEs. Apart from these four, the analysis also considers firm participation in SME programs provided by state governments, which encompasses a wide variety of program types. For a more complete survey of SME programs in Mexico, see World Bank (2010).

The STPS established CIMO-PAC as a pilot program in 1988 to support SMEs. CIMO-PAC's stated goal is improving SME productivity by increasing training levels, helping design training plans, and expanding the availability of training. CIMO does not directly provide training, but instead subsidizes the hiring of independent instructors to design and deliver training to SMEs. It also subsidizes the costs of producing training materials, developing training programs, and assessing workers' skills based on labor competency standards. CIMO encourages employers to spread training across a wide cross-section of the firm's workforce to ensure that the benefits of training are more evenly distributed. CIMO subsidizes up to 50 percent of the costs of training, subject to some restrictions on maximum expenditures. From 2001 to 2006, CIMO-PAC benefited about 1.6 million workers in about 227,000 firms, and subsidies amounted to US$75 million.

The Fiscal Incentives and Technological Innovation program of CONACyT, begun in 2001, is a policy directed towards taxpaying individuals and firms that invest in research and technology in order to develop new products, materials, and processes. The main objective of this program is to increase the annual investment and spending of firms for technological innovation. From 2001 to 2006, the program benefited 8,701 projects and 2,620 firms and provided tax incentives for about US$873 million.

Implemented by the Ministry of the Economy, the Sector Promotions Program (PROSEC) seeks to promote national industry and commerce, and is similar to the Fiscal Incentives and Technological Innovation program. This program also provides tax breaks for imported inputs needed for manufacturing. The imported products should be used for the production of goods, regardless of whether they were intended for internal or external markets. The program is divided into 22 sectors. The beneficiaries of PROSEC are all formal sector firms that manufacture goods using specific products described in PROSEC's decree. Unfortunately, information is not publicly available on the number or projects or firms which benefited from the program or the amount of subsidies.

The National Environmental Audit Program (PNNA) was created in 1992 under the supervision of the Federal Environmental Protection Attorney (PROFEPA). The main objective of the PNAA is to help protect the environment through certification of firms' environmentally-friendly processes. The program promotes environmental audits in firms in order to acquire knowledge about how their operations generate pollution and environmental risks, and how they could comply with environmental regulations and apply best practices through consultancy services. Between 2002 and 2006, the program tripled the number of audits initiated—from 293 in 2002 to 933 in 2006—and the number of clean industry certificates issued doubled in the same period, going from 169 in 2002 to 338 in 2006. Each firm's investment in PNAA seems to be low, but exact data are unavailable.

3. EVALUATIONS OF SME PROGRAMS

The extant literature evaluating enterprise programs is almost exclusively non-experimental. Starting with the U.S. in the late 1990s, impact evaluations of enterprise support programs using non-experimental designs have been conducted in a growing number of developed countries. While they differ in how they derive their non-experimental data, these studies share a strong focus on addressing the selection bias issues that arise with this evaluation design.

In the U.S., the Manufacturing Extensions Partnership (MEP) to promote industrial modernization of small enterprises was studied by Oldsman and Heye (1997), who compared the performance of a treatment group of MEP clients to a control group of non-MEP enterprises, matched by sector and firm size. Compared to the control group, the study found that MEP clients changed critical business practices, improved manufacturing performance, and increased value added as a direct result of the program. Jarmin (1999) further analyzed MEP and concluded that MEP clients enjoyed between 3 and 16 percent higher labor productivity growth over the 1987-1992 period than non-MEP firms.

Roper and Hewitt-Dundas (2001) examined the impact of grant support on small business performance in Ireland using similar non-experimental evaluations. The authors identified several regional clusters of firms that had received grant support, and a control group. They found that firms in assisted clusters tended to grow faster in terms of sales and be more profitable compared to non-assisted firms. However, selectivity-corrected estimates suggested that while grant assistance significantly boosted employment growth among assisted firms relative to the control group, assistance in general, as well as different mixes of support, did not have a statistically significant effect on sales growth or profitability.

In Japan, Motohashi (2001) used plant-level longitudinal survey data to identify companies that had participated in support programs sponsored by the government's Creative Business Activities (CAL) and a control group with similar observable attributes that did not participate. The study found evidence suggesting that CAL participation increased sales growth by 6.8 percent between 1996 and 1999.

Government-sponsored SME programs in developing countries are rarely evaluated, and those that are evaluated tend to involve beneficiary satisfaction surveys, small case studies, or simple regression analysis of outcomes without accounting for selectivity bias. While these approaches can provide some insights, for example into how service delivery can be improved, they cannot tell program administrators whether a program is leading to improvements in SME performance that would not otherwise have occurred. The exceptions are several recent impact evaluation studies, principally in Latin America,[4] that use non-experimental designs to rigorously evaluate a variety of SME interventions, taking into account selection bias.

[4] This literature review focuses only on studies of programs providing business development services to SMEs. Non-experimental impact evaluations of SME finance programs include a study by Jõeveer, Pissarides and Svejnar (2006) on the firm-level performance impacts of EBRD lending to micro, small and medium size companies in transition countries.

Benavente and Crespi (2003) evaluated Chile's Program of Development (PROFO), which seeks to boost the productivity of SME clusters by directing assistance in training, new management practices and technology upgrading to groups of firms rather than individual companies. The authors administered a survey to a random sample of firms participating in PROFO, and to a control group of firms in the same productive sectors and of similar size. Using a variety of econometric methods to address selectivity bias in program participation—including before-and-after comparisons, difference-in-difference (DID) techniques and DID with common support (propensity score matching)—they found significant net improvements in total factor productivity growth (TFP) ranging from 11.7 to 22.9 percent. Qualitative analysis of the survey of PROFO beneficiaries attributed many of these gains to reorganization of the production process, implementation of joint marketing strategies, introduction of quality control techniques, and managerial training.

In Mexico, Tan and Lopez-Acevedo (2005) used panel data (1991-1996) collected by the Ministry of Labor on two cohorts of CIMO participants and a control group to rigorously evaluate the net impacts of CIMO on SME performance. Two previous evaluations of CIMO by the ministry found seemingly contradictory results—evidence of improvements in intermediate outputs (worker training, production processes and adoption of quality control), but no or negative impacts on productivity. The authors tested the hypothesis that this derives from the self-selection of low productivity SMEs into the program using DID methods to remove pre-intervention productivity differences between groups, and found a sign reversal of the program impact coefficient, with CIMO participants enjoying a 6-11 percent net performance gain.

In a second Mexico study, Tan and Lopez-Acevedo (2007) evaluated CIMO and two other SME programs, CRECE and COMPITE, using three rounds of enterprise surveys with information on program participation linked to panel annual industry surveys. These data were used to identify different cohorts of treatment and control groups matched on propensity scores of pre-program attributes. All three programs showed net gains in training, adoption of new technology and use of quality control methods. While improvements in these intermediate outputs were supposed to translate into improved performance, DID estimates did not find statistically significant net gains in wages, export orientation or productivity. The authors conclude that larger sample sizes and a longer horizon may be needed to determine whether these interventions have the hypothesized positive performance impacts—themes that will be pursued in this paper.

Maffioli (2006) evaluated the impact of technology development funds (TDFs) in five Latin American countries—Argentina, Brazil, Chile, Colombia, and Panama—supporting research and development (R&D), innovation, and technology upgrading among enterprises, principally SMEs. Using non-experimental data drawn from innovation and industrial surveys linked to firm balance sheets and patent databases, Maffioli used a variety of econometric techniques (propensity scores matching, DID estimators, and instrumental variable models) to estimate TDF impacts, with mixed results. On the positive side, TDFs improved the capacity of firms to interact with external sources of knowledge and financing, helped increase R&D intensity in Argentina and Brazil, and did not crowd out private R&D funding. However, the study did not find statistically significant improvements in innovative outputs such as patent registrations or new product sales. Patent applications showed significant improvements only in one country, Brazil, where firm-university cooperation may have played a role. On firm performance, Maffioli found positive net gains in sales and employment growth, but no statistically significant impacts on firm-level productivity.

The Colombian government commissioned Econometria (2007) to evaluate FOMIPYME, a fund initiated in 2001 to promote modernization and technological development of SMEs by disbursing funds through a network of public and private operators that deliver support services to SMEs. Econometria first selected a set of operators from which it drew a sample of FOMIPYME participants stratified by sector, region and size, and a control group of non-participants. The study found positive net impacts on sales growth from training courses, on innovation as measured by R&D intensity and introduction of new production methods, and on human capital (share of skilled workers), particularly among firms that also invested in technology. It found that FOMIPYE increased formalization of enterprises, as measured by their propensity to pay taxes, with the implication that the program yielded additional fiscal benefits to the state. Some project lines—business startups, for example—were not effective, while others such as innovation, marketing, and mini-clusters network had positive effects.

In summary, a small but growing empirical literature in both high income and developing countries is rigorously evaluating the intermediate impacts and outcomes of enterprise participation in SME programs. All the studies have been non-experimental, and as such their focus has been on addressing the selectivity biases that might arise from such a design using control function models, propensity score matching and DID methods. Collectively, they find some evidence of negative selection of low productivity firms into SME programs, generally positive effects of treatment on intermediate impacts, but mixed results on longer-run outcomes.

4. DATA

The paper uses the National Employment Salary, Training and Technology (*Encuesta Nacional de Empleo, Salarios, Capacitación y Tecnología*—ENESTYC) and Annual Industry Survey (*Encuesta Industrial Annual*—EIA) surveys maintained by INEGI to create the non-experimental panel dataset.

The ENESTYC periodically surveys manufacturing firms, and was fielded in 1995, 1999, 2001, and 2005. The universe for the ENESTYC is the Economic Census, and its sampling design is probabilistic and stratified by 54 activity sectors and four size categories—micro with 1-15 employees, small with 16-100, medium with 101-250, and large with over 250. The sample size was around 8,200 establishments in 2001 and 7,500 in 2005. In 2001 and 2005, the ENESTYC included a module of questions on participation in major government SME support programs, including date of participation, duration, and type of services used. The 2005 ENESTYC dropped several SME programs that had since ceased operation, and included a number of other SME initiatives introduced since 2001.

The EIA is the annual manufacturing survey and uses the same sampling frame as the ENESTYC. Although its sampling design is not probabilistic, the EIA is highly representative of the manufacturing sector because it comprises 65 percent of occupied personal and 85 percent of gross value of manufacturing production. The sample size varies from 5,500 to 7,300 establishments.

A linked panel of establishments can be created over the 1994-2005 period from the annual surveys. It contains annual data on measures of firm performance such as sales, gross value of production, employment, total compensation, and income from exports, as well as some intermediate outputs that the programs may affect, such as technology transfers. The authors worked with INEGI on linking establishments from the ENESTYC with the EIA panel through an identification code constructed by INEGI. Our strategy was to link the 2001 and 2005 ENESTYC to the 1994-2005 EIA, to exploit the availability of annual panel data in the latter and program participation in the former. This dataset provides information before and after the intervention. However, because ENESTYC was never designed to be a panel survey and smaller firms in each survey were randomly sampled, only a small proportion of SMEs can be tracked over time.

The ENESTYC-EIA panel is conformed of near 2,600 firms, of which around 1,500 firms reported having participated in one or more programs (the potential treatment group) and 1,100 stated that they had never participated in any program (the potential control group). Firms in the treatment group are characterized as either currently participating in a program or having participated in the past, with the former category having more respondents. CIMO-PAC, CONACyT's Fiscal Incentives and Technological Innovation program, PROSEC, PNAA, and state government support were the most commonly used by firms, according to the ENESTYC surveys (Table 1), and as such are analyzed in this paper. The analysis also considers firm participation in any SME program captured in the ENESTYC 2001 or in the ENESTYC 2005. Considering that the 2,600 firms in the panel were observed for about 12 years, it gives 30,199 observations at year-firm level, from which 18,435 were reported in the control and 11,764 in the treatment group.

Table 1. SME Program Participation

SME program	Number of participating firms	Participation Status	
		Currently	Not now, in the past
CIMO-PAC	282	142	140
PNAA	247	189	58
Fiscal Incentives and Technological Innovation	187	124	63
PROSEC	113	88	25
Other	100	75	25
State government support	67	40	27
COMPITE	60	23	37
Productive Chains	47	33	14
Crediexporta	47	28	19
PAT	44	31	13
Financing	39	23	16
CRECE	38	16	22
Municipal government support	37	29	8
MEX-EX	36	18	18
Mixed or sectoral funds	36	23	13
Fondo Pyme	27	13	14

PMT	20	10	10
PAIDEC	13	9	4
FAMPYME	10	6	4
PATCI	10	4	6
PROMODE	9	3	6
FIDECAP	8	6	2
PCI	8	6	2
Total	1485	939	546

Source: Linked ENESTYC-EIA panel data.

5. METHODOLOGY

Impact evaluations generally use a variety of econometric methods to address selection bias issues that may arise with non-experimental designs, including: (a) regression models to control for observed variables that affect the outcome variable and that are also correlated with program use; (b) propensity score matching to more closely match the treated group with a control group (Heckman, Ichimura and Todd, 1998); and (c) DID methods, which use pre- and post-intervention information for both treatment and control groups to eliminate the effects of unobserved firm-specific attributes, as well as time-varying effects from stochastic shocks.[5]

However, traditional propensity score matching and DID methods are not suitable to the specific structure of our data. We adopt a more flexible approach that allows us to estimate treatment effects taking into account differing entry points into programs, use of multiple types of programs, year-specific shocks, and varying time since program participation. We rely on fixed-effects models to eliminate the effects of observable and unobserved firm heterogeneity, but fixed over time, as a source of bias in estimates of program impacts.

5.1 Selection biases from observables and time-invariant unobservables through fixed effects models

Consider a general linear model for firm i in time t that relates outcomes Y to observable firm attributes X and a dummy variable for participation in a program D:

$$Y_{it} = v_i + \lambda_t + \beta X_{it} + \alpha D_{it} + u_{it} \quad (1)$$

where v_i is a vector of unobserved but fixed confounders (i.e., a time-invariant firm-specific component), λ_t is the year effect treated as a parameter to be estimated, and a randomly distributed error term u. The observed Y_{it} is either Y_{1it} or Y_{0it}, depending on participation status.

Equation (1) is a fixed-effects model and α is the impact of program participation. Under this model, α is free of bias from self-selection of firms into programs based on their observable and unobservable time-invariant productivity attributes. To see this, notice that the expected average effect of program, $E[Y_{1it} - Y_{0it} | v_i, X_{it}, t, D_{it} = 1]$, can be rewrite as:

[5] For an exposition of these methods in evaluating enterprise programs, see Oldsman and Hallberg (2003).

$$E[Y_{1it}-Y_{0it}|v_i,X_{it},t,D_{it}=1]=E[Y_{1it}|v_i,X_{it},t,D_{it}=1]-E[Y_{0it}|v_i,X_{it},t,D_{it}=1]$$

$$=E[Y_{1it}|v_i,X_{it},t,D_{it}=1]-E[Y_{0it}|v_i,X_{it},t,D_{it}=1]$$
$$+E[Y_{0it}|v_i,X_{it},t,D_{it}=0]-E[Y_{0it}|v_i,X_{it},t,D_{it}=0]$$

$$=E[Y_{1it}|v_i,X_{it},t,D_{it}=1]-E[Y_{0it}|v_i,X_{it},t,D_{it}=0]$$
$$+E[Y_{0it}|v_i,X_{it},t,D_{it}=0]-E[Y_{0it}|v_i,X_{it},t,D_{it}=1] \quad (2)$$

The two last terms in (2) correspond to the selection bias. If participation could be considered to be randomly assigned conditional on v_i and X_{it} (the unobserved time-invariant and observed covariates, respectively), then

$$E[Y_{0it}|v_i,X_{it},t,D_{it}=0]=E[Y_{0it}|v_i,X_{it},t,D_{it}=1]$$

Thus,

$$E[Y_{1it}-Y_{0it}|v_i,X_{it},t,D_{it}=1]=E[Y_{1it}|v_i,X_{it},t,D_{it}=1]-E[Y_{0it}|v_i,X_{it},t,D_{it}=0]$$
$$=\alpha.$$

The key to fixed-effects estimation of α free of bias from self-selection is (i) the assumption that the unobserved v_i appears without a time subscript and (ii) the linear model for the outcome variable. The unobserved individual effects are coefficients on dummies for each firm, while the year effects are coefficients on time dummies. It seems that there are many parameters to be estimated, however this is not a problem, because treating v_i as parameters to be estimated is algebraically the same as estimation in deviations from means. The individual means are

$$Y_i=v_i+\bar{\lambda}+\beta X_i+\alpha D_i+u_i$$

where firm variable means are denoted by a single subscript i. Subtracting this from (1) gives

$$Y_{it}-Y_i=\lambda_t-\bar{\lambda}+\beta(X_{it}-X_i)+\alpha(D_{it}-D_i)+u_{it}-u_i.$$

The fixed effects transformation eliminates the potentially confounding effects of v_i. Another option to deviations from means is differencing, i.e.,

$$\Delta Y_{it}=\Delta\lambda_t+\beta\Delta X_{it}+\alpha\Delta D_{it}+\Delta u_{it},$$

where Δ denotes the change between year $t-1$ and t. With two periods, differencing is algebraically the same as deviations from means.

In place of D, an indicator for participation in one program, we include indicator variables for participation in different SME programs $D_{1i},D_{2i},\ldots,D_{ni}$. This specification allows for (but does not explicitly model) multiple program use, since each program used by firm i has its own program start date.

We use a parsimonious model specification. Seven final outcome measures were selected for study: value added, gross production, total sales, worked hours, wages, fixed assets, and exports; and two intermediate variables: technology transfers and maquila services.[6] These outcome measures are related to program indicator variables that take on a value of 0 for all years preceding the first year of participation (pre-program period) and 1 for all years that follow, including the first year (post-program period). In addition to the program variable(s), our explanatory variables include indicator variables for location, firm size (small, medium and large relative to the omitted micro firm), and year dummy variables for 1994 through 2005 to control for the effects of year-specific stochastic shocks.

5.2 Selection bias from time-variant variables

In some cases, as in the evaluation of training programs, the assumption that the most important omitted variables are time-invariant does not seem plausible. One solution is to include lagged dependent variables, Y_{it-h}, in equation (1), to ensure that the control group has the same pre-treatment trend as the treatment group. Unfortunately, as Angrist and Pischke (2009) discuss, this causes problems in consistently estimating α.

When propensity score matching is used to minimize selectivity bias, estimators have been found to be more reliable when several period lags of the dependent variable can be used in forming the match (Dehejia and Wahba, 2002; Heckman, Ichimura and Todd, 1997), and also when propensity scores are estimated from multiple observed attributes rather than just several broad attributes (Heckman, Ichimura and Todd, 1998).

On other hand, Crump et al. (2009) suggest that propensity scores can be used for systematic sample selection as a precursor to regression estimation. Angrist and Pischke (2009) implement that suggestion, estimating the propensity score and picking only those observations with score between 0.1 and 0.9. This ensures that regressions are estimated in a sample including only covariate cells with at least a few treated and control observations, eliminating only firms that are very different from one another.

Following this approach, and to minimize the bias due to time-variant covariates, we restricted our sample to treatment and control firms falling in the common support region, where propensity scores were estimated including pre-treatment productivity factors like sales, as well as firm attributes.

Instead of using logit or probit models for program participation, we use a Cox proportional hazard model to estimate the propensity score of the likelihood of program participation for the sample of treatment and control groups followed over the 1994 to 2005 period.[7] The Cox

[6] Spending on maquila services.

[7] An alternative approach is to estimate separate logit models of program participation for different cross-sections (or year intervals) to derive propensity scores for each treatment cohort (or groups of cohorts). This did not prove

proportional hazard model relates the likelihood of entry into a program, conditional upon survival (non-entry) up to that point in time, to a baseline hazard function and a set of independent variables. The underlying hazard function $h(\cdot)$ may be written as follows:

$$h(t, Z_1, Z_2, ..Z_m) = h_0(t)\exp(\phi_1 Z_i + \phi_2 Z_2 + ..\phi_m Z_m) \quad (3)$$

where Z is a vector of m covariates, and $h_0(t)$ is the baseline hazard when the values of all the covariates are set to 0.

We use the relative hazard of program entry for firms with attributes Z as the propensity score for defining the region of common support and keep only enterprises that fall inside this region. The attributes Z included in the Cox model were a) characteristics of the firm such as region, sector, age of the firm, etc., and b) time-varying productivity factors like pre-participation sales growth and one year lag of the logarithm of sales. From the estimated hazard ratios,[8] firms located outside Mexico City, older firms, and firms with higher production have a higher probability of enrolling in SME programs. As the propensity score for each firm, we use the mean of their hazard rates for all years in which they are available.[9] The hazard rate averaged 3.53 for the treatment group and 3.24 for the control group, consistent with the treatment group as a whole having a higher relative probability of program participation.

5.3 Attrition

The SME enterprise support programs typically require firms to be in business for at least one to two years before they participate in the program, and are not themselves designed to promote entry. It is theoretically possible that the presence of such programs may induce additional entry of firms that would not otherwise have entered, but this is not the primary focus of the programs, and we focus on estimating the impact of the program on firms already in business. Thus there is no bias from ignoring entry of firms, only the need to be careful in stating the treatment effect to be estimated.

However, ignoring exit of firms may involve bias if attrition from the panel is non-random. It is possible that firms that would have failed in the absence of the program are able to remain in business as a result of the program. As a result, one might understate the impact of the programs, since the treated firms that stay in are likely to be of worse average productivity and/or be experiencing more negative shocks than control firms.

To test the sensitivity of the results to exit, the bounding approach of Lee (2005) was followed. This was used in McKenzie and Woodruff (2008) to get upper and lower bounds of the treatment effect. To implement the Lee (2005) bounds, a monotonicity assumption is required, which states that treatment assignment affects sample selection only in one

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feasible because of small sample sizes, which led to very imprecise estimates of the logit model. The Cox proportional hazards model was preferred not only because of sample size considerations but also for its unified treatment of the underlying process of selection into programs over time.

[8] A table with the estimated hazard ratios is available online to subscribers on the journal's web site.

[9] For the treatment group, the means are computed for all years up to the year of program participation, after which relative hazards rates are not defined because the failure event has occurred.

direction. In our context, it requires assuming that there are some firms who would have exited if they had not been in the SME program, but that firms do not exit because of receiving the SME program. This seems plausible in our case.

To construct the Lee (2005) bounds, one trims the distribution of the outcome variable for the group assigned to treatment by the difference in attrition rates between the two groups as a proportion of the retention rate of the group assigned to treatment.[10] An upper bound on the treatment effect is constructed by trimming the lower tail of the distribution and then estimating the effect.

5.4 Time effects

Finally, we investigate how long it takes for program impacts to be realized. We test for time effects of program impacts from α_2, the estimated coefficient on the interaction term between D and YRS, a variable measuring years-since-first-participated in the program. Rather than forcing a functional form on these time effects (for example, with a quadratic specification of time and time squared), we define a set of indicator variables for different intervals (1, 2, 3, 4, 5-6, 7-9, and over 10 years) following the date of entering the program. This allows the effects of the interaction terms between the program indicator and time since participation to vary non-linearly with time in and after the program:

$$Y_{it} = v_i + \lambda_t + \beta X_{it} + \alpha_1 D_{it} + \alpha_2 D_{it} * YRS_{it} + u_{it}. \qquad (4)$$

The resulting estimates can be interpreted as the time effects of treatment, if several assumptions hold. First, these effects are estimated holding constant all other time-varying factors, including inflation and macroeconomic shocks. The model accounts for these factors by including year dummy variables to capture year-specific stochastic shocks. A second assumption is that self-selection into treatment is not dependent upon time. The presence of cohort effects in treatment—firms that choose to participate early are different from those that join in later years—can introduce bias into these estimates.

6. RESULTS

Our objective is to estimate the longer-term impacts of program participation controlling for the effects of observed and unobservable productivity attributes, and to test for differences in the treatment effects of the programs mentioned in Section 4. Also we are interested in testing the sensitivity of program impact estimates to the possibility that program participation inhibits firm exit from our panel data. And lastly, we investigate how quickly or slowly program impacts are realized over time.

Using the fixed-effects model presented in equation (1) in the common support region, we find that for PROSEC, Fiscal Incentives and Technological Innovation, state government support, and PNAA, the average effects were positive, suggesting positive impacts from

[10] For example, McKenzie and Woodruff (2007) trimmed the upper or lower 6.7 percent of the profits distribution for the group assigned to treatment.

these interventions in outcome variables such as value added, gross production, sales, employment and worked hours (Annex Table A1). The estimated effect of PNAA ranges from a 4 to 7 percent increase, while the other three programs ranges from 6 to 17 percent increases in outcome variables.

Participation in any SME program has positive and significant effects—close to 6 percent for value added, 5 percent for gross production, 5 percent for total sales, and 6 percent for employment and on fixed assets (Annex Table A2).

The results for CIMO were negative, which suggests that CIMO did not have an impact on the performance of firms. The authors carried out additional estimations on the impact of CIMO before and after 2001, since the program was decentralized after 2001. The authors found that firms that participated in CIMO up to 2001 showed a positive impact on selected outcome variables such as foreign sales, fixed assets, and technology transfers.[11] These results are in line with the earlier impact evaluation from Tan and Lopez-Acevedo (2005), which found positive impacts on intermediate outputs but no significant impacts on final outcomes before 2001.

One issue that arises is that the data set does not include firms that exit, which could lead to a survivor bias if program participation increases the possibility of firms surviving that would otherwise exit (stop operations). As discussed in Section 5.3, we bound our estimates of program impacts by sensitivity analysis in which we re-estimate outcome models after dropping the bottom 5 percent of the treatment group in terms of outcome variables (e.g productivity, assuming that the lowest productivity firms would otherwise exit). The significance and magnitude of the program impact is quite similar under the trimming and the original estimates (Annex Table A1). The sensitivity analysis indicates that the direction and size of treatment effects are robust to controls for potential biases from firm exit.

None of the indicator variables for time since participation are statistically significant before four years (Annex Table A3). Taking the example of fixed assets, the estimated coefficient in fixed assets becomes positive beginning four years after program entry and increases in value and statistical significance. The treatment effect is 7 percent at four years, increasing to 14 percent at 5-6 years, 22 percent at 7-9 years and 42 percent from 10 years since program entry. Other outcome variables such as sales show similar patterns over time.

7. Conclusions

This paper uses firm panel data to evaluate the impacts of SME programs in Mexico. The paper makes use of the program module in the ENESTYC 2001 and 2005, which includes retrospective questions about firm participation, date of participation, type of support received, and familiarity with SME programs administered by several agencies. The ENESTYC was linked to the EIA to form a panel of firms over 10 years. In this way, pre- and post-program outcome variables are tracked over time for both the treatment and control groups.

[11] Effect estimations of CIMO 2001 are available online to subscribers on the journal's web site.

Our results indicate that firm participation in PROSEC, PNAA, and the Fiscal Incentives and Technological Innovation program is associated with higher value added, sales, export, and employment. Impacts on different outcome variables ranged from 4 to 7 percent for PNAA and 6 to 17 percent for the other three programs. Why did these programs yielded positive effects and programs like CIMO after 2001 did not? One potential explanation may be because PROSEC, PNAA, and the Fiscal Incentive program have invested considerable resources per firm, while CIMO resources per firm have been declining. SME programs run by state governments also have a statistically significant impact on outcome variables, but unfortunately information on the types of support provided or resources is almost impossible to get, making it difficult to draw much in the way of conclusions from this result.

Our panel also identifies the timing of the effects of program participation on outcomes. We found that none of the indicator variables for time since participation are statistically significant before four years. Beginning with 4-5 years after program entry, the estimated coefficients become positive and increase in value and statistical significance for selected variables. Using the example of fixed assets, the treatment effect is 7 percent at 4-5 years, rising to 13 percent at 6-7 years and 21 percent at eight years since program entry. The results suggest that the effect of enterprise support programs might not be immediate, which could account for negative results in previous studies using a shorter time horizon. This finding is consistent with the Peru and Chile country papers of the World Bank (2010) regional report, where authors also found a strong and increasing time effect. These results remain robust after trimming the bottom 5 percent of our treatment group, to account for possible firm exit bias.

Several research lines emerge from our analysis. One key finding is that the very high number of programs and their constant evolution over time (changing names and structures, closing old programs and opening new ones) makes rigorous impact evaluations a major challenge. Thus one area to investigate is cohort effects in the panel for those programs that underwent major design changes. Initial research was carried out in this paper with CIMO, which could serve as an example for similar analysis of other programs.

ANNEX 1

Table A1. Program Impacts by Program. Fixed Effects Model in the Common Support Region

	Value added	Gross production	Total sales	Employment	Worked hours	Wages	Fixed assets	Foreign sales	Tech. transfers payments	Maquila services
CIMO	-0.060 **	-0.040 *	-0.046 **	-0.030 *	-0.030 *	-0.003	0.053	-0.084	-0.010	-0.322 **
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.04)	(0.08)	(0.15)	(0.13)
PROSEC	0.167 ***	0.164 ***	0.133 ***	0.058 ***	0.069 ***	0.003	0.136 ***	0.153 *	0.217	0.137
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.05)	(0.08)	(0.15)	(0.14)
Environmental Audit Program	0.065 **	0.043 **	0.058 ***	-0.023	-0.014	0.052 ***	0.074 **	0.065	-0.032	-0.199
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.04)	(0.07)	(0.12)	(0.13)
Fiscal Incentives & Tech. Innovation	0.146 ***	0.094 ***	0.094 ***	0.108 ***	0.100 ***	-0.015	-0.001	0.160 *	-0.106	0.370 ***
	(0.04)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.04)	(0.09)	(0.14)	(0.14)
State Goverment Support	0.155 **	0.025	-0.017	0.057	0.075 *	-0.040	0.127	-0.101	0.255	0.165
	(0.07)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.09)	(0.15)	(0.32)	(0.46)
Trimmed sample										
CIMO	-0.036	-0.032	-0.037 *	-0.031 **	-0.028 *	0.003	0.051	-0.128 *	0.098	-0.260 **
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.04)	(0.08)	(0.15)	(0.13)
PROSEC	0.127 ***	0.169 ***	0.140 ***	0.048 **	0.048 **	0.029 *	0.125 **	0.195 **	0.182	0.224
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.05)	(0.08)	(0.15)	(0.14)
Environmental Audit Program	0.068 **	0.053 **	0.069 ***	-0.041 ***	-0.036 **	0.063 ***	0.107 ***	0.055	-0.033	-0.090
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.04)	(0.07)	(0.12)	(0.13)
Fiscal Incentives & Tech. Innovation	0.160 ***	0.114 ***	0.110 ***	0.073 ***	0.071 ***	-0.020	0.001	0.225 ***	-0.208	0.373 ***
	(0.04)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.05)	(0.08)	(0.14)	(0.14)
State Goverment Support	0.128 *	0.029	-0.036	0.057	0.065 *	-0.052 *	0.106	-0.035	0.272	0.514
	(0.07)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.09)	(0.15)	(0.31)	(0.47)

Number of year-firm observations	21140	21329	21017	21269	21260	20792	20982	10701	4516	7084

Source: Linked ENESTYC-EIA panel data.

Notes: 1) ***, ** and * denote statistical significance at the 1, 5 and 10 percent level, respectively.

2) Numbers in () correspond to standard errors.

Table A2. Program Impacts of Any Program Participation. Fixed Effects Model in the Common Support Region

	Value added	Gross production	Total sales	Employment	Worked hours	Wages	Fixed assets	Foreign sales	Tech. transfers payments	Maquila services
Any program	0.052 ***	0.049 ***	0.046 ***	0.060 ***	0.060 ***	0.006	0.059 ***	-0.035	-0.091	-0.025
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.04)	(0.06)	(0.06)
Trimmed sample										
Any program	0.064 ***	0.053 ***	0.052 ***	0.057 ***	0.056 ***	0.019 ***	0.081 ***	0.015	-0.097 *	0.005
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.06)	(0.06)
Number of year-firm observations	27006	27299	26878	27181	27159	26496	26786	13161	5802	8882

Source: Linked ENESTYC-EIA panel data.

Notes: 1) ***, ** and * denote statistical significance at the 1, 5 and 10 percent level, respectively.

2) Numbers in () correspond to standard errors.

Table A3. Time Effects of Any Program Participation (Time Since Started the Program). Fixed Effects Model in the Common Support Region

Outcome variable	*1 year later*	*2 years later*	*3 years later*	*4 years later*	*5 - 6 years later*	*7 - 9 year later*	*10 + year later*
Value added	0.000	0.003	-0.026	0.013	0.029	-0.073	0.054
	(0.029)	(0.031)	(0.033)	(0.034)	(0.033)	(0.049)	(0.133)
Gross production	0.005	0.019	0.004	0.006	0.029	0.028	0.150
	(0.02)	(0.022)	(0.022)	(0.023)	(0.023)	(0.034)	(0.092)
Total sales	0.014	0.024	0.005	0.006	0.016	0.060 *	0.196 **
	(0.02)	(0.021)	(0.022)	(0.023)	(0.022)	(0.034)	(0.091)
Employment	0.010	0.006	-0.020	-0.034 *	-0.028	-0.054 **	0.043
	(0.015)	(0.016)	(0.017)	(0.018)	(0.017)	(0.026)	(0.069)
Worked hours	0.008	0.008	-0.024	-0.039 **	-0.021	-0.050 *	0.044
	(0.016)	(0.017)	(0.018)	(0.019)	(0.018)	(0.027)	(0.074)
Wages	-0.012	-0.004	0.003	0.000	-0.009	0.035 *	0.096 *
	(0.012)	(0.013)	(0.014)	(0.014)	(0.014)	(0.021)	(0.057)
Fixed assets	-0.011	-0.015	0.013	0.071 *	0.136 ***	0.217 ***	0.416 ***
	(0.036)	(0.038)	(0.04)	(0.041)	(0.04)	(0.06)	(0.162)
Inputs	0.008	0.027	0.005	0.006	0.038	0.091 **	0.262 ***
	(0.021)	(0.023)	(0.024)	(0.025)	(0.024)	(0.036)	(0.098)
Foreign sales	-0.022	-0.026	0.015	-0.059	-0.052	-0.079	-0.304
	(0.065)	(0.071)	(0.075)	(0.079)	(0.075)	(0.116)	(0.34)

Tech. transfers payments	0.043	-0.005	-0.115	0.043	0.042	-0.215	0.210
	(0.105)	(0.113)	(0.122)	(0.127)	(0.127)	(0.183)	(0.54)
Maquila services	-0.102	-0.049	0.174	0.125	0.018	-0.190	0.238
	(0.106)	(0.115)	(0.122)	(0.13)	(0.127)	(0.179)	(0.432)

Source: Linked ENESTYC-EIA panel data.

Notes: 1) ***, ** and * denote statistical significance at the 1, 5 and 10 percent level, respectively.

2) Numbers in () correspond to standard errors.

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