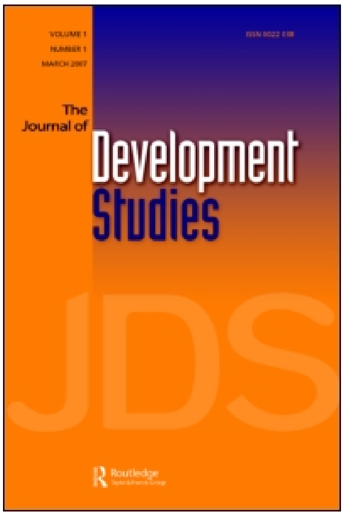

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Releasing Constraints to Growth or Pushing on a String? Policies and Performance of Mexican Micro-Firms

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ABSTRACT *Using firm-level data from Mexico, this paper investigates the firm characteristics associated with participation in credit markets, access to training, tax payments and membership in business associations. We find that firms which participate in these institutions exhibit significantly higher profits. Moreover, firms that borrow from formal or informal sources and those that pay taxes are significantly more likely to stay in business but firms that received credit exhibit lower rates of income growth. These results persist when firm characteristics that are arguably correlated with unobserved entrepreneurial ability are controlled for. Our findings suggest that the significant within-country differences in firm productivity observed in developing economies are due in part to market and government failures that limit the ability of micro-firms to reach their optimal sizes.*

I. Introduction

Micro-firms account for roughly 50 per cent of employment in Latin America and substantially more in Africa and Southeast Asia. Raising their productivity, therefore, ranks high as a development goal and eliminating impediments to their dynamism is a central concern of policy. Using a very detailed micro-firm data set from Mexico with a panel structure, this paper examines how micro firm performance is affected by several dimensions of their environment: access to formal and informal credit, training programmes, business associations or guilds, and government taxation and services.

On the one hand, the analysis can be seen in the tradition of the 'business climate' literature which focuses on the sources of within country differences in firm

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productivity and growth (recent examples include Dollar et al., 2005, 2006; Klapper et al., 2006; for a literature review see Banerjee and Duflo, 2005). Following the convention of the literature on evaluation in quasi-experimental contexts, we can conceive of engagement with each dimension as *treatments* and attempt to measure their impact on micro-firm performance. The literature on credit constraints (for recent examples see Banerjee and Duflo, 2004; Ayyagari et al., 2006; Laeven and Woodruff, 2007) is clearly relevant here, although we are casting the net far more widely.

On the other hand, however, to the extent that one can interpret the above *treatments* as capturing increases in the degree of firm *formalization*, the paper is also related to the literature on the impact of formality on productivity and growth (see for example Loayza, 1996; Schneider and Enste, 2000).[1] Here, the endogeneity issues customarily complicating the measurement of the treatment effect are related to the decision to formalise.

The underlying policy issues are of first order. In our sample, the participation in societal institutions is low: 10 per cent of micro-firms have received credit or training services and less than 17 per cent participate in industry or trade associations. This is often seen as evidence of supply bottlenecks that limit access to financing, human capital and information. Releasing those constraints would permit firms to approach the steady state size dictated by their intrinsic entrepreneurial ability.[2] However, it may also be that for many firms, the steady state size is very small and thus there is, in fact, little demand for these treatments. For example, a sizable literature dating from Harris and Todaro (1970) argues that most informal micro-firms are run by individuals who did not enter the sector due to their high entrepreneurial ability or other cost advantages but, instead, are essentially queuing for entry into salaried jobs and are unlikely to be thinking of future growth. Alternatively, mainstream models of firm dynamics (Lucas, 1978; Jovanovic, 1982) postulate that the small steady state size is driven by the underlying cost structure or entrepreneurial ability rather than external constraints. As argued by Levenson and Maloney (1998), if one accepts that formality broadly construed as participation in the institutions of civil society operates as a normal input in the production function of firms, it is possible that the intrinsic cost structure of many informal enterprises may dictate, in fact, that they never grow large enough to need those institutions.

This adds a new perspective on the nature of our remaining treatment – payment of at least some taxes (31% in our sample) – a common indicator of informality in the literature and one with potentially distinct selection issues. One literature views informality as evasion of taxes (see, Loayza, 1996): firms that pay them do so because they had the bad luck of being detected by the government. However, similar to our other treatments, tax payments may be also seen as the cost of gaining access to social services that are necessary for the expansion of businesses. For instance, as de Soto (1989) pointed out in the case of Peruvian street vendors, weak property rights made expansion risky and the vendors actively sought to pay their taxes to gain 'quasi-property rights' over their respective pitches. In another example, de Paula and Scheinkman (2007) have argued that firms would prefer to pay taxes for having subcontracting relations with bigger firms. More generally, registration with the government makes it possible to engage in legal contracts, participate in risk pooling and worker insurance mechanisms, and borrow from formal financial institutions.

This view is consistent with the emerging literature on social capital and networks that sees formal and informal services as substitutes (see, for example, Portes and Landolt, 2000; Stiglitz, 2000; Tybout, 2000 provides an excellent literature review on this topic). This literature has highlighted that absence of participation in formal market institutions does not imply that agents do not acquire similar services through less formal means and, to a lesser degree, that informal alternatives are necessarily inferior to formal ones.[3] It is also supported by recent evidence on Mexico by McKenzie and Woodruff (2006). Using a survey of informal micro firms, they show that the vast majority of them give as the principle reason for not being registered, not that it is too expensive or time consuming to do so (respectively 2 and 8% of surveyed firms), nor that the costs of operating as registered businesses are too high (4% of firms) but, that they are too small to make it worth their while (75%).

This discussion thus frames the empirical question of whether improving the business climate and/or formalisation has an impact on micro-firms' performance and, if any, which dimensions are more effective in doing so. In the bottlenecks view, increasing the supply of credit, training information/business services and access to government services would lead to improved business performance. In the case of a very small steady state size, it would amount to pushing on a string and have little impact. The particular case of being forced to pay taxes without any resulting benefit also leaves the possibility that the treatment would lead to a fall in performance.

This paper uses a set of estimation techniques in non-experimental settings to cast light on the various relationships between treatment and firm performance. Empirically, estimating the magnitude of any treatment impact is not trivial. We begin with standard ordinary least squares (OLS) estimates and, particularly in the case of credit, a treatment that has received the most attention, we offer a rich set of descriptive statistics. We then move to a more refined non-parametric approach using propensity score matching. The increased attraction of this method in recent years has been, precisely, that it does not impose functional form assumptions, in particular linearity among regressors that can induce bias in the OLS results.[4]

However, both techniques have problems dealing with self selection into the treatments considered that potentially imparts bias to the estimates. As an example, applying for bank credit, seeking out training, and joining a business association could all reflect superior underlying entrepreneurial ability, rather than any impact of the treatment. Since both OLS and propensity score matching condition on observables which may omit such ability, the estimated treatment effects may be biased. To minimise the potential estimation biases arising from the self-selection of firms into the various treatments, we take two approaches. First, we attempt to control for the effect of unobservables using a conventional control function approach that seeks to explicitly model the selection on unobservables. However, in the absence of good exclusion restrictions (a covariate that affect the selection process but not the outcome), this method imposes strong assumptions on the functional forms of the selection and outcome equations.[5]

Second, with all methods we progressively introduce an increasingly rich set of firm characteristics that are themselves dependent on (and thus capture) the unobservables and, hence, their inclusion reduces the likely bias. The trade-off is that since these variables may be affected by the treatment, we may be progressively foreclosing channels through which the treatment is working and, therefore, masking the true treatment effect. If we believe that the inclusion of these variables largely captures the unobservables, then these estimates are approaching the true estimates from below while the estimates with only exogenous regressors are biased upwards. This conclusion is complicated if, in fact, the correlation between the unobservables driving selection into the chosen treatment and the included variables is low. In this case, the different estimates may still substantially reflect the residual selection bias of the chosen treatment and no conclusions about the bias of the estimates can be drawn.[6] We argue that since the main unobservable cited in the literature is unobserved entrepreneurial ability and this is likely to be an important determinant across all treatments and other endogenous variables, the masking effect is likely to dominate the residual selection effect and the estimates will be biased downwards. Ideally, our results should eventually be contrasted with the new experimental studies on small firms in developing countries (see for instance de Mel et al., 2007).

The paper is organised as follows. Section II presents the different methodologies and discusses the assumptions that allow us to use them. Section III describes the data. Section IV presents the estimations for the probability of participation and treatment effects. Finally, conclusions are presented in the last section.

II. Methodological Details

Very generally, we want to estimate

$$Y_d = g_d(X_d, U_d) \quad d = 0, 1 \tag{1}$$

where d indexes the occurrence of a certain *treatment* (whose random variable will be denoted by $D \in \{0,1\}$), Y_d denotes an outcome of interest, and X and U are observable and unobservable characteristics of the firm. Again, we can associate treatment with the formal status of the firm: taking out a bank loan may lead to upgrading of accounting procedures, greater efficiency of capital and higher profits; joining a business association may introduce the entrepreneur to new technologies or ways of doing business; training workers may also lead to greater efficiency in the use of capital and labour. We define Y_1 to be the outcome if the firm is exposed to the treatment of interest and Y_0 to be the outcome if the firm is untreated. For a given individual i, the observed outcome Y_i can be written $Y_i = Y_{0i} + D_i\,(Y_{1i} - Y_{0i})$. The fundamental problem, of course, is the inability to observe the same individual both with and without treatment effects. Although this problem cannot be solved at the level of any individual, we can identify the average treatment effect of the population. Parameters that only depend on the marginal distribution of Y_{1i} and Y_{0i} are the 'average treatment effect' (ATE) $E(Y_1 - Y_0)$, the 'average treatment effect on the non-treated' $E(Y_1 - Y_0|D = 0)$ and the 'average treatment effect on the treated' (ATT) $E(Y_1 - Y_0|D = 1)$. The last is the effect of most interest in the literature because it defines the potential effect of a given outcome on the universe of firms which are likely to engage in the treatment.

Matching Techniques

Though numerous arguments have been put forward for using matching techniques, their most attractive quality is perhaps not imposing functional relationships between the treatment and the outcome. Matching techniques effectively create a pseudo control group with observable characteristics that are arbitrarily 'similar' to the 'treated' group.

In practice, matching techniques use one of several possible distance measures to find observations that are 'close' to those in the control group. As the set of characteristics that are taken into account increases, and/or if they contain continuous variables, the number of observations in each cell to be matched gets very small (or null). One popular solution is that of basing the matching of treated and controls on the conditional predicted probability of participating in the programme – the so-called propensity score $P(x) = Pr\{D = 1|X = x\}$ the conditional probability of participation given a vector of observed characteristics x. This variable should contain all the information in X relevant for the ATT. In practice, the propensity score can be derived parametrically from either probit or logit models.

It is important to note that the estimated average effects are reduced form impacts operating through any number of channels of unspecified functional form. In other words, we do not impose any functional restrictions on $g(.)$, nor do we need a set of excluded variables to identify it. Thus, matching offers a very flexible non-parametric form of estimating the impact of a treatment.

However, the validity of the matching estimators relies on the conditional independence assumption (CIA), which states that conditional on the observable variables used to perform the matching (X), the control group does not differ from the treatment group by any variable which is systematically linked to the non-participation outcome Y_0 (Heckman and Robb, 1985). In short, the CIA can be expressed as:

$$Y_0 \perp D|X \tag{2}$$

where $\perp$ denotes stochastic independence.

How innocuous the CIA is, depends on how much we believe that unobserved variables are irrelevant to both the likelihood of treatment and the outcome realisation.

Following Dehejia and Wahba (1999), matching estimation is done in two steps. First, we estimate the propensity score by probit models. Second, two matching procedures are used in this paper. First, we compute Nearest Neighbour estimates and secondly, we apply a Kernel-based method using Epanichnikov Kernel and a fixed bandwidth of 0.06.

The Control Function Approach

As argued by Heckman et al. (1999), the standard treatment effects regression has the important advantage of allowing for selection on unobservables and hence not requiring the relatively strong CIA assumption underlying matching methods. As is generally well known, control function approaches introduce to Equation (1) an additional selection equation describing a latent variable D^*, such that the treatment occurs if $D^* > 0$ only. If X can be partitioned into two, not necessarily disjoint, sets (Q,T) such that Q affects the outcome equation while T determines the selection part it is possible to estimate the following model:

$$\text{Outcome equation: } Y = g(Q) + \delta D + u$$
$$\text{Selection equation: } D^* = h(T) + v$$

where $D = 1$ if $D^* > 0$ and $D = 0$ otherwise. Selection bias is then treated as an omitted variable problem. As is common, linearity is assumed in the g and h functions. This set up imposes a much stricter parameterisation on the outcome equation than matching does and than we would prefer, since we think that treatment may be accompanied by changes in the production process. In our case, no justifiable exclusion restrictions can be found to separate Q and T and, therefore, semiparametric estimators cannot identify $g(.)$. The identifying restrictions arise from the assumptions about the joint distribution of u and v, that is, they follow a bivariate normal distribution.

Expanding the Conditioning Set to Deal with Unobservables

As a further attempt to control for unobservables, in all exercises, we progressively expand the conditioning set. We start by including only those X variables which can credibly be considered exogenous to the treatment, namely a set of individual characteristics H. When conditioning only on this set of variables, the treatment indicator is likely to be positively correlated with the unobservables. Therefore, the corresponding estimated treatment effects can be expected to be upward biased. We then add to the conditioning set a vector of variables Z which are likely to control for unobservables but can also be expected to be affected by the treatment. These variables are the firm's capital stock, number of workers and time in business. The motivation for this procedure is linked to the literature on firm dynamics and entrepreneurship (see for instance Jovanovic, 1982), which assumes that those entrepreneurs with unusually high entrepreneurial ability (or those firms with 'good' unobservable characteristics) have a lower probability of failure and are more likely to grow. Thus, the age and size of firms can arguably be used as proxies for entrepreneurial ability and other unobservable firm characteristics associated with improved performance. Finally, in our third set of conditioning variables, we add to X and Z the set of other treatment variables (F). We interpret the corresponding estimates as capturing not only additional unobservables that may drive both firm performance and participation in various societal institutions, but also isolating the pure effect of each treatment from others with which it might be correlated.

As mentioned above, one concern is that some of the variables included in the second and third set of conditioning variables may be endogenous. As argued by Heckman et al. (1998), the exogeneity of the conditioning set X is not strictly required for matching methods, but the real treatment effect is masked if this condition is violated. If the treatment has a positive impact on those regressors, the estimation will understate its impact. Indeed, we are excluding the treatment effect that operates through the regressors of the propensity score model, so that the estimated treatment effects are in practice conditional on these other regressors not changing. Thus, for instance, when we add employment size and capital stock to the X vector, we are effectively measuring not the total effect of our treatments on firm profits but, rather, the effects on the efficiency with which existing factors of production are used by similar treated and non-treated micro-firms.[7]

III. Data and Descriptive Statistics

We employ the National Survey of Microfirms (Encuesta Nacional de Micro-negocios, ENAMIN) for the years 1992, 1994, 1996 and 1998. These cross-sectional surveys cover a sample of individuals who declare that they are self-employed in a broader labour survey, the Encuesta Nacional de Empleo Urbano (ENEU). The ENAMIN survey is restricted to micro-firms with at most six workers (including owners) for all economic sectors except manufacturing, where the range is from 1–16 workers. We restrict the sample to micro-firms with one to six workers to be consistent across industries. The ENAMIN allows a relatively precise construction of a wide variety of variables that represent basic firm and entrepreneur characteristics: profits, employment size, capital stock, time in business, and engagement with a wide variety of societal institutions, including those for which treatment effects are estimated in this paper. In addition, we are able to construct, from the ENEU, several variables representing personal characteristics of the entrepreneur and his/her household. These include the size of the household and the entrepreneur's gender, age, level of schooling and position in the household (head or not).

As measures of firm performance we work primarily with 'profits' as reported in the ENAMIN. This is calculated as revenues minus declared costs not including the owner's labour and is perhaps better seen as a measure of income. In fact, as a robustness check, we also use the net income from self-employment reported by the owner of the firms when interviewed in the ENEU. In logs, the correlation of the two variables is 0.59. We also construct variables measuring firm growth and survival, by exploiting the panel nature of the ENEU. This survey follows the owners of the firms covered by the ENAMIN for four quarters after the initial interview so that we can calculate the log difference between initial and final income, over a period of one year, as a measure of growth. Survival is captured by a dummy variable indicating whether individuals that initially reported being self-employed (with the detailed firm characteristics covered in the ENAMIN) were still self-employed a year later or had transitioned to being salaried workers, unemployed or out of the labour market. Since the nature of the rotating panel implies that only 20 per cent of the sample linked to the ENAMIN can be observed a year later, the samples in which we explore dynamic outcomes are reduced to about one-fifth of the original ENAMIN sample.

The variables employed in the paper are described in Online Appendix A with summary statistics presented in Table 1. Within the group of micro-firms on which we focus – those with at most six workers – the average number of workers (besides the owner) is 0.6, and two-thirds of the firms are owner-only (own-account workers). However, despite their small size, the sampled micro-firms have been in business for

Table 1. Summary statistics

	ENAMIN (Complete sample)			ENAMIN-ENEU (Small sample)		
Variable	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.
log Real income*	31867	7.42	0.95			
log Real profits	32077	7.42	1.20			
Dif. log Real income*				4480	−0.06	0.79
log Real income (all)*				5392	7.44	0.97
log Real profits (all)				5418	7.44	1.19
log Real income (surv.)*				3993	7.54	0.93
log Real profits (surv.)				4043	7.54	1.17
Credit	35044	0.11	0.31	5925	0.16	0.37
Formal credit	35044	0.04	0.20	5925	0.04	0.20
Informal credit	35044	0.07	0.26	5925	0.13	0.33
Training	35044	0.06	0.24	5925	0.06	0.24
Taxes	35044	0.31	0.46	5925	0.31	0.46
Guild	35044	0.17	0.38	5925	0.17	0.37
log Capital stock	35044	7.97	3.39	5925	8.03	3.38
log Workers	35044	0.33	0.48	5925	0.34	0.49
Time in business	35037	8.21	9.19	5923	8.30	9.41
Female	35044	0.30	0.46	5925	0.30	0.46
Head*	35044	0.67	0.47	5925	0.69	0.46
Npers*	35044	4.81	2.22	5925	4.81	2.20
Married	35044	0.74	0.44	5925	0.75	0.43
Age	35044	42.13	13.55	5925	42.73	13.54
Age2	35044	1958.59	1253.38	5925	2008.75	1263.41
Prim	35044	0.46	0.50	5925	0.47	0.50
Sec	35044	0.33	0.47	5925	0.32	0.47
High	35044	0.16	0.36	5925	0.15	0.36
Survival				5925	0.74	0.44

Notes: *ENEU (surv.): survivors only.

eight years on average, and 50 per cent are more than five years old. Average monthly profits are about US$360, in prices of late 1997, with the median entrepreneur earning US$205 per month.[8] The median enterprise has a capital stock of about US$600.

As for personal and household characteristics, the average Mexican micro-entrepreneur in our sample is 42 years old and lives in a household with four other individuals. Almost 70 per cent are males, 83 per cent of which are heads of household, compared to only 28 per cent of female entrepreneurs that are household heads. Almost half of the micro-firm owners in the sample have at most some primary education and only 15 per cent have attended college.

Treatment Indicators

The treatments are all captured as indicator (dummy) variables. The first is the receipt of at least one loan at some undetermined date after the business was established (*Credit*). The ENAMIN provides comprehensive information to study the characteristics of those firms that ask and receive credit. The survey contains a question asking if the firm had asked for a loan (either from financial institutions or from informal sources) at some undetermined date after the business was established and whether it was successful in obtaining it. Micro-firms that give a positive answer to both questions are the ones hereby considered as having received the treatment.

The ENAMIN also provides information about the source of the corresponding loans. We are able to break this out into the receipt of loans from formal credit providers, namely banks or non-bank financial institutions (*Formal Credit*), and loans from informal sources, including family, friends, clients and suppliers (*Informal Credit*). Unfortunately, the survey does not provide information on the specific dates on which credit was obtained or on the amounts of credit received.

Table 2 compares firm characteristics by whether or not the firm sought credit; if it did, whether or not it was obtained; and, finally, whether it was from a formal or informal source. Only 12 per cent of the micro-firms asked for any type of credit and, of those, 90 per cent obtained it. Furthermore, only 4 per cent of the sampled firms report having received a loan from financial institutions, and 7 per cent has borrowed from informal sources. The unusually high approval rates suggest the existence of a strong self-selection mechanism, by which only firms with a high likelihood of being granted credit chose to apply. In fact, when asked about the reasons for not asking for credit, most of the entrepreneurs sampled in the 1994 and

Table 2. Descriptive statistics on firms with and without access to credit

	Sought credit →		Obtained it →		Source	
Credit	Yes	No	Yes	No	Formal	Informal
	12.1%	87.9%	90.0%	10.0%	36.4%	63.6%
Number of workers	2.20	1.51	2.20	2.24	2.55	1.99
including owner and partners	(1.92)	(2.32)	(2.31)	(1.42)	(2.44)	(1.19)
Log Capital stock	9.94	7.69	9.93	10.01	10.96	9.34
	(2.78)	(3.38)	(2.77)	(2.80)	(2.43)	(2.79)
Profit (in Mex$, 1997)	4868	2636	4878	4775	7506	3399
	(20615)	(9217)	(21122)	(15302)	(29796)	(13848)
Time in business	8.54	8.16	8.57	8.25	10.42	7.52
	(8.72)	(9.24)	(8.74)	(8.55)	(9.42)	(8.14)
Credit at start-up	0.46	0.21	0.48	0.28	0.46	0.50
Pay taxes	0.53	0.28	0.52	0.56	0.66	0.45
Accounts	0.51	0.22	0.50	0.56	0.67	0.41
Registered	0.69	0.36	0.69	0.71	0.84	0.61
Hours worked	48.9	41.2	48.9	49.2	49.1	48.7
	(20.1)	(19.6)	(20.2)	(19.0)	(18.3)	(21.2)
Schooling	9.22	7.59	9.18	9.65	10.70	8.31
	(4.96)	(4.91)	(4.70)	(4.92)	(5.08)	(4.69)
Age	41.5	42.2	41.3	42.8	42.8	40.5
	(11.9)	(13.7)	(12.0)	(11.0)	(11.3)	(12.4)

Note: Standard errors in parentheses.
Source: Author's calculations using pooled ENAMIN 1992, 1994, 1996 and 1998.

1996 ENAMIN surveys – the question is not available for other years – point to a lack of demand for external finance. In particular, 56 per cent declare that they 'didn't need it,' 28 per cent 'prefer to use own resources,' 8 per cent mention that 'interest rates are too high,' 4 per cent say that there are 'too many requisites,' and 4 per cent 'didn't know how to obtain a credit' or declare that 'there are no credit or loans.'

In order to explore the selection mechanisms at work in the decision to seek formal or informal credit, the first two columns of Table 2 compare the characteristics of those firms that did ask for loans with those that did not. On average, the former have more than double the number of employees, 85 per cent higher profits and about nine times higher capital stocks than those who did not apply for credit. The first group of firms is also about twice as likely to have used credit at the time of starting-up, pay taxes, keep accounting records and be registered with the government. As for differences in personal entrepreneur characteristics, Table 2 shows that the owners of firms which applied for credit have about one and a half more years of schooling and work about 8 hours more per week.

By contrast, among firms that applied for credit, columns 3 and 4 of Table 2 show very strong similarities between those that obtained it and those that did not. The only noticeable – albeit perhaps unsurprising – difference is that 48 per cent of the first group report having used credit at the time of starting up, compared to 28 per cent for those firms that had their credit applications denied. In contrast, there are only very small differences between the two groups with regard to firm size, broadly defined formality and personal entrepreneur characteristics.

Within the group of firms that applied for and received credit, however, there are again considerable differences between those that report having used formal credit and those that had access to informal credit (columns 5 and 6). Thus, firms with access to formal credit have on average 50 per cent more workers, four times more capital, twice as large profits and three more years in business. Those firms are also between 40 and 50 per cent more likely to be formal – either in the sense of paying taxes, having accounting records or being registered – and their owners are on average two years older and have about two and a half more years of schooling. Overall, the above descriptive statistics suggest that larger and more formal firms owned by individuals with higher levels of human capital are more likely to be self-selected into the *Credit* treatment, especially from formal sources.

We revisit these findings below in the context of our probit estimates of the determinants of receiving credit or being selected into the other treatments on which the paper focuses. The second of those treatments is the receipt of some type of training during the year preceding the survey (*Training*), either by the owner of the firm or any of its workers. The third is the payment of at least some taxes as one of the regular expenditures made by the micro-firm (*Taxes*). Finally, our last treatment is membership in either the union or business association related to the firm's activity or sector (*Guild*). As in the case of *Credit,* a low proportion of micro-firm owners (6%) report having received training themselves or offered it to their workers during the previous year. A higher proportion (17%) belongs to some type of union or business association. Finally, as mentioned before, the high degree of informality of the sector is illustrated by the fact that only about 30 per cent of the sampled firms report paying taxes on a regular basis.

IV. Results

Propensity Scores Estimates

Table 3 presents the effect of selected covariates from the standard probit models used to generate the propensity score for the six treatments analysed: *Credit*, *Formal Credit*, *Informal Credit*, *Training*, *Taxes* and *Guild*. The marginal effect on the probability of participating in a treatment is reported. Tables showing all the covariates used in the probit models appear in Online Appendix C.

To begin, using the smallest conditioning set H suggests that individual entrepreneur characteristics are significantly related to the probability of being treated. In general, older workers are more likely to engage in most treatments, as are the more educated. In the case of credit, this is an unsurprising result because debts are personal liabilities of the firm's owner, so that lending to a firm is legally equivalent to lending to its owner, and human capital is likely to increase the chances of obtaining credit.[9] The magnitude of the effect of higher levels of human capital on the probability of being treated is largest for the cases of *Training*, *Taxes* and *Formal Credit*; it is smallest for *Informal Credit* and *Guild*.[10]

When we add to the conditioning set variables that are specific to the firm (Z), the effect of several entrepreneur characteristics changes significantly, suggesting that H and Z cannot be treated as orthogonal. For example, the magnitude and significance of the coefficients on human capital variables (the firm owner's age and schooling) generally fall. A possible explanation is that the effect of personal characteristics could now, to some extent, be captured by the inclusion of variables measuring firm size and time in business. These variables enter positively and significantly in virtually all cases (Table 3, columns 1–6), which is consistent with Levenson and Maloney's (1998) formulation of formality, broadly conceived as participation across a variety of societal institutions, as a normal factor of production for firms operating in a Jovanovic (1982) type environment of noisy selection.

In particular, larger and older firms could have a higher demand for the services provided by civil society institutions because they face a lower probability of failing, and a greater capacity for amortising the fixed costs involved in participation (for example, the cost of revealing their existence to tax authorities, membership fees, formal accounting procedures and so forth). More specifically, firm size and time in business may capture both greater credit worthiness and the availability of collateral and, hence, higher access to credit. Moreover, since participation in business associations and training is largely voluntary, the correlation with size and time in business could also be explained through the above mentioned normal good theory of participation.[11]

As for the positive correlation of size and time in business with the probability of paying taxes, it is consistent with two observationally equivalent stories. On the one hand, bigger, more established firms need government services more. On the other, size and time in business are also likely to be correlated with the probability of detection by tax authorities under the evasion hypothesis of informality. Hence, we cannot empirically identify the relative importance of the 'paying taxes to purchase services' and the 'evasion' views by looking at these correlates alone but must wait for the impact of the treatment itself. In all cases, since in many theoretical models both size and time in business are actually driven by entrepreneurial ability (again see

Table 3. Probit estimates (selected covariates)

	Credit	Formal credit	Informal credit	Training	Taxes	Guild	Credit	Formal credit	Informal credit	Training	Taxes	Guild
			Conditioning set (*H*,*Z*)						Conditioning set (*H*,*Z*,*F*)			
ln Capital stock	0.079	0.12	0.056	0.071	0.198	0.067	0.068	0.103	0.049	0.057	0.193	0.052
	(19.49)**	(16.59)**	(12.75)**	(13.84)**	(54.65)**	(19.58)**	(15.96)**	(13.91)**	(10.68)**	(10.62)**	(52.77)**	(14.64)**
ln Workers	0.418	0.445	0.283	0.248	0.483	0.319	0.375	0.398	0.254	0.175	0.454	0.258
	(20.63)**	(15.85)**	(12.37)**	(9.66)**	(27.26)**	(16.97)**	(18.08)**	(13.87)**	(10.89)**	(6.56)**	(25.24)**	(13.37)**
Time in business	0.011	0.018	0.004	−0.001	0.012	0.018	0.01	0.017	0.003	−0.003	0.011	0.017
	(8.68)**	(9.98)**	(2.97)**	−(0.42)	(11.78)**	(16.18)**	(7.45)**	(9.24)**	(2.12)*	−(1.70)	(10.66)**	(15.11)**
Formal credit										0.30	0.15	0.20
										(6.12)**	(3.68)**	(5.29)**
Informal credit										0.16	0.06	0.23
										(3.49)**	(1.97)*	(7.63)**
Training							0.28	0.27	0.16		0.21	0.23
							(7.22)**	(5.58)**	(3.66)**		(6.19)**	(6.47)**
Taxes							0.11	0.17	0.05	0.16		0.20
							(4.86)**	(5.13)**	(2.10)*	(5.53)**		(9.66)**
Guild							0.23	0.17	0.19	0.22	0.20	
							(9.64)**	(5.17)**	(7.29)**	(6.66)**	(9.10)**	
Observations	35044	35044	35044	35044	35044	35044	35037	35037	35037	35037	35037	35037
Pseudo R^2	0.0870	0.1213	0.0757	0.1978	0.1307	0.1318	0.1548	0.2247	0.1095	0.2363	0.2866	0.1821
Prob > Chi^2	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Additional covariates not reported here: Female, Head, Npers, Married, Age, Age2, Prim, Sec, High, dummies by year, industry and state. See Online Appendix A for a definition of the variables and for the complete set of results. *z* values in parentheses. *significant at 5 per cent; **significant at 1 per cent.

Jovanovic, 1982 for an example), by including those variables in our conditioning set we are likely to be purging the effect of this unobservable determinant of firm performance.

In Table 3, columns 7–12, we add the other treatments (F) to the conditioning set. The results indicate a statistically significant positive correlation between all the dimensions of formality or participation in societal institutions considered. The expansion of the conditioning set therefore has the effect of capturing a possible general movement toward formalisation, and isolating the particular impact of each treatment. For instance, if the impact of registering with the tax authority is purely driven by the fact that registering may be a precondition for accessing formal credit markets, then conditioning on the latter variable in the propensity score should reduce the measured impact of paying taxes on firm performance.

Probit estimates for the restricted sample that will be used for estimating treatment effects on growth and survival are not reported but they are available from the authors upon request. Overall, the results are very similar to those in the full sample. Moreover, as could be expected given their high correlation with size and education, first period profits are positively correlated with the likelihood of receiving all treatments.

Matching and Control Function Estimates

Profit and income levels. Tables 4, 5 and 6 report estimates of the treatment effects on profit and income levels, using OLS, matching and control function techniques respectively. The OLS estimates indicate that all the treatments have positive and significant effects on micro-firm profits, a result that is confirmed by the propensity score matching estimates. In both cases, the magnitude of the estimated effects is considerably sensitive to the set of regressors included in the propensity score model.

Table 4. Treatment effects on profits: OLS

Covariates	H	H,Z	H,Z,F
Profits (ENAMIN)			
Credit	0.379 (0.020)**	0.122 (0.019)**	0.095 (0.019)**
Formal Credit	0.622 (0.032)**	0.268 (0.030)**	0.227 (0.030)**
Informal Credit	0.217 (0.024)**	0.030 (0.023)**	0.014 (0.022)
Training	0.298 (0.028)**	0.112 (0.026)**	0.073 (0.026)*
Taxes	0.626 (0.013)**	0.310 (0.014)**	0.298 (0.014)**
Guild	0.335 (0.017)**	0.130 (0.016)**	0.099 (0.016)**
Income (ENEU)			
Credit	0.255 (0.015)**	0.090 (0.014)**	0.072 (0.014)**
Formal Credit	0.419 (0.024)**	0.193 (0.023)**	0.165 (0.023)**
Informal Credit	0.152 (0.017)**	0.030 (0.016)*	0.019 (0.016)
Training	0.235 (0.021)**	0.110 (0.020)**	0.083 (0.020)**
Taxes	0.423 (0.010)**	0.219 (0.010)**	0.210 (0.010)**
Guild	0.204 (0.012)**	0.073 (0.012)**	0.049 (0.012)**

Notes: *significant at 5 per cent; **significant at 1 per cent.

Table 5. Treatment effects on profits: ATT propensity score matching estimates – different methods

Matching on:	$P(H)$	$P(H,Z)$	$P(H,Z,F)$	$P(H)$	$P(H,Z)$	$P(H,Z,F)$
Profits (ENAMIN)	*Nearest neighbour*			*Kernel*		
Credit	0.353	0.085	0.095	0.394	0.115	0.093
	(0.041)**	(0.032)**	(0.032)**	(0.023)**	(0.022)**	(0.021)**
Formal credit	0.611	0.250	0.209	0.734	0.331	0.313
	(0.054)**	(0.047)**	(0.043)**	(0.034)**	(0.033)**	(0.029)**
Informal credit	0.200	−0.026	−0.011	0.226	0.047	0.033
	(0.051)**	(0.035)	(0.043)	(0.022)**	(0.022)*	(0.027)
Training	0.307	0.124	0.085	0.325	0.163	0.121
	(0.043)**	(0.053)**	(0.043)**	(0.028)**	(0.029)**	(0.033)**
Taxes	0.656	0.266	0.229	0.636	0.232	0.225
	(0.020)**	(0.022)**	(0.024)**	(0.016)**	(0.018)**	(0.021)**
Guild	0.345	0.102	0.094	0.339	0.118	0.092
	(0.026)**	(0.025)**	(0.026)**	(0.020)**	(0.018)**	(0.016)**
Income (ENEU)	*Nearest neighbour*			*Kernel*		
Credit	0.243	0.054	0.100	0.263	0.096	0.083
	(0.023)**	(0.023)**	(0.022)**	(0.014)**	(0.017)**	(0.015)**
Formal credit	0.382	0.153	0.177	0.503	0.257	0.251
	(0.041)**	(0.050)**	(0.038)**	(0.025)**	(0.023)**	(0.025)**
Informal credit	0.146	0.028	0.022	0.169	0.054	0.044
	(0.031)**	(0.037)	(0.028)	(0.022)**	(0.02)*	(0.017)**
Training	0.246	0.126	0.075	0.253	0.140	0.113
	(0.031)**	(0.038)**	(0.037)*	(0.024)**	(0.024)**	(0.028)**
Taxes	0.405	0.173	0.154	0.402	0.154	0.147
	(0.016)**	(0.015)**	(0.016)**	(0.011)**	(0.014)**	(0.015)**
Guild	0.198	0.055	0.045	0.199	0.062	0.044
	(0.019)**	(0.021)*	(0.023)**	(0.014)**	(0.013)**	(0.016)**

Notes: Kernel based matching methods using Epanichnikov kernel and a fixed bandwidth of 0.06. Bootstrapping standard errors based on 50 replications with 100 per cent sampling. *significant at 5 per cent; **significant at 1 per cent.

In fact, in all cases, moving from the minimal to the complete conditioning set reduces the impact of the various treatments, in some cases dramatically: for example, the impact of *Credit* declines by a factor of almost four. How much of this is due to really eliminating the bias arising from violation of the CIA and how much to the masking of the true effect by conditioning on *X*s affected by the treatment is difficult to nail down. The matching estimates do allow for speculation on the direction of bias in the estimates of each treatment. When only the covariate set H is used, we obtain the greatest ATT estimates, while their magnitude decreases as this set is enlarged (adding the sets Z and F). As mentioned earlier, if, in fact, including these regressors progressively lowers the selection bias in the estimate of all treatments, the former estimates should be considered as the *upper bound* and the latter the *lower bound*.

Online Appendix B analyses the quality of matching, looking both at the standardised differences in the covariates and at the common support before and after matching. We are generally able to construct a distribution quite close to that of the treated group. As a result, estimates with and without imposing the common

Table 6. Treatment effects on profits: control function estimates

Covariates:	*H*	*H,Z*	*H,Z,F*
Profits (ENAMIN)			
Credit	0.313 (0.053)**	0.362 (0.038)**	0.280 (0.038)**
Formal credit	0.393 (0.253)	0.604 (0.056)**	0.490 (0.061)**
Informal credit	0.114 (0.068)	0.181 (0.044)**	0.130 (0.043)**
Training	−0.176 (0.157)	0.021 (0.142)	−0.199 (0.260)
Taxes	0.574 (0.037)**	0.608 (0.027)**	0.584 (0.028)**
Guild	0.073 (0.091)	0.219 (0.042)**	0.089 (0.052)
Income (ENEU)			
Credit	0.114 (0.050)**	0.204 (0.045)**	0.133 (0.048)**
Formal credit	0.238 (0.081)**	0.333 (0.080)**	0.188 (0.086)*
Informal credit	−0.037 (0.068)	0.084 (0.061)	0.048 (0.062)
Training	0.217 (0.049)**	0.197 (0.047)**	0.128 (0.048)**
Taxes	0.161 (0.102)	0.451 (0.033)**	0.429 (0.035)**
Guild	−0.191 (0.069)**	−0.080 (0.094)	−0.263 (0.082)**

Notes: *significant at 5 per cent; **significant at 1 per cent. Maximum likelihood treatment affects regression.

support restriction are very similar. Thus, we report only the estimates based on the whole support of the propensity score domain.

Table 6 presents the maximum likelihood (ML) control function estimates. In all the cases we compute robust standard errors. Simple tests for the structural break in g_0 and g_1 suggest that the non-parametric approach is more desirable.[12]

Taken together, the set of estimation techniques and assumption sets generate commonalities that, subject to the caveats above, allow us to bracket the likely 'true' value of the parameters. Although the magnitudes are generally somewhat lower, in virtually all cases, the estimates with the ENEU income variable are broadly consistent with those that emerge from using the ENAMIN profit variable with the same estimation techniques. For this reason, we focus primarily on the results based on the larger ENAMIN sample.

The impact of credit. The matching and OLS estimates suggest that firms treated with any kind of credit (*Credit*) have a profit level that is between 9 and 37 per cent higher than the control group, with the estimates falling dramatically moving from $P(H)$ to $P(H,Z,F)$ as would be predicted. The ML control function estimates fall between these bounds (28% and 36%). A similar pattern is visible when we focus on the effect of receiving credit from formal sector sources (*Formal Credit*) but the returns are substantially higher. Here, estimates range from 22–73 per cent in the matching and OLS estimators, and from 39–60 per cent in the ML control function approach. Access to informal credit, on the other hand, yields more modest results ranging from 3–23 per cent in the matching and OLS estimates, with statistically non-significant effects when the full conditioning sets are used. With the ML control function approach, however, the impact of informal credit on profits is significant with wider conditioning sets, and varies from 13–18 per cent.

Taken together, the results suggest that access to credit does improve performance and that formal credit may have an impact perhaps two or three times that of informal credit. These estimates need to be taken with caution as the matching procedure cannot significantly reduce the post-matching bias in some Z-covariates (see Online Appendix B): the comparison group has on average less capital stock and employees than the treated group (25% and 10% respectively for formal credit, 9% and 7% for informal credit). In consequence, our estimates are believed to be biased upward, and no further reduction in the bias can be done in this context.

Paying taxes: bad luck or the price of useful government services? The impact of paying taxes (*Taxes*) is large and significant with all three methods, ranging from 30–63 per cent with OLS, 22–66 per cent with propensity score matching and close to 60 per cent in the ML control function. Strikingly, not a single specification yields a negative coefficient that would be expected if being discovered by the government and being made to pay taxes were completely without benefit and constituted a pure cost. Overall, it would seem that, on average for those complying, paying taxes is not detrimental and something to be evaded but actually improves firm performance. Again, under the evasion hypothesis, unlike the other treatments, there may be no bias if once controlling for the factors that would make a firm more visible, such as size or having been in business a long time, detection is more or less random or, perhaps negative if less clever entrepreneurs are detected more often. On the other hand, if registering to pay taxes buys services that increase income, the biases move in the same direction as those of the other treatments and the interpretation of the results needs to proceed similarly. In fact, the behaviour of the estimates under different conditioning sets does seem to follow patterns similar to those of the other treatments, falling significantly as the conditioning set is expanded, at least with OLS and matching. In the case of the control function approach, however, the argument for declining effects is in any case weaker, given that unobservables are expected to be controlled for in even the narrowest conditioning set. Thus, at a minimum, formality in the straightforward sense of paying government taxes leads to a 20 per cent increase in profits and, arguably, substantially more.

Training and business associations. OLS and matching estimates suggest that firms where the owner or some workers received training exhibit profits that are between 7 and 33 per cent higher than comparable non-training firms. However, non-significant and even negative effects are obtained with control function models, potentially suggesting that unobserved entrepreneur characteristics could be driving the positive treatment effects derived with the other estimation methods. Moreover, Online Appendix B shows that some post-matching bias in the Z-covariate set persists, revealing that training may be an exclusive attribute of large firms. Finally, participation in a business association or guild has an impact of 9–35 per cent with OLS and matching methods, and an overlapping range of 7–22 per cent with the control function. The lower bound across all techniques is around 10 per cent.

Income Growth

The ability to link the ENAMIN to the ENEU means that we can create a panel where profits are observed at two points in time, and extensive firm and personal entrepreneur characteristics that would determine selection into treatment are available for the initial period. While the data is not structured to permit the use of a difference-in-differences matching approach that would clear out any unobserved characteristics that could violate the CIA, the possibility of controlling for initial profits provides some additional defence for the CIA.

Table 7, columns 1 and 2, report unconditional average income growth by treatment status. It clearly appears that for all treatments, without controlling by any enterprise or personal characteristics, non-treated firms exhibit higher – although still negative, on average – rates of income growth. Our OLS, matching and control function estimates reduce this gap but do not reverse its sign (Table 7, columns 3–6, using the conditioning set *H, Z*; see Online Appendix C for the complete set of results). Moreover, with both OLS and matching, receiving credit – particularly if from informal sources – is significantly related to lower rates of income growth. The negative results are strongest with the Kernel matching and they are weakly echoed by the nearest neighbour matching estimates. For the other treatments – those not related to credit – we find non-statistically significant effects on income growth.

These negative effects of access to credit on growth could suggest that this treatment brings the corresponding firms closer to their steady state sizes with a consequent reduction in growth rates as they converge. Alternatively, as matching is done on after-treatment initial levels of profits, one could argue that the worse performing treated are matched to the best non-treated, thus downwardly biasing the estimated treatment effects.

Table 7. Income growth statistics and treatment effects estimates

	Mean by treatment status			ATT propensity score matching		
Treatment	Treated (%)	Non-treated (%)	OLS	Nearest neighbour	Kernel	Control function
Credit	−9.8	−4.9	−0.098	−0.094	−0.106	−0.064
			(0.041)*	(0.056)	(0.040)**	(0.111)
Formal credit	−18.2	−5.2	−0.090	−0.074	−0.104	0.171
			(0.071)	(0.109)	(0.063)	(0.203)
Informal credit	−8.1	−5.4	−0.099	−0.038	−0.095	−0.122
			(0.045)*	(0.066)	(0.051)	(0.110)
Training	−11.5	−5.4	−0.061	−0.115	−0.053	−0.065
			(0.070)	(0.090)	(0.064)	(0.191)
Taxes	−8.4	−4.4	−0.006	−0.031	−0.039	0.088
			(0.034)	(0.058)	(0.046)	(0.143)
Guild	−8.9	−5.1	−0.027	0.003	−0.036	0.182
			(0.039)	(0.054)	(0.041)	(0.121)

Notes: ENEU-ENAMIN small sample. Survivors only. Standard errors in parentheses. *significant at 5 per cent; **significant at 1 per cent . Conditioning set (*H*,*Z*). Kernel-based matching methods using Epanichnikov kernel and a fixed bandwidth of 0.06. Bootstrapping standard errors based on 50 replications with 100 per cent sampling. Control function: Maximum likelihood treatment effects regression.

Survival

Table 8, columns 1 and 2, show the raw survival likelihood by treatment status. For all treatments, the probability of survival is higher for treated than for non-treated firms, with an average difference of 10 percentage points. It thus appears that, despite the lack of positive effects of the various treatments on growth, they do increase the likelihood of micro-firm survival. Since control function techniques are not applicable in this context, we use propensity score matching estimates to check whether these differences are driven by the factors that determine selection into treatment.[13]

Both matching techniques suggest that access to credit is associated with higher survival probabilities (see Table 8, columns 3–8). When using the Kernel method, these positive effects are also significant for loans received from either formal or informal sources, with larger magnitudes obtained in the former case. Moreover, while access to training and guild membership appear unrelated to firm survival, we find that firms that pay taxes are significantly more likely to stay in business, a result that is robust to changes in estimation methods and in the set of conditioning variables. Concerns about reverse causality cannot be completely ruled out, as firms which foresee a bleaker future than their fundamental and observable characteristics would lead to predict would have no incentive to comply with government regulations – including paying taxes. However, another possible interpretation is that credit and registering with the government provide firms with means for

Table 8. Firm survival likelihood statistics and ATT propensity score matching estimates – different methods

	Mean by treatment status		ATT propensity score matching					
			Nearest neighbour			Kernel		
Treatment	Treated (%)	Non-treated (%)	*P*(*H*)	*P*(*H*,*Z*)	*P*(*H*,*Z*,*F*)	*P*(*H*)	*P*(*H*,*Z*)	*P*(*H*,*Z*,*F*)
Credit	80.3	72.7	0.048	0.069	0.023	0.027	0.042	0.022
			(0.024)*	(0.022)**	(0.023)	(0.015)	(0.015)**	(0.018)
Formal credit	85.7	73.5	0.009	0.075	−0.005	0.035	0.061	0.032
			(0.042)	(0.041)	(0.040)	(0.026)	(0.021)**	(0.028)
Informal credit	78.9	73.3	0.022	0.040	−0.006	0.023	0.035	0.020
			(0.030)	(0.029)	(0.033)	(0.020)	(0.017)*	(0.015)
Training	77.6	73.7	−0.003	0.009	−0.028	0.006	0.014	0.004
			(0.044)	(0.037)	(0.049)	(0.024)	(0.023)	(0.034)
Taxes	82.6	70.0	0.051	0.090	0.036	0.051	0.071	0.044
			(0.022)*	(0.020)**	(0.017)*	(0.014)**	(0.014)**	(0.019)*
Guild	82.7	72.2	0.033	0.033	0.019	0.025	0.042	0.018
			(0.023)	(0.025)	(0.024)	(0.019)	(0.017)*	(0.014)

Notes: ENEU-ENAMIN small sample. Survivors only. Standard errors in parentheses. *significant at 5 per cent; **significant at 1 per cent. Kernel based matching methods using Epanichnikov kernel and a fixed bandwidth of 0.06. Bootstrapping standard errors in parenthesis based on 50 replications with 100 per cent sampling.

insuring themselves against negative shocks – for example, ex-ante by diversifying their types of customers or ex-post by borrowing in bad times – thus increasing their odds of surviving them.

V. Conclusions

This paper has employed different techniques to explore the impact of credit, training, tax payments and participation in business guilds on firm profits, growth and survival likelihood. We compare the effects of these various 'treatments' using OLS estimates, propensity score matching and control function methods with several different conditioning sets. We discuss the potential selection biases that affect our estimates and which cannot be controlled for in a non-experimental setting. However, we attempt to provide reasonable lower and upper bounds for the various treatment effects listed above.

In the end, the results of the matching and control function techniques are reasonably compatible with each other. Our estimates suggest positive impacts of all treatments on the profit levels of micro-firms, even in the cases where the use of wide conditioning sets may lead to underestimating true treatment effects. This suggests that even if best performing firms may be selected into participating in an increasing array of societal institutions, this participation does feed back into further improvements of firm performance.

We also find that at least access to credit and straightforwardly defined formality – the fact of paying government taxes – are significantly related to an increase in the likelihood of firm survival, a result that we interpret as reflecting increased access to better ex-ante and ex-post risk coping mechanisms. Finally, somewhat surprisingly, we find negative – although, except in the case of credit, non-significant – treatment effects on growth, a result that is consistent with treated firms being closer to their optimal or steady state sizes.

Overall, our results indicate that increases in strictly or broadly defined formality have the potential for increasing profits and survival rates, and appear to bring micro-firms closer to their optimal sizes. This suggests that the significant within-country differences in firm productivity that are observed in developing economies are at least partially driven by market and government failures that limit the ability of micro-firms to reach their steady state size. In practical terms, this implies that efforts to improve the functioning of the markets for credit and business development services, as well as policies aimed at facilitating micro-firm formalisation, could contribute to reducing some of the sizable productivity gaps observed within developing economies.

Notes

1. Defining informality has proved elusive in the literature (see Perry et al., 2007). Here we define formality broadly as participation in societal institutions.
2. In Lucas' (1978) model, for instance, factors such as location and entrepreneurial ability determine firm-specific costs structures and optimal firm sizes. Newly created firms would tend to converge to their steady state sizes, provided that they are not supply-constrained in their access to factor or goods' markets.

3. For instance, at low levels of production, informal capital markets may suffice for fulfilling a firm's external financing needs (Besley, 1995). Similarly, where 'peer monitoring' generates more information than that available to market insurers, informal insurance provided by friends or communities can be a good complement to market insurance (Arnott and Stiglitz, 1991).
4. For a review of this literature see Heckman et al., 1999; Imbens, 2004.
5. As Heckman and Navarro-Lozano (2004) argue matching can be seen as a special case of control function methods where the former assumes that conditioning eliminates bias through the 'conditional independence assumption' (CIA), whereas control function methods explicitly model selection bias. In this approach, the preference for one or the other method cannot be driven by their intrinsic technical advantages but, rather, should be based on a judgment on which choice of underlying assumptions is most plausible.
6. We are grateful to an anonymous referee for this point.
7. Implicitly, we assume the existence of two different ways through which the treatment affects the outcome. First, a direct effect, which is the result of buying new equipment in the case of credit or increasing labour productivity in training. Second, an indirect effect, related to the impact on the firm's market performance and efficiency. Using regressors affected by the outcome may restrict our estimates to the latter.
8. Similar values are obtained using the information reported as total net income from self-employment in the ENEU: that variable has an average of US$325 and a median of US$219.
9. See, for instance, Berkowitz and White (2002).
10. This is consistent with many studies of credit markets in the US (see for instance Jappelli, 1990; Blanchflower et al., 2003) that find age is highly significant for determining whether an individual is credit rationed. For training, many papers have studied individual decisions to be treated but fewer have looked at the firm (see for example Frazis et al., 1998).
11. In the case of training, evidence from the US (Lynch and Black, 1995) shows that employers who invest more in physical capital are more likely to invest in workers instruction.
12. The MLE estimates are less volatile than two-step procedures in the absence of exclusion restrictions, although they impose further restrictions in the distribution of the error terms. Two-step estimates and structural break tests are not reported here but they are available from the authors upon request.
13. See also Fajnzylber et al. (2006) who study the determinants of micro-firm survival using probit models.

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