The Impact of Consulting Services on Small and Medium Enterprises: Evidence from a Randomized Trial in Mexico

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A randomized control trial with 432 small and medium enterprises in Mexico shows positive impact of access to 1 year of management consulting services on total factor productivity and return on assets. Owners also had an increase in "entrepreneurial spirit" (an index that measures entrepreneurial confidence and goal setting). Using Mexican social security data, we find a persistent large increase (about 50 percent) in the number of employees and total wage bill even 5 years after the program. We document large heterogeneity in the specific managerial practices that improved as a result of the consulting, with the most prominent being marketing, financial accounting, and long-term business planning.

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I. Introduction

A large literature in development economics and entrepreneurship aims to understand the impediments to firm growth, especially for small and medium-sized enterprises. Most of the focus thus far has been on financial constraints as a central obstacle to firm growth. For example, empirical studies have examined these constraints at the micro level (with lending experiments, see the review article by Banerjee, Karlan, and Zinman [2015]; with cash grant experiments, see de Mel, McKenzie, and Woodruff [2008], Karlan, Knight, and Udry [2015], and McKenzie [2015]) as well as at the macro level (King and Levine 1993; Rajan and Zingales 1998). However, capital alone cannot explain the entirety of firm growth; "managerial capital" is needed to know how to employ the capital best. We argue that managerial capital can directly affect the firm by improving strategic and operational decisions but can also affect the firm by increasing the productivity of other factors, such as physical capital and labor, by helping the firm use them more efficiently.[1] The multidimensional impact of managerial capital and its interaction with other factors often makes its effect difficult to measure empirically.

Recent work has shown enormous heterogeneity in management practices and CEO styles across firms; see, for example, Bertrand and Schoar (2003), Bennedsen et al. (2007), and Bloom and Van Reenen (2007, 2010). At the same time, there is also large heterogeneity in the measured productivity of firms; see, for example, Syverson (2011). But a central question remains: Is this observed heterogeneity a reflection of an optimal match between the underlying fundamentals of different firms and the type of management that is needed given the firm's state of development? Or is lack of managerial capital a first-order impediment to firm growth and profitability, since managers might be constrained in the acquisition of these skills? See, for example, Gompers, Lerner, and Scharfstein (2005) or Caselli and Gennaioli (2013).

We test if alleviating the constraints on managerial capital has a first-order effect on the performance and growth of small enterprises in emerging markets and, if so, which dimensions of managerial capital are particularly important for firm performance. For that purpose, we set up a randomized controlled trial in Puebla, Mexico, in which 432 mi-

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[1] Bruhn, Karlan, and Schoar (2010) discuss at more length the role of "managerial capital" as a key component for enterprise development, distinct from human capital.

cro, small, and medium-sized enterprises applied to receive subsidized consulting services, and 150 out of the 432 were randomly chosen to receive the treatment. The remaining 282 enterprises served as a control group that did not receive any subsidized consulting services. We focus on micro, small, and medium-sized enterprises since they are often seen as highly affected by limitations in managerial capital and have strong potential for scale-up if bottlenecks to their growth can be removed.[2] The intervention aims to expand the managerial skills of the owner-managers by giving them access to subsidized consulting and mentoring services. Treated enterprises were matched with one of nine local consulting firms on the basis of the specialized services they needed. Enterprises met with their consultants for 4 hours per week over a 1-year period. The enterprise owner and consulting firm decided jointly on the focus and scope of the consulting services based on a daylong diagnostic consultation between the enterprise and the consulting firm.

We measure impacts on the firms and the owner-managers in two different ways: (1) we administer surveys at baseline and a 1-year follow-up, and (2) we obtain confidential administrative data on employment levels and total wages for the firms in our treatment and control groups using 7 years of annual data (2 years prior to 5 years after the intervention) from the Mexican Social Security Institute (IMSS). The administrative data on firm outcomes remove the self-reporting biases that can be present in survey data.

We have three primary sets of results: First, we show that the consulting intervention has a positive short-run impact on the productivity, return on assets (ROA), and profits of the enterprises in the treatment group in the 1-year follow-up. Productivity and ROA increase by one-fifth of a standard deviation and profits increase by about one-tenth of a standard deviation compared to the control group. However, the effects on profits and ROA are not robust to all econometric specifications and assumptions regarding outliers. At the same time, the coefficients on individual input factors such as change in sales, assets, and the number of workers employed are not statistically significantly different from zero. This result is consistent with the idea that both the mistakes that firms were making and the impact of the consulting intervention are heterogeneous: for some, the improved managerial knowledge might have led to the realization that they need to invest more while for others it led to the realization that they need to shed unproductive assets and inputs or lay off unproductive workers.

Second, in the longer run, administrative data collected from the IMSS reveal important impacts on employment: the number of employ-

[2] In addition, for small businesses run by the owner-manager, it is simple to determine the appropriate target for a managerial capital intervention.

ees increases 57 percent and the total wage bill increases 72 percent. While we do not find an immediate increase in employment within the first year of treatment (which is in line with the results reported from the follow-up survey), we see an important increase in aggregate employment over the five post years 2010–14. And although the year-to-year change is imprecisely estimated, the difference between treatment and control increases each year except the fifth. These results suggest a persistent impact of the consulting on managerial capital. The point estimates of the positive treatment effects are quite large, but plausible, particularly given that the confidence interval includes more modest impacts and excludes zero. Furthermore, large treatment effects are plausible; since the majority of the enterprises in our sample were relatively small and the majority of owner-managers had not received any formal management training prior to our intervention, any improvements that led to the hiring of even a single worker would have been a noticeable increase in employment. The long-term results from the administrative data also suggest that the 1-year survey results were not merely a by-product of a positive reporting bias.

Third, and finally, we analyze the specific channels (the management practices) by which small businesses improve in response to the interventions, such as finance, marketing, operations management, and so forth. We find that there is a lot of heterogeneity in the business practices that small and medium-sized enterprises (SMEs) seek to improve. Out of 11 management practices that we asked about in the surveys, we find only two that are consistently mentioned and show statistically significant changes in likelihood after the intervention: (1) engaging in marketing efforts and (2) keeping formal accounts about their firms. The other management dimensions are mentioned with almost equal frequency across the enterprise owners, again highlighting that there are important heterogeneities in management needs of different SMEs. From case study evidence, we also identify long-term planning and business mission definition as a key activity with the consultants (see table 1).

We show that as a whole, these changes led to improvement in the overall confidence and control that micro, small, and medium-sized enterprise owners have in their business based on an index of "entrepreneurial spirit." The entrepreneurial spirit index was constructed using a number of questions we asked owners/managers about their confidence in their management skills and their ability to grow their firm and handle difficulties.[3] Although the individual components of the index are each imprecisely estimated, in particular, the components that seem to drive the result are goal related: having professional goals, revis-

[3] These questions were inspired by the "locus of control" literature in psychology (see, e.g., Furnham and Steele 1993).

TABLE 1
TOPICS THAT FIRMS WORKED ON WITH THEIR CONSULTANT BASED ON EIGHT QUALITATIVE CASE STUDIES OF TREATED FIRMS

Topic	Number of Firms That Covered This Topic
Define mission and vision statements	6
Accounting and record keeping (training and/or new software)	5
Clarify organizational structure, clearly assign responsibilities	5
Sales strategy and advertising (marketing)	4
Strategically select location and number of sales points	2
Quality control	2
Access to credit or alternative financing solutions	2
Human resources management and hiring practices	2
Mediate family problems in family firms	1
Pricing strategy	1
Reduce costs (negotiate with suppliers, find alternative suppliers)	1
Figure out which products are most profitable and focus on these	1
Teamwork and communications training for employees	1
Leadership training for firm owners	1

ing goals periodically, and needing daily goals to feel satisfied. To better understand the nature of these answers we also conducted in-depth interviews with two of the consulting firms after the intervention was concluded. In line with our case study evidence, the consultants highlighted that the enterprises they worked with during the program lacked a clear vision and definition of goals for the future and that they focused only on their day-to-day operations prior to the treatment. During the program, the consultant helped the owners/managers to define a growth strategy or business plan. This suggests that the large long-run impact of the treatment seems to have been in part due to firms defining clear goals and laying out a strategy for how to get there.

Our intervention documents the complexity and multidimensional nature of managerial decisions. While gaps in marketing and accounting knowledge as well as lack of long-term planning were most prominent across the sample, there seems to be a lot of heterogeneity in the specific bundle of knowledge gaps that enterprises face. This heterogeneity poses particular challenges for assessing interventions that aim to improve managerial capital and business outcomes (for more discussion of this, see Fischer and Karlan [2015]). To help us put more texture around the specific types of problems that were addressed in the consultations, in online appendix 1, we provide eight detailed narratives of the consulting advice provided to firms and the perceptions of the owners and consultants of their impact. These narratives tell a consistent story of complexity: lack of managerial capital is a first-order constraint for SMEs. However, there seems to be no silver bullet, that is, no single mechanism that when taught unleashes growth for these enterprises.

As one caveat, it is important to note that this intervention, like all skill-building experiments that have been conducted thus far, is a joint test of two closely related hypotheses: on the one hand, we aim to establish if managerial capital is a limiting factor in the growth of enterprises. But at the same time, we can find a positive answer only if this knowledge can be conveyed via a consulting intervention in the first place. It could be that managerial capital is indeed a hindrance to growth, but it might not be possible to transfer this knowledge by simply providing consulting services. Therefore, failure to find a result here would not prove that managerial capital does not matter, but may simply mean that this program was not effective in the transmission of managerial skills (or that managerial skills are innate skills and simply not teachable). However, this exercise provides a lower bound on the potential impact of improvements in managerial capital, given the limitation of the efficacy of this particular intervention to actually improve managerial capital.

Research and practice have recently seen a flurry of programs focused on developing managerial capital for micro enterprises. The interventions vary widely in the scope of the management skills that are transmitted and the type of enterprises that are targeted. The training is typically provided as in-class training and often linked with a microcredit program. For example, Cole, Sampson, and Zia (2011) and Karlan and Valdivia (2011) evaluate what is best described as in-class programs. These papers show that traditional micro enterprise training seems to affect the command of accounting practices for micro enterprises but has limited to no effects on actual firm outcomes and performance. More recently, Bruhn and Zia (2011) and Giné and Mansuri (2014) also find that in-class training for micro entrepreneurs leads to improvements in business practices but has only limited effects on business performance and sales. Drexler, Fischer, and Schoar (2014) show that training programs for SMEs increase in impact if they are targeted to the owner's level of sophistication: a simple rule-of-thumb training has large impacts on real outcomes for micro entrepreneurs who have low educational attainment and poor business practices prior to the intervention, but not on more advanced businesses. For micro-sized firms, Karlan et al. (2015) provide the closest analogue in terms of the intervention design, as it is one-on-one consulting services and not group-based training; however, the results are starkly different, as Karlan et al. find short-run negative treatment effects from consulting and long-run null effects.

The study by Bloom et al. (2013) is more closely related to our study in that they evaluate the impact of intensive consulting services from an international management consulting firm on the business practices of large Indian textile firms. The average firm in their sample has about 270 employees, whereas the average number of employees in our study is 14. Bloom et al. find that even these larger firms were unaware of many

modern management practices, and treated plants improved their management practices during the intervention. The approaches of Bloom et al. and this study are complementary in nature: Bloom et al. focus on a small set of large firms in one industry—textile manufacturing—with a tightly defined intervention employing a major international consulting firm. Such focus provides clear estimates of a specific management intervention, including mechanisms in terms of business practice changes, but it does not allow the authors to test if lack of managerial capital is a widespread problem. Our current study includes a larger set of firms and industries (close to 400 firms compared to 20 experimental plants in Bloom et al.'s study) and employs a heterogeneous set of local consulting firms. Therefore, we are able to establish that managerial capital constraints are important for a wider set of small businesses and affect business practices on many dimensions. We can provide proof of the concept that general increases in managerial capital for small businesses can improve firm performance and growth. But the trade-off is that we cannot estimate the returns to one specific management intervention or specific changes in particular business practices.

Using a different methodological approach, Giorcelli (2016) also provides similar evidence for the positive impact from building management practice on business outcomes in Italy during the 1950s. On the basis of a natural experiment from the Marshall Plan, Giorcelli finds limited evidence of an immediate impact on business outcomes but growing returns at 5, 10, and 15 years after the treatment (in this case the treatment is management training visits of Italian managers to US firms).

The remainder of this paper is structured as follows: In Section II, we describe the subsidized consulting program. Section III discusses the experimental setup, data collection, and characteristics of our sample. Section IV gives the results, examining both business outcomes and business process variables. Section V asks why more enterprises do not use consulting services, that is, given these results, what the possible market failures in the consulting services industry are. Section VI presents conclusions.

II. Consulting Program

The randomized controlled trial was conducted with the Puebla Institute for Competitive Productivity (known as IPPC, after its Spanish acronym), a training institute set up by the Ministry of Labor of the Mexican State of Puebla. IPPC implemented a business development program to provide participating enterprises with subsidized consulting services from one of a number of local consulting firms. The program, which started in March 2008 and ended in February 2009, aimed to include 100 micro, 40 small, and 10 medium-sized enterprises but eventually included 108 micro en-

terprises, 34 small enterprises, and 8 medium-sized enterprises.[4] The primary goal was to help enterprises reach the next size category by the end of the program and thus contribute to job creation and economic growth of the region.

Consultants were asked to (1) diagnose the problems that prevented the enterprises from growing, (2) suggest solutions that would help to solve these problems, and (3) assist enterprises in implementing the solutions. The consultants dedicated 4 hours per week to each enterprise. The program was originally intended to last 2 years but ended prematurely after 1 year because of government funding issues. (No results from the study had been released when the funding decision was made; thus, the decision was not related to perceived performance of the program.)

The consulting services were highly subsidized by the State of Puebla. Micro enterprises paid only 10 percent of the market cost of the consulting services, small enterprises 20 percent, and medium-sized enterprises about 30 percent. The unsubsidized cost of the consulting services varied by firm size but was equivalent to about US$57 (700 Mexican pesos) per hour on average, amounting to US$11,856 per firm for 1 year (4 hours for 52 weeks).

Consulting firms were selected through a competitive bidding process. In response to a call for proposals put out by IPPC, 11 consulting firms submitted proposals to participate in the program. Two firms were eliminated on the basis of inadequate references from former clients. The majority of the participating firms were private local consulting firms that usually work with micro, small, and medium-sized enterprises. All consulting firms signed a contract with IPPC that required them to spend 4 hours per week with each enterprise. IPPC monitored consultants by requiring consultants and enterprises to periodically submit documentation related to the program. Enterprise owners also came to IPPC's offices in person every quarter to pay their share of the program costs, which provided an opportunity to voice complaints. In addition, a local project supervisor from Innovations for Poverty Action (IPA), who was living in Puebla to manage the project evaluation, conducted monitoring visits to program enterprises.

At the beginning of the program, principal decision makers from all program enterprises, as well as most employees, completed a computerized test that determined their individual strengths and talents. This test was based on Gallup's StrengthFinder method, and IPPC was licensed to

[4] As defined by the Mexican Ministry of the Economy, micro enterprises have up to 10 full-time employees. Small enterprises have between 11 and 50 full-time employees in the manufacturing and services sectors and between 11 and 30 full-time employees in the commerce sector. Medium-sized enterprises have up to 100 full-time employees in the service and commerce sectors and up to 250 full-time employees in the manufacturing sector.

conduct this test in Puebla. IPPC encouraged enterprises to use the results of this test to help assign employees to responsibilities on the basis of their strengths as identified by the StrengthFinder method. The consultants were trained to help the enterprises interpret and apply the results to their labor decisions. For example, one talent was "communication" whereas another was "operations." Employees with the communication talent were particularly suited to interacting with clients, while employees with the operations talent would do well at record keeping and accounting.

Apart from the employee talent diagnostic, the content of the consulting varied across enterprises depending on their needs. In order to gain an understanding of the issues that enterprises worked on with their mentors, we conducted in-depth, qualitative case studies of eight treatment enterprises. Table 1 lists the areas that these eight enterprises covered with their consultants, along with the number of enterprises that worked on each topic. Almost all enterprises started by establishing mission and vision statements with their consultants, setting specific goals for what they wanted to achieve in the future and throughout the program. Most enterprises also worked on improving accounting and record keeping (through training and/or use of new software), clearly assigning staff responsibilities, and sales strategy and advertising. Apart from these common topics, the remaining topics covered are diverse, including optimizing the number and location of points of sale, quality control, access to credit or alternative financing solutions, pricing strategy, teamwork, and leadership training. This diversity reflects the fact that the consultants tailored their advice to each enterprise's individual challenges, leading them to work on different areas with each enterprise.

Each of the eight case studies is presented in online appendix 1.

III. Experimental Setup and Data

IPPC advertised the program throughout the State of Puebla via business associations, at trade fairs, and at various media outlets in order to attract an initial sample of interested micro, small, and medium-sized enterprises.[5] The program was open to enterprises that were formally registered with the government and were paying taxes. In response to the advertising, 432 enterprises expressed interest in the program and signed a letter of interest.

Data come from two sources: first, baseline and follow-up surveys of these interested enterprises were conducted between October and December 2007 (baseline) and between March 2009 and June 2009

[5] We do not have data on the channel through which enterprises learned of the program and thus cannot test any theories of heterogeneity with respect to this.

(follow-up).[6] These surveys collected information on enterprise characteristics and performance, as well as on business practices and characteristics of the enterprise's principal decision maker (typically the owner or manager). Second, from the IMSS, we secured wage and employment data for two pre-intervention years (2005 and 2006) and five post-intervention years (2010, 2011, 2012, 2013, and 2014). We tried to conduct a second follow-up survey in 2014 but encountered a very high nonresponse rate (see online app. 3: 2014 Follow-Up Survey).

Using data from the baseline survey, 150 enterprises were randomly selected to participate in the program.[7] The randomization was stratified by sector (manufacturing, services, and commerce) and enterprise size (micro, small, and medium-sized) and was conducted through a Stata program that was run on the premises of IPPC in the presence of government officials and a public notary, who certified that the assignment to the treatment group was random, that is, not rerun depending on any particular assignment.[8]

Out of the 150 enterprises in the treatment group, 80 then took up the consulting services.[9] The remaining 70 treatment group enterprises declined to participate in the program although they had initially signed a letter of interest saying that they would participate if offered a spot. The

[6] The baseline survey was conducted by a local professional survey firm under the supervision of the Mexico country office of IPA. For the follow-up survey, IPA hired surveyors (graduate students and recent graduates) directly. IPA trained the surveyors, and our local project staff managed and supervised the implementation of the follow-up survey.

[7] We originally had 434 observations in the randomization and assigned 150 of them to treatment, but we later discovered that two firms had expressed interest in the program twice under separate names. For this reason, we had to drop two observations, giving us 432 unique firms. In one of the cases, both separate names were in the control group, and we dropped one of these. In the other case, one name was assigned to the treatment group and the other to the control group. Here, we had to keep the firm in the treatment group since it had already been notified that it had been randomly selected to participate in the program.

[8] Within strata, the Stata code automatically re-randomized as follows. We first allocated firms to the treatment and control groups on the basis of a randomly generated number. Using this allocation, we then calculated the maximum and the average *t*-statistics on the differences in averages across the treatment and control groups for the following variables: within Puebla City dummy, business age, total asset value, profit margin, measured risk aversion, entrepreneurial spirit index, currently has a loan from a financial institution dummy, principal decision maker's hours worked, principal decision maker's age, principal decision maker's gender, principal decision maker's years of schooling, principal decision maker is of indigenous background dummy, as well as two dummies indicating whether the firm has participated in other IPPC programs. If the maximum *t*-statistic for these variables was higher than 1.25 or the average *t*-statistic was higher than 0.35, we drew a new random number and allocated firms to the treatment and control groups on the basis of this new number. We repeated this process until the maximum *t*-statistic was 1.25 or lower and the average *t*-statistic was 0.35 or lower. Research by Bruhn and McKenzie (2009) that was conducted after our randomization finds that this way of re-randomizing is no longer the preferred method. In our data analysis, we make the necessary adjustments for the randomization method suggested by Bruhn and McKenzie; i.e., in our regressions we control for all variables used in the re-randomization.

[9] Because of an administrative error, there was also one control group firm that was invited to participate, and did, in the program. For analysis purposes, we adhere to the random assignment, and this enterprise is included in the control group.

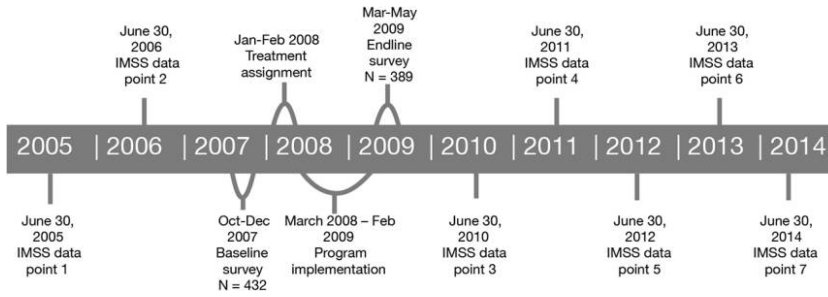

FIG. 1.—Time line

take-up rate was higher among enterprises in the services and manufacturing sectors (56.6 percent and 53.5 percent, respectively), compared to enterprises in the commerce sector (48.7 percent). Most enterprises that chose not to participate said their financial situation had changed since they signed the letter of interest and they no longer had sufficient funds to pay the fee (albeit subsidized) for the consulting services. IPPC paired the 80 treatment group enterprises that took up the program with consulting firms according to the consultants' sector and enterprise size expertise, as well as geographic restrictions. Figure 1 includes a comprehensive project time line, illustrating how the dates for data collection, randomization, and program implementation line up.

Table 2 provides summary statistics of baseline characteristics for enterprises and their principal decision makers in the treatment and control groups. About 30 percent of enterprises in each group operated in the manufacturing sector, 25 percent in the commerce sector, and 45 percent in the services sector. On average, the enterprises in the study had about 14 full-time paid employees and were slightly over 10 years old. The enterprises' principal decision makers were, on average, 43 years old, 72 percent of them were men, and on average they had completed 16 years of schooling.

Panel C of table 2 displays our main measures of business performance, starting with sales (online app. 2: Surveys and Data Definitions provides details of the survey questions and definitions). Our baseline measure of sales is the average of monthly sales in July, August, and September 2007.[10] This variable varies widely in our sample. At baseline,

[10] About 2.5 percent of enterprises report zero sales for all 3 months (this percentage is not statistically different across the treatment and control groups). Since these enterprises report having employees, as well as assets, and report nonzero hours worked and costs, we assume that they did not want to report their sales and thus replaced their sales with missing (it is unlikely that they had zero sales in all 3 months and are still in business). We apply the same procedure to the follow-up data, where about 3.5 percent of both treatment and control enterprises report zero sales for all 3 months (December 2008, January 2009, and February 2009 in the follow-up survey). Our measure of sales is thus greater than zero for all enterprises.

TABLE 2
Baseline Summary Statistics and Take-Up Analysis: Means and Standard Deviations

	Treatment (1)	Control (2)	Orthogonality Verification (1)−(2) Difference (p-Value) (3)	Took Up Treatment (4)	Did Not Take Up Treatment (5)	(4)−(5) Difference (p-Value) (6)	N (Treatment+ Control) (7)
			A. Stratification Variables				
Manufacturing sector dummy	.300	.323	−.023	.363	.229	.134*	
	(.460)	(.468)	(.628)	(.484)	(.423)	(.072)	
Commerce sector dummy	.253	.230	.023	.225	.286	−.061	
	(.436)	(.422)	(.600)	(.420)	(.455)	(.400)	
Services sector dummy	.447	.447	.000	.413	.486	−.073	
	(.499)	(.498)	(.998)	(.495)	(.503)	(.372)	
Full-time paid employees	14.400	13.684	.716	18.825	9.343	9.482*	
	(30.887)	(31.479)	(.820)	(36.288)	(22.444)	(.053)	
			B. Re-randomization Variables				
Principal decision maker's age (years)	42.561	42.876	−.315	42.443	42.696	−.253	
	(10.212)	(9.878)	(.759)	(9.540)	(10.999)	(.882)	
Male principal decision maker dummy	.727	.720	.007	.800	.643	.157**	
	(.447)	(.450)	(.881)	(.403)	(.483)	(.033)	
Principal decision maker's years of schooling	15.630	15.932	−.302	16.138	15.050	1.088	
	(4.919)	(5.196)	(.552)	(4.472)	(5.358)	(.182)	
Business age (years)	11.053	13.652	−2.599	12.825	9.029	3.796**	
	(10.330)	(28.120)	(.168)	(11.501)	(8.437)	(.022)	
Observations	150	282	432	80	70	150	

	C. Other Variables: Business Outcomes						
Average sales July, Aug., and Sept. 2007 (US$1,000s)	79.163	55.258	23.905	105.916	48.260	57.656	368
	(288.679)	(140.493)	(.382)	(349.912)	(194.184)	(.249)	
Average sales July, Aug., and Sept. 2007 (US$1,000s), 1% winsorized	67.434	54.450	12.984	85.530	46.531	38.999	368
	(196.519)	(131.771)	(.506)	(207.938)	(181.989)	(.266)	
Sept. 2007 costs (US$1,000s)	44.565	56.216	−11.651	58.736	26.974	31.762	377
	(120.341)	(263.955)	(.557)	(145.907)	(75.425)	(.112)	
Sept. 2007 costs (US$1,000s), 1% winsorized	44.471	40.611	3.860	58.566	26.974	31.592	377
	(117.222)	(99.258)	(.749)	(141.235)	(75.425)	(.105)	
Profits (Sept. 2007 sales minus costs, US$1,000s)	13.281	−3.797	17.078	8.375	19.365	−10.991	337
	(112.277)	(204.743)	(.324)	(87.198)	(137.923)	(.625)	
Profits (Sept. 2007 sales minus costs, US$1,000s), 1% winsorized	10.540	10.499	.041	11.722	9.075	2.648	337
	(73.332)	(76.507)	(.996)	(69.715)	(78.279)	(.852)	
Business assets (US$1,000s)	296.964	945.842	−648.879	341.570	246.574	94.996	313
	(767.969)	(7,822.005)	(.248)	(779.399)	(758.949)	(.510)	
Business assets (US$1,000s), 1% winsorized	288.056	395.699	−107.643	324.777	246.574	78.203	313
	(710.962)	(1,267.199)	(.337)	(669.823)	(758.949)	(.561)	

TABLE 2 (*Continued*)

	Treatment (1)	Control (2)	Orthogonality Verification (1)−(2) Difference (*p*-Value) (3)	Took Up Treatment (4)	Did Not Take Up Treatment (5)	(4)−(5) Difference (*p*-Value) (6)	*N* (Treatment+ Control) (7)
	C. Other Variables: Business Outcomes						
Productivity residual from regression of log Sept. 2007 sales on log employees and log assets	.028	−.016	.045	.439	−.437	.876***	265
	(1.349)	(1.253)	(.791)	(1.477)	(1.017)	(.001)	
Productivity residual, 1% winsorized	.024	−.015	.040	.431	−.437	.868***	265
	(1.320)	(1.251)	(.811)	(1.429)	(1.017)	(.001)	
ROA: Sept. 2007 sales minus costs divided by assets	−.026	.152	−.178	.160	−.254	.414*	252
	(.956)	(.817)	(.137)	(.366)	(1.342)	(.061)	
ROA, 1% winsorized	.033	.120	−.087	.160	−.121	.281**	252
	(.596)	(.647)	(.286)	(.366)	(.770)	(.037)	
F-test *p*-value: joint significance of all nonwinsorized business outcomes			.000			.000	
F-test *p*-value: joint significance of all 1% winsorized business outcomes			.281			.000	

NOTE.—Columns 1, 2, 4, and 5 present means and standard deviations (in parentheses). Column 3 shows the difference in means across the treatment and control group with the corresponding *p*-value in parentheses. Column 6 shows the difference in means across treatment enterprises that did and did not take up the program with the corresponding *p*-value in parentheses. The 1 percent winsorized variables are winsorized at the top and bottom 1 percent. The *N* reported here refers to the total amount of people in each group. Note that not all of these people had a value for each of the stratification variables, so the individual *N* for each of these characteristics might differ slightly from the one reported.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

average sales in the treatment group were US$79,163 with a standard deviation of 288,679, and US$55,258 in the control group, with a standard deviation of 140,493. To reduce the noise in this variable, we winsorize the top and bottom 1 percent of outliers.[11] The averages of winsorized sales are more similar across the treatment and control groups (US$67,434 and US$54,450, respectively) than for the unwinsorized variables, although no differences are statistically significant for either the winsorized or unwinsorized data.

Our baseline measure of profits is calculated as September 2007 sales minus September 2007 costs (unlike sales, we collected costs for only 1 month in the surveys).[12]

We calculate two separate measures of enterprise productivity. The first is the residual from a regression of log sales on log employees and log business assets. The second is return on assets (ROA), defined as profits (calculated as sales minus costs) divided by business assets.

Similarly to sales, the variances of profits, productivity, and ROA are large.[13] For this reason, we include the averages of the 1 percent winsorized variables in table 2, where we winsorized the top and bottom 1 percent of outliers as described in note 11. After winsorizing, average baseline profits are the same in the treatment and control groups (about US$10,000). Overall, we find no statistically significant differences in business performance variables at baseline.

Columns 4, 5, and 6 of table 2 examine whether there are differences between the treatment group enterprises that took up the program at baseline and treatment group enterprises that did not take up the program. We find that enterprises that took up the program are more likely to be in manufacturing, have a larger number of full-time paid employees, have male decision makers, and are older. In addition, panel C of table 2 shows that enterprises that took up the program were more productive at baseline than enterprises that did not take up the program.

[11] Since our sample includes micro, small, and medium-sized firms and to avoid simply winsorizing the largest firms instead of true outliers, we use the following procedure when winsorizing sales. We regress sales on a set of firm size dummies (micro, small, and medium), and for firms with residuals from this regression that are below the bottom 1 percent or above the top 1 percent, we replace sales with the predicted value plus the residual at the top and bottom 1 percent or top 1 percent. We use an analogous procedure for all other winsorized and trimmed variables.

[12] De Mel, McKenzie, and Woodruff (2009) suggest asking business owners what their profits are in one simple question as an alternative to calculating profits based on responses to specific components. We tried this approach but had a very high nonresponse rate to this question.

[13] Also note that not all enterprises that answered the baseline survey reported the business performance variables, as shown in col. 7 of table 2 (panel C). The response rate is lowest for business assets, which only 313 out of 432 enterprises (72 percent) reported.

We conducted the follow-up survey between March and June 2009 (i.e., 1–4 months after the intervention ended, which is 12–16 months after the intervention began), reinterviewing 378 enterprises or 88 percent of the 432 enterprises interviewed at baseline, to measure the impact of the consulting services on business outcomes. Out of the 54 enterprises that could not be reinterviewed, 11 enterprises were confirmed closed, 31 declined to participate in the interview, and seven enterprises could not be tracked down despite repeated contact attempts.[14] The remaining five enterprises had merged with another enterprise—one of them with an enterprise outside our sample and two with two other enterprises in the sample. For these five enterprises, we were not able to obtain separate data for the unit corresponding to the original enterprise, and thus they are not included in the analysis. We provide an analysis of attrition rates and correlates with baseline information in appendix table A1. This analysis shows that there are no differential attrition rates in the follow-up survey across treatment and control groups; neither do we see compositional shifts (col. 3).[15]

Next, since all enterprises were formally registered with the tax authority, we secured administrative employment data (number of employees and total wage bill) from the IMSS, the equivalent of the US Social Security Administration. We collected each firm's taxpayer number (RFC) during our baseline and follow-up surveys. Using these RFC numbers, we were able to obtain the mean and standard deviation in the treatment and control groups (but not individual firm-level data) for 2 years prior to the intervention and 5 years following.

In Mexico, all enterprises are required to register their paid employees with IMSS, but in practice, not all enterprises register their workers, even if the enterprise itself is registered with the tax authority. Some enterprises also register only a fraction of their paid workers with IMSS. Close to 57 percent of the enterprises in our sample were matched with IMSS records. In addition to underregistration, two other potential reasons why enterprises are not found in the IMSS data are that (1) some firms in our sample do not have paid employees and (2) some RFC num-

[14] We verified with the former principal decision maker and/or neighbors that these enterprises had indeed closed. The percentage of closed enterprises was lower in the treatment group (1.4 percent) than in the control group (3.3 percent). However, the difference is not statistically significant. The percentage of enterprises that refused the interview was slightly higher in the control group (8.7 percent) than in the treatment group (5.6 percent), but the difference is not statistically significant.

[15] Not all enterprises that answered the follow-up survey responded to each question. For this reason, our business outcome variables are missing for part of the sample. We tested whether the likelihood of having missing business outcomes variables due to either attrition or nonresponse differed statistically significantly across the treatment and control groups and do not find this to be the case.

bers may contain typos, although we tried to clean them up as much as possible. The percentage of matched enterprises is not statistically significantly different in the treatment and the control groups (58.7 percent and 56.7 percent, respectively).

We obtained IMSS data for two pre-intervention time periods (June 30, 2005, and June 30, 2006), as well as five post-intervention time periods (June 30, 2010, June 30, 2011, June 30, 2012, June 30, 2013, and June 30, 2014), on (1) number of full-time employees and (2) total daily wage bill paid to these employees. For confidentiality reasons, IMSS staff could not share enterprise-level data. Instead, they provided averages and standard deviations for the treatment and control groups. IMSS also provided a list of the firms that had successfully been matched with their database. Appendix table A2 reports attrition analysis for IMSS data; we find neither differential attrition for treatment on average (cols. 1 and 2) nor compositional changes (col. 3; aggregate p-value of .122 for the F-test of joint significance all interaction terms). The analysis does suggest though that firms with a higher number of baseline employees are somewhat more likely to be found in the IMSS data in the control group compared to the treatment group. For this reason, average employment in the IMSS data before the intervention, that is, in both 2005 and 2006, is higher in the control group (about eight full-time employees) than in the treatment group (6.2 full-time employees).

IV. Results and Discussion

A. Short-Run Business Performance

Table 3 reports the main specification, using ordinary least squares (OLS) to compare treatment to control in the cross section. All regressions include controls for the variables used for stratification (both the strata dummies and the re-randomization variables) as suggested in Bruhn and McKenzie (2009) and a control for the timing of the survey.[16] In column 1, we estimate the average intent-to-treat (ITT) effect without

[16] Because of baseline data entry typos that were discovered and corrected after the randomization took place, a few values of the variables included in the randomization procedure do not correspond to the true baseline values. The strata dummies and re-randomization controls included in the regressions contain the values originally used in the randomization procedure. All other baseline data used in the summary statistics and regressions contain the correct baseline values. Appendix table A3 shows that 70.4 percent of treatment group enterprises and 62.6 percent of control group enterprises were interviewed in March ($p = .12$). Almost all the remaining enterprises were interviewed in April 2009 or May 2009, with only four enterprises being interviewed in June 2009. Treatment and control enterprises are equally likely to have been interviewed in either March or April (percentage point difference of 1.3 and p-value of .66).

TABLE 3
ITT Treatment Effect Estimates, Short-Run Business Outcomes: OLS

Outcome Variable	ITT Treatment Effect Estimates (1)	(2)	1% Winsorized (3)	(4)	1% Trimmed (5)	(6)	Control Group Mean (SD) (7)
Full-time paid employees	1.475	.516	1.271	.516	−.562	−.485	12.428
	(1.421)	(1.260)	(1.332)	(1.152)	(.953)	(.911)	(22.281)
	378	378	378	378	370	370	243
Log(total employees)	−.117	−.074	−.113	−.070	−.126	−.087	2.319
	(.092)	(.071)	(.091)	(.070)	(.088)	(.064)	(1.106)
	375	375	375	375	367	367	241
Average sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s)	−5.108	−11.886	−5.423	−10.226	−2.146	−2.664	63.384
	(15.452)	(10.876)	(10.930)	(8.283)	(8.616)	(7.497)	(163.643)
	307	307	307	307	299	299	200
Log(average sales Dec. 2008, Jan. and Feb. 2009 in US$1,000s)	.006	.050	.005	.048	.049	.092	2.391
	(.175)	(.144)	(.174)	(.143)	(.177)	(.147)	(2.023)
	307	307	307	307	299	299	200
Feb. 2009 costs (US$1,000s)	5.525	5.657	−.290	−.525	−8.117	−8.328	43.157
	(14.694)	(14.551)	(8.750)	(8.465)	(5.661)	(5.734)	(113.758)
	304	304	304	304	296	296	204

Profits (Feb. 2009 sales minus costs, US$1,000s)	5.802	5.330	6.108	6.156	8.219*	8.384*	11.460
	(5.831)	(5.705)	(5.016)	(4.936)	(4.606)	(4.536)	(97.044)
	265	265	265	265	259	259	176
Log(business assets)	−.055	−.106	−.060	−.111	−.170	−.158	4.307
	(.176)	(.157)	(.175)	(.156)	(.173)	(.157)	(1.699)
	319	319	319	319	311	311	203
Productivity residual from regression of log Feb. 2009 sales on log employees and log business assets	.270*	.250*	.261*	.242*	.204*	.195*	−.095
	(.141)	(.130)	(.137)	(.127)	(.123)	(.117)	(1.272)
	250	250	250	250	244	244	158
ROA: Feb. 2009 sales minus costs divided by assets	.105*	.098	.085*	.080	.070*	.064	.012
	(.060)	(.064)	(.050)	(.054)	(.041)	(.046)	(.471)
	236	236	236	236	230	230	154
Controls for baseline value of outcome	No	Yes	No	Yes	No	Yes	. . .

NOTE.—Each row in cols. 1 and 2 contains the treatment effect point estimate, robust standard error, and number of observations for a separate OLS estimation. For the regressions that control for the outcome variable measured at baseline (cols. 2, 4, and 6), when the baseline outcome variable is missing, the missing value is filled in with zero and a dummy variable indicating that the baseline observation is missing is added to the model. All regressions include controls for strata dummies and re-randomization variables, as well as a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up. In cols. 3 and 4, outcome variables are winsorized at the top and bottom 1 percent. In cols. 5 and 6, outcome variables are trimmed at the top and bottom 1 percent. Column 7 contains nonwinsorized, untrimmed means and standard deviations for the control group at follow-up.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

controlling for the baseline value of the outcome variable, and in column 2 we report the average ITT effect with controlling for the baseline value of the outcome variable. For observations in which the baseline value of the outcome is missing, we replace this value with zero and include a dummy variable indicating that the value is missing, in order to keep the observation in the sample.

Columns 1 and 2 of table 3 show short-term treatment effects of consulting on enterprise productivity as measured by the residual from a productivity regression (0.21 standard deviations increase, standard error [SE] = 0.11 standard deviations) and by ROA (0.22 standard deviations, SE = 0.13 standard deviations). We find positive but not statistically significant point estimates on the short-term treatment effects for paid employees, log sales, and profits, and we find negative but also not statistically significant point estimates on sales, log total employees, and log of firm assets. The confidence interval for the null results (seen by dividing the standard error from cols. 1 and 2 by the control group standard deviation in col. 7) is typically about 0.1–0.2 standard deviations. To deal with noise in survey responses we also perform three main robustness checks of the results. First, we winsorize the outcome variables at the 1 percent level to check whether the results are driven by outliers (table 3, cols. 3 and 4).[17] Second, we trim the outcome variables at the 1 percent level (table 3, cols. 5 and 6). The results do not change qualitatively, but in the trimmed sample the treatment effect on profits becomes statistically significant at the 10 percent level. Third, we restrict the sample to the 221 enterprises that report all outcome variables at follow-up and run all of the same regressions (app. table A4).

In app. table A5 we also estimate a difference-in-difference specification for the same set of dependent variables as in table 3 rather than a cross-sectional specification. These results are not as efficient as the analysis of covariance in table 3 when outcomes have low autocorrelation, as in our data (see McKenzie 2012). The estimates for ROA remain similar in terms of statistical significance, but the standard errors for the productivity residual increase. For app. table A5, we again use the full sample, a 1 percent winsorized sample, and a 1 percent trimmed sample.[18]

[17] We also replicated the analysis using a 5 percent winsorized sample, and the results are virtually unchanged.

[18] As an additional check, app. table A6 displays average business outcomes from the follow-up survey in the treatment and control groups, as well as in the group of treatment enterprises that took up the program. A simple comparison of follow-up survey means in the treatment and control groups shows a positive effect of the consulting services on productivity. Comparing only enterprises that took up the program to control group enterprises shows even larger differences in both productivity and ROA (this comparison is not causal; in particular, note that at baseline the enterprises that took up the program already had higher productivity and ROA than enterprises that did not take up the program, as shown in table 2).

We explore the robustness of the total factor productivity (TFP) results from table 3 further in appendix table A7. This table reports estimation results for all the variables that we use to calculate TFP. In particular, our TFP measure uses log(February 2009 sales) instead of log(average sales for December 2008, January, and February 2009), which we report in our main tables, including table 3, since we have employees and business assets only for February 2009, not the other two months. For sales, we report the 3-month average in the main tables since, unlike February 2009 sales, the average does not include zeroes and we can thus show the log specification without losing observations. When we estimate the effect of the program on log(February 2009 sales) instead, we get larger point estimates than for the 3-month average.

We use the estimates in table A7 to check whether our TFP results are consistent with the sales and input results. Our TFP measure is defined as log(February 2009 sales) − alpha × log(total employees) − beta × log (business assets). Because the sum of alpha and beta is theoretically expected to be (and empirically almost always estimated to be) smaller than or equal to one, changes in TFP are generally equal to the difference between change in log sales and a weighted average of changes in labor and capital. Since our estimated change in sales is positive and changes in inputs are negative, an upper bound on the change in TFP is given by change in log sales + max[abs(change in log labor), abs(change in log capital)]. Using the coefficients in column 3 of table A7, this calculation gives a max of about $0.135 + 0.106 = 0.241$, which is close to the estimated TFP change of 0.25.

Another robustness check in table A7 includes materials costs in our TFP calculations. We do not include materials costs in the main tables since this variable has many missing values, and using it would thus have reduced our sample size further. In table A7, we recalculated TFP also controlling for materials costs, which we define as the sum of material inputs (raw materials, merchandise, etc.), utilities, and maintenance. The estimated effect on TFP is slightly smaller in absolute terms when controlling for materials costs, but the magnitude also corresponds to about one-fifth of a standard deviation. As a final check in table A7, we followed a Solow residual approach by calculating TFP as

$$\log(\text{February 2009 sales}) - 0.24 \times \log(\text{total employment}) - 0.16 \times \log(\text{business assets}) - 0.6 \times \log(\text{materials costs}).$$

The estimated effect size is again one-fifth of a standard deviation.

Since only 80 of the 150 firms took up the consulting service and there are systematic differences between those that took up and those that declined treatment, we repeat our analysis from table 3 using a matched

control group. We match on the variables that are statistically significantly different across the two groups, as per table 2: an indicator for the manufacturing sector, an indicator for male principal decision maker, number of employees, firm age, and productivity. All of these variables are consistently reported for all firms at baseline, except for productivity, which is missing for a subset of firms at baseline. In these cases we replace productivity with zero if it is missing and include an indicator variable equal to one if missing. We use caliper matching on the logit of the propensity score, with a caliper of 0.2 standard deviations of the logit. This process is based on Austin (2011, 2014).

The summary statistics for comparing firms that took up the program to their matches from the control group are in appendix table A8. The estimated effects from this exercise are in appendix table A9. We see that the point estimate for productivity is statistically significant and larger than in the ITT estimation on the full sample. Results for log sales, profits, and ROA are also positive, and the estimated coefficients, on average, are larger than in the equivalent ITT estimation, but none is statistically significant.

In appendix tables A10 and A11, we show results on heterogeneity by enterprise size and sector, respectively. We find essentially no pattern of impact; but noting the limited power for these tests given the sample size, we do not draw any conclusion from this analysis.

One final concern with the outcome data from the follow-up survey is that since the information is self-reported, treatment enterprises could have reported more positive outcomes to please the surveyors (for transparency reasons, enterprises were informed that the survey was linked to the consulting program). To address this concern, we test whether (1) treated enterprises were more likely to provide alternative contact persons on the survey,[19] and (2) treatment enterprises were more likely to report sales on the follow-up survey, which should be the case if they wanted to please the interviewer. Appendix table A12 displays the results for these tests. We find no statistically significant differences in either measure across the treatment and control groups, although we recognize that this is not definitive proof against all self-reporting bias.

B. Long-Run Business Performance

Therefore, we also use administrative data from the IMSS to avoid reporting biases stemming from survey data. In addition, the IMSS data allow us to follow enterprises for a longer time period. Table 4 reports the

[19] We asked for alternative contact persons in case we needed to get in touch with the enterprises at a later stage for clarifications or additional questions and could not reach the enterprise through our contact information on record.

TABLE 4
ITT Treatment Effect Estimates, Long-Run Business Outcomes Difference-in-Difference: OLS

	A. ITT Regression Results					
	Outcome Variable					
	Number of Employees			Daily Wage Bill (US$)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × post			5.765***			125.210***
			(1.332)			(38.064)
Treatment (= 1 if mean is for treatment group)			−1.766***			−30.602***
			(.495)			(9.512)
Post (= 1 for years 2010–14)			2.116***			49.802***
			(.495)			(12.885)
Constant			7.991***			136.76***
			(.492)			(6.646)
Observations			14			14

	B. Raw Data					
	Treatment Mean (SD)	Control Mean (SD)	Difference (*p*-Value)	Treatment Mean (SD)	Control Mean (SD)	Difference (*p*-Value)
2005	6.169	7.402	−1.233	98.02	128.81	−30.79
	(13.226)	(16.490)	(.544)	(154.126)	(248.604)	(.289)
2006	6.281	8.579	−2.298	114.29	144.70	−30.41
	(11.865)	(18.853)	(.298)	(159.164)	(293.049)	(.365)
2010	9.787	10.262	−.475	162.98	161.89	1.09
	(35.958)	(21.181)	(.895)	(353.039)	(169.098)	(.974)
2011	14.067	10.098	3.970	263.02	169.10	93.92
	(66.707)	(19.916)	(.480)	(717.155)	(321.992)	(.153)
2012	14.551	10.055	4.496	294.252	184.856	109.40
	(67.984)	(20.668)	(.434)	(798.781)	(343.924)	(.131)
2013	16.674	9.963	6.711	339.82	196.25	143.57
	(90.136)	(19.734)	(.361)	(967.228)	(375.661)	(.094)
2014	15.449	10.152	5.297	345.77	220.70	125.07
	(79.492)	(22.129)	(.425)	(903.255)	(450.607)	(.143)
Number of enterprises	89	164	253	89	164	253

Note.—Administrative data are from Mexico's Social Security Institute (IMSS) for years 2005, 2006, 2010, 2011, 2012, 2013, and 2014. All enterprises are required by law to register their workers with IMSS (although compliance is not universal); 57 percent of the enterprises in our sample were found in IMSS records. Both number of employees and daily wage bill refer to permanent employees with pay. Column 3 in panel A displays the results from a regression of mean number of employees on a dummy for the mean being for the treatment group, a dummy for the postconsulting intervention period, and the interaction of these two dummies. Column 6 in panel A shows results for the corresponding regression with the mean daily wage bill as the outcome variable. Panel B displays the raw data, where the means in cols. 1, 2, 4, and 5 are the observations used in the regressions in panel A.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

long-run impact on employment. As discussed above, we do not have individual firm data for privacy reasons, but rather have the average number of employees and the average wage bill for 2 years before the treatment and 5 years after (2009–14), separated by treatment and control groups. We consider having administrative records rather than self-reported firm data to be a major advantage and reconfirmation of our results. We use a difference-in-difference specification, with the treatment effect being identified by the interaction of treatment and post and the unit of observation being the treatment group × year (i.e., 14 data points, where each data point is the average of all the firms in that treatment assignment × year). We find an increase of 5.7 employees (SE = 1.3), which corresponds to 57 percent (the average number of employees in the control group across the 5 post years is 10.1), and an increase of US$125 in the daily wage bill (SE = $38), which is 72 percent (the average daily wage bill in the control group across the 5 post years is $172).

Figures 2 and 3 illustrate these results graphically. They show that both the average number of employees and the daily wage bill were similar across the treatment and control groups before the consulting program was implemented (in 2005 and 2006) and were about 50 percent higher 4 and 5 years after the program (in 2013 and 2014).

A caveat here is that when we compare number of full-time employees from the IMSS data to our follow-up survey data, the IMSS numbers are lower, suggesting that the enterprises in our sample did not register all their employees with IMSS (the follow-up survey suggests that our enter-

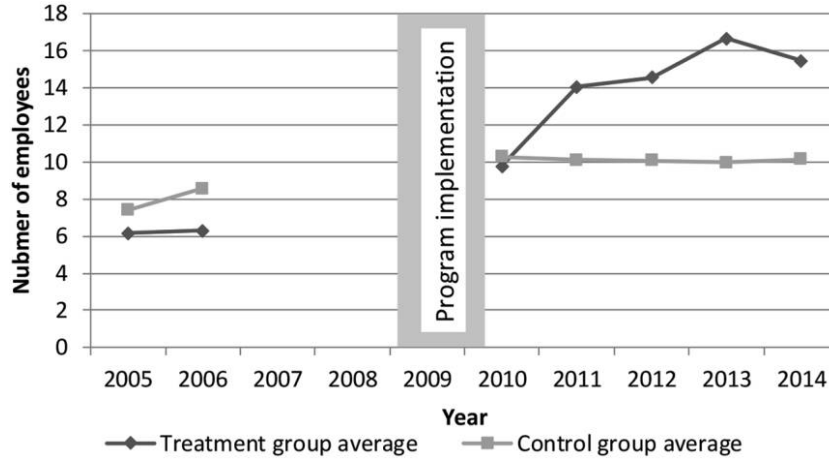

FIG. 2.—Average number of employees in the treatment and control groups over time (administrative data). Source: Administrative data from the Mexican Social Security Institute (IMSS). Includes only the 253 firms in our sample that were found in IMSS records (89 treatment group firms and 164 control group firms).

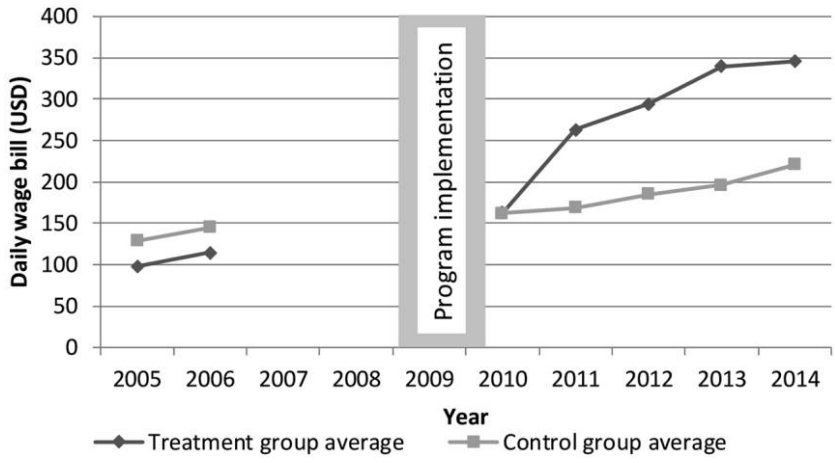

FIG. 3.—Average daily wage bill (US dollars) in the treatment and control groups over time (administrative data). Source: Administrative data from the Mexican Social Security Institute (IMSS). Includes only the 253 firms in our sample that were found in IMSS records (89 treatment group firms and 164 control group firms).

prises had about 15 full-time paid employees on average in 2009, and 2010 IMSS data show about 10 employees on average). The increase in number of employees in the IMSS data could thus reflect more employees being registered instead of more employees being hired (though this still is a desirable outcome from a societal perspective). However, we believe that had the impact on employees occurred merely through an increase in honest reporting, we would have seen the impact in the short run. Instead, we observe the effect in the longer run only, which we believe is an indication that the change is a by-product of firm efficiency, which takes time to develop, and is not merely a reporting bias.

Not all of the firms in our sample were found in IMSS records, so the results in table 4 are based on about 57 percent of our sample. To assess whether there are important compositional changes that influence our key results, appendix table A2 reports attrition analysis for IMSS data; we find neither differential attrition for treatment on average (cols. 1 and 2) nor compositional changes (col. 3; aggregate p-value of .122 for the F-test of joint significance, all interaction terms). In addition, in appendix table A13 we report the main specifications for impact as measured by the follow-up survey but restrict to the sample of firms for which we have IMSS data. The results are similar.

C. *Process Variables*

In order to investigate the channels that drive the observed treatment effects, we now study how the consulting program changed processes

within the enterprise. We measure these processes as follows: first, the surveys asked enterprise owners whether or not they implemented certain changes during the past year, such as developing new products, attracting new investors, and launching a new marketing campaign. Note that if treatment enterprises believed they should please the program by reporting process changes that did not actually occur, these estimates will be upwardly biased.

Table 5 displays the treatment effects on business process variables. We start with an all-encompassing standardized index, calculated as per Kling, Liebman, and Katz (2007), and are not able to reject the null hypothesis of no change (0.072 standard deviation, SE = 0.104). We find statistically significant improvements in only two processes: made a new marketing effort (13 percentage point increase, SE = 5.5 percentage points) and the percentage of enterprises that keep formal accounts (8 percentage point increase, SE = 3 percentage points; "formal" is defined as using either an accountant or a computerized system as opposed to keeping handwritten records or no notes at all). The finding that the program increased marketing efforts and the use of formal accounting practices is consistent with the case study evidence mentioned above, which suggests that many enterprises worked with their mentors on accounting and record keeping, as well as sales strategy and advertising.

Other processes examined, such as registering a patent, developing new products, or attracting new investors, do not change (or are imprecisely estimated; typical standard errors are small, however, about 2–6 percentage points). These could be more difficult to detect because they are more heterogeneous across enterprises or because they require a longer time to change than is observable in the treatment period. To measure human resource management practices, we create an index using principal component analysis (PCA) based on the six questions listed in online appendix 2.[20] We are not able to reject the null hypothesis of no effect on this index (−0.062, SE = 0.152). In summary, since the content of the consulting was tailored to each firm's needs, it is perhaps not surprising that we do not see, on average, improvements in either the collective index or most individual processes.

To confirm the existence of a pathway between consulting and performance, we regress business outcomes on the business process index, as well as each of the individual components of the index, and report the

[20] All PCA indices were created in Stata using the "pca" command. This command computes the leading eigenvectors from the eigen decomposition of the covariance matrix of the variables used to create the index. We choose the first eigenvector as our PCA index. In other words, the PCA index is a weighted linear combination of the underlying variables, where the weights are optimal in the sense that they give the index the largest possible variance.

results in the last three columns of table 5. As before, our different outcome variables are average sales, profits, and the productivity residual. Since some firms have missing data at baseline and follow-up, we use data from the follow-up survey only, that is, outcomes in levels, and use the 1 percent winsorized sample. We find a positive relationship between business outcomes and the business process index for all outcomes variables. The results are statistically significant at conventional levels only for the regressions with sales and productivity as the dependent variables. It is also reassuring that the two process variables in which we find a positive and statistically significant treatment effect in the ITT regressions (making marketing efforts and keeping financial accounts) also show a statistically significant and positive correlation with business performance. These results suggest a correlation between improved performance and some of the most relevant business processes. Of course, this analysis does not provide causal evidence, but it suggests a pathway by which consulting affects specific practices that may lead to improved firm outcomes.

D. Entrepreneurial Spirit

We construct two entrepreneurial spirit indices, developed in collaboration with IPPC. These indices are based on the answers to the eight questions listed in online appendix 2, which intend to capture entrepreneurial attitudes of the principal decision maker. One index is generated using PCA, and the other is a standardized index using the Kling et al. (2007) method. Thus the indices are a combined measure of answers to a set of questions on the enterprise owners' beliefs about their ability to control the success of their business (or whether they are merely subject to external forces outside of their control) and on the owners' drive for success.

Table 6 reports the results. We find a positive impact using the PCA method (0.237, SE = 0.140) and positive but not a statistically significant impact using the Kling et al. method (0.130 standard deviation increase, SE = 0.103). The increase in this index might reflect the fact that enterprise owners set new goals as part of the program and that consultants helped to provide motivation and strategy for how to achieve these goals. In addition, enterprise owners' increased confidence in their ability to control the success of their business could be driven by having better command of management tools such as marketing and bookkeeping.

We cannot distinguish whether the training had a direct effect on entrepreneurial spirit (e.g., enterprise owners set new goals as part of the program and consultants helped to provide motivation and strategies for how to achieve these goals) or whether the improvements in the business that led to higher productivity then improved the spirit of the entrepreneurs.

TABLE 5
ITT Treatment Effect Estimates, Business Processes: OLS

Outcome Variable	ITT Treatment Effect Estimates (1)	(2)	Observations (3)	Control Group Mean (SD) (4)	Correlations with Short-Run Business Outcomes: Average Sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s) (5)	Profits (Feb. 2009 Sales Minus Costs, US$1,000s) (6)	Productivity Residual (7)
Index of all process measures listed below	.072	.030	378	−.036	25.963***	6.268	.180**
	(.104)	(.098)		(.975)	(7.626)	(4.612)	(.070)
Index components:							
Developed new products during last year	−.048	−.046	378	.531	−10.604	−4.134	.099
	(.055)	(.053)		(.500)	(13.936)	(5.901)	(.145)
Attracted new clients during last year	−.020	−.033	376	.789	38.116***	12.687***	.502***
	(.046)	(.045)		(.409)	(10.357)	(4.267)	(.170)
Implemented new process during last year	−.062	−.070	378	.617	13.583	−.132	.142
	(.053)	(.052)		(.487)	(13.872)	(5.836)	(.148)
Attracted new investors during last year	.027	.024	378	.074	35.637	−4.747	.549**
	(.032)	(.031)		(.262)	(30.885)	(11.664)	(.244)
Began process to register a patent during last year	.045		376	.079	23.044	29.295*	−.006
	(.034)			(.270)	(33.608)	(17.155)	(.223)
Began certification process for an international standard (e.g., ISO)	−.024		378	.156	78.912***	15.033	−.165
	(.035)			(.364)	(24.591)	(15.080)	(.210)

Made new marketing effort during last year	.129**		378	.440	40.005***	15.026**	.250*
	(.055)			(.497)	(14.196)	(6.070)	(.143)
Expanded installations during last year	−.030		377	.240	46.718**	−.473	.423**
	(.045)			(.428)	(19.880)	(9.120)	(.176)
Remodeled installations during last year	.022		377	.459	29.010**	5.910	.189
	(.054)			(.499)	(13.859)	(5.935)	(.144)
Human resources management index	−.062	−.061	363	.022	1.873	1.610	−.049
	(.152)	(.146)		(1.450)	(4.980)	(2.143)	(.051)
Keeps formal accounts	.076**	.068**	378	.852	45.609***	5.459	.525**
	(.030)	(.029)		(.356)	(11.947)	(5.033)	(.245)
Controls for baseline value of outcome	No	Yes	. . .	. . .	. . .	. . .	. . .

NOTE.—The index follows the methodology in Kling et al. (2007) and is the normalized average of *z*-scores for all nonmissing process measures, using mean and standard deviation in the control group to calculate the *z*-scores. Each cell in cols. 1 and 2 contains the treatment effect point estimates and robust standard errors for separate OLS estimations. All regressions in cols. 1 and 2 include controls for strata dummies and re-randomization variables, as well as a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up. Some variables are not available at baseline, which is why the corresponding cells in col. 2 are empty. Column 4 contains means and standard deviations for the control group at follow-up. Each cell in cols. 5, 6, and 7 shows the coefficients and robust standard errors for a separate OLS regression of business outcomes on one process variable at the time, where all variables are from the 2009 follow-up survey. The productivity residual is the residual from regression of log February 2009 sales on log employees and log business assets. Business outcome variables are winsorized at the top and bottom 1 percent.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE 6
ITT Treatment Effect Estimates, Entrepreneurial Spirit: OLS

Outcome Variable	ITT Treatment Effect Estimates (1)	(2)	Observations (3)	Control Group Mean (SD) (4)
PCA entrepreneurial spirit index	.237*	.223	373	−.094
	(.140)	(.139)		(1.371)
PCA entrepreneurial spirit index without components *d* and *e*	.240*	.208	373	−.095
	(.140)	(.138)		(1.343)
KLK entrepreneurial spirit index	.130	.128	378	−.055
	(.103)	(.102)		(.964)
KLK entrepreneurial spirit index without components *d* and *e*	.153	.140	378	−.064
	(.107)	(.105)		(.961)
Index components:				
a. I have professional goals	.114*	.112*	378	4.531
	(.060)	(.060)		(.651)
b. I revise my goals periodically	.128	.115	378	4.029
	(.085)	(.082)		(.840)
c. If I don't reach a goal in the way I wanted to I try again	−.033	−.035	378	4.374
	(.077)	(.077)		(.683)
d. I can't motivate my business partners[a]	.064	.055	376	2.277
	(.121)	(.121)		(1.086)
e. Everything I need for success lies in myself	.074	.090	378	3.938
	(.112)	(.108)		(1.025)
f. I prefer to do routine tasks instead of doing something new in my work[a]	−.013	−.017	376	2.000
	(.104)	(.103)		(.964)
g. I think the government should give me opportunities[a]	−.061	−.075	377	3.545
	(.139)	(.132)		(1.215)
h. I have to reach some goals every day to feel satisfied	.126	.114	378	3.897
	(.109)	(.107)		(1.076)
Controls for baseline value of outcome	No	Yes		. . .

Note.—The PCA index is generated using principal components analysis. The KLK index follows the methodology in Kling et al. (2007) and is the normalized average of *z*-scores for all nonmissing process measures, using means and standard deviations in the control group to calculate the *z*-scores. Each row in cols. 1 and 2 contains the treatment effect point estimates and robust standard errors for separate OLS estimations. All regressions include controls for strata dummies and re-randomization variables, as well as a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up. Column 4 contains means and standard deviations for the control group at follow-up.

[a] Component is reverse-coded in the indices.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

We believe two of the questions used to construct the index are particularly subject to this second interpretation (questions d and e in online app. 2). As a robustness check, we construct the indices without these two questions, and the results do not change.

E. Response to Economic Shocks

The program could have also improved enterprise performance by helping enterprises to better cope with the 2008 economic crisis. In the follow-up survey, about 89 percent of enterprises—in both the treatment and control groups—reported that they had been affected by the crisis. We asked these enterprises what changes they made in response to the crisis. Table 7 reports the answers to these questions and examines whether the responses differ across the treatment and control groups. The results show that treatment enterprises are 8 percentage points (SE = 4 percent-

TABLE 7
ITT Treatment Effect Estimates, Changes in Response to Crisis: OLS

Outcome Variable	ITT Treatment Effect Estimates (1)	Observations (2)	Control Group Mean (SD) (3)
Laid off staff or cut down on hiring	.047	340	.257
	(.051)		(.438)
Lowered employee salaries	−.026	340	.092
	(.032)		(.289)
Cut production	−.080**	340	.206
	(.040)		(.406)
Diversified business activities	−.015	340	.431
	(.057)		(.496)
Sought government assistance	.056	340	.128
	(.044)		(.335)
None	−.006	340	.115
	(.037)		(.319)
Other	.043	340	.216
	(.050)		(.412)
Number of changes made	.025	340	1.330
	(.092)		(.810)

NOTE.—Column 1 contains the treatment effect point estimates and robust standard errors for separate OLS estimations. All outcome variables, except for number of changes made, are binary variables for the responses to the question, "Which changes has your firm made in response to the current economic situation?" (multiple answers were allowed). This question was asked at follow-up in reference to the recent economic crisis. Number of changes made is a count of the number of changes reported in response to the question above. These questions were not asked at baseline, which is why we do not control for the baseline outcome variable in this table. All regressions include controls for strata dummies and re-randomization variables, as well as a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up. Column 3 contains means and standard deviations for the control group at follow-up.

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

age points) less likely than control enterprises to report that they had to cut production in response to the crisis. The ability to weather shocks more effectively could be a result of being able to more proactively engage in marketing activities and better control finances, as shown in the previous section. Enterprises that are less well trained in these skills might experience economic shocks more passively and do not have tools to counteract a shortfall in demand.

Other changes in response to the crisis are not statistically significant across the treatment and control groups, but one of magnitude (but not statistical significance) to note is a positive impact on seeking government assistance (a 5.6 percentage point increase, SE = 4.4 percentage points, relative to an average of 12.8 percent in the control group). For enterprises that reported seeking government assistance, we asked which program or agency they contacted. Most answers indicated state or federal programs that provide funding or subsidies to micro, small, and medium-sized enterprises.

V. Cost-Effectiveness: Why Don't More Enterprises Use Consulting Services?

Given the large increases in productivity, and eventual growth in employees, we ask why more firms do not use consulting services. In particular, a cost-effectiveness calculation suggests that the returns to hiring a consultant may be well worth the cost. The measured effect of the program on the daily wage bill of U$125 implies an increase in the annual wage bill of US$125 × 365 = US$45,625.[21] The annual cost of the consulting services was US$11,856. Since the program was highly subsidized, participating enterprises had to pay only between 10 percent and 30 percent of this cost (depending on firm size). Among the enterprises in the treatment group, only 53 percent chose to participate in the subsidized consulting program once offered a spot. Although we do not attempt to translate the job growth to firm profits, given the relative magnitude, we note that the annual return on labor to the firm need not be very high in order to justify the one-time consulting expenditure.

Several issues may hinder the market for consulting services. First, there may be no failure at all: those who opt in may be the ones who can benefit, and those who do not opt in would not benefit. Naturally, we do not observe what the impact would have been on those who did not opt in, but given the large increase in productivity and long-term employment on the intent to treat, there seems to remain a failure for those

[21] A potential caveat with this calculation is that the measured increase in the wage bill may be influenced by outliers. Since we do not have firm-level data on the wage bill and our estimates are based on the simple average in the treatment and control groups, we cannot examine how the effect size would vary when winsorizing or trimming outliers.

who did opt in, in that they had not taken up the services before, even at the unsubsidized rate. It is important to emphasize that all enterprises in our study had initially expressed interest in the subsidized consulting program, and thus their views are not representative of enterprises that do not have a preexisting interest in consulting services. It could be that firms expressed an interest, learned more about the service, and then decided that this was unlikely to yield profitable results for them, and thus failure to take up remains a rational and correct decision.

Second, there may be a credit market failure. In fact, most of the enterprises in the treatment group that declined participation in the program once offered a spot gave liquidity constraints as the reason. However, this does not fully satisfy the question, Why do we not observe consulting firms accepting delayed payment or working with financial services firms to provide credit to cover their services? Either way, it suggests that a credit market failure is the source of the problem for some enterprises. This may be particularly relevant given the timing of the impacts; that is, in the short run we do not observe higher profits but rather increased productivity. It is not until the long run that we see evidence of likely increased firm size that could be useful for generating liquidity to pay for consulting services.

Third, entrepreneurs may be risk or ambiguity averse with respect to the potential returns from hiring a consultant. This aversion could be perpetuated by lack of information in the market on the returns to consulting advice (of which consulting firms have difficulty credibly signaling).

To examine this issue, in the follow-up survey we included some qualitative questions for the control group on whether they were using any consulting or mentoring services, and if not, why not. About 21 percent of control group enterprises said that they were indeed using some services and provided the name of the consulting firm they were using. Examining these names reveals that only about half of these firms offer management consulting services similar to the consulting firms that worked with the treatment group enterprises. The other firms mentioned by the control group provide specialized services, such as accounting or technical assistance. Overall, the incidence of using management consulting services in the control group appears to be around 10 percent. Table 8 lists the self-reported reasons why control group enterprises do not use consulting services. By far, the most frequently mentioned reason is lack of funds (46.3 percent of enterprises mention this reason), followed by uncertainty about the benefits of consulting services (22.2 percent) and simply not having considered hiring a consultant (18.5 percent). The response could be genuine disinterest in consulting services or ambiguity about a service whose quality is not assured.

Our findings indicate that management consulting services can have high returns for micro, small, and medium-sized enterprises, and we

TABLE 8
SELF-REPORTED REASONS FOR NOT USING CONSULTING SERVICES IN CONTROL GROUP FIRMS

Reasons for Not Using Consulting Services	Percent of Enterprises Mentioning This Reason (Multiple Mention)
Would be a good investment, but don't have funds	46.3
Don't know what the benefits would be	22.2
Simply hadn't considered it	18.5
Didn't need the services	13.9
Other	11.1
Didn't know these services existed	7.4
Not worth the cost	5.6
Observations	108

NOTE.—This table includes all control group firms that, at the time of the follow-up survey, reported never having used consulting services.

consider funding constraints and uncertainty about the benefits to be the most likely explanations for the lack of market transactions in consulting services.

VI. Discussion and Conclusion

Our results suggest that lack of managerial skills constitutes an important constraint to firm growth and the ability to withstand economic shocks. The documented effects on productivity and return on assets in the short run, and employment in the long run, are large. However, the short-run impact on productivity and return on assets, albeit statistically significant only at 10 percent, is similar to smaller point estimates from other studies. Thus, while we believe that the magnitude of the impact is not unreasonable given that many enterprises in the sample had not received any formal management training prior to our intervention, we note that the confidence intervals exclude zero but do include fairly small but positive treatment effects.

Comparing our results to those of Bloom et al. (2013) provides some potentially useful insights. While our point estimates are considerably larger, the confidence intervals encompass each other's results. However, there is good reason to believe that there may be decreasing returns to consulting with firm size between the small and medium to large-size firms across those two studies. First, the management advice delivered here is much simpler and thus easier to transmit and implement, as compared to that of Bloom et al. Second, the firms are smaller, with fewer levels of administration, and thus can more easily implement organizational changes. On the other end of the spectrum for firm size, a consulting intervention for micro entrepreneurs in Ghana (tailors) actually led to negative treatment effects, as tailors shifted toward advice given that did not yield higher profits and then switched back to earlier practices

(Karlan et al. 2015). A possible explanation is that these micro entrepreneurs had aspirations to grow (and false optimism) but did not have the capacity to adopt the more advanced business practices. Analysis across our larger and smaller firms is not precisely estimated enough to draw robust conclusions, but we note that the point estimates are inconsistent: smaller firms have larger impacts on business assets and return on assets but smaller impacts on the productivity residual outcome.

The organizational changes implemented as a result of the consulting services seem to be most focused around improvements in marketing and financial controls and more efficient use of inputs in the short run. Consultants also appear to have helped enterprises set clear goals and define a strategy for how to achieve these goals. We see that the overall "entrepreneurial spirit" or confidence in owners increases as a result of the intervention. However, the evidence on any one specific mechanism is weak, with only two out of 11 individual management practices showing an increase. Although desirable to identify specific mechanisms, we conjecture that such a one-size-fits-all solution is not realistic. The large impact on employment in the long run is not observed in the short run, indicating that growth occurred through a combination of mechanisms such as improved marketing and goal setting as well as efficient use of existing capital. These short-run changes did then lead to a more efficient and scalable enterprise, as evidenced by the long-term growth in number of employees and wages paid.[22]

Naturally, if consulting generates high returns, an obvious question to pose is whether the consulting arrangements persisted after the subsidy ended. In 2014, we completed interviews with two of the consulting firms (either the other firms did not respond or the key parties were no longer at the firm). The interviews focused on understanding whether relationships with any of the participating enterprises continued after the subsidy period and furthermore to learn how these consulting firms typically market their services. For one of the consulting firms, 13 out of 19 of the participating enterprises continued the consulting after the subsidy ended, whereas for the other consulting firm, zero enterprises continued the consulting (the second firm has shifted away from consulting and now focuses on government contracts for evaluation and planning). This could be due to differential quality of the consulting but also could be due to enterprise size, as the first consulting firm's participating clients were considerably larger than the second's, and smaller firms were reportedly less likely to act strategically in planning for the future and expanding. In discussing

[22] A study that separately taught or tackled one type of problem at a time is likely an unrealistic method for conducting randomized trials, as it would require massive sample sizes to tease out each mechanism separately. Even then, the external validity of any one discovered magic mechanism would be at risk of being context specific (e.g., to that particular regulatory, industry, macroeconomic, political, or natural resource environment).

marketing of the consulting services, both firms reported that mass marketing would likely lead to a poor selection of clients, clients who are not fully committed to engaging with the consultants and adopting recommended changes, and they thus rely instead on word of mouth to generate new business.

Overall, our results confirm that managerial inputs have a large and important impact on firm performance and even hiring decisions in the intermediate run. However, there is still much to learn about the way this information affects firm performance as a whole and, more specifically, how it interacts with the marginal productivity of inputs such as labor and capital. In addition, while there may be a lot of heterogeneity in effects, our sample is not large enough to allow us to look at all the firm-level interactions that might be of interest, such as competitive nature of the industry, age and gender of the owner, owner's ambition level, risk-taking ability, or general skill levels. We believe that this is a critical area for further research.

Appendix

TABLE A1
ANALYSIS OF ATTRITION IN FOLLOW-UP SURVEY: OLS

	DEPENDENT VARIABLE: BINARY = 1 IF ENTERPRISE WAS NOT INTERVIEWED OR NOT CONFIRMED CLOSED AT FOLLOW-UP		
	(1)	(2)	(3)
Treatment	−.020	−.017	.141
	(.029)	(.030)	(.157)
Commerce sector dummy		.021	.047
		(.039)	(.049)
Services sector dummy		.041	.072*
		(.033)	(.042)
Full-time paid employees		.001	.001
		(.001)	(.001)
Age of principal decision maker (years)		.001	.001
		(.001)	(.002)
Male principal decision maker dummy		−.043	−.020
		(.036)	(.045)
Business age (years)		.000	.000
		(.000)	(.000)
Log(average sales July, Aug., and Sept. 2007 in US$1,000s)		.002	.000
		(.008)	(.011)
Profits (Sept. 2007 sales minus costs, US$1,000s)		−.000	−.000
		(.000)	(.000)
ROA		.002	−.021
		(.025)	(.038)

TABLE A1 (*Continued*)

	Dependent Variable: Binary = 1 if Enterprise Was Not Interviewed or Not Confirmed Closed at Follow-Up		
	(1)	(2)	(3)
Commerce sector dummy × treatment			−.071
			(.080)
Services sector dummy × treatment			−.094
			(.073)
Full-time paid employees × treatment			.000
			(.002)
Age of principal decision maker (years) × treatment			−.001
			(.003)
Male principal decision maker dummy × treatment			−.078
			(.074)
Business age (years) × treatment			−.000
			(.003)
Log(average sales) × treatment			.006
			(.017)
Profits × treatment			−.000
			(.000)
ROA × treatment			.057
			(.045)
Constant	.106***	.056	.003
	(.018)	(.076)	(.102)
R^2	.001	.039	.053
Observations	432	432	432
F-test *p*-value: joint significance of interaction terms			.850
Mean of dependent variable	.100	.100	.100

Note.—All explanatory variables are measured at baseline. Binary control variables are included for when covariate is missing, and then the missing covariate is coded as zero. Variables with × treatment are interacted with a treatment group dummy. Robust standard errors are in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE A2
ANALYSIS OF MATCHING WITH IMSS DATA: OLS

	DEPENDENT VARIABLE: BINARY = 1 IF ENTERPRISE WAS MATCHED WITH IMSS DATA		
	(1)	(2)	(3)
Treatment	.019	.014	−.011
	(.050)	(.049)	(.257)
Commerce sector dummy		−.017	.016
		(.061)	(.075)
Services sector dummy		.042	.088
		(.054)	(.069)
Full-time paid employees		.000	.002***
		(.001)	(.001)
Age of principal decision maker (years)		−.001	−.002
		(.002)	(.003)
Male principal decision maker dummy		.102*	.036
		(.053)	(.064)
Business age (years)		−.000	−.001
		(.001)	(.001)
Log(average sales July, Aug., and Sept. 2007 in US$1,000s)		.082***	.084***
		(.014)	(.017)
Profits (Sept. 2007 sales minus costs, US$1,000s)		.000	.000*
		(.000)	(.000)
ROA		−.004	−.020
		(.030)	(.043)
Commerce sector dummy × treatment			−.072
			(.132)
Services sector dummy × treatment			−.077
			(.120)
Full-time paid employees × treatment			−.006**
			(.002)
Age of principal decision maker (years) × treatment			.001
			(.005)
Male principal decision maker dummy × treatment			.146
			(.110)
Business age (years) × treatment			.005
			(.004)
Log(average sales) × treatment			.008
			(.035)
Profits × treatment			−.000
			(.000)
ROA × treatment			.010
			(.057)
Constant	.567***	.327***	.331**
	(.030)	(.120)	(.147)
R^2	.000	.121	.152
Observations	432	432	432

TABLE A2 (*Continued*)

	Dependent Variable: Binary = 1 if Enterprise Was Matched with IMSS Data		
	(1)	(2)	(3)
F-test *p*-value: joint significance of interaction terms			.124
Mean of dependent variable	.574	.574	.574

Note.—All explanatory variables are measured at baseline. Binary control variables are included for when covariate is missing, and then the missing covariate is coded as zero. Variables with × treatment are interacted with a treatment group dummy. Robust standard errors are in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE A3
Number of Enterprises Surveyed Each Month: Follow-Up Survey

Survey Month	Number of Treatments	Percent of Treatments	Number of Controls	Percent of Controls
March 2009	95	70.37	152	62.55
April 2009	29	21.48	68	27.98
May 2009	10	7.41	20	8.23
June 2009	1	.74	3	1.23

TABLE A4
ITT Treatment Effect Estimates, Short-Run Business Outcomes, Restricted Sample: OLS

Outcome Variable	ITT Treatment Effect Estimates (1)	(2)	Control Group Mean (SD) (3)
Full-time paid employees	−1.771	−1.341	13.182
	(1.389)	(1.103)	(19.925)
	221	221	143
Log(total employees)	−.158	−.069	2.352
	(.124)	(.092)	(1.064)
	221	221	143
Average sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s)	−14.720	−11.426	69.450
	(14.976)	(12.218)	(181.105)
	221	221	143
Log(average sales Dec. 2008, Jan. and Feb. 2009 in US$1,000s)	−.016	−.019	2.520
	(.211)	(.176)	(1.985)
	221	221	143
Feb. 2009 costs (US$1,000s)	−12.367	−13.203	46.598
	(11.412)	(10.956)	(111.674)
	221	221	143
Profits (Feb. 2009 sales minus costs, US$1,000s)	3.793	3.788	13.161
	(6.628)	(6.332)	(102.459)
	221	221	143
Log(business assets)	−.078	−.118	4.432
	(.192)	(.169)	(1.738)
	221	221	143
Productivity residual from regression of log Feb. 2009 sales on log employees and log business assets	.261*	.249*	−.073
	(.158)	(.146)	(1.302)
	221	221	143
ROA Feb. 2009 sales minus costs divided by assets	.118*	.112	.018
	(.065)	(.068)	(.487)
	221	221	143
Controls for baseline value of outcome	No	Yes	. . .

Note.—This table includes only enterprises that report all outcome variables. Each cell in cols. 1 and 2 contains the treatment effect point estimate, robust standard error, and number of observations for a separate OLS estimation. For the regressions that control for the outcome variable measured at baseline (col. 2), when the baseline outcome variable is missing, the missing value is filled in with zero and a dummy variable indicating that the baseline observation is missing is added to the model. All regressions include controls for strata dummies and re-randomization variables, as well as a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up. Column 3 contains means and standard deviations for the control group at follow-up.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE A5
ITT Treatment Effect Estimates, Short-Run Business Outcomes, Difference-in-Difference: OLS

Outcome Variable	ITT Treatment Effect Estimates (1)	1% Winsorized (2)	1% Trimmed (3)	Control Group Mean (SD) (4)
Full-time paid employees	.578	−.447	−1.301	12.428
	(2.351)	(2.004)	(1.307)	(22.281)
	810	810	792	243
Log(total employees)	−.046	−.046	−.039	2.319
	(.111)	(.111)	(.107)	(1.106)
	805	805	787	241
Average sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s)	−14.464	−8.096	−2.791	63.384
	(23.358)	(16.725)	(11.838)	(163.643)
	675	675	659	200
Log(average sales Dec. 2008, Jan. and Feb. 2009 in US$1,000s)	.017	.025	.062	2.391
	(.237)	(.236)	(.238)	(2.023)
	675	675	659	200
Feb. 2009 costs (US$1,000s)	27.333	1.697	−3.942	43.157
	(25.419)	(12.144)	(8.900)	(113.758)
	681	681	665	204
Profits (Feb. 2009 sales minus costs, US$1,000s)	−15.357	8.052	10.149	11.460
	(20.485)	(9.859)	(7.911)	(97.044)
	602	602	588	176
Log(business assets)	−.098	−.104	−.207	4.307
	(.227)	(.224)	(.220)	(1.699)
	627	627	611	203
Productivity residual from regression of log Feb. 2009 sales on log employees and log business assets	.306	.302	.251	−.095
	(.217)	(.212)	(.190)	(1.272)
	515	515	503	158
ROA Feb. 2009 sales minus costs divided by assets	.272**	.160*	.062	.012
	(.133)	(.094)	(.065)	(.471)
	488	488	476	154

Note.—Each cell in cols. 1, 2, and 3 contains the treatment effect point estimate, robust standard error, and number of observations for a separate OLS difference-in-difference estimation. Each regression uses the full sample of enterprises at baseline and follow-up and includes a dummy for being in the treatment group, a dummy for the follow-up period, an interaction term between the treatment and follow-up dummies, as well as controls for strata dummies, re-randomization variables, and a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up. The point estimates displayed in cols. 1, 2, and 3 are coefficients on the interaction term between treatment and follow-up. In col. 2, outcome variables are winsorized at the top and bottom 1 percent. In col. 3, outcome variables are trimmed at the top and bottom 1 percent. Column 4 contains nonwinsorized, untrimmed means and standard deviations for the control group at follow-up.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE A6
FOLLOW-UP SUMMARY STATISTICS: SHORT-RUN BUSINESS OUTCOMES: MEANS AND STANDARD DEVIATIONS

	Treatment (1)	Control (2)	(1)−(2) Difference (*p*-Value) (3)	Treatment and Took Up (4)	(4)−(2) Difference (*p*-Value) (5)
Average sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s)	53.889	63.384	−9.495	61.075	−2.309
	(160.545)	(163.643)	(.624)	(103.216)	(.897)
Average sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s), 1% winsorized	48.298	57.721	−9.422	61.075	3.354
	(114.908)	(124.174)	(.506)	(103.216)	(.835)
Feb. 2009 costs (US$1,000s)	42.353	43.157	−.804	42.882	−.275
	(167.711)	(113.758)	(.965)	(85.825)	(.985)
Feb. 2009 costs (US$1,000s), 1% winsorized	33.788	39.310	−5.522	43.519	4.210
	(92.327)	(83.425)	(.613)	(85.991)	(.752)
Profits (Feb. 2009 sales minus costs, US$1,000s)	10.964	11.460	−.496	15.804	4.344
	(45.858)	(97.044)	(.955)	(57.117)	(.693)
Profits (Feb. 2009 sales minus costs, US$1,000s), 1% winsorized	10.964	6.758	4.206	15.804	9.046
	(45.858)	(48.976)	(.491)	(57.117)	(.315)
Business assets (US$1,000s)	258.923	331.416	−72.493	313.187	−18.229
	(508.865)	(1236.195)	(.464)	(560.688)	(.872)
Business assets (US$1,000s), 1% winsorized	259.310	267.828	−8.519	314.068	46.239
	(508.444)	(607.125)	(.893)	(560.489)	(.581)
Productivity residual from regression of log Feb. 2009 sales on log employees and log assets	.163	−.095	.257*	.429	.524***
	(.967)	(1.272)	(.073)	(.854)	(.001)
Productivity residual, 1% winsorized	.157	−.089	.247*	.419	.508***
	(.951)	(1.229)	(.078)	(.820)	(.001)
ROA: Feb. 2009 sales minus costs divided by assets	.091	.012	.080	.174	.162**
	(.402)	(.471)	(.174)	(.429)	(.032)
ROA, 1% winsorized	.089	.031	.058	.174	.143**
	(.393)	(.290)	(.236)	(.429)	(.039)

NOTE.—Columns 1, 2, and 4 present means and standard deviations (in parentheses). Column 3 shows the difference in means across the treatment and control groups with the corresponding *p*-value in parentheses. Column 5 shows the nonexperimental difference between those who took up in treatment minus those in control and the corresponding *p*-value in parentheses. The 1 percent winsorized variables are winsorized at the top and bottom 1 percent.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE A7
ROBUSTNESS CHECKS FOR TFP MEASURE: OLS

OUTCOME VARIABLE	ITT TREATMENT EFFECT ESTIMATES (1)	(2)	CONTROL GROUP MEAN (SD) (3)
Productivity residual from regression of log Feb. 2009 sales on log employees and log business assets	.270*	.250*	−.095
	(.141)	(.130)	(1.272)
	250	250	158
Log(Feb. 2009 sales)	.097	.135	11.720
	(.183)	(.154)	(2.003)
	285	285	184
Log(total employees)	−.117	−.074	2.319
	(.092)	(.071)	(1.106)
	375	375	241
Log(business assets)	−.055	−.106	4.307
	(.176)	(.157)	(1.699)
	319	319	203
Log(materials costs)	−.122	−.054	1.486
	(.218)	(.190)	(2.103)
	299	299	198
Productivity residual including materials costs from regression of log Feb. 2009 sales on log employees, log business assets, and log materials costs	.212*	.197*	−.083
	(.120)	(.111)	(.879)
	220	220	140
Solow residual: log(Feb, 2009 sales) − .24 × log employees − .16 × log assets − .6 × log materials costs	.197	.194*	9.462
	(.125)	(.116)	(.907)
	220	220	140
Controls for baseline value of outcome	No	Yes	. . .

NOTE.—Each row in cols. 1 and 2 contains the treatment effect point estimate, robust standard error, and number of observations for a separate OLS estimation. For the regressions that control for the outcome variable measured at baseline (col. 2), when the baseline outcome variable is missing, the missing value is filled in with zero and a dummy variable indicating that the baseline observation is missing is added to the model. All regressions include controls for strata dummies and re-randomization variables, as well as a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up. Column 3 contains means and standard deviations for the control group at follow-up.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE A8
BASELINE SUMMARY STATISTICS FOR MATCHED SAMPLE:
MEANS AND STANDARD DEVIATIONS

	Took Up Treatment (1)	Matched Control (2)	Orthogonality Verification (1) − (2) Difference (*p*-Value) (3)
	A. Stratification Variables		
Manufacturing sector dummy	.351	.377	−.026
	(.480)	(.488)	(.740)
Commerce sector dummy	.234	.221	.013
	(.426)	(.417)	(.849)
Services sector dummy	.416	.403	.013
	(.496)	(.494)	(.871)
Full-time paid employees	18.623	15.675	2.948
	(36.772)	(27.900)	(.576)
	B. Re-randomization Variables		
Principal decision maker's age (years)	42.195	41.701	.494
	(10.584)	(10.395)	(.771)
Male principal decision maker dummy	.792	.831	−.039
	(.408)	(.377)	(.539)
Principal decision maker's years of schooling	16.060	16.744	−.683
	(4.540)	(4.655)	(.358)
Business age (years)	12.779	11.649	1.130
	(11.668)	(12.552)	(.564)
Observations	77	77	154
	C. Other Variables: Business Outcomes		
Average sales July, Aug., and Sept. 2007 (US$1,000s)	106.420	73.254	33.166
	(357.791)	(166.201)	(.501)
Average sales July, Aug., and Sept. 2007 (US$1,000s), 1% winsorized	85.079	69.063	16.016
	(212.235)	(137.431)	(.612)
Sept. 2007 costs (US$1,000s)	49.518	68.860	−19.341
	(131.473)	(223.200)	(.540)
Sept. 2007 costs (US$1,000s), 1% winsorized	49.343	52.008	−2.665
	(126.101)	(101.520)	(.892)
Profits (Sept. 2007 sales minus costs, US$1,000s)	16.514	2.916	13.598
	(58.843)	(215.671)	(.636)
Profits (Sept. 2007 sales minus costs, US$1,000s), 1% winsorized	16.514	21.598	−5.084
	(58.843)	(81.898)	(.695)
Business assets (US$1,000s)	326.745	282.504	44.242
	(779.147)	(359.395)	(.695)

TABLE A8 (*Continued*)

	Took Up Treatment (1)	Matched Control (2)	Orthogonality Verification (1) − (2) Difference (*p*-Value) (3)
Business assets (US$1,000s), 1% winsorized	309.084	283.555	25.528
	(662.846)	(358.922)	(.797)
Productivity residual (from regression of log Sept. 2007 sales on log employees and log assets)	.516	.455	.061
	(1.339)	(1.269)	(.817)
Productivity residual, 1% winsorized	.501	.455	.046
	(1.307)	(1.269)	(.859)
ROA: Sept. 2007 sales minus costs divided by assets	.170	.268	−.098
	(.368)	(1.088)	(.559)
ROA: 1% winsorized	.170	.204	−.034
	(.368)	(.811)	(.791)
Observations	77	77	154

NOTE.—Results may change depending on the version of Stata used. To replicate the values in this table, use Stata 13. Columns 1 and 2 present means and standard deviations (in parentheses). Column 3 shows the difference in means across the treatment enterprises that took up the program and the matched control group, with the corresponding *p*-value in parentheses. The 1 percent winsorized variables are winsorized at the top and bottom 1 percent.

TABLE A9
Treatment Effect Estimates for Matched Sample, Short-Run Business Outcomes: OLS

Outcome Variable	Treatment Effect Estimates (1)	(2)	1% Winsorized (3)	(4)	1% Trimmed (5)	(6)	Matched Control Group Mean (SD) (7)
Full-time paid employees	−.322	−.741	.119	−.233	−2.291	−.009	16.382
	(5.253)	(1.599)	(4.633)	(1.707)	(2.996)	(1.456)	(32.887)
	137	137	137	137	132	132	68
Log(total employees)	−.175	−.025	−.163	−.013	−.078	.035	2.495
	(.182)	(.102)	(.180)	(.097)	(.173)	(.088)	(1.089)
	135	135	135	135	131	131	67
Average sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s)	−13.789	−14.691	−13.789	−14.691	−13.789	−14.691	68.982
	(19.707)	(15.611)	(19.707)	(15.611)	(19.707)	(15.611)	(108.124)
	116	116	116	116	116	116	61
Log(average sales Dec. 2008, Jan. and Feb. 2009 in US$1,000s)	.120	.182	.120	.182	.120	.182	2.870
	(.329)	(.223)	(.329)	(.223)	(.329)	(.223)	(1.969)
	116	116	116	116	116	116	61
Feb. 2009 costs (US$1,000s)	−6.911	−4.337	−6.745	−4.169	−16.466	−14.288	48.635
	(16.070)	(15.193)	(16.096)	(15.194)	(13.398)	(13.024)	(81.714)
	109	109	109	109	106	106	59

Profits (Feb. 2009 sales minus costs, US$1,000s)	8.521	8.711	8.521	8.711	7.740	8.016	8.078
	(10.740)	(10.860)	(10.740)	(10.860)	(10.813)	(10.993)	(55.538)
	101	101	101	101	100	100	54
Log(business assets)	−.151	−.025	−.147	−.021	−.227	−.055	4.674
	(.295)	(.238)	(.295)	(.238)	(.296)	(.243)	(1.572)
	119	119	119	119	116	116	61
Productivity residual from regression of log Feb. 2009 sales on log employees and log business assets	.572**	.445**	.558**	.432**	.403**	.300*	−.066
	(.241)	(.201)	(.237)	(.198)	(.198)	(.165)	(1.391)
	98	98	98	98	94	94	52
ROA: Feb. 2009 sales minus costs divided by assets	.080	.084	.093	.096	.113	.119	.087
	(.095)	(.092)	(.086)	(.083)	(.077)	(.075)	(.446)
	91	91	91	91	89	89	49
Controls for baseline value of outcome	No	Yes	No	Yes	No	Yes	. . .

NOTE.—Results may change depending on the version of Stata used. To replicate the values in this table, use Stata 13. Each row in cols. 1 and 2 contains the treatment effect point estimate, robust standard error, and number of observations for a separate OLS estimation. For the regressions that control for the outcome variable measured at baseline (cols. 2, 4, and 6), when the baseline outcome variable is missing, the missing value is filled in with zero and a dummy variable indicating that the baseline observation is missing is added to the model. All regressions include a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up. In cols. 3 and 4, outcome variables are winsorized at the top and bottom 1 percent (in the full, nonmatched sample). In cols. 5 and 6, outcome variables are trimmed at the top and bottom 1 percent (in the full, nonmatched sample). Column 7 contains nonwinsorized, untrimmed means and standard deviations for the matched control group at follow-up.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE A10
HETEROGENEOUS TREATMENT EFFECT ESTIMATES, SHORT-RUN BUSINESS OUTCOMES, BY ENTERPRISE SIZE: OLS

Outcome Variable	Coefficient on Treatment Dummy (1)	Coefficient on Treatment Interacted with Small or Medium Enterprise Dummy (2)	*F*-Test *p*-Value: Sum of Coefficients in Cols. 1 + 2 (3)
Full-time paid employees	−.000	1.918	.641
	(.739)	(4.127)	
	378	378	
Log(total employees)	−.066	−.029	.427
	(.086)	(.148)	
	375	375	
Average sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s)	−6.058	−21.293	.410
	(9.575)	(35.412)	
	307	307	
Log(average sales Dec. 2008, Jan. and Feb. 2009 in US$1,000s)	.021	.104	.583
	(.184)	(.302)	
	307	307	
Feb. 2009 costs (US$1,000s)	−6.285	47.314	.428
	(7.169)	(51.588)	
	304	304	
Profits (Feb. 2009 sales minus costs, US$1,000s)	5.781	−1.743	.835
	(5.740)	(21.492)	
	265	265	
Log(business assets)	−.179	.298	.730
	(.172)	(.378)	
	319	319	
Productivity residual: from regression of log Feb. 2009 sales on log employees and log business assets	.337**	−.321	.939
	(.158)	(.268)	
	250	250	
ROA: Feb. 2009 sales minus costs divided by assets	.059	.154	.171
	(.069)	(.172)	
	236	236	

NOTE.—Each row contains the treatment effect point estimates, robust standard errors, and number of observations for a separate OLS estimation. All regressions control for the outcome variable measured at baseline; when the baseline outcome variable is missing, the missing value is filled in with zero and a dummy variable indicating that the baseline observation is missing is added to the model. All regressions also include controls for strata dummies and re-randomization variables, as well as a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE A11
HETEROGENEOUS TREATMENT EFFECT ESTIMATES, SHORT-RUN BUSINESS OUTCOMES, BY SECTOR: OLS

Outcome Variable	Coefficient on Treatment Dummy (1)	Coefficient on Treatment Interacted with Manufacturing Sector Dummy (2)	*F*-Test *p*-Value: Sum of Coefficients in Cols. 1 + 2 (3)	Coefficient on Treatment Interacted with Commerce Sector Dummy (4)	*F*-Test *p*-Value: Sum of Coefficients in Cols. 1 + 4 (5)
Full-time paid employees	1.841	−1.403	.837	−3.473	.504
	(1.379)	(2.199)		(2.727)	
	378	378		378	
Log(total employees)	−.113	.085	.842	.049	.615
	(.105)	(.182)		(.164)	
	375	375		375	
Average sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s)	5.114	−34.754	.156	−22.597	.537
	(11.072)	(23.254)		(28.990)	
	307	307		307	
Log(average sales Dec. 2008, Jan. and Feb. 2009 in US$1,000s)	−.053	.047	.984	.344	.342
	(.213)	(.366)		(.380)	
	307	307		307	
Feb. 2009 costs (US$1,000s)	32.367	−26.989	.793	−68.615**	.089
	(23.793)	(29.645)		(29.346)	
	304	304		304	
Profits (Feb. 2009 sales minus costs, US$1,000s)	.216	−7.842	.582	26.730*	.007
	(8.512)	(17.342)		(13.905)	
	265	265		265	

TABLE A11 (*Continued*)

Outcome Variable	Coefficient on Treatment Dummy (1)	Coefficient on Treatment Interacted with Manufacturing Sector Dummy (2)	*F*-Test *p*-Value: Sum of Coefficients in Cols. 1 + 2 (3)	Coefficient on Treatment Interacted with Commerce Sector Dummy (4)	*F*-Test *p*-Value: Sum of Coefficients in Cols. 1 + 4 (5)
Log(business assets)	−.237	.412	.482	−.011	.461
	(.245)	(.348)		(.419)	
	319	319		319	
Productivity residual: from regression of log Feb. 2009 sales on log employees and log business assets	.309*	−.151	.552	−.034	.283
	(.187)	(.336)		(.316)	
	250	250		250	
ROA: Feb. 2009 sales minus costs divided by assets	.037	−.058	.807	.287	.047
	(.085)	(.124)		(.180)	
	236	236		236	

NOTE.—Each row contains the treatment effect point estimates, robust standard errors, and number of observations for a separate OLS estimation. All regressions control for the outcome variable measured at baseline; when the baseline outcome variable is missing, the missing value is filled in with zero and a dummy variable indicating that the baseline observation is missing is added to the model. All regressions also include controls for strata dummies and re-randomization variables, as well as a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE A12
NONRESPONSE ON FOLLOW-UP SURVEY

	Treatment (1)	Control (2)	(1) − (2) Difference (*p*-Value) (3)
% that did not provide alternative contact person	16.296	18.519	−2.222 (.584)
% that did not report Dec. 2008, Jan. and Feb. 2009 sales	20.741	17.695	3.045 (.477)
Observations	135	243	378

NOTE.—Columns 1 and 2 show the percentage of enterprises in the treatment and control groups, respectively, that did not provide an alternative contact person or sales on the follow-up survey, conditional on participating in the survey. Column 3 shows the difference in percentages between the treatment and control groups with the corresponding *p*-value in parentheses.

TABLE A13
ITT TREATMENT EFFECT ESTIMATES, SHORT-RUN BUSINESS OUTCOMES, IMSS SAMPLE: OLS

	ITT Treatment Effect Estimates		Control Group Mean (SD)
Outcome Variable	(1)	(2)	(3)
Full-time paid employees	.764	.380	15.932
	(1.841)	(1.405)	(25.703)
	229	229	148
Log(total employees)	−.161	−.157*	2.588
	(.111)	(.084)	(1.084)
	229	229	148
Average sales Dec. 2008, Jan. and Feb. 2009 (US$1,000s)	−17.701	−16.381	77.325
	(23.164)	(14.531)	(161.353)
	187	187	125
Log(average sales Dec. 2008, Jan. and Feb. 2009 in US$1,000s)	−.182	−.104	3.045
	(.204)	(.168)	(1.713)
	187	187	125
Feb. 2009 costs (US$1,000s)	−1.529	−3.804	63.156
	(23.042)	(19.729)	(141.134)
	176	176	120
Profits (Feb. 2009 sales minus costs, US$1,000s)	5.566	5.904	2.321
	(8.197)	(8.102)	(43.806)
	160	160	109
Log(business assets)	−.012	−.008	4.655
	(.208)	(.202)	(1.624)
	187	187	121
Productivity residual: from regression of log Feb. 2009 sales on log employees and log business assets	.080	.088	.116
	(.181)	(.170)	(1.154)
	152	152	99

TABLE A13 (*Continued*)

Outcome Variable	ITT Treatment Effect Estimates (1)	(2)	Control Group Mean (SD) (3)
ROA: Feb. 2009 sales minus costs divided by assets	.173*	.197*	−.001
	(.098)	(.113)	(.574)
	140	140	95
Controls for baseline value of outcome	No	Yes	. . .

Note.—This table includes only enterprises that were successfully matched with IMSS data. Each cell in cols. 1 and 2 contains the treatment effect point estimate, robust standard error, and number of observations for a separate OLS estimation. For the regressions that control for the outcome variable measured at baseline (col. 2), when the baseline outcome variable is missing, the missing value is filled in with zero and a dummy variable indicating that the baseline observation is missing is added to the model. All regressions include controls for strata dummies and re-randomization variables, as well as a dummy for having been surveyed in March 2009 (vs. April, May, or June) at follow-up. Column 3 contains means and standard deviations for the control group at follow-up.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

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