

# Evaluating Entrepreneurship Training: How Important are Field Experiments for Estimating Impacts?

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## Abstract

Governments and donors around the world spend billions of dollars subsidizing entrepreneurship training programs. Unbiased evaluation estimates are paramount to understanding whether subsidies and investments in these programs are justified. The goal of this paper is to compare non-experimental to experimental methods for evaluating the effectiveness of entrepreneurship training programs that provide training in management, marketing, accounting, legal and other aspects of starting and running businesses. For the comparison, I take advantage of an unprecedented setting in which experimental and non-experimental estimates are derived from the same underlying population of study participants. The Growing America through Entrepreneurship (GATE) field experiment provides a uniquely large sample allowing for a separate non-experimental analysis using only the control group. Experimental estimates indicate null effects of entrepreneurship training on business outcomes: business ownership, sales and employment. In contrast, non-experimental estimates using an extremely rich set of controls including typically unobservable characteristics such as previous family business experience, credit problems, and personality traits find large, positive and statistically significant effects. The non-experimental estimates range from 21-22 percentage points for business ownership, \$1,300-2,000 for average monthly sales, and 4-6 percentage points for hiring any employees at 1.5-year and 5-year follow-ups. Nearest neighbor and propensity score matching models using detailed individual characteristics provide similarly large, positive and statistically significant effects of entrepreneurship training on business outcomes. The findings have implications for choosing evaluation methods of government programs and provide evidence of positive selection bias which has more general implications on the scalability of entrepreneurship training programs to broader populations.

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# 1 Introduction

Expenditures on subsidizing entrepreneurship training are large and growing rapidly around the world (OECD 2017). In the United States alone, there exist more than 1,000 SBA-subsidized Small Business Development Centers (SBDC) and at least 800 other not-for-profit programs providing entrepreneurship training, which includes counseling, consulting and classroom training in management, marketing, accounting, legal and other aspects of starting and running businesses.<sup>1</sup> Justifying these expenditures, administrators often document how many participants are served, how many businesses are created, and/or how many employees those businesses hired.<sup>2</sup> Interestingly, however, the few RCT evaluations of entrepreneurship training programs to date show mostly null effects (Benus et al. 2010; Davis et al. 2013; Fairlie et al. 2016).<sup>3</sup> Even a very expensive training program targeted at youth (12,000 euros) had mixed success when evaluated with an RCT (Astebro and Hoos 2016).

Are entrepreneurship training programs susceptible to selection bias because training is opted into and not exogenously determined? This paper provides a novel comparison of experimental and non-experimental methods in evaluating the effectiveness of entrepreneurship training and development programs. Estimates might differ due to either positive or negative selection bias or any other unobservable characteristic of the individual correlated with training receipt and business outcomes. Positive bias might occur, for example, if the individuals most willing to invest in training programs are the ones that benefit the most from these programs. On the other hand,

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<sup>1</sup> SBDCs exist in all 50 states, and are administered and funded through partnerships between the SBA and public colleges and not-for-profits. See <http://www.sba.gov/content/small-business-development-centers-sbdc> for a directory of SBDCs, Aspen Institute (2012) for information on other non-profit programs, and European Commission (2016) for a description of programs in the European Union.

<sup>2</sup> For example, Chrisman's (1994, ..., 2017) ongoing annual evaluations of national SBDC contributions focus on the "difference between the growth rate of clients and weighted average growth rates for all businesses was used to estimate the incremental or marginal changes in sales and employment growth of the sample." See also Guo, Karoly and Zissimopoulos (2008) for a discussion.

<sup>3</sup> One exception is a small earlier demonstration experiment conducted in Washington and Massachusetts in 1992 (Benus et al. 1994). The study found positive impacts on self-employment, total earnings, and job creation from a training assistance program that allowed for concurrent U.I. benefit payments and a lump-sum benefit payment. Another exception is that the second round of experimental GATE programs targeting rural dislocated workers in North Carolina found training to increase self-employment roughly three years later (Davis et al. 2013). The other experimental GATE II program targeted older dislocated workers in Virginia and found no increase in self-employment two years later. For research using non-randomized approaches to identifying effects of self-employment training programs, see, e.g., Kosanovich and Fleck (2001) and Rodriguez-Planas (2010).

selection might be negative if the marginal, most in-need individuals might be the ones who sign up for entrepreneurship training and development programs. In either case, improving our understanding of the potential bias, either positive or negative, from non-experimental estimates of the effectiveness of entrepreneurship training programs is of paramount importance for policy as enthusiasm and expenditures on these programs continue to rise around the world.

To compare experimental and non-experimental estimates, I take advantage of an unprecedented setting in which experimental and non-experimental estimates are derived from the same underlying sample of study participants. To the best of my knowledge, this approach has not been previously taken to estimate the potential bias from non-experimental estimates of entrepreneurship training and development programs. I analyze the largest randomized control trial on entrepreneurship training ever conducted: Growing America through Entrepreneurship (GATE). The uniquely large size of this field experiment permits a separate non-experimental analysis with only the control group, which consists of roughly two thousand observations. Using this sample, I estimate training effects to compare to the experimental estimates using full sample. Another advantage of the study is that the surveys conducted for GATE provide extensive information on study participants including detailed demographic, baseline income and business ownership, and personality characteristics. The availability of detailed controls removes the concern that the non-experimental evaluation techniques used such as OLS, nearest neighbor matching, and propensity score matching are missing some key individual characteristics to use as controls or for matching observations. In addition to estimating the “bias” from these techniques the comparison provides some information on whether individuals positively or negatively select into entrepreneurship training, which is an interesting and important question in its own right. Information on the direction of selection is useful for policymakers considering broadening the reach of existing entrepreneurship training programs.

GATE was a longitudinal field experiment conducted by the U.S. Department of Labor and the Small Business Administration (SBA) in which free entrepreneurship training was randomly offered to individuals interested in starting or improving a business. More than four thousand individuals applied for a limited number of slots at 14 different SBDCs and non-profit community-based organizations (CBOs) located across 7 sites in 3 states. SBDCs and CBOs are the predominant providers of entrepreneurship training services in the U.S. market. Subjects assigned to the treatment group were offered an array of best-practice training services whereas subjects

assigned to the control group were not offered any free services. Follow-up surveys at 6, 18, and 60 months after treatment assignment yield a rich set of outcome measures. The 60-month follow-up provides rare measures of long-run outcomes.

Although business ownership rates increased 20-25 percentage points among recipients of entrepreneurship training from the GATE program, estimates of positive effects on business ownership rates and other outcomes disappear once the control group is used. Experimental estimates of average treatment effects suggest that entrepreneurship training has null impacts on business ownership, sales and employment. The only exception is that entrepreneurship training increases the likelihood of business ownership in the short-run (by 13 percentage points 6-months after random assignment). However, by the 18-month followup survey wave the effect disappears. Using the same underlying sample of study participants but drawing from the control group, non-experimental estimates are different. I find that receipt of entrepreneurship training has large, positive and statistically significant “effects” on business ownership, sales and employment. For example, at the 1.5-year follow up the non-experimental estimates indicate increases of 21 percentage points for business ownership, \$1,500 for average monthly sales, and 4 percentage points for hiring any employees. Nearest neighbor and propensity score matching models using detailed characteristics provide similarly large, positive and statistically significant effects of entrepreneurship training on business outcomes. Additionally, the experimental vs. non-experimental estimate comparison draws a similar conclusion when focusing on one-on-one counseling services within entrepreneurship training and whether the applicant was self-employed or not self-employed at baseline. These findings indicate that positive selection into entrepreneurship training is an important concern and needs to be addressed in evaluations of programs that provide entrepreneurship training and related services.

The rest of the paper proceeds as follows. Section 2 provides more details on GATE, including its research design and implementation, and a treatment/control group balance check. Section 3 presents the nature of the training services received by subjects. Section 4 presents experimental estimates. Section 5 presents non-experimental estimates, and results for one-on-one counseling services and by baseline self-employment. Section 6 concludes.

## **2 Experimental Design and Study Participants**

The field experiment used for the analysis is the Growing America through Entrepreneurship (GATE) project. The experimental evaluation was designed and implemented by the U.S. Department of Labor (DOL) in partnership with the U.S. Small Business Administration (SBA). The goal of GATE was to assist would-be-entrepreneurs to create and/or grow businesses.<sup>4</sup> The evaluation was designed to capture existing representative training providers and recipients. The intake/treatment phase of the evaluation ran from September 2003 to July 2005 in seven sites that captured both urban and rural areas. Follow-up surveys were conducted at 6-, 18-, and 60-months post-random assignment for each participant.

Individuals entered the study by completing an application process for a standard offer of free training from one of 14 established providers.<sup>5</sup> The application process started with an orientation meeting at one of 21 One-Stop Career Centers in the seven sites. Anyone attending the orientation meeting could then apply by completing and mailing a form with questions on demographics, work and business experience, and the individual's current business or new business idea. Program coordinators randomized applicants to treatment or control with equal probability. Program administrators for each training provider then offered treatment applicants a standard array of free training services, and told control applicants that the GATE program did not have the capacity to offer them services and did not offer referrals to any other services. Individuals in both treatment and control groups were notified that they would be mailed follow-up surveys.

GATE is the largest-ever randomized evaluation of entrepreneurship training and assistance, with 4,197 individuals randomized at baseline. The uniquely large size of the field experiment is essential for creating a large enough sample size for the control group to explore non-experimental estimates of the effects of entrepreneurship training in addition to experimental estimates.<sup>6</sup>

## **2.1 Entrepreneurship Training Services**

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<sup>4</sup> See <http://www.doleta.gov/projectgate/> for more information.

<sup>5</sup> Training providers marketed GATE through several channels: online; on-site electronic kiosks, merchandising, and paper materials; direct mail (insert with Unemployment Insurance checks); mass media; and referral networks with community-based organizations.

<sup>6</sup> In contrast, the RCT evaluation of the GATE II program in North Carolina (dislocated rural workers) yielded only 294 observations in the control group (196 with answers to the training receipt question) and the evaluation of the Virginia program yielded only 191 observations in the control group (158 with answers to the training receipt question).

GATE training providers were chosen with representativeness of the subsidized training market in mind. Fourteen different, established organizations from seven different sites participated in the GATE study, with a mix of SBA-funded Small Business Development Centers (SBDCs) and non-profit community-based organizations (CBOs), and urban and rural locations that is characteristic of the market. The fourteen participating providers deliver services in and around Philadelphia; Pittsburgh; Minneapolis/St. Paul; Duluth, Minnesota; Virginia, Minnesota; Portland, Maine; Lewiston, Maine; and Bangor, Maine (see Bellotti 2006 for more details). SBDCs and CBOs offer similar services, as detailed below, but differ somewhat in their stated goals. The SBDCs tend to emphasize helping small business owners grow (or start) their businesses to contribute to the local economy, whereas the CBOs tend to emphasize small business ownership as a path to self-sufficiency. Both types of organizations employ experienced business consultants to deliver one-on-one and group trainings.

GATE training was customized for the individual from an array of services offered by the provider, as is typical in the subsidized market.<sup>7</sup> Training began with a one-on-one assessment to produce a service plan that typically combined one-on-one with selected group services. The majority of treatment group individuals then received one-on-one counseling/consulting that was customized to the individual's experience, capability, circumstances, and opportunities. The largest component of training, however, was classroom/group training(s). These targeted a variety of general and specialized topics at different experience levels. Introductory courses/seminars/workshops covered subjects such as management practices, legal structure, business plans, and marketing. Intermediate and advanced group trainings covered subjects including managing growth, obtaining financing, legal risks, and personnel issues. More specialized group trainings covered, for example, accounting, information technology, and web-based businesses. Benus et al. (2009) estimate that the total cost of providing training to GATE recipients is \$1,321 per person.

## **2.2 Representation of Study Participant Sample**

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<sup>7</sup> E.g., sba.gov describes SBDCs as providing "...extensive, one-on-one, long-term professional business advising, low-cost training and other specialized services" (<http://www.sba.gov/content/small-business-development-centers-sbdc>).

GATE was designed to estimate treatment effects on recipients who are representative of those served by subsidized training providers. GATE services, like most subsidized training programs in the U.S, were marketed to any individual interested in starting or growing a business.<sup>8</sup> Although there are no sources of nationally representative data on the characteristics of training entrants, a comparison to the characteristics of self-employment entrants is useful as a very rough approximation. To examine the characteristics of self-employment entrants I create panel data from the Current Population Survey (CPS) for similar years. Although the CPS data are usually used as cross-sectional data, panel data can be created from the underlying data files to measure business starts by individuals (Fairlie 2013; Fairlie and Chatterji 2013). Appendix Table 1 reports the average characteristics of GATE participants and self-employment entrants from the CPS. GATE participants do not differ substantially from self-employment entrants on observable characteristics. For example, 47 percent of GATE participants are female compared with 42 percent of self-employment entrants. GATE participants are more likely to be African-American, but less likely to be Latino.<sup>9</sup> The percentage of white and Asian race are similar. Average schooling levels are also reasonably similar with 14 years of schooling among GATE participants and 13 years of schooling among self-employment entrants. Average age and income are roughly similar (although GATE participants are less likely to come from the highest income group). The biggest difference is that 55 percent of GATE participants are not employed at the time of application, which is expected because participants are seeking entrepreneurship training services. But, self-employment entrants also have a high percentage coming from unemployment (21 percent) which is consistent with the findings from previous studies showing a strong relationship between unemployment and self-employment entry (Parker 2009; Krashinsky 2005; Fairlie 2013).

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<sup>8</sup> Some assistance programs target recipients of social insurance. Demonstration programs in Washington and Massachusetts starting in 1989, and Self-Employment Assistance programs in several states starting in 1993, target unemployment insurance recipients. These programs provide concurrent UI benefits or lump sum payments, and exempt search requirements for wage/salary jobs to help start self-employment activities (Weigensberg et al. 2017). Similar programs exist in Europe (e.g. Baumgartner and Caliendo 2008). The Self-Employment Investment Demonstration, implemented from 1988 to 1992 in five states, targeted AFDC recipients.

<sup>9</sup> An extensive literature documents and explores the causes and consequences of low rates of business entry and success among African-Americans (see Fairlie and Robb 2008; 2009 for example). Interestingly, there is no evidence that blacks have less preference for becoming self-employed and in fact might have a stronger preference (Walstad and Kourilsky 1998; Koellinger and Minniti 2006).

In all, the available data suggests that GATE obtained a representative sample of subsidized training recipients. In the choice of sites for training service providers, one of the main goals was to create a representative sample (Bellotti 2006). Although self-employment entrants are not a perfect comparison group they provide at least some assurance that GATE is capturing a wide range of interested entrants.

### 2.3 Balance Check

Table 1 checks for treatment vs. control balance on baseline characteristics at both baseline, and each of the three follow-ups (sample composition changes over time due to attrition). Random assignment was not stratified by site, but the top rows show that each site produced roughly 50-50 assignments nevertheless (Columns 1-3). Among the numerous baseline characteristics measured in the application, only one, age, is statistically different between treatment and control. One would expect to find one or two significant differences by chance, and the magnitude of the age difference is small ( $< 1$  year). In any case, when estimating treatment effects results are presented both without covariates as well as with controls for a large set of detailed baseline characteristics.

Table 1 also compares treatment and control completion rates and baseline characteristics for each of the three follow-up surveys. The bottom row shows that control group members are more likely to attrit: the completion rate differs by 4-5 percentage points, on a base of 56-80 percent, for each follow-up wave. However, despite differential attrition rates overall, we do not find differences in the observable composition of the treatment versus control groups, based on characteristics observed in the baseline.<sup>10</sup> The number of significant differences is about what one would expect to find by chance, and the magnitude of these differences is small.

### 2.4 Econometric Model

The base specification for non-experimental estimates of entrepreneurship training effects is a standard regression of business outcomes on training *receipt*. Thus, the comparison experimental specification for estimating average treatment effects on business outcomes estimates the effects of *receiving* entrepreneurship training (i.e. local average treatment effects LATE) instead of estimating the effects of being *offered* free entrepreneurship training (i.e. “intent-to-treat” effects

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<sup>10</sup> See Fairlie et al. (2016) for a detailed analysis of the effects of attrition. The experimental results are not sensitive to differential attrition.

ITT). LATE estimates are needed for the comparison to non-experimental estimates because they adjust for treatment and control differences in takeup of actual training. Non-experimental estimates by necessity focus on the effects of actual training received.

I estimate first-stage OLS regressions of the form:

$$(2.1) E_{it} = \omega + \gamma X_{ib} + \pi T_{ib} + u_{it}.$$

where  $E_{it}$  measures whether individual  $i$  had obtained any training by follow-up survey  $t$ ,<sup>11</sup>  $X_{ib}$  is a vector of baseline covariates (indexed by  $b$  for “baseline”) reported in Table 1, and  $T_{ib} = 1$  if  $i$  was assigned to the treatment group. The second-stage regression for an outcome of interest  $y_{it}$ , measured for individual  $i$  at time  $t$ , is then:

$$(2.2) y_{it} = \alpha + \beta X_{ib} + \Delta \hat{E}_{ib} + \varepsilon_{it},$$

where  $\hat{E}_{ib}$  is the predicted likelihood of training receipt and  $u_{it}$  and  $\varepsilon_{it}$  are error terms.  $\Delta$  provides an estimate of the local average treatment effect (LATE). The LATE estimates are essentially scaled-up values of the ITT estimates (which are estimated from the regression of  $y_{it}$  on  $T_{ib}$ ).

The main non-experimental regressions simply replace predicted training receipt,  $\hat{E}_{ib}$ , with actual training receipt,  $E_{it}$ , in (2.2). These regressions are estimated with only the control group sample.

### 3 Entrepreneurship Training Receipt

Before turning to experimental and non-experimental estimates of entrepreneurship training I examine training receipt. Given that not all of the treatment group might have received training and the control group was not restricted from obtaining training elsewhere, it is important for the experimental analysis to first examine whether and how the GATE treatment changed the use of training services. The training receipt differential between the treatment and control groups needs to be large enough to be able to detect treatment effects. Additionally, to estimate non-experimental training effects for the control group it is important to verify that at least some of the control group received training services on their own creating variation in the training receipt variable.

Table 2 reports entrepreneurship training receipt by the control and treatment groups. Both the treatment and control groups received entrepreneurship training. Among the control group, 44%

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<sup>11</sup> Estimates of first-stage relationships between treatment assignment and intensive margins of training receipt reveal similar results (see Fairlie et al. 2016). Note that there is only one instrument and hence separate effects on extensive and intensive margins cannot be identified.

received any training by Wave 1 (6 months following random assignment). Among the treatment group, 81% received training which is 37 percentage points higher than the control group.<sup>12</sup> Cumulating across waves, 65% of the control group received any training by 5 years after random assignment. Among the treatment group, 89% received any training. The results indicate that: i) a relatively large share of the control group received at least some entrepreneurship training, and ii) there is a large treatment/control group difference in training receipt. The first result is important for the non-experimental control group analysis, and the second result is important for the experimental analysis.

Turning to hours of training, it is important to distinguish between mean hours and hours per recipient. For the control group, each of the training recipients received, on average, 15 hours of training in the first 6 months, and 29 hours of training over the 5-year period. Training per recipient was higher in the first 6 months for the treatment group (19 hours), but was similar over the full study period, 31 hours. For the experimental design, it is important to focus on mean hours unconditional on receipt instead of mean hours per recipient to remove selection concerns. The control group received an average of 6.6 hours of training by Wave 1. The treatment group received more than twice the number of hours of training by Wave 1: 15.6 vs. 6.6. By the 5-year follow up mean hours of training receipt for the treatment group was 28 hours which was 8.5 more hours than the control group (Column 10).<sup>13</sup>

Returning to Table 2, note also that the cumulative differences in training hours are driven by the Wave 1 effects; this is unsurprising, given that the sample is comprised of people with demand for training at the time of random assignment. We would only expect to find differences in training obtained at later horizons if there is strong complementarity between training obtained in the short-

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<sup>12</sup> Examining who receives entrepreneurship training, I find only a few characteristics that predict take-up of training by each follow-up wave. Focusing on the main effects I find some evidence that African Americans and the more educated are more likely to receive training (see Appendix Table 2). Examining differential take up between the treatment and control groups, I find only a few significant differences. F-tests for differential take up for all covariates do not reject equality in any of the three follow-up waves.

<sup>13</sup> The 8.5 hour differential only measures training time and not any “homework or other multiplier.” The multiplier could be substantial. In evaluating the effectiveness of a standard 5-credit college course, for example, treatment effects would work through much more than the 30 or so hours of instructional time per term. Similarly, the impact of preventative medical care works through more than the few hours (or minutes) of office visits. Finally, many board members and advisors of small companies only provide a handful of hours of advice or training each quarter. The provision of key bits of information, and how recipients then apply that information to the businesses is the key factor (Fairlie et al. 2016).

run (between random assignment and Wave 1 follow-up) and training obtained later (between Wave 1 and Wave 2, and/or between Wave 2 and Wave 3).

Table 2 also reports evidence on how treatment assignment affects the type/quality of training received. The results suggest that an individual in the treatment group is more likely to obtain customized training; e.g., 30 percentage points more likely to receive one-on-one assistance. Conditional on receiving any training the percentage of the control and treatment groups receiving one-on-one training services, the difference is smaller (44% of control group recipients and 65% of treatment recipients). And, among recipients of customized training the average hours received by the control and treatment groups are similar.

Appendix Table 3 reports self-reports by study participants on whether and what parts of training services they found to be helpful. All of the reported estimates condition on training receipt. The results suggest that an individual in the treatment group receiving training is 16 percentage points more likely to receive help refining their business idea. Among treatment group respondents 52% of recipients reported it “very useful” and 34% “somewhat useful.” The comparable proportions for those in the control group who obtained any training are 36% and 41%. Appendix Table 3 also reports treatment and control group responses to questions about whether training helped with 12 specific aspects of business planning and operation. (The sample is again comprised of training recipients only). The treatment group is more likely to respond that training was helpful for each of the 12 aspects.

In all, the evidence suggests that the experiment produced training in substantially greater quantity and quality for treatment relative to control individuals. Hence any null effects are likely due to training that is ineffective per se, rather than to a low-powered identification strategy (Fairlie et al. 2005). The evidence also suggests that, if anything, the control group who received training received lower quality training than the treatment group, and thus any larger estimated effects of entrepreneurship training using non-experimental methods with the control group sample are not simply due to higher-quality training.

## **4 Experimental Estimates of Entrepreneurship Training**

In this section, I first estimate experimental effects using the full study participant sample. I focus on estimating the effects of entrepreneurship training on business ownership and other business

outcomes Table 3 reports experimental LATE estimates of equation (2.2).<sup>14</sup> Results with and without controls for baseline covariates are similar, and the discussions below focus on the former (Column 2). The average impact of entrepreneurship training on business ownership at Wave 1 (the 6-month follow-up) is positive and significant: 13.4 percentage points on a base (control group mean) of 35.9 pp. At the 18-month follow-up, the treatment effect point estimate is smaller and no longer statistically significant. 60 months after random assignment, the treatment and control groups have nearly identical levels of business ownership. These results are not driven by changes in sample composition: I get similar results after restricting the sample to Wave 3 respondents. In all, the results in this first panel show only positive short-term effects of entrepreneurship training on business ownership that die out quickly over time.<sup>15</sup>

I turn to experimental estimates of impacts on business outcomes. Examining the average treatment effects on business sales and hiring employees (Table 3), I find no significant effects at any horizon, suggesting that the marginal businesses had low levels of sales and generally did not hire employees. Note that these results do not condition on business ownership, and thus capture the treatment's overall impact on sales and hiring employees.

I focus on these three common measures of business success, however, estimates from the experiment show quite resounding evidence of null effects across numerous additional outcomes (see Fairlie et al. 20016). For example, when switching from the extensive margin of employment to using average number of employees as the employment outcome the results are the same: no effects on any time horizon. The null results are also not sensitive to alternative measures of outcomes, outliers, and sample restrictions.

## **5 Non-Experimental Estimates of Entrepreneurship Training**

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<sup>14</sup> ITT estimates are reported in Appendix Table 4. As expected given the non-compliance rates, the point estimates are generally scaled down by a factor of 2 to 3 relative to the LATE estimates. None of the statistical inferences change. Because most of the LATE estimates are close to zero the "scaled down" ITT estimates also tend to be close to zero.

<sup>15</sup> The results are not due to the influence of side or casual businesses, or disguised unemployment (Carter and Sutch 1994). Defining business ownership with 30 or more hours worked per week, I find lower rates of business ownership, but similar treatment-control differences. I also restrict business ownership to only include businesses reporting positive sales at each survey wave to remove non-serious self-employment activities. Again, I find similar results.

In this section, I estimate non-experimental regressions for the effects of entrepreneurship training using the control group sample. I start by estimating a straightforward OLS regression in which the dependent variable is business ownership.<sup>16</sup> Specifications with no controls, main demographic controls (i.e. those found in datasets such as the CPS and ACS), and a rich set of detailed controls are reported in Table 4. All of these variables are measured at baseline. For business ownership, the non-experimental entrepreneurship training effect estimates are large, positive and statistically significant at the 6-month, 18-month and 60-month followup waves. For the specification using the extensive set of controls, the estimates indicate that entrepreneurship training increases business ownership rates by 21 percentage points (base of 38 pp) at the 5-year followup. In contrast, the experimental results reveal a point estimate of 1 percentage point that is not statistically significant. At the 1.5 year followup, the non-experimental estimates indicate an equally large and significant effect (22 percentage points) compared to a null experimental finding. The only followup wave in which the estimates are more similar is the first one at 6 months. The non-experimental estimates indicate a short run effect of 15.3 percentage points compared with an experimental estimate of 13.3 percentage points. This finding might just be the result of study participants initially trying self-employment in the first few months after random assignment given their interest in entrepreneurship training and the followup survey only being 6 months later.

The large, positive and statistically significant estimates of entrepreneurship training effects are found with and without including controls. Specifications 2, 5 and 8 include the standard set of demographic and education controls included in most large datasets such as the CPS and ACS. The point estimates on training become smaller after the inclusion of this standard set of controls relative to the specification without controls, implying that they capture at least some heterogeneity.

The baseline survey data collected as part of the GATE experiment provide a unique set of detailed characteristics of individuals. Typically unobservable characteristics such as baseline household income, whether self-employed at time of application, previous family business/work experience (has relatives or friends who have been previously self-employed; has ever worked for relatives or friends who are self-employed), health related information (has health insurance from current employer; has health problems) credit history problems, currently receiving UI

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<sup>16</sup> Estimates of marginal effects from probit and logit models are similar.

benefits, and personality characteristics (autonomy index; risk tolerance index) are included. Previous research indicates that these characteristics along with the demographic characteristics are important determinants of business ownership and outcomes.<sup>17</sup> Even after controlling for this rich set of typically unobservable characteristics the non-experimental regressions indicate large, positive and statistically significant training effects.

The comparison of findings is similar for business sales. Table 4 also reports estimates for the same three followup waves where sales is the dependent variable. The non-experimental estimates indicate that training increases monthly sales by \$2,035 at the 5 year followup. Monthly sales are \$1,285 higher at the 1.5 year followup. In contrast, experimentally there is no estimated effect of training on business sales, with precise estimates of null treatment effects.<sup>18</sup>

Employment by the business also follows the same pattern. Table 5 reports estimates. Non-experimental estimates indicate that training has large, positive and statistically significant effects on having any employees. At the 1.5 year followup the increase is 4.4 percentage points, and at the 5 year followup the increase is 6.4 percentage points. For comparison, in both cases the experimental estimates indicate no effect on having employees.

Although the non-experimental regressions attempt to control for the long list of detailed individual characteristics there might be unobservable differences between those who choose to obtain entrepreneurial training and those who do not. One method of investigating this issue is to compare the average observable characteristics of the two groups. Large differences in these observable characteristics potentially indicate differences in unobservable characteristics that are correlated with both entrepreneurship training receipt and business outcomes. Table 5 recreates the balance check from randomization into treatment and control groups reported in Table 1, but in this case only the control group sample is included and the groups are distinguished by receipt or no receipt of entrepreneurship training. Several of the variables show a statistical difference

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<sup>17</sup> See, for example, evidence on the importance of gender (Koellinger, Minniti, and Schade 2013), race (Fairlie and Robb 2007), age (Zissimopoulos and Karoly 2007), marital status and children (Hundly 2001; Parker 2008), education (van der Sluis, van Praag, and Vijverberg 2008), family business experience (Dunn and Holtz-Eakin 2000; Fairlie and Robb 2007), liquidity constraints (Evans and Jovanovic 1989), health insurance (Fairlie, Kapur and Gates 2011), unemployment, and personality traits (Caliendo, Fossen, and Kritikos 2014). Also, see Parker (2009) and Simoes, Crespo, and Moreira (2016) for reviews of the literature.

<sup>18</sup> Estimates from log specifications provide similar results. Monthly sales are 0.45 log points higher with entrepreneurship training receipt at the 5-year followup, and are 0.55 log points higher at the 1.5 year followup.

between the training receipt and no training receipt groups. For example, the training receipt group has higher baseline income, self-employment probability, age and risk tolerance, but none of these differences is very large in magnitude. The finding is disconcerting because even small observed differences between groups may lead to substantially biased non-experimental estimates of entrepreneurship training effects on business outcomes.

## 5.1 Matching Estimator Estimates

To further control for unobserved heterogeneity, I estimate the relationship using matching estimators. These estimators attempt to compare the outcomes of individuals who are as similar as possible with the exception of treatment status (i.e. receipt or no receipt of training among the control group). I first estimate the entrepreneurship training effect using nearest-neighbor matching. In nearest-neighbor matching the goal is to first create a similarity measure which determines how “close” two observations are to each other. Then the “nearest neighbor” observations are identified from the calculated distances between pairs of observations based on the specified set of covariates, and are matched for the comparison of mean outcomes between the two groups.<sup>19</sup>

Table 6 reports estimates for the three business outcome measures and three followup waves. Estimates are provided for two sets of matches -- the standard set of background characteristics and the expanded set of detailed demographic, income, health and previous business/work experience characteristics. Expanded sets of characteristics for matching increase the enforced similarity between groups, but increase estimate variance compared with more limited sets of characteristics for matching. Nearest neighbor matching estimates indicate large effects on business ownership. At the 1.5-year and 5-year follow up waves business ownership is estimated to be 20 to 24 percentage points higher among individuals receiving entrepreneurship training than not receiving training in the control group. The estimates are less clean for monthly business sales, but half the reported estimates show large, positive and statistically significant effect estimates of entrepreneurship training. All of the point estimates are large and positive. Business employment is also estimated to be much higher with the receipt of entrepreneurship

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<sup>19</sup> The nearest-neighbor matches use the Mahalanobis distance between the vectors of covariates of observations, which accounts for correlation between covariates and having different measurement scales.

training. The likelihood of having employees increases by 4-5 percentage points 1.5 years later, and 7 percentage points 5 years later.

The estimates are robust to several alternative specifications. First, I remove the continuous and somewhat continuous variables such as age, highest grade completed, the risk tolerance index, and the autonomy index. The estimates are similar. I continue to find large, positive and statistically significant effects on outcomes. Next, I allow for multiple nearest neighbor matches instead of only one match. Allowing for multiple matches results in very similar estimates. Finally, I match exactly on baseline self-employment given its importance in determining future self-employment. I continue to include all of the other characteristics in the nearest neighbor matches. Again, the estimates are robust to this alternative specification of the model.

Table 7 reports estimates from an alternative method of matching, propensity score matching. In this case, individuals are matched on predicted probabilities of treatment, referred to as propensity scores.<sup>20</sup> Predicted probabilities are calculated in a first-stage logit regression of training receipt within the control group. The propensity score estimates also indicate large positive effects on all business outcomes. The estimates for business ownership in the 1.5 and 5 year followup waves range from 17 to 25 percentage points. The estimates for monthly business sales are \$1,000 to \$2,200. Employment levels are estimated to be 5 to 6 percentage points higher among training recipients in the control sample.

These estimates of the effect of entrepreneurship training on business ownership, sales and employees using the control sample are large, positive, and statistically significant, contrasting sharply with the estimates of null effects found in the experiment. This discrepancy raises concerns about positive selection into entrepreneurship training resulting in an overstatement of the non-experimental estimates of the effects of training on business outcomes. Furthermore, even a rich set of controls-- for baseline household income level, self-employment status, health problems, work experience in a family business, credit history, unemployment insurance receipt, employer-provided health insurance, personality traits, and standard demographic controls—cannot purge non-experimental treatment effect estimates of strong

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<sup>20</sup> A logit model is estimated to recover the predicted probabilities of training receipt. Estimates are similar using a probit model to estimate the predicted probabilities. I also verify that there is common support for the propensity score match.

upward selection bias. Techniques such as nearest neighbor matching and propensity score matching to address selection also do not change the conclusion.

## **5.2 Comparing Estimates of the Impact of One-on-one Counseling Services**

One-on-one counseling services relative to classroom instruction might provide the largest impacts on business outcomes within broader entrepreneurship training assistance. I compare experimental and non-experimental estimates of individualized one-on-one counseling effects. For the treatment group, 64 percent received individualized counseling services compared with 34 percent of the control group (Table 2). For any training services, as noted above, 81 percent of the treatment group and 44 percent of the control group received services. The shift in the treatment-control differential suggests that the LATE (IV) treatment estimates for one-on-one counseling will be rescaled versions of the ones for any entrepreneurship training services according to the equations in Section 2.4.

Table 8 reports experimental and non-experimental estimates of the impact of one-on-one counseling services on business outcomes. I compare LATE estimates from the full experimental sample, and three non-experimental methods: OLS, nearest neighbor and propensity score matching. The experimental estimates show null effects for all outcomes (other than 6 month impacts on self-employment) which is expected because the switch from the receipt of any entrepreneurship training services to only one-on-one counseling services essentially just scales up the estimates. And, scaling up zero impact estimates results in zero impact estimates. For self-employment at all follow up waves, the non-experimental estimates indicate large effects. The receipt of one-on-one counseling is associated with a 20 to 24 percentage point higher business ownership rate at the 1.5- and 5-year follow up surveys. The estimates for business sales vary more, but generally indicate large positive effects from individualized counseling. Finally, non-experimental estimates of the effects of one-on-one counseling on hiring employees consistently show large, positive effects.

Overall, changing the focus from any entrepreneurship training services to only one-on-one counseling services, which are likely to have the largest impact, does not change the conclusions. Experimental estimates for one-on-one counseling show null effects whereas non-experimental estimates indicate large, positive effects.

### 5.3 Comparing Estimates based on Baseline Self-Employment

As noted above, roughly 20 percent of both the treatment and control groups are self-employed at the time of random assignment. The GATE program was open to both individuals who wanted to start a business and to individuals who wanted to grow their pre-existing business. The objective corresponds with the general objectives of SBDCs which is to provide “assistance to *current* and *prospective* small business owners.” The bias in non-experimental estimates might differ between non-self-employed and self-employed at baseline because of potential differences in selection into who seeks training among the two groups.

To investigate this question, Tables 9 and 10 report experimental and non-experimental estimates of the impact of entrepreneurship training on self-employment, sales and employees at the three follow up waves for the baseline not self-employed and self-employed, respectively. For those interested in starting a business the estimates do not differ substantially from the full sample. Experimental estimates indicate null effects on business ownership (other than the 6-month wave) and on sales and employees. The non-experimental estimates consistently show large, positive effects on business ownership and employees. For sales, the point estimates become less precise, but are large and positive in all cases, and marginally significant in a couple of cases.

Table 10 reports estimates for the smaller sample of self-employed individuals who were self-employed at the time of application to the field experiment. The comparison between experimental and non-experimental estimates shows even larger differences. Experimentally, I find null effects for all outcomes. Non-experimentally, I find very large positive estimates of entrepreneurship training on business ownership. At the five-year followup the three non-experimental techniques each show a roughly 30 percentage point effect on business ownership. The estimated training effects on monthly sales after five years are also large in magnitude indicating 6 to 7.5 thousand dollars. Estimated effects on employees are less precise, but consistently show positive point estimates and several statistically significant effects.

Overall, even with smaller sample sizes and less precise estimates the results indicate major differences between experimental and non-experimental estimates regardless of the goal of training receipt. For both those who want to start a business and those who want to grow a pre-existing business non-experimental estimates indicate that entrepreneurship training substantially improves business outcomes, whereas experimental estimates indicate null effects.

## 6 Conclusion

Governments and philanthropic organizations around the world devote substantial resources to entrepreneurship training even though there is little causal evidence on its effectiveness. Instead, reporting the total number of clients served, making simple pre-post comparisons, or non-experimental estimates of positive effects are cited as justification of the “success” of programs. This paper provides the first evidence in the literature on whether non-experimental estimates of the effects of entrepreneurship training are similar to experimental estimates for the same underlying population. Additionally, the study provides the first estimates of whether there is positive, negative or no selection into entrepreneurship training.

Experimental estimates of entrepreneurship training indicate null effects on business ownership, sales and employees, and many additional business outcomes. The findings are robust to alternative specifications, outliers, and differential attrition. In contrast, for the same underlying group of study participants but focusing on the control group subsample, I find large, positive non-experimental estimates of the effects of entrepreneurship training receipt on business ownership, sales and employment. The largest ever field experiment on entrepreneurship training, GATE, makes it possible to isolate the control group sample to conduct this non-experimental analysis. Another feature of the study design is that in conducting the GATE experiment a very detailed set of baseline demographic characteristics, previous business, work and unemployment experience, and health insurance was collected. Even information on family business experience, credit history, and personality characteristics such as risk tolerance and preference for autonomy was collected. In most cases, this type of information is not available to researchers evaluating the effectiveness of training programs. Finally, information on the receipt of entrepreneurship training services was collected for the control group in addition to the treatment group in the experiment.

The differential estimates do not appear to be due to the control group receiving more or superior training than the treatment group. If anything the detailed evidence from the followup surveys on use of services, types of services, and satisfaction with services suggests that the control group appears to have, if anything, received less and lower-quality services, which would create a downward bias on non-experimental estimates relative experimental estimates.<sup>21</sup> Another concern

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<sup>21</sup> Additionally, any Hawthorne effects should similarly bias experimental estimates upward relative to non-experimental estimates.

is that the participants in the GATE study are different than individuals typically seeking entrepreneurship training. GATE was implemented with this concern in mind, however, by being careful to use a broad range of training providers in several locations, and advertise to a wide range of potentially interested individuals. More research from other programs and locations would be useful, but the findings here raises general concerns that are likely generalizable to many other settings.

The simplest evaluations of programs essentially perform a before vs. after calculation. Using this “evaluation” approach, I can estimate what GATE training services produced for the treatment group that received entrepreneurship training.<sup>22</sup> At baseline, 21 percent of the treatment group receiving training services was self-employed. At the followup surveys from 41-46 percent of the group was self-employed. Thus, GATE training services “created” a new business owner for every 4-5 participants (20-25 pp increase). Focusing on employees, however, GATE entrepreneurship training apparently “created” no jobs – roughly 10 percent of participants had a business with any employees entering the program and roughly 10 percent of participants had a business with any employees at each of the follow up waves.<sup>23</sup> Revenues are not available for business owners at baseline so a before/after comparison cannot be made.

The results presented here speak to the importance of choosing evaluation methods for entrepreneurship training. Although RCTs are expensive and difficult to implement they might be necessary given the unambiguous evidence provided here of a large upward bias in non-experimental estimates of the effectiveness of entrepreneurship training. Positive selection might be especially strong in entrepreneurship training receipt suggesting that those most able to benefit from the management, marketing, accounting, legal and other training provided are the ones most likely to obtain training. This has broad implications for policy. On the one hand, program administrators may want the individuals who are most likely to benefit to be the same individuals who receive training. But, on the other hand, if governments and philanthropists want to scale up programs they will need to temper expectations about large, positive effects. Although the null

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<sup>22</sup> E.g., the New York Senate (2011) justified extending the SEA program by stating that it "has been extremely successful in helping individuals who are likely to exhaust their regular unemployment insurance benefits to develop and establish small businesses in New York...The success of this program is evident. Over 4,000 jobs have been created and \$16 million in state tax revenue has been generated at no cost to the state."

<sup>23</sup> Similarly, the average number of employees does not increase.

effects found for the GATE program make these two points moot, other entrepreneurship training programs perhaps with additional features or higher and longer intensity of services may have positive effects that policymakers want to replicate and provide at a larger scale.

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Table 1: Treatment/Control Comparison of Characteristics for GATE Experiment

	Baseline			Follow-up Wave 1			Follow-up Wave 2			Follow-up Wave 3		
	Treatment Group	Control Group	P-Value for Treat- Control	Treatment Group	Control Group	P-Value for Treat- Control	Treatment Group	Control Group	P-Value for Treat- Control	Treatment Group	Control Group	P-Value for Treat- Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Philadelphia	28.7%	27.5%	0.40	26.8%	25.6%	0.43	25.1%	24.0%	0.49	23.1%	22.0%	0.53
Pittsburgh	13.8%	14.6%	0.43	13.7%	14.3%	0.58	14.0%	14.2%	0.82	14.5%	14.4%	0.92
Minneapolis-St. Paul	39.8%	39.0%	0.58	41.1%	39.1%	0.24	42.3%	40.4%	0.29	43.9%	42.0%	0.35
Duluth	4.6%	5.0%	0.54	4.6%	5.1%	0.51	4.7%	5.1%	0.60	5.0%	4.9%	0.99
Maine	13.1%	13.9%	0.48	13.9%	15.9%	0.09	14.0%	16.3%	0.08	13.6%	16.7%	0.03
Female	47.2%	45.7%	0.32	48.5%	46.4%	0.22	48.8%	46.9%	0.31	48.1%	47.1%	0.62
Black	30.5%	30.6%	0.91	29.1%	29.8%	0.65	27.6%	28.3%	0.69	25.3%	26.0%	0.70
Latino	6.2%	5.1%	0.12	6.3%	4.9%	0.09	6.4%	5.1%	0.12	6.4%	5.2%	0.19
Asian	4.6%	4.5%	0.86	3.8%	3.3%	0.42	3.3%	2.9%	0.52	3.1%	2.8%	0.71
Other	7.9%	8.1%	0.80	7.7%	7.6%	0.91	7.4%	7.0%	0.64	7.4%	6.6%	0.47
Not U.S. born	10.0%	10.2%	0.83	8.9%	9.2%	0.81	8.3%	8.7%	0.67	7.1%	8.1%	0.34
Age	42.08	42.77	0.03	42.73	43.42	0.04	43.16	43.81	0.07	43.91	44.16	0.54
Married	48.1%	48.4%	0.81	49.4%	48.6%	0.64	50.2%	49.0%	0.54	51.4%	49.6%	0.38
Has children	46.7%	46.1%	0.68	45.4%	45.1%	0.88	45.4%	44.6%	0.69	44.0%	42.8%	0.58
Highest grade completed	14.39	14.52	0.07	14.53	14.61	0.28	14.59	14.66	0.38	14.75	14.78	0.77
HH Income \$25,000-49,999	32.6%	33.7%	0.46	33.0%	34.0%	0.56	32.9%	33.4%	0.77	31.9%	34.5%	0.18
HH Income \$50,000-74,999	17.9%	17.2%	0.55	18.5%	17.5%	0.45	19.2%	17.8%	0.31	20.1%	17.2%	0.06
HH Income \$75,000-99,999	6.9%	7.2%	0.70	7.1%	7.2%	0.91	7.4%	7.3%	0.92	8.1%	7.4%	0.53
HH Income \$100,000+	6.3%	7.0%	0.31	6.9%	7.4%	0.56	7.5%	8.0%	0.59	8.8%	8.9%	0.96
Self-Emp. at application	18.3%	19.5%	0.33	19.3%	20.4%	0.41	19.8%	21.2%	0.34	20.3%	21.5%	0.48
Has a health problem	8.7%	8.3%	0.63	9.0%	8.9%	0.90	9.1%	8.9%	0.85	8.9%	8.4%	0.69
Has relatives or friends who have been previously S.E.	70.3%	70.4%	0.93	71.7%	72.0%	0.85	72.9%	72.5%	0.81	73.6%	73.1%	0.78
Ever worked for relatives or friends who are S.E.	31.7%	32.0%	0.81	31.7%	31.8%	0.96	31.6%	31.7%	0.97	30.9%	31.5%	0.77
Has a bad credit history	45.4%	43.9%	0.34	43.3%	43.2%	0.94	41.8%	41.5%	0.87	38.9%	39.4%	0.79
Currently receiving UI benefit	39.9%	38.1%	0.24	41.1%	39.7%	0.40	42.1%	39.3%	0.12	43.0%	41.1%	0.35
Has health insurance from current employer	16.8%	18.1%	0.26	16.6%	17.5%	0.48	16.6%	17.6%	0.46	16.8%	17.1%	0.84
Autonomy index	1.7%	-1.7%	0.27	-1.1%	-1.9%	0.81	-0.7%	-1.7%	0.79	-2.0%	-4.9%	0.49
Risk tolerance index	-0.2%	0.2%	0.87	2.6%	-1.1%	0.27	1.3%	-2.0%	0.34	-0.7%	-4.4%	0.35
F-Test for all variables			0.56			0.53			0.69			0.80
Sample Size	2,094	2,103		1,758	1,691		1,563	1,475		1,274	1,176	
Percent of baseline sample	100.0%	100.0%		84.0%	80.4%	0.003	74.6%	70.1%	0.001	60.8%	55.9%	0.001

Notes: (1) All reported characteristics are measured at time of application, prior to random assignment. (2) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (3) The autonomy index is created from standardizing responses on a scale of 1 to 5 to whether the statement "I enjoy working independently" is true about myself. The risk aversion index is created from combining standardized responses to "I'm only willing to take a risk if I am sure everything will work out" and "I am not prepared to risk my savings for my business."

Table 2: Treatment and Control Groups Receipt of Entrepreneurship Training

	R.A. to Wave 1 (6 month period)		Wave 1 to Wave 2 (12 month period)		Year Prior to Wave 3 (12 month period)		Cumulative to Wave 2		Cumulative to Wave 3	
	Percent Receiving	Mean Hours	Percent Receiving	Mean Hours	Percent Receiving	Mean Hours	Percent Receiving	Mean Hours	Percent Receiving	Mean Hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment group										
Any entrepreneurship training	81.2%	15.6	41.5%	7.3	26.1%	4.6	86.4%	22.9	88.7%	27.6
Attended classes, workshops or seminars	66.8%	13.8	35.0%	6.6	22.1%	4.0	74.1%	20.4	77.0%	24.4
Received one-on-one counseling or technical	52.5%	1.8	18.0%	0.8	10.0%	0.6	58.8%	2.6	63.7%	3.3
Control group										
Any entrepreneurship training	44.0%	6.6	37.9%	6.7	28.7%	5.7	57.3%	13.3	65.4%	19.1
Attended classes, workshops or seminars	37.7%	5.8	32.7%	6.1	25.1%	5.2	50.6%	11.9	58.7%	17.1
Received one-on-one counseling or technical	19.2%	0.9	13.8%	0.7	10.3%	0.6	27.9%	1.5	34.4%	2.1
Treatment-control difference and (standard error)										
Any entrepreneurship training	0.372 (0.015)	8.99 (0.72)	0.036 (0.018)	0.63 (0.73)	-0.026 (0.018)	-1.10 (0.71)	0.291 (0.016)	9.62 (1.15)	0.232 (0.017)	8.52 (1.53)
Attended classes, workshops	0.290 (0.016)	7.97 (0.68)	0.024 (0.017)	0.50 (0.68)	-0.030 (0.017)	-1.15 (0.68)	0.235 (0.017)	8.47 (1.08)	0.183 (0.019)	7.31 (1.38)
Received one-on-one	0.333 (0.015)	0.99 (0.15)	0.043 (0.013)	0.10 (0.11)	-0.003 (0.012)	0.05 (0.17)	0.310 (0.017)	1.09 (0.21)	0.293 (0.019)	1.14 (0.30)

Notes: (1)The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application.

Table 3: Experimental Estimates of Entrepreneurship Training Effects on Business Outcomes

Dependent Variable	Entrepreneurship Training Coefficient			N
	Without Controls (1)	With Controls (2)	Control Mean (3)	
Business owner at W1 survey date	0.1252 (0.0446)	0.1337 (0.0395)	0.3592	3443
Business owner at W2 survey date	0.0742 (0.0616)	0.0691 (0.0570)	0.4091	3032
Business owner at W3 survey date	0.0406 (0.0844)	0.0105 (0.0810)	0.3794	2446
Monthly business sales at W1 survey date	-1081.7 (751.0)	-940.2 (733.6)	1828.8	3210
Monthly business sales at W2 survey date	-606.0 (1153.9)	-441.1 (1115.1)	2132.7	2794
Monthly business sales at W3 survey date	-2097.7 (2280.4)	-2552.2 (2288.5)	2909.2	2323
Has any employees at W1 survey date	0.0353 (0.0248)	0.0363 (0.0245)	0.0722	3438
Has any employees at W2 survey date	0.0133 (0.0368)	0.0065 (0.0362)	0.0939	3023
Has any employees at W3 survey date	-0.0736 (0.0525)	-0.0871 (0.0534)	0.1104	2436

Notes: (1) The first-stage in the IV (LATE) model regresses receipt of entrepreneurship training on treatment. The second-stage regresses the listed outcome on predicted receipt of entrepreneurship training. (2) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (3) Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Table 4: Non-Experimental Regression Estimates of Entrepreneurship Training Effects on Business Outcomes

Dependent Variable	Entrepreneurship Training Coefficient			N
	Without Controls (1)	Basic Controls (2)	Detailed Controls (3)	
Business owner at W1 survey date	0.196*** (0.023)	0.178*** (0.023)	0.153*** (0.022)	1,685
Business owner at W2 survey date	0.258*** (0.025)	0.238*** (0.025)	0.221*** (0.025)	1,462
Business owner at W3 survey date	0.228*** (0.029)	0.222*** (0.030)	0.210*** (0.031)	1,162
Monthly business sales at W1 survey date	836.0* (466.300)	888.0* (480.426)	561.2 (498.920)	1,575
Monthly business sales at W2 survey date	1,248.0** (492.490)	1,418.8*** (506.489)	1,284.6** (533.815)	1,337
Monthly business sales at W3 survey date	2,058.1** (877.957)	2,508.8*** (910.799)	2,034.5** (962.796)	1,101
Has any employees at W1 survey date	0.035*** (0.013)	0.039*** (0.013)	0.030** (0.013)	1,685
Has any employees at W2 survey date	0.050*** (0.015)	0.050*** (0.016)	0.044*** (0.016)	1,457
Has any employees at W3 survey date	0.068*** (0.019)	0.064*** (0.020)	0.064*** (0.021)	1,158

Notes: (1) The sample includes only control group observations. Each cell represents an OLS regression of the listed outcome on receipt of entrepreneurship training. (2) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (3) Covariates include basic set (program sites, female, race, immigrant, age, married, children, and education level) and detailed set (+ household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance).

Table 5: Training Receipt/No Receipt Comparison of Control Group

	Follow-up Wave 1			Follow-up Wave 2			Follow-up Wave 3		
	No		P-Value for R-NR	No		P-Value for R-NR	No		P-Value for R-NR
	Receipt Group	Receipt Group		Receipt Group	Receipt Group		Receipt Group	Receipt Group	
	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Philadelphia	23.2%	27.4%	0.05	22.8%	25.8%	0.19	22.3%	21.6%	0.78
Pittsburgh	11.5%	16.6%	0.00	12.5%	16.8%	0.02	12.6%	18.1%	0.02
Minneapolis-St. Paul	41.1%	37.5%	0.13	42.0%	38.1%	0.13	42.3%	41.2%	0.73
Duluth	4.5%	5.5%	0.32	4.8%	5.4%	0.57	5.4%	4.2%	0.37
Maine	19.8%	12.9%	0.00	17.9%	13.9%	0.04	17.5%	14.9%	0.25
Female	48.3%	44.7%	0.15	48.7%	44.5%	0.11	49.1%	43.2%	0.05
Black	29.1%	30.3%	0.59	28.0%	28.9%	0.74	27.2%	24.1%	0.26
Latino	5.3%	4.7%	0.58	6.0%	4.0%	0.08	6.2%	3.5%	0.03
Asian	2.4%	3.9%	0.08	2.6%	3.4%	0.42	2.6%	3.2%	0.56
Other	7.4%	7.9%	0.74	7.2%	6.9%	0.84	7.2%	5.7%	0.32
Not U.S. born	10.2%	8.4%	0.19	9.6%	7.7%	0.21	9.3%	6.0%	0.03
Age	44.29	42.75	0.00	44.53	42.81	0.00	44.72	43.01	0.01
Married	52.4%	45.7%	0.01	51.6%	45.5%	0.02	52.6%	43.6%	0.00
Has children	47.0%	43.7%	0.18	45.5%	43.7%	0.48	44.9%	39.2%	0.06
Highest grade completed	14.93	14.36	0.00	14.96	14.26	0.00	15.04	14.29	0.00
HH Income \$25,000-49,999	33.4%	34.3%	0.70	32.9%	33.8%	0.72	34.3%	34.6%	0.91
HH Income \$50,000-74,999	17.5%	17.5%	0.99	17.4%	18.3%	0.68	17.2%	17.2%	0.99
HH Income \$75,000-99,999	8.1%	6.3%	0.14	8.3%	5.6%	0.04	8.9%	4.2%	0.00
HH Income \$100,000+	9.5%	5.8%	0.01	9.4%	6.1%	0.02	9.8%	7.2%	0.13
Self-Emp. at application	22.9%	18.5%	0.03	23.7%	18.0%	0.01	23.0%	18.8%	0.09
Has a health problem	9.2%	8.7%	0.71	8.9%	8.7%	0.93	8.2%	8.6%	0.82
Has relatives or friends who have been previously S.E.	75.3%	69.3%	0.01	74.1%	69.9%	0.08	75.1%	69.0%	0.03
Ever worked for relatives or friends who are S.E.	32.9%	30.9%	0.39	32.8%	29.9%	0.24	33.2%	28.0%	0.07
Has a bad credit history	39.9%	45.8%	0.02	41.2%	42.1%	0.73	39.8%	39.0%	0.79
Currently receiving UI benefit	39.0%	40.2%	0.64	38.9%	39.6%	0.78	39.7%	43.4%	0.23
Has health insurance from current employer	15.5%	19.1%	0.05	15.4%	20.7%	0.01	15.7%	20.1%	0.07
Autonomy index	1.6%	-4.6%	0.21	-3.0%	0.1%	0.55	-6.3%	-2.5%	0.55
Risk tolerance index	4.3%	-5.3%	0.05	2.8%	-7.9%	0.04	2.3%	-16.4%	0.00
F-Test for all variables			0.53			0.69			0.80
Sample Size	742	944		838	625		762	403	

Notes: (1) The sample includes only control group observations. (2) All reported characteristics are measured at time of application, prior to random assignment. (3) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (4) The autonomy index is created from standardizing responses on a scale of 1 to 5 to whether the statement "I enjoy working independently" is true about themselves. The risk aversion index is created from combining standardized responses to "I'm only willing to take a risk if I am sure everything will work out" and "I am not prepared to risk my savings for my business."

Table 6: Nearest Neighbor Matching Estimates of Entrepreneurship Training Effects on Business Outcomes

Dependent Variable	Entrepreneurship Training Coefficient		
	Basic Controls (2)	Detailed Controls (3)	N (4)
Business owner at W1 survey date	0.181*** (0.029)	0.143*** (0.027)	1,685
Business owner at W2 survey date	0.243*** (0.029)	0.221*** (0.029)	1,462
Business owner at W3 survey date	0.233*** (0.032)	0.192*** (0.035)	1,162
Monthly business sales at W1 survey date	696.204 (708.477)	516.117 (536.663)	1,575
Monthly business sales at W2 survey date	1,188.312*** (450.127)	813.636 (598.017)	1,337
Monthly business sales at W3 survey date	2,024.934*** (662.356)	1,996.344* (1,120.232)	1,101
Has any employees at W1 survey date	0.045*** (0.016)	0.031* (0.017)	1,685
Has any employees at W2 survey date	0.041** (0.018)	0.047*** (0.018)	1,457
Has any employees at W3 survey date	0.067*** (0.018)	0.067*** (0.019)	1,158

Notes: (1) The sample only includes control group observations. (2) Nearest neighbor matching compares observations in the receipt and no receipt groups based on the closest values of the set of covariates using the Mahalanobis distance. (3) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (4) Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Table 7: Propensity Score Matching Estimates of Entrepreneurship Training Effects on Business Outcomes

Dependent Variable	Entrepreneurship Training Coefficient		
	Basic Controls (2)	Detailed Controls (3)	N (4)
Business owner at W1 survey date	0.219*** (0.029)	0.163*** (0.027)	1,685
Business owner at W2 survey date	0.249*** (0.029)	0.242*** (0.029)	1,462
Business owner at W3 survey date	0.224*** (0.034)	0.174*** (0.037)	1,162
Monthly business sales at W1 survey date	547.596 (514.012)	1,182.939** (560.219)	1,575
Monthly business sales at W2 survey date	1,684.058*** (566.036)	1,006.384** (457.623)	1,337
Monthly business sales at W3 survey date	1,987.190* (1,028.732)	2,191.743*** (735.579)	1,101
Has any employees at W1 survey date	0.033** (0.015)	0.040** (0.016)	1,685
Has any employees at W2 survey date	0.050*** (0.018)	0.053*** (0.018)	1,457
Has any employees at W3 survey date	0.059*** (0.020)	0.059*** (0.020)	1,158

Notes: (1) The sample only includes control group observations. (2) Propensity score matching compares observations in the receipt and no receipt groups based on the closest values of predicted probabilities of receipt from a first-stage logit regression. (3) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (4) Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Table 8: Estimates of One-On-One Counseling Training Effects on Business Outcomes

Dependent Variable	One-on-One Counseling Training Coefficient			
	Experimental	OLS	Nearest	Propensity
	(LATE)		Neighbor	Score
Sample	(1)	(2)	(3)	(4)
Business owner at W1 survey date	0.149*** (0.043)	0.145*** (0.028)	0.203*** (0.036)	0.203*** (0.036)
Business owner at W2 survey date	0.065 (0.053)	0.203*** (0.028)	0.238*** (0.035)	0.238*** (0.035)
Business owner at W3 survey date	0.008 (0.064)	0.199*** (0.030)	0.230*** (0.037)	0.230*** (0.037)
Monthly business sales at W1 survey date	-1,065.084 (835.542)	662.204 (637.185)	1,755.178** (885.688)	1,755.178** (885.688)
Monthly business sales at W2 survey date	-420.809 (1,075.931)	683.900 (597.858)	1,171.716** (563.184)	1,171.716** (563.184)
Monthly business sales at W3 survey date	-2,031.623 (1,813.822)	1,756.910* (953.078)	1,675.750 (1,032.057)	1,675.750 (1,032.057)
Has any employees at W1 survey date	0.042 (0.027)	0.013 (0.017)	0.040** (0.020)	0.040** (0.020)
Has any employees at W2 survey date	0.006 (0.034)	0.062*** (0.018)	0.070*** (0.023)	0.070*** (0.023)
Has any employees at W3 survey date	-0.073* (0.043)	0.059*** (0.021)	0.072*** (0.024)	0.072*** (0.024)

Notes: (1) The sample used in Specifications 2-4 includes only control group observations. (2) Nearest neighbor matching compares observations in the receipt and no receipt groups based on the closest values of the set of covariates using the Mahalanobis distance. (3) Propensity score matching compares observations in the receipt and no receipt groups based on the closest values of predicted probabilities of receipt from a first-stage logit regression. (4) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (5) Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Table 9: Estimates of Entrepreneurship Training Effects on Business Outcomes - Not Self-Employed at Application

Dependent Variable	Entrepreneurship Training Coefficient				N	N
	Experimental	OLS	Nearest	Propensity		
	(LATE)		Neighbor	Score		
Sample	(1)	(2)	(3)	(4)	Full	Control
Business owner at W1 survey date	0.160*** (0.044)	0.137*** (0.025)	0.139*** (0.030)	0.171*** (0.033)	2,566	1,241
Business owner at W2 survey date	0.077 (0.062)	0.226*** (0.029)	0.220*** (0.032)	0.216*** (0.030)	2,226	1,065
Business owner at W3 survey date	-0.015 (0.088)	0.185*** (0.034)	0.207*** (0.034)	0.208*** (0.036)	1,790	845
Monthly business sales at W1 survey date	-428.510 (674.895)	-208.166 (466.608)	-109.798 (492.714)	1,242.956 (769.061)	2,445	1,187
Monthly business sales at W2 survey date	24.438 (1,219.336)	1,066.353* (594.452)	1,023.739* (534.969)	1,154.469 (737.935)	2,090	993
Monthly business sales at W3 survey date	-1,180.045 (2,194.538)	706.537 (975.723)	943.842 (745.844)	1,377.236 (1,159.634)	1,711	801
Has any employees at W1 survey date	0.027 (0.023)	0.011 (0.013)	0.022 (0.016)	0.017 (0.016)	2,562	1,241
Has any employees at W2 survey date	0.021 (0.036)	0.046*** (0.016)	0.036** (0.018)	0.065*** (0.019)	2,222	1,063
Has any employees at W3 survey date	-0.034 (0.054)	0.064*** (0.022)	0.079*** (0.018)	0.064*** (0.021)	1,786	842

Notes: (1) Only participants who are not self-employed at the time of application are included. The sample used in Specifications 2-4 includes only control group observations. (2) Nearest neighbor matching compares observations in the receipt and no receipt groups based on the closest values of the set of covariates using the Mahalanobis distance. (3) Propensity score matching compares observations in the receipt and no receipt groups based on the closest values of predicted probabilities of receipt from a first-stage logit regression. (4) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (5) Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Table 10: Estimates of Entrepreneurship Training Effects on Business Outcomes - Self-Employed at Application

Dependent Variable	Entrepreneurship Training Coefficient				N	N
	Experimental	OLS	Nearest	Propensity		
	(LATE)		Neighbor	Score		
	(1)	(2)	(3)	(4)		
Sample	Full	Control	Control	Control	Full	Control
Business owner at W1 survey date	0.077 (0.086)	0.181*** (0.048)	0.227*** (0.049)	0.183*** (0.048)	640	322
Business owner at W2 survey date	0.058 (0.131)	0.182*** (0.059)	0.191*** (0.060)	0.140** (0.063)	580	290
Business owner at W3 survey date	0.122 (0.196)	0.307*** (0.070)	0.296*** (0.080)	0.285*** (0.088)	476	233
Monthly business sales at W1 survey date	-2,596.191 (2,720.277)	3,909.813** (1,791.482)	2,698.961 (2,074.084)	3,292.096 (2,383.883)	544	274
Monthly business sales at W2 survey date	-1,754.785 (2,870.949)	2,468.448* (1,275.841)	2,093.737** (1,026.019)	1,561.131* (947.648)	493	243
Monthly business sales at W3 survey date	-6,760.638 (8,326.640)	7,409.598** (2,860.896)	7,359.209*** (2,394.290)	5,916.062*** (1,524.700)	444	219
Has any employees at W1 survey date	0.127 (0.081)	0.123*** (0.043)	0.140*** (0.051)	0.113** (0.045)	639	322
Has any employees at W2 survey date	-0.015 (0.113)	0.035 (0.051)	0.093* (0.050)	0.069 (0.054)	575	287
Has any employees at W3 survey date	-0.317* (0.171)	0.064 (0.063)	0.077 (0.060)	0.132** (0.052)	471	232

Notes: (1) Only participants who are self-employed at the time of application are included. The sample used in Specifications 2-4 includes only control group observations. (2) Nearest neighbor matching compares observations in the receipt and no receipt groups based on the closest values of the set of covariates using the Mahalanobis distance. (3) Propensity score matching compares observations in the receipt and no receipt groups based on the closest values of predicted probabilities of receipt from a first-stage logit regression. (4) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (5) Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Appendix Table 1: Comparison of GATE Participants to Self-Employment Entrants

	GATE Participants	Self- Employment Entrants
	(1)	(2)
Female	46.5%	41.9%
Black	30.6%	9.2%
Latino	5.6%	14.6%
Asian	4.6%	4.9%
Other	8.0%	2.7%
Not U.S. born	10.1%	20.0%
Age	42.42	42.71
Married	48.3%	64.3%
Highest grade completed	14.45	13.14
HH Income \$25,000-49,999	33.2%	29.8%
HH Income \$50,000-74,999	17.6%	18.2%
HH Income \$75,000-99,999	7.0%	10.7%
HH Income \$100,000+	6.6%	16.1%
Unemployed (baseline/time 0)	55.3%	20.7%
Sample Size	4,197	6,086

Notes: (1) For GATE experiment participants, all reported characteristics are measured at time of application, prior to random assignment. (2) Self-employment entrants are derived from the Current Population Survey (CPS) microdata for similar years. The sample consists of individuals who are not self-employed in the initial survey month of the two-month pair, but are self-employed in the second survey month.

Appendix Table 2: Regressions for Probability of Receiving  
Entrepreneurship Training

	W1	W2	W3
	(1)	(2)	(3)
Female	0.0242 (0.0259)	0.0410 (0.0276)	0.0413 (0.0295)
Black	0.0843 (0.0365)	0.0694 (0.0403)	0.1123 (0.0446)
Latino	0.0688 (0.0628)	0.0878 (0.0643)	0.1027 (0.0642)
Asian	-0.1439 (0.0846)	-0.0679 (0.0988)	-0.0730 (0.1129)
Other	0.0072 (0.0487)	0.0260 (0.0536)	0.1006 (0.0550)
Not U.S. born	0.0706 (0.0523)	0.0498 (0.0564)	0.0719 (0.0582)
Age	0.0100 (0.0098)	0.0056 (0.0105)	0.0017 (0.0113)
Age squared	-0.0001 (0.0001)	0.0000 (0.0001)	0.0000 (0.0001)
Married	0.0202 (0.0306)	0.0308 (0.0334)	0.0485 (0.0365)
Has children	0.0267 (0.0286)	0.0154 (0.0311)	0.0327 (0.0331)
Highest grade completed	0.0213 (0.0112)	0.0393 (0.0123)	0.0446 (0.0135)
College education	0.0136 (0.0498)	-0.0304 (0.0540)	-0.0652 (0.0570)
HH Income \$25,000-49,999	0.0232 (0.0326)	0.0250 (0.0359)	0.0709 (0.0390)
HH Income \$50,000-74,999	0.0185 (0.0414)	0.0199 (0.0456)	0.0705 (0.0498)
HH Income \$75,000-99,999	0.0808 (0.0577)	0.1354 (0.0594)	0.2253 (0.0599)
HH Income \$100,000+	0.0917 (0.0565)	0.0795 (0.0603)	0.0980 (0.0651)
Wage/salary work	-0.0133 (0.0380)	-0.0089 (0.0425)	-0.0311 (0.0453)
Self-employed with no employees	0.0795 (0.0460)	0.0867 (0.0457)	0.0274 (0.0493)
Self-employed with employees	0.0118 (0.0455)	0.0315 (0.0472)	0.0014 (0.0503)
Has a health problem	0.0037 (0.0470)	-0.0063 (0.0520)	-0.0032 (0.0586)
Has relatives or friends who have been previously S.E.	0.0406 (0.0319)	0.0216 (0.0341)	0.0270 (0.0370)
Ever worked for relatives or friends who are S.E.	0.0078 (0.0306)	0.0248 (0.0329)	0.0410 (0.0348)
Has a bad credit history	-0.0395 (0.0293)	0.0277 (0.0318)	0.0369 (0.0346)
Currently receiving UI benefits	-0.0386 (0.0292)	-0.0297 (0.0312)	-0.0610 (0.0330)
Has health insurance from current employer	-0.0586 (0.0385)	-0.0900 (0.0426)	-0.0707 (0.0462)
Autonomy index	0.0066 (0.0119)	-0.0156 (0.0131)	-0.0078 (0.0139)
Risk tolerance index	0.0174 (0.0127)	0.0176 (0.0140)	0.0404 (0.0150)
Managerial experience	0.0142 (0.0279)	0.0455 (0.0299)	0.0554 (0.0327)
Treatment	0.1198 (0.3111)	0.2980 (0.3319)	0.4986 (0.3616)

(Continued)

Appendix Table 2: Continued

	(1)	(2)	(3)
Female*treatment	-0.0216 (0.0327)	-0.0557 (0.0333)	-0.0635 (0.0352)
Black*treatment	-0.0726 (0.0475)	-0.0443 (0.0500)	-0.0424 (0.0546)
Latino*treatment	-0.0673 (0.0765)	-0.0716 (0.0754)	-0.0872 (0.0770)
Asian*treatment	0.1176 (0.1081)	0.0108 (0.1202)	0.0860 (0.1342)
Other*treatment	0.0194 (0.0612)	0.0126 (0.0631)	-0.0407 (0.0629)
Not U.S. born*treatment	-0.1275 (0.0683)	-0.0941 (0.0703)	-0.1264 (0.0763)
Age*treatment	0.0090 (0.0120)	0.0069 (0.0125)	0.0040 (0.0134)
Age squared*treatment	-0.0001 (0.0001)	-0.0001 (0.0001)	-0.0001 (0.0002)
Married*treatment	0.0040 (0.0385)	-0.0086 (0.0401)	-0.0441 (0.0429)
Has children*treatment	-0.0196 (0.0358)	0.0039 (0.0369)	0.0161 (0.0390)
Highest grade completed*treatment	0.0109 (0.0144)	0.0003 (0.0150)	-0.0124 (0.0163)
College education*treatment	-0.0352 (0.0624)	-0.0327 (0.0642)	0.0356 (0.0678)
HH Income \$25,000-49,999*treatment	-0.0554 (0.0424)	-0.0262 (0.0443)	-0.0829 (0.0472)
HH Income \$50,000-74,999*treatment	-0.0216 (0.0518)	-0.0321 (0.0547)	-0.0875 (0.0581)
HH Income \$75,000-99,999*treatment	-0.0813 (0.0708)	-0.1646 (0.0720)	-0.2571 (0.0722)
HH Income \$100,000+*treatment	-0.0573 (0.0677)	-0.0680 (0.0708)	-0.0968 (0.0751)
Wage/salary work*treatment	-0.0189 (0.0482)	-0.0173 (0.0508)	0.0127 (0.0537)
Self-employed with no employees*treatment	-0.0613 (0.0557)	-0.0610 (0.0539)	-0.0432 (0.0584)
Self-employed with employees*treatment	0.0488 (0.0545)	0.0310 (0.0533)	0.0283 (0.0569)
Has a health problem*treatment	0.0285 (0.0585)	0.0052 (0.0617)	0.0059 (0.0682)
Has relatives or friends who have been previously S.E.*treatment	-0.0264 (0.0404)	-0.0083 (0.0414)	-0.0123 (0.0447)
Ever worked for relatives or friends who are S.E.*treatment	-0.0099 (0.0377)	-0.0086 (0.0386)	-0.0060 (0.0402)
Has a bad credit history*treatment	0.0457 (0.0367)	0.0010 (0.0377)	-0.0225 (0.0404)
Currently receiving UI benefits*treatment	0.0552 (0.0373)	0.0533 (0.0380)	0.0827 (0.0399)
Has health insurance from current employer*treatment	0.1029 (0.0484)	0.1439 (0.0503)	0.1085 (0.0537)
Autonomy index*treatment	-0.0046 (0.0155)	0.0166 (0.0163)	0.0137 (0.0175)
Risk tolerance index*treatment	-0.0091 (0.0164)	-0.0094 (0.0169)	-0.0325 (0.0180)
Managerial experience*treatment	0.0302 (0.0351)	-0.0100 (0.0362)	-0.0212 (0.0392)

Notes: (1) All reported characteristics are measured at time of application, prior to random assignment. (2) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application.

Appendix Table 3: Self-Reported Amount that Entrepreneurship Training Helped Recipients in Various Ways

	Very Useful	Somewhat Useful	Not Very Useful	Not at All Useful
<u>How would you rate the overall usefulness of the services you have received?</u>				
Treatment group	51.7%	33.7%	8.5%	6.1%
Control group	35.8%	40.8%	10.8%	12.7%

  

GATE Services	Treatment Group			Control Group		
	A Lot	Somewhat	Not at All	A Lot	Somewhat	Not at All
Helped with applying for loans	12.6%	21.5%	65.9%	5.9%	17.2%	76.8%
Helped with deciding whether to pursue self. em	39.5%	33.1%	27.4%	23.6%	30.0%	46.4%
Helped with refining the business idea	34.1%	37.2%	28.8%	23.0%	32.3%	44.7%
Helped with credit issues	16.4%	25.8%	57.7%	10.9%	17.3%	71.7%
Helped with developing a marketing strategy	31.4%	37.4%	31.2%	19.6%	31.6%	48.8%
Helped with legal issues	19.3%	35.5%	45.2%	11.3%	28.2%	60.6%
Helped with accounting issues	23.7%	35.9%	40.4%	12.1%	26.9%	61.0%
Helped with hiring and dealing with employees	12.7%	24.7%	62.6%	7.3%	18.1%	74.5%
Helped with networking	28.7%	37.9%	33.4%	23.1%	31.2%	45.7%
Helped with using computers and technology	13.3%	26.5%	60.2%	12.1%	22.2%	65.7%
Helped with dealing with clients	16.7%	35.1%	48.2%	11.3%	30.4%	58.3%
Helped with providing psychological support	16.6%	31.0%	52.4%	13.1%	23.8%	63.1%

Notes: (1) Sample includes treatment and control group participants who received any entrepreneurship training by wave 1 follow-up survey (6 months). (2) Evaluation of services was asked at W1.

Appendix Table 4: ITT Estimates  
Compare to Table 3

Dependent Variable	Intent-to-Treat Estimates		
	No Covars (1)	Covariates (2)	N (3)
Business owner at W1 survey date	0.0464 (0.0166)	0.0517 (0.0153)	3443
Business owner at W2 survey date	0.0216 (0.0179)	0.0208 (0.0172)	3032
Business owner at W3 survey date	0.0095 (0.0197)	0.0025 (0.0194)	2446
Monthly business sales at W1 survey date	-406 (282)	-369 (288)	3210
Monthly business sales at W2 survey date	-186 (353)	-140 (353)	2794
Monthly business sales at W3 survey date	-495 (539)	-620 (556)	2323
Has any employees at W1 survey date	0.0131 (0.0092)	0.0140 (0.0095)	3438
Has any employees at W2 survey date	0.0039 (0.0107)	0.0020 (0.0110)	3023
Has any employees at W3 survey date	-0.0172 (0.0123)	-0.0209 (0.0128)	2436

Notes: (1) The ITT model regresses the listed outcome on treatment. (2) The wave 1, wave 2 and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. (3) Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.