

# REPORT



SUPPLEMENTAL MATERIAL FOR FINAL REPORT

## **Supporting Self-Employment as a Reemployment Strategy: Impacts of a Pilot Program for Dislocated Workers After 18 Months [Appendices]**

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Heinrich Hock • Mary Anne Anderson • Robert Santillano

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200 Constitution Avenue NW  
Washington, DC 20210

Project Officer: Janet Javar

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**Submitted by:**

Mathematica Policy Research  
1100 1st Street, NE, 12th Floor  
Washington, DC 20002-4221  
Telephone: (202) 484-9220

Project Director: Heinrich Hock

Reference Number: 40247

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## CONTENTS

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APPENDIX A: DESIGN AND IMPLEMENTATION OF THE SET PILOT PROGRAM.....	A-1
1. SET program design.....	A-3
A. Design considerations based on past research and current practice.....	A-3
B. Main elements of the SET program model.....	A-6
C. Partnerships for implementing SET.....	A-8
2. SET providers: selection, support, and monitoring.....	A-9
A. Selecting providers to carry out SET.....	A-9
B. Technical assistance, provider payments, and tracking.....	A-10
3. Overview of findings from the implementation analysis.....	A-11
A. Perceptions of SET program.....	A-11
B. Implementation fidelity.....	A-12
References cited in this appendix.....	A-14
APPENDIX B: IMPACT STUDY METHODOLOGY.....	B-1
1. Study enrollment.....	B-3
2. Baseline data collected from applicants.....	B-5
3. Follow-up data from the 18-month survey.....	B-7
4. Baseline equivalence of survey respondents.....	B-12
5. Main statistical approach for estimating impacts.....	B-13
6. Sensitivity of results to alternative statistical approaches.....	B-16
References cited in this appendix.....	B-17
APPENDIX C: DESCRIPTIVE TABLES OF STUDY ENROLLEE CHARACTERISTICS AND SITE-LEVEL IMPLEMENTATION MEASURES.....	C-1
APPENDIX D: TABLES OF RESULTS FROM MAIN THE IMPACT ANALYSIS.....	D-1
APPENDIX E: TABLES OF RESULTS FROM SENSITIVITY ANALYSES FOR PRIMARY IMPACT MEASURES.....	E-1

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## TABLES

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A.1	Overview of previous DOL pilot self-employment assistance initiatives.....	A-4
B.1	Outcomes examined in the impact analysis, by topic.....	B-9
C.1	Baseline characteristics of all study enrollees, by random assignment group .....	C-3
C.2	Baseline characteristics of all study enrollees, by 18-month survey response status.....	C-6
C.3	Baseline characteristics of 18-month survey respondents (unweighted), by random assignment group.....	C-9
C.4	Baseline characteristics of 18-month survey respondents (weighted), by random assignment group.....	C-12
C.5	Select characteristics of study enrollees, by site .....	C-15
C.6	Select program intake and engagement measures, by site .....	C-16
D.1	Primary work outcomes at the time of the 18-month survey .....	D-3
D.2	Receipt of self-employment services, training, and other job placement services between study enrollment and the 18-month survey .....	D-4
D.3	Business development activity between study enrollment and the 18-month survey .....	D-6
D.4	Self-employment activity based on the 18-month survey.....	D-8
D.5	Work in wage/salary jobs based on the 18-month survey.....	D-10
D.6	Other measures of work and wellbeing based on the 18-month survey .....	D-11
D.7	Self-employment at the time of the 18-month survey, by subgroup .....	D-12
D.8	Employment in any job at the time of the 18-month survey, by subgroup .....	D-13
D.9	Total earnings in the 12 months leading up to the 18-month survey, by subgroup.....	D-14
E.1	Self-employment at the time of the 18-month survey: Sensitivity analysis.....	E-3
E.2	Employed in any job at the time of the 18-month survey: Sensitivity analysis .....	E-4
E.3	Total earnings in the 12 months leading up to the 18-month survey: Sensitivity analysis .....	E-5

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**APPENDIX A:**  
**DESIGN AND IMPLEMENTATION**  
**OF THE SET PILOT PROGRAM**

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In this appendix, we describe the design of the Self-Employment Training (SET) pilot program, present information about the local service providers who delivered the program, and summarize key findings from the evaluation's implementation analysis. This appendix is intended to complement the overview given in Chapter I of the main text of this impacts report, and it draws extensively on the previous study report on SET implementation by Amin et al. (2017).

- In Section 1, we discuss findings from a review of research and practice that informed the design of SET, describe the main elements of the SET program model, and outline the structure of partnerships established to implement it in practice.
- In Section 2, we present additional information about the microenterprise development organization providers who carried out the SET program. We then discuss how the study team supported and monitored these providers, including information about how providers recorded participation information in the study's management information system (MIS).
- In Section 3, we summarize key findings on program implementation, including perceptions of SET's usefulness and the degree of providers' fidelity to the program model.

Our discussion in this appendix focuses on the services and supports offered to eligible applicants who were randomly assigned to the study's program group in one of four sites: Chicago, Illinois; Cleveland, Ohio; Los Angeles, California; and Portland, Oregon. (Appendix B of this report contains more information about random assignment and the study's control group.) In addition, much of the implementation discussion focuses on members of the program group who actually engaged with SET services, whom we refer to as "SET participants."

## 1. SET program design

### A. Design considerations based on past research and current practice

When working with DOL to design SET, Mathematica incorporated information about (1) program elements used in previous DOL-funded pilot programs; (2) factors shown to be associated with self-employment success in the research literature on self-employment and entrepreneurship; and (3) lessons learned from the practitioner literature on microenterprise development assistance, as well as discussions with self-employment assistance experts and service providers. In this section, we discuss how the study team at Mathematica used each of these sources to develop components of the SET program.

**The structure of the SET program was informed, in part, by the packages of services and supports that were found to be effective for dislocated workers through previous DOL pilot tests.** The four pilot self-employment programs that Mathematica considered were the Self-Employment and Enterprise Development (SEED) project; the Massachusetts Enterprise Project (MEP); and the first- and second-generation Project Growing America Through Entrepreneurship studies (GATE) I and II. As indicated in the Appendix Table A.1, the SEED project served UI recipients, MEP focused on UI recipients who were likely to claim all of their benefits, and GATE II was open to various subgroups of dislocated workers. Although anyone interested in starting a business could enroll in the GATE I program, a substantial share of enrollees were unemployed, and this group appears to have particularly benefited from access to the program (Michaelides and Benus 2015).

**Table A.1. Overview of previous DOL pilot self-employment assistance initiatives**

	Self-Employment and Enterprise Development (SEED) Project	Massachusetts Enterprise Project (MEP)	Project Growing America Through Entrepreneurship (GATE I)	Project Growing America Through Entrepreneurship (GATE II)
<b>Implementation context and program targeting</b>				
Period of operations	1989–1991	1990–1993	2003–2005	2009–2011
Location of service sites	Washington State (6 sites)	Massachusetts (8 sites)	Maine (3 sites), Minnesota (2 sites), and Pennsylvania (2 sites)	Statewide in Alabama, Minnesota, and North Carolina; regional in Virginia
Eligible population <sup>a</sup>	Unemployment insurance (UI) claimants	UI claimants profiled as likely to exhaust regular UI benefits with more than 26 weeks of UI remaining	Anyone who wished to create, sustain, or expand a business that was legitimate and appropriate	In Alabama and North Carolina, rural dislocated workers; in Minnesota and Virginia, older dislocated workers (at least 45 years old)
<b>Program features</b>				
Business development services	Classroom training, business plan assistance, counseling, and peer support groups	Enterprise seminar, biweekly workshops, and counseling sessions	Assessment, classroom training, and counseling	Business readiness assessment, one-on-one counseling, and classroom training
Financial assistance	Self-employment allowance payments equal to weekly benefit amounts and a lump-sum payment equal to remaining UI entitlement for those meeting program milestones	Self-employment allowance payments equal to weekly benefit amounts	None	None
Work-search waiver	Yes	Yes	Yes, in 1 out of 3 states	Yes
Length of service receipt	No maximum specified, but average program participation was 7–8 weeks	Up to 12 weeks	No maximum specified, but average program participation was 4 months	No information available

Sources: Benus et al. (1995) for SEED project and MEP; Benus et al. (2008, 2009) for GATE I; and Davis et al. (2013) for GATE II.

<sup>a</sup>All demonstrations restricted the program to anyone 18 years of age or older who was lawfully able to work in the United States and resided in the service areas of the study site(s). The SEED project excluded UI claimants who were filing interstate claims or were employer attached. The MEP excluded UI claimants who were filing interstate claims, employer attached, full-referral union members, or filing claims backdated 14 days or more.

As indicated in Figure I.1 of the main text of this report, results from evaluations of these programs have found that self-employment assistance led to greater entry into self-employment across a range of time periods, target populations, and program models. In addition, these programs have in some cases—but not universally—produced sustained impacts on self-employment that were still apparent 1.5 to 3 years after study enrollment (Amin et al. 2017). Further, although only the MEP program increased participants' earnings, none of these programs had negative impacts on the earnings for those who were unemployed at enrollment (Benus et al. 1995; Michaelides and Benus 2015; Davis et al. 2013).

Although the implementation contexts of these programs all differed, they included common elements that helped form the basis for the design of SET (along with other research and feedback from practitioners). For example, participants in these previous programs could receive business development counseling, a service component that Mathematica intensified under the SET model. Previous program participants also accessed business development classes and training; similarly, Mathematica made available free business development classes and training through its SET providers. SEED and MEP offered their participants continued access to UI benefits, contingent on meeting certain program milestones. Likewise, Mathematica sought waivers from state UI agencies so that participants eligible for UI could continue receiving benefits while working full time on their businesses, rather than having to meet work search requirements.

**The design of SET also incorporated information from a scan of non-experimental and survey research about the factors that might mitigate the risks associated with self-employment.** As discussed in Chapter I, a large share of small businesses fail within five years. These failures may come with a considerable financial and psychological toll—particularly for those who have less experience with entrepreneurship (Ucbasaran et al. 2013). Therefore, Mathematica reviewed the research literature to identify factors that have been shown to be correlated with entry into and success in self-employment and that could also be used to inform program design. The study team identified two key factors that meet these criteria.

First, several research studies point to a correlation between **specific experience or knowledge in a field** and success in starting a new business in that field. For example, experience within an industry or in an occupation may lead to longer self-employment (van Praag 2003), and industry-specific experience may be a major determinant of small business success (Loscocco et al. 1991). Whether a small business's founder has prior experience in an industry may also substantially improve the business's prospects for survival, profitability, sales, and growth (Bosma et al. 2004; Delmar and Shane 2006; Harada 2003). Therefore, as discussed in Chapter I of the main text of this report, SET eligibility was limited to people who were pursuing a business in a field in which they had expertise or experience.

Second, having **access to financial capital** has also been correlated to success in starting a new business. For example, having access to financial assets may influence the transition to self-employment (Dunn and Holtz-Eakin 2000), and may predict improved personal well-being, which has been correlated with business starts, increased income, and small business job creation (Schmidt and Kolodinsky 2006). Based on these findings, Mathematica and DOL considered options for increasing SET participants' access to funds that could be used to start or grow their businesses. Given the risks inherent in self-employment, this discussion focused on grants rather than loans.

**The SET program model included key supports informed by guidance and lessons learned from experts and frontline staff.** Mathematica scanned the practitioner literature on microenterprise assistance programs, popular books on starting businesses, and discussions with workforce staff and microenterprise service provider staff. Input from these sources led the study team to emphasize the following features when designing the SET model for dislocated workers:

- **Intensive one-on-one assistance and customized service plans.** Dislocated workers pursuing self-employment may face challenges (and needs) that differ from those of other aspiring entrepreneurs typically served by microenterprise service providers. Job displacement has been associated with declines in workers' physical and psychological well-being (Brand 2015), which suggests that dislocated workers pursuing self-employment may benefit from sustained one-on-one assistance, encouragement, and support. This type of support may include customized service planning to help them better gain mastery of the range of topics required for running a business, which could be an overwhelming endeavor for individuals also grappling with the shock of job loss. However, existing self-employment infrastructure (including Small Business Development Centers, Women's Business Centers, and other microenterprise service providers) do not typically provide this level of support to newer entrepreneurs. Generally, providers offer no more than a few hours of one-on-one services per client (due to staffing and resource constraints), and they typically reserve those services for clients running businesses that have already reached key milestones (such as completing a business plan or beginning operations). Therefore, the SET program sought to provide dislocated workers with a level of individualized attention and guidance not typically available to nascent entrepreneurs.
- **Microgrants as a source of start-up capital.** Dislocated workers may face considerable challenges with their finances. Our discussions with microenterprise service providers serving unemployed workers suggested that these individuals were struggling to make even relatively modest investments in their businesses. Furthermore, some of these customers had poor credit that made it difficult for them to qualify for loans. According to microenterprise service literature and experts, microgrants—which would allow customers to access capital without having to go into debt—were not readily available and typical microloans were sizable and required that borrowers have good credit and collateral. Hence, to help participants make modest investments in their businesses without having to take on loans or debt, DOL offered microgrants as a benefit of the SET program.

## **B. Main elements of the SET program model**

Individualized assistance and microgrants were part of a broader, integrated package of services and supports offered to SET participants. In this subsection, we describe the features of the SET program model in more detail. Chapter I of the main text of this report and the previous subsection of this appendix contain additional information about how the SET program targeted dislocated workers who were pursuing businesses in a field in which they had experience or expertise.

**Timely access to program services to promote engagement.** Program intake was designed to facilitate speedy access to SET services with the goal of reducing drop-offs in interest that could happen after a longer wait. Therefore, as discussed in greater detail in Appendix B of this report, Mathematica

developed a process to screen applications within two business days. In addition, after randomly assigning eligible applicants, the study team matched program group members to a local microenterprise provider ("SET provider"), usually on the basis of geographic proximity and each provider's capacity to serve new participants at the time. SET providers were expected to begin delivering services with two weeks after random assignment.

**Case management to provide personalized and ongoing support.** In addition to providing timely access, SET providers were tasked with providing business development assistance that was (1) sustained, with follow-ups provided on at least a monthly basis for up to a full year, and (2) customized, with tailored supports based on a careful assessment of the participant's business development needs and ongoing progress. SET providers were asked to assign each participant to an experienced business development consultant, called a SET advisor, who would serve as the participant's main point of contact and be responsible for his or her progress through the program. SET advisors were responsible for the following types of engagement with participants:

- In-person intake meetings within two weeks of program acceptance. During the intake meeting, the SET advisors provided an overview of the services participants could receive; sought to understand their business idea, stage of business development, and support needs; and worked with participants to devise a service plan of training and technical assistance.
- Monthly follow-ups by telephone or in person, to learn about progress, identify new business development needs, and provide additional assistance.
- Quarterly reassessments to provide a more comprehensive review of progress since intake, reevaluate the participant's needs, and update the service plan, as needed.

In addition, SET advisors were asked to refer participants to American Job Centers (AJCs) for other job search assistance, including training and employment services, if the advisor and participant concluded that self-employment was not a good fit.

**A customized training and technical assistance plan to build knowledge about specific business development topics.** Based on their case management interactions, SET advisors were expected to connect participants to training, technical assistance, coaching, and other business development supports. The SET provider organizations were also expected to offer participants access to free or reduced-cost business development services, such as training and technical assistance, for up to one year. Since SET did not offer a standardized curriculum, the SET providers recommended training and technical assistance that matched each participant's needs based on their available offerings.

**Access to \$1,000 in SET seed capital microgrants to defray start-up costs.** SET participants could receive these microgrants if they met the following eligibility requirements:

- Registering their business, completing a business plan, and engaging satisfactorily with the program (as determined by their SET advisors).

- Using the microgrant funds only for business start-up expenses—for example, buying inventory, equipment, or software; or investing in a website or marketing materials. Participants could not use the funds for ongoing costs (such as salaries or rent) or personal expenses.

**Access to work search waivers, if available, to allow UI recipients to focus on business development.** In two of the four study sites, state UI offices agreed to offer work search waivers to SET participants receiving UI benefits. These waivers allowed participants to continue receiving UI benefits while working full time on their business (instead of searching for work). In Portland, SET participants could obtain waivers through Oregon’s Self-Employment Assistance (SEA) program, and members of the control group could also obtain waivers by meeting the requirements of the Oregon SEA program (such as completing a business plan). In Cleveland, the waivers were available to SET participants only, as Ohio did not have an SEA program.

### C. Partnerships for implementing SET

Making the SET model operational meant developing an implementation strategy that specified complementary but separate roles for workforce and UI system staff, the study team, and SET providers. We describe these partnerships in this subsection, and the following section contains a more detailed discussion of the roles played by SET providers, who were instrumental in delivering services and supports.

- **Local workforce, state employment services, and UI staff all promoted SET.** Mathematica partnered with these entities for outreach because dislocated workers typically interact with them. In planning the role that these entities would play in SET, the study team was cognizant of the need to minimize burden on these partners. At the time of implementation, high unemployment combined with shrinking workforce budgets meant that workforce agencies had to serve more clients with fewer resources than in the past. Therefore, the role of workforce and UI system partners was mainly to market the program and conduct mass outreach. Local workforce staff put up program posters and distributed brochures, allowed applicants to use AJC computer resource rooms when applying to SET, and directed applicants with questions to the SET helpline and email (monitored by the study team). Depending on their capacity, state UI or employment services partners conducted robocalls, email blasts, or mailings to promote SET. Partners in Cleveland and Portland were able to quickly leverage existing mass outreach. This took longer to establish in Chicago, but eventually resulted in the largest recruitment flow of any site. Partners in Los Angeles, however, did not have the capacity to engage in mass outreach.
- **The study team supported outreach efforts and facilitated centralized application processing.** Mathematica provided training, outreach materials, technical assistance, and a modest amount of financial support to local partners that conducted outreach. To minimize burden on workforce partners, Mathematica also designed and hosted an online orientation and application system, received and processed applications, determined eligibility, conducted random assignment, and assigned SET participants to providers in each site. The online procedures allowed applicants to choose where and when they accessed the orientation and application. It also enabled the study team to share consistent information about SET across sites. Centralized eligibility determinations allowed



for consistent screening of applications across sites. Appendix B includes more information about how the study team applied the SET eligibility criteria to screen applications.

- **Providers delivered services and supports to SET participants.** Because the SET program model emphasized sustained, tailored assistance, it was expected to require substantial effort to deliver. Conducting a true test of the model required partnering with high-capacity providers whose staffing structure and service approach aligned well with the program’s objectives. The next section describes how the Mathematica study team vetted, supported, and monitored potential providers.

## 2. SET providers: selection, support, and monitoring

### A. Selecting providers to carry out SET

Mathematica sought to enlist microenterprise service providers that could implement the intensive and personalized assistance specified in the SET program model. Providers were identified and prescreened through internet research about organizational characteristics and site visits. Those that seemed promising were invited to submit written applications in response to a detailed request for proposals. The study team carefully reviewed the submitted applications and rated them based on how well providers demonstrated (1) a track record of offering a wide range of self-employment supports and serving clients in the early stages of establishing a business, (2) an understanding of the SET model and capacity for implementing it, and (3) sufficient staffing to serve a substantial volume of additional clients.

Mathematica ultimately selected 11 providers in the four study sites to carry out the SET program.<sup>1</sup> Eight of the selected providers received Small Business Administration funding, either as Small Business Development Centers (SBDCs) or Women’s Business Centers (WBCs). The other three providers were community-based organizations (CBOs), or nonprofits that received most of their funding from nonfederal sources. As noted in the GATE I final evaluation report, SBDCs and CBOs differed in several fundamental ways (Benus et al. 2009). SBDCs generally supported economic development, whereas CBOs tended to focus on workforce development and helping people become self-sufficient. The study team’s discussions with staff at both SBDCs and CBOs, including several organizations that did not serve as partner providers, suggested that the two types of organizations also served different clientele. For example, compared to CBOs, SBDCs tended to serve clients who were further along in starting or planning their businesses.

Most of the SET providers had historically expected their clients to be past the start-up phase before the provider would deliver intensive one-on-one technical assistance. Consequently, delivering SET required them to adapt their existing model to also serve clients who were in the early stages of starting a business. In addition, most providers had to learn how to deliver the structured follow-ups and reassessments specified by the SET model and integrate these case management features into their existing service structure.

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<sup>1</sup> Ten providers in the four sites were judged to have submitted strong proposals and were invited to serve as partner providers. One organization with a similarly strong capacity did not submit a proposal but was recruited to participate by the study team.

## B. Technical assistance, provider payments, and tracking

Mathematica provided ongoing technical assistance and oversight to help providers implement the SET model as it was designed. The study team documented program procedures in a detailed operational manual (Amin et al. 2013) and delivered full-day, in-person training sessions in each of the program sites to staff from all provider partners. The study team also designed and made available an online MIS used for referring participants to providers, tracking participants' progress, and processing seed capital microgrant applications. Providers were responsible for tracking participants' progress and engagement in the program on at least a monthly basis in the MIS. The study team monitored the MIS regularly (as discussed below) and also held monthly check-in telephone calls with providers to oversee implementation and provide guidance and assistance as needed. The study team also visited each site at least once to review participants' records and explore providers' understanding and implementation of the model.

To further promote program fidelity, providers were compensated according to a payment schedule that included performance-based provisions (see Box A.1). Mathematica offered staged payments to encourage monthly reporting on participants' engagement with the program and on services received; payments were also tied to performance and the timely provision of monitoring data. These payments could total up to \$825 per participant, although providers ended up receiving an average of \$522 per participant because of variation in the extent to which the SET group engaged with the program (as indicated in Chapter III of the main report and Appendix Table C.6). Providers were also expected to leverage their existing programs and funding to cover at least some of the business development services for SET participants. When asked about the adequacy of the payment scheme, most providers reported that they deemed the compensation insufficient and recommended payments of \$1,000 to \$4,500 per participant.

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### Box A.1. Compensation schedule for SET provider

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Providers received a mix of upfront and pay-for-performance payments. For each SET participant who was matched to them, providers could receive:

- An **initial commitment payment** of \$100 for each participant they agreed to serve during the program's full implementation period; providers agreed to serve 50 to 300 referred SET participants.
- An **intake payment** of \$400 for each referred participant for whom they completed intake; this payment was intended to cover the costs of the initial assessment, service planning, and service delivery.
- Up to **three ongoing engagement payments** of \$75 per participant for conducting quarterly reassessments and delivering services in each month of the preceding quarter.
- A **milestone payment** of \$100 for each participant who completed a satisfactory business plan.
- A termination payment of \$25 for each participant who left the program early, to encourage providers to formally close out participants who were no longer actively engaging with the program.

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To facilitate the monthly payments, providers were expected to track participants' progress in the study's MIS on a monthly basis. Providers recorded details, including dates, about intake meetings, monthly follow-up meetings, and quarterly reassessments with each participant; they also recorded the dates when participants reached business development milestones, including business plan completion. In addition, seed capital microgrant applications were documented in the MIS.



### 3. Overview of findings from the implementation analysis

The SET evaluation's implementation report by Amin et al. (2017) synthesized information from interviews with provider staff and participants and MIS data to assess the experiences of providers and participants. In this section, we provide a summary of the main findings from that report, focusing on the perceived usefulness of the SET program and the fidelity of providers' implementation of the program model.

#### A. Perceptions of SET program

To assess providers' and participants' perceptions of the SET program, the implementation study drew on several sources of data, including a provider survey, site visit and telephone interviews with provider staff, and telephone interviews with a purposefully selected sample of SET participants. In this section, we summarize findings about how these two groups regarded their experiences with the SET program.

**Providers and participants indicated that core elements of the SET model were generally beneficial to participants.** All providers agreed that the in-person intake was very beneficial for participants and most thought that technical assistance and seed capital microgrants were very beneficial. Provider opinions about ongoing case management were more mixed, with slightly over half rating monthly follow-ups to be very beneficial and slightly fewer than half rating quarterly reassessments as very beneficial. Nonetheless, most providers thought these program elements were at least moderately beneficial. In addition, the study team noted that some providers that had been initially skeptical of monthly follow-ups and quarterly assessments began to view these features of case management more positively toward the end of SET operations.

In addition, most SET participants who were interviewed found the program to be useful, and a majority found it to be instrumental in helping them make progress in developing their businesses. The benefits they most commonly noted were the one-on-one support and assistance from a business advisor and help with writing a business plan. Other benefits that participants noted included gaining knowledge of how to start and run a business, the seed capital microgrant, and networking and support from other participants through classes and training.

**However, some providers thought that the program as a whole benefited fewer than half of the participants they served.** Staff from most providers estimated that at least half of the participants assigned to them benefited from the program, but a sizable number (staff from three providers) also indicated that this was true for fewer than half of their assigned participants. These different perspectives might have been partly related to providers' perceptions about participants' engagement. Staff from three providers estimated that the minority of the participants they served had been fully engaged with the program. (These providers included two of the three providers whose staff thought that less than half of their assigned participants had benefited from SET.) In addition, all providers thought fewer than 75 percent of their participants had been fully engaged. When asked about what challenges participants faced in engaging with the program, the main reasons providers cited were participants' personal challenges (such as health, child care or other family commitments, or lack of housing), insufficient commitment, wanting to find a wage or salary job, and only being interested in accessing the seed capital microgrants.

**Providers also highlighted the challenges participants faced in qualifying for seed capital microgrants.** Approximately 36 percent of the program group overall and 42 percent of those program group members who completed an intake received seed capital microgrants. To qualify for these microgrants, participants had to register a business and complete a business plan. Based on conversations with non-SET microenterprise service providers, the study team established these milestones when designing SET because they signaled a serious commitment to starting a business. However, during SET implementation, staff from multiple providers said participants—even those who were engaged in the program—found it difficult to complete their business plan, one of two prerequisites for becoming eligible for the microgrant. Staff at several providers identified the financial projections section of the plan as being particularly difficult for participants to complete, because they did not understand how to realistically estimate their expected revenue and expenses.

**Participants identified additional areas for improvement.** Among interviewed participants, the two most commonly reported weaknesses were that the amount of the seed capital microgrant was insufficient and that the resources provided by the service providers were lacking or generic. In terms of specific weaknesses of services, participants cited insufficient access to networking opportunities and tools, services being suitable only for new entrepreneurs or being too advanced, and insufficient help with developing business plans.

## **B. Implementation fidelity**

To assess whether SET providers maintained fidelity to the program model, the implementation study drew on MIS data to determine whether (1) intake happened, (2) intake was timely, (3) two-thirds of monthly follow-up meetings occurred on time, and (4) all quarterly reassessments were conducted when due. For each of these indicators, the study team assigned a ranking of low, medium, or high fidelity (scored 1, 2, or 3), which were then summed to determine provider-level fidelity scores. These findings on fidelity could have implications for understanding SET's impacts since they indicate variation in the extent to which the program model was implemented as intended.

**Overall, implementing case management proved feasible.** On aggregate, there was medium fidelity to the case management model among all providers. Fidelity was highest for the indicators related to intake and lowest for those related to quarterly reassessments. However, fidelity scores varied across providers, over time, and across different elements of the SET case management model, as discussed below.

**Providers were generally successful in conducting in-person intake meetings.** Providers were expected to conduct an intake meeting with new participants within two weeks of their assignment to the program. Across all providers, 85 percent of SET participants in our final study sample received an in-person intake meeting. Seven of 11 providers showed high fidelity by conducting intake for over 90 percent of assigned participants. The remaining four providers demonstrated medium fidelity by conducting intake for 75 to 90 percent of assigned participants. Although fewer than half of the providers conducted intake within the required two weeks, most conducted it within three weeks.

**There was notable variability in the extent to which providers implemented ongoing case management services:**

- **About half of the providers completed most expected monthly follow-ups on time, but some providers conducted few of these follow-ups.** At least once a month, SET advisors were expected to check in with each active participant over the telephone, in person, or by email. Overall, for SET participants who received an intake meeting, 63 percent received timely monthly follow-up meetings (defined as conducting monthly meetings on time with more than two-thirds of active participants). Five of the 11 SET providers demonstrated high fidelity. Another three providers conducted timely monthly follow-ups with at least 40 percent of their active participants, but the remaining three providers met this standard for 10 percent or fewer of their active participants.
- **There was a wide range of variation in the extent to which providers conducted quarterly reassessments, and no provider did so with high fidelity.** Every quarter, SET advisors were expected to hold an in-person meeting with each participant who was still active in the program. Only 21 percent of SET participants received all of the expected quarterly reassessments, given their tenure in the program. In addition, the percentage of participants who received the expected number of assessments varied widely across providers. No provider completed the expected number of quarterly reassessments with high fidelity (that is, conducted them for at least half of their active participants). Four providers conducted all expected quarterly reassessments with 30 to 50 percent of participants, four providers conducted them with 10 to 21 percent of participants, and three providers did not conduct any expected reassessments with their active SET participants.

These findings might be partly due to providers' experiences with and perceptions of case management, participant disengagement, or the resources available for program delivery. As noted previously, most providers were new to the structured follow-ups and reassessments specified by SET, and several providers were skeptical about them (particularly at the start of SET operations). Providers also thought that a sizeable number of SET participants were not fully engaged with SET, which might have limited the capacity for ongoing case management interactions. In addition, providers generally thought that the compensation available for the SET program was not sufficient, and they indicated that existing funding from other sources could not support extensive one-on-one services.

At the same time, the MIS data might contain inconsistent or incomplete information because staff from most providers found it challenging to enter information in the MIS to track participants. SET advisors at the majority of providers created alternative methods to track participants' information for SET service provision and reporting purposes. Several staff also noted that they did duplicate data entry, entering information into their organization's own tracking system and into the MIS, but it is not clear that this was done consistently or universally. Nonetheless, the study team's assessment was that the potential MIS data coverages issues could also be a signal of implementation challenges.

**Although not part of the implementation study's fidelity assessment, additional analyses also revealed notable differences across providers in disbursement of SET seed capital microgrants.**

As documented in the implementation report, these microgrants were received by fewer than one-third of the participants assigned to four providers, between one-third and two-thirds of the participants assigned

to another six providers, and more than two-thirds of the participants assigned to one provider. SET providers were not asked to estimate the extent to which their participants appeared qualified for microgrants, but the implementation analysis found a similar amount of provider-level variation in the share of participants who registered their businesses or completed business plans—two steps required for microgrant eligibility.

**These provider-level differences appear to have translated into some meaningful site-level differences in the extent to which the program group became or remained engaged with SET.**

For example, an analysis of MIS data suggests that important components of the SET program model were underused in Chicago and Los Angeles (Appendix Table C.6 of this report). This could be partly due to differences in provider fidelity across sites, which might in turn have been the result of cross-site differences in the flow of recruitment. For example, Chicago providers sometimes were inundated with program group members, whereas study intake in Los Angeles likely did not reach the critical mass needed for providers to dedicate staff time to SET. However, staffing issues could also have translated into recordkeeping challenges that might explain some of the apparent differences in implementation measures across sites. See Chapter III of this report for additional discussion.

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**APPENDIX B:**  
**IMPACT STUDY METHODOLOGY**

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In this appendix, we provide details about our approach to evaluating the impacts of the Self-Employment Training (SET) pilot program, which the U.S. Department of Labor (DOL) commissioned. The information in this appendix builds on the overview in Chapter II of the main text of the report.

- In Section 1, we discuss our process for enrolling applicants into the study by screening applications and conducting random assignment. The main text of this report (Chapter II) and a previous study report by Amin et al. (2017) include additional details about how we worked with DOL to select study sites and chose the microenterprise service providers that served the SET program group.
- In Section 2, we describe the baseline data we collected from SET applicants and how we prepared these data to use in the analysis.
- In Section 3, we discuss the follow-up survey that we fielded to learn about study enrollees' outcomes about 18 months after enrollment. This discussion provides information about how we fielded the survey and the response rates we obtained, how we selected and prepared outcomes data for the impact analysis, and how we developed weights to account for nonresponse.
- In Section 4, we describe our assessment of the degree to which survey respondents in the SET program and control groups tended to have equivalent baseline characteristics, both before and after weighting.
- In Section 5, we provide information about our main statistical approach for estimating impacts—overall, by site, and by subgroup.
- In Section 6, we describe a series of checks we conducted to determine whether our primary impact estimates would have differed substantively if we had chosen alternative statistical modeling options. We also summarize results from these sensitivity checks, which indicate that all modeling options would lead to the same basic conclusions about SET's effectiveness.

## **1. Study enrollment**

Mathematica designed the evaluation to estimate the impacts of the SET pilot program for dislocated workers who were interested in self-employment and could demonstrate experience or expertise to support their business ideas. As discussed in Chapter II of the main text of this report, the study team at Mathematica collaborated with workforce development and UI system partners to identify potential study enrollees and provide them with information about the study. Interested individuals could submit applications through an online system from July 2013 through January 2016.

In this section, we provide more information about the study eligibility criteria and how we used them to screen applications. We then describe Mathematica's procedures for randomly assigning eligible applicants; this includes a discussion of additional steps to communicate with the SET program and control groups and the service options available to each group.

### **Applying study eligibility criteria to screen applications**

The SET pilot program was designed for dislocated workers who wanted to start a business in a field in which they had they had experience or expertise. The study team sought to engage with potential study

enrollees quickly to avoid a drop-off in interest—particularly among those who would be assigned to SET providers.

Accordingly, Mathematica screened applications within two business days of receiving them based on the following criteria:

- **Living in one of the four SET study sites.** To be eligible, applicants had to reside in one of the areas where identified workforce partners and microenterprise services whose approaches and resources enabled them to promote and implement the program model. As discussed in Chapter II of the main text, these four areas were (1) Chicago, Illinois (city of Chicago and Cook County); (2) Cleveland, Ohio (Cuyahoga and Lorain counties); (3) Los Angeles, California (Los Angeles City and Los Angeles County); and (4) Portland, Oregon (Multnomah and Washington counties).
- **Dislocated worker/unemployed or underemployed.** Mathematica initially developed an automated screener to identify workers who met one of the dislocated worker definitions specified by federal legislation—see Section I.A of the main text. This screener accepted only unemployed applicants who stated that they were unlikely to find a job in their traditional field, and it used a complex series of questions to identify different categories of dislocated workers. About 18 months into the study enrollment period, the study team found that more than one-third of the applicants were ineligible, and they frequently called to request a chance to reapply because that they had misunderstood questions in the screener or had been too optimistic about their job prospects. Mathematica updated the screener at that point to remove the question about applicants' expected chances of finding a job in their traditional field and to simplify the questionnaire logic. The revised screener also sought to more simply identify workers who had lost jobs through no fault of their own and whose search efforts had not resulted in reemployment in a job that paid a comparable salary.
- **Legal business idea.** Study outreach materials instructed potential study enrollees to submit applications only if they had business ideas that were legal, ethical, and moral. In practice, the study team screened out business that were likely illegal, forwarding cases to DOL for review if there was any ambiguity.
- **Demonstrated link between business idea and previous experience or expertise.** As discussed in Chapter I of the main text, the SET program targeted aspiring business owners who had practical knowledge about the products or services they planned to offer. Their experience or expertise could be based on past self-employment activity, professional experience in a wage/salary job, education and training, or an informal hobby from which they had earned money. Study team members assessed whether applicants met this criterion based on narrative responses to an application question requesting information about experience and expertise related to the proposed business idea.

As discussed in Chapter II of the main text, Mathematica screened 2,470 applications, and identified 1,981 eligible applicants. The study team notified applicants of the eligibility decision immediately by e-mail.

## Conducting random assignment of eligible applicants

After screening applications, Mathematica randomly assigned all eligible applicants to either the SET program group or a control group. The study team conducted random assignment separately within each study site. The application processing system also included screens to make sure that each individual was randomly assigned only once. Both the program and control groups received a notification about their random assignment outcome within one week.

The 991 people assigned to the **SET program group** were then matched to a local microenterprise service provider, usually on the basis of geographic proximity and each provider's capacity to serve new participants at the time of random assignment. Those who made satisfactory progress and maintained contact with the provider had access to all of the services and supports described in Chapter I of the main text. As noted in Chapter III and Appendix Table C.6, only 84 percent of the program group actually had any contact with a SET provider. (This share was particularly low in Chicago, likely because of a large increase in the volume of applicants partway through the study enrollment period [Amin et al. 2017].) Nonetheless, as discussed later in this appendix, the impact analysis framework calculated average outcomes for all who were assigned to the SET program group, regardless of whether they actually participated in SET.<sup>2</sup>

The 990 people assigned to the **control group** were not eligible for the specific services and supports offered through the SET program, but they could obtain other self-employment assistance from providers in their communities. The study team asked that the microenterprise service provider partners offer the SET model of case management services and intensive follow-ups only for those in the program group, and they received funds only for serving the program group. In addition, members of the control group were also referred to a local American Job Center (AJC) for potential career counseling, job search assistance, and job training. However, members of the control group could seek self-employment advice, mentoring, technical assistance, and training, including services similar to what was offered through SET. Some of them might also have engaged with SET service providers, although the study team did not have the capacity to track this activity.

## 2. Baseline data collected from applicants

As noted in Chapter II, study application forms included a questionnaire asking for information about applicants' demographic and socioeconomic characteristics, work experience, employment status, and motivations for pursuing self-employment. These forms also requested information about specific qualifications of applicants that could support their proposed business ideas. In a previous study report, Amin et al. (2017) discuss these baseline data elements in greater detail and use them to provide a comprehensive description of study enrollees.

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<sup>2</sup> In one case, the provider was not notified of the match due to a technological error, so the potential participant did not receive any services; this case was also considered part of the SET program group for the purposes of the impact analysis.

In the rest of this section, we describe how we used baseline data in the impact analysis. We then discuss our rationales for selecting specific baseline characteristics and for recoding some of the corresponding variables. Finally, we explain our approach to addressing missing values in the baseline data.

### Using baseline data in the impact analysis

As discussed later in this appendix, we used data on baseline characteristics for the impact analysis in four distinct ways:

1. We assessed the extent to which the SET program group and control group members tended to have **equivalent or different characteristics**, particularly among those who responded to the 18-month survey.
2. We created **survey weights** to account for potential differences in response rates among types of study enrollees within the program and control groups.
3. We used **regression adjustment** to account for any potential differences in characteristics between survey respondents in the program and control groups, after applying survey weights, and to improve the precision of the impact estimates.
4. We identified **subgroups** to assess whether the effectiveness of SET varied across different types of enrollees.

Appendix Table C.1 lists the full set of variables defined from baseline characteristics that we used in any of these components of the impact analysis, and it presents means for the program and control groups.

### Selecting and coding of baseline variables

We selected the specific characteristics listed in Appendix Table C.1 to measure several broad domains that have been shown to be related to self-employment success:

- Demographics and relationships (Fairlie and Robb 2008; Hundley 2000; Zissimopoulos and Karoly 2007)
- Education, work experience, and employment status (Bosma et al. 2004; Lee and Tsang 2001; Thurik et al. 2008; van der Sluis et al. 2005)
- Income and finances (Evans and Jovanovic 1989; Harding and Rosenthal 2017)
- Personality traits (Caliendo et al. 2014; Fairlie and Holleran 2012)

As indicated in the table, we created parsimonious categorical analysis measures for several characteristics that were originally continuous or took a large number of mutually exclusive values. For example, we organized age into four groups, and we collapsed 11 educational attainment values into four analysis categories. Our motivating principle was to improve the expected accuracy of inference from our statistical models (described later in this appendix) by reducing the potential influence of outlier values or characteristic-based groups with relatively small numbers of study enrollees.

## Addressing missing values in the baseline variables

Study applicants were not required to answer every question on the study intake questionnaire, but blank or incomplete answers were relatively rare. All eligible applicants provided valid responses to the questions underlying most of the baseline variables listed in Appendix Table C.1. Missing-value rates for the remaining variables were generally less than 0.5 percent and never exceeded 1.3 percent.

For almost all of the evaluation's descriptive analyses, we calculated summary measures for each baseline variable based only on study enrollees who provided the information needed to create that variable. As we explain in Section 3, the one exception was our final assessment of the baseline characteristics of 18-month survey respondents, after accounting for nonresponse.

For the impact analysis, however, we used an imputation procedure to fill in missing values for the baseline variables.

- We did this because our impact analysis framework used regression methods to adjust for a select set of baseline variables. Without imputation, the analysis would exclude about 2.5 percent of cases with a missing value for any baseline variable. In contrast, using imputation to fill in missing values enabled us to preserve as many cases as possible for the analysis, which aligns with one of our main goals of adjusting for baseline variables in the first place—improving the precision of the impact analysis.
- The specific method we used to impute missing values preserved the original distribution of nonmissing values across baseline variables. We specifically used a stochastic, chained-equation multivariate imputation algorithm, implemented using the “mi impute” command in Stata.
  - We ran this imputation procedure separately for the SET program and control groups to avoid diminishing any existing chance differences between groups after randomization.
  - We included in the imputation model all of the variables listed in Appendix Table C.1, converting household income into a categorical measure for this procedure to reduce the potential influence of outliers.
  - We specified variable-specific distribution types—for example, logistic for binary variables, multivariate logistic for non-ordinal categorical variables, and ordered logistic for ordinal categorical variable. The last group included continuous variables that had been converted to categorical measures, as described earlier. In addition, we specified an option to augment these logistic models to avoid issues that arise from perfect model prediction.
  - Given the relatively small rates of missing values, we used a single draw from this stochastic algorithm—chosen using random-number seed—to populate missing values.

We used imputed values of the baseline variables as covariates in our regression models, as already noted, and when developing nonresponse weights. However, we excluded missing values when defining the analysis subgroups described in Chapter II of the main text to avoid the potential for attenuating any differences across groups.

### 3. Follow-up data from the 18-month survey

Mathematica surveyed study enrollees about 18-months after they were randomly assigned to collect data on how they fared. In this section, we discuss the fielding of the survey, including a discussion of response

rates. We then describe the specific outcomes we derived from the survey for the impact analysis. Finally, we describe our process for developing the nonresponse weights used in the analysis.

### Fielding the survey

Mathematica asked all study enrollees—in both the program and control groups—to complete an online survey, timing our initial follow-up contact to be about 18 months after each of them was randomly assigned. To efficiently manage the survey, the study team grouped enrollees into “releases” that occurred on a rolling basis starting in April 2015. Subsequent releases were typically spaced 2 to 3 months apart and continued until 18 months after the last study enrollees were randomly assigned. The study team initiated contact with members of each release through a letter and an email to remind them about the survey, which was described in the study application materials, and provided a link to the survey website. This initial contact was followed by a structured, timed series of additional attempts to contact (via emails, postcards, and telephone calls) nonrespondents, asking them to complete the survey.<sup>3</sup> The study team accepted responses from enrollees in all releases through November 2017, when the survey website closed.

The survey took about 20 minutes to complete, and the study team offered incentive payments to respondents to promote timely responses. Mathematica ran an experiment to test how different incentive payment options affected response rates for the first two releases. This experiment compared three options: (1) a graduated incentive, offering \$50 to those who completed the survey within four weeks of being contacted and \$25 to those who took longer to complete the survey; (2) a fixed \$25 incentive to all respondents; and (3) no monetary incentive. The study team found that both monetary incentives led to significantly higher response rates. In addition, the graduated incentive was associated with faster completion times (among those who responded). This translated into the least expensive cost per complete of the three options we tested; the higher dollar amount offered through the graduated incentive appeared to pay for itself by reducing the amount of effort needed to contact potential respondents and encourage them to complete the survey. Accordingly, the study team offered the graduated incentive to all remaining releases after the experiment concluded.

Survey response rates were high enough to support reliable conclusions: the overall response rate was 80 percent, with over 82 percent of the SET program group and nearly 78 percent of the control group responding. Based on DOL’s (2015) Clearinghouse for Labor and Evaluation Research (CLEAR) guidelines, this combination of response rates constitutes an “acceptable” level of attrition from the standpoint of potential bias for random assignment impact estimates of labor interventions. Hence, comparing survey respondents in the program and control groups should yield impact estimates that are sufficiently close to what would have been observed for all study enrollees (including nonrespondents) for the purposes of DOL policymaking. Nonetheless, as discussed later in this section, following our original design plans, we developed survey weights and used regression adjustment to account for differences between nonrespondents and respondents in their baseline characteristics.

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<sup>3</sup> In addition, for releases occurring in the second half of the survey fielding period, the study team re-sent the initial letter using FedEx to those who had not responded within three weeks. The study team also sent additional “last chance” notifications to nonrespondents about one month before the survey closed.



## Selecting outcomes and preparing data for the analysis

Mathematica designed the survey to provide information about study enrollees' experiences and outcomes since they were randomly assigned. The survey asked for information on current work status, receipt of self-employment supports and other job services, and business development milestones attained. It also asked for information on the extent to which respondents were engaged in both self-employment and wage/salary jobs, and it included a few additional measures of work-related well-being, such as job satisfaction and receipt of unemployment insurance (UI) benefits.

In the impact analysis, we examined a select set of service receipt, milestone, and work outcomes to understand how SET affected the program group. We summarize these outcomes in A.1, grouping them by topic and providing cross-references to report chapters and appendix data tables that present findings for the given outcomes. The data tables also provide more specific details about how we defined each outcome.

**Table B.1. Outcomes examined in the impact analysis, by topic**

Topic	Specific outcome measures	Report chapter(s)	Data table(s) with detailed information
Effectiveness of SET based on the 18-month survey (primary outcomes)	<ul style="list-style-type: none"> <li>• Self-employed at the survey date</li> <li>• Employed in any job at the survey date</li> <li>• Total earnings over the past 12 months leading up to the survey</li> </ul>	IV, IX	Appendix Tables D.1, D.7-D.9, and E.1-E.3
Receipt of self-employment services, training, and other job placement services between study enrollment and the 18-month survey	<ul style="list-style-type: none"> <li>• Received any self-employment service or support</li> <li>• Attended any in-person self-employment classes or training</li> <li>• Accessed any online courses on starting, operating, or growing a business</li> <li>• Attended any in-person peer advice or networking meetings for self-employment</li> <li>• Received any personalized self-employment support</li> <li>• Number of times received personalized self-employment support</li> <li>• Received any job placement services/career counseling from American Job Center or state labor exchange</li> </ul>	V	Appendix Table D.2
Business development activity between study enrollment and the 18-month survey	<ul style="list-style-type: none"> <li>• Wrote or completed a business plan</li> <li>• Received nonborrowed funds from any source to start or grow a business</li> <li>• Borrowed money from any source to start or grow a business</li> <li>• Took active steps to formalize main business venture by registering it, incorporating it, or obtaining an employer identification number or tax identification number</li> </ul>	VI	Appendix Table D.3
Self-employment activity based on the 18-month survey	<ul style="list-style-type: none"> <li>• Was self-employed at any point since study enrollment</li> <li>• Total hours worked in self-employment over the past 12 months</li> <li>• Worked in self-employment for at least 20 hours per week, on average, over the past year</li> <li>• Earnings from self-employment over the past 12 months</li> <li>• Number of employees in main business venture since study enrollment</li> </ul>	VII	Appendix Table D.4

Topic	Specific outcome measures	Report chapter(s)	Data table(s) with detailed information
Work in wage/salary jobs based on the 18-month survey	<ul style="list-style-type: none"> <li>• Employed in wage/salary job at the survey date</li> <li>• Total hours worked in wage/salary jobs over the past 12 months</li> <li>• Earnings from wage/salary jobs over the past 12 months</li> </ul>	VIII	Appendix Table D.5
Other measures of work and wellbeing based on the 18-month survey	<ul style="list-style-type: none"> <li>• Dually employed in both self-employment and wage/salary employment at survey date</li> <li>• Satisfied with employment situation, among those employed at survey date</li> <li>• Received UI benefits during past 12 months</li> </ul>	VIII	Appendix Table D.6

Note: Definitions for each specific analysis measure are provided in the notes to the corresponding data table listed in the last column of this table.

Based on decisions established during a pre-analysis planning stage, we made relatively minimal changes when preparing analysis measures from the survey data:

- We accounted for the logic skip pattern to fill in known values that were simply missing due to the design of the survey. For example, someone who had not been self-employed at any point since enrollment was not asked about his or her self-employment earnings; in this case, we replaced missing values with zeroes.
- We did not impute missing values of outcomes when survey respondents had not answered a specific follow-up survey question. Summary statistics and regression estimates for each outcome are based only on survey respondents who answered the corresponding question(s). Although the survey required responses to a few key questions only, item-level nonresponse was relatively rare. More than 92 percent of respondents provided all of the information needed to create every outcome listed in A.1, and the missing-value rate was less than 3 percent for each individual outcome measure.
- We winsorized continuous outcomes (for example, earnings or hours worked) to limit the potential influence of outliers. Specifically, we replaced values that were initially above the 97.5th percentile (among nonzero values) with the 97.5th percentile threshold value.

In addition, we constructed a few analysis measures by combining information from multiple survey data items. For example, total earnings is the sum of what respondents reported for self-employment earning and wage/salary earnings. Similarly, we determined whether they had worked in self-employment for at least 20 hours per week, on average, over the past year based on their responses to questions about the number of weeks worked over the year and the average number of hours worked per week.

### Developing survey nonresponse weights

When designing the evaluation of the SET pilot program, the study team decided to use weights when analyzing survey data to account for potential differences between respondents and nonrespondents. Although the degree of attrition turned out to be relatively low, response rates differed across types of study enrollees. As a result, some differences in the baseline characteristics between respondents and nonrespondents were moderately large and statistically significant (Appendix Table C.2). For example, older



enrollees, those with college degrees, those working in wage/salary jobs, and those with more assets were disproportionately represented among survey respondents. Following the original design plans, we developed weights to account for such differences and, therefore, should improve our capacity to generalize our results for survey respondents to the full set of study enrollees. (As discussed in Section 5, we also conducted a sensitivity check, which showed that using these weights did not substantively affect the study's conclusions.)

We created **person-level weights** based on the degree to which various types of study enrollees were likely to respond to the survey, as predicted by their baseline characteristics using a statistical model. This approach is based on the assumption that baseline covariates can adequately account for the relationship between study enrollees' likelihood of responding to the survey and their outcomes (Rubin 1976). Under this assumption, baseline characteristics can be used to create nonresponse weights that allow the outcomes of respondents to be rescaled in a way that better reflects the full population of study enrollees. We generated these survey weights using the following five steps:

- 1. Identifying a set of potential characteristics to include in the nonresponse model.** The candidate list of variables we identified is similar to those listed in Appendix Table C.1. The main difference is that we converted all of the variables to be binary, which included dividing up continuous variables into ranges. These binary measures were based on baseline characteristics after we imputed missing values, as previously discussed, so that we could include all study enrollees in the response model and create weights for each respondent.
- 2. Splitting the sample so that all remaining steps were performed separately for the program and control groups.** This enabled us to model potentially different response patterns in each group, which we expected to improve our capacity to produce summary outcome measures specifically for each group.
- 3. Identifying highly predictive two-way interactions to consider for inclusion in the final nonresponse model.** We started with a list of all two-way interaction terms that could be created from the binary variables identified in Step 1. We then narrowed this list to exclude those based on binary variables for which fewer than 20 people fell in either category, as well as those that would result in fewer than 7 people in one of the resulting subcategories. Finally, we used the algorithm described by Biggs et al. (1991) to incrementally add interaction terms to a logistic prediction model according to a set of prespecified criteria. We set this up to select the interaction term with the smallest  $p$ -value at each iteration until we reached the point at which no interaction term had a  $p$ -value less than 0.05.
- 4. Performing a stepwise search to identify a well-fitted nonresponse model.** We used a stepwise logistic regression procedure to choose a subset of the candidate binary variables and interaction terms described previously. All models included site fixed effects, given the site-level random assignment design. The stepwise procedure added and removed other variables through a series of forward and backward steps. Forward steps examined each variable not already in the model, added them to the model one at a time, identified the one with the lowest  $p$ -value, and retained it in the model so long as its  $p$ -value was less than 0.3. Backward steps examined variables already in the model, tested the statistical significance of each variable, identified the one with the highest  $p$ -value, and eliminated it so long as its  $p$ -value was greater than 0.35. The procedure stopped if no new variable was added in a

forward step and no new variable was removed in a backward step, or if the same variable was added and removed (or vice versa) in sequential steps.

**5. Creating probability weights to use when analyzing outcomes.** We estimated nonresponse probabilities for each study enrollee using the final logistic model developed in Step 4. We then created initial survey weights as the inverse of these estimated probabilities, adjusting them in two ways. First, we rescaled the weights so that, with each site and random assignment group, they summed to the original number of study enrollees. Second, we winsorized extreme values using a procedure developed for the National Assessment of Educational Progress (National Center for Education Statistics 2009) that preserved the sum of weights by site and group.

When using these weights, we also made an **outcome-level adjustment** to maintain the distribution across sites for all impact estimates and to post-stratify subgroups. Missing-value rates were relatively low—less than 3 percent for any given outcome—but were not necessarily distributed evenly across sites. We therefore rescaled the weights on an outcome-by-outcome basis so that they summed to the original number of study enrollees by site and random assignment group for every outcome. This maintained consistency in the effective (weighted) contribution of each site when we calculated means for the SET program and control groups and used them to estimate impacts. Based on the same rationale, we made a similar outcome-specific adjustment to equalize the weighted distribution of SET program and control group members in each subgroup across sites when producing subgroup means and impact estimates.

#### **4. Baseline equivalence of survey respondents**

Study enrollees in the SET program group should be fundamentally similar to those in the control group because we formed the two groups using a random assignment design. This tendency is borne out in Appendix Table C.1, which presents estimated differences in baseline characteristics between the program and control groups. The estimates are based on a regression model that include site fixed effects to account for the stratified, site-level random assignment design. As Appendix Table C.1 shows, we found uniformly small to moderate average differences between groups across a range of baseline characteristics, and (as expected) these differences can be explained by chance.<sup>4</sup>

Our analysis indicates that the baseline characteristics of survey respondents also tended to be fairly similar between the program and control groups.

- Equivalence of the full program and control groups did not guarantee this result because, as noted previously, the baseline characteristics of survey respondents and nonrespondents differed notably (Appendix Table C.2). These differences could have broken out differently between the program and control groups, resulting in survey respondents who were not sufficiently comparable across groups.
- However, in practice, we found average differences in baseline characteristics between survey respondents in the program and control groups that were small to moderate in size and that could

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<sup>4</sup> We use “small to moderate” to refer to differences that are smaller in magnitude than one-tenth of the pooled standard deviation across sites and groups. A few differences were statistically significant for the full population of study enrollees, but this was expected because we were comparing multiple characteristics. For example, less than one-twentieth of the program–control differences were large enough to be deemed statistically significant at the 5 percent level.

plausibly be the product of chance.

- We found that this was true when considering the unweighted sample of respondents and using only nonmissing values of each baseline characteristic (Appendix Table C.3).
- We also found that this was true after applying nonresponse weights and using baseline data with missing values imputed, as described previously (Appendix Table C.4), which aligns our main impact analysis model specification.
- In both cases, the estimated program–control differences for survey respondents were similar in magnitude to the differences between the full program and control groups. Further, none of these program–control differences was statistically significant at the 5 percent level.

These results provide additional support for the idea that the observed degree of sample attrition due to nonresponse is unlikely to produce a substantial amount of bias when comparing the average outcomes of the program and control groups.

## 5. Main statistical approach for estimating impacts

We estimated impacts using an “intention-to-treat” (ITT) framework that followed directly from the stratified random assignment design of the evaluation. ITT estimates are based on comparing the average outcomes of those assigned to the program and control groups, irrespective of whether they actually received any self-employment services or supports. The resulting impact estimates measure the effects of being offered access to the SET program in its entirety. These estimates might understate the effects of the program on those who used it.<sup>5</sup> In addition, we cannot estimate the effects of specific program components (such as technical assistance or microgrants); random assignment determined whether study enrollees had access to the whole package of SET services and supports, rather than individual components.

The rest of this section describes our specific methods used to estimate overall impacts, after which we discuss adaptation of those methods that we used to estimate impacts by site and subgroup.

### Estimating overall impacts

Based on the initial design of the evaluation, we used nonresponse weights and regression adjustment to reduce the chances of drawing incorrect conclusions about SET’s impacts. Specifically, we used weighted least squares (WLS) to estimate the following linear regression model:

$$[1] \quad y_{is} = \alpha_s + \beta T_i + \gamma' x_i + \varepsilon_{is},$$

where  $y_{is}$  is the outcome for individual  $i$  in site  $s$ ,  $\alpha_s$  is a site-level constant,  $T_i$  is a binary indicator set to 1 for those assigned to the program group,  $x_i$  is a vector of baseline control variables, and  $\varepsilon_{is}$  is an individual-level error term. We estimated the model using the nonresponse weights described previously so

<sup>5</sup> We did not estimate treatment-on-treated impacts or complier average causal effects (CACE) for the SET program because of limited data on the extent to which the control group received SET-like services and supports. The microenterprise service providers we partnered with tracked engagement with the SET program group only, and we did not think that respondents to an online survey would be able to differentiate between the unique features of the SET model (for example, case management from a personal advisor) and what might be otherwise available in the community (for example, mentoring through the Small Business Administration’s SCORE network). Future research might consider estimating CACE impacts for those who were induced to use any self-employment assistance because they were assigned to the program group. Such an analysis would need to account for potential measurement error arising from using survey questions to identify the “compliers” for whom this estimate is applicable.

that the sample of survey respondents better reflects the full set of study enrollees by site and random assignment status. We also estimated heteroscedasticity-consistent standard errors using the method described by White (1980).

The impact of SET is captured by  $\beta$  in Equation [1]. For continuous outcome variables,  $\beta$  measures the effect of the program for the average study enrollee, given that the WLS approach implicitly gives more weight to sites that had more enrollees. For binary outcome variables,  $\beta$  should provide a close approximation of estimates of SET's effects on the average enrollee obtained from a nonlinear probability response model (Wooldridge 2002). The sensitivity checks in Section 6 include assessments of whether weighting sites equally or using a logit model produce meaningfully different impact estimates for the study's primary outcome measures that were binary.

We chose the specific baseline variables to include in the regression during a pre-analysis planning stage. These regression control variables included the following subset of the baseline measures listed in Appendix Tables C.1 through C.4—all defined at or before the time of study enrollment.

- “Demographics and relationships” domain: all variables
- “Education, work experience, and employment status” domain: all variables
- “Income and finances” domain: home ownership, cash assets, and receipt of UI benefits—with an additional interaction between current UI receipt and site-level availability of work search waivers

We also specified an additional control variable indicating whether enrollees were randomly assigned in the first or second half of the study enrollment period for their site. We included this measure because the experiences and outcomes of earlier entrants likely differed from later entrants and because the rate of study enrollments increased markedly over time, straining the capacity of some SET providers.

### Producing site-level impact estimates

Before starting the impact analysis, we decided that we would estimate site-level impacts for all outcome measures because (1) randomization occurred separately within sites; and (2) as discussed in Appendix A, the implementation analysis found notable differences in fidelity across providers, who were nested within sites. We did this using WLS based on a modified version of Equation [1] that includes an interaction term to differentiate between the given site and the other sites:

$$[2] \quad y_{is} = \alpha_s + \beta_s T_i h_s + \theta T_i (1 - h_s) + \gamma' \mathbf{x}_i + \varepsilon_{is}$$

In this expression,  $h_s$  is a binary indicator for enrollees in site  $s$ ; we defined all the other terms previously. With this set-up,  $\beta_s$  measures the impact for site  $s$  and  $\theta$  measures the average impact for enrollees in the other sites. We used a chi-squared test to compare  $\beta_s$  against  $\theta$  to assess whether there was a statistically significant difference in impact estimates between site  $s$  and the other three sites combined.

Using an approach similar to Schochet (2016), we adjusted the standard errors from the site-level regression model to account for two site-specific factors: (1) the number of study enrollees and (2) the degrees of freedom lost due to its contribution to estimating  $\gamma$ . Specifically, if the regression model produced an estimated standard error of  $\hat{\sigma}_{\beta_g}$  for site  $s$ , we created the adjusted standard error as follows:

$$[3] \quad \hat{\sigma}_{\beta_s}^* = \hat{\sigma}_{\beta_s} \times \sqrt{\left(\frac{N-K-4}{N}\right)\left(\frac{N_s}{N_s - p_s K - 2}\right)},$$

where  $N$  is the overall sample size,  $K$  is the number of control variables included in  $\mathbf{x}$ ,  $p_s$  is the fraction of the sample in site  $s$ , and  $N_s$  is the corresponding number of individuals. We made a similar adjustment to the estimated standard error for  $\theta$ .

### Producing subgroup impact estimates

We conducted a subgroup analysis to assess the effectiveness of SET for the socioeconomic and demographic groups described in Box II.4 of the main text of the report using the three primary study outcome measures described in A.1. We produced these estimates using a regression model similar to Equation [2]. Specifically, for two mutually exclusive groups defined by the subgroup indicator  $d_i$ , we used WLS to estimate the following model:

$$[4] \quad y_{is} = \alpha_s + \beta_1 T_i d_i + \beta_2 T_i (1 - d_i) + \xi d_i + \gamma' \tilde{\mathbf{x}}_i + \varepsilon_{is}$$

All of the terms in this equation have been defined previously with the exception of  $\beta_1$  and  $\beta_2$ , which measure the impact for each group, and  $\tilde{\mathbf{x}}$ , which is a modified version of the baseline control variables from Equation [1] that excludes the characteristics that define subgroup membership. We adjusted estimated standard errors for the subgroup-specific impact estimates using a scaling factor similar to what Equation [3] describes.

For this analysis, we also adjusted the weights so that each site would contribute equally to every subgroup's impact estimate; this avoids confounding differences across subgroups with differences across sites. For example, as noted in Chapter III of the main text, a relatively high share of study enrollees in Cleveland and Portland were UI recipients, and these cities appeared to implement the SET model in a uniformly stronger fashion. Consequently, without adjusting the weights, we might obtain larger impact estimates for UI recipients than for nonrecipients based solely on their location. Weighting sites equally for the subgroup analysis enabled us to isolate the differences in effectiveness by subgroup that are not attributable to where they enrolled in SET.

One downside to this adjustment to the weights is that it can produce unstable results if any site-subgroup-random assignment group cell contains a small number of study enrollees. Estimates for small cells are relatively imprecise, but exert a substantial influence on the subgroup impact estimate based on how the weights are adjusted. To avoid this potential problem, if a given site-subgroup-random assignment group cell contained fewer than 10 survey respondents, we excluded it from the impact analysis for the corresponding subgroup comparison. In practice, this meant excluding Los Angeles when estimating impacts by race and ethnicity.

## 6. Sensitivity of results to alternative statistical approaches

The statistical approach described in the previous section represents one of several options that we could have chosen when designing the study. In this section, we describe five alternative variations we might have considered, and we then discuss the relatively small differences in the overall impact estimates for primary study outcomes that would arise if we had used one of those alternatives.

### Alternative statistical approaches considered

Our main statistical approach to estimating overall impacts involves using a linear regression model to adjust for site-specific factors and individual-level baseline characteristics, applying survey weights to account for nonresponse, and (implicitly) scaling each site up or down in proportion to the number of study enrollees in the given site. We conducted five sensitivity checks that assessed the implications of changing our approach in a specific way:

- 1. Logistic regression instead of linear regression for binary outcomes.** When implementing this check, we calculated the average partial effect based on the logistic regression and compared it to the estimated impact from our main approach.
- 2. No controls for baseline characteristics.** For this check, we excluded the baseline control variables,  $x$ , from Equation [1]. However, we continued to include site-level fixed effects to account for the stratified random assignment design.
- 3. Unweighted regression.** For this check, we did not apply the nonresponse weights described previously, and we used ordinary least squares instead of WLS to estimate equation [1].
- 4. Adjusting for differential attrition by trimming the sample according to response times.** As noted previously, the survey response rate was higher for the SET program group than for the control group. The magnitude of the response rate differential between groups is considered acceptable according to DOL's (2015) CLEAR standards. Nonetheless, there is still the potential for biased impact estimates if the differential was driven by additional respondents in the SET program group who had better (or worse) outcomes than those who would have also responded had they been in the control group. We used an approach similar to that of the National Job Corps Study (Schochet et al. 2003) to assess this potential bias. Specifically, we removed program group members who took the longest to respond to the survey until the effective response rate for that group was the same as for the control group; we then estimated impacts using this smaller sample.<sup>6</sup>
- 5. Sites weighted equally.** For our main analysis approach, we scaled the survey weights so that each site contributed to the overall impact estimates in proportion to the number of people who enrolled in the given site. This sensitivity check considered the alternative of having each site contribute equally to the impact estimates, which should mitigate the potential influence of local factors affecting both intake yields and SET's effectiveness.

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<sup>6</sup> This approach is based on the idea that the length of time a respondent took to return the survey tells us something about her or his interest in completing the survey. Hence, one way to identify the "marginal" respondents in the program group is to select those with the longest response times. If excluding those respondents were to result in different impact estimates, then we might especially be concerned about the potential nonresponse bias arising from the modest gap in response rates between the program and control groups.



## Results of sensitivity checks: similar impact estimates across a range of models

We implemented the sensitivity checks for each of the study's three primary outcome measures and found that each alternative statistical approach led to the same basic conclusions about SET's effectiveness as the main approach we selected when designing the study.

- Considering the self-employment rate at the time of the 18-month survey, our main approach yielded an impact estimate of 11.3 percentage points and the alternative approaches produced impact estimates ranging from 10.8 to 11.7 percentage points (Appendix Table E.1). All of these impact estimates were statistically significant with  $p$ -values less than 0.01.
- Considering the rate of employment in any job at the time of the 18-month survey, our main approach yielded an impact estimate of 3.3 percentage points ( $p < 0.05$ ). As Appendix Table E.2 shows, the alternative approaches produced impact estimates ranging from 2.9 percentage points ( $p < 0.1$ ) to 4.4 percentage points ( $p < 0.01$ ).
- Considering average total earnings during the year leading up to the survey, our main approach yielded an impact estimate of -\$646 and the alternative approaches produced estimates ranging from -\$1,057 to -\$438 (Appendix Table E.3). None of these impact estimates was statistically significant.

Taken together, these results indicate relatively small differences in our findings across the range of alternative approaches we might have used. Estimates based on any of those approaches would have indicated that SET had large impacts on self-employment rates, modest reemployment impacts, and no discernable impact on earnings during the period covered by the survey.

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**APPENDIX C:**  
**DESCRIPTIVE TABLES OF STUDY ENROLLEE**  
**CHARACTERISTICS AND SITE-LEVEL**  
**IMPLEMENTATION MEASURES**

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**Table C.1. Baseline characteristics of all study enrollees, by random assignment group**

	Mean for SET program group	Mean for control group	Difference in group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
<b>Site</b>					
Chicago	39.4	38.9	0.5	0.5	(2.2)
Cleveland	22.8	23.2	-0.4	-0.4	(1.9)
Los Angeles	10.4	10.4	0.0	0.0	(1.4)
Portland	27.4	27.5	0.0	0.0	(2.0)
<b>Demographics and relationships</b>					
<b>Age group</b>					
21 to 34	19.9	18.5	1.4	1.4	(1.8)
35 to 44	28.9	29.8	-0.9	-1.0	(2.0)
45 to 54	28.7	27.3	1.4	1.4	(2.0)
55 and older	22.6	24.4	-1.8	-1.8	(1.9)
<b>Gender and parenthood</b>					
Female, no children	34.0	35.2	-1.1	-1.2	(2.1)
Female parent	24.5	24.4	0.1	0.1	(1.9)
Male, no children	27.2	24.6	2.6	2.6	(2.0)
Male parent	14.2	15.8	-1.5	-1.5	(1.6)
<b>Race/ethnicity</b>					
Hispanic	8.5	9.8	-1.3	-1.3	(1.3)
Black, non-Hispanic	42.6	39.1	3.5	3.4*	(2.0)
White, non-Hispanic	39.0	41.2	-2.2	-2.1	(2.0)
Other race, non-Hispanic	9.9	9.9	0.0	0.0	(1.3)
<b>Marital/partnership status</b>					
Married, civil union, or living with partner	39.3	39.0	0.3	0.3	(2.1)
Never married	33.9	34.5	-0.6	-0.7	(2.1)
Separated, divorced, or widowed	26.8	26.5	0.4	0.4	(2.0)
<b>Relative or close friend of small business owner</b>					
No	20.8	22.3	-1.5	-1.5	(1.8)
Yes	79.2	77.7	1.5	1.5	(1.8)
<b>Education, work experience, and employment status</b>					
<b>Highest level of education</b>					
Did not attend college	7.4	7.1	0.3	0.3	(1.2)
Two-year degree or some college without degree	35.2	36.3	-1.0	-1.0	(2.2)
Bachelor's degree	30.7	32.1	-1.4	-1.4	(2.1)
Advanced degree	26.7	24.5	2.2	2.2	(2.0)
<b>Previous managerial experience</b>					
No	19.5	22.4	-2.9	-3.0	(1.8)
Yes	80.5	77.6	2.9	3.0	(1.8)
<b>Previous work in same industry as proposed small business</b>					
No	13.3	14.9	-1.6	-1.6	(1.6)
Yes	86.7	85.1	1.6	1.6	(1.6)

	Mean for SET program group	Mean for control group	Difference in group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
<b>Self-employment</b>					
Currently self-employed, formal business <sup>a</sup>	13.1	15.1	-1.9	-1.9	(1.6)
Currently self-employed, informal business <sup>a</sup>	7.1	7.1	0.0	0.0	(1.2)
Self-employed in past 5 years, but not at time of enrollment	15.2	16.2	-0.9	-0.9	(1.6)
No self-employment experience in past 5 years	64.6	61.7	2.9	2.9	(2.2)
<b>Working in a wage/salary job</b>					
No	87.5	88.9	-1.4	-1.4	(1.4)
Yes	12.5	11.1	1.4	1.4	(1.4)
<b>Income and finances</b>					
<b>Household income over past 12 months (\$)</b>	43,891	42,941	950	961	(1,652)
<b>Receipt of unemployment insurance (UI) benefits</b>					
Current recipient	49.4	48.2	1.3	1.4	(2.1)
Exhausted UI benefits within the past 24 months	23.1	24.9	-1.8	-1.9	(1.8)
Received UI benefits within the past 24 months but did not exhaust entitlement	5.9	5.7	0.2	0.2	(1.0)
Did not receive any UI benefits during the past 24 months	21.6	21.2	0.4	0.4	(1.8)
<b>Owns residence</b>					
No	64.8	63.5	1.2	1.2	(2.1)
Yes	35.2	36.5	-1.2	-1.2	(2.1)
<b>Cash assets</b>					
\$0	42.7	40.0	2.7	2.7	(2.2)
\$1 to \$1,000	20.8	21.9	-1.1	-1.1	(1.8)
\$1,001 to \$5,000	13.0	16.0	-3.1	-3.0*	(1.6)
More than \$5,000	23.6	22.1	1.5	1.5	(1.8)
<b>Previous bankruptcy, delinquency, or court-ordered repayment to creditor</b>					
No	63.1	64.8	-1.8	-1.8	(2.1)
Yes	36.9	35.2	1.8	1.8	(2.1)
<b>Personality traits<sup>b</sup></b>					
<b>Attitude toward risk</b>					
Will not take risks or will take limited risks to earn limited returns	13.3	13.4	-0.2	-0.2	(1.5)
Will take average risks to earn average returns	48.6	50.5	-1.9	-1.9	(2.2)
Will take above-average risks to earn above-average returns	38.2	36.1	2.1	2.1	(2.2)

	Mean for SET program group	Mean for control group	Difference in group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
<b>Big Five personality factors: Extroversion</b>					
Low	29.5	30.9	-1.5	-1.5	(2.1)
Medium	36.0	37.6	-1.6	-1.6	(2.2)
High	34.5	31.4	3.1	3.1	(2.1)
<b>Big Five personality factors: Agreeableness</b>					
Low	36.8	33.4	3.4	3.4	(2.1)
High	63.2	66.6	-3.4	-3.4	(2.1)
<b>Big Five personality factors: Conscientiousness</b>					
Low	43.8	39.3	4.5	4.5**	(2.2)
High	56.2	60.7	-4.5	-4.5**	(2.2)
<b>Big Five personality factors: Emotional stability</b>					
Low	40.3	39.4	0.9	0.9	(2.2)
High	59.7	60.6	-0.9	-0.9	(2.2)
<b>Big Five personality factors: Openness to new experiences</b>					
Low	28.4	28.8	-0.5	-0.5	(2.0)
High	71.6	71.2	0.5	0.5	(2.0)
<b>Locus of control: Internal</b>					
Low	36.1	38.7	-2.7	-2.7	(2.2)
Medium	21.9	20.8	1.1	1.1	(1.8)
High	42.0	40.4	1.6	1.6	(2.2)
<b>Locus of control: External</b>					
Low	32.9	32.5	0.4	0.4	(2.1)
Medium	33.2	33.8	-0.6	-0.6	(2.1)
High	34.0	33.8	0.2	0.2	(2.1)

Source: SET study intake forms.

Note: Unless otherwise noted, all table entries are percentages (means) and percentage points (differences) for baseline characteristics reported at the time of study enrollment. Summary statistics and estimates for each characteristic are based on enrollees who answered the corresponding question(s) on the intake forms. All numbers in the table have been rounded; consequently (1) reported percentages might not sum across categories to exactly 100, and (2) reported differences in group means might not exactly equal the reported program group mean minus the reported control group mean. As discussed in Appendix B, most estimated differences that account for the site-level design are based on regression models with site fixed effects, but estimated differences in the distribution across sites are based on simple regression models that do not include site fixed effects. Standard errors for all these estimates are robust to heteroscedasticity.

<sup>a</sup> Formal businesses are those that were incorporated or registered with the enrollee's state, county, or city.

<sup>b</sup> See Amin et al. (2017) for how the Big Five personality traits and locus of control measures were measured and coded.

\* / \*\* / \*\*\* Denotes an estimated difference between groups that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

**Table C.2. Baseline characteristics of all study enrollees, by 18-month survey response status**

	Mean for survey respondents	Mean for survey non-respondents	Difference in overall group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
<b>Site</b>					
Chicago	39.3	38.3	1.0	1.0	(2.7)
Cleveland	21.8	27.9	-6.1	-6.1**	(2.5)
Los Angeles	10.8	8.9	1.9	1.9	(1.6)
Portland	28.1	24.9	3.2	3.2	(2.5)
<b>Demographics and relationships</b>					
<b>Age group</b>					
21 to 34	18.5	21.8	-3.3	-3.1	(2.3)
35 to 44	28.9	31.2	-2.4	-2.5	(2.6)
45 to 54	27.9	28.2	-0.3	-0.3	(2.5)
55 and older	24.7	18.8	5.9	5.9***	(2.2)
<b>Gender and parenthood</b>					
Female, no children	36.1	28.4	7.7	7.5***	(2.6)
Female parent	24.2	25.6	-1.4	-0.9	(2.5)
Male, no children	25.5	27.7	-2.1	-2.6	(2.5)
Male parent	14.2	18.3	-4.1	-4.0*	(2.1)
<b>Race/ethnicity</b>					
Hispanic	8.6	11.3	-2.7	-3.1*	(1.7)
Black, non-Hispanic	40.2	43.4	-3.2	-1.8	(2.6)
White, non-Hispanic	41.7	33.7	8.0	7.4***	(2.4)
Other race, non-Hispanic	9.5	11.6	-2.1	-2.4	(1.8)
<b>Marital/partnership status</b>					
Married, civil union, or living with partner	40.1	35.0	5.1	4.8*	(2.7)
Never married	34.2	34.3	0.0	0.1	(2.7)
Separated, divorced, or widowed	25.6	30.7	-5.1	-4.9*	(2.6)
<b>Relative or close friend of small business owner</b>					
No	20.4	26.2	-5.9	-5.6**	(2.4)
Yes	79.6	73.8	5.9	5.6**	(2.4)
<b>Education, work experience, and employment status</b>					
<b>Highest level of education</b>					
Did not attend college	6.0	12.2	-6.2	-6.1***	(1.7)
Two-year degree or some college without degree	33.8	43.7	-9.9	-9.7***	(2.8)
Bachelor's degree	32.8	25.6	7.2	7.1***	(2.5)
Advanced degree	27.4	18.5	8.9	8.7***	(2.3)
<b>Previous managerial experience</b>					
No	20.5	22.6	-2.0	-2.0	(2.3)
Yes	79.5	77.4	2.0	2.0	(2.3)
<b>Previous work in same industry as proposed small business</b>					
No	14.4	12.9	1.5	1.6	(1.9)
Yes	85.6	87.1	-1.5	-1.6	(1.9)
<b>Self-employment</b>					
Currently self-employed, formal business <sup>a</sup>	14.7	11.7	3.0	2.6	(1.8)
Currently self-employed, informal business <sup>a</sup>	7.5	5.3	2.2	2.0	(1.3)

	Mean for survey respondents	Mean for survey non-respondents	Difference in overall group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
Self-employed in past 5 years, but not at time of enrollment	14.8	19.3	-4.5	-4.4**	(2.2)
No self-employment experience in past 5 years	63.0	63.7	-0.7	-0.2	(2.7)
<b>Working in a wage/salary job</b>					
No	87.5	90.9	-3.3	-3.4**	(1.7)
Yes	12.5	9.1	3.3	3.4**	(1.7)
<b>Income and finances</b>					
<b>Household income over past 12 months (\$)</b>	44,441	39,277	5,164	4,568**	(2,062)
<b>Receipt of unemployment insurance (UI) benefits</b>					
Current recipient	49.0	48.2	0.7	0.8	(2.7)
Exhausted UI benefits within the past 24 months	24.3	22.8	1.5	1.7	(2.3)
Received UI benefits within the past 24 months but did not exhaust entitlement	5.5	6.9	-1.4	-1.2	(1.4)
Did not receive any UI benefits during the past 24 months	21.2	22.1	-0.8	-1.3	(2.3)
<b>Owns residence</b>					
No	62.1	72.6	-10.5	-10.9***	(2.5)
Yes	37.9	27.4	10.5	10.9***	(2.5)
<b>Cash assets</b>					
\$0	38.9	51.2	-12.3	-11.7***	(2.8)
\$1 to \$1,000	21.5	20.7	0.8	1.1	(2.3)
\$1,001 to \$5,000	14.7	13.6	1.2	1.0	(2.0)
More than \$5,000	24.9	14.6	10.3	9.6***	(2.1)
<b>Previous bankruptcy, delinquency, or court-ordered repayment to creditor</b>					
No	64.9	60.2	4.8	4.0	(2.7)
Yes	35.1	39.8	-4.8	-4.0	(2.7)
<b>Personality traits<sup>b</sup></b>					
<b>Attitude toward risk</b>					
Will not take risks or will take limited risks to earn limited returns	12.9	15.0	-2.1	-2.3	(2.0)
Will take average risks to earn average returns	50.5	45.5	5.0	4.8*	(2.8)
Will take above-average risks to earn above-average returns	36.5	39.4	-2.9	-2.5	(2.7)
<b>Big Five personality factors: Extroversion</b>					
Low	30.1	30.5	-0.4	-0.5	(2.6)
Medium	36.9	36.6	0.2	0.4	(2.7)
High	33.0	32.8	0.2	0.2	(2.7)
<b>Big Five personality factors: Agreeableness</b>					
Low	34.0	39.2	-5.1	-5.2*	(2.7)
High	66.0	60.8	5.1	5.2*	(2.7)
<b>Big Five personality factors: Conscientiousness</b>					
Low	41.2	43.0	-1.8	-2.1	(2.8)
High	58.8	57.0	1.8	2.1	(2.8)

	Mean for survey respondents	Mean for survey non-respondents	Difference in overall group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
<b>Big Five personality factors: Emotional stability</b>					
Low	39.4	41.7	-2.3	-2.3	(2.8)
High	60.6	58.3	2.3	2.3	(2.8)
<b>Big Five personality factors: Openness to new experiences</b>					
Low	28.8	28.0	0.8	0.8	(2.5)
High	71.2	72.0	-0.8	-0.8	(2.5)
<b>Locus of control: Internal</b>					
Low	38.5	33.1	5.4	5.8**	(2.7)
Medium	20.9	23.2	-2.2	-2.6	(2.4)
High	40.6	43.8	-3.2	-3.2	(2.8)
<b>Locus of control: External</b>					
Low	32.1	35.1	-3.1	-2.9	(2.7)
Medium	33.6	32.8	0.8	0.8	(2.6)
High	34.3	32.1	2.3	2.1	(2.6)

Source: SET study intake forms.

Note: Unless otherwise noted, all table entries are percentages (means) and percentage points (differences) for baseline characteristics reported at the time of study enrollment. Summary statistics and estimates for each characteristic are based on enrollees who answered the corresponding question(s) on the intake forms. All numbers in the table have been rounded; consequently (1) reported percentages might not sum across categories to exactly 100, and (2) reported differences in group means might not exactly equal the reported program group mean minus the reported control group mean. As discussed in Appendix B, most estimated differences that account for the site-level design are based on regression models with site fixed effects, but estimated differences in the distribution across sites are based on simple regression models that do not include site fixed effects. Standard errors for all these estimates are robust to heteroscedasticity.

<sup>a</sup> Formal businesses are those that were incorporated or registered with the enrollee's state, county, or city.

<sup>b</sup> See Amin et al. (2017) for how the Big Five personality traits and locus of control measures were measured and coded.

\* / \*\* / \*\*\* Denotes an estimated difference between groups that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.



**Table C.3. Baseline characteristics of 18-month survey respondents (unweighted), by random assignment group**

	Mean for SET program group	Mean for control group	Difference in group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
<b>Site</b>					
Chicago	39.4	39.3	0.1	0.1	(2.5)
Cleveland	22.0	21.6	0.4	0.4	(2.1)
Los Angeles	11.0	10.5	0.5	0.5	(1.6)
Portland	27.6	28.6	-1.0	-1.0	(2.3)
<b>Demographics and relationships</b>					
<b>Age group</b>					
21 to 34	19.2	17.8	1.4	1.4	(1.9)
35 to 44	28.5	29.3	-0.8	-0.7	(2.3)
45 to 54	28.7	27.0	1.7	1.7	(2.3)
55 and older	23.6	25.9	-2.3	-2.3	(2.2)
<b>Gender and parenthood</b>					
Female, no children	35.6	36.7	-1.1	-1.1	(2.4)
Female parent	24.7	23.7	1.0	1.0	(2.1)
Male, no children	26.0	25.0	1.1	1.1	(2.2)
Male parent	13.7	14.7	-1.0	-1.0	(1.8)
<b>Race/ethnicity</b>					
Hispanic	8.6	8.7	-0.1	-0.2	(1.4)
Black, non-Hispanic	42.3	38.0	4.3	4.0*	(2.2)
White, non-Hispanic	40.1	43.4	-3.3	-2.8	(2.2)
Other race, non-Hispanic	9.1	10.0	-0.9	-1.0	(1.5)
<b>Marital/partnership status</b>					
Married, civil union, or living with partner	40.5	39.8	0.7	0.9	(2.4)
Never married	33.7	34.7	-1.0	-1.1	(2.4)
Separated, divorced, or widowed	25.8	25.5	0.3	0.2	(2.2)
<b>Relative or close friend of small business owner</b>					
No	20.0	20.7	-0.7	-0.8	(2.0)
Yes	80.0	79.3	0.7	0.8	(2.0)
<b>Education, work experience, and employment status</b>					
<b>Highest level of education</b>					
Did not attend college	6.0	6.0	0.0	0.0	(1.2)
Two-year degree or some college without degree	34.1	33.4	0.7	0.6	(2.4)
Bachelor's degree	31.7	34.1	-2.4	-2.3	(2.4)
Advanced degree	28.2	26.5	1.7	1.7	(2.2)
<b>Previous managerial experience</b>					
No	18.9	22.2	-3.3	-3.3	(2.0)
Yes	81.1	77.8	3.3	3.3	(2.0)
<b>Previous work in same industry as proposed small business</b>					
No	13.9	15.0	-1.0	-1.0	(1.8)
Yes	86.1	85.0	1.0	1.0	(1.8)
<b>Self-employment</b>					
Currently self-employed, formal business <sup>a</sup>	13.6	15.9	-2.3	-2.3	(1.8)
Currently self-employed, informal business <sup>a</sup>	7.7	7.3	0.4	0.4	(1.3)

	Mean for SET program group	Mean for control group	Difference in group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
Self-employed in past 5 years, but not at time of enrollment	14.8	14.8	0.0	-0.1	(1.8)
No self-employment experience in past 5 years	63.9	62.0	1.9	1.9	(2.4)
<b>Working in a wage/salary job</b>					
No	87.0	88.0	-1.0	-1.0	(1.7)
Yes	13.0	12.0	1.0	1.0	(1.7)
<b>Income and finances</b>					
<b>Household income over past 12 months (\$)</b>	43,833	45,087	-1,254	-1,004	(1,844)
<b>Receipt of unemployment insurance (UI) benefits</b>					
Current recipient	49.6	48.2	1.4	1.7	(2.4)
Exhausted UI benefits within the past 24 months	23.1	25.6	-2.5	-2.7	(2.1)
Received UI benefits within the past 24 months but did not exhaust entitlement	6.1	4.8	1.3	1.3	(1.1)
Did not receive any UI benefits during the past 24 months	21.1	21.3	-0.2	-0.3	(2.0)
<b>Owns residence</b>					
No	63.1	61.0	2.1	1.9	(2.4)
Yes	36.9	39.0	-2.1	-1.9	(2.4)
<b>Cash assets</b>					
\$0	40.1	37.6	2.5	2.2	(2.4)
\$1 to \$1,000	21.3	21.7	-0.3	-0.4	(2.1)
\$1,001 to \$5,000	13.4	16.2	-2.8	-2.7	(1.8)
More than \$5,000	25.2	24.5	0.7	1.0	(2.1)
<b>Previous bankruptcy, delinquency, or court-ordered repayment to creditor</b>					
No	64.1	65.8	-1.7	-1.6	(2.4)
Yes	35.9	34.2	1.7	1.6	(2.4)
<b>Personality traits<sup>b</sup></b>					
<b>Attitude toward risk</b>					
Will not take risks or will take limited risks to earn limited returns	12.9	13.0	-0.1	-0.1	(1.7)
Will take average risks to earn average returns	49.8	51.3	-1.5	-1.4	(2.5)
Will take above-average risks to earn above-average returns	37.3	35.7	1.6	1.5	(2.4)
<b>Big Five personality factors: Extroversion</b>					
Low	29.2	31.1	-1.9	-1.8	(2.3)
Medium	35.9	37.8	-1.9	-1.9	(2.4)
High	34.8	31.1	3.8	3.7	(2.4)
<b>Big Five personality factors: Agreeableness</b>					
Low	35.0	33.0	2.0	2.0	(2.4)
High	65.0	67.0	-2.0	-2.0	(2.4)
<b>Big Five personality factors: Conscientiousness</b>					
Low	43.2	39.1	4.0	4.1	(2.5)
High	56.8	60.9	-4.0	-4.1	(2.5)

	Mean for SET program group	Mean for control group	Difference in group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
<b>Big Five personality factors: Emotional stability</b>					
Low	39.0	39.8	-0.7	-0.7	(2.5)
High	61.0	60.2	0.7	0.7	(2.5)
<b>Big Five personality factors: Openness to new experiences</b>					
Low	27.9	29.7	-1.8	-1.8	(2.3)
High	72.1	70.3	1.8	1.8	(2.3)
<b>Locus of control: Internal</b>					
Low	37.2	39.8	-2.6	-2.6	(2.4)
Medium	21.2	20.7	0.5	0.5	(2.0)
High	41.6	39.5	2.1	2.1	(2.5)
<b>Locus of control: External</b>					
Low	33.0	31.1	1.9	1.8	(2.3)
Medium	33.1	34.2	-1.1	-1.0	(2.4)
High	33.9	34.7	-0.8	-0.8	(2.4)

Source: SET study intake forms.

Note: Unless otherwise noted, all table entries are percentages (means) and percentage points (differences) for baseline characteristics reported at the time of study enrollment. Summary statistics and estimates for each characteristic are based on 18-month survey respondents who answered the corresponding question(s) on the intake forms. All numbers in the table have been rounded; consequently (1) reported percentages might not sum across categories to exactly 100, and (2) reported differences in group means might not exactly equal the reported program group mean minus the reported control group mean. As discussed in Appendix B, most estimated differences that account for the site-level design are based on regression models with site fixed effects, but estimated differences in the distribution across sites are based on simple regression models that do not include site fixed effects. Standard errors for all these estimates are robust to heteroscedasticity.

<sup>a</sup> Formal businesses are those that were incorporated or registered with the enrollee's state, county, or city.

<sup>b</sup> See Amin et al. (2017) for how the Big Five personality traits and locus of control measures were measured and coded.

\* / \*\* / \*\*\* Denotes an estimated difference between groups that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

**Table C.4. Baseline characteristics of 18-month survey respondents (weighted), by random assignment group**

	Mean for SET program group	Mean for control group	Difference in group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
<b>Site</b>					
Chicago	39.4	38.9	0.5	0.5	(2.5)
Cleveland	22.8	23.2	-0.4	-0.4	(2.2)
Los Angeles	10.4	10.4	0.0	0.0	(1.5)
Portland	27.4	27.5	0.0	0.0	(2.3)
<b>Demographics and relationships</b>					
<b>Age group</b>					
21 to 34	19.2	19.0	0.2	0.2	(2.0)
35 to 44	29.1	29.6	-0.4	-0.5	(2.3)
45 to 54	29.0	27.0	2.0	2.1	(2.3)
55 and older	22.6	24.5	-1.8	-1.8	(2.1)
<b>Gender and parenthood</b>					
Female, no children	34.3	35.1	-0.8	-0.8	(2.4)
Female parent	24.6	24.6	0.0	0.0	(2.2)
Male, no children	27.0	24.5	2.5	2.5	(2.2)
Male parent	14.1	15.8	-1.7	-1.7	(1.9)
<b>Race/ethnicity</b>					
Hispanic	8.4	9.8	-1.4	-1.4	(1.5)
Black, non-Hispanic	42.3	39.1	3.2	3.1	(2.3)
White, non-Hispanic	39.6	40.9	-1.3	-1.3	(2.2)
Other race, non-Hispanic	9.8	10.2	-0.5	-0.5	(1.6)
<b>Marital/partnership status</b>					
Married, civil union, or living with partner	39.3	38.9	0.4	0.4	(2.4)
Never married	34.0	35.5	-1.6	-1.6	(2.4)
Separated, divorced, or widowed	26.8	25.6	1.2	1.2	(2.3)
<b>Relative or close friend of small business owner</b>					
No	20.3	22.1	-1.8	-1.8	(2.1)
Yes	79.7	77.9	1.8	1.8	(2.1)
<b>Education, work experience, and employment status</b>					
<b>Highest level of education</b>					
Did not attend college	7.6	6.9	0.7	0.7	(1.5)
Two-year degree or some college without degree	34.9	36.4	-1.6	-1.6	(2.5)
Bachelor's degree	30.8	32.2	-1.4	-1.4	(2.3)
Advanced degree	26.7	24.5	2.2	2.2	(2.2)
<b>Previous managerial experience</b>					
No	19.1	22.7	-3.5	-3.6*	(2.1)
Yes	80.9	77.3	3.5	3.6*	(2.1)
<b>Previous work in same industry as proposed small business</b>					
No	13.4	15.3	-1.8	-1.8	(1.8)
Yes	86.6	84.7	1.8	1.8	(1.8)
<b>Self-employment</b>					
Currently self-employed, formal business <sup>a</sup>	13.1	15.2	-2.0	-2.1	(1.7)
Currently self-employed, informal business <sup>a</sup>	7.1	7.1	0.0	0.0	(1.3)

	Mean for SET program group	Mean for control group	Difference in group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
Self-employed in past 5 years, but not at time of enrollment	15.4	15.1	0.2	0.2	(1.9)
No self-employment experience in past 5 years	64.4	62.5	1.8	1.9	(2.4)
<b>Working in a wage/salary job</b>					
No	87.5	88.9	-1.4	-1.4	(1.6)
Yes	12.5	11.1	1.4	1.4	(1.6)
<b>Income and finances</b>					
<b>Household income over past 12 months (\$)</b>	43,714	42,940	774	791	(1,832)
<b>Receipt of unemployment insurance (UI) benefits</b>					
Current recipient	49.4	48.1	1.3	1.4	(2.4)
Exhausted UI benefits within the past 24 months	23.1	25.0	-1.9	-2.0	(2.1)
Received UI benefits within the past 24 months but did not exhaust entitlement	5.7	5.7	0.1	0.1	(1.2)
Did not receive any UI benefits during the past 24 months	21.7	21.2	0.5	0.5	(2.1)
<b>Owns residence</b>					
No	64.4	63.7	0.7	0.7	(2.4)
Yes	35.6	36.3	-0.7	-0.7	(2.4)
<b>Cash assets</b>					
\$0	42.6	39.2	3.4	3.4	(2.5)
\$1 to \$1,000	20.7	21.9	-1.2	-1.2	(2.1)
\$1,001 to \$5,000	12.9	16.0	-3.1	-3.0*	(1.8)
More than \$5,000	23.7	22.9	0.9	0.9	(2.0)
<b>Previous bankruptcy, delinquency, or court-ordered repayment to creditor</b>					
No	63.0	64.9	-1.8	-1.8	(2.4)
Yes	37.0	35.1	1.8	1.8	(2.4)
<b>Personality traits<sup>b</sup></b>					
<b>Attitude toward risk</b>					
Will not take risks or will take limited risks to earn limited returns	13.0	12.9	0.1	0.1	(1.7)
Will take average risks to earn average returns	49.1	50.5	-1.4	-1.4	(2.5)
Will take above- average risks to earn above-average returns	37.9	36.6	1.3	1.3	(2.5)
<b>Big Five personality factors: Extroversion</b>					
Low	29.7	31.3	-1.6	-1.6	(2.4)
Medium	36.1	38.0	-1.9	-1.9	(2.5)
High	34.2	30.8	3.5	3.5	(2.4)
<b>Big Five personality factors: Agreeableness</b>					
Low	36.9	33.5	3.5	3.5	(2.5)
High	63.1	66.5	-3.5	-3.5	(2.5)
<b>Big Five personality factors: Conscientiousness</b>					
Low	44.1	39.3	4.8	4.8*	(2.5)
High	55.9	60.7	-4.8	-4.8*	(2.5)

	Mean for SET program group	Mean for control group	Difference in group means	Difference between groups after accounting for site-level design	
				Estimate	Standard error
<b>Big Five personality factors: Emotional stability</b>					
Low	40.5	39.5	0.9	0.9	(2.5)
High	59.5	60.5	-0.9	-0.9	(2.5)
<b>Big Five personality factors: Openness to new experiences</b>					
Low	27.9	29.0	-1.0	-1.0	(2.3)
High	72.1	71.0	1.0	1.0	(2.3)
<b>Locus of control: Internal</b>					
Low	36.0	39.6	-3.6	-3.6	(2.5)
Medium	22.1	20.3	1.8	1.8	(2.1)
High	41.9	40.1	1.8	1.8	(2.5)
<b>Locus of control: External</b>					
Low	33.6	31.9	1.7	1.7	(2.4)
Medium	32.7	33.7	-1.0	-1.0	(2.4)
High	33.8	34.5	-0.7	-0.7	(2.4)

Source: SET study intake forms.

Note: Unless otherwise noted, all table entries are percentages (means) and percentage points (differences) for baseline characteristics reported at the time of study enrollment. Summary statistics and estimates based on the full sample of 18-month survey respondents after applying weights developed to account for survey nonresponse and imputing missing values of baseline characteristics. See Appendix B for additional details. All numbers in the table have been rounded; consequently (1) reported percentages might not sum across categories to exactly 100, and (2) reported differences in group means might not exactly equal the reported program group mean minus the reported control group mean. As also discussed in Appendix B, most estimated differences that account for the site-level design are based on regression models with site fixed effects, but estimated differences in the distribution across sites are based on simple regression models that do not include site fixed effects. Standard errors for all these estimates are robust to heteroscedasticity.

<sup>a</sup> Formal businesses are those that were incorporated or registered with the enrollee's state, county, or city.

<sup>b</sup> See Amin et al. (2017) for how the Big Five personality factors and locus of control traits were measured and coded.

\* / \*\* / \*\*\* Denotes an estimated difference between groups that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

**Table C.5. Select characteristics of study enrollees, by site**

	Overall mean	Chicago	Cleveland	Los Angeles	Portland
<b>Age</b>					
21 to 34	19.2	21.3 <sup>†</sup>	20.0	14.1 <sup>††</sup>	17.5
35 to 44	29.3	30.2	27.4	26.7	30.7
45 to 54	28.0	27.9	28.1	29.6	27.4
55 and older	23.5	20.6 <sup>††</sup>	24.6	29.6 <sup>††</sup>	24.4
<b>Female</b>	59.1	62.7 <sup>†††</sup>	61.0	59.2	52.2 <sup>†††</sup>
<b>Race/ethnicity</b>					
Hispanic	9.1	9.5	5.0 <sup>†††</sup>	22.8 <sup>†††</sup>	6.6 <sup>††</sup>
Black, non-Hispanic	40.7	58.8 <sup>†††</sup>	48.5 <sup>†††</sup>	41.3	8.1 <sup>†††</sup>
White, non-Hispanic	40.3	22.5 <sup>†††</sup>	39.3	16.5 <sup>†††</sup>	75.6 <sup>†††</sup>
Other race, non-Hispanic	9.9	9.2	7.2 <sup>††</sup>	19.4 <sup>†††</sup>	9.7
<b>Bachelor's degree or advanced degree</b>	57.0	58.7	51.5 <sup>†††</sup>	54.4	60.3 <sup>†</sup>
<b>Previous managerial experience</b>	79.1	77.8	79.2	77.2	81.4 <sup>†</sup>
<b>Previous work in same industry as proposed business</b>	85.9	84.4	85.3	85.9	88.4 <sup>††</sup>
<b>Self-employed within past 5 years</b>	36.9	41.2 <sup>†††</sup>	30.3 <sup>†††</sup>	39.8	35.1
<b>Employed in wage/salary job</b>	11.8	14.6 <sup>†††</sup>	11.6	10.2	8.6 <sup>†††</sup>
<b>Currently receiving unemployment insurance benefits</b>	48.8	33.7 <sup>†††</sup>	54.6 <sup>†††</sup>	33.5 <sup>†††</sup>	71.3 <sup>†††</sup>
<b>Cash assets greater than \$1,000</b>	37.4	28.7 <sup>†††</sup>	28.8 <sup>†††</sup>	27.3 <sup>†††</sup>	60.5 <sup>†††</sup>
<b>Sample size</b>	1,981	775	456	206	554

Source: SET study intake forms.

Note: The table presents percentages for baseline characteristics reported at the time of study enrollment. All numbers in the table have been rounded; consequently reported percentages might not sum across categories to exactly 100. Percentages are calculated among all study enrollees who answered the corresponding intake question(s) on the intake forms. Approximately 98 percent of the 1,981 enrollees completed all questions on the background characteristics form.

† / †† / ††† Denotes a difference between the given site and the other three sites combined that is statistically significant at the 0.10 / 0.05 / 0.01 level based on a chi-squared test.

**Table C.6. Select program intake and engagement measures, by site**

	Overall mean	Chicago	Cleveland	Los Angeles	Portland
<b>SET intake</b>					
Had intake meeting	84.6	78.7 <sup>†††</sup>	82.7	91.3 <sup>††</sup>	92.3 <sup>†††</sup>
Had intake meeting within two weeks of enrollment	52.9	37.3 <sup>†††</sup>	49.6	66.0 <sup>†††</sup>	73.1 <sup>†††</sup>
<b>SET engagement</b>					
Had any contact with SET provider	83.8	77.8 <sup>†††</sup>	82.3	89.2 <sup>†</sup>	91.6 <sup>†††</sup>
Active in program for at least 6 months	68.2	59.8 <sup>†††</sup>	61.1 <sup>††</sup>	86.3 <sup>†††</sup>	79.5 <sup>†††</sup>
Provider follow-up contacts in at least 3 of the first 6 months	69.0	65.6 <sup>†</sup>	63.7 <sup>†</sup>	59.8 <sup>††</sup>	82.1 <sup>†††</sup>
Had at least one quarterly reassessment	46.1	47.9	43.8	8.8 <sup>†††</sup>	60.1 <sup>†††</sup>
Submitted business plan that was approved by SET provider	41.0	27.8 <sup>†††</sup>	36.7	69.6 <sup>†††</sup>	52.5 <sup>†††</sup>
Received seed capital microgrant	36.0	24.1 <sup>†††</sup>	35.0	52.4 <sup>†††</sup>	47.6 <sup>†††</sup>
<b>Sample size</b>	<b>990</b>	<b>390</b>	<b>226</b>	<b>103</b>	<b>271</b>

Source: SET study management information system (MIS).

Note: The table presents percentages for each measure calculated among SET program group members for whom the corresponding MIS data elements were available. All numbers in the table have been rounded; consequently reported percentages might not sum across categories to exactly 100. One member of the SET program group was not assigned to a provider and thus had no MIS data available. Almost 98 percent of the remaining 990 people assigned to SET providers had nonmissing values for every MIS measure, although providers did not necessarily record all instances of program engagement or milestone attainment.

† / †† / ††† Denotes a difference between the given site and the other three sites combined that is statistically significant at the 0.10 / 0.05 / 0.01 level based on a chi-squared test.



**APPENDIX D:**  
**TABLES OF RESULTS FROM**  
**THE MAIN IMPACT ANALYSIS**

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**Table D.1. Primary work outcomes at the time of the 18-month survey**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Self-employed at survey date (%)<sup>a</sup></b>							
<b>Overall</b>	818	67.5	769	56.7	56.2	11.3***	(2.4)
Chicago	322	64.5	302	60.3	59.1	5.4 <sup>†</sup>	(3.9)
Cleveland	180	60.3	166	43.7	44.6	15.6***	(5.4)
Los Angeles	90	73.9	81	68.4	65.3	8.6	(6.9)
Portland	226	75.6	220	58.1	58.7	16.9***	(4.4)
<b>Employed in any job at survey date (%)<sup>b</sup></b>							
<b>Overall</b>	818	91.7	769	88.5	88.4	3.3**	(1.5)
Chicago	322	88.6	302	91.6	91.0	-2.4 <sup>††</sup>	(2.4)
Cleveland	180	93.5	166	83.1	84.0	9.5*** <sup>††</sup>	(3.5)
Los Angeles	90	92.6	81	88.0	87.8	4.9	(4.7)
Portland	226	94.3	220	88.8	88.7	5.6**	(2.6)
<b>Total earnings over past 12 months (\$) <sup>c</sup></b>							
<b>Overall</b>	797	21,118	746	21,533	21,744	-626	(1,158)
Chicago	314	20,060	287	20,695	20,775	-715	(1,877)
Cleveland	174	20,496	162	18,710	20,053	443	(2,263)
Los Angeles	87	13,662	80	17,330	15,889	-2,228	(3,139)
Portland	222	25,904	217	26,598	26,689	-785	(2,383)

Source: SET study intake forms and 18-month follow-up survey data.

Note: Summary statistics and regression estimates for each outcome are based only on survey respondents who answered the corresponding question(s). As discussed in Appendix B, regression estimates are based on a model that accounts for the characteristics of study enrollees at intake, includes site fixed effects, uses weights to adjust for survey nonresponse, and produces heteroscedasticity-robust standard errors. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup> Self-employment is based on the question: "Are you currently self-employed in your own business, professional practice, farm, or some other business venture?"

<sup>b</sup> Employment in any job includes both self-employment and "working for a company or someone else in a job [that pays] a salary, hourly wage, or commissions."

<sup>c</sup> Total earnings are the sum of (1) reported earnings from all self-employment ventures undertaken or pursued over the past 12 months, as defined in Appendix Table D.4; and (2) reported earnings from all wage/salary jobs held over the past 12 months, as defined in Appendix Table D.5.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

† / †† / ††† Denotes a difference in impact estimates for the given site and the other three sites combined that is statistically significant at the .10 / .05 / .01 level based on a chi-squared test.

**Table D.2. Receipt of self-employment services, training, and other job placement services between study enrollment and the 18-month survey**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Received any self-employment service or support (%)<sup>a</sup></b>							
<b>Overall</b>	818	87.0	769	62.4	62.8	24.2***	(2.1)
Chicago	322	86.1	302	64.2	64.3	21.8***	(3.4)
Cleveland	180	85.8	166	53.4	54.6	31.3***†	(4.7)
Los Angeles	90	85.8	81	70.5	68.2	17.5***	(6.3)
Portland	226	89.9	220	64.4	65.6	24.2***	(3.9)
<b>Received any personalized self-employment support (%)<sup>b</sup></b>							
<b>Overall</b>	814	61.9	768	23.6	23.7	38.2***	(2.3)
Chicago	318	59.1	301	24.5	24.7	34.4***	(3.7)
Cleveland	180	63.7	166	17.3	17.9	45.8***†	(4.6)
Los Angeles	90	54.6	81	29.3	26.5	28.1***	(7.4)
Portland	226	67.0	220	25.4	26.0	41.0***	(4.4)
<b>Number of times received personalized self-employment support<sup>b</sup></b>							
<b>Overall</b>	814	4.7	767	1.6	1.6	3.1***	(0.2)
Chicago	318	4.8	301	1.8	1.8	3.0***	(0.4)
Cleveland	180	4.2	166	1.0	1.1	3.1***	(0.5)
Los Angeles	90	3.6	80	1.5	1.1	2.5***	(0.7)
Portland	226	5.4	220	1.8	1.9	3.6***	(0.5)
<b>Attended any in-person self-employment classes/training (%)</b>							
<b>Overall</b>	816	63.0	767	33.1	33.2	29.8***	(2.4)
Chicago	321	61.5	301	33.6	33.2	28.4***	(3.9)
Cleveland	180	57.4	166	26.4	26.6	30.8***	(5.0)
Los Angeles	89	66.0	81	46.4	45.8	20.2***	(7.5)
Portland	226	68.7	219	33.2	34.2	34.5***	(4.6)
<b>Accessed any online courses on starting, operating, or growing a business (%)</b>							
<b>Overall</b>	814	41.8	767	34.0	34.5	7.3***	(2.4)
Chicago	320	51.1	301	36.1	36.6	14.5***††	(4.0)
Cleveland	179	40.4	165	30.0	30.9	9.5*	(5.1)
Los Angeles	89	32.0	81	36.2	35.2	-3.1	(7.5)
Portland	226	33.5	220	33.7	34.1	-0.6††	(4.6)

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Attended any in-person peer advice or networking meetings for self-employment (%)</b>							
<b>Overall</b>	813	56.6	765	38.2	38.8	17.8***	(2.5)
Chicago	321	54.6	301	40.1	40.4	14.2***	(4.0)
Cleveland	180	57.2	166	30.4	31.0	26.2***†	(5.1)
Los Angeles	87	53.3	81	48.5	48.2	5.2†	(7.8)
Portland	225	60.1	217	38.2	39.7	20.4***	(4.8)
<b>Received any job placement services/career counseling from American Job Center or state labor exchange (%)</b>							
<b>Overall</b>	803	9.1	746	9.2	9.3	-0.2	(1.5)
Chicago	317	8.7	290	9.4	9.1	-0.4	(2.4)
Cleveland	178	11.7	161	12.0	11.8	-0.1	(3.5)
Los Angeles	86	10.5	80	15.0	16.1	-5.6	(5.5)
Portland	222	7.1	215	4.2	5.2	1.9	(2.4)

Source: SET study intake forms and 18-month follow-up survey data.

Note: All outcomes are measured over the time between random assignment and the 18-month survey. Summary statistics and regression estimates for each outcome are based only on survey respondents who answered the corresponding question(s). As discussed in Appendix B, regression estimates are based on a model that accounts for the characteristics of study enrollees at intake, includes site fixed effects, uses weights to adjust for survey nonresponse, and produces heteroscedasticity-robust standard errors. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup> This measure includes the following self-employment services and supports: attendance of in-person classes or training, completion of online courses, participation in peer advice or networking groups, work with a mentor, and/or receipt of personalized self-employment support (as defined below).

<sup>b</sup> Personalized self-employment support includes regular, one-on-one meetings with self-employment advisors and/or “individualized support ... tailored to the needs or specific issues [encountered] in starting or developing [a] business” (that is, technical assistance). This measure excludes meetings with an experienced business owner mentor, unless survey respondents also indicated that the mentor acted as an advisor or provided individualized technical assistance.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

† / †† / ††† Denotes a difference in impact estimates for the given site and the other three sites combined that is statistically significant at the .10 / .05 / .01 level based on a chi-squared test.

**Table D.3. Business development activity between study enrollment and the 18-month survey**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Wrote or completed business plan (%)</b>							
<b>Overall</b>	814	66.4	763	47.9	48.9	17.5***	(2.5)
Chicago	320	57.8	301	41.3	41.4	16.4***	(4.0)
Cleveland	179	66.4	165	44.2	45.5	20.9***	(5.4)
Los Angeles	89	75.6	80	46.9	48.7	26.9***	(7.6)
Portland	226	75.1	217	60.7	62.4	12.6***	(4.5)
<b>Took active steps to formalize main business venture (%)<sup>a</sup></b>							
<b>Overall</b>	818	64.5	765	54.6	54.2	10.3***	(2.4)
Chicago	322	53.9	300	51.3	49.4	4.5 <sup>†</sup>	(4.0)
Cleveland	180	59.8	166	43.7	45.1	14.7***	(5.2)
Los Angeles	90	70.6	80	61.1	60.1	10.5	(7.2)
Portland	226	81.4	219	66.0	66.6	14.7***	(4.1)
<b>Received nonborrowed funds from any source to start/grow business (%)<sup>b</sup></b>							
<b>Overall</b>	818	48.6	765	20.5	20.7	27.9***	(2.3)
Chicago	322	40.3	301	24.6	23.8	16.5***†††	(3.7)
Cleveland	180	45.9	166	17.8	19.3	26.5***	(4.8)
Los Angeles	90	64.6	80	19.8	18.5	46.1***†††	(6.7)
Portland	226	56.8	218	17.1	18.3	38.4***†††	(4.3)
<b>Borrowed money from any source to start/grow business (%)</b>							
<b>Overall</b>	816	26.8	769	25.8	25.6	1.2	(2.2)
Chicago	321	22.8	302	22.4	22.4	0.4	(3.4)
Cleveland	180	26.3	166	21.0	22.2	4.1	(4.6)
Los Angeles	90	22.9	81	26.6	23.3	-0.4	(6.7)
Portland	225	34.4	220	34.3	34.2	0.3	(4.5)

Source: SET study intake forms and 18-month follow-up survey data.

Note: All outcomes are measured over the time between random assignment and the 18-month survey. Summary statistics and regression estimates for each outcome are based only on survey respondents who answered the corresponding question(s). As discussed in Appendix B, regression estimates are based on a model that accounts for the characteristics of study enrollees at intake, includes site fixed effects, uses weights to adjust for survey nonresponse, and produces heteroscedasticity-robust standard errors. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup> Business formalization steps include having registered the business with the state, country, and/or municipality; having incorporated the business; and/or having obtained an employer identification number or tax identification number. This question was asked of survey respondents who had been self-employed at any point since random assignment (see Appendix Table D.4). Among those who worked on more than one business venture since random assignment, the question was asked only for what they identified as their “main” venture over that timeframe.

<sup>b</sup> Nonborrowed funds include any SET seed capital microgrants reported by the program group.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

† / †† / ††† Denotes a difference in impact estimates for the given site and the other three sites combined that is statistically significant at the .10 / .05 / .01 level based on a chi-squared test.

**Table D.4. Self-employment activity based on the 18-month survey**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Was self-employed at any point since study enrollment (%)<sup>a</sup></b>							
<b>Overall</b>	818	77.2	769	71.2	70.6	6.6***	(2.2)
Chicago	322	72.8	302	74.8	73.2	-0.5†††	(3.5)
Cleveland	180	72.0	166	56.2	57.1	15.0***††	(5.1)
Los Angeles	90	82.4	81	79.1	77.7	4.7	(6.1)
Portland	226	86.0	220	75.5	75.9	10.1***	(3.7)
<b>Total hours worked in self-employment over the past 12 months<sup>b</sup></b>							
<b>Overall</b>	811	1,079	767	929	929	150***	(55)
Chicago	318	993	300	900	871	122	(85)
Cleveland	180	982	166	661	700	282**	(117)
Los Angeles	90	1,142	81	1,139	1,131	11	(179)
Portland	223	1,261	220	1,118	1,133	128	(109)
<b>Worked in self-employment for at least 20 hours per week in every week of the past 12 months (%)<sup>c</sup></b>							
<b>Overall</b>	812	30.9	767	24.5	24.6	6.3***	(2.2)
Chicago	319	24.5	300	25.0	24.2	0.3††	(3.5)
Cleveland	180	34.3	166	17.1	18.6	15.7***††	(4.8)
Los Angeles	90	27.9	81	31.8	31.4	-3.5	(7.1)
Portland	223	38.4	220	27.4	27.7	10.7**	(4.5)
<b>Earnings from self-employment over the past 12 months (\$) <sup>d</sup></b>							
<b>Overall</b>	807	4,870	759	4,819	4,787	83	(598)
Chicago	318	3,776	296	3,290	3,217	559	(769)
Cleveland	178	3,151	164	2,444	3,019	132	(1,061)
Los Angeles	88	2,501	80	4,368	3,355	-854	(1,714)
Portland	223	8,732	219	9,122	9,013	-281	(1,570)
<b>Number of employees in main business venture since study enrollment<sup>e</sup></b>							
<b>Overall</b>	816	0.2	765	0.4	0.3	-0.1	(0.1)
Chicago	322	0.2	302	0.5	0.5	-0.3	(0.2)
Cleveland	180	0.2	166	0.4	0.4	-0.2	(0.2)
Los Angeles	89	0.1	79	0.2	0.3	-0.2	(0.1)
Portland	225	0.3	218	0.2	0.2	0.1†	(0.1)

Source: SET study intake forms and 18-month follow-up survey data.

Note: Summary statistics and regression estimates for each outcome are based only on survey respondents who answered the corresponding question(s). As discussed in Appendix B, regression estimates are based on a model that accounts for the characteristics of study enrollees at intake, includes site fixed



effects, uses weights to adjust for survey nonresponse, and produces heteroscedasticity-robust standard errors. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup> This measure includes those who were either (1) currently self-employed, as define in Appendix Table D.1; or (2) had “owned [a] business or been self-employed in [their] own business, professional practice, farm, or some other business venture” since the random assignment date.

<sup>b</sup> Respondents were asked to provide the following information, aggregated across all businesses operated or self-employment ventures undertaken over the past 12 months: (1) the reported average number of hours worked per week, and (2) the reported total number of weeks worked. We calculated total hours worked as the product of these two components.

<sup>c</sup> We calculated this measure based on the two measures used to calculate total hours worked and current employment status. We set the measure to one only for people who reported that they had: (1) worked at least 20 hours per week, on average, in self-employment over the past 12 months, (2) had worked on a self-employment venture for all 52 weeks of the past year, and (3) were self-employed as of the survey date.

<sup>d</sup> Respondents were asked how much they earned or paid themselves in total over the past 12 months from all businesses operated or self-employment ventures undertaken over that period.

<sup>e</sup> Respondents who had worked on more than one business venture since the random assignment date were asked to report how many employees they had in their main venture. This information was recorded both for business ventures still operating at the time of the survey and business ventures that had ended.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

† / †† / ††† Denotes a difference in impact estimates for the given site and the other three sites combined that is statistically significant at the .10 / .05 / .01 level based on a chi-squared test.

**Table D.5. Work in wage/salary jobs based on the 18-month survey**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Employed in wage/salary job at the survey date (%)<sup>a</sup></b>							
<b>Overall</b>	818	59.4	769	60.1	60.2	-0.8	(2.5)
Chicago	322	57.8	302	63.3	63.1	-5.3	(3.9)
Cleveland	180	66.2	166	61.1	61.5	4.7	(5.2)
Los Angeles	90	51.1	81	53.2	54.6	-3.5	(7.6)
Portland	226	59.1	220	57.5	57.1	1.9	(4.7)
<b>Total hours worked in wage/salary jobs over the past 12 months<sup>b</sup></b>							
<b>Overall</b>	804	922	755	949	962	-40	(46)
Chicago	318	928	294	997	1,004	-77	(74)
Cleveland	174	1,119	164	1,080	1,094	25	(106)
Los Angeles	89	666	80	758	805	-139	(122)
Portland	223	849	217	844	855	-5	(85)
<b>Earnings from wage/salary jobs over the past 12 months (\$)<sup>c</sup></b>							
<b>Overall</b>	805	15,914	752	16,462	16,649	-735	(1,052)
Chicago	316	16,087	289	17,095	17,321	-1,234	(1,733)
Cleveland	176	16,682	164	16,098	16,481	202	(2,089)
Los Angeles	89	10,579	81	13,797	13,555	-2,976	(2,753)
Portland	224	17,061	218	16,896	17,036	25	(2,098)

Source: SET study intake forms and 18-month follow-up survey data.

Note: Summary statistics and regression estimates for each outcome are based only on survey respondents who answered the corresponding question(s). As discussed in Appendix B, regression estimates are based on a model that accounts for the characteristics of study enrollees at intake, includes site fixed effects, uses weights to adjust for survey nonresponse, and produces heteroscedasticity-robust standard errors. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup> Wage/salary employment is based on the question: "Are you currently working for a company or someone else in a job where you are paid a salary, hourly wage, or commissions?"

<sup>b</sup> Respondents were asked to provide the following information, aggregated across all wage/salary jobs over the past 12 months: (1) the reported average number of hours worked per week, and (2) the reported total number of weeks worked. We calculated total hours worked as the product of these two components.

<sup>c</sup> Respondents were asked how much they earned in wages, salaries, commissions, bonuses, or tips over the past 12 months from all jobs in which they "worked for a company or someone else."

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

† / †† / ††† Denotes a difference in impact estimates for the given site and the other three sites combined that is statistically significant at the .10 / .05 / .01 level based on a chi-squared test.

**Table D.6. Other measures of work and wellbeing based on the 18-month survey**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Dually employed in both self-employment and wage/salary employment at survey date (%)<sup>a</sup></b>							
<b>Overall</b>	818	35.3	769	28.4	28.1	7.2***	(2.4)
Chicago	322	33.7	302	32.0	31.2	2.6	(3.9)
Cleveland	180	33.0	166	21.6	22.2	10.8**	(4.9)
Los Angeles	90	32.3	81	33.6	32.1	0.2	(7.3)
Portland	226	40.4	220	26.8	27.1	13.2***	(4.5)
<b>Satisfied with employment situation, among those employed at survey date (%)<sup>b</sup></b>							
<b>Overall</b>	757	49.3	690	48.6	47.4	1.9	(2.6)
Chicago	289	45.1	281	40.6	40.8	4.3	(4.2)
Cleveland	169	40.0	140	43.8	43.7	-3.8	(5.5)
Los Angeles	83	42.6	73	45.6	40.9	1.8	(8.1)
Portland	216	65.1	196	64.9	62.1	3.0	(4.8)
<b>Received unemployment insurance benefits during past 12 months (%)</b>							
<b>Overall</b>	811	16.1	762	15.8	16.1	-0.0	(1.9)
Chicago	321	17.3	299	18.5	18.5	-1.2	(3.2)
Cleveland	178	12.0	165	16.0	16.1	-4.1	(3.7)
Los Angeles	88	21.8	80	14.5	16.1	5.8	(5.9)
Portland	224	15.5	218	12.4	12.6	2.9	(3.3)

Source: SET study intake forms and 18-month follow-up survey data.

Note: Summary statistics and regression estimates for each outcome are based only on survey respondents who answered the corresponding question(s). As discussed in Appendix B, regression estimates are based on a model that accounts for the characteristics of study enrollees at intake, includes site fixed effects, uses weights to adjust for survey nonresponse, and produces heteroscedasticity-robust standard errors. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup> This measure includes those who were both (1) currently self-employed, as defined in Appendix Table D.1; and (2) currently working in a wage/salary job, as defined in Appendix Table D.5.

<sup>b</sup> Job satisfaction questions were posed to two groups of survey respondents: (1) those who were employed in any job at the survey date, as defined in Appendix Table D.1 (90 percent of respondents); and (2) those who were not employed at that time but had taken steps to formalize their main small business venture, as defined in Appendix Table D.3 (2 percent of respondents). Hence, this measure applies primarily to survey respondents who were working, and the estimated differences between the program and control groups may not correspond to impacts for all study enrollees responding to the survey. The measure in the table indicates those who said that they were “extremely satisfied” or “somewhat satisfied” with their current employment situation versus those who said they were “neither satisfied nor dissatisfied,” “somewhat dissatisfied,” or “extremely dissatisfied.”

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

† / †† / ††† Denotes a difference in impact estimates for the given site and the other three sites combined that is statistically significant at the .10 / .05 / .01 level based on a chi-squared test.

**Table D.7. Self-employment at the time of the 18-month survey, by subgroup**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Recent self-employment experience</b>							
None in past 5 years	523	64.2	477	47.9	48.0	16.1****†	(3.5)
Self-employed at time of enrollment or in past 5 years	295	76.8	292	72.9	72.4	4.4††	(3.9)
<b>Cash assets</b>							
\$1,000 or less	501	65.5	454	53.1	52.1	13.4***	(3.5)
More than \$1,000	315	73.9	312	64.3	63.7	10.2**	(4.4)
<b>Receipt of unemployment insurance benefits</b>							
No	412	68.7	398	56.8	56.7	12.0***	(3.7)
Yes	406	68.4	371	57.7	56.1	12.2***	(4.1)
<b>Age</b>							
Less than 45	390	67.7	362	55.6	55.3	12.4***	(3.9)
45 or older	428	69.7	407	59.4	58.4	11.3***	(3.5)
<b>Gender</b>							
Female	493	67.5	464	57.5	57.7	9.8***	(3.4)
Male	325	70.2	305	57.4	56.1	14.1***	(4.1)
<b>Race/ethnicity<sup>a</sup></b>							
Hispanic, black, or other nonwhite race	403	62.7	375	53.8	54.3	8.4**	(4.2)
White, non-Hispanic	315	70.7	307	55.2	54.4	16.3***	(4.1)

Source: SET study intake forms and 18-month follow-up survey data.

Note: All subgroups were defined based on characteristics measured at the time of study enrollment. Means are reported as percentages; impact estimates and standard errors are reported as percentage points. See Appendix Table D.1 for how this outcome is defined; all respondents answered the corresponding question on the 18-month survey. As discussed in Appendix B, regression estimates are based on a model that accounts for the characteristics of study enrollees at intake, includes site fixed effects, uses weights to adjust for survey nonresponse, scales the weights so that sites contribute equally to each impact estimate, and produces heteroscedasticity-robust standard errors. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup>Los Angeles was not included in comparisons by race/ethnicity due to an insufficient sample size. See Appendix B for more information.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

† / †† / ††† Denotes a difference in impact estimates between subgroups that is statistically significant at the .10 / .05 / .01 level based on a chi-squared test.

**Table D.8. Employment in any job at the time of the 18-month survey, by subgroup**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Recent self-employment experience</b>							
None in past 5 years	523	92.3	477	86.4	86.5	5.8***	(2.2)
Self-employed at time of enrollment or in past 5 years	295	92.3	292	89.9	89.8	2.5	(2.6)
<b>Cash assets</b>							
\$1,000 or less	501	91.3	454	86.3	86.4	4.9**	(2.2)
More than \$1,000	315	94.0	312	91.6	91.3	2.7	(2.7)
<b>Receipt of unemployment insurance benefits</b>							
No	412	90.5	398	87.1	86.9	3.6	(2.5)
Yes	406	94.6	371	88.6	88.6	5.9**	(2.5)
<b>Age</b>							
Less than 45	390	93.2	362	90.4	90.4	2.8	(2.4)
45 or older	428	91.3	407	85.6	85.5	5.8**	(2.4)
<b>Gender</b>							
Female	493	92.6	464	87.9	88.4	4.2*	(2.2)
Male	325	91.9	305	87.5	86.9	5.0*	(2.7)
<b>Race/ethnicity<sup>a</sup></b>							
Hispanic, black, or other nonwhite race	403	92.1	375	86.8	86.1	6.0**	(2.6)
White, non-Hispanic	315	94.2	307	89.4	89.8	4.4*	(2.3)

Source: SET study intake forms and 18-month follow-up survey data.

Note: All subgroups were defined based on characteristics measured at the time of study enrollment. Means are reported as percentages; impact estimates and standard errors are reported as percentage points. See Appendix Table D.1 for how this outcome is defined; all respondents answered the corresponding questions on the 18-month survey. As discussed in Appendix B, regression estimates are based on a model that accounts for the characteristics of study enrollees at intake, includes site fixed effects, uses weights to adjust for survey nonresponse, scales the weights so that sites contribute equally to each impact estimate, and produces heteroscedasticity-robust standard errors. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup>Los Angeles was not included in comparisons by race/ethnicity due to an insufficient sample size. See Appendix B for more information.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

† / †† / ††† Denotes a difference in impact estimates between subgroups that is statistically significant at the .10 / .05 / .01 level based on a chi-squared test.

**Table D.9. Total earnings in the 12 months leading up to the 18-month survey, by subgroup**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
<b>Recent self-employment experience</b>							
None in past 5 years	510	19,820	464	21,156	21,170	-1,350	(1,548)
Self-employed at time of enrollment or in past 5 years	287	20,342	282	20,544	20,685	-343	(1,947)
<b>Cash assets</b>							
\$1,000 or less	484	16,189	435	17,284	17,067	-878	(1,347)
More than \$1,000	311	26,576	308	28,137	29,412	-2,836	(2,973)
<b>Receipt of unemployment insurance benefits</b>							
No	401	18,388	386	17,999	18,105	282	(1,739)
Yes	396	21,727	360	25,090	25,391	-3,664*	(1,972)
<b>Age</b>							
Less than 45	381	21,246	351	22,435	22,682	-1,435	(1,709)
45 or older	416	19,094	395	19,469	19,599	-505	(1,721)
<b>Gender</b>							
Female	477	20,251	451	18,852	19,758	492	(1,513)
Male	320	19,937	295	23,672	22,676	-2,740	(2,015)
<b>Race/ethnicity</b>							
Hispanic, black, or other nonwhite race	387	20,522	356	18,416	18,177	2,345	(1,837)
White, non-Hispanic	313	26,073	304	26,215	26,655	-582	(2,241)

Source: SET study intake forms and 18-month follow-up survey data.

Note: All subgroups were defined based on characteristics measured at the time of study enrollment. Means and impact estimates are reported as dollar amounts, and they calculated based only on data for survey respondents who answered the corresponding questions on the 18-month survey. See Appendix Table D.1 for how this outcome is defined. As discussed in Appendix B, regression estimates are based on a model that accounts for the characteristics of study enrollees at intake, includes site fixed effects, uses weights to adjust for survey nonresponse, scales the weights so that sites contribute equally to each impact estimate, and produces heteroscedasticity-robust standard errors. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup>Los Angeles was not included in comparisons by race/ethnicity due to an insufficient sample size. See Appendix B for more information.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

† / †† / ††† Denotes a difference in impact estimates between subgroups that is statistically significant at the .10 / .05 / .01 level based on a chi-squared test.

**APPENDIX E:**

**TABLES OF RESULTS FROM SENSITIVITY  
ANALYSES FOR PRIMARY IMPACT MEASURES**

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**Table E.1. Self-employment at the time of the 18-month survey: Sensitivity analysis**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
Primary approach: linear regression, nonresponse weights, sites scaled by sample size	818	67.5	769	56.7	56.2	11.3***	(2.4)
Sensitivity check 1: logistic instead of linear regression <sup>a</sup>	818	67.5	769	56.7	56.2	11.3***	(2.4)
Sensitivity check 2: no controls for baseline characteristics	818	67.5	769	56.7	56.7	10.8***	(2.5)
Sensitivity check 3: unweighted regression	818	67.4	769	57.2	56.6	10.8***	(2.4)
Sensitivity check 4: adjust for differential attrition by trimming the sample based on response times <sup>a</sup>	770	68.3	769	56.8	56.4	11.9***	(2.4)
Sensitivity check 5: sites weighted equally	818	68.6	769	57.6	57.0	11.6***	(2.6)

Source: SET study intake forms and 18-month follow-up survey data.

Note: Means are reported as percentages; impact estimates and standard errors are reported as percentage points. See Appendix Table D.1 for how this outcome is defined; all respondents answered the corresponding question on the 18-month survey. All regression models include site fixed effects and produce heteroscedasticity-robust standard errors. Our primary approach also uses a linear model to account for baseline characteristics and uses weights to adjust for survey nonresponse. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup> For this sensitivity check, we convert logit estimates into percentage point impacts by calculating average partial effects for the program assignment indicator.

<sup>b</sup> For this sensitivity check, we calculated amount of time elapsed between when respondents were asked to complete the 18-month survey and when they completed the survey. We then removed the SET program group members with the longest response times until the effective response rate (after trimming) for the group was the same as the actual response rate for the control group.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

**Table E.2. Employed in any job at the time of the 18-month survey: Sensitivity analysis**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
Primary approach: linear regression, nonresponse weights, sites scaled by sample size	818	91.7	769	88.5	88.4	3.3**	(1.5)
Sensitivity check 1: logistic instead of linear regression <sup>a</sup>	818	91.7	769	88.5	88.3	3.4**	(1.5)
Sensitivity check 2: no controls for baseline characteristics	818	91.7	769	88.5	88.5	3.2**	(1.5)
Sensitivity check 3: unweighted regression	818	91.5	769	88.5	88.6	2.9*	(1.5)
Sensitivity check 4: adjust for differential attrition by trimming the sample based on response times <sup>a</sup>	770	91.6	769	88.5	88.5	3.1**	(1.5)
Sensitivity check 5: sites weighted equally	818	92.2	769	87.9	87.8	4.4***	(1.7)

Source: SET study intake forms and 18-month follow-up survey data.

Note: Means are reported as percentages; impact estimates and standard errors are reported as percentage points. See Appendix Table D.1 for how this outcome is defined; all respondents answered the corresponding question on the 18-month survey. All regression models include site fixed effects and produce heteroscedasticity-robust standard errors. Our primary approach also uses a linear model to account for baseline characteristics and uses weights to adjust for survey nonresponse. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup> For this sensitivity check, we convert logit estimates into percentage point impacts by calculating average partial effects for the program assignment indicator.

<sup>b</sup> For this sensitivity check, we calculated amount of time elapsed between when respondents were asked to complete the 18-month survey and when they completed the survey. We then removed the SET program group members with the longest response times until the effective response rate (after trimming) for the group was the same as the actual response rate for the control group.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

**Table E.3. Total earnings in the 12 months leading up to the 18-month survey: Sensitivity analysis**

	SET program group		Control group		Regression estimates		
	Sample size	Mean of outcome	Sample size	Mean of outcome	Adjusted control group outcome mean	Impact	Standard error
Primary approach: linear regression, nonresponse weights, sites scaled by sample size	797	21,118	746	21,533	21,744	-626	(1,158)
Sensitivity check 1: logistic instead of linear regression <sup>a</sup>	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Sensitivity check 2: no controls for baseline characteristics	797	21,118	746	21,533	21,533	-415	(1,210)
Sensitivity check 3: unweighted regression	797	21,483	746	22,170	22,380	-897	(1,186)
Sensitivity check 4: adjust for differential attrition by trimming the sample based on response times <sup>a</sup>	755	20,825	746	21,545	21,896	-1,071	(1,171)
Sensitivity check 5: sites weighted equally	797	20,030	746	20,833	20,947	-917	(1,198)

Source: SET study intake forms and 18-month follow-up survey data.

Note: Means and impact estimates are reported as dollar amounts, and they are calculated based only on data for survey respondents who answered the corresponding questions on the 18-month survey. See Appendix Table D.1 for how this outcome is defined. All regression models include site fixed effects and produce heteroscedasticity-robust standard errors. Our primary approach also uses a linear model to account for baseline characteristics and weights to adjust for survey nonresponse. The adjusted control group mean is the mean of the SET program group minus the estimated impact.

<sup>a</sup> Entries of “n.a.” in this row indicate “not applicable” because logistic regression is used for binary outcomes, but total earnings is a continuous outcome measure.

<sup>b</sup> For this sensitivity check, we calculated amount of time elapsed between when respondents were asked to complete the 18-month survey and when they completed the survey. We then removed the SET program group members with the longest response times until the effective response rate (after trimming) for the group was the same as the actual response rate for the control group.

\* / \*\* / \*\*\* Denotes an impact estimate that is significantly different from zero at the .10 / .05 / .01 level based on a two-tailed *t*-test.

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