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FIGHTING POVERTY ONE FAMILY AT A TIME: EXPERIMENTAL EVIDENCE FROM AN INTERVENTION WITH HOLISTIC, INDIVIDUALIZED, WRAP-AROUND SERVICES

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Fighting Poverty One Family at a Time: Experimental Evidence from an Intervention with Holistic, Individualized, Wrap-Around Services
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ABSTRACT

Families in poverty face numerous barriers to establishing stable economic footing. This paper examines the effect of a holistic, individualized wrap-around service intervention on outcomes for low-income individuals. The intervention includes a detailed assessment, an individualized service plan, intensive case management administered by a two-person team with small caseloads, and temporary financial assistance used to overcome obstacles to self-sufficiency and incentivize behavior. We evaluate the intervention through a randomized controlled trial among participants seeking assistance at a local social service provider. Results indicate that the program improved labor market and housing outcomes two years after enrollment. Given the customized nature of the services, overall program effects might mask important heterogeneity. Exploratory analysis suggests the program helped employ participants who lacked employment but had stable housing, and that those without stable housing were helped in securing it.

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A data appendix is available at http://www.nber.org/data-appendix/w30992

I Introduction

Designing policies that effectively move families out of poverty is challenging because the reasons a family may find themselves in poverty can be quite varied. Their current economic situation can be the result of a disability, lack of skills, loss of a home to crippling medical debt, being a single parent, recovering from an addiction, having a criminal record, being an undocumented worker, not having adequate transportation to reliably get to a job, just to name a few. Moreover, in many situations, poor households have multiple hurdles to cross to move to a more stable economic footing. In the National Survey of America's families, Loprest and Zedlewski (2006) identified eight barriers to work among current welfare recipients. Only 20 percent had no barriers to work, while 29 percent had one barrier, 29 percent had two, and 22 percent had three or more. In a survey of long-term welfare recipients, Taylor and Barusch (2004) found 57 percent had two to four barriers to work and 23 percent had five to eight. A successful policy response to poverty, therefore, may need to attack many fronts at once.

The problem becomes more complicated for families in poverty as their current situation may adversely impact their problem-solving skills. Mullainathan and Shafir (2013) argue individuals in poor financial situation have reduced bandwidth to deal with some longer-term issues than others. Someone worried about how to feed their family this evening or where they will be living next week are not in a position to think about longer-term investments that need to be made to move them on a path to self-sufficiency. Experimental work by Mani et al. (2013) demonstrates that poverty impedes cognitive function, especially in dealing with more complicated financial problems, indicating that decision-making during this time may be impaired as well. Even after a well thought-out plan is devised, there are many obstacles to success. The poor and near-poor are often one setback away from crisis and deep hardship. Blank and Barr (2009) note that "low-income families lack access to many of the basic financial services middle-class families take for granted and are particularly susceptible to financial emergencies, unemployment, loss of a home, and uninsured medical problems." Bertrand, Mullainathan and Shafir (2004) describe this aspect of poverty in terms of having "narrow margins for error."

In this paper, we examine the effect of a new social service delivery model intended to reduce poverty. This intervention, Padua, was designed and implemented by Catholic Charities Fort Worth (CCFW) and is explicitly constructed to help individuals and families recognize and overcome the barriers they face and permanently lift themselves out of poverty.[1] The program targets working age adults who are able and willing to work, but face significant barriers to self-sufficiency or the ability to earn sufficient earnings, limit the need for public or private assistance. This holistic, individualized wrap-around intervention requires substantial time and resources. Initially, the individual completes a detailed assessment that identifies the individual's strengths, goals, and obstacles they face. The individual then works with their two-person case management team to devise a service plan that maps out their journey to success. The service plan recognizes that individuals face multiple and interrelated obstacles to financial security and is meant to be a tool that helps them prioritize and methodically tackle issues and achieve goals. The plan is individualized to the particular client and holistic in that it considers many different aspects of

[1] The formal name of the program is the Padua^{TM} Pilot. For brevity, we refer to the program throughout the paper as simply Padua.

the client's life. The case management team then supports the client with services and referrals needed to make progress on the service plan. These services encompass a variety of interventions including job training, housing assistance, immigration assistance, budgeting, financial literacy, and coaching for overall well-being. Where case management teams cannot provide assistance directly, they coordinate access to other private or public programs and services. To provide sufficient time to work with clients, case management teams carry smaller caseloads. Finally, clients have access to flexible financial assistance to help reduce liquidity issues that threaten their success. Despite the intuitive appeal of the program, Padua is an expensive intervention and must alter outcomes greatly in order to be cost effective.

Previous studies have examined the impact of interventions that include some of the components of Padua in contexts where participants face specific challenges, such as severe mental illness, chronic homelessness, reentry from prison, neighborhood choice, and education. While the results from these studies are mixed, the evidence is promising in many specific circumstances. For example, comprehensive interventions have shown to improve housing stability for the chronically homeless (Gulcur et al., 2003; Goering et al., 2014) or graduation for community college students (Evans et al., 2020; Weiss et al., 2019). The evidence from studies of comprehensive programs that focus on labor market outcomes have generally shown sizable, positive effects on earnings, particularly while clients are receiving services (Meckstroth et al., 2008; Duncan, Huston and Weisner, 2007; Miller et al., 2008; Fein and Hamadyk, 2018; Barden et al., 2018; Barham, Cadena and Turner, 2022). More generally, there is some suggestive evidence that intensive, holistic programs can be more effective than usual care, particularly among individuals with low baseline values of the primary outcomes being targeted by the intervention, for example, non-employment in a workforce intervention (Barden et al., 2018). Last, while successful interventions have paired intensive case management with substantial financial assistance (e.g., housing vouchers, subsidized employment, emergency financial assistance), there is some evidence that assistance alone may not be as effective (Evans et al., 2019).

Our study builds on this literature in three important ways. First, we test whether comprehensive services are effective for a broader population of low-income individuals and families. Second, Padua relies on a case management team that focuses on a larger set of outcomes, provides more intensive services, and works with clients for longer periods of time than most other programs, allowing us to consider whether such a comprehensive program is effective. Finally, we explore whether comprehensive interventions that focus on improving labor market outcomes are more effective for some disadvantaged groups than others.

To determine the impact of this intervention, we implemented a randomized controlled trial evaluation that enrolled 427 participants who were seeking assistance from CCFW over the course of two years from spring 2015 through fall 2016. Study participants were randomized either into a treatment group that was offered the full Padua program or a control group that was offered usual care, which entailed a short-term, modest amount of assistance. Our research design allows us to capture the overall effect of Padua, but not the effect of the program's specific components. Outcomes for study participants were then collected through one- and two-year, in-person, follow-up surveys, as well as administrative data on earnings, government program participation, and credit usage. Our results indicate that Padua leads to improved labor market outcomes, with increases in both work and earnings, although the effect on earnings is not precisely estimated. The intervention increased full-time employment by 25 percent, and this effect is evident two years after initial enrollment. We also find that the intervention leads to improved self-reported health. There is less encouraging evidence for the effect of the intervention on outcomes such as savings and borrowing or on receipt of government benefits.

Given the customized nature of the services that Padua provides, the program is likely to have different effects for different subgroups. Consequently, program effects, or the lack thereof, for the full sample might mask important effects for subgroups. For example, participants who faced acute challenges such as homelessness or a health shock at enrollment prioritized goals that addressed those specific challenges first. For these participants, the intervention is not designed to improve labor market outcomes in the short run. Rather, the goal of the program for such groups is to stabilize their circumstances first, before working on moving to self-sufficiency.

The pattern of results is consistent with this individualized service delivery. Exploratory analyses of subgroups suggest an even larger effect of Padua on labor market outcomes for individuals who either enter Padua without employment or with stable housing, which we define as owning or renting your own home. For those who were not working at baseline, the program raised employment and earnings. For this group, the program increased the probability of working full time by 67 percent, the chance of working by 26 percent and monthly earnings by 46 percent. For this group, we also provide exploratory evidence that the program increased participation in post-secondary education. In contrast, the program was less successful after two years at moving earnings for those already working. Similarly, those that are stably housed at the time of program entry also experience labor market gains. As we document below, given the nature of our recruitment process, families enter the program in very precarious financial situations. That said, some families are in more stable environments than others. Given the nature of the intervention, an unstably housed family such as those experiencing homelessness or living doubled-up, must first work on stabilizing their housing situation before they can focus on job skills or improving employment prospects. In contrast, those un- or under-employed but living in a relatively safe neighborhood in a suitable house or apartment can begin to work on improving labor market outcomes right away. For those in an unstable housing situation, after two years we see no statistically significant change in earnings but a 64 percent increase in housing stability. Conversely, those stably housed at assignment experience a 34 percent increase in earnings and a (marginally significant) 19 percent reduction in receipt of government support. Moreover, our exploratory analysis suggests that the gains experienced by stably-housed individuals without employment at baseline experience the largest labor market gains among many potential subgroups one might consider.

Finally, this paper provides some insight into whether the benefits generated by a more comprehensive program outweigh the added costs of providing more intensive services. The program costs roughly $22,950 (2016 dollars) per study participant, which is more expensive than other programs that are designed to improve labor market outcomes for low-income adults. With relatively strong assumptions (e.g., program effects persist until age 65), we estimate the marginal value of public funds for the typical participant to be 0.506, which is in line with other job training interventions for adults (Hendren and Sprung-Keyser, 2020). However, we provide suggestive evidence that improved program targeting toward the unemployed or those with stable housing might lead to improvements in cost effectiveness.

II The Intervention

II.A How Padua was Designed

Padua is a holistic, individualized, wrap-around service program designed by Catholic Charities Fort Worth (CCFW), a large urban service provider that, as of 2019, serves more than 50,000 unduplicated clients annually.[2] Padua targets those who, with assistance, currently have the potential to be self-sufficient. As a result, the program works with working-age adults who are able and willing to work. The program is not designed to serve those with severe mental illness, substance abuse problems, or other disabilities that would prevent them from working or make it likely that they would need permanent public assistance. While the primary client is an individual, a client's spouse, partner, or other family member may be eligible for some services through Catholic Charities, but they do not receive the same amount of services as the client.

The program is designed to promote self-sufficiency by focusing on key goals, referred to as "out-of-poverty benchmarks" by CCFW. These benchmarks include: (1) achieving a living wage appropriate for their family size, (2) reducing participation in transfer programs, (3) decreasing debt, and (4) individualized targeted savings goals. Despite these common goals, the program recognizes that all families have different sets of strengths and challenges. As a result, each client's short- and medium-term goals are individualized and tailored to their particular situation.

Padua has five key features:

1. Detailed Assessment. Immediately following enrollment, case managers engage in a lengthy and detailed assessment designed to uncover not only the participant's goals but their skills and barriers to success, what the program calls "assets". Case managers use a set of standardized tools to collect and record information about participants' assets across twelve domains: education and skills, emotional well-being, faith, finances, health, hope, language and communication, legal obstacles, physical well-being, relationships, social skills and support skills. Information on each asset is recorded in a 'self-sufficiency matrix' that allow case managers to assign each asset a number on a five-point scale from 1 ("In Crisis") to 5 ("Thriving").[3] See Appendix B for an example of a scoring tool for the self-sufficiency matrix. The assessment takes place over five to seven in-person meetings including home visits and various skills inventories. Case management teams were trained to use a narrative approach to engage clients in conversation during the assessment, rather than asking a series of questions, and approached topics in the same order with each new client. The goal is to complete the assessment within 45 days of commencing services.

2. Service Plan. Based on this assessment, case management teams begin working with participants to develop a clear and supported plan for obtaining self-sufficiency for each client. Padua is a 'strengths-based' intervention that emphasizes the client's role in determining their own path, so the service planning process is purposely collaborative and driven by the client's strengths and preferences. This collaborative and client-led process is meant to produce a service plan in which the client is highly invested.

Case management teams work with their clients to set strength-based goals. Each goal is broken down

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[2] The description of Padua provided in this section is based on information provided by CCFW as well as from a 2017 report by Marci Ybarra, Professor at the University of Chicago, titled "Padua Year 1 Treatment Model."

[3] The values of the scale were as follows: 1 – "In Crisis"; 2 – "Vulnerable"; 3- "Stable"; 4 – "Safe"; and 5 – "Thriving".

into a series of action steps that a client could take towards achieving that goal. Clients are encouraged to complete an action step at least once every three weeks. Goals vary across clients depending on their strengths and present circumstances. For example, families that enter Padua with unstable or unsafe housing may choose to prioritize improving their housing situation first, while others might focus on improving job skills or finding quality and affordable child care that would allow them to work more hours.

Clients were encouraged to choose goals that would help them make progress against the benchmarks that CCFW established for each asset. Service plans were frequently revisited and revised as clients achieved their goals and set new ones. Clients were encouraged to set short-term goals first, to focus on immediate needs and areas that had been judged "in crisis."[4] Building stability in these areas sets the stage for clients to accomplish longer-term education and career goals.

3. Case Management Teams and Small Caseloads. Padua participants are assigned to a two-person case management team composed of a case manager and a case worker that implements the customized service plan and works closely with the client throughout their path towards self-sufficiency. Case managers are required to have a master's degree, while case workers must have a bachelor's degree. Case managers and case workers work in tandem and share the same set of clients. A program manager oversees the intervention and provides support to all case management teams. Case management teams receive extensive training not only when they are hired but throughout their tenure at CCFW.

The detailed assessment and the case management are time-consuming activities that necessitate small caseloads ranging from 18–24 clients per team, considerably lower than typical programs that offer case management services. Half of the teams are bilingual and can serve Spanish speaking clients.

Given the detailed nature of the assessment and the likely scenario that they will have to discuss some of the most intimate details of a client's life, the program can only work if there is a high degree of trust between the client and the case management teams. This trust can be disrupted by turnover among social workers, and the team model promotes long-term client engagement. This continuity is an important feature of an intervention that relies heavily on the relationship between client and case management team.

4. Case Management. Case management activities include personal coaching and mentoring, provision of CCFW services, referrals to a broad network of external services, and regular evaluation of progress. After the assessment and initial service planning period, clients meet at least bi-weekly with their case management team and are expected to have reciprocal weekly contact with their team. Meetings involve reviewing action steps, setting new goals and discussing emergent issues in the client's life. Clients also begin receiving financial coaching and are encouraged to pay off credit card debt or payday loans and/or start a savings plan. As clients have potentially experienced a number of shocks to their finances, health, work and family life, etc., a key component of case management is preparing clients both mentally and emotionally for the tasks ahead. The case management teams often talk about a two-stage process for many families where teams try to stabilize the household first before they begin the process of acquiring skills or looking for work. For example, for families experiencing homelessness or living doubled up, case management teams prioritize acquisition of stable housing. This approach is similar in spirit to housing first programs for the homeless that first work to cover basic necessities, such as food and shelter, before

[4] These short term goals also provide the client with some early successes that encourage persistence.

addressing other more global problems.[5] As clients are at different emotional and mental stages when they enter the program, the speed of progress will also vary across families.

Regular engagement in problem-solving, goal-setting and actively re-evaluating and comparing multiple priorities and goals is designed to improve executive function, confidence and decision-making abilities. For instance, clients are prompted to regularly revisit their service plan by considering a series of questions with their case management teams: What do I need to accomplish by my next session? How does that move me toward my big picture? What do I need to make it happen? What might get in my way? What do I need to do to avoid those barriers? Why is now the right time for this change?

Supported by additional resources from within CCFW, case managers work with employment-ready clients to find stable, well-paying jobs taking a holistic approach that addresses not only employment, but education, transportation, and housing. Employment specialists may help clients connect with specific jobs or provide mock interviewing and resume writing assistance. For some clients, education specialists help clients find credential, certificate, or degree-granting programs in the community, identify scholarship funding, and plan out logistics related to the application process. Finally, transportation and housing specialists may work in tandem to help address other common barriers to ensure clients can get to interviews and jobs on time.

Case management teams are also coached on using standardized tools for session content to guide service planning, including workbooks that focus on financial coaching and keeping client conversations focused on solutions, empowerment and client-led planning. They received regular in-house training sessions on mental health, legal issues, trauma-informed client engagement and cognitive behavioral therapy, among other topics.

5. Flexible Financial Resources. Financial assistance is made available on a case-by-case basis to address potential obstacles that, if left unaddressed, may derail a client's path out of poverty. Financial assistance can be used to fix a family's only car, pay for a security deposit on a new apartment in a safer neighborhood, or pay for the first month of day-care service. In many cases, case management teams use financial assistance as an incentive. For example, case managers might match savings for a new car, pay for the third month of day care if a client keeps a job for two months, or cover the security deposit for an apartment conditional on "paying the money back" monthly into a personal savings account.

II.B How Padua Operates in Practice

To illustrate how Padua works, CCFW provided us with a number of detailed vignettes of actual interactions between the case management team and the clients. While these stories are based on actual case manager/client interactions, we have changed names and slightly altered details to protect the identity of the clients. The first two examples demonstrate how Padua promotes success in the labor market. J was working in law enforcement, but a workplace injury placed him on workers' compensation for two years. His benefits were expiring and his rehabilitation was unsuccessful at getting him to the point where he could

[5] Descriptions of the housing first model can be found at the web pages of:
the National Alliance to End Homelessness (https://endhomelessness.org/resource/housing-first/)
and HUD (https://files.hudexchange.info/resources/documents/Housing-First-Permanent-Supportive-Housing-Brief.pdf).

return to his previous occupation. Working with his case management team, they identified commercial driving as an occupation J would both enjoy and could work in with his injuries. While he worked part time as a security guard, J enrolled in a six-week training course to obtain a commercial driver's license. The Texas Workforce Commission funded his schooling and Padua provided assistance for J to help him financially while he was in school and searching for work. J is now working full-time as a commercial driver in a job that frequently offers over-time hours.

When K first joined Padua, she was a stay-at-home mom and her undocumented husband worked as a cook in area restaurants. They struggled to make ends meet as her husband was poorly paid. K's goal was to work to help the family financially, but she did not have a college degree and her earning potential was limited. K had started at a local community college but never graduated and used up all the education subsidies available to her so she could not afford school. Her previous experience in college generated a lack of confidence that stood in the way of her acquiring more skill. Her case management team worked with her extensively to get her to a point where she was ready for training and employment. During that time, the case management team at times provided financial assistance to help with rent and expenses, always with a plan in place as to how these expenses were to be paid the next month. An education specialist working for CCFW helped K enroll in a medical assistant program and prepared K to apply. The case management team arranged for a local charity to pay for K's tuition, and Padua paid for school supplies. K graduated with a medical assistant's degree and is working in a job she finds rewarding. The household's earnings now place them well above the poverty line for their family.

For many, the goal is to obtain a more stable housing situation. When M first enrolled in Padua, his son had just had a health crisis that required emergency surgery that devastated the family financially. Because of these expenses, they could not pay their rent and were evicted. They found a temporary residence with family but this was a 90-minute commute from M's job. M was stressed, overwhelmed, frustrated, and near hopeless. The case management team helped M create a budget to save resources to settle his outstanding debts. Using these savings and some financial assistance from Padua, a CCFW Housing Specialist helped M though the process of having his landlord dismiss his previous eviction. This allowed M to obtain a lease on an apartment much closer to his work and stabilize his housing situation. M now has a lease in his name and the case management team continues to work with M on his family's budget. In Appendix C we share two additional vignettes that demonstrate other unique aspects of how Padua works.

These stories have some common elements. First, the case management teams take the client's goals as given and strive to devise a plan that supports these goals while building strategies for addressing the key barriers. Second, the families in Padua face many challenges that are complicated and often need to be solved in a sequenced order. For example, case managers will work to get homeless individuals and families stably housed before focusing on improving labor market outcomes. Third, the situations are very diverse so the solutions are equally diverse. Finally, the case management teams frequently use the financial assistance component as a way of incentivizing behavior.

III Prior Evidence on Intensive Case Management

Programs that incorporate holistic or intensive case management have been implemented to help clients facing specific barriers or to address particular outcomes. For example, recent evaluations have explored the ability of case management interventions to improve housing stability among the chronically homeless, to reduce recidivism among prisoners re-entering society, to improve neighborhood choice among housing voucher holders, to increase persistence and graduation among community college students, and improve general economic well-being. We summarize these interventions in Appendix D, limiting our focus to those that have been evaluated using a randomized controlled trial.[6]

This literature provides two key takeaways. First, results from these studies have been mixed on the effectiveness of case management programs at improving outcomes, but well-implemented programs with clear treatment contrast have shown promise. For example, intensive programs that targeted chronically homeless using a housing first approach improved housing stability (Gulcur et al., 2003; Goering et al., 2014) and intensive/integrated case management services among associate-degree seeking students have improved persistence and degree completion (Evans et al., 2020; Weiss et al., 2019; Hallberg et al., 2022). In contrast, programs that have not been implemented well or did not generate sufficient treatment contrast across study groups saw little impact (e.g., De Vet et al., 2013). Second, these programs have often been implemented alongside other substantial investments in clients—housing vouchers (Rosenheck et al., 2003; Bergman et al., 2020), financial assistance (Evans et al., 2020; Weiss et al., 2019), or subsidized employment (Barden et al., 2018; Barham, Cadena and Turner, 2022)—and case management potentially helps to make these investments more effective. In the case of housing choice vouchers, Creating Moves to Opportunity led clients to be more likely to select upward-mobility neighborhoods and be more likely to renew future leases, when compared to just providing a voucher, which suggests case management might aid in removing barriers and increasing a client's neighborhood choice set (Bergman et al., 2020). Complementing this finding, evidence from Stay the Course, a community college case management intervention, suggests that assistance alone was not enough to increase persistence and degree completion (Evans et al., 2019).

There have been a smaller set of interventions that contain many of the elements of Padua but focus more generally on economic mobility. We discuss four such programs that, like Padua, were designed to move individuals from no or low-wage employment into living-wage employment and were evaluated by an RCT.[7] In this discussion, we do not include other similar programs that do not have employment and earnings as primary outcomes, for example Creating Moves to Opportunity, which focuses on neighborhood choice (Bergman et al., 2020), or Stay the Course and ASAP, which focus on associate degree completion (Evans et al., 2020; Weiss et al., 2019). Appendix E documents program characteristics and experimental impacts of these four programs to facilitate a comparison with Padua.

The first two programs were anti-poverty programs targeted to low-income workers. Building Nebraska Families (BNF) (Meckstroth et al., 2008) was a welfare-to-work program that provided individualized

[6] In addition to these area, the use of service plans and case management have been used and evaluated extensively among patients with chronic mental illness. For systematic reviews of this literature, see Burns et al. (2007); Dieterich et al. (2017)

[7] In addition to these four programs, there are two on-going RCT evaluations of programs similar to Padua, Bridges to Success (Espinosa, Evans and Phillips, 2021) in Rochester, NY and AMP Up Boston Study of EMPath (Engle, Katz and Tebes, 2021), both programs that use mentoring to improve economic mobility.

education, life skills and service coordination to hard-to-employ TANF recipients. The program was home-based with small caseloads of 12 to 18 clients per caseworker. Clients met their case workers two to three times a month for eight months. Unlike Padua, the BNF case managers were characterized more as educators than coaches or mentors. Also, BNF did not provide flexible temporary financial assistance. The New Hope Program in Milwaukee (Duncan, Huston and Weisner, 2007; Miller et al., 2008) provided a large earnings supplement to participants working more than 30 hours per week. While there was some individual-level coaching and counseling, this was not a focus of the program. In contrast to the one-on-one service delivery of Padua, many sessions were in small groups, and 25 percent of the time was spent processing benefits. The New Hope Program had substantially larger caseloads (75 clients, as compared to 18 to 24 for Padua), and it did not offer flexible temporary financial assistance to address negative economic shocks that might have prevented full-time employment.

More recent workforce programs have enhanced traditional job training or subsidized employment interventions with case management and flexible financial assistance. First, Year Up partners with local community college partners to provide intense professional and technical skills training to youth with high school diplomas (Fein and Hamadyk, 2018). In lieu of flexible financial assistance, students receive a substantial weekly stipend making the program relatively expensive (about $28,000 per student during the year). Additionally, the program targets a much narrower population than Padua, restricting the program to a young population (aged 18–24) that have been selected for high levels of motivation and their ability to manage life's challenges. Finally, the Enhanced Transitional Jobs Demonstration evaluated a set of subsidized employment programs that also provided some set of case management, supportive services, and flexible financial assistance (Barden et al., 2018). The enhanced services participants received varied by program site, and not all participants received access to an individual case manager that provided consistent mentoring or flexible financial assistance. Importantly, the target population was more narrowly defined than Padua—recently released prisoners or non-custodial parents with an outstanding child support payment.

IV The Experimental Evaluation

IV.A Study Design and Sample

We implemented a randomized controlled trial (RCT) evaluation to measure the impact of Padua on short- and intermediate-term outcomes measured 12 and 24 months after enrollment into the study. We recruited participants from CCFW clients who contacted the agency's central intake system seeking either emergency financial assistance for rent or utilities (82 percent of participants), immigration services (15 percent) or other programs (3 percent).[8] CCFW intake staff were trained to flag clients who met the initial eligibility criteria for Padua:

- Individual is between 18 and 55 years of age;

[8] Appendix Table A-2 provides information on how the characteristics of study participants varied across referral sources.

- Total family income is not sufficient to meet needs;[9]
- Individual resides in Tarrant County, TX; and
- Individual's family includes at least one working-age adult who is willing and able to work.

These eligibility criteria were designed to target those who were most likely to benefit from the unique nature of this intervention—disadvantaged individuals with some capacity to work.

If, during the intake process, CCFW determined that a client met these eligibility criteria, the intake staff briefly described the program to the client and informed them that the program was being run as part of a research study. They also explained that because of limited funds, enrollment would be based on a lottery, and asked whether they wanted to learn more about the program. The contact information for interested clients was forwarded to Padua staff, who contacted potential clients to discuss the program and study in more detail. Interested clients scheduled an intake interview, which typically occurred within a few days. At this interview, a CCFW program manager confirmed eligibility, reviewed the study and the intervention in detail with the client, and invited them to complete a 60-minute baseline survey. Clients who agreed to participate in the study and complete a baseline survey were then brought to a private office where they connected via phone with an interviewer from the University of Wisconsin Survey Center (UWSC). Clients were consented to participate in the study and then administered the survey. The survey instrument included questions related to the demographic characteristics of the respondent and their family, as well as intended outcomes like income, assets, debts, employment, spending, participation in government programs, physical and emotional health, and social systems and relationships.[10] Clients were provided a $25 cash incentive for completing the baseline survey. CCFW scheduled enrollment sessions during specific weeks each month. At the conclusion of each enrollment week, the research team randomly assigned those clients who consented and completed the baseline survey to either the treatment group or the control group (see Appendix F for additional details). Caseworkers invited those in the treatment group to begin the process of enrolling in Padua by attending an initial meeting with a case manager. Control group participants were provided with the services they originally sought, which were the standard services provided by the agency.

CCFW enrolled clients into the study over the course of two years. A first cohort of clients enrolled from March 2015 to October 2015, and a second cohort enrolled from March 2016 to October 2016.[11] The diagram in Figure 1 lays out the process of how clients enrolled in the study. Approximately 11,000 individuals contacted CCFW seeking assistance during the enrollment periods. Of these, 1,517 satisfied an initial screening defined by income, age, interest and zip code. These clients were then screened on all eligibility criteria, resulting in 1,072 eligible clients. Of the 1,072 eligible, 40 percent agreed to participate

[9]CCFW based its income eligibility cutoff on the living wage for Tarrant County as defined by MIT's Living Wage Calculator for 2015. This cutoff is roughly 185 percent of the Federal poverty line.

[10]The research team designed the survey in consultation with the UWSC. We modeled the survey after well-tested questions in large surveys including the Current Population Survey, the Panel Study of Income Dynamics, the Detroit Area Household Financial Services survey, the Women's Employment Study, and the Behavioral Risk Factor Surveillance System.

[11]This program was rolled out in the context of a fairly strong local economy. During the period of enrollment, unemployment rates in Tarrant County ranged from 3.7–4.6 percent. To understand generalizability, it would be important to test the effectiveness of this program in other macroeconomic contexts.

in the study and of this group, we randomized 193 participants into the treatment group and 234 into the control group for a total of 427 participants.[12,13]

To measure the impact of the intervention on key outcomes, the UWSC conducted in-person, follow-up surveys 12 and 24 months after study enrollment.[14] These follow-up surveys were identical to the baseline surveys, except for minor edits.[15] As a modest incentive, those that responded to the survey received $75 in cash. Response rates for the follow-up surveys were high, with 82 and 81 percent of the participants responding to the 12- and 24-month follow-up surveys, respectively, while 74 percent of participants responded to both. Response rates did not vary in a statistically significant way across treatment; 82 percent of the control group and 81 percent of the treatment group responded to the 24-month survey; 81 percent and 84 percent, respectively, responded to the 12-month survey. To complement the outcome information collected in these surveys, we also linked study participants to administrative data on government program participation, earnings and employment from UI records, and financial information from credit report data.

Table 1 reports the baseline characteristics for the 346 study participants who responded to the 24-month survey. In the first two columns, we report means for the control and treatment groups, respectively, while in the next two columns, we report the difference in these means and the p-values on the test of the hypothesis that these means are equal, respectively. In the final three columns of the table, we use data from the 2012–2016 5-year American Community Survey (Ruggles et al., 2021) to calculate similar means for adults likely eligible for the experiment from Tarrant County, the state of Texas, and the nation as a whole.[16]

Three things are of note from Table 1. First, our random assignment process achieved balance. For all characteristics, we fail to reject the hypothesis that the means are equal for the treatment and control groups. A joint F-test also indicates balance (Prob $> F = 0.732$). We find comparable evidence of balance for the full baseline sample that does not condition on responding to the follow-up survey (Appendix Table A-1) and among the subset of study participants linked to administrative data (Appendix Tables G-2

[12] The probability of assignment to the control group was roughly 25 percent greater than the probability of assignment to the treatment group to account for anticipated higher attrition for the follow-up surveys for the control group. If there were more than two Spanish-speaking clients to be randomized, we stratified randomization by preferred language (English or Spanish). See Appendix F for more details.

[13] When designing the experiment, we conducted power calculations using information on household income of Tarrant County residents from the American Community Survey. Based on these calculations, we designed the experiment to include at least 185 treatment individuals after accounting for expected attrition due to nonresponse to the follow-up survey. This sample was designed to detect an impact on annual income of $2,050. More details can be found in our analysis plan at the AEA RCT Registry (AEARCTR-0000722), which is a compilation of grant proposals submitted prior to data collection and analysis.

[14] The UWSC completes thousands of interviews each year, often using long, complex survey instruments. They have achieved consistently high response rates across all types of survey methodologies and populations. The staff have extensive experience with Computer-Assisted Personal Interviewing (CAPI) drawing from a staff of over 30–60 CAPI interviewers trained rigorously, especially in how to conduct CAPI with populations that are challenging to locate.

[15] Questions about static traits (e.g. race) were removed from follow-up surveys. Additionally, per the request of the provider, a series of questions on hope were added to the follow-up surveys.

[16] This sample includes adults living in households under 180 percent of the federal poverty level who have at least one able-bodied adult aged 18-55 in the household, where we define able-bodied as someone who is working, looking for work, available for work, or in school. To select a respondent from each household similar to the Padua participants, we selected one able-bodied respondent within the eligible age range, prioritizing female heads of household or their spouses, then male heads or their spouse, and finally the oldest respondent.

and G-4).[17] Importantly, differences in response rates to the 24-month survey between the treatment and control groups are small (0.4 percentage points) which is not a statistically significant difference.[18]

Second, participants were recruited to Padua when some shock to their family required them to seek assistance from a social service provider. As such, the participants were facing poor economic circumstances. As shown in Table 1, only 40 percent of the sample was employed at baseline, family income placed them at about 62 percent of the federal poverty line, about 60 percent reported having their utilities shut off or having received a disconnection notice in the past 12 months, and over 20 percent reported a recent medical hardship.[19] Appendix Table A-4 provides further evidence of the multifaceted challenges facing Padua participants at program enrollment, though the table only provides information for those enrolled in the treatment group. With the exception of the Legal and Faith categories, fewer than 10 percent of the program participants scored as "Thriving" on any of the components of the Self-Sufficiency matrix at program entry. More than 50 percent of the sample scored as "In-Crisis" or "Vulnerable" on multiple components, including more than 75 percent on the financial component.

Third, comparisons of our study sample to a broader population (the final three columns of Table 1) indicate that our main study sample appears to be worse off financially than a broader set of households likely eligible for the program. Compared to these other households in Tarrant County, Texas and the U.S., our sample is less likely to have graduated high school, older, less likely to be employed, has lower monthly earnings, is more likely to be a single mother, and is substantially less likely to be white, non-Hispanic. More than half of the sample is comprised of non-white, single mothers. Among the men in our sample, nearly all have children and two-fifths are single dads (not reported). A key difference between our sample and the broader samples from the ACS is that our sample was drawn from a group of individuals and families that had recently received a negative economic shock—most had come to CCFW seeking emergency financial assistance.

IV.B Program Take-up and Timing

Random assignment generated differences in treatment intensity during the two years following enrollment. Ninety-one percent of the treatment group had at least one meeting with their case management team (Table 2). These clients spent an average of 16 months in the program over the first 24 months after random assignment. The typical client met with their case management team, primarily in-person or over the phone, for over 47 hours, with half of all clients spending within 25 hours of this median. Clients met

[17] Appendix Table A-2 presents average baseline characteristics of Padua applicants by referral source. Of note, applicants who were recruited through immigration services were less educated, more likely to be Hispanic, and less likely to receive government benefits. Results presented in Section VI are qualitatively unchanged when further controlling for referral source (results not reported).

[18] The differences are also not statistically significant once we control for observed characteristics. Appendix Table A-3 shows estimates from regressions of an indicator of non-response to the 12- and 24-month surveys on the listed baseline controls, as well as their interaction with a treatment group indicator. Column 3 shows that respondents were older, more educated, more likely to be female, more likely to receive SNAP, and were less likely to have experienced a medical hardship—though many of these differences are not statistically significant. However, we fail to reject the null hypothesis that non-response rates given characteristics are not different between the treatment and control groups (Prob $> F = 0.627$).

[19] The fraction who have had their utilities disconnected, or that have received a notice of disconnection, is high for our study sample because many study participants initially contacted CCFW for utility bill assistance.

most intensely with their case management team during the first few months of the program: on average, 6 times in the first month, about 4 by the sixth month, and still nearly 2–3 times per month after two years (Figure 2). Most of the case management team's time (35 percent) was spent on the coordination of services such as arranging job training, housing, immigration services, etc (Figure 3), although a large share (29 percent) was spent in routine meetings where clients check in with their case management team and update them on what they have been working on since the last meeting. The initial client assessment took about one-ninth of their time.

Clients also received substantial cash assistance during their time in the program. Over two years, 77 percent of the treatment group received cash assistance (Table 2). The typical recipient received $2,742 across 11 disbursements. Not surprisingly, cash assistance was primarily disbursed during the first few months following enrollment. Roughly three-quarters of assistance was disbursed in the first program year (Figure 4). This money was most commonly used for rental assistance (42 percent), which included security deposits for a new lease, though transportation (13 percent) was also a common expenditure (Figure 5). The shares of funds directed to household items, education, utilities, and childcare were all in the single digits.

In contrast, the control group only had access to the other usual services provided by CCFW. Only a small share of control group participants (3 percent) received access to case management from CCFW, participating in a different program with larger caseloads (Table 2). Similarly, the control group was much less likely to receive financial assistance, and those who did receive assistance did not receive as much. Using information on calls made to the CCFW call center by control group clients, we estimate that roughly 26 percent of the control group received cash assistance. The typical amount of assistance received, such as one-time rent or utility assistance, was roughly $460.

Finally, the timing of the follow-up surveys means that many Padua clients were still receiving services when we measured outcomes. After 12 months, more than 60 percent of clients remained active in the program. By 24 months, roughly 56 percent of clients had exited the program, and nearly all who exited did so prior to meeting program benchmarks (Figure 6).

V Methods

For our primary analyses, we estimate the differences in outcomes between treatment and control group participants using a standard intent-to-treat (ITT) model that controls for baseline covariates. Given the balance across groups in baseline characteristics, including these controls is primarily to reduce residual variance and improve precision. The standard regression model we estimate is of the form:

$$y_{ij} = \beta_0 + T_i\beta_1 + x_i\beta_2 + y_{i0}\beta_3 + \epsilon_i \tag{1}$$

where y_{ij} is an outcome for participant i at either the 12-month ($j = 1$) or 24-month ($j = 2$) follow-up. The parameter of interest is β_1 which is the coefficient on the dummy variable T_i that equals 1 if the respondent is in the treatment group and zero otherwise. We have two sets of baseline controls. The first is the vector x_i that represents a set of observable characteristics collected during the baseline survey including the age, race, gender, educational attainment, marital status, employment status and earnings of the respondent, as well as family-level characteristics such as household size. In addition, x_i includes interview characteristics such as indicators for cohort, interview month, and the number of months between the baseline interview and the follow-up interview.[20] In most cases, we also include the value of the outcome measured at baseline, indicated by the variable y_{i0}.[21] When estimating effects using administrative data that have multiple pre-randomization measures of the outcomes (e.g., 8 quarters of employment indicators), we use post-double selection LASSO (Belloni, Chernozhukov and Hansen, 2014) to select among all controls. We estimate equation 1 for outcomes measured at different points in time, for example 12 months and 24 months after baseline in the survey data.

We designed the consent and enrollment process for this RCT to yield high take-up rates, screening potential clients on their willingness to participate. In fact, about 91 percent of those in the treatment group who were offered services actually completed the initial assessment and received some services. Given this high take-up rate, our analyses focus on ITT estimates, but one can obtain the estimated impact of Padua for those who actually received services (treatment on the treated) by dividing the ITT by the take-up rate.

V.A Outcomes

Our main results rely on information on outcomes collected in the two follow-up surveys, although we also observe some outcomes in administrative data sources. As Padua was designed to promote self-sufficiency through work, we initially emphasize labor market outcomes such as employment and earnings. Given the holistic nature of the intervention, Padua also hoped to help clients increase savings, limit debt and reduce dependence on government programs. As a result, we examine outcomes along these dimensions. We also look at outcomes that indicate housing stability, because the case management team often focused on improving housing situations prior to working on improving labor market outcomes. Finally, we look at outcomes related to spending and health as additional indicators of overall well-being.[22]

To summarize the program impact for similar sets of outcomes, we estimate the average standardized treatment effect for each of six domains: labor market, housing, government support, debt and savings,

[20] Although we aimed to schedule follow-up interviews at 12 and 24 months after baseline, due to scheduling challenges (such as interviewer or interviewee availability, difficulty tracking down respondent, etc.) some follow-up interviews did not occur at precisely these intervals. However, more than 92 percent of 12-month follow-up interviews occurred within 10–14 months after baseline, and 92 percent of 24-month follow-up interviews occurred within 22–26 months after baseline.

[21] Some outcomes are measured as a change in value from a prior period—for example, an indicator for whether total assets increase—and therefore do not have a baseline value. Additionally, we have estimated models where controls are sequentially added to check for sensitivity of estimates to the addition of different covariates. Estimates are stable across different specifications.

[22] Prior to the start of data collection, we specified in grant documents that we would examine key short-term outcomes including family income, employment, reliance on government programs, self-reported health, and measures of self-efficacy. After one year of data collection covering roughly half the sample for the 12-month survey, we expanded our list of outcomes and classified them into four broad categories: labor market, spending, debt and savings, and use of supportive services. Outcomes on health and neighborhood conditions and relationships were also considered. These outcomes are listed in an analysis plan derived from early grant proposals and reports. This analysis plan, which was created ex post, can be accessed at the AEA RCT Registry (AEARCTR-0000722). We present results in our main tables for key outcomes, but we also report results for any other outcome that had been mentioned at early stages of the study in Appendix Tables A-11 and A-12.

spending, and health. Specifically, we estimate

$$\hat{\tau} = \frac{1}{K} \sum_{k=1}^{K} \frac{\hat{\beta}_1^k}{\hat{\sigma}_k} \tag{2}$$

where $\hat{\beta}_1^k$ is the ITT estimate for outcome k and $\hat{\sigma}_k$ is the standard deviation of outcome k in the control group. For each outcome, we sign the ITT estimate such that a positive estimate indicates an improvement. To allow error terms on the coefficients to be correlated, we follow Finkelstein et al. (2012) and stack data across all K outcomes within a domain, estimating a single regression clustered at the individual level.

In addition to the survey-based outcomes, we consider the effect of Padua on several outcomes measured in administrative data including earnings, employment, receipt of SNAP, and credit outcomes. These results are presented to complement those for the survey-based outcomes, and they allow us to examine a particular set of outcomes over a longer time frame both prior to and after enrollment in the study. For additional information on the data sources and baseline balance in the matched samples see Appendix G.

V.B Inference

We estimate Padua treatment effects across two survey waves with six domains comprised of a number of outcomes. For all survey-based estimates, we have constructed randomization-based p-values from 10,000 permutations of the treatment assignment procedure (see Appendix F). When summarizing the standardized treatment effect of Padua on six domains, we report these unadjusted randomization-based p-values that test the sharp null hypotheses of zero treatment effect among all study participants.[23] One might be concerned, however, that the probability we reject the null for any one domain is greater than a given significance level because we test six hypotheses in a given survey wave. To overcome this concern, we also report adjusted p-values that control for the family-wise error rate using the step-down procedure of Westfall and Young (1993).[24] When reporting treatment effects on individual outcomes, we report adjusted p-values where the hypothesis family includes all outcomes in the domain, but also indicate on our tables when effects are statistically significant according to the unadjusted p-values.[25]

In exploratory analysis, we also estimate subgroup-specific treatment effects on the six domains across a large number of potential subgroups. In principle, there are many ways of constructing subgroups to explore treatment effect heterogeneity. Because this analysis was pursued after receipt of data, we construct point estimates for a large number of subgroups that could have been plausible *a priori*. For

[23] Under this null hypothesis, the potential outcome under treatment or control can be inferred from the observed outcome. Therefore, the distribution of treatment effects from the placebo samples provides the exact distribution of those values under the null hypothesis Athey and Imbens (2017). The p-value represents the share of placebo trials that yield a p-value smaller than the observed p-value.

[24] In our domain-level analysis, a family is comprised of the six domains measured at the same follow-up period. This choice follows Jones, Molitor and Reif (2019) who construct families of outcomes "that originate from similar data sources" (see footnote 14). Our analysis benefits from Stata code from Jones, Molitor and Reif (2019). Unlike their paper, however, we use the distribution of permutation-based p-values similar to Young (2019) as opposed to a distribution constructed from bootstrap samples.

[25] Across our results tables, we report effects on individual outcomes and the standardized treatment for the domains for both the full sample of study participants, as well as a limited set of subgroups. Multiple hypothesis testing adjustments are made among the set of hypotheses considered for the particular sample under consideration.

example, we construct estimates based on a number of demographic characteristics, levels of the outcomes at baseline, degree of crisis at intake, etc. In total, we present estimates for the full sample, as well as 36 different potential subgroups. As noted above when considering many outcomes, this approach opens up inferential concerns related to multiple comparisons. Following Chetty, Hendren and Katz (2016), we use a permutation-based test to estimate the likelihood one would find through random chance an effect of at least the statistical significance we find across any of the potential subgroups. For each placebo assignment, we estimate the standardized treatment effect on each domain for all of the subgroups. Using randomization-based p-values as critical values, we calculate the share of placebo samples where at least one subgroup had a p-value for that domain below the corresponding actual p-value.

VI Survey Results

We present our main results from our follow-up surveys for outcomes within each of our 6 broad outcome categories at both 12 months (Tables A-5 through A-10) and 24 months (Tables 4 through 9) after enrollment in the study. For each treatment effect estimate, we report standard errors clustered at the individual level and denote statistical significance using the unadjusted p-values. We also report an adjusted p-value that controls for the family-wise error rate within the domain and sample following the Westfall-Young step-down procedure (Westfall and Young, 1993). In the last row of each of these tables we report the standardized treatment effect, the standard error, and an adjusted p-value that controls for the family-wise error rate across the six domains within the survey wave. For each domain and period, the standardized treatment effect is calculated using all of the outcomes we considered in that domain.[26]

To summarize our results for the many outcomes collected in our our surveys, we first report the average standardized treatment effect across outcomes within each of our six domains at 12 and 24 months after enrollment in the study (Table 3). For labor market outcomes that were the primary focus of Padua, there is suggestive evidence of a positive effect one year after enrollment–these outcomes were 0.139 standard deviations greater for those in the treatment group (unadjusted p-value = 0.075). This standardized treatment effect persisted through two years post enrollment, where we see a positive effect of 0.149 standard deviations with an unadjusted p-value of 0.049. If we adjust for the family-wise error rate across all six domains within the survey wave, this estimate is no longer statistically significant (p-value = 0.218). In fact, none of the standardized treatment effects across the 6 domains at 12 or 24 month are statistically significant after accounting for the six domains examined in this study.

We also find some suggestive evidence of a positive effect of the intervention on housing outcomes, which improved by 0.067 standard deviations (unadjusted p-value = 0.141) at 12 months and by 0.096 standard deviations (unadjusted p-value = 0.030) at 24 months. We find little evidence of an overall effect of Padua on the other domains–support, spending, data, and health–at either 12 or 24 months. Although most of the estimates are positive, all but one of these domain-level estimates is smaller than 0.065 standard deviations, and none of these estimates are statistically significant. As we discuss below, that Padua is an

[26] We include all of the outcomes reported in the main tables for these domains (Tables 4 through 9), as well as additional outcomes we examined within each domain as noted in the table notes and reported in Appendix Tables A-11 and A-12.

individualized intervention, with sometimes competing goals for different groups, null effects for the full sample could potentially mask program impacts for subgroups.

VI.A Labor Market Outcomes

Given Padua's focus on employment, we first present results for specific outcomes within the labor market domain. Treatment effects for labor market results at the 24-month follow-up for different outcomes are reported in Table 4. Results from the 12-month survey are reported in Appendix Table A-5. The first six outcomes are: a dummy for whether the survey respondent is currently employed; the respondent's monthly earnings (zero for non-workers); a dummy for whether the respondent is employed full time, defined as 35 or more hours of work per week; hours worked per week; the household's income as a percent of the federal poverty line; and a dummy for whether the respondent is legally authorized to work in the US. The average standardized treatment effect reported in the final row is the same estimate that was reported in Table 3. For each regression, we report the ITT estimate from equation 1 and its standard error, the multiple-hypothesis-adjusted p-value on the null hypothesis that the coefficient is zero, and the control group mean. Stars and pluses indicate statistical significance using unadjusted p-values. We report some additional labor market outcomes in Appendix Table A-11.[27]

We report the regression-adjusted ITT results from equation 1 for the full sample in the first column of Table 4. The signs of the estimated effects for all these outcomes indicate improved labor market outcomes (e.g. increased labor market participation, increased labor supply, and higher earnings), although many of these estimates are not statistically significant at conventional levels. Those in the treatment group were 6.1 percentage points (9.7 percent) more likely to be working 24 months after application, although this estimate is not statistically significant. The fraction working full-time increased by 10.5 percentage points, which is a 25 percent increase over the control group mean, and is significant at the 5 percent level.[28] The increase in the likelihood of working is associated with an increase in monthly earnings of $208 (18 percent), but this estimate is not statistically significant. Treatment effect estimates also suggest that Padua leads to a 9 percent increase in household income as a percentage of the poverty line, although this estimate is imprecise.[29] Taking this estimate at face value, the increase in family income is somewhat smaller than the increase in respondent earnings, which may in part be due to a decline in receipt of government benefits as we discuss below.[30]

[27] For all individual outcomes reported in Tables 4 through 9, the adjusted p-values control for the family-wise error among the outcomes included within each domain, both those reported in the main table as well as those reported in Tables A-11 and A-12 for the sample considered in the table column. The adjusted p-values for the standardized treatment effect controls for the family-wise error rate among the six domains measured in the 24-month survey for the sample considered in the table column.

[28] Our measure of full-time employment includes hours worked across many jobs. This effect appears to be driven by an increased likelihood of having one main job with more than 35 hours of work per week, as opposed to working more than 35 hours across multiple part-time jobs. The effect on an indicator of full-time work in one's main job is similar in magnitude as our primary measure.

[29] We also examine how Padua affects income relative to various multiples and fractions of the FPL, ranging from 0 to 300 percent (Figure A-1). The results suggest that gains in income led to households being above 100-200 percent of the poverty line.

[30] In separate analyses, we find no evidence that Padua leads to a decline in labor supply for adults in the household other than the respondent.

In unreported results, we explored whether gains in earnings occurred alongside increases in educational attainment that might have led to higher paying jobs or if they are primarily driven by increased attachment to the labor market. The pattern of results suggests that Padua might have increased educational attainment. The treatment group was 11.4 percentage points less likely to have only completed a high school diploma or GED by the time of the 24-month survey (unadjusted p-value = 0.009), which can be decomposed into increases in the likelihood of having less than a high school diploma (6.0 percentage points, unadjusted p-value = 0.061) or having completed some college or more (5.4 percentage points, unadjusted p-value = 0.150). The increase in college going and completion is relatively large, a 12.7 percent increase compared to the control group mean of 47.3 percent. On one hand, these changes are consistent with either the control group being more likely to access others services that encouraged GED completion or that Padua case workers were less likely to use the GED as a means for improving job-readiness skills for clients without a diploma. On the other hand, these results do suggest that Padua helped some clients connect with postsecondary education programs and resources as a means for improving labor market prospects.

Padua participants were also more likely to have legal authorization to work. One referral source for the program was CCFW's immigration services program, and by the 24-month follow-up survey, treatment group members were 3 percentage points more likely to be legally allowed to work in the U.S., an effect that is significant at the 10 percent level. This result suggests that one mechanism by which Padua leads to increased work is by addressing legal barriers.[31]

As discussed above, the standardized treatment effect for this domain reported in the final row is positive and has an unadjusted p-value of 0.049, but a p-value of 0.218 when adjusting for the family-wise error rate across all six domains within the survey wave.

The positive impact of Padua on employment is occurring alongside an increasing trend in employment for the control group, which is expected. Enrollees to the experiment entered Padua because some shock forced them to seek assistance from a social service provider. This context—where prior to enrollment study participants experienced a shock that perhaps led to a temporary decline in wages and employment—is a classic "Ashenfelter dip" (Ashenfelter, 1978).[32] Figure 7 plots full-time employment rates for the treatment and control groups over the three survey waves. Full-time employment nearly doubled for the control group in the year after enrollment (from about 20 percent to about 40 percent), which is to be expected to some degree given the way study participants were recruited. While the figure highlights that many individuals are able to improve their labor market outcomes on their own or with the help of other services available to this population, Padua had an effect over and beyond this pattern and the effect persisted over two

[31] However, immigration services cannot explain the majority of the effect given that 84 percent of the 24-month respondents were legally authorized to work at baseline. When we split the sample by baseline legal status, the labor market effects are not precise for either subgroup. To explore this further, we also estimated the effect of Padua for the subsample of Hispanic respondents (about 30 percent of the sample), a group that is more likely to have immigrated. The effect of Padua on labor market outcomes for this subsample was typically larger but less precisely estimated.

[32] Administrative data on employment and earnings before and after baseline indicate an Ashenfelter dip, as we discuss below. Moreover, the data on employment for all respondents to the baseline survey supports the idea that many have experienced recent detachment from the labor market. Of respondents not working at baseline, 77 percent reported having worked in the previous 12 months. Also, income for respondents at baseline is about a quarter of their income in the previous calendar year.

years. We find a very similar pattern when we examine earnings.

The magnitudes of the effects on full-time work and earnings are large relative to other interventions designed to promote work. For example, a study of National Jobs Corps, a vocationally-focused education and training program for disadvantaged youths, found modest impacts on earnings in the short run and no persistent differences in earnings in the long run (Schochet, Burghardt and McConnell, 2008). Our findings are comparable to the effects on full-time employment experienced by BNF participants (23 percent; Meckstroth et al., 2008) and ETJD participants (17.5 percent; Barden et al., 2018). Additionally, the effect on earnings, although imprecisely estimated, is comparable to what Marcotte et al. (2005) and Jepsen, Troske and Coomes (2014) found for returns to two-plus years of community college education or completing a community college degree.

VI.B Who Benefits Most from Padua?

Lifting families out of poverty involves addressing a multitude of barriers faced by individuals as they work to find and maintain stable employment. Importantly, different types of individuals face different barriers to exiting poverty and Padua is designed to develop individualized plans to address these barriers and move towards self-sufficiency. In this section, we consider whether Padua had a different effect on different types of participants. Understanding the heterogeneity in program impacts is important for a few reasons. First, comprehensive interventions such as Padua are expensive and time consuming so improving a benefit-to-cost ratio may require targeting services to particular types of clients using easily measurable attributes. Second, currently clients can continue to receive Padua services until they have reached their goals for self-sufficiency. In fact, more than 40 percent of clients remained in the program for more than two years. However, some clients may advance toward their goals more rapidly than others. Identifying what cases may take longer or shorter will help social services agencies better plan enrollment and more efficiently operate the program. Third, the program is broad in terms of both client backgrounds and program services, making it more difficult for some agencies to replicate. Reducing the focus of the program in terms of clients or services offered might allow more agencies to adopt the intervention.

To examine heterogeneity in program impacts, we separate the study sample into subgroups based on baseline characteristics to estimate the within-group ITT effect of Padua. For each subgroup, we report results on the same set of outcomes as for the main effects, focusing on the 24-month results. We perform three such exercises and we selected subgroups because individuals within the subgroup may face a unique set of barriers, relative to the rest of the sample. These particular groups were identified through unstructured interviews we had with program case managers describing anecdotally the different ways certain groups of participants were interacting with and benefitting from the program.[33]

An important component of Padua is helping clients find a pathway to stable employment. A minority

[33] We split the sample by employment status at baseline to follow the approach taken by other studies of interventions designed to improve labor market outcomes. We also examine outcomes separately by housing stability at baseline, because this was a factor our provider partner highlighted as important for success of the intervention. Prior to the launch of the study, we did not specify examining these sub-samples. Below, we characterize the effects of these groups alongside many other subgroups that could have plausibly been chosen instead, and we discuss concerns about multiple comparisons in Section V.B.

of the study applicants enrolled in the program were employed at baseline. It is reasonable to expect that the program may have different impacts for those who were already employed at baseline as compared to those who were not. On one hand, if Padua is particularly effective at helping clients secure employment, then the program may have more limited impact for those seeking to increase the intensive margin of work. On the other hand, one might expect those who already have employment to progress more quickly on some of the other program goals, such as higher, more stable wages.

In columns 2 and 3 of Table 4, we report the ITT results for those not working and working at baseline, respectively. There is a stark difference in labor market outcomes between those two groups. Comparing control group means (presented in the 4^{th} row for each outcome), those that enter the experiment unemployed are less likely to be employed (50 vs. 82 percent) at the 24 month follow-up, have lower monthly earnings ($918 vs. $1,489) and are half as likely to be employed full-time (29 vs. 63 percent). Thus, there is more room for Padua to have an impact among those unemployed at baseline. Indeed, that is what we find. Among those not employed at baseline, the treatment group is 13.1 percentage points more likely to be employed (a 26 percent, marginally significant increase), earn 46 percent more each month (significant at the 5 percent level), are 67 percent more likely to be employed full-time (significant at the 1 percent level), and work 39 percent more hours per week (significant at the 5 percent level). The standardized treatment effect for this group is 0.24, which is significant at the 5 percent level, although the p-value is 0.13 if one controls for the family-wise error rate among the six domain indices for this sample. Additionally, we find that this group is 9.5 percentage points (21 percent) more likely to have completed some college or more (unadjusted p-value = 0.050, results not reported). In contrast, we find a decline in work, earnings, full-time work, and hours among those originally employed at baseline, but all of these estimates are small and not statistically significant (all p-values are in excess of 0.45). We also find no evidence of improved education among this group (not reported). These results suggests Padua is not helping those who were already able to find employment move into better quality work.

As discussed in the vignettes earlier, Padua clients were at various levels of duress in their life at baseline, and the case management team would customize the service plan to each client's level of duress. For example, one measure of duress is whether clients were unstably housed when they entered the program. Unstable housing can be a substantial barrier to self-sufficiency. If an individual is worried about where they will sleep at night, they are less able to focus on other goals. Thus, the goal of the case management team was to address the issue of unstable housing before they began helping the client find employment or improve other financial outcomes. In contrast, someone that entered Padua in a more stable housing situation began working on these other domains at once.

The results in Table 4 support this narrative. In columns 4 and 5, we split the sample by their housing stability at baseline. We define someone as stably housed if they reported owning or renting their own place at baseline. The unstably housed group includes those who responded that they were paying some of the rent, living rent free with relatives or friends, experiencing homelessness or living in another non-leased situation.[34] The results show stark differences in labor market outcomes by baseline housing status.

[34] We classify those that respond "paying some of the rent" as unstably housed because this group is likely to include those who are living with relatives and friends because they cannot afford living independently. Although it is possible that those who receive rent subsidies could also respond that they are "paying some of the rent", in actuality this was not the case–95

Among the unstably housed at baseline, there is little positive effect of Padua on labor market outcomes. Except for the outcome on legal work, the sign of the ITT estimates are negative and not significant. In contrast, there are large labor market benefits for those stably housed at baseline. Employment is up 14.7 percent (marginally significant), earnings are up 34 percent (significant at the 1 percent level), and full-time work is up 37 percent (significant at the 1 percent level). While Padua does little to improve labor market outcomes for the unstably housed at baseline, as we show below, Padua leads to a large improvement in housing stability for this group.

These treatment effects for subgroups suggest that the intervention appears to be most helpful in terms of employment for those with some, but not too many barriers to work. To explore this further, we examine the treatment effects for the intersection of two of the subgroups: those who are stably housed but not employed at baseline (column 6).[35] Our results suggest that Padua has a large, positive effect on labor market outcomes for this group. The treatment effect is positive and statistically significant for five of the six outcomes reported (with legal work status being the exception), as well as for the standardized treatment effect, which is significant at the 1 percent level, and has a p-value of 0.006 after adjusting for the family-wise error rate across all six domains.

Because these five subgroups were not specified prior to data collection, we further construct a number of sample splits that a priori could have been plausible groups to consider. In Figure 8, we report the standardized treatment effect for labor market outcomes for more than 36 subgroups that are determined from responses to the baseline survey. Subgroups, which are indicated along the vertical axis, are based on parental education to capture inter-generational poverty, housing stability to measure degree of crisis at baseline, employment, gender, age, level of education, presence of children, language, as well as groups determined by high or low values of the 6 domain indices at baseline. The horizontal bars represent the 95% confidence interval for the estimate, and the gold vertical line shows the standardized treatment effect for the full sample. In addition, we use a randomization-based inference approach discussed in Section V.B to calculate adjusted p-values that account for the multiple comparisons across subgroups (Chetty, Hendren and Katz, 2016), which are reported in brackets. We denote statistical significance based on this approach with the marker. The reported treatment effects are sorted by the magnitude of the estimate. For nearly all of the subgroups, the standardized treatment effect is positive, although many of these estimates are imprecise. Interestingly, the treatment effects for those not employed at baseline and for those stably housed are among the largest among the subgroups considered, and the intersection of these two subgroups, a subgroup proposed by an anonymous reviewer, has the largest standardized treatment effect on labor market outcomes of any group we examine. The adjusted p-value for this group is 0.037, which indicates only 3.7 percent of the 10,000 placebo replications generated at least one subgroup with an unadjusted p-value smaller than the actual undjusted p-value for this group. This result suggests that

percent of respondents who reported receiving housing assistance also responded that they were renting their own place, and were therefore classified as stably housed. While this measures imperfectly captures whether an applicant has unstable housing, we verify the measure by comparing the housing assets of Padua participants from their baseline assessment. Twenty-seven percent of those categorized as unstably housed report lacking safe housing or being at risk of losing their housing versus 9 percent among the stably housed group. The unstably housed group is also more likely (11 percentage points) to be living in a temporary or unaffordable housing situation.

[35] We thank an anonymous reviewer for suggesting that we look at effects for the intersection of these two groups.

the large effect for this group is not the artifact of making multiple comparisons across subgroups.

VI.C Results for Other Outcomes

As an intervention, Padua primarily focused on improving labor market outcomes for their clients. At the same time, the intervention was holistic and worked on many dimensions of a family's financial, social, emotional, and physical well-being. In the next five subsections, we examine results for housing, participation in government transfer programs, spending, debt and savings, and health, as these were all outcome domains that the program was designed to address. We present a separate table in each of these domains and the structure of these tables is identical to that in Table 4.

VI.C1 Housing

In Table 5, we report 24-month results for five housing-related outcomes (whether a person owns or rents, whether they live in public housing, whether their utilities were threatened or were disconnected in the past 6 months, whether they rated any neighborhood problems as a "medium problem" or greater, and whether they rated two or more problems as medium or greater) and the average standardized treatment effects for these outcomes.[36] The structure of the table is identical to that of Table 4 in that we report results for the full sample in column 1, the results by baseline employment status in columns 2 and 3, by baseline housing stability in columns 4 and 5, and for the subgroup that is both not employed and stably housed at baseline in column 6. Similar results from the 12-month follow-up are reported in Appendix Table A-6.

For the full sample, none of the treatment effects for specific outcomes are statistically significant, but the signs of these estimates all point towards improved housing outcomes, and the standardized treatment effect suggests improvement. Padua clients are 6.5 percentage points (9 percent) more likely to be stably housed at the 24-month follow-up, although this estimate is not statistically significant.[37] The signs of the treatment effect estimates are consistent with the notion that Padua clients are living in better neighborhoods. At the two-year follow-up, they are between 5 and 7 percentage points less likely to be living in neighborhoods with any or two or more "medium" or greater neighborhood problems, respectively, though these estimates are not individually statistically significant. The last row of column 1 indicates that Padua leads to about a tenth of a standard deviation improvement in housing outcomes, an estimate that is significant at the 5 percent level, although adjusting for the family-wise error rate among the six domain indices yields a p-value of 0.162. There do not appear to be differences in treatment effects by employment status at baseline (columns 2 and 3); for both groups the standardized treatment effects are about a tenth of a standard deviation. This evidence suggesting that Padua led to improved housing

[36]The two dummy variables concerning neighborhood problems were constructed using respondent answers to a series of survey questions about how much of a problem there was in their neighborhood with each of the following items: vandalism, teens creating a nuisance, police non-response, prostitution, sexual assault, drug dealing and use, mugging and gang violence. Respondents rated each issue as either "Not a problem at all;" "A small problem;" "A medium-size problem;" "A large problem;" or "A huge problem."

[37]We also examined homelessness as an outcome (see Appendix Table A-11). While the point estimates suggest a reduction in homelessness for the full sample and across the various splits, the homelessness incidence rates are very low and the result is highly sensitive to small changes in the sample.

outcomes complements other recent studies that have found that customized assistance to help households address their specific housing needs can lead to improved housing outcomes (Bergman et al., 2020).

When we look at housing outcomes by housing status at baseline (columns 4 and 5), we see positive and statistically significant results for the unstably housed group and little to no impact on the stably housed group, which is again consistent with how the case management team prioritized services based on the clients' situation at enrollment. The estimates indicate no effect on housing stability (defined as either owning or renting the unit) for those who were stably housed at baseline, but for those who were not stably housed, the fraction in a lease or ownership arrangement at follow-up increased by 34 percentage points (significant at the 1 percent level), which is 64 percent more than the control-group mean. The estimate for the standardized treatment effect for those unstably housed at baseline is positive but not statistically significant and similar in magnitude to the estimate for those who are stably housed at baseline. However, this estimate for the standardized treatment effect is very sensitive to including the effect on utilities disconnection. Padua doubles the likelihood that utilities are disconnected for those unstably housed, a marginally significant estimate that may be due to the fact that many in this group are moving into more independent living situations and therefore are more exposed to having utilities shut off. In fact, when we re-estimate the standardized treatment effect for this group excluding the outcome for utilities being disconnected the estimate is 0.26 standard deviations with an unadjusted p-value of 0.06. Treatment effect estimates for all the other subgroups we considered are reported in Figure A-2. The treatment effect is positive for all but one of the subgroups, and in many cases the estimate is significant or marginally significant. There is some evidence that the effect of Padua on Housing outcomes is greatest for groups that tend to be more disadvantaged, such as single mothers and those with less education, but there are exceptions to this pattern.

VI.C2 Participation in Government Transfer Programs

The primary goal of Padua was to move clients to self-sufficiency. An important component of this goal was reduced dependence on public (or private) programs that are designed to meet basic needs such as SNAP, TANF, Medicaid, and WIC. In practice, however, the intervention did not pursue this goal for all participants. As discussed above, for families facing an immediate crisis, Padua would often aim to help them receive government benefits or access other services in the community. In addition, although we find some evidence that the program led to increased earnings, the increase was not large enough to make clients ineligible for many of the programs we examine in most cases. So, the expected effect of the intervention on program participation for the full sample is ambiguous.

In Table 6 we examine the effect of Padua on the use of government transfer programs. Similar results at the 12-month follow-up are reported in Appendix Table A-7. At 24 months, 62 percent of control group participants are receiving some form of government transfers with the largest program being the Supplemental Nutrition Assistance Program (SNAP) with a 51 percent participation rate for the control group. Conversely, TANF participation is virtually non-existent in this population; less than 2 percent receive this form of cash assistance.[38]

[38]This low share is representative of the study setting. Among a set of likely eligible households in the ACS (Table 1), only

For the full sample, there is little evidence to indicate that Padua reduces government program participation. The point estimate for receipt of any government benefits (among the programs listed in the table) is negative, but very imprecisely estimated, so we cannot reject the hypothesis of no effect.[39] There is some evidence of a sharp decline in receipt of WIC benefits that is statistically significant, but there is not a clear pattern for the effect of Padua across other programs, and the estimated standardized treatment effect is small and not statistically significant.[40]

The results do not reveal noticeable differences across subgroups when separated by baseline employment. However, we again find statistically significant results when splitting the sample by housing status at baseline. The results provide some suggestive evidence of an increase in government benefit use among the unstably housed (column 4). For this population, the positive point estimates suggest an increase in participation rates in SNAP, TANF, SDA, and WIC, but only the increase in TANF participation is statistically significant. These results are consistent with the notion that case managers helped certain clients obtain housing stability by connecting them with public benefits. However, the standardized treatment, while negative (indicating more reliant on support), is not statistically significant for this subgroup. For those who are stably housed at baseline, by contrast, the point estimates suggest that Padua leads to a reduction in use of government programs. For this group, across all the programs, the point estimates are negative, suggesting less receipt of government support, and the standardized treatment effect indicates nearly a tenth of a standard deviation improvement (i.e. reduced support) and this estimate is statistically significant at the 5% level.

VI.C3 Spending

Padua case managers often worked with their clients to improve their budgeting in order to promote greater financial stability. We examine the extent to which Padua affected spending behavior by examining the treatment effects for a number of spending related outcomes including monthly rent expenditure, monthly spending on childcare, the use of a budget, total non-housing spending, and spending on food (Table 7). Similar results from the 12-month follow-up are reported in Appendix Table A-8. For the full sample, these results indicate that Padua had little effect on spending, but a sizable effect on budgeting behavior. It increased the likelihood that participants were using a budget 24 months after enrollment by 14 percentage points (24 percent, significant at the 1% level). This effect is very large for the groups that were more disadvantaged at baseline—those that did not work at baseline (39 percent, significant at the 1% level) and for those that were unstably housed at baseline (58 percent, significant at the 1% level). The large and statistically significant effect on monthly rent for the unstably housed is consistent with the results reported above suggesting that Padua helped those in this group obtain stable housing, increasing the

2 percent of households in Tarrant County or Texas report receiving welfare income in the past 12 months.

[39] We do not directly observe receipt of the EITC. Padua could have reduced use of the EITC for some participants with sufficient baseline earnings to put them in the phase-out range of the credit. However, given the low baseline employment rates of study participants, it is likely that EITC receipt increased for many participants as Padua led to greater involvement in the labor market.

[40] This decline in WIC benefits does not appear to be driven by a loss in benefits due to higher earnings. Only 2 percent of the sample has young children and household income greater than the eligibility threshold for for WIC of 185% of the poverty line in the 24-month follow-up survey.

likelihood that participants own or rent their living unit. For that outcome, we saw in Table 5 a very large and significant effect for the group that was unstably housed at baseline.

VI.C4 Debt and Savings

Padua also emphasized debt reduction and increased savings as goals for participants. In Table 8, we report the impact of Padua on savings and debt outcomes from the 24-month survey. Similar results from the 12-month follow-up are reported in Appendix Table A-9.

The study sample has some connection to both the formal banking industry and subprime credit market. Roughly two-thirds of study participants have a checking or savings account, and about 15 percent have borrowed using a payday loan in the last year. In the full sample, we find no differences in the use of these financial products between the treatment and control groups after two years. In contrast, we do see large, statistically significant decreases in the likelihood of rolling over a payday loan for the more disadvantaged subsamples, with 5–15 percentage point declines for those not employed and unstably housed. In general, the survey evidence on the effect of Padua on savings and debt is mixed and typically imprecisely estimated. We do see some indication of increased savings in the full sample ($4,900, not statistically significant), as well as the more advantaged subgroups. However, further analysis suggests these differences in mean assets are largely driven by a few outliers.[41] Finally, the results for whether the respondent has a retirement account are consistent with the increase in full-time employment that may come with additional benefits. For the full sample, the effect of Padua on whether the respondent has a retirement account is positive but not statistically significant, and the effect is large and statistically significant for those stably housed at baseline and for those stably housed and not employed at baseline—two groups for which we found large effects on full-time work.

We also see greater non-mortgage debt for the treatment group. Whereas, the treatment group has less non-mortgage debt at 12-month survey (18 percent, Appendix Table A-9), by the 24-month follow-up the difference becomes positive, large (33 percent), and statistically significant (p-value = 0.025). A component of the program was to address any human capital deficiencies, potentially through community college enrollment. It could be that Padua participants are taking on more debt in order to invest in human capital, which could lead to further gains in earnings in the future. We do find a statistically significant increase in student debt. A decomposition of the debt category suggests that the increase in overall debt is driven by changes in student and medical/legal debt. There is no evidence of an increase in credit card debt—in fact, results reported below based on administrative data indicate that credit card debt fell. An alternative explanation is that this difference is due to measurement error. In particular, this measurement error could be different across the treatment and control group due to Padua's focus on budgeting with clients. In year two of the program, case managers began pulling credit reports for its clients to help them better understand their financial situation. Thus, Padua clients had greater information about their

[41] To explore the sensitivity of estimates to outliers, we re-estimated treatment effects dropping those respondents who had values of total assets above various thresholds. Dropping the 3 observations above the 99^{th} percentile reduces the point estimate for the full sample to $574 (unadjusted p-value = 0.32). Lower thresholds (e.g., the 98^{th} and 97^{th} percentiles) further attenuate the point estimate. Similarly, the estimate from an inverse hyperbolic sine transformation suggests an increase in assets (roughly 17 percent), though it is imprecisely estimated (unadjusted p-value = 0.64).

outstanding liabilities. This may have led the treatment group to report more debt. While the timing of when case managers started pulling credit reports lines up with the estimates of the effect of Padua on non-mortgage debt (i.e. the treatment group reports more debt after year two), we cannot directly test the effect of pulling credit reports on reported debt with the survey data.[42] To further explore the effect of Padua on financial stability, we link study participants to administrative credit bureau data as discussed below.

VI.C5 Health

Finally, we explore the effect of Padua at the 24-month follow-up on health (Table 9), while the results at the 12-month mark are reported in the Appendix Table A-10. In the baseline and follow-up surveys, we asked participants to rate their health on a five-point scale from poor to excellent. At 24 months, we constructed an indicator for whether their health had improved or stayed "Excellent" since baseline. We find this effect is positive and large for the full sample and for all subgroups but the effect is statistically significant for those in better economics circumstances at baseline: those employed and those stably housed. Treatment group members are 14.7 percentage points (53 percent) more likely to report improved health from baseline (significant at the 1% level). This finding is consistent with other work that documents the relationship between income and health among low-earning households (e.g., Evans and Garthwaite, 2014). The evidence for other outcomes does not indicate that the improvement in self-reported health is due to greater access to or use of health care; we find no large differences in self-reported medical insurance coverage, ER visits, or doctor visits in the preceding 12 months. The treatment effect estimate when looking at whether the individual experienced a medical hardship at the time of the 24-month survey is negative and large (21 percent), but is not precise. The standardized treatment effect for the full sample is positive, but small (0.06 standard deviations) and not statistically significant.

VII Results from Administrative Data

To complement our results for some of the key survey-based outcomes, we also linked study participants to administrative data on earnings and employment, government program participation, and financial information. In addition to providing an independent source for several key outcomes for our study, the administrative data have the advantage of being available for two-years prior to enrollment in the study and for at least four and a half years after. Also, the administrative data will help address concerns about potential biases that might arise due to survey non-response and under-reporting of income in surveys (Celhay, Meyer and Mittag, 2022; Meyer, Mok and Sullivan, 2015; Meyer et al., 2021), although it is important to note that these administrative data may also introduce other biases related to geographic attrition (Foote and Stange, 2022).

[42] We also examined whether this result is sensitive to how we treat outliers. For example, we tried restricting from the sample those in the top 1, 2, and 3 percent of the distribution of nonmortgage debt; we estimated median regressions; and we estimated models with an inverse hyperbolic sine transformation of nonmortgage debt. For all of these alternative approaches, the estimated treatment effect was qualitatively similar to that reported in Table 8 (still positive although not always significant).

VII.A Labor Market Outcomes

We linked our study participants to Unemployment Insurance (UI) system earnings and employment information for 2 years prior to and four and half years after randomization. These UI records were accessed from the Texas Workforce Commission through the Ray Marshall Center (RMC) at the University of Texas. Although the sample size for our analysis sample for administrative employment and earnings ($N = 325$) is similar to that for our two-year follow-up survey ($N = 346$), the composition of these samples is different for two reasons. First, we have administrative records for some study participants who did not respond to the second survey wave. Second, we can only link an individual to UI records if we obtain their SSN through SNAP and TANF records, which we have for those in our sample who participated in one of these programs at any point since January 2013 (76% of our study participants). See Appendix G.1 for more details. Among these individuals, those without a wage record in a given quarter are considered to not have formal-sector employment in Texas during that quarter and are assigned a value of 0 for earnings and employment. Thus, an individual's outcome could have a value of 0 in the data if they (i) did not work that quarter, (ii) worked in non-covered employment (e.g., worked informally "under the table"), or (iii) worked in UI-covered employment outside of Texas. While we do not have good information on the prevalence of non-covered employment, previous research indicates that movement out of state is infrequent for low-income populations.

In Figures 9a through 9f, we report average quarterly employment and earnings by quarter and by treatment status for the full sample as well as for those not employed at baseline and those stably housed but not employed at baseline—two subgroups for which we found large treatment effects for labor market outcomes captured in the 24-month follow-up survey. In each of the figures, the x-axis shows time relative to random assignment. Quarter 0 is the quarter that contains the individual's study enrollment date. For the employment figures, individuals are counted as employed if they have at least \$1 of earnings covered by the Texas UI system in a given quarter. We also report in Table 10 regression-adjusted treatment effects for earnings and employment in the 8^{th} quarter after enrollment, which aligns with the timing for when we observe these outcomes in the 24-month follow-up surveys, as well as average differences through 18 quarters following random assignment. In each regression, we include as potential controls all baseline characteristics used as controls in Table 4 and 8 quarters of pre-randomization employment indicators and earnings, selecting among all of these controls using the post-double selection LASSO methodology (Belloni, Chernozhukov and Hansen, 2014).

For the full sample, average quarterly employment rates are very similar for the treatment and control groups for four quarters prior to baseline, indicating balance in pre-randomization employment trends (Figure 9a). However, for the period from five to eight quarters prior to randomization, average employment rates appear to be higher for the treatment group. Employment drops noticeably for both the treatment and control groups just before enrollment in the program (Figure 9a). This pattern is consistent with the notion that many in our sample had recently experienced an earnings shock at the time of enrollment. At enrollment (quarter 0), about 40 percent of our sample were employed according to baseline surveys (Table 1). This is somewhat lower than the 60 percent observed with earnings in the administrative data. We would expect the rate to be higher in the administrative data given that those with any positive earnings throughout the quarter are classified as employed. For example, suppose an applicant worked in January lost their job in February and applied for Padua in March. This person would correctly report being unemployed in the intake survey, but would be considered employed in quarter 0 in the UI data.

In the first quarter after baseline, we see that employment for the control group bounces back, which is consistent with the Ashenfelter dip that was evident in the survey data (Figure 7). In subsequent quarters post-enrollment, employment rates are largely similar across data sources. Employment rates from the surveys for the control group at months 12 and 24 were 59 and 63 percent, respectively. For the administrative data, the rates are quite similar at 62 and 61 percent in quarters 4 and 8.

Although there are some differences, in general, the estimated treatment effects for labor market outcomes from the administrative data are comparable to the survey evidence. In quarter 4, the treatment group is 4.5 percentage points more likely to be employed based on UI records (Table A-13), as compared to 6.9 percentage points based on the 12-month survey. In quarter 8, this difference is 4.6 percentage points (Table 10), as compared to 6.1 percentage points based on the 24-month survey. Although the estimates are qualitatively similar, none of the treatment effects for the labor market outcomes measured in administrative data that we consider are statistically significant (Table 10). Recall that in the survey results we found significant treatment effects (based on un-adjusted p-values) for full-time employment at both 12 and 24 months. In the administrative data, we do not observe hours worked so we cannot construct a measure of full-time work.

The trends for earnings are qualitatively similar (Figure 9d and Table 10). We again see that both treatment and control group members experience a decline in earnings prior to application, and then rise afterwards. Average earnings for the control group in the 8^{th} quarter after enrollment was \$3,207, as compared to \$3,447 for the control group based on the 24-month follow-up survey (Table 4). We also again see evidence of treatment-control differences in post-period earnings. In the administrative data, however, the regression-adjusted treatment effect for the full sample is small (\$27) and not statistically significant.[43]

Figures 9b, 9c, 9e, and 9f show trends in outcomes among the subgroups for whom we found large effects based on the survey data. The regression-adjusted treatment effects for the same subgroups are shown in Table 10. In particular, we present earnings and employment results for those not employed at baseline and those that were not employed but stably housed at baseline. The estimates for these groups indicate noticeable differences in both employment and earnings, although these estimates are imprecise. The regression-adjusted difference in employment in the 8^{th} quarter after enrollment for those not employed at baseline was 4.4 percentage points (unadjusted p-value = 0.506) and the difference for earnings was \$317 (unadjusted p-value = 0.498). When looking at the average employment rate over the first eight quarters following enrollment, the difference in employment is 0.078 (unadjusted p-value = 0.106). For those not employed and stably housed at baseline, Padua leads to a 6.9 percentage point increase in employment in the 8^{th} quarter (unadjusted p-value = 0.360) and a 9 percentage point increase in average employment over the first eight quarters (unadjusted p-value = 0.110).

One benefit of the administrative data is that it allows us to explore effects beyond the two years

[43] Specifications that do not control for pre-existing differences in earnings using administrative data or that only control for characteristics measured in the baseline survey yield larger differences in earnings. For example, the unadjusted treatment-control difference in earnings in quarter 8 is \$428 (unadjusted p-value = 0.322).

measured by the follow-up surveys. We summarize effects over the 18 quarters following random assignment in the last two outcomes reported on Table 10. Over this 4.5 year period, the average employment rate in the control group, 62.1 percent, was similar to the employment rate during the first two years. The conclusions over this time period remain similar. The treatment group was 5 percentage points more likely to work over 18 quarters, and earned on average \$50 per quarter more, though these regression-adjusted differences are not statistically significant. Over this longer time horizon, we also see a similar pattern of results across the not employed and not employed and stably housed groups, relatively larger, but imprecise increases in employment rates and earnings. Regression-adjusted differences in employment and earnings by quarter are reported in Appendix Tables A-13 and A-14, respectively.

Taken together, that we find treatment effects of the same sign and similar magnitude using the administrative data as we did using the survey data is encouraging. However, it is important to qualify that the estimates from administrative data are imprecise. So we caution against drawing strong conclusions about the effects of Padua on labor market outcomes based on the administrative data results alone.

VII.B Government Programs

We also linked the Padua sample to benefits data for SNAP and TANF through the RMC as described in Appendix G.1. Because we have sufficient identifying information to link all of our study participants to the SNAP/TANF data, we specify the participants who are not in these administrative records at a point in time as not participating and not receiving any benefits. Consequently, we have an indicator for program participation and dollars of benefits received for all 427 study participants. Figures 10a through 10c report the average monthly SNAP participation rate and Figures 10d through 10f report the average dollars of SNAP benefits received by treatment status for 24 months before and 60 months after study enrollment for the full sample as well as for some subgroups. We also report regression adjusted treatment effects in in Table 10. Similar to the decreasing trends in earnings before applying to the program, the trends for SNAP receipt indicate that families received a negative shock just prior to application. SNAP participation increases sharply for both the treatment and control groups in the year prior to study enrollment, with nearly 60 percent receiving SNAP in the months just following randomization. Based on the baseline survey 61.4 percent of the control group and 64.8 percent of the treatment group reported receiving SNAP benefits. The baseline rates of SNAP receipt from the administrative data (50.5 percent of the control group and 50.8 percent of the treatment group) are somewhat lower than those from the survey. For the full sample, we see no noticeable difference by treatment status for either participation or dollars received as measured in the administrative data, and the regression-adjusted treatment effects for SNAP receipt 24 months after baseline are small and not statistically significant (Table 11). This null effect is consistent with the evidence from the survey data at 24 months (Table6).

Recall that for some Padua participants, particularly those who were in unstable situations and not ready to work, case managers helped them enroll in public benefits. For these clients we might expect Padua to lead to an increase in benefit receipt. Using the 24-month survey data, we found that Padua led to a large but imprecise increase in SNAP receipt for those who were unstably housed at baseline (Table 6). The results using the administrative data are consistent with this finding; for this subgroup we find that Padua leads to a 12.5 percentage point increase in SNAP (unadjusted p-value $= 0.091$) at 24 months after study enrollment (Table 11).

We also linked study participants to TANF data, but we do not include these results. Very few individuals receive TANF during our outcome window, less than 5 percent at the time of study enrollment.

VII.C Debt

We have linked Padua participants to their historical credit records from Experian, one of the largest credit bureaus in the US, from three quarters prior to random assignment through 17 quarters after. These data provide additional evidence on debt outcomes to complement those captured by our follow-up surveys, and allow us to track these key outcomes over a much longer period. We were able to match about three-quarters ($N = 326$) of the Padua study sample to a credit record in at least 1 of 21 quarters (from Q2-2014 through Q1-2021), and about two-thirds ($N = 286$) were linked to a credit record in every quarter in the panel. The treatment group was just as likely as the control group to have been linked to a credit record during the entire panel. See Appendix G.2 for more details on the data and the linking process.

The evidence from the credit report data suggest that Padua leads to a reduction in credit card debt, but it has no noticeable effect on credit score. We report the average credit score by treatment status over the 21 quarters for our balanced panel (Figure 11a) as well as the distributions of the credit scores by treatment status in quarter 8 (Figure 11b). These data indicate that Padua study participants had very low credit ratings–about 90 percent of both the treatment and control groups have a subprime credit score (score of below 650) during quarter 8–and these ratings changed very little over time. We also find no discernible difference in the patterns by treatment status–for both groups, the average credit score remains slightly below 550 throughout the period. In Table 12, we report the estimated treatment effects at 8 quarters following random assignment for outcomes that are similar to those presented in Table 8. In these regressions, we use the post-double section LASSO procedure (Belloni, Chernozhukov and Hansen, 2014) to select among our primary controls as well as three pre-randomization quarters of the outcome. These results indicate that we can reject even a small effect of Padua on credit score. Additionally, we see no evidence of differences in the distribution of credit scores by treatment status two years after study enrollment (Figure 11b).

In Figure 12a, we plot the the share of study participants with any credit card debt (including bank cards, revolving charge accounts, and charge cards) by treatment status. While there appears to be a slight rise in the share with credit card debt after study enrollment, there is no evidence of a difference across groups. At 8 quarters after enrollment, the difference in the share with credit card debt across groups is small and not statistically significant (Table 12). In contrast, we do see evidence of a treatment effect on the total amount of credit card debt (Figure 12b and Table 12). Eight quarters after random assignment, outstanding credit card balances are \$404 (66 percent) lower for the treatment group as compared to the control group and this difference is statistically significant. The analogous treatment effects using survey data (Table 8) also suggested a negative effect, but these estimates were smaller and very imprecise. Using credit report data, the negative effect is evident for all of the subgroups we report in Table 12, although it is most noticeable for those employed at baseline and for those stably housed at baseline.

The most noticeable difference between the results from the survey and those from the credit report data are for the outcome: total non-mortgage debt. Using the survey, we find that Padua leads to a statistically significant increase in this debt, while the credit report data suggest a negative treatment effect that is not statistically significant. As we discussed above, the positive effect from the survey data could be due to Padua's focus on budgeting with clients, which could have led the treatment group to be more aware of their debt and therefore report more of it. That we do not find this positive effect of Padua on non-mortgage debt in credit report data is consistent with this possible explanation.

VIII The Benefits and Costs of Padua

In this section, we benchmark the benefits experienced by Padua participants to the cost of providing the program. To allow for a direct comparison of cost effectiveness across a range of potential interventions for low-income workers, we construct estimates of the marginal value of public funds (MVPF) to implement Padua (Hendren and Sprung-Keyser, 2020). This approach requires constructing the ratio of the willingness to pay (WTP) of the program participant and the net fiscal cost of providing the program.

We first estimate the WTP of Padua. Padua is a comprehensive program designed to improve the human capital of participants. Through their work with caseworkers, participants might be connected to training or education programs that directly target soft and hard skills. Moreover, case managers might increase a client's human capital in other ways by expanding their network or through barrier removal. Following Hendren and Sprung-Keyser (2020) when estimating the WTP of programs designed to increase labor market opportunities, we assume a client's willingness to pay is equal to their after-tax earnings gains, as well as differences in financial assistance received by the treatment and control group. For each quarter following random assignment—quarters 0 through 19—we estimate treatment-control differences following Equation 1 and report these effects on employment and earnings in Appendix Tables A-13 and A-14, respectively.[44] We calculate the net present value of these experimental impacts during the five years following random assignment.[45]

Including the costs of the case management team, the program managers, the financial assistance and other operating costs, Padua's total cost per study participant is roughly $22,950 (2016 dollars). Our cost estimate includes monthly expenses for program years 2015–2018 provided by CCFW that include salary and wages, fringe and payroll taxes, professional fees (i.e. training), operating costs, an occupancy and use allowance, indirect costs, and financial assistance.[46] Just over $3,000 of the total program cost is the

[44] In addition to the set of controls used in our analysis based on survey data, we further control for eight quarters of pre-randomization earnings and indicators for employment. We use a post-double selection LASSO approach to select among this set of controls (Belloni, Chernozhukov and Hansen, 2014).

[45] We deflate point estimates to 2015 dollars using the CPI-U for the Dallas-Fort Worth-Arlington, TX region accessed through the FRED database, and assume a 3 percent discount rate. We follow Hendren and Sprung-Keyser (2020) by cross-walking between percent of the federal poverty line (FPL) and tax-transfer rates given in their paper, using average earnings (conditional on positive) of the treatment group relative to a federal poverty line of $24,250 (the poverty threshold for a household of 4 in 2015).

[46] In determining total costs, we allocate monthly program costs to study participants receiving services. Because only study participants were on the Padua case load from 2015 through 2017, we use total monthly costs for these program years. In 2018, CCFW began enrolling new clients into Padua who are not included in this study. We remove a proportion of total costs for program year 2018 and beyond to account for the roughly 20 percent of new clients enrolled that year.

difference in assistance received between the treatment and control groups, though this difference in costs does not account for any services or assistance received by the control group at other service agencies.[47] Of the remaining approximately \$19,900 in program cost, 82 percent is attributable to the total compensation (salary, wages, fringe, and payroll taxes) of the program staff. The net fiscal cost of the program that enters the denominator of the MVPF includes the per participant cost less any estimated fiscal savings from changes in taxes paid due to earnings gains.

Table 13 summarizes the WTP and net fiscal cost of Padua and reports estimates of the MVPF. Columns (1), (3), and (5) report estimates of the MVPF, WTP, and net cost, respectively. Columns (2), (4), and (6) report 95 percent confidence intervals of these estimates that are based on 10,000 simulated draws of earnings effects drawn from a joint normal distribution with means and standard errors of the actual effects.[48] We report estimates for the full sample, as well as the subgroups considered in Table 4. For each sample, we vary the time horizon over which earnings effects accrue: actual experimental impacts years 1 through 5; projecting effects 10 years after random assignment; and projecting effects 28 years after random assignment when the typical applicant turns 65.[49]

Using 5 years of administrative data on earnings, we estimate the willingness to pay of the Padua program to be roughly \$4,000 for the typical participant. Taking into account increases in taxes paid, the net present cost of the program is \$22,725. These figures yield an MVPF estimate of 0.176. This figure falls below estimates of other workforce programs that target low-income adults. For example, the average MVPF estimate among job training programs analyzed by Hendren and Sprung-Keyser (2020) was 0.44, and changes to unemployment insurance (e.g., changes in benefit generosity or duration) yield an average MVPF of 0.61. This main estimate is sensitive to assumptions about how long gains in earnings persist into the future. Under the assumption that effects in year 5 persist for another 10 years, the MVPF increases to 0.265. Similarly, under the assumption gains persist until retirement at age 65 (28 years after random assignment for the typical applicant), then the MVPF is 0.506.

Our exploratory results on treatment effect heterogeneity suggest that the program could be more cost-effective if targeted to individuals who are more likely to benefit. Similar to our survey findings, those who entered the program lacking employment or having stable housing experience larger gains in earnings in the five years following random assignment. WTP estimates are \$7,261 for those without employment, \$6,581 for those with stable housing, and \$11,759 for those without employment but who have stable housing. This translates into larger MVPF estimates. For these targeted groups, MVPF estimates range from 0.30

[47] Assistance for the treatment group includes the value of cash and in-kind assistance recorded in the CCFW program database. We impute assistance received by the control group using information from the CCFW call center and average assistance payments. The amount here is larger than the amount reported in Table 2, which only reports assistance received during the first 24 months of the program. Because we measure benefits beyond 24 months in the administrative data, we include costs incurred beyond 24 months in this analysis. The average Padua client is served for 20 months, so the average cost per participant per year is \$13,770 = (\$22,950 / 20 months) * (12 months / year). Per participant program costs incurred over the first 24 months—the time at which the final follow-up survey was conducted—were approximately \$18,300 (2016 dollars), of which \$2,260 is the value of assistance received by the treatment group relative to the control group.

[48] We follow Hendren and Sprung-Keyser (2020) in adding in a correlation structure where effects in quarters 0 through 2 are negatively correlated with the effects in quarters 3 and beyond.

[49] Projections hold constant the proportional effect on earnings in year five through the rest of the time horizon, accounting for changes in control group earnings based on lifecycle effects measured in the 2014–2016 American Community Survey Ruggles et al. (2021)

to 0.56 when relying on experimental estimates, and grow to 1.02 to 5.98 when assuming earnings gains persist into the future.

We conclude this discussion by noting important caveats for analysis. First, we abstract away from benefits clients may receive that could be important for cost-benefit calculations. Health, in particular, could be a sizable component of a client's willingness to pay that is missing from our analysis. We find evidence that Padua improved self-reported health both in our full sample, but also among the employed and stably housed groups. We also find increases in housing stability and the likelihood of having legal work status. Improvements on all three margins would suggest our WTP estimates might understate the total benefits. Third, we do not account for the time clients spend with their case management team. While clients do face an opportunity cost of their time spent with their case management team (e.g., foregone leisure), they do choose to attend meetings to receive free coaching and case management services, which suggests positive welfare gains. The average client spends 48 hours in phone or in-person meetings with their case management team over the first two years. Suppose the market rate for individual coaching/counseling is about \$75 per hour. Then, perhaps an upper bound for this benefit is roughly \$3,600 (or just under the NPV of after-tax earnings gains). Second, using changes in after-tax earnings might overstate the WTP of clients that are related to increased income. The results in Section VI suggest that the earnings gains experienced by Padua clients can in part be attributed to increases in labor supply—clients are more likely to work and are working more hours. An appeal to the envelope theorem would suggest that earnings gains from changes in labor supply should not be included in the WTP if clients were optimizing their labor supply prior to the intervention. However, this assumption might not hold in this context. The Padua intervention is designed to remove barriers to employment that likely create a wedge between the marginal benefit and marginal cost of effort (i.e., clients would work more in the absence of these barriers). We also see some suggestive evidence for education gains that are concentrated among the not employed group. Thus, the true welfare benefits associated with earnings gains are likely bounded by zero—per the envelope theorem—and the increase in after-tax earnings. Finally, estimates that come from the administrative UI data are still imprecisely measured. Under no scenario, are we able to reject the null that the MVPF is 0, and under the scenario that estimates persist until retirement, we are unable to rule out an MVPF estimate of ∞ for the "Not Employed & Stably Housed" group, which is the scenario when the Padua program would fully pay for itself. From January 2018 through June 2022, an additional 700 study participants were enrolled into Padua. Later work will update these estimates using a sample three times the present size.

IX Conclusion

Families in poverty often face multiple obstacles to obtaining a more secure economic footing, and each year more than \$200 billion is spent in the private charity and non-profit sector serving clients facing poverty. In response to client need, a large urban social service provider designed a holistic, individualized wrap-around service intervention designed to help families obtain their economic goals given their unique situations. We evaluate through an RCT the short- and medium-term impacts of this program.

We view the results in this work as encouraging but not definitive. Our results suggest that Padua leads to improved labor market outcomes. Padua clients saw improvements in both work and earnings, although the effect on earnings is not precisely estimated. The intervention increased full-time employment by 25 percent two years after initial enrollment. We also find that the intervention leads to improved self-reported health and reductions in credit card debt. For the full sample, there is less encouraging evidence for the effect of the intervention on outcomes such as reduced dependence on government benefits.

Given the customized nature of the services that Padua provides, the program is likely to have heterogenous effects for clients who enter the program with different circumstances. It is then not surprising that the most encouraging evidence is for particular subgroups. One third of the program participants are in unstable housing situations at randomization. As the case managers prioritize stabilizing the family first before trying to invest in the future, we find large and precise estimates that treatment families improved their housing situation and, through our 24-month follow-up survey, find little evidence that this group has improved employment outcomes. In contrast, those in stable housing were able to start working right away on solving employment issues and we find large impacts for this group.

There are two key research questions that we hope to address in future work. The first is understanding what components of Padua are driving outcomes. The small size of this initial RCT made this difficult to disentangle. As the intervention is expensive, finding if some of the costlier components can be reduced or eliminated without deleterious impacts on outcomes will make more comprehensive programs like this more viable for local social service organizations to operate. Second, we need a better understanding of what subgroups benefit from these intensive, wrap-around services. The success of Padua for particular subgroups in this analysis and the previous success of these types of programs for other subgroups (e.g., low-income college students) indicate these programs work in some situations, but understanding in a more systematic way who these groups are a priori is also key for improving the cost-effectiveness of such programs by better targeting benefits.

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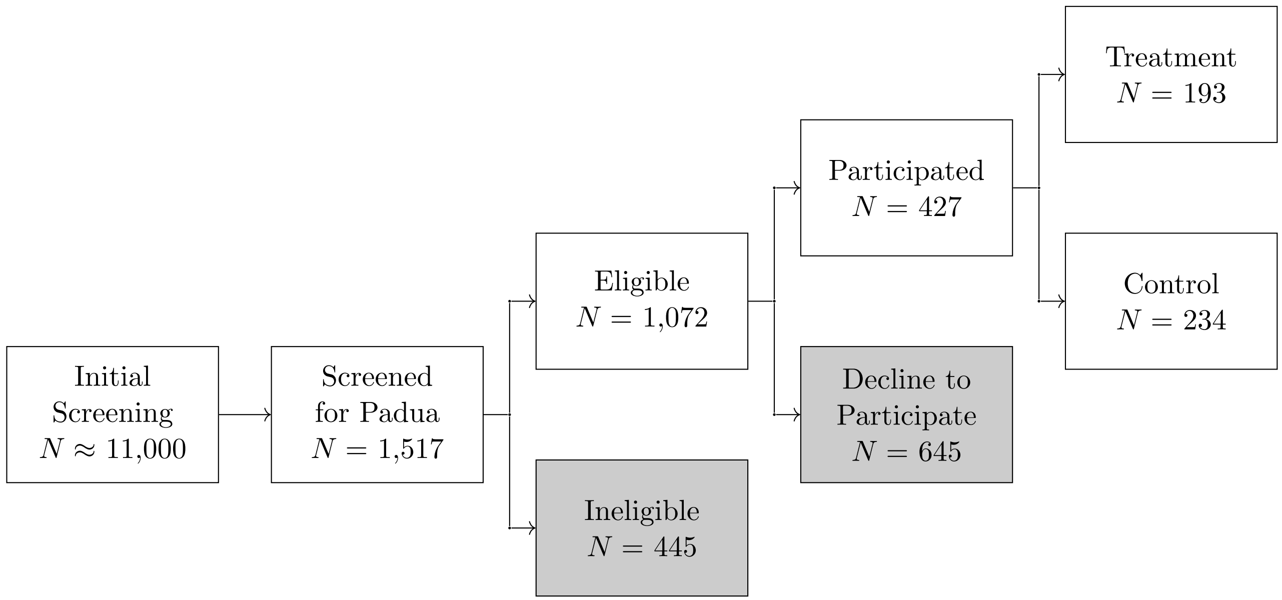
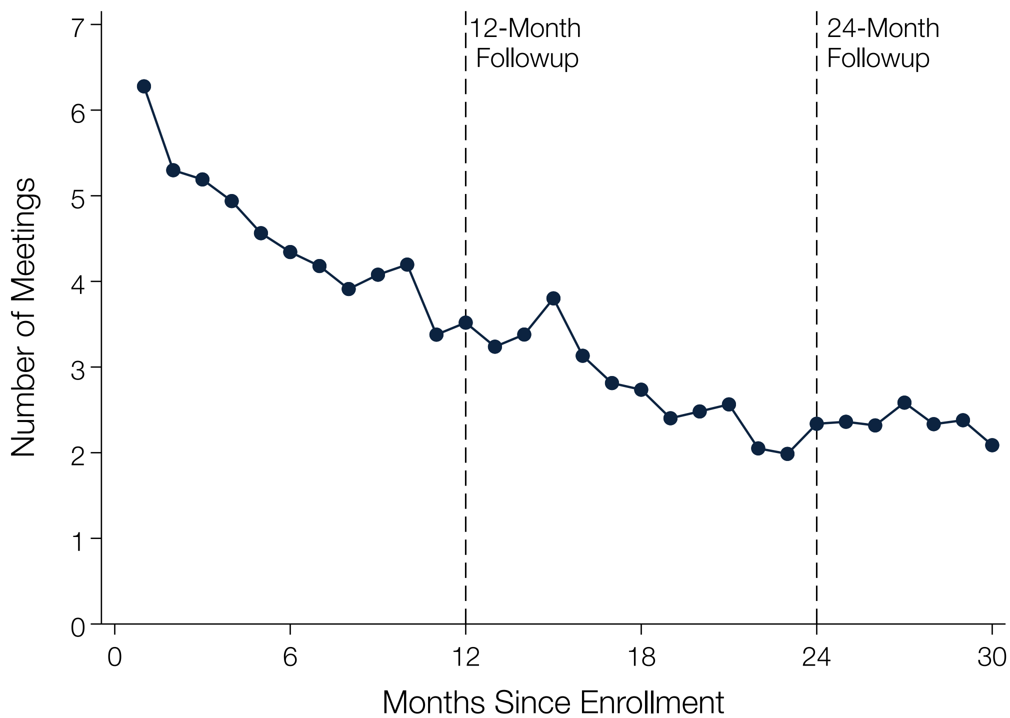
Figure 1: Recruitment for Padua



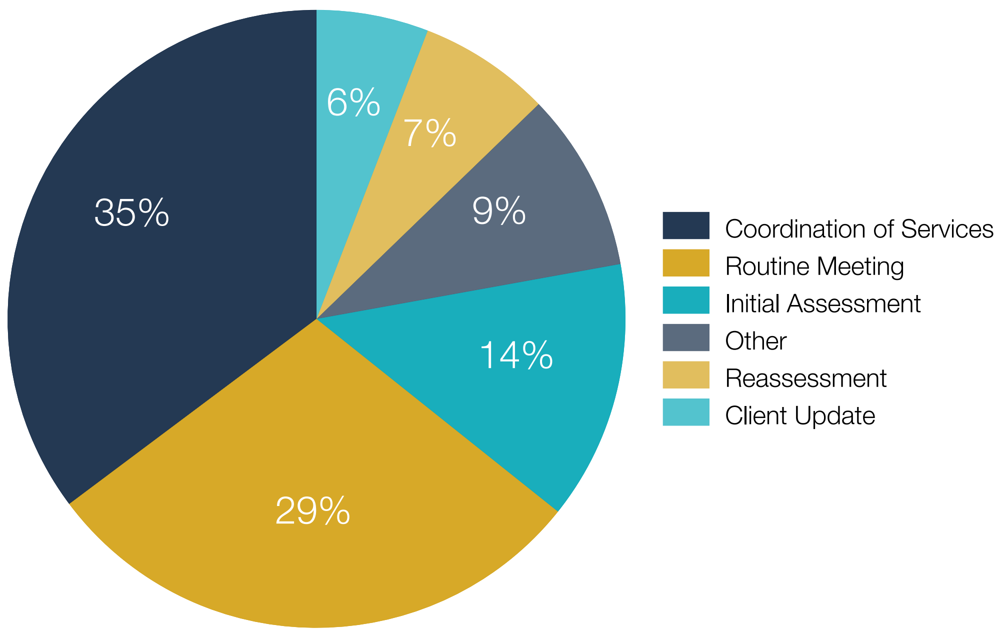
Notes: Data on program recruitment and intake come from Catholic Charities Fort Worth. See text for details on program eligibility and the randomization protocol.

Figure 2: Number of In-Person or Phone Meetings,
by Months Since Enrollment for Active Padua Clients



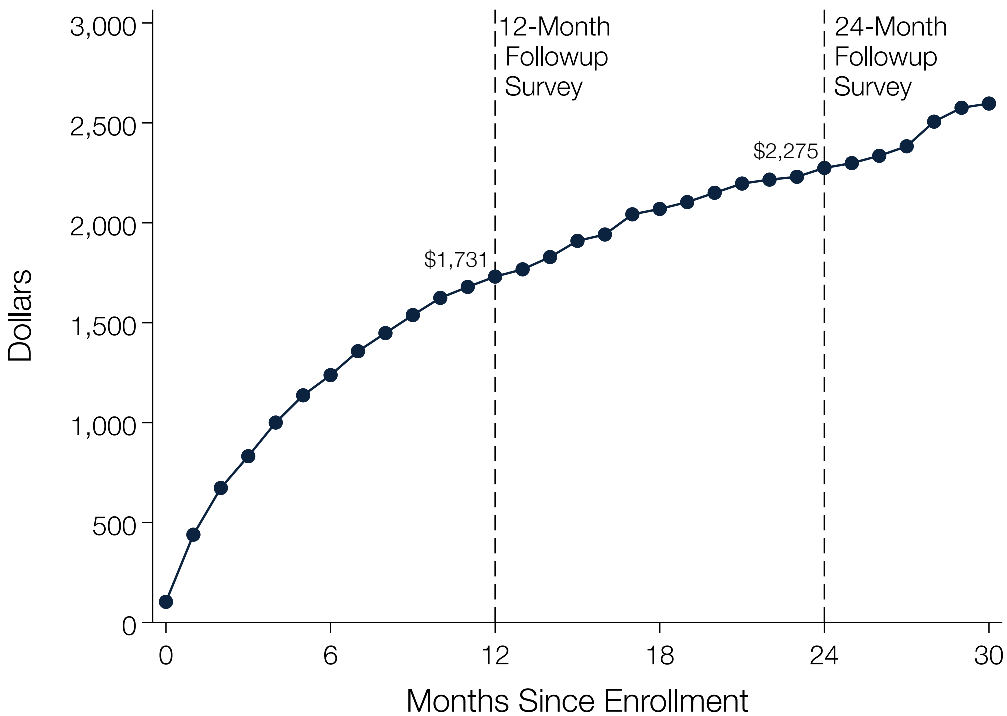
Notes: Data are from program case notes for the 176 study participants who were assigned to the treatment group and enrolled and took up Padua services. An exit is defined as the month in which a client's case was closed or dismissed in the program database. Exits typically occurred for one of the following reasons: Client met programmatic benchmarks and graduated from Padua; Client chose to exit the program; Client inactivity; Client moved out of Tarrant County and became ineligible.

Figure 3: Share of Case Management Time Spent on Various Activities, First 24 Months



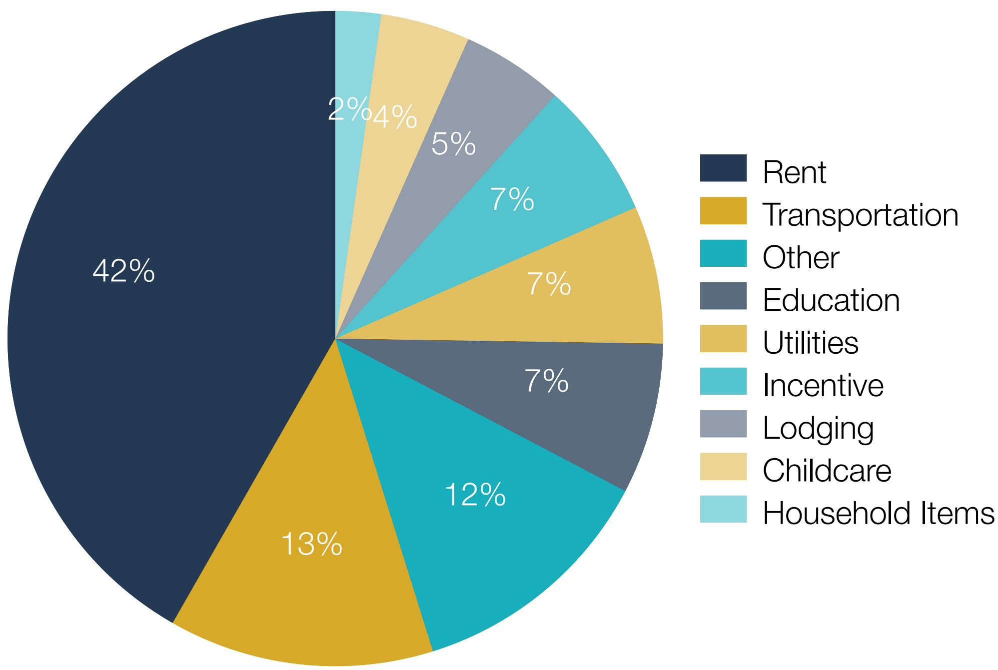
Notes: Data are from program case records for the 176 study participants who were assigned to the treatment group and who took up Padua services. Time is reported in individual service records. The sample is restricted to service activity records that occurred within 24 months following random assignment.

Figure 4: Average Cumulative Cash Assistance Received by Padua Clients, by Months Since Enrollment



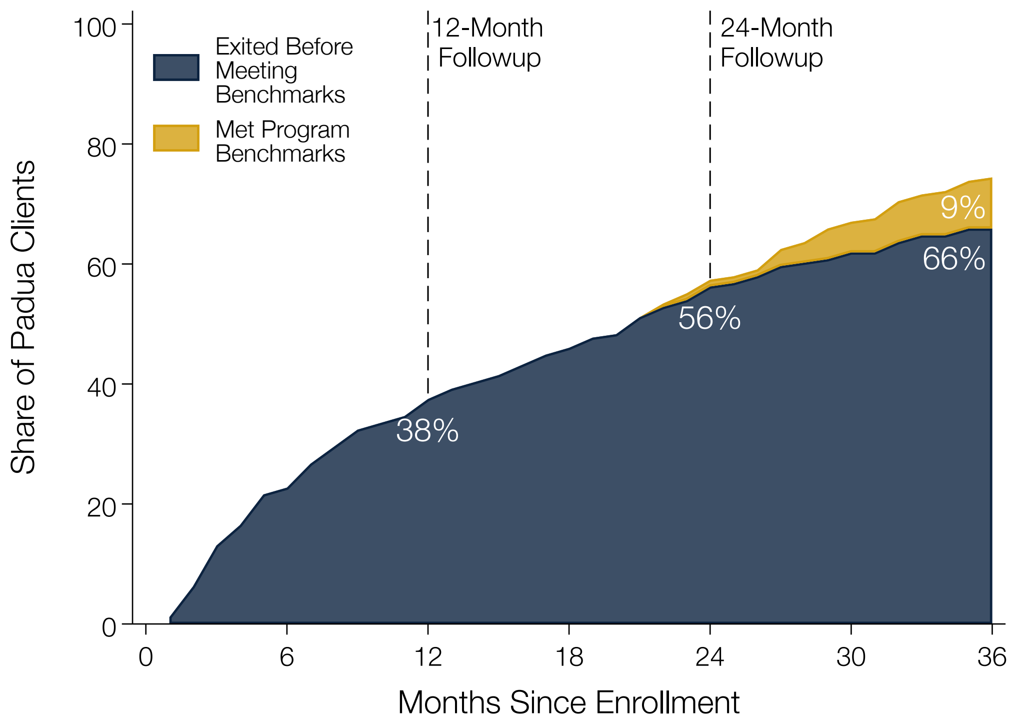
Notes: Data are from program case notes for the 176 study participants who were assigned to the treatment group and enrolled and took up Padua services. The sample of financial transactions excludes "In-Kind" assistance provided to the client.

Figure 5: Use of Cash Assistance, First 24 Months



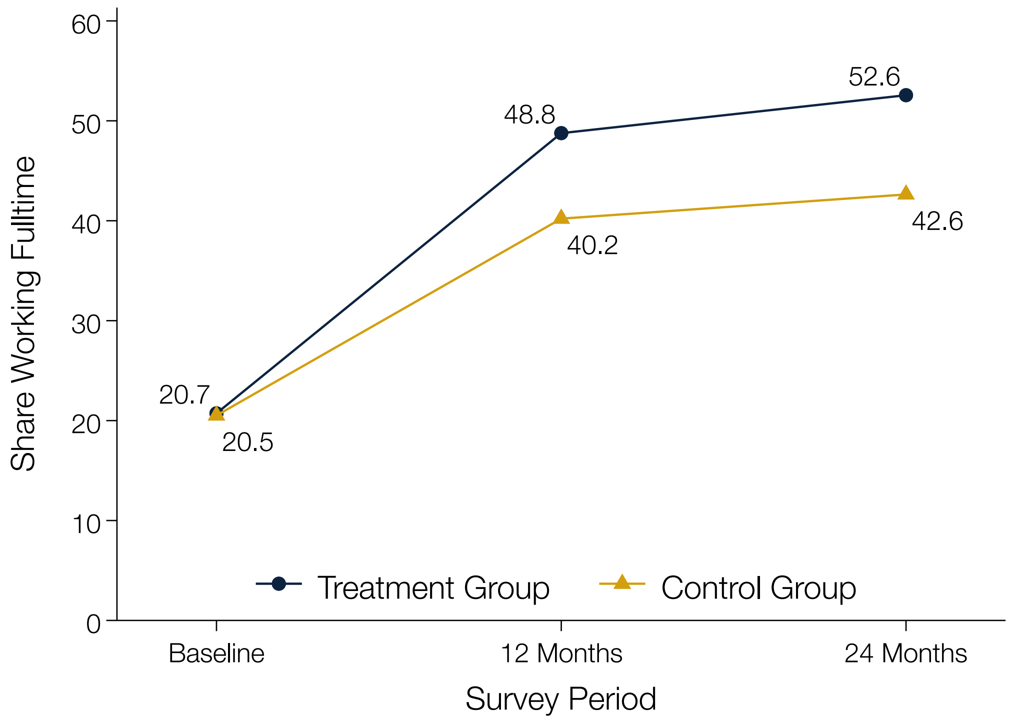
Notes: Data are from program case records for the 176 study participants who were assigned to the treatment group and who took up Padua services. The sample of financial transactions excludes "In-Kind" assistance provided to the client, and is restricted to requests that occurred within 24 months following random assignment.

Figure 6: Share of Padua Clients Exited Padua,
by Months Since Enrollment



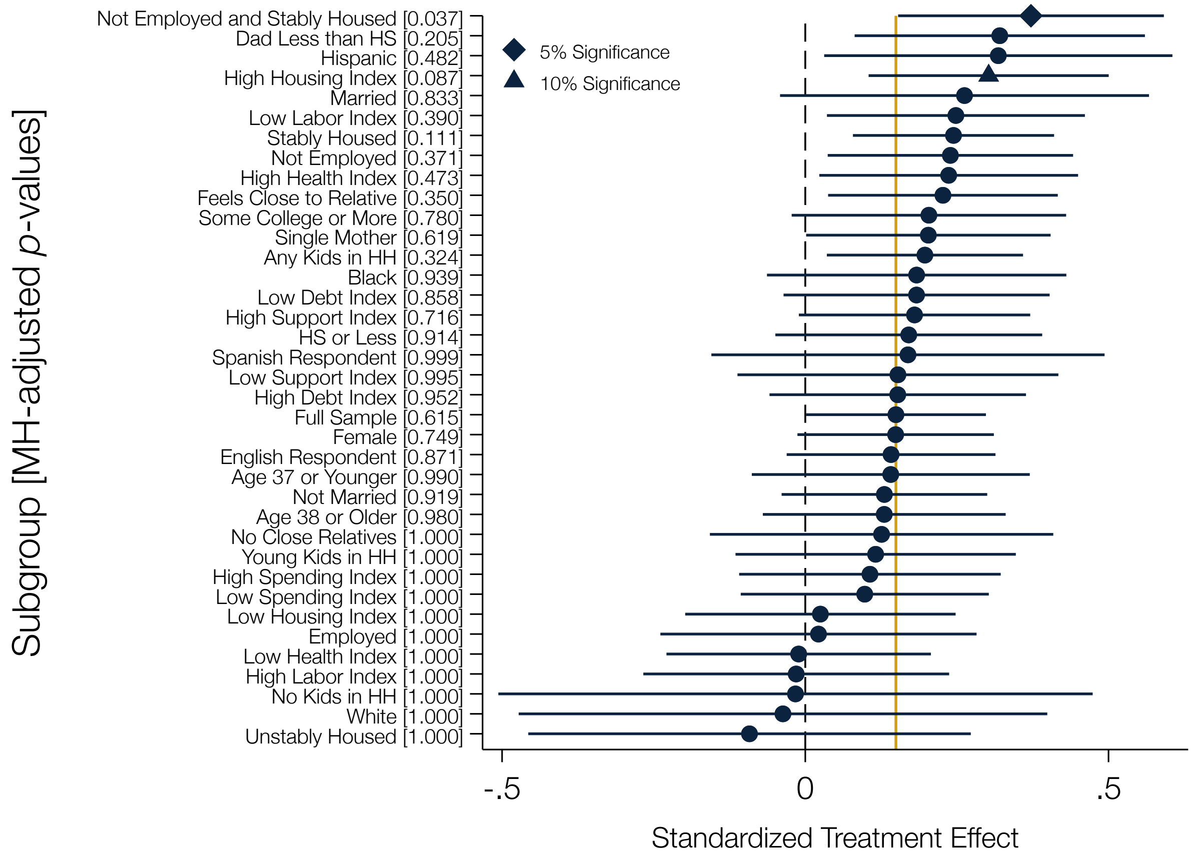
Notes: Data are from program case notes for the 176 study participants who were assigned to the treatment group and enrolled and took up Padua services. The figure plots the share of enrolled Padua clients who exited Padua by months since program enrollment. An exit is defined as the month in which a client's case was closed or dismissed in the program database. The figure plots cumulatively the share who exited Padua prior to meeting program benchmarks (navy) and share who graduated Padua having met all three out-of-poverty benchmarks (gold). Individuals who exited prior to meeting program benchmarks did so for one of the following reasons: client chose to exit the program; client inactivity; client moved out of Tarrant County and became ineligible. The complement of the share plotted in this figure (the white area) represents the share of Padua clients who are still active in Padua. For example, roughly 43 percent of Padua clients remained active 24 months after program enrollment.

Figure 7: Percent Working Full-Time By Treatment Status Over Time



Notes: Data are from baseline and follow-up surveys. The estimates reflect the share of participants that are working more than 35 hours or more per week by treatment assignment. The sample changes in each period to reflect the number of respondents in each survey. The sample includes 427 respondents at baseline, 351 respondents to the 12-month follow-up survey, and 346 respondents to the 24-month follow-up survey.

Figure 8: Effect of Padua on Labor Market Outcomes, by Subgroups

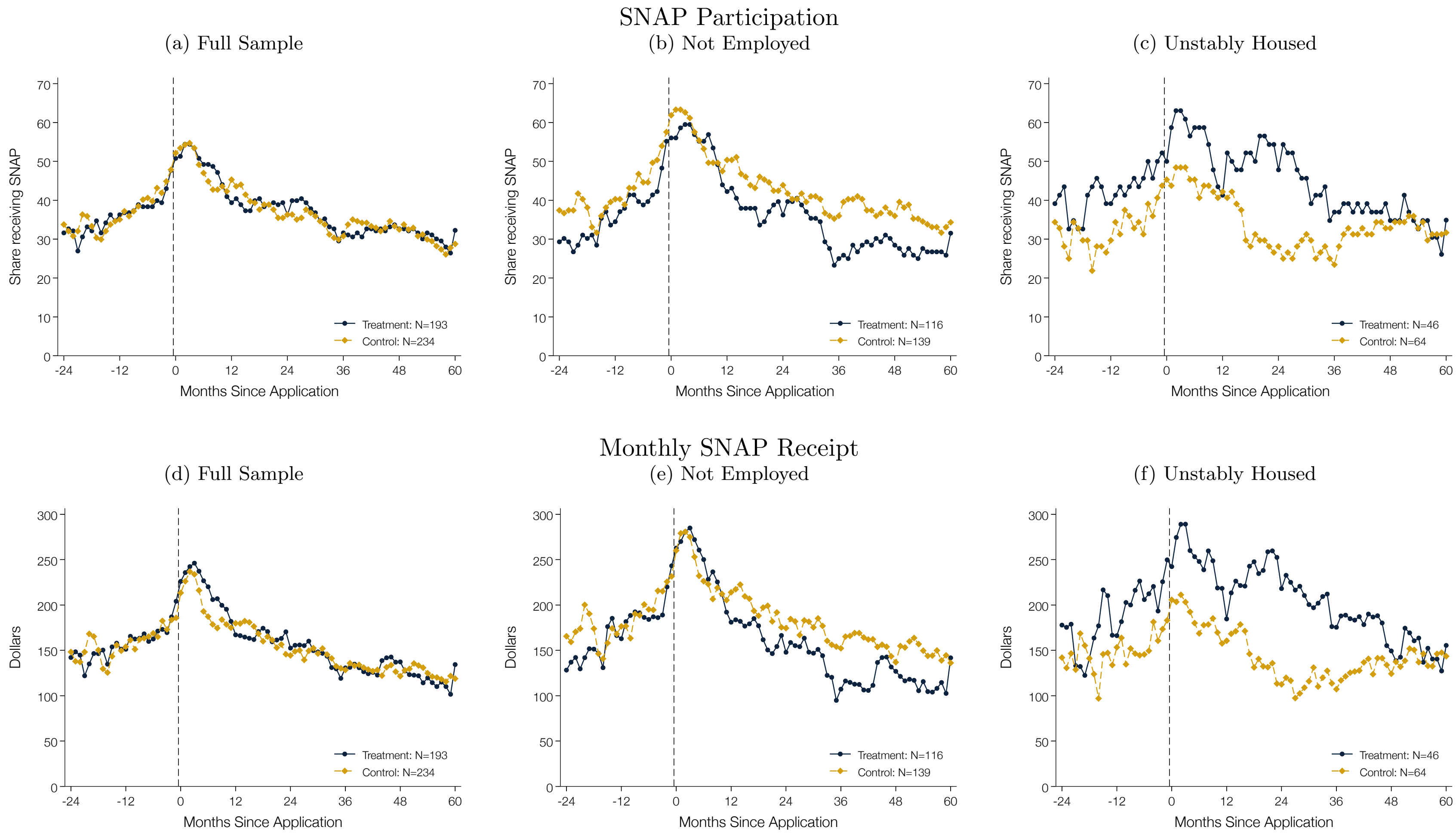


Notes: Data come from the baseline 24-month follow-up surveys. Each point depicts the estimated standardized treatment effect on outcomes in the Labor domain for the subgroup listed on the vertical axis. Subgroups are determined from responses to the baseline survey. The horizontal bars represent the 95% confidence interval for the estimate using heteroskedasticity-robust standard errors. p-values that adjust for the multiple comparisons made in the figure are listed in brackets next to the subgroup name (see Section V.B for details). Statistical significance based on these adjusted p-values are represented by diamond (5% significance) and triangle (10% significance) markers. The gold vertical line shows the standardized treatment effect for the full sample. See Table 4 for the list of outcomes that comprise the Labor domain.

Figure 9: Trends in Quarterly Employment and Earnings, by Subgroups

Notes: Data source is administrative UI earnings data from TWC. The sample includes 325 Padua study participants who were linked to UI records (see Appendix G for details). Quarter 0 represents the quarter in which a study participant completed the baseline survey and was randomized, and is thus a different calendar quarter for each person. Panels (a) through (c) plot formal-sector employment rates, which is defined as having UI-covered earnings in Texas greater than $0. Panels (d) through (f) plot average quarterly earnings. Treatment (navy circles) and control (gold diamonds) groups are based on an individual's randomly assigned treatment status. Panels (a) and (d) include all linked study participants. Panels (b) and (e) restrict the sample to individuals without employment in the baseline survey. Panels (c) and (f) restrict the sample to individuals with stable housing but no employment in the baseline survey.

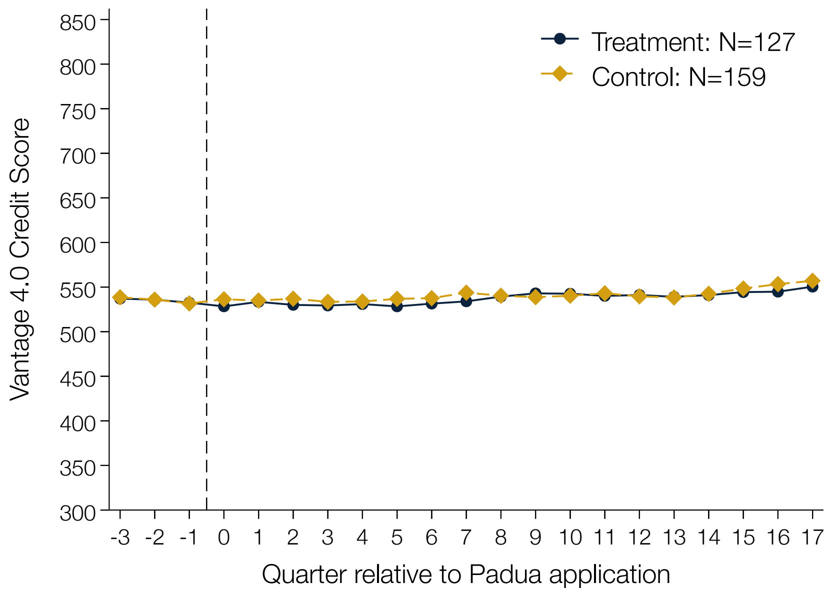
Figure 10: Trends in SNAP Receipt, by Subgroups



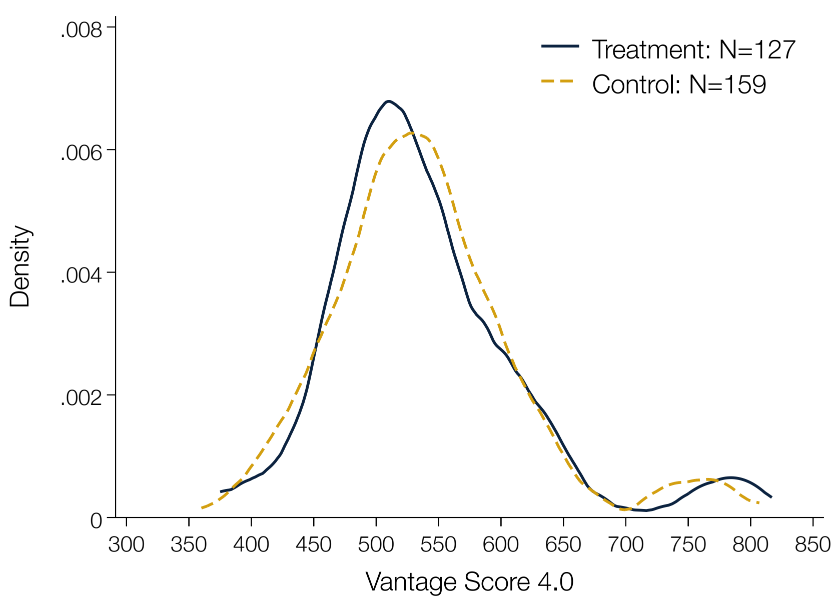
Notes: Data source is administrative SNAP receipt data from THHSC. The sample includes all 427 Padua study participants (see Appendix G for details on data linking). Month 0 represents the month in which a study participant completed the baseline survey and was randomized, and is thus a different calendar month for each person. Panels (a) through (c) plot SNAP participation rates. Panels (d) through (f) plot average monthly SNAP receipt. Treatment (navy circles) and control (gold diamonds) groups are based on an individual's randomly assigned treatment status. Panels (a) and (d) include all linked study participants. Panels (b) and (e) restrict the sample to individuals without employment in the baseline survey. Panels (c) and (f) restrict the sample to individuals with stable housing but no employment in the baseline survey.

Figure 11: Vantage 4.0 Credit Score, by Treatment Status

(a) Trends over Time Relative to Application



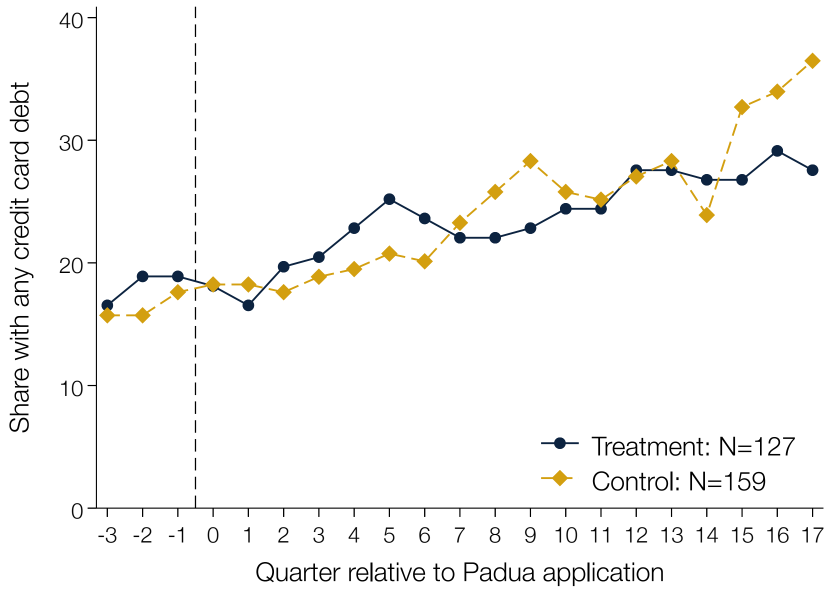
(b) Q8 Credit Score Distribution



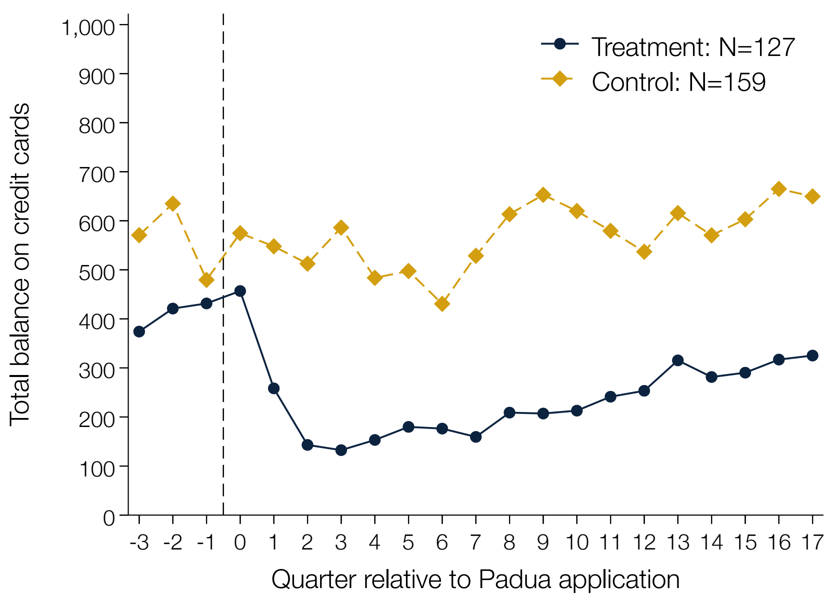
Notes: Data source is credit attributes from Experian. The sample includes 286 Padua study participants with a balanced panel of credit records (see Appendix G for details). Panel (a) plots average credit scores over time relative to randomization. Quarter 0 represents the quarter in which a study participant completed the baseline survey and was randomized, and is thus a different calendar quarter for each person. Panels (b) plots the distribution of credit scores in the eighth quarter following random assignment. Treatment (solid line, navy circles) and control (dashed line, gold diamonds) groups are based on an individual's randomly assigned treatment status.

Figure 12: Usage of Credit Cards, by Treatment Status

(a) Has Credit Card Debt



(b) Total Credit Card Debt



Notes: Data source is credit attributes from Experian. The sample includes 286 Padua study participants with a balanced panel of credit records (see Appendix G for details). Quarter 0 represents the quarter in which a study participant completed the baseline survey and was randomized, and is thus a different calendar quarter for each person. Panel (a) plots the share of individuals with any credit card debt. Panels (b) plots the average credit card balance. Treatment (solid line, navy circles) and control (dashed line, gold diamonds) groups are based on an individual's randomly assigned treatment status.

Table 1: Baseline Characteristics – Clients Who Responded to 24-Month Follow-up

	Study Sample				2012–2016 5-Year ACS		
	Control (1)	Treatment (2)	Difference in Means (3)	P-value of Difference in Means Test (4)	Tarrant County (5)	Texas (6)	USA (7)
Less than High School Education	0.274	0.321	0.047	0.345	0.236	0.237	0.162
High School Degree of GED	0.284	0.199	-0.085	0.063	0.295	0.282	0.286
Some College	0.247	0.282	0.035	0.469	0.287	0.319	0.350
College Degree	0.195	0.199	0.004	0.926	0.182	0.162	0.202
Black	0.500	0.449	-0.051	0.343	0.198	0.150	0.172
White	0.163	0.135	-0.029	0.458	0.343	0.310	0.509
Hispanic	0.263	0.340	0.077	0.124	0.369	0.472	0.222
Other/Multiple Races or Ethnicities	0.074	0.077	0.003	0.910	0.091	0.068	0.098
Age	37.2	37.3	0.1	0.882	33.8	33.4	32.7
Currently Employed	0.400	0.410	0.010	0.847	0.619	0.618	0.612
Female	0.847	0.853	0.005	0.893	0.732	0.718	0.686
Married	0.226	0.250	0.024	0.609	0.301	0.300	0.233
Household Size	3.89	4.04	0.15	0.457	3.34	3.28	3.05
Receives SNAP Benefits	0.626	0.679	0.053	0.302	0.302	0.299	0.301
Respondent Monthly Earnings	$562	$539	-$23	0.787	$767	$730	$712
Took Baseline Survey in English	0.789	0.801	0.012	0.787			
Experienced a Medical Hardship	0.216	0.205	-0.011	0.809			
Currently Experiencing Homelessness	0.058	0.058	-0.000	0.994			
Has Stable Housing	0.758	0.776	0.018	0.698			
Util. Disconnected/Notice of Disconnect, Past Year	0.571	0.635	0.063	0.233			
Percentage of Poverty Line	62.0%	62.4%	0.4%	0.952	93.8%	88.8%	83.1%
Single Mother	0.563	0.564	0.001	0.986	0.250	0.230	0.216
N	190	156			6,663	105,844	1,292,295
Prob > F				0.779			

Notes: Data are from baseline surveys for all participants who responded to the 24-month follow-up survey. ACS data aredownloaded from IPUMS (Ruggles et al., 2021), and include households that have heads between the ages of 18 and 55, have at least one adult worker, and have household income below 180 percent FPL.

Table 2: Services Provided by Catholic Charities Fort Worth Within 24 Months of Application

Panel A: Service Receipt by Treatment Assignment	Treatment	Control
Percent of Study Participants Enrolled in Case Management	91%	3%
Average Months in Case Management among Enrolled Clients	16.1	17.3
Percent of Study Participants Who Received Any Cash Assistance	77%	26%
Amount of Cash Assistance Per Allocation	$248	$317
Number of Allocations among Recipients	11.0	1.5
Total Cash Assistance among Recipients	$2,742	$461

		Percentile		
Panel B: Average Service Receipt among Padua Clients:	Mean	25th	50th	75th
Total In-Kind Assistance	$379	$0	$102	$477
Total Hours of Case Management Time	54.7	24	47	77
Total Hours or Phone or In-Person Meetings with Case Manager	47.9	21	40	69
Total Number of Phone or In-Person Meetings with Case Manager	61.8	28	56	88
Total Number of Electronic Communications	14.4	1	11	22
Total Number of Two-Way Communications	72.1	33	72	104

Notes: Data are from program records from Catholic Charities Fort Worth (CCFW). Panel A reports services received by the treatment and control groups. While the control group did not have access to Padua, CCFW does operate other less-intensive case management programs and the control group did have access to financial assistance. Most control group clients who received financial assistance did so through the CCFW call center. For these interactions, we only observe when a control group person contacted the call center, but not whether they received assistance or how much the assistance was for. Roughly 28 percent of callers receive assistance and the average amount given is $196. For these interactions, we impute the expected likelihood of getting assistance and how much they received. Panel B reports summary statistics of in-kind assistance received and time spent by and with the case management team for those enrolled in the program.

Table 3: Effect of Padua on Outcome Domains

Regression-Adjusted ITT (Standard Error) [Per Comparison p-value] {MH-Adjusted p-Value}

	Domains					
	Labor	Housing	Support	Spending	Debt	Health
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 12-Month Survey (N=351)						
Standardized Treatment Effect	0.139	0.067	0.024	-0.052	0.050	0.005
	(0.078)	(0.046)	(0.051)	(0.051)	(0.037)	(0.042)
	[0.075]	[0.141]	[0.676]	[0.322]	[0.178]	[0.910]
	{0.368}	{0.532}	{0.895}	{0.692}	{0.544}	{0.910}
Panel B: 24-Month Survey (N=346)						
Standardized Treatment Effect	0.149	0.096	-0.018	0.109	0.037	0.062
	(0.076)	(0.045)	(0.055)	(0.085)	(0.050)	(0.044)
	[0.049]	[0.030]	[0.766]	[0.212]	[0.480]	[0.160]
	{0.218}	{0.162}	{0.767}	{0.510}	{0.729}	{0.499}

Notes: Data come from surveys collected at baseline, 12 months after application, and 24 months after application. The table reports domain-level standardized treatment effects. The columns in each panel report the estimated standardized treatment effect on the outcome domain listed in the column header. Each estimate comes from estimating a single regression with stacked data for all outcomes in the domain (clustering standard errors at the applicant level), and averaging the ITT estimates for each outcome re-scaled by the outcome's standard deviation. Below the ITT estimates, we report standard errors in parentheses, randomization-based p-values in brackets, and p-values that control for the family-wise error rate across the six domains within the survey wave (Westfall and Young, 1993; Jones, Molitor and Reif, 2019) in braces.

Table 4: ITT Estimates of the Effect of the Padua Program on Labor Market Outcomes, 24-Month Results

Regression-adjusted ITT (Standard error) [MH-adjusted p-value] {Control group mean}

	Full Sample	Subgroups Defined by Baseline Characteristics				
		Not Employed	Employed	Unstably Housed	Stably Housed	Not Empl./ Stbl. Housed
	(1)	(2)	(3)	(4)	(5)	(6)
Currently Employed	0.061	0.131^{+}	-0.051	-0.060	0.095^{+}	0.167^{*}
	(0.050)	(0.069)	(0.072)	(0.128)	(0.054)	(0.075)
	[0.439]	[0.194]	[0.918]	[0.944]	[0.215]	[0.090]
	{0.626}	{0.500}	{0.816}	{0.565}	{0.646}	{0.512}
Respondent Monthly Earnings	$208	$421*	-$5	-$362	$387**	$708**
	(133)	(196)	(184)	(353)	(139)	(196)
	[0.308]	[0.125]	[0.978]	[0.716]	[0.028]	[0.002]
	{$1,149}	{$918}	{$1,489}	{$1,186}	{$1,137}	{$878}
Employed Full Time	0.105^{*}	0.193^{**}	-0.022	-0.164	0.163^{**}	0.270^{**}
	(0.052)	(0.067)	(0.091)	(0.117)	(0.059)	(0.071)
	[0.200]	[0.030]	[0.959]	[0.474]	[0.033]	[0.002]
	{0.426}	{0.289}	{0.632}	{0.391}	{0.438}	{0.286}
Hours Worked Per Week	3.86^{+}	6.86^{*}	-0.94	-2.75	4.99^{*}	8.73^{**}
	(2.26)	(2.96)	(3.57)	(6.30)	(2.44)	(3.16)
	[0.277]	[0.101]	[0.983]	[0.918]	[0.140]	[0.030]
	{23.29}	{17.69}	{31.70}	{20.74}	{24.11}	{18.13}
Percentage of Poverty Line	0.10	0.10	0.11	-0.03	0.22^{*}	0.27^{*}
	(0.10)	(0.14)	(0.16)	(0.32)	(0.11)	(0.14)
	[0.333]	[0.707]	[0.901]	[0.932]	[0.152]	[0.108]
	{1.14}	{1.10}	{1.20}	{1.19}	{1.13}	{1.06}
Can Legally Work in U.S.	0.033^{+}	0.001	0.072^{*}	0.054^{*}	0.029	-0.009
	(0.017)	(0.021)	(0.031)	(0.039)	(0.020)	(0.023)
	[0.214]	[0.960]	[0.094]	[0.156]	[0.133]	[0.721]
	{0.856}	{0.893}	{0.803}	{0.870}	{0.852}	{0.890}
Standardized Treatment Effect	0.149^{*}	0.239^{*}	0.022	-0.092	0.244^{**}	0.372^{**}
	(0.076)	(0.103)	(0.133)	(0.186)	(0.085)	(0.112)
	[0.218]	[0.132]	[0.869]	[0.868]	[0.024]	[0.006]
N	346	206	140	81	265	157

Notes: Data come from the 24-month follow-up survey. Column 1 includes all 24-month follow-up respondents. Each subsequent column uses a different sample of respondents based on listed baseline characteristic(s). Stable housing is defined as living in a dwelling that was owned or rented by the respondent. Unstable housing includes categories such as paying some of the rent, living rent free, homelessness, and other situations that did not qualify as renting or owning. Each set of estimates reports the treatment effect from a regression of the outcome on the treatment indicator, an indicator for cohort, the baseline value of the outcome, length of time between interviews, age, and indicators for month of interview, education, race, marital status, household size, employment status and earnings at baseline. Below the ITT estimates, we report standard errors in parentheses, p-values that control for the family-wise error rate within the domain and sample (Westfall and Young, 1993; Jones, Molitor and Reif, 2019) in brackets, and control group means in braces. The p-value for the Standardized Treatment Effect controls for the family-wise error rate among the six domain indices for that sample. The standardized treatment effect and adjusted p-values include estimates of the following outcomes reported in Appendix Table A-11: hours worked in primary job; and total household income (including benefits).

**, *, $^{+}$ report 0.01, 0.05, and 0.10 significance levels, respectively, using unadjusted p-values.

Table 5: ITT Estimates of the Effect of the Padua Program on Housing Outcomes, 24-Month Results

Regression-adjusted ITT (Standard error) [MH-adjusted p-value] {Control group mean}

	Full Sample	Subgroups Defined by Baseline Characteristics				
		Not Employed	Employed	Unstably Housed	Stably Housed	Not Empl./ Stbl. Housed
	(1)	(2)	(3)	(4)	(5)	(6)
Owns or Rents	0.065	0.100^{+}	0.016	0.337^{**}	0.003	0.068
	(0.042)	(0.055)	(0.070)	(0.113)	(0.044)	(0.059)
	[0.500]	[0.279]	[0.994]	[0.033]	[0.943]	[0.673]
	{0.763}	{0.737}	{0.803}	{0.565}	{0.826}	{0.786}
Lives in Public Housing	-0.020	-0.068^{+}	0.003	0.012	-0.032	-0.069^{+}
	(0.027)	(0.035)	(0.044)	(0.092)	(0.028)	(0.037)
	[0.697]	[0.281]	[0.942]	[0.895]	[0.701]	[0.336]
	{0.090}	{0.107}	{0.066}	{0.087}	{0.092}	{0.098}
Utilities Disconnected/Received Notice of Disconnect in Past Year	-0.006	-0.002	-0.016	0.244^{+}	-0.087^{+}	-0.078
	(0.051)	(0.066)	(0.081)	(0.127)	(0.052)	(0.067)
	[0.900]	[0.969]	[0.978]	[0.271]	[0.438]	[0.581]
	{0.466}	{0.451}	{0.487}	{0.267}	{0.528}	{0.536}
Any Neighborhood Problems (Medium or Worse)	-0.054	-0.005	-0.159^{*}	-0.065	-0.059	0.016
	(0.053)	(0.070)	(0.078)	(0.137)	(0.059)	(0.075)
	[0.762]	[0.997]	[0.229]	[0.865]	[0.691]	[0.825]
	{0.437}	{0.421}	{0.461}	{0.500}	{0.417}	{0.417}
Two or More Neighborhood Problems (Medium or Worse)	-0.069	-0.075	-0.054	-0.143	-0.059	-0.078
	(0.045)	(0.059)	(0.073)	(0.112)	(0.050)	(0.065)
	[0.445]	[0.564]	[0.914]	[0.558]	[0.735]	[0.705]
	{0.305}	{0.316}	{0.289}	{0.391}	{0.278}	{0.321}
Standardized Treatment Effect	0.096^{*}	0.103^{+}	0.113	0.130	0.101^{*}	0.114^{+}
	(0.045)	(0.054)	(0.073)	(0.102)	(0.048)	(0.059)
	[0.162]	[0.233]	[0.540]	[0.694]	[0.147]	[0.209]
N	346	206	140	81	265	157

Notes: Data come from the 24-month follow-up survey. Column 1 includes all 24-month follow-up respondents. Each subsequent column uses a different sample of respondents based on listed baseline characteristic(s). Stable housing is defined as living in a dwelling that was owned or rented by the respondent. Unstable housing includes categories such as paying some of the rent, living rent free, homelessness, and other situations that did not qualify as renting or owning. Each set of estimates reports the treatment effect from a regression of the outcome on the treatment indicator, an indicator for cohort, the baseline value of the outcome, length of time between interviews, age, and indicators for month of interview, education, race, marital status, household size, employment status and earnings at baseline. Below the ITT estimates, we report standard errors in parentheses, p-values that control for the family-wise error rate within the domain (Westfall and Young, 1993; Jones, Molitor and Reif, 2019) in brackets, and control group means in braces. The p-value for the Standardized Treatment Effect controls for the family-wise error rate among the six domain indices for that sample. The standardized treatment effect and adjusted p-values include estimates of the following outcomes reported in Appendix Table A-11: an indicator for currently homeless. **, *, $^{+}$ report 0.01, 0.05, and 0.10 significance levels, respectively, using unadjusted p-values.

Table 6: ITT Estimates of the Effect of the Padua Program on Support Outcomes, 24-Month Results

Regression-adjusted ITT (Standard error) [MH-adjusted p-value] {Control group mean}

	Full Sample	Subgroups Defined by Baseline Characteristics				
		Not Employed	Employed	Unstably Housed	Stably Housed	Not Empl./ Stbl. Housed
	(1)	(2)	(3)	(4)	(5)	(6)
Receives Any Government Benefits	-0.058	-0.099	0.009	0.113	-0.102^{+}	-0.132^{+}
	(0.049)	(0.063)	(0.082)	(0.119)	(0.055)	(0.072)
	[0.760]	[0.589]	[0.918]	[0.925]	[0.414]	[0.420]
	{0.621}	{0.667}	{0.553}	{0.587}	{0.632}	{0.679}
Receives SNAP Benefits	0.027	-0.018	0.092	0.189	-0.036	-0.054
	(0.049)	(0.065)	(0.084)	(0.132)	(0.056)	(0.076)
	[0.981]	[0.779]	[0.934]	[0.755]	[0.961]	[0.919]
	{0.505}	{0.535}	{0.461}	{0.457}	{0.521}	{0.548}
Receives TANF Benefits	0.024	0.026	0.016	0.117*	-0.020	-0.022
	(0.018)	(0.023)	(0.028)	(0.055)	(0.012)	(0.016)
	[0.765]	[0.850]	[0.980]	[0.338]	[0.602]	[0.772]
	{0.016}	{0.018}	{0.013}	{0.000}	{0.021}	{0.024}
Receives SDA Benefits	-0.026	-0.048	0.019	0.033	-0.025	-0.053
	(0.032)	(0.043)	(0.052)	(0.097)	(0.034)	(0.046)
	[0.954]	[0.804]	[0.981]	[0.743]	[0.953]	[0.812]
	{0.166}	{0.179}	{0.147}	{0.156}	{0.169}	{0.193}
Receives SSI Benefits	-0.017	-0.015	-0.015	-0.020	-0.012	-0.009
	(0.010)	(0.014)	(0.016)	(0.024)	(0.011)	(0.011)
	[0.587]	[0.782]	[0.985]	[0.936]	[0.908]	[0.883]
	{0.027}	{0.036}	{0.013}	{0.043}	{0.021}	{0.024}
Receives Unemployment Benefits	-0.003	-0.021	0.025	0.000	-0.001	-0.028
	(0.012)	(0.014)	(0.020)	(0.000)	(0.015)	(0.018)
	[0.829]	[0.844]	[0.948]	[0.881]	[0.943]	[0.791]
	{0.016}	{0.027}	{0.000}	{0.000}	{0.021}	{0.036}
Receives WIC Benefits	-0.065*	-0.068^{+}	-0.035	0.039	-0.085*	-0.058
	(0.032)	(0.040)	(0.053)	(0.075)	(0.035)	(0.044)
	[0.340]	[0.561]	[0.985]	[0.844]	[0.159]	[0.807]
	{0.147}	{0.132}	{0.171}	{0.087}	{0.167}	{0.143}
Standardized Treatment Effect	-0.018	0.047	-0.126	-0.106	0.086*	0.117**
	(0.055)	(0.054)	(0.126)	(0.077)	(0.040)	(0.050)
	[0.767]	[0.868]	[0.501]	[0.708]	[0.082]	[0.047]
N	346	206	140	81	265	157

Notes: Data come from the 24-month follow-up survey. Column 1 includes all 24-month follow-up respondents. Each subsequent column uses a different sample of respondents based on listed baseline characteristic(s). Stable housing is defined as living in a dwelling that was owned or rented by the respondent. Unstable housing includes categories such as paying some of the rent, living rent free, homelessness, and other situations that did not qualify as renting or owning. Each set of estimates reports the treatment effect from a regression of the outcome on the treatment indicator, an indicator for cohort, the baseline value of the outcome, length of time between interviews, age, and indicators for month of interview, education, race, marital status, household size, employment status and earnings at baseline. Below the ITT estimates, we report standard errors in parentheses, p-values that control for the family-wise error rate within the domain (Westfall and Young, 1993; Jones, Molitor and Reif, 2019) in brackets, and control group means in braces. The p-value for the Standardized Treatment Effect controls for the family-wise error rate among the six domain indices for that sample. The standardized treatment effect and adjusted p-values include estimates of the following outcomes reported in Appendix Table A-11: monthly SNAP benefit amount; monthly TANF benefit amount; monthly SDA benefit amount; monthly SSI benefit amount; amount of unemployment or worker's compensation received; and amount of support received from family or friends.

**, *, $^{+}$ report 0.01, 0.05, and 0.10 significance levels, respectively, using unadjusted p-values.

Table 7: ITT Estimates of the Effect of the Padua Program on Spending Outcomes, 24-Month Results

Regression-adjusted ITT (Standard error) [MH-adjusted p-value] {Control group mean}

	Full Sample	Subgroups Defined by Baseline Characteristics				
		Not Employed	Employed	Unstably Housed	Stably Housed	Not Empl./ Stbl. Housed
	(1)	(2)	(3)	(4)	(5)	(6)
Monthly Rent	$69	$88	$77	$408**	$3	$43
	(44)	(63)	(66)	(138)	(47)	(67)
	[0.542]	[0.672]	[0.872]	[0.041]	[0.998]	[0.974]
	{$516}	{$519}	{$513}	{$402}	{$553}	{$553}
Monthly Spending on Childcare	$100.93	$18.55	$308.70	-$2.26	$113.98	$42.50
	(83.88)	(34.23)	(257.66)	(73.50)	(98.20)	(40.19)
	[0.734]	[0.838]	[0.690]	[0.984]	[0.818]	[0.884]
	{$53.76}	{$49.17}	{$60.64}	{$64.89}	{$50.20}	{$43.82}
Uses a Budget to Detmine Spending	0.140**	0.231**	-0.016	0.351**	0.087	0.176*
	(0.048)	(0.061)	(0.083)	(0.104)	(0.055)	(0.070)
	[0.035]	[0.004]	[0.999]	[0.027]	[0.614]	[0.110]
	{0.595}	{0.596}	{0.592}	{0.609}	{0.590}	{0.595}
Total Monthly Spending without Rent	$67	-$50	$333	-$60	$75	$16
	(106)	(84)	(288)	(175)	(125)	(95)
	[0.934]	[0.929]	[0.845]	[0.991]	[0.983]	[0.972]
	{$1,175}	{$1,181}	{$1,167}	{$1,181}	{$1,174}	{$1,159}
Monthly Spending on Food	-$21	-$26	-$6	$7	-$28	$3
	(35)	(47)	(58)	(91)	(41)	(55)
	[0.965]	[0.923]	[0.993]	[0.996]	[0.983]	[0.948]
	{$616}	{$623}	{$605}	{$640}	{$608}	{$597}
Standardized Treatment Effect	0.109	0.024	0.343	0.200	0.077	0.051
	(0.085)	(0.072)	(0.256)	(0.166)	(0.093)	(0.083)
	[0.510]	[0.739]	[0.676]	[0.703]	[0.687]	[0.797]
N	346	206	140	81	265	157

Notes: Data come from the 24-month follow-up survey. Column 1 includes all 24-month follow-up respondents. Each subsequent column uses a different sample of respondents based on listed baseline characteristic(s). Stable housing is defined as living in a dwelling that was owned or rented by the respondent. Unstable housing includes categories such as paying some of the rent, living rent free, homelessness, and other situations that did not qualify as renting or owning. Each set of estimates reports the treatment effect from a regression of the outcome on the treatment indicator, an indicator for cohort, the baseline value of the outcome, length of time between interviews, age, and indicators for month of interview, education, race, marital status, household size, employment status and earnings at baseline. Below the ITT estimates, we report standard errors in parentheses, p-values that control for the family-wise error rate within the domain (Westfall and Young, 1993; Jones, Molitor and Reif, 2019) in brackets, and control group means in braces. The p-value for the Standardized Treatment Effect controls for the family-wise error rate among the six domain indices for that sample. The standardized treatment effect and adjusted p-values include estimates of the following outcomes reported in Appendix Table A-12: monthly utility spending; monthly spending on phone, TV, and internet; monthly amount paid to support others; and monthly spending on fuel.
**, *, $^{+}$ report 0.01, 0.05, and 0.10 significance levels, respectively, using unadjusted p-values.

Table 8: ITT Estimates of the Effect of the Padua Program on Debt and Savings Outcomes, 24-Month Results

Regression-adjusted ITT (Standard error) [MH-adjusted p-value] {Control group mean}

	Full Sample	Subgroups Defined by Baseline Characteristics				
		Not Employed	Employed	Unstably Housed	Stably Housed	Not Empl./ Stbl. Housed
	(1)	(2)	(3)	(4)	(5)	(6)
Has Checkings or Savings Account	-0.009	-0.008	0.028	-0.026	0.036	0.023
	(0.045)	(0.061)	(0.063)	(0.142)	(0.045)	(0.060)
	[0.966]	[0.985]	[0.957]	[0.977]	[0.956]	[0.991]
	{0.683}	{0.628}	{0.763}	{0.644}	{0.694}	{0.667}
Total Assets	$4,821	-$189	$15,662*	$4,057	$3,597	$311
	(3,386)	(778)	(9,288)	(8,045)	(3,324)	(997)
	[0.832]	[1.000]	[0.168]	[1.000]	[0.931]	[0.967]
	{$1,884}	{$1,792}	{$2,017}	{$895}	{$2,181}	{$2,040}
Did Total Assets Increase?	-0.001	-0.018	0.064	-0.167	0.064	0.034
	(0.054)	(0.071)	(0.090)	(0.124)	(0.062)	(0.076)
	[0.984]	[1.000]	[0.922]	[0.825]	[0.931]	[0.998]
	{0.478}	{0.491}	{0.461}	{0.442}	{0.490}	{0.530}
Has a Reitrement Account	0.047	0.055	0.070	-0.042	0.093*	0.125*
	(0.037)	(0.049)	(0.059)	(0.063)	(0.044)	(0.059)
	[0.825]	[0.925]	[0.851]	[0.988]	[0.301]	[0.296]
	{0.132}	{0.105}	{0.171}	{0.152}	{0.125}	{0.095}
Total Amount of Credit Card Debt	-$232	-$641	$568	$87	-$425	-$1,089
	(445)	(692)	(444)	(687)	(557)	(860)
	[0.989]	[0.972]	[0.826]	[0.910]	[0.942]	[0.794]
	{$1,748}	{$2,003}	{$1,360}	{$1,177}	{$1,931}	{$2,481}
Total Debt without Mortgage	$8,782*	$8,367	$2,123	$14,766	$6,307	$8,838
	(4,063)	(5,587)	(5,817)	(12,321)	(3,888)	(6,101)
	[0.284]	[0.760]	[0.920]	[0.896]	[0.599]	[0.744]
	{$26,818}	{$25,060}	{$29,480}	{$30,343}	{$25,693}	{$26,131}
Has Used a Payday Loan in the Past Year	0.015	-0.029	0.098	0.024	0.010	-0.017
	(0.037)	(0.045)	(0.066)	(0.121)	(0.039)	(0.048)
	[0.964]	[0.992]	[0.756]	[0.997]	[0.960]	[0.980]
	{0.128}	{0.124}	{0.133}	{0.152}	{0.120}	{0.108}
Rolled Over Payday Loan	-0.035	-0.056*	0.004	-0.149*	-0.009	-0.039
	(0.026)	(0.027)	(0.051)	(0.071)	(0.031)	(0.030)
	[0.838]	[0.336]	[0.933]	[0.185]	[0.981]	[0.807]
	{0.084}	{0.079}	{0.092}	{0.087}	{0.083}	{0.071}
Standardized Treatment Effect	0.037	0.020	0.166	0.031	0.090^{+}	0.101
	(0.050)	(0.051)	(0.133)	(0.261)	(0.054)	(0.060)
	[0.729]	[0.915]	[0.693]	[0.930]	[0.262]	[0.275]
N	346	206	140	81	265	157

Notes: Data come from the 24-month follow-up survey. Column 1 includes all 24-month follow-up respondents. Each subsequent column uses a different sample of respondents based on listed baseline characteristic(s). Stable housing is defined as living in a dwelling that was owned or rented by the respondent. Unstable housing includes categories such as paying some of the rent, living rent free, homelessness, and other situations that did not qualify as renting or owning. Each set of estimates reports the treatment effect from a regression of the outcome on the treatment indicator, an indicator for cohort, the baseline value of the outcome, length of time between interviews, age, and indicators for month of interview, education, race, marital status, household size, employment status and earnings at baseline. Below the ITT estimates, we report standard errors in parentheses, p-values that control for the family-wise error rate within the domain (Westfall and Young, 1993; Jones, Molitor and Reif, 2019) in brackets, and control group means in braces. The p-value for the Standardized Treatment Effect controls for the family-wise error rate among the six domain indices for that sample. The standardized treatment effect and adjusted p-values include estimates of the following outcomes reported in Appendix Table A-12: has credit card debt; owns stocks, bonds, or mutual funds; and has any debt.

**, *, $^{+}$ report 0.01, 0.05, and 0.10 significance levels, respectively, using unadjusted p-values.

Table 9: ITT Estimates of the Effect of the Padua Program on Health Outcomes, 24-Month Results

Regression-adjusted ITT (Standard error) [MH-adjusted p-value] {Control group mean}

	Full Sample	Subgroups Defined by Baseline Characteristics				
		Not Employed	Employed	Unstably Housed	Stably Housed	Not Empl./ Stbl. Housed
	(1)	(2)	(3)	(4)	(5)	(6)
Self-Rating of Health Improved or Stayed at Excellent	0.147**	0.081	0.245**	0.143	0.155*	0.074
	(0.051)	(0.065)	(0.083)	(0.131)	(0.059)	(0.073)
	[0.030]	[0.743]	[0.020]	[0.795]	[0.061]	[0.901]
	{0.279}	{0.281}	{0.276}	{0.326}	{0.264}	{0.238}
Covered by Medical Insurance	-0.004	0.033	-0.028	-0.052	-0.015	0.020
	(0.051)	(0.069)	(0.081)	(0.149)	(0.059)	(0.079)
	[0.948]	[0.944]	[0.729]	[0.979]	[0.798]	[0.959]
	{0.547}	{0.544}	{0.553}	{0.522}	{0.556}	{0.548}
Visited ER in Past 12 Months	-0.017	0.055	-0.074	0.106	-0.051	0.025
	(0.048)	(0.064)	(0.080)	(0.119)	(0.055)	(0.076)
	[0.921]	[0.906]	[0.709]	[0.857]	[0.724]	[0.981]
	{0.537}	{0.544}	{0.526}	{0.565}	{0.528}	{0.536}
Visited Doctor in Past 12 Months	-0.024	0.040	-0.127+	0.187	-0.078	-0.013
	(0.044)	(0.059)	(0.074)	(0.130)	(0.050)	(0.067)
	[0.918]	[0.931]	[0.344]	[0.638]	[0.463]	[0.844]
	{0.784}	{0.763}	{0.816}	{0.674}	{0.819}	{0.798}
Experienced a Medical Hardship	-0.056	-0.022	-0.097	-0.020	-0.048	-0.028
	(0.045)	(0.060)	(0.066)	(0.111)	(0.052)	(0.070)
	[0.706]	[0.918]	[0.475]	[0.860]	[0.590]	[0.996]
	{0.265}	{0.272}	{0.253}	{0.244}	{0.271}	{0.286}
Standardized Treatment Effect	0.062	0.040	0.080	0.080	0.038	0.023
	(0.044)	(0.054)	(0.074)	(0.088)	(0.053)	(0.065)
	[0.499]	[0.844]	[0.628]	[0.759]	[0.486]	[0.733]
N	346	206	140	81	265	157

Notes: Data come from the 24-month follow-up survey. Column 1 includes all 24-month follow-up respondents. Each subsequent column uses a different sample of respondents based on listed baseline characteristic(s). Stable housing is defined as living in a dwelling that was owned or rented by the respondent. Unstable housing includes categories such as paying some of the rent, living rent free, homelessness, and other situations that did not qualify as renting or owning. Each set of estimates reports the treatment effect from a regression of the outcome on the treatment indicator, an indicator for cohort, the baseline value of the outcome, length of time between interviews, age, and indicators for month of interview, education, race, marital status, household size, employment status and earnings at baseline. Below the ITT estimates, we report standard errors in parentheses, p-values that control for the family-wise error rate within the domain (Westfall and Young, 1993; Jones, Molitor and Reif, 2019) in brackets, and control group means in braces. The p-value for the Standardized Treatment Effect controls for the family-wise error rate among the six domain indices for that sample. The standardized treatment effect and adjusted p-values include estimates of the following outcome reported in Appendix Table A-12: personal views index.
**, *, + report 0.01, 0.05, and 0.10 significance levels, respectively, using unadjusted p-values.

Table 10: ITT Estimates of the Effect of the Padua Program on UI Administrative Data Outcomes, 8 Quarters Following Random Assignment

Regression-adjusted ITT (Standard error) [Unadjusted p-value] {Control group mean}

	Full Sample	Subgroups Defined by Baseline Characteristics				
		Not Employed	Employed	Unstably Housed	Stably Housed	Not Empl./ Stbl. Housed
	(1)	(2)	(3)	(4)	(5)	(6)
Worked in Quarter 8	0.046	0.044	0.051	-0.027	0.059	0.069
	(0.052)	(0.066)	(0.084)	(0.109)	(0.058)	(0.075)
	[0.373]	[0.506]	[0.549]	[0.805]	[0.313]	[0.360]
	{0.606}	{0.577}	{0.656}	{0.553}	{0.625}	{0.573}
Quarter 8 Earnings	$27	$317	-$435	-$1,001	$445	$739
	(384)	(467)	(654)	(773)	(436)	(521)
	[0.944]	[0.498]	[0.506]	[0.195]	[0.307]	[0.156]
	{$3,207}	{$2,765}	{$3,974}	{$3,101}	{$3,246}	{$2,726}
Employment Rate Through Quarter 8	0.049	0.078	0.004	0.052	0.054	0.090
	(0.035)	(0.048)	(0.048)	(0.074)	(0.040)	(0.056)
	[0.168]	[0.106]	[0.941]	[0.485]	[0.177]	[0.110]
	{0.622}	{0.527}	{0.787}	{0.561}	{0.645}	{0.537}
Average Quarterly Earnings Through Quarter 8	$142	$286	-$162	-$243	$277	$521
	(304)	(338)	(547)	(445)	(374)	(412)
	[0.641]	[0.398]	[0.767]	[0.585]	[0.460]	[0.206]
	{$3,032}	{$2,237}	{$4,411}	{$2,435}	{$3,251}	{$2,356}
Employment Rate Through Quarter 18	0.050	0.067	0.021	0.021	0.046	0.086
	(0.034)	(0.045)	(0.052)	(0.075)	(0.039)	(0.052)
	[0.144]	[0.135]	[0.687]	[0.780]	[0.235]	[0.101]
	{0.621}	{0.551}	{0.743}	{0.551}	{0.647}	{0.556}
Average Quarterly Earnings Through Quarter 18	$50	$302	-$475	-$558	$250	$627
	(326)	(399)	(543)	(523)	(389)	(473)
	[0.879]	[0.449]	[0.382]	[0.286]	[0.521]	[0.185]
	{$3,408}	{$2,786}	{$4,486}	{$2,778}	{$3,639}	{$2,870}
N	325	203	122	85	240	150

Notes: Data come from the administrative UI earnings records from the TX Workforce Commission. The sample include study participants who linked to administrative SNAP records (see Appendix G for details). Column 1 includes all linked study participants. Each subsequent column uses a different sample of respondents based on listed baseline characteristic(s). Stable housing is defined as living in a dwelling that was owned or rented by the respondent. Unstable housing includes categories such as paying some of the rent, living rent free, homelessness, and other situations that did not qualify as renting or owning. Each set of estimates reports the treatment effect from a regression of the outcome on the treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). The choice set for control variables includes the controls included in Table 4, as well as 8 pre-randomization quarters of quarterly employment indicators and quarterly earnings. Below the ITT estimates, we report standard errors in parentheses, unadjusted p-values in brackets, and control group means in braces.
**, *, $^{+}$ report 0.01, 0.05, and 0.10 significance levels, respectively, using unadjusted p-values.

Table 11: ITT Estimates of the Effect of the Padua Program on SNAP Administrative Data Outcomes, 24 Months Following Random Assignment

Regression-adjusted ITT (Standard error) [Unadjusted p-value] {Control group mean}

	Full Sample	Subgroups Defined by Baseline Characteristics				
		Not Employed	Employed	Unstably Housed	Stably Housed	Not Empl./ Stbl. Housed
	(1)	(2)	(3)	(4)	(5)	(6)
Received SNAP in Month 24	0.011	-0.029	0.085	0.125^{+}	-0.032	-0.074
	(0.037)	(0.050)	(0.055)	(0.074)	(0.043)	(0.057)
	[0.760]	[0.556]	[0.123]	[0.091]	[0.458]	[0.194]
	{0.363}	{0.439}	{0.253}	{0.281}	{0.394}	{0.469}
Month 24 SNAP Amount	$13	-$5	$41	$79^{+}	-$9	-$31
	(20)	(27)	(29)	(44)	(22)	(29)
	[0.516]	[0.847]	[0.152]	[0.070]	[0.690]	[0.290]
	{$144}	{$174}	{$100}	{$112}	{$156}	{$192}
SNAP Participation Rate Through Month 24	0.014	0.001	0.021	0.089^{+}	-0.006	-0.050
	(0.024)	(0.032)	(0.037)	(0.046)	(0.028)	(0.037)
	[0.568]	[0.977]	[0.563]	[0.053]	[0.824]	[0.167]
	{0.434}	{0.504}	{0.332}	{0.385}	{0.453}	{0.545}
Average SNAP Receipt Through Month 24	$10	$7	$20	$35	$3	-$13
	(15)	(21)	(22)	(34)	(17)	(23)
	[0.502]	[0.751]	[0.370]	[0.301]	[0.877]	[0.573]
	{$181}	{$216}	{$129}	{$163}	{$187}	{$230}
SNAP Participation Rate Through Month 60	0.015	-0.035	0.057	0.061	0.008	-0.060
	(0.024)	(0.033)	(0.036)	(0.051)	(0.028)	(0.037)
	[0.528]	[0.283]	[0.116]	[0.239]	[0.764]	[0.111]
	{0.369}	{0.427}	{0.284}	{0.338}	{0.380}	{0.454}
Average SNAP Receipt Through Month 60	$5	-$7	$24	$19	-$1	-$23
	(14)	(19)	(19)	(35)	(14)	(19)
	[0.729]	[0.723]	[0.206]	[0.596]	[0.931]	[0.234]
	{$152}	{$182}	{$107}	{$142}	{$155}	{$193}
N	427	255	172	110	317	186

Notes: Data come from the administrative SNAP records from the TX Health and Human Services Commission. The sample include all study participants. Column 1 includes all study participants. Each subsequent column uses a different sample of individuals based on listed baseline characteristic(s). Stable housing is defined as living in a dwelling that was owned or rented by the respondent. Unstable housing includes categories such as paying some of the rent, living rent free, homelessness, and other situations that did not qualify as renting or owning. Each set of estimates reports the treatment effect from a regression of the outcome on the treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). The choice set for control variables includes the controls included in Table 4, as well as 24 pre-randomization months of monthly SNAP participation indicators and monthly SNAP receipt. Below the ITT estimates, we report standard errors in parentheses, unadjusted p-values in brackets, and control group means in braces.

**, *, $^{+}$ report 0.01, 0.05, and 0.10 significance levels, respectively, using unadjusted p-values.

Table 12: ITT Estimates of the Effect of the Padua Program on Administrative Credit Outcomes, 8 Quarters Following Random Assignment

	Full Sample	Subgroups Defined by Baseline Characteristics				
		Not Employed	Employed	Unstably Housed	Stably Housed	Not Empl./ Stbl. Housed
	(1)	(2)	(3)	(4)	(5)	(6)
Vantage Score 4.0 Credit Score	-3	-1	-13	-5	-2	3
	(6)	(8)	(10)	(14)	(7)	(9)
	[0.623]	[0.931]	[0.181]	[0.701]	[0.779]	[0.767]
	{540}	{533}	{552}	{539}	{541}	{534}
Has Debt	0.013	-0.028	0.057	-0.020	0.019	-0.028
	(0.053)	(0.070)	(0.079)	(0.114)	(0.060)	(0.081)
	[0.809]	[0.688]	[0.469]	[0.863]	[0.752]	[0.733]
	{0.553}	{0.546}	{0.565}	{0.541}	{0.557}	{0.533}
Total Debt	$1,173	$4,751	-$3,204	-$7,988**	$3,755	$8,378
	(3,339)	(4,369)	(5,012)	(2,983)	(4,168)	(5,280)
	[0.725]	[0.277]	[0.523]	[0.007]	[0.368]	[0.113]
	{$16,275}	{$14,878}	{$18,462}	{$21,534}	{$14,681}	{$12,327}
Has Credit Card Debt	-0.047	-0.063	-0.038	-0.041	-0.060	-0.036
	(0.045)	(0.053)	(0.077)	(0.094)	(0.052)	(0.061)
	[0.288]	[0.227]	[0.627]	[0.662]	[0.247]	[0.556]
	{0.258}	{0.196}	{0.355}	{0.189}	{0.279}	{0.213}
Total Amount of Credit Card Debt	-$404*	-$438+	-$364	-$244	-$452*	-$528+
	(173)	(239)	(239)	(303)	(207)	(304)
	[0.020]	[0.067]	[0.128]	[0.420]	[0.029]	[0.083]
	{$613}	{$592}	{$648}	{$494}	{$650}	{$718}
Total Debt without Mortgage	-$2,227	-$1,155	-$3,944	-$8,370**	-$650	$748
	(1,811)	(2,458)	(2,428)	(3,121)	(2,099)	(2,856)
	[0.219]	[0.638]	[0.104]	[0.007]	[0.757]	[0.793]
	{$13,521}	{$12,993}	{$14,347}	{$17,550}	{$12,299}	{$11,855}
N	286	170	116	64	222	134

Notes: Data come from Experian credit records. The sample include study participants who have a balanced panel of credit reports (see Appendix G for details). Column 1 includes all linked study participants. Each subsequent column uses a different sample of respondents based on listed baseline characteristic(s). Stable housing is defined as living in a dwelling that was owned or rented by the respondent. Unstable housing includes categories such as paying some of the rent, living rent free, homelessness, and other situations that did not qualify as renting or owning. Each set of estimates reports the treatment effect from a regression of the outcome measured 8 quarters after random assignment on the treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). The choice set for control variables includes the controls included in Table 4, as well as 3 pre-randomization quarters of the outcome. Below the ITT estimates, we report standard errors in parentheses, unadjusted p-values in brackets, and control group means in braces.
**, *, $^{+}$ report 0.01, 0.05, and 0.10 significance levels, respectively, using unadjusted p-values.

Table 13: Marginal Value of Public Funds by Sub-Sample and Time Horizon

	MVPF		WTP		Net Cost	
	Estimate	CI	Estimate	CI	Estimate	CI
	(1)	(2)	(3)	(4)	(5)	(6)
Full Sample: N=325						
Experimental Impacts (Years 1–5)	0.176	[-0.201, 0.622]	4,009	[-4,975, 12,865]	22,725	[20,672, 24,805]
Project Effects (10 Years)	0.265	[-0.558, 1.538]	5,908	[-15,202, 26,715]	22,272	[17,363, 27,245]
Project Effects (28 Years)	0.506	[-1.224, 6.834]	10,681	[-40,910, 61,530]	21,125	[8,999, 33,412]
Employed: N=122						
Experimental Impacts (Years 1–5)	-0.076	[-0.618, 0.620]	-1,823	[-17,061, 12,821]	24,073	[20,685, 27,595]
Project Effects (10 Years)	-0.363	[-1.326, 1.408]	-9,359	[-45,235, 25,117]	25,818	[17,836, 34,117]
Project Effects (28 Years)	-0.938	[-2.299, 5.250]	-28,328	[-116,147, 56,067]	30,207	[10,669, 50,520]
Not Employed: N=203						
Experimental Impacts (Years 1–5)	0.331	[-0.155, 0.937]	7,261	[-3,801, 18,167]	21,955	[19,396, 24,547]
Project Effects (10 Years)	0.739	[-0.430, 2.923]	14,852	[-11,333, 40,668]	20,104	[13,909, 26,380]
Project Effects (28 Years)	2.195	[-0.979, 446.572]	33,994	[-30,324, 97,405]	15,484	[212, 30,957]
Stably Housed: N=240						
Experimental Impacts (Years 1–5)	0.298	[-0.159, 0.861]	6,581	[-3,904, 16,949]	22,121	[19,679, 24,586]
Project Effects (10 Years)	0.483	[-0.529, 2.250]	10,253	[-14,368, 34,600]	21,225	[15,371, 27,136]
Project Effects (28 Years)	1.027	[-1.216, 17.058]	19,513	[-40,755, 79,105]	18,991	[4,617, 33,501]
Unstably Housed: N=85						
Experimental Impacts (Years 1–5)	-0.124	[-0.690, 0.640]	-3,017	[-19,373, 13,197]	24,333	[20,632, 28,065]
Project Effects (10 Years)	-0.359	[-1.389, 1.730]	-9,238	[-48,128, 29,315]	25,756	[16,944, 34,644]
Project Effects (28 Years)	-0.847	[-2.345, 9.190]	-24,865	[-120,369, 69,807]	29,364	[7,594, 51,323]
Not Employed & Stably Housed: N=150						
Experimental Impacts (Years 1–5)	0.563	[-0.048, 1.350]	11,759	[-1,157, 24,260]	20,900	[17,968, 23,928]
Project Effects (10 Years)	1.406	[-0.232, 5.163]	24,883	[-5,812, 54,593]	17,700	[10,573, 25,062]
Project Effects (28 Years)	5.978	[-0.629, ∞]	57,959	[-17,542, 131,037]	9,696	[-7,928, 27,898]

Notes: Data source is administrative UI earnings data from the TWC and program data from CCFW. The sample includes all Padua study participants who could be linked to UI wage records through SNAP records (see Appendix G for details). Columns (1), (3), and (5) report estimates of the marginal value of public funds (MVPF), willingness to pay of the program, and per person program cost net of fiscal externalities, respectively. Columns (2), (4), and (6) report 95% confidence intervals that come from 10,000 bootstrap draws based on estimated earnings effects and standard errors. The estimates assume a 3% annual discount rate and a per treatment group member cost of \$19,072, which includes program related costs as well as the difference in assistance received between the treatment group and control group. For each subgroup of the data, we present estimates that vary the time horizon of earnings impacts: only the experimental impacts estimated in years 1 through 5; assuming a constant relative earnings impact through year 10; and assuming a constant relative earnings impact through age 65 (28 years). See Section VIII for additional details.