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Fighting Poverty One Family at a Time: Experimental Evidence from an Intervention with Holistic, Individualized, Wraparound Services†

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Families in poverty face numerous barriers to establishing stable economic footing. This paper estimates experimental effects of a holistic, individualized wraparound service intervention 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 strategic financial assistance. The program improved employment and housing outcomes two years postenrollment. Given the customized nature of the services, overall program effects mask heterogeneity. Exploratory analysis suggests that the program helped employ participants who lacked employment but had stable housing and that those without stable housing were helped in securing it. (*JEL* I32, I38, L31, R23)

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, and 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. A recent study of the low-income unemployed showed that

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a substantial fraction experienced significant barriers to work such as difficulty finding childcare (9 percent), being unable to drive (22 percent), limiting health conditions (11 percent), having a felony conviction (22 percent), as well as other barriers (Barham, Cadena, and Turner 2023). At least two of these barriers were faced by 35 percent. Data for other vulnerable populations, such as those receiving welfare, also show that many face multiple barriers. In a survey of long-term welfare recipients, Taylor and Barusch (2004) found that 57 percent had 2 to 4 barriers to work and 23 percent had 5 to 8 (also see Loprest and Zedlewski (2006)). A successful policy response to poverty, therefore, may need to attack many fronts at once.

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. This holistic, individualized, wraparound intervention requires substantial time and resources. Initially, the individual completes a detailed assessment that identifies the individual's strengths, goals, and obstacles faced. 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 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

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

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 2023). 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, nonemployment 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, or 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 (RCT) 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 effects are not statistically significant after adjusting inference for multiple hypothesis testing. 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 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 results are consistent with the individualized service approach of Padua. Exploratory analyses indicate a larger impact on labor market outcomes for the subgroup of individuals who enter Padua without employment and for the subgroup of

individuals with stable housing (defined as owning or renting). Treatment group members who were not working at baseline experienced an increase in the likelihood of full-time employment (67 percent), the likelihood of any employment (26 percent), and monthly earnings (46 percent) 2 years after enrollment. Additional exploratory evidence suggests the program increased participation in postsecondary education for this subgroup. Individuals with stable housing at program entry also experienced labor market gains, a 34 percent increase in earnings and a 37 percent increase in full-time employment. Moreover, our exploratory analysis suggests that the subgroup of stably housed individuals without employment at baseline experienced the largest labor market gains among many potential subgroups one might consider. Results are different, however, among individuals who entered the program in more precarious situations. 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. While employment and earnings did not improve among the subgroup of individuals with unstable housing at baseline, this subgroup experienced a 64 percent increase in the likelihood of having stable housing relative to the subgroup control mean of 0.54.

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 US 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 (MVPF) for the typical participant to be around one half, 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.

I. The Intervention

A. *How Padua Was Designed*

Padua is a holistic, individualized, wraparound service program designed by CCFW, a large urban service provider serving more than 50,000 clients annually as of 2019.[2] Padua targets working-age adults capable of achieving self-sufficiency with assistance, excluding those with severe mental illness, substance abuse issues, or disabilities that hinder work or require permanent public assistance. Although primarily focused on the individual client, family members may be eligible for some services from CCFW.

Clients work toward key goals, which CCFW terms "out-of-poverty benchmarks." These benchmarks include: (i) achieving a living wage, (ii) reducing reliance on transfer programs, (iii) decreasing debt, and (iv) increasing personal savings. Despite these common goals, the program tailors short- and medium-term goals to each client's unique situation.

[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."

Padua has five key features:

(*i*) *Detailed Assessment*.—Case managers conduct an extensive assessment within 45 days of enrollment using a standardized tool. The assessment is designed to uncover a participant's goals, skills, and barriers to success 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.[3] 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.

(*ii*) *Service Plan*.—Case management teams work collaboratively with clients to develop a plan for obtaining self-sufficiency. Padua is a "strengths-based" intervention that emphasizes the client's role in determining their own path. The planning process is driven by the client's strengths and preferences and is meant to produce a service plan in which the client is highly invested.

As a part of their service plan, clients work with their case management teams to establish goals that help them make progress toward their benchmarks. 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 childcare that would allow them to work more hours. Goals are broken down into actionable steps that can be taken weekly, with frequent revisions to adapt to evolving circumstances. Clients are 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.

(*iii*) *Case Management Teams and Small Caseloads*.—Two-person teams, comprising a case manager and a caseworker, support a small caseload of 18–24 clients. Case managers are required to have a master's degree, while caseworkers must have a bachelor's degree. Both work in tandem and share the same set of clients. A program manager oversees the intervention and provides support to all case management teams. Each team receives extensive training not only when they are hired but throughout their tenure at CCFW. Continuity is an important feature of the intervention, as the relationship between the client and case management team is critical in establishing trust.

[3] 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: (1) In Crisis; (2) Vulnerable; (3) Stable; (4) Safe; and (5) Thriving. See Supplemental Appendix B for an example of the scoring tool for the self-sufficiency matrix.

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

(*iv*) *Case Management*.—Biweekly client meetings with the case management team include personal coaching and mentoring, provision of CCFW services, referrals to a broad network of external services, and regular evaluation of progress. A two-stage process prioritizes stabilization before addressing skills or employment. 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 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. A key component of case management is preparing clients both mentally and emotionally for the tasks ahead.

Regular engagement in problem-solving, goal setting, and actively reevaluating and comparing multiple priorities and goals is designed to improve executive function, confidence, and decision-making abilities. 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?

Clients receive additional employment, education, transportation, and housing resources provided by CCFW. Employment specialists help clients connect with specific jobs or provide mock interviewing and resume-writing assistance. Education specialists help some 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 work in tandem to help address barriers to ensure clients can get to interviews and jobs on time.

(*v*) *Flexible Financial Resources*.—Financial assistance is available on a case-by-case basis to help clients overcome obstacles. Case managers have the flexibility to tailor support toward challenges like car repairs, securing a new apartment, or childcare expenses, reinforcing client commitment to their path out of poverty. These resources are often used as incentives to encourage client progress, such as matching savings or covering security deposits.

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

[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).

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 return to his previous occupation. His case management team 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 overtime 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 through 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 Supplemental 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.

II. 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 reentering society, to improve neighborhood choice among housing voucher holders, to increase persistence and graduation among community college students, and to improve general economic well-being. We summarize these interventions in Supplemental Appendix D, limiting our focus to those that have been evaluated using an RCT.[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 the 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. 2023). 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. 2023), financial assistance (Evans et al. 2020; Weiss et al. 2019), or subsidized employment (Barden et al. 2018; Barham, Cadena, and Turner 2023)—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 that case management might aid in removing barriers and increasing a client's neighborhood choice set (Bergman et al. 2023). Complementing this finding, evidence from Stay the Course, a community college case management intervention, suggests that assistance alone is 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 to living-wage employment and were evaluated by an RCT.[7] In this

[6] In addition to these areas, 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 ongoing RCT evaluations of programs similar to Padua—Bridges to Success (Espinosa, Evans, and Phillips 2020) in Rochester, NY, and AMP Up Boston Study of EMPath (Engle, Katz, and Tebes 2021)—both programs that use mentoring to improve economic mobility.

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. 2023), or Stay the Course and ASAP, which focus on associate degree completion (Evans et al. 2020; Weiss et al. 2019). Supplemental Appendix E documents program characteristics and experimental impacts of these four programs to facilitate a comparison with Padua.

The first two programs were antipoverty programs targeted to low-income workers. Building Nebraska Families (BNF) (Meckstroth et al., 2008) was a welfare-to-work program that provided individualized education, life skills and service coordination to hard-to-employ Temporary Assistance for Needy Families (TANF) recipients. The program was home based with small caseloads of 12 to 18 clients per caseworker. Clients met their caseworkers 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 has 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 noncustodial parents with an outstanding child support payment.

III. The Experimental Evaluation

A. *Study Design and Sample*

We implemented an 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;
- total family income is not sufficient to meet needs;[9]
- individual resides in Tarrant County, Texas; 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 consented to study participation and were 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 Supplemental 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.

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

[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.

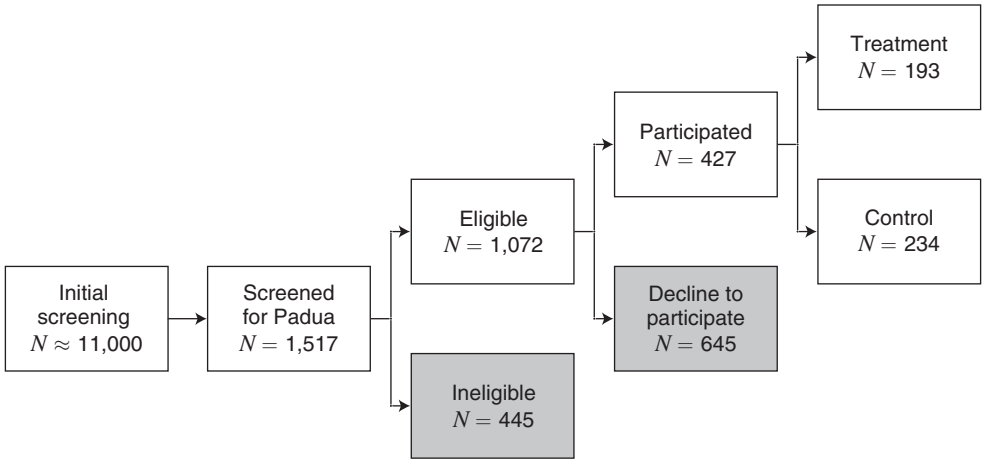


FIGURE 1. RECRUITMENT FOR PADUA

Notes: Data on program recruitment and intake come from CCFW. See text for details on program eligibility and the randomization protocol.

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 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

[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.

[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 Supplemental 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

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 unemployment insurance (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 five-year American Community Survey (Ruggles et al. 2023) 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 ($\Pr(> F) = 0.732$). We find comparable evidence of balance for the full baseline sample that does not condition on responding to the follow-up survey (Supplemental Appendix Table A-1) and among the subset of study participants linked to administrative data (Supplemental Appendix Tables G-2 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]

staff have extensive experience with Computer-Assisted Personal Interviewing (CAPI), paricularly among hard-to-locate populations, drawing from a team of over 30–60 interviewers.

[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 spouses, and finally the oldest respondents.

[17] Supplemental 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 V 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. Supplemental Appendix Table A-3 shows estimates from regressions of an indicator of nonresponse 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 Supplemental Nutrition Assistance Program (SNAP), and 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 of differential selection between the treatment and control groups among the set of individual baseline characteristics ($\Pr(> F) = 0.627$).

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)	United States (7)
Less than high school education	0.274	0.321	0.047	0.345	0.236	0.238	0.162
High school degree or GED	0.284	0.199	−0.085	0.063	0.295	0.281	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	0.846	−0.001	0.975	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	$731	$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	0.063	0.077	0.014	0.620			
Has stable housing	0.774	0.782	0.008	0.853			
Util. disconnected / notice of disconnect, past year	0.571	0.635	0.063	0.233			
Percentage of poverty line	61.7%	62.4%	0.7%	0.910	93.8%	88.8%	83.1%
Single mother	0.563	0.564	0.001	0.986	0.250	0.230	0.216
Observations	190	156			6,663	105,843	1,292,265
$\Pr(> F)$				0.745			

Notes: Data are from baseline surveys for all participants who responded to the 24-month follow-up survey. ACS data are downloaded from IPUMS (Ruggles et al. 2023) and include households that have heads between the ages of 18 and 55, have at least one adult worker, and have a household income below 180 percent FPL.

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] Supplemental 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

[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.

Table 2—Services Provided by CCFW within 24 Months of Application

	Treatment	Control
Panel A. Service receipt by treatment assignment		
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		
	Mean	25th	50th	75th
Panel B. Average service receipt among Padua clients:				
Total in-kind assistance	$379	$0	$102	$477
Total hours of case management time	54.7	24	47	77
Total hours of 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 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. Roughly 28 percent of callers received assistance, and the average amount given was $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.

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 United States, our sample is less likely to have graduated high school, is older, is 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. 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.

B. *Program Take-Up and Timing*

Random assignment generated differences in treatment intensity during the two years following enrollment. Of the treatment group, 91 percent had at least 1 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

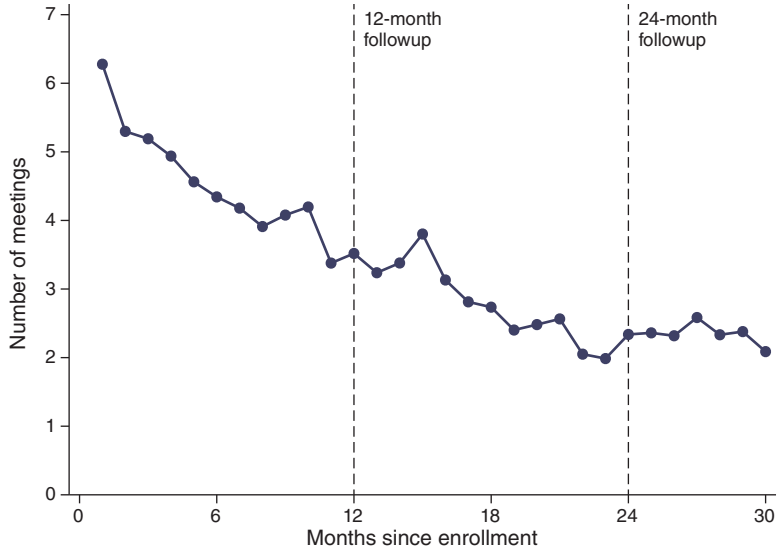


FIGURE 2. NUMBER OF IN-PERSON OR PHONE MEETINGS, BY MONTHS SINCE ENROLLMENT FOR ACTIVE PADUA CLIENTS

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; or client moved out of Tarrant County and became ineligible.

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 most intensely with their case management team during the first few months of the program: on average, six times in the first month, about four times by the sixth month, and still nearly two to three 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.

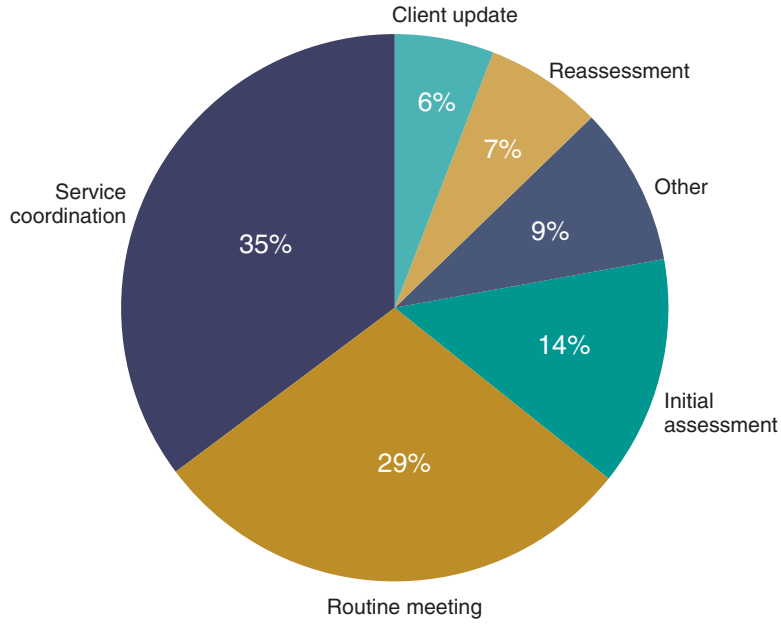


Figure 3. Share of Case Management Time Spent on Various Activities, First 24 Months

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.

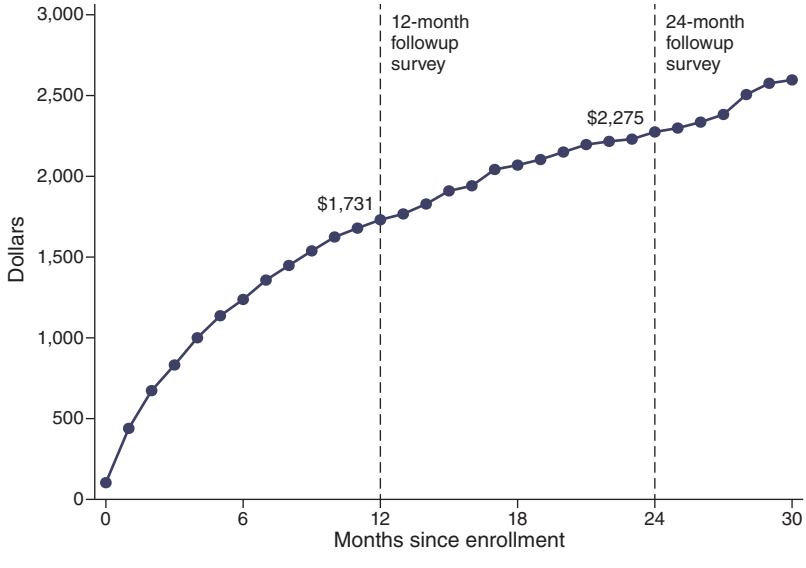


Figure 4. Average Cumulative Cash Assistance Received by Padua Clients, by Months Since Enrollment

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.

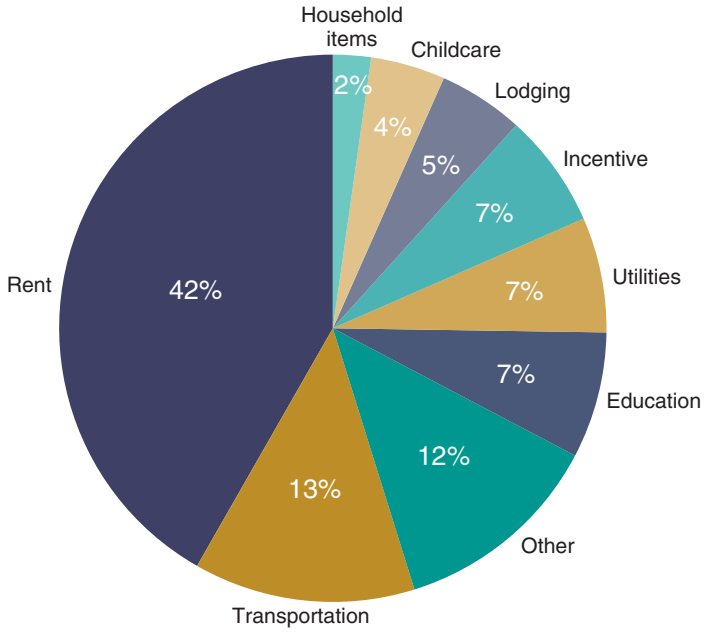


FIGURE 5. USE OF CASH ASSISTANCE, FIRST 24 MONTHS

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.

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).

IV. 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

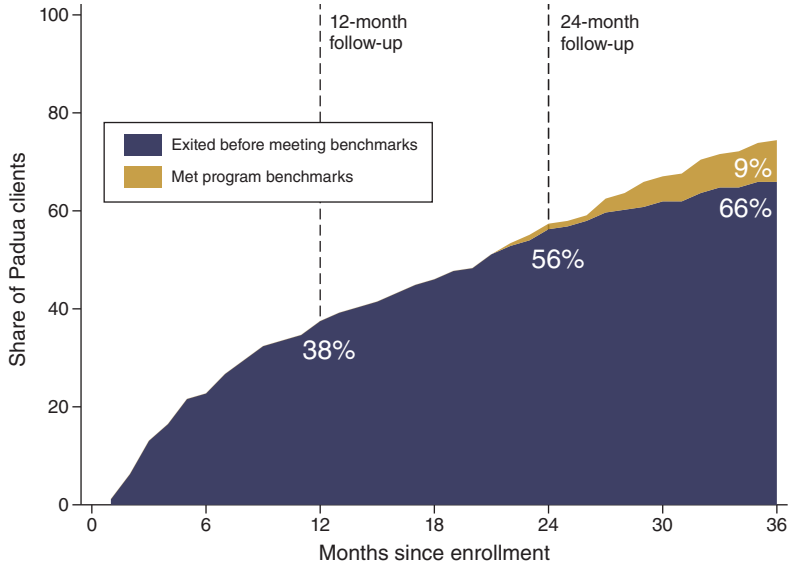


FIGURE 6. SHARE OF PADUA CLIENTS THAT EXITED PADUA, BY MONTHS SINCE ENROLLMENT

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 that exited Padua prior to meeting program benchmarks (navy) and the share that 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; or 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 that are still active in Padua. For example, roughly 43 percent of Padua clients remained active 24 months after program enrollment.

characteristics, including these controls is primarily to reduce residual variance and improve precision. The standard regression model we estimate is of the form

$$y_{ij} = \beta_0 + T_i\beta_1 + \mathbf{x}_i'\boldsymbol{\beta}_2 + y_{i0}\beta_3 + \epsilon_i, \tag{1}$$

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 0 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

[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 respondents, 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.

the outcome measured at baseline, indicated by the variable y_{i0}.[21] When estimating effects using administrative data that have multiple prerandomization measures of the outcomes (e.g., eight quarters of employment indicators), we use postdouble 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.

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, spending, and health. Specifically, we estimate

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

[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 was mentioned at early stages of the study in Supplemental Appendix Tables A-15 and A-16.

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 the Supplemental Nutrition Assistance Program (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 Supplemental Appendix G.

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 Supplemental 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.[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.

[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.

TABLE 3—EFFECT OF PADUA ON OUTCOME DOMAINS

	Domains					
	Labor (1)	Housing (2)	Support (3)	Spending (4)	Debt (5)	Health (6)
Panel A. 12-month survey ($N = 351$)						
Standardized treatment effect	0.105	0.059	0.028	−0.074	0.051	0.006
	(0.071)	(0.048)	(0.051)	(0.045)	(0.037)	(0.042)
	[0.132]	[0.218]	[0.618]	[0.095]	[0.175]	[0.897]
	{0.499}	{0.523}	{0.854}	{0.437}	{0.530}	{0.897}
Panel B. 24-month survey ($N = 346$)						
Standardized treatment effect	0.149	0.100	−0.017	0.023	0.037	0.061
	(0.076)	(0.047)	(0.054)	(0.056)	(0.050)	(0.044)
	[0.048]	[0.036]	[0.785]	[0.684]	[0.493]	[0.161]
	{0.219}	{0.194}	{0.785}	{0.900}	{0.869}	{0.506}

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 rescaled 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.

For 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.

V. Survey Results

To summarize our results for the many outcomes collected in 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).[26]

We find suggestive evidence that Padua improved clients' labor market and housing outcomes. 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

[26] To construct domain indices, we include all of the outcomes reported in the domain-specific tables discussed below, as well as additional outcomes we examined within each domain as noted in the respective table notes. For each treatment effect estimate, we report standard errors in parentheses, a per comparison *p*-value in brackets, and an adjusted *p*-value in braces that controls for the family-wise error rate following the Westfall-Young step-down procedure (Westfall and Young 1993).

were 0.105 standard deviations greater for those in the treatment group. This standardized treatment effect persisted through two years postenrollment, where we see a positive effect of 0.149 standard deviations. We also find some suggestive evidence of a positive effect of the intervention on housing outcomes, which improved by 0.059 standard deviations at 12 months and by 0.100 standard deviations at 24 months. While the 24-month effects on labor and housing outcomes are statistically significant at the 5 percent level, if we adjust for the family-wise error rate across all 6 domains within the survey wave, these estimates are no longer statistically significant.

We find little evidence of an overall effect of Padua on the other domains—support, spending, debt, and health—at either 12 or 24 months. There is some evidence that Padua clients were spending less at the time of the 12-month survey, though the effect does not persist to the second year. Estimates for effects on the other domains are mostly positive, but small in magnitude. None are statistically significant. In fact, none of the standardized treatment effects across the six domains at 12 or 24 months are statistically significant after accounting for the six domains examined in this study.

As we discuss below, Padua is an individualized intervention with sometimes competing goals for different groups. Null effects for the full sample could potentially mask program impacts for subgroups.

A. *Labor Market Outcomes*

Given Padua's focus on employment, we next 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 Supplemental Appendix Table A-9. The first six outcomes are a dummy for whether the survey respondent is currently employed; the respondent's monthly earnings (zero for nonworkers); 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 United States. The average standardized treatment effect reported in the final row is the same estimate that was reported in Table 3. For each outcome, we report the ITT estimate from equation (1) and its standard error, the adjusted p-value on the null hypothesis that the coefficient is zero, and the control group mean. We report some additional labor market outcomes in Supplemental Appendix Table A-15.[27]

The signs of the estimated effects for the full study sample (column 1) indicate improved labor market outcomes (e.g., increased labor market participation, increased labor supply, and higher earnings), although none of the estimates are

[27] For all individual outcomes, the adjusted p-values control for the family-wise error among the outcomes included within the domain 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.

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

		Subgroups defined by baseline characteristics				
	Full sample (1)	*Not employed* (2)	*Employed* (3)	*Unstably housed* (4)	*Stably housed* (5)	*Not empl./ stbl. housed* (6)
Currently employed	0.061	0.131	−0.049	−0.033	0.087	0.155
	(0.050)	(0.069)	(0.072)	(0.130)	(0.054)	(0.074)
	[0.455]	[0.191]	[0.926]	[0.957]	[0.296]	[0.132]
	{0.626}	{0.500}	{0.816}	{0.558}	{0.646}	{0.517}
Respondent monthly earnings	$200	$421	−$13	−$394	$378	$700
	(133)	(196)	(185)	(380)	(136)	(187)
	[0.353]	[0.132]	[0.944]	[0.700]	[0.032]	[0.003]
	{$1,149}	{$918}	{$1,489}	{$1,232}	{$1,125}	{$867}
Employed full-time	0.106	0.193	−0.020	−0.156	0.163	0.264
	(0.052)	(0.067)	(0.091)	(0.121)	(0.059)	(0.070)
	[0.199]	[0.024]	[0.963]	[0.528]	[0.034]	[0.003]
	{0.426}	{0.289}	{0.632}	{0.395}	{0.435}	{0.287}
Hours worked per week	3.88	6.86	−0.88	−2.37	5.01	8.53
	(2.26)	(2.96)	(3.56)	(6.57)	(2.40)	(3.11)
	[0.270]	[0.105]	[0.985]	[0.974]	[0.136]	[0.027]
	{23.29}	{17.69}	{31.70}	{21.07}	{23.95}	{18.06}
Percentage of poverty line	0.10	0.11	0.10	−0.02	0.20	0.26
	(0.10)	(0.14)	(0.16)	(0.35)	(0.11)	(0.13)
	[0.353]	[0.667]	[0.935]	[0.962]	[0.222]	[0.101]
	{1.14}	{1.10}	{1.20}	{1.18}	{1.13}	{1.06}
Can legally work in US	0.033	0.001	0.073	0.060	0.031	−0.009
	(0.017)	(0.021)	(0.032)	(0.042)	(0.020)	(0.022)
	[0.206]	[0.957]	[0.089]	[0.097]	[0.222]	[0.724]
	{0.856}	{0.893}	{0.803}	{0.907}	{0.841}	{0.871}
Standardized treatment effect	0.149	0.242	0.021	−0.069	0.236	0.363
	(0.076)	(0.103)	(0.133)	(0.196)	(0.083)	(0.109)
	[0.219]	[0.131]	[0.875]	[0.925]	[0.031]	[0.008]
Observations	346	206	140	77	269	161

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 Supplemental Appendix Table A-15: hours worked in primary job and total household income (including benefits).

statistically significant after adjusting for multiple hypothesis testing.[28] Those in the treatment group were 6.1 percentage points (9.7 percent) more likely to

[28] We do not report unadjusted *p*-values, but a traditional test statistic can be constructed by dividing the point estimate by the standard error. Full-time employment and legal work status are statistically significant at the 5 and 10 percent levels, respectively, when considering unadjusted *p*-values.

be working 24 months after application. The fraction working full-time increased by 10.6 percentage points, which is a 25 percent increase over the control group mean.[29] The increase in the likelihood of working is associated with an increase in monthly earnings of $200 (17 percent). 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.[30] Taking this estimate at face value, the increase in family income is somewhat smaller than the increase in respondent earnings.[31]

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 $= 0.010$), 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 $= 0.149$). The increase in college attendance is relatively large, a 11.4 percent increase compared to the control group mean of 47.4 percent. On one hand, these changes are consistent with either the control group being more likely to access other services that encouraged GED completion or that Padua caseworkers 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 3.3 percentage points more likely to be legally allowed to work in the US. This result suggests that one mechanism by which Padua leads to increased work is by addressing legal barriers.[32]

As noted above in our discussion of Table 3, the standardized treatment effect for the labor market domain reported in the final row is positive and has an unadjusted p-value of 0.048, but a p-value of 0.218 when adjusting for the family-wise error rate across all six domains within the survey wave.

[29] 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 to our primary measure.

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

[31] 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.

[32] 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.

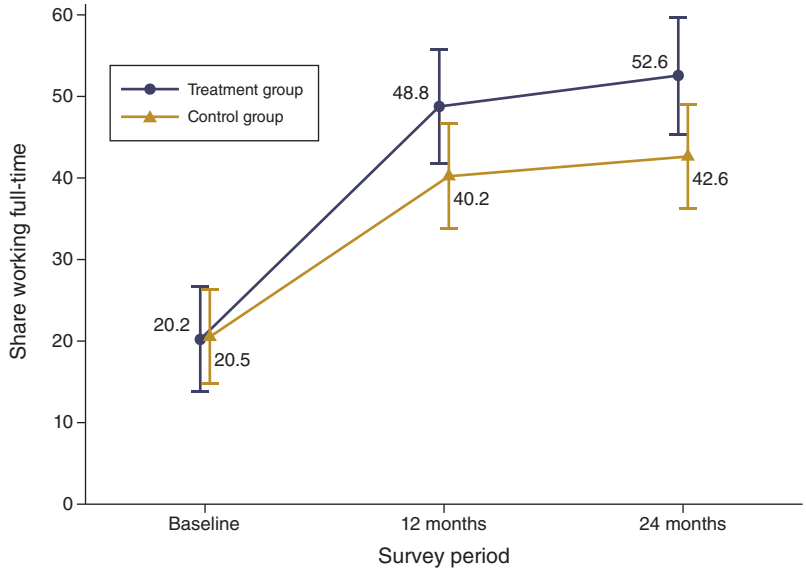


FIGURE 7. PERCENT WORKING FULL-TIME BY TREATMENT STATUS OVER TIME

Notes: Data are from baseline and follow-up surveys. The estimates reflect the share of participants that were 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.

The positive impact of Padua on employment occurred 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).[33] 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 were 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 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

[33] 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 support 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.

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 completion of a community college degree.

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 toward 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 more or less time 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 select 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 benefitingbenefiting from the program.[34]

[34] 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 the success of the intervention. Prior to the launch of the study, we did not specify examining these subsamples. 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 IVB.

An important component of Padua is helping clients find a pathway to stable employment. A minority 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 compared to those who were not. On one hand, if Padua is particularly effective at helping clients secure employment, then the program may have a 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 row 4 for each outcome), those that enter the experiment unemployed are less likely to be employed (50 versus 82 percent) at the 24-month follow-up, have lower monthly earnings ($918 versus $1,489), and are half as likely to be employed full-time (29 versus 63 percent). Thus, there is more room for Padua to have an impact among those not employed at baseline. Indeed, that is what we find. Among those not employed at baseline, the treatment group is 13.1 percentage points (26 percent) more likely to be employed, earns 46 percent more each month, is 67 percent more likely to be employed full-time, and works 39 percent more hours per week. Only the effect on full-time employment is statistically significant at the 5 percent level after adjusting for multiple hypothesis testing. The standardized treatment effect for this group is 0.24 with a p-value of 0.13 after controlling for the family-wise error rate among the six domain indices for this sample. Additionally, we find that this group is 9.6 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 adjusted p-values are in excess of 0.9). We also find no evidence of improved education among this group (not reported). These results suggest that 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 who 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 nonleased situation.[35] The results show stark differences in labor market outcomes by baseline housing status. 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 13.4 percent, earnings are up 34 percent, and full-time work is up 37 percent, with the effects on earnings and full-time employment significant at the 5 percent level when adjusting for multiple hypothesis testing. 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).[36] Our results suggest that Padua has a large, positive effect on labor market outcomes for this group. For this group, the intervention led to an increase in labor market outcomes of 0.36 standard deviations, with a p-value of 0.008 after adjusting for the family-wise error rate across all six domains. For five of the six reported outcomes in this domain, the treatment effect is positive and statistically significant or close to significance, with legal work status being the exception.

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 intergenerational poverty, housing stability to measure degree of crisis at baseline, employment, gender, age, level of education, presence of children, and language, as well as groups determined by high or low values of the six domain indices at baseline. The horizontal bars represent the 95 percent 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 IVB 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

[35] 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 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. Of those categorized as unstably housed 27 percent 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 (9 percentage points) to be living in a temporary or unaffordable housing situation.

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

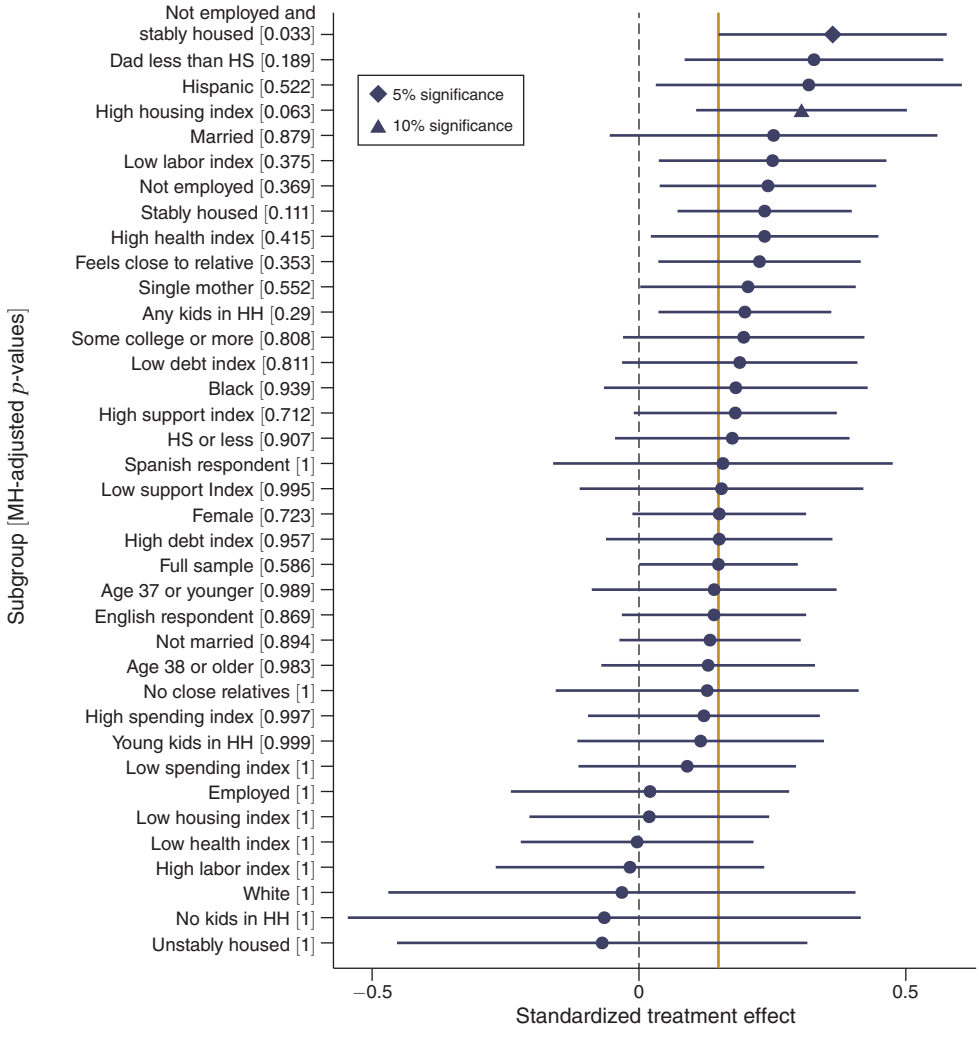


FIGURE 8. EFFECT OF PADUA ON LABOR MARKET OUTCOMES, BY SUBGROUPS

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. HH is household. The horizontal bars represent the 95 percent 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 IVB for details). Statistical significance based on these adjusted *p*-values are represented by diamond (5 percent significance) and triangle (10 percent significance) markers. The gold vertical line shows the standardized treatment effect for the full sample. See Table 4 for the list of outcomes that comprises the labor domain.

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 0.033, which indicates that only 3.3 percent of the 10,000 placebo replications generated at least 1 subgroup with an unadjusted *p*-value smaller than the actual unadjusted *p*-value for this group. This result suggests that the large effect for this group is not the artifact of making multiple comparisons across subgroups.

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 table that reports effects on housing outcomes (Table 5) that has the same structure as Table 4. For the remaining outcome domains, we summarize average standardized treatment effects in Table 6 and report effects on the underlying outcomes in the Supplemental Appendix.

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.[37] The structure of the table is identical to that of Table 4 in that we report results for the full sample in column 1, 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 Supplemental Appendix Table A-10.

For the full sample, none of the treatment effects for specific outcomes are statistically significant, but the signs of these estimates all point toward improved housing outcomes, and the standardized treatment effect suggests improvement. Padua clients are 6.9 percentage points (9 percent) more likely to be stably housed at the 24-month follow-up, although this estimate is not statistically significant.[38] 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

[37] 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 nonresponse, 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."

[38] We also examined homelessness as an outcome (see Supplemental Appendix Table A-15). 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.

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

		Subgroups defined by baseline characteristics				
	Full sample (1)	*Not employed* (2)	*Employed* (3)	*Unstably housed* (4)	*Stably housed* (5)	*Not empl./ stbl. housed* (6)
Owns or rents	0.069	0.093	0.035	0.343	0.003	0.067
	(0.042)	(0.055)	(0.070)	(0.114)	(0.043)	(0.057)
	[0.439]	[0.350]	[0.940]	[0.027]	[0.942]	[0.661]
	{0.763}	{0.746}	{0.789}	{0.535}	{0.830}	{0.793}
Lives in public housing	−0.033	−0.079	−0.013	−0.025	−0.036	−0.073
	(0.028)	(0.039)	(0.044)	(0.113)	(0.028)	(0.037)
	[0.682]	[0.238]	[0.950]	[0.840]	[0.597]	[0.249]
	{0.100}	{0.121}	{0.068}	{0.132}	{0.092}	{0.098}
Utilities disconnected/received notice of disconnect in past year	−0.005	−0.002	−0.015	0.235	−0.085	−0.075
	(0.051)	(0.067)	(0.081)	(0.124)	(0.051)	(0.065)
	[0.918]	[0.968]	[0.848]	[0.292]	[0.449]	[0.569]
	{0.466}	{0.451}	{0.487}	{0.238}	{0.531}	{0.540}
Any neighborhood problems (medium or worse)	−0.054	−0.005	−0.160	−0.037	−0.072	−0.011
	(0.053)	(0.070)	(0.078)	(0.142)	(0.058)	(0.075)
	[0.655]	[0.997]	[0.248]	[0.960]	[0.527]	[0.878]
	{0.437}	{0.421}	{0.461}	{0.465}	{0.429}	{0.437}
Two or more neighborhood problems (medium or worse)	−0.069	−0.075	−0.053	−0.136	−0.064	−0.089
	(0.045)	(0.060)	(0.073)	(0.115)	(0.049)	(0.065)
	[0.475]	[0.583]	[0.919]	[0.505]	[0.630]	[0.586]
	{0.305}	{0.316}	{0.289}	{0.372}	{0.286}	{0.333}
Standardized treatment effect	0.100	0.078	0.130	0.151	0.102	0.118
	(0.047)	(0.070)	(0.070)	(0.100)	(0.052)	(0.070)
	[0.194]	[0.765]	[0.343]	[0.527]	[0.179]	[0.305]
Observations	346	206	140	77	269	161

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 Supplemental Appendix Table A-15: an indicator for currently homeless.

column 1 indicates that Padua leads to 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 6 domain indices yields a *p*-value of 0.194. 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 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. 2023).

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

		Subgroups defined by baseline characteristics				
	Full sample (1)	*Not employed* (2)	*Employed* (3)	*Unstably housed* (4)	*Stably housed* (5)	*Not empl./ stbl. housed* (6)
Panel A. 24-month survey outcomes						
Support index	−0.017	0.047	−0.119	−0.139	0.094	0.127
	(0.054)	(0.054)	(0.125)	(0.076)	(0.039)	(0.050)
	[0.785]	[0.869]	[0.696]	[0.431]	[0.051]	[0.030]
Spending index	0.023	0.025	0.075	0.153	0.010	0.051
	(0.056)	(0.072)	(0.106)	(0.136)	(0.061)	(0.081)
	[0.900]	[0.727]	[0.761]	[0.741]	[0.867]	[0.785]
Debt index	0.037	0.020	0.168	0.039	0.086	0.092
	(0.050)	(0.051)	(0.133)	(0.283)	(0.054)	(0.059)
	[0.869]	[0.913]	[0.762]	[0.913]	[0.312]	[0.342]
Health index	0.061	0.040	0.082	0.066	0.047	0.036
	(0.044)	(0.054)	(0.074)	(0.092)	(0.053)	(0.064)
	[0.506]	[0.826]	[0.720]	[0.868]	[0.608]	[0.573]
Observations	346	206	140	77	269	161
Panel B. Selected administrative UI outcomes						
Employment rate	0.050	0.067	0.021	0.021	0.046	0.086
through quarter 18	(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	$50	$302	−$475	−$558	$250	$627
through quarter 18	(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}
Observations	325	203	122	85	240	150
Panel C. Selected administrative SNAP outcomes						
SNAP participation rate	0.016	−0.031	0.054	0.048	0.010	−0.049
through month 59	(0.024)	(0.033)	(0.035)	(0.051)	(0.027)	(0.037)
	[0.497]	[0.338]	[0.129]	[0.342]	[0.715]	[0.183]
	{0.370}	{0.428}	{0.285}	{0.338}	{0.382}	{0.456}
Observations	427	255	172	110	317	186
Panel D. Administrative credit outcomes						
Credit index (quarter 8)	0.057	0.048	0.042	0.130	0.037	−0.026
	(0.050)	(0.062)	(0.083)	(0.111)	(0.059)	(0.073)
	[0.275]	[0.457]	[0.636]	[0.287]	[0.541]	[0.726]
Observations	286	170	116	61	225	137

Notes: Panel A reports standardized treatment effects on four outcome domains for the sample of all 24-month follow-up respondents. Panel B reports effects on average employment and earnings over the 18 quarters following random assignment for the sample of individuals who were able to be linked to the HHSC data. Panel C reports effects on average monthly SNAP receipt over the 59 months following random assignment for the sample of all study participants. Panel D reports the standardized treatment effect on credit outcomes measured 8 quarters after random assignment for the sample of study participants who have a balanced panel of credit reports. Column 1 includes the full sample. Each subsequent column uses a different sample 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(s) 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 in brackets, and control group means in braces. The *p*-values reported in panel A control for the family-wise error rate among the six domain indices for that sample (Westfall and Young 1993; Jones, Molitor, and Reif 2019).

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 (adjusted p-value $= 0.027$), 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, which 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 reestimate the standardized treatment effect for this group, excluding the outcome for utilities being disconnected, the estimate is 0.29 standard deviations with an unadjusted p-value of 0.015. Treatment effect estimates for all the other subgroups we considered are reported in Supplemental Appendix Figure A-2. The treatment effect is positive for all but one of the subgroups, and in many cases, the estimates are significant or marginally significant using unadjusted p-values. 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.

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 the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). In practice, however, the intervention did not pursue this goal for all participants. As discussed above, for families facing immediate crises, 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.

Padua serves a population that participates in many government transfer programs. At 24 months, 62 percent of control group participants are receiving some form of government transfers, the largest program being SNAP with a 51 percent participation rate for the control group (see Supplemental Appendix Table A-5). Conversely, TANF participation is virtually nonexistent in this population: less than 2 percent receive this form of cash assistance.[39]

[39] This low share is representative of the study setting. Among a set of likely eligible households in the ACS (Table 1), only 2 percent of households in Tarrant County or Texas report receiving welfare income in the past 12 months.

For the full sample, there is little evidence to indicate that Padua reduces government program participation. Table 6 reports the standardized treatment effects on the reliance of government support, where a positive standardized treatment effect denotes a reduction in the use of benefits. The estimated standardized treatment effect is small, negative, and not statistically significant. Across subgroups, only the effects for the stably housed (0.09 standard deviations, adjusted p-value = 0.051) and the not employed but stably housed (0.13 standard deviations, adjusted p-value 0.030) subgroups are statistically significant.

In Supplemental Appendix Table A-5, we examine the effect of Padua on the use of specific government transfer programs. Similar results at the 12-month follow-up are reported in Supplemental Appendix Table A-11. For the full sample, 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.[40] 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.[41]

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 summarizing the treatment effects for a number of spending-related outcomes including monthly rent expenditure, monthly spending on childcare, the use of a budget, total nonhousing spending, and spending on food in a single index reported in Table 6. In the full sample and across subgroups defined by baseline employment or housing situations, we see positive but small and statistically insignificant effects on spending.

Supplemental Appendix Table A-6 reports effects on underlying domain outcomes from the 24-month follow-up survey. Similar results from the 12-month follow-up are reported in Supplemental Appendix Table A-12. While overall spending was largely unaffected, these results indicate that Padua had a sizable effect on budgeting behavior for the full sample. It increased the likelihood that participants were using a budget 24 months after enrollment by 14 percentage points (24 percent, adjusted p-value $= 0.024$). This effect is very large for the groups that were more disadvantaged at baseline—those that did not work at baseline (39 percent, adjusted p-value $= 0.003$) and those that were unstably housed at baseline (54 percent, adjusted p-value $= 0.028$). 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 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.

[40] 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 phaseout 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.

[41] This decline in WIC benefits does not appear to be driven by a loss in benefits due to higher earnings. Less than 5 percent of the sample has young children and household income greater than the eligibility threshold for WIC of 185 percent of the poverty line in the 24-month follow-up survey.

Debt and Savings.—Padua also emphasized debt reduction and increased savings as goals for participants. Results reported in Table 6 summarize the effect of Padua on debt and savings outcomes. A positive effect denotes a reduction in debt and improvements in savings. For the full sample, and across subgroups, the standardized treatment effect is positive, but small and not statistically significant. In Supplemental Appendix Table A-7, we report the impact of Padua on individual savings and debt outcomes from the 24-month survey. Similar results from the 12-month follow-up are reported in Supplemental Appendix Table A-13.

The study sample has some connection to both the formal banking industry and subprime credit market (see Supplemental Appendix Table A-13). Roughly two-thirds of study participants have a checking or savings account, and about one in seven 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 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. These decreases, however, are not statistically significant when adjusting for multiple hypothesis testing.

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 in the more advantaged subgroups. However, further analysis suggests these differences in mean assets are largely driven by a few outliers.[42] 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 this effect is driven by those who are stably housed at baseline and those who are stably housed and not employed at baseline—two groups for which we found large effects on full-time work.

We also see greater nonmortgage debt for the treatment group. Whereas the treatment group has less nonmortgage debt at the 12-month survey (18 percent, Supplemental Appendix Table A-13), by the 24-month follow-up, the difference becomes positive and economically meaningful (about $8,800, a 33 percent increase), but it is not statistically significant (adjusted p-value $= 0.315$). 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

[42] To explore the sensitivity of estimates to outliers, we reestimated treatment effects dropping those respondents who had values of total assets above various thresholds. Dropping the three observations above the 99 percentile reduces the point estimate for the full sample to $574 (unadjusted p-value $= 0.25$). Lower thresholds (e.g., the 98 and 97 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 $= 0.64$).

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 clients to help them better understand their financial situation. Thus, Padua clients had greater information about their 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 nonmortgage 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.[43] To further explore the effect of Padua on financial stability, we link study participants to administrative credit bureau data as discussed below.

Health.—Finally, we summarize the effect of Padua at the 24-month follow-up on health (Table 6), where a positive effect on the index denotes an improvement in health, insurance coverage, fewer ER visits, and more doctor visits. For the full sample, and across subgroups, the standardized treatment effect is positive, but small and not statistically significant. Supplemental Appendix Table A-8 reports effects on underlying outcomes, while the results at the 12-month mark are reported in the Supplemental Appendix Table A-14.

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 economic 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 (adjusted p-value $= 0.021$). 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 it is not precise.

VI. Results from Administrative Data

To complement our results for some of the key survey-based outcomes, we also link study participants to administrative data on earnings and employment, government program participation, and financial information. In addition to providing an

[43] 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 Supplemental Appendix Table A-7 (still positive although not always significant).

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 nonresponse and underreporting 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).

A. *Labor Market Outcomes*

We link our study participants to UI system earnings and employment information for two 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 percent of our study participants). See Supplemental 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 noncovered 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 noncovered employment, previous research indicates that movement out of state is infrequent for low-income populations.

In Figure 9, panels A through F, 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 horizontal 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 6 regression-adjusted treatment effects for cumulative average earnings and employment through the eighteenth quarter after enrollment (panel B).[44] In each

[44] Supplemental Appendix Table A-17 also reports treatment effects on earnings and employment in quarter 8 and average effects through quarter 8 to align with the timing of the 24-month follow-up survey. Supplemental Appendix Tables A-18 and A-19 report quarterly effects on employment and earnings for each postenrollment quarter, respectively.



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 Supplemental 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 are 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.

regression, we include as potential controls all baseline characteristics used as controls in Table 4 and eight quarters of prerandomization employment indicators and earnings, selecting among all of these controls using the postdouble 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 prerandomization employment trends (Figure 9, panel A). 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 9, panel A). 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 was 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 postenrollment, 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.

During the 18 quarters following random assignment, the treatment group was 5 percentage points more likely to work (unadjusted p-value 0.144) relative to the average employment rate in the control group of 62 percent (Table 6). 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 (Supplemental Appendix Table A-18), as compared to 6.9 percentage points based on the 12-month survey. In quarter 8, this difference is 4.6 percentage points (Supplemental Appendix Table A-17), 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 (Supplemental Appendix Table A-17). Recall that in the survey results we found significant treatment effects (based on unadjusted 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 9, panel D). As with employment, the treatment group has higher earnings than the control group in the quarters prior to Padua application, and we again see that both treatment and control group members experience a decline in earnings prior to application. After application, earnings rise for both groups. Average earnings for the control group in the eighth quarter after enrollment was $3,207 in the UI data, as compared to $3,447 for the control group based on the 24-month follow-up survey (Table 4). We also again see some evidence of treatment-control differences in postperiod earnings, but these differences attenuate when controlling for preapplication differences in earnings. During the 18 quarters following random assignment, the control group averaged $3,408 in quarterly earnings, and the regression-adjusted treatment effect for the full sample is small ($50) and not statistically significant.[45]

[45] Specifications that do not control for preexisting 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 through quarter 18 is $331 (unadjusted p-value $= 0.384$).

Panels B, C, E, and F of Figure 9 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 6, panel B. 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 smaller in magnitude than the survey results and imprecise. The regression-adjusted difference in employment through the eighteenth quarter after enrollment for those not employed at baseline was 6.7 percentage points (unadjusted p-value $=$ 0.135), and the difference for earnings was \$302 (unadjusted p-value $=$ 0.449). For those not employed and stably housed at baseline, Padua leads to a 8.6 percentage point increase in employment through the eighteenth quarter (unadjusted p-value $=$ 0.101) and a \$627 increase in average quarterly earnings over the same time period (unadjusted p-value $=$ 0.185).

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.

B. *Government Programs*

We also link the Padua sample to benefits data for SNAP and TANF through the RMC as described in Supplemental 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. Figure 10, panels A through C report the average monthly SNAP participation rate, and Figures 10, panels D through F report the average dollars of SNAP benefits received by treatment status for 24 months before and 59 months after study enrollment for the full sample as well as for some subgroups. We also report regression-adjusted treatment effects on average monthly SNAP participation over this time period in Table 6. 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

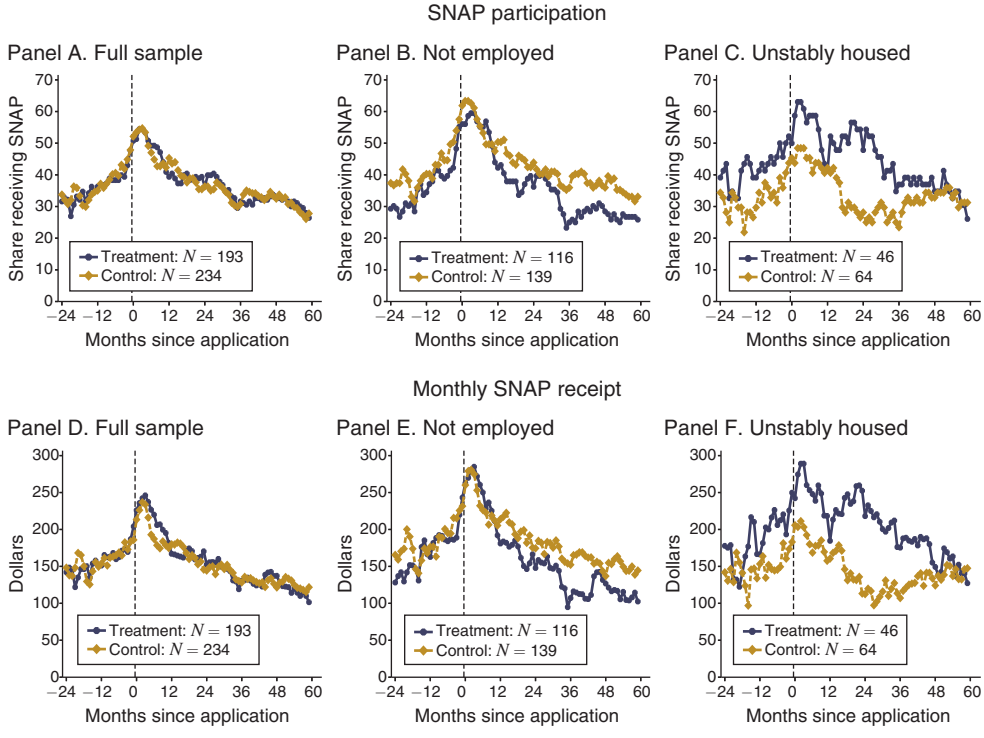


FIGURE 10. TRENDS IN SNAP RECEIPT, BY SUBGROUPS

Notes: Data source is administrative SNAP receipt data from THHSC. The sample includes all 427 Padua study participants (see Supplemental 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.

through 59 months after baseline are small and not statistically significant (Table 6, panel C).[46] This null effect is consistent with the evidence from the survey data at 24 months (Supplemental Appendix Table A-5).

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 (Supplemental Appendix Table A-5). 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

[46] Supplemental Appendix Table A-20 additionally reports effects on SNAP receipt and SNAP receipt amount in month 24 and averaged over the two years following random assignment to coincide with the timing of the follow-up survey.

increase in SNAP (unadjusted p-value $= 0.091$) at 24 months after study enrollment (Supplemental Appendix Table A-20).

We also link 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.

C. *Debt*

We link Padua participants to their historical credit records from Experian, one of the largest credit bureaus in the US, from 3 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 are 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 2014:II through 2021:I), and about two-thirds ($N = 286$) are linked to a credit record in every quarter in the panel. The treatment group is just as likely as the control group to have been linked to a credit record during the entire panel. See Supplemental Appendix G.2 for more details on the data and the linking process.

The evidence from the credit report data suggests 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 11, panel A) as well as the distributions of the credit scores by treatment status in quarter 8 (Figure 11, panel B). 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 Supplemental Appendix Table A-21, we report the estimated treatment effects at eight quarters following random assignment for outcomes that are similar to those presented in Table A-7. In these regressions, we use the postdouble section LASSO procedure (Belloni, Chernozhukov, and Hansen 2014) to select among our primary controls as well as three prerandomization 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 11, panel B).

In Figure 12, panel A, we plot 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 eight quarters after enrollment, the difference in the share with credit card debt across groups is small and not statistically significant (Supplemental Appendix Table A-21). In contrast, we do see evidence of a treatment effect on the total amount of credit card debt (Figure 12, panel B and Supplemental Appendix Table A-21). 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

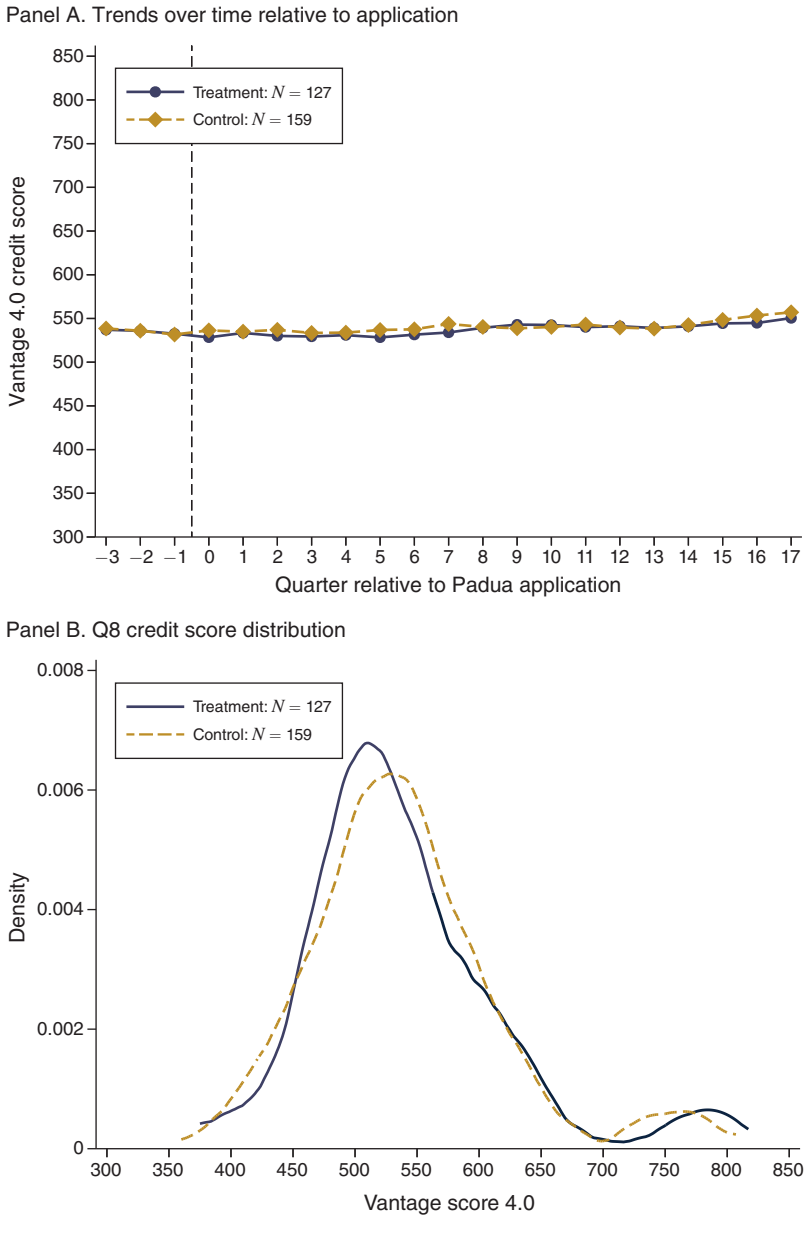


FIGURE 11. VANTAGE 4.0 CREDIT SCORE, BY TREATMENT STATUS

Notes: Data source is credit attributes from Experian. The sample includes 286 Padua study participants with a balanced panel of credit records (see Supplemental 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. Panel 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.

(Supplemental Appendix Table A-7) also suggest a negative effect, but these estimates are smaller and very imprecise. Using credit report data, the negative effect is evident for all of the subgroups we report in Supplemental Appendix

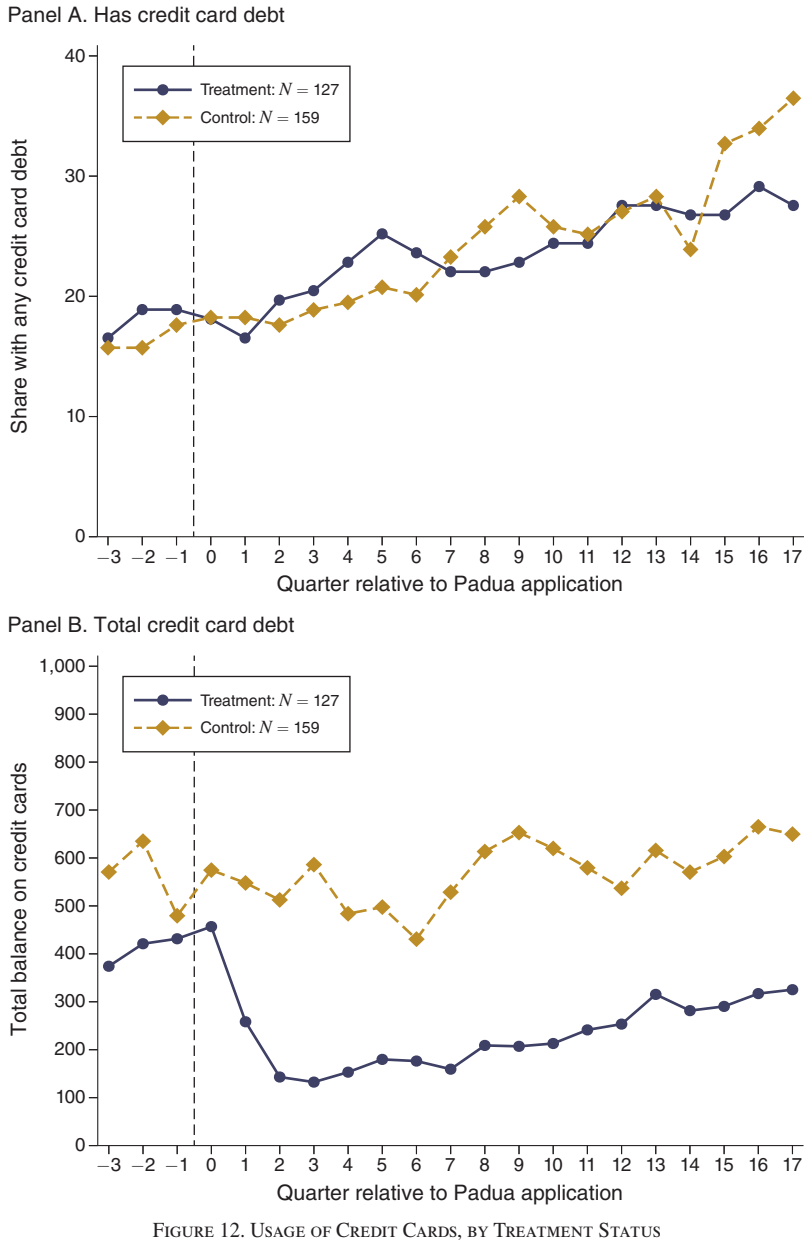


FIGURE 12. USAGE OF CREDIT CARDS, BY TREATMENT STATUS

Notes: Data source is credit attributes from Experian. The sample includes 286 Padua study participants with a balanced panel of credit records (see Supplemental 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. Panel 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 A-21, 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 nonmortgage 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 discuss 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 nonmortgage debt in credit report data is consistent with this possible explanation.

We summarize these effects on credit outcomes from quarter 8 in panel D of Table 6, which reports the standardized treatment effect on the administrative credit outcomes. Generally, we see small improvements in credit outcomes ranging from 0.4 to 0.13 standard deviations, but no effects are statistically significant.

VII. 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 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 WTP 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 Supplemental Appendix Tables A-18 and A-19, respectively.[47] We calculate the net present value of these experimental impacts during the five years following random assignment.[48]

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 US$(2016)22,950. 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

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

[48] We deflate point estimates to 2015 US$ using the CPI-U for the Dallas-Fort Worth-Arlington, Texas, 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).

and use allowance, indirect costs, and financial assistance.[49] Just over $3,000 of the total program cost is the 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.[50] 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 7 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.[51] 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.[52]

Using five years of administrative data on earnings, we estimate the WTP 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 roughly 0.18. 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 five years, the MVPF increases to about 0.26. Similarly, under the assumption that gains persist until

[49] 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.

[50] 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 = ($22,950 / 20 months) × (12 months / year). Per participant program costs incurred over the first 24 months—the time at which the final follow-up survey was conducted—were approximately US$(2016)18,300, of which $2,260 is the value of assistance received by the treatment group relative to the control group.

[51] 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.

[52] 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 life cycle effects measured in the 2014–2016 American Community Survey (Ruggles et al. 2023).

Table 7—Marginal Value of Public Funds by Subsample and Time Horizon

	MVPF		*WTP*		*Net cost*	
	Estimate (1)	CI (2)	Estimate (3)	CI (4)	Estimate (5)	CI (6)
Full sample: $N = 325$						
Experimental impacts (Years 1–5)	0.176	[−0.198, 0.634]	4,009	[−4,906, 13,074]	22,725	[20,624, 24,791]
Project effects (10 Years)	0.265	[−0.552, 1.576]	5,906	[−15,030, 27,196]	22,272	[17,252, 27,208]
Project effects (28 Years)	0.502	[−1.209, 7.026]	10,615	[−40,156, 62,239]	21,144	[8,858, 33,225]
Employed: $N = 122$						
Experimental impacts (Years 1–5)	−0.076	[−0.625, 0.660]	−1,823	[−17,279, 13,543]	24,073	[20,518, 27,644]
Project effects (10 Years)	−0.362	[−1.336, 1.536]	−9,354	[−45,729, 26,809]	25,816	[17,445, 34,226]
Project effects (28 Years)	−0.931	[−2.301, 6.080]	−28,057	[−116,382, 59,753]	30,143	[9,819, 50,558]
Not employed: $N = 203$						
Experimental impacts (Years 1–5)	0.331	[−0.155, 0.940]	7,261	[−3,812, 18,223]	21,955	[19,384, 24,548]
Project effects (10 Years)	0.738	[−0.430, 2.937]	14,847	[−11,353, 40,784]	20,106	[13,883, 26,382]
Project effects (28 Years)	2.168	[−0.974, 272.259]	33,726	[−30,121, 96,933]	15,557	[353, 30,891]
Stably housed: $N = 240$						
Experimental impacts (Years 1–5)	0.298	[−0.149, 0.877]	6,581	[−3,658, 17,209]	22,121	[19,621, 24,529]
Project effects (10 Years)	0.483	[−0.511, 2.310]	10,251	[−13,783, 35,198]	21,226	[15,235, 26,997]
Project effects (28 Years)	1.019	[−1.178, 18.039]	19,379	[−38,973, 79,949]	19,022	[4,431, 33,077]
Unstably housed: $N = 85$						
Experimental impacts (Years 1–5)	−0.124	[−0.688, 0.636]	−3,017	[−19,286, 13,138]	24,333	[20,645, 28,037]
Project effects (10 Years)	−0.359	[−1.385, 1.717]	−9,235	[−47,905, 29,166]	25,753	[16,982, 34,565]
Project effects (28 Years)	−0.841	[−2.333, 8.809]	−24,646	[−118,846, 68,895]	29,307	[7,818, 50,897]
Not employed and stably housed: $N = 150$						
Experimental impacts (Years 1–5)	0.563	[−0.050, 1.374]	11,759	[−1,206, 24,584]	20,900	[17,887, 23,938]
Project effects (10 Years)	1.405	[−0.236, 5.325]	24,874	[−5,924, 55,342]	17,703	[10,383, 25,085]
Project effects (28 Years)	5.859	[−0.632, ∞]	57,486	[−17,656, 131,826]	9,812	[−8,139, 27,916]

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 Supplemental Appendix G for details). Columns 1, 3, and 5 report estimates of the MVPF, WTP of the program, and per person program cost net of fiscal externalities, respectively. Columns 2, 4, and 6 report 95 percent confidence intervals that come from 10,000 bootstrap draws based on estimated earnings effects and standard errors. The estimates assume a 3 percent 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 VII for additional details.

retirement at age 65 (28 years after random assignment for the typical applicant), then the MVPF is around 0.5.

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 experienced 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 0.3 to 0.5 when relying on experimental estimates and ranges from 1 to 5.8 when assuming 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 WTP that is missing from our analysis. We find evidence that Padua improved self-reported health both in our full sample and 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 that 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 net present value 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 V 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 and 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.

VIII. 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 nonprofit sector serving clients facing poverty. In response to client need, a large urban social service provider designed a holistic, individualized wraparound 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 were 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 had 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. This heterogeneity highlights that it is challenging to identify primary outcomes that are appropriate to measure program success for all participants. Future work should consider collecting information on participants' goals and examine outcomes that measure the extent to which progress is made toward those goals. For example, recent work on comprehensive interventions has collected baseline information on goals with the intention of examining how successful such programs are at achieving those goals (Espinosa, Evans, and Phillips 2020).

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, wraparound 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 that 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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