

The Effect of Emergency Financial Assistance on Employment and Earnings

Daniel Hungerman
University of Notre Dame
and NBER

David Phillips
University of Notre Dame
and LEO

Kevin Rinz
Washington Center
for Equitable Growth

James Sullivan
University of Notre Dame
and LEO

May 9, 2024

Abstract

We examine the labor supply effects of short-term income transfers for families experiencing a housing crisis. We link callers of an emergency assistance homelessness prevention hotline to their federal tax records and measure their employment & earnings in years surrounding their calls. Our methodology exploits quasi-random variation in the availability of assistance to compare similar families receiving and not receiving funds. Looking up to five years post-assistance, we find evidence, especially for the lowest earners, of earnings and employment gains, and overall we find no evidence that assistance lowers earnings or employment. Our results indicate that any income effect of temporary transfers for those in crisis is minimal and that these transfers may convey labor market benefits for the poorest of the poor.

Any opinions and conclusions expressed herein are those of the authors and do not represent the views of the U.S. Census Bureau. The Census Bureau has ensured appropriate access and use of confidential data and has reviewed these results for disclosure avoidance protection (Project 7523252: CBDRB-FY23-0267 and CBDRB-FY24-0033). Work on this paper was completed while Rinz was an employee of the U.S. Census Bureau. This research was supported by the University of Notre Dame's Wilson Sheehan Lab for Economic Opportunities (LEO). We also appreciate the cooperation and help of Catholic Charities of the Archdiocese of Chicago and its Homelessness Prevention Call Center with special thanks to Kathy Donohue. Email the authors at dhungerm@nd.edu, David.Phillips.184@nd.edu, krinz@equitablegrowth.org, and James.X.Sullivan.197@nd.edu.

1. Introduction

A primary goal of social safety net programs is to help households maintain economic well-being when faced with a transitory shock ([Gruber, 1997, 2000](#)). Research has shown that such shocks, particularly for the poor, can create a downward spiral of economic challenges. For example, a temporary loss of income may result in an inability to pay rent and ultimately eviction, which, in turn, leads to increased likelihood of homelessness and reduced earnings ([Collinson, Humphries, Mader, Reed, Tannenbaum, and Van Dijk, 2022](#)). To prevent this type of chain reaction, most US communities have programs that make emergency financial assistance available to low-income families facing housing instability. This type of aid expanded dramatically during the COVID-19 pandemic, with Congress allocating more than \$70 billion to state and local governments for emergency housing assistance ([CARES Act, 2020](#); [Consolidated Appropriations Act, 2020](#); [American Rescue Plan Act, 2021](#)).

However, there is little direct evidence on whether such assistance can preserve or improve earnings and employment for those experiencing a housing crisis. On the one hand, an income transfer that targets those in crisis may improve a recipient’s economic stability, leading to better job search or job matching outcomes and increased employment or earnings ([Desmond, 2016](#); [Collinson et al., 2022](#)). Moreover, policymakers often claim that housing assistance stabilizes recipients’ lives in a way that better positions them to work. For example, the executive director of the US Interagency Council on Homelessness states, “[H]ousing is indeed the foundation for getting and keeping a job, addressing mental health and substance use, and reconnecting with family, friends, and community” ([Olivet, 2023](#)). A growing body of research indicates that temporary income transfers targeted towards those facing a housing crisis can have important nonpecuniary benefits for recipients, including reduced homelessness ([Evans, Sullivan, and Wallskog, 2016](#); [Phillips and Sullivan, 2023](#)) and arrests for violent crimes ([Palmer, Phillips, and Sullivan, 2019](#)).

On the other hand, behavioral responses to short-term income transfers might lessen the incentive to work. A simple labor supply model predicts that a pure non-labor income trans-

fer such as emergency financial assistance would reduce labor supply if leisure is a normal good. A large literature has shown evidence of a negative income effect in a variety of contexts, including the Negative Income Tax experiments (Robins, 1985; Price and Song, 2018) and cash transfers that result from lotteries (Imbens, Rubin, and Sacerdote, 2001; Cesarini, Lindqvist, Notowidigdo, and Östling, 2017). As we discuss below, work on housing-based transfers for the disadvantaged has found both negative and positive effects on employment and earnings, although it is unclear if these results would extend to a temporary emergency assistance context.

We examine whether short-term income transfers for housing affect labor supply using a quasi-experimental setting where, conditional on a set of observable characteristics, the availability of funding is functionally random. In particular, we examine the effect of emergency financial assistance coordinated by the Homelessness Prevention Call Center (HPCC) in Chicago, Illinois, which connects individuals and families in need of rent or utility assistance with agencies that provide emergency financial assistance. We link callers to federal administrative tax data on earnings and other outcomes to determine whether those who call at a time when funding is available have lower earnings or are less likely to work than those who call when funding is unavailable. Our linking allows us to follow callers for many years both before and after they call.

We have several findings. First, our results do not support the hypothesis that families in crisis reduce their labor supply in response to temporary income transfers. For the full sample of eligible callers, our results suggest that, if anything, temporary assistance leads to greater work, although the estimated effect varies somewhat across specifications. Generally, we can reject that earnings fall by more than 3 percent of their pre-transfer average.

Second, any positive effect on earnings is driven by callers with low baseline earnings. For this group, we can strongly reject the hypothesis that temporary assistance discourages work. In fact for this group, we find that being referred to financial assistance increases annual earnings over the next four years by \$400-\$500. This is a reasonably large effect,

representing nearly 10% of the control group mean or about 55% of the total long-term effect of eviction on earnings in [Collinson et al. \(2022\)](#). The estimated effects on employment are largely consistent with those for earnings—our results do not support a negative employment effect but there is some evidence of a positive employment effect for those with low baseline earnings.

Third, looking beyond labor supply, we find that one-time emergency assistance has little impact on non-labor income sources—the effects for adjusted gross income (AGI) are consistent with those for earnings. We also do not find evidence that financial assistance reduces reliance on government programs such as unemployment insurance or SNAP. Our results suggest that those who receive emergency assistance are slightly more likely to receive SNAP benefits, although the magnitude of this effect is very small; we can reject the hypothesis that being referred to assistance increases the likelihood of being on SNAP by more than 2 percentage points. This effect could be driven by those referred to a delegate agency for financial assistance receiving information on other programs like SNAP.

Fourth, we also find little evidence that the impact of financial assistance on labor market outcomes differed across groups, other than the noticeable difference by baseline earnings. Our main specification provides some evidence that earnings gains may be largest for those who are seeking assistance because they lost their job and for those without kids. However, these heterogeneous effects are less apparent with alternative specifications.

These results have several implications. First, any work disincentives created by housing assistance appear to depend on the intensity of assistance. While some long-term subsidies have shown signs of moderate work disincentives ([Jacob and Ludwig, 2012](#); [Gubits, Shinn, Wood, Brown, Dastrup, and Bell, 2018](#)), we can reject large work disincentives for temporary housing assistance.

Second, any improvements in employment resulting from stable housing appear over the long run, through the first 4 years post-treatment. This persistence is important because even modest income gains each year, if accrued, could dramatically bolster the overall estimated

net present value of emergency assistance. Also, future work on the benefits of housing support should explore effects not just at the time of award, but in the years following, as the benefits we document would have been impossible to identify with only short-term outcome data.

Third, these results are much stronger for the lowest-income individuals. All people in the study face serious disadvantages—those calling the HPCC start out in worse economic situations than their neighbors, as we show below. But even among a particularly low-socioeconomic-status population, our results differ by baseline earnings.

Together, these results support the idea that housing instability matters for labor market outcomes. If exceptionally low-income individuals lack the ability to insure against housing shocks, then these shocks can lead to negative effects in other outcomes. Our estimates suggest that, for the poorest of poor, emergency housing assistance can help to prevent the type of downward spiral that can be associated with a negative shock.

2. Income Transfers and Labor Market Outcomes

Economists have long been interested in the labor supply effects of a wide variety of cash transfer programs including means-tested transfer programs such as Temporary Assistance to Needy Families ([Ziliak, 2015](#)), the Supplemental Nutrition Assistance Program ([Han, 2022](#); [Hoynes and Schanzenbach, 2015](#)), and Supplemental Security Income ([Duggan, Kearney, and Rennane, 2015](#); [Deshpande, 2016](#)). Because these programs provide an income guarantee for those who do not work and then apply a benefit reduction rate for those with earnings that exceed some (typically low) threshold, they disincentivize work through negative income and substitution effects. Other redistribution programs, such as the Earned Income Tax Credit ([Nichols and Rothstein, 2015](#); [Whitmore Schanzenbach and Strain, 2021](#)), share the feature of having a negative income effect, but because they provide a wage subsidy the substitution effect is positive over certain ranges of pre-tax earnings.

Emergency financial assistance differs from these programs in important ways. Unlike the means-tested transfer programs mentioned above, financial assistance is very short-term and eligibility for assistance or the size of the benefit typically does not depend on employment status or earnings level. For this reason, financial assistance can be viewed as a pure income transfer, implying no substitution effect but potentially a negative income effect. A recent literature has examined pure income transfers such as casino profits distributed to tribal members ([Akee, Copeland, Keeler, Angold, and Costello, 2010](#)), or the distribution of Alaska Permanent Fund dividends to state residents ([Jones and Marinescu, 2022](#)). In addition, many recent policies have been implemented, or are under consideration, that provide pure income transfers to low-income households including the expanded Child Tax Credit that was available in 2021 and that many have proposed making permanent, as well as scores of local guaranteed income programs.¹ The possible work disincentives have been central to debates over expanding these transfers ([Corinth et al., 2021](#); [Hoynes and Rothstein, 2019](#)). Our context differs from these studies because we focus on transfers that are less permanent and target a uniquely vulnerable population: those facing a housing crisis.

Understanding the labor supply effects of transfers has also been the focus of studies of programs that aim to promote housing stability, such as housing subsidies. [Jacob and Ludwig \(2012\)](#) find that winning a housing voucher lottery reduces subsequent employment, although the effect is modest—a decline of 6 percent. [Gubits et al. \(2018\)](#) find that long-term subsidies provided to families at the moment they are entering emergency shelters reduces future employment. However, there is some evidence that the work disincentive may differ when the subsidy is temporary. The Family Options study finds that rapid re-housing subsidies, which typically phase out within 12 to 24 months, increase earnings in the short-term ([Gubits et al., 2018](#)). But the study’s power for measuring long-term effects is limited due to relatively low take-up. This is important, since one of the rationales for

¹The American Rescue Plan Act of 2021, which was signed into law in March 2021, expanded the Child Tax Credit and made it fully refundable, eliminating the work incentives of the original Child Tax Credit ([Corinth, Meyer, Stadnicki, and Wu, 2021](#); [Enriquez, Jones, and Tedeschi, 2023](#)).

these temporary subsidy programs concerns their impact on future income.

Previous studies of emergency financial assistance have not focused on labor market outcomes. A series of studies examine how such assistance affects homelessness (Evans et al., 2016; Phillips and Sullivan, 2023), crime (Palmer et al., 2019), and healthcare use (Downes, Phillips, and Sullivan, 2022) but do not measure employment outcomes. The one existing emergency assistance study that does measure employment shows large pre-treatment imbalances in employment that make inference difficult (Rolston, Geyer, Locke, Metraux, and Treglia, 2013). Perhaps the closest papers to the present study examine small, one-time income transfers during the COVID-19 pandemic. Jaroszewicz, Jachimowicz, Hauser, and Jamison (2022) find a small negative impact of providing \$2000 on a financial index mostly composed of employment outcomes, and Pilkauskas, Jacob, Rhodes, Richard, and Shaefer (2023) find little impact of a \$1000 transfer on employment. Of course, these papers study programs implemented in the unique context of a global pandemic that dramatically altered labor market outcomes, and although the transfers were targeted towards low-income households, they did not target those facing a negative income shock or other crisis.

We also contribute to the understanding of labor supply effects of cash transfers by exploring heterogeneity in this effect. Whether housing assistance encourages or discourages employment may vary across individuals. In particular, although cash transfers may lower earnings and employment through an income effect, these work disincentives might be lower for those with very little income, either because their attachment to the labor market is already weak or because the potential for improved housing stability to lead to better labor market outcomes might be greater for low-income families. In fact, previous work (Evans et al., 2016) finds that temporary financial assistance is most effective at preventing homelessness among people with the least income, and housing loss may be particularly disruptive among people facing other barriers (Collinson et al., 2022).

3. The Homelessness Prevention Call Center

Most US communities have a network of providers who make emergency assistance available to those at risk of eviction. In 2019, hotlines that provide essential community services were available to 95% of the U.S. population ([Federal Communications Commission, 2023](#)), and the most common request made to these hotlines was for help paying rent and utilities ([211.org, 2023](#)). In Chicago, the process of allocating emergency assistance is fairly centralized. Individuals and families in need of rent or utility assistance can call the city’s services hotline, 3-1-1, for help. Such calls are routed to the HPCC, which is operated by Catholic Charities and serves as the central hub for emergency financial assistance. An intake specialist at the HPCC collects contact and demographic information from the caller. This step is important for the evaluation because the HPCC collects personal identifying information and demographic characteristics before determining eligibility and regardless of whether any financial assistance is available. This step allows us to link eligible callers to outcome data regardless of whether the caller is referred to assistance or turned away because no assistance is currently available.

After collecting basic demographic information, the intake specialist collects additional information necessary to determine if the caller is eligible for assistance. To be eligible, the caller must demonstrate the presence of a crisis that (a) is on a list of eligible crises (such as the loss of a job), (b) causes imminent risk of homelessness (as evidenced by an eviction notice, for example), (c) can be solved with limited financial assistance (typically less than \$1,500), and (d) is temporary (i.e. the caller has sufficient future income to pay essential expenses). Callers ineligible for the HPCC and callers who are not referred to funds are referred to non-financial assistance.²

The HPCC does not disburse assistance itself. Rather, it connects callers to government and private entities, or delegate agencies, that administer assistance. If a caller is determined

²This assistance includes legal aid, domestic violence counseling, assistance with utility complaints, workforce development, senior services, disability services, public benefit screening, and other general support services ([George, Hilvers, Patel, and Guelespe, 2011](#)).

to be eligible, the HPCC then checks to see if funding is available at one of the delegate agencies. The staff member works through a pre-set ordered list to see if any of the agencies has funds available immediately and if the caller meets any fund-specific eligibility criteria including request type (rent assistance, security deposit, or utilities), amount of assistance needed, veteran status, receipt of housing subsidies, and whether the total debt exceeds one month of rent. For example, some funds have more restrictive payment limits or may pay only for back rent, not security deposits or utilities. If an agency with funds is identified, the HPCC refers the caller to that delegate agency for assistance. If not, the caller is referred to non-financial assistance.

On any given day, the availability of funds is sporadic and hard to predict. New delegate agencies come online and existing agencies shut down routinely. Also, funding may not be available continuously at currently operating agencies because they may temporarily run out of funds or are unable to distribute them immediately. Whether funds can be distributed on any given day, or time within a day, depends on many factors. For example, some delegate agencies require that callers meet with a financial counselor before receiving assistance, and a HPCC intake specialist will not refer a caller for assistance if an interview slot is not available at the time of the call. For some agencies, there are only a fixed number of appointments available each week or month, but the availability of slots may vary due to cancellations or changes in staff capacity. Variation in funding also results from the fact that some delegate agencies are supported by local or state programs that provide an inconsistent and unpredictable funding stream. Therefore, an eligible caller who calls when delegate agencies happen to have funding and staff availability will be referred to assistance, while the same caller would have been turned away had they called when funding was unavailable or staff lacked capacity to process a request.

Figure 1.a shows the weekly funding rate for all eligible callers during our full period from July 2013 through December 2015. This figure shows that the probability of funding varies considerably from week to week. While the average funding rate for this sample period was

65%, in some weeks more than 90% of eligible callers were referred to funds, while in other weeks less than 30% were referred. This variation may differ across different caller types because of fund-specific eligibility requirements. When looking at the shorter time period during which we observe fund-specific eligibility requirements (Figure 1.b), and when we restrict the sample to callers within this shorter period that share some of the same fund-specific eligibility requirements, i.e. callers who do not have other housing subsidies and are asking for more than one month of rent assistance totalling more than \$900 (1.c), we still find considerable variation in the likelihood of being referred to funds.

Because fund availability is determined by factors outside the control of the HPCC, the call center has limited information about when funds will be available. Any information they might have about future funding is not shared with callers. The instructions for HPCC staff state, “If anyone asks, ‘when will a fund be available?’ please respond the following: ‘I do not have information on when funds will be available. Unfortunately, there are not enough funds for everyone who needs assistance and availability is sporadic.’ If anyone asks, ‘should I call back?’ please reply: ‘That is up to you.’ If anyone asks, ‘but what is the best time to call?’ please reply: ‘There is no best time to call. The need is so high in Chicago/the Suburbs, there are so many people trying to get access to the limited number of grants’” (HPCC, 2013).

As a result, the HPCC process generates a natural comparison group. Observationally identical callers are sometimes referred to funds and other times not. The unpredictable and high frequency variation in the funding rate ensures that callers who are referred to funds versus not will be similar, conditional on characteristics that affect fund-specific eligibility requirements.

4. Data

4.1. HPCC Call Center Data

This study uses a sample of call information from the HPCC that covers all calls between January 20, 2010 and March 29, 2018. Following [Downes et al. \(2022\)](#), we limit the sample to calls received between July 1, 2013 and December 31, 2015. Prior to matching this sample to outcome data, we exclude calls in categories unrelated to financial assistance, e.g. calls that are incomplete or do not include client interaction. The call data include personal identifiers (e.g. name) fundamental to the data linking process. They also cover various characteristics related to the program eligibility process. These variables include those related to overall eligibility (e.g. income) as well as those related to fund-specific eligibility (e.g. amount needed). They also collect general demographic information (e.g. gender) that we use as covariates and for balance tests. We also link caller address ZIP codes to 2009-2013 American Community Survey data on ZIP-code level characteristics.

4.2. Administrative Records and Census Data

We use administrative and survey data to construct a panel of labor market, family structure, and residential outcomes, supplemented with additional demographic information on callers. We construct outcome measures using data from three types of tax forms: form 1040, form W-2, and various information returns (e.g., 1099 forms). In each case, we have access to a limited set of fields from the universe of records. Form 1040 is available annually starting in 1998 and provides tax unit-level information on income, filing status, presence of children, and place of residence. Because we have access to the universe of returns, the data also tell us whether a person appears on any tax return in a given year. Form W-2 is available annually beginning in 2005 and provides individual-by-employer-level information on earnings. Our primary analysis aggregates earnings across employers within person. In addition to analyzing earnings, we use data from W-2s to construct measures of employ-

ment, with people considered employed if they have earnings greater than zero, and more substantially employed if they have earnings exceeding the amount associated with half a year of full-time work at the contemporaneous federal minimum wage. In supplementary analysis, we also use administrative data from the state of Illinois to measure participation in the Supplemental Nutrition Assistance Program (SNAP).

The information return data includes whether a person receives various information returns and the addresses at which forms were received. The address information is available beginning in 2003. Codes identifying the type of information return received are available consistently beginning in 2010.³ The US Census Bureau’s Numident file provides demographic information for every person with a social security number. Information provided includes date of birth, place of birth, sex, and citizenship.

For our comparisons of callers to noncallers, we also use data from the decennial census and American Community Survey (ACS) to provide other demographic data at the individual level, most notably information on race and ethnicity. This information is drawn from the 2000 (short form) and 2010 decennial censuses and all available years of the ACS, which samples one percent of households annually beginning in 2005. We determine place of residence for callers and non-callers using location information available from tax forms and the Census Bureau’s Master Address File Auxiliary Reference File (MAF-ARF) which collects person-year-address combinations from additional administrative data sources.

4.3. *Data Linkage*

The HPCC data are linked to outcome data at the individual level using the Census Bureau’s Protected Identification Keys (PIKs), assigned using personally identifiable information (PII) by the Person Identification Validation System (PVS).⁴ Each PIK is associated with a unique social security number (SSN) or individual taxpayer identification number

³The specific information returns identified by these codes are 1099-MISC, 1099-R, 1099-S, 1099-G, 1099-SSA, 1099-INT, 1099-DIV, and 1098. The file also includes an indicator for receipt of and address information associated with Form W-2.

⁴See [Wagner and Layne \(2014\)](#) for details.

(ITIN). For this reason, files that include information on people’s SSNs/ITINs (e.g. administrative records such as data from tax forms) typically have extremely high PIK assignment rates (97 percent or higher). PIK assignment rates for the decennial census and ACS, which do not contain SSN/ITIN information, are typically somewhat lower but still high in absolute terms at about 90 to 93 percent (Mulrow, Mushtaq, Pramanik, and Fontes, 2011). PVS’s false match rates are generally extremely low (Layne, Wagner, and Rothhaas, 2014).

In the HPCC data, about 95 percent of records are successfully assigned a PIK, in line with the high PIK assignment rates for the other data we are using. For callers who meet the criteria to be included in our main analysis sample, Table A.1 compares those who are assigned a PIK to those who are not, using information collected by the HPCC. In general, the differences between the two groups are fairly small. One notable exception is that the people not assigned a PIK are nearly three times as likely to be Hispanic as people who are assigned a PIK (21 percent vs. 7.5 percent). However, given the small size of the group not assigned PIKs, their exclusion from the sample does little to shift the baseline characteristics of the HPCC callers whose subsequent outcomes we are able to analyze.

4.4. *Sample for Analysis*

Because we have access to the universe of records from the data sources described in Section 4.2, we can compare HPCC callers to non-callers from the same narrow geographic area based on contemporaneous and pre-call characteristics. This comparison, which has not been feasible in prior studies of this program, improves our understanding of who seeks emergency financial assistance, and specifically who calls the HPCC.

We start broadly, comparing all callers, regardless of whether they are eligible for assistance, to non-callers. The first two columns of Table 1 compare HPCC callers from 2013 through 2015 to non-callers who were living in the same set of Census blocks at the time.⁵

⁵Census blocks are the smallest geographic unit used by the Census Bureau for official data tabulations. Their boundaries are generally formed by streets and highways, especially in more urban areas where they often consist of city blocks, but may also consist of landscape features such as bodies of water or political

The comparison must be based on information that is available for both groups and so is limited to demographic characteristics obtained from full population data available through the Census Bureau. There are large differences on several characteristics between callers and their geographic neighbors. Callers are three years younger on average, 25 percentage points more likely to be women, and nearly 21 percentage points more likely to be Black than are non-callers living in the same Census blocks. Callers are also nearly 14 percentage points less likely to have been born outside the United States, and correspondingly more than 16 percentage points more likely to be citizens.

Columns 3 through 5 show how these characteristics of callers change as we narrow the sample to our analysis samples. While there are some small fluctuations on these characteristics across subsets of callers, these changes are small compared to the gaps between the full set of callers and non-callers living in the same places. Even when we restrict the sample to those with low earnings, which is a sample we focus on in our analysis, the observable characteristics are quite similar to those for the full sample of eligible callers.

The final column of Table 1 shows characteristics of non-callers matched one-to-one with eligible callers in the low-earnings sample using propensity score matching. Propensity scores are estimated using a logistic regression of calling HPCC on the characteristics shown in the table. Matches are drawn without replacement. Where possible, each caller’s match is selected from the same Census block as the caller.⁶ As one might expect, the differences between the HPCC callers and the matched sample on the characteristics used in the matching process are fairly small.

We use similarly identified matched samples to examine differences between callers and observably similar non-callers on time-varying, pre-call characteristics. Figure 2 shows trends in select outcomes for all HPCC callers in 2013–2015 and matched non-callers, beginning at

borders, such as county/city/town boundaries.

⁶If matching within block is not possible, the match is selected from the same tract. If matching within tract is not possible, the match is selected randomly from non-callers living in the union of all blocks that were home to HPCC callers that year.

least ten years before a call.⁷ There are clear differences between these two groups: HPCC callers are less likely to be employed (i.e. to have any income reported on a W-2), have lower AGI, claim more children as dependents on their tax returns, and change addresses more frequently than demographically similar non-callers living in the same parts of Cook County. These trends do not point to particularly sharp changes in circumstances preceding callers' interactions with the HPCC, though the number of children claimed on tax returns does trend up for callers over the preceding years, with a notable jump up five years prior, while trending down for matched non-callers, suggesting that the presence of children could contribute to financial need for people seeking assistance from HPCC. Trends are similar for our full analysis sample and the corresponding matched non-callers (Figure 3), though the difference in the likelihood of being employed is notably smaller for this sample. The smaller gap between callers in our analysis sample and matched non-callers with respect to employment likely reflects the screening done by the HPCC to ensure that aid is provided to people facing a temporary crisis that causes an imminent risk of homelessness but can be solved with limited financial assistance. The fact that potential contributors to financial need such as the presence of children do not meaningfully change relative to non-callers between Figure 2 and 3 while employment gaps close suggests that callers with relatively weak employment prospects are screened out by the HPCC, likely because a modest amount of financial assistance is less likely to fully meet their financial obligations.

Altogether, HPCC callers appear to be negatively selected based on characteristics that could affect labor market success. This is not surprising given that the HPCC program is intended for individuals facing difficult economic circumstances. However, if one were to estimate the effects of receiving call-center assistance by comparing recipients of financial assistance to noncallers, the comparison could confound the effects of assistance with these underlying characteristics. Instead, our empirical approach will focus on callers, and more specifically callers eligible for assistance, to measure how assistance matters. The next section

⁷For non-callers, the year of the call is the year in which they are matched to a caller.

describes our empirical strategy.

5. Empirical Strategy

5.1. Regression Specifications

We focus on callers to the HPCC for our estimates. If funding were randomly assigned by the HPCC, the causal effect of being referred to emergency financial assistance could be determined through OLS estimation of the following:

$$Y_i = \alpha + Funds_i\beta + \epsilon_i \tag{1}$$

where Y_i is an outcome such as subsequent earnings for individual i ; $Funds_i$ is an indicator for whether the person was referred to funds, and ϵ_i is an error term.⁸ The estimate of β would reflect the difference in average outcomes for those referred to funds and those not referred to funds.

As indicated earlier, multiple levels of selection mean that referral to funds by the HPCC is not unconditionally exogenous, but depends on observable factors. First, people select into calling because they face greater challenges than their neighbors. Second, the HPCC limits assistance based on eligibility rules. We address these levels of selection by limiting the sample to eligible callers. However, agencies may go beyond the HPCC’s eligibility criteria and use fund-specific criteria, such as caps on the amount of need for eligible callers. Some callers are eligible for aid from more delegate agencies than others, and this can increase the likelihood of referral; the probability of treatment can thus depend on these observable characteristics.

The first two columns of Table 2 compare those referred and not referred to funds.⁹ The

⁸For simplicity and consistency we refer to i as an individual caller, but some outcomes Y_i below could be household-level outcomes (such as AGI), in which case the subscript i could be taken as representing the household of the caller.

⁹See Appendix Table A.2 for a more complete set of baseline characteristics.

sample is restricted to eligible callers, with a PIK, who have not called in the prior six months and with nonmissing characteristic information. The third column shows differences in mean characteristics; a standard error for the difference in means is in parentheses. The first row indicates that eligible households referred to funds have on average 1.39 adults in the household, while those not referred have on average 1.45 adults, and this difference is statistically significant. The table shows that fund-specific criteria vary with fund referral. For example, 19% of those referred to funds receive a housing subsidy, while 58% not referred receive a housing subsidy. This difference is consistent with the fact that some delegate agencies do not provide assistance to those receiving housing subsidies. Those receiving funds are 15 percentage points more likely to receive EITC support, and have higher monthly income at the time of the call.

Addressing the fact that referral varies with these characteristics is a central focus of our methodology. First, to account for the fact that some groups are more likely to be referred to funds, we focus on variation in funding access after controlling for fund specific criteria. The HPCC is the central screening point for the delegate agencies, so one can observe and control for the factors that explicitly affect funding. We can thus estimate:

$$Y_i = \alpha + Funds_i\beta + X_i\Gamma + Z_i\Pi + \epsilon_i \quad (2)$$

where Z_i is the set of caller characteristics that affect fund-specific eligibility and the probability of referral. These controls include request type (i.e. rent assistance, security deposit, etc.), controls for need amount, whether the caller is a military veteran, receipt of housing subsidies, and whether the individual's debt exceeds one month of rent. Controls in Z_i also capture call characteristics including the rank of the call within the day, the day of the week and month of the call, the time within the month (first five days, last five days, and middle days), and a set of year-quarter interactions. We also include interactions of need amount with year and quarter indicators to capture the fact that the maximum amount offered by different delegate agencies changes over the sample period. We further include a set of con-

trols for caller characteristics X_i including age, gender, race, ethnicity, income, and receipt of benefits, to reduce residual variance in the outcome.

The last column in Table 2 shows differences in characteristics after controlling for these Z_i characteristics. Many differences are still statistically significant, but are now smaller. The difference in monthly rent reported by callers at the time of the call flips signs; those referred to funds have \$248 greater monthly rent but conditional on fund characteristics this difference becomes $-\$20$. Similarly, the difference in reported monthly earnings goes from \$260 to $-\$57$. These figures are based on information taken during calls to the HPCC, but we can verify this information by looking at pre-call earnings data from linked W-2s. Pre-call average earnings and AGI data are reported, near the bottom of the table. (The bottom-most rows of the table, on predicted earnings and AGI, will be discussed more momentarily.) These pre-period differences also become smaller after the controls in Z_i are included.

The differences in column 4 have countervailing implications for how referred and non-referred households differ. With controls, referred individuals have slightly lower monthly income and rent, and are less likely to receive SNAP. But they report higher EITC participation and earnings. In the last two rows of the table, we characterize the overall implications of these differences. Using the non-referred group only, we regress two administrative pre-call income measures, earnings and AGI, on all the caller characteristics in Table 2. We then use these regressions to predict earnings and AGI for the treatment and control group. As the last column shows, these predicted differences are small and not statistically significant after explicit fund characteristics are accounted for.

The availability of pre-period administrative tax and earnings data raises the fact that we can track individuals over time to further address differences in observables. Our main estimation equation will be

$$Y_{it} = \alpha + Funds_{it}\beta + X_{it}\Gamma + Z_{it}\Pi + Y_{i,-1}\delta + \epsilon_{it} \quad (3)$$

where time period is indexed by t , $Y_{i,-1}$ denotes the outcome observed for individual i in the

year prior to calling (thus it is for the year $t = -1$, where year zero is the year of the call, rather than year $t - 1$), and the variable $Funds_{it}$ is an indicator for whether an individual was referred to funds at the time of the call.¹⁰ This specification pools all time periods starting with the year of the call. The results can thus control for differences in outcomes prior to referral. We will also pursue event-study approaches that limit the sample to a single treatment year and allow the effects of funds β_t to vary over time. Additionally, since our administrative tax data allows us to track individuals over time, we can also consider individual-fixed-effect specifications:

$$Y_{it} = \alpha + Funds_{it}\beta + X_{it}\Gamma + Z_{it}\Pi + \mu_i + \epsilon_{it} \quad (4)$$

where the individual-specific effects μ_i will preclude estimation of coefficients for the $Y_{i,-1}$ control along with many of the X and Z controls. Equation (4) can account for any fixed differences, observed or unobserved, between individuals referred to funds and other individuals. The administrative tax data further allows a comparison of the outcome variables for the treatment and control groups in the years leading up to contact with the HPCC. Similar to the results in Table 2, we find below consistent and growing differences in pre-treatment outcomes in the raw data, but this is driven by observable differences in fund characteristics and is largely eliminated after controls are included.

To test whether the lowest-income households benefit most from intervention, we also use the panel data to break our estimates of (3) and (4) apart for low- and high-income households using pre-period income. For each individual, we take average earnings over the five years prior to contacting the call center; we split the sample into low-earnings and high-earnings groups based on whether individuals' average pre-call earnings are below or above the sample median of about \$6,000. (Below we discuss effects across a range of earnings values beyond the median.) We find stronger evidence that assistance increases earnings for low-earnings

¹⁰When a caller has made multiple calls that satisfy our sample conditions within our analysis period, each call record is included in our analysis. For the purposes of event study analysis, each call has relative year set to zero in the calendar year it was made.

households; these households, moreover, have more balanced observables across treatment and control than the overall sample, and this is true both before and after controlling for Z_{it} characteristics. The low earnings version of Table 2 is reported in Appendix Table A.3. Pre-call outcomes for this group are extremely balanced in an event-study analysis, as shown below.

Overall, callers differ from noncallers; we focus on callers eligible for fund referral. While delegate agencies' ability to target the aid they provide means that referral to funds should and does vary by observable characteristics among eligible callers, we address these differences by controlling for these characteristics and exploiting the panel nature of the data, and after doing so we find that referral occurs for similar looking households, with similar pre-trends and similar predicted future values (absent treatment) in outcomes, and that this is true for overall callers and for those who benefit most from the intervention.

5.2. *Take-Up*

Our empirical model estimates the (regression-adjusted) difference in mean outcomes between those who are referred to financial assistance and those who are not, which can be thought of as an intent-to-treat (ITT) effect. For interpreting the magnitude of our estimates it is important to note that we are not estimating the impact of actual receipt of financial assistance for two reasons. First, not all callers who are initially deemed eligible and offered assistance ultimately receive assistance either because the caller does not follow through with the application process or the delegate agency determines that the caller is not eligible. Second, callers who are initially denied funds may subsequently receive assistance, either because they call the HPCC back and ultimately receive assistance, or they subsequently receive assistance from some entity besides the HPCC. Despite this imperfect compliance, follow-up information on eligible callers indicates significant contrast in the probability of assistance between those initially referred to assistance and those who are not.

Evidence from the Center for Urban Research and Learning (CURL) at Loyola University,

which conducted a descriptive evaluation of the HPCC ([George et al., 2011](#)), indicates that most callers to the HPCC who are referred to assistance do actually receive help. Their survey of HPCC callers who had been approved for funding showed that of 105 people in the survey, 71 percent had already received assistance, expected to receive assistance, or had a request in process within 7 days of the initial call. The remainder had either not been contacted (18 percent) or had been denied as ineligible (10 percent).

In addition, we observe that relatively few households that are initially not referred to funding subsequently succeed in being referred in the future. Using our HPCC data on callers, we find that among those who call when funds are not available in our sample of first-time eligible callers, only 5.4% call back at some later date and are referred to funds. Assuming that actual receipt of funds occurs at the same rate as the full sample of callers who are referred to funds (71%), approximately 4% of our control group would receive assistance through repeat calling. It is also possible for rejected callers to receive assistance from other sources, such as small charities, friends, and family, but these alternative sources are less of a concern in our setting because the HPCC coordinates the distribution of such assistance for all major providers of emergency rental assistance in Chicago. [George et al. \(2011\)](#) find that the call center “operates under the assumption that they are screening for all homelessness prevention funds [in the city]” and that only 8.4% of callers turned away by HPCC had their need met elsewhere. Taken together, this suggests that roughly 12% of those initially turned down for assistance eventually receive some assistance either from repeat calls to the HPCC or from other organizations. Overall, these estimates suggest that those who are initially referred to assistance are 59% more likely to actually receive assistance than those not referred, indicating that the 2SLS estimate would be about 69% (or $1/0.59$) larger than the ITT effects we report.

6. Results

6.1. Trends in Outcomes

Figure 4 shows trends in earnings in the five years prior to an individual calling the HPCC (denoted year 0) and in the four years after. Panels a and b show raw means for all individuals. The remaining panels split on median pre-period earnings with panels c and d showing results using the low-earnings sample and panels e and f using the high-earnings sample.

The three figures on the left report sample means for eligible callers referred to funds and eligible callers not referred. These figures show rising earnings over time for all groups, but differences in the levels and the trends in pre-call earnings, especially for the high earnings group. Those referred to funds have higher annual earnings than those not referred, as suggested earlier by Table 2, and earnings that rise faster. The flattening of earnings for the treatment group in period zero in the upper left figure is driven entirely by the high-earnings sample, where there is a large drop in earnings in year zero. This drop resembles the “Ashenfelter dip” in earnings observed for evicted individuals in Collinson et al. (2022), though they compare evicted to non-evicted individuals, whereas the comparison here is to individuals who (voluntarily) call the HPCC and are referred to funds against callers who are not referred. Finally, both this pre-trend and the period-0 decline are absent for the low earnings group.

The rightmost three figures present event-study-style estimations based off of year-by-year regressions of equation 3. Each coefficient is the estimate β taken from a regression of the outcome Y_{it} on a dummy for whether an individual is ever referred to funds, along with the controls X_{it} , Z_{it} , and $Y_{i,-1}$. A separate regression is done for each t . Robust standard errors are used to construct the confidence intervals.

Several things are noteworthy. First, following Table 2 earlier, the results are much different after controls are introduced. The increasing pre-trend for the high earnings group

in the bottom row is eliminated, and the pre-treatment gap in earnings between those referred and not-referred to funds is much smaller and insignificant. Next, there are no significant differences in post-treatment earnings for the high-earnings group, but there are for the low earnings group, where earnings appear to be higher following referral even five years later. The large gains in earnings two years after referral indicates a *positive* effect on earnings for this group.

These results appear consistent with a view that significant disemployment effects of housing assistance are limited to more generous programs. The study of housing vouchers in [Jacob and Ludwig \(2012\)](#) and the long-term subsidy arm of the Family Options Study ([Gubits et al., 2018](#)) both find that housing assistance reduces employment, but both of these studies focus on ostensibly permanent vouchers. More temporary interventions may have different effects. For example, the time-limited Community Based Rapid Re-Housing arm of Family Options increased employment, though only temporarily ([Gubits et al., 2018](#)). Our two-year result for the low earnings group is similar in sign and magnitude to these rapid re-housing estimates. Similarly, the dip in earnings observed around the time of the call is very close in magnitude to what is reported in [Collinson et al. \(2022\)](#) (accounting for the fact that our result uses annual data and theirs uses quarterly data), though they observe more persistent effects.

6.2. *Effects on Labor Supply*

Table 3 consolidates these treatment results using several different specifications. The first column, which reports estimates from equation (3), combines all post-treatment years into one sample, while including the controls X_{it} , Z_{it} and the pre-calling control $Y_{i,-1}$; the resulting coefficient is an aggregate treatment effect that combines the post-treatment event-study estimates in Figure 4. Standard errors are clustered by PIK, and each reported number of observations is rounded to preserve anonymity. Column 2, which reports estimates from equation (4), uses individual fixed effects and includes the full sample (i.e. all years)

of observations. The inclusion of fixed effects produces stronger results than the baseline estimates; here the overall sample estimate now indicates gains in earnings.

Column 3 uses inverse propensity weights; we estimate a logit model of the probability of referral on the full sample and use this regression to predict referral for each caller in the sample. We then weight both referred and non-referred observations by the inverse of the probability that the logit assigned to the caller’s true referral status.¹¹ Column 4 uses both fixed effects and inverse propensity weights. The final column repeats the baseline specification except that the year $t = -1$ earnings are omitted.

These various specifications consistently reject large decreases in earnings. In the overall sample, whether being referred to assistance increases earnings is sensitive to the specification. The significant positive effect when we omit a control for lagged earnings is consistent with some callers (as suggested by Figure 4.e) who have higher earnings in period -1 being more likely to see referral. This is also suggested by the negative weighted result for high-earnings callers in column 3, although this result becomes positive and insignificant when fixed effects are used in column 4. In this main specification, we can reject that earnings decrease by more than \$353, or 3% of average pre-call earnings. The fixed effect specification can reject any decline and the most demanding specification with weight and fixed effects can reject declines of \$239 (2%).

Any positive earnings effects we observe from referral are driven by low-earnings callers. For this group, the effects are positive for all specifications and suggest long-term increased earnings of \$400-\$500 from referral, consistent with Figure 4. This represents nearly 10% of the control group mean for low earners and about 55% of the total long-term effect of eviction on earnings in Collinson et al. (2022).¹² All of the estimates are at or near statistical significance (the p-value in the first column is about 0.104) and for all specifications we can

¹¹The controls used in the logit include earnings in year -1, earnings averaged across the pre-period, monthly income reported to the HPCC, and indicators for caller need amount category, year-quarter of call, need type, and the presence of missing data for any of the prior variables.

¹²Collinson et al. (2022) report quarterly effects of \$323 in the first year, \$613 in the second (Table 6), and \$217 in the third & fourth (Appendix table H6), for an average of \$342.5. Adjusting a \$450 ITT effect by a factor of 1.69 to make it TOT and dividing by 4 to make it quarterly produces \$191.1; $191/343 \approx .55$.

reject the hypothesis that being referred to funds leads to a decline in earnings, which is an important result.

Figure 5 explores heterogeneity by earning status further. The figure depicts two plots, each made up of 101 regressions. In each regression, the dependent variable is an indicator for whether an individual has earnings of at least x for varying amounts of x in increments of \$1,000, from \$0 to \$100,000. The top plot uses the main specification given in equation (3), while the bottom plot uses individual fixed effects as in equation (4). Both plots are negative to the left of the figure—suggesting that individuals who are referred are less likely to report very low levels of earnings. Both plots become positive around \$15,000, with the line approaching zero for values around \$40,000 and above. As with our prior results, the figure suggests negligible earnings effects for high-earnings referrals. But the figure depicts a shift in the earnings distribution at the lowest levels.

We next measure treatment effects on other labor market outcomes. Table 4 shows our main specification and individual-fixed-effect specification for a number of outcomes beyond earnings. The table presents first a set of three columns using our main specification on combined, low-earnings, and high-earnings samples. The last three columns use individual fixed effects. The top panel of Table 4 considers other indicators of labor supply. The first row of this panel repeats our main estimates from Table 3, and below that we report estimates for two indicators of employment: first, any employment, as measured by receiving any W-2 earnings. Next, we look at whether earnings on a W-2 are equal to or above what would be earned for half-time (20 hours a week) work at minimum wage for one quarter. For individuals with low earnings, the fixed-effect estimates suggest gains in both measures of employment, although the increase is not significant in the main specification. Notably, even the imprecise estimates here can *rule out* sizeable declines in employment, with confidence intervals generally excluding even a decline of 2 percentage points.¹³ This suggests that

¹³In a few instances (notably employment) the full-sample fixed-effect estimates differ from the average of the effects in the last two columns and are more different from the low-wage-sample estimates than one might expect. This is driven by a second-order effect in the full sample estimates caused by restricting all covariate coefficients to be the same for the two sub-samples. Relaxing this restriction for the year dummies

referral to funds does not have sizeable disemployment effects. It also suggests that the drop in earnings for individuals with high ex-ante earnings shown earlier in Figure 4 reflects an Ashenfelter dip, rather than a negative impact of referral on work effort. The last row of the top panel redoes the estimates using tax unit earnings (e.g., joint earnings for joint filers). This matters for understanding the joint labor supply decision of households (Cesarini et al., 2017). The results continue to suggest gains for the low-income group. The other estimates are insignificant and mostly positive. There is little evidence that emergency financial assistance substitutes for the earnings either of the caller or of other wage earners in the household.

6.3. *Effects on Other Outcomes*

Looking beyond labor market outcomes, Table 4 also reports the effects of financial assistance on housing stability, receipt of non-labor income, and other outcomes. We report these results for both our main specification and the individual-fixed-effect specification.

The first set of estimates are results for housing stability, which are of direct interest for a housing intervention. We measure housing stability by any new address, a new tract, a new county, or newly missing address information after referral. These measures are based on address information obtained mainly from tax forms (both 1040s and information returns). For people not filing or receiving tax forms, we incorporate address information from a variety of other administrative sources, such as data from the Department of Housing and Urban Development or the U.S. Postal Service. There is some suggestive evidence of an increase in address changes, but it is not robust across any sample or specification. Along with evidence from Phillips (2020), these results suggest that emergency financial assistance has limited effects on the total number of address changes. However, the welfare implications of address changes themselves may be positive or negative depending on the destination and the household’s desire to move, and other studies establish that emergency financial

generally returns full sample estimates close to the last two columns and in particular closer to the low wage estimates; we omit this ad-hoc specification adjustment from the table for brevity.

assistance reduce homelessness both in the context of the HPCC (Evans et al., 2016) and more generally (Phillips and Sullivan, 2023).

We can also use the data to measure household income and benefits more broadly. The next row reports results on Adjusted Gross Income. Relative to the baseline estimates on earnings, AGI both combines outcomes for multiple earners in the tax unit and includes non-wage sources of taxable income, such as unemployment compensation or any state tax credit (AGI will exclude cash assistance such as TANF, SSI, and child support payments). By comparing these results to the prior tax-unit estimates, one can observe how the AGI's broader income base affects the results. There is some suggestive evidence of larger effects, especially for the low earners, although the difference between the low- and high-earnings groups is not statistically significant. Also, both specifications return a positive and significant coefficient on the likelihood of filing a 1040 for low-earnings households.

The next two rows report results for effects on whether an individual receives a 1099-G (which captures local government payments, such as for unemployment compensation or state tax refunds) and 1099-MISC (which prior to 2020 included business payments to a non-employee). For low-earnings workers the estimates show an increase in the likelihood of receiving a 1099-G. The last rows look at SNAP participation using administrative data for the state of Illinois (SNAP receipt in other states is not observed). These estimates also suggest a positive effect for low-earnings workers. The overall sample results suggest a positive effect, but the result again is not robust across specifications. SNAP usage is high for both high- and low-earnings callers in the sample (averaging 80% overall). This, combined with the reasonably small standard errors in the table, again allows us to rule out large negative effects in SNAP receipt.

The 1099-G and SNAP results, like the AGI results, indicate stable or perhaps modestly increasing income from government payments for low-income households referred to financial assistance. A key issue of many low-income housing programs is to promote self-sufficiency (e.g., Gubits, 2015); one might wonder whether these results are evidence that HPCC refer-

ral decreases self-sufficiency. This interpretation seems inconsistent with the fact that the primary driver of the increase in AGI is from earnings, and this is driven by a group (initially low earners) that if anything appears to have gains in employment after referral.

An alternate story could involve unemployment compensation: individuals who are more likely to work are, mechanically, more likely to receive UI, and this compensation would lead to both higher AGI and receipt of form 1099-G. But this mechanical mechanism would not explain higher receipt of SNAP coupled with higher earnings, both of which are robustly documented for low-income workers.

Combined with null/positive employment effects and positive earnings effects, the results on AGI, 1040 filing, 1099-G, and SNAP are consistent with financial assistance leading to positive labor market outcomes and greater likelihood of accessing ancillary income (such as UI, refundable state tax credits, or SNAP) for low-earnings individuals. This latter effect could be driven by those referred to a delegate agency receiving information on programs like SNAP. But the main increase in income observed for this group comes from earnings. Moreover, none of these effects are observed for the ex-ante high-earnings group, although the estimates rule out large negative effects for this group. It is worth noting again that our use of the terms “low earnings” and “high earnings” are relative, as the sample overall is disadvantaged. But the strongest evidence of benefits in Tables 3 and 4 comes from effects of assistance on the poorest of the poor.

6.4. Heterogeneous Effects

Tables 5 and 6 report the results for several subgroups. Table 5 uses the main specification and Table 6 uses fixed effects. In both tables, the first three columns are for the full sample and the last three columns are just for the low-earnings sample. The dependent variables for each sample are earnings, AGI, and (any) employment. For each table, the first row shows the main result. The next two rows split the sample by reason for calling the HPCC—job loss versus any other reason. The last two rows break the result down by having kids or not

having kids, as reported to the call center at the time of the call.¹⁴

The full-sample estimates in the first three columns show little evidence of heterogeneity. AGI appears to increase and employment appears to decrease in several specifications, but neither result is observed consistently; the employment result switches sign when fixed effects are added. Moreover, the results consistently suggest earnings gains for the ex-ante low-earnings group. The main-specification estimates suggest that these gains may be largest for those who lost their job and for those without kids. However, these heterogeneous effects are less apparent with the fixed-effects estimates. The results with AGI are less conclusive, but this is in part from the increase in the standard errors. For example, the estimate for the no-kids sub-sample has 2-3 times larger standard errors for estimates for a sub-sample about half the size of the original sample. In summary, the results continue to indicate earnings gains for those with initially low earnings, with some evidence for heterogeneity by presence of children and reason for calling.

7. Conclusion

This study finds that a temporary financial transfer aimed at preventing homelessness has minimal disemployment effects and in fact facilitates increased earnings for people with the lowest baseline income. We use federal tax records to measure earnings for people who request assistance from a call center after they have experienced a negative shock that puts their housing at risk. Due to external constraints on funding and staffing, the call center refers some callers to temporary financial assistance but not others. We compare these two groups and find that average earnings do not differ statistically between callers referred to assistance versus those not referred to assistance, conditional on observable factors that affect eligibility for funding. We can reject that earnings fall by more than 3% of the baseline mean. The full-sample point estimate is positive, and we find evidence that temporary financial assistance

¹⁴We have considered many other dimensions of heterogeneity including marital status, gender, age, receipt of benefits, whether it is a first-time call, among others. These results do not generate clear differences in the effect of assistance across groups.

encourages work among the lowest-income participants, increasing earnings by \$400-\$500 among people with earnings below the sample median at baseline.

In principle, these results could be specific to a particular income measure or empirical specification, but in practice, they do not vary much as those things change. Other measures of income and work, such as adjusted gross income or receipt of certain 1099 forms, does not respond noticeably to the offer of financial assistance. Results that account for conditional treatment assignment using linear controls, individual fixed effects, and inverse propensity score weights all yield similar qualitative results.

Our results imply that existing analyses of homelessness prevention programs underestimate their benefits. Existing studies ([Rolston et al., 2013](#); [Evans et al., 2016](#); [Phillips and Sullivan, 2023](#)) demonstrate that such programs stabilize housing. While studies of other housing subsidies might lead to concerns of disemployment effects ([Jacob and Ludwig, 2012](#)), our results suggest any disemployment effect is small for temporary assistance. In fact, our results suggest that temporary assistance encourages employment for many beneficiaries. For this group, the present value of increased earnings far exceeds the cost of the program. As a result, existing cost-benefit analyses ([Phillips and Sullivan, 2023](#)) that value the private benefits of financial assistance at the payment amount are conservative.

More generally, our results suggest that many low-income households are underinsured against shocks to their income. If future labor market earnings can pay for the cost of temporary financial assistance, then households likely face barriers to credit and insurance that would otherwise allow them to move resources across time or states of the world. Some have argued that housing is a necessary platform for stable employment, and temporary disruptions to housing can permanently shift people into poverty ([Desmond, 2016](#)). Our results suggest that, to the extent that this is true, providing insurance against temporary shocks can help to undo such a poverty trap.

References

211.org, 2023. Help paying bills.

Akee, R. K., Copeland, W. E., Keeler, G., Angold, A., Costello, E. J., 2010. Parents’ incomes and children’s outcomes: a quasi-experiment using transfer payments from casino profits. *American Economic Journal: Applied Economics* 2, 86–115.

American Rescue Plan Act, 2021. U.S. House of Representatives. 117th Congress, 1st Session.

CARES Act, 2020. 15 U.S.C. § 9001.

Cesarini, D., Lindqvist, E., Notowidigdo, M. J., Östling, R., 2017. The effect of wealth on individual and household labor supply: evidence from Swedish lotteries. *American Economic Review* 107, 3917–3946.

Collinson, R., Humphries, J. E., Mader, N. S., Reed, D. K., Tannenbaum, D. I., Van Dijk, W., 2022. Eviction and poverty in American cities. Tech. rep., National Bureau of Economic Research.

Consolidated Appropriations Act, 2020. U.S. House of Representatives. 116th Congress, 2nd Session.

Corinth, K., Meyer, B. D., Stadnicki, M., Wu, D., 2021. The anti-poverty, targeting, and labor supply effects of replacing a child tax credit with a child allowance. Tech. rep., National Bureau of Economic Research.

Deshpande, M., 2016. The effect of disability payments on household earnings and income: Evidence from the SSI children’s program. *Review of Economics and Statistics* 98, 638–654.

Desmond, M., 2016. *Evicted: Poverty and profit in the American city*. Crown.

Downes, H., Phillips, D. C., Sullivan, J. X., 2022. The effect of emergency financial assistance on healthcare use. *Journal of Public Economics* 208, 104626.

Duggan, M., Kearney, M. S., Rennane, S., 2015. The Supplemental Security Income program. In: *Economics of Means-Tested Transfer Programs in the United States, Volume 2*, University of Chicago Press, pp. 1–58.

Enriquez, B., Jones, D., Tedeschi, E., 2023. Short-term labor supply response to the expanded Child Tax Credit. In: *AEA Papers and Proceedings*, American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203, vol. 113, pp. 401–405.

Evans, W. N., Sullivan, J. X., Wallskog, M., 2016. The impact of homelessness prevention programs on homelessness. *Science* 353, 694–699.

Federal Communications Commission, 2023. Dial 211 for essential community services.

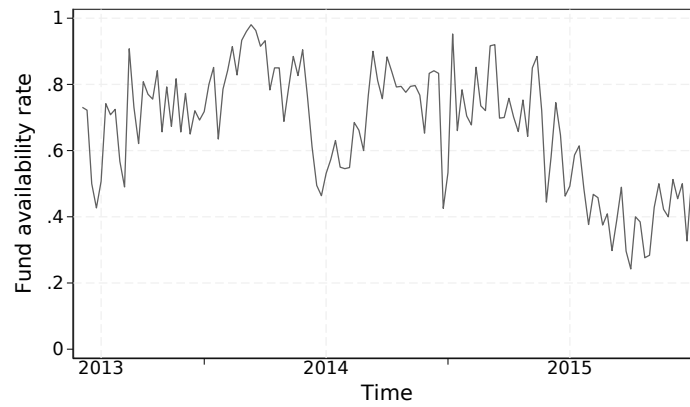
- George, C., Hilvers, J., Patel, K., Guelespe, D., 2011. Evaluation of the Homelessness Prevention Call Center. Loyola U Chicago Center for Urban Research and Learning (CURL) Report .
- Gruber, J., 1997. The consumption smoothing benefits of unemployment insurance. *American Economic Review* 87, 192.
- Gruber, J., 2000. Cash welfare as a consumption smoothing mechanism for divorced mothers. *Journal of Public Economics* 75, 157–182.
- Gubits, D., Shinn, M., Wood, M., Brown, S. R., Dastrup, S. R., Bell, S. H., 2018. What interventions work best for families who experience homelessness? Impact estimates from the family options study. *Journal of Policy Analysis and Management* 37, 835–866.
- Han, J., 2022. The impact of SNAP work requirements on labor supply. *Labour Economics* 74, 102089.
- Hoynes, H., Rothstein, J., 2019. Universal basic income in the United States and advanced countries. *Annual Review of Economics* 11, 929–958.
- Hoynes, H., Schanzenbach, D. W., 2015. US food and nutrition programs. In: *Economics of means-tested transfer programs in the United States, volume 1*, University of Chicago Press, pp. 219–301.
- HPCC, 2013. Homelessness Prevention Call Center Script Guidelines. Homelessness Prevention Call Center Document .
- Imbens, G. W., Rubin, D. B., Sacerdote, B. I., 2001. Estimating the effect of unearned income on labor earnings, savings, and consumption: Evidence from a survey of lottery players. *American Economic Review* 91, 778–794.
- Jacob, B. A., Ludwig, J., 2012. The effects of housing assistance on labor supply: Evidence from a voucher lottery. *American Economic Review* 102, 272–304.
- Jaroszewicz, A., Jachimowicz, J., Hauser, O., Jamison, J., 2022. How effective is (more) money? Randomizing unconditional cash transfer amounts in the US. Unpublished working paper .
- Jones, D., Marinescu, I., 2022. The labor market impacts of universal and permanent cash transfers: Evidence from the Alaska Permanent Fund. *American Economic Journal: Economic Policy* 14, 315–340.
- Layne, M., Wagner, D., Rothhaas, C., 2014. Estimating Record Linkage False Match Rate for the Person Identification Validation System, cARRA Working Paper No. 2014-02.
- Mulrow, E., Mushtaq, A., Pramanik, S., Fontes, A., 2011. Assessment of the U.S. Census Bureau’s Person Identification Validation System. Tech. rep., NORC at the University of Chicago.

- Nichols, A., Rothstein, J., 2015. The Earned Income Tax Credit. In: *Economics of Means-Tested Transfer Programs in the United States, Volume 1*, University of Chicago Press, pp. 137–218.
- Olivet, J., 2023. Keynote Remarks to the HUD Office of Policy Development and Research Quarterly Updated on Housing First.
- Palmer, C., Phillips, D. C., Sullivan, J. X., 2019. Does emergency financial assistance reduce crime? *Journal of Public Economics* 169, 34–51.
- Phillips, D. C., 2020. Measuring housing stability with consumer reference data. *Demography* 57, 1323–1344.
- Phillips, D. C., Sullivan, J. X., 2023. Do homelessness prevention programs prevent homelessness? Evidence from a randomized controlled trial. *The Review of Economics and Statistics* pp. 1–30.
- Pilkaukas, N. V., Jacob, B. A., Rhodes, E., Richard, K., Shaefer, H. L., 2023. The COVID cash transfer study: The impacts of a one-time unconditional cash transfer on the well-being of families receiving SNAP in twelve states. *Journal of Policy Analysis and Management* 42, 771–795.
- Price, D. J., Song, J., 2018. The long-term effects of cash assistance. *Industrial Relations Section working paper* 621.
- Robins, P. K., 1985. A comparison of the labor supply findings from the four negative income tax experiments. *Journal of Human Resources* pp. 567–582.
- Rolston, H., Geyer, J., Locke, G., Metraux, S., Treglia, D., 2013. Evaluation of the Homebase Community Prevention Program. Final Report, Abt Associates Inc, June 6, 2013.
- Wagner, D., Layne, M., 2014. The Person Identification Validation System (PVS): Applying the Center for Administrative Records Research and Applications’ (CARRA) Record Linkage Software, cARRA Working Paper No. 2014-01.
- Whitmore Schanzenbach, D., Strain, M. R., 2021. Employment effects of the Earned Income Tax Credit: Taking the long view. *Tax policy and the economy* 35, 87–129.
- Ziliak, J. P., 2015. Temporary Assistance for Needy Families. In: *Economics of Means-Tested Transfer Programs in the United States, Volume 1*, University of Chicago Press, pp. 303–393.

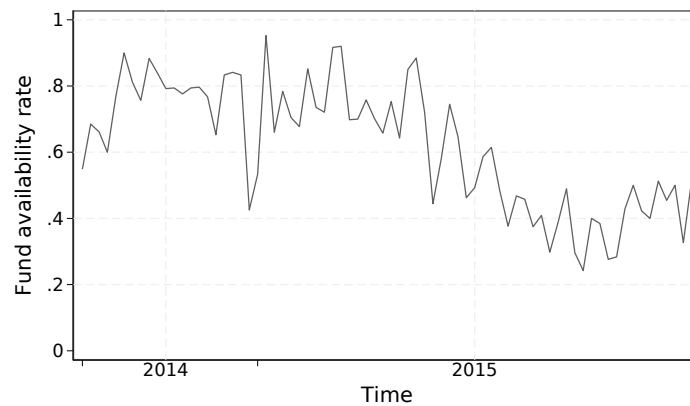
Tables and Figures

Fig. 1. Funding Rate by Week

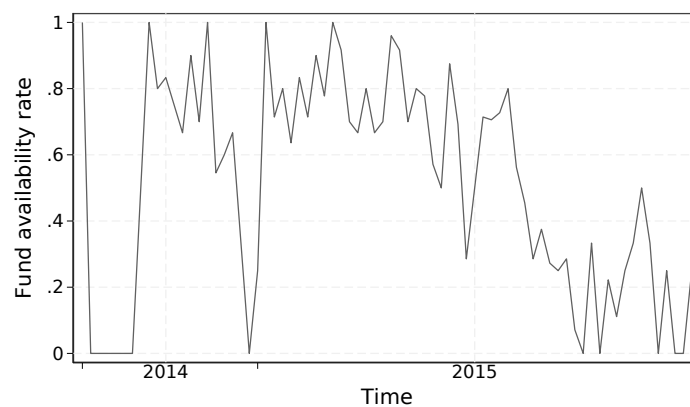
(a) Longer Sample - All



(b) Shorter Sample - All

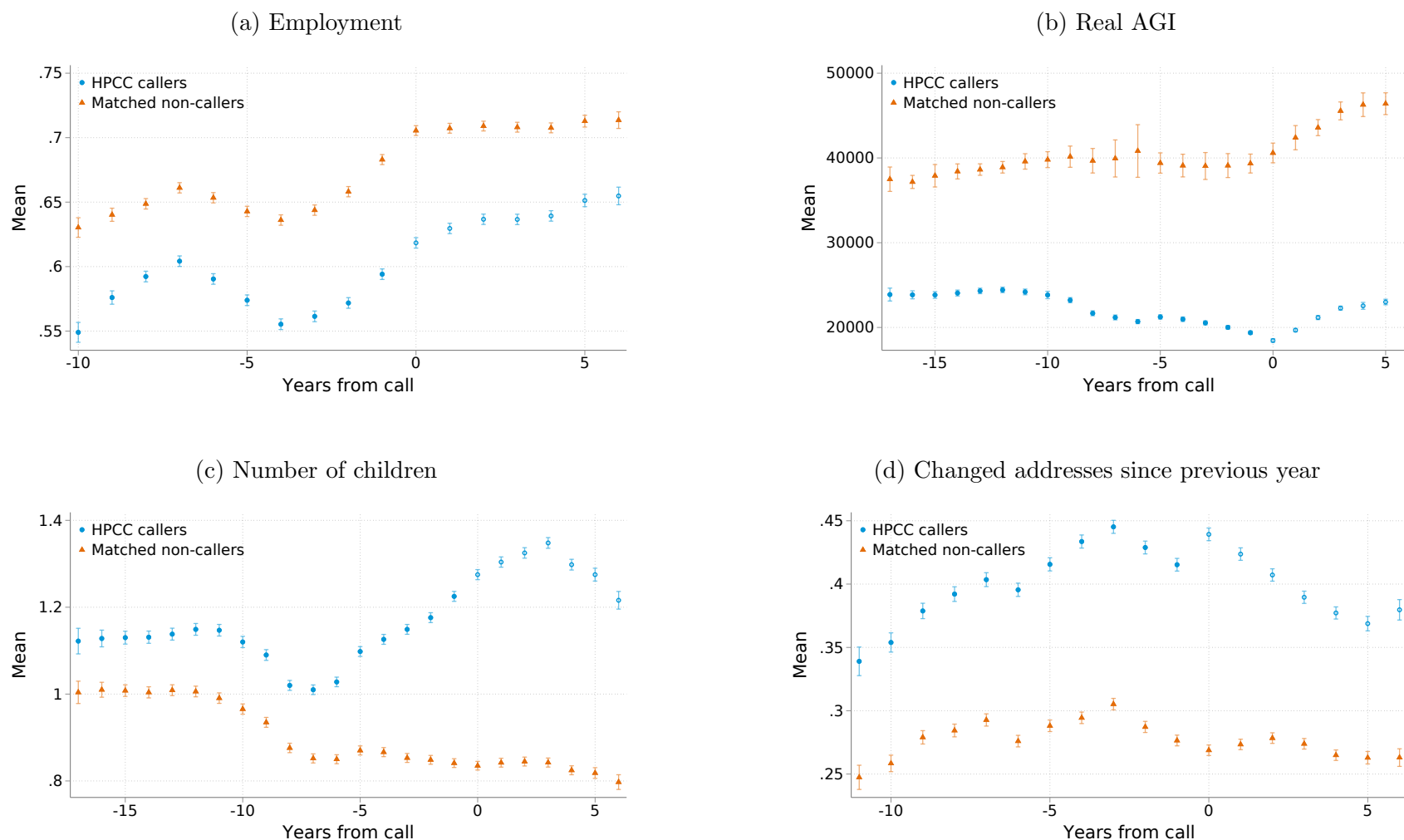


(c) Shorter Sample - Needs rent, need ≥ 1 month rent, need $> \$900$, no other housing subsidy



Source: Homelessness Prevention Call Center data and authors' calculations.
Note: Figure shows availability of funding by week for three samples of interest. Panel (a) is based on the sample of all callers between mid-2013 and the end of 2015. Panel (b) is based on callers from August 2014 through December 2015, the period for which the data contain complete information on the reason for and amount of callers' needs. Panel (c) is based on the same period as panel (b) but limits the sample to callers with the characteristics indicated in the figure title.

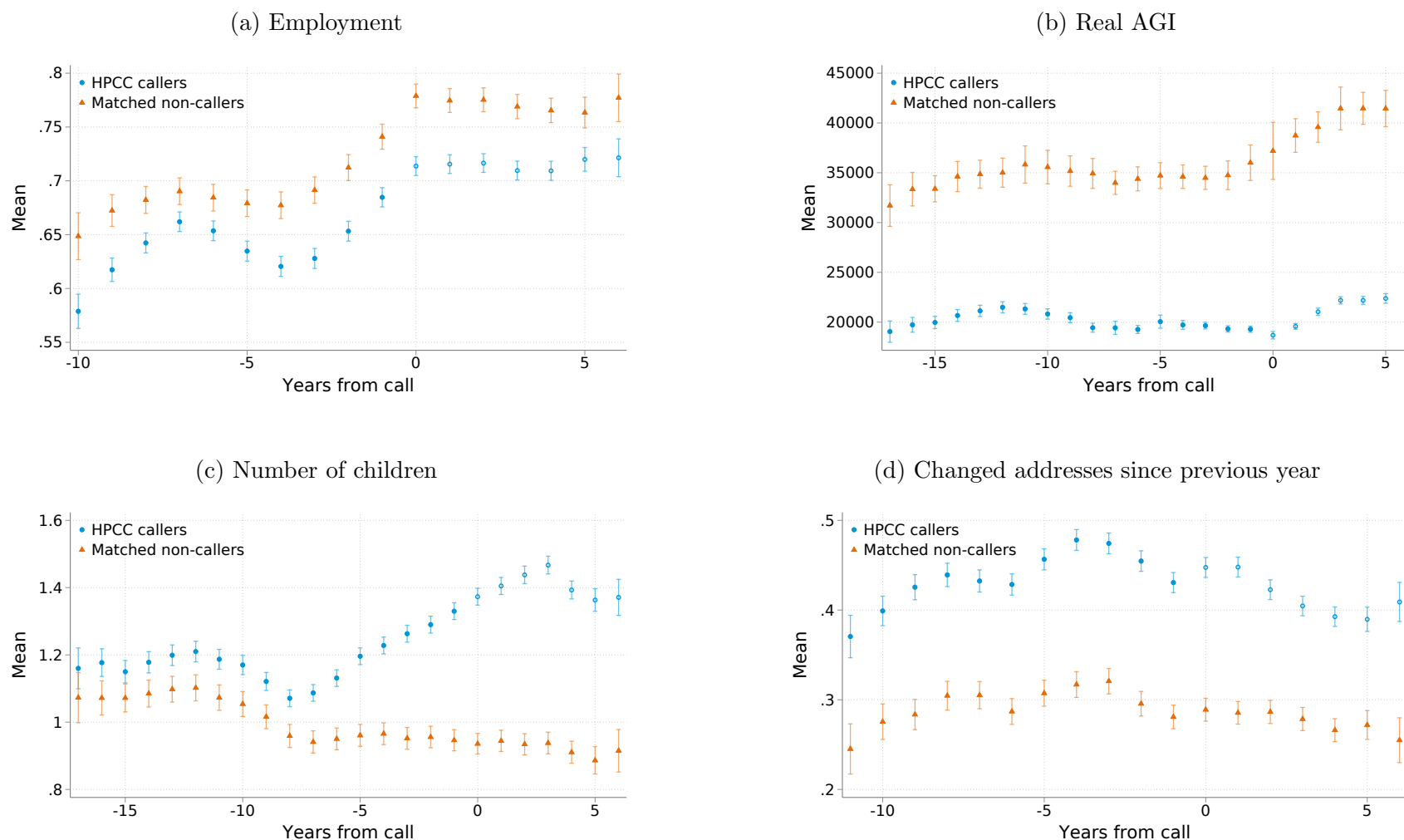
Fig. 2. Selection into Calling, All Callers



Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: Figure plots average outcomes for all callers to the Homelessness Prevention Call Center between mid-2013 and 2015 and matched non-callers. Callers are matched to non-callers based on demographic characteristics and place of residence in the year of the call, with matches drawn from the same Census block as callers where possible. Bars extending from plotted points represent 95 percent confidence intervals. Release authorization CBDRB-FY24-0033.

Fig. 3. Selection into Calling, Full Analysis Sample



Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

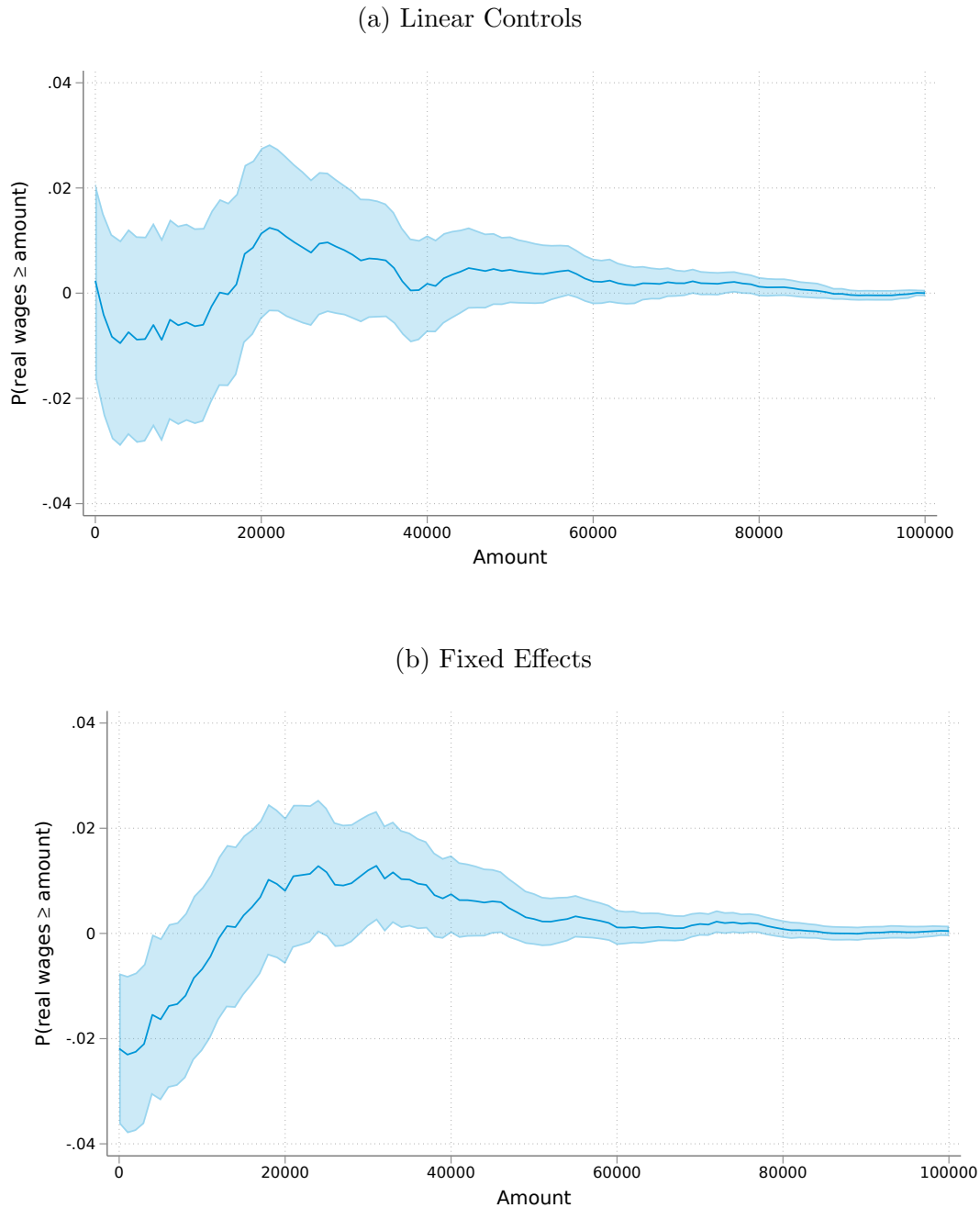
Note: Figure plots average outcomes for all callers in our main analysis sample and 2015 and matched non-callers. Callers are matched to non-callers based on demographic characteristics and place of residence in the year of the call, with matches drawn from the same Census block as callers where possible. Bars extending from plotted points represent 95 percent confidence intervals. Release authorization CBDRB-FY24-0033.

Fig. 4. Wages, Trends in Means and Event Studies



Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations. Note: Figure presents sample means and event study estimates comparing callers referred to funds and callers not referred. Event study estimates are based on equation (3). Shaded regions represent 95 percent confidence intervals. Release authorization CBDRB-FY23-0267.

Fig. 5. Treatment Effects Across the Income Distribution



Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: Figures report estimates of the effect of being referred to funds by the Homelessness Prevention Call Center on the probability of having wage and salary income at least great as various amounts between \$0 and \$100,000. Shaded regions represent 95 percent confidence intervals. Release authorization CBDRB-FY23-0267 and CBDRB-FY24-0033.

Table 1: Sample Composition under Various Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
	Neighbors	Callers	Eligible Callers	Eligible Callers full sample	Eligible Callers low-wage sample	Matched non-callers low-wage sample
Age	43.42 (0.0135)	40.16 (0.0598)	38.41 (0.0864)	38 (0.124)	38.82 (0.226)	40.21 (0.231)
Male	0.466 (0.000356)	0.221 (0.00176)	0.187 (0.00265)	0.188 (0.00384)	0.216 (0.00646)	0.221 (0.00652)
Citizen	0.796 (0.000287)	0.960 (0.000831)	0.975 (0.00107)	0.972 (0.00163)	0.974 (0.00249)	0.965 (0.00289)
Foreign-born	0.171 (0.000269)	0.0336 (0.000765)	0.0209 (0.000972)	0.0248 (0.00153)	0.0252 (0.00246)	0.0254 (0.00248)
White	0.199 (0.000285)	0.0618 (0.00102)	0.0458 (0.00142)	0.0433 (0.00200)	0.0479 (0.00336)	0.0383 (0.00302)
Black	0.521 (0.000356)	0.729 (0.00189)	0.764 (0.00289)	0.770 (0.00413)	0.749 (0.00681)	0.770 (0.00661)
Asian	0.0343 (0.000130)	0.00274 (0.000222)	0.00199 (0.000303)	0.00202 (0.000441)	0.000988 (0.000494)	0.00173 (0.000653)
Hispanic	0.134 (0.000243)	0.0956 (0.00125)	0.0767 (0.00181)	0.0795 (0.00266)	0.0785 (0.00423)	0.0837 (0.00435)
Other race	0.0152 (8.72e-05)	0.0166 (0.000543)	0.0176 (0.000894)	0.0162 (0.00124)	0.0156 (0.00195)	0.0166 (0.00201)
Observations	1,966,000	55,500	21,500	10,500	4,000	4,000

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: Table reports average demographic characteristics of callers from 2013-2015 in various samples, their neighbors (people living in the same set of Census blocks), and a sample of matched non-callers. Age is in years. Other rows report the share of callers having the indicated demographic characteristic. Standard errors are in parentheses. Release authorization CBDRB-FY24-0033.

Table 2: Select Baseline Characteristics, by Referral Status

	Referred to Funds	Not Referred	Difference	Adjusted Difference
Number of adults	1.39	1.45	-0.059*** (0.017)	-0.049** (0.022)
Number of minors	1.30	1.53	-0.23*** (0.035)	-0.16*** (0.046)
Receives housing subsidy	0.19	0.58	-0.40*** (0.011)	-1.1e-18 (7.4e-18)
Monthly rent	704.8	456.9	247.9*** (7.91)	-20.1*** (6.74)
Net dollars owed	935.8	981.2	-45.4* (25.1)	75.7*** (23.3)
Monthly income (thousands)	1.37	1.11	0.26*** (0.016)	-0.057*** (0.017)
Nbd median income	39760	38970	795.2*** (297.3)	1185*** (410.1)
Nbd pct Black	0.64	0.67	-0.021*** (0.0077)	-0.021** (0.011)
Age	38.6	40.6	-1.95*** (0.30)	0.59* (0.34)
Female	0.79	0.84	-0.052*** (0.0089)	-0.028** (0.012)
Black	0.87	0.92	-0.043*** (0.0069)	-0.025*** (0.0094)
Rents home	0.86	0.84	0.019** (0.0083)	0.037*** (0.010)
Pre-call average wages	11510	7394	4112*** (254.9)	1416*** (340.7)
Pre-call average AGI	20290	16740	3551*** (394.8)	2042** (999.9)
Predicted wages, 2 years post call	13320	10420	2900*** (151.4)	-20.6 (157.2)
Predicted AGI, 2 years post call	20260	18570	1687*** (101.2)	109 (106.3)

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: The sample consists of 5300 callers referred to funds and 2800 callers not referred to funds. Sample sizes are rounded in compliance with disclosure restrictions. The sample is restricted to eligible callers who have not called in the prior six months with non-missing characteristic information. The first two columns present means for each group. The third column reports the difference in means, and the last column reports this difference after controlling for a vector of caller characteristics Z_i related to fund eligibility; robust standard errors in parentheses. Statistical significance at the 10, 5, and 1% levels are denoted by *, **, and ***, respectively. The full set of baseline characteristics considered is reported in Table A.2. Release authorization CBDRB-FY23-0267.

Table 3: Effects on Earnings

	Main	Individual Fixed Effects	Weighted	Fixed Effects and Weighted	No Control for Lag Wages
Full Sample	197.4 (280.8)	574.3** (252.2)	-358.9 (380.1)	851.3 (556.5)	871** (358)
Call Center Controls	Yes	No	Yes	No	Yes
Lagged Outcome	Yes	No	Yes	No	No
Fixed Effects	No	Yes	No	Yes	No
Control Mean	10380	10380	10380	10380	10380
N	40500	81500	40500	81500	40500
Low Wage	430.6 (265.1)	1410*** (232.6)	460.5* (267.7)	669.3** (285.5)	483.9* (292.2)
Call Center Controls	Yes	No	Yes	No	Yes
Lagged Outcome	Yes	No	Yes	No	No
Fixed Effects	No	Yes	No	Yes	No
Control Mean	4944	4944	4944	4944	4944
N	20000	40500	20000	40500	20000
High Wage	49.78 (510.2)	310.5 (481.3)	-1147* (649.3)	1106 (1061)	803.2 (579.7)
Call Center Controls	Yes	No	Yes	No	Yes
Lagged Outcome	Yes	No	Yes	No	No
Fixed Effects	No	Yes	No	Yes	No
Control Mean	18140	18140	18140	18140	18140
N	20500	41000	20500	41000	20500

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: Table reports effects of being referred to funds by the Homelessness Prevention Call Center on earnings for various samples and specifications. The main specification includes controls for various characteristics of callers, as well as a measure of earnings from the year before a person called, and is estimated using data from the four years following each call. The fixed effects specifications include individual, calendar year, age, and relative year fixed effects and are estimated using data from five years prior through four years after each call. Weighted specifications use inverse propensity score weights to further adjust for observable differences between callers who are referred and not referred to funds. Statistical significance at the 10, 5, and 1% levels are denoted by *, **, and ***, respectively. Release authorization CBDRB-FY24-0033.

Table 4: Effect of Referral to Funds on Other Outcomes

	Ctrl Mean Full Sample	Main Specification			Fixed Effects Specification		
		Full Sample	Low Wage	High Wage	Full Sample	Low Wage	High Wage
Individual Wages	10380	197.4 (280.8)	430.6 (265.1)	49.78 (510.2)	574.3** (252.2)	1410*** (232.6)	310.5 (481.3)
Employed	.6401	.007568 (.008696)	.007342 (.01282)	.008567 (.01046)	-.01341* (.007906)	.02053* (.01159)	.01412 (.009421)
Employed > Half Time Min Wage	.4278	-.0003555 (.009743)	.002892 (.01249)	-.00284 (.01435)	-.0003092 (.008667)	.05109*** (.01071)	.001784 (.01372)
Tax Unit Wages	15660	344 (376.3)	609.6 (444.1)	240.3 (581.7)	25.02 (344.8)	909.6** (440.4)	-182.2 (497.8)
<i>N</i>	8800	27000	11000	16000	54000	21000	33500
Address Change - Any	.4013	-.0000782 (.00795)	.0008338 (.01056)	-.002616 (.01206)	.01547* (.008476)	.01358 (.01169)	.009399 (.01284)
Address Change - Tract	.3223	-.00223 (.007505)	-.003053 (.01022)	-.001216 (.01111)	.01698** (.008134)	.0104 (.01139)	.01169 (.01197)
Address Change - County	.1046	.002086 (.005481)	.004767 (.007972)	.001499 (.007254)	.02019*** (.006004)	.01504 (.009227)	.01129 (.007547)
Address Change - Missing	.1049	.003789 (.006344)	.01268 (.01005)	-.00336 (.006852)	.01362** (.006509)	.01771* (.0107)	.001788 (.007026)
<i>N</i>	14000	40000	20000	20500	72500	36000	36500
Adj Gross Income	19130	714.2* (369.2)	811.3** (395.8)	692.9 (578.7)	96.25 (313.6)	1155*** (433.6)	-412.6 (448.6)
Filed 1040	.6206	.01157 (.008368)	.02198* (.01148)	.002456 (.01203)	-.007525 (.007854)	.02317** (.01133)	-.005436 (.01077)
Received 1099-Misc	.09356	-.001634 (.006081)	-.0009758 (.007773)	.0000881 (.00958)	.006595 (.005727)	.003293 (.007564)	-.001414 (.009014)
Received 1099-G	.4127	.01525 (.009418)	.0225* (.01179)	.0023 (.01455)	.01344 (.009057)	.04411*** (.01154)	.02249 (.01428)
Any SNAP	.8042	.003753 (.008226)	.01684* (.009825)	-.007386 (.01379)	.009106 (.007948)	.02101** (.009795)	-.00874 (.01323)
Months Receiving SNAP	7.332	.1158 (.0817)	.2109** (.1029)	.02996 (.1298)	.2859*** (.08631)	.302*** (.1127)	.0969 (.1377)
SNAP Amount	2753	36.82 (39.57)	37.04 (54.13)	24.07 (57.23)	161.6*** (47.82)	110.3* (66.91)	119* (71.78)
<i>N</i>	12500	37500	18500	19000	76500	38500	38500

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Illinois SNAP administrative data, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: Other than the first column, which reports mean outcomes for non-referred callers, the table reports the effect of being referred to funds by the Homelessness Prevention Call Center on assorted outcomes for various samples and OLS specifications. The main specification includes controls for various characteristics of callers, as well as a measure of earnings from the year before a person called. The fixed effects specifications include individual, calendar year, age, and relative year fixed effects. Statistical significance at the 10, 5, and 1% levels are denoted by *, **, and ***, respectively. Release authorization CBDRB-FY24-0033.

Table 5: Heterogeneous Effects, Linear Controls

	Full Sample			Low Wage		
	Wages	AGI	Employed	Wages	AGI	Employed
Full Sample	197.4 (280.8)	714.2* (369.2)	0.00757 (0.00870)	430.6 (265.1)	811.3** (395.8)	0.00734 (0.0128)
Lost Job	1257 (912)	2818*** (1094)	-0.00894 (0.0216)	1970** (936.1)	1643 (1338)	-0.00687 (0.0366)
Other Shock	-160.4 (424.4)	609.5 (565.5)	0.0174 (0.0143)	163.5 (398.6)	631.9 (531.9)	0.0274 (0.0206)
Has Kids	63.49 (361.1)	505.1 (382)	0.00174 (0.0106)	319.7 (354)	280.7 (342.6)	-0.00104 (0.0168)
No Kids	570.4 (440.2)	1733* (984.9)	0.0179 (0.0149)	758.8* (400.7)	1705 (1526)	0.0261 (0.0198)

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: Table reports effects of being referred to funds by the Homelessness Prevention Call Center on income and employment outcomes for various samples using the specification described in equation (3). Statistical significance at the 10, 5, and 1% levels are denoted by *, **, and ***, respectively. Release authorization CBDRB-FY24-0033.

Table 6: Heterogeneous Effects, Fixed Effects

	Full Sample			Low Wage		
	Wages	AGI	Employed	Wages	AGI	Employed
Full Sample	574.3** (252.2)	96.25 (313.6)	-0.0134* (0.00791)	1410*** (232.6)	1155*** (433.6)	0.0205* (0.0116)
Lost Job	-314.9 (706.6)	658 (835.3)	-0.0432** (0.0203)	1053 (699.2)	2338 (1660)	-0.0413 (0.0334)
Other Shock	465.1 (401.9)	-101.5 (479.3)	-0.00428 (0.0124)	1256*** (359.8)	556.3 (541.4)	0.0411** (0.0177)
Has Kids	541.3* (302)	270.4 (296.5)	-0.0144 (0.00970)	1160*** (290)	781.8** (323.8)	0.0131 (0.0148)
No Kids	1052** (450.3)	104.7 (942.4)	0.00275 (0.0134)	1983*** (381.8)	2369 (1702)	0.0425** (0.0179)

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: Table reports effects of being referred to funds by the Homelessness Prevention Call Center on income and employment outcomes for various samples using the specification described in equation (4). Statistical significance at the 10, 5, and 1% levels are denoted by *, **, and ***, respectively. Release authorization CBDRB-FY24-0033.

Appendix A. Additional Tables

Table A.1: Representativeness of the Analysis Sample

	Has PIK	No PIK	Dif.	Adj. Dif.
Number of adults	1.41	1.54	-0.13*** (0.046)	-0.093** (0.045)
Number of minors	1.38	1.44	-0.066 (0.075)	-0.11 (0.073)
Monthly rent	618.2	705.4	-87.1*** (18.7)	-18.4* (11.0)
Net dollars owed	951.6	998.2	-46.6 (31.8)	28.6 (20.3)
Monthly income (thousands)	1.28	1.44	-0.15*** (0.037)	-0.069** (0.030)
Veteran	0.016	0.025	-0.0088 (0.0078)	6.2e-18** (2.6e-18)
Senior	0.038	0.025	0.013 (0.0080)	-3.3e-18 (2.1e-18)
Nbd median income	39480	36970	2511*** (848.3)	2377*** (832.2)
Nbd pct Black	0.65	0.53	0.12*** (0.019)	0.12*** (0.020)
Age	39.3	39.2	0.14 (0.64)	-0.034 (0.57)
Reason missing	0.44	0.47	-0.033 (0.025)	-0.0070 (0.0051)
Reason: lost job	0.17	0.18	-0.0062 (0.020)	-0.0098 (0.017)
Reason: job cut hours	0.062	0.044	0.018* (0.011)	0.015 (0.011)
Reason: medical work absence	0.058	0.057	0.00081 (0.012)	0.0030 (0.012)
Reason: health	0.023	0.022	0.00077 (0.0075)	-0.00098 (0.0074)
Reason: housing change	0.13	0.10	0.028* (0.015)	0.018 (0.013)
Reason: lost benefits	0.055	0.049	0.0055 (0.011)	-0.00039 (0.011)
Reason: other	0.060	0.074	-0.014 (0.013)	-0.018 (0.013)
Female	0.81	0.81	-0.0037 (0.020)	-0.021 (0.020)
Hispanic	0.075	0.21	-0.13*** (0.020)	-0.13*** (0.020)
White	0.074	0.14	-0.064*** (0.017)	-0.060*** (0.017)
Black	0.89	0.77	0.12*** (0.021)	0.11*** (0.021)
Other	0.032	0.074	-0.043*** (0.013)	-0.040*** (0.013)
Receives housing subsidy	0.33	0.20	0.12*** (0.021)	-2.6e-17 (1.7e-17)
Receiving SNAP	0.50	0.40	0.10*** (0.025)	0.087*** (0.020)
Receives income from disability	0.047	0.035	0.012 (0.0094)	-2.4e-17*** (7.1e-18)
Receiving child support	0.022	0.030	-0.0074 (0.0086)	-0.0056 (0.0087)
Receiving EITC	0.26	0.33	-0.075*** (0.024)	-0.030** (0.015)
Receiving disability	0.047	0.035	0.012 (0.0094)	-2.4e-17*** (7.1e-18)
Receiving SSI	0.067	0.079	-0.012 (0.014)	-0.014 (0.013)
Receiving TANF	0.037	0.012	0.024*** (0.0059)	0.022*** (0.0063)
Receiving UI	0.034	0.025	0.0097 (0.0080)	0.013 (0.0083)
Owns home	0.018	0.030	-0.012 (0.0086)	-3.3e-18 (4.0e-18)
Lives with family or friends	0.13	0.13	0.00053 (0.017)	0.0011 (0.016)
Rents home	0.86	0.84	0.011 (0.018)	-0.0011 (0.016)

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: The table compares callers who do and do not have a PIK (protected identification key) from the US Census Bureau. The sample includes about 8200 individuals with a PIK and about 400 individuals without a PIK. Sample sizes are rounded in compliance with disclosure restrictions. The first two columns present means for each group. The third column reports the difference in means, estimated via linear regression; *, **, and *** denote significance at the 10%, 5% and 1% levels. The last column reports this difference after controlling for a vector of caller characteristics Z_i related to fund eligibility. Robust standard errors in parentheses. Release authorization CBDRB-FY23-0267.

Table A.2: Baseline Characteristics, by Referral Status

	Referred to Funds	Not Referred	Difference	Adjusted Difference
Number of adults	1.39	1.45	-0.059*** (0.017)	-0.049** (0.022)
Number of minors	1.30	1.53	-0.23*** (0.035)	-0.16*** (0.046)
Monthly rent	704.8	456.9	247.9*** (7.91)	-20.1*** (6.74)
Net dollars owed	935.8	981.2	-45.4* (25.1)	75.7*** (23.3)
Monthly income (thousands)	1.37	1.11	0.26*** (0.016)	-0.057*** (0.017)
Veteran	0.015	0.018	-0.0036 (0.0030)	-1.7e-18 (2.2e-18)
Senior	0.027	0.058	-0.031*** (0.0049)	-4.1e-18*** (7.3e-19)
Nbd median income	39760	38970	795.2*** (297.3)	1185*** (410.1)
Nbd pct Black	0.64	0.67	-0.021*** (0.0077)	-0.021** (0.011)
Age	38.6	40.6	-1.95*** (0.30)	0.59* (0.34)
Reason missing	0.47	0.37	0.10*** (0.011)	-0.0058*** (0.0020)
Reason: lost job	0.18	0.16	0.020** (0.0087)	0.011 (0.010)
Reason: job cut hours	0.068	0.051	0.017*** (0.0054)	-0.0069 (0.0067)
Reason: medical work absence	0.065	0.044	0.022*** (0.0051)	0.0082 (0.0063)
Reason: health	0.017	0.034	-0.018*** (0.0038)	-0.0026 (0.0048)
Reason: housing change	0.096	0.19	-0.095*** (0.0084)	0.016* (0.0093)
Reason: lost benefits	0.053	0.059	-0.0062 (0.0054)	-0.00084 (0.0063)
Reason: other	0.045	0.090	-0.045*** (0.0061)	-0.019*** (0.0066)
Female	0.79	0.84	-0.052*** (0.0089)	-0.028** (0.012)
Hispanic	0.084	0.058	0.026*** (0.0058)	0.018** (0.0076)
White	0.085	0.053	0.032***	0.012

			(0.0057)	(0.0080)
Black	0.87	0.92	-0.043***	-0.025***
			(0.0069)	(0.0094)
Other	0.035	0.026	0.0086**	0.010**
			(0.0039)	(0.0052)
Receives housing subsidy	0.19	0.58	-0.40***	-1.1e-18
			(0.011)	(7.4e-18)
Receiving SNAP	0.54	0.42	0.13***	-0.018*
			(0.011)	(0.010)
Receives income from disability	0.045	0.051	-0.0062	1.2e-18
			(0.0050)	(3.2e-18)
Receiving child support	0.024	0.019	0.0051	0.0017
			(0.0033)	(0.0045)
Receiving EITC	0.31	0.16	0.15***	0.026***
			(0.0093)	(0.0097)
Receiving disability	0.045	0.051	-0.0062	1.2e-18
			(0.0050)	(3.2e-18)
Receiving SSI	0.064	0.071	-0.0069	-0.020**
			(0.0059)	(0.0080)
Receiving TANF	0.035	0.040	-0.0051	-0.0048
			(0.0045)	(0.0065)
Receiving UI	0.039	0.025	0.014***	0.0057
			(0.0040)	(0.0056)
Owns home	0.018	0.018	-0.000071	2.7e-19
			(0.0031)	(1.8e-18)
Lives with family or friends	0.12	0.14	-0.019**	-0.037***
			(0.0079)	(0.010)
Rents home	0.86	0.84	0.019**	0.037***
			(0.0083)	(0.010)
Pre-call average wages	11510	7394	4112***	1416***
			(254.9)	(340.7)
Pre-call average AGI	20290	16740	3551***	2042**
			(394.8)	(999.9)
Predicted wages, 2 years post call	13320	10420	2900***	-20.6
			(151.4)	(157.2)
Predicted AGI, 2 years post call	20260	18570	1687***	109
			(101.2)	(106.3)

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: The sample consists of 5300 callers referred to funds and 2800 callers not referred to funds. Sample sizes are rounded in compliance with disclosure restrictions. The sample is restricted to eligible callers who have not called in the prior six months with non-missing characteristic information. The first two columns present means for each group. The third column reports the difference in means, and the last column reports this difference after controlling for a vector of caller characteristics Z_i related to fund eligibility; robust standard errors in parentheses. Release authorization CBDRB-FY23-0267.

Table A.3: Baseline Characteristics for Low Wage Group, by Fund Availability

	Referred to Funds	Not Referred	Dif.	Adj. Dif.
Number of adults	1.38	1.44	-0.060*** (0.023)	-0.030 (0.030)
Number of minors	1.37	1.52	-0.15*** (0.051)	-0.083 (0.067)
Monthly rent	623.5	389.4	234*** (10.5)	-10.8 (8.60)
Net dollars owed	859.4	877.4	-18.0 (22.0)	64.8** (28.9)
Monthly income (thousands)	1.22	0.98	0.24*** (0.020)	-0.053** (0.022)
Veteran	0.015	0.021	-0.0066 (0.0043)	-7.0e-19 (8.6e-19)
Senior	0.048	0.082	-0.035*** (0.0080)	9.6e-18*** (2.0e-18)
Nbd median income	39450	38480	969** (406.9)	1652*** (546.9)
Nbd pct Black	0.65	0.68	-0.022** (0.011)	-0.025* (0.014)
Age	38.9	41.8	-2.95*** (0.46)	0.19 (0.50)
Reason missing	0.49	0.37	0.11*** (0.016)	-0.0031 (0.0025)
Reason: lost job	0.14	0.13	0.015 (0.011)	0.012 (0.013)
Reason: job cut hours	0.048	0.024	0.024*** (0.0057)	0.011 (0.0074)
Reason: medical work absence	0.038	0.026	0.012** (0.0055)	0.00031 (0.0071)
Reason: health	0.021	0.050	-0.029*** (0.0060)	-0.0035 (0.0075)
Reason: housing change	0.14	0.22	-0.080*** (0.012)	0.014 (0.014)
Reason: lost benefits	0.063	0.068	-0.0046 (0.0079)	-0.0036 (0.0088)
Reason: other	0.060	0.11	-0.049*** (0.0090)	-0.027*** (0.0099)
Female	0.77	0.82	-0.055*** (0.013)	-0.034** (0.017)
Hispanic	0.079	0.049	0.030*** (0.0076)	0.020** (0.0096)
White	0.087	0.056	0.031*** (0.0080)	0.0093 (0.011)
Black	0.87	0.91	-0.039*** (0.0096)	-0.018 (0.013)
Other	0.033	0.026	0.0075 (0.0053)	0.0071 (0.0068)
Receives housing subsidy	0.25	0.62	-0.37*** (0.015)	4.4e-17*** (5.8e-18)
Receiving SNAP	0.62	0.45	0.17*** (0.016)	-0.0041 (0.012)
Receives income from disability	0.079	0.072	0.0076 (0.0084)	-1.1e-18 (4.0e-18)
Receiving child support	0.026	0.018	0.0083* (0.0046)	0.0026 (0.0059)
Receiving EITC	0.26	0.11	0.15*** (0.012)	0.026** (0.013)
Receiving disability	0.079	0.072	0.0076 (0.0084)	-1.1e-18 (4.0e-18)
Receiving SSI	0.11	0.098	0.0076 (0.0096)	-0.021* (0.012)
Receiving TANF	0.064	0.052	0.013* (0.0074)	0.0061 (0.011)
Receiving UI	0.017	0.011	0.0060 (0.0037)	-0.00023 (0.0054)
Owns home	0.011	0.015	-0.0040 (0.0037)	1.5e-18 (1.1e-18)
Lives with family or friends	0.16	0.15	0.0071 (0.012)	-0.038** (0.015)
Rents home	0.83	0.83	-0.0031 (0.012)	0.038** (0.015)
Pre-call average wages	1726	1320	406.1*** (57.4)	210.7*** (76.6)
Pre-call average AGI	14210	13290	925 (672.6)	1265 (1678)
Predicted wages, 2 years post call	10760	8176	2586*** (200)	57.5 (208.6)
Predicted AGI, 2 years post call	18780	17390	1389*** (131.9)	126.1 (136.2)

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: The table compares callers who are and are not referred to funds within the sample of people with below-median pre-call wage and salary income. The first two columns present means for each group. The third column reports the difference in means; *, **, and *** denote significance at the 10%, 5% and 1% levels. The last column reports this difference after controlling for a vector of caller characteristics Z_i related to fund eligibility; robust standard errors in parentheses. Release authorization CBDRB-FY23-0267.

Table A.4: Baseline Characteristics for High Wage Group, by Fund Availability

	Referred to Funds	Not Referred	Dif.	Adj. Dif.
Number of adults	1.39	1.45	-0.062** (0.025)	-0.056* (0.031)
Number of minors	1.24	1.54	-0.30*** (0.049)	-0.22*** (0.063)
Monthly rent	771.4	556.4	215*** (11.8)	-25.5** (10.4)
Net dollars owed	997.9	1133	-135.4** (52.8)	74.9 (47.9)
Monthly income (thousands)	1.50	1.31	0.19*** (0.024)	-0.055** (0.026)
Veteran	0.015	0.014	0.00074 (0.0041)	1.7e-18 (2.4e-18)
Senior	0.010	0.021	-0.011** (0.0046)	1.6e-18 (3.1e-18)
Nbd median income	40010	39680	334.3 (447.7)	637.4 (629.1)
Nbd pct Black	0.64	0.65	-0.013 (0.012)	-0.021 (0.016)
Age	38.4	38.8	-0.31 (0.38)	0.91* (0.48)
Reason missing	0.47	0.36	0.10*** (0.017)	-0.010*** (0.0031)
Reason: lost job	0.21	0.21	0.0049 (0.014)	0.016 (0.016)
Reason: job cut hours	0.084	0.091	-0.0068 (0.0099)	-0.028** (0.012)
Reason: medical work absence	0.087	0.069	0.018** (0.0091)	0.020* (0.011)
Reason: health	0.014	0.011	0.0023 (0.0038)	-0.0021 (0.0054)
Reason: housing change	0.063	0.15	-0.089*** (0.011)	0.011 (0.012)
Reason: lost benefits	0.044	0.046	-0.0018 (0.0073)	0.0010 (0.0092)
Reason: other	0.032	0.061	-0.028*** (0.0078)	-0.0083 (0.0086)
Female	0.81	0.87	-0.062*** (0.012)	-0.019 (0.017)
Hispanic	0.087	0.070	0.017* (0.0092)	0.013 (0.012)
White	0.084	0.049	0.035*** (0.0081)	0.014 (0.012)
Black	0.87	0.92	-0.048*** (0.010)	-0.035** (0.014)
Other	0.035	0.026	0.0093 (0.0058)	0.016** (0.0080)
Receives housing subsidy	0.13	0.53	-0.40*** (0.016)	9.0e-17*** (1.2e-17)
Receiving SNAP	0.48	0.36	0.11*** (0.017)	-0.026 (0.017)
Receives income from disability	0.016	0.020	-0.0036 (0.0047)	-6.5e-18* (3.4e-18)
Receiving child support	0.022	0.021	0.0016 (0.0050)	-0.00024 (0.0070)
Receiving EITC	0.35	0.23	0.12*** (0.015)	0.018 (0.014)
Receiving disability	0.016	0.020	-0.0036 (0.0047)	-6.5e-18* (3.4e-18)
Receiving SSI	0.031	0.032	-0.0012 (0.0061)	-0.0071 (0.0089)
Receiving TANF	0.011	0.023	-0.012** (0.0048)	-0.014** (0.0067)
Receiving UI	0.057	0.046	0.011 (0.0075)	0.0033 (0.011)
Owns home	0.023	0.022	0.0014 (0.0051)	-9.1e-21 (5.6e-18)
Lives with family or friends	0.086	0.12	-0.029*** (0.011)	-0.024* (0.014)
Rents home	0.89	0.86	0.028** (0.012)	0.024* (0.014)
Pre-call average wages	19470	16360	3113*** (373.2)	1582*** (519.9)
Pre-vall average AGI	24250	20540	3708*** (466.8)	1910*** (654.6)
Predicted wages, 2 years post call	15420	13700	1715*** (200.5)	-181.6 (222.4)
Predicted wages, 2 years post call	21470	20300	1174*** (144.3)	67.0 (159.6)

Source: Homelessness Prevention Call Center data, American Community Survey, Decennial Census, IRS Form 1040, Form W-2, IRS information returns, Census Numident, Master Address File Auxiliary Reference File, and authors' calculations.

Note: The table compares callers who are and are not referred to funds within the sample of people with above-median pre-call wage and salary income. The first two columns present means for each group. The third column reports the difference in means; *, **, and *** denote significance at the 10%, 5% and 1% levels. The last column reports this difference after controlling for a vector of caller characteristics Z_i related to fund eligibility; robust standard errors in parentheses. Release authorization CBDRB-FY23-0267.