The Effect of Emergency Financial Assistance on Healthcare Use

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Abstract

Does providing financial assistance to people who have just experienced an income shock affect their healthcare use? To address this question, we examine healthcare outcomes in a setting where people at risk of homelessness due to an income shock were offered or denied referral to financial assistance quasi-randomly. Among callers who have been screened as eligible for assistance at Chicago's Homelessness Prevention Call Center (HPCC), some are denied assistance because the availability of funding varies. Conditional on some observable characteristics, funding availability is as-good-as-randomly assigned to callers. We link callers to healthcare utilization records and observe their inpatient hospital stays and emergency department visits. We find that referral to financial assistance has little effect on overall healthcare use–we can reject increases in total utilization greater than 7% of the base rate and decreases of more than 4%. This null effect can be explained, in part, by the fact that the income shock does not significantly change overall healthcare use among those not receiving assistance, suggesting that these individuals can insure health and healthcare demand against these shocks in other ways.

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1 Introduction

Income volatility dramatically affects the lives of many poor people, often resulting in severe consequences such as eviction, having utilities shut off, or other hardships. Even in a strong economy in 2015, call centers that provide emergency financial assistance to prevent such hardships in the United States received 15 million calls (211.org, 2015). Many programs have been developed to help those facing an economic crisis, and funding for such programs expanded dramatically during the height of the COVID-19 pandemic. In the year following March 2020, Congress allocated more than $70 billion[1] to state and local governments for emergency housing assistance, including more than $46 billion for direct rent and utility assistance (CARES Act, 2020; Consolidated Appropriations Act, 2020; American Rescue Plan Act, 2021). Negative income shocks, which disproportionately affect those with low asset levels and people of color (Ganong et al., 2020), can affect health, though not always in expected ways. Suicides rise during recessions (Ruhm, 2000), people working at firms with mass layoffs have higher mortality rates (Sullivan and Von Wachter, 2009), and evictions lead to greater emergency room use (Collinson and Reed, 2018). On the other hand, positive income shocks (Evans and Moore, 2011) and economic booms (Ruhm, 2000) can increase overall mortality. Social insurance to smooth income shocks is one policy response. However, because social insurance is widely available, it is difficult to separate the direct benefits of smoothing income from behavioral responses to its presence.

This paper tests whether emergency financial assistance that smooths out income shocks affects use of healthcare. We examine data on callers to Chicago's Homelessness Prevention Call Center (HPCC), which pays temporary financial assistance for people at risk of losing their housing. Following an approach similar to previous studies (Evans et al., 2016; Palmer et al., 2019), our research design is built around three novel features of the program and related data. First, the

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[1]This figure includes: $4 billion in Emergency Solutions Grants (ESG) and $5 billion in Community Development Block Grants (CDBG) from the March 2020 CARES Act; $25 billion in rent and utility relief from the December 2020 Consolidated Appropriations Bill; $5 billion for homelessness prevention, $10 billion in mortgage homeowner assistance, and $21.6 billion in emergency rental assistance from the March 2021 American Rescue Plan. Reina et al. (2021) estimates that as of October 2020 state and local governments had already spent $3.9 billion of the CARES funding on housing assistance.

HPCC screens callers on having a shock, most often a reduction in earned income or public benefits income. Observing such shocks in administrative records is typically difficult. Second, due to limited funding, many eligible callers who have experienced shocks do not receive assistance. The probability of funding appears to be as good as random, conditional on observable characteristics, which allows us to form a comparable control group. Third, the HPCC collects identifiers for both groups which we use to link callers to state healthcare records on their inpatient and emergency department visits.

We find little evidence of an effect of financial assistance on overall healthcare use. The difference in the likelihood of having a hospital or emergency department visit within 2 years between those who are referred to assistance and those who are not is small (0.8 percentage points, or 1% of the baseline mean of 58%). This difference is not statistically significant but is sufficiently precise that we can reject increases in total use greater than 7% of the base rate and decreases of more than 4%. Similarly, emergency visits decrease by no more than 4% and inpatient visits decrease by no more than 11%.

To consider possible explanations for this null effect, we examine the impact of the income shocks on healthcare use for those not treated: eligible callers who are denied assistance. The change in healthcare use before versus after the call for this group helps gauge how shocks affect healthcare use in the absence of financial assistance. For this group, the effect of the income shock is small despite evidence from other work that these shocks lead to other disruptions such as an increased likelihood of moving and entering homeless shelters (Phillips, 2020). For those who call when funding is not available, we find that the probability of an emergency department or inpatient visit is 3% higher in the six months after the call than the six months before the call; this difference is not statistically different from zero; and the 95% confidence interval excludes increases of more than 10% of the pre-period mean. This small effect of the income shocks for the control group suggests that these individuals are able to insure health outcomes against these shocks in other ways, which may explain our null finding for the effect of financial assistance on overall healthcare use.

We also conduct exploratory analysis by diagnosis categories. We find that, unlike overall healthcare, the likelihood of receiving healthcare due to an assault increases after the call among those denied assistance, and referral to emergency financial assistance leads to a large decrease in the likelihood of being treated as a victim of violence. This result aligns with prior evidence showing that those referred to emergency financial assistance are less likely to be arrested as perpetrators of violence (Palmer et al., 2019) and evidence from the literature that negative shocks (Card and Dahl, 2011; Leslie and Wilson, 2020; Bullinger et al., 2020) and social insurance (Gubits et al., 2018; Carr and Packham, 2019) can affect the prevalence of violence. We also see suggestive evidence of a similar but noisier pattern for rare serious events, like suicide. The large number of potential categories to be tested suggests caution in interpreting these sub-group effects. Still, exploring these effects may be useful in guiding future work on how income shocks and social insurance affect particular aspects of health and healthcare.

This study contributes to the literature on the effects of homelessness prevention programs by examining healthcare outcomes. Previous research has shown that people who are homeless experience much worse health outcomes than the general population (Barrow et al., 1999; Roncarati et al., 2018), and most public expenditures on people who are homeless go to healthcare and criminal justice rather than housing or other public benefits (Flaming et al., 2015). Many studies examine the health and healthcare use effects of much more intensive and expensive interventions for people who are homeless, such as permanent supportive housing (Stergiopoulos et al., 2015; Rosenheck et al., 2003; Gubits et al., 2018), or long-term housing vouchers for already housed people (Ludwig et al., 2012; Jacob et al., 2015). However, the existing literature on the effects of temporary financial assistance does not examine health or healthcare outcomes (Rolston et al., 2013; Evans et al., 2016; Palmer et al., 2019).

More broadly, our findings support a literature showing that the causal effect of income volatility on healthcare use is relatively narrow. Negative shocks can have specific effects on suicides (Ruhm, 2000), mortality (Sullivan and Von Wachter, 2009), and mental health (Collinson and Reed, 2018). Studies of lottery winners similarly find some mental health benefits of large positive income shocks

but not much effect on health and healthcare use overall (Cesarini et al., 2016; Gardner and Oswald, 2007). We complement this work by providing evidence on social insurance that smooths out negative shocks. While a large literature has studied how social insurance affects people in developing countries and the long-run outcomes of children, more direct studies on the healthcare outcomes of recipients of social insurance are less common. Social insurance may have larger effects on health when its presence induces changes in behavior, such as employment (Snyder and Evans, 2006). But in isolating the direct effect of smoothing out income, we find similar results to the lottery studies: social insurance has limited effects on overall healthcare use but perhaps larger effects on narrower outcomes.

2   The Homelessness Prevention Call Center

We first provide an overview of the operation of the Homelessness Prevention Call Center (HPCC) in Chicago. The institutional context is the same as in Evans et al. (2016) and Palmer et al. (2019), so we describe it only briefly here. Readers wishing for greater detail should refer to those studies.

Most US communities have call centers that refer callers who are at risk of eviction or having their utilities shut off to agencies providing emergency financial assistance. In the US, 93% of the population has access to such a call center, and these call centers receive 15 million calls per year (211.org, 2015). In Chicago, Catholic Charities operates the HPCC, which serves as the central hub for emergency financial assistance.

In Chicago, individuals and families in need of rent or utility assistance can call 3-1-1 for help. Such calls are routed to the HPCC where a staff person from HPCC collects contact and demographic information from the caller. This step is important for the evaluation because the HPCC collects personal identifiers and demographics before determining eligibility and regardless of whether any funds are available. Thus, we can link to outcome data and observe balancing characteristics for both people referred to assistance and those who are turned away.

Second, the HPCC interviews the caller to determine if the person 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, (b) causes imminent risk of homelessness, (c) can be solved with limited financial assistance, and (d) is temporary. A common example of an eligible person would be a person who lost her job, has received an eviction notice, needs to pay 1 month of back rent, and has a new job lined up for next month. Callers ineligible for the HPCC and callers who are not referred to funds are referred to alternative resources.[2]

Third, for an eligible caller, the HPCC determines if funding is available. Rather than disbursing assistance itself, the HPCC connects callers to government and private entities, or delegate agencies, that administer assistance. The staff member works through a pre-set ordered list of delegate agencies to see if the the agency has funds available immediately and if the caller meets any fund-specific criteria including request type (rent assistance, security deposit, or utilities), need amount, 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.

Whether a particular person will be referred to funds is hard to predict. New delegate agencies come online and existing agencies shut down throughout the year. In addition, currently operating agencies may not provide assistance continuously because they may temporarily run out of funds. The availability of funding on any given day depends on many factors. For example, some delegate agencies require that callers meet with a financial counselor before funds are dispersed, and an I&R 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 new interview slots might become available throughout the month due to cancellations. 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

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[2]Alternative resources include legal aid, domestic violence counseling, assistance with utility complaints, workforce development, senior services, disability services, public benefit screening, and other general support services (George et al., 2011).

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 from August 2014 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 57%, in some weeks more than 90% of eligible callers were referred to funds, while in other weeks only 20% were referred. This variation may differ across different caller types because of fund-specific eligibility requirements. However, even if we restrict the sample to callers who meet fund-specific criteria for the same set of funds, we still find considerable variation in the likelihood of being referred to funds. Figure 1.b shows even greater variation in the funding rate over time for the largest such group in our data, callers who do not have other housing subsidies and are asking for more than one month of rent assistance totalling more than $900. Some of the overall variation in the funding rate, though, will be absorbed when we control for these fund-specific criteria, because for some subgroups there is little variation in fund availability over time. For example, Figure 1.c shows that a group of callers with lower need amounts is referred to funds 96 percent of the time, and for most weeks the funding rate for these callers is greater than 90 percent.

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 vs. not will be similar, conditional on characteristics that affect fund-specific eligibility requirements.

3   Data

3.1   HPCC Call Center Data

The HPCC provided us with detailed call information for all calls from January 20, 2010 to March 29, 2018. These HPCC records include the call date, demographic information (such as name, date of birth, address, last four digits of Social Security Number (SSN), age, and gender), request type (for rent, security deposit, or utilities), other information gathered to determine general eligibility (such as sources and dollar amounts of income, type of crises, and whether they have an eviction notice), and information to determine whether they satisfy fund-specific restrictions (such as need amount, veteran status, receipt of housing subsidies, and whether the total debt exceeds one month of rent). Because we have the ZIP code for each caller's residence at the time of the call, we also merge in ZIP-code level data from the 2009-2013 American Community Survey (ACS).

3.2   IDPH Healthcare Use Data

Data on healthcare use come from administrative records maintained by the Illinois Department of Public Health (IDPH). These data cover all inpatient stays and emergency department visits to acute care hospitals, specialty hospitals, and ambulatory surgical treatment centers in the state of Illinois. For each record, we observe whether the visit was inpatient or emergency, total charges, the payer (Medicare, Medicaid, private insurance, self-pay, or other), the length of stay, and a series of International Classification of Diseases (ICD) diagnosis codes and emergency codes.

We provided HPCC caller records to IDPH, who matched them to their records based on date of birth, last 4 digits of SSN, first name, and last name. The match is limited to the person calling because we do not have identifiers for other members of the household. IDPH then sent the

research team a de-identified file that included the linked health outcomes as well as the variables from HPCC[3]. The IDPH healthcare records cover the years 2013 to 2017. Records after 2017 postdate the data sharing agreement with IDPH, and the administrative health records before 2013 do not include the exact dates needed to determine whether the healthcare visit occurred before or after the call.

We construct diagnosis categories using groups of ICD codes. Because hospitals and treatment centers in Illinois transitioned from the ICD-9 to the ICD-10 protocol during our measurement period, we first make some adjustments to generate consistent ICD codes over time. Specifically, we use the 2018 general equivalence mappings (GEMs) from the National Center for Health Statistics to map ICD-9 codes from visits before October 2015 to ICD-10 codes (NCHS, 2018a). Next, we aggregate unique 7-digit ICD-10 codes up to the diagnosis category (3 digit) level. We then combine diagnosis categories into groups informed by the chapters of the ICD-10 Codebook (WHO, 2008), definitions from the Centers for Disease Control's reference materials (CDC, 2013, 2007; Annest et al., 2014), and categorization schema from previous work in the public health and economics literatures (Roncarati et al., 2018; Ruhm, 2018; Shah et al., 2015). We describe the construction of outcomes from ICD codes in greater detail in Appendix A.

3.3   Sample for Analysis

The sample used for this study is drawn from the extract of all calls to the HPCC from January 20, 2010 to March 29, 2018 matched to IDPH records on healthcare use from January 1, 2013 to December 31, 2017. We include callers seeking all forms of assistance, including rent, security deposit, utilities, and other needs. As shown in the left side of Table 1, the HPCC received 165,319 calls during the period from January 1, 2013 to December 31, 2017.

We restrict our sample based on eligibility for funds. Most callers fail at least one of the four criteria discussed above and are not eligible for assistance. Following Evans et al. (2016), we also

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[3]Because we only observe admit month (rather than the exact admit date), outcomes for the same calendar month as the call straddle the call date. Due to this ambiguity, we omit the call month from both the pre-call and post-call measures in our main specifications.

exclude observations for which HPCC does not record the current living situation of the caller (i.e., whether one rents housing, owns housing, or lives in shared housing).[4] During our sample period, the HPCC received 21,497 calls from individuals who satisfied these eligibility criteria.

Availability of healthcare use outcome data limits our sample due to a number of restrictions, as shown in Figure 2. We observe healthcare use from January 1, 2013 to December 31, 2017. To observe 24 months of post-call outcome data, we restrict the sample to the 15,499 eligible calls on or before December 31, 2015. We further limit the sample to the 13,762 calls that occur on or after July 1, 2013 so that we can observe 6 months of healthcare records before the call.

We also limit the sample based on previous call history. People often call the HPCC multiple times if they do not receive funds on the first call. Funding on subsequent calls may not be exogenous because people who call multiple times may have different underlying healthcare use than those who call once. Thus, we restrict our attention to people who have not called recently for whom availability of funds should be exogenous. Our main analysis will use a sample of calls for which the caller has not called the HPCC in the past six months, as has been done in past work (Evans et al., 2016; Palmer et al., 2019). This restriction reduces the sample for our main analysis to 8,585 callers. To ensure our results are not sensitive to this restriction, we also examine results for the sample of 6,459 calls from people who had no preceding call since at least the start of our HPCC data, January 20, 2010.

To determine how healthcare use for HPCC eligible callers aligns with that of the broader population, we compare the average number of inpatient discharges for our sample to those for the population of Cook County, Illinois, the main service area for the HPCC. Since seniors rarely call the HPCC, we limit this comparison to people aged 18-64. Table 2 shows the comparison. For HPCC callers, we report health care utilization in the calendar year prior to the call to capture these outcomes for a period that predates any receipt of assistance. Over the period from 2014 to 2017, eligible callers had an average of 149 inpatient visits per 1,000 callers in the preceding calendar year. This value is 53% larger than the value of 97.4 for all of Cook County (column 3).

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[4]Applying this restriction excludes only 0.2% of callers in our main analysis sample.

To determine if this large difference is an artifact of underlying demographic differences between the HPCC sample and the population of Cook County, we re-weight the HPCC sample to match Cook County along observable characteristics in column 2.[5] After weighting, the sample of 2014-2017 HPCC eligible callers averages 154 inpatient visits per 1,000 callers, which is 58% larger than the corresponding Cook County value. Thus, most of the raw difference in healthcare usage cannot be explained by differences in demographics. In each year of our sample period, the hospital utilization rates are significantly higher for our HPCC sample than for Cook County on the whole. The level of healthcare usage for HPCC callers is also higher than estimates typically reported for the general Medicaid population (Sommers et al., 2016; Garthwaite et al., 2019) but lower than that of people who are chronically homeless (Kushel et al., 2002; Bharel et al., 2013; Chambers et al., 2013). These results suggest that individuals in our sample, who are selected on receiving an income shock which leads them to seek financial assistance, are somewhat more disadvantaged than the population overall.

These levels of healthcare use are consistent with the characterization that HPCC callers are typically low-income families who are unstably housed but not chronically homeless. The average household head in our sample is 39 years old, lives with two other people, and has monthly income of $1,284 and monthly rent of $642. 81% are female, 64% have a child in the household, and 89% are Black. Compared to all poor residents of Cook County, HPCC callers are younger, have higher income, and are more likely to be Black, female, and have kids.[6] These characteristics make HPCC callers similar to housing voucher lottery applicants (Jacob and Ludwig, 2012) but different from a cross section of currently homeless individuals (Meyer et al., 2021), who are much more likely be single, male, veterans, and receiving cash public assistance. We provide more detailed comparisons in Appendix Table B1.

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[5]Specifically, we weight observations in the HPCC data so that the proportions of gender-age-income-race cells in the HPCC data match the corresponding proportions in the Cook County data. HPCC callers are disproportionately female, young, poor, and people of color.

[6]Hispanic individuals, in particular, are under-represented among HPCC callers. A previous descriptive study of the HPCC noted that the automated call menu was not in Spanish, that Spanish-speaking callers often encountered a delay when interpreters had to be brought in, and that Spanish-speakers' calls were misdirected at more than twice the rate of non-Spanish speakers George et al. (2011). Jacob and Ludwig (2012) also find that Hispanics are under-represented in their sample of voucher-eligible Chicagoans.

4 Empirical Strategy

4.1 Regression Specification

If funding were randomly assigned, the causal effect of being referred to emergency financial assistance could be determined through OLS estimation of the following:

$$Y_i = \alpha_1 + Funds_i\beta_1 + \varepsilon_{1i}, \tag{1}$$

where Y_i is an outcome, such as whether person i has an inpatient visit after calling; $Funds_i$ is an indicator for whether the person was referred to funds; and ε_{1i} is an individual-specific error term. The estimate of the coefficient β_1 measures the difference in mean outcomes between those referred to funds and those not referred to funds.

Table 3 displays this simple comparison across those referred and not referred to funds. The top half of Table 3 shows healthcare use outcomes. The first three columns show mean outcomes for all eligible callers, callers who are not referred to funds, and callers who are referred to funds, respectively. In all cases, we restrict to our main analysis sample as described above. For example, 56% of callers in our sample have at least one emergency or inpatient visit in the 24 months after the call. This value is 58% for those who are turned away without funds and 55% for those who are referred to funds. The final column shows the difference in means, in this case 2.6%, which is statistically significant at the 5% level.

However, referral to funds by the HPCC is not unconditionally exogenous, but depends on a few observable factors. As discussed above, some delegate agencies go beyond HPCC's general eligibility criteria and have fund-specific eligibility criteria, including limits on the type of need and the size of the need. Some callers are eligible at more delegate agencies than others, which makes the probability of treatment depend on these observable characteristics. To the extent that healthcare outcomes correlate with these screening factors, simple differences in means cannot be interpreted as the causal effect of referral to financial assistance on healthcare use.

The lower half of Table 3 shows how these fund-specific criteria vary with fund availability. For

example, 80% of callers who are eventually referred to funds apply for assistance with back rent while only 32% of those who are turned away need back rent. Those who are turned away are more likely to need assistance with a security deposit or utility payments, because fewer agencies will support these needs. Similarly, treatment is negatively correlated with large need amounts, not being a veteran, housing subsidies, income, and being a senior.

To address the fact that some groups are more likely to receive funding than others due to fund-specific eligibility rules, we estimate the effect of emergency financial assistance by exploiting only the variation in funding that remains after controlling for fund-specific criteria. Because HPCC centralizes the screening process for delegate agencies, we observe all of the factors that explicitly affect funding and can control for them. We estimate the following regression equation by OLS.

$$Y_i = \alpha_2 + Funds_i\beta_2 + \boldsymbol{X_i}\Gamma_2 + \boldsymbol{Z_i}\Pi_2 + \varepsilon_{2i} \tag{2}$$

In this specification, Z_i is the set of individual characteristics that affect fund-specific eligibility and thus the probability of funding. This set of variables includes request type (i.e. rent assistance, security deposit, etc.), need amount categories, veteran status, receipt of housing subsidies, and whether the total debt exceeds one month of rent. To account for patterns in call volume we also include in Z_i measures of call characteristics such as the rank of the call within the day, day of the week, month, time of the month (first five days, last five days, and middle days), year, and year-quarter. Because the maximum amount offered by various delegate agencies changes somewhat over the sample period, we also include interactions of need amount with year and quarter indicators. We also observe a vector of caller characteristics X_i that should not affect the availability of funding (including age, gender, race, ethnicity, income, and receipt of benefits), but we include these characteristics as well to reduce residual variance in the outcome. Other variables are defined as before. The coefficient of interest is β_2 which measures the difference in the outcome between those referred to funds and those not, controlling for X_i and Z_i.

Due to incomplete administrative data from the HPCC, we do not observe all of the relevant

observable characteristics for our entire sample period.[7] For the first part of our sample period, from January 20, 2010 to April 3, 2013, we have complete data. For the next five quarters or so, corresponding to the period from April 4, 2013 to July 31, 2014, some control variables in Z_i cannot be reconstructed from the variables that were extracted from the HPCC administrative records. These missing variables, such as whether the need is for back rent or a security deposit, affect the probability of fund availability, so in robustness checks (see Section 5.4) we restrict the sample to exclude calls from April 4, 2013 to July 31, 2014. For the remainder of the sample period, we essentially have complete data, lacking only data on public sources of income, which are in X_i and therefore should not affect the probability of fund availability.

4.2 Take-up

Our empirical strategy focuses on the effect of being referred to financial assistance. To the extent that take-up is incomplete, this intent-to-treat (ITT) effect will differ from the effect for those who actually receive assistance. There are two possible reasons for a difference. First, not all people offered assistance ultimately receive assistance due to lack of follow through by either the caller or the delegate agency that provides funds. Second, people who are initially denied funds may eventually receive assistance. For example, callers who are initially denied may call the HPCC back and ultimately receive assistance, or they may receive assistance from some entity besides the HPCC. However, all the available evidence indicates a large contrast in the probability of assistance between people who are initially approved vs. denied.

Most callers to the HPCC who are referred to assistance do actually receive help. The Center for Urban Research and Learning (CURL) at Loyola University conducted a descriptive evaluation of the HPCC (George et al., 2011). They surveyed HPCC callers who had been approved for funding and referred to a delegate agency within 7 days of their initial call to the HPCC. Of 105 people in the survey, 71 percent had already received assistance, expected to receive assistance, or had a

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[7]HPCC implemented changes to its IT infrastructure twice during our measurement period: first on April 4, 2013, and then again on August 1, 2014. The various sets of caller characteristics which are missing following each change result from these updates to the underlying information systems but do not reflect substantive changes to the caller interview process.

request in process. The remainder had either not been contacted (18 percent) or had been denied as ineligible (10 percent).

Receipt of funds is uncommon in the control group because few households succeed in obtaining assistance from future calls. We can directly observe repeated calls in our data. 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 these callers actually receive assistance at the same 71% rate as the full sample of callers, approximately 4% of our control group would receive assistance through repeat calling. In other cities, rejected callers would be likely to receive assistance from other sources as well, but the HPCC screens people for all major sources of emergency rental assistance in Chicago. Of course callers may seek assistance from smaller organizations, friends, and family, but the HPCC's coverage of the primary sources means that most people will not get assistance outside the HPCC. George et al. (2011) find that the call center "operates under the assumption that 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. Therefore, we expect that approximately 12% of control group callers receive some assistance either from repeat calls to the HPCC or from other organizations.

Overall, imperfect take-up in the treatment group and future funding of the control group imply that the treatment group is about 59% more likely to receive assistance. Thus, the 2SLS estimate would be about 69% (or 1/0.59) larger than the ITT effects we report.

4.3 Exogeneity of Fund Availability

Our empirical strategy relies on the assumption that callers referred versus not referred to funds are similar conditional on a small set of known, fund-specific eligibility criteria. This identification assumption implies that other observable demographic characteristics should be balanced across the two groups once we account for factors known to affect funding. We test this implication using the follow regression equation, estimated by OLS:

$$X_i = \alpha_3 + Funds_i\beta_3 + \mathbf{Z_i}\Gamma_3 + \varepsilon_{3i} \qquad (3)$$

The dependent variable, X_i, is a characteristic known not to affect the probability of receiving funding, such as whether the caller is female. The coefficient β_3 measures the difference in that characteristic between those referred to funds and those not, adjusting for differences in factors that do affect fund availability (Z_i).

Table 4 reports the results of these baseline balance tests. The columns display, respectively, the overall sample mean, the control group mean, the treatment group mean, and the estimated coefficient $\hat{\beta}_3$. Each row reports a different baseline characteristic. For example, the first row of the section on lagged outcomes shows that lagged healthcare is balanced: 28% of those for whom funds are available had an inpatient or emergency visit in the 6 months before the call, as compared to 29% of those for whom no funds are available. After adjusting for the controls, the -0.9 percentage point gap narrows to a statistically insignificant 0.4 percentage points.

Overall, baseline characteristics predict similar future healthcare use for the treatment and control group. To summarize expected healthcare use, we estimate an OLS regression of a dummy for any healthcare use in the 24 months after the call on all of the X_i and Z_i variables as well as healthcare use in the 6 months prior to calling, using only control group data. We then predict fitted values for both treatment and control groups using the coefficients from that regression. The first line of Table 4 shows the means for the fitted values. Based on their observable characteristics, we would predict that 58% of the control group will have a healthcare visit within 24 months. We predict a similar value of 57% for the treatment group. Adjusted for fund-specific eligibility factors and calendar controls (Z_i), there is a difference of -0.4 percentage points, which is statistically insignificant and small relative to the mean. As shown in the following rows, differences are similarly small if we predict particular categories of healthcare use.

We detect differences in some particular baseline characteristics, but these differences tend to be small in magnitude, for variables uncorrelated with outcomes, and counterbalanced against each other. We detect differences at the 5% level for 8 variables, which is more than would be expected

by chance. For example, those for whom funds are available have \$54 less monthly income at baseline. This difference is only 5% of mean income. Because the imbalance itself is small and because income correlates only weakly with overall healthcare use for our sample, the implied bias in treatment effects is small. For an outcome of any visit within 24 months, the \$54 imbalance implies a bias of 0.002 relative to a control mean of 0.58. In Appendix Table B2, we show each baseline variable's treatment-control imbalance, its correlation with healthcare use, and the bias in treatment effects implied by the imbalance (the product of the first two). Variables with baseline imbalances tend to be uncorrelated with healthcare use. Variables that strongly predict future healthcare use, such as past healthcare use, are balanced. Appendix Figure B1 plots the implied bias for each variable and shows values that are small and offsetting. In fact, no variable implies larger bias than the income variable considered above.

Some of the small baseline differences we observe in Table 4 may be due to missing data on some Z_i's for part of the sample period, as discussed in Section 4.1.[8] In Appendix Table B3, we reproduce the results in Table 4, restricting the sample to the period for which we observe all fund-specific eligibility criteria. For this sample, balance is similar to the full sample, which provides some assurance that missing data does not invalidate our main results. Below, we also test the robustness of our results to restricting the sample to the periods where we typically observe all the fund-specific eligibility criteria.

5 Results

5.1 Effects on Overall Healthcare Use

In Table 5 we present estimates of the effect of emergency financial assistance on our key health care utilization outcomes. We present these estimates for different time horizons ranging from 6 months (column 1) to 24 months (column 4) after the call. These outcomes are cumulative, so for example, the 24-month outcomes indicate whether a caller received any healthcare usage of a particular type

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[8]This issue was not present in Evans et al. (2016) or Palmer et al. (2019), which use earlier samples.

within the first 24 months after the call. The first three outcomes measure health care utilization (either an emergency department or inpatient hospital stay) for any diagnosis. The results for these outcomes indicate that financial assistance has little effect on overall healthcare use for the full sample. For example, the difference in the likelihood of having an inpatient discharge or emergency department visit within 24 months of the call between those who are referred to assistance and those who are not is 0.8 percentage points, which is only 1% of the baseline mean of 58%. This difference is not statistically significant but is sufficiently precise that we can reject increases in healthcare use greater than 7% of the base rate and decreases of more than 4%. The evidence is similar when looking at emergency and inpatient visits separately. We can reject decreases of more than 4% for emergency visits and 11% for inpatient visits. We also find no evidence of a sizeable effect of financial assistance on healthcare usage overall for shorter time horizons, such as at 6, 12, and 18 months after the call.

We estimate these treatment effects at many different time horizons and report results in Figure 3. Each point on the solid line shows the coefficient β_2 from estimating Equation (2) at a different time horizon, ranging from 6 months prior to the call to 24 months after the call. A 95% confidence interval is shown in dashed lines. Time zero indicates the month of the call. Figure 3.a shows that the estimated effect on emergency department visits hovers around zero for the first 10 months after the call. After that, the estimated effect is positive, but small and statistically insignificant. Similarly, Figure 3.b shows little change in the likelihood of an inpatient hospital visit. We also report the estimated difference in health care use for up to 6 months prior to the call. These estimates are near zero prior to the time of the call in both Figures 3.a and 3.b, which indicates that the treatment and control groups were similar prior to calling.

5.2   Responsiveness of Healthcare Use to Shocks

To understand the small effects of financial assistance on overall healthcare use, we examine the extent to which healthcare use responds to the large income and housing shocks that HPCC callers face; shocks that other work has shown leads to other disruptions such as an increased likelihood

of moving and entering homeless shelters (Phillips, 2020). One might expect healthcare use to spike after the call for those who experience shocks but are not referred to financial assistance. For example, income and housing shocks might reduce stable access to inputs like healthcare, medications or nutrition (Kushel et al., 2006; Reid et al., 2008), induce stress (Burgard et al., 2012), or lead to crowded housing situations.

Figure 4 compares healthcare use before and after the call for HPCC callers who are turned away. It displays quarterly flow probabilities of using different types of healthcare.[9] Figure 4.a shows that emergency department usage is flat over time for those turned away from financial assistance. The lack of a visible spike after the call suggests that income shocks do not affect healthcare usage for callers. Figure 4.b shows similar results for inpatient visits.

Table 6 quantifies how healthcare use evolves over time for callers who are denied assistance. Each column compares utilization in the 6 months prior to the call to utilization after the call for the range of months indicated in the column title.

The results indicate that overall healthcare use does not respond much to the income and housing shocks faced by HPCC callers who are denied assistance. The pre-post differences in the first row are close to zero and precisely estimated for all time frames. Health care use is 0.2 percentage points, or 3%, higher in the first six months after the call for those who experience shocks but are not referred to financial assistance. The top of the 95% confidence interval excludes increases of more than 10% of the pre-period mean. Similarly, we can reject increases of more than 12%, 7%, and 2% in months 7-12, 13-18, and 19-24, respectively. Similarly, the two main components of healthcare we measure, emergency and inpatient visits, show no signs of large increases; if anything, inpatient visits decrease slightly.

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[9]We report flow probabilities to allow a clear comparison between time periods with similar lengths while keeping our main analysis sample. We obtain similar results when analyzing cumulative probabilities for smaller samples that have a longer pre-period available.

5.3 Effects by Diagnosis Category

We also explore whether some specific components of healthcare respond to shocks and financial assistance. The results of this analysis should be taken with some caution because of the large number of diagnosis code categories, multiple hypothesis testing concerns, and our inability to pre-specify categories of particular interest. At the same time, individuals who have recently experienced a negative shock that has put them on the brink of eviction might experience no change in overall healthcare but a higher risk of health issues related to stress or large disruptions in economic stability. Outcomes groups by diagnosis code help explore this possibility.

As shown in the lower panels of Table 5, we find some evidence that healthcare for assaults and suicides declines in response to financial assistance.[10] These events are rare and so results are noisier than for overall healthcare, but the estimates show some signs of treatment effects. Our estimates indicate that the probability of having a healthcare visit associated with an assault within 24 months of the call decreases by 1.0 percentage point (p-value = 0.058). Relative to a base rate of 2.8 percentage points, this represents a 36% reduction in the likelihood of being treated for the effects of an assault. The probability of receiving healthcare in response to suicide drops by 0.3 percentage points within 24 months, a large change relative to a base rate of 1.8 percentage points, though statistical precision is lacking. Figures 3.c and 3.d display these results graphically over time. The effect on assaults accumulates slowly, while any change in suicide occurs almost immediately.

The control group's response to shocks helps explain treatment effects for diagnoses related to stress and economic disruption. In Figures 4.c and 4.d, we show control group healthcare use over time in response to assaults and suicide, respectively. Table 6 quantifies these changes. The likelihood of receiving healthcare to treat an assault injury is similar 6 months before and 6 months after the call, but in months 7-12 after the call the monthly probability of a healthcare visit for an assault increases by 0.08 percentage points among those who experience shocks but are not

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[10]Categories are based on diagnosis codes. Injuries from assaults largely consist of fights and domestic abuse, though they also includes rarer events like shootings, stabbings, and sexual assault. Healthcare for suicide includes responding to both ideation and attempts.

referred to assistance. This increase is large relative to a pre-call rate of 0.09 percentage points and statistically significant at the 5% level. The difference remains positive but diminishes in magnitude and statistical significance in the second year after the call. Similarly, the point estimates suggest that health treatment related to suicide also increases after the call, though this estimate is imprecise.

Figure 5 places these two diagnosis categories in a broader context, showing treatment effects for a variety of diagnostic groupings. We follow the ICD-10 Codebook in grouping healthcare diagnosis into natural causes, which have a biological origin, and external causes, which typically are associated with a more immediate trauma (see Appendix Table A2). Panels (a) and (b) show 6-month and 24-month treatment effects, respectively. Each row shows the point estimate for the coefficient on a funds dummy from a separate regression (β_2 from Equation (2)), with the top three repeating the estimates for all visits, inpatient hospital visits, and emergency visits that were reported in Table 5. Panels (c) and (d) of Figure 5 express these treatment effects relative to the control group mean.

As before, assaults decrease significantly in a manner that accumulates slowly over time – the effect is small at 6 months, but large at 24 months. We see little change in external causes overall, of which assaults are a subgroup. Most externally-caused healthcare visits are for common injuries, such as a broken bone, that do not respond to financial assistance. We see some suggestion that other very serious but rare external events drop: suicides, overdoses, and alcohol-related ailments. While large in relative terms, effects on these events are imprecisely estimated.

For visits with natural causes we similarly see no overall effect. As shown in Figure 5, we find some marginally statistically significant evidence of decreased healthcare visits for flu/pneumonia and skin ailments in the 6 months after the call. This effect fades over time and only appears for these two sub-categories. See Appendix Figure B2 for results for all categories of natural causes. Respiratory and skin ailments are over-represented among people who are homeless (Raoult et al., 2001; Fazel et al., 2014), and the spread of upper respiratory disease at homeless shelters has been an area of concern during the COVID-19 pandemic (Tobolowsky et al., 2020). Evans et al. (2016) show

that emergency financial assistance reduces the likelihood that one ends up in a homeless shelter. Therefore, assistance may reduce the incidence of such ailments through its effect on homelessness.

5.4   Robustness to Sample Selection, Specification, and Attrition

We consider the extent to which our results are sensitive to alternative specifications and samples. In Table 7, we find similar results when changing the estimation sample, the specification of controls, and the construction of the outcome variable. Column (1) replicates our main results at 24 months and each additional column explores an alternative specification. Appendix Table B4 provides similar results at 12 months. These results indicate that both the insensitivity of total healthcare use to financial assistance and the large effect for utilization related to assaults are robust.

First, we measure similar results under different definitions of the estimation sample. In the main results, we limit the sample to calls for which the caller has not called in the past 6 months. In column (2) we relax this restriction, limiting the sample to those without a call in the past 3 months. In column (3) we further restrict to callers with no previous call in our data. In both cases, the likelihood of receiving healthcare after an assault decreases by 1 percentage point when referred to assistance, just as in our main results. Similarly, expanding the time period of the sample does not matter much. In our main sample we exclude calls during the first half of 2013 so that we can measure 6 months of pre-call healthcare use (as in Figure 3). In column (4) of Table 7 we expand the sample to include calls from January through June of 2013. The effect on healthcare after an assault increases in magnitude slightly to 1.2 percentage points, which is significant at the 5% level (p-value 0.016).

Second, the results are robust to how we control for fund-specific eligibility factors. As noted above, referral to funds is as-good-as-randomly assigned conditional on a set of calendar and fund-specific eligibility controls (Z_i), but we do not observe all of these controls from April 4, 2013 to July 31. Column (5) of Table 7 excludes this time period, limiting the sample to the period in which we observe the Z_i controls fully. The effect for assaults becomes statistically insignificant in the smaller sample but has a similar point estimate of -0.8 percentage points. The exact set of

controls that we use also does not affect the results much. As shown in column (6), our results are similar when we omit the set of controls for call and caller characteristics that do not determine fund eligibility (X_i).

Third, results are similar if we change how we handle events in the month of the call. Because we do not observe the exact date of the healthcare visit (only the month and year), for visits observed in the same calendar month as the call, we do not know if they occurred before or after the call. In our main specification, we exclude healthcare visits that occur during the call month from both our pre- and post-call outcome measures. Column (7) of Table 7 instead includes this month as part of the post period and obtains results similar to our main specification.

Finally, although we only observe outcomes associated with care received in the state of Illinois in the IDPH data, attrition due to out-of-state migration is unlikely to bias our estimates. We directly assess the importance of attrition related to migration in our sample using consumer reference data from Infutor Data Solutions. Infutor accumulates records from various commercial transactions and constructs individual-level address histories for the near-universe of adults in the United States. We identify people in the Infutor data who have ever lived in Cook County, Illinois, and fuzzy match these observations to the HPCC data on name, date of birth, and SSN. We find that out-of-state migration is exceedingly rare in our setting: only 1.6% of matched control group callers are observed to move out of Illinois within 2 years of calling the HPCC. Moreover, we do not find any evidence of differential attrition between treatment and control group callers in the matched sample. See Appendix C for details on the Infutor data, our fuzzy matching procedure, and the full migration results.

5.5   Effects for Sub-groups of Callers

We also examine how effects vary by sub-group and report these results in Table 8. Each column shows the results of a model that fully interacts an indicator for the group of interest with the treatment indicator and all of the control variables. For example, in column (1) the coefficient of 0.008 on the funds treatment indicates being referred to financial assistance raises the likelihood that

female callers have a healthcare visit within 2 years of calling the HPCC by 0.8 percentage points. The coefficient of -0.015 on the interaction term indicates that the treatment effect for men is 1.5 percentage points less than that for women. This estimate is not statistically different from zero, indicating that there is not strong evidence of heterogeneity by gender. Columns (2)-(5) provide some evidence that effects on healthcare use vary with baseline income, need amount, and perhaps presence of children, but not age. For example, the treatment effect is 9.7 percentage points larger for callers with above median income as compared to those with below median income.[11] However, we find some suggestive evidence that the high and low income sub-samples already have differences between treatment and control in healthcare use prior to the call.[12]

To more succinctly summarize heterogeneity based on observable baseline variables, we examine how treatment effects vary with baseline risk of healthcare use. Similar to the predicted values in Table 4, we calculate predicted use of healthcare based on our control variables and lagged outcomes. We then split the sample in half based on this index.[13] We find some evidence of heterogeneity, as shown in the final column of Table 8. For total utilization, the interaction coefficient of -0.066 has a p-value of 0.02. This indicates that the treatment effects for low and high risk groups, 0.039 and -0.027, differ from each other, though neither is different from zero at the 5% level. This result provides some evidence of heterogeneity in treatment effects and suggests that financial assistance squeezes the distribution of healthcare use, moving the likelihood that a person uses healthcare closer together for high and low risk callers.

We do not find similar systematic heterogeneity for outcomes that focus on particular forms of healthcare or diagnosis groups. The second and third panels of Table 8 split healthcare use by emergency and inpatient visits. The point estimates for heterogeneity for emergency department visits

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[11]The median monthly income for callers in our main analysis sample (N = 8,585) is 1,164 in 2017 USD.

[12]Results available on request.

[13]We follow the repeated split sample procedure from Abadie et al. (2018) to avoid bias from over fitting. In a randomly selected half of the control group we predict actual use with all of our control variables and lagged outcomes. We use the coefficients from that regression to create an index of healthcare use risk, predicting the outcome in the treatment group and the other half of the control group. Using that index, we split the sample into high and low risk halves. We then measure the difference in treatment effects between above and below median predicted risk using a model that fully interacts an indicator for being above median risk with the treatment indicator and all of the control variables, as in our other sub-group specifications. We repeat this random split and estimation 100 times and compute means. Finally, we bootstrap the entire procedure 1,000 times to estimate standard errors.

appear to roughly follow that for the overall healthcare use outcome, suggesting that emergency department use drives most of the heterogeneity. However, with the exception of the income split, these estimates are less precise than those for total utilization. For more focused diagnosis groups, we find some evidence that financial assistance reduces healthcare related to suicide for those with greater need amounts and this difference is statistically significant, but for other subgroups we do not detect statistically significant heterogeneity, though our power to detect sub-group differences is limited for less frequent outcomes.

5.6   Effects on Total Charges

Because inpatient and emergency department visits are often expensive, we consider the potential cost savings associated with emergency financial assistance by examining the effect of such assistance on total charges that result from these visits. This analysis also allows us to test effects on the intensive margin for care. To calculate total charges (in thousands of dollars) for each caller, we sum charges across all visits during the relevant time period. For a given visit, the charge includes the total amount that the hospital bills to the payer, whether that is a public program, a private insurer, or the patient. In Table 9 we report estimates of the effect of financial assistance on total healthcare charges. The first row reports OLS estimates of equation (2), with total charges as the outcome. Each column displays a different time horizon. In the the final column, which shows results with a 24 month time horizon, the coefficient of -1.397 indicates that those referred to funds have charges that are $1,397 less than those not referred to funds, on average. However, this estimate is very imprecise so we cannot rule out substantial increases or decreases in charges. As shown in the second row, results are similar if we only control for fund-specific eligibility controls.

The distribution of total charges is highly skewed, so we also test how access to financial assistance affects the full distribution of total costs. Many callers have no charges because they have no healthcare visits during our follow-up period. At 12 months, 57% of control group callers have no charges, and 42% have none within 24 months. As shown in our main results above, referral to financial assistance does not induce large changes in the likelihood of incurring any healthcare

cost. To examine effects further up the distribution, we estimate the effect on the 75th and 90th percentiles of total cost. Because quantile regressions frequently do not converge with the full set of controls, we estimate them with only calendar and fund-specific eligibility controls (Z_i). As shown in Table 9, these quantile estimates also provide little evidence that financial assistance has a substantial effect on healthcare charges, though results become quite imprecise at high percentiles. The effect at 24 months for the 75th percentile is small (\$27), and we can reject differences larger than ±14% of the mean. The estimates are less precise further up the distribution. At the 90th percentile, we cannot reject the hypothesis that charges declined by \$7,246 (15.5%) or increased by \$7,842 (16.8%).

We similarly find no strong evidence of changes in total charges to public insurance programs. In our sample, 62% of healthcare visits list Medicaid or Medicare as the primary payer. Since healthcare facilities may charge different prices to different types of payers, treatment effects on healthcare usage may be obscured if treatment shifts how people pay for healthcare, e.g. from public programs to private insurance that reimburses at higher rates for the same service. First, in Appendix Table B5 we show that treatment does not appear to affect the propensity to have publicly-paid visits.[14] Then, as with total charges, the results in Appendix Table B6 show that we cannot reject a null effect on publicly-paid charges and can rule out moderate-to-large effects in either direction.

6   Conclusion

In this paper, we study healthcare use for people who are seeking emergency financial assistance to address a large negative income shock. We compare the outcomes of callers who are referred to assistance to those of callers who are denied. Conditional on a small set of observable characteristics that help determine access to some funds, the difference in outcomes between these two groups measures the causal effect of being referred to funds because denial only occurs for observable

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[14]Note that this result implies that gaining access to publicly-provided healthcare (e.g., via public benefit screening from HPCC staff) is not likely to be an important channel for the main utilization effects.

reasons or due to quasi-random variation in the availability of funds. We link both groups to healthcare use records and observe outcomes before and after the call.

Our results suggest that the effect of smoothing income shocks on overall healthcare use is small. The 95% confidence interval can reject increases in the incidence of healthcare visits of more than 7% and decreases of more than 4%. Financial assistance appears to have little impact on overall healthcare use because the shock itself does not increase healthcare use; callers who are turned away from financial assistance use similar amounts of healthcare before and after calling. These facts align with previous null effects for the effect of positive income shocks on healthcare use, e.g. from winning the lottery (Cesarini et al., 2016). However, exploratory analysis suggests that those referred to funds make fewer visits for more narrowly defined outcomes, such as those related to mental health and violence.

At least two different theories could explain the unresponsiveness of healthcare use to both shocks and assistance. On one hand, a response of consumption to income shocks is usually interpreted as evidence of imperfect access to insurance, credit, and savings (Gruber, 1994; Ganong et al., 2020). Our results may indicate that the low-income households that call the HPCC are able to smooth healthcare consumption using existing public programs. On the other hand, our results could result from offsetting effects of income on the need for and the ability to pay for healthcare. Negative shocks may increase healthcare use by increasing morbidity, but simultaneously dampen it by making it harder to access care when needed.

Even if overall health effects are limited, emergency financial assistance would still likely pass a cost-benefit test. Other studies of the same intervention argue that emergency financial assistance generates net positive social benefits by reducing emergency shelter entry (Evans et al., 2016) and perpetration of violence (Palmer et al., 2019). Any reduction in healthcare use related to violence victimization would only tilt the cost-benefit test in favor of assistance. We also note that temporary assistance may affect health in ways which are meaningful but outside the scope of this analysis. In particular, our data only include treatments received during emergency and inpatient hospital visits, so we cannot measure outcomes related to mortality nor outcomes that relate to morbidity

but do not result in measurable healthcare. Finally, we are unable to estimate changes in health outcomes over time horizons longer than two years, which limits our measurement of the impact of assistance on chronic or slow-evolving conditions.

Programs that provide emergency financial assistance have expanded significantly during the COVID-19 pandemic to address the sharp rise in economic shocks and other disruptions. For example, rates of utility disconnections doubled in 2020 (Cicala, 2021). Police calls for domestic violence also increased (Leslie and Wilson, 2020; Bullinger et al., 2020). In response, the Federal Government has allocated more than $70 billion to state and local governments for emergency housing assistance programs since the beginning of the pandemic. (CARES Act, 2020; Consolidated Appropriations Act, 2020; American Rescue Plan Act, 2021). Evidence on the effectiveness of emergency financial assistance will continue to be of particular importance given the massive need for and provision of emergency financial assistance during times of crisis.

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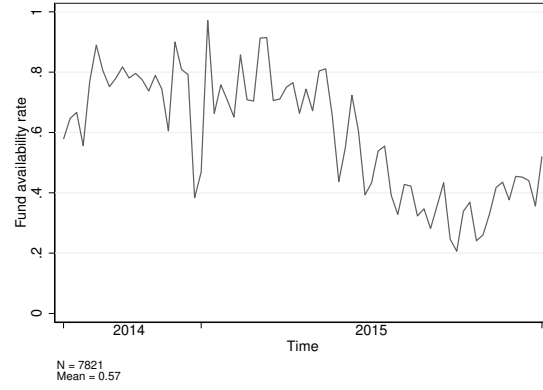
(a) Full sample
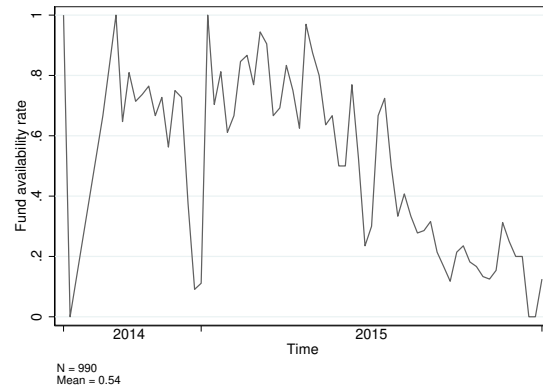
(b) Needs rent, need > 1 month's rent, need >
$900, no other housing subsidy
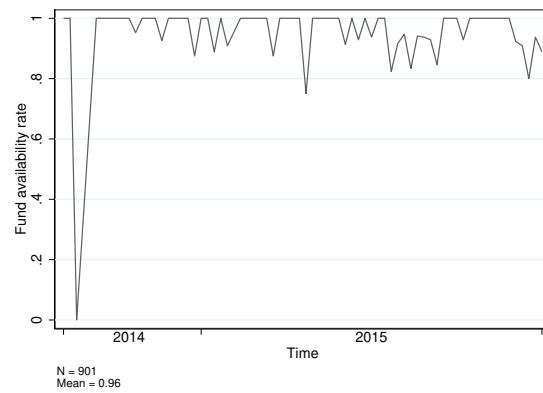
(c) Needs rent, need ≤ 1 month's rent, $650 <
need ≤ $900, no other housing subsidy

Figure 1: Fund availablity rate, by week

Notes: Samples include all eligible callers from August 1, 2014 to December 31, 2015, with varying criteria applied. For any week, the fund availability rate is equal to the number of eligible callers for whom funds were available divided by the total number of eligible callers.
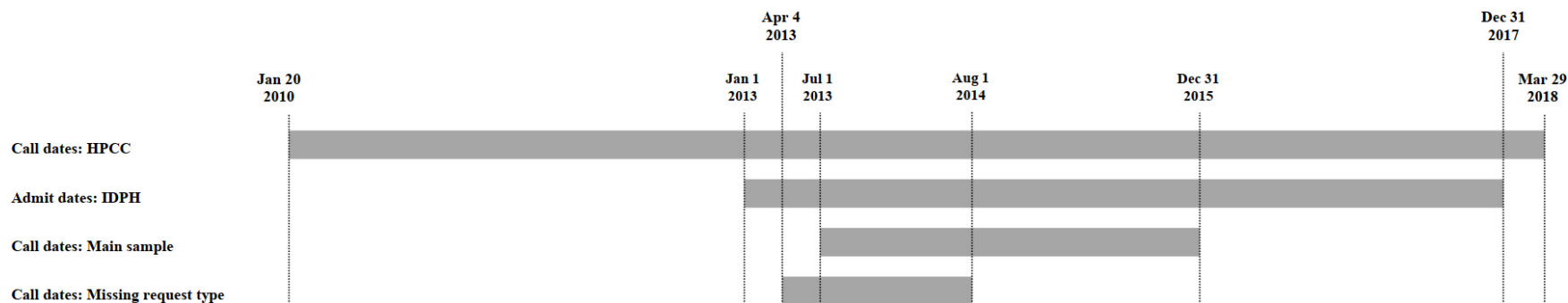
Figure 2: Data and sample timelines

Notes: The main sample refers to the sample defined in Section 3.3 (N = 8,585). The July 1, 2013 start date for the main sample guarantees that all calls are associated with at least 6 months of pre-call IDPH healthcare use data; the December 31, 2015 end date for the main sample guarantees that all calls are associated with at least 24 months of post-call IDPH data. See Section 3.1 for a full discussion of the HPCC call center data, and see Section 3.2 for a full discussion of the IDPH healthcare use data. We do not observe the type of need request (e.g., rent, security deposit) for calls between April 4, 2013 and July 31, 2014.
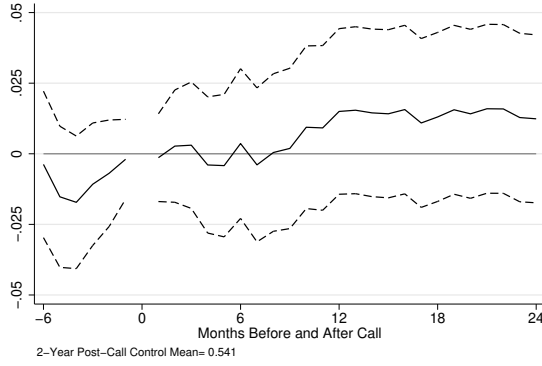
(a) ED visit
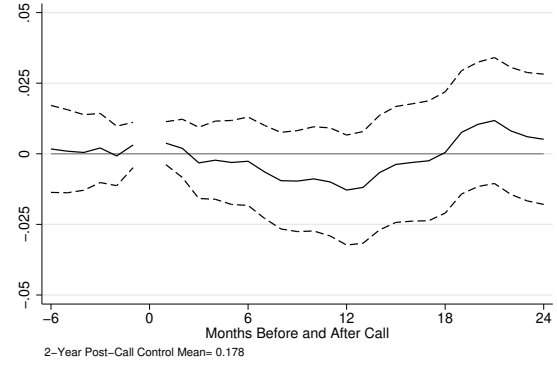
(b) Inpatient discharge
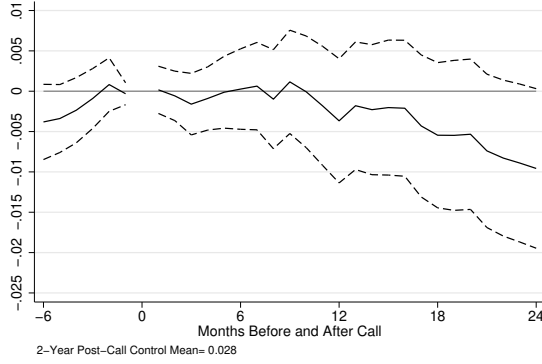
(c) Assault
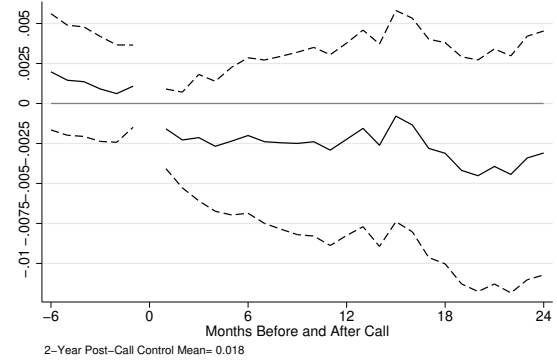
(d) Suicide

Figure 3: Effect of referral to funds on probability of healthcare use since call

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. N = 8,585. The solid line plots the coefficient on fund availability from a regression where the dependent variable is a dummy for having ever received care for the specified outcome in the τ months before or after calling. To the left of zero, the outcome is pre-multiplied by -1, so that the interpretation of the pre-trend matches that of the post-trend. However, pre-multiplication also implies that the signs of pre-period coefficients are flipped. The regression includes a fund availability dummy and controls (vectors Z_i and X_i), as in Eq. (2). See the notes of Table 5 for a list of controls. The dashed lines show 95% confidence intervals with heteroskedasticity-robust standard errors.

(a) ED visit

(b) Inpatient discharge

(c) Assault

(d) Suicide
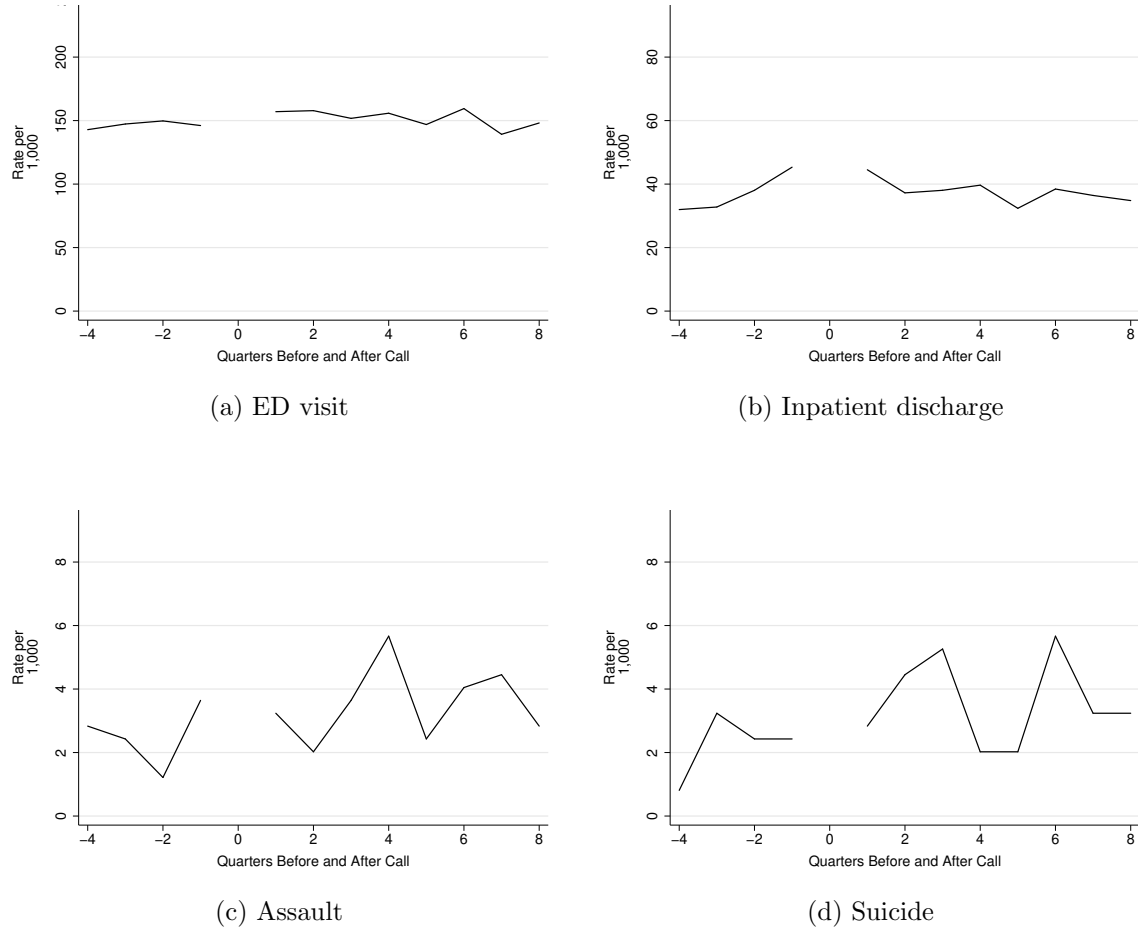
Figure 4: Unconditional Utilization Means: Control Group

Notes: N = 2,471. Outcomes are dummy variables for having received the specified type of treatment in each respective quarter before/after the call, and are centered around the month of the call. Outcomes during the call month are omitted. Results are for a sample of eligible first-time callers within the last 6 months. See Section 3.3 for additional restrictions. Calls from Jan 1, 2014 - Dec 31, 2015 are included, to allow for a 12-month pre-period and a 24-month post-period.
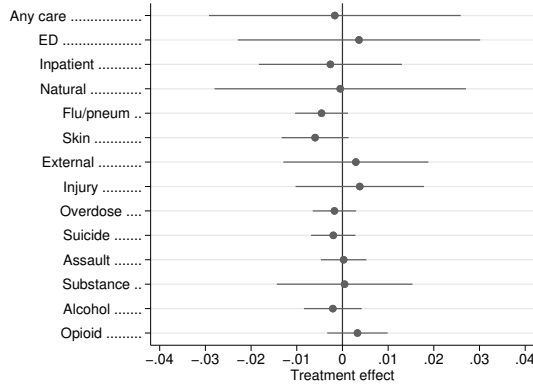
(a) 6 Months - Absolute Magnitude
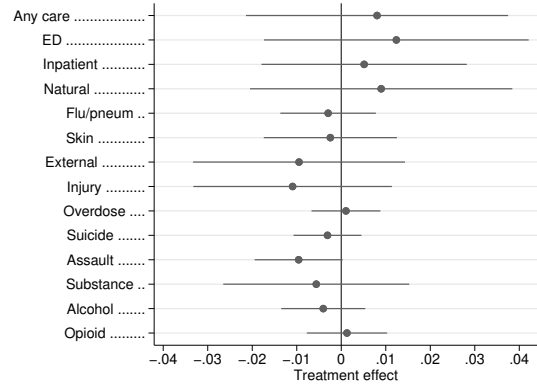
(b) 24 Months - Absolute Magnitude
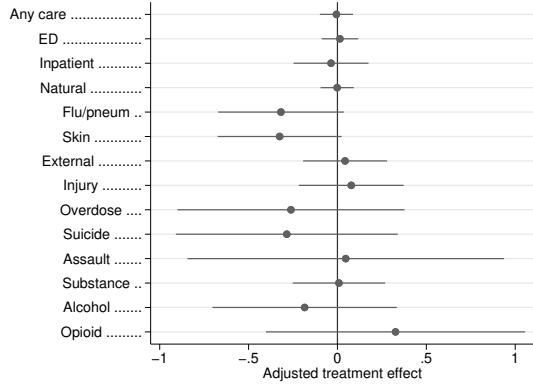
(c) 6 Months - Relative to Mean
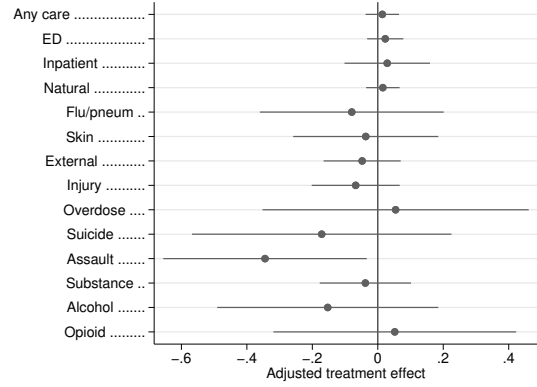
(d) 24 Months - Relative to Mean

Figure 5: Effect of referral to funds on probability of healthcare, by treatment type

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. N = 8,585. Each point estimate corresponds to a separate regression coefficient on fund availability. The outcome is a dummy for receiving treatment for the listed type of care within the specified time frame after the call. All regressions include calendar and fund-specific eligibility controls (Z_i), as well as controls for other observables (X_i), as in Eq. (2). See the notes of Table 5 for a list of controls. In panels (a) and (b), point estimates are absolute treatment effects, and we compute heteroskedasticity-robust standard errors. In panels (c) and (d), we scale treatment effects by dividing the funds coefficient by the control group mean for the specified outcome and time frame. We derive standard errors using a non-linear Wald test. In all cases, the whiskers depict 95% confidence intervals.

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Table 1: HPCC Call Volume
January 1, 2013-December 31, 2017

| | (1) | (2) |
Sample composition	N	% funds avail.
All calls	165,319	11.5
All eligible calls	21,497	66.3
All calls on or before 12/31/2015	15,499	64.7
All calls on or after 7/1/2013	13,762	62.7
First call in 6 months	8,585	65.2
First call ever	6,459	65.8

Notes: The sample restrictions for each row include the restrictions imposed in all rows above it. For example, the sample defined in row 3 includes all eligible calls on or before 12/31/2015. Row 1 includes all calls from January 1, 2013 - December 31, 2017. Eligible calls (row 2) are defined as in Section 2. Since health data from IDPH is available until December 31, 2017, calls on or before December 31, 2015 (row 3) are guaranteed to have at least 24 months of observed health outcomes post-call. And, since the IDPH data begins on January 1, 2013, calls on or after July 1, 2013 (row 4) are guaranteed to have at least 6 months of observed health outcomes pre-call. The sample in row 5, column 1 (N = 8,585) is the main analysis sample. Also see Figure 2 for a visual representation of data and sample time frames. The fund availability rate in column (2) is defined to be the number of callers who are referred to temporary financial assistance divided by the total number of callers.

Table 2: Hospital utilization, HPCC vs. Cook County, IL
Inpatient discharges per 1,000 population, individuals aged 18-64

Admit Year	(1) HPCC Raw	(2) HPCC Adjusted	(3) Cook County
2013	149.0 (12.7)	143.0 (28.7)	99.0
N	3,382	3,382	3,343,950
2014	147.9 (10.6)	185.6 (24.0)	99.1
N	3,319	3,319	3,352,020
2015	161.6 (16.9)	147.4 (31.0)	97.6
N	1,955	1,955	3,349,827
2016	137.8 (14.2)	126.5 (25.7)	94.1
N	1,771	1,771	3,347,700
All	149.1 (6.6)	153.5 (14.1)	97.4
N	10,427	10,427	13,393,497

Notes: All rates are inpatient discharges per 1,000 population. In the bottom row, we present the average rate for all years (2013-2016), where N is equal to the sum of Ns over years. Standard errors are in parentheses. Rates in columns (1) and (2) are based on data from a sample of HPCC callers. As in our main sample, we restrict to eligible callers, first calls in 6 months, and individuals for whom living situation (e.g., rent, own, shared housing) is not missing. Here, we consider calls from January 1, 2014 - December 31, 2017 in order to include a maximum number of admit years. To ensure that treatment does not affect reported healthcare utilization, we define inpatient discharges for this HPCC sample as the sum of all visits made during the calendar year prior to the call year. As a result, discharges associated with calls in year t are categorized under Admit Year t-1 in the table. For example, the numerator for the raw HPCC rate in admit year 2013 is equal to the number of inpatient discharges which (1) are associated with calls in 2014 and (2) occurred in 2013. Column (1) shows raw rates, while Column (2) shows rates weighted according to gender, age, poverty status and race. Probability weights are constructed using data from the 2009-2013 ACS 5-year Illinois person file. We adjust the HPCC sample so that it demographically matches the Cook County population. In some cases where cell size is small, we collapse adjacent cells to improve precision. Annual data from Cook County, presented in column (3), are publicly available from the Healthcare Utilization Project (https://hcupnet.ahrq.gov).

Table 3: Outcomes and fund-specific eligibility factors among HPCC callers, by availability of funds

	(1) All	(2) Control	(3) Treatment	(4) Difference
Outcomes, 24 mos. post call				
Any care	0.563	0.580	0.554	-0.026**
				(0.011)
ED visit	0.529	0.541	0.522	-0.019*
				(0.011)
Inpatient discharge	0.169	0.178	0.164	-0.015*
				(0.009)
Assault	0.026	0.028	0.025	-0.003
				(0.004)
Suicide	0.014	0.018	0.012	-0.006**
				(0.003)
Fund-specific eligibility factors				
Applying for rent assistance	0.607	0.319	0.795	0.475***
				(0.013)
Applying for security deposit assistance	0.073	0.114	0.047	-0.067***
				(0.008)
$900 or more in need	0.394	0.443	0.368	-0.075***
				(0.011)
Veteran	0.033	0.030	0.034	0.004
				(0.005)
Receiving housing subsidy	0.320	0.576	0.184	-0.393***
				(0.010)
Requesting > 1 month of rent	0.511	0.762	0.376	-0.386***
				(0.010)
Income > 2x poverty line	0.022	0.010	0.028	0.018***
				(0.003)
Has Social Security no.	0.985	0.983	0.986	0.003
				(0.003)
Living situation: own housing	0.018	0.018	0.018	-0.000
				(0.003)
Senior	0.031	0.049	0.021	-0.028***
				(0.004)
Disabled	0.046	0.049	0.045	-0.004
				(0.005)
N	8,585	2,988	5,597	8,585

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. See Section 3.3 for additional restrictions. The first three columns show simple means for observations with non-missing values. The final column shows the difference (treatment minus control) as measured by a regression of the outcome on a fund availability dummy and no controls; heteroskedasticity-robust standard errors are in parentheses.
* - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Table 4: Baseline balance, all periods

	(1)	(2)	(3)	(4)
	Sample Mean	Control Mean	Treatment Mean	Adjusted Difference
Predicted outcomes, 24 mos. post call				
Any care	0.574	0.580	0.570	-0.004
ED visit	0.533	0.541	0.528	-0.004
Inpatient discharge	0.169	0.178	0.164	-0.003
Assault	0.036	0.028	0.040	-0.001
Suicide	0.015	0.018	0.014	-0.000
Lagged outcomes, 6 mos. pre call				
Any care	0.284	0.290	0.281	0.004
ED visit	0.248	0.252	0.246	0.001
Inpatient discharge	0.068	0.075	0.065	-0.003
Assault	0.008	0.005	0.009	0.004
Suicide	0.005	0.006	0.005	-0.002
Caller covariates				
Female	0.806	0.838	0.789	-0.019
Black	0.888	0.915	0.874	-0.020**
White	0.077	0.057	0.088	0.011
Hispanic	0.081	0.064	0.090	0.016**
Age	38.8	40.0	38.1	0.600*
Number of adults in caller's household	1.413	1.448	1.394	-0.041*
Number of minors in caller's household	1.381	1.524	1.305	-0.142***
ACS - Median log household income	10.38	10.37	10.39	-0.003
ACS - Median monthly housing cost	987.82	984.24	989.74	11.84**
ACS - Pct. of population = Black	0.645	0.658	0.639	-0.017
Applying due to benefit loss	0.098	0.094	0.100	-0.004
Applying due to inability to pay bills	0.026	0.053	0.008	-0.019***
Applying due to shared housing exit	0.017	0.022	0.013	-0.026***
Applying due to job loss	0.312	0.258	0.346	0.016
Monthly income (thousands)	1.237	1.073	1.324	-0.056***
Living in rental housing	0.855	0.845	0.860	0.032***
Receiving SNAP	0.494	0.408	0.540	-0.010
N	8,585	2,988	5,597	8,585

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. See Section 3.3 for additional restrictions. The first three columns show simple means for observations with non-missing values. The final column shows the difference (treatment minus control) as measured by a regression of the outcome on a fund availability dummy and a vector of calendar and fund-specific eligibility controls (Z_i). Calendar and fund-specific eligibility controls include linear controls for: the rank of the call within the day, dummies for need amount category interacted with call year and call year-quarter, dummies for whether caller contribution or debt need are missing interacted with call year, day of the week, time of month, veteran status, housing subsidy receipt, need > 1 month rent, income > twice the poverty line, having an SSN, need request type, owning one's dwelling, senior status, receiving disability payments, and age.
* - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Table 5: OLS estimates of the effect of fund availability on the cumulative probability of healthcare use

	(1)	(2)	(3)	(4)
	6 mos.	12 mos.	18 mos.	24 mos.
Any care	-0.002	0.005	0.004	0.008
	(0.014)	(0.015)	(0.015)	(0.015)
	[0.905]	[0.719]	[0.776]	[0.592]
Control Mean	0.295	0.430	0.519	0.580
ED visit	0.004	0.015	0.013	0.012
	(0.014)	(0.015)	(0.015)	(0.015)
	[0.789]	[0.316]	[0.393]	[0.414]
Control Mean	0.259	0.390	0.480	0.541
Inpatient discharge	-0.003	-0.013	0.000	0.005
	(0.008)	(0.010)	(0.011)	(0.012)
	[0.741]	[0.196]	[0.966]	[0.662]
Control Mean	0.073	0.120	0.151	0.178
Assault	0.000	-0.004	-0.005	-0.010*
	(0.003)	(0.004)	(0.005)	(0.005)
	[0.917]	[0.350]	[0.235]	[0.058]
Control Mean	0.006	0.015	0.020	0.028
Suicide	-0.002	-0.002	-0.003	-0.003
	(0.002)	(0.003)	(0.004)	(0.004)
	[0.420]	[0.466]	[0.377]	[0.426]
Control Mean	0.007	0.011	0.016	0.018
N	8,585	8,585	8,585	8,585

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. See Section 3.3 for additional restrictions. Each cell shows the coefficient on funds availability from a separate regression. The outcome is a dummy for receiving treatment for the listed type of care within the specified timeframe after the call. All regressions include calendar and fund-specific eligibility controls (Z_i), as well as controls for other observables (X_i). Calendar and fund-specific eligibility controls (Z_i) include linear controls for: the rank of the call within the day, dummies for need amount category interacted with call year and call year-quarter, dummies for whether caller contribution or debt need are missing interacted with call year, day of the week, time of month, veteran status, housing subsidy receipt, need > 1 month rent, income > twice the poverty line, having an SSN, need request type, owning one's dwelling, senior status, receiving disability payments, and age. Other observable characteristics (X_i) include zipcode-level variables from the American Community Survey (ACS) for high school graduation rate, household income, median age, housing cost, labor force participation rate, racial composition, and unemployment rate, and also include individual-level variables for the number of adults in the home, the number of minors in the home, monthly income, and dummies for the reason for fund application (benefit loss, can't afford bills, exiting shared housing, exiting shelter, fleeing abuse, job loss), gender, race, Hispanic, and whether the caller is currently receiving various types of public benefits (child support, EITC, disability income, SSI, TANF, SNAP, unemployment insurance, other). Heteroskedasticity-robust standard errors are in parentheses; p-values are in brackets. * - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Table 6: Control group pre-post differences in the monthly flow probability of receiving healthcare

	(1)	(2)	(3)	(4)
		Months after call		
	1-6	7-12	13-18	19-24
Any care	0.0021	0.0033	-0.0004	-0.0049
	(0.0027)	(0.0029)	(0.0029)	(0.003)
	[0.435]	[0.250]	[0.894]	[0.111]
Pre period mean	0.0734	0.0734	0.0734	0.0734
ED visit	0.0028	0.0036	0.0016	-0.0027
	(0.0024)	(0.0027)	(0.0027)	(0.0028)
	[0.243]	[0.178]	[0.562]	[0.331]
Pre period mean	0.0601	0.0601	0.0601	0.0601
Inpatient discharge	-0.0011	-0.0008	-0.0027*	-0.0028*
	(0.0014)	(0.0015)	(0.0014)	(0.0015)
	[0.420]	[0.594]	[0.060]	[0.064]
Pre period mean	0.0175	0.0175	0.0175	0.0175
Assault	0.0001	0.0008**	0.0003	0.0005
	(0.0003)	(0.0004)	(0.0003)	(0.0004)
	[0.862]	[0.036]	[0.423]	[0.170]
Pre period mean	0.0009	0.0009	0.0009	0.0009
Suicide	0.0003	0.0003	0.0001	0.0001
	(0.0004)	(0.0004)	(0.0004)	(0.0005)
	[0.423]	[0.446]	[0.889]	[0.908]
Pre period mean	0.0013	0.0013	0.0013	0.0013
N (caller-months)	35,856	35,856	35,856	35,856
N (callers)	2,988	2,988	2,988	2,988

Notes: Results are for control group callers from our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. See Section 3.3 for additional restrictions. We construct estimates from a panel dataset at the caller-month level. Each cell shows the coefficient on a dummy for the post-call period from a separate regression. The outcome is a dummy for receiving treatment for the specified type of care during a given month. All columns include observations from the 6 months before the call; in addition, column (1) includes the first 6 months after the call, column (2) includes months 7-12 after the call, and so on. In all cases, we omit outcomes from the month of the call. The pre-period mean is interpretable as the mean monthly propensity to receive the specified type of care for control group callers during pre-call months. We report standard errors, clustered at the caller-level, in parentheses; p-values are in brackets. No covariates are included in estimation.
* - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Table 7: Robustness, 24-month outcomes

	(1) Baseline	(2) 1st call in 3 months	(3) 1st call ever	(4) Calls on or after 1/1/2013	(5) Period w/ all fund elig. controls	(6) Calendar, fund elig. controls only	(7) Outcomes incl. call months
Any care	0.008 (0.015) [0.592]	0.003 (0.014) [0.820]	0.018 (0.017) [0.298]	0.003 (0.014) [0.806]	0.004 (0.021) [0.862]	0.002 (0.015) [0.872]	0.003 (0.015) [0.865]
Control Mean	0.580	0.584	0.558	0.582	0.568	0.580	0.594
ED visit	0.012 (0.015) [0.414]	0.005 (0.015) [0.727]	0.025 (0.018) [0.157]	0.007 (0.014) [0.603]	0.010 (0.021) [0.643]	0.007 (0.015) [0.644]	0.006 (0.015) [0.711]
Control Mean	0.541	0.547	0.518	0.543	0.535	0.541	0.556
Inpatient discharge	0.005 (0.012) [0.662]	0.002 (0.011) [0.836]	0.004 (0.014) [0.754]	0.008 (0.011) [0.446]	0.001 (0.016) [0.928]	0.003 (0.012) [0.803]	0.006 (0.012) [0.606]
Control Mean	0.178	0.181	0.175	0.179	0.171	0.178	0.182
Assault	-0.010* (0.005) [0.058]	-0.010** (0.005) [0.040]	-0.010* (0.006) [0.093]	-0.012** (0.005) [0.016]	-0.008 (0.007) [0.253]	-0.011** (0.005) [0.026]	-0.011** (0.005) [0.027]
Control Mean	0.028	0.028	0.026	0.030	0.027	0.028	0.029
Suicide	-0.003 (0.004) [0.426]	-0.004 (0.004) [0.277]	-0.004 (0.005) [0.333]	-0.003 (0.004) [0.360]	-0.005 (0.005) [0.259]	-0.003 (0.004) [0.511]	-0.004 (0.004) [0.353]
Control Mean	0.018	0.018	0.018	0.017	0.017	0.018	0.019
N	8,585	9,318	6,459	9,749	4,845	8,585	8,585

Notes: Each cell shows the coefficient on funds availability from a separate regression. The outcome is a dummy for receiving treatment for the listed type of care within 24 months of calling. All specifications use a 24-month follow-up period. Column (1) is identical to the main analysis in Table 5. This sample restricts to first calls in 6 months and to calls on or after July 1, 2013; regressions include all available controls (see Table 5 notes for a full description), and outcomes are defined to exclude the month of the call. Specifications (2)-(7) each deviate from the baseline specification in only the way described. (2) restricts to first calls in 3 months, while (3) restricts to first calls ever. (4) expands the sample to include calls on or after January 1, 2013. (5) revises the sample to specifically exclude calls from April 4, 2013 through July 31, 2014, when the type of need request (e.g., rent, security deposit) is not observed. (6) includes only calendar and fund-specific eligibility controls (Z_i, as defined in the notes of Table 5). (7) re-defines outcomes to include visits from the month of the call. Heteroskedasticity-robust standard errors are in parentheses; p-values are in brackets.
* - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Table 8: Effect of fund availability on healthcare use, by subgroup, 24 months

	(1)	(2)	(3)	(4)	(5)	(6)
	Male	> Median income	Age 30+	> Median need	Any Kids	> Median risk
Any care						
Funds	0.008	-0.037*	0.029	-0.018	-0.029	0.039*
	(0.017)	(0.021)	(0.026)	(0.020)	(0.025)	(0.022)
Funds x Characteristic	-0.015	0.097***	-0.027	0.067**	0.059*	-0.066**
	(0.039)	(0.031)	(0.032)	(0.031)	(0.031)	(0.028)
Control mean, char. = 0	0.595	0.610	0.605	0.593	0.578	0.458
Control mean, char. = 1	0.501	0.534	0.568	0.562	0.580	0.719
ED visit						
Funds	0.011	-0.035*	0.041	-0.007	-0.022	0.032
	(0.017)	(0.021)	(0.027)	(0.020)	(0.025)	(0.023)
Funds x Characteristic	-0.006	0.103***	-0.039	0.046	0.056*	-0.043
	(0.039)	(0.031)	(0.033)	(0.031)	(0.032)	(0.029)
Control mean, char. = 0	0.557	0.571	0.565	0.551	0.533	0.462
Control mean, char. = 1	0.462	0.496	0.531	0.528	0.545	0.635
Inpatient discharge						
Funds	0.019	-0.007	0.009	-0.006	-0.024	0.010
	(0.013)	(0.017)	(0.021)	(0.016)	(0.020)	(0.014)
Funds x Characteristic	-0.087***	0.029	-0.005	0.025	0.044*	-0.007
	(0.029)	(0.024)	(0.026)	(0.024)	(0.025)	(0.021)
Control mean, char. = 0	0.177	0.203	0.193	0.190	0.216	0.136
Control mean, char. = 1	0.184	0.141	0.172	0.162	0.159	0.218
Assault						
Funds	-0.008	-0.011*	0.001	-0.013*	-0.011	-0.008
	(0.005)	(0.007)	(0.010)	(0.007)	(0.008)	(0.005)
Funds x Characteristic	-0.014	0.003	-0.014	0.001	0.001	-0.003
	(0.017)	(0.011)	(0.012)	(0.011)	(0.010)	(0.009)
Control mean, char. = 0	0.024	0.027	0.034	0.028	0.029	0.026
Control mean, char. = 1	0.050	0.030	0.025	0.028	0.027	0.028
Suicide						
Funds	-0.002	-0.000	-0.008	0.006	0.002	-0.003
	(0.004)	(0.006)	(0.006)	(0.005)	(0.008)	(0.003)
Funds x Characteristic	-0.010	-0.005	0.008	-0.019**	-0.008	0.001
	(0.013)	(0.008)	(0.008)	(0.008)	(0.009)	(0.007)
Control mean, char. = 0	0.014	0.022	0.020	0.016	0.029	0.014
Control mean, char. = 1	0.037	0.013	0.017	0.020	0.013	0.022

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013–December 31, 2015. N = 8,585. See Section 3.3 for additional restrictions. The outcome is a dummy for receiving the specified type of care within 2 years of calling. "Funds" refers to an indicator for fund availability and "characteristic" refers to a dummy for the characteristic listed in the column titles. We estimate the funds and funds × characteristic coefficients from a regression that includes the control variables from Table 5 as well as the interaction of these controls with the characteristic dummy. Heteroskedasticity-robust standard errors are in parentheses. In column (5), we estimate coefficients using the repeated sample split method of Abadie et al. (2018) as described in Section 5.5, and estimate standard errors from 1,000 bootstrap iterations.
* - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Table 9: OLS and quantile regression esimtates of the effect of fund availability on charges

	(1)	(2)	(3)
	12 mos.	18 mos.	24 mos.
Total Charges (thousands of USD)			
OLS: Baseline	-1.707	-1.083	-1.397
	(1.452)	(1.828)	(2.932)
	{11.783}	{16.896}	{23.060}
OLS: Fund-specific eligibility controls only	-1.652	-1.082	-1.355
	(1.442)	(1.826)	(2.998)
	{11.783}	{16.896}	{23.060}
Quantile: 75th percentile	-0.457	-1.163	-0.027
	(0.437)	(0.767)	(0.923)
	{4.826}	{8.803}	{12.825}
Quantile: 90th percentile	-1.668	-1.290	0.298
	(2.205)	(3.620)	(3.772)
	{23.172}	{37.195}	{46.500}
Control Pct. = 0	0.57	0.48	0.42
N	8,585	8,585	8,585

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. See 3.3 for additional restrictions. Each cell shows the coefficient on funds availability from a separate regression from our main sample of callers (as defined in Section 3.3). The outcome is: the sum of all charges (in thousands of 2017 USD), from the specified quantile of the distribution (75th or 90th), within the specified time frame after calling (12, 18 or 24 months). We do not show outcomes for 6 months after the call, since more than 70% of callers in the control group do not record any charges within 6 months of calling. The baseline OLS specifications include all available controls, while the second set includes only calendar and fund-specific eligibility controls Z_i. Quantile regressions include only calendar and fund-specific eligibility controls to ensure model convergence. For quantile regressions, the corresponding control percentile value is in curly braces; for OLS regressions, the control mean is in curly braces. We estimate analytic standard errors (in parentheses) as in (Parente and Silva, 2016).
* - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Appendix Material

Appendix A. ICD Codes to Outcomes

We construct outcomes by grouping healthcare visits by ICD codes. This procedure involves three steps: (1) mapping all diagnosis codes into the ICD-10 protocol, (2) aggregating ICD-10 codes up to the diagnosis category level, and (3) binning ICD-10 diagnosis categories into broad outcome groups.

(1) Map all diagnosis codes to ICD-10

Because hospitals and treatment centers in Illinois transitioned from the ICD-9 to the ICD-10 protocol during the time frame of our analysis, we make some adjustments to generate consistent ICD codes over time. Specifically, we use the 2018 general equivalence mappings (GEMs) from the National Center for Health Statistics (NCHS) to map ICD-9 codes from visits before October 2015 into ICD-10 (NCHS, 2018a). The GEM files do not provide a simple "crosswalk" from ICD-9 to ICD-10. Instead, the GEMs provide a link for each ICD-9 code to a set of all valid and acceptable ICD-10 codes (NCHS, 2018b). In the vast majority of cases, it is straightforward to link ICD-9 codes with corresponding ICD-10 codes in this way. In 0.9% of cases in our sample, however, the GEM file does not map an ICD-9 code to a usable ICD-10 code. We deal with these cases manually (for details, see Appendix Table A1).

(2) Aggregate ICD-10 codes to diagnosis categories

ICD-10 codes may contain up to 7 digits: characters 1-3 define the diagnosis category, characters 4-6 indicate the related etiology (i.e., cause, severity, anatomic site, or other clinical details), and the 7th character is an extension. When defining outcomes from ICD-10 codes, we first aggregate 7-digit ICD-10 codes up to the diagnosis category (3-digit) level.[15] We do this because ICD-10 codes are generally more precise than ICD-9 codes: there are approximately 70,000 ICD-10 codes and approximately 13,000 ICD-9 codes. But while ICD-9 codes often map to more than one 7-digit ICD-10 code, they rarely map to more than one 3-digit ICD-10 diagnosis category. In other words, the difference in precision between the two regimes generally exists at the etiology or extension level but not at the category level. For example, a single ICD-9 diagnosis code may be associated with several distinct 7-digit ICD-10 codes. In such cases, it is not possible

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[15]One important exception is our inclusion of R45851, "suicidal ideation," in the suicide outcome group. This is consistent with the ARHQ (2020)'s roster of suicide-related diagnosis codes. The category associated with this code (R45, "symptoms and signs involving emotional state") contains many codes which do not pertain to suicide, so it is necessary to specify to the etiology-level in this case.

to use mappings to add specificity when the original information is general, so there is no definitive way to deduce which 7-digit ICD-10 code is the correct mapping. However, if all candidate ICD-10 codes share the first 3 digits, we can complete the mapping at the diagnosis category level.[16] A key advantage of our categorization scheme, then, is that the transition from ICD-9 to ICD-10 is unlikely to affect the definition of outcomes, since it is robust to cases where ICD-10 codes are more precise than ICD-9 codes, and also to the limited number of cases where ICD-9 codes are more precise than ICD-10 codes.

(3) Group ICD-10 diagnosis categories

In a final step of aggregation, we bin each diagnosis category into groups informed by the chapters of the ICD-10 Codebook (WHO, 2008), definitions from CDC reference materials (2007, 2013 and 2018), and categorization schema from previous work in the public health and economics literatures (Roncarati et al. (2018), Ruhm (2018), Shah et al. (2015)). We present these groups in detail in Appendix Table A2. Any given visit may correspond to multiple diagnosis codes, and therefore may be associated with various outcomes. While some outcomes (e.g., natural vs. external cause) are mutually exclusive, outcomes may be subsets of other outcomes, or may otherwise overlap. If any one of the diagnosis codes associated with a visit belongs to a certain outcome group, we code the patient as having received healthcare for that outcome group regardless of whether the linking code is the admitting diagnosis or an auxiliary diagnosis documented after admission.

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[16]One diagnosis under the ICD-9 system may also link to multiple ICD-10 codes in different outcome groups. In these cases, we count the visit as belonging to all such outcome groups.

Table A1: Manual ICD Mappings

ICD-9 Code	ICD-9 Description	Issue	ICD-10 Code(s)	ICD-10 Description(s)
29285	Drug-induced sleep disorders	Maps to many plausible codes in various outcome groups	F19	Mental and behavioural disorders due to multiple drug use and use of other psychoactive substances
29289	Other specified drug-induced sleep disorders	Maps to many plausible codes in various outcome groups	F19	Mental and behavioural disorders due to multiple drug use and use of other psychoactive substances
E9309	Unspecified antibiotic causing adverse effects in therapeutic use	Maps to "No Dx"	T36	Poisoning by systemic antibiotics
E9318	Other antimycobacterial drugs causing adverse effects in therapeutic use	Maps to "No Dx"	T37	Poisoning by other systemic anti-infectives and antiparasitics
E8508	Accidental poisoning by other specified analgesics and antipyretics	Maps to "No Dx"	T39	Poisoning by nonopioid analgesics, antipyretics and antirheumatics
E8509	Accidental poisoning by unspecified analgesic or antipyretic	Maps to "No Dx"	T39	Poisoning by nonopioid analgesics, antipyretics and antirheumatics
E9354	Aromatic analgesics, not elsewhere classified, causing adverse effects in therapeutic use	Maps to "No Dx"	T39	Poisoning by nonopioid analgesics, antipyretics and antirheumatics
E9358	Other specified analgesics and antipyretics causing adverse effects in therapeutic use	Maps to "No Dx"	T39	Poisoning by nonopioid analgesics, antipyretics and antirheumatics
E9359	Unspecified analgesic and antipyretic causing adverse effects in therapeutic use	Maps to "No Dx"	T39	Poisoning by nonopioid analgesics, antipyretics and antirheumatics
E9386	Peripheral nerve- and plexus-blocking anesthetics causing adverse effects in therapeutic use	Maps to "No Dx"	T41	Poisoning by anaesthetics and therapeutic gases
E9387	Spinal anesthetics causing adverse effects in therapeutic use	Maps to "No Dx"	T41	Poisoning by anaesthetics and therapeutic gases
E851	Accidental poisoning by barbiturates	Maps to "No Dx"	T42	Poisoning by antiepileptic, sedative-hypnotic and antiparkinsonism drugs
E8529	Accidental poisoning by unspecified sedative or hypnotic	Maps to "No Dx"	T42	Poisoning by antiepileptic, sedative-hypnotic and antiparkinsonism drugs
E9380	Central nervous system muscle-tone depressants causing adverse effects in therapeutic use	Maps to "No Dx"	T42	Poisoning by antiepileptic, sedative-hypnotic and antiparkinsonism drugs
E8538	Accidental poisoning by other specified tranquilizers	Maps to "No Dx"	T43	Poisoning by psychotropic drugs, not elsewhere classified
E8559	Accidental poisoning by unspecified drug acting on central and autonomic nervous systems	Maps to "No Dx"	T44	Poisoning by drugs primarily affecting the autonomic nervous system
E9307	Antineoplastic antibiotics causing adverse effects in therapeutic use	Maps to "No Dx"	T45	Poisoning by primarily systemic and haematological agents, not elsewhere classified
E9347	Natural blood and blood products causing adverse effects in therapeutic use	Maps to "No Dx"	T45	Poisoning by primarily systemic and haematological agents, not elsewhere classified
99529	Unspecified adverse effect of other drug, medicinal and biological substance	Maps to many plausible codes in various outcome groups	T50	Poisoning by diuretics and other and unspecified drugs, medicaments and biological substances
E8589	Accidental poisoning by unspecified drug	Maps to "No Dx"	T50	Poisoning by diuretics and other and unspecified drugs, medicaments and biological substances
E9443	Saluretics causing adverse effects in therapeutic use	Maps to "No Dx"	T50	Poisoning by diuretics and other and unspecified drugs, medicaments and biological substances
E860	Accidental poisoning by alcohol not elsewhere classified	Maps to "No Dx"	T51	Toxic effect of alcohol
E8600	Accidental poisoning by alcoholic beverages	Maps to "No Dx"	T51	Toxic effect of alcohol

E867	Accidental poisoning by gas distributed by pipeline	Maps to "No Dx"	T59	Toxic effect of other gases, fumes and vapours
E8694	Second hand tobacco smoke	Maps to "No Dx"	T59	Toxic effect of other gases, fumes and vapours
E9829	Poisoning by unspecified gases and vapors, undetermined whether accidentally or purposely inflicted	Maps to "No Dx"	T59	Toxic effect of other gases, fumes and vapours
E8633	Accidental poisoning by mixtures of insecticides	Maps to "No Dx"	T60	Toxic effect of pesticides
E8636	Accidental poisoning by fungicides	Maps to "No Dx"	T60	Toxic effect of pesticides
E9059	Poisoning and toxic reactions caused by unspecified animals and plants	Maps to "No Dx"	T63	Toxic effect of contact with venomous animals
E8614	Accidental poisoning by disinfectants	Maps to "No Dx"	T65	Toxic effect of other and unspecified substances
E8669	Accidental poisoning by unspecified solid or liquid substance	Maps to "No Dx"	T65	Toxic effect of other and unspecified substances
E989	Late effects of injury, undetermined whether accidentally or purposely inflicted	Maps to many plausible codes in various outcome groups	T98	Sequelae of other and unspecified effects of external causes
E95xx[17]	Suicide and self-inflicted injury or poisoning	Maps to "No Dx"	X84	Intentional self-harm by unspecified means
E96xx[18]	Assault	Maps to "No Dx"	Y09	Assault by unspecified means
E9804	Poisoning by other specified drugs and medicinal substances, undetermined whether accidentally or purposely inflicted	Maps to many plausible codes in various outcome groups	Y14	Poisoning by and exposure to other and unspecified drugs, medicaments and biological substances, undetermined intent
E9809	Poisoning by other and unspecified solid and liquid substances, undetermined whether accidentally or purposely inflicted	Maps to many plausible codes in various outcome groups	Y14	Poisoning by and exposure to other and unspecified drugs, medicaments and biological substances, undetermined intent
E872	Failure of sterile precautions during procedure	No mapping provided	Y62	Failure of sterile precautions during surgical and medical care
E875	Contaminated or infected blood other fluid drug or biological substance	No mapping provided	Y64	Contaminated medical or biological substances
E8700	Accidental cut, puncture, perforation or hemorrhage during surgical operation	Maps to "No Dx"	Y65	Other misadventures during surgical and medical care
E8708	Accidental cut, puncture, perforation or hemorrhage during other specified medical care	Maps to "No Dx"	Y65	Other misadventures during surgical and medical care
E871	Foreign object left in body during procedure	Maps to "No Dx"	Y65	Other misadventures during surgical and medical care
E8710	Foreign object left in body during surgical operation	Maps to "No Dx"	Y65	Other misadventures during surgical and medical care
E8718	Foreign object left in body during other specified procedures	Maps to "No Dx"	Y65	Other misadventures during surgical and medical care
E876	Other and unspecified misadventures during medical care	No mapping provided	Y65; Y69	Other misadventures during surgical and medical care; Unspecified misadventure during surgical and medical care
E870	Accidental cut puncture perforation or hemorrhage during medical care	Maps to "No Dx"	Y65; Y83; Y84	Misadventures and complications from misadvantures of surgical or medical care
E8704	Accidental cut, puncture, perforation or hemorrhage during endoscopic examination	Maps to "No Dx"	Y83	Misadventures and complications from misadvantures of surgical or medical care
E8705	Accidental cut, puncture, perforation or hemorrhage during aspiration of fluid or tissue, puncture, and catheterization	Maps to "No Dx"	Y84	Misadventures and complications from misadvantures of surgical or medical care

[17]See CDC (2007).
[18]See CDC (2007).

Table A2: ICD Code Categorization

Outcome Group	ICD-10 Diag. Category	Description	Source(s)
External causes			
Alcohol use	F10, K70, X45, Y15	Diagnoses relating to chronic or excessive alcohol consumption, excluding cases of suicidal intent.	Roncarati et al. (2018)
Assault	T74,[19] X85-99, Y00-09	Injuries inflicted by another person with intent to injure or kill, by any means.	Roncarati et al. (2018); Annest et al. (2014)
Drug overdose	X40-44, Y10-14, T36-T50	Accidental or unintentional poisonings from one or more drugs, excluding alcohol poisonings.	CDC (2013)
Injury	V00-99, W00-X39, X00-99, Y00-34, Y85-87, Y89	Unintentional and intentional injuries of various causes, assaults, and poisonings.	CDC (2007)
Opioid use	F11, F19, X42-44, Y12-14, T40[20]	Diagnoses relating to chronic or excessive opioid use, excluding cases of suicidal intent.	Roncarati et al. (2018); CDC (2013)
Substance use	F10-19, K70, X40-45, Y10-15, T36-T50	Diagnoses relating to chronic or excessive substance use, including alcohol and drugs and excluding cases of suicidal intent.	Roncarati et al. (2018); CDC (2013)
Suicide	R45851[21], X60-84, Y87	Diagnoses relating to intentional self-harm, including suicidal ideation.	Ruhm (2018); CDC (2007); ARHQ (2020)
Natural causes			
Infectious disease	A00-A99, B00-B99	Diagnoses relating to communicable diseases	WHO (2008), Ch. I
Cancer	C00-C99, D00-D48	Diagnoses relating to benign or malignant neoplasms	WHO (2008), Ch. II
Blood	D49-D99	Diagnoses relating to diseases of the blood or blood-forming organs	WHO (2008), Ch. III
Endocrine / metabolic	E00-E99	Diagnoses relating to endocrine, nutritional or metabolic diseases	WHO (2008), Ch. IV
Mental Health	F00-F99	Diagnoses relating to mental or behavioral disorders	WHO (2008), Ch. V
Nervous	G00-G99	Diagnoses relating to diseases of the nervous system	WHO (2008), Ch. VI
Ear / eye	H00-H99	Diagnoses relating to diseases of the ear or diseases of the eye	WHO (2008), Chs. VII-VIII

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[19]T74 corresponds to "maltreatment syndromes," including physical abuse, sexual abuse and child abuse. Sources which define outcomes based on underlying causes of death (e.g., Roncarati et al. (2018)) generally do not include T74 in the assault group, since T74 codes are unlikely to be listed as primary causes of death. However, we include T74 codes here because we are primarily interested in morbidity. This is consistent with Annest et al. (2014).

[20]Roncarati et al. (2018) include F11, F19, X42-44 and Y12-14 in their "opioid use disorder" category. We add T40 ("poisoning by narcotics"), consistent with the CDC (2013)'s "opioid poisoning" category.

[21]Ruhm (2018) includes X60-84 and Y87 in the "suicide" group. We add R45851, "suicidal ideation," consistent with the ARHQ (2020)'s roster of suicide-related diagnosis codes.

Circulatory	I00-I99	Diagnoses relating to diseases of the circulatory system	WHO (2008), Ch. IX
Respiratory	J00-J99	Diagnoses relating to diseases of the respiratory system	WHO (2008), Ch. X
Flu / pneumonia	J09-J18	Diagnoses relating to influenza and pneumonia	WHO (2008), Ch. X
Digestive	K00-K99	Diagnoses relating to diseases of the digestive system	WHO (2008), Ch. XI
Skin	L00-L99	Diagnoses relating to diseases of the skin	WHO (2008), Ch. XII
Muscle / bone	M00-M99	Diagnoses relating to diseases of the musculoskeletal system	WHO (2008), Ch. XIII
Urogenital	N00-N99	Diagnoses relating to diseases of the urinary or reproductive systems	WHO (2008), Ch. XIV
Pregnancy	O00-O99, P00-P99	Diagnoses relating to pregnancy, childbirth, and/or the perinatal period	WHO (2008), Chs. XV-XVI
Non-pregnancy	All codes except O00-O99 and P00-P99	Any diagnosis, excluding those relating to pregnancy, childbirth, and/or the perinatal period	WHO (2008), Chs. I-XIV, XVII-XXII
Ill-defined symptoms	R00-R99	Symptoms, signs, abnormal results of clinical or other investigative procedures, and ill-defined conditions not classified elsewhere.	WHO (2008), Ch. XVIII

Appendix B. Supplementary Tables and Figures

Table B1: Selected Sample Characteristics: HPCC vs. Comparable Populations

	HPCC Sample	Cook County All	Cook County < FPL	Chicago Voucher Eligible	Chicago Homeless	National Sheltered Homeless
Demographic						
Female	0.806	0.522	0.601	0.878	0.355	0.356
Age	38.8	44.9	42.2	32.0	N/A	43.8
Black	0.888	0.236	0.436	0.943	0.763	0.431
White	0.077	0.589	0.370	0.033	0.197	0.456
Other race	0.034	0.174	0.193	0.004	0.040	0.112
Hispanic	0.081	0.212	0.256	0.035	0.162	0.146
Children in HH	1.381	0.696	0.946	1.750	N/A	N/A
Adults in HH	1.413	2.290	2.252	1.254	N/A	N/A
Veteran	0.033	0.053	0.029	N/A	0.111	0.123
Economic						
HH monthly income	1284	7095	808	1416	N/A	977
Receiving welfare	0.079	0.022	0.077	0.146	N/A	0.201
Receiving SSI	0.067	0.035	0.111	0.059	N/A	0.120
Receiving other income	0.056	0.075	0.093	N/A	N/A	0.070
Receiving SNAP	0.494	0.140	0.480	0.375	N/A	0.658
Monthly rent	642	1031	795	805	N/A	N/A
Own housing	0.018	0.580	0.242	N/A	N/A	N/A
Rent housing	0.855	0.420	0.758	N/A	N/A	N/A

Notes: All income and rent estimates are in 2017 USD. Unless otherwise noted "welfare" receipt is defined as TANF and/or SSA disability receipt. "Other income" is defined as child support, alimony, unemployment and/or VA payments. "N/A" indicates that data is not available for the specified sample.

HPCC estimates are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. See Section 3.3 for additional restrictions.

Cook County estimates are from the 2013 ACS 5-year estimates, restricted to individuals aged 17+. "HH monthly income" is defined as HHINCOME divided by 12.

Estimates for housing "voucher-eligible" individuals are from the main analysis sample of Jacob and Ludwig (2012), which includes all working-age, able-bodied, income-eligible individuals who applied to the Chicago Housing Authority Corporation's housing voucher lottery in 1997 and were living in unsubsidized private housing at the time of application. All individual-level variables reflect characteristics of the head of HH. "HH monthly income" is defined as the annual mean from the main analysis sample divided by 12 (see Jacob and Ludwig (2012)'s Appendix D3). For this sample only "welfare" is defined as TANF receipt, as estimates for disability are not available. Estimates for welfare and SNAP receipt in the past quarter are available for control group individuals only (see Jacob and Ludwig (2012)'s Table 3.

Estimates for the "sheltered homeless" are from Meyer et al. (2021)'s sample of homeless adults identified as living in emergency or transitional shelters in restricted use Census/ACS data. Means for demographic characteristics are from the 2006-2016 ACS 1-year estimates; see Meyer et al. (2021)'s Table 9a. Means for economic characteristics are from the 2011-2018 1-year estimates; see Meyer et al. (2021)'s Table 12b.

Table B2: Baseline balance, estimates of bias

	(1) $\Delta X/\Delta Funds$	(2) $\Delta Y/\Delta X$	(3) $\Delta Y/\Delta Funds$	(4) $\Delta X/\Delta Funds$ (%)
Any care	0.004	0.318***	0.0012	0.013
	(0.014)	(0.018)	(0.0045)	(0.048)
ED visit	0.001	0.301***	0.0004	0.006
	(0.013)	(0.018)	(0.0042)	(0.053)
Inpatient discharge	-0.003	0.303***	-0.0008	-0.035
	(0.008)	(0.026)	(0.0024)	(0.104)
Assault	0.004	0.087	0.0003	0.687
	(0.002)	(0.115)	(0.0005)	(0.534)
Suicide	-0.002	0.349***	-0.0007	-0.330
	(0.002)	(0.074)	(0.0007)	(0.301)
Female	-0.019	0.083***	-0.0016	-0.023
	(0.012)	(0.026)	(0.0011)	(0.014)
Black, non-Hispanic	-0.020**	-0.000	0.0000	-0.022**
	(0.009)	(0.033)	(0.0007)	(0.010)
White, non-Hispanic	0.011	0.029	0.0003	0.187
	(0.008)	(0.040)	(0.0006)	(0.148)
Hispanic	0.016**	-0.011	-0.0002	0.252*
	(0.008)	(0.038)	(0.0007)	(0.131)
Age	0.600*	-0.001	-0.0003	0.015*
	(0.341)	(0.001)	(0.0006)	(0.009)
Number of adults in caller's household	-0.041*	-0.006	0.0003	-0.028*
	(0.021)	(0.012)	(0.0006)	(0.015)
Number of minors in caller's household	-0.142***	0.003	-0.0005	-0.093***
	(0.045)	(0.006)	(0.0009)	(0.029)
ACS - Median log household income	-0.003	0.045***	-0.0001	-0.000
	(0.026)	(0.006)	(0.0012)	(0.003)
ACS - Median monthly housing cost	11.839**	0.000**	0.0014	0.012**
	(5.213)	(0.000)	(0.0010)	(0.005)
ACS - Pct. of population = Black	-0.017	-0.008	0.0001	-0.026
	(0.010)	(0.028)	(0.0006)	(0.016)
Applying due to benefit loss	-0.004	-0.003	0.0000	-0.038
	(0.012)	(0.040)	(0.0005)	(0.121)
Applying due to inability to pay bills	-0.019***	0.052	-0.0010	-0.362***
	(0.006)	(0.053)	(0.0012)	(0.108)
Applying due to shared housing exit	-0.026***	-0.033	0.0009	-1.182***
	(0.006)	(0.076)	(0.0020)	(0.153)
Applying due to job loss	0.016	-0.030	-0.0005	0.061
	(0.018)	(0.028)	(0.0009)	(0.073)
Monthly income (thousands)	-0.056***	-0.032*	0.0018	-0.052***
	(0.016)	(0.018)	(0.0011)	(0.015)
Living in rented housing	0.032***	-0.044	-0.0014	0.038***
	(0.010)	(0.028)	(0.0011)	(0.012)
Receiving SNAP	-0.010	0.117***	-0.0012	-0.024
	(0.010)	(0.033)	(0.0013)	(0.025)

Notes: Results are for the sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. Column 1 shows the difference in means (T - C), as measured by Eq. 3: a regression of the balance variable (X_i) on a fund availability dummy (Funds) and a vector of calendar and fund-specific eligibility controls (Z_i). Column 2 shows the coefficient from a regression of care utilization (Y_i) on the balance variable and the control vector using only the control group. The size of the point estimate in column 3 is interpretable as the magnitude of the bias, and the sign is interpretable as the direction of the bias. Column 4 shows the coefficient from column 1 as a percent of the control mean. Standard errors are in parentheses. In columns 1 and 2, we present heteroskedasticity-robust SEs; in column 3 we estimate SEs from a simple bootstrap routine (1,000 replications); in column 4, we estimate SEs from a non-linear Wald test. * - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Table B3: Baseline balance, period w/ full set of fund-specific eligiblity controls

	(1)	(2)	(3)	(4)
	Sample Mean	Control Mean	Treatment Mean	Adjusted Difference
Predicted outcomes, 24 mos. post call				
Any care	0.581	0.568	0.590	0.007
ED visit	0.539	0.535	0.541	0.002
Inpatient discharge	0.148	0.171	0.133	0.001
Assault	0.031	0.027	0.035	-0.001
Suicide	0.012	0.017	0.009	-0.002
Lagged outcomes, 6 mos. pre call				
Any care	0.286	0.283	0.288	0.035*
ED visit	0.250	0.248	0.251	0.026
Inpatient discharge	0.067	0.069	0.066	0.008
Assault	0.008	0.006	0.009	0.004
Suicide	0.005	0.004	0.005	-0.001
Caller covariates				
Female	0.806	0.838	0.785	0.008
Black	0.882	0.904	0.867	-0.018
White	0.080	0.063	0.091	0.008
Hispanic	0.089	0.078	0.096	0.005
Age	39.0	39.9	38.4	1.262***
Number of adults in caller's household	1.421	1.459	1.396	-0.027
Number of minors in caller's household	1.378	1.542	1.271	-0.055
ACS - Median log household income	10.36	10.36	10.36	-0.002
ACS - Median monthly housing cost	989.65	986.21	991.89	8.30
ACS - Pct. of population = Black	0.638	0.651	0.629	-0.004
Applying due to benefit loss	0.098	0.094	0.100	-0.004
Applying due to inability to pay bills	0.026	0.053	0.008	-0.019***
Applying due to shared housing exit	0.017	0.022	0.013	-0.026***
Applying due to job loss	0.312	0.258	0.346	0.016
Monthly income (thousands)	1.252	1.103	1.350	-0.080***
Living in rental housing	0.868	0.836	0.889	0.056***
Receiving SNAP	0.291	0.183	0.362	0.018*
N	4,845	1,914	2,931	4,845

Notes: Results are for the sample of eligible first-time callers within the last six months, August 1, 2014 - December 31, 2015. See Section 3.3 for additional restrictions. The first three columns show simple means for observations with non-missing values. The final column shows the difference (treatment minus control) as measured by a regression of the outcome on a fund availability dummy and a vector of calendar and fund-specific eligibility controls ($\mathbf{Z}_i$. $Calendar and fund-specific eligibility controls include linear controls for$: $the rank of the call within the day, dummies for need amount category interacted with call year and call year-quarter, dummies for whether caller contribution$ $*-significant at 10\%, **-significant at 5\%, ***-significant at 1\%$.

Table B4: Robustness, 12-month outcomes

	(1) Baseline	(2) 1st call in 3 months	(3) 1st call ever	(4) Calls on or after 1/1/2013	(5) Period w/ all fund elig. controls	(6) Calendar, fund elig. controls only	(7) Outcomes incl. call months
Any care	-0.002	-0.005	0.008	-0.004	0.003	-0.006	-0.006
	(0.014)	(0.013)	(0.016)	(0.013)	(0.017)	(0.014)	(0.014)
	[0.887]	[0.689]	[0.597]	[0.771]	[0.884]	[0.645]	[0.683]
Control Mean	0.436	0.440	0.415	0.436	0.430	0.436	0.461
ED visit	0.003	-0.002	0.014	0.000	-0.002	-0.001	-0.002
	(0.013)	(0.013)	(0.016)	(0.013)	(0.017)	(0.013)	(0.014)
	[0.834]	[0.883]	[0.362]	[0.977]	[0.886]	[0.940]	[0.905]
Control Mean	0.396	0.400	0.375	0.396	0.396	0.396	0.419
Inpatient discharge	-0.005	-0.007	-0.006	-0.002	0.006	-0.006	-0.005
	(0.009)	(0.008)	(0.010)	(0.008)	(0.011)	(0.009)	(0.009)
	[0.552]	[0.396]	[0.547]	[0.807]	[0.596]	[0.499]	[0.605]
Control Mean	0.116	0.117	0.115	0.114	0.107	0.116	0.122
Assault	-0.006*	-0.005	-0.006	-0.006*	-0.005	-0.006*	-0.006*
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
	[0.094]	[0.134]	[0.131]	[0.082]	[0.281]	[0.076]	[0.086]
Control Mean	0.016	0.014	0.015	0.016	0.015	0.016	0.017
Suicide	-0.003	-0.003	-0.000	-0.003	-0.004	-0.003	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	[0.327]	[0.239]	[0.890]	[0.286]	[0.171]	[0.366]	[0.185]
Control Mean	0.010	0.010	0.010	0.010	0.010	0.010	0.012
N	10,635	11,541	7,754	11,799	6,895	10,635	10,635

Notes: Each cell shows the coefficient on funds availability from a separate regression. The outcome is a dummy for receiving treatment for the listed type of care within 12 months of calling. All specifications use a 12-month follow-up period. Column (1) is identical to the main analysis in Table 5. This sample restricts to first calls in 6 months and to calls on or after July 1, 2013; regressions include all available controls (see Table 5 notes for a full description), and outcomes are defined to exclude the month of the call. Specifications (2)-(7) each deviate from the baseline specification in only the way described. (2) restricts to first calls in 3 months, while (3) restricts to first calls ever. (4) expands the sample to include calls on or after January 1, 2013. (5) revises the sample to specifically exclude calls from April 4, 2013 through July 31, 2014, when the type of need request (e.g., rent, security deposit) is not observed. (6) includes only calendar and fund-specific eligibility controls (Z_i), as defined in the notes of Table 5. (7) re-defines outcomes to include visits from the month of the call. Heteroskedasticity-robust standard errors are in parentheses; p-values are in brackets. * - significant at 10%, ** - significant at 5%, *** - significant at 1%.

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Table B5: OLS estimates of the effect of fund availability on the cumulative probability of healthcare use

	(1) 6 Months	(2) 12 Months	(3) 18 Months	(4) 24 Months
Public Payer	-0.003	0.001	0.002	0.001
	(0.013)	(0.014)	(0.015)	(0.015)
	0.831	0.927	0.903	0.967
Control Mean	0.218	0.325	0.403	0.462
N	8585	8585	8585	8585

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. See Section 3.3 for additional restrictions. Each cell shows the coefficient on funds availability from a separate regression. The outcome is a dummy for having a visit paid for by Medicare or Medicaid within the specified timeframe after the call. All regressions include calendar and fund-specific eligibility controls (Z_i), as well as controls for other observables (X_i), as defined in the notes of Table 5 Heteroskedasticity-robust standard errors are in parentheses; p-values are in brackets.
- significant at 10%, ** - significant at 5%, *** - significant at 1%.

Table B6: OLS and quantile regression estimates of the effect of fund availability on public charges

	(1)	(2)
	12 mos.	24 mos.
Public Charges (thousands of USD)		
OLS: Baseline	-1.239	-1.442
	(1.022)	(2.650)
	{9.432}	{18.488}
OLS: Fund-specific eligibility controls only	-1.258	-1.411
	(1.023)	(2.731)
	{9.432}	{18.488}
Quantile: 75th percentile	-0.129	-0.575
	(0.168)	(0.589)
	{2.040}	{7.507}
Quantile: 90th percentile	-0.091	-0.843
	(1.205)	(1.946)
	{17.151}	{36.735}
Control Pct. = 0	0.68	0.54
N	8585	8585

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. See text for additional restrictions. Each cell shows the coefficient on funds availability from a separate regression from our main sample of callers (as defined in Section 3.3). The outcome is: the sum of all public charges (in thousands of 2017 USD), from the specified quantile of the distribution (75th or 90th), within the specified time frame after calling (12 or 24 months). Public charges are defined as the total charge for visits paid by public health insurance programs (Medicaid and Medicare). We do not show outcomes for 6 months after the call, since more than 78% of callers in the control group do not record any public charges within 6 months of calling. We do not show outcomes for 18 months after the call due to lack of model convergence. The baseline OLS specifications include all available controls (Z_i and X_i), while the second set includes only calendar and fund-specific eligibility controls (Z_i). Quantile regressions include only calendar and fund-specific eligibility controls to ensure model convergence. For quantile regressions, the corresponding control percentile value is in curly braces; for OLS regressions, the control mean is in curly braces. We estimate analytic standard errors (in parentheses) as in (Parente and Silva, 2016).
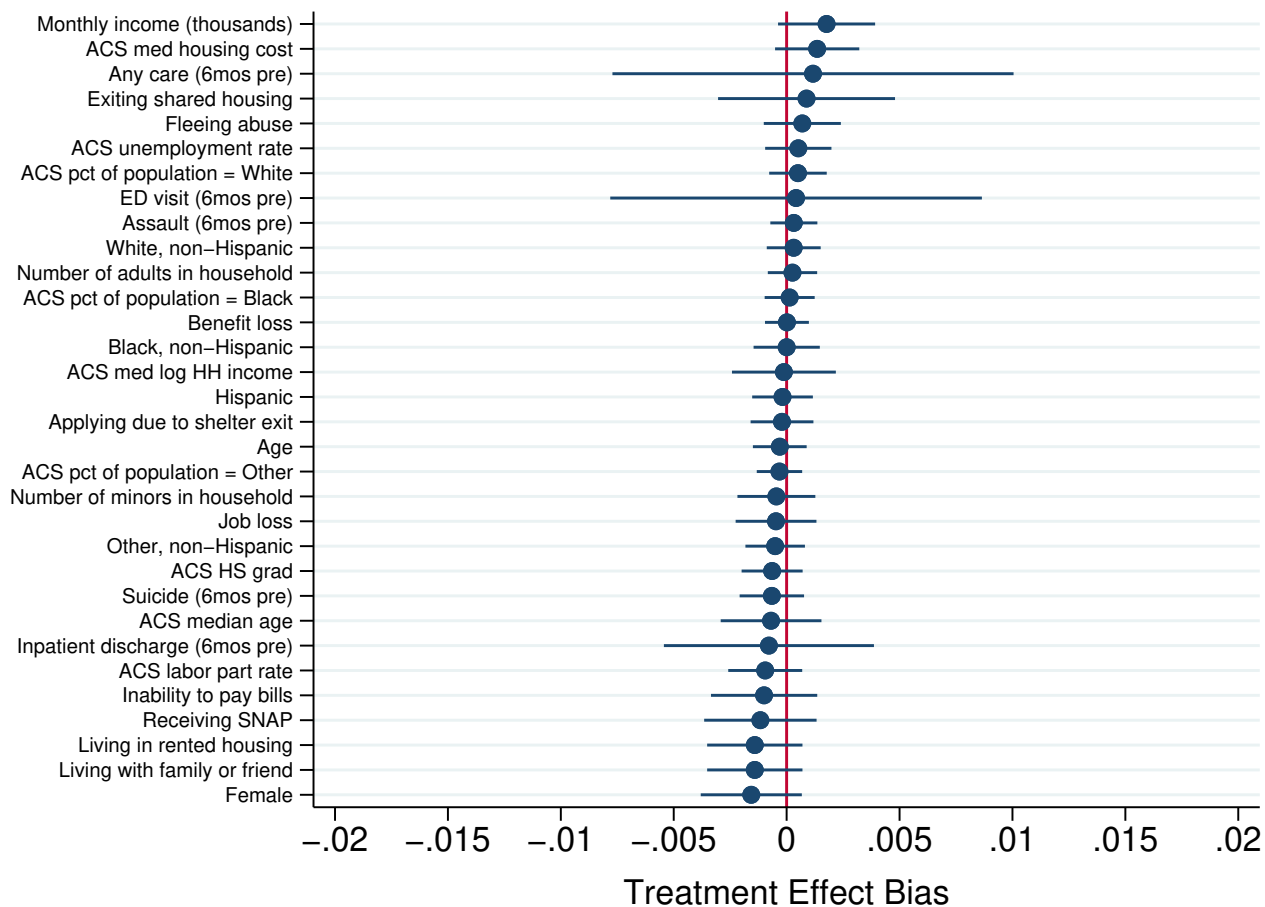* - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Figure B1: Baseline balance: Estimated bias

Notes: Results are for the sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. See Section 3.3 for additional restrictions. For each balance variable (X_i), the point estimate matches the coefficient reported in column 3 of Table B2 and can be interpreted as the treatment effect bias which would result from imbalance in randomization if X_i was unobserved. See the table notes of Table B2 for details. As in Table B2, we estimate standard errors from a simple bootstrap routine (1,000 replications). Whiskers depict 95% confidence intervals.
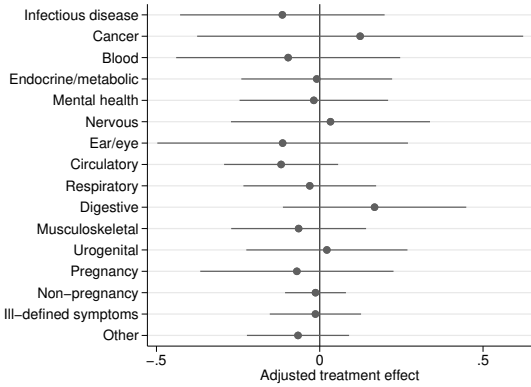
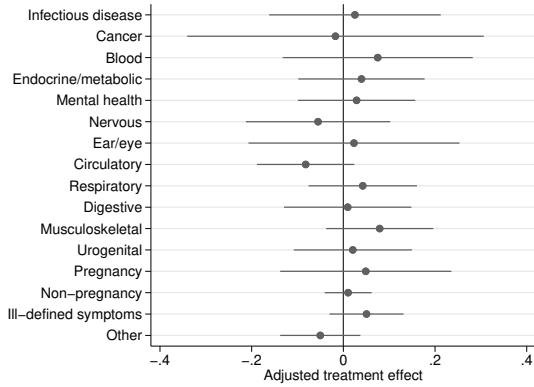* - significant at 10%, ** - significant at 5%, *** - significant at 1%.
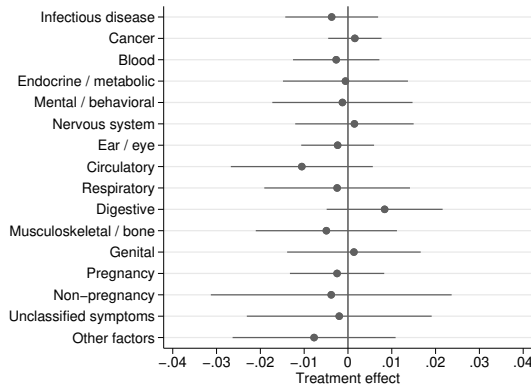
(a) 6 Months - Absolute Magnitude
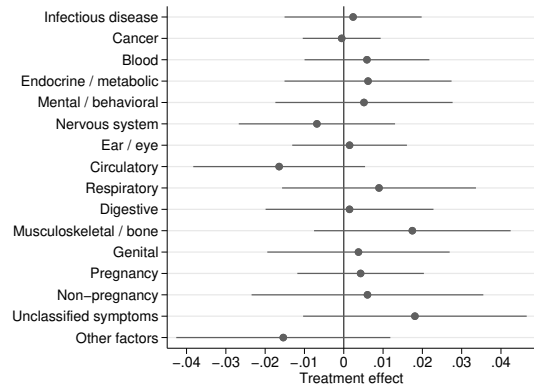
(b) 24 Months - Absolute Magnitude

(c) 6 Months - Relative to Mean

(d) 24 Months - Relative to Mean

Figure B2: Effect of referral to funds on probability of healthcare, By diagnosis type

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. N = 8,585. Each point estimate corresponds to a separate regression coefficient on fund availability. The outcome is a dummy for receiving treatment for the listed type of care within the specified timeframe after the call. All regressions include calendar and fund-specific eligibility controls (Z_i), as well as controls for other observables (X_i), as in Eq. (2). See the notes of Table 5 for a list of controls. In panels (a) and (b), point estimates are absolute treatment effects, and we compute heteroskedasticity-robust standard errors. In panels (c) and (d), we scale treatment effects by dividing the funds coefficient by the control group mean for the specified outcome and timeframe. We derive standard errors using a non-linear Wald test. In all cases, the whiskers depict 95% confidence intervals.

Appendix C. Attrition Analysis

C.1 Infutor Address Histories

To assess whether differential attrition due to migration might bias our results, we examine migration in our sample using consumer reference data from Infutor Data Solutions. Infutor accumulates records from various commercial transactions and constructs individual-level address histories for the near-universe of adults in the United States. Diamond et al. (2018) introduced Infutor to the economic literature and several papers have since used the database to study migration.[22] See Phillips (2020) for further validation that Infutor data can adequately track unstably housed individuals, including HPCC callers.

Each Infutor record contains full name and aliases, month and date of birth, and detailed address information (including approximate move dates) for each address in an individual's history. It is possible to link some records to Social Security Numbers. We use an Infutor extract that includes address histories that cover the entire United States. In particular, we identify people in the Infutor data who have ever lived in Cook County, Illinois. This allows us to observe both people who leave Illinois and those who remain.

C.2 Fuzzy Matching Procedure

We match Infutor and HPCC records using a fuzzy matching procedure based on name, date of birth, and SSN. Each record in Infutor includes up to 10 aliases associated with a particular person. Aliases allow us to observe name changes over time (e.g., in the case of marriage or divorce) and also help mitigate possible transcription errors. For every first name or first name alias, we also map to common hypocorisms and nicknames (e.g., William maps to Bill, Billy, Will, Willie, and Willy). Finally, we map last names to possible variants by splitting out compound and hyphenated surnames (e.g., Smith-Jones maps to Smith, Jones, and Smith-Jones). We allow matches based on any combination of the associated name variants.

We conduct the fuzzy match at the name level using the user-written Stata command *dtalink* (Kranker, 2018).

Dtalink accepts multiple matching arguments, exact matches on each argument, and scores matches

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[22]We are aware of related work by: Bernstein et al. (2018); Diamond et al. (2019); Mast (2021); Asquith et al. (2021); Qian and Tan (2021); Pennington (2021).

based on the values of positive and negative weights attributed to each argument. To make the match "fuzzier," we enter first and last names separately and also include the Soundex code of both names.

Other matching arguments include the last 4 digits of SSN, month of birth, and year of birth. The quality of the match is computed as an index based on linear weights for matching on the various arguments.

We use a data-driven process to train the matching algorithm. Our training dataset, from Barham et al. (2020), includes full Social Security Numbers in addition to all the matching arguments available in the HPCC data. Using full SSNs, we identify true matches in Infutor for individuals in the training data and use those true matches to construct optimal weights for each argument.[23] We save the weights generated from the training data and apply them to the HPCC-Infutor match. To define the score threshold for high quality matches, we directly compute the Type I and Type II error rates associated with each score. We choose a score threshold which retains a sufficiently high proportion of true matches while also taking into account the trade-off in error rates. We keep any match at/above the threshold and discard matches below the threshold. Finally, from the set of retained high-quality matches, we aggregate the data to the HPCC caller level.

We match 74.6% (6403/8585) of main sample callers to at least one Infutor record. To determine whether there is selection into being matched, we compare observed characteristics of matched and unmatched callers in Table C1. For these comparisons, we further restrict to a sample of non-Hispanic callers over the age of 25, since previous work documents Infutor's relatively poor coverage of Hispanics and young people (Phillips, 2020). We retain 82.3% of main sample callers after applying these restrictions. Column (4) of Table C1 shows the regression-adjusted difference between matched and unmatched callers.[24] Importantly, these results show that the probability of being referred to funds (conditional on Z_i) is good-as-random between matched and unmatched callers. In addition, the differences between matched and unmatched individuals tend to be small in magnitude, statistically insignificant, and/or consistent

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[23]*dtalink* calculates weights as follows:

$$PositiveWeight_i = [log(2)] * (\frac{p_1}{p_2})$$

$$NegativeWeight_i = [log(2)] * (\frac{1 - p_1}{1 - p_2})$$

where p_1 is the percent of cases where argument i matched among true matches and p_2 is the percent of cases where argument i matched among false matches.

[24]As in our main baseline balance specifications, the adjusted difference is measured by a regression of the specified covariate on a dummy for fund referral and a vector of calendar and fund-specific eligibility controls (Z_i).

with known limitations of the Infutor data.[25] Moreover, although matched callers are slightly more likely to be female and non-White, the differences between matched and unmatched individuals are not clearly systematic: while some factors (proportion black) suggest that matched callers face greater barriers to housing and healthcare, other factors (personal monthly income, local median HH income) suggest that matched callers are, if anything, better off.

C.3 Attrition Results

We examine out-of-state mobility rates for the matched sample in Table C2. The outcome is a dummy for whether a caller is observed to move out-of-state within x months after calling HPCC. Since callers may fuzzy match to more than one unique Infutor record, we define this outcome as 1 if any record associated with a caller documents an out-of-state move. Out-of-state migration is our parameter of interest because we observe health outcomes for all individuals in Illinois in the IDPH data.

Our first key result is that out-of-state migration is exceedingly rare in our setting: only 1.6% of matched control group callers move out of Illinois within 2 years of calling the HPCC. Moreover, we do not find any evidence of differential attrition between treatment and control group callers in the matched sample. The coefficient on fund availability is not statistically significant for any time horizon, and we can reject differences greater than $\pm$ 0.8% in the propensity to move out-of-state within 24 months post-call. Consequently, attrition from out-of-state migration is unlikely to lead to significant bias of our main estimates for the effect of financial assistance on health care utilization.

These findings on migration-related attrition are consistent with those from related work. Using 2007-2011 ACS estimates, Palmer et al. (2019) find that only 4% of households in Cook County, IL with income below $25,000 migrate out of the county within 1 year. Jacob and Ludwig (2012) argue that attrition is not a threat to identification in their sample of housing voucher-eligible families in Chicago. Their address data suggest that "the same fraction of treatment and control households — roughly 10% — spent at least some time between 1997Q3 and 2005Q4 out of Illinois. Similarly, the average fraction of quarters spent outside of Illinois was 0.042 for both treatment and control households."

Attrition due to mortality also seems unlikely to noticeably affect our results. Unfortunately, we cannot

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[25]For example, even after restricting to those 25 and older, matched callers are still more than 2 years older, on average, than callers in the unmatched sample. This is consistent with the evidence that Infutor is less likely to capture younger individuals.

directly assess the possible influence of attrition related to mortality because we do not observe mortality in the IDPH data, and our data sharing agreement with the HPCC does not allow us to link mortality records to our sample of callers. However, our main effects are measured within a relatively short window of time after calling (2 years or less), which limits the possible impact of differential mortality. Also, our main sample of callers is relatively young: the median age is 36, and only 3% of callers are 65 or older. According to 2013 data for all Americans from the Social Security Administration, the probability of dying within 1 year is 0.00170 for 36 year-olds and 0.00177 for 37 year-olds. Therefore, the probability that an average 36 year-old will die within 2 years is only 0.35%. Even if the mortality risk for individuals in our sample is somewhat higher than that for the average American, these estimates indicate that mortality is much rarer than out-of-state migration, and that - given our sample of 8,585 callers - we would not be able to precisely measure a differential effect of mortality. This analysis also implies that mortality records are unlikely to meaningfully enrich our analysis of suicide diagnoses, since mortality is much rarer than suicide-related utilization (2 year post-call control mean = 1.8%).

In conclusion, we believe that attrition from measurement, whether as a result of out-of-state migration or mortality, is unlikely to meaningfully bias the headline utilization results in the paper.

Table C1: Match Balance

	(1)	(2)	(3)	(4)
	Sample Mean	Non-matched Mean	Matched Mean	Adjusted Difference
Referred to Funds	0.642	0.656	0.638	0.009
Caller covariates				
Female	0.806	0.768	0.816	0.041***
Black	0.944	0.919	0.950	0.026***
White	0.047	0.065	0.042	-0.020***
Other race	0.009	0.015	0.008	-0.006*
Age	41.333	39.267	41.886	2.335***
Number of adults in caller's household	1.408	1.395	1.411	0.019
Number of minors in caller's household	1.374	1.459	1.352	-0.138***
ACS - High school graduation rate	29.740	29.500	29.803	0.400*
ACS - Median log household income	10.370	10.269	10.397	0.116***
ACS - Median age	34.025	33.861	34.068	0.240**
ACS - Median monthly housing cost	984.551	988.876	983.409	-5.963
ACS - Labor participation rate	59.284	59.483	59.232	-0.323
ACS - Pct. of population = Black	0.676	0.656	0.681	0.028***
ACS - Pct. of population = White	0.213	0.230	0.208	-0.024***
ACS - Unemployment rate	20.903	20.596	20.985	0.446**
Applying due to benefit loss	0.103	0.109	0.102	-0.010
Applying due to inability to pay bills	0.027	0.033	0.026	-0.009
Applying due to shared housing exit	0.016	0.022	0.014	-0.005
Applying due to shelter exit	0.007	0.015	0.005	-0.006
Applying to flee abuse	0.001	0.002	0.000	-0.002
Applying due to job loss	0.299	0.300	0.298	-0.008
Monthly income (thousands)	1.195	1.194	1.195	0.038**
Living in rental housing	0.862	0.849	0.865	0.020**
Receiving SNAP	0.493	0.479	0.496	0.024**
Receiving child support	0.025	0.029	0.024	-0.004
Receiving EITC	0.253	0.285	0.245	-0.019**
Receiving SSI	0.070	0.065	0.072	0.005
Receiving TANF	0.030	0.035	0.028	-0.007
Receiving unemployment	0.036	0.024	0.040	0.017***
N	7073	1493	5580	7073

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. Since Infutor coverage is known to be poor for Hispanics and young people, we follow Phillips (2020) in restricting to non-Hispanic callers aged 25 and older. The first three columns show simple means for observations with non-missing values. The final column shows the difference (matched minus unmatched) as measured by a regression of the outcome on a fund availability dummy and a vector of calendar and fund-specific eligibility controls (Z_i). Calendar and fund-specific eligibility controls include linear controls for: the rank of the call within the day, dummies for need amount category interacted with call year and call year-quarter, dummies for whether caller contribution or debt need are missing interacted with call year, day of the week, time of month, veteran status, housing subsidy receipt, need > 1 month rent, income > twice the poverty line, need request type, owning one's dwelling, senior status, receiving disability payments, and age. * - significant at 10%, ** - significant at 5%, *** - significant at 1%.

Table C2: OLS estimates of the effect of fund availability on the cumulative probability of out-of-state migration

	(1) 6 Months	(2) 12 Months	(3) 18 Months	(4) 24 Months
Any out-of-state move	-0.001	-0.003	-0.002	-0.000
	(0.002)	(0.002)	(0.003)	(0.004)
	0.463	0.186	0.648	0.986
Control Mean	0.004	0.008	0.013	0.016
N	6403	6403	6403	6403

Notes: Results are for our main sample of eligible first-time callers within the last six months, July 1, 2013 - December 31, 2015. We further restrict to callers who are matched to at least one Infutor record. Each cell shows the coefficient on funds availability from a separate regression. The outcome is a dummy for recording any out-of-state move within the specified timeframe after the call. All regressions include calendar and fund-specific eligibility controls (Z_i, as well as controls for other observables (X_i), as defined in the notes of Table 5. Heteroskedasticity-robust standard errors are in parentheses; p-values are in brackets. * - significant at 10%, ** - significant at 5%, *** - significant at 1%.