Emergency Assistance Grants and Household Stability: Evidence from COVID Assistance Lotteries *

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Abstract

PRELIMINARY: DO NOT CITE OR CIRCULATE

This paper studies the largest expansion of emergency rental assistance in U.S. history. Using application data from four major urban areas linked to novel survey data and administrative credit panel data, and leveraging lotteries of oversubscribed assistance, we estimate the effects of assistance on housing instability, financial insecurity, and mental health. We find that assistance increased rent payment modestly in the months immediately after application and improved self-reported mental health. However, we find that assistance had little detectable short- or long-run effects on financial security or housing stability.

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1 Introduction

The economic disruption brought by the COVID-19 pandemic created substantial turmoil in the U.S. housing market. By August 2020, more than 5.4 million Americans reported that they were likely to face either eviction or foreclosure within two months.[1] While policymakers relied on lessons from the Great Recession to forestall foreclosures and stabilize the owner-occupied segment of the market (Piskorski and Seru, 2018; Ganong and Noel, 2020), there was comparatively little research to guide policies seeking to prevent evictions and stabilize the rental market. Yet, concerned about the economic and public health consequences of individuals and families losing their homes during the pandemic, policy makers nonetheless devoted unprecedented resources to stabilize the housing situation of vulnerable renters. The federal government allocated nearly $200 billion in emergency assistance to renters through the CARES Act and subsequent Coronavirus relief packages. This assistance represented a four-fold increase in the typical amount of federal assistance for renters and a major component of the federal government's economic response to the COVID-19 pandemic.[2]

Despite the unprecedented scale, there is little, if any, evidence on the effectiveness of these programs at boosting rent payments, preventing evictions or homelessness, or keeping families in their homes. Recent studies have connected COVID-19 emergency assistance to improvements in housing outcomes, financial well-being, and mental health, but these studies do not permit causal interpretation (Airgood-Obrycki, 2022; Reina and Lee, 2023). The lack of existing evidence on these policies stems from several distinct challenges. First, there was no centralized data collection on the administration of these emergency rental assistance programs. Second, the ability to track key outcomes such as rent payment, eviction, or homelessness is hampered by an absence of comprehensive national administrative data. Third, comparisons of recipients of assistance to unassisted individuals are likely to suffer from selection bias.

In this paper, we provide the first empirical evidence on both the causal effects of pandemic era emergency rental assistance and the targeting of these programs. We study five emergency assistance programs across four major urban areas: Chicago, Harris County (Houston), King County (Seattle), and Los Angeles administered from May-December 2020. Collectively, these programs received more than 200,000 applications for assistance. Using program data linked to novel survey and administrative data, we estimate the effects of assistance on several policy relevant outcomes related to housing stability, economic security,

[1] Census Pulse Survey, week 13, August 2020

[2] The federal government typically spends roughly $50 billion annually on assistance to renters (Collinson et al., 2019).

and mental health. We use credit bureau records and consumer reference data to track the evolution of financial stability and housing outcomes for program applicants throughout the pandemic. We augment these data with novel surveys that we administered during the pandemic across each site to capture hardship not easily measured in administrative sources. In select sites, we also link applications to eviction court filings and administrative data on homeless system use to investigate impacts on extreme measures of housing instability. To estimate the causal effects of receiving assistance, we leverage exogenous variation arising from lotteries for assistance in the five programs, which arose due to excess demand.

Receipt of assistance increased rent payment modestly in the short-run and reduced self-reported anxiety. Using harmonized surveys fielded across the five lotteries, we find that those who received assistance were 5–13 percentage points (8–36 percent) more likely to pay their rent in full in the months immediately after the lottery. The estimated effects on rent payment are similar across sites, but are somewhat larger in communities which offered more generous assistance and made payments directly to landlords. We use the same surveys to examine the impacts of assistance on measures of mental health. Receipt of assistance reduced self-reported anxiety or depression by an average of 3.3 percentage points across sites.

Despite the policy objective of keeping individuals and families in their homes, we find no consistent effects of assistance on the likelihood that applicants moved. We analyze applications linked to two separate administrative sources to draw this conclusion. First, we evaluate changes in neighborhood using our panel of linked credit data and find that treated individuals were no less likely to change neighborhood (ZIP code), either in the immediate aftermath of applying for assistance or in the longer-run. Second, we link our lottery samples to consumer reference data that provides address histories to track moves at the address level. Here too, we find little evidence to suggest that assistance made individuals less likely to move from their application address either in the two months immediately after or in the year after applying for assistance. Overall, move rates in our samples appear similar leading up to the lottery and in the post-lottery period.

Next, we examine whether assistance impacts short- and longer-run measures of financial distress using linked credit reports. While assistance has small short-run impacts on some credit measures, these effects are not consistently present across sites and do not persist over time. Averaging across sites, individuals assisted through the lottery have roughly $130 less in collections in the two months after applying for assistance. However, these effects appear to fade over time. Ten months after the lottery, the financial characteristics of those randomized into receiving assistance are largely indistinguishable from those who missed out.

The lack of financial effects may be a consequence of improving economic circumstances for both the treatment and control group over time. Our linked credit data reveal that individuals who applied for assistance experienced rising credit scores and falling debt in the lead-up to application. These patterns likely reflect a mix of broad-based improvements in macroeconomic conditions after the initial COVID shock and the effects of other expanded government assistance during this period. The improvement in household balance sheets continues post-lottery but is similar for both treatment and control applicants, consistent with our findings of small estimated impacts on credit measures.

Finally, we examine whether assistance reduced the chances that applicants experienced more acute forms of housing instability, including eviction or homelessness. Using application data linked to homeless system interactions in King County (WA), we find that assistance actually *increased* homeless system interactions, though the effect is small. We then investigate whether assistance reduced the probability of new eviction filing or judgments in Harris County (TX), where new cases could be still initiated during the CDC moratorium. We find that assistance had no significant effects on eviction activity and that baseline rates of new filings remained relatively low for applicants that are were not assisted through the lottery.

Taken together, our results suggest that receipt of assistance helped households pay their rent in the short-run and relieved mental health distress but did not substantially change applicants' housing or financially stability. This stands in contrast to prior work on smaller-scale emergency rental assistance programs targeting idiosyncratic shocks faced by vulnerable renters (Phillips and Sullivan, 2022b; Evans et al., 2016), which finds large relative effects of assistance on housing stability. We interpret these differences in effectiveness as informative about the role of emergency assistance as a tool to respond to macroeconomic shocks compared to idiosyncratic shocks. When alternative forms of assistance are expanded—as is common during macroeconomic downturns—emergency assistance may be less effective.

While we find generally muted effects of assistance, we nevertheless find that assistance was relatively well-targeted to financially distressed households. Recipients of assistance across our four sites appear substantially more disadvantaged than the typical renter in each community. We find that, compared to the general population or other renters from the same community, those who received assistance had 50-100 points lower credit scores, higher balances in collections, and greater levels of debt. When we investigate heterogeneity within our sample, we find little evidence that more disadvantaged individuals experienced differential effects from assistance. Nonetheless, our results suggest that the targeting of the programs we study, which focused largely on low-income renters, was mostly successful in directing assistance to disadvantaged individuals.

Our paper contributes to a large literature studying the effects of various policies seeking

to stabilize housing markets during aggregate economic downturns. Much of this literature has focused on the owner-occupied segment of the market, where frictions in mortgage financing created substantial turmoil during the 2009 financial crisis (Agarwal et al., 2017; Piskorski and Seru, 2018; Agarwal et al., 2022; Ganong and Noel, 2020). In contrast, our paper is one of the first to study the effects of large scale relief programs targeted specifically at renters during a time of macroeconomic distress. This emphasis is particularly relevant because, during the pandemic, renters reported significantly higher rates of missing housing payments than owner-occupants (15 % compared to 9.5%), and substantially higher risk of losing their home (7 % compared to 2%).[3]

In focusing on renters, this paper also connects to the broader literature on housing instability and the effects of assistance to renters. One strand of the literature focuses on the effects of evictions (Collinson et al., 2022) and landlord foreclosure (Diamond et al., 2019). A second strand of this literature estimates the effects of short-term housing assistance on homelessness (Evans et al., 2016; Phillips and Sullivan, 2022b).[4] These papers focus on idiosyncratic shocks outside of broader economic downturn. In contrast, we provide rigorous evidence on the causal effects of emergency assistance during a time of *aggregate* distress and concomitant large-scale policy responses. We also study a wider range of outcomes including housing stability, financial strain, and the mental and physical health of program applicants.

We contribute to a growing literature analyzing the impacts of economic policies during the COVID-19 pandemic by providing the first comprehensive analysis of the effects of emergency assistance to renters during the pandemic. Ganong et al. (2022) examine the impacts of pandemic unemployment insurance on spending and job-finding. Karger and Rajan (2021) examine the impacts of COVID-19 Economic Impact Payments on spending. Many recent studies have analyzed the short term effects of the 2021 expansion of the Child Tax Credit (CTC) on household financial and mental well-being (Collyer et al., 2022; Glasner et al., 2022; Kovski et al., 2023; Pignatti and Parolin, 2023; Pilkauskas et al., 2023). While past work has sought to estimate the impact of the eviction moratoriums during the pandemic on COVID-19 infections and mortality (Jowers et al., 2021) as well as credit and mental health outcomes (An et al., 2021), we provide the first large-scale evidence on the causal effects of a key part of the federal economic response to COVID—the significant expansion of emergency assistance to renters—across four large urban areas.

Finally, our work relates to recent studies of supplemental cash assistance during the COVID-19 pandemic. Jaroszewicz et al. (2022) compare the effects of $2,000 one-time cash

[3]Census Pulse 2020, week 13.

[4]Another branch of literature examines the effects of longer-term means-tested rental assistance on labor supply and child well-being (Jacob and Ludwig, 2012; Jacob et al., 2015).

grants to one-time payments of $500 on bank account spending and survey measures of mental health and find small increases in spending, but no effects on mental health. Pilkauskas et al. (2022) examine the effects of one-time $1,000 payments to families receiving SNAP. They find limited effects on measures of material hardship and mental health. We study the effects of assistance targeting renters that was administered as a major component of the federal policy response to the pandemic. While we find some evidence for improvements in mental health in contrast to these studies, our estimates of impacts on bill payment and financial strain appear qualitatively similar. Our results draw from four different metropolitan areas, feature much larger samples, and speak directly to the effects of pandemic rental assistance programs.

The rest of the paper is organized as follows. Section 2 details the assistance programs and lottery processes. Section 3 describes the data. Section 4 explains our research design and empirical strategy. Section 5 reports our main results. Section 6 includes a further discussion of our findings, and Section 7 concludes.

2 Program Details

The analysis in this paper uses administrative data from five emergency housing assistance programs administered across four different communities. This section provides basic information on each of these programs including their timing, eligibility requirements, lottery selection processes, and payment amounts. We also discuss the timing of any local eviction moratoria, which may have differed from the national moratoria in place from March 27, 2020 to August 23, 2020 under the CARES Act and from September 4, 2020 to October 3, 2021 under the Centers for Disease Control (CDC).

2.1 Chicago

We consider two housing assistance lotteries run in the City of Chicago in 2020. First, in the spring of 2020, the City of Chicago Department of Housing (DOH) partnered with UpTogether to implement the first round of an Emergency Financial Assistance (EFA) program for city residents at risk of housing instability. The program opened to applications on March 27th and closed on April 5th. During this period, the program received a total of 75,659 applications, approximately 80% of which were from renters. Due to the large number of applicants, grants were given out through a lottery process. All applicants were assigned a random rank and then eligibility screening and offers were made in rank order until funds were exhausted. Ultimately, 1,648 grants of $1,000 were distributed to eligible applicants

selected by the lottery.

People applied through a simple online portal. Upon being selected, applicants had to provide proof that they were Chicago residents whose income was impacted by the pandemic and was no greater than 60 percent of the area median income (AMI) prior to the pandemic. Grants were distributed directly to the applicants on a rolling basis through April and May of 2020.[5]

The second program we study was a similar program run by The Resurrection Project (TRP) in partnership with the Chicago Resiliency Fund. The program conducted a first round which was not over-subscribed. We study their second round which opened for applications on June 22nd, 2020. Similar to the DOH program, the TRP program was open to homeowners and renters, with 75% of applications coming from renters. In total, 10,300 individuals applied, and 1,718 grants of $1,000 were distributed via lottery.

The TRP program specifically targeted those who were not eligible and did not receive aid from the CARES Act and who were below 300% of the federal poverty line. While open to all Chicago residents meeting the eligibility criteria, undocumented individuals, mixed-status families, dependent adults and returning residents were particularly encouraged to apply. Those selected had to provide identification, proof of Chicago residence, and proof of income.

During the pandemic, Chicago renters were protected from eviction under both federal statutes and a citywide moratorium, which started on March 21, 2020 and ended on October 3, 2021.

2.2 Harris County

In November 2020, the government of Harris County, Texas partnered with Catholic Charities Galveston-Houston (CCGH) to administer $60 million in assistance to households experiencing economic hardship due to the COVID-19 pandemic. This was the second round of assistance distributed by the County, who administered an earlier round in July 2020.

Eligibility was limited to individuals earning less than 60 percent of local AMI or were receiving public assistance who could demonstrate that the pandemic had a negative impact on their income. Upon applying online they could upload a proof of hardship (e.g. letter of termination/furlough, paystub showing pay reduction of hours, etc) or self-attest using a self-certification form. When selected for processing, they would undergo an income eligibility screening.

[5]DOH conducted two further waves of grant programs later in 2020, but the applicant pools were much smaller with no, or very small, control groups.

Applications for assistance opened for five days and ended on November 5, 2020. The County sought to distribute the money equally across four county precincts and therefore capped the amount of assistance going to each precinct. With this constraint, there was excess demand in the lowest-income precinct (Precinct 1). A lottery was used to select among applicants. To prioritize more vulnerable applicants, the lottery featured varying odds of getting assistance depending on the applicant's census tract. Using the CDC's census tract level Vulnerability Index, census tracts were divided into quartiles and the odds of selection depended on quartile. Applicants in the top quartile received 50% greater odds of selection. Funds generally were dispersed in November and December 2020, with some payments going out in early 2021.

Applicants that were eligible and selected via the lottery received $1,200 in direct cash assistance. Applicants not selected for cash assistance received nothing. Receipt of assistance did not impact future eligibility for subsequent rounds of assistance.

Harris County tenants were protected by a state eviction moratorium at the beginning of the pandemic, covering March 19–May 19, 2020. Eviction hearings were allowed to resume on May 19, 2020, with evictions allowed to begin on May 26, 2020. While the CDC moratorium covered tenants in Harris County, there was significant local concern that Harris County eviction court judges were not honoring this order.[6] Even though law enforcement could not enforce an eviction order during the CDC's eviction moratorium and new eviction orders were automatically "stayed" (delayed) (Benfer et al., 2020), new eviction cases could be filed and new judgments could be issued throughout the moratorium. Throughout the duration of the CDC moratorium, Harris County averaged more than 500 new eviction filings per week, about roughly 60% of the pre-COVID volume (Hepburn et al., 2020).

2.3 King County

In late 2020, the government of King County, WA implemented an EFA program through their Department of Community and Human Services (DCHS). DCHS operated separate programs for large-scale landlords, who could apply directly for assistance for all eligible tenants, and a program for tenants of small-scale landlords through which tenants requested assistance directly. We study the latter program. Tenants requested assistance through an on-line interest form that was open from August 20, 2020 to December 4, 2020.

Eligibility for the program was based on responses to the on-line interest form, some of which was then verified if selected for the program. To be eligible for this program,

[6]Local news coverage emphasized uncertainty in how eviction moratoriums were interpreted by local judges: https://www.houstonchronicle.com/business/real-estate/article/Renewed-eviction-moratorium-comes-too-late-for-16364601.php

individuals had to verify income at or below 50% of the local AMI during the 60 days prior to application, demonstrate experience of financial hardship due to COVID-19 that threatened their ability to pay rent when due, and indicate risk of experiencing housing instability.

Starting on September 22, 2020, all eligible applicants who had not been offered assistance previously were entered into a lottery conducted weekly by DCHS. The number of applicants selected weekly depended on DCHS's staff capacity. Since people who completed the interest form earlier were eligible for more weekly lotteries, the probability of treatment varies with application week. Funds for the program were distributed from October 2020 to January 2021.

The assistance provided in King County was somewhat more generous than at other sites. Those selected for treatment received an average of three months of back rent or credit towards future rent paid to the landlord. Maximum available support covered six months of rent. To receive this payment, the landlord needed to agree to the following conditions: (i) to be paid reduced rent equal to the lesser of 80% of contracted rent or HUD fair market rent, (ii) to forgive any rental debt owed by the tenant beyond three months, (iii) to retain the tenant (except for cause), and (iv) to not raise rent through March 2022.

King County renters received protections from both state- and city-level eviction moratoria during our entire sample period. A statewide moratorium was instituted from February 29, 2020 through July 31, 2021. The moratorium statute ended in July, but its protections extended through October, with landlords' abilities to file evictions reinstated on November 1, 2021. Seattle, Burien, and Kenmore additionally extended citywide moratoria through January 15, 2022.

2.4 Los Angeles

In July 2020, the Los Angeles City Council approved the creation of the citywide Emergency Rental Assistance Subsidy (ERAS) program. The program was administered by the City's Housing and Community Investment Department (HCIDLA) and allocated $103 million in federal CARES Act and City funding to be used toward rent subsidies for low-income tenants.

To be eligible for assistance, renters needed to be City residents who could verify a pre-pandemic income at or below 80 percent of local AMI and provide documentation of a COVID-related loss of income occurring after March 13, 2020. The application period for the program spanned five days from July 13 to July 17, 2020. During this time, the City received roughly twice as many applications from renters as they anticipated being able to serve.

To allocate funding, applicants were entered into a randomized lottery. Those initially selected by the lottery were notified of their selection in late July 2020 and asked to submit documentation to prove their eligibility. Those not initially selected were placed on a waiting list to be contacted only after attempts were made to process applications for all those initially selected.

Applicants who were awarded funding received rent subsidies of up to $1,000 per month, with a maximum of $2,000 per household to be applied to current or future payments. Initially, rent subsidies were paid directly to landlords, who had to agree to the following three conditions to receive payment: (i) not to impose interest or late fees on rent owed, (ii) not to evict tenants during the local declaration of emergency due to COVID-19, and (iii) not to impose a rent increase during the 12-month period following the awarding of funding. Limited landlord participation led the city to approve a change to the policy in November 2020 that allowed the subsidy to be paid directly to the tenant as a one-time grant in the event that the landlord opted out or did not respond. The first payment went out in September 2020, and funds were fully exhausted by the end of December 2020. Ultimately, 56 percent of all grants were paid to landlords and 44 percent to tenants.

In addition to the ERAS program, LA City residents received protection via a residential eviction moratorium that was in effect from March 4, 2020 to June 31, 2022. Beginning on July 1, 2022, this moratorium only applied to households with income at or below 80 percent of AMI with demonstrated COVID-19-related financial hardship.

3 Data

3.1 Credit Data

Our analysis of credit market outcomes uses individual-level credit reporting data from Experian, one of the three major credit bureaus. The data contain a total of twelve monthly snapshots of consumer credit profiles observed bi-monthly beginning at the end of January 2020 and running until the end of November 2021. These data allow us to track the evolution of credit outcomes throughout the early pandemic and include information on credit scores as well as debt levels and delinquency status for all major forms of consumer debt. In addition to these standard credit variables, the data also include a dynamically updated consumer ZIP code, which we use to infer applicants' residential mobility.

Experian matched program application data to credit reporting data for all sites using the applicant's name and address. All lottery participants were eligible to be matched, except in King County where only survey respondents were eligible to be matched. To

protect applicant confidentiality, all personally identifiable information was removed from the matched data prior to being returned to the research team from Experian. Among people eligible to be matched, match rates across all sites were generally high, ranging from roughly 74 percent in Harris County, TX to 81 percent in King County, WA. Conditional on being observed in the credit data, nearly all applicants were present in every monthly snapshot. Our analysis will therefore focus on a fully balanced panel of credit outcomes.

3.2 Infutor Data

As an alternative way to measure address changes, we also use consumer reference data from Infutor Data Solutions. Infutor combines a variety of consumer information from many sources (e.g. phone bills, voter files, magazine subscriptions, and property deeds) and uses it to create a residential history for most adults in the United States. The result is a database of exact addresses that crosses state boundaries and includes start and end dates for each address. While created originally for commercial applications like identity verification and marketing, academics have recently used this data to measure housing stability and migration in the wake of rent control, eviction, and new housing construction (Diamond et al., 2019; Collinson et al., 2022; Asquith et al., 2023). For the present study, the Infutor data proves useful because it provides a measure of housing stability that can be measured consistently across several metropolitan areas. Because it is compiled from consumer records, it matches to other data at a somewhat lower rate than other sources of administrative records and tends to miss groups of people who tend to have shorter paper trails (e.g. young or Hispanic). Even so, prior work shows that it can measure housing instability in similar high-risk populations; for example, address changes as measured in the Infutor data spike around the time people at risk of homelessness request and are denied emergency financial assistance from a large call center (Phillips, 2020).

We measure address changes among the set of people for whom we can match program and Infutor records. Infutor records include name, date of birth, and address, and we match on combinations of these identifiers in each location, depending on their availability for a given site. We limit our analysis to individuals who have an address in Infutor with a start date prior to their application for financial assistance. This process yields match rates of 35% in Chicago DOH, 21% in Chicago TRP, 35% in Harris county, 46% in King County, and 50% in Los Angeles. For this sample, we identify the address at which the person resided at the time of applying for assistance and measure as an outcome whether Infutor later observes this person at a different address.

3.3 Survey Data

In addition to the credit reporting and address history data, our analysis also makes use of follow-up surveys that were administered in the months following treatment for all five programs. Table A.1 reports the range of dates over which funding was dispersed and over which follow-up surveys were administered for each site. Funds for King County, Harris County, and Los Angeles were distributed in the last quarter of 2020. Funds for Chicago DOH were distributed in the second quarter of 2020, while funds for Chicago TRP were distributed largely in the third and fourth quarter of 2020. The follow-up surveys were administered one to six months after treatment, with the Chicago DOH and Los Angeles surveys coming in the month after treatment and other sites, such as Harris County, administering their survey four to five months after treatment. In some cases, retrospective questions were used to better align the elapsed time between treatment and measured outcomes across sites.

This research began as three independent projects, and site-specific surveys were initially developed independently and in collaboration with partner organizations. Upon realizing that the three projects were similar, surveys were shared among research teams and, to the extent possible, aligned. In practice, this resulted in similar, but not identical, survey questions across sites. Thus, many outcomes are measured similarly with only small differences in wording or timing. There are three cases worth noting where we were unable to closely align questions. First, for constructing the "All Rent Paid" outcome, the Los Angeles survey asks if tenants are behind on rent, while the other sites ask if a particular month's rent was paid in full. Second, for the "COVID Positive" outcome, the Chicago DOH survey codes this outcome as one if the respondent selects "I got sick" to the multi-response question "How has the COVID-19 epidemic impacted you?" Other sites use questions specifically about if the respondent suspected that they were infected with COVID-19 or had a positive self test. For the outcomes "Experienced Homelessness" and "Stayed in a Homeless Shelter," the Los Angeles survey asks if either have happened since May, while all other sites ask about the past month. Appendix Section A provides a detailed discussion of how survey-based outcomes are constructed and aligned across sites.

The bottom panel of Table I reports the response rates for treatment and control groups at each site. Survey response rates by treatment group vary between 13 and 40 percent. For most sites, treated individuals are somewhat more likely to respond to surveys than the control group. Appendix Table A.3 shows that survey non-response leads to modest imbalances in baseline characteristics. As we describe in the next section, our main analysis will correct for this imbalance by weighting responses by the applicant's inverse probability of responding to the survey as predicted from a set of baseline characteristics observed for

all applicants (Wooldridge, 2007).

3.4 Evictions and Homelessness Data

We supplement the data sources above with eviction data from Harris County, TX and homeless program use data from King County, WA. These data allow us to track more extreme forms of housing instability that we cannot measures across all sites.

Data on eviction filings and judgments was collected from the Harris County Justices of the Peace, the court entity that handles eviction cases, for calendar years 2019–2021. The eviction records contain basic details of the case, such as filing date, amount owed, and disposition or judgment details. We link these to Harris County application data using name and address as described in the appendix. A limitation of this data is that we can only link to evictions occurring *at the application address*, which will under-count total exposure to eviction activity.

We measure homeless program use with King County Homeless Management Information System (HMIS) data. HMIS systems record use of homeless programs in a community in a structured manner, following US Department of Housing and Urban Development guidelines. In King County, as in other communities, various public and private providers of homeless programs must record who accesses their programming and when. Starting from the set of all HMIS program enrollments between March 2019 to September 2021, King County staff extract records that fuzzy match to lottery applicants based on name, date of birth, and ZIP code. We limit the data to programs only offered to people who are already homeless (e.g. not prevention programs) so that program use indicates both receipt of services and being homeless at the point of program entry.[7] The existing literature uses similar measures to study the effect of temporary financial assistance on homelessness (Evans et al., 2016; Phillips and Sullivan, 2022a,b).

4 Research Design

4.1 Empirical Specification

The core empirical goal of our analysis is to estimate the causal effect of receiving emergency financial assistance on various mean outcomes of interest. However, simply comparing av-

[7]These categories of programs include emergency shelter, street outreach for people living unsheltered, coordinated entry (entries for requesting but not yet accessing oversubscribed services), diversion programs provided to people who have just become homeless, and more intensive housing assistance (permanent supportive housing, rapid re-housing, and transitional housing).

erage outcomes across those who did and did not receive assistance will typically provide biased estimates of this effect. Most financial assistance programs select clients based on their vulnerability or their recent experience of a large negative shock. Even in settings where assistance is administered via lottery, take-up of assistance is typically endogenous. Households that fail to take-up assistance may be different along dimensions that are unobservable to the econometrician. For example, they might have more tumultuous lives, making it difficult for them to submit necessary paperwork or respond to attempts to contact. Alternatively, they might not take-up assistance because they have relatively less need for it, due to access to other financial support.

In this paper, we overcome these challenges by estimating treatment effects using random variation in *offers* of emergency financial assistance. Specifically, we limit our sample to people who apply for assistance and study contexts in which local jurisdictions rationed oversubscribed assistance by lottery. To measure the effect of being randomly offered assistance, we estimate simple linear regressions of the following form:

$$Y = \gamma_0 + \gamma_1 S + \mathbf{X}'\phi + \omega, \tag{1}$$

where Y is an outcome of interest, such as credit scores, and S is an indicator for being selected to receive an offer for assistance by the lottery. Because several jurisdictions administered their lotteries in a way that resulted in conditionally random offers that depended on observable applicant characteristics, X, we also directly control for these factors in the regression. In particular, we control for week of application fixed effects in King County, and census tract fixed effects in Harris County. In Chicago, we control for application-count fixed effects to account for a small number of individuals applying more than once.[8]

The coefficient of interest is γ_1, which measures the difference in conditional mean outcomes between people selected and not selected for assistance. Because we limit our sample to people who apply for assistance and the programs randomly select among applicants, this coefficient provides an unbiased estimate of the intent-to-treat effect of being selected to receive an offer of emergency financial assistance. To increase precision, we additionally control for several baseline applicant characteristics observable in all sites: gender and race indicators. When analyzing credit report data, we also control for lagged credit outcomes for the period prior to lottery applications and for January 2020.[9]

While γ_1 provides an unbiased estimate of the effect of being offered assistance, not

[8]Section 2 discusses how these factors were used in the lottery allocation in more detail.

[9]Our credit outcomes are credit score, total debt balance, balance in collections, balance delinquent, utility balance in collections, and indicators for having any delinquency, any revolving credit account, and any auto loan.

everyone who was initially offered funding through the lottery actually received it. There are several reasons this could occur, such as failure to make contact, determining the applicant was ineligible, or failure to complete required forms. In these cases, it is also possible that applicants who were not originally selected for assistance via the lottery nonetheless ended up receiving it. To account for this type of imperfect take-up, we focus primarily on instrumental variables (IV) estimates of the effect of the treatment-on-the-treated. We calculate the IV estimates using two-stage least squares (2SLS) with the first and second stages of the IV regression given by:

$$R = \alpha_0 + \alpha_1 S + \mathbf{X}'\delta + \varepsilon \tag{2}$$

$$Y = \beta_0 + \beta_1 \widehat{R} + \mathbf{X}'\lambda + \omega, \tag{3}$$

where R is an indicator variable for actually receiving assistance, and all other variables are as previously defined.

Table II reports the coefficient on winning the lottery from the first-stage regression for each site. The table also reports the control-group mean and the partial F-statistic on the coefficient. For each site, we have a large and statistically significant first stage (with the F statistics ranging between 476 and 6,000). The size of the first-stage coefficients differ as there were differential take-up rates across site. Some of these differences come from how the lotteries were implemented and specifically on how much screening took place at the time of the application versus once being selected as a lottery winner. For example, Chicago's initial application required little work and candidates were screened more rigorously upon being selected. Sites also differed in the extent to which assistance required the agreement of landlords. In King County and during the early stages of Los Angeles's program, assistance was paid to landlords, some of whom did not accept program limits on future evictions or rent increases. In contrast, Harris County made direct payments to the household applying for assistance.

We estimate all treatment effects site-by-site. For example, when measuring the effect of treatment-on-the-treated, we estimate equations (2) and (3) separately for each site. We then compute the simple, un-weighted average of these treatment effects across sites. We estimate heteroskedasticity robust standard errors for each site and compute a full sample standard error assuming independence across sites.

4.2 Balance Tests and Descriptive Statistics

4.2.1 Balance

Table I provides evidence of balance between the treatment and control groups in each site. For each site, we report the control mean ("Control"), the treatment group mean ("Treatment"), and their difference ("Diff"), measured using an OLS regression of equation (1) that includes any site-specific design controls. The table summarizes characteristics of applicants, which we group into four broad areas. The first panel uses the complete lottery sample and provides information on demographics (or imputed demographics) derived from applications for assistance.[10] Similarly, the second panel provides details on baseline financial characteristics, such as income and amount of rent owed, collected at the time of application. The third panel reports ZIP code characteristics of applicants from the American Community Survey. Finally, for those who match to credit records, we report baseline credit measures from January 2020.

Overall, applicants selected by the lottery appear observably similar to applicants that are not selected at baseline. Across the demographic and financial characteristics, the treatment and control groups are generally balanced. Where there are some statistically significant differences between the two groups' populations, the differences are quantitatively small.

Of the four sites, King County is generally the least balanced across treatment and control. This reflects the fact that data in King County is only available for applicants who responded to the survey. As discussed in Section 4.2.1, survey response rates were not fully balanced across treatment and control groups. To address this issue, in any analysis of survey outcomes we will always weight answers according to the applicant's inverse probability of responding to the survey (Wooldridge, 2007). To calculate these weights, for each site we first estimate a logit regression of a whether an applicant responded to the survey, a dummy for treatment assignment, and a host of baseline control variables.[11] We then predict an applicant's probability of responding under the control condition and use the inverse of this predicted probability as the weight in our regressions. As we will discuss in Section 5.1, results are similar with and without these weights.

[10] In Harris County we impute gender and race using R packages gender, genderdata, and wru.

[11] In all sites except King County, these controls included ZIP-code level median rent, poverty rate, share high-school educated, share Hispanic, share White, share Black, and share Asian. Additional applicant-level controls varied across sites depending on data availability. In Los Angeles these controls included: age, household size, gender, language (english-vs-non), ethnicity, race, income, monthly rent, total rent owed, months rent owed, COVID hardship reason. In Harris county they included: language (English-vs-non), contact information(valid email, two phone numbers, ect.), public assistance type, income eligible, gender, race, and ethnicity. In King County they included indicators for race, gender, and primary language, and in Chicago they included...

4.2.2 Trends in Financial Status

Figure I extends the descriptive statistics from Table I to plot the full evolution of outcomes for several key measures of financial health: credit scores, total balances in delinquent accounts, and total balances in collections for utility bills. Each panel reports the raw means (and standard errors) for a given outcome and site separately for the treatment and control groups. Means are plotted by month in all months for which credit reporting data is available. The vertically dashed line denotes the month in which the lottery was conducted for each site. The vertical red line marks the beginning of the pandemic (March, 2020).[12]

Three features of this figure stand out. First, the balance between treatment and control groups documented in Table I for January, 2020 persists across all pre-treatment months. An exception is King County where we can only link credit records of survey respondents, introducing selection as described above. Second, the overall trend for both treatment and control groups across all sites is one of generally improving financial health. Credit scores trend upward throughout the sample period while delinquent balances and utilities collections both trend downward. This finding is consistent with evidence from other work showing that various measures of financial well-being actually *improved* for most households during the early pandemic (Han et al., 2020). Third, there is no meaningful divergence in outcomes between treatment and control groups either at the onset of the pandemic or in the months following treatment. This finding, which previews our main results, is consistent with the idea that the emergency assistance grants we study likely did not have meaningful effects on downstream measures of financial health like delinquency or default.

5 Results

In this section, we report effects of receiving assistance on outcomes measured in the follow-up surveys, in administrative credit data from Experian, and on geographic mobility measured in both Experian and Infutor data. When depicting effects over time in figures, we report ITT and IV estimates separately by location. When presenting results in tables, we report IV estimates separately by sites across columns, and then pool data across sites to report the overall combined effect in the final column of the table.

[12] In King County, applications and lotteries happened over time. So, we display data by relative time, limiting to time periods for which data is available for the full sample. We indicate the start of the pandemic using the average for the sample.

5.1 Impacts on Survey Outcomes

Table III reports IV estimates of the effect of assistance receipt on the likelihood of paying rent and bills, self-reported measures of health, and feelings of economic insecurity. Outcomes were typically measured with respect to the month prior to survey collection, which occurred at different times across locations. However, questions about rent payment asked about specific months following the month of the lottery.[13] Columns (1) through (5) report effects separately by location, and column (6) pools effects across sites. Given differences in question wording in the Los Angeles survey, we also report pooled effects excluding Los Angeles in column (7). Control group means are reported in brackets. While Table III reports estimates that come from weighted regressions that adjust for attrition, the results are qualitatively similar when not weighting (Appendix Table A.2).

Survey data reveal three key results. First, assistance receipt led to higher rates of rent payment at the time of follow-up. Consistently across Harris County, King County, and Chicago, recipients were 5.2–13.1 percentage points more likely to have paid their rent in full in the previous month. Across those three sites, the combined increase in rent payment was 7.3 pp, a 13.8 percent increase over the control group mean. Effects are noticeably different for Los Angeles, though this may result from a different survey question that focuses on cumulative rent payment (i.e., "behind on rent"). The effects of assistance on other types of bill payment appear more mixed. Across four of the five lotteries we find no evidence of impacts of assistance on other bill payment, but some indication in L.A. that receipt increased bill payment other than rent.

Second, assistance recipients experienced improved physical and mental health. Recipients in the Chicago DOH lottery experienced a 4.9 percentage point decline in the likelihood of getting sick in the previous month as a consequence of COVID-19, and point estimates in King County and Harris County that measure differences in COVID-19 infections are of similar magnitude, albeit measured with less precision. Similarly, recipients were less likely to report feelings of anxiety or depression, a 3.3 percentage point decrease (6.5 percent) across Harris County, King County, and Chicago.

Finally, while recipients were less likely to report feeling worried about evictions, we find no consistent evidence across sites that they experienced declines in actual housing or food insecurity. Recipients are less likely to state they have experienced homelessness, but this difference does not translate to lower rates of emergency shelter use. Administrative records from King County similarly show no sign of decreased shelter use (see Appendix Table A.6). Respondents also report less frequent food insecurity in the previous month, but

[13] See appendix A for details about the survey questions across each site

this difference is not statistically significant. Overall, results using survey measures suggest modest positive effects for recipients.

5.2 Impacts on Credit Measures

Figure III confirms that credit outcomes for those who received assistance and those who did not were similar both before and after the lotteries. Differences in outcomes are plotted for Houston (left column), Seattle (middle column), and L.A. (right column). Each row of figures reports effects on a different outcome: credit score (top row), balance in delinquent accounts (middle row), and balance in utilities collections (bottom row). ITT and IV estimates are depicted by navy diamonds and gold circles, respectively. Across outcomes and locations there is a consistent pattern of results, namely that being selected to receive assistance (ITT) or receiving assistance (IV) did not improve financial well-being. For only one outcome, in one location, in one time period, we are able to reject the null hypothesis of no effect: there is a small increase in credit scores in Houston two months after treatment.

Table IV summarizes the IV effects of assistance across a broad range of credit outcomes measured two months and ten months after receipt. There is some evidence that recipients used the additional resources to pay down some of their debt in the short-run. Assistance led to a $129 decrease in debt balance in collections (column 5), which is driven by declines in the Seattle and Los Angeles samples. However, there are no consistent effects on credit score, total balances, balance in utility collections, delinquent accounts, or bankruptcy. Recipient households were no more likely to increase their durable consumption by taking out a car loan/lease, and no more likely to use a revolving line of credit. Within ten months of assistance receipt, there were no statistically significant differences in administrative credit outcomes.

These estimates are precisely estimated. For example, the 95% confidence interval for the short-run effect on credit scores ranges from -3.4 to 3.6. Thus, we can reject large effects on credit scores estimated in the literature, such as the 16.5 point decrease in response to an eviction (Collinson et al., 2022) or the 9.4 point increase in response to the removal of a bankruptcy flag (Dobbie et al., 2020). Similarly the 95% confidence interval on the 10-month balance in collections, which ranges from -$175 to $196, allows us to reject the $302 increase in collections balances that occurred 4 years after a hospitalization among non-elderly insured adults (Dobkin et al., 2018). Overall effects on credit outcomes are small.

One possible explanation for the modest effects on credit outcomes is that they mask larger effects across some subgroups. We investigate this possibility by examining several subgroups based on financial stability in the lead-up to applying for assistance. We consider

subgroups based on both levels and trends in credit measures, including groups with credit scores below (above) 580, delinquent account balances above (below) $5,000, decreasing (increasing) credit scores, and increasing (decreasing) delinquent account balances. Figure A.3 plots the standardized IV estimates for the effects of assistance on credit scores, balance in delinquent accounts, and utility collections separately by subgroup and site.[14] Within sites there are few meaningful differences among subgroups. Moreover, there is no consistent pattern *across* sites. Taken together, we find little evidence that more disadvantaged individuals in our sample experienced larger gains from assistance.

5.3 Impacts on Residential Mobility

Table V reports IV effects of assistance on residential mobility. We measure address changes in two administrative data sets during the short-run (two months after lottery) and long-run (ten months after lottery). Using the Infutor data, we indicate a change of address if an applicant's current address differs from their address at time time of application. In the Experian data, address changes are measured by changes in ZIP code relative to their application ZIP code. Move rates do not differ noticeably with receipt of assistance. For example, 10-month move rates in the Infutor data decrease by a statistically insignificant 0.8 percentage points from a mean of 9.3, and the 95% confidence interval on this estimate ranges from -2.7 to 1.2 percentage points. For context, an eviction order is estimated to increase the likelihood of an address change by 8.2 percentage points one year later Collinson et al. (2022). We find no consistent evidence that geographic mobility changed as a result of receiving emergency assistance.

5.4 Impacts on Other Measures of Housing Instability

Next, we investigate whether assistance reduced other measures of acute housing instability, including evictions and homelessness. These are not outcomes we can track uniformly across locations, so we draw on evidence from individual locations. In particular, we study effects on eviction activity in Harris County, TX, and effects on homelessness in King County, WA.

As discussed in section 2, new eviction cases could be initiated and new judgments issued throughout the study period in Harris County, including during the period covered by the CDC moratorium. However, the enforcement of eviction orders by law enforcement was stayed during the moratorium. In contrast, eviction moratoria cover the entire sample period in King County, so use of homeless programs provides a more useful outcome. To

[14] We standardize IV estimates by dividing the point estimates by the control group standard deviation of the outcome.

examine impacts on evictions filing and judgment activity, we link the sample of Harris County assistance applicants to eviction data, and to measure effects on homeless program use we match King County applicants to data on homeless program use, as described in Appendix .

Appendix Table A.5 reports the effects of assistance on eviction filing (initiating a new case) and eviction judgments against the tenant at 2 months and 10 months after application, to mirror our credit results. Column 1 summarizes the control mean and standard deviation. Even for this quite financially-distressed sample, new eviction filings are rare during this period: only 1.2% of not-assisted applicants had an eviction filing 10 months after application. For reference, we compare the eviction filing rate in our sample to the number of filings per renter household in the application tracts in our Harris County sample. In application tracts, about 7% of renter households had a filing in 2019. This suggests that while eviction activity continued, filing remained depressed relative to typical levels during 2020–2021.

Column 2 of Appendix Table A.5 reports the IV estimates for the effects of assistance. Receiving assistance through the Harris County lottery appears to lower risk of eviction filing and judgment, but the point estimates are all economically small and none are statistically significant. These results suggests that receipt of assistance did not substantially change risk of eviction in Harris County.

Effects on homeless program use similarly show no signs that financial assistance reduces homelessness. Appendix Table A.6 displays IV estimates of treatment effects for King County. Baseline rates of homeless program usage are somewhat muted: 2.7% of the control group accesses any homeless programs within 9 months, and 0.6% access emergency shelter. These rates are somewhat lower than 6-month control group rates of 4.1% for program use in Phillips and Sullivan (2022b) and 2.9% for shelter entry in Evans et al. (2016). Treatment effects on use of any homeless services are actually positive, 2.0 percentage points, though with no detectable effect on shelter entry. Results are similar if we focus on either the full sample of lottery applicants or restrict to survey respondents as in the prior analysis of King County records. Despite suggestions from surveys that financial assistance may reduce homelessness, administrative records show no signs of decreased homelessness.

5.5 Targeting

While we find small or no impacts of emergency financial assistance on survey outcomes, credit outcomes and housing stability, the large-scale emergency assistance programs we evaluate may have helped better target pandemic assistance to households with more need.

Across all sites, these programs required applicants to demonstrate they met the eligibility requirements outlined in Section 2. These requirements both targeted funding towards those with lower income and also required applicants to go through the ordeal of applying. As a result, these programs may have reached lower-income or more financially distressed households.

To evaluate the targeting of these emergency assistance programs, we compare credit outcomes to a random sample of renters from the same city. To do so, we use a panel consisting of a 10% sample of individuals from Equifax. We restrict to renters living within one of the catchment areas for the programs in the months the lotteries took place. We focus on credit scores (VantageScore 4.0), balance in delinquent accounts, and balance in collections as these outcomes can be best aligned between the Equifax and Experian data.

Appendix Figure A.5 plots the outcome trajectories for treatment, control, and the random samples of renters over time relative to the treatment month for Harris County, King County, and Los Angeles. Across all sites, we see very large gaps between the random sample of renters and those who participated in the lottery. In Harris and King Counties the gap in credit scores is more than 100 points in all periods, while the gap is 50 or more points in all periods in Los Angeles. The lottery participants also largely have very low baseline credit scores ranging between 550 and 630 (with the standard cut-off for sub-prime being 600), which rise smoothly by around 25 points during the pandemic.

Similar gaps are present for balance in delinquent accounts and balance in collections with those participating in the lottery having average balances in collection ranging from about $3,000 in Harris county, $1,400 to $1,800 in King County, and around $1,000 in Los Angeles. Trends are not widely different between lottery participants and the random sample of renters though, on average the gaps between the two groups close somewhat over time. The random sample of renters have largely flat trajectories, while the treatment and control group have rising credit scores and flat or decreasing balances over the whole time period.

Overall, the evidence suggests that the emergency assistance programs were successful at targeting renters who were substantially more financially distressed.

6 Relationship with Prior Literature on Emergency Assistance to Renters

Our results on housing stability contrast somewhat with other evidence on emergency rental assistance programs. Prior work finds that emergency assistance decreases rates of homelessness by about three-quarters in Chicago (Evans et al., 2016) and San Jose (Phillips and

Sullivan, 2022b). Measured effects on housing moves are noisier, but Phillips (2020) finds decreases of 25–40 percent, and follow-on studies in Chicago find changes in arrests (Palmer et al., 2019) but not healthcare use (Downes et al., 2022). In contrast, we find no or small effects on housing moves, finances, and health. In survey data, we see perhaps slight decreases in homelessness, but in the one site (King County) where we observe administrative records on homeless service use similar to those used in prior studies, being offered assistance actually slightly increased homelessness program use. The lack of effects we observe is not due to statistical imprecision; we fail to find effects like those observed in prior studies despite studying much larger samples.

The contrast between our results and those of the literature does not arise from major differences in the interventions being studied. Emergency financial assistance programs offered during COVID were largely designed based on existing programs and even operated by the same agencies. Traditional programs and COVID programs both provide temporary assistance in relatively modest amounts. For example, clients offered financial assistance in Evans et al. (2016), Phillips and Sullivan (2022b), and Phillips and Sullivan (2022a) receive an average of 0.7, 1.0, and 0.7 months of rent, respectively, which is similar to the programs we study in Chicago, Harris County, and Los Angeles, though the King County program is somewhat more generous. Finally, traditional programs typically pay assistance directly to the landlord, which was the case in King County and for the majority of payments in Los Angeles. The COVID-era programs in Harris County and Chicago, in contrast, paid assistance directly to households. Altogether, the programs we study are similar to those documented in the existing literature.

The programs we study also target clients that appear largely similar to participants in pre-COVID financial assistance programs. As noted in Section 5.5, COVID-era programs attracted clients with many financial barriers and precarious living arrangements. Across our four sites, 13.0 percent of the control group changed addresses in the 10 months after random assignment. These values are similar to prior studies: control group move rates were 7.9 percent in Chicago (Phillips, 2020) and 17.7 percent in San Jose (Phillips and Sullivan, 2022b). Lottery applicants also appear to have similar housing histories to individuals applying for traditional emergency financial assistance. The King County COVID EFA lottery and the pre-COVID homelessness prevention program studied in Phillips and Sullivan (2022a) were operated by the same agency, use the same data sources and methods, and had enrollment periods separated by only 5 months, providing an ideal comparison. In the King County COVID program, 19.7 percent of the sample had moved in the 12 months prior to the onset of COVID in March 2020, compared to 18.1 percent for the sample in Phillips and Sullivan (2022a). Homeless service usage is slightly different between the two samples, 4.8 and 7.9

percent, respectively, but nevertheless still high. Indeed, if the COVID assistance achieved the significant reductions in homeless service use found in pre-COVID studies of emergency, we would be well-powered to detect such an effect.[15]

These results suggest that the broader policy context, rather than differences in the programs or targeting, led to different outcomes for COVID-era temporary financial assistance programs. First, local and national eviction moratoria could have prevented housing instability and its potential negative consequences during our sample period. This explanation is possible but seems unlikely, because people who were denied assistance in the lotteries we study experience similar levels of housing instability as people in prior studies. More likely, several large federal programs provided similar cash or cash-like assistance to broad groups that overlap with our lottery samples: expanded unemployment benefits, economic impact payments, expansion of food assistance, and so on. While we do not have data on participant use of these broader programs, we do know that many people in our study sample accessed later rounds of rental assistance. For example, 31 percent of lottery winners in King County (and 28 percent of those not selected) accessed a second round of assistance that was offered from July 2021 to May 2022. Also, as demonstrated above, measures of creditworthiness and financial distress were already improving for households in our sample prior to the provision of the assistance we study, which is consistent with the idea that households in the sample may have been sufficiently aided from other sources to avoid more extreme hardship or instability from the COVID shock. Temporary financial assistance programs appear to matter more during periods where more generous alternatives are not available.

7 Conclusion

This paper studies an unprecedented expansion of emergency rental assistance in response to economic disruption from the COVID-19 pandemic. Our analysis overcomes three major hurdles in examining assistance policies in rental markets: 1) lack of centralized data collection on rental assistance, 2) lack of administrative data on key outcomes, and 3) selection bias when comparing those who did and did not receive rental assistance.

Our multi-site study uses application data from five programs that received over 200,000 applicants in four major urban areas and leverages variation from oversubscribed lotteries to cleanly identify the causal effect of emergency housing assistance. For each site, we link data on applications for assistance to novel survey data, administrative credit panel data, and

[15]The 75 percent decrease observed in prior studies would imply a 3.6 percentage point decrease from the base HMIS rate of 4.8 percent. The standard error of 0.8 percentage points from Appendix Table A.6 implies we have 80 percent power to detect a 2.2 percentage point decrease.

consumer reference data to estimate the effects of assistance on housing instability, financial insecurity, and mental health. We also separately estimate effects on homeless system use and evictions in two of our sites.

We find that assistance caused an increase in rent payment by 5 to 13 percentage points in the months immediately after application and improved self-reported mental health. However, rental assistance had little detectable short- or long-run effects on broader measures of financial security. The effects of assistance appear modest for all subgroups. We also find little evidence that assistance reduced homelessness in King County or evictions in Harris County. However, we show that, compared to the typical renter from the community, the typical recipient of assistance had substantially lower credit and higher debt prior to applying for assistance.

In contrast to previous research on smaller-scale emergency rental assistance programs (Phillips and Sullivan, 2022b; Evans et al., 2016), which finds large relative effects of assistance on housing instability, we find only modest impacts of assistance on housing and financial outcomes. This prior work has focused on the effects of programs which target idiosyncratic shocks faced by vulnerable renters. We study much more widespread assistance distributed in response to a macroeconomic shock and find less pronounced effects. A straightforward interpretation is that macroeconomic shocks tend to be accompanied by other fiscal stimulus that protects renters from some of the idiosyncratic risks they face. Consistent with this channel, we find that individuals who applied for assistance — regardless of whether they actually received assistance — experienced broad-based improvements in financial circumstances in the post-application period, likely due to the myriad efforts to support the pandemic economy. This finding suggests that temporary emergency assistance may be less effective during periods when alternative forms of assistance are expanded.

A separate question is how effective these programs were in transferring resources to vulnerable households. Our results imply that emergency housing assistance programs, despite being rolled out quickly in response to a looming eviction crisis, were successful in getting assistance to individuals that were more financially distressed. The recipients of assistance across our sites averaged 50 to 100 points lower credit scores and from $3,000 to $7,000 more in unpaid bills than other renters from the same community. The programs that we study targeted low-income renters but relied heavily on self-attestation of need for eligibility screening. An implication from this finding is that targeting assistance to low-income renters during macroeconomic downturns through organizations that already work with people on the edge of eviction and homelessness may be a reasonable tag for reaching financially distressed individuals.

Federal, state, and local governments used a variety of tools to keep individuals in their

homes during the height of the pandemic. Lessons from the Great Recession generated extensive evidence on foreclosure prevention policies, but less is known about policies for renters. Policy responses to COVID included a greater emphasis on renters, which provides an opportunity to learn about these policy options. Our lottery-based estimates of the effectiveness of emergency housing assistance measure the impact and targeting properties of one major component of the policy response. They also serve as a starting point for further research. Other major policies, such as eviction moratoria, were more near universal, making it difficult to assign specific credit. Further analysis will be required to discern which elements of the CARES Act relief package-like the moratoria or other state and local policies-were most essential to household economic stability.

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8 Tables and Figures

TABLE I

Summary Statistics and Balance

	Harris County			King County			Los Angeles			Chicago DOH			Chicago TRP		
	Control	Treat	Diff	Control	Treat	Diff	Control	Treat	Diff	Control	Treat	Diff	Control	Treat	Diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Demographics															
Age	–	–	–	38.729	38.528	0.098	41.577	41.315	−0.262	34.950	35.737	0.788*	38.999	38.948	−0.051
Female	0.649	0.643	0.012	0.618	0.618	0.005	0.546	0.541	−0.005	0.543	0.526	−0.017	0.562	0.561	−0.002
Household Size	–	–	–	–	–	–	2.660	2.664	0.005	2.990	3.002	0.012	4.051	4.004	−0.047
White	0.044	0.037	−0.003	0.231	0.235	0.007	0.213	0.217	0.003	0.218	0.223	0.004	0.039	0.034	−0.005
Black	0.632	0.649	−0.009	0.325	0.345	−0.027*	0.116	0.115	−0.001	0.333	0.336	0.003	0.119	0.112	−0.008
Hispanic	0.256	0.253	0.014*	0.177	0.153	0.008	0.535	0.540	0.005	0.406	0.390	−0.016	0.800	0.821	0.021*
Asian	0.021	0.021	−0.005**	0.074	0.073	−0.001	0.109	0.105	−0.004	0.043	0.052	0.009*	0.041	0.032	−0.009*
Financial Characteristics															
Income	–	–	–	–	–	–	29.533	29.835	0.301	–	–	–	–	–	–
Rent	–	–	–	–	–	–	1.614	1.624	0.009	–	–	–	–	–	–
Rent Owed	–	–	–	–	–	–	2.509	2.580	0.071*	–	–	–	–	–	–
ACS Zip Code Data															
Median Rent	0.988	0.991	−0.001	1.485	1.484	−0.000	1.419	1.424	0.005	0.883	0.889	0.006	1.004	1.000	−0.004
Poverty Rate	0.625	0.622	0.008**	0.115	0.116	−0.001	0.198	0.197	−0.001	0.205	0.204	−0.000	0.199	0.200	0.001
% with High School Diploma	0.757	0.749	−0.000	0.902	0.902	0.001	0.753	0.756	0.003	0.818	0.818	−0.001	0.767	0.768	0.000
% Hispanic	0.437	0.456	−0.000	0.123	0.123	−0.001	0.487	0.483	−0.005	0.271	0.269	−0.002	0.487	0.484	−0.002
%Non-Hispanic White	0.118	0.111	0.000	0.519	0.518	0.000	0.264	0.268	0.004	0.215	0.216	0.001	0.207	0.208	0.001
% Black	0.384	0.380	−0.000	0.101	0.102	−0.000	0.090	0.089	−0.001	0.291	0.291	−0.001	0.236	0.239	0.003
% Asian	0.046	0.039	0.000	0.176	0.176	0.000	0.128	0.130	0.002	0.047	0.049	0.002	0.053	0.052	−0.001
Experian Linked Data: Jan 2020															
Credit Score	552	551	−0.985	583	575	8.510*	637	636	−0.961	–	–	–	–	–	–
Balance Across All Trades	20.616	21.843	−1.336*	19.314	18.346	2.624	19.775	20.011	0.236	–	–	–	–	–	–
Balance in Collections	3.232	3.299	−0.015	1.436	1.829	−0.398**	0.917	0.932	0.015	–	–	–	–	–	–
Balance in Utility Collections	0.585	0.577	0.020	0.298	0.386	−0.084**	0.125	0.120	−0.005	–	–	–	–	–	–
Balance in Delinquent Accts.	10.059	9.816	0.296	7.776	8.355	−0.567	4.986	4.988	0.003	–	–	–	–	–	–
Any Auto Loan or Lease	0.285	0.290	−0.002	0.308	0.334	−0.019	0.279	0.274	−0.005	–	–	–	–	–	–
Any Personal Bankruptcy	0.019	0.019	−0.000	0.067	0.087	−0.016	0.050	0.043	−0.007**	–	–	–	–	–	–
Any Open Revolving Line of Credit	0.334	0.329	0.001	0.529	0.499	0.035	0.656	0.658	0.003	–	–	–	–	–	–
Survey															
Response Rates	0.187	0.222	0.036***	0.235	0.264	0.016	0.129	0.148	0.020***	0.283	0.409	0.126***	0.176	0.310	0.135***
N†	4,930	13,827	18,777	6,868	5,087	11,955	13,062	11,315	24,377	73,126	1,537	74,663	4,688	1,765	6,453

Notes: Data come from program applications, the 20XX American Community Survey (ACS), and credit records from Experian. The sample includes all program applicants, except credit attributes which are restricted to individuals linked to the balanced panel of Experian records. For each site, we report the average characteristics for individuals not selected by the lottery (Control) and selected by the lottery (Treat). Conditional differences in average characteristics between these two groups come from regressions that control for site-specific design controls. See Section 4. All monetary values expressed in 2020 U.S. dollars divided by 1000. ***0.01, **0.05, *0.10 significance levels. †We report the maximum observation counts. Observation counts for Experian characteristics are lower than the rest of the characteristics.

TABLE II
First Stage

	Received Assistance				
	Harris County	King County	Los Angeles	Chicago DOH	Chicago TRP
	(1)	(2)	(3)	(4)	(5)
Selected by Lottery	0.594***	0.372***	0.207***	0.431***	0.910***
	(0.004)	(0.008)	(0.006)	(0.002)	(0.004)
Control Mean	0.000	0.046	0.347	0.001	0.000
F Statistic	23,735	2,442	1,093	24,021	23,835
N	23,026	12,062	24,377	74,663	6,453

Notes: This table reports the effects of lottery selection on assistance receipt. The sample includes all program applicants from Harris County (column 1), King County (column 2), Los Angeles (column 3), and Chicago (column 4). Estimated coefficients come from a regression that includes site-specific design controls. See Section 4. The control mean reports the share who received assistance among individuals not selected by the lottery. Robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

TABLE III
Survey Outcomes

	Harris County (1)	King County (2)	Los Angeles (3)	Chicago: DOH (4)	Chicago: TRP (5)	(All) Combined (6)	(No LA) Combined (7)
In the Last Month:							
Expenditures							
All Rent Paid	0.054	0.131**	−0.147	0.052*	0.055**	0.029	0.073***
	(0.042)	(0.053)	(0.122)	(0.031)	(0.028)	(0.029)	(0.020)
	[0.416]	[0.368]	[0.385]	[0.649]	[0.678]	[0.499]	[0.528]
All Bills Paid	−0.015	0.018	0.242*	−0.033	0.016	0.046	−0.004
	(0.037)	(0.043)	(0.134)	(0.033)	(0.033)	(0.031)	(0.018)
	[0.284]	[0.203]	[0.361]	[0.358]	[0.496]	[0.340]	[0.335]
Health							
COVID Positive (self-tested)	−0.104	−0.056	−0.026	−0.049***	0.003	−0.046	−0.052
	(0.202)	(0.073)	(0.125)	(0.017)	(0.018)	(0.050)	(0.054)
	[0.182]	[0.074]	[0.263]	[0.101]	[0.095]	[0.143]	[0.113]
Feeling Anxious	−0.083**	0.004	0.044	−0.080**	−0.001	−0.023	−0.040**
	(0.042)	(0.051)	(0.135)	(0.033)	(0.028)	(0.031)	(0.020)
	[0.525]	[0.633]	[0.543]	[0.549]	[0.319]	[0.514]	[0.506]
Economic Insecurity							
Worried about Eviction	−0.076*	−0.052	0.052	−0.018	−0.022	−0.023	−0.042**
	(0.041)	(0.053)	(0.128)	(0.030)	(0.020)	(0.030)	(0.019)
	[0.320]	[0.443]	[0.343]	[0.287]	[0.121]	[0.303]	[0.293]
Experienced Homelessness	−0.043	−0.009	−0.123	−0.020	−0.006	−0.040**	−0.020
	(0.036)	(0.037)	(0.076)	(0.014)	(0.018)	(0.019)	(0.014)
	[0.241]	[0.146]	[0.086]	[0.063]	[0.079]	[0.123]	[0.132]
Stayed in a Homeless Shelter	−0.020**	−0.001	−0.002	0.009	0.000	−0.003	−0.003
	(0.009)	(0.013)	(0.016)	(0.007)	(0.003)	(0.005)	(0.004)
	[0.015]	[0.014]	[0.003]	[0.005]	[0.001]	[0.008]	[0.009]
Was Food Insecure	−0.066*	0.026	−0.054	−0.026	−0.010	−0.026	−0.019
	(0.040)	(0.049)	(0.127)	(0.029)	(0.026)	(0.029)	(0.019)
	[0.335]	[0.325]	[0.291]	[0.270]	[0.256]	[0.296]	[0.297]
N	1,410	2,621	3,360	14,681	1,352	23,424	20,064

Notes: This table reports the effects of assistance on expenditures, health, and economic insecurity. Outcomes are derived from surveys described in section 3. Survey timing and treatment dates appear in Appendix Table A.1. See Appendix A for details on the survey questions. Columns (1)–(5) report the IV estimates of the effects of assistance on the measure listed in the row, separately by site, re-weighted to adjust for survey non-response as described in Section 3 Columns (6) and (7) report the combined averages of all sites and all sites excluding LA, respectively. Robust standard errors are reported in parentheses and the control group mean is reported in brackets. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

TABLE IV
Impacts on Credit Outcomes

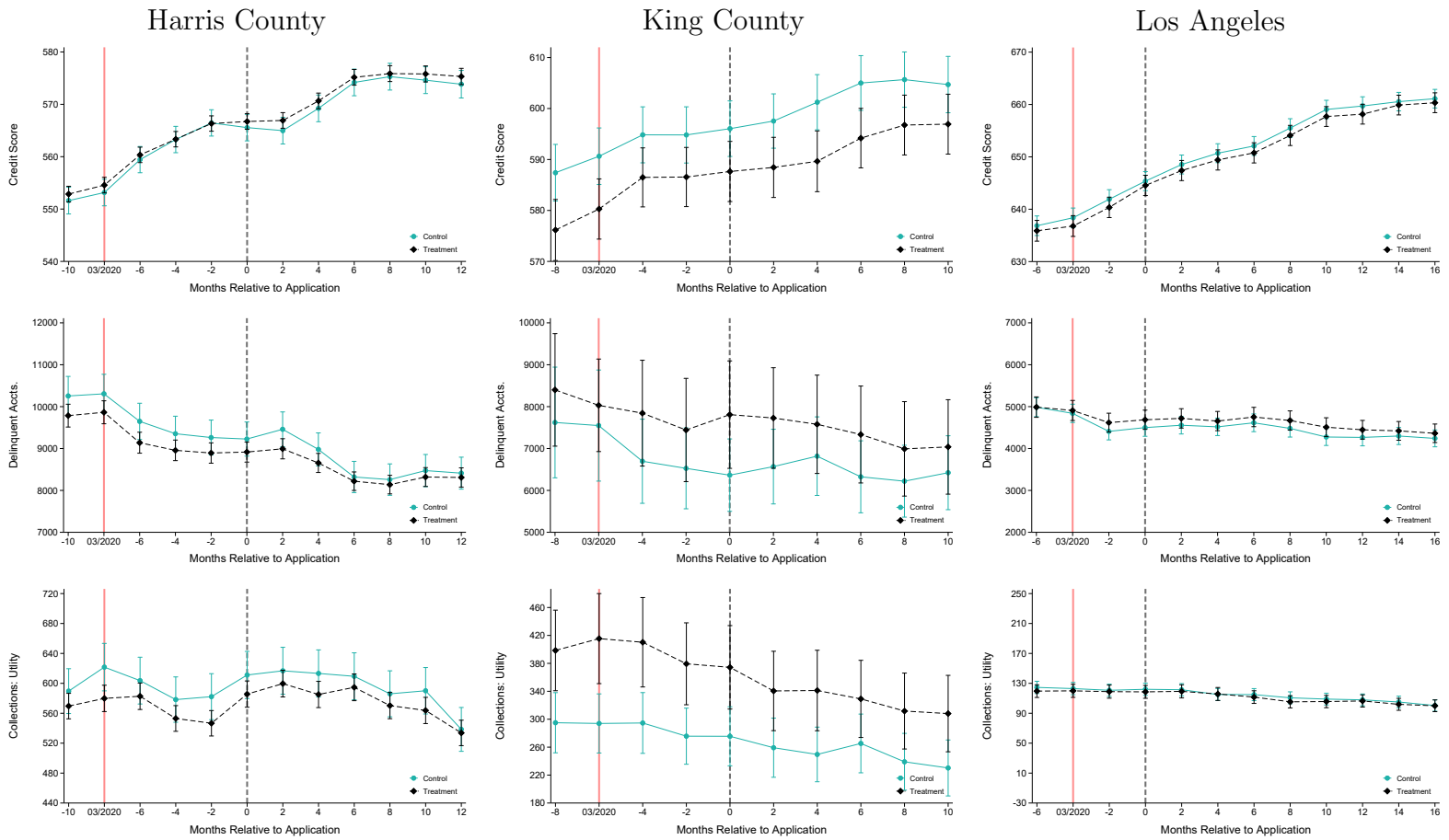
	Harris County (1)	King County (2)	Los Angeles (3)	Chicago (4)	Combined (5)
Short Run: 2 months after lottery					
Credit Score	2.863**	−3.413	0.854	–	0.101
	(1.235)	(4.511)	(2.480)		(1.765)
	[565]	[598]	[649]		[604]
Balance Across All Trades ($1000s)	0.259	0.113	0.929	–	0.434
	(0.500)	(2.392)	(1.086)		(0.891)
	[24.825]	[22.230]	[19.628]		[22.228]
Balance in Collections ($1000s)	0.003	−0.233	−0.157**	–	−0.129**
	(0.058)	(0.161)	(0.070)		(0.062)
	[3.258]	[1.439]	[0.965]		[1.888]
Balance in Utility Collections ($1000s)	0.013	−0.024	−0.000	–	−0.004
	(0.019)	(0.048)	(0.015)		(0.018)
	[0.617]	[0.259]	[0.121]		[0.332]
Balance in Delinquent Accounts ($1000s)	−0.230	0.639	0.132	–	0.180
	(0.218)	(0.850)	(0.374)		(0.318)
	[9.459]	[6.568]	[4.556]		[6.861]
Any Auto Loan or Lease	−0.004	−0.013	0.007	–	−0.003
	(0.007)	(0.024)	(0.015)		(0.010)
	[0.300]	[0.326]	[0.277]		[0.301]
Any Personal Bankruptcy	0.001	−0.003	−0.007*	–	−0.003
	(0.001)	(0.006)	(0.004)		(0.002)
	[0.019]	[0.069]	[0.047]		[0.045]
Any Open Revolving Line of Credit	0.009	−0.023	0.007	–	−0.003
	(0.007)	(0.028)	(0.012)		(0.010)
	[0.372]	[0.562]	[0.667]		[0.534]
Long Run: 10 months after lottery					
Credit Score	1.142	−2.633	−0.932	–	−0.808
	(1.604)	(6.344)	(3.279)		(2.440)
	[575]	[605]	[659]		[613]
Balance Across All Trades ($1000s)	−0.240	−3.166	2.237	–	−0.390
	(0.825)	(4.300)	(2.054)		(1.612)
	[26.756]	[24.927]	[20.659]		[24.114]
Balance in Collections ($1000s)	0.009	0.011	0.013	–	0.011
	(0.084)	(0.250)	(0.110)		(0.095)
	[3.085]	[1.453]	[0.969]		[1.836]
Balance in Utility Collections ($1000s)	−0.005	−0.016	−0.009	–	−0.010
	(0.022)	(0.059)	(0.020)		(0.022)
	[0.590]	[0.230]	[0.109]		[0.310]
Balance in Delinquent Accounts ($1000s)	0.123	−0.687	0.574	–	0.003
	(0.261)	(1.019)	(0.482)		(0.386)
	[8.473]	[6.422]	[4.275]		[6.390]
Any Auto Loan or Lease	0.002	−0.027	−0.013	–	−0.013
	(0.010)	(0.039)	(0.022)		(0.015)
	[0.314]	[0.333]	[0.285]		[0.311]
Any Personal Bankruptcy	−0.001	0.010	−0.014*	–	−0.002
	(0.002)	(0.013)	(0.008)		(0.005)
	[0.019]	[0.063]	[0.046]		[0.042]
Any Open Revolving Line of Credit	−0.018	−0.019	0.021	–	−0.002
	(0.011)	(0.042)	(0.019)		(0.016)
	[0.435]	[0.605]	[0.676]		[0.572]
N	14,159	1,889	18,559	–	34,607

Notes: This table reports the effects of assistance on short run (2 months) and longer run (10 months) credit outcomes. Columns (1)–(4) report the IV estimates of the the effects of assistance on the measure listed in the row, separately by site. Column (5) reports the combined results of all of the locations. Sample is restricted to lottery applicants that were linked to Experian credit data. All monetary values are expressed in thousands of 2020 U.S. dollars. Robust standard errors are reported in parentheses and the control group mean is reported in brackets. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

TABLE V
Moves

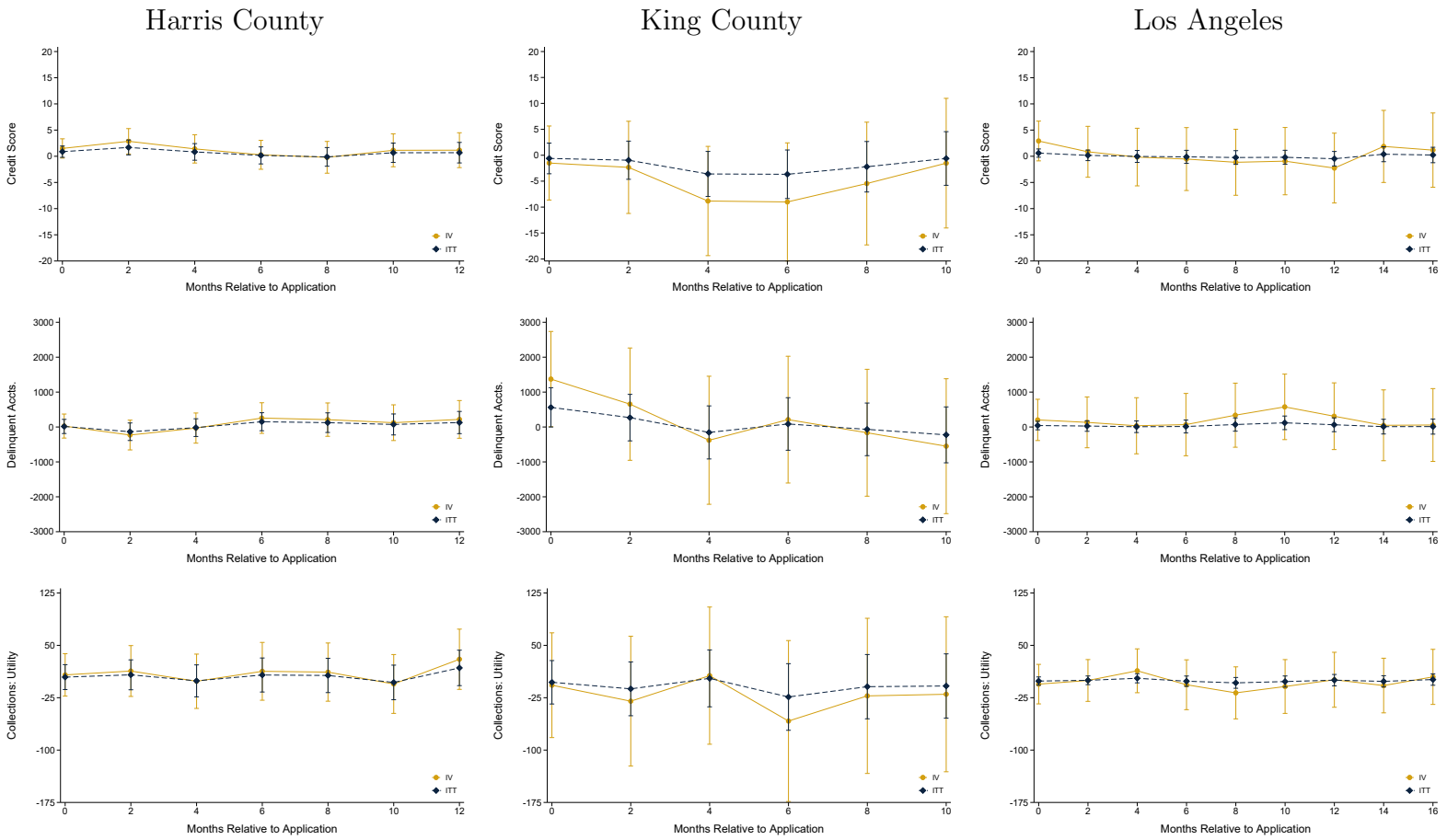
	Harris County (1)	King County (2)	Los Angeles (3)	Chicago DOH (4)	Chicago TRP (5)	Combined (6)
Short Run: 2 months after lottery						
Change of Address(Infutor)	0.002	−0.002	0.002	0.001	−0.002	0.000
	(0.006)	(0.016)	(0.009)	(0.009)	(0.003)	(0.004)
	[0.011]	[0.057]	[0.012]	[0.009]	[0.004]	[0.019]
Change of Address(Experian)	−0.007	0.040	−0.008	–	–	0.008
	(0.010)	(0.036)	(0.017)			(0.014)
	[0.102]	[0.105]	[0.066]			[0.091]
Long Run: 10 months after lottery						
Change of Address(Infutor)	−0.012	0.012	−0.016	−0.022	−0.018***	−0.011
	(0.010)	(0.029)	(0.022)	(0.018)	(0.007)	(0.008)
	[0.044]	[0.209]	[0.069]	[0.049]	[0.025]	[0.079]
Change of Address(Experian)	−0.003	0.046	−0.007	–	–	0.012
	(0.014)	(0.053)	(0.027)			(0.020)
	[0.250]	[0.303]	[0.187]			[0.247]
N (Infutor)	6,562	5,570	11,864	26,373	1,616	51,985
N (Experian)	14,159	1,889	18,559	-	-	34,607

Notes: This table reports the effects of assistance on moves in the short run (2 months) and long run (10 months). Change of address is measured in two ways: a direct measure of change of address via Infutor, and a change in zip code in the Experian linked data. The sample consists of the application sample linked to the respective outcome data source, and varies by outcome. Columns (1)–(4) report the IV estimates of the the effects of assistance on the measure listed in the row, separately by site. Column (5) reports the combined results of all of the locations. Robust standard errors are reported in parentheses and the control group mean is reported in brackets. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.



Notes: This figure plots the average credit characteristic for treatment (black diamonds) and control groups (teal circles) against months relative to the application. Applications were submitted at different times across sites and month 0 represents the site's application month. Each column is a different location- from left: Harris County; King County; Los Angeles. Each row is a different credit characteristic- from top: credit score; balance in delinquent accounts; balance in utility collections. The dashed vertical line indicates the month of application and the red vertical line indicates March 2020. The sample includes applicants that were linked to Experian credit data. Vertical bars plot the 95 percent confidence intervals of the estimates.

FIGURE I
Credit Measure by Month Relative to Application



Notes: This figure plots the IV and ITT estimates for different credit characteristics against months relative to the application. Applications were submitted at different times across sites. Each column is a different location- from left: Harris County; King County; Los Angeles. Each row is a different credit characteristic- from top: credit score; balance in delinquent accounts; balance in utility collections. The sample includes applicants that were linked to Experian credit data. Vertical bars plot the 95 percent confidence intervals of the estimates.

FIGURE III
ITT & IV Estimates by Month Relative to Application

A Construction of survey outcomes

Table III reports the results for a series of survey outcomes measured across sites. While surveys were similar across sites, they were not identical, and the exact definitions of the outcomes vary somewhat across locations. This appendix describes how each of the survey outcomes studied in this paper is constructed.

All Rent Paid

- For King County, this outcome is based on Q30 and Q31 and is the amount of rent paid in January 2021 divided by the amount of rent owed in January 2021
- For Harris County, this is based on Q29, which is a sliding percentage bar where the respondent reports what proportion of rent was paid in January 2021.
- For Los Angeles, this outcome is based on Q9, which asks "Are you behind on rent?"
- For Chicago DOH and Chicago TRP, it is based on a question on if the individual paid the full amount of rent due (on time or late). For DOH, the reference month is May 2020 and is based on Q147. For TRP, the reference month is March 2021 and is based on Q229

All Bills Paid

- In King and Harris Counties, this is based on Q68, which asks separately if the respondent paid all of their gas, electric, phone, tv/cable, car payment, car insurance, health insurance, and student loan bills in the past month. An index is then created scoring "yes" as 1, "some" as 0.5, and "none" as 0. Bills answered as "did not have bill" are omitted from the index.
- Los Angeles uses Q15, which asks about changes in a number of specific payment and consumption outcomes. If the respondent reports having delayed bill payments since May 2020 to make life more affordable, then they are scored as 0, and otherwise scored as 1.
- Chicago DOH and TRP use Q67 and Q68, which as "In the past month, did you or your household pay all of your utility bills such as internet, phone, gas, electricity?" and "In the past month, did you or your household pay all of your other bills such as care payments, car insurance, health insurance, or student loans?". Where we code the outcome as 1 if the answer is "Yes" to both questions.

COVID Positive (self-tested) For this outcome, we use the same set of two questions across sites which ask "have you suspected that you were infected with COVID-19" and "have you tested positive for COVID-19". The exact timing of the question varies a bit across site. For King and Harris Counties, this question begins with "Since March 1st," while in Los Angeles it is "Since May," and in the Chicago TRP survey it is "Since March 13th. The Chicago DOH survey did not ask these questions and instead codes this outcome as one if the respondent selects "I got sick" to the question "How has the COVID-19 epidemic impacted you?"

Feeling Anxious or Depressed Across all sites the question "how often have you felt nervous, anxious, or on edge?" from the PHQ4 is used, and is scored as one if the respondent reports "more than half the days" or "nearly every day." For Harris County, King County, and Chicago, these are asked about the last week, while in Los Angeles, the question asks about the last two weeks.

Worried about eviction In King and Harris Counties, we use the question "How worried are you about being evicted or foreclosed on in the next three months?" We code the outcome as 1 if the respondent reports "very worried" and zero otherwise. All other sites use the same question, but asked about the next two rather than three months.

Experience homelessness Across all sites, this is based on a question that asks if the respondent has spent any nights couch surfing, in a homeless shelter, on the street, in an abandoned building, in a car or van, or in a hotel or motel (for non-travel reasons). The outcome is coded as yes if they answer yes to any of the the above options. For King County, Harris County, Chicago DOH, and Chicago TRP, this question is asked about "the last month". For Los Angeles, the question is phrased as "Since May, ...".

Stayed in Shelter This outcome is constructed the same was as the "Experience homelessness" outcome, but is only coded as 1 if they respond yes to the "in a homeless shelter" option. Similar to the "Experience homelessness" outcome, for Los Angeles this is asked as "since May", while all other sites ask about the last month.

Was Food Insecure The question "For the past month, which of these statements best describes the food eaten in your household?" was asked across all locations and coded as of the responded selected "sometimes not enough to eat" or "Often not enough to eat."

B Additional tables and figures

Appendix Table A.1
Treatment and survey timing

Site	Treatment dates	Survey dates
King County	09/2020-12/2020	02/2021-03/2021
Harris County	11/2020-01/2021	05/2021-06/2021
Los Angeles	09/2020-12/2020	01/2021-02/2021
Chicago (DOH)	04/2020-05/2020	05/2020-06/2020
Chicago (TRP)	08/2020-10/2020*	02/2021-04/2021

Notes: This table documents the program and survey timing for each site. In Harris County, we study the second round of assistance that was offered in November 2020 rather than the earlier round that was distributed in July 2020. In Chicago, we study the first round of the Department of Housing (DOH) program, which provided grants between April and May 2020, and the second round of The Resurrection Project (TRP) grants program, which opened to applications in late June 2020.

Appendix Table A.2
Survey Outcomes; No weights

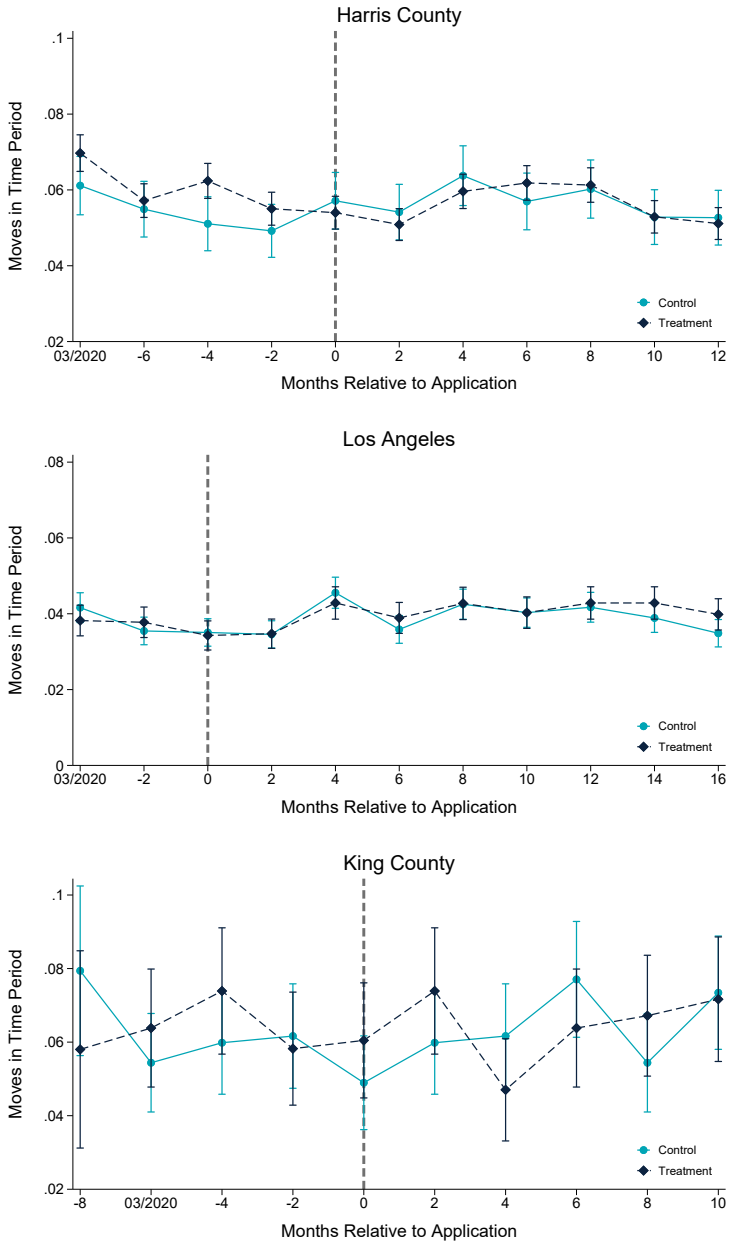
	Harris County (1)	King County (2)	Los Angeles (3)	Chicago: DOH (4)	Chicago: TRP (5)	(All) Combined (6)	(No LA) Combined (7)
In the Last Month:							
Expenditures							
All Rent Paid	0.045	0.125**	−0.120	0.052*	0.047*	0.030	0.067***
	(0.038)	(0.052)	(0.120)	(0.030)	(0.027)	(0.028)	(0.019)
All Bills Paid	−0.008	0.023	0.222*	−0.031	0.018	0.045	0.001
	(0.034)	(0.042)	(0.130)	(0.032)	(0.032)	(0.030)	(0.018)
Health							
COVID Positive (self-tested)	−0.096	−0.066	0.039	−0.049***	−0.001	−0.035	−0.053
	(0.181)	(0.063)	(0.122)	(0.016)	(0.017)	(0.046)	(0.048)
Feeling Anxious	−0.076**	0.007	0.056	−0.077**	0.005	−0.017	−0.035*
	(0.039)	(0.049)	(0.132)	(0.032)	(0.027)	(0.030)	(0.019)
Economic Insecurity							
Worried about Eviction	−0.070*	−0.061	0.046	−0.019	−0.014	−0.023	−0.041**
	(0.038)	(0.052)	(0.123)	(0.029)	(0.020)	(0.029)	(0.018)
Experienced Homelessness	−0.042	−0.006	−0.105	−0.021	−0.004	−0.036**	−0.018
	(0.032)	(0.036)	(0.073)	(0.013)	(0.016)	(0.018)	(0.013)
Stayed in a Homeless Shelter	−0.015**	−0.004	−0.001	0.009	0.001	−0.002	−0.002
	(0.007)	(0.012)	(0.014)	(0.007)	(0.002)	(0.004)	(0.004)
Was Food Insecure	−0.063*	0.022	−0.101	−0.027	−0.008	−0.035	−0.019
	(0.036)	(0.048)	(0.119)	(0.027)	(0.025)	(0.028)	(0.018)
N	1,410	2,626	3,360	14,681	1,352	23,429	20,069

Notes: This table reports the effects of assistance on expenditures, health, and economic insecurity. Outcomes were measured using surveys and ask about the month prior to the month the survey was conducted. Surveys were conducted in May for Harris County, February for King County, July -is this right? for Los Angles, XX for Chicago DOH, and XX for Chicago TRP. Column (6) reports the combined averages of all sites and Column (7) reports the combined averages of all sites minus LA. Standard errors are reported in (). Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table A.3
Balance Table - Survey Response

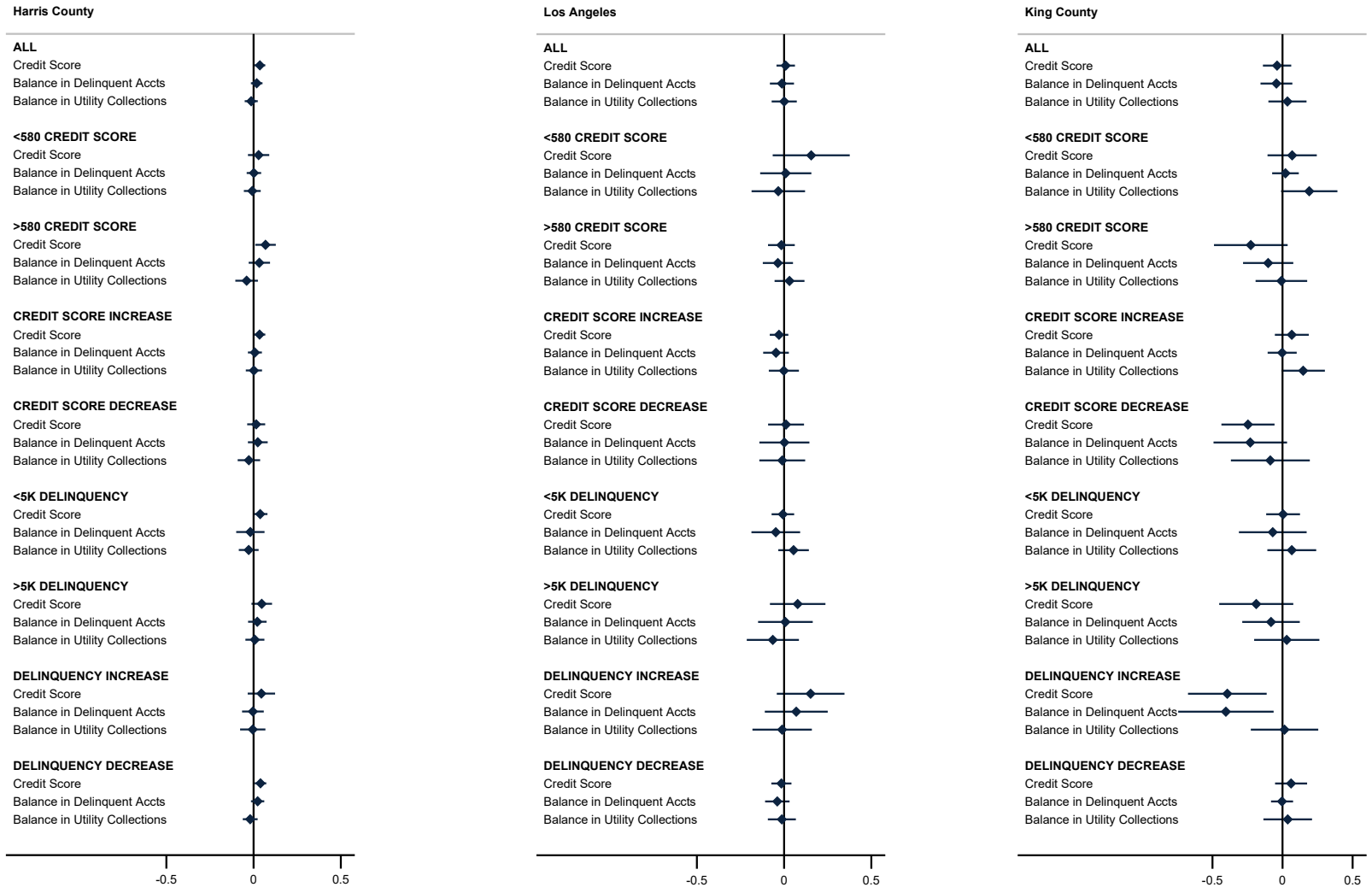
	Harris County			King County			Los Angeles			Chicago DOH			Chicago TRP		
	Control	Treat	Diff	Control	Treat	Diff	Control	Treat	Diff	Control	Treat	Diff	Control	Treat	Diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Demographics															
Age	–	–	–	37.175	37.705	0.530	42.070	41.082	0.988**	38.731	39.028	0.297	39.854	39.122	−0.732
Household Size	.	.	.	–	–	–	2.605	2.654	−0.049	2.980	2.802	−0.178**	4.043	4.004	−0.039
White	0.071	0.073	−0.008	0.297	0.286	−0.011	0.240	0.237	0.003	0.131	0.132	0.001	0.036	0.022	−0.013
Black	0.616	0.588	0.012	0.268	0.313	0.045**	0.127	0.101	0.026**	0.383	0.333	−0.050**	0.097	0.089	−0.008
Hispanic	0.239	0.272	0.004	0.157	0.151	−0.005	0.519	0.543	−0.024	0.412	0.450	0.038*	0.843	0.857	0.015
Asian	0.014	0.030	0.016**	0.078	0.072	−0.006	0.095	0.089	0.006	0.039	0.060	0.021**	0.025	0.032	0.007
N†	804	536	1,410	1,456	1,170	2,626	1,680	1,680	3,360	14,114	567	14,681	813	539	1,352

Notes: This table reports the average of demographic characteristics for applicants not selected by the lottery (Control) and applicants selected by the lottery (Treat), separately by site. Conditional differences in average characteristics (Diff) between these two groups come from regressions that control for site-specific design controls. See Section 4. The sample includes only applicants who completed the survey. Harris County did not ask about age or household size and King County did not ask about household size. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.



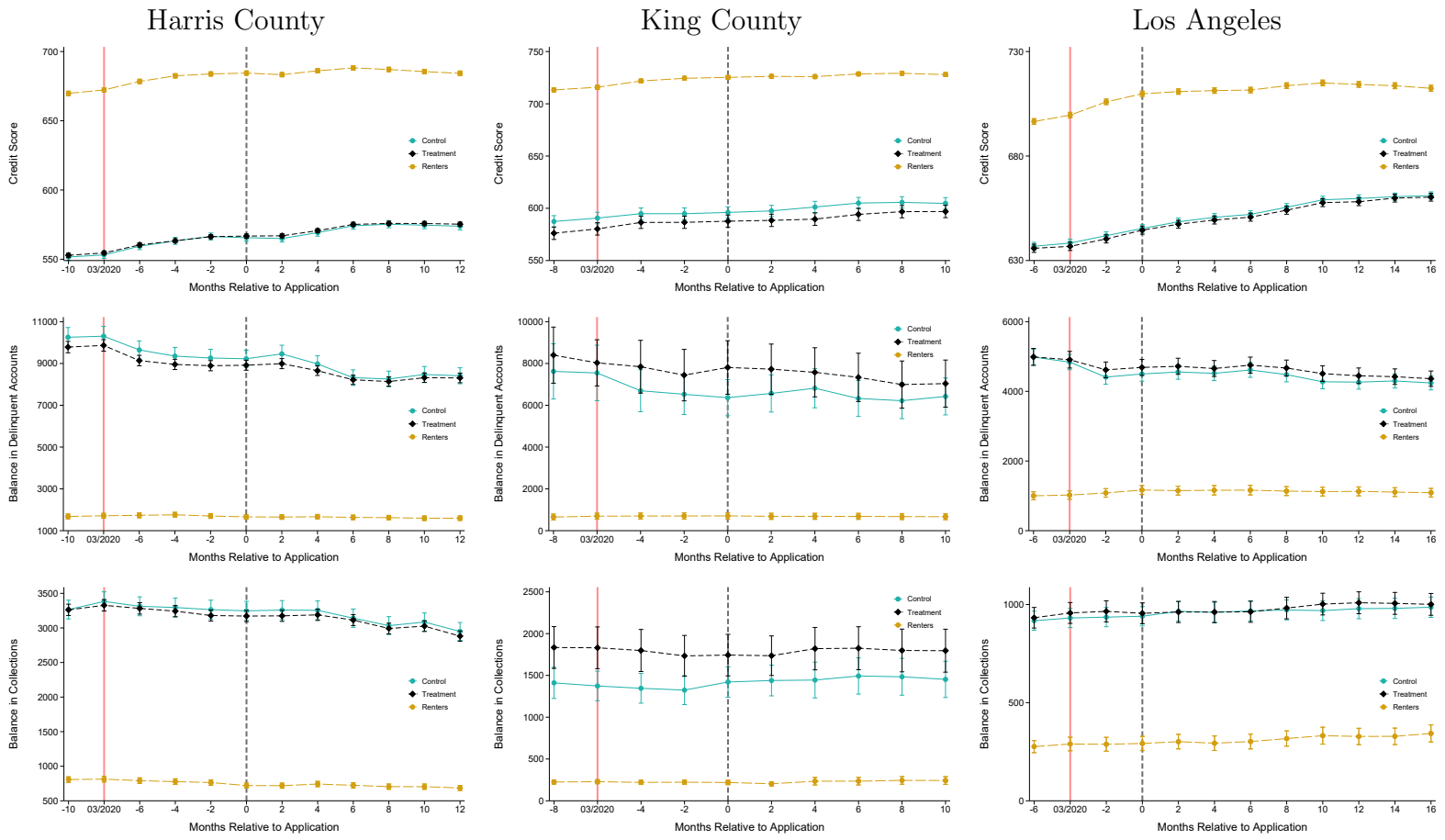
Notes: This figure plots the average number of moves for treatment and control groups against months relative to application. Moves are derived from address changes observed in the Experian data. The dashed vertical line indicates the month of application (month 0). Applications were submitted at different times across sites. March 2020 is marked on the horizontal axis.

Appendix Figure A.1
Moves in Time Period



Notes: This figure plots the IV estimates of the effects on credit outcomes for different subgroups. The subgroups are in bold and followed by the three credit outcomes: Credit Score; Balance in Delinquent Accounts; Balance in Utility collections. Point estimates are standrized by dividing the IV estimate by the standard deviation of the outcome in the control group. The left most forest plot is for Harris County followed by Los Angeles and King County. The sample includes applicants who were linked to Experian credit data.

Appendix Figure A.3
Forest Plots



Notes: This figure plots the average credit characteristics for the treatment and control groups and for renters against months relative to the application. Data on local renters come from a 10% sample of individuals from Equifax and includes individuals outside of the applicant pool. Control and treatment samples are inclusive only of applicants and applicants who were linked to Experian credit data. Applications were submitted at different times across sites. Each column is a different location- from left: Harris Count; King County; Los Angeles. Each row is a different credit characteristic- from top: credit score; balance in delinquent accounts; balance in utility collections. The dashed vertical line indicates the month of application and the red vertical line indicates March 2020.

Appendix Figure A.5
Credit Outcomes by Month Relative to Application, Program Applicants vs Local Renter Population

Appendix Table A.4
Average Credit Characteristics, Program Applicants vs Local Population

	Harris County			King County			Los Angeles			Chicago		
	All	Renters	Assisted	All	Renters	Assisted	All	Renters	Assisted	All	Renters	Assisted
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Credit Characteristics:												
Credit Score	707	684	566	748	725	584	727	710	649	712	691	–
	(94)	(94)	(78)	(75)	(80)	(85)	(84)	(85)	(1)	(95)	(97)	–
Balance Across All Trades ($1000s)	68.8	19.1	27.9	118.8	15.4	21.4	103.2	16.7	22.1	71.9	19.5	–
	(155.2)	(30.6)	(47.4)	(263.1)	(32.2)	(60.8)	(345.8)	(37.6)	(0.7)	(161.3)	(36.4)	–
Balance in Collections ($1000s)	0.591	0.721	3.320	0.168	0.219	1.651	0.229	0.292	0.918	0.281	0.327	–
	(2.637)	(2.285)	(4.299)	(1.220)	(1.166)	(3.451)	(2.033)	(2.356)	(0.038)	(1.794)	(1.575)	–
Balance In Delinquent Accounts ($1000s)	1.514	1.661	9.426	0.549	0.709	7.428	1.025	1.167	4.671	1.126	1.126	–
	(6.898)	(6.966)	(12.816)	(5.776)	(6.549)	(20.045)	(8.040)	(8.407)	(0.168)	(8.397)	(7.045)	–
Any Auto Loan or Lease	0.398	0.355	0.326	0.282	0.261	0.349	0.346	0.322	0.279	0.274	0.250	–
	(0.490)	(0.479)	(0.459)	(0.450)	(0.439)	(0.477)	(0.476)	(0.467)	(0.007)	(0.446)	(0.433)	–
Any Personal Banckruptcy	0.012	0.012	0.019	0.019	0.026	0.100	0.026	0.031	0.046	0.050	0.061	–
	(0.108)	(0.108)	(0.137)	(0.135)	(0.158)	(0.300)	(0.161)	(0.173)	(0.003)	(0.217)	(0.239)	–
Any Open Revolving Line of Credit	0.847	0.807	0.384	0.929	0.906	0.505	0.887	0.870	0.703	0.874	0.848	–
	(0.360)	(0.395)	(0.469)	(0.256)	(0.291)	(0.500)	(0.316)	(0.336)	(0.007)	(0.332)	(0.359)	–
Observations:	26,086	14,803	6,169	14,408	7,358	491	24,579	16,603	8,702	14,399	9,200	–

Notes: This table reports the average credit characteristics for program applicants and a sample of local residents from outside the applicant pool for each site. Data on local residents come from a 10% sample of individuals from Equifax. 'All' includes both renters and homeowners and 'renters' is limited to individuals who rent. 'Applicants' includes both treatment and control group individuals who were linked to Experian credit data at the time = 0. All monetary values are expressed in 2020 U.S. dollars divided by 1000. Standard deviations are reported in parentheses.

Appendix Table A.5
Treatment Effects on Evictions (Harris County)

	Control Mean	Treatment Effect
2 months after lottery		
Eviction Filing	0.0032	-0.0007
	(0.0567)	(0.00183)
Eviction Judgment	0.0014	-0.0012
	(0.0376)	(0.00118)
10 months after lottery		
Eviction Filing	0.0121	-0.0021
	(0.1094)	(0.00327)
Eviction Judgment	0.0038	-0.0018
	(0.0618)	(0.00197)
N	4,956	18,776

Notes: This table reports the effects of assistance receipt on eviction filings and judgments. Outcomes were measured 2 and 10 months after application using administrative from the Harris Count Justice of Peace. Columns (1) reports the control mean and standard deviation (in parenthesis) and (2) report the IV estimates of the effects of assistance on the measure listed in the row. Robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table A.6
Treatment Effects on Homelessness System Use (King County)

	King County Full Sample	King County Survey Respondents
Any Homeless Services	0.020**	0.027*
	(0.0083)	(0.016)
	[0.027]	[0.027]
	{0.048}	{0.047}
Any Emergency Shelter	0.0028	-0.000065
	(0.0039)	(0.0077)
	[0.0064]	[0.0081]
	{0.018}	{0.022}
Any Street Outreach	0.0067*	0.015**
	(0.0036)	(0.0068)
	[0.0037]	[0.0023]
	{0.0091}	{0.0098}
Any Coordinated Entry	0.00100	0.0013
	(0.0024)	(0.0038)
	[0.0024]	[0.0023]
	{0.0078}	{0.0070}
Any Diversion	0.0081*	0.0020
	(0.0045)	(0.0095)
	[0.0070]	[0.010]
	{0.019}	{0.017}
Any Longer Term Subsidies	0.0012	0.010
	(0.0059)	(0.011)
	[0.015]	[0.013]
	{0.021}	{0.019}
N	12148	3152

Notes: This table reports the effects of assistance receipt on homelessness services receipt among King County program applicants. Outcomes were measured 9 months after application using administrative homelessness management information system (HMIS) data. Columns (1) and (2) report the IV estimates of the effects of assistance on the measure listed in the row for the full King County Sample and the sample of King County survey respondents, respectively. Robust standard errors are reported in parentheses and the control group means are reported in brackets. Pre-COVID means for March 2019 to February 2020 are reported in braces. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table A.7
Additional Survey Characteristics

	Harris County (1)	King County (2)	Los Angeles (3)	Chicago: DOH (4)	Chicago: TRP (5)
Financial Characteristics:					
Monthly Rent	1.021	1.472	1.571	1.122	1.026
Months of Rent Owed	1.090	3.259	1.745	–	0.839
Monthly Income (Feb 2020)	1.498	1.722	–	1.374	1.150
Any Paid Work (Feb 2020)	0.891	0.837	–	0.762	0.759
Demographics:					
Age	–	37.287	41.576	38.409	39.289
Female	0.783	0.620	0.564	0.637	0.613
Household Size	3.520	2.867	2.629	3.314	3.988
White	0.083	0.343	0.239	0.120	0.021
Black	0.613	0.364	0.114	0.373	0.117
Hispanic	0.271	0.186	0.531	0.430	0.807
N	1422	2621	24374	14681	1352

Notes: This table reports the mean of each row characteristic for all who filled out the survey in each site (includes treatment and control). We report re-weighted means to adjust for survey non-response. All monetary values (Rent and Income (Feb 2020) variables) are divided by 1,000. We report the maximum sample size for each site. Monthly income is household income in Feburary of 2020. Note that for King County the variable "months of rent owed" comes from administrative payment data, not the survey. (Consider whether to include a note about this on the final table, or whether to leave this blank).

Appendix Table A.8
Fraction of Participants Receiving Assistance in Future Rounds

	King County		Chicago: DOH	
	Control (1)	Treatment (2)	Control (3)	Treatment (4)
Received Assistance in Future Round	0.28	0.31	0.07	0.13
N	6,982	5,166	73,126	1,537

Notes: This table reports the fraction of lottery participants who received assistance in a future round. The definition of the treatment and control group is from round one assignments. For Chicago DOH, these numbers include those who received assistance from round 2 of DOH or from TRP.