The Effects of Emergency Rental Assistance During the Pandemic: Evidence from Four Cities*

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Abstract

The COVID-19 pandemic saw an unprecedented expansion of federal emergency rental assistance (ERA). Using applications to ERA lotteries in four cities linked to survey and administrative data, we assess ERA's impacts on housing stability, financial security, and mental health. We find that assistance increased rent payment modestly and improved mental health. However, in contrast to pre-pandemic studies of similar assistance programs, we find limited effects on financial or housing stability. Several pieces of suggestive evidence indicate this discrepancy is likely due to macroeconomic conditions, including expanded government support and rental market slackness, rather than ERA generosity or targeting.

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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, an estimated 5.4 million Americans reported that they were likely to face eviction or foreclosure within two months.[1] While policymakers were able to draw on lessons from the Great Recession in designing policy to forestall foreclosures and stabilize the owner-occupied segment of the market (e.g., 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. Nonetheless, concerned about the economic and public health consequences of individuals and families losing their homes during the pandemic, policymakers devoted unprecedented resources to stabilizing the housing situation of vulnerable renters. The federal government allocated over $50 billion in emergency assistance to renters. This assistance represented a doubling in the typical amount of federal rental assistance.[2]

Despite the unprecedented scale, there is little, if any, causal evidence on the effectiveness of these programs in boosting rent payments, preventing evictions or homelessness, or keeping families in their homes. The lack of evidence on these policies stems from several 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 the absence of comprehensive national administrative data. Third, descriptive comparisons of assistance recipients and unassisted individuals are likely to suffer from selection bias given the way most funds were allocated.[3]

In this paper, we provide the first empirical evidence on both the causal effects of the pandemic-era emergency rental assistance and the targeting of these programs. We study five emergency rental assistance programs providing $1,000 to $3,400 per household from May to December 2020 and targeting renters across four major urban areas: Chicago, Harris County (Houston), King County (Seattle), and Los Angeles.[4] Collectively, these programs received more than 200,000 applications for assistance and were broadly representative of early rounds of emergency rental assistance. Subsequent rounds, especially the Treasury-

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

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

[3] Recent studies have connected the COVID-19 emergency rental assistance to improvements in housing outcomes, financial well-being, and mental health, but the methodologies used in these studies require strong assumptions to support a causal interpretation (Airgood-Obrycki, 2022; Reina and Lee, 2023).

[4] Most of the programs we study offered flat assistance amounts that were not based on individual rent levels or arrears. An exception was the King County program which typically offered 3 months of assistance at up to 80% of tenant's rent.

funded Emergency Rental Assistance Program (ERAP), took place during the tail end of the pandemic and provided more generous assistance, although a large share of these payments were used to pay off rental arrears. The programs we study are therefore more comparable to pre-pandemic temporary rental assistance, with the main difference being that they were implemented around the peak of the pandemic.

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, 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 outcomes not easily measured through administrative sources and to provide suggestive evidence on possible mechanisms. For select sites, we also link the applications to eviction court filings and administrative data on homelessness system use to investigate the impacts on measures of extreme 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 for communities that 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 3.4 percentage points (7 percent) across sites. We find, specifically, that the assistance reduced worries about being evicted by 4.6 percentage points (15 percent).

Despite the policy objective of keeping individuals and families in their homes, we find no consistent effects of the assistance on the likelihood that applicants moved. We analyze applications linked to two separate administrative sources to draw this conclusion. First, using our panel of linked credit data, we evaluate whether recipients moved to different neighborhoods and find that treated individuals were no less likely to change 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 provide 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 in either the two months or the year after applying for assistance. Overall, the move rates in our samples appear similar for the period leading up to the lottery and the post-lottery period.

Next, using linked credit reports, we examine whether the assistance affected short- and longer-run measures of financial distress. While the assistance had small short-run impacts on some credit measures, these effects are not consistently present across sites and did not persist over time. Averaging across sites, individuals assisted through the lottery had approximately $74 less in collections in the two months after applying for assistance. However, these effects appear to have faded over time. The financial characteristics of applicants randomized into receiving assistance are largely indistinguishable ten months after the lottery from those of applicants who missed out.

The lack of effects on financial stability may be a consequence of improving economic circumstances for both the treatment and control groups 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, including COVID relief payments, expansions of unemployment insurance and the Earned Income Tax Credit (EITC), the Child Tax Credit, and the Paycheck Protection Program.[5] The improvement in household balance sheets continues post-lottery but is similar for both treatment and control applicants, consistent with our findings of small impacts on the credit measures.

Our last set of causal estimates sheds light on whether receipt of assistance reduced the chances that applicants experienced more acute forms of housing instability, including eviction or homelessness. To measure the impacts on homelessness, we link the application data for three of our lotteries to homelessness system data from King County (Washington) and Chicago (Illinois). We do not find consistent evidence that receiving assistance reduced applicants' likelihood of appearing in the homelessness system.[6] We then investigate whether assistance reduced the probability of new eviction filing or judgments in Harris County (Texas), where new cases could be still initiated during the Centers for Disease Control (CDC) eviction moratorium. We find that the assistance had no significant effects on eviction activity and that baseline rates of new filings remained relatively low for applicants not assisted through the lottery.

[5]See, for example Blanchet et al. (2022), who observe that *"government programs enacted during the pandemic led to an [sic]unprecedented—but short-lived—improvements in living standards for the working class. After accounting for taxes and cash and quasi-cash transfers, disposable income for adults in the bottom 50% was 20% higher in 2021 than in 2019."*

[6]The only group for whom we find such evidence are applicants to a program in Chicago targeting individuals who might have limited access to other parts of the pandemic safety net.

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 financial stability. These conclusions stand in contrast to those of prior work on smaller-scale emergency rental assistance programs targeting idiosyncratic shocks faced by vulnerable renters (Phillips and Sullivan, 2023; Evans et al., 2016), which finds large relative effects of such assistance on housing stability.

We interpret these differences in effectiveness as informative about the role of emergency rental assistance as a tool to respond to macroeconomic, relative to idiosyncratic, shocks. During macroeconomic crises, two forces may mitigate the effectiveness or need for emergency rental assistance: the first is the expansion of alternative forms of assistance that is common during downturns, the second is that rental market slackness may lead landlords to work out private payment plans with tenants that are struggling to pay rent. Combining information from our surveys and data points from other sources, we find descriptive evidence consistent with both channels.[7]

While we find generally muted effects of the assistance, we nevertheless find that it was relatively well targeted to financially distressed households. The recipients of assistance across our four sites appear more financially distressed 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 33–100 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 of the assistance. Nonetheless, our results suggest that the programs that we study, which focused largely on low-income renters, were 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 percent compared to 9.5 percent) and faced

[7]The increase in receipt of social insurance and safety-net programs during the pandemic is well-documented (Blanchet et al., 2022; Han et al., 2020; Raphael and Schneider, 2023). In support of the second channel, we find very high rates of reported landlord agreements (greater than 70 percent) in our surveys among tenants that are having trouble paying their rent.

substantially higher risk of losing their home (7 percent compared to 2 percent).[8]

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., 2024) 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, 2023).[9] These papers focus on recipients experiencing individual- or household-level shocks outside of broader economic downturns. In contrast, we provide rigorous evidence on the causal effects of emergency rental 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 the emergency assistance distributed to renters during the pandemic. Chetty et al. (2023) evaluate a range of pandemic economic policies such as the Paycheck Production Program, Pandemic Unemployment Insurance, and State-level Stay-at-home orders on spending and economic outcomes using nearly real-time data on spending from private sector data sources. Ganong et al. (2022) examine the impacts of pandemic unemployment insurance on spending and job-finding. Karger and Rajan (2021) examine the impacts of the COVID-19 Economic Impact Payments on spending. 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) and on 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—the significant expansion of emergency assistance to renters—across four large urban areas.

Finally, our work relates to recent studies of the supplemental cash assistance provided during the COVID-19 pandemic. Jaroszewicz et al. (2022) compare the effects of one-time cash grants of $2,000 to those of one-time payments of $500 on bank account spending and survey measures of mental health and find the additional assistance led to small increases in spending but no improvements in mental health. Pilkauskas et al. (2022) examine the effects of one-time $1,000 payments to families receiving SNAP. They find limited effects

[8] Census Pulse 2020, week 13.

[9] Another branch of the 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).

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, in contrast to these studies, we find some evidence for improvements in mental health, our estimates of the impacts on bill payment and financial strain appear qualitatively similar. Our results reflect program outcomes in 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 further discusses our findings and 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. The programs we study were reasonably representative of other early emergency rental assistance programs during the pandemic, as we detail in Appendix Table C.9. We also discuss below the timing of any local eviction moratoria, which may have differed from the timing of 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 CDC order.

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 27 and closed on April 5. During this period, the program received a total of 75,659 applications, approximately 80 percent of which were from renters. Because of the large number of applicants, grants were given out through a lottery process. All applicants were assigned a random rank and screened for eligibility, and offers were made in rank order until the 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.[10]

The second program that we study was a similar program run by The Resurrection Project (TRP) in partnership with the Chicago Resiliency Fund. The program's first round of assistance was not oversubscribed. We study its second round, which opened for applications on June 22, 2020. Similar to the DOH program, the TRP program was open to homeowners and renters, with 75 percent of the applications coming from renters. In total, 10,300 individuals applied, and 1,718 grants of $1,000 were distributed via lottery.[11]

The TRP program specifically targeted those who were not eligible and did not receive aid from the CARES Act and who had income below 300 percent 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 because of the COVID-19 pandemic. This was the second round of assistance distributed by the county, which administered an earlier round in July 2020.

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

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

[11]In total, TRP distributed more than nine million dollars of assistance to over 9,000 people through the Chicago Resiliency Fund during the pandemic.

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 receipt of assistance depending on the applicant's census tract. Based on the CDC's Social Vulnerability Index, the census tracts were divided into quartiles, with the odds of selection depending on the quartile. Applicants in the top quartile had 50 percent greater odds of selection. The funds generally were dispersed in November and December 2020, with some payments going out in early 2021.

Applicants who 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.

At the beginning of the pandemic, Harris County tenants were protected by a state eviction moratorium covering March 19 to 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.[12] 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, approximately 60 percent of the pre-COVID volume (Hepburn et al., 2020).

2.3 King County

In late 2020, the government of King County, Washington, implemented an EFA program through its 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 tenants of small-scale landlords, who could request assistance directly. We study the latter program. Tenants requested assistance through an online interest form that was open from August 20, 2020, to December 4, 2020.

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

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

program, individuals had to verify income at or below 50 percent 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 not 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 that in the other sites. Those selected for treatment received an average of three months' back rent or credit toward future rent paid to the landlord. The maximum available support covered six months' 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 percent of contracted rent or HUD fair market rent, (ii) to forgive any rental debt owed by the tenant beyond three months' rent, (iii) to retain the tenant (except for cause), and (iv) to not raise rent through March 2022.

King County renters received protection through 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 their 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 prepandemic 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 approximately twice as many applications from renters as it anticipated being able to serve.

To allocate funding, the city entered applicants 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 the 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, the 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 the 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 applied only 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 bimonthly 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 and debt levels and delinquency status for all major forms of consumer debt.[13] In addition

[13] Under the CARES Act, creditors were required to make a variety of hardship "accommodations" for households directly impacted by COVID-19, including deferring or forbearing payments, or reporting accounts as current, but required households to actively request relief (Fiano, 2020). Aggregate statistics suggest that delinquency on household credit continued to be reported to the credit bureaus, with the exception of Federal students loans, which were reported as current while payments were paused (*Household Debit and Credit Report*, 2023)

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 the 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 those 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 before the latter were returned to the research team by Experian. Among people eligible to be matched, match rates across all sites are generally high, ranging from approximately 74 percent for Harris County, Texas, to 81 percent for King County, Washington. Conditional on being observed in the credit data, nearly all applicants are present in every monthly snapshot. Our analysis therefore focuses 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 these data were originally created for commercial applications such as identity verification and marketing, academics have recently used them to measure housing stability and migration in the wake of rent control, eviction, and new housing construction (Diamond et al., 2019; Collinson et al., 2024; Asquith et al., 2023). For the present study, the Infutor data prove useful because they provide a measure of housing stability that is consistent across several metropolitan areas. Because the data are compiled from consumer records, they match with other data at a somewhat lower rate than records from other administrative sources and tend to miss groups of people more inclined to have shorter paper trails (e.g., young or Hispanic individuals). Even so, prior work shows that the data 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. The 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 percent for Chicago DOH, 21 percent for Chicago TRP, 35 percent for Harris County, 46 percent for King County, and 50 percent for 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 makes use of follow-up surveys administered in the months following treatment for all five programs. Table C.1 reports the range of dates over which the funding was dispersed and follow-up surveys were administered for each site. The funds for King County, Harris County, and Los Angeles were distributed in the last quarter of 2020. The funds for Chicago DOH were distributed in the second quarter of 2020, while the funds for Chicago TRP were distributed largely in the third and fourth quarters 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 surveys four to five months after treatment. In some cases, retrospective questions were used to improve the alignment between the treatment and measured outcomes across sites given the elapsed time between lottery and survey completion.

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, the research teams shared the surveys among themselves and, to the extent possible, aligned them. 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 the relevant questions. First, for constructing the "All Rent Paid" outcome, the Los Angeles survey asks if tenants are behind on rent, while the survey questions for 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 multiresponse question "How has the COVID-19 epidemic impacted you?" The other sites' surveys specifically ask if the respondent suspected that she was 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 the surveys for all the other sites ask about exposure to these experiences in the past month. Appendix Section A provides a detailed discussion of how our survey-based outcomes are constructed and aligned across sites.

The bottom panel of Table I reports the response rates for the 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 C.3 shows that survey nonresponse leads to modest imbalances in baseline characteristics. As we describe in the next section, our main analysis corrects 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 and homeless program use data from King County and Chicago. These data allow us to track more extreme forms of housing instability that we cannot measure across all sites.

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

We measure homeless program use in King County and Chicago using local Homeless Management Information System (HMIS) data. These 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 and Chicago, 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 and September 2021, King County staff extracted records that fuzzy matched with lottery applicants based on name, date of birth, and ZIP code. We limit the data to types of programs offered only to people who are already homeless (e.g., not prevention programs) so that program use indicates that the individual both receives services and is homeless at the point of program entry.[14] The existing literature uses similar measures to study the effect of temporary

[14]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, transitional housing, permanent supportive housing, and

financial assistance on homelessness (Evans et al., 2016; Phillips and Sullivan, 2022, 2023).

The DOH and TRP applicant records were similarly matched on name and date of birth to Chicago HMIS program enrollments spanning January 2017 to December 2023 through a probabilistic linkage procedure. In the case of multiple viable matches to a single applicant, HMIS service utilization histories were reconciled to take the highest level of engagement on any given day. All outcomes for King County and Chiago were constructed according to HUD project types, with the exception of diversion services, which King County has uniquely represented in its HMIS.

4 Research Design

4.1 Empirical Specification

The goal of our empirical analysis is to estimate the causal effects of receiving emergency financial assistance on various mean outcomes of interest. However, simply comparing average outcomes across those who did and did not receive assistance may yield 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 the assistance may differ along dimensions 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 them. Alternatively, they might not take up the assistance because they have relatively less need for it because of 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 applied 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 score, and S is an indicator for whether the applicant was selected by lottery to receive an assistance offer. Because several jurisdictions administered their lotteries in a way that resulted in conditionally random offers that de-

rapid rehousing.

pended on observable applicant characteristics, X, we also directly control for these factors in the regression. In particular, we include week-of-application fixed effects for King County and census tract fixed effects for Harris County. For Chicago, we include application-count fixed effects to account for a small number of individuals applying more than once.[15]

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 applied for assistance and the programs randomly selected among applicants, this coefficient provides an unbiased estimate of the intent-to-treat (ITT) effect of an applicant's 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 application and for January 2020.[16]

While γ_1 provides an unbiased estimate of the effect of being offered assistance, not everyone initially offered funding through the lottery actually received it. There are several reasons this could have occurred, such as failure of the program to make contact, determination that the applicant was ineligible, or failure of the applicant to complete required forms. It is also possible that applicants not originally selected for assistance via the lottery nonetheless ended up receiving it. To account for this type of imperfect take-up, our analysis focuses 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 whether an applicant received 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 2,442 and 24,021). The size of the first-stage coefficients differ as there were differential take-up rates across sites. Some of these differences come from

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

[16] 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.

how the lotteries were implemented and specifically how much screening took place at the time of the application versus upon selection 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 the Los Angeles program, the assistance was paid to landlords, some of whom did not accept the program's 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, unweighted 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 with an ordinary least squares (OLS) regression of equation (1) that controls for any site-specific design features. The table summarizes applicant characteristics, 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 the applications for assistance.[17] 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 at baseline to applicants not selected. 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 the data for King County are available only for applicants who responded to the survey. As discussed in Section 4.2.1, survey response rates were not

[17] For Harris County, we impute gender and race using the R packages *gender*, *genderdata*, and *wru*.

fully balanced across treatment and control groups. To address this issue, in any analysis of survey outcomes, we always weight the answers by 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 whether an applicant responded to the survey, a dummy for treatment assignment, and a host of baseline control variables. For 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 vary across sites depending on data availability.[18] 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 discussed in Section 5.1, we obtain similar results with and without these weights.

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 are available. The vertical 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).[19]

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 pretreatment months. An exception is King County, for which we can link credit records of survey respondents only, introducing selection as described above. Second, the overall trend for both the 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

[18] For Los Angeles, these controls include age, household size, gender, language (English speaking vs non-English speaking), ethnicity, race, income, monthly rent, total rent owed, months' rent owed, and COVID hardship reason. For Harris County, they include language (English speaking vs non-English speaking), contact information (valid email, two phone numbers, etc.), public assistance type, income eligible, gender, race, and ethnicity. For King County, they include indicators for race, gender, and primary language, and for Chicago, they include..

[19] In King County, the applications were made and lotteries conducted over time. Thus, we display the data by relative time, limiting to time periods for which data are available for the full sample. We indicate the start of the pandemic using the average for the sample.

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 that we study likely did not have meaningful effects on downstream measures of financial health such as delinquency or default.

5 Results

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

5.1 Impacts on Survey Outcomes

Table III reports IV estimates of the effect of assistance receipt on applicants' likelihood of paying rent and bills, self-reported measures of health, and feelings of economic insecurity. Outcomes are typically measured with respect to the month prior to survey collection, which occurred at different times across locations. However, the questions about rent payment ask about specific months following the month of the lottery.[20] 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 from the weighted regressions that adjust for attrition, the unweighted results are qualitatively similar (Appendix Table C.2).

The 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 are 5.2–13.1 percentage points more likely to have paid their rent in full in the month prior to the survey. Across these three sites, the combined increase in the likelihood of rent payment is 7.8 percentage points, a 14.2 percent increase over the control group mean. The effects are noticeably different for Los Angeles, though this may result from the difference in the respective survey question, which focused on cumulative rent payment (i.e., it asks if the respondent is "behind on rent"). The effects of assistance

[20] See Appendix A for details about the survey questions across each site.

on other types of bill payment appear more mixed. Across four of the five lotteries, we find no evidence of impacts of the assistance on other bill payment but some indication in LA that receipt increased payment of bills 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 their likelihood of becoming sick in the previous month as a consequence of COVID-19, and the point estimates for King County and Harris County of the differences in COVID-19 infections are of similar magnitude, albeit measured with less precision. Similarly, recipients are less likely to have reported feelings of anxiety or depression, a 3.6-percentage-point (7.1 percent) decrease across Harris County, King County, and Chicago.

Finally, while recipients are less likely to have reported 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 reported rates of emergency shelter use. We consider impacts on homelessness as measured by administrative records in Section 5.5. Respondents also report less frequent food insecurity in the previous month, but this difference is not statistically significant. Overall, our results using the survey measures suggest modest positive effects for recipients.

5.2 Impacts on Credit Measures

Figure II confirms that credit outcomes were similar for those who received assistance and those who did not, both before and after the lotteries. The differences in outcomes are plotted for the Harris County (1st row), King County (2nd row), Los Angeles (3rd row), and Chicago (4th and 5th rows) lotteries. Each column of figures reports effects on a different outcome: credit score (left column), balance in delinquent accounts (middle column), and balance in utilities collections (right column). The ITT and IV estimates are depicted by navy diamonds and gold circles, respectively. Across outcomes and locations, there is a consistent pattern in the results—namely, that being selected to receive assistance (ITT) or receiving assistance (IV) did not meaningfully improve applicants' financial well-being.

Table IV summarizes the IV estimates of the 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. Receipt of assistance led to a $74 decrease in the debt balance in collections (column (6)), driven by declines in the King County 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 are no more likely to have increased their durable consumption by taking out a car loan/lease and no more likely to have used a revolving line of credit. Within ten months of assistance receipt, there are no statistically significant differences in the administrative credit outcomes.

The overall effects on credit outcomes are small and precisely estimated. For example, the 95 percent confidence interval for the short-run effect on credit scores ranges from -3.1 to 1.8. Thus, we can rule out large effects on credit scores like the 16.5-point decrease in response to an eviction estimated in Collinson et al. (2024) or the 9.4-point increase in response to the removal of a bankruptcy flag estimated in Dobbie et al. (2020). Similarly, the 95 percent confidence interval on the 10-month balance in collections, which ranges from -\$175 to \$196, allows us to rule out effects of the magnitude of the \$302 increase in collections balances 4 years after a hospitalization among nonelderly insured adults estimated in Dobkin et al. (2018).

One possible explanation for these modest effects 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 their applying for assistance. We divide the 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 C.2 plots the standardized IV estimates for the effects of assistance on credit scores and balances in delinquent accounts and utility collections separately by subgroup and site.[21] Within sites, there are few meaningful differences among the subgroups. Moreover, there is no consistent pattern *across* sites. Taking these results together, we find little evidence that the more disadvantaged individuals in our sample experienced larger gains from the assistance.

5.3 Impacts on Residential Mobility

We find no consistent evidence that geographic mobility changed as a result of receiving emergency rental assistance. Table V reports our IV estimates of the effects of assistance on residential mobility. We measure address changes in two administrative data sets in the short run (two months post-lottery) and long run (ten months post-lottery). Using the Infutor data, we code a change of address if an applicant's current address differs from her address at the time of application. In the Experian data, address changes are measured by changes in ZIP code relative to the 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

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

a statistically insignificant 0.8 percentage points from a mean of 9.3, and the 95 percent 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 a changed address one year later by 8.2 percentage points (Collinson et al., 2024).

5.4 Targeting

While we find small or no impacts of the emergency financial assistance on the survey outcomes, credit outcomes or housing stability, the large-scale emergency rental assistance programs that we evaluate may have helped better target pandemic assistance to households in greater need. Across all sites, these programs required applicants to demonstrate that they met the eligibility requirements outlined in Section 2. These requirements both targeted funding toward those with lower income and 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 rental assistance programs, we compare the credit outcomes of program applicants to those of a random sample of renters from the same city. To do so, we use a panel consisting of a 10 percent sample of individuals from Equifax. We restrict to renters living within one of the catchment areas for the programs in the months that 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 C.3 plots the outcome trajectories for the treatment and control groups and the random samples of renters over time relative to the treatment month for all five programs. Across all sites, we see very large gaps between the random sample of renters and those who participated in the lottery. For example, 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 and Chicago DOH. The lottery participants also largely have very low baseline credit scores ranging between 550 and 640 (with the standard cut-off for subprime being 600), with a smooth rise of approximately 25 points during the pandemic.

Similar gaps are present for the balances in delinquent accounts and in collections, with those participating in the lottery having average balances in collections ranging from approximately \$3,000 in Harris County and \$1,400 to \$1,800 in King County to \$1,000 in Los Angeles and \$500 to \$1,100 in Chicago. The 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 samples of renters have largely flat trajectories, while the treatment and control groups 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.

5.5 Impacts on Other Measures of Housing Instability

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

In Table VI, we report the effects of receiving assistance on homelessness system use in the 9 months after the lottery for three of the lotteries that we study: King County, Chicago DOH, and Chicago TRP. We examine the impacts on any homelessness system use as in Collinson et al. (2024) and Phillips and Sullivan (2023) and the impacts on the types of services used. The effects of assistance on homelessness are mixed. In King County, receipt of assistance *increased* interactions with the homelessness system by 2 percentage points. Descriptively, this result appears to be driven primarily by unassisted members of the assigned treatment group being made *more* likely to show up in the homelessness system, as we explore in Appendix Figure C.4. This could be due to applicants whose applications were incomplete or who were found to be ineligible being steered to other resources in the homelessness system by program staff.

In Chicago, applicants who received assistance through the TRP lottery were 0.35 percentage points less likely to appear in the homelessness system in the 9 months after the lottery. This is a small absolute reduction but a large relative reduction of 65 percent since homelessness system interactions are rare even among the TRP applicants not selected for assistance via lottery (only 0.55 percent of control group applicants end up in the homelessness system). Applicants who received assistance through the Chicago DOH lottery are slightly less likely to have interacted with the homelessness system, but the difference is not statistically significant.

Our results on homelessness from King County and Chicago DOH contrast with other evidence on emergency rental assistance programs from outside the pandemic context. Prior work finds that emergency rental assistance decreases rates of homelessness by approximately three-quarters in Chicago (Evans et al., 2016) and San Jose (Phillips and Sullivan, 2023). This contrast between our results and others from the literature does not arise from major differences in the interventions being studied. The emergency financial assistance programs offered during COVID were largely designed based on existing programs and in most cases were even operated by the same agencies. The traditional and COVID-era programs both feature temporary assistance in relatively modest amounts. For example, in Evans et al. (2016), Phillips and Sullivan (2023), and Phillips and Sullivan (2022), the clients offered financial assistance received an average of 0.7, 1.0, and 0.7 months of rent, respectively, which are magnitudes similar to those of the programs that we study in Chicago, Harris County, and Los Angeles, though the King County program is somewhat more generous.

The programs that we study also target clients who appear largely similar to the participants in pre-COVID-era rental assistance programs. The King County COVID rental assistance lottery and the pre-COVID homelessness prevention program studied in Phillips and Sullivan (2022) were operated by the same agency, are evaluated by means of 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 the pandemic in March 2020, compared to 18.1 percent for the sample in Phillips and Sullivan (2022). Homelessness service usage differs slightly between the two samples, at 4.8 and 7.9 percent, respectively, but nevertheless is still high. Similarly, the DOH lottery population in Chicago has rates of prior homelessness comparable to those studied in (Evans et al., 2016). An exception is the Chicago TRP lottery, which served a population with much lower pre-COVID rates of homelessness system use than those of the populations studied in prior work.

Our results on homelessness for the Chicago TRP lottery are qualitatively similar to the findings from prior studies of pre-COVID emergency rental assistance programs (Evans et al., 2016; Phillips and Sullivan, 2023), showing a sharper reduction in homelessness for those who received assistance through this lottery than for those treated by the other lotteries that we evaluate here. A plausible explanation for this difference is that the TRP program targeted populations who might have more limited access to other aspects of the pandemic safety net, such as expanded unemployment insurance or COVID rebate payments, either because they were undocumented immigrants or lacked formal sector employment. In Appendix Table C.11, we report the fraction of applicants receiving government assistance across lotteries. Consistent with programmatic differences in target populations, we find that TRP applicants report much lower rates of assistance from government sources in our survey than Chicago DOH applicants or applicants in King County.

Finally, we examine impacts on eviction. As discussed in Section 2, new eviction cases could be initiated and new judgments issued in Harris County throughout the studied period, including the period covered by the CDC moratorium. However, enforcement of eviction orders by law enforcement was stayed during the moratorium. In contrast, eviction moratoria cover the entire sample period in King County and Chicago, so use of homelessness programs serves as a more useful outcome measure for these locations. To examine the impacts on filing of evictions and related judgment activity, we link the sample of Harris County assistance applicants to eviction data, as described in Appendix B.

Appendix Table C.5 reports the effects of receipt of assistance on eviction filing (initiation of 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 were rare during this period: only 1.2 percent of unassisted 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 these application tracts, approximately 7 percent of renter households had a filing in 2019. This difference suggests that while eviction activity continued, filing remained depressed relative to typical levels during 2020–2021. [22] Column (2) of Appendix Table C.5 reports the IV estimates for the effects of the assistance. Receiving assistance through the Harris County lottery appears to have lowered the risk of an 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 the risk of eviction in Harris County.

Taking the above results together, we find little evidence that applicants who received emergency rental assistance through the lottery were less likely than nonselected applicants to experience extreme housing instability in the months after applying. These results are consistent with our finding of limited to no effects of assistance on residential mobility or credit market outcomes.

6 Discussion and Concluding Remarks

This paper studies the unprecedented expansion of emergency rental assistance that occurred in response to the economic disruption caused by the COVID-19 pandemic. Our multisite study links administrative application data from five program lotteries conducted in four large cities to credit records and novel survey data to overcome three major hurdles to examining assistance policies in rental markets: 1) lack of centralized data collection on

[22] In Appendix Figure C.5 we plot monthly eviction filings in Harris County. Filing volumes drop with the onset of COVID-19 and the CARES Act Moratorium. Filings remain roughly 50 percent lower than pre-pandemic levels (though still average about 2,000 cases per month) through the CDC moratorium.

rental assistance, 2) lack of administrative data on key outcomes, and 3) selection bias in comparisons of those who did and did not receive rental assistance.

We find that the rental assistance was successful in reducing rental arrears and led to modest improvements in self-reported mental health. However, it had little detectable short- or longer-run effect on broader measures of financial security, including evictions, homelessness, residential mobility, credit scores, indebtedness, or delinquency.

The key open question raised by these findings is: why are the effects so small? Several pieces of evidence suggest that the modest effects that we see are unlikely to be a result of poor targeting or insufficient support. The program applicants were substantially more financially distressed than the average renter in their communities, and the null effects that we find persist even in subsamples of applicants with severe financial distress at the time of application. The size of the grants that we study is also similar to that of other, smaller-scale assistance programs that have been shown to have large effects (Phillips and Sullivan, 2023; Evans et al., 2016). Moreover, we find very limited heterogeneity across sites, despite the fact that the assistance was significantly more generous in some places (e.g., King County).

These results suggest that the broader economic and policy context, rather than program design or targeting, was responsible for the modest size of the effects that we find. Unlike other emergency rental assistance programs, which target shocks faced by individual renters, the programs that we study were created with the specific aim of responding to a macroeconomic shock. There are two natural explanations for why ERA could be less impactful during a macroeconomic downturn. First, macro-level shocks tend to be accompanied by other fiscal stimulus that provides additional protection to vulnerable renters. The COVID pandemic, in particular, featured a massive expansion of nearly every aspect of the social safety net, and created unprecedented limits on eviction, which may have muted the marginal effectiveness of additional rental assistance. Consistent with this conjecture, we find that individuals who applied for assistance—regardless of whether they actually received it—experienced broad-based improvements in their financial circumstances in the post-application period.

A second explanation is that, even in the absence of expanded fiscal stimulus, general equilibrium market responses to aggregate shocks may limit the impact of additional rental assistance. For landlords, evicting a tenant is likely a less attractive option during a widespread downturn than during normal times, when vacancies can be more easily filled at higher prices. Because of this, renegotiations or temporary agreements between landlords and tenants may be sufficient to forestall eviction without additional emergency assistance. Consistent with this possibility, we find that more than 70 percent of the lottery applicants who missed rental payments reported having some type of agreement with their landlord

(Appendix Table C.10). This evidence is in line with pandemic-era surveys of landlords that find substantial increases in reported rental forbearance in 2020, relative to pre-pandemic levels (de la Campa and Reina, 2023; Kneebone and Herbert, 2021; Decker, 2021).[23] In such a setting, rental assistance may reduce rental arrears without changing tenants' longer-run housing situation, because these arrears would not have led to eviction or homelessness even in the absence of assistance.

While extensive evidence from the Great Recession has yielded lessons on the effectiveness of foreclosure prevention policies during macroeconomic downturns, far less is known about policies for renters. Our lottery-based estimates of the impact of emergency rental assistance allow us to measure the effects of one such policy and offer a potentially valuable lesson about the effectiveness of these programs as macroeconomic stabilizers. Whether our results generalize to subsequent rounds of emergency rental assistance (which were typically more generous, prioritized paying-down arrears, and were implemented during the tail end of the pandemic), is an open question. Future work should seek to study the effects of these programs on both tenants and *landlords*.

[23] Data from an earlier downturn points in a similar direction: during the 2009 financial crisis, eviction rates remained approximately constant (Gromis et al., 2022) despite an an approximate doubling in the unemployment rate among renters (see Appendix Figure C.6), in contrast to foreclosures, which spiked during this period.

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7 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	38.367	38.741	0.374	38.991	38.944	−0.046
Female	0.648	0.642	0.012	0.618	0.618	0.005	0.546	0.541	−0.005	0.610	0.592	−0.019	0.562	0.560	−0.002
Household Size	–	–	–	–	–	–	2.660	2.664	0.005	2.990	3.002	0.012	4.050	4.004	−0.047
White	0.061	0.050	−0.004	0.231	0.235	0.007	0.213	0.217	0.003	0.218	0.223	0.004	0.039	0.034	−0.006
Black	0.609	0.630	−0.006	0.325	0.345	−0.027*	0.116	0.115	−0.001	0.333	0.336	0.003	0.120	0.112	−0.008
Hispanic	0.259	0.254	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	0.024	0.024	−0.006**	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	1.005	1.003	−0.002	1.485	1.484	−0.000	1.419	1.424	0.005	1.050	1.057	0.007	1.004	1.000	−0.004
Poverty Rate	0.592	0.591	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	0.766	0.757	−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	0.431	0.450	−0.000	0.123	0.123	−0.001	0.487	0.483	−0.005	0.322	0.319	−0.003	0.487	0.485	−0.002
%Non-Hispanic White	0.129	0.120	−0.000	0.519	0.518	0.000	0.264	0.268	0.004	0.256	0.257	0.001	0.207	0.208	0.001
% Black	0.375	0.373	0.000	0.101	0.102	−0.000	0.090	0.089	−0.001	0.346	0.345	−0.001	0.236	0.239	0.003
% Asian	0.050	0.041	0.000	0.176	0.176	0.000	0.128	0.130	0.002	0.056	0.058	0.003	0.053	0.052	−0.001
Experian Linked: Jan 2020															
Credit Score	554	553	−1.299	583	575	8.510*	637	636	−0.961	603	610	6.096**	644	651	4.399
Balance Across All Trades	21.421	22.916	−1.790**	19.314	18.346	2.624	19.775	20.011	0.236	32.008	34.483	2.320	19.114	22.391	2.913
Balance in Collections	3.206	3.265	0.001	1.436	1.829	−0.398**	0.917	0.932	0.015	1.121	1.133	0.001	0.491	0.432	−0.046
Balance in Utility Collections	0.574	0.568	0.021	0.298	0.386	−0.084**	0.125	0.120	−0.005	0.306	0.311	0.005	0.147	0.114	−0.029
Balance in Delinquent Accts.	10.211	9.867	0.423	7.776	8.355	−0.567	4.986	4.988	0.003	6.753	6.774	−0.040	2.271	2.136	−0.061
Auto Loan or Lease	0.292	0.296	−0.002	0.308	0.334	−0.019	0.279	0.274	−0.005	0.306	0.338	0.031*	0.214	0.227	0.010
Personal Bankruptcy	0.020	0.019	0.001	0.067	0.087	−0.016	0.050	0.043	−0.007**	0.143	0.151	0.008	0.032	0.030	−0.000
Rev. Line of Credit	0.347	0.342	−0.003	0.529	0.499	0.035	0.656	0.658	0.003	0.569	0.594	0.018	0.553	0.569	0.011
Survey															
Response Rates	0.185	0.219	0.035***	0.235	0.264	0.016	0.129	0.148	0.020***	0.283	0.409	0.126***	0.175	0.311	0.136***
N†	4,946	13,815	18,781	6,868	5,087	11,955	13,062	11,315	24,377	73,126	1,537	68,075	4,688	1,765	6,414

Notes: Data come from program applications, the 2015-2019 American Community Survey, and credit records from Experian. The sample includes all program applicants for all variables except credit attributes, for which the sample is 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 features. See Section 4. All monetary values expressed in 2020 U.S. dollars divided by 1000. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

†We report the maximum observation counts. Observation counts for Experian characteristics are lower than those for the rest of the characteristics.

TABLE II

First Stage

	Received Assistance				
	Harris County (1)	King County (2)	Los Angeles (3)	Chicago DOH (4)	Chicago TRP (5)
Selected by Lottery	0.575***	0.372***	0.207***	0.431***	0.910***
	(0.004)	(0.008)	(0.006)	(0.013)	(0.007)
Control Mean	0.000	0.046	0.347	0.001	0.000
F Statistic	16,469	2,442	1,093	47,977	47,332
N	18,834	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	0.076*	0.131**	−0.147	0.058*	0.059**	0.035	0.081***
	(0.040)	(0.053)	(0.122)	(0.031)	(0.028)	(0.029)	(0.020)
	[0.497]	[0.368]	[0.385]	[0.649]	[0.678]	[0.515]	[0.548]
All Bills Paid	−0.022	0.018	0.242*	−0.025	0.023	0.047	−0.001
	(0.032)	(0.043)	(0.134)	(0.033)	(0.033)	(0.030)	(0.018)
	[0.284]	[0.203]	[0.361]	[0.358]	[0.496]	[0.340]	[0.335]
Health							
COVID Positive (self-tested)	−0.110	−0.056	−0.026	−0.051***	0.002	−0.048	−0.054*
	(0.105)	(0.073)	(0.125)	(0.017)	(0.018)	(0.036)	(0.033)
	[0.182]	[0.074]	[0.263]	[0.101]	[0.095]	[0.143]	[0.113]
Feeling Anxious	−0.069*	0.004	0.044	−0.075**	0.002	−0.019	−0.034*
	(0.036)	(0.051)	(0.135)	(0.033)	(0.028)	(0.031)	(0.019)
	[0.525]	[0.633]	[0.543]	[0.549]	[0.319]	[0.514]	[0.506]
Economic Insecurity							
Worried about Eviction	−0.082**	−0.052	0.052	−0.029	−0.022	−0.027	−0.046**
	(0.034)	(0.053)	(0.128)	(0.030)	(0.020)	(0.029)	(0.018)
	[0.320]	[0.443]	[0.343]	[0.287]	[0.121]	[0.303]	[0.293]
Experienced Homelessness	−0.031	−0.009	−0.123	−0.022	−0.005	−0.038**	−0.017
	(0.030)	(0.037)	(0.076)	(0.014)	(0.017)	(0.018)	(0.013)
	[0.241]	[0.146]	[0.086]	[0.063]	[0.079]	[0.123]	[0.132]
Stayed in a Homeless Shelter	−0.016**	−0.001	−0.002	0.008	−0.000	−0.002	−0.002
	(0.008)	(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	−0.048	0.026	−0.054	−0.039	−0.016	−0.026	−0.019
	(0.033)	(0.049)	(0.127)	(0.029)	(0.026)	(0.029)	(0.018)
	[0.335]	[0.325]	[0.291]	[0.270]	[0.256]	[0.296]	[0.297]
N	1,671	2,621	3,360	14,681	1,352	23,685	20,325

Notes: This table reports the effects of assistance on expenditures, health, and economic insecurity. Outcomes are derived from the surveys described in Section 3. Survey timing and treatment dates appear in Appendix Table C.1. See Appendix A for details on the survey questions. Columns (1)–(5) report separately by site the IV estimates of the effects of assistance on the measure listed in the row, reweighting to adjust for survey nonresponse 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 DOH (4)	Chicago TRP (5)	Combined (6)
Short Run: 2 months after lottery						
Credit Score	2.863**	−3.413	0.854	−3.494	0.155	−0.607
	(1.235)	(4.511)	(2.480)	(2.755)	(1.566)	(1.234)
	[565]	[597]	[649]	[612]	[654]	[615]
Balance Across All Trades ($1000s)	0.259	0.113	0.929	1.655	−1.351*	0.321
	(0.500)	(2.392)	(1.086)	(1.937)	(0.742)	(0.677)
	[24.825]	[21.318]	[19.628]	[33.758]	[20.784]	[24.063]
Balance in Collections ($1000s)	0.003	−0.233	−0.157**	0.026	−0.008	−0.074*
	(0.058)	(0.161)	(0.070)	(0.067)	(0.029)	(0.040)
	[3.258]	[1.371]	[0.965]	[1.120]	[0.489]	[1.441]
Balance in Utility Collections ($1000s)	0.013	−0.024	−0.000	−0.030	−0.020**	−0.012
	(0.019)	(0.048)	(0.015)	(0.031)	(0.010)	(0.013)
	[0.617]	[0.255]	[0.121]	[0.322]	[0.150]	[0.293]
Balance in Delinquent Accounts ($1000s)	−0.230	0.639	0.132	−0.384	−0.146	0.002
	(0.218)	(0.850)	(0.374)	(0.559)	(0.208)	(0.225)
	[9.459]	[6.315]	[4.556]	[5.812]	[2.152]	[5.659]
Any Auto Loan or Lease	−0.004	−0.013	0.007	−0.004	0.007	−0.001
	(0.007)	(0.024)	(0.015)	(0.016)	(0.009)	(0.007)
	[0.300]	[0.311]	[0.277]	[0.293]	[0.212]	[0.279]
Any Personal Bankruptcy	0.001	−0.003	−0.007*	−0.005	0.000	−0.003
	(0.001)	(0.006)	(0.004)	(0.007)	(0.002)	(0.002)
	[0.019]	[0.065]	[0.047]	[0.144]	[0.032]	[0.061]
Any Open Revolving Line of Credit	0.009	−0.023	0.007	0.026	−0.008	0.002
	(0.007)	(0.028)	(0.012)	(0.016)	(0.008)	(0.007)
	[0.372]	[0.536]	[0.667]	[0.576]	[0.571]	[0.544]
Long Run: 10 months after lottery						
Credit Score	0.881	−2.633	−0.932	−6.940*	1.253	−1.674
	(1.597)	(6.344)	(3.713)	(2.077)	(3.279)	(1.693)
	[575]	[604]	[659]	[624]	[663]	[625]
Balance Across All Trades ($1000s)	−0.243	−3.166	2.237	−1.329	−0.732	−0.647
	(0.830)	(4.300)	(2.054)	(2.623)	(1.256)	(1.129)
	[26.756]	[23.879]	[20.659]	[35.893]	[21.809]	[25.799]
Balance in Collections ($1000s)	0.007	0.011	0.013	0.065	0.006	0.020
	(0.084)	(0.250)	(0.110)	(0.101)	(0.043)	(0.061)
	[3.085]	[1.385]	[0.969]	[1.124]	[0.501]	[1.413]
Balance in Utility Collections ($1000s)	−0.005	−0.016	−0.009	−0.044	−0.011	−0.017
	(0.022)	(0.059)	(0.020)	(0.038)	(0.014)	(0.015)
	[0.590]	[0.229]	[0.109]	[0.286]	[0.129]	[0.269]
Balance in Delinquent Accounts ($1000s)	0.116	−0.687	0.574	0.381	−0.103	0.056
	(0.262)	(1.019)	(0.482)	(0.669)	(0.222)	(0.271)
	[8.473]	[6.176]	[4.275]	[5.267]	[1.973]	[5.233]
Any Auto Loan or Lease	0.001	−0.027	−0.013	0.003	−0.001	−0.007
	(0.010)	(0.039)	(0.022)	(0.022)	(0.014)	(0.011)
	[0.314]	[0.320]	[0.285]	[0.328]	[0.224]	[0.294]
Any Personal Bankruptcy	−0.001	0.010	−0.014*	−0.024**	0.005*	−0.005
	(0.002)	(0.013)	(0.008)	(0.010)	(0.003)	(0.004)
	[0.019]	[0.059]	[0.046]	[0.141]	[0.030]	[0.059]
Any Open Revolving Line of Credit	−0.018	−0.019	0.021	0.009	−0.012	−0.005
	(0.011)	(0.042)	(0.019)	(0.022)	(0.011)	(0.011)
	[0.435]	[0.582]	[0.676]	[0.603]	[0.587]	[0.577]
N	14,159	1,889	18,559	45,853	2,577	83,037

Notes: This table reports the effects of assistance on short-run (2 months) and longer-run (10 months) credit outcomes. Columns (1)–(4) report separately by site the IV estimates of the effects of assistance on the measure listed in the row. Column (5) reports the combined results of all of the locations. Sample is restricted to lottery applicants 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
Impacts on Residential Mobility

	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	0.000	−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	0.001	−0.003	0.005
	(0.010)	(0.036)	(0.017)	(0.019)	(0.008)	(0.009)
	[0.102]	[0.105]	[0.066]	[0.080]	[0.034]	[0.077]
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	−0.000	0.016	0.010
	(0.014)	(0.053)	(0.027)	(0.028)	(0.014)	(0.014)
	[0.250]	[0.303]	[0.187]	[0.202]	[0.092]	[0.207]
N (Infutor)	6,562	5,570	11,864	26,373	1,616	51,985
N (Experian)	14,159	1,889	18,559	45,853	2,577	83,037

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: by a direct measure of change of address via Infutor, and by 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 separately by site the IV estimates of the effects of assistance on the measure listed in the row. 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: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

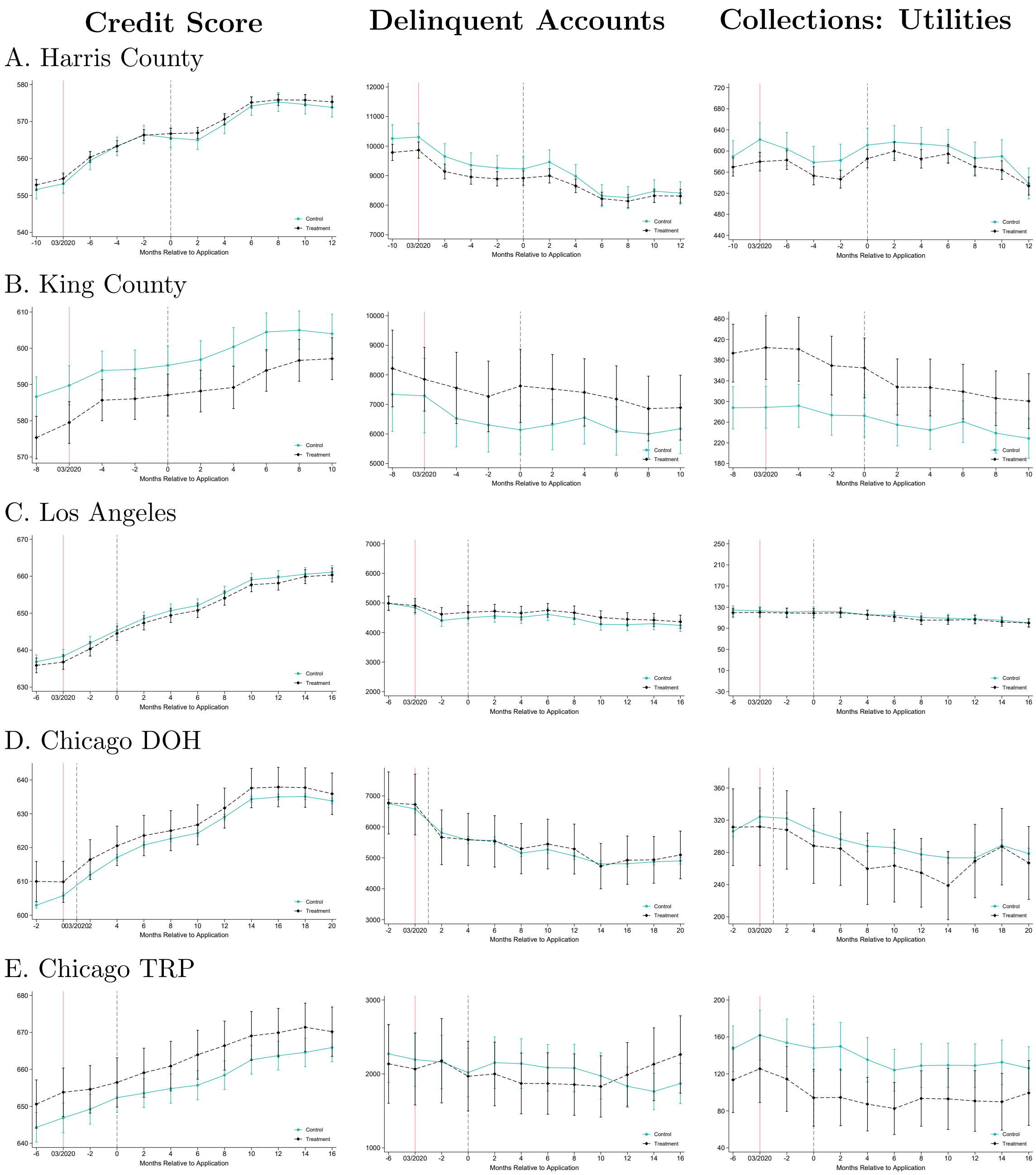
TABLE VI
Impacts on Homelessness

	King County (1)	Chicago DOH (2)	Chicago TRP (3)
Any Homeless Services	0.020**	−0.0021	−0.0035**
	(0.0083)	(0.0055)	(0.0017)
	[0.027]	[0.0092]	[0.0055]
	{0.048}	{0.0109}	{0.0045}
Emergency Shelter	0.0028	−0.0003	−0.0003
	(0.0039)	(0.0015)	(0.0004)
	[0.0064]	[0.0008]	[0.0085]
	{0.018}	{0.0024}	{0.0015}
Street Outreach	0.0067*	0.0016	−0.0007*
	(0.0036)	(0.0021)	(0.0004)
	[0.0037]	[0.0006]	[0.0006]
	{0.0091}	{0.0021}	{0.0014}
Diversion/Prevention	0.0081*	−0.0019	−0.0022
	(0.0045)	(0.005)	(0.0015)
	[0.0070]	[0.0078]	[0.0038]
	{0.019}	{0.007}	{0.0017}
Long-Term Subsidies	0.0012	−0.00097***	−0.00065*
	(0.0059)	(0.0002)	(0.0004)
	[0.015]	[0.00042]	[0.0006]
	{0.021}	{0.0006}	{0.0006}
Coordinated Entry	0.0010		
	(0.0024)		
	[0.0024]		
	{0.0078}		
N	12,148	74,663	6,453

Notes: This table reports the effects of assistance on measures of homelessness system use in the 9 months after the lottery in three lottery samples: King County, Chicago DOH, and Chicago TRP. The first row reports impacts on any appearance in the homelessness system ("Any Homeless Services"), and the subsequent rows report impacts on specific types of system use. "Emergency Shelter" includes emergency shelter, transitional housing, and Safe Haven in Chicago. "Diversion/Prevention" refers to diversion programs in King County and homelessness prevention projects in Chicago. "Long-term Subsidies"" includes Rapid Rehousing and Permanent Supportive Housing in Chicago. Information on Coordinated entry is only available for King County. Columns (1)–(3) report separately by site the IV estimates of the effects of assistance on the measure listed in the row. Robust standard errors are reported in parentheses, and the control group mean of the outcome is reported in brackets. The pre-COVID means from March 2019 to February 2020 for the listed measure are reported in braces. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

FIGURE I

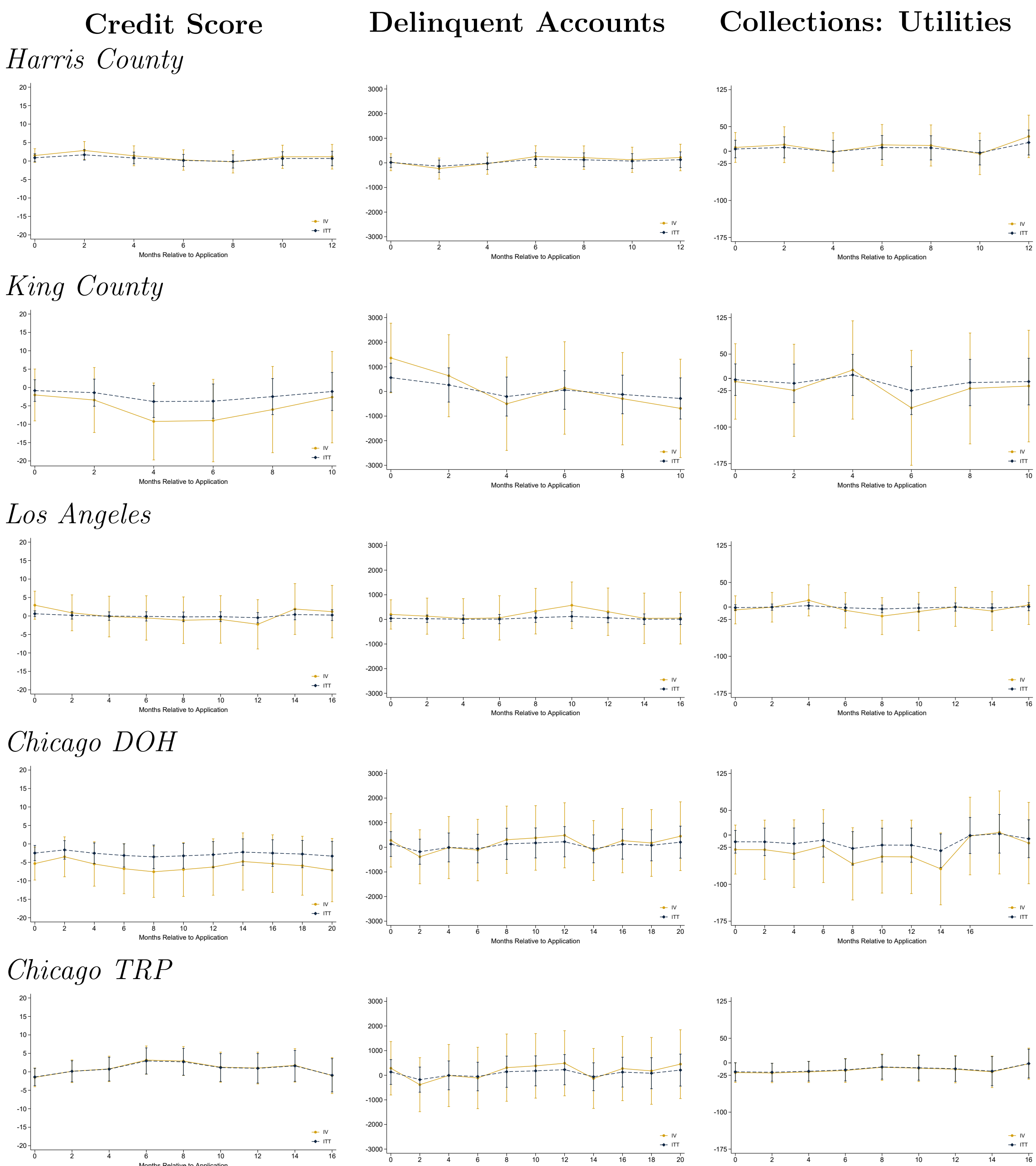
Credit Measure by Month Relative to Application



Notes: This figure plots the average credit characteristic for the treatment (black diamonds) and control groups (teal circles) against months relative to the time of application. Applications were submitted at different times across sites, and month 0 represents the site's application month. Each row corresponds to a different location: Harris County, King County, Los Angeles, Chicago DOH and Chicago TRP. Each column corresponds to a different credit characteristic: credit score, balance in delinquent accounts, and 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 linked to Experian credit data. Vertical bars plot the 95 percent confidence intervals of the estimates.

FIGURE II

ITT & IV Estimates by Month Relative to Application



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

A Construction of Survey Outcomes

Table III reports the results for a series of survey outcomes measured across sites. While the surveys were similar across sites, they were not identical, and the exact definitions of the outcome variables 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 outcome is based on Q29, for which the response is provided through a sliding percentage bar indicating the proportion of rent 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, this outcome is based on a question about whether 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

- For King and Harris Counties, this outcome is based on Q68, which asks separately if the respondent paid all of her gas, electric, phone, TV/cable, car payment, car insurance, health insurance, and student loan bills in the past month. An index is then created with "yes" coded as 1, "some" as 0.5, and "none" as 0. Bills for which the respondent answered "did not have bill" are omitted from the index.
- For Los Angeles, this outcome is based on 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 the variable is coded as 0, and otherwise as 1.
- For Chicago DOH and TRP, this outcome is based on Q67 and Q68, which ask "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?" 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: "Have you suspected that you were infected with COVID-19?" and "Have you tested positive for COVID-19?" The exact time period to which the question refers varies slightly across sites. For King and Harris Counties, this question begins with "Since March 1st," while for Los Angeles, it begins with "Since May," and for the Chicago TRP survey, it is "Since March 13th". The Chicago DOH survey did not ask these questions; instead, we code this outcome as 1 if the respondent selects "I got sick" in response 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 for this outcome, which is coded as 1 if the respondent reports "more than half the days" or "nearly every day". For Harris County, King County, and Chicago, the question refers to the last week, while for Los Angeles, the question asks about the last two weeks.

Worried about Eviction For 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. For all other sites, we use the same question, but it asks about the next two rather than three months.

Experience Homelessness Across all sites, this outcome is based on a question that asks whether the respondent has spent any nights couchsurfing, in a homeless shelter, on the street, in an abandoned building, in a car or van, or in a hotel or motel (for nontravel reasons). The outcome is coded as "yes" if the respondent answers "yes" to any of the the above options. For King County, Harris County, Chicago DOH, and Chicago TRP, this question refers to "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 coded as 1 only if the respondent answers "yes" to the "in a homeless shelter" option. Similarly to the "Experience Homelessness" outcome, for Los Angeles, the phrasing refers to "since May", while for all other sites, the question asks 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 1 if the responded selected "sometimes not enough to eat" or "often not enough to eat".

B Eviction Linking

We collect eviction case data from 2019 to December 2021 from the Harris County Justice of the Peace. These data include case information such as case date and judgment, along with defendant name and address. We link the lottery applications to eviction records using the following process:

1. Geocode lottery applications and eviction records
2. Construct soundex of first names and last names in lottery applications and eviction records
3. Exact match on 9-digit ZIP code, soundex of last name and soundex of first name

Approximately 2.5 percent of applicants can be linked to one or more eviction records from 2019 to 2021. Our analysis focuses on the linking to any eviction in the 2 months after the lottery or in the first 10 months after the lottery.

C Additional Tables and Figures

Appendix Table C.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. For Harris County, we study the second round of assistance that was offered in November 2020 rather than the earlier round distributed in July 2020. For 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 C.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	0.056	0.125**	−0.120	0.057*	0.051*	0.034	0.072***
	(0.036)	(0.052)	(0.120)	(0.030)	(0.027)	(0.028)	(0.019)
All Bills Paid	−0.007	0.023	0.222*	−0.024	0.024	0.047	0.004
	(0.029)	(0.042)	(0.130)	(0.033)	(0.032)	(0.029)	(0.017)
Health							
COVID Positive (self-tested)	−0.091	−0.066	0.039	−0.051***	−0.003	−0.035	−0.053**
	(0.084)	(0.063)	(0.122)	(0.016)	(0.017)	(0.033)	(0.027)
Feeling Anxious	−0.049	0.007	0.056	−0.072**	0.006	−0.010	−0.027
	(0.032)	(0.049)	(0.132)	(0.032)	(0.027)	(0.030)	(0.018)
Economic Insecurity							
Worried about Eviction	−0.072**	−0.061	0.046	−0.029	−0.015	−0.026	−0.044**
	(0.030)	(0.052)	(0.123)	(0.029)	(0.020)	(0.028)	(0.017)
Experienced Homelessness	−0.027	−0.006	−0.105	−0.022*	−0.003	−0.033*	−0.015
	(0.026)	(0.036)	(0.073)	(0.013)	(0.016)	(0.018)	(0.012)
Stayed in a Homeless Shelter	−0.011*	−0.004	−0.001	0.009	0.001	−0.001	−0.001
	(0.007)	(0.012)	(0.014)	(0.007)	(0.003)	(0.004)	(0.004)
Was Food Insecure	−0.042	0.022	−0.101	−0.038	−0.013	−0.034	−0.018
	(0.030)	(0.048)	(0.119)	(0.028)	(0.025)	(0.027)	(0.017)
N	1,671	2,626	3,360	14,681	1,352	23,690	20,330

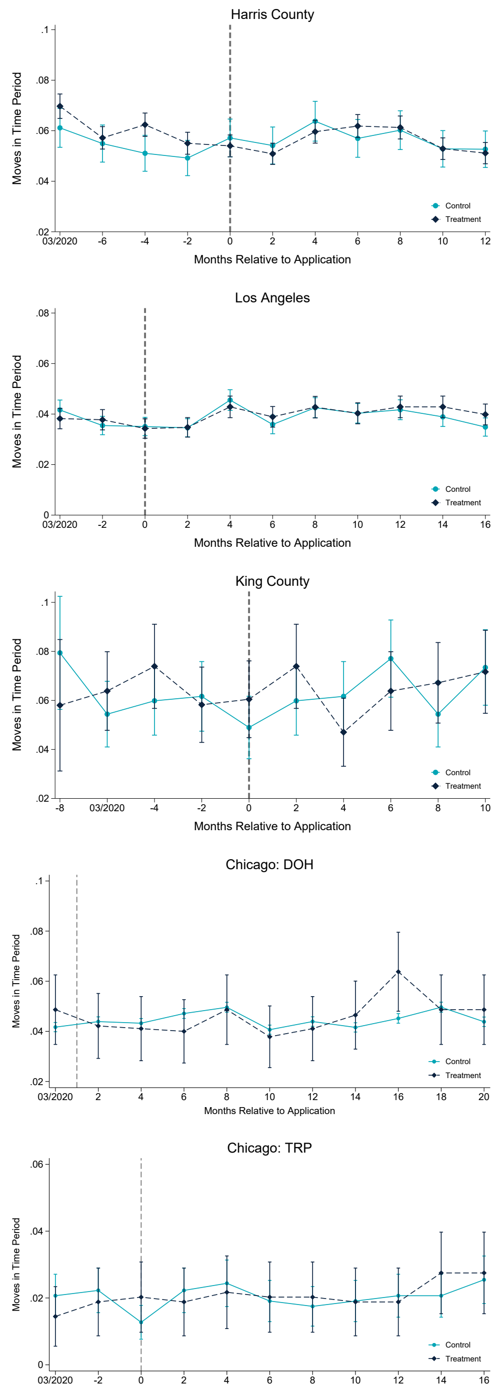
Notes: This table reports the effects of assistance on expenditures, health, and economic insecurity with no reweighting of the survey responses. Outcomes are derived from the surveys described in Section 3. Survey timing and treatment dates appear in Appendix Table C.1. See Appendix A for details on the survey questions. Columns (1)–(5) report separately by site the IV estimates of the effects of assistance on the measure listed in the row, 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: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table C.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

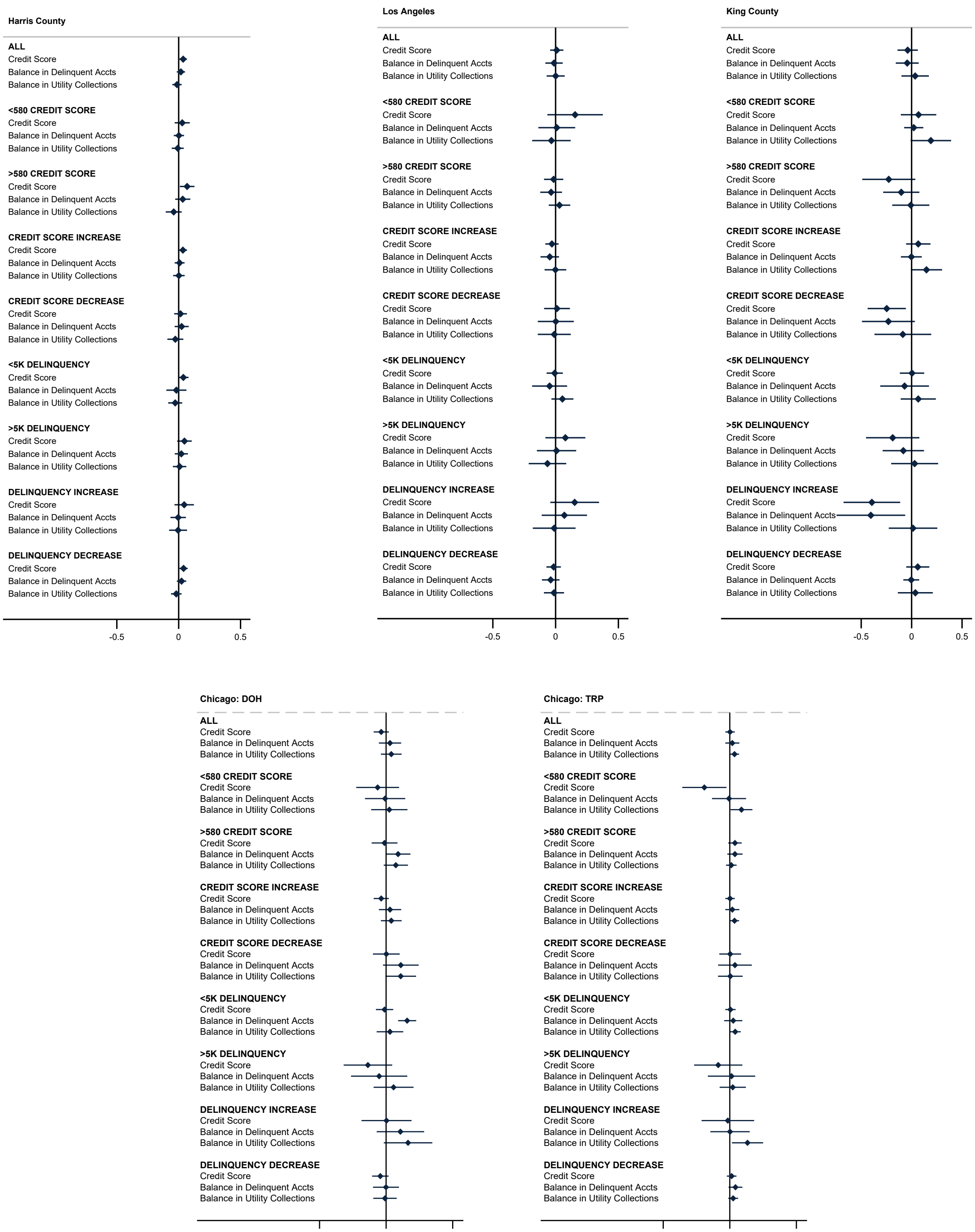
Notes: This table reports separately by site the average of demographic characteristics for applicants not selected by the lottery (Control) and applicants selected by the lottery (Treat). Conditional differences in average characteristics (Diff) between these two groups come from regressions that control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

Appendix Figure C.1
Moves in Time Period



Notes: This figure plots the average number of moves for the treatment and control groups against the number of months since 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.

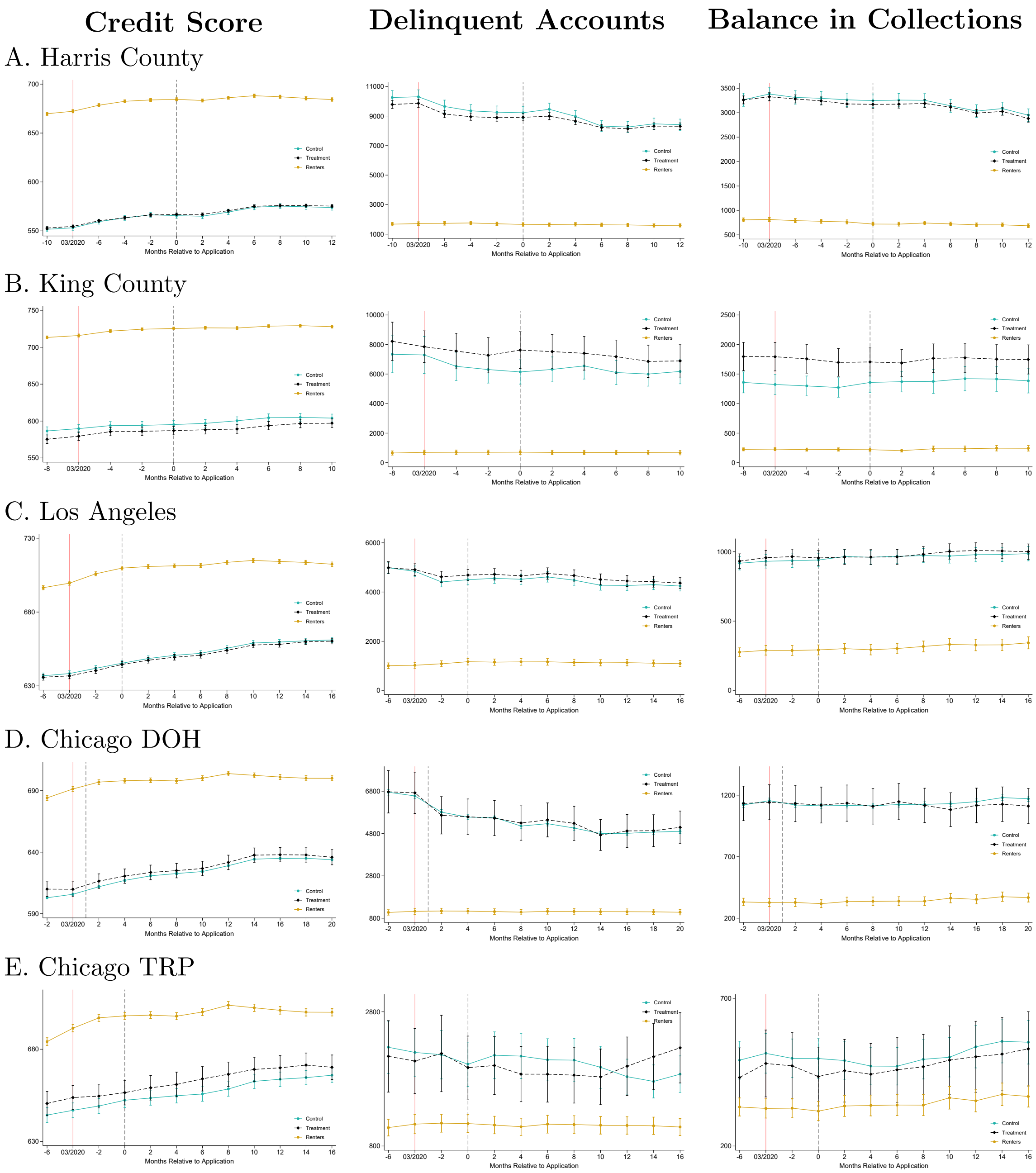
Appendix Figure C.2
Forest Plots



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 standardized by dividing the IV estimate by the standard deviation of the outcome in the control group. The leftmost forest plot is for Harris County and is followed by the plots of Los Angeles and King County. The sample includes applicants linked to the Experian credit data.

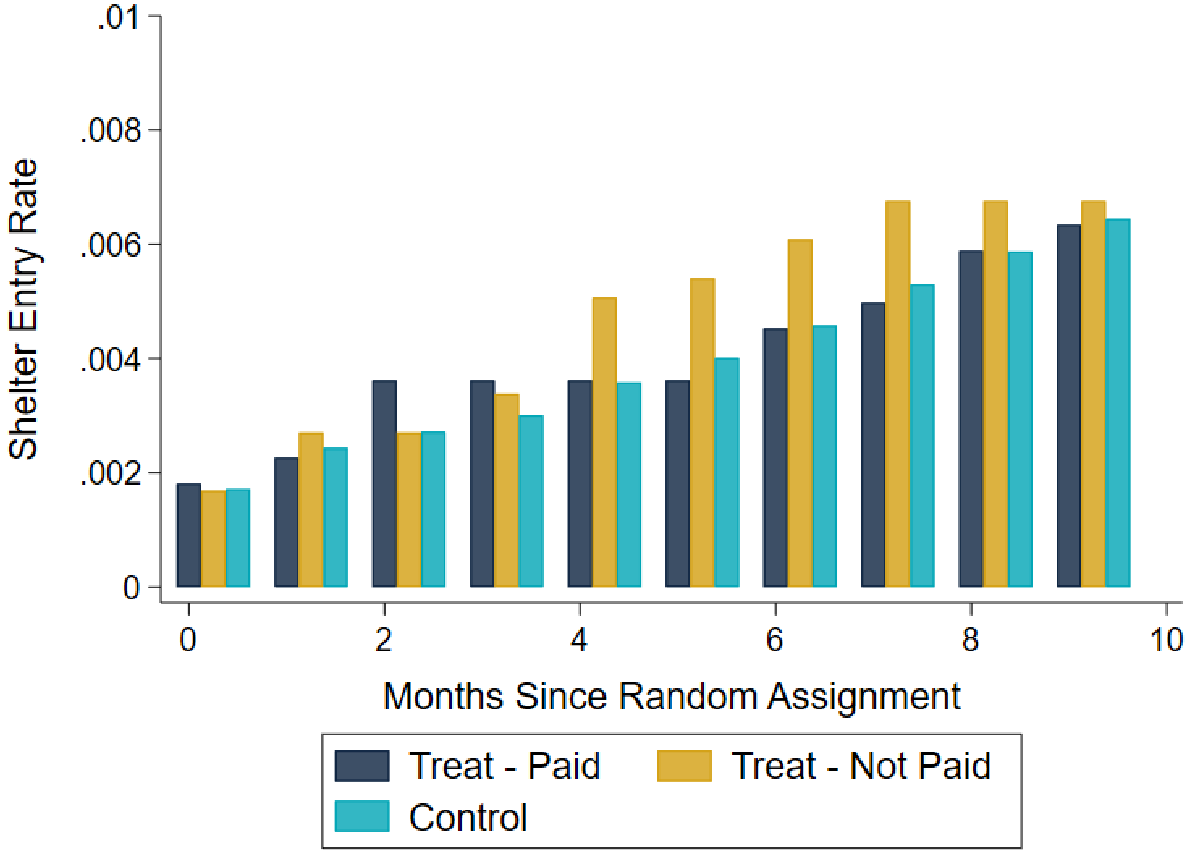
Appendix Figure C.3

Credit Outcomes by Month Relative to Application – Program Applicants vs Local Renter Population



Notes: This figure plots the average credit characteristics for the treatment and control groups and for renters against the number of months since application. Data on local renters come from a 10 percent sample of individuals from Equifax and include individuals outside of the applicant pool. Control and treatment samples include only applicants linked to the Experian credit data. Applications were submitted at different times across sites. Each column corresponds to a different credit characteristic: credit score, balance in delinquent accounts, and balance in utility collections. Each row corresponds to a different location: Harris County, King County, Los Angeles, Chicago DOH, and Chicago TRP. The dashed vertical line indicates the month of application, and the red vertical line indicates March 2020.

Appendix Figure C.4
Emergency Shelter Entry Rate by Random Assignment and Take-up of Treatment



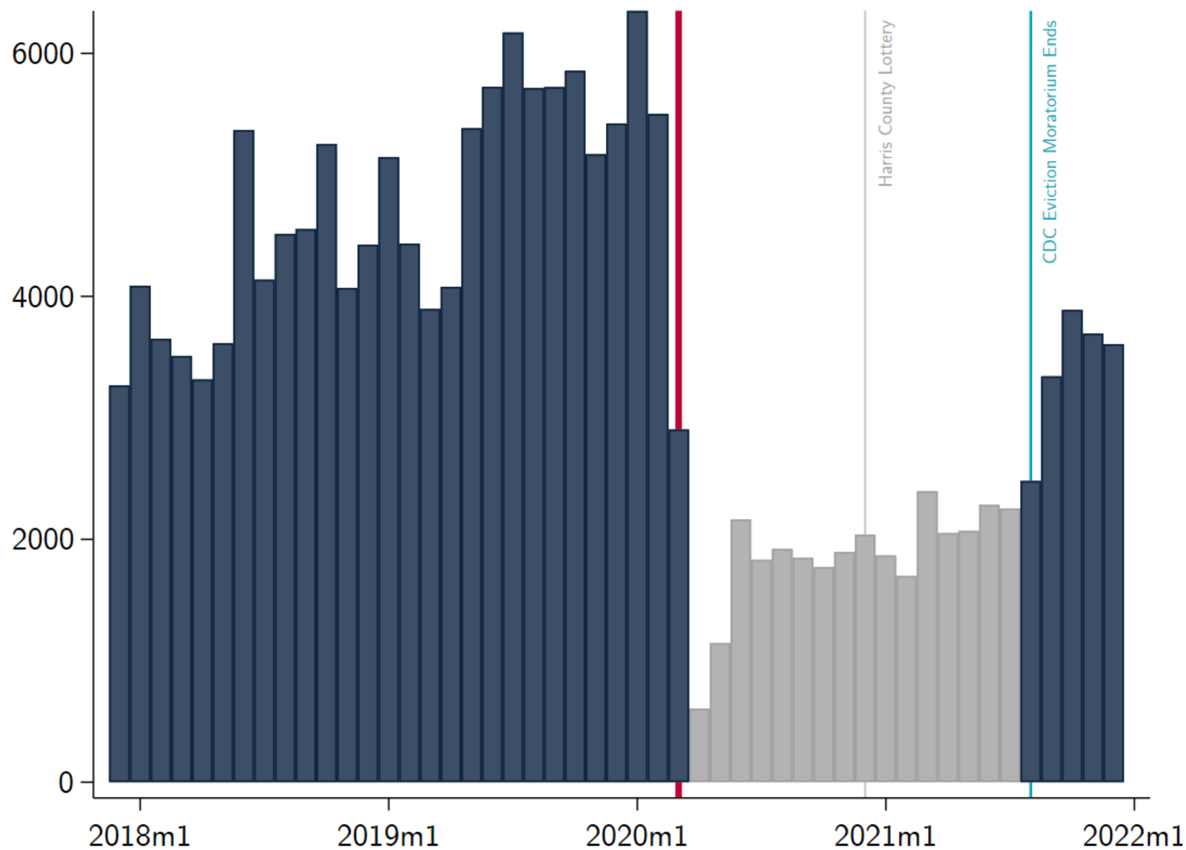
Notes: This figure plots the rate at which assistance applicants enroll in emergency shelters as tracked by HMIS data. The sample includes all lottery applicants in King County. Treatment and control groups are split by random assignment; the treatment group is further split by whether the assistance was successfully paid.

Appendix Table C.4
Average Credit Characteristics – Program Applicants vs Local Population

	Harris County			King County			Los Angeles			Chicago			
												DOH	TRP
	All	Renters	Assisted	All	Renters	Assisted	All	Renters	Assisted	All	Renters	Assisted	Assisted
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Credit Characteristics:													
Credit Score	707	684	566	748	725	584	727	710	649	712	691	613	658
	(94)	(94)	(78)	(75)	(80)	(85)	(84)	(85)	(1)	(95)	(97)	(93)	(89)
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	30.7	23.6
	(155.2)	(30.6)	(47.4)	(263.1)	(32.2)	(60.8)	(345.8)	(37.6)	(0.7)	(161.3)	(36.4)	(60.7)	(54.3)
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	1.226	0.419
	(2.637)	(2.285)	(4.299)	(1.220)	(1.166)	(3.451)	(2.033)	(2.356)	(0.038)	(1.794)	(1.575)	(2.346)	(1.311)
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	6.626	1.821
	(6.898)	(6.966)	(12.816)	(5.776)	(6.549)	(20.045)	(8.040)	(8.407)	(0.168)	(8.397)	(7.045)	(15.320)	(6.099)
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	0.305	0.214
	(0.490)	(0.479)	(0.459)	(0.450)	(0.439)	(0.477)	(0.476)	(0.467)	(0.007)	(0.446)	(0.433)	(0.461)	(0.410)
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	0.140	0.031
	(0.108)	(0.108)	(0.137)	(0.135)	(0.158)	(0.300)	(0.161)	(0.173)	(0.003)	(0.217)	(0.239)	(0.348)	(0.174)
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	0.599	0.583
	(0.360)	(0.395)	(0.469)	(0.256)	(0.291)	(0.500)	(0.316)	(0.336)	(0.007)	(0.332)	(0.359)	(0.491)	(0.493)
Observations:	26,086	14,803	6,169	14,408	7,358	491	24,579	16,603	8,702	14,399	9,200	357	640

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 percent sample of individuals from Equifax. "All" includes both renters and homeowners, and "Renters" is limited to individuals who rent. "Assisted" are the treated individuals who are linked to the 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 Figure C.5
Eviction Filing by Month: Harris County



Notes: This figure plots monthly eviction filing volumes in Harris County. Months with no eviction moratorium are in blue, months with an active moratorium are in gray. The red line denotes the start of the CARES Act mortorium, the gray line denotes the time of the Harris County lottery that we study. The teal line denotes the end of the CDC Eviction mortatorium. Data is the authors calculations based on administrative data from the Harris County Justice of the Peace.

Appendix Table C.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 based on administrative data from the Harris County Justice of the Peace. Column (1) reports the control mean and standard deviation (in parentheses), and column (2) reports 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 C.6

Treatment Effects on Homelessness System Use (King County and Chicago)

	King County Full Sample	King County Survey Respondents	Chicago DOH Full Sample	Chicago TRP Full Sample
Any Homelessness	0.020**	0.027*	-0.00213	-0.00346**
Services	(0.0083)	(0.016)	(0.00546)	(0.00172)
	[0.027]	[0.027]	[0.0092]	[0.00555]
	{0.048}	{0.047}	{0.0109}	{0.00449}
Emergency	0.0028	-0.000065	-0.00028	-0.00026
Shelter	(0.0039)	(0.0077)	(0.00153)	(0.00041)
	[0.0064]	[0.0081]	[0.000779]	[0.00853]
	{0.018}	{0.022}	{0.00242}	{0.00155}
Street	0.0067*	0.015**	0.00157	-0.0007*
Outreach	(0.0036)	(0.0068)	(0.00215)	(0.00041)
	[0.0037]	[0.0023]	[0.000602]	[0.00064]
	{0.0091}	{0.0098}	{0.00208}	{0.00139}
Diversion	0.0081*	0.0020	-0.0019	-0.00225
	(0.0045)	(0.0095)	(0.00502)	(0.00148)
	[0.0070]	[0.010]	[0.00782]	[0.00384]
	{0.019}	{0.017}	{0.00702}	{0.0017}
Any Longer-	0.0012	0.010	-0.00097***	-0.00065*
Term Subsidies	(0.0059)	(0.011)	(0.00018)	(0.00039)
	[0.015]	[0.013]	[0.000424]	[0.00064]
	{0.021}	{0.019}	{0.000656}	{0.00062}
Coordinated	0.00100	0.0013		
Entry	(0.0024)	(0.0038)		
	[0.0024]	[0.0023]		
	{0.0078}	{0.0070}		
N	12148	3152	74663	6453

Notes: This table reports the effects of assistance receipt on homelessness services use among King County and Chicago program applicants. Outcomes were measured 9 months after application based on 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. Columns (3) and (4) report the IV estimates of the effects of assistance for the Chicago DOH and TRP samples, respectively. Robust standard errors are reported in parentheses, and the control group means are reported in brackets. The 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 C.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 characteristic for all who filled out the survey in each site (includes treatment and control). We report reweighted means to adjust for survey nonresponse. All monetary values ("Monthly Rent" and "Monthly Income (Feb 2020)") are divided by 1,000. We report the maximum sample size for each site. Monthly income is household income in February 2020. Note that, for King County, the variable "Months of Rent Owed" comes from administrative payment data, not the survey.

Appendix Table C.8

Fraction of Participants Receiving Assistance in Future Rounds

	King County		Chicago DOH	
	Control	Treatment	Control	Treatment
	(1)	(2)	(3)	(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 groups is from the round one assignments. For Chicago DOH, these numbers include those who received assistance from round 2 of DOH or from TRP.

Appendix Table C.9
Comparison of Lotteries to Other Emergency Relief Assistance Programs by Site

	Chicago DOH (1)	Chicago TRP (2)	Houston (3)	LA (4)	Seattle (5)	CARES Act ERAP (6)	Treasury ERAP† (7)
Monthly Amount of Assistance	\$1,000	\$1,000	\$1,200	\$1,000-\$2,000	min(80% of rent or HUD FMR)	Median: \$1,200 Mean: \$1,700 Range: \$300 – \$3,300 (94%)	Max: \$17,000
Duration of Assistance	1 Month	1 Month	1 Month	1 Month	1-6 Months (Median:3)	≤3 Months (60%) 6–12 Months (30%)	Max: 15 Months
Direct to Tenant	Tenant	Tenant	Tenant	Tenant or Landlord	Landlord	Landlord (94%) Tenant (6%)	Tenant (71%)
Application Dates	Apr-20	Jul-21	Nov-20	Jul-20	Sept–Nov-20	Mar/Apr-20 (15%) May/Jun-20 (39%) Jul/Aug-20 (41%)‡	Jan/Mar-21 (ERA1) Mar-21/Jun-22 (ERA2)*
Dates of Assistance	Apr–Jun-20	Jul–Aug-21	Nov–Dec-20	Aug–Dec-20	Oct–Dec-20	First-come, first-served (45%) Lottery (16%)	First-come, first-served (56%) Lottery (3%)

Notes: The data for CARES Act ERAP come from Reina et al. (2021) and Yae et al. (2023) from the National Low Income Housing Coalition and the Housing Initiative at Penn. The sample of programs in this study includes 220 programs spanning 40 states (including Washington, DC). The percentages in parentheses represent the number of sites in the sample with the listed characteristics.

† The sample for Treasury ERAP consists of 389 programs across the US. For *Monthly Amount of Assistance*, we report the median of the maximum amount of assistance programs reported giving. Data for average monthly assistance across programs are currently unavailable. For *Duration of Assistance*, we also report the median of the maximum number of months programs reported providing assistance.

‡ These dates show the months when the programs first began accepting applications. These percentages are based on 179 of the 220 total programs. Only 1% of programs began in January of 2020, and 4% in September of 2020.

* The December 2020 Consolidated Appropriations Act of 2021 created the Treasury ERA Program, establishing ERA1. In March of 2021, Congress provided additional funds to the Treasury ERA Program through the American Rescue Plan Act, establishing ERA2. June 2022 was the last month for which states with ERA programs were required to provide monthly reporting to the treasury. ERA spending rapidly increased in the months leading up to the end of the federal eviction moratorium in August of 2021.

Appendix Table C.10
Rental Agreements with Landlords

	Harris County (1)	King County (2)	Chicago: DOH (3)	Chicago: TRP (4)
P(**Missed Payment** $\bigcap$ **Had an Agreement**)	0.304	0.487	0.316	0.286
P(**Had an Agreement**|**Missed Payment**)	0.721	0.724	0.735	0.880
Agreement with:				
Written agreement	0.273	0.277	0.223	0.133
Some rent forgiven	0.073	0.035	0.061	0.040
Had more time	0.741	0.331	0.556	0.715
Had payment plan	0.306	0.161	0.174	0.195
Had more time- late fee	0.306	0.045	0.116	0.074
Late fees waived	0.177	0.146	0.221	0.118
Other	0.086	0.061	0.069	0.059
N	1,675	2,626	14,681	1,352

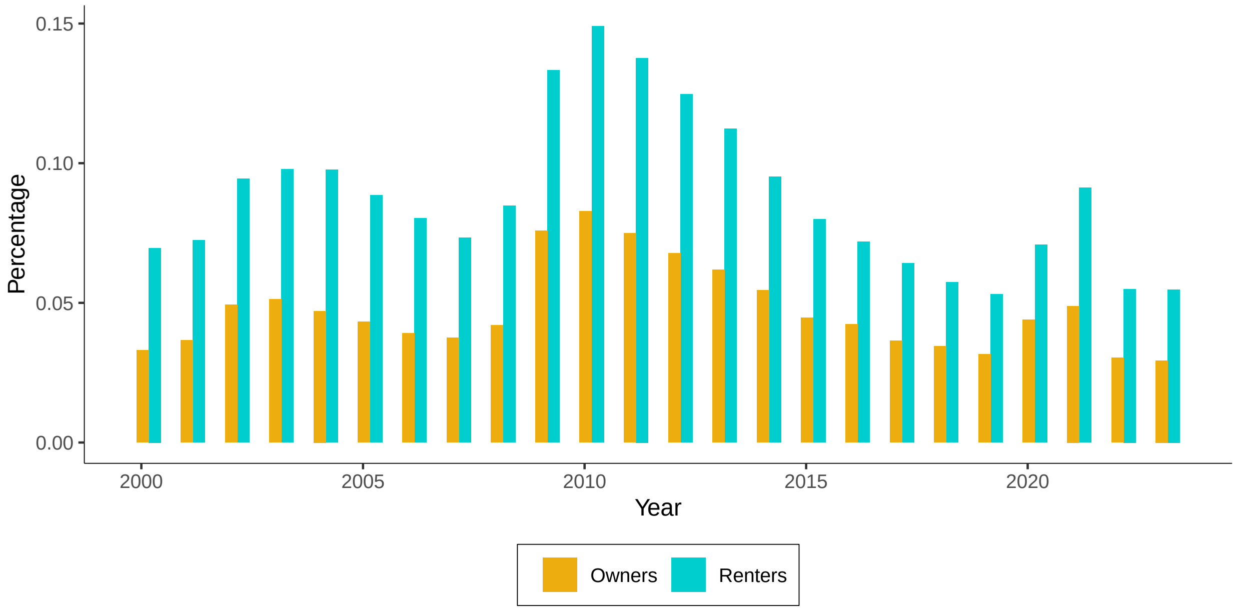
Notes: This table describes the types of agreements that tenants who missed rent payments reached with their landlords. These numbers are taken from the answers to Q38–Q40 in the Harris County and King County surveys. The first row is the fraction of respondents in each site who missed a payment and had an agreement with their landlord. The second row is the fraction among those respondents who missed a payment who had an agreement with their landlord. The rows that follow describe the content of these agreements. The categories listed are not mutually exclusive: respondents were instructed to select all features included in their agreement. These numbers should be interpreted as the fraction of respondents whose agreement included *at least* this feature.

Appendix Table C.11
Rates of Government Assistance

	King County (1)	Chicago: DOH (2)	Chicago: TRP (3)
Pre-Pandemic			
UI	0.117	0.041	0.014
Disability	0.030	0.018	0.013
Medicaid	0.168	0.196	0.137
Medicare	0.090	0.074	0.070
SNAP	0.293	0.348	0.339
WIC	0.091	0.062	0.057
TANF	0.059	0.013	0.005
Social Security	0.091	0.059	0.050
Community	0.035	–	–
Food Bank	0.166	0.048	0.078
Housing	–	0.043	0.016
Union	–	0.009	0.015
Post-Lottery			
UI	0.166	0.140	0.022
Disability	0.013	0.017	0.014
Medicaid	0.101	0.210	0.137
Medicare	0.047	0.080	0.078
SNAP	0.188	0.411	0.367
WIC	0.039	0.062	0.058
TANF	0.032	0.014	0.006
Social Security	0.045	0.059	0.054
Community	0.029	–	–
Food Bank	0.086	0.059	0.086
Housing	0.056	0.042	0.017
Union	–	0.015	0.015
N	2,626	14,681	1,352

Notes: This table reports the pooled estimates for the treatment and control groups of the rates of government assistance pre-pandemic and post-lottery for the different sites (based on survey responses). For King County, the data from the pre-pandemic period are from February 2020 and from August 2020 for the post-lottery period. For Chicago DOH, the pre-pandemic period is February 2020, and the post-lottery period is April 2020. For Chicago TRP, the pre-pandemic period is February 2020 and the post-lottery period is February 2021.

Appendix Figure C.6
Unemployment Rate by Tenure



Notes: This figure shows unemployment rates for renters and homeowners. Source: Current Population Survey Data Annual Social and Economic Supplements (CPS ASEC).