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

Robert Collinson
University of Notre Dame

Anthony A. DeFusco
University of Wisconsin–Madison

John Eric Humphries
Yale University

Benjamin J. Keys
University of Pennsylvania

David C. Phillips
University of Notre Dame

Vincent Reina
University of Pennsylvania

Patrick S. Turner
University of Notre Dame

Winnie van Dijk
Yale University

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Abstract

Short-term rental assistance expanded to unprecedented scale during the COVID-19 pandemic. We evaluate five programs distributing over $200 million through lottery, using administrative and survey data to assess effects on rent payment, housing stability, financial distress, and health. Assistance led to increases in rent payment and reduced concerns about eviction, with suggestive improvements in self-reported mental and physical health. In contrast with pre-pandemic emergency rental assistance, we find little effect on housing stability or financial distress. Explanations for these muted effects include: eviction moratoria weakening the link between rent and displacement, expanded safety net programs, and market softening favoring tenants.

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1 Introduction

The COVID-19 pandemic created new health risks and upended economic activity. The US government responded by rolling out policies to provide relief to households through tools used during other moments of crisis, including direct cash assistance ("Economic Impact Payments") and the expansion of traditional transfer programs (such as Unemployment Insurance benefits). However, unlike previous economic crises, policymakers also allocated an unprecedented amount of resources to rental assistance. Federal funding for Emergency Rental Assistance (ERA) totaled over $50 billion, representing an amount roughly equivalent to the annual budget for all federal rental assistance programs prior to the pandemic (Collinson et al., 2019).

Despite the unprecedented expansion of funding for ERA, there is little, if any, evidence available on its effectiveness as a relief mechanism during the pandemic. This lack of evidence is due to several factors. First, there was no centralized data collection on ERA program administration. Second, the ability to track outcomes such as rent payment, eviction, homelessness, and households' health and financial situations is limited by the absence of comprehensive national administrative data. Third, comparing assisted and unassisted individuals is likely affected by selection bias, calling into question any causal interpretation.

In normal economic times, theory and prior evidence suggest ERA may affect tenant outcomes through several channels. Payment of rent arrears should prevent eviction court proceedings, maintaining housing stability and avoiding the documented negative consequences of forced moves on credit, employment, and health (Collinson et al., 2024). By reducing financial strain, assistance may also improve mental health and allow households to meet other financial obligations. Indeed, pre-pandemic ERA programs providing similar assistance amounts have been shown to effectively prevent homelessness (Evans et al., 2016; Phillips and Sullivan, 2025).

However, the pandemic created unique policy and market conditions that could fundamentally alter the expected effects of rental assistance. Unprecedented expansions of unemployment insurance, direct cash payments, and other safety net programs may have diminished the marginal impact of additional rental assistance. Simultaneously, federal and local moratoria were placed on eviction for nonpayment of rent. These moratoria did not necessarily eliminate housing instability, as landlords could still pursue evictions for lease violations, decline lease renewals, or apply informal pressure. Nevertheless, they likely weakened the typical link between rental arrears and displacement, a primary mechanism through which ERA preserves housing stability. Finally, the pandemic also shifted rental market dynamics, with falling rents and rising vacancy rates potentially making landlords more accommodating, regardless of formal assistance programs.

In this paper, we provide the first empirical evidence on the causal impacts of ERA programs deployed in response to the pandemic, allowing us to test how these unique circumstances affected program effectiveness. We study five local ERA programs, each oversubscribed and selecting participants through a lottery, providing \$1,000 to \$3,400 to households in four major urban areas: Chicago, Harris County (Houston), King County (Seattle), and Los Angeles. Collectively, these programs distributed approximately \$200 million and received more than 200,000 applications for assistance. Figure I illustrates the backdrop against which they were implemented. Their timing coincided with the initial economic impact of the pandemic, the peak of reported rates of rental arrears, and the roll-out of other timely government relief initiatives. Because these local programs provided assistance well before a larger federal ERA funding round, they are well-suited for evaluating ERA as a crisis-response mechanism supplementing more traditional relief programs.

To learn about the causal impact of receiving assistance, we use exogenous variation from oversubscribed assistance lotteries in the five programs.[1] Using data on applications to these programs linked to survey and administrative data, we estimate their causal impact on policy-relevant outcomes related to housing stability, financial distress, and mental health. We use credit bureau records and consumer reference data to track the evolution of financial stability and housing outcomes. We augment these data with novel harmonized surveys to capture results that were not easily measured in administrative sources. For select sites, we also incorporate eviction court filings and homelessness system records to examine extreme housing instability.

Receipt of assistance increased rent payments and reduced self-reported anxiety. Using harmonized surveys fielded across four of the lotteries, we find that those who received assistance were 6–13 percentage points (8–36 percent) more likely to pay their rent in full in the months shortly 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 and on concerns about eviction. Receipt of assistance reduced self-reported anxiety by an average of 3.2 percentage points (6 percent) across sites. We additionally find that assistance reduced concern about eviction, with a 4.7 percentage point reduction (16 percent) across sites.

Despite the policy objective of keeping individuals and families in their homes, we find

[1] Our analysis follows a pre-specified plan. See AEA RCT Registry #0007731 and #0007167 and Appendix C for details.

no consistent effects of the assistance on the likelihood that applicants moved. We analyze applications to all five programs linked to two separate administrative sources to draw this conclusion. Using both a panel of linked credit data and address histories from consumer reference data, 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 fade 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.

Our last set of estimates examines whether receiving assistance decreased applicants' chances of facing severe 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 the likelihood of applicants appearing in the homelessness system.[2] We then investigate whether assistance reduced the probability of a new eviction filing in Chicago and Harris County, both shortly after assistance and after the national Centers for Disease Control (CDC) eviction moratorium was lifted. We find that the assistance had little effect on eviction filings and that baseline rates of new filings remained relatively low for applicants not assisted through the lottery.

While we estimate generally muted effects of ERA, we nevertheless find that it was 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 in the same community, those who received assistance had 33–141 points lower credit scores, higher collections balances, and higher levels of debt. Assisted households were also from higher-poverty neighborhoods and were more likely to be minorities. We reach similar conclusions when comparing those who received ERA assistance to those who received Economic Impact Payments (EIP).

Our results suggest that receipt of assistance was well targeted, helped households pay

[2] 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.

their rent in the short run, and led to modest improvements in concerns about eviction, with suggestive evidence that it improved mental and physical health. However, assistance did not substantially change applicants' housing or financial stability. Furthermore, when we investigate treatment effect heterogeneity across a wide range of individual characteristics, as well as across sites, we find little evidence to support heterogeneous effects of the assistance. These conclusions contrast with prior research on pre-pandemic ERA programs that provided similarly-sized assistance to renters facing household-level shocks. Evans et al. (2016) and Phillips and Sullivan (2025) find large effects on recipients' housing stability. This contrast is particularly striking given that the programs share similar design features and target populations.

What forces mitigated the effectiveness of pandemic ERA? We explore three potential channels: eviction moratoria, the expansion of other safety net programs, and equilibrium responses in the rental market. Eviction moratoria likely reduced the marginal value of assistance in preventing displacement. However, we find that concern about future eviction was elevated during the period when the ERA programs we study distributed their funds (see Figure I). Moreover, the fact that we find little heterogeneity across sites, despite differences in either the interpretation of the federal moratorium or the existence of extended local moratoriums, suggests that this cannot be the only explanation.

Next, we examine the role of an expansion in other forms of assistance. Our linked credit data reveal that individuals who applied for assistance experienced rising credit scores and falling debt in the lead-up to application (in line with Fulford, 2023). These patterns likely reflect a mix of broad-based improvements in macroeconomic conditions after the initial COVID shock and the effects of other expanded social insurance and safety-net programs during the pandemic.[3] The improvement in household balance sheets continues post-lottery but is similar for both treatment and control applicants, consistent with our findings of small causal impacts on the credit measures. Two additional pieces of evidence point to the importance of other safety-net expansions to understanding our small estimated impacts. First, lottery applicants with lower pre-existing attachments to the safety-net appear to experience larger reductions in housing instability from assistance than those already connected to the safety-net. Second, applicants to the Chicago TRP lottery, which sought to serve individuals with less access to other safety-net help (such as undocumented or mixed-status families), experience a statistically significant reduction in contact with the homelessness system.

Lastly, we consider equilibrium responses in the rental market. The early phase of the

[3]See, for example Blanchet et al. (2022), who observe that *"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."* See also Han et al. (2020) and Raphael and Schneider (2023).

pandemic saw sharp decreases in rent prices and increases in vacancy rates. 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 rents. Therefore, renegotiation or temporary agreements between landlords and tenants may be sufficient to forestall eviction without additional emergency assistance. Consistent with this mechanism, our surveys show very high rates of reported landlord agreements (over 70 percent) among tenants who are having trouble paying their rent. This evidence is in line with other pandemic-era surveys of landlords that find substantial increases in reported rental forbearance in 2020, relative to pre-pandemic levels (Decker, 2021; de la Campa and Reina, 2023).

Our paper contributes to a large literature studying the effects of various policies seeking to stabilize housing markets during 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 global financial crisis (Agarwal et al., 2017; Piskorski and Seru, 2018; Ganong and Noel, 2020; Agarwal et al., 2022). 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 renters reported significantly higher rates of missing housing payments than owner-occupants (15 percent compared to 9.5 percent) and higher risk of losing their home (7 percent compared to 2 percent) during the pandemic (U.S. Census Bureau, 2020c).

Other work has studied COVID-19 ERA, mostly focusing on the federal ERA program that was instituted near the tail end of the pandemic. Some of these studies have connected COVID-19 ERA to improvements in housing outcomes, financial well-being, and mental health (Airgood-Obrycki, 2022; Reina and Lee, 2023). However, these studies rely on descriptive comparisons of households that received assistance to those that did not, whereas we are able to leverage randomized receipt of assistance generated via lottery. Rafkin and Soltas (2024) find similarly small ERA impacts on eviction using event-study methods.

This paper also connects to the literature on housing instability and rental assistance. Prior work examines the effects of evictions (Collinson et al., 2024), landlord foreclosure (Diamond et al., 2019), and short-term housing assistance for recipients experiencing household-level shocks (Evans et al., 2016; Phillips and Sullivan, 2025). We provide the first rigorous evidence on the causal effects of ERA 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. Chetty et al. (2023) evaluate a range of other pandemic economic policies using nearly real-time data on spending from private sector sources. Ganong et al. (2022) examine the impacts of pandemic unemployment insurance on spending and job-finding, while Karger and Rajan (2021) study the effect of Economic Impact Payments on spending.[4] While past work has sought to estimate the impact of the eviction moratoria 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 impact of a key part of the immediate economic response across four large urban areas. In contrast to the existing work, which uses county-level data, we also use our individual- and household-level data to study how renters' financial circumstances developed throughout the pandemic.

Finally, our work relates to recent studies of pandemic cash assistance. Jaroszewicz et al. (2022) and Pilkauskas et al. (2022) examine one-time cash payments ($500-$2,000 and $1,000, respectively) and find minimal effects on spending, material hardship, and mental health. We study rental assistance—a major component of pandemic relief—across four metropolitan areas with larger samples. While we find similar limited effects on bill payment and financial strain, we do find some evidence of mental health improvements, in contrast to these studies.

2 Background

2.1 Program Details

We study five emergency rental assistance lotteries administered in four locations during 2020. The programs launched at different points during the pandemic's first year: Chicago's Department of Housing (DOH) program operated earliest (applications March 27-April 5, 2020), followed by Los Angeles (July 13-17), The Resurrection Project (TRP) in Chicago (applications opened June 22), King County (August 20-December 4), and Harris County (applications closed November 5).

Most programs provided similar amounts of assistance: Chicago DOH and TRP each distributed $1,000 grants paid directly to tenants, Harris County provided $1,200 in direct cash assistance, and Los Angeles offered $2,000 per household distributed either directly to the tenant or their landlord. King County was notably more generous, providing an average of three months' back rent paid to landlords (with a maximum of six months), though landlords had to agree to substantial conditions including forgiving rental debt beyond six months and not raising rent through March 2022.

[4] Recent studies have also 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).

Income eligibility requirements varied across sites. Chicago DOH and Harris County limited eligibility to households earning no more than 60 percent of area median income (AMI), King County set a stricter threshold of 50 percent AMI, and Los Angeles allowed incomes up to 80 percent AMI. Chicago TRP was distinctive in explicitly targeting populations ineligible for CARES Act assistance: while nominally open to all Chicago residents with incomes below 300 percent of the federal poverty line, applications from undocumented individuals, mixed-status families, dependent adults, and returning residents were particularly encouraged. This focus on reaching undocumented populations represents a key contrast with the other programs we study.

Complete program details, including application procedures and lottery mechanisms, are provided in Appendix A.

2.2 Policy Context and Program Timing

Figure I illustrates the timing of the five programs we study relative to the broader macroeconomic and policy environment of the pandemic. Panel A shows that unemployment spiked dramatically in early 2020, reaching nearly 15 percent before gradually declining. Panel B documents the impact of federal relief initiatives on disposable household income for the bottom half of the income distribution, comparing income before and after taxes and transfers. While factor income declined sharply during the pandemic, post-tax disposable income—which includes Economic Impact Payments, Child Tax Credit, Earned Income Tax Credit, and expanded Unemployment Insurance benefits—actually increased substantially for lower-income households. These federal transfers were implemented around the same time as the programs we study.

The federal government also provided funding specifically for emergency rental assistance, but in a later phase of the pandemic. As Figure I makes clear, all five programs we study distributed assistance well before the large federal ERA1 and ERA2 funding rounds began in mid-2021. Given these differences in timing, as well as differences in implementation and assistance amounts, our results are not necessarily representative of the impact of the federal ERA programs. However, the lotteries we study provide arguably the best available setting for evaluating ERA as a crisis-response mechanism during the most immediate and highest-intensity economic impact of the pandemic. Panel C shows the scale of renter distress during this period: approximately 10 to 13 million households—representing 23-30 percent of all renter households—reported being behind on rent when these programs were rolled out, according to Census Household Pulse survey data. This number fell sharply to just above 7 million households (approximately 16 percent) by spring 2021 and never again reached the peak levels seen during the programs' implementation period.[5] Panel D documents how eviction filings varied across our study sites during this time.

National and local eviction moratoria also shaped the environment in which these programs operated, though their practical impact varied considerably. The CARES Act instituted a 120-day national moratorium on eviction filings for properties with federal financing (March 27-July 24, 2020), followed by a broader CDC moratorium (September 4, 2020-July 31, 2021, with a short second moratorium August 3-26, 2021, that the Supreme Court struck down). These national moratoria covered nonpayment evictions but allowed evictions for other causes. Their implementation also differed across locations due to inconsistent judicial interpretation. Harris County proved particularly notable for weak enforcement. Despite the CDC moratorium, the county averaged over 500 weekly eviction filings during this period, approximately 60 percent of pre-pandemic volume, amid widespread concerns that local judges were not honoring federal protections.

All four study sites also enacted local moratoria, but their strength and duration varied: Los Angeles maintained its moratorium through June 2022, Chicago and the state of Washington extended protections through October 2021 (with Seattle and other Washington cities extending to January 2022), while Harris County's brief state moratorium (March-May 2020) ended early.

3 Data

We link administrative records from each of the five programs to five types of data sources. This section provides details on each of these data sources.

3.1 Credit Bureau Data

Our analysis of financial distress 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. They include information on credit scores, debt

[5] Figure I includes three related measures of renter households' distress in Panel C: the number of households behind on rent, those unconfident in their ability to pay next month's rent, and those in rental arrears reporting themselves at risk of eviction. The data show lower levels but very similar trends for the latter two measures. All three measures strongly suggest that tenants had the most trouble paying rent in the early phase of the pandemic.

levels and delinquency status for all major forms of consumer debt.[6] In addition to these credit variables, the data also include the ZIP code of residence at the time of each snapshot, 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 to 81 percent for King County. Conditional on being observed in the credit data, nearly all applicants are present in every monthly snapshot. Our analysis therefore uses a balanced panel of credit outcomes.

3.2 Consumer Reference Data

As an alternative way to measure address changes, we use consumer reference data from Infutor Data Solutions. Infutor combines various information from many sources (e.g., phone bills, voter files, magazine subscriptions, and property deeds) 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; Asquith et al., 2023; Collinson et al., 2024). The Infutor data are well-suited for our study because they provide a consistent measure of housing stability 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 residential mobility in similar high-risk populations; for example, address changes measured in 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).

[6]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, except for federal student loans, which were reported as current while payments were paused (*Household Debit and Credit Report*, 2023).

We measure address changes among the people we are able to match to the Infutor data. 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 in the application records 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 when applying for assistance and measure whether Infutor later observes this person at a different address.

3.3 Survey Data

In addition to administrative data, our analysis uses surveys administered in the months following treatment from four of the five programs studied. The surveys carried out in Chicago, Harris County, and King County relied on largely harmonized survey instruments, with questions that were similar but not identical across sites.[7] When processing the survey data, we construct outcomes to align as closely as possible across sites. However, the survey questions used in Los Angeles were substantially different from the other sites. For this reason, we report the results for Los Angeles separately in the appendix. Appendix Section B provides a detailed discussion of how our survey-based outcomes are constructed.[8]

The amount of time that elapsed between assistance and the administration of the survey also varied across sites. Appendix Table E.2 reports the survey dates for each site. The surveys were administered one to six months after assistance was received. The Chicago DOH survey was administered the month after treatment, while other sites, such as Harris County, administered their surveys four to five months after assistance was received. For some of the survey outcome variables, retrospective questions were used to improve alignment across sites so as to account for differences in the elapsed time between the lottery and the completion of the survey.

The bottom panel of Table I reports survey response rates at each site. Survey response rates vary between 13 and 40 percent. For most sites, individuals who were offered assistance are more likely to respond to surveys than the those who were not. Appendix Table E.5

[7] This research began as three independent projects. Site-specific surveys were initially developed independently and in collaboration with partner organizations. The research teams eventually shared the survey instruments and, to the extent possible, aligned them. The full survey instruments can be found at https://github.com/robcollinson/covid_era_surveys/.

[8] Even among the sites where survey questions were largely aligned, one notable exception is 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.

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 baseline characteristics observed for all applicants (Wooldridge, 2007).

3.4 Evictions Data

We supplement the data sources above with eviction data from Harris County and Cook County. These data, and the homelessness data described in the next subsection, 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 2019–2023. 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 D.

Eviction court records for Cook County were shared by the Cook County Circuit Court and supplemented with data from Record Information Services. Combined, the data span the years 2019-2024. The data contain filing date, name, and address, and include filings that were sealed from the public record as part of the state's pandemic-related tenant protection measures. Appendix D provides details on the linkage process. A limitation of both eviction datasets is that we can link only to evictions occurring *at the application address*, which undercounts total exposure to eviction.

3.5 Homelessness Data

We measure homeless services use in King County and Chicago using local Homeless Management Information System (HMIS) data. These databases systematically record the use of homeless programs in a community, following the guidelines of the US Department of Housing and Urban Development.

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 program types offered only to people who are already homeless (e.g., we do not include homelessness prevention programs) so that program use indicates that the individual both receives services and is homeless at the point of program entry.[9] The existing literature uses similar measures to

[9] The categories of programs we consider include emergency shelter, street outreach for people living unsheltered, coordinated entry (entries for requesting but not yet accessing oversubscribed services), diversion

evaluate the impact of rental assistance and eviction on homelessness (Evans et al., 2016; Phillips and Sullivan, 2024, 2025; Collinson et al., 2024).

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 Chicago were constructed to align with HUD project types, except diversion services, which are only available in the King County HMIS.

4 Who Were the Program Recipients?

4.1 Lottery Applicants

Table I provides summary statistics for each of the program sites. We report means separately for applicants who were offered assistance ($Z = 1$) and those who were not ($Z = 0$). The table summarizes applicant characteristics, which we group into three broad areas. The first panel uses the complete lottery sample and provides information on demographics (or imputed demographics) derived from the applications for assistance.[10] The second 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.

Applicants were, on average, 39 to 41 years old, and 54-64 percent of applicants were female. The racial composition differs across lotteries. Black individuals account for over one-half of applications in Harris County, and around a third in King County and Chicago DOH. For Los Angeles and Chicago TRP, over 50 and 80 percent of applicants are Hispanic, respectively. The median rent in applicants' ZIP codes is between \$1,000 and \$1,500 across sites, and poverty rates range from 11 to 21 percent. The average credit scores are low across sites, ranging from 550 to 650, and applicants have relatively large balances in delinquency or collections. Overall, the racial compositions differ across lotteries; otherwise, applicants across sites are broadly similar.

Figure III extends the descriptive statistics from Table I to plot the evolution of several key measures of financial distress: 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 applicants who were and were not offered

programs provided to people who have just become homeless, transitional housing, permanent supportive housing, and rapid rehousing.

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

assistance.[11] Means are plotted for all months with available credit reporting data. 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).[12]

Three features of this figure stand out. First, the balance between applicants who were and were not offered assistance, which is documented in Table I for January 2020, persists across all pretreatment months. The gap is somewhat larger in King County, where assistance was randomly assigned but we only observe credit records for survey respondents, as described in Section 3.1. Second, the overall trend for applicants across all sites is one of generally improving financial distress. Credit scores trend upward throughout the sample period, while delinquent balances and utilities collections trend downward. This finding is consistent with evidence from other work showing that various measures of financial well-being improved for most households during the early pandemic (Han et al., 2020). Third, there is no meaningful divergence in outcomes between applicants who were and were not offered assistance either at the onset of the pandemic or in the months following assistance. This finding, which foreshadows our main results, is consistent with the idea that the emergency assistance grants we study did not have meaningful effects on financial distress.

4.2 Did ERA Programs Target Those in Need?

As noted in Section 2, the ERA programs required proof of eligibility, including being below income limits and an inability to pay rent or pandemic-related income loss. These criteria may have led to the programs being accessed by lower-income or more financially strained households compared to other pandemic-relief initiatives. To assess the extent to which ERA programs targeted those in greater need, we first compare program recipients with other renters in the same cities. Then, we compare ERA recipients to those who received federal Economic Impact Payments (EIP), which were distributed around the same time.

Table II compares the characteristics of those who received assistance to the characteristics of all renters who live within the relevant city or county. We compare demographic characteristics (top panel), ZIP code characteristics (middle panel), and credit characteristics (bottom panel). The first two panels use data on renters in the ACS in 2019.[13] The final

[11] We report standard errors for each mean separately rather than their difference to provide a sense of the overall variability in the data. Formal tests for differences in means between applicants who were and were not offered assistance are discussed in Section 6.

[12] 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.

[13] The first panel uses the 2019 ACS public-use micro-data and is a subset of all renters in each area. The second panel uses aggregate ZIP code (ZCTA) characteristics from the 5-year 2015-2019 ACS, where ZIP-level characteristics are weighted by the number of renters in the ZIP or the number of applicants in

panel uses data from a 10 percent random sample of individuals from Equifax. We restrict to renters who live within one of the catchment areas for the programs in the months the lotteries took place. We focus on credit scores (VantageScore 4.0), balance in delinquent accounts, and balance in collections, as these outcomes can be best aligned between the Equifax and Experian data.

Across all sites, those assisted are less likely to be White, and more likely to be Black or Hispanic than renters on average. Those assisted are also from ZIP codes with higher poverty rates, a lower share of White residents, and moderately lower median rent. Finally, those assisted have notably lower credit scores, higher balances in collections or delinquency, and are less likely to have a revolving line of credit (e.g., a credit card).

We further compare trends in the financial characteristics of those assisted compared to all renters using the panel nature of the Equifax and Experian data. Appendix Figure E.6 plots the outcome trajectories for applicants who were and were not offered assistance and the random samples of renters over time relative to the treatment month for all five programs. Across all sites, we see large gaps between the random sample of renters and those who participated in the lottery. For example, in Harris and King Counties, the credit score gap 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 have low average 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 both applicant groups have rising credit scores and flat or decreasing balances over the whole time period.

Our second benchmark to evaluate the targeting properties of the ERA programs are the recipients of the first round of federal Economic Impact Payments – cash checks provided under the CARES Act. Figure II provides a comparison between recipients of the ERA programs we study and EIP Round 1 recipients, based on ZIP code-level poverty rates.[14]

the ZIP.

[14]We make comparisons based on ZIP code-level characteristics because we observe ZIP codes for all recipients in the programs we study, and counts of EIP recipients by ZIP code are available from the Internal Revenue Service Statistics of Income (Internal Revenue Service, 2020, 2021). We focus on EIP round 1 recipients because recipients in rounds 2 and 3 have nearly identical ZIP-level characteristics, as can be seen

Economic Impact Payments were the federal government's most immediate response to the economic impact of the pandemic and the first round consisted of checks in the amount of \$1,200 per adult plus \$500 per qualifying child under age 17, sent to individuals with less than \$75,000 (\$150,000 for married couples filing a joint return) in income reported on their 2018 or 2019 federal tax returns. Payment amounts were reduced for eligible individuals with higher adjusted gross income. Across all sites, ERA recipients live in ZIP codes with higher poverty rates as compared to EIP recipients.[15]

Appendix Figures E.1 and E.2 make similar comparisons based on ZIP-level minority and Hispanic population shares for ERA recipients and EIP recipients. With the exception of Los Angeles, all sites distributed assistance to recipients in neighborhoods with higher minority shares. As expected given that it was designed to target undocumented immigrants, recipients in the Chicago TRP program were substantially more likely to be from neighborhoods with high Hispanic shares. The differences between ERA and EIP recipient pools could be the result of various factors, including the automatic transfer of the EIP payments versus the opt-in procedure for ERA programs, documentation requirements for ERA versus selection based on past tax returns for EIP, or the fact that ERA was more explicitly aimed towards renters, while the EIP payments did not distinguish among homeowners and renters.

Overall, the evidence suggests that the ERA programs targeted renters who were substantially more financially distressed, and who were living in higher-poverty and higher-minority share neighborhoods, relative to renters not assisted by the programs. These findings are in line with the results in Figinski et al. (2024), who find that the federal ERAP payments that were implemented in a later phase of the pandemic (see Figure I) disproportionately went to Census tracts with more disadvantaged renters.

5 Using Lotteries to Study the Impact of ERA

5.1 Regression Specifications

Our empirical analysis aims to estimate the causal effects of receiving ERA on various 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

in Appendix Figures E.3 and E.4.

[15] Karger and Rajan (2021) provide evidence that those with lower income and less savings had higher marginal propensities to consume from the EIP payments. This suggests that ERA may have targeted those with higher marginal propensities to consume.

assistance is typically endogenous. Households that do not take up the assistance may differ along dimensions unobservable to the econometrician. For example, they might have more tumultuous lives, making it difficult 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 or have access to other financial support.

We address these challenges by estimating treatment effects using random variation in *offers* of 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 offered assistance (which we will refer to as "intent-to-treat effect," or ITT), we estimate simple linear regressions of the following form:

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

where Y is an outcome of interest, such as credit score, γ_1 is the intent-to-treat effect, Z is an indicator for whether the applicant was selected by lottery to receive an assistance offer, and $\mathbf{X}$ is a set of control variables.[16]

As discussed in Appendix A, several jurisdictions administered their lotteries in a way that resulted in offers that were random conditional on observable information, such as time of application or Census tract. We include controls for these "site-specific design features" in $\mathbf{X}$ in equation (1). 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. To increase precision, we additionally control for individual-level 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.[17]

Not everyone initially offered funding actually received it. There are several reasons those selected for funding may not have received it, such as program administrators not being able to contact the applicant, the applicant being determined ineligible, or the applicant failing to complete the required forms. It is also possible that applicants not originally selected for assistance via the lottery nonetheless ended up receiving it. To account for both directions of imperfect take-up, our analysis focuses on instrumental variables estimates. We calculate these estimates using two-stage least squares (TSLS) regressions, with the first and second

[16]Below, we use the terminology "offered treatment" and "selected for an offer of treatment" interchangeably, even though not all applicants selected through the lottery were offered assistance, for example due to difficulties contacting the applicant.

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

stages given by:

$$R = \alpha_0 + \alpha_1 Z + \mathbf{X}'\delta + V \tag{2}$$

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

where R is an indicator variable for whether an applicant received assistance, and all other variables are as previously defined. The coefficient β_1 is our parameter of interest, and represents a local average treatment effect (LATE) of assistance receipt if the standard IV assumptions (Imbens and Angrist, 1994) are satisfied and (2)–(3) are correctly specified (Blandhol et al., 2022).

Table III reports estimates of α_1 from the first-stage regression for each site. The table also reports the partial F-statistic on that coefficient, the control-group mean, and the share offered assistance. For each site, we observe a large and statistically significant first stage (with the F statistics ranging between 2,442 and 47,977). The size of the first-stage coefficients differ as there were differential take-up rates across sites. Some of these differences come from 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 process was easy to complete, and candidates were screened more rigorously upon being selected. Programs 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 payments directly to the household applying for assistance.

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

5.2 Balance Tests

We check whether the data is consistent with randomization of offers of assistance by program administrators. Table I shows the results of tests for balance on observable characteristics between the groups offered and not offered assistance. For each site, we report the difference between these group means ("Diff"), adjusted using an ordinary least squares (OLS) regression of equation (1) that controls for any site-specific design features (see Section 5.1).

Overall, the applicants selected for assistance are observably similar at baseline to applicants not selected on nearly all variables. Across the demographic and credit characteristics, the two groups are generally balanced. Where there are some statistically significant differences between the two groups' populations, the differences are quantitatively small. Credit report characteristics in King County show somewhat larger gaps between the two groups. This is likely the result of the fact that we can only access Experian records in King County for people who respond to the survey, as discussed in Section 3. This sample selection generates the somewhat larger baseline differences in attributes measured in credit reports, the largest difference in credit scores across sites, 8.5 points. Reassuringly, the top two panels of Table I use the full lottery sample for King County and show balance, suggesting that the imbalance in Experian data is indeed due to selection into the survey in King County. Appendix Table E.1 reports balance for additional baseline applicant characteristics available in Los Angeles, but not the other sites.

For most sites, survey response rates were not equal across the groups offered and not offered assistance. To address potential bias due to differential selection into responding to the survey, 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, we first estimate site-specific logit regressions of whether an applicant responded to the survey, using a dummy for being offered assistance (Z) and a host of baseline control variables. For all sites except King County, these controls include 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 if they had not been offered assistance (i.e., setting $Z = 0$) and use the inverse of this predicted probability as the weight in our regressions. As discussed in Section 6.1, we obtain similar results with and without these weights.

6 Results

In this section, we report estimates of treatment effects from receiving assistance on outcomes measured in our surveys, financial distress measures observed in the administrative data from Experian, residential mobility as measured in both the Infutor and Experian data, and

[18]For Harris County, the controls 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 both Chicago lotteries, they include age and indicators for race, gender, missing age, and if the applicant applied to the lottery more than once. 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.

eviction and homelessness data as measured from court records and local homeless services administrative systems. We report both site-specific estimates and equal-weighted averages across sites.

6.1 Impacts on Survey Outcomes

Table IV reports IV estimates of the effect of assistance receipt on survey measures of rent and bill payment, mental and physical health, material hardship, and feelings of economic insecurity. Most of our survey questions ask about the month prior to survey collection. However, the questions about rent payment ask about specific months following the month of the lottery.[19] Columns (1)–(4) report estimates separately by location, and column (5) combines estimates across sites. Control group means (i.e., means for the group not selected for assistance) are reported in brackets. While Table IV reports estimates from the weighted regressions that adjust for survey nonresponse, the unweighted results are qualitatively similar (Appendix Table E.3).

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 (both DOH and TRP), recipients are 5.8–13.4 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 8.2 percentage points, a 15 percent increase over the control group mean. The effects of assistance on other types of bill payment appear more mixed. Across the four lotteries, we find no evidence of impacts of the assistance on other bill payments. Due to the substantive differences in the questions asked in the Los Angeles survey, we report these estimates separately in Appendix Table E.4.[20]

Second, recipients are less likely to report feeling worried about being evicted (a 4.7 percentage-point decrease, or 16 percent). The point estimates for experiencing homelessness (self-reported) are consistently negative, but none are statistically significant. The combined estimate for staying in a homeless shelter is not statistically significant overall. However, we do find a statistically significant decrease in self-reported sheltered homelessness in Harris County (where the CDC moratorium was not as stringently enforced). The other sites have a mixture of negative and positive point estimates which are not significant.[21] We also find no statistically significant evidence of reduced food insecurity.

[19] See Appendix B for details about the survey questions across each site.

[20] The effects are noticeably different for Los Angeles, likely due to difference in the wording of the survey question, which focused on cumulative rent payment. At the same time, there is some indication in LA that receipt of assistance resulted in increased payment of bills other than rent, though the estimates are somewhat imprecise.

[21] We consider impacts on homelessness as measured by administrative records in Section 6.4.

Finally, there is suggestive evidence of improved physical and mental health among recipients. Recipients in the Chicago DOH lottery experienced a 5.1 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 are of similar magnitude, albeit measured with less precision. Across all three sites, we estimate a marginally significant 5.6 percentage point reduction. Similarly, recipients are less likely to have reported feelings of anxiety or depression, a 3.2-percentage-point (6.3 percent) decrease across Harris County, King County, and Chicago, which is marginally significant.

Overall, the results based on the survey data suggest that recipients were more likely to pay rent and to be less worried about being evicted. The point estimates for staying in shelter also suggest reductions in housing instability, but are generally not significant. Lastly, we find suggestive evidence that assistance modestly improved recipients' physical and mental health.

6.2 Impacts on Credit Measures

Next, we consider how financial assistance affected financial health using panel data from Experian. Figure IV depicts reduced-form (ITT) and IV estimates, constructed as described in Section 5.1, for a subset of our credit outcomes. Each column reports effects on a different outcome: credit score (left column), balance in delinquent accounts (middle column), and balance in utilities collections (right column). The reduced-form (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 V 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).

6.3 Impacts on Residential Mobility

We find no consistent evidence that residential mobility changed as a result of receiving emergency rental assistance. Table VI 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 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).

6.4 Impacts on Eviction and Homelessness

Next, we investigate whether the assistance reduced other measures of acute housing instability, including evictions and homelessness. We are not able to track these outcomes across all sites, 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 Chicago and Harris County.

In Table VII, 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 (2025) 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 E.7. This finding 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 decline is a small absolute reduction but a large relative reduction of 65 percent since homelessness system interactions are rare 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, 2025). 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 (2025), and Phillips and Sullivan (2024), 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 (2024) were operated by the same agency, are evaluated using 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 (2024). Homelessness service usage differs slightly between the two samples, at 4.8 and 7.9 percent, respectively, but both are 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.

In contrast to King County and Chicago DOH, 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, 2025), 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 Economic Impact Payments, either because they were undocumented immigrants or lacked formal sector employment. In Appendix Table E.12, 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, to examine the impacts on eviction filing, we link the sample of Harris County and Chicago assistance applicants to eviction court records.[22] As detailed in Appendix A, the interpretation of the CDC moratorium varied by local courts, as did the presence of extended state or local moratoriums. In Harris County, judges frequently permitted eviction non-payment cases to proceed during the federal moratorium when tenants failed to file a Covid hardship declaration form. Additionally, cases for reasons other than non-payment were generally permitted to continue as usual (Schuetz, 2021). However, formal enforcement of eviction orders by law enforcement was stayed during the moratorium. In Chicago, adherence to the CDC moratorium was stronger, and both a statewide moratorium and a city ordinance extended protections several months beyond the CDC moratorium.

Table VIII reports the effects of receipt of assistance on eviction filing (initiation of a new case), which we observe in Harris County and Chicago (both DOH and TRP). One limitation of this analysis is that we can only consider evictions occurring at the same address listed on the application for assistance. Due to the aforementioned differences across courts in eviction activity during this period, and the variation over time in the presence of the federal moratorium, we focus on evaluating the effects of assistance separately by lottery, as well as during the CDC moratorium and after it was lifted. The top panel considers 2SLS on any eviction filing within two, six, and ten months after the lottery (i.e. during the moratorium). The bottom panel focuses on impacts on eviction filings two, six, and ten months after the

[22]This linkage is described in Appendix D.

CDC moratorium was lifted. The first column for each lottery reports the control mean. As expected, the baseline means during the moratorium are quite low, especially in Chicago.[23] After the moratorium, baseline means remain low, potentially partly due to applicants no longer living at the same address, but are several times higher than during the moratorium.

The second column for each site in Table VIII reports the IV estimates for the effects of the assistance. For Harris County, we find no statistically significant effects, with small negative point estimates in all periods. For Chicago DOH, we find some suggestive evidence of reduced evictions. During the moratorium, we find a reduction two months after the lottery. Yet, the control mean (0.0004) and treatment effect (-0.0004) are so small that this is based only on an extremely small difference in filings and should be interpreted cautiously. After the moratorium the estimates for Chicago DOH are also negative, with a weakly statistically significant reduction of -0.003 after two months, off a baseline mean of 0.005. The estimates for Chicago TRP are all statistically insignificant and small. In summary, the results are consistent with a small reduction in eviction filings during and after the moratorium, but the estimates are quantitatively small.

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.

6.5 Heterogeneity and Mechanisms

The modest overall effects may mask larger effects across some subgroups. Mechanisms that might drive those modest results (such as the presence of other generous federal benefits, a weak housing market that allows for negotiation with landlords, or eviction moratoria) also imply possible heterogeneous effects. Here, we test for heterogeneity across both subgroups and sites.

We find little subgroup heterogeneity in outcomes measured by credit reports. Appendix Figure F.1 shows credit report outcomes, dividing 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. All these figures plot standardized IV estimates for the

[23] In Panel D of Figure I we plot monthly eviction filings for all four sites. In Harris County, filing volumes drop with the onset of COVID-19 and the CARES Act Moratorium. Filings remain about one-third of pre-pandemic levels (though still average about 2,000 cases per month) through the CDC moratorium. In contrast, the number of filings in Chicago (and King County) remain lower during the CDC moratorium.

effects of assistance.[24] Overall, there are few meaningful differences between the subgroups.

We also find little heterogeneity in the survey results. While we lack baseline surveys, we use retrospective survey questions about pre-lottery conditions to construct 12 subgroups capturing different economic circumstances, housing situations, and health conditions. In particular, we examine effects separately by the following pre-lottery subgroups: high-/low-income, some/no safety-net utilization, good/poor mental health, stable/unstable employment, current/behind on rent, and moved vs not in the past year. Due to the variation in the sample size of some subgroups between sites, we inverse-variance weight the estimates from individual locations when combining point estimates across sites.[25] The results of the heterogeneity of the survey appear in table form in Appendix Tables F.2 and F.1, and as forest plots in Appendix Figures F.2 and F.3. The effects on rent payment and bill payment in Appendix Figure F.1 appear to vary relatively little across subgroups. There are slightly larger point estimates for rent payment among those not stably employed and those those who report being behind on their rent in the past. The health effects also appear mostly stable across subgroup. We find some evidence that reductions in anxious feelings are larger for individuals who report more economic precarity before the lottery, particularly those who report being behind on rent before the lottery, and those not stably employed. Overall, however, we observe little heterogeneity.

One possible exception to this lack of individual-level, within-site, heterogeneity is pre-lottery, self-reported safety net utilization. This particular sample split is of interest because people with limited pre-lottery experience with the safety net probably have less access to substitutes for ERA.[26] In Appendix Table F.2 we report the impacts of assistance on economic security measures from Table IV separately for those who report some pre-lottery safety-net utilization versus those who report no safety-net program receipt. We consistently find larger point estimates for the applicants who report not receiving other safety-net help prior to the lottery. For example, those with no reported baseline safety-net access experience a 3.9 percentage point reduction in reported homelessness, which is statistically significant. Meanwhile, the effect for those reporting pre-existing safety-net access is an estimated zero. Effects appear similarly larger for worries about eviction, stays in shelters, and food insecurity, though in most cases we are not able to formally reject equality. Overall, while only suggestive, this evidence is consistent with the idea that ERA was more effective among

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

[25] We show in Appendix Tables F.3 and F.4 that the point estimates are quite similar to the equally-weighted estimates, but are considerably more precise.

[26] While there are conceptual reasons to think this group might have different effects, we note that this was *not* a pre-registered subgroup, so we encourage caution in interpreting the results.

people with less access to other parts of the safety net.

One strength of this study is the ability to observe treatment effects of ERA across four metropolitan areas that varied in how they implemented both ERA and other relevant pandemic-era policies, like eviction moratoria. Some cross-site results, like stronger effects on housing stability among primarily undocumented applicants in the Chicago TRP lottery, suggest that such comparisons may be informative about mechanisms. So, in addition to heterogeneity on observables, we formally test for heterogeneous treatment effects across sites. Using tools from the meta-analysis literature, we consider a random-effects model where site-specific estimates may differ from one another due to sampling variability and differences in the true impacts across sites (DerSimonian and Laird, 1986; Jackson and Mackevicius, 2024). In Appendix F.2, we describe the implementation and present the results. Overall, we find limited evidence of cross-site heterogeneity. The strongest evidence is for financial outcomes 10 months later, where we reject the null of homogeneity across sites for credit scores and for an indicator for any personal bankruptcy. Yet, overall, we rarely reject the null of no site-level heterogeneity. These null results are consistent with the idea that the modest effects of ERA that we measure are due to national context shared by all sites, like the availability of other federal assistance or a slack housing market, rather than features that vary more across sites, like ERA program design or the strictness of eviction moratoria. However, with only five lotteries and four locations, these tests have limited statistical power.

7 Discussion and Concluding Remarks

This paper studies the unprecedented expansion of emergency rental assistance in response to the economic disruption caused by the COVID-19 pandemic. Using application data from five program lotteries conducted in four large cities linked to credit records, administrative homeless system data, and novel surveys, we provide the first experimental evidence on the effects of pandemic-era ERA on a wide range of policy-relevant outcomes.

We show that these programs were relatively well-targeted. Compared to other renters in the area, applicants were more financially distressed, more likely to live in high-poverty neighborhoods, and more likely to be minorities. Similar conclusions are reached when comparing applicants to EIP recipients. Lastly, the assistance went out relatively early in the pandemic while unemployment rates and rental arrears were still quite high, and well before the federal ERA programs.

We find that rental assistance reduced rental arrears and concern about eviction, and may have improved self-reported physical and mental health. However, it had little detectable short- or longer-run effect on broader measures of housing stability and financial security, including evictions, homelessness, residential mobility, credit scores, indebtedness, or delinquency.

The key question raised by these findings is: why did these programs not have more of a detectable impact? 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 are also similar to that of other smaller-scale assistance programs that have been shown to have large effects (Phillips and Sullivan, 2025; Evans et al., 2016). Moreover, we find little heterogeneity across sites, even though 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. Unlike other ERA programs, which target shocks faced by individual renters, the programs that we study were created with the specific aim of responding to the COVID pandemic, which was an aggregate shock affecting all households. Below, we discuss three natural explanations for why ERA could be less impactful in this context.

First, aggregate shocks tend to be accompanied by other fiscal stimulus that provides additional support to low-income households, which tend to be renters. The COVID-19 pandemic, in particular, featured a massive expansion of nearly every aspect of the social safety net, including Unemployment Insurance, Economic Impact Payments, and expansions of the Earned Income Tax Credit and Child Tax Credit. Consistent with this explanation, 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. The improvement in household balance sheets continues post-lottery but is similar for both treatment and control applicants. Furthermore, we find evidence that applicants with less access to other safety net alternatives appear to benefit more from ERA receipt. Together, this suggests that the widespread availability of other forms of assistance reduced the marginal impact of ERA.

Second, eviction moratoria likely weakened the link between rental arrears and displacement. While moratoria did not eliminate housing instability—landlords could still pursue evictions for lease violations, decline to renew expiring leases, or pressure tenants through informal means—they nonetheless reduced the immediate risk of court-ordered displacement following nonpayment. This disruption to the typical mechanism through which ERA preserves housing stability likely reduced the program's effectiveness. The threat of future eviction action remained, which likely explains why we observe ERA reducing tenant anxiety about eviction. However, the temporary legal protection from displacement meant that ERA's marginal contribution to preventing actual moves was limited during the period when moratoria were in effect.

A third explanation is that general equilibrium market responses to aggregate shocks may limit the impact of additional rental assistance, even in the absence of moratoria or expanded fiscal stimulus. During the beginning of the COVID pandemic, landlords faced both falling rents and rising vacancies, creating incentives to retain tenants, even those with arrears. The prospect of extended vacancies and uncertain rental income could make both immediate eviction and lease non-renewal less attractive options. This shift in market dynamics could reduce displacement risk regardless of formal assistance, further weakening the connection between ERA and housing stability outcomes. Thus, renegotiation 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 E.11). This evidence is in line with other pandemic-era surveys of landlords that find substantial increases in reported rental forbearance in 2020, relative to pre-pandemic levels (Decker, 2021; de la Campa and Reina, 2023). In this context, rental assistance may reduce rental arrears without changing tenants' longer-run housing stability, 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, far less is known about crisis-response policies for renters. Our lottery-based estimates of the impact of local ERA programs implemented during the COVID-19 pandemic allow us to measure the effects of one such policy during an unprecedented combination of economic disruption and policy intervention. Our results suggest that ERA's effectiveness may depend critically on the broader policy environment: Programs that substantially reduced homelessness in normal times had muted effects when deployed alongside eviction moratoria and expanded safety net programs. This apparent context-dependence raises important considerations for future crisis response, particularly regarding how and when to deploy rental assistance relative to other emergency measures. Whether our results generalize to later rounds of ERA—which were implemented after the pandemic's initial impact, were typically more generous, and prioritized paying down arrears—remains an open question. Future work should examine both the longer-term effects of these programs and their impacts on landlord behavior and rental market dynamics.

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Tables and Figures

TABLE I

Summary Statistics and Balance

	Harris County			King County			Los Angeles			Chicago DOH			Chicago TRP		
	Z=0	Z=1	Diff	Z=0	Z=1	Diff	Z=0	Z=1	Diff	Z=0	Z=1	Diff	Z=0	Z=1	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	0.013	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	0.051	−0.003	0.231	0.235	0.007	0.213	0.217	0.003	0.218	0.223	0.004	0.039	0.034	−0.006
Black	0.607	0.629	−0.007	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	0.253	0.015**	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	0.025	−0.007***	0.074	0.073	−0.001	0.109	0.105	−0.004	0.043	0.052	0.009*	0.041	0.032	−0.009
ACS Zip Code Data															
Median Rent	1.005	1.005	−0.001	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	0.214	0.215	0.001*	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	0.758	−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	0.130	0.121	−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	0.374	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	−1.798	583	575	8.510*	637	636	−0.961	603	610	6.096**	644	651	4.399
Balance Across All Trades	21.430	23.168	−1.899**	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	3.212	3.264	0.007	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	0.576	0.566	0.025	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.	10.225	9.911	0.410	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	0.293	0.299	−0.003	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	0.343	−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	0.221	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†	4,911	13,460	18,390	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 ($Z = 0$) and selected by the lottery ($Z = 1$). Conditional differences in average characteristics between these two groups come from regressions that control for site-specific design features. See Section 5. 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

Assistance Recipients and Targeting

	Harris County		King County		Los Angeles			Chicago DOH	TRP
	Renters	Assisted	Renters	Assisted	Renters	Assisted	Renters	Assisted	Assisted
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Demographics:									
Age	39.447	.	39.288	39.433	41.659	41.367	40.986	37.969	39.428
Female	0.514	0.669	0.485	0.643	0.510	0.545	0.523	0.601	0.562
Household Size	2.869	2.470	2.252	.	2.856	2.535	2.303	2.886	3.994
White	0.517	0.053	0.580	0.239	0.487	0.250	0.473	0.224	0.033
Black	0.273	0.667	0.112	0.359	0.099	0.114	0.341	0.359	0.100
Hispanic	0.441	0.220	0.120	0.147	0.494	0.494	0.249	0.349	0.837
Asian	0.062	0.020	0.187	0.073	0.127	0.114	0.071	0.068	0.029
ACS Zip Code Data:									
Median Rent	1.139	1.008	1.598	1.479	1.521	1.443	1.177	1.052	0.999
Poverty Rate	0.163	0.214	0.098	0.111	0.180	0.193	0.192	0.209	0.199
% with High School Diploma	0.786	0.760	0.894	0.895	0.772	0.769	0.830	0.821	0.765
% Hispanic	0.408	0.446	0.111	0.135	0.436	0.462	0.247	0.307	0.492
% Non-Hispanic White	0.310	0.121	0.558	0.508	0.318	0.289	0.350	0.253	0.206
% Black	0.184	0.377	0.072	0.099	0.091	0.083	0.308	0.354	0.234
% Asian	0.077	0.041	0.187	0.176	0.122	0.134	0.072	0.065	0.051
Credit Characteristics:									
Credit Score	684	566	725	584	710	649	691	613	658
Balance Across All Trades ($1000s)	19.1	27.9	15.4	21.4	16.7	22.1	19.5	30.7	23.6
Balance in Collections ($1000s)	0.721	3.320	0.219	1.651	0.292	0.918	0.327	1.226	0.419
Balance In Delinquent Accounts	1.661	9.426	0.709	7.428	1.167	4.671	1.126	6.626	1.821
Any Auto Loan or Lease	0.355	0.326	0.261	0.349	0.322	0.279	0.250	0.305	0.214
Any Personal Banckruptcy	0.012	0.019	0.026	0.100	0.031	0.046	0.061	0.140	0.031
Any Open Revolving Line of Credit	0.807	0.384	0.906	0.505	0.870	0.703	0.848	0.599	0.583
N		8,245		2,532		10,794		722	1,607

Notes: This table reports mean characteristics for renters and those who received assistance at each of our four sites. For Chicago those assisted through the DOH program and the TRP program are reported separately. The first panel reports demographic characteristics. The Assisted include those who received assistance despite not being selected through the lottery. Renter characteristics are from the 2019 American Community Survey, restricted to renters in Harris County, King County, Los Angeles, and Chicago. The second panel reports ZIP code characteristics. For renters, these are the weighted average of aggregate ZIP (ZCTA) code characteristics for ZIP codes in the county/city from the 2015-2019 ACS, weighted by the number of renters in the ZIP code. For those assisted, it is the same, but weighted by the number of applicants. The third panel reports credit characteristics. For the renter columns, these are from a 10% sample from Equifax restricted to renter in the relevant county/city. For those assisted, these are for those who were linked to Experian. Both are from January 2020.

TABLE III
First Stage

	Received Assistance				
	Harris County (1)	King County (2)	Los Angeles (3)	Chicago DOH (4)	Chicago TRP (5)
Selected by Lottery	0.590***	0.372***	0.207***	0.431***	0.910***
	(0.005)	(0.008)	(0.006)	(0.013)	(0.007)
Control Mean	0.000	0.046	0.347	0.001	0.000
Frac. Selected for Assistance	0.742	0.426	0.464	0.010	0.249
F Statistic	17,096	2,442	1,093	47,977	47,332
N	18,439	12,062	24,377	74,663	6,453

Notes: This table reports OLS estimates of α_1 in equation (2). The sample includes all program applicants from Harris County (column (1)), King County (column (2)), Los Angeles (column (3)), Chicago DOH (column (4)) and Chicago TRP (column (5)). Regressions include site-specific design controls described in Section 5. The control mean reports the share who received assistance among individuals not selected by the lottery. "Frac. Selected for Assistance" reports the share of applicants offered treatment. 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 IV
SURVEY OUTCOMES

	Harris County (1)	King County (2)	Chicago: DOH (3)	Chicago: TRP (4)	Combined (5)
In the Last Month:					
Expenditures					
All Rent Paid	0.076*	0.134**	0.058*	0.059**	0.082***
	(0.040)	(0.053)	(0.031)	(0.028)	(0.020)
	[0.497]	[0.368]	[0.649]	[0.678]	[0.548]
All Bills Paid	−0.022	0.026	−0.025	0.023	0.001
	(0.032)	(0.042)	(0.033)	(0.033)	(0.018)
	[0.284]	[0.203]	[0.358]	[0.496]	[0.335]
Health					
COVID Positive (self-tested)	−0.110	−0.064	−0.051***	0.002	−0.056*
	(0.105)	(0.074)	(0.017)	(0.018)	(0.033)
	[0.182]	[0.074]	[0.101]	[0.095]	[0.113]
Feeling Anxious	−0.069*	0.014	−0.075**	0.002	−0.032*
	(0.036)	(0.050)	(0.033)	(0.028)	(0.019)
	[0.525]	[0.633]	[0.549]	[0.319]	[0.506]
Economic Insecurity					
Worried about Eviction	−0.082**	−0.056	−0.029	−0.022	−0.047***
	(0.034)	(0.053)	(0.030)	(0.020)	(0.018)
	[0.320]	[0.443]	[0.287]	[0.121]	[0.293]
Experienced Homelessness	−0.031	−0.008	−0.022	−0.005	−0.016
	(0.030)	(0.037)	(0.014)	(0.017)	(0.013)
	[0.241]	[0.146]	[0.063]	[0.079]	[0.132]
Stayed in a Homeless Shelter	−0.016**	−0.003	0.008	−0.000	−0.003
	(0.008)	(0.013)	(0.007)	(0.003)	(0.004)
	[0.015]	[0.014]	[0.005]	[0.001]	[0.009]
Was Food Insecure	−0.048	0.025	−0.039	−0.016	−0.019
	(0.033)	(0.049)	(0.029)	(0.026)	(0.018)
	[0.335]	[0.325]	[0.270]	[0.256]	[0.297]
N	1,671	2,621	14,681	1,352	20,325

Notes: This table reports TSLS estimates of β_1 in equation (3), for several outcomes: expenditures, health, and economic insecurity. Outcomes are derived from the surveys as described in Section 3 and Appendix B. Survey timing and treatment dates appear in Appendix Table E.2. Columns (1)–(4) report the TSLS estimates for each site, reweighting to adjust for survey nonresponse as described in Section 3. Column (5) reports the combined averages across all sites. Robust standard errors are reported in parentheses, and the control group mean is reported in brackets (this corresponds to the mean for the group not offered assistance, i.e., $Z = 0$). Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

TABLE V

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 TSLS estimates of β_1 in equation (3), for short-run (2 months) and longer-run (10 months) credit outcomes. Columns (1)–(4) report the TSLS estimates for each site. Column (5) reports the combined results across all of the locations. The 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 (this corresponds to the mean for the group not offered assistance, i.e. $Z = 0$). Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

TABLE VI

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 TSLS estimates of β_1 in equation (3), for residential 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 TSLS estimates for each site. Column (5) reports the combined results across all of the locations. Robust standard errors are reported in parentheses, and the control group mean is reported in brackets (this corresponds to the mean for the group not offered assistance, i.e. $Z = 0$). Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

TABLE VII

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 TSLS estimates of β_1 in equation (3), for 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. We assign project types to categories to align with local differences in the nature of services and how the data were provided. "Emergency Shelter" includes Emergency Shelter, Transitional Housing and Safe Haven in Chicago and Emergency Shelter and Day Shelter in King County. "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 both sites and also includes Transitional Housing in King County. Information on participation in Coordinated Entry is only available for King County. Columns (1)–(3) report TSLS estimates for each site. Robust standard errors are reported in parentheses, and the control group mean of the outcome is reported in brackets (this corresponds to the mean for the group not offered assistance, i.e. $Z = 0$). The pre-COVID means from March 2019 to February 2020 for the listed measures are reported in braces. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

TABLE VIII
Impacts on Eviction Filings

	Harris County		Chicago: DOH		Chicago: TRP	
	Control Mean	Treatment Effect	Control Mean	Treatment Effect	Control Mean	Treatment Effect
	(1)	(2)	(3)	(4)	(5)	(6)
Evictions Post Lottery:						
Two Months	0.0032	−0.0008	0.0004	−0.0004***	0.0000	−0.0002
		(0.0018)		(0.0001)		(0.0002)
Six Months	0.0097	−0.0017	0.0012	0.0004	0.0002	−0.0004
		(0.0029)		(0.0018)		(0.0003)
Ten Months	0.0121	−0.0022	0.0019	−0.0004	0.0002	−0.0004
		(0.0033)		(0.0018)		(0.0003)
Evictions Post Moratorium:						
Two Months	0.0129	−0.0022	0.0051	−0.0030*	0.0004	0.0000
		(0.0034)		(0.0022)		(0.0007)
Six Months	0.0144	−0.0023	0.0078	−0.0024	0.0004	0.0007
		(0.0036)		(0.0033)		(0.0010)
Ten Months	0.0156	−0.0029	0.0118	−0.0050	0.0011	0.0007
		(0.0037)		(0.0038)		(0.0012)
N		18,390		55,519		6,414

Notes: This table reports TSLS estimates of β_1 in equation (3), for measures of eviction filings in three lottery samples: Harris County, Chicago DOH, and Chicago TRP. The top panel reports impacts on cumulative eviction filings by month relative to the lottery. The bottom panel summarizes impacts on cumulative eviction filings by month relative to the expiration of the CDC eviction moratorium. Columns (1), (3), and (5) report the mean among those assigned to the control group (i.e., those for whom $Z = 0$). Columns (2),(4), and (6) report TSLS estimates for each site. Robust standard errors are reported in parentheses, and the control group mean of the outcome is reported in brackets (this corresponds to the mean for the group not offered assistance, i.e. $Z = 0$). Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

FIGURE I
Timing of ERA Programs Relative to Macroeconomic Trends

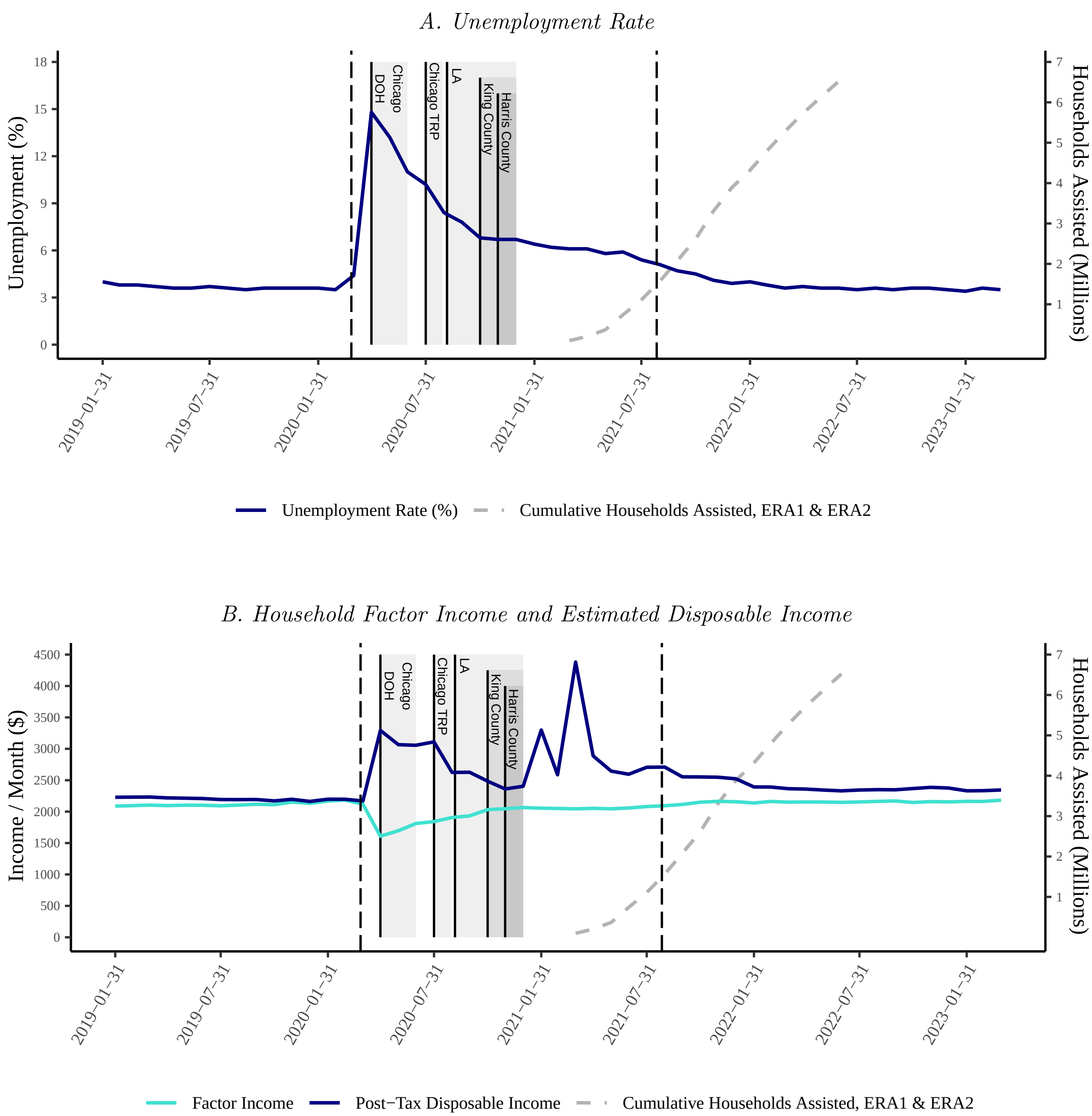
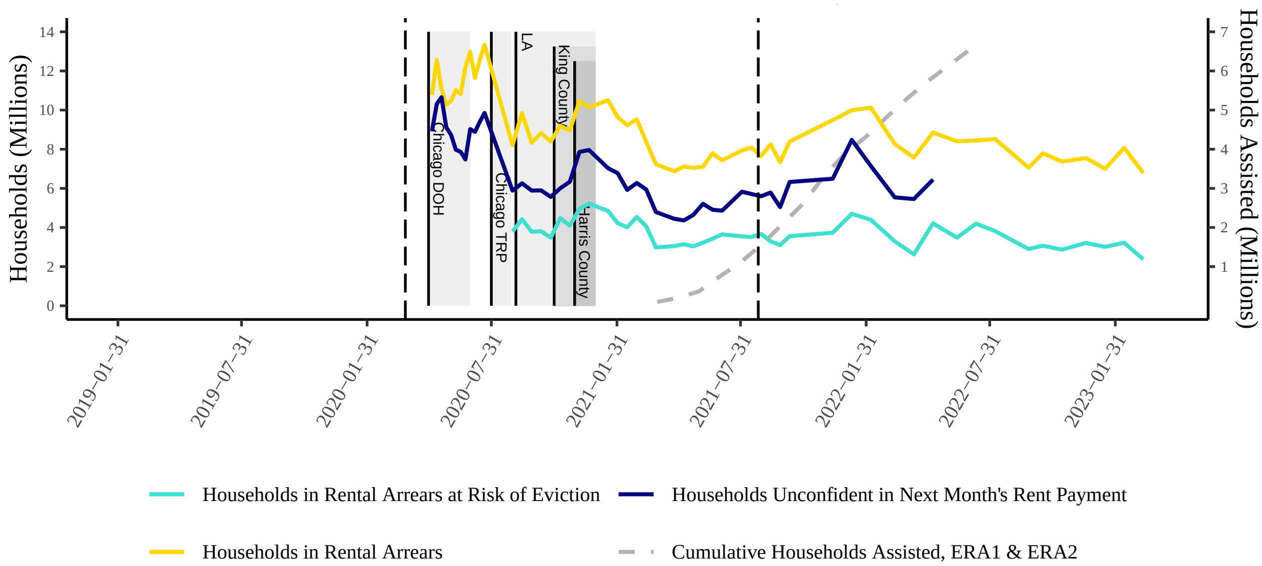


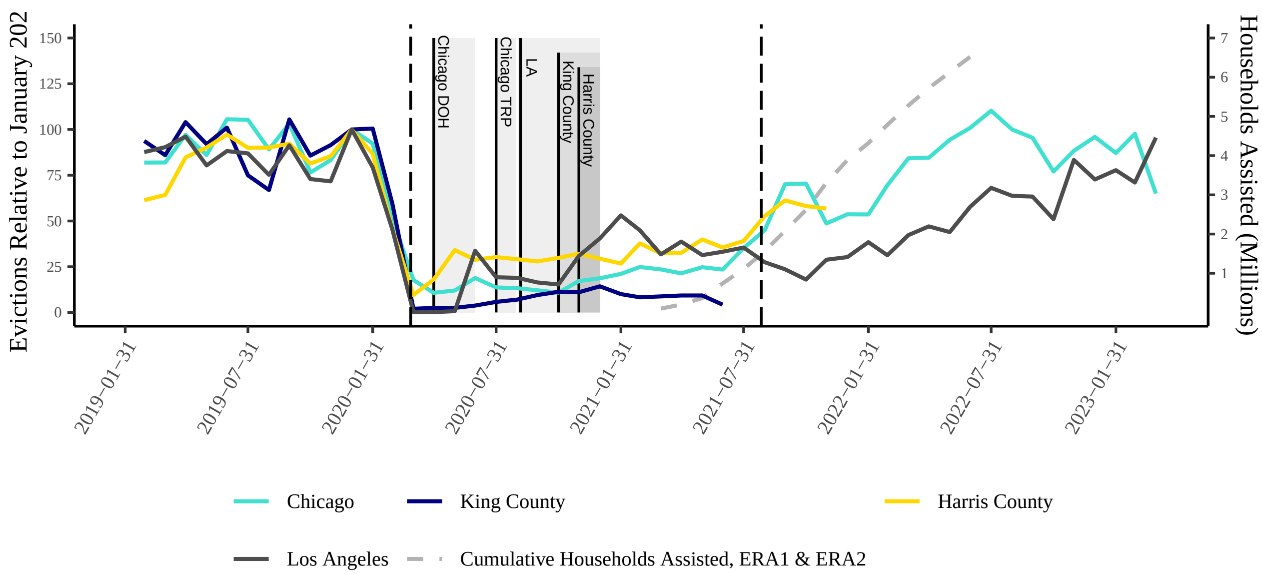
FIGURE I

Timing of ERA Programs Relative to Macroeconomic Trends (cont.)

C. Households Behind on Rent, Worried About Next Month's Rent, Worried About Eviction



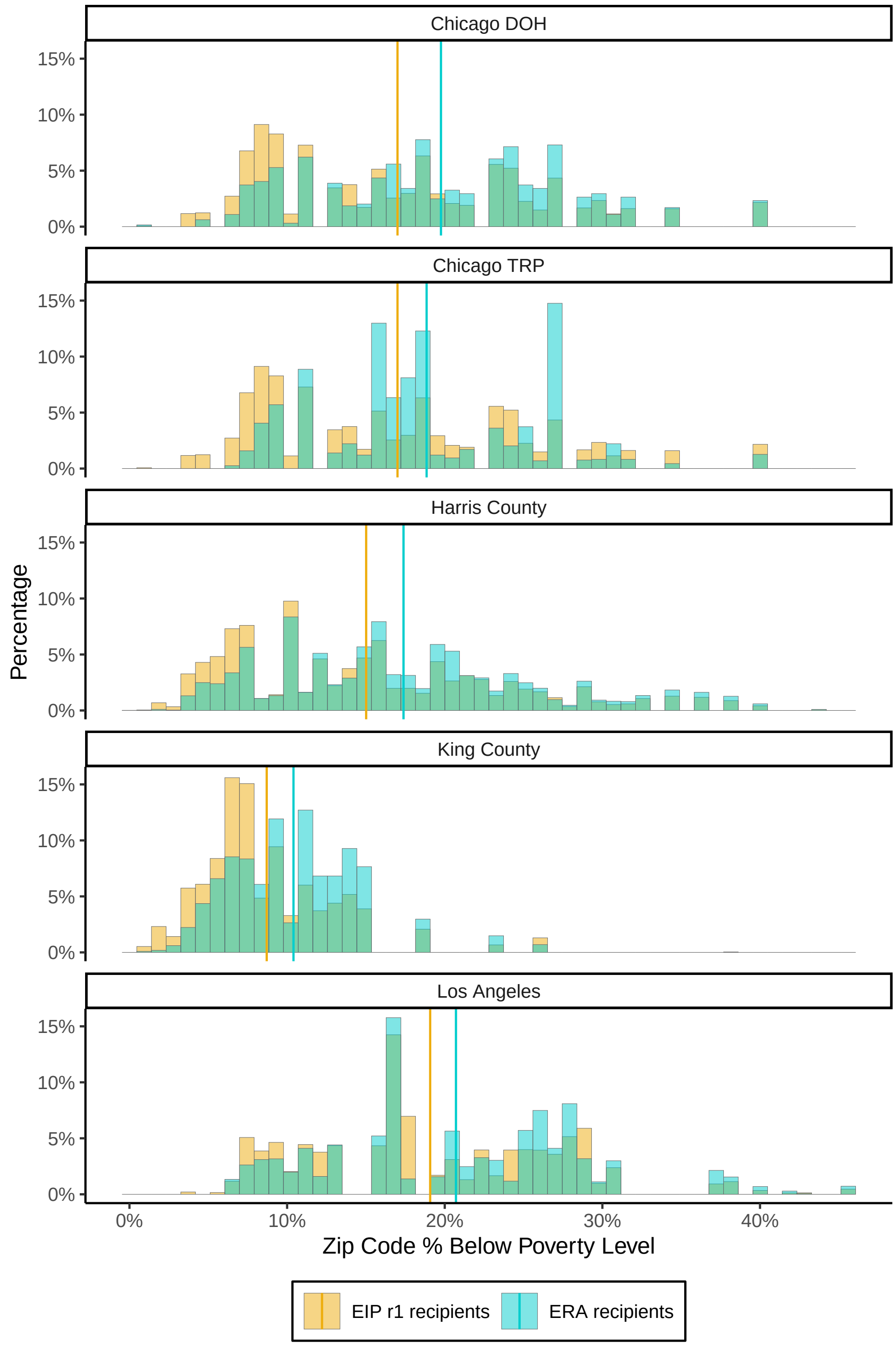
D. Monthly Evictions by Site



Notes: This figure shows the timing of the ERA programs we study relative to macroeconomic indicators. Panel A plots the civilian unemployment rate as measured by the Current Population Survey (CPS). Panel B shows Factor Income and Post-Tax disposable income using estimates from Blanchet et al. (2022). Panel C is based on the authors' calculations using the Census Household Pulse Surveys. Panel D shows eviction filings for all sites except Los Angeles where we use completed evictions because filings data are not available. These series are based on administrative court records. The dashed vertical lines indicate the start of the CARES Act moratorium and the end of the CDC Eviction Moratorium. ERA1 and ERA2 cumulative households assisted are constructed from the U.S. Treasury Department's ERA1 and ERA2 Grantees Compliance Reports.

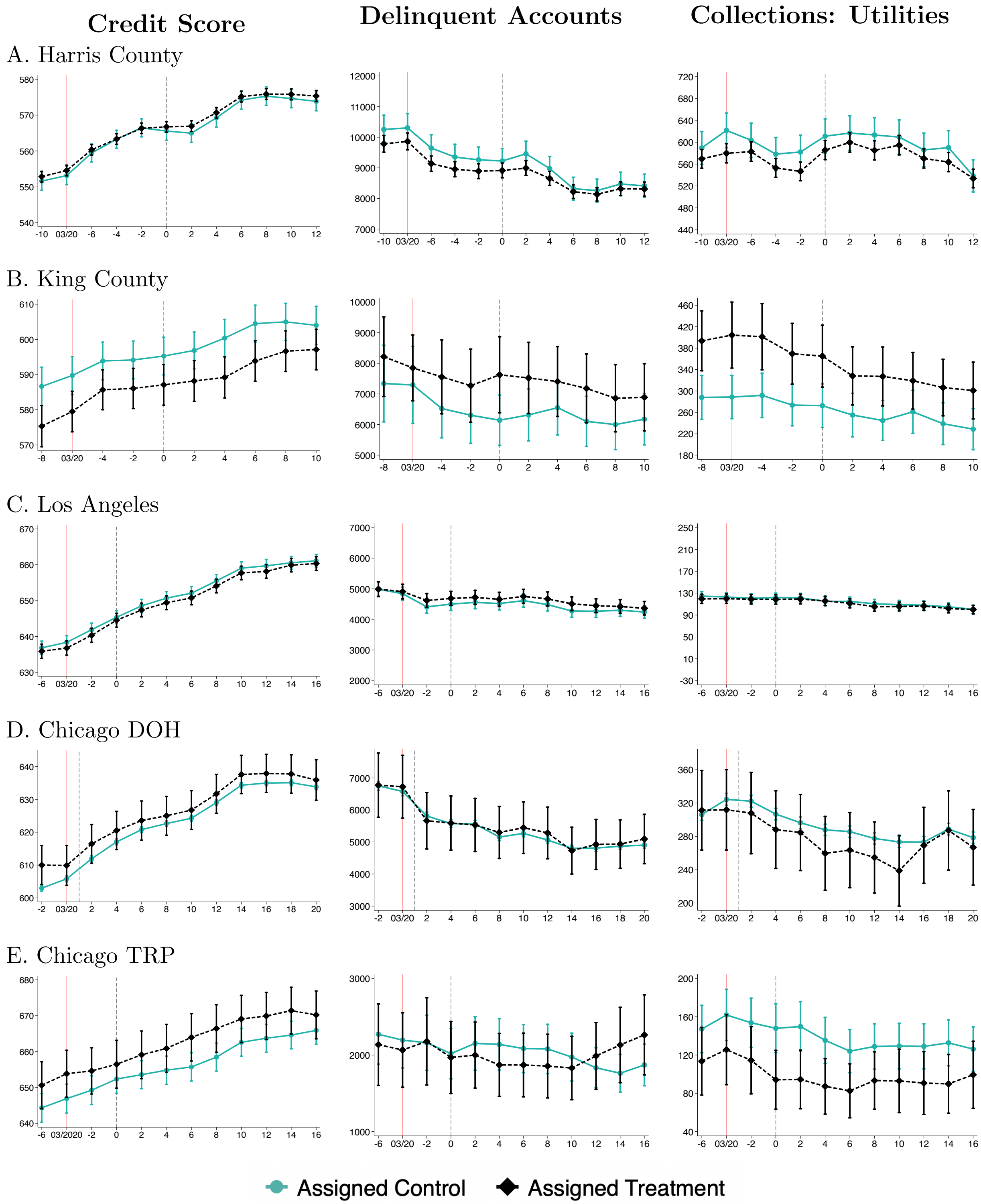
FIGURE II

Targeting: ERA Recipients Vs. Economic Impact Payments Round 1 Recipients (ZIP-Level Poverty Rate)



Notes: This figure shows the distribution of ZIP-level population poverty status rates for Emergency Rental Assistance recipients (cyan) and Economic Impact Payments Round 1 recipients (yellow). The poverty rate measure is from the 2020 American Community Survey (ACS) 5-Year Estimates and is the percentage of the population for whom poverty status is determined (U.S. Census Bureau, 2020b). EIP r1 data is from the 2020 Internal Revenue Service Statistics of Income and measures the number of income tax returns filed in 2020 that received round one EIP provided under the CARES Act (Internal Revenue Service, 2020). The vertical lines indicate the mean poverty rate for each group within each site.

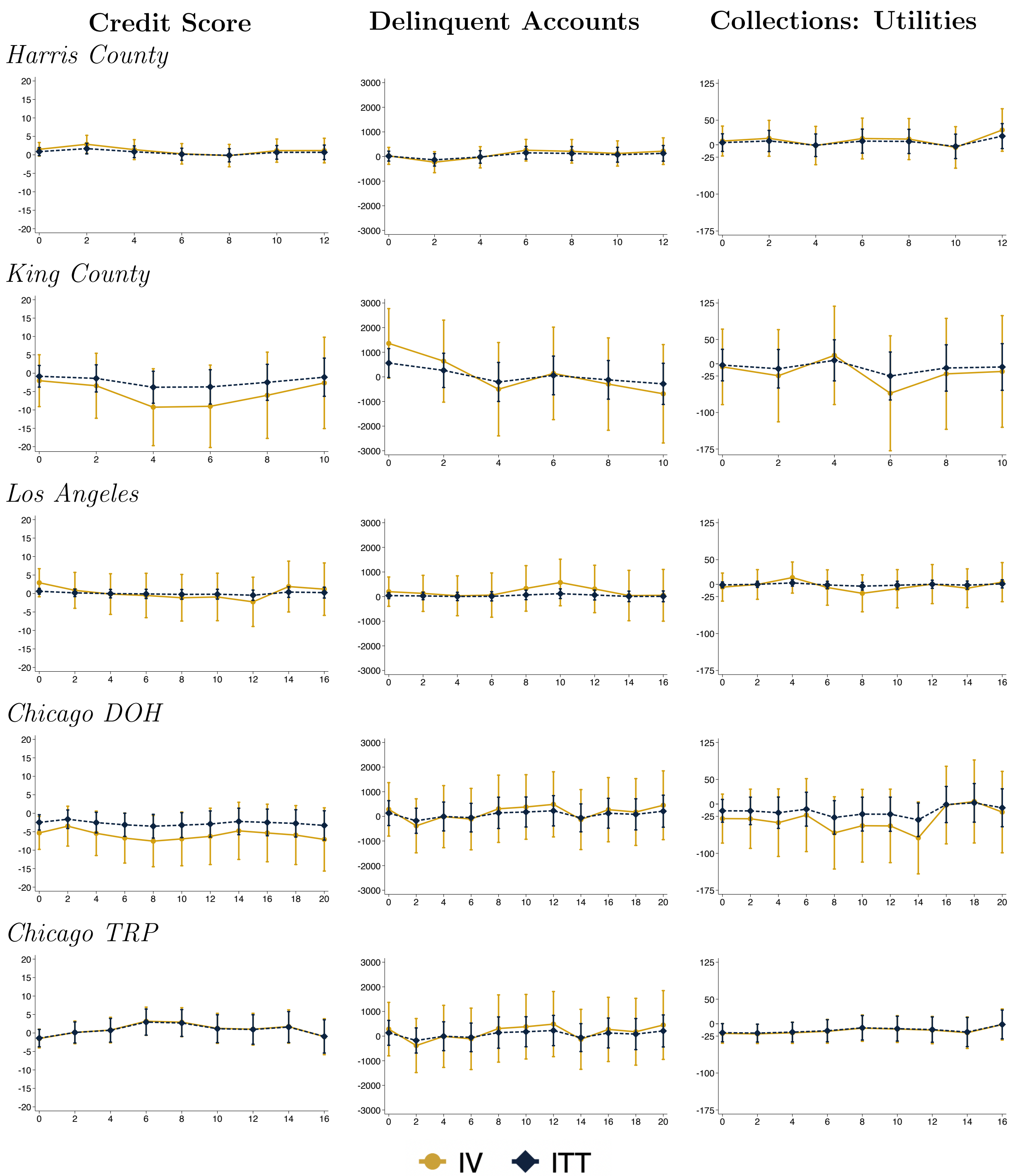
FIGURE III
Credit Measure by Month Relative to Application



Notes: This figure shows the average credit outcome for the group selected for treatment (black diamonds, $Z = 1$) and the group not selected for treatment (teal circles, $Z = 0$), by month relative to the time of application. Month 0 represents the site's application month. 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 IV

Reduced-form (ITT) & TSLS Estimates by Month Relative to Application



Notes: This figure shows the reduced-form (ITT) and TSLS estimates for different credit outcomes by month relative to the application date. The former corresponds to OLS estimates of γ_1 in equation (1), while the latter corresponds to TSLS estimates of β_1 in equation (3). The sample includes applicants linked to Experian credit data. Vertical bars plot the 95 percent confidence intervals of the estimates.

A Additional Program Details

This section provides information on each program's timing, eligibility requirements, application procedure, and payment amounts. Appendix Table E.10 summarizes these program details.

A.1 Chicago

We study two assistance lotteries that were run in Chicago in 2020. The first lottery was run by the City of Chicago Department of Housing (DOH). The second was organized by The Resurrection Project (TRP), a local nonprofit organization.

Chicago Department of Housing. The City of Chicago Department of Housing (DOH) partnered with UpTogether to implement an emergency financial assistance program for city residents at risk of eviction, foreclosure, or homelessness. The program opened to applications on March 27 and closed on April 5. Applications were submitted through a simple online form. The program received a total of 75,659 applications, approximately 80 percent of which were from renters, and the remainder from homeowners.

Due to the large number of applicants, grants were distributed through a lottery process. All applicants were assigned a random rank and were screened for eligibility, and offers were made in rank order until the funds were exhausted. Upon being selected, applicants had to provide proof that they were Chicago residents, that the pandemic negatively impacted their income, and that their pre-pandemic income was no greater than 60 percent of the area median income (AMI). Ultimately, 722 grants of $1,000 were distributed to eligible applicants selected by the lottery. Grants were paid directly to tenants on a rolling basis through April and May of 2020.[27]

The Resurrection Project. The second program was run by The Resurrection Project (TRP), a local nonprofit organization, in partnership with the Chicago Resiliency Fund. The TRP program aimed to provide assistance to a population of people who were not eligible and did not receive aid from provisions of the CARES Act. Although open to all Chicago residents with incomes below 300 percent of the federal poverty line, applications from undocumented individuals, mixed-status families, dependent adults, and returning residents were particularly encouraged. Those selected had to provide identification, proof of Chicago residence, and proof of income.

The program's first round of assistance was not oversubscribed. We study its second round, which opened for applications on June 22, 2020. Like the DOH program, the TRP program was open to homeowners and renters, with 75 percent of the applications coming from renters. In the second round, 10,300 applications were submitted, and 1,607 grants of $1,000 were distributed directly to tenants by lottery.[28]

[27] DOH conducted two further waves of grant programs later in 2020, but the applicant pools were smaller, leaving no, or very small, control groups. Therefore, we include only the first round in this study.

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

A.2 Harris County

In November 2020, Harris County, Texas, partnered with Catholic Charities Galveston-Houston (CCGH) to administer $60 million in assistance to households experiencing economic hardship due to the COVID-19 pandemic.[29] Eligibility was limited to individuals earning less than 60 percent of local AMI or receiving public assistance, and who could demonstrate that the pandemic negatively impacted their income. When applying online, applicants could upload proof of hardship (e.g., a letter of termination/furlough, a paystub showing a reduction in pay or hours) or self-attest to hardship using a self-certification form. Upon selection for processing, applicants underwent an income eligibility screening.

Applications for assistance opened for five days and ended on November 5, 2020. The county sought to distribute funds 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 receiving assistance depending on the applicant's Census tract.[30] 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. Applicants selected through the lottery and determined eligible received $1,200 in cash assistance paid directly to the tenant. The funds were mostly distributed in November and December 2020, with some payments going out in early 2021.

A.3 King County

In late 2020, King County, Washington, implemented an emergency financial assistance 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 form that was open from August 20, 2020, to December 4, 2020.

Starting on September 22, 2020, all eligible applicants who had not been offered previous assistance were entered into a weekly lottery conducted 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 the week of application. The funds for the program were distributed from October 2020 to January 2021.

To be eligible for this program, individuals had to verify income at or below 50 percent of the local AMI during the 60 days prior to the application, demonstrate experience of financial hardship due to COVID-19 that threatened their ability to pay rent on time, and indicate risk of experiencing housing instability.

The assistance provided in King County was somewhat more generous than that provided

[29] This was the second round of assistance distributed by the county, which administered an earlier round in July 2020.

[30] We account for this in our analysis by including Census tract fixed effects (see Section 5.1 for more detail).

at the other sites. Those selected 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 six months' rent, (iii) to retain the tenant (except for cause), and (iv) to not raise rent through March 2022.

A.4 Los Angeles

In July 2020, the Los Angeles City Council approved the creation of the 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 provide rent subsidies to 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 13-17, 2020. 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 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 of all those initially selected.

Applicants who were awarded funding received rent subsidies of $2,000 per household to be applied to current or future rent 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 award 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 was made in September 2020 and the funds were fully exhausted at the end of December 2020. Ultimately, 56 percent of all grants were paid to landlords and 44 percent to tenants.

A.5 Details on Eviction Moratoria

National and local eviction moratoria constituted an important feature of the regulatory environment for our setting. The CARES Act included a 120-day national moratorium on eviction filings for non-payment of rent and related fees for properties participating in federal assistance programs or with federally backed financing.[31] This measure was in place from March 27th to July 24th, 2020. Next, the Centers for Disease Control and Prevention (CDC) issued a national moratorium on evictions for nonpayment of rent and related fees on September 4, 2020 (McCarty and Perl, 2021), which lasted until July 31, 2021 (McCarty et

[31] This set of properties are estimated to include 28–46% of occupied rental units (Stein and Sutaria, 2020).

al., 2021).[32] The moratorium allowed evictions for reasons other than non-payment, such as property damage, criminal activity, health and safety issues, and other contractual violations. The practical implementation of the moratorium varied by state; some judges saw it as a ban on filing new cases for non-payment, while others viewed it as a ban on enforcing eviction orders (Sudeall et al., 2023). Consequently, the impact of the federal moratorium differed by location.

In addition to the national moratorium, local moratoria were implemented in all our study sites.

Chicago. Chicago renters were covered by an Illinois state-wide eviction moratorium between March 20, 2020 and October 3, 2021. The moratorium prohibited eviction filings for nonpayment of rent, but other types of filings were allowed to continue. Chicago was also subject to an eviction sealing policy that went into effect on May 17th. The policy sealed cases filed between March 9th, 2021, and March 31st, 2022, removing the cases from the public record. These cases remained sealed after March 31st, 2022, while new filings were part of the public record (ILCS 5/9-122).

Harris County. Harris County renters were covered by a state eviction moratorium from March 19 to May 19, 2020. Eviction hearings were allowed to resume on May 19, 2020, and evictions were allowed to begin on May 26, 2020. Although the CDC moratorium covered renters in Harris County, there was significant local concern that Harris County eviction court judges were not honoring this order.[33] Even though law enforcement could not enforce an eviction order during the national CDC 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 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).

King County. King County renters were covered by both state- and city-level eviction moratoria. 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' ability to file evictions reinstated on November 1, 2021. Seattle, Burien, and Kenmore further extended their city-wide moratoria through January 15, 2022.

Los Angeles. LA city renters were covered by a residential eviction moratorium that was in effect from March 4, 2020, to June 30, 2022. Starting 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.

[32] The federal government reinstated a more limited moratorium on August 3, 2021, which the Supreme Court struck down on August 26, 2021.

[33] Local news coverage emphasized uncertainty in how the eviction moratoria were interpreted by local judges (e.g., Schuetz, 2021).

B Construction of Survey Outcomes

This appendix describes how each of the survey outcomes studied in this paper is constructed. Although similar surveys were administered across sites, they were not identical. Below, we provide exact definitions and across-site harmonization procedures for each survey outcome variable we consider (i.e., for each survey variable included in Table IV). Our discussion references question numbers in the survey instruments, which are provided at https://github.com/robcollinson/covid_era_surveys/.

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. The "All Rent Paid" variable is coded as 1 if this ratio is 1, and 0 otherwise.
- 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. The "All Rent Paid" variable is coded as 1 if the bar was set to 100%, and 0 otherwise.
- 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 their gas, electric, phone, TV/cable, car payment, car insurance, health insurance, and student loan bills in the past month. An index is then created 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 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 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 as the "Experience Homelessness" outcome, but it 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".

C Pre-Analysis Plan

The lotteries analyzed in this paper were originally studied by three independent research teams. The studies were combined and attempts were made to harmonize approaches across sites as much as possible. Once linked data became available for analysis, pre-registrations existed for three out of five lotteries. These can be found in the American Economic Association's RCT Registry: the study IDs are AEARCTR-0007731 (Chicago) and AEARCTR-0007167 (King County). Of these, the King County registration had a detailed pre-analysis analysis plan (PAP), which can be downloaded from the registry.

The analysis in this paper follows the PAP for King County. Given the urgency in getting surveys to respondents once a lottery was implemented by the local partner, it was not always possible to field the exact same questions across all sites. To facilitate harmonizing survey outcomes across sites, we deviated in constructing outcomes in the following ways:

- *All Rent Paid*: While the PAP specified using the fraction of rent paid as a primary outcome, we use a binary variable indicating whether rent was paid in full. This was done to create a harmonized measure of rent payment across sites. As described in Appendix B, three out of five surveys asked about full rent payment (a yes/no question), while the others asked about the fraction of rent paid. To present a single measure across the different sites, the fraction of rent was binarized.

- *Housing Stability Outcomes*: This paper presents estimates for homelessness and address changes separately, while the PAP specified as the primary outcome a binary variable that indicated either of these two outcomes. Because homelessness data was not available in all sites, it was not possible to construct such an outcome everywhere. Hence, we opted to present the outcomes separately. Each outcome was listed as a secondary outcome in the PAP.

- *Bills Paid*: The PAP designated as our primary outcome the fraction of bills paid. Similar to the *All Rent Paid* variable, we instead used a binary outcome indicating whether all bills were paid, so that we could harmonize across sites. The construction of this variable is described in Appendix B.

- *Feeling Anxious or Depressed*: As described in Appendix B, we construct a binary variable based on multiple choice responses. By contrast, the PAP describes using the number of days during which the respondent indicated feeling anxious or depressed as the outcome. Because surveys in different sites asked about different time spans, and to make the outcome easier to interpret, we harmonized these outcomes across sites by binarizing them as described in B.

We also harmonized control variables across sites. The King County PAP indicated that, beyond design-based controls, we would select additional covariates from the following: lagged outcomes, gender, race/ethnicity, age, city, disability status, and preferred language. We have limited these additional controls to those which overlap across sites: gender and race.

D Linking Eviction Data

Harris County. We collect eviction case data from 2019 to December 2023 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 2.65 percent of applicants can be linked to one or more eviction records from 2019 to 2023. Our analysis focuses on the linking to any eviction filing at 2 months, 6 months, and 10 months after the lottery, and 2, 6, and 10 months after the expiration of the CDC moratorium.

Cook County. Our data is a combination of data from Record Information Services and data received from the Cook County Clerk's office, collectively covering the period between 2019 and April 2024, and including case filings that were sealed from the public record during the eviction moratorium. The data include filing date, each defendant's name, and the address of the filing.

We link the records to applications to the DOH and TRP programs as follows:

1. Clean name fields by removing prefix/suffixes and omitting initials.
2. Geocode addresses via ArcGIS Street maps Premium (geocode rates ¿ 97%).
3. Block data by latitude/longitude match up to three decimal places (equal to 1 km) and conduct fuzzy matching algorithm using Jaro-winkler string distance.
4. Retain only matches with sufficiently similar name field and where geocoordinates are within 33 meters.
5. Take best match for each eviction filing by name distance and geodistance.

E Additional Tables and Figures

Appendix Table E.1
Summary and Balance: LA Financial

	Los Angeles		
	Z=0	Z=1	Diff
	(1)	(2)	(3)
Financial Characteristics			
Income	29.533	29.835	0.301
Rent	1.614	1.624	0.009
Rent Owed	2.509	2.580	0.071*
N	13,062	11,315	24,377

Notes: Data on financial characteristics come from applications to the LA program. The sample includes all program applicants. We report the average characteristics for individuals selected by the lottery in column (1) ($Z = 1$), and the average characteristics for individuals not selected by the lottery in column (2) ($Z = 0$). Conditional differences (column (3)) in average characteristics between these two groups come from regressions that control for design features specific to the LA program. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$. We report the maximum observation counts.

Appendix Table E.2
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 E.3
Survey Outcomes – No Weights

	Harris County (1)	King County (2)	Chicago: DOH (3)	Chicago: TRP (4)	Combined (5)
In the Last Month:					
Expenditures					
All Rent Paid	0.056	0.125**	0.057*	0.051*	0.072***
	(0.036)	(0.052)	(0.030)	(0.027)	(0.019)
All Bills Paid	−0.007	0.023	−0.024	0.024	0.004
	(0.029)	(0.042)	(0.033)	(0.032)	(0.017)
Health					
COVID Positive (self-tested)	−0.091	−0.066	−0.051***	−0.003	−0.053**
	(0.084)	(0.063)	(0.016)	(0.017)	(0.027)
Feeling Anxious	−0.049	0.007	−0.072**	0.006	−0.027
	(0.032)	(0.049)	(0.032)	(0.027)	(0.018)
Economic Insecurity					
Worried about Eviction	−0.072**	−0.061	−0.029	−0.015	−0.044**
	(0.030)	(0.052)	(0.029)	(0.020)	(0.017)
Experienced Homelessness	−0.027	−0.006	−0.022*	−0.003	−0.015
	(0.026)	(0.036)	(0.013)	(0.016)	(0.012)
Stayed in a Homeless Shelter	−0.011*	−0.004	0.009	0.001	−0.001
	(0.007)	(0.012)	(0.007)	(0.003)	(0.004)
Was Food Insecure	−0.042	0.022	−0.038	−0.013	−0.018
	(0.030)	(0.048)	(0.028)	(0.025)	(0.017)
N	1,671	2,626	14,681	1,352	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 E.2. See Appendix B 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 E.4
LA Survey Outcomes – Weights and No Weights

	Los Angeles	
	Weights (1)	No Weights (2)
In the Last Month:		
Expenditures		
All Rent Paid	−0.264*	−0.215
	(0.136)	(0.134)
	[0.385]	[0.385]
All Bills Paid	0.246*	0.239*
	(0.140)	(0.135)
	[0.361]	[0.361]
Health		
COVID Positive (self-tested)	0.065	0.117
	(0.135)	(0.129)
	[0.263]	[0.263]
Feeling Anxious	0.040	0.041
	(0.140)	(0.136)
	[0.543]	[0.543]
Economic Insecurity		
Worried about Eviction	0.141	0.115
	(0.137)	(0.130)
	[0.343]	[0.343]
Experienced Homelessness	−0.095	−0.083
	(0.079)	(0.075)
	[0.086]	[0.086]
Stayed in a Homeless Shelter	0.000	0.002
	(0.016)	(0.014)
	[0.003]	[0.003]
Was Food Insecure	−0.004	−0.053
	(0.130)	(0.122)
	[0.291]	[0.291]
N	3,360	3,360

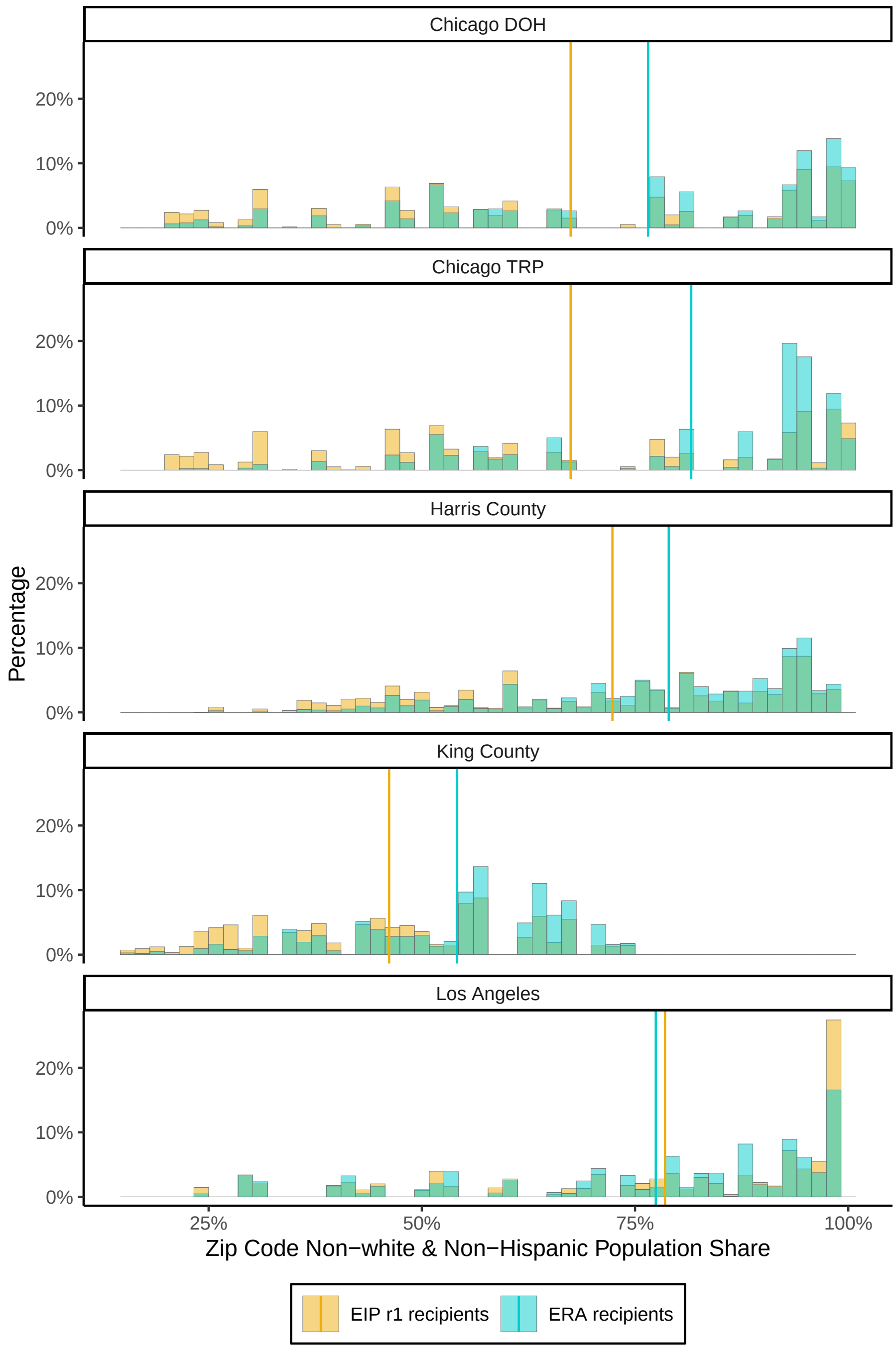
Notes: Robust standard errors are reported in parentheses, and the control group mean is reported in brackets. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table E.5
Balance Table – Survey Response

	Harris County			King County			Los Angeles			Chicago DOH			Chicago TRP		
	$Z = 0$	$Z = 1$	Diff	$Z = 0$	$Z = 1$	Diff	$Z = 0$	$Z = 1$	Diff	$Z = 0$	$Z = 1$	Diff	$Z = 0$	$Z = 1$	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

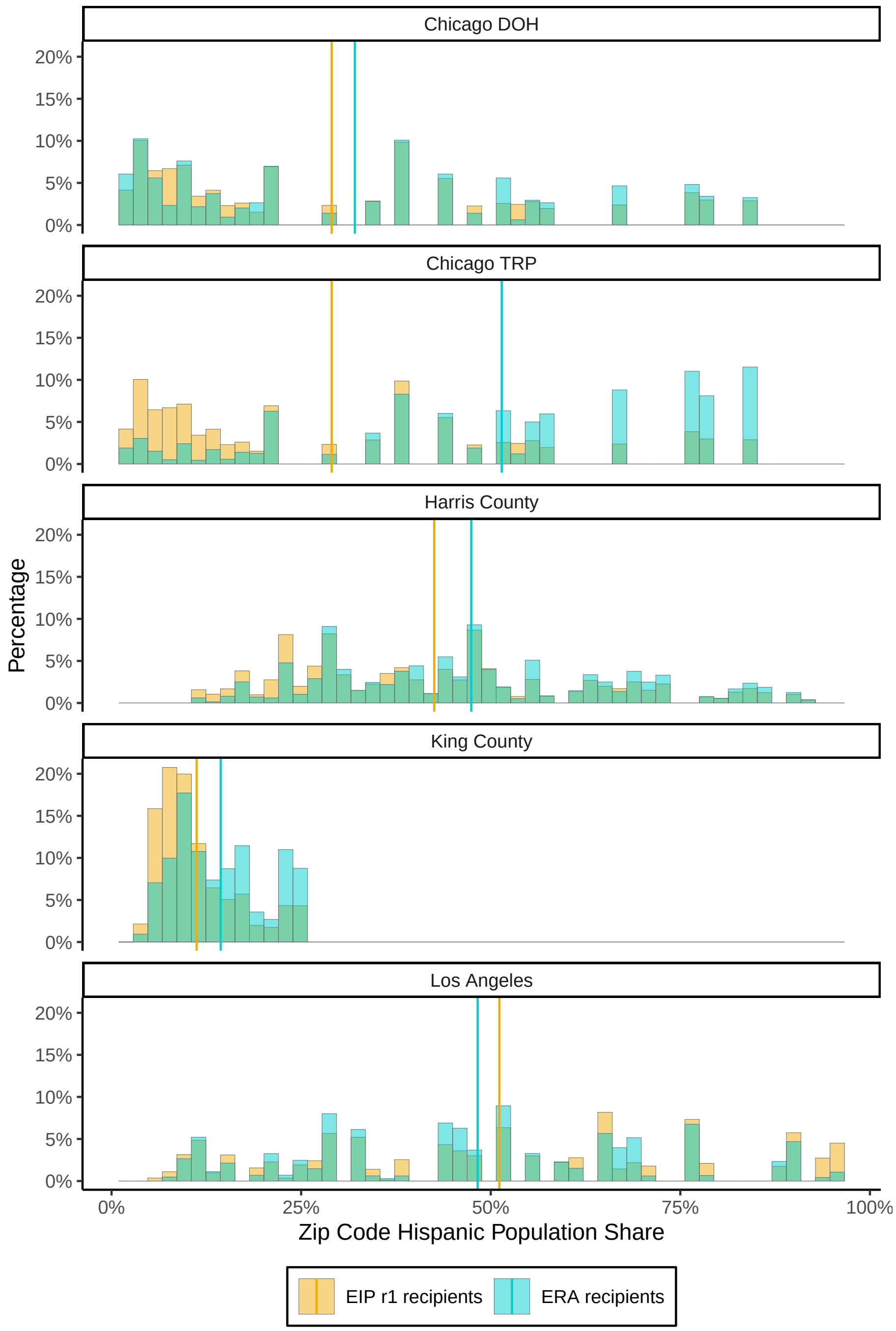
Notes: This table reports separately by site the average of demographic characteristics for applicants not selected by the lottery ($Z = 0$) and applicants selected by the lottery ($Z = 1$). Conditional differences in average characteristics (Diff) between these two groups come from regressions that control for site-specific design features. See Section 5. 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. X Was there another reason why we had the note on observations? X Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Figure E.1
Targeting: ERA Recipients Vs. Economic Impact Payments Round 1 Recipients (ZIP-Level Minority Share)



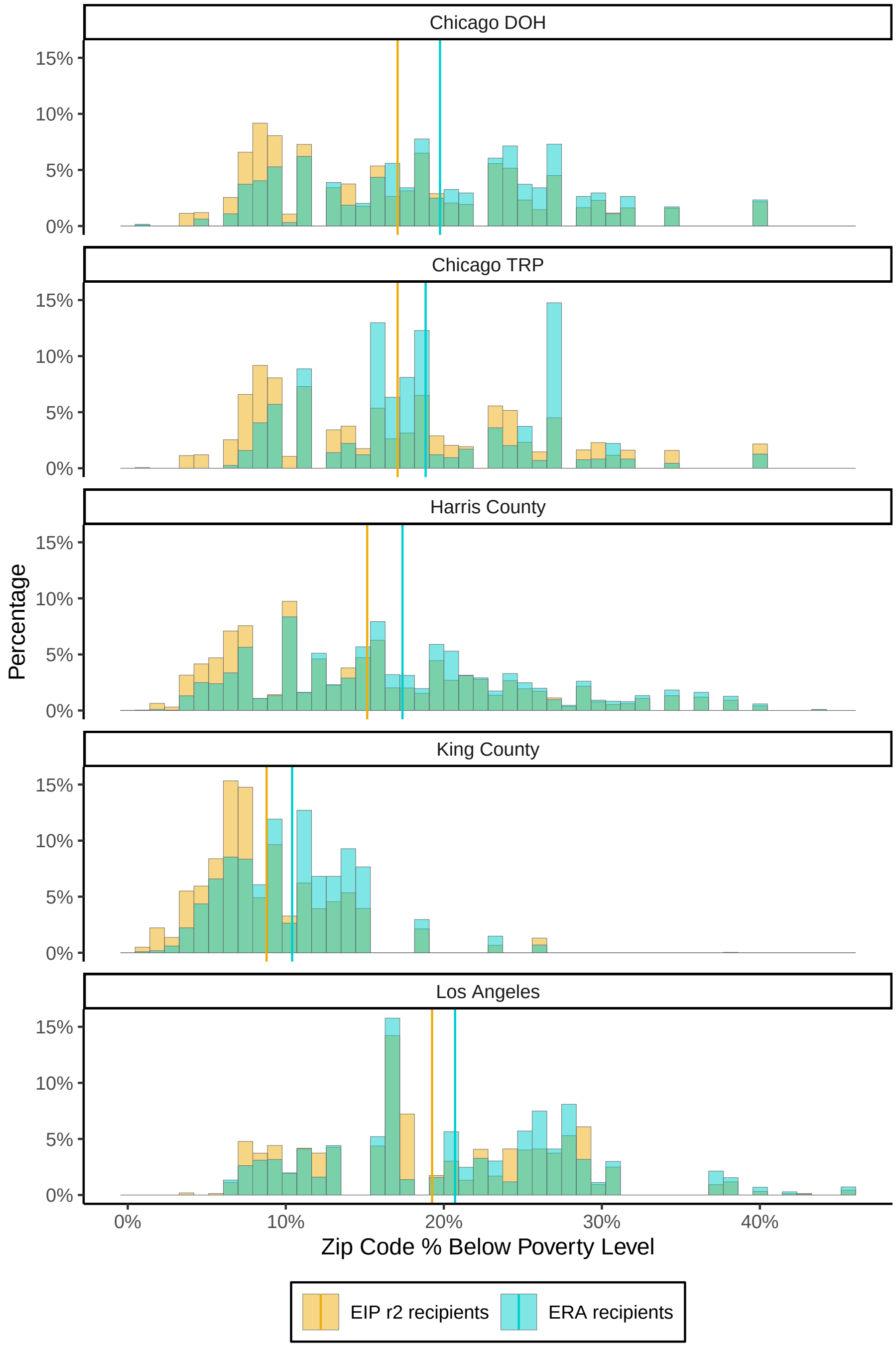
Notes: This figure shows the distribution of ZIP-level minority share for Emergency Rental Assistance recipients (cyan) and Economic Impact Payments Round 1 recipients (yellow). The minority share measure is from the 2020 Census Demographic and Housing Characteristics File and is calculated as the percentage of the population within a ZIP code that identifies as a minority (U.S. Census Bureau, 2020a). Minority status is determined based on self-reported race and ethnicity, with individuals identifying as non-white or Hispanic considered to be minorities. EIP r1 data is from the 2020 Internal Revenue Service Statistics of Income and measures the number of income tax returns filed in 2020 that received round one EIP provided under the CARES Act (Internal Revenue Service, 2020). The vertical lines indicate the mean minority share for recipients in each group within each site.

Appendix Figure E.2
Targeting: ERA Recipients Vs. Economic Impact Payments Round 1 Recipients (ZIP-Level Hispanic Share)



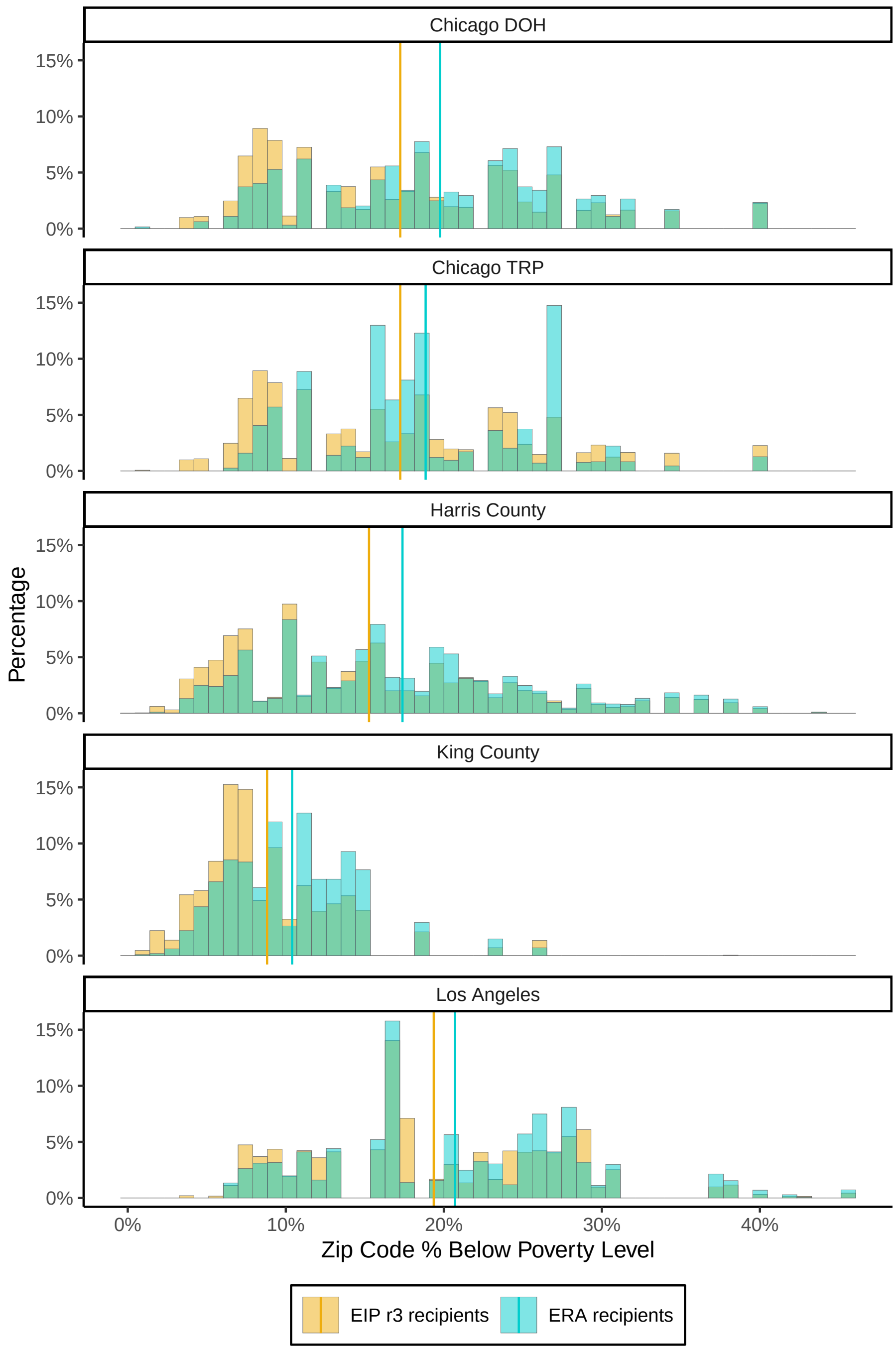
Notes: This figure shows the distribution of ZIP-level Hispanic share for Emergency Rental Assistance recipients (cyan) and Economic Impact Payments Round 1 recipients (yellow). The Hispanic share measure is from the 2020 Census Demographic and Housing Characteristics File and is calculated as the percentage of the population within a ZIP code that identifies as Hispanic (U.S. Census Bureau, 2020a). Hispanic status is determined based on self-reported ethnicity. EIP r1 data is from the 2020 Internal Revenue Service Statistics of Income and measures the number of income tax returns filed in 2020 that received round one EIP provided under the CARES Act (Internal Revenue Service, 2020). The vertical lines indicate the mean Hispanic share for recipients in each group within each site.

Appendix Figure E.3
Targeting: ERA Recipients Vs. Economic Impact Payments Round 2 Recipients (ZIP-Level Poverty Rate)



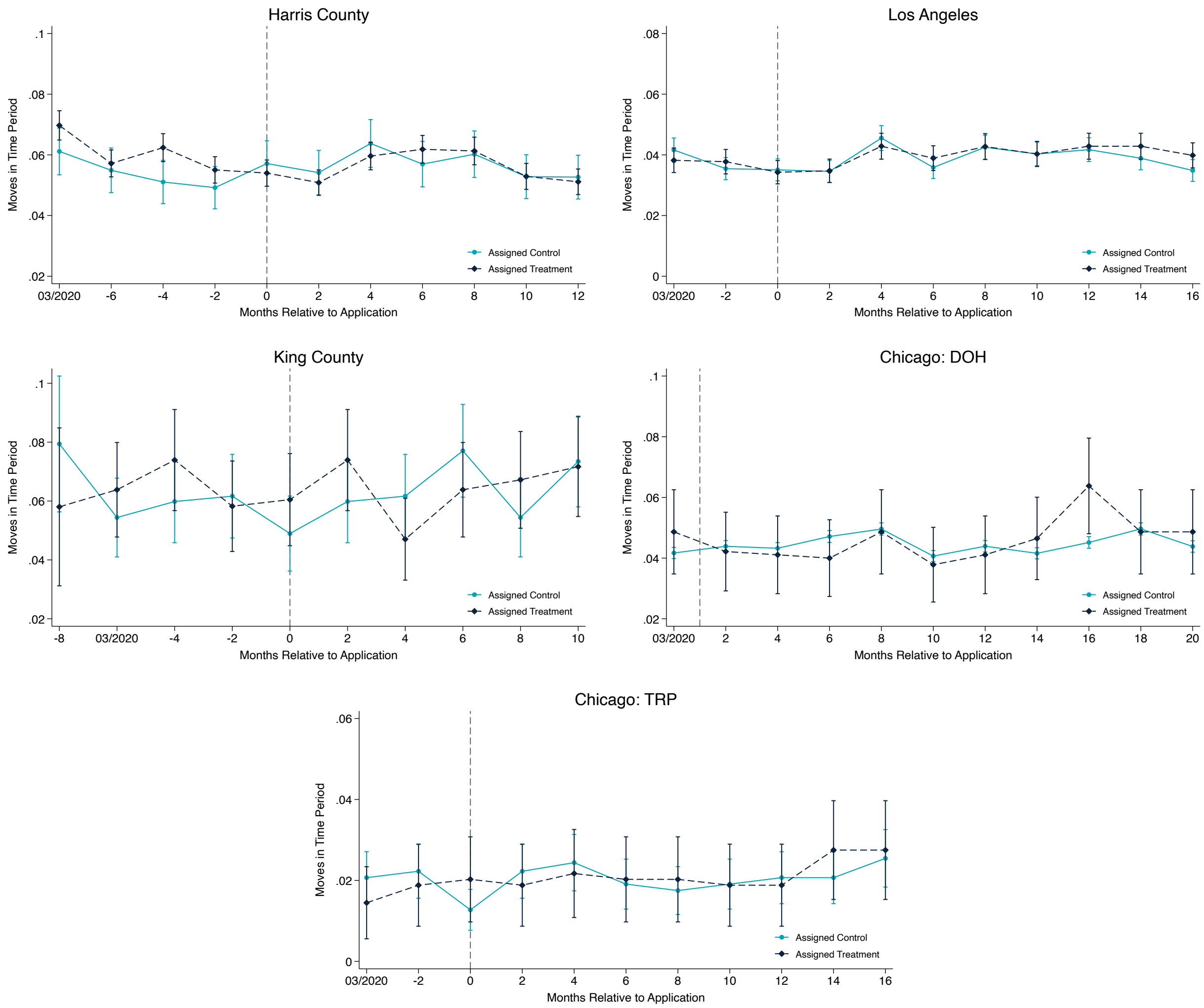
Notes: This figure shows the distribution of ZIP-level population poverty status rates for Emergency Rental Assistance recipients (cyan) and Economic Impact Payments Round 2 recipients (yellow). The poverty rate measure is from the 2020 American Community Survey 5-Year Estimates and is the percentage of the population for whom poverty status is determined (U.S. Census Bureau, 2020b). EIP r2 data is from the 2020 Internal Revenue Service Statistics of Income and measures the number of income tax returns filed in 2020 that received round two EIP provided under the COVID-related Tax Relief Act of 2020 (Internal Revenue Service, 2021). The vertical lines indicate the mean poverty rate for recipients in each group within each site.

Appendix Figure E.4
Targeting: ERA Recipients Vs. Economic Impact Payments Round 3 Recipients (ZIP-Level Poverty Rate)



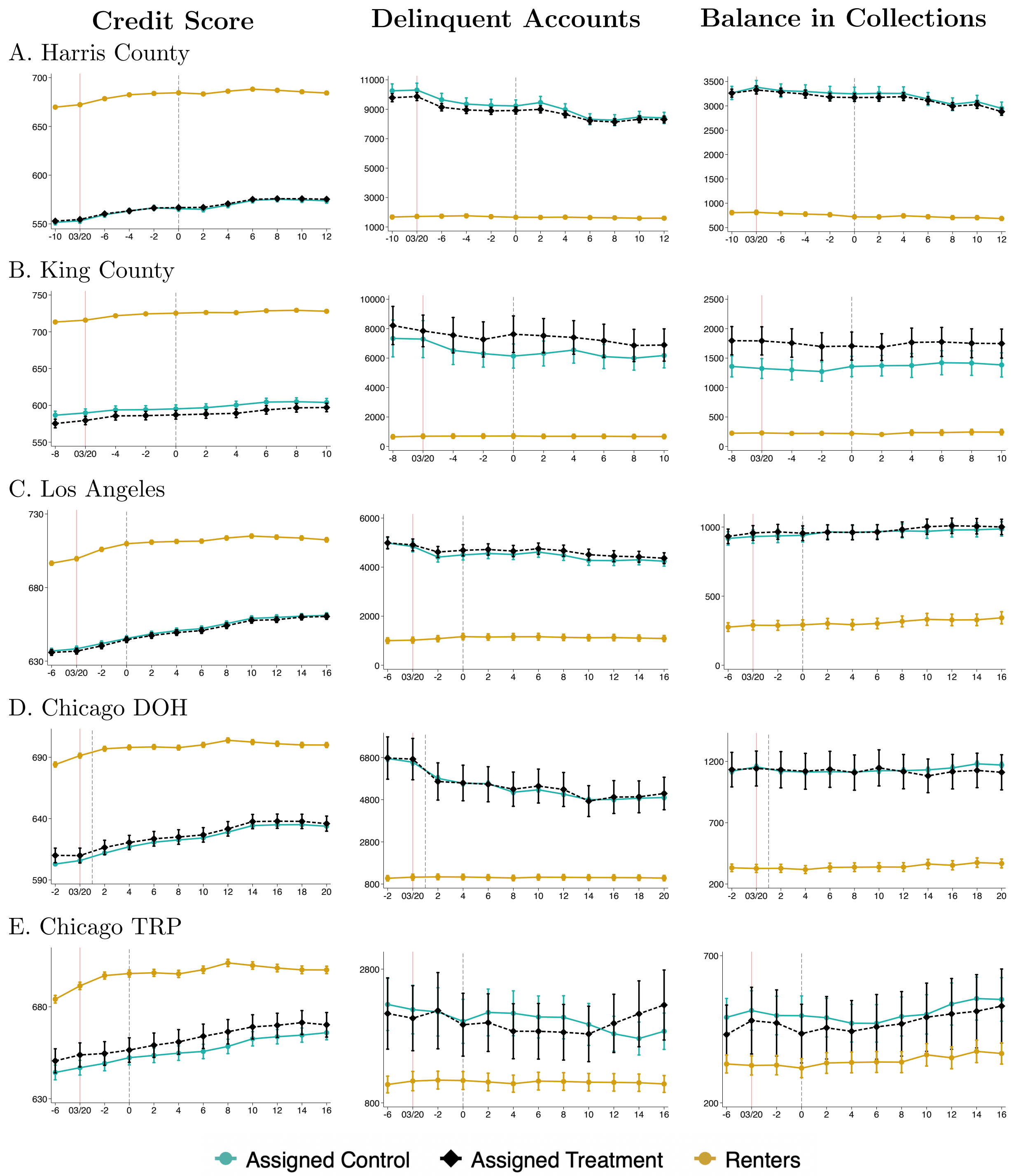
Notes: This figure shows the distribution of ZIP-level population poverty status rates for Emergency Rental Assistance recipients (cyan) and Economic Impact Payments Round 3 recipients (yellow). The poverty rate measure is from the 2020 American Community Survey 5-Year Estimates and is the percentage of the population for whom poverty status is determined. EIP r3 data is from the 2021 Internal Revenue Service Statistics of Income and measures the number of income tax returns filed in 2021 that received round three EIP provided under the American Rescue Plan Act of 2021 (Internal Revenue Service, 2021). The vertical lines indicate the mean poverty rate for recipients in each group within each site.

Appendix Figure E.5
Moves in Time Period



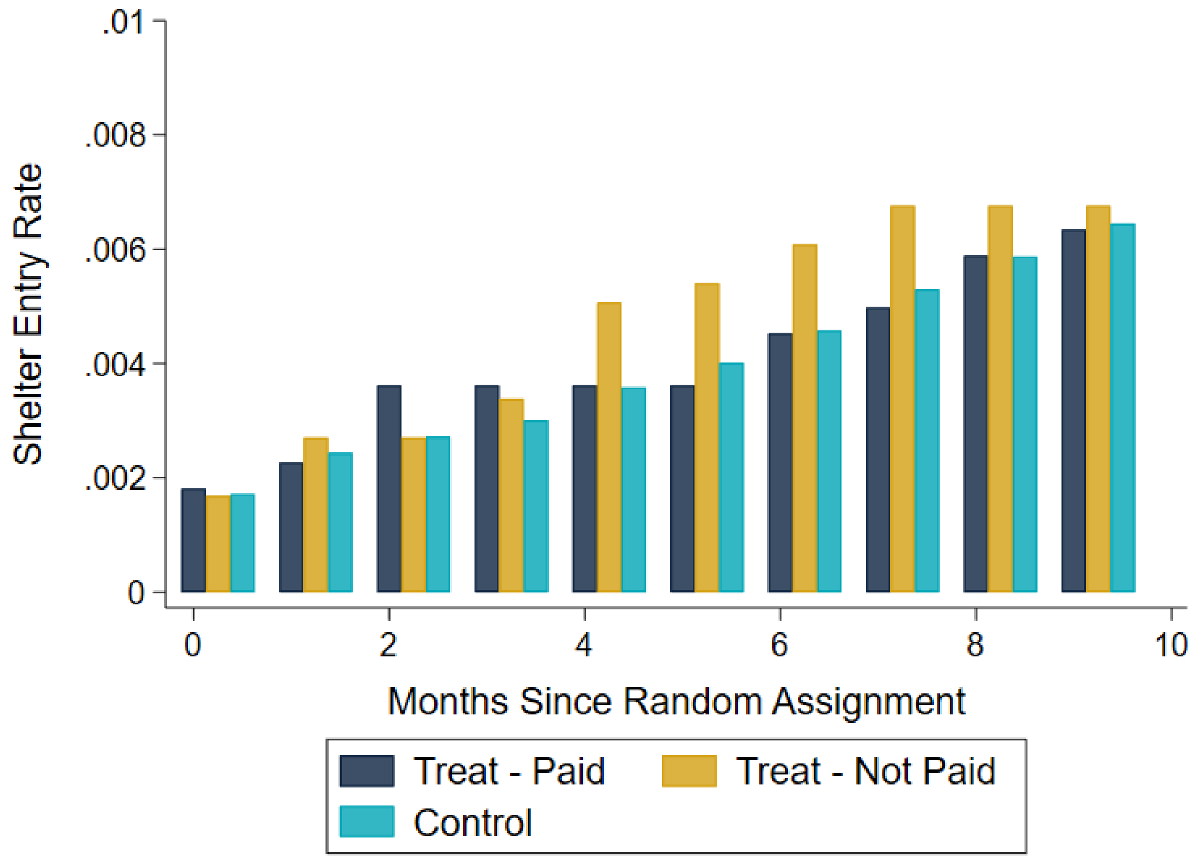
Notes: This figure plots the average number of moves for the treatment and control groups against the number of months since application. Moves are derived from address changes observed in the Experian data. The dashed vertical line indicates the month of application (month 0). Applications were submitted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

Appendix Figure E.6
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 E.7
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 E.6
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 Table E.7

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 E.8
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 E.9
Fraction of Participants Receiving Assistance in Future Rounds

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

Notes: This table reports the fraction of lottery participants who received assistance in a future round. The definition of the treatment and control 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 E.10
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
Recipient	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-20	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 E.11
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

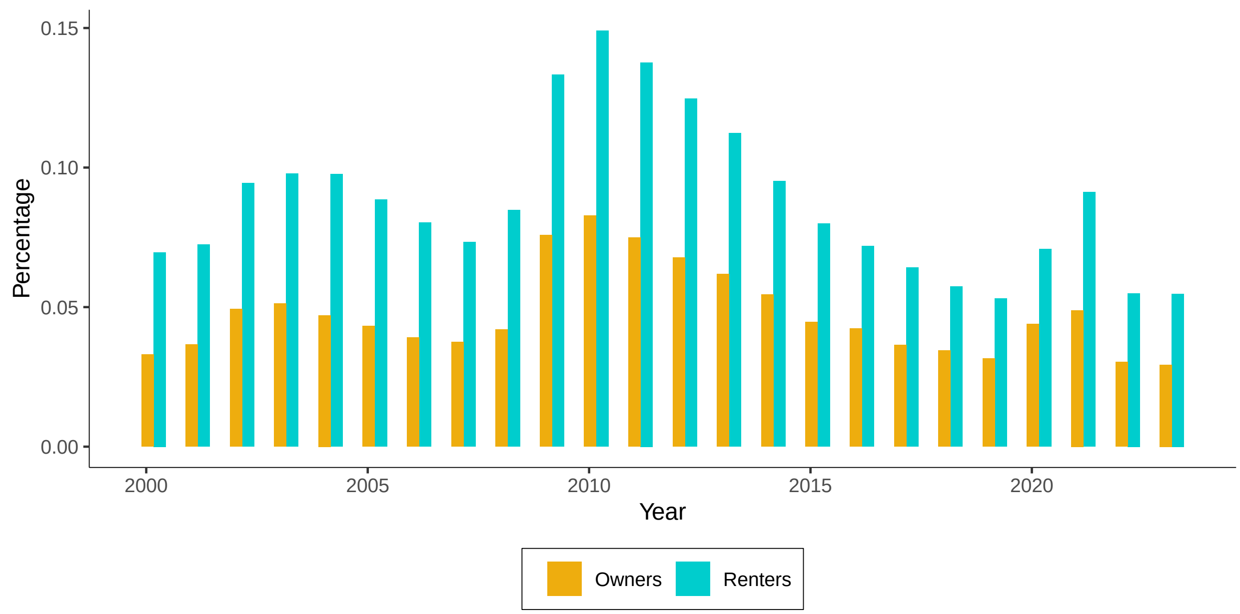
Notes: This table describes the types of agreements that tenants who missed rent payments reached with their landlords. These numbers are taken from the answers to Q38–Q40 in the Harris County and King County surveys. The first row is the fraction of respondents in each site who missed a payment and 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 E.12
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 E.8
Unemployment Rate by Tenure

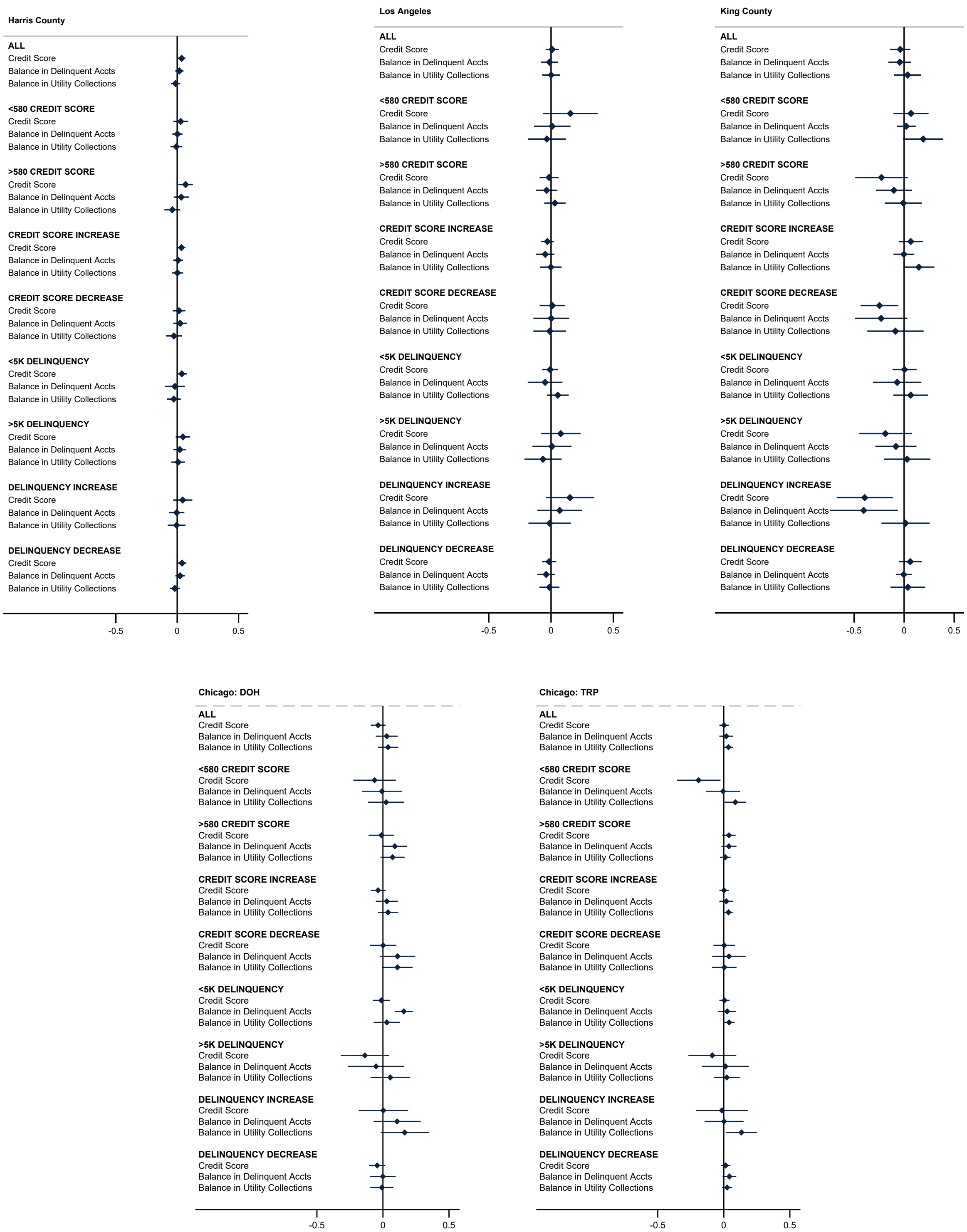


Notes: This figure shows unemployment rates for renters and homeowners. Source: Current Population Survey Data Annual Social and Economic Supplements (CPS ASEC).

F Heterogeneity-Analysis

F.1 Heterogeneity on individual observable characteristics

Appendix Figure F.1
Heterogeneity in Financial Health Outcomes by Baseline Financial Characteristics



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 Table F.1

Heterogeneity in Impacts on Expenditures and Health

	Expenditures		Health	
	All Rent Paid (1)	All Bills Paid (2)	COVID Positive (3)	Feeling Anxious (4)
Mental Health: Good	0.072***	0.003	−0.031**	−0.036*
	(0.020)	(0.022)	(0.014)	(0.021)
Mental Health: Bad	0.053	0.005	−0.010	−0.029
	(0.034)	(0.030)	(0.025)	(0.033)
Higher Income	0.058**	−0.019	−0.028**	−0.035
	(0.023)	(0.024)	(0.014)	(0.023)
Lower Income	0.060**	0.014	−0.025	−0.022
	(0.027)	(0.026)	(0.022)	(0.027)
Some Safety Net	0.082***	−0.016	−0.042***	−0.034
	(0.023)	(0.023)	(0.016)	(0.023)
No Safety Net	0.049*	0.023	−0.014	−0.024
	(0.026)	(0.028)	(0.019)	(0.027)
Employed	0.026	−0.010	−0.048***	−0.010
	(0.027)	(0.027)	(0.019)	(0.026)
Not Employed	0.077***	−0.005	−0.011	−0.059**
	(0.025)	(0.025)	(0.017)	(0.026)
Not Behind on Rent	0.071***	0.002	−0.032**	−0.054**
	(0.021)	(0.022)	(0.014)	(0.022)
Behind on Rent	0.040	0.018	−0.009	−0.005
	(0.030)	(0.029)	(0.024)	(0.030)
Moved Last Yr	0.072***	−0.020	−0.030**	−0.026
	(0.019)	(0.020)	(0.013)	(0.020)
Not Moved Last Yr	0.048	0.072*	−0.013	−0.075*
	(0.040)	(0.041)	(0.033)	(0.040)
N	8,594	8,778	8,064	9,554

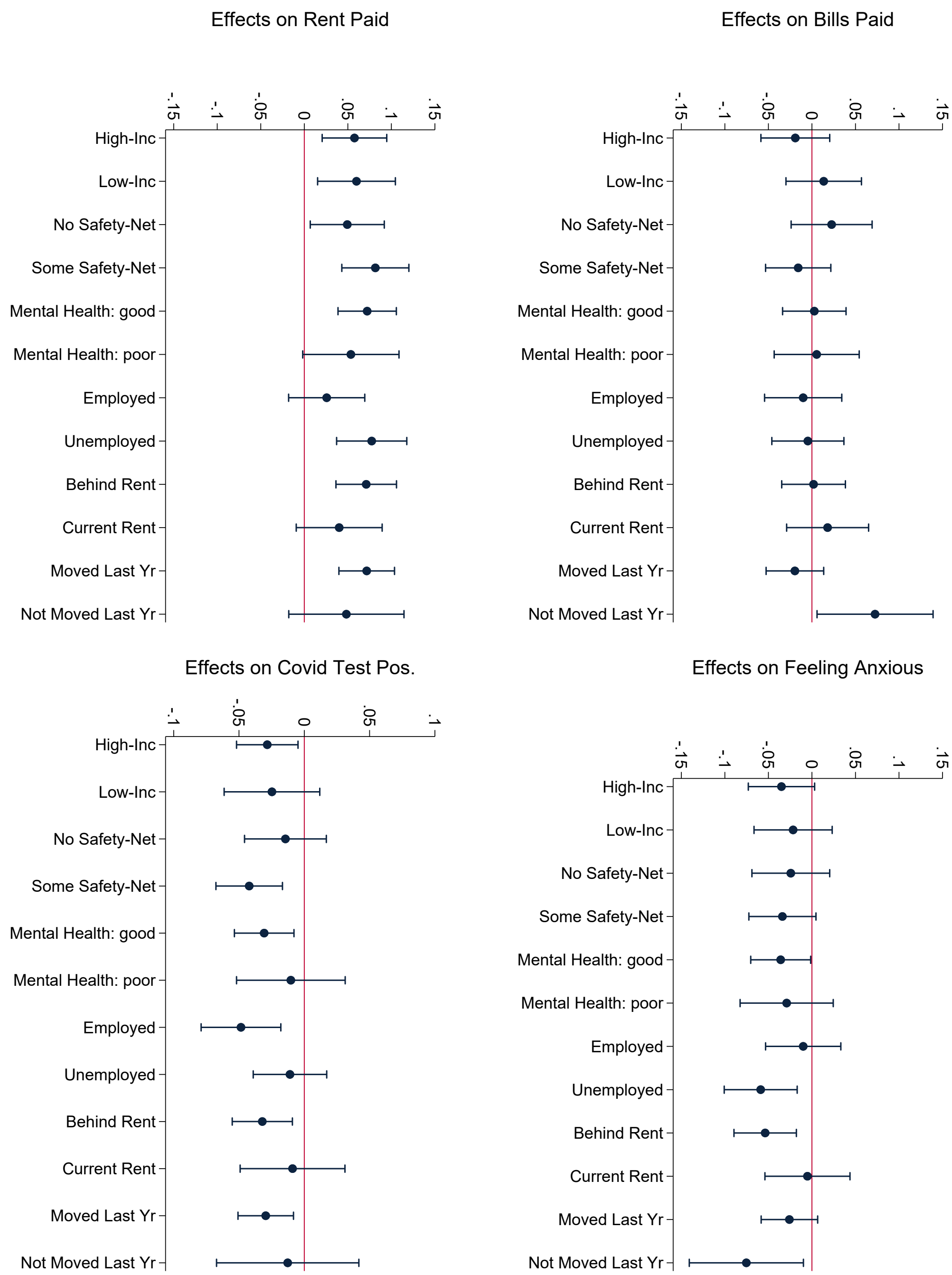
Notes: This table reports heterogeneity in the effects of assistance on survey measures of expenditures and health outcomes by reported pre-lottery characteristics. Each column is a different survey outcome. The reported estimates are combined inverse-variance weighted averages across all non-LA sites. Robust standard errors are reported in parentheses, and the control group mean is reported in brackets (this corresponds to the mean for the group not offered assistance, i.e., $Z = 0$). Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table F.2
Heterogeneity in Impacts on Economic Insecurity

	Economic Insecurity			
	Worried Abt. Eviction (1)	Expeienced Homeless-ness (2)	Stayed in Homeless Shelter (3)	Was Food Insecure (4)
Mental Health: Good	−0.022	−0.019**	−0.002	−0.033*
	(0.016)	(0.009)	(0.004)	(0.018)
Mental Health: Bad	−0.069**	−0.012	−0.002	0.002
	(0.031)	(0.025)	(0.007)	(0.033)
Higher Income	−0.013	−0.017	−0.002	−0.033*
	(0.018)	(0.011)	(0.003)	(0.020)
Lower Income	−0.077***	−0.017	0.002	0.011
	(0.025)	(0.019)	(0.005)	(0.027)
Some Safety Net	−0.019	0.002	0.002	−0.005
	(0.020)	(0.015)	(0.003)	(0.021)
No Safety Net	−0.047**	−0.039***	−0.005***	−0.048*
	(0.022)	(0.011)	(0.001)	(0.025)
Employed	−0.015	−0.024*	−0.003	−0.032
	(0.023)	(0.013)	(0.005)	(0.024)
Not Employed	−0.036*	−0.013	0.001	0.011
	(0.021)	(0.016)	(0.003)	(0.023)
Not Behind on Rent	−0.023	−0.020**	0.003	−0.016
	(0.018)	(0.009)	(0.004)	(0.020)
Behind on Rent	−0.067***	−0.005	−0.007	−0.038
	(0.026)	(0.022)	(0.006)	(0.029)
Moved Last Yr	−0.036**	−0.024***	−0.003	−0.032*
	(0.016)	(0.008)	(0.004)	(0.018)
Not Moved Last Yr	−0.051	0.029	0.010	0.020
	(0.037)	(0.032)	(0.011)	(0.039)
N	8,478	9,539	9,310	9,562

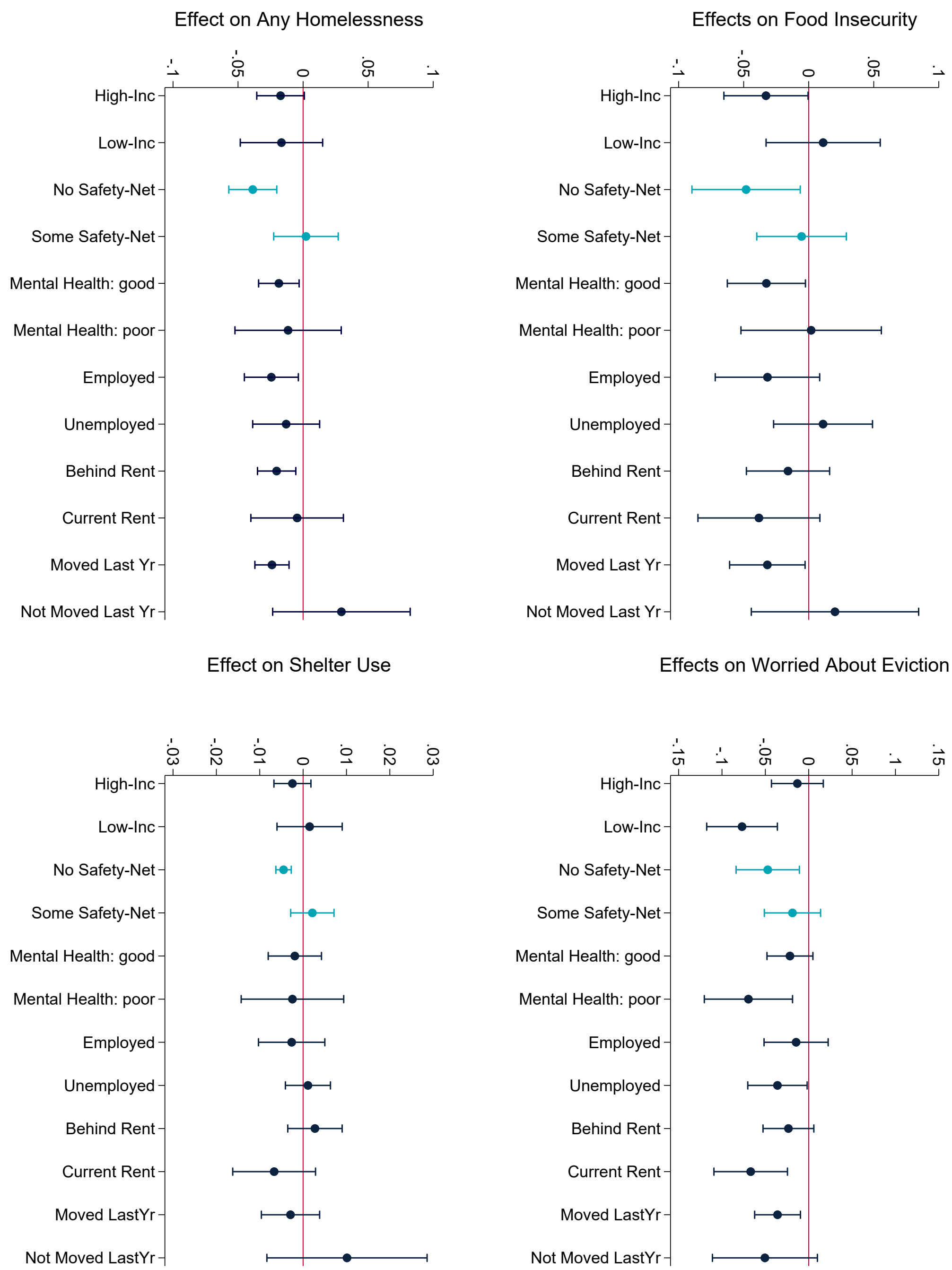
Notes: This table reports heterogeneity in the effects of assistance on survey measures of economic insecurity by reported pre-lottery characteristics. Each column is a different survey outcome. The reported estimates are combined inverse-variance weighted averages across all non-LA sites. Robust standard errors are reported in parentheses, and the control group mean is reported in brackets (this corresponds to the mean for the group not offered assistance, i.e., $Z = 0$). Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Figure F.2
Survey Heterogeneity: Expenditure and Health Outcomes



Notes: This figure plots the combined IV estimates of the effects of assistance across sites on survey outcomes for various subgroups. The subgroups appear in the rows of each figure. The outcome is listed in the column. The combined estimates are inverse-variance weighted averages across sites. The whiskers are 90 percent confidence intervals.

Appendix Figure F.3
Survey Heterogeneity: Economic Insecurity Outcomes



Notes: This figure plots the combined IV estimates of the effects of assistance across sites on survey outcomes for various subgroups. The subgroups appear in the rows of each figure. The outcome is listed in the column. The combined estimates are inverse-variance weighted averages across sites. The whiskers are 90 percent confidence intervals.

Appendix Table F.3

Heterogeneity in Impacts on Expenditures and Health (Equally-Weighted by Lottery)

	Expenditures		Health	
	All Rent Paid (1)	All Bills Paid (2)	COVID Positive (3)	Feeling Anxious (4)
Mental Health: Good	0.088***	0.010	−0.033	−0.042*
	(0.025)	(0.024)	(0.024)	(0.025)
Mental Health: Bad	0.041	−0.001	−0.017	−0.026
	(0.036)	(0.031)	(0.034)	(0.034)
Higher Income	0.077***	−0.012	−0.043	−0.053*
	(0.029)	(0.027)	(0.035)	(0.028)
Lower Income	0.063**	0.020	−0.018	−0.010
	(0.029)	(0.028)	(0.026)	(0.029)
Some Safety Net	0.090***	−0.013	−0.036	−0.032
	(0.026)	(0.024)	(0.025)	(0.026)
No Safety Net	0.067**	0.020	−0.020	−0.025
	(0.031)	(0.030)	(0.028)	(0.032)
Employed	0.063**	−0.003	−0.053*	−0.028
	(0.031)	(0.028)	(0.031)	(0.030)
Not Employed	0.077***	−0.004	−0.013	−0.051*
	(0.026)	(0.025)	(0.023)	(0.027)
Not Behind on Rent	0.076***	0.004	−0.006	−0.050*
	(0.026)	(0.023)	(0.027)	(0.026)
Behind on Rent	0.050	0.010	−0.026	−0.014
	(0.032)	(0.029)	(0.029)	(0.031)
Moved Last Yr	0.085***	−0.017	−0.015	−0.033
	(0.023)	(0.021)	(0.023)	(0.023)
Not Moved Last Yr	0.070*	0.081**	−0.035	−0.079*
	(0.042)	(0.041)	(0.040)	(0.041)
N	8,594	8,778	8,064	9,554

Notes: This table reports heterogeneity in the effects of assistance on survey measures of expenditures and health outcomes by reported pre-lottery characteristics. Each column is a different survey outcome. The reported estimates are combined equally-weighted averages across all non-LA lotteries. Robust standard errors are reported in parentheses, and the control group mean is reported in brackets (this corresponds to the mean for the group not offered assistance, i.e., $Z = 0$). Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table F.4
Heterogeneity in Impacts on Economic Insecurity (Equally-Weighted by Lottery)

	Economic Insecurity			
	Worried Abt. Eviction (1)	Expeienced Homeless-ness (2)	Stayed in Homeless Shelter (3)	Was Food Insecure (4)
Mental Health: Good	−0.032	−0.021	−0.006	−0.032
	(0.023)	(0.017)	(0.004)	(0.023)
Mental Health: Bad	−0.070**	−0.015	0.003	0.020
	(0.035)	(0.026)	(0.009)	(0.035)
Higher Income	−0.019	−0.012	−0.007	−0.047*
	(0.026)	(0.019)	(0.005)	(0.025)
Lower Income	−0.086***	−0.030	0.000	0.030
	(0.028)	(0.021)	(0.007)	(0.028)
Some Safety Net	−0.042*	−0.007	0.002	0.007
	(0.025)	(0.019)	(0.006)	(0.024)
No Safety Net	−0.050	−0.038	−0.009	−0.059**
	(0.031)	(0.024)	(0.006)	(0.030)
Employed	−0.047	−0.011	−0.003	−0.031
	(0.029)	(0.020)	(0.005)	(0.028)
Not Employed	−0.049*	−0.022	−0.005	0.014
	(0.025)	(0.019)	(0.006)	(0.025)
Not Behind on Rent	−0.051**	−0.010	0.007	0.001
	(0.025)	(0.019)	(0.006)	(0.024)
Behind on Rent	−0.082***	−0.005	−0.004	−0.040
	(0.030)	(0.024)	(0.008)	(0.030)
Moved Last Yr	−0.075***	−0.022	−0.006	−0.027
	(0.022)	(0.014)	(0.004)	(0.021)
Not Moved Last Yr	−0.040	0.031	0.013	0.015
	(0.040)	(0.035)	(0.012)	(0.040)
N	8,478	9,539	9,310	9,562

Notes: This table reports heterogeneity in the effects of assistance on survey measures of economic insecurity by reported pre-lottery characteristics. Each column is a different survey outcome. The reported estimates are combined equally-weighted averages across all non-LA lotteries. Robust standard errors are reported in parentheses, and the control group mean is reported in brackets (this corresponds to the mean for the group not offered assistance, i.e., $Z = 0$). Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

F.2 Cross-Site Heterogeneity

F.2.1 Methodology and Notation

This appendix section draws on tools from meta-analysis to test for site-specific heterogeneity using a random-effects framework. Following DerSimonian and Laird (1986) (DL), we model site-specific heterogeneity as a random effect, which we estimate with noise due to sampling variation.[34] One advantage of this approach is that, as it is a method-of-moments estimator, we do not need to make distributional assumptions about the true underlying site-specific effects.

Consider our site-level estimates $\{\hat{\beta}_i\}_{i=1}^k$ with sampling variances $\{s_i^2\}$. Following a standard random-effects model, we will decompose each observed estimate into a site-specific "true" effect plus sampling error:

$$\begin{aligned} \hat{\beta}_i &= \beta_i + \varepsilon_i, \qquad \varepsilon_i \overset{\text{approx}}{\sim} N(0, s_i^2), \\ \beta_i &= \beta + e_i, \qquad \mathbb{E}[e_i] = 0, \quad \mathrm{Var}(e_i) = \tau^2, \end{aligned}$$

so $\hat{\beta}_i \overset{\text{approx}}{\sim} N(\beta,\, s_i^2 + \tau^2)$. Here, β is $E[\beta_i]$ and τ^2 captures between-site heterogeneity.

Estimating τ^2: Following DL, we use a method-of-moments estimator based on Cochran's Q statistic computed under fixed-effect weights $w_i \equiv 1/s_i^2$. Defined as

$$\hat{\beta} \equiv \frac{\sum_{i=1}^k w_i \hat{\beta}_i}{\sum_{i=1}^k w_i}, \qquad Q \equiv \sum_{i=1}^k w_i \big(\hat{\beta}_i - \hat{\beta}\big)^2, \qquad C \equiv \sum_{i=1}^k w_i - \frac{\sum_{i=1}^k w_i^2}{\sum_{i=1}^k w_i}.$$

With $df = k - 1$, the DL estimator is

$$\hat{\tau}^2_{\mathrm{DL}} \;=\; \max\left\{0,\; \frac{Q - df}{C}\right\}.$$

Pooled Effect, Standard Error, and Inference: Given $\hat{\tau}^2_{\mathrm{DL}}$, define random-effects inverse-variance weights

$$w_i^* \;\equiv\; \frac{1}{s_i^2 + \hat{\tau}^2_{\mathrm{DL}}}.$$

The pooled estimator of β is the weighted mean

$$\hat{\beta} \;=\; \frac{\sum_{i=1}^k w_i^* \hat{\beta}_i}{\sum_{i=1}^k w_i^*}, \qquad \mathrm{SE}\Big(\hat{\beta}\Big) \;=\; \left(\sum_{i=1}^k w_i^*\right)^{-1/2}.$$

Testing for heterogeneity: Cochran's Q tests homogeneity and is approximately χ^2_{df} under $H_0 : \tau^2 = 0$, allowing us to test the null of no heterogeneity in effects across sites. We report the test statistic and p-value for each outcome.

[34] See also Jackson and Mackevicius (2024) and Jackson (2025), who implement similar analyses.

Appendix Table F.5
Testing for cross-site heterogeneity in survey outcomes

	Meta-analysis					
outcome	$\hat{\beta}$	$SE(\hat{\beta})$	p-value	$\hat{\tau}^2$	Q	Q p-value
All Rent Paid	0.070	0.017	>0.000	0.0000	1.785	0.618
All Bills Paid	-0.003	0.017	0.883	0.0000	1.894	0.595
COVID Positive	-0.031	0.021	0.139	0.0007	5.455	0.141
Feeling Anxious	-0.035	0.023	0.133	0.0009	5.063	0.167
Worried About Eviction	-0.037	0.014	0.010	0.0000	2.514	0.473
Experienced Homelessness	-0.016	0.010	0.096	0.0000	0.898	0.826
Stayed in a Homeless Shelter	-0.002	0.004	0.716	0.0000	5.288	0.152
Was Food Insecure	-0.026	0.016	0.101	0.0000	1.877	0.598

Notes: This table reports the output from a random-effects model designed to test for heterogeneity across sites following Jackson (2025) using DerSimonian and Laird (1986) for survey outcomes. Column one reports $\hat{mu}$ reports the pooled effect across sites, while columns 2 and 3 report the standard error and p-value for the pooled effect. Column four reports $\hat{\tau}^2$, the estimate of the variance in true site-specific effects. Columns five and six report Cochran's Q and its p-value under the null that $\tau^2 = 0$.

F.2.2 Site heterogeneity-analysis results

Tables F.5–F.9 report the results of our analysis of cross-site heterogeneity. For all survey outcomes, we fail to reject the null that $\tau^2 = 0$, with the strongest evedince of cross-site heterogeneity for the "COVID Positive," "Feeling Anxious," and "Stayed in a Homeless Shelter" outcomes. We similarly fail to reject the null for short-run financial health outcomes and short- and long-run address mobility. For longer-run financial health outcomes we reject the null for Credit Scores and on Any Personal Bankruptcy. Overall, we interpret our findings from this analysis as providing little evidence of site-specific heterogeneity, except for some indication that sites may have differed in their impacts on longer-term financial health. The null findings may be partly driven by the modest number of sites in our study.

Appendix Table F.6
Testing for cross-site heterogeneity in financial health outcomes (2 months)

	Meta-analysis					
outcome	$\hat{\beta}$	$SE(\hat{\beta})$	p-value	$\hat{\tau}^2$	Q	Q p-value
Credit Score	0.514	1.166	0.659	2.3010	6.187	0.186
Balance Across all Trades ($1,000s)	-0.020	0.477	0.966	0.2441	5.043	0.283
Balance in Collections ($1,000s)	-0.034	0.034	0.321	0.0021	6.369	0.173
Balance in Utility Collections ($1,000s)	-0.011	0.007	0.133	0.0000	3.392	0.494
Balance in Delinquent Accounts ($1,000s)	-0.136	0.134	0.308	0.0000	1.730	0.785
Any Auto Loan or Lease	-0.000	0.005	0.991	0.0000	1.505	0.826
Any Personal Bankruptcy	-0.000	0.001	0.870	0.0000	4.718	0.317
Any Open Revolving Line of Credit	0.004	0.006	0.498	0.0000	5.640	0.228

Notes: This table reports the output from a random-effects model designed to test for heterogeneity across sites following Jackson (2025) using DerSimonian and Laird (1986) for short-run financial health outcomes. Column one reports $\hat{mu}$ reports the pooled effect across sites, while columns 2 and 3 report the standard error and p-value for the pooled effect. Column four reports $\hat{\tau}^2$, the estimate of the variance in true site-specific effects. Columns five and six report Cochran's Q and its p-value under the null that $\tau^2 = 0$.

Appendix Table F.7
Testing for cross-site heterogeneity in financial health outcomes (10 months)

	Meta-analysis					
outcome	$\hat{\beta}$	$SE(\hat{\beta})$	p-value	$\hat{\tau}^2$	Q	Q p-value
Credit Score	-1.714	2.008	0.393	10.7796	9.839	0.043
Balance Across all Trades ($1,000s)	-0.258	0.630	0.682	0.0000	2.242	0.691
Balance in Collections ($1,000s)	0.013	0.034	0.689	0.0000	0.297	0.990
Balance in Utility Collections ($1,000s)	-0.012	0.010	0.229	0.0000	0.841	0.933
Balance in Delinquent Accounts ($1,000s)	0.053	0.154	0.728	0.0000	2.488	0.647
Any Auto Loan or Lease	-0.002	0.007	0.812	0.0000	0.806	0.938
Any Personal Bankruptcy	-0.003	0.004	0.478	0.0000	12.419	0.014
Any Open Revolving Line of Credit	-0.008	0.007	0.219	0.0000	3.952	0.413

Notes: This table reports the output from a random-effects model designed to test for heterogeneity across sites following Jackson (2025) using DerSimonian and Laird (1986) for longer-run financial health outcomes. Column one reports $\hat{mu}$ reports the pooled effect across sites, while columns 2 and 3 report the standard error and p-value for the pooled effect. Column four reports $\hat{\tau}^2$, the estimate of the variance in true site-specific effects. Columns five and six report Cochran's Q and its p-value under the null that $\tau^2 = 0$.

Appendix Table F.8
Testing for cross-site heterogeneity in mobility (2 months)

	Meta-analysis					
outcome	$\hat{\beta}$	$SE(\hat{\beta})$	p-value	$\hat{\tau}^2$	Q	Q p-value
Change of Address Infutor	-0.001	0.002	0.714	0.0000	0.487	0.975
Change of Address Experian	-0.003	0.006	0.539	0.0000	1.712	0.788

Notes: This table reports the output from a random-effects model designed to test for heterogeneity across sites following Jackson (2025) using DerSimonian and Laird (1986) for short-run residential mobility outcomes. Column one reports $\hat{mu}$ reports the pooled effect across sites, while columns 2 and 3 report the standard error and p-value for the pooled effect. Column four reports $\hat{\tau}^2$, the estimate of the variance in true site-specific effects. Columns five and six report Cochran's Q and its p-value under the null that $\tau^2 = 0$.

Appendix Table F.9
Testing for cross-site heterogeneity in mobility (10 months)

	Meta-analysis					
outcome	$\hat{\beta}$	$SE(\hat{\beta})$	p-value	$\hat{\tau}^2$	Q	Q p-value
Change of Address Infutor	-0.016	0.005	0.003	0.0000	1.280	0.865
Change of Address Experian	0.006	0.009	0.525	0.0000	1.768	0.778

Notes: This table reports the output from a random-effects model designed to test for heterogeneity across sites following Jackson (2025) using DerSimonian and Laird (1986) for longer-run residential mobility outcomes. Column one reports $\hat{mu}$ reports the pooled effect across sites, while columns 2 and 3 report the standard error and p-value for the pooled effect. Column four reports $\hat{\tau}^2$, the estimate of the variance in true site-specific effects. Columns five and six report Cochran's Q and its p-value under the null that $\tau^2 = 0$.