Analysis Plan

Project Name: Does Reducing Documentation Burden Increase Access to Emergency Rental Assistance? Quasi-experimental evidence from Kentucky

Project Code: 2305

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Preregistration Details

This Analysis Plan will be posted on the OES website at oes.gsa.gov before outcome data are received and analyzed.

Analysis Plan Summary

This evaluation is part of the Office of Evaluation Sciences (OES) American Rescue Plan Act of 2021 (ARP) portfolio. The ARP was designed to address immediate needs related to the pandemic, with a specific focus on addressing historically disparate outcomes across race, class, and geography that were further exacerbated by the pandemic. As federal programs are innovating and finding new ways to achieve these goals, the OES portfolio of evaluations will measure whether ARP-funded interventions are working as intended and share lessons learned.

In support of the ARP Equity Learning Agenda, OES is working with agency partners to better understand how to improve awareness, access, and allocation of ARP programs and resources, focusing on ARP programs with equity goals. This set of evaluations will be intentional and strategic in building evidence to understand the role of ARP programs and supported interventions in improving outcomes for historically underserved populations.

This analysis plan describes a quasi-experimental evaluation of the impact of simplifying documentation requirements when applying for Emergency Rental Assistance. We examine the effects of a "fact-specific proxy" (FSP) introduced by the Commonwealth of Kentucky's Housing Corporation (KHC) to streamline access to assistance. All ZIP Code Tabulation Areas (ZCTAs — referred to as ZIP codes throughout) whose Census-defined renter median income was lower than the 80% area median income limit defined by the U.S. Department of Housing and Urban Development (HUD) were deemed FSP-eligible. For applications originating from FSP-eligible ZIP codes, file processors (administrative staff who processed applications) could use the applicant's ZIP code as a proxy for income eligibility, simplifying the process of verifying income eligibility. While this change was not visible to applicants and therefore likely did not impact application decisions, simplifying income eligibility verification represents a substantial burden reduction for file processors that may have enabled them to get funds out to more people, more quickly, especially to disadvantaged renters who may be disproportionately impacted by administrative burdens such as income verification. Our main research question is: to what extent does simplifying the process to determine applicants' income eligibility increase access to ERA? We intend to analyze application data in order to answer this question.

Project Description

The Consolidated Appropriations Act (2021) and the American Rescue Plan Act (2021) created the Emergency Rental Assistance (ERA) Programs (known as ERA1 and ERA2, respectively), making approximately $46B in funding available to cities, counties, tribal communities (for ERA1), the District of Columbia, U.S. Territories, and states ("grantees") to assist households that experience financial hardship to pay rent or utilities, with the goal of preventing eviction or housing instability in the wake of the pandemic. The program provided financial assistance to renters and landlords for rent, utilities, and other housing related expenses. Renters had to meet eligibility criteria to receive assistance, outlined as follows:

1. At risk of housing instability or homelessness;
2. Experience of financial hardship due, directly or indirectly, to COVID-19 (ERA1); or experience of financial hardship during or due, directly or indirectly, to COVID-19 (ERA2);
3. Have income that falls below an area-specific threshold (often referred to as the AMI or Area Median Income)

Grantees had latitude in how they could design their programs, and notably took advantage of program flexibilities that were highlighted by the US Department of the Treasury for distributing ERA more quickly and equitably. Some examples included simplifying application forms, incorporating self-attestation of income (or self-certification), using fact-specific proxies to streamline application processing, using categorical eligibility, and adding additional prioritization tiers for those with highest needs. These innovations offered promising opportunities to learn what works to reduce documentation burdens for underserved groups to increase program access and/or successful receipt of funds.[1] The ARP Equity Learning Agenda identifies learning opportunities about ERA program flexibilities: "*To what extent did low income renters benefit from the administrative flexibilities (such as self-attestation) that Treasury made available to Emergency Rental Assistance grantees?*" ?" The Office of Recovery Programs Learning Agenda asks: "How has the use of promising practices that Treasury encouraged grantees to adopt (such as self-attestation, categorical eligibility, and fact-specific proxies) affected the equitable distribution of ERA funds?"

Kentucky's Housing Corporation (KHC) was an early adopter of program flexibilities to streamline application processing. Using a fact-specific proxy (FSP), KHC simplified income eligibility verification for applicants in FSP-eligible ZIP codes.

[1] Such documentation burdens constitute what the Office of Management and Budget considers administrative burdens. Others (in addition to time spent on applications and paperwork) include factors like time spent traveling to in-person visits, answering notices and phone calls to verify eligibility, navigating web interfaces, and collecting any documentation required to prove eligibility.

Status quo before the program change

The Commonwealth of Kentucky entered into an MOA with KHC and the Public Protection Cabinet (PPC) for the administration and implementation of the Healthy at Home Eviction Relief Fund (HHERF) with ERA1 funds. HHERF started in February of 2021. KHC and PPC crafted an online application for tenants and landlords as well as a back-end application file processing and reporting system. KHC onboarded approximately 40 temporary staff. KHC trained in-house and temporary employees on program design, policies, application file processing, and quality control. Program design also involved Kentucky's two other ERA grantees, Louisville-Jefferson and Lexington.

HHERF began accepting applications for ERA on February 15, 2021, and demand for the program was higher than expected. The first payments were sent out March 5, 2021. After that date, KHC sent out electronic ACH payments each Friday.

In addition to a written attestation of their income in the application form, tenants and landlords who applied needed to document their income eligibility using official and recent documents uploaded to the online system (e.g., a pay stub, W-2, other wage statement, tax filing, or third party income verification form). Landlords applying on their tenants' behalf would need to collect these documents from eligible tenants and submit them alongside their landlord application.[2] There was a significant backlog of applications awaiting processing, and a number of applications that lacked complete income documentation. When income documentation was missing, KHC staff had to follow up to get the right document; as a result, applications could not be processed while waiting for follow up by applicants. Reviewing income documentation was time consuming for application file processors. The FSP was intended to improve this situation by making it easier for file processors — KHC staff in charge of processing applications — to document applicants' income eligibility, thereby increasing the speed and overall amount of ERA1 assistance paid out.

How the fact-specific proxy (FSP) worked

In February 2021, the US Department of the Treasury made changes to the guidance for the ERA program, to provide additional flexibility with respect to documenting the eligibility of households. This program change allowed the use of program flexibilities such as an FSP (see detailed timeline in Figure 1).

KHC's use of a fact-specific proxy started with the development of a list of FSP-eligible ZIP codes in May of 2021. In order to determine this list, KHC compared median renter household income (US Census Bureau American Community Survey 2019 5-Year Estimates for ZIP Code Tabulation Areas in Kentucky) to 80% AMI (more specifically, the HUD Low Income Limit) for the county associated with the zip code. ZIP code eligibility data for the FSP were integrated into the HHERF Administrative Portal as an automatic look up. If the median renter household income in the specified ZIP Code Tabulation Area (ZCTA — referred to as ZIP code throughout) was lower than the 80% AMI limit for the associated county, KHC viewed the tenant households in that ZIP code

[2] As explained below, the data we will receive only pertains to tenant-initiated applications.

as income eligible via this fact-specific proxy (along with the tenant's attestation of their household income). In instances where a ZIP code spans two or more counties, the ZIP code is associated with the county in which a majority of the ZIP Code's population resides. ZIP codes whose median renter income estimate was unavailable because the survey sample size fell below Census data suppression thresholds were not eligible for FSP. There were 297 such ZIP codes.

An in-house attorney provided a written determination of reasonableness of the intended fact specific proxy approach. The FSP was integrated into the file processing system so that file processors were automatically notified of FSP eligibility. File processors would see a large, red message at the top of an application file that read, "Income Eligible Via Fact Specific Proxy". In early June of 2021, KHC adopted the FSP policy as a backup option for income eligibility. This meant that when an application had insufficient income documentation, KHC would use the FSP to determine income eligibility.

In July 2021, KHC determined that the FSP along with attestation would be used as the first approach when determining income eligibility to expedite application processing. Rather than using the FSP as a backup option for eligibility determination, KHC began using FSP along with attestation as a primary method. Thus, no matter the documentation provided, if a tenant attested to having household income at or below 80% AMI and resided in an FSP-eligible ZIP code, file processors used the FSP to corroborate income eligibility without further follow up. Figure 2 shows how processors reviewed documentation required for differently-situated applicants from July 2021 onwards.

Importantly for our analytic strategy, KHC confirmed that there were no substantive changes to the public-facing application portal after the launch of the FSP. Further, KHC never publicized FSP eligibility status for individual applicants or ZIP codes in any way. This means that applicants were almost certainly unaware of their FSP status and income documentation requirements when applying. An implication is that we do not expect FSP to have altered potential applicants' decisions to apply, and so focus on post-application outcomes.

KHC made additional changes to their program in October 2021, when they brought on a vendor to process HHERF applications alongside existing staff. They also increased marketing efforts to targeted census tracts. In 2022, KHC continued using the FSP for their ERA2 program. They updated their FSP-eligible zip code list when Louisville-Jefferson County was added to the HHERF service area in May 2022. FSP continued as the primary means of income eligibility verification along with attestation up through the program close in March of 2023.

Figure 1. *Implementation of the Healthy at Home Eviction Relief Fund*

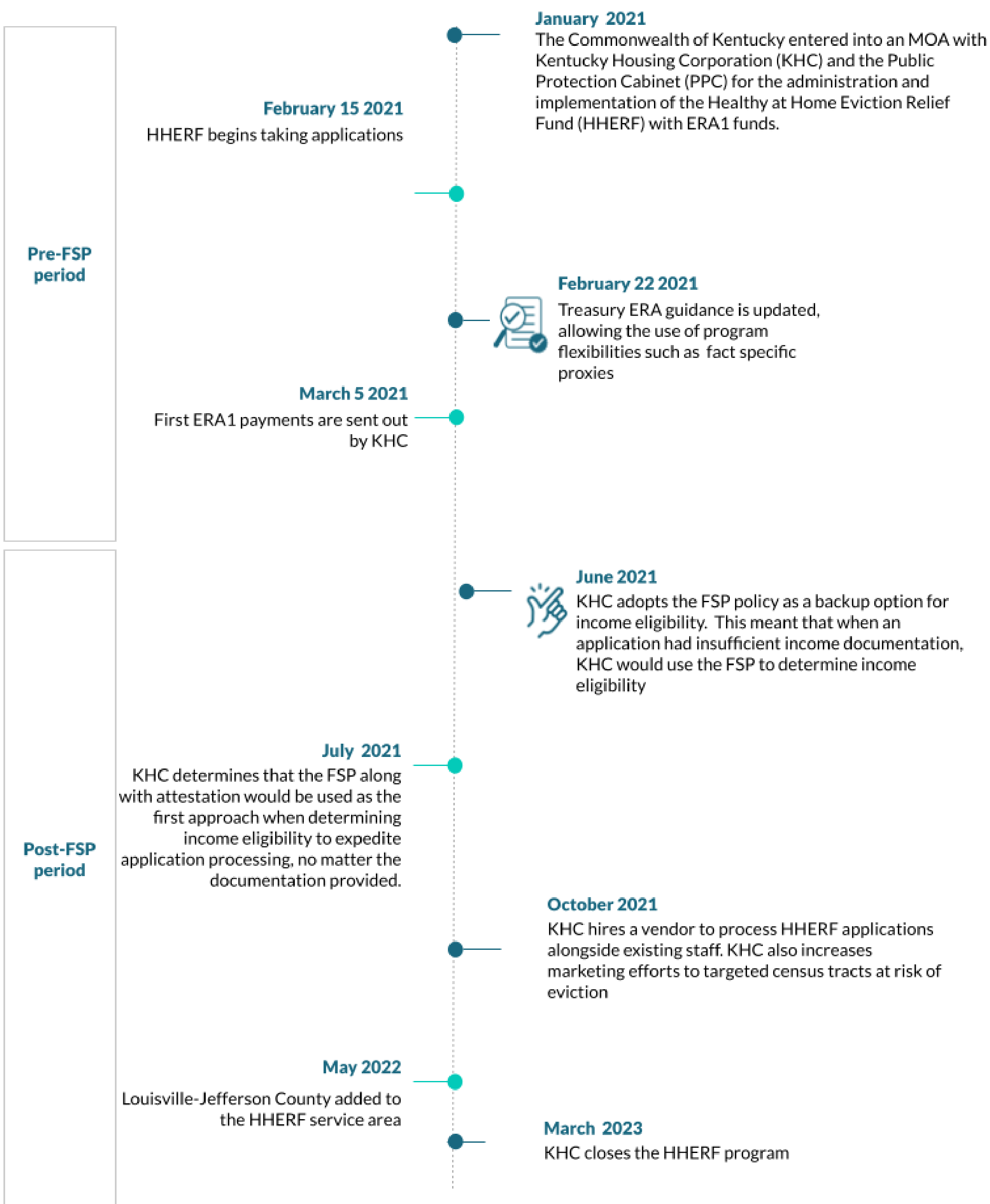
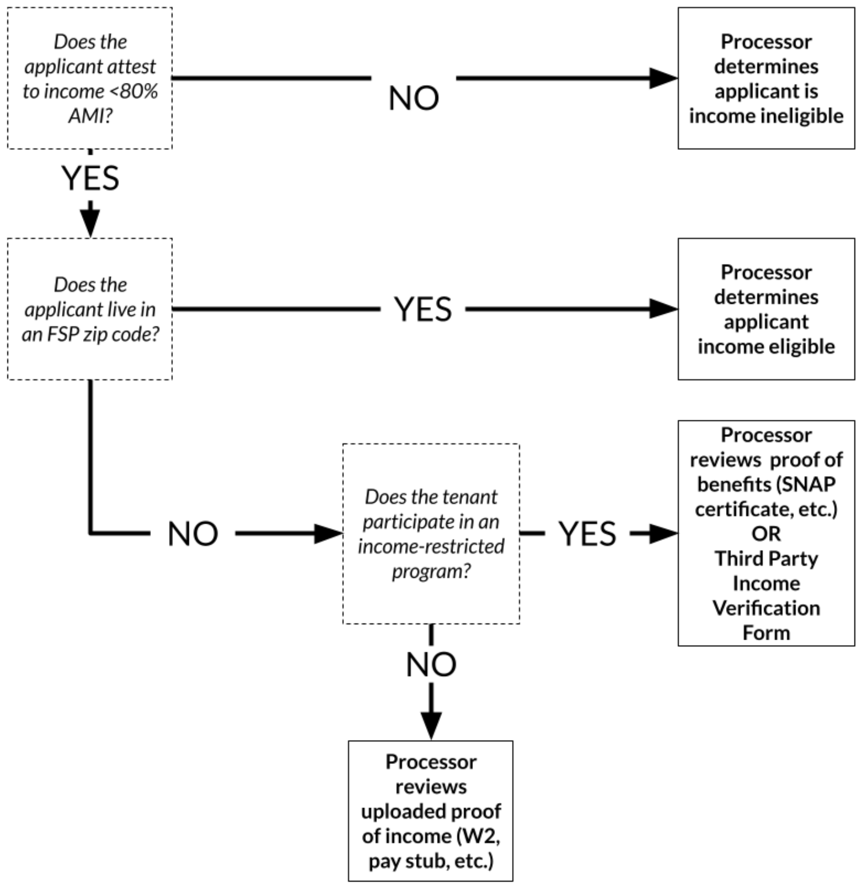


Figure 2. *Changes to income documentation processing after final FSP program change (7/21)*



Hypotheses

Our main research question is: to what extent does simplifying the process to determine applicants' income eligibility increase access to ERA? Specifically, we wish to measure the degree to which FSP increased the likelihood applicants were approved. Given that documentation burdens such as income eligibility documentation may be hardest to overcome for underserved groups, we also wish to know whether FSP reduced disparities in the likelihood of approval. Finally, we wish to examine whether FSP increased the total amount of ERA paid out, and reduced the days between application submission and payment. We specify four hypotheses.

First, we hypothesize FSP increased the overall **application approval rate**. As described above, removing barriers to approval was a key motivation for the FSP. Administrators were concerned that many eligible applicants simply gave up on their applications if their first attempt at getting ERA was rejected due to insufficient or otherwise incomplete documentation. For applicants in highly informal employment situations or who lack access to traditional banking, it may have been close to impossible to upload W2s, paystubs, and other forms of official documentation. Thus, in the absence of FSP, many eligible applicants may never have been approved because they were simply unable to upload sufficient income documentation.

Second, we hypothesize that FSP reduced **disparities in application approval rates** between applicants who did and did not belong to either of three underserved groups, specifically: applicants belonging to very low-income households, applicants living in rural areas, and applicants who identify as people of color.[3] We focus on these three groups as they were identified by KHC as groups who experienced greater difficulties than others with income verification and therefore may have benefited more from FSP. Prior research suggests resource constraints due to poverty and racial and ethnic disadvantage increase cognitive stress and thereby exacerbate the difficulties imposed by administrative burdens.[4] As for rural populations who already face issues with accessing government services due to remoteness and poor internet access, their mental health and economic outlook was hit particularly hard by the COVID-19 pandemic.[5] Thus it is reasonable to assume that income eligibility documentation may have posed a higher barrier for applicants belonging to those specific groups, above the difficulties faced by other eligible applicants not belonging to those groups. By reducing the burden among applicants in underserved groups by a greater degree, FSP may have improved their access to ERA relatively more, reducing disparities.

Third, we hypothesize that FSP increased the total amount of ERA paid out to households in eligible zip codes. If the implementation of FSP worked to improve access to Kentucky's ERA program, it follows that it should have also increased the total amount paid in a given ZIP code.

Finally, we hypothesize that FSP reduced the days between application submission and payment. Reviewing income documentation, such as a W2, paystub, or certification of SNAP benefits, requires time and effort from file processors, who must verify the recency, accuracy, and validity of the income documentation provided. Applications in FSP-eligible areas could be approved with no further income verification, removing this manual part of the approval process. Further, FSP could increase the chances a given application is approvable the first time it is submitted, removing the need for file processors to revisit the same application multiple times requesting changes, a process that can take weeks or months.[6] Therefore, we hypothesize that simplifying the requirement to document income eligibility for applications pertaining to households in FSP ZIP codes will reduce the number of days between application submission and payment. Below, we

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[3] We follow HUD's definition of extremely low income, which generally refers to a person living in a household whose income falls below 30% of the median household income for households of equivalent size in the same Metropolitan Statistical Area (MSA) and Primary Metropolitan Statistical Area (PMSA). Note that the population eligible for the program includes household whose income is at or below 80% of the area median income. We define a person of color as someone who self-identifies in their application as anything other than a non-Hispanic or Latino White person. Note both groups are defined as underserved in Executive Order 13985, namely: "Black, Latino, and Indigenous and Native American persons, Asian Americans and Pacific Islanders and other persons of color [...] persons who live in rural areas; and persons otherwise adversely affected by persistent poverty or inequality."

[4] See: Christensen et al. "Human Capital and Administrative Burden." Public Administration Review, 80: 1 (2020): 127-136 and Brondolo et al. "Stress and health disparities: Contexts, mechanisms, and interventions among racial/ethnic minority and low-socioeconomic status populations." American Psychological Association (2017).

[5] See Mueller et al. "Impacts of the COVID-19 pandemic on rural America." PNAS, 118:1 (2020)

[6] See, for example, pg. 11 from this report by the National Low Income Housing Coalition about FSP removing the need to revisit applications several times over the course of the approval process

describe certain challenges we anticipate facing when trying to estimate these potential reductions.

Evaluation Design, Statistical Models & Hypothesis Tests

Quasi-experimental analogy

We leverage the fact that the requirement to upload proof of income eligibility was removed via an FSP to identify the causal impact of this simplification on access to relief. In particular, we use our understanding of how the FSP simplified income eligibility verification for some potential applicants and not others to draw an analogy to an "ideal experiment" in which potential applicants are randomly assigned to have or not have the requirement to upload proof of income eligibility.

Whether a ZIP code was classified as FSP eligible or not depended only on two variables:

1. Whether the data was suppressed. This is a random variable because it depends on the number of cases randomly sampled in a ZIP code. The number of interviews conducted by the ACS in a given ZIP determines whether a renter median income estimate is available in the data. As explained above, 297 ZIP codes' 2019 5-year median renter income estimate was suppressed by the ACS. Those ZIP codes were ineligible for the FSP.
2. Whether the available renter median income estimate fell below the applicable county 80% AMI threshold. When the available renter median income estimate falls below the county threshold, the ZIP code is eligible for FSP, and is otherwise ineligible.

Our study design leverages the fact that these two variables — suppression and the renter median income estimate — are random because they depend upon the ACS sampling methodology.

Data suppression depends upon the number of cases (successful interviews) that the ACS randomly selects in a given ZIP code. Because the ACS is not stratified by ZCTA, but rather by county, there is no guarantee that every ZIP code will have a sufficient number of respondents to prevent disclosure violations.[7] In principle, even ZIP codes with large populations could be suppressed, but this issue mechanically exerts a greater effect on smaller ZIP codes because they have a higher chance of having no single renter sampled. We do not have access to the number of cases sampled in a ZCTA. However, using the publicly available ACS ZCTA-level data on the estimated number of renter households and on the estimated renter median income, OES was able to predict suppression of the renter median income estimate with 85% accuracy based simply on whether the estimated number of renters in that ZIP code was below 70.[8]

The random sampling also implies that the median renter income estimates that *are* available are random variables. The margin of error that accompanies the median renter income estimate

[7] For a full description of the ACS sampling methodology, see here. For rules on data suppression, see here.
[8] Using 70 as a cutoff for the number of estimated renter households below which the ZIP code is predicted to be suppressed, we found 423 ZCTAs are correctly predicted to be non-suppressed, 230 are correctly predicted to be suppressed, 67 are predicted to not be suppressed when they in fact are and 50 are predicted to be suppressed when they are not.

provides an estimate of the underlying variance in all possible median income estimates that could be produced under hypothetical repetitions of the random sampling method. Unfortunately, however, because we do not know the true underlying mean or standard deviation of the sampling distributions that govern the number of cases or renter median income estimate, we do not know for certain what the probabilities of assignment to FSP are. One minimal definition of an experiment is a procedure that assigns units to two or more treatment conditions with *known* probabilities between 0 and 1. We thus have a "quasi-experiment."

Replicating FSP assignment

A critical piece of the quasi-experimental design is ensuring we understand how ZIP codes were assigned to FSP. Our understanding based on materials shared by KHC is that households in 414 zip codes out of 769 eligible ZIP codes, as shown in Figure 3, were deemed presumptively income eligible for HHERF. We replicate this designation of the ZIP codes using ZCTA 5 year estimates data from the 2019 American Community Survey and the 2019 county-wide low-income household limits from HUD. We note that we were able to replicate these features of the program based on information about the FSP ZIP code designation alone — no OES team member accessed or used data on applications prior to the drafting and posting of this plan. We replicated the list using the following steps:

1. Using the American Community Survey 2019 5 Year Estimates, we identified the estimated renter median income at the ZCTA level.
2. ZIPs were assigned to FSP if the ACS renter median income was not suppressed and was lower than the 80% AMI level for the county. To replicate this, we merged 2019 county-level AMI measures downloaded from the HUD website to the dataset created in the previous step.
3. We designated ZIP codes in which the renter-occupied household median income estimate was available and was less than the associated county 80% low-income limit as FSP-eligible, and all other ZIP codes as FSP-ineligible.

This procedure successfully replicated the FSP designations provided by KHC for all ZIP codes. This replication exercise clarified that Census Bureau Zip Code Tabulation Areas (ZCTAs) were used, rather than US Postal Service (USPS) ZIPs. We continue to refer to the ZCTAs using "ZIP codes" in this project for ease of reference. Figure 3 shows the geographic distribution of FSP-eligible and -ineligible ZIP codes. Figure 4 shows the frequency distribution of ZIP codes by the difference between their ACS-reported renter median income estimate and their HUD-defined county-level 80% AMI cutoff. This allows us to visualize how close each ZIP code's estimated income was to being designated as eligible for FSP.

Figure 3. *Geographic distribution of FSP and non-FSP eligible ZIP codes*

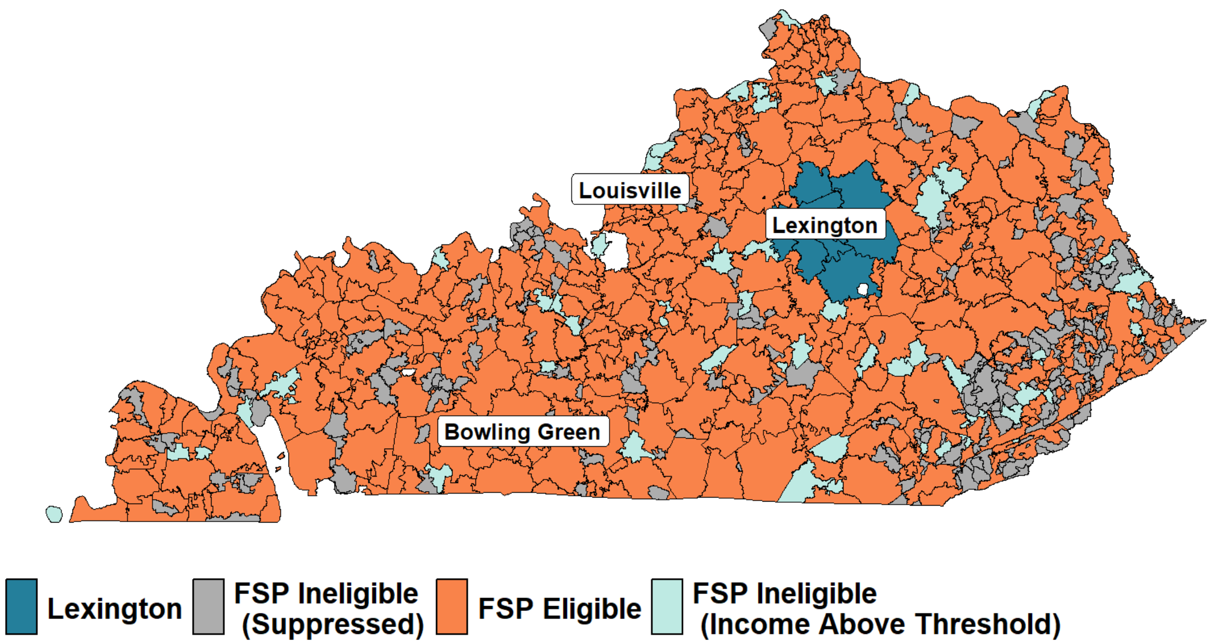
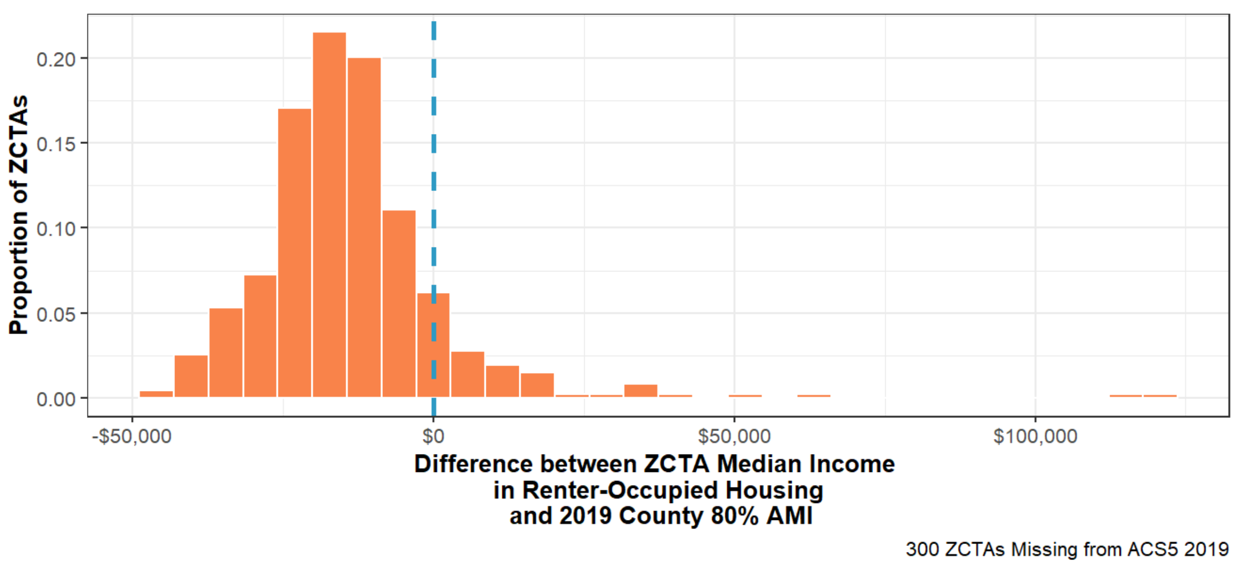


Figure 4. *Frequency distribution of ZIP codes by median income*

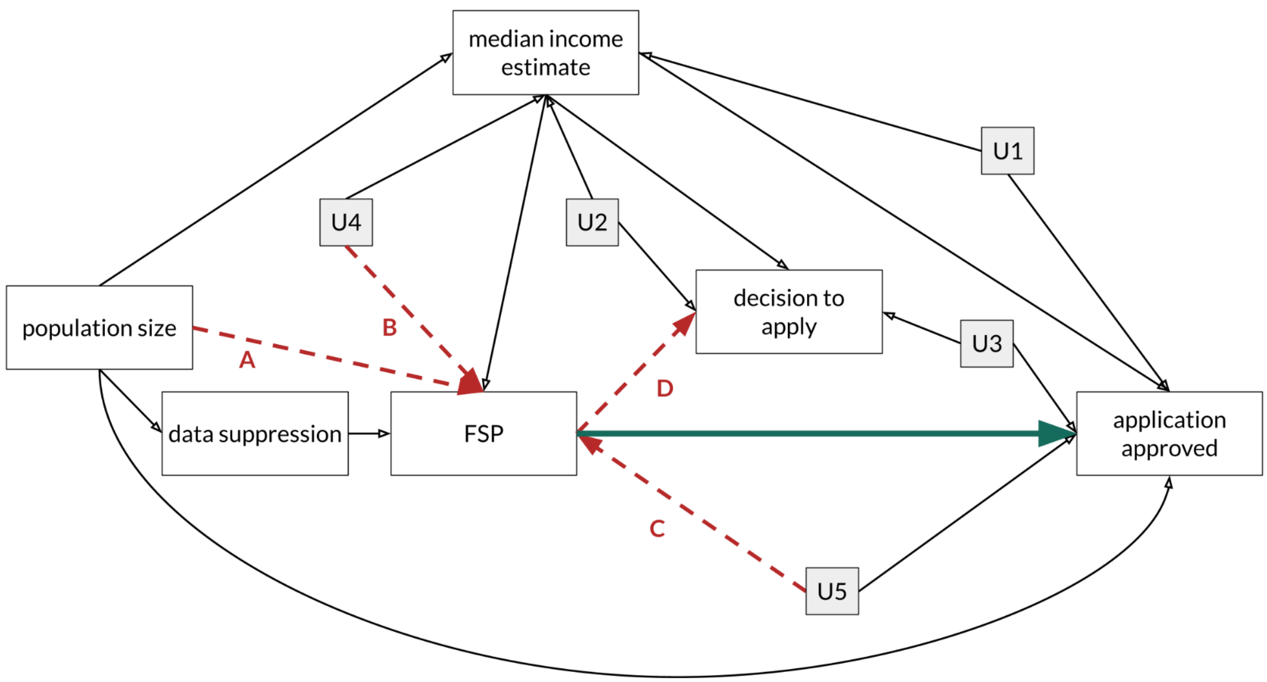


Causal identification strategy

To estimate the effect of FSP on our main outcome, application approval, our main analysis will rely on a two-period difference-in-differences estimator. We chose this approach over a regression discontinuity for reasons described in the robustness analyses section below.

Estimating the effect of the FSP on the application approval in an unbiased manner requires that our variable adjustment set (our list of control variables) satisfies two main conditions: 1) conditional ignorability; 2) no mediators among adjustment set. The diagram on Figure 5 is called a Directed Acyclic Graph (DAG) and is used to represent a set of assumptions about the dependencies between variables in the study. Specifically, in our case the DAG represents assumptions under which conditions 1 and 2 can be satisfied.

Figure 5. Assumed relationships between observable and unobservable variables.



The white boxes represent variables we can measure and plan to control for in our analysis (note: we implicitly condition on the decision to apply by only estimating analyses among applicants). The gray boxes represent "unobserved" variables or collections of variables we cannot measure and cannot control for in our analyses. Arrows represent a relationship of causal dependence — when one variable points to another it means that the first variable causes changes in the second. Red arrows indicate the relationships that we are stipulating do not exist in support of conditions 1 and 2 above. The green arrow is the particular causal relationship we seek to estimate — the causal effect of the FSP on the likelihood that an individual application was approved.

1. *Conditional ignorability*. This condition stipulates that, if we condition on the estimate of median income, the estimate of the renter population size, whether the renter median income estimate was suppressed, and the decision to apply, the potential outcomes of application approval are distributed independently from the FSP variable. This condition is violated whenever the causal relationships represented by arrows A, B, or C exist. Arrow A represents a direct effect of the population size on the FSP designation (for example, if certain ZIP codes had been redesignated as FSP-eligible just because they had a large population). Arrow B represents some backdoor path between unobserved variables that affect median income and the FSP designation. Arrow C represents some backdoor path between unobserved variables that affect the FSP designation and application approval. As we showed in the previous section, however, we know that these relationships do not exist because we were able to perfectly replicate the FSP designation based solely on whether the renter median income estimate was available (not suppressed) and if so where it fell with respect to county AMI limits.
2. *No mediators among adjustment set.* Even if A, B, and C do not exist, our analysis of approval conditions on the decision to apply by only analyzing the approval rate of applicants (as opposed to, say, the approval rate of all eligible renters, whether they apply or not). If FSP causes people to apply to the program (relationship D), then the decision to apply becomes what is known as a "mediator" — a variable caused by the treatment in which we are interested (FSP) that lies on the path to the outcome we care about (application approval). As we note above, there is no reason to believe applicants could have known that they were or were not in an FSP ZIP code, because the FSP list was available only to program administrators. Applicants were not informed, prior to, during, or after their application that they were in an FSP ZIP code. KHC confirmed that there were no substantive changes to the public-facing application portal after FSP launch. Further, KHC never publicized the FSP-eligible ZIP code list in any way. This means that applicants were unaware of their FSP status when applying. An implication is that we do not expect FSP to have altered potential applicants' decisions to apply, and so do not believe that arrow D exists. We describe an empirical test of this assumption below.

Under these assumptions, our adjustment set — the variables we plan to control for, represented by the white boxes on Figure 5 — is sufficient to causally identify the effect of FSP on application approval. We now turn to a description of the data before describing our main analyses.

Data and Data Structure:

KHC has shared the following column headers with us, representing application-level data elements for all tenant-initiated applications to the program:

- CLAIM ID
- RECEIVED DT
- STATUS
- APPROVED AMOUNT
- PAYMENT DATE
  - This variable is only available for applications that were ultimately paid.
- CITY
- STATE
- ZIP
- RACE
- ETHNICITY
- GENDER
- DISABILITY
- VETERAN
- AMI INFO
- INC_ELI_BY_FB_P ROXY
- INCOME DOC SUBMITTED
- YEARLY INCOME
- TOTAL MEMBERS

We will use this application data to create two analytic datasets. The first is defined at the individual application level and contains the following newly created variables:

- **Type of Income Documentation Submitted (income_submitted):** We will create a categorical variable indicating the type of income documentation that was submitted.
- **Approved Application (approval):** We will create a binary indicator that tracks whether the submitted application was ultimately approved to receive payment (1) or not (0).
- **Extremely Low Income (x_low_income):** We will create a binary indicator representing whether the applicant's household was at or below 30% AMI.
- **Person of Color (poc):** We will create a binary indicator representing whether the primary applicant reports being White and not Hispanic or Latino (0 if yes, 1 if no).

- **Rural (rural):** We will create a binary indicator for whether the applicant reports living in a rural ZIP code (1) or not (0). To do so, we use the Federal Office of Rural Health Policy data ZIP designations here, released through the Health Resources & Services Administration.
- **Marketing (marketing):** Binary indicator that takes the value 1 if the application originated from a ZIP code that received additional marketing for the ERA program *and* was submitted after the marketing campaign began, 0 otherwise. We explain what this variable is and how it is used below.
- **Days to Payment (days_to_payment):** We take the difference (in days) between the date the application was submitted (RECEIVED DT) and the payment date (PAYMENT DATE). The payment date is only available for applications that were ultimately paid and is missing for any application that was denied or abandoned, so this variable is only defined for paid applications.
- **County AMI (county_ami)**: a ZIP code benefited from FSP if the renter median income in that ZIP code was below 80% of AMI, where areas were typically counties. We merge in the county 80% AMI limit to construct other variables below.
- **Median Income (med_inc):** we merge in the 2019 ACS 5-year estimates for median renter income at the ZCTA level. As mentioned above, some ZIP codes are missing a renter median income estimate. So that we are able to condition on this variable for all ZIP codes independently from whether their data was suppressed, we use a machine-learning model to predict their renter median income. The procedure for these predictions is described below in Appendix B.
- **Suppressed** (**suppressed**): a variable that takes the value 1 if the ZIP code's renter median income estimate was suppressed and 0 otherwise.
- **FSP (fsp)**: this variable takes the value 1 if the estimated renter median income is not suppressed by the ACS and is at or below **county_ami**, 0 otherwise.
- **Renter population size (pop_size):** the ACS estimate of the number of renter households in the ZIP code.
- **Pre- or Post-FSP (pre_post):** this variable takes the value 1 if the application was submitted on or after the first day FSP was implemented (6/1/2021 – see Figure 1), 0 otherwise.
- **Running Variable (running_variable):** we take the difference between the county 80% AMI limit and the median income and multiply the difference by -1. This gives us a variable that is negative for most ZIP codes above the AMI threshold for FSP, positive for most ZIP codes below it, and is exactly 0 at this threshold. (It is "fuzzy" in that it divides most but not all ZIP codes into the correct sides of the FSP threshold due to data suppression.) We use this variable for robustness analyses employing a regression discontinuity estimator described below.

The second dataset is created by aggregating the individual-level dataset to the ZIP code level, creating two observations for every ZIP code based on applications submitted across the pre- or post-FSP periods. New variables added at this level are:

- **Total Payment Amount (total_paid):** For our exploratory analysis, we sum the total dollar amount paid to all applicants, summing all payments across payees in a ZIP code. We impute a value of 0 for ZIP codes with zero payees.
- **Count of Applications (N_app):** Here, we count all applications submitted in a given ZIP code.

Outcomes to Be Analyzed:

The primary outcome is whether an application is approved for payment. Exploratory analyses focus on the number of days between application submission and payment (at the application level) and on the total amount paid to renters (at the ZIP code level).

Data Exclusions:

We plan to drop any applications from Lexington, as this county implemented their own, separate ERA program throughout.

Residents that lived in Fayette and Jefferson counties applied for assistance directly to their county of residence—with the exception of the period of May 1, 2022 to December 22, 2022 when Jefferson County tenants and landlords were served by HHERF rather than Louisville's local program due to a lack of funds. We will therefore include applications from Louisville-Jefferson county from May 1, 2022 - December 22, 2022, and exclude the rest from that county. Program administration communicated with these other counties to ensure applicants were served by the appropriate ERA program based on their address, and that duplicate benefits were not received.

Treatment of Missing Data:

We anticipate that the primary remaining sources of missing data come from our demographic variables used in our exploratory analyses, as some tenants may have declined to report their demographic information. Our approach is to restrict our analysis to self-reporters. A limitation to this approach is that it requires the assumption that FSP does not affect self-reporting of demographics. We have no reason to believe that FSP affected self-reporting, given the information on FSP eligibility was unavailable to applicants.

Statistical Models & Hypothesis Tests

Our analyses fall into three categories: *confirmatory analysis*, our main results that will be the headline results in the abstract; *exploratory analysis*, which look at different outcomes that are policy relevant but not the central focus of the study; and *robustness checks*, which are mainly intended to contextualize the confirmatory analyses by showing how the results change under different analytical choices.

Confirmatory Analyses:

As noted, our main estimator is a difference-in-differences regression that contains a specific set of control variables chosen to satisfy the causal identification conditions described above (Causal identification strategy). Although it is possible to estimate a regression discontinuity here, we do not do so for our main analyses for reasons explained in the robustness analysis section below. We also opt for a simple pre- and post-, two-period setup, rather than including month- or week-level fixed effects. The reason for not using more detailed temporal variables is that they can introduce complicated regression weights that do not necessarily estimate the effect of interest. We cluster our standard errors at the ZIP code level and base our inferences on asymptotic p-values derived from those standard error estimates. We use the lm_robust() command from the estimatr package in R to estimate the effects. There are four main confirmatory analyses.

Approval rates: We are interested in estimating the *causal effect* of simplifying the process to determine income eligibility on **approval rates** for applications. We plan to use the following code to estimate this effect:

```
lm_robust(
  formula = approved ~ pre_post + fsp + pre_post * fsp + med_inc + pop_size + suppressed,
  clusters = zip,
  se_type = "CR2",
  data = dat)
```

Differential effects on underserved groups:

Our second hypothesis is that FSP may reduce disparities in approval rates between applicants who did and did not belong to either of three underserved groups, specifically: applicants belonging to very low-income households, applicants living in rural areas, and applicants who identify as people of color.

We estimate these disparity reductions using three linear models, each of which interact all of the terms in the main analysis above with a binary indicator for membership in one of the groups of interests. For each of our three groups of interest (applicants belonging to very low-income households, applicants living in rural areas, and applicants who identify as people of color), we will estimate interacted linear models using the binary variables defined in Data and Data Structure (*x_low_income*, *rural*, *poc*). Specifically, for a given binary *group* variable, we will fit a fully interacted model:

```
lm_robust(
  formula = approved ~ group * (fsp * pre_post + med_inc + pop + suppressed),
  clusters = zip,
  se_type = "CR2",
  data = dat)
```

This corresponds to a model where *group*, *fsp*, and *pre-post* are fully interacted with each other (in R syntax, the * operator implies a regression model containing all constitutive individual and interaction terms) and *group* is further interacted with our three control variables *med_inc*, *pop*, and *suppressed*. The coefficient on the three-way interaction term represents our estimate of the difference in the effect of FSP for members who do and do not belong to the group of interest.

To facilitate interpretation of the results, we will estimate the following models separately for members and non-members of each group:

```
lm_robust(
  formula = approved ~ pre_post + fsp + pre_post * fsp + med_inc + pop_size + suppressed ,
  clusters = zip,
  se_type = "CR2",
  data = subset(dat, group == 1))

lm_robust(
  formula = approved ~ pre_post + fsp + pre_post * fsp + med_inc + pop_size + suppressed ,
  clusters = zip,
  se_type = "CR2",
  data = subset(dat, group == 0))
```

Then, the coefficients on *pre_post * fsp* for each model are the difference-in-difference estimates for members and non-members of the group. These represent the disparity reductions for each group. The difference in these coefficients is equal to the coefficient on the triple interaction term in the first model.

Finally, we will use the fully interacted model above to make predictions along a series of covariates at each group. These predictions will allow us to calculate the expected predicted values with and without FSP for a representative member of each group. For each *group*, we will make the following predicted values. In all predictions, we will hold the value of *pop_size* at the statewide median, *med_inc* at the statewide HUD-defined 80% AMI limit in 2019, and suppressed at 0.

1. A non-group member in the pre-period who did not receive FSP (*group* = 0, *pre_post* = 0, *fsp* = 0).
2. A group member in the pre-period who did not receive FSP (*group* = 1, *pre_post* = 0, *fsp* = 0).
3. A non-group member in the post-period who did not receive FSP (*group* = 0, *pre_post* = 1, *fsp* = 0).
4. A group member in the post-period who did not receive FSP (*group* = 1, *pre_post* = 1, *fsp* = 0).
5. A non-group member in the pre-period who did receive FSP (*group* = 0, *pre_post* = 0, *fsp* = 1).
6. A group member in the pre-period who did receive FSP (*group* = 1, *pre_post* = 0, *fsp* = 1).
7. A non-group member in the post-period who did receive FSP (*group* = 0, *pre_post* = 1, *fsp* = 1).
8. A group member in the post-period who did receive FSP (*group* = 1, *pre_post* = 1, *fsp* = 1).

These estimates will allow us to visualize and compare predicted changes between groups. We will infer that there has been a reduction in disparities using the following decision rule:

- If the FSP is statistically significantly more effective at improving application rates for members of underserved groups than for non-members (based on the triple interaction) *and* the predicted values suggest a substantively meaningful narrowing of the post-intervention gap in approval rates for groups, then we will infer that the FSP reduced disparities among the relevant groups.

We will base our decision on statistical significance without making further adjustments for multiple comparisons. However, we will also report the Holm-Bonferroni-corrected p-values across the three triple interactions.

Exploratory Analysis:

Days between application submission and payment: Previous OES research on Virginia's FSP found a substantial reduction in processing times for ERA applications submitted but not processed prior to the implementation of the FSP. That analysis was facilitated by the availability of a processing time for every application, irrespective of its status. For example, file processors eventually processed abandoned applications as "inactive" and that decision was timestamped. By contrast, the Kentucky data only records a processing time for applications that were eventually paid.

An issue called "post-treatment bias" arises in situations where an outcome we think is affected by the treatment is only observed among a group whose membership is also affected by the treatment (see Rosenbaum (1984)). In this analysis, we hypothesize that the number of days between the application submission and payment – the processing time – is affected by the FSP, but we only observe the processing time for paid applications and we think the probability of being paid is also affected by the FSP.[9] This implies our estimates could be biased if we conduct a naive analysis of the impact of FSP on the processing times of all paid applications, without any additional adjustments to account for the fact that some applications will only appear as paid if they are in the FSP group.

Our approach to addressing post-treatment bias involves identifying a type of application whose probability of being paid is unaffected by FSP. Such applications would have been paid irrespective of their FSP status, so we can estimate the effect of FSP on their processing time without worrying that the FSP and non-FSP group averages are biased upward or downward by selection into the paid group. Empirically, we cannot identify such applications ahead of time. Instead, we must identify specific subgroups and make a determination about whether the estimate of FSP on their probability of being paid is sufficiently small to justify analyzing the effect of FSP on their

---

[9] To see why this is a problem, suppose that we could define, for every application, a hypothetical processing time that would occur if it were paid, using the notation T(F,P = 1), which denotes processing time in days, T, as a function of the FSP variable, F, and the payment variable, P. Suppose further that the average effect of FSP on that outcome (defined as [T(F = 1, P = 1) - T(F = 0, P = 1)]/N) is negative. Even so, if the FSP causes applications with longer processing times to *be paid*, the average processing time in the FSP group could be longer than in the non-FSP group, and the researcher might erroneously infer that the FSP increased processing times.

processing time. Note that is not enough to find a group among whom the estimate of FSP on the probability of payment is statistically insignificant: this might simply reflect insufficient statistical power (and is especially likely given this approach involves reducing the sample size through subsetting). Our main conjecture is that FSP is most likely to increase approval rates for individuals who submit *insufficient* income documentation, which is measured in the application data through the flag "INCOME DOC SUBMITTED". We therefore focus on the group who submitted income documentation. We plan to conduct this analysis according to the following decision rule:

- If, among the group of applicants who submitted income documentation, we are able to reject the null hypothesis that the average impact of FSP on the probability of being paid is 1 percentage point or greater (in absolute value) at the alpha = .05 level, we will conduct an analysis of the effect of FSP on the days between application submission and payment among this group. If we cannot reject this null hypothesis, we will not conduct the analysis.

Note that the estimates will not pertain to the sample as a whole: they will only pertain to those who submit income documentation and were eventually paid. If the condition above is satisfied, we plan to conduct the analysis using the following code:

```
lm_robust(
  formula = days_to_payment ~ pre_post + fsp + pre_post * fsp + med_inc + pop_size +
suppressed,
  clusters = zip,
  subset = include_in_days_to_payment_analysis == 1,
  se_type = "CR2",
  data = dat)
```

Total amount paid. For our analyses on the total amount paid, we use the ZIP code-level dataset described above. Based on prior OES research on Virginia's FSP, we suspect this outcome may exhibit a great deal of variance, given the many sources of variation from one ZIP code to another. However, we emphasize that it is not subject to the post-treatment bias issue which would arise if we sought to estimate the impact of FSP on the *average* payment amount to individuals.[10] Here, the ZIP code is the unit of analysis, and we are simply interested in whether more money was spent overall in a ZIP code due to the broadening of program access brought about by FSP. In prior OES work on FSPs, we found this outcome to be skewed by the fact that some ZIP codes are large and therefore have many applicants and others are very small and have few applicants. To protect against bias that can affect skewed outcomes, we will take the log plus one of the outcome, using the following code:

```
lm_robust(
  formula = log1p(total_paid_post) ~ fsp + med_inc + pop_size + suppressed + total_paid_pre,
  data = dat)
```

Note we do not need to cluster errors here because the analysis is at the ZIP code level.

[10] For example, if the program induced people with lower rental arrears to apply, the average payment amount in the treatment group would be lower than in the control group, leading to an incorrect inference that the FSP reduces the amount that people received.

Robustness Checks:

We plan to conduct robustness checks of the main confirmatory analysis:

Accounting for Targeted Outreach: As described in How the fact-specific proxy (FSP) worked, KHC engaged in several marketing campaigns to publicize the ERA program. Crucial for our design (as explained in Evaluation Design, Statistical Models & Hypothesis Tests), KHC did not publicize lists of FSP eligible ZIP codes. However, KHC may have targeted outreach efforts in low-income areas, which would be predominantly FSP areas.

Using a list of ZIP codes targeted for KHC marketing, we evaluated the relationship between marketing and FSP assignment. We identified 98 ZIP codes (out of 769) that received additional marketing efforts. Of those, 77 were in FSP areas. Although these 77 ZIP codes only represent a modest proportion of the 414 total ZIP codes that received FSP, FSP ZIP codes did receive a significant fraction of marketing. This is likely due to their lower median incomes.

We have no reason to believe the marketing biases our estimates: as our DAG on Figure 5 illustrates, our design does not require that median income is independent of application rates (see the unobserved confounder, U1, as well as the direct path from median income to approval). However, since it is possible the marketing may have caused certain types of applicants to apply whose approval rates may be different, as a robustness check we plan to run the following version of our main results analysis, augmented to include the control described above:

```
lm_robust(
  formula = approved ~ pre_post + fsp + pre_post * fsp + med_inc + pop_size + suppressed +
marketing,
  clusters = zip,
  se_type = "CR2",
  data = dat)
```

Regression Discontinuity Approach: In prior OES work on Virginia's FSP, our main analyses included a regression discontinuity estimator. For several reasons, we plan to report the results of this estimator primarily as a robustness check in this study.

First, the results from the regression discontinuity in the previous study were much more variant than expected and did not yield reliable inferences as a result. Second, because there are two forcing variables here (the number of cases interviewed in the ACS within a ZIP code and the estimate of median income), we have a two-dimensional regression discontinuity, which is subject to a host of additional complications that do not exist for single-dimensional RDs. Third, we cannot measure either of the forcing variables perfectly because the number of cases is not available and the median income estimate is not available for all ZIP codes. So the regression discontinuity we plan to run will be based on a single, "fuzzy" running variable. Finally, regression discontinuities are most useful in situations where a treatment is *not* randomly assigned but is assigned deterministically by a threshold. Causal identification is achieved by changing the estimand to focus on a hypothetical point at the threshold where units reveal both their treated and untreated potential outcomes, by extrapolating polynomial regression functions to the threshold. Thus, external validity is traded off for internal validity given the absence of randomization. Here, as we have established, applicants are randomly assigned through ZIP code clusters to either receive or not receive FSP. Thus, the costs to using regression discontinuity as the main approach do not appear to outweigh the benefits.

We will run the following RD model as a robustness analysis, subsetting to post-FSP observations:

```
with(dat, rdrobust(y = approved,
                   x = running_variable,
                   c = 0,
                   bwselect = "mserd"))
```

Number of applications. As described above, a key assumption in the study is that FSP does not impact the decision to apply (see Figure 5). While we have no reason to believe that it would have in this case, we test this empirically by estimating FSP's impact on the ZIP code-level count of applications. We take the log plus 1 of this outcome, as our prior work suggests it may be heavily left-skewed by ZIP codes with zero applications, which our prior work suggests can bias the results towards a positive finding. We plan to use the following code for this analysis:

```
lm_robust(n_app ~ pre_post + fsp + fsp * pre_post + med_inc + pop_size + suppressed, data =
dat)
```

Interaction of DiD with controls. Researchers have expressed concerns that adding controls to difference-in-differences regressions can cause regression to the mean.[11] One solution is to interact the DiD terms with the controls (see: Woolridge (2021), equation 5.7). We plan to run the following regression:

```
lm_robust(
  formula = approved ~ (pre_post + fsp + pre_post * fsp) * (med_inc + pop_size + suppressed),
  clusters = zip,
  se_type = "CR2",
  data = dat)
```

Alternative specification of median income. As described above, we plan to control for median renter income for all ZIP codes, even where it was suppressed, through the use of predictions from a machine learning model (see Appendix). Fong and Tyler (2020) show that controlling for machine learning predictions can lead to attenuation bias, increasing the risk of false negatives. To address this concern, we plan to recode the renter median income variable so that it does not rely on predictions. Specifically, we will bin the observed renter median income into deciles and code a categorical variable in which "Suppressed" is one of the categories, alongside the income deciles. We then rerun the main analysis with the renter median income specified as a list of dummy variables, captured by `med_inc_cat`:

```
lm_robust(
  formula = approved ~ pre_post + fsp + pre_post * fsp + med_inc_cat + pop_size + suppressed,
  clusters = zip,
  se_type = "CR2",
  data = dat)
```

[11] See here for example.

Inference Criteria, Including Any Adjustments for Multiple Comparisons:

We rely on standard errors estimated as described above in order to form p-values used in statistical significance tests. In all analyses, the null hypothesis is that the average effect of the treatment is zero and the test is two-tailed. We will use an alpha of 0.05 to determine statistical significance. To adjust for multiple comparisons in our main confirmatory analysis, we plan to report the Holm-Bonferroni-adjusted p-values on the three triple interactions we are using to assess disparity reduction.

Appendix

Appendix A: Figure 5 DAG Code

The following code can be used to reproduce the DAG in Figure 5 at dagitty.net

```
dag {
    bb="-3.959,-5.575,4.33,5.941"
    "Application approved" [outcome,pos="3.312,2.674"]
    "Data suppression" [adjusted,pos="-1.846,1.770"]
    "Decision to apply" [adjusted,pos="1.034,0.924"]
    "Median income" [adjusted,pos="0.454,-2.076"]
    "Population size" [adjusted,pos="-3.631,2.523"]
    FSP [exposure,pos="-0.247,2.187"]
    U1 [latent,pos="2.189,-1.856"]
    U2 [pos="0.333,-0.095"]
    U3 [latent,pos="1.812,0.820"]
    U4 [latent,pos="-2.317,-0.732"]
    U5 [latent,pos="0.947,3.682"]
    "Data suppression" -> FSP
    "Decision to apply" -> "Application approved"
    "Median income" -> "Application approved"
    "Median income" -> "Decision to apply"
    "Median income" -> FSP
    "Population size" -> "Application approved"
    "Population size" -> "Data suppression"
    "Population size" -> "Median income"
    FSP -> "Application approved"
    U1 -> "Application approved"
    U1 -> "Median income"
    U2 -> "Decision to apply"
    U2 -> "Median income"
    U3 -> "Application approved"
    U3 -> "Decision to apply"
    U4 -> "Median income"
    U5 -> "Application approved"
}
```

Appendix B: Prediction of Median Incomes

This section describes how we predict renter median income estimates for our model. Many ZIP-level median income estimates are suppressed by the ACS, as described in Evaluation Design, Statistical Models & Hypothesis Tests. However, we can make highly accurate predictions of renter median income by using Census data. We evaluated the potential accuracy of predicted suppressed median income estimates using two simple machine learning models — LASSO and Random Forests. We downloaded the following variables for all 33,120 ZCTAs identified in the ACS: median renter income, median household income and total renter population and total population, total white population, median income, and *county*-level median renter income. By keeping the list of predictors small, we avoid additional missingness.

We subset the data to ZCTAs whose median renter income was not suppressed, then split our sample into 80% training data and 20% testing data, and further split 20% of the training data into a validation set. We tune penalty parameters (using the validation set) for the LASSO model and both (1) the number of predictors and (2) the minimum number of data points per node for the random forest model. Tuned models were then used to predict median renter incomes for the testing data.

We find that both models are highly accurate. We find the LASSO accurately predicts 96.1% of ZCTA median incomes within one standard deviation (~$19,700) of the true value, and 86.2% within $10,000 of the true value. Random forest produces similar estimates, with 96.3% within one standard deviation and 85.9% within $10,000. We plan to use a random forest model with similar variables to predict the renter median income of suppressed ZCTAs in the Kentucky data.

Figure B1: Predicted and actual median renter income.