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Factors Influencing Customer Participation in a Program to Replace Lead Pipes for Drinking Water

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Abstract
Many public water systems are struggling to locate and replace lead pipes that distribute drinking water across the United States. This study investigates factors associated with customer participation in a voluntary lead service line (LSL) inspection and replacement program. It also uses quasi-experimental and experimental methods to evaluate the causal impacts of two grant programs that subsidized homeowner replacement costs on LSL program participation. LSLs were more prevalent in areas with a higher concentration of older housing stock, Black and Hispanic residents, renters, and lower property values. Owner-occupied and higher valued properties were more likely to participate in the LSL program. Results from the two grant program evaluations suggest that subsidies for low-income homeowners to cover LSL replacement costs can significantly boost participation, but only when the programs are well publicized and easy to access. Even then, there was still significant non-participation among properties with confirmed LSLs.

Keywords Lead exposure · Lead service lines · Drinking water · Field experiment · Quasi-experiment · Environmental justice

1 Introduction

Exposure to lead is a critical public health issue in the United States. Lead exposure has a causal relationship with reduced cognitive function and disorders related to attention, impulsivity, and hyperactivity in children, as well as hypertension, coronary heart disease, and other adverse health effects in adults (EPA 2013). No safe level of lead exposure has been identified. Neurocognitive damage caused by early childhood lead exposure manifests in lower intelligence and academic performance and higher rates of school suspension, juvenile detention, and criminal behavior (Lanphear et al. 2005, 2019 erratum; Aizer et al.

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2018; Miranda et al. 2009; Aizer and Currie 2019; Reyes 2015). These adverse effects persist into adolescence and adulthood, resulting in lifelong deficits in educational and labor market outcomes (Shadbegian et al. 2019; Reuben et al. 2017; Banzhaf and Banzhaf 2023; Salkever 1995). The aggregate lost economic productivity in the US is estimated in the tens of billions of dollars per year (Transande and Liu 2011). Black children and children in poverty have persistently higher exposures to lead from multiple sources than other children in the United States, as indicated by higher blood lead levels (Egan et al. 2021).

Drinking water remains a primary pathway for lead exposure in the US, particularly for infants (Zartarian et al. 2017). Lead service lines (LSLs), the pipes that supply drinking water from water mains into homes, are the largest contributor to water lead levels nationally (US EPA 2019). While federal law banned the installation of new LSLs in 1986, an estimated 10 million LSLs remain in use (EPA 2021a). These pipes pose an increased risk of lead exposure to residents (Brown et al. 2011; EPA 2021; Triantafyllidou and Edwards 2012). Pervasive LSLs coupled with ineffective corrosion control in Flint, Michigan, and Newark, New Jersey, illustrate the risks of elevated lead exposure and adverse health effects from LSLs (Dave and Yang 2020; Pieper et al. 2017).

In response to legal requirements, public pressure, and new federal funding, water systems are developing programs to identify and replace LSLs. The U.S. Environmental Protection Agency's (EPA's) 2021 revisions to the Lead and Copper Rule require every water system to develop a service line material inventory and require systems exceeding EPA's action level for water lead concentrations to implement lead service line replacement (LSLR) programs. The Biden Administration announced a plan for the eventual replacement of all LSLs (White House 2021). Recent legislation authorized $15 billion in direct funding for LSLR and made LSLR eligible for roughly $90 billion of additional funding.

Water systems cannot successfully implement LSLR programs without the participation of their customer base. Unlike many other types of publicly managed infrastructure, the ownership of LSLs in many communities in the US is split between the water system and the property owner, so homeowner participation is needed to allow the water system to inspect and replace the privately owned portion of the water line. Participation in LSLR programs is typically voluntary; water systems rarely have the authority to require replacement. Replacement is also costly, running several thousand dollars per property. Even when full subsidies are offered, non-financial barriers could impede household participation in both inspections and replacements, including time and inconvenience costs, lack of trust in public institutions, and mismatched incentives between landlords and renters. These barriers are often more pronounced for low-income residents. LSLR programs that do not address both financial and non-financial barriers to participation could exacerbate pre-existing economic and racial disparities in lead exposure. While some water systems have used cost sharing and various outreach approaches to reduce barriers to customer participation, there is no empirical evidence in the LSLR context about what strategies are most effective.

This study investigates factors affecting customer participation in a voluntary LSL inspection and replacement program using customer-level data from a large water system in New Jersey. To our knowledge, this study is the first to examine individual property and neighborhood characteristics associated with LSL occurrence and LSL inspection and replacement program participation. We also evaluate the causal impact on LSLR participation of two interventions offering full-cost subsidies to certain residents for replacement. The first intervention is a community-based program offering grants plus extensive outreach and education about LSLs to residents of a specific neighborhood in the urban area of the water system with a high percentage of Black and Hispanic residents and high poverty

rates. We estimate the effect of the community grant program using a synthetic control approach comparing LSLR participation before and after the grant program was launched among treatment and control groups that were perfectly balanced in terms of observable characteristics affecting participation rates. In the second intervention, postcards were sent to customers randomly selected from a larger pool of potentially eligible residents informing them about a different program run by the city housing department offering grants to low-income homeowners for urgent home repairs, including LSL replacements.

Results show that LSLs were more prevalent in the urban municipality of the water system service area, which has a higher concentration of older housing stock, Black and Hispanic residents, renters, and poverty than the suburban municipalities of the service area. A regression analysis shows that renter-occupied and lower-valued properties were less likely to participate in LSL program registration, inspection, and replacement. Results also indicate that properties receiving more outreach letters were more likely to participate.

The evaluations of the two grant programs show that the community-based program was highly effective in boosting participation in the LSLR program. However, sending postcards with information about the housing department grant program did not have statistically significant effects on program participation. A comparison of the two interventions suggests that programs covering homeowners' LSL costs alone did not increase participation in LSLR. Programs must also be well publicized and easy to access to substantially boost participation by low-income customers. Even then, this type of program was insufficient to replace 100% of LSLs in a voluntary program. Understanding the characteristics of customers who participate in LSLR and the program characteristics that yield the highest participation rates can help inform equitable and effective program design.

2 LSL Replacement Case Study

Using customer-level data from a water system serving more than 60,000 properties in Trenton, New Jersey, and four neighboring suburban municipalities, we examine the characteristics of properties and neighborhoods more likely to have LSLs and to participate in the inspection and replacement program. The water system launched an LSLR program in 2019 in response to exceedances of EPA's action level for drinking water lead concentrations in 2017 and 2018. It offered replacement of homeowner-side LSLs for a subsidized cost of $1000 payable over five years without interest. The water system used various outreach approaches to encourage participation, including community meetings, door-to-door visits, and mailings, including two letters sent to properties suspected of having LSLs prior to launching the program. Because mailings were sent to property addresses, renters rather than landlords received them at non-owner-occupied properties. By the end of our study period in July 2022, the system had conducted more than 17,000 service line inspections and replaced more than 9000 lines found to contain lead, including 2500 homeowner-side lines. The Appendix provides more details about the LSLR program.

We also evaluate the causal impact on LSLR participation of two different interventions that offered full-cost subsidies to certain residents in Trenton but not the suburban municipalities. The first intervention is a community-based program offering grants for LSLR plus extensive outreach and education about LSLs to residents of East Trenton, a low-income neighborhood in Trenton. We assess the first intervention using a quasi-experimental synthetic control approach. The second intervention was a field experiment in which postcards were sent to customers in neighborhoods throughout the urban municipality (except East

Trenton) informing them about a different program run by the city housing department offering grants to low-income homeowners for urgent home repairs, including LSLRs. For the second intervention, households in the treatment group were randomly selected from a larger pool of potentially eligible residents. The two grant programs were offered during the same time period, beginning in 2021, but there was no spatial overlap between households in the target neighborhood for the community-based grant and households included in the field experiment.

2.1 Community-Based Grant Program

From March 2021 through late 2022, an established community organization offered a grant program to cover the full $1000 LSL replacement cost for owner-occupied homes and $500 of the cost for renter-occupied homes (with landlords covering the remainder). Only properties in the community organization's target neighborhood of East Trenton were eligible. This neighborhood, comprised of four U.S. Census block groups, has a higher share of Black and Hispanic residents and poverty rates than the remainder of the city.[1] The grants required minimal paperwork from applicants, and the community organization leveraged its established communication channels, conducting extensive outreach through in-person and virtual events, door-to-door and neighborhood canvassing, and social media, to notify residents about the program. We refer to this subsidy program as the "community-based grant program."

2.2 Housing Department Grant Program

In addition, Trenton's Department of Housing and Economic Development offered an existing program funded by a U.S. Department of Housing and Urban Development Community Development Block Grant to subsidize urgent home repairs for low- and moderate-income homeowners. LSLR was added as a qualifying home repair in 2021, meaning that the entire $1000 cost to customers would be covered for successful applicants. Applicants were required to provide extensive documentation to prove their eligibility, such as wage and withholding statements and tax returns. Only owner-occupied properties in Trenton were eligible. Information about this grant program was publicly available on the city government website but was not advertised beyond that. Participation in the program for other urgent repairs was low prior to the addition of LSLR as a qualifying repair in 2021.[2] We refer to this subsidy program as the "housing department grant program."

3 Barriers to Customer Participation

Inspection and replacement of LSLs is challenging due to numerous financial and non-financial barriers. Non-financial barriers may include lack of awareness about the risks of lead exposure, lack of trust in water systems, and language barriers. Households might also

[1] A block group is a geographical unit defined by the US Census. Our study area includes 165 block groups, 78 of which are in the urban municipality.

[2] A housing department representative indicated that there were fewer than 50 applicants for urgent rehabilitation grants in a typical year (personal communication, Farrah Gee, City of Trenton Department of Housing and Economic Development).

decline to participate in an inspection to avoid potential bad news about their water quality, exhibiting an "ostrich effect" (Karlsson et al. 2009).

Non-financial barriers may also reflect the time cost and inconvenience of scheduling an inspection or replacement and disruption to one's home or yard. Furthermore, ownership of the service line is often split, with the utility owning the portion from the water main to the property line, and the homeowner owning the remainder (Appendix Fig. 5). Consequently, the utility may require homeowner permission for inspection and replacement. The need for landlord consent can exacerbate barriers faced by rental properties. In addition, low-income households may face outsized barriers due to tradeoffs between time spent meeting basic needs versus preparing documents or attending appointments needed for program participation (Mullainathan and Shafir 2013). Barriers to participation were also likely exacerbated during the COVID-19 pandemic, which may have decreased households' willingness to allow in-person inspections and construction.

Financial barriers are relevant in systems that do not fully cover the costs of replacing the homeowner side of the line. Some U.S. utilities are constrained by local or state laws limiting the use of public funds to make investments in private property, in which case homeowners are expected to cover these costs. EPA (2021a) estimated average homeowner-side LSLR construction costs to be $4000, and others have found costs ranging up to $10,000 (American Water Works Association 2022).

Utilities encouraging LSL inspection and replacement often focus on improving outreach and reducing costs (American Water Works Association 2005; EPA 2019). Approaches include providing subsidies and translating materials into languages other than English.[3] Subsidies can take the form of discounted upfront prices or 0% interest loans with payments spread over several years. Systems in some cities, such as Washington, DC, and Providence, Rhode Island, have targeted free or subsidized replacements to low-income homeowners or properties in disadvantaged neighborhoods (EPA 2019; Kuffner 2022). The state of Michigan requires most water systems to finance the full cost of LSLR for all properties, including the homeowner side (EPA 2019). Costs are recouped through a combination of grants, bond financing, and rate increases spread over all utility customers. While voluntary participation is typical, some cities require replacements for confirmed LSLs while fully covering the costs. For example, Newark, New Jersey, adopted a free and mandatory approach including fines for non-compliance (City of Newark 2019).

The existing literature on barriers to LSL replacement and ways to address them is minimal. A study found that LSL replacement rates in Washington, DC, were significantly higher in neighborhoods with higher incomes and a lower proportion of Black residents (Baehler et al. 2022). A discussion of LSLR by the Federal Reserve Bank of Chicago posited that voluntary programs face greater coordination challenges than mandatory programs (Hull , Anderson, and Saxena 2022). To our knowledge, there are no previous studies on the effectiveness of LSLR program interventions.

A growing literature has investigated whether interventions to lower financial and non-financial barriers to participation in a variety of public assistance programs are effective. Evidence is mixed and context dependent. Studies of school choice and tax credit programs

[3] Participants in roundtable discussions held in ten Northeastern and Midwestern communities by the US Environmental Protection Agency (EPA 2021b, 2021c, 2021d, 2021e, 2021f, 2021g, 2021h, 2021i, 2021j, 2021k) mentioned financial constraints, language barriers, landlord-renter split incentives, and lack of community trust in water utilities as barriers to LSLR. Participants in these sessions suggested subsidies, multiple types of outreach approaches (e.g., mailers, in person, online, door hangers), translation of materials into different languages, and partnerships with organizations viewed as "trusted messengers" in target communities as potential solutions to these barriers.

have found that providing information to eligible households increased uptake, and the type of information did not affect participation (Linos et al 2022; Bhargava and Manoli 2015; Hastings and Weinstein 2007). However, barriers can be more difficult to overcome for some households. In the context of disability benefit and food assistance programs, those who were less disabled or higher income than the average participant were more likely to enroll after the intervention (Deshpande and Li 2019; Finkelstein and Notowidigdo 2019). While uptake of more complex or time-intensive activities among low-income households—for example, disaster preparedness or applying for college aid—did not respond to information treatments alone, it increased with more intensive forms of outreach that involved interpersonal communication and assistance (Glik et al 2014; Bettinger et al 2012).

Researchers have also investigated interventions designed to overcome barriers to participation in environmental programs. Most of these studies have found small impacts. Studies to encourage testing of private wells found that targeted household-specific information and information about nearby well contamination increased testing, but well testing rates were low in the baseline and often remained low after these interventions (Renaud et al. 2011; MacDonald and Tippett 2020). Other interventions (e.g., distributing free test kits and offering next-day pickup) had no effect on participation (Kreutzwiser et al. 2011; Hexemer et al. 2008). Surveys have showed that even when well contamination was found, relatively few residents treated their water (Severtson et al. 2006). A lack of observed health effects or obvious indicators of contamination (odor or taste) were listed as reasons for failing to treat.

Likewise, studies of energy audit programs targeted at improving energy efficiency for low-income households found that household participation remained low even when subsidies and personalized assistance were offered (Fowlie et al. 2015, 2018; Holladay et al. 2016; Allcott and Greenstone 2017). Some authors have posited that this may be due to non-monetary costs not fully addressed by the intervention (Fowlie et al. 2015). Reminder mailings to encourage sign-up for energy audits also had small impacts (Gillingham and Tsvetanov 2018). This finding has also held true in other settings (e.g., participation in farm conservation programs; see Wallander et al. 2017). While not examined in an experimental setting, split incentives between landlords and tenants can contribute to low adoption of energy efficiency investments in rental properties (Gerarden et al. 2017; Giraudet 2020). In sum, it has proven difficult to boost participation in public programs, particularly among low-income populations, but more intensive outreach that addressed financial and non-financial barriers has worked in some contexts.

3.1 Study Objectives

Based on our review of the literature and the specifics of the LSLR program and the two interventions that offered subsidies to certain low-income urban residents, we test the following hypotheses:

Hypothesis 1: Rental properties, properties with lower assessed value, and properties in neighborhoods with higher poverty, lower educational attainment, and other indicators of economic disadvantage were less likely to participate in the LSLR program than other properties, absent additional interventions, due to greater financial and non-financial barriers.

Hypothesis 2: Households receiving outreach letters encouraging registration in the LSLR program participated at higher rates due to reduced non-financial barriers.

Hypothesis 3: Low-income homeowners offered either of the two grant programs to cover the $1000 cost participated in the LSLR program at higher rates due to reduced financial barriers compared to customers who were otherwise similar but were not offered a subsidy.

Hypothesis 4: Low-income households offered the community-based grant program were more likely to participate in the LSLR program than those sent a postcard about the housing department grant program due to the community grant program's more intensive outreach and simpler application process, which reduced non-financial barriers.

4 Data

We linked individual water system records with property tax assessment data and Census neighborhood characteristics. We obtained data from Trenton Water Works on 62,529 customer accounts reflecting LSLR program status in July 2020, May 2021, and July 2022. The data included property address, property type (residential or non-residential), account status (active or inactive), utility-side and homeowner-side service line material prior to any replacement work occuring as part of the LSLR program (if known), LSLR program registration and date of response, and date of utility-side and/or homeowner-side replacement occuring as part of the LSLR program (if applicable).[4,5] We also obtained data from the water system on which properties were sent letters and postcards about the LSLR program and which street segments were under a moratorium for non-emergency water line construction due to recent street paving.

We linked 91% of water system accounts to property tax assessment records for tax year 2020 using an exact string match of street address and city name, and when available in the assessor data, zip code. Tax records included data on property type, number of dwellings, year constructed, assessed value, location address, and property owner address.[6] We excluded duplicate accounts, accounts that tax assessment or water system records identified as non-residential, and—because larger buildings require larger diameter service lines that are rarely made of lead (EPA 2022)—apartment buildings with more than four dwellings (7% of accounts). We also excluded properties located in one suburban municipality in the service area where LSLs were never installed (3% of accounts). We geocoded property addresses and identified Census block groups using the Census Geocoder batch address processing tool. We excluded accounts in one block group that Census data indicated had no occupied housing units (less than 1% of accounts). We considered a property

[4] It is also possible for individual homeowners to hire their own contractor to conduct an LSLR. The water system lacks data on whether or when any such replacements may have occurred but believes that they are rare and that any such replacements were typically undertaken for another reason besides reducing lead exposure, such as fixing a water leak or other construction/plumbing work occuring at the same time. The water system estimates that the total cost of replacement to a homeowner would be roughly $8000 rather than the $1000 cost-share offered by the LSLR program.

[5] In the July 2022 data, program registration dates were missing for 55% of accounts that had registered, and lead service line replacement dates were missing for 2% of properties that had a replacement. In these cases, we used the July 2020 and May 2021 data to determine when registration and replacements occurred.

[6] We categorized properties as owner-occupied if the first seven characters of the property address matched the first seven characters of the owner address. This indicator is extremely similar to a variable denoting a match of the entire character string of the property and owner addresses ($\rho = 0.99$) but allows for flexibility due to spelling mistakes or differences in the way apartment numbers are recorded across the datasets.

to be affected by a construction moratorium if it was located within 50 m of a street segment under moratorium.

We incorporated data on Census block group sociodemographic characteristics from the 2019 American Community Survey (ACS) 5-year estimates obtained from the IPUMS National Historical Geographic Information System (Manson et al. 2022). The organization implementing the community-based grant program provided neighborhood boundaries, which corresponded to four Census block groups. Our final data set consisted of 55,917 water system accounts (20,529 urban and 35,388 suburban).

5 Empirical Approaches

5.1 Program Participation Regressions

Our first analysis used the full-sample data set to examine the factors associated with three outcome variables reflecting different aspects of program participation: registration for the LSLR program, participation in an interior home inspection conducted by a water system contractor or employee, and completion of an LSL replacement conditional on having an LSL. We regressed each participation variable on a set of property and neighborhood characteristics as follows:

$$y_{ij} = \beta_0 + \boldsymbol{\beta}_1 \boldsymbol{X}_i + \boldsymbol{\beta}_2 \boldsymbol{Z}_j + \varepsilon_{ij}, \tag{1}$$

where y_{ij} is one of the three participation outcomes observed for property i in neighborhood j from the start of the program in 2019 through July 2022, the end of our study period. We used a linear probability model because it accommodates a mix of continuous and categorical variables and offers easily interpretable marginal effects (Angrist and Pischke 2008).

$\boldsymbol{X}_i$ represents individual property characteristics, including whether an account was inactive at any point during the study period, the number of letters sent to the property to encourage registration in 2018 or 2019 (prior to the start of the LSLR program), and whether the property was located on a street segment with a water line construction moratorium during 2020 and 2021 due to recent street paving. Inactive accounts denote vacant properties; failure to pay water bills did not trigger inactive status or water shutoff during the study period. We also included a variable denoting whether the utility side of the service line contained lead; contractors had incentives to seek out these properties because they were able to conduct a replacement even if the homeowner side of the line was found to be lead-free. Furthermore, if the homeowner side was found to be an LSL, contractors were paid more for the work because both sides of the line required replacement. For properties linked to tax assessment data, we included year built (pre-1951 and 1951–1960, after which LSLs were uncommon in the study area according to water system records), the natural logarithm of assessed value, and indicators for owner-occupied and multi-unit (2–4 family) properties. We also included an indicator variable for properties that we were unable to link to assessment data. We imputed missing assessment data values using the average value for linked residential properties in the same municipality.[7]

$\boldsymbol{Z}_j$ represents neighborhood characteristics at the Census block group level, including percent of residents that are Black, percent of residents of Hispanic ethnicity, percent below the

[7] Municipality dummy variables jointly explained a statistically significant portion of the variation in the property characteristics included in our study ($p < 0.0001$ in all cases).

poverty level, percent under age 5, percent above age 64, and percent age 25 and over with a college degree. The β terms are coefficients to be estimated, and ε_{ij} is a heteroskedasticity-robust error term. We estimated Eq. (1) separately for urban and suburban municipalities to allow for heterogeneity in the association of characteristics with LSLR program participation.

For two of the three outcomes in our analysis—registrations and inspections—we included the full samples of urban and suburban properties regardless of service line material, since the material was typically unknown prior to inspection. The third outcome, replacement, is only relevant for properties found to have a homeowner-side LSL. Therefore, we estimated the LSLR outcome equation restricting the sample to properties with a confirmed homeowner-side LSL. This approach provides ready interpretation for water systems and policymakers about characteristics associated with willingness to participate in LSLR among the most relevant customer population. We included the same property and neighborhood characteristics as explanatory variables for this outcome, except for housing age because 98% of properties with a confirmed LSL were built before 1961.

5.2 Program Evaluations

Next, we evaluated the impacts on willingness to participate in the LSLR program of two subsidy programs offered to certain residents in the urban municipality during the study period: a community-based grant program for residents of a specific neighborhood and a housing department grant program for low- and moderate-income homeowners.

5.3 Synthetic Control Evaluation of Community-Based Grant Program

We conducted a quasi-experimental analysis to evaluate the effect of the community-based grant program on LSLR program participation.The sample included 18,879 individual properties, 1010 of which were located in the four block groups comprising the target neighborhood of East Trenton.[8] We estimated the difference-in-difference linear probability model

$$y_{ijt} = \gamma_0 + \gamma_1 Treat_j + \gamma_2 After_t + \delta Treat_j * After_t + \varepsilon_{ijt}, \tag{2}$$

where y_{ijt} represents the same three participation outcomes already discussed but includes subscript t to denote time. The model includes $Treat_j$ to denote location in one of the four Census block groups that comprise the neighborhood eligible for the grant program and $After_t$ to represent the period after the grant program's launch. The coefficients γ_0, γ_1, and γ_2 represent city-wide LSLR participation pre-launch, the difference in LSLR participation in the target neighborhood from the rest of the city pre-launch, and the city-wide increase in LSLR participation post-launch, respectively. The coefficient δ represents the impact of the grant program on participation in the target neighborhood. ε_{ijt} is a robust error term that is clustered at the block group level to match the spatial scale of the grant intervention (Abadie et al. 2022).

[8] The quasi-experimental sample includes all urban properties in our final data set, except for 1338 properties that received postcards about the housing department grant program as part of the field experiment, 201 properties receiving other water system outreach mailings after the launch of the community-based grant program, 80 properties whose program registration and LSLR dates could not be determined from the account data, and 22 properties in neighborhoods that were not visited by water system contractors during both the pre- and post-grant periods (none of which were in the target neighborhood).

Because location in the treatment neighborhood is not random and may be correlated with factors affecting LSLR program participation, we employed a synthetic control approach to estimate the causal effect of the community-based grant program. A synthetic control is a weighted average of observations in the untreated sample that serves as a counterfactual for the treatment group (Abadie 2021). In settings with many treated units, the weights balance the mean values (or other moments) of a set of observed characteristics across the treated and synthetic control groups (Hainmueller 2012; Robbins et al. 2017). To implement the synthetic control approach, we estimated the difference-in-difference model as a weighted regression.

We used entropy balancing to derive the weights for our synthetic control group. Entropy balancing is a matching method that identifies a set of non-negative weights that satisfy a set of balance constraints (in our case, equality of means of several variables), sum to the number of observations in the treatment group, and are as close as possible to uniform (Hainmueller 2012). We constrained the treatment and synthetic control groups to have equal mean values across all three pre-intervention outcomes, all property-level characteristics, and the neighborhood characteristics found to have a statistically significant association with LSLR program participation in the urban municipality. The target neighborhood has particularly low property values and a high share of adults without a college degree, so properties elsewhere in the urban municipality with these same characteristics were given greater weight in the synthetic control group. Appendix Fig. 6 illustrates the spatial distribution of property values, college attainment, and our synthetic control weights in the study area.

We excluded households in the treatment group for the community-based grant program from the postcard intervention field experiment to avoid spatial overlap across households affected by the two interventions. While it is possible that low- and moderate-income homeowners in the target neighborhood and synthetic control group might have been eligible for the housing department grant program, we think it is unlikely that they knew about the program given the lack of publicity and the fact that none were sent postcards. However, given this possibility, our approach isolates the impact of the community-based grant program from other factors affecting participation, including potential awareness of the housing department grant program.

As already noted, we examined the LSLR outcome using a restricted sample of properties confirmed to have a homeowner-side LSL, but we examined the registration and inspection outcomes using the broader sample of properties regardless of having a confirmed LSL. Consequently, we derived two sets of weights using entropy balancing corresponding to these two samples. The weights used with the broader sample to examine the likelihood of registration and inspection were derived using all three pre-treatment outcomes, all individual property characteristics, and all neighborhood characteristics except for share of the population under age 5 because this variable was not a significant predictor of any of the outcomes in the urban municipality (Table 1, columns 1 and 2). The weights used when examining LSL replacement conditional on having an LSL were derived using all three pre-treatment outcomes, all property characteristics, and the share of Black residents because this is the only neighborhood characteristic that is significantly associated with LSL replacement in this sample (Table 1, column 3). The two sets of weights are highly correlated ($\rho = 0.82$). Because the weights exactly balance the characteristics of all variables associated with LSLR program participation across the treatment and synthetic control groups, it is not necessary to include control variables in Eq. (2) to obtain an unbiased estimate of δ. However, we also present results of a model that included property characteristics and block group fixed effects in the Appendix.

We also estimated a standard difference-in-difference model controlling for property characteristics and block group fixed effects, as well as a model using coarsened exact matching to derive the control group (Iacus et al. 2012).

5.4 Experimental Evaluation of Housing Department Grant Program

The second evaluation is a field experiment to assess the impact of postcards informing residents about a program run by the city's housing department to cover homeowner-side LSLR costs (and other urgent home repairs) for low- and moderate-income homeowners. We identified 3100 properties in the urban municipality that were potentially eligible for the housing department grant program. To construct the sample for the field experiment, properties needed to be within the urban municipality, not be located in the neighborhood offered the community-based grant program, have an unknown homeowner-side service line material, and meet several other criteria to ensure the account was active and reasonable to include in the study.[9] The sample was stratified by block group and split into equally sized treatment and control groups of 1550 each.[10] Households were randomized at the individual level within block groups, meaning that within a neighborhood, some households received postcards while others did not. Randomizing at the individual rather than neighborhood level raises the prospect of spillover effects if residents receiving postcards discuss them with neighbors who did not receive them but avoids the potential for unobservable neighborhood characteristics to be confounded with the treatment. Rerandomization (Morgan and Rubin 2012) was used to ensure balance on pre-intervention LSLR program registration, participation in an inspection, and, for properties linked to assessor data, the inflation-adjusted home sales price and an indicator for pre-1951 construction.

The water system sent English-language postcards to the treatment group providing information on the availability of the housing department grant program to cover the costs of LSLR, eligibility criteria, information on how to apply, and a brief reminder about the health benefits of LSLR (Appendix Fig. 7). To evaluate the impact of notifying customers about the housing department grant program on participation in the LSLR program, we estimated the equation

$$y_{ijt} = \beta_0 + \gamma_1 Treat_i + \gamma_2 After_t + \delta Treat_i * After_t + \varepsilon_{it}. \quad (3)$$

In this case, $Treat_i$—being randomized into the group sent a postcard with information about the housing department grant program—was assigned at the individual rather than neighborhood level, so we use subscript i rather than j as in Eq. (2). We also estimated a version of the model that included property characteristics and block group fixed effects. The heteroskedasticity-robust error term, ε_{it}, is not clustered at the neighborhood level,

[9] Specifically, we excluded properties that met at least one of the following criteria prior to the intervention: the property was located outside of the urban municipality; the property was located inside the target neighborhood for the community-based grant program; the homeowner-side service line was confirmed to not contain lead; a LSLR already occurred; the water system account was inactive, vacant, or a non-valid address according to water system records; the property was not owner-occupied according to assessor data or the municipality's registry of rental properties; occupants had previously refused water system staff or contractors access to the property; the property was located in a Census block in which no properties had previously registered for the LSLR program.

[10] Prior to randomly assigning addresses to the treatment and control groups, power calculations were estimated using a sample size of 1500 treatment and 1500 control with an assumed baseline take-up rate of 10%, resulting in a minimum detectable effect of 3.2%. The 10% take-up rate was based on the rate of program registration among urban households before postcards were sent.

consistent with random assignment of individual properties to the treatment after stratifying by block group.

Fifteen percent of the assigned treatment group did not receive the mailing, because either the mailing address was deemed invalid by the firm contracted to print and mail the postcards or the postcard was returned to the water system by the postal service. Estimating the impact of the postcard based only on those that received it (i.e., the "as-treated" group) could yield a biased estimate if properties with undeliverable addresses are systematically different from the rest of the sample in terms of propensity to participate in the LSLR program. Alternately, estimating the impact of the postcard using an "intent-to-treat" model that uses *assignment* to the treatment group as the treatment variable could bias the estimate of the impact of receiving a postcard downward because 15% of the assigned treatment group did not receive the treatment. To mitigate the potential for either type of bias, our preferred estimate of (3) uses an instrumental variables (IV) model. In the IV approach, we define the treatment variable as actual receipt of the postcard (i.e., the postcard was sent to an address that was not deemed invalid or returned to sender). We define the instrument as assignment to the treatment group (whether or not the treatment was actually received). The IV approach mitigates the potential for bias (Sussman and Hayward 2010), but we also estimate variants of the model that use the "as-treated" and "intent-to-treat" approaches for comparison.

6 Results

6.1 Characteristics of LSL Incidence and LSLR Participation

Figure 1 summarizes key characteristics of properties in our analysis for urban (i.e., City of Trenton) and suburban municipalities. Urban properties were more likely to register for the program, have a contractor inspection, and replace confirmed homeowner-side LSLs than suburban properties. LSLs were more prevalent in the urban municipality (prior to any replacements), while it was more common for service line material to still be unknown by the end of the study period in the suburbs. Due to higher expected prevalence of LSLs, urban properties were more likely to receive an outreach letter from the utility encouraging LSLR program registration. The higher prevalence of LSLs in the urban municipality is consistent with the much higher proportion of pre-1951 housing stock compared to the suburbs. Urban properties were also more likely to have utility-side LSLs, be renter-occupied, have lower assessed values, and be in neighborhoods with higher rates of Black and Hispanic residents, people living below the poverty level, and lower rates of college graduation. These results are similar to an analysis of block group characteristics in four cities that found that likelihood of having an LSL was associated with a higher share of older homes, multi-unit buildings, and poverty in all four cities and a higher share of minority residents, renters, and adults without a college degree in three out of the four cities (GAO 2020). Figure 2 shows the spatial distribution of property and neighborhood characteristics and program participation in the study area. Appendix Tables 2, 3 and 4 include additional data comparisons by location and material type.

Table 1 and Fig. 3 display results from multivariate regressions examining the characteristics of properties that participated in the LSLR program in urban and suburban municipalities. Results are generally consistent with the hypotheses that owner-occupied properties, higher valued properties, and properties that were sent outreach letters from the water

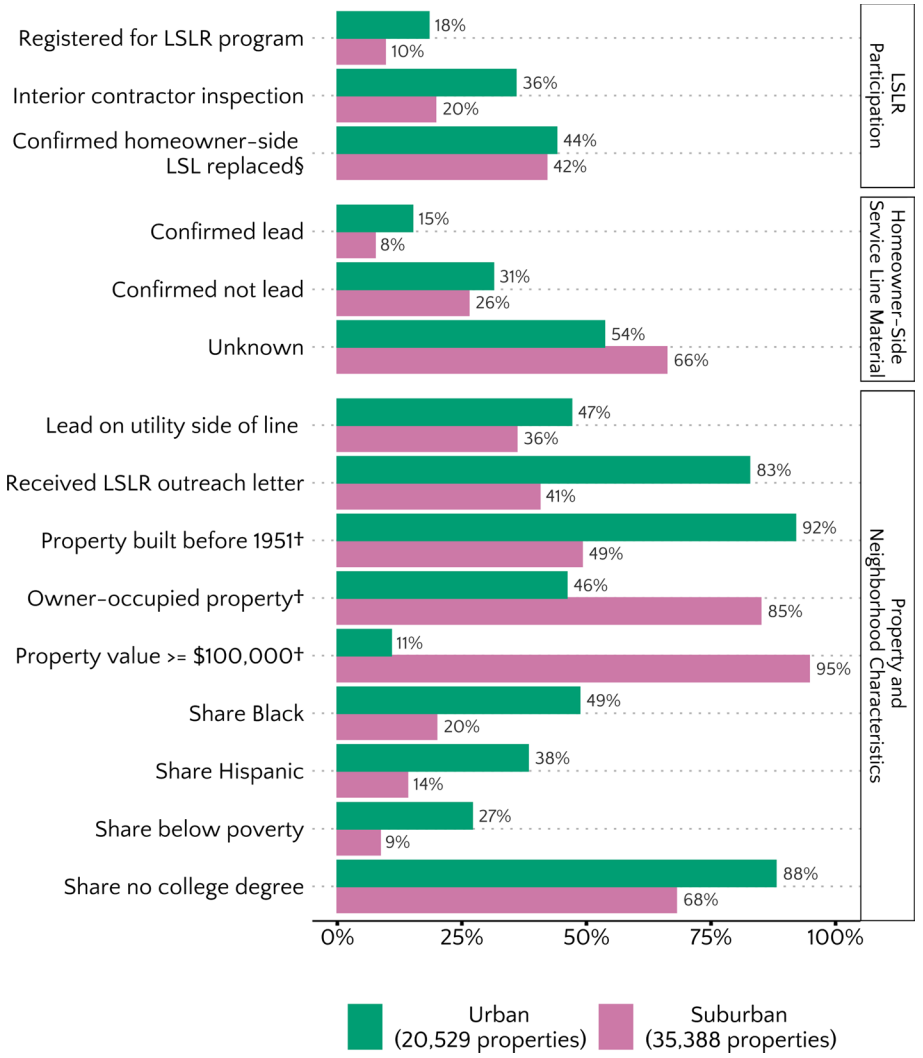

Fig. 1 Select characteristics of the study sample. All differences between mean urban and suburban municipality characteristics are statistically significant ($p < 0.05$) based on two-tailed *t*-tests. Homeowner-side service line material refers to the material before replacements were conducted as part of the LSLR program. §Data presented only for properties with confirmed homeowner-side LSL. †Data presented only for properties linked to assessor data

system were all more likely to participate in the LSLR program, even after controlling for home age and utility-side service line material, the characteristics used by the water system to target properties for LSLR outreach.

An examination of neighborhood-level variables yields less consistent results across program outcomes and locations. Neighborhood indicators of disadvantage were sometimes, but not always, associated with lower program participation. Neighborhoods with a higher share of Black residents and households below the poverty line had higher

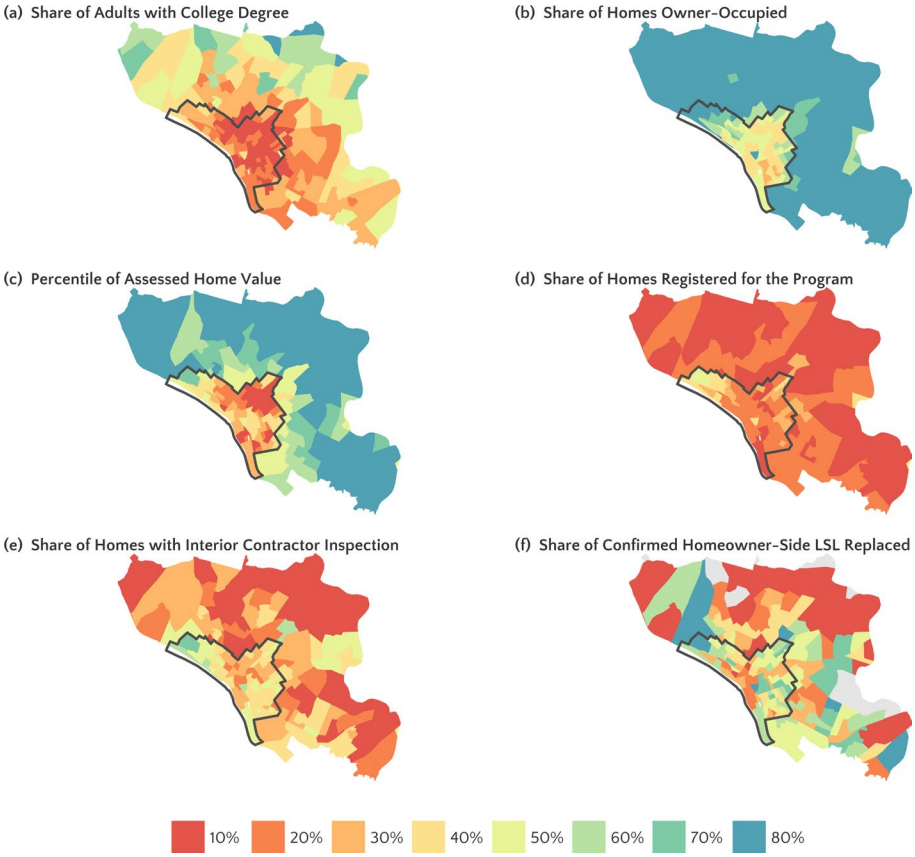

Fig. 2 Spatial distribution of neighborhood and property characteristics and LSL replacement program participation. Individual LSLR program participation and property assessor data on tenancy and home values were aggregated to the Census block group level. Census block groups are included if they fall within the service boundary of the public water system and are part of one of the four municipalities where the water system conducted the LSLR program. The interior of the black outline denotes Trenton, the urban municipality, and the remaining areas comprise the suburban municipalities in the study area. Gray shading in panel (**f**) denotes block groups where LSLR is not applicable because there were no confirmed homeowner-side LSLs

registration rates in the urban municipality, and neighborhoods with a higher share of Hispanic residents had higher registration rates in the suburban municipalities.[11] However, suburban neighborhoods with higher shares of Black residents were less likely to have inspections, and those with confirmed homeowner-side LSLs were ultimately less likely to

[11] These results are not driven by multicollinearity with other socioeconomic variables included in the analysis. While the shares of Black and Hispanic residents in the urban municipality are strongly negatively correlated ($\rho = -0.88$), there is low correlation among the other explanatory variables included in the analysis. The variance inflation factors for share Black and share Hispanic in the urban regressions are 6.3 and 7.6, respectively. Variance inflation factors for all variables in all regressions shown in Table 1 are below 3, indicating relatively stable coefficient estimates.

Table 1 Regression analysis of determinants of LSLR program participation

	Urban municipality			Suburban municipalities		
	(1)	(2)	(3)	(4)	(5)	(6)
	Registration	Inspection	Replacement of confirmed LSL	Registration	Inspection	Replacement of confirmed LSL
Number of LSLR letters	0.026***	0.051***	0.066***	0.099***	0.148***	0.113***
	(0.005)	(0.006)	(0.014)	(0.004)	(0.004)	(0.017)
Account inactive	− 0.024***	0.002	− 0.027	− 0.005	− 0.017**	− 0.013
	(0.008)	(0.010)	(0.028)	(0.007)	(0.008)	(0.033)
Lead on utility side	0.180***	0.360***	0.350***	0.037***	0.171***	0.355***
	(0.007)	(0.008)	(0.021)	(0.006)	(0.007)	(0.029)
Street paving moratorium	0.001	− 0.061***	− 0.008	− 0.015***	− 0.127***	− 0.133***
	(0.008)	(0.009)	(0.023)	(0.005)	(0.005)	(0.029)
Owner-occupied property	0.074***	0.038***	0.215***	0.022***	0.043***	0.072***
	(0.006)	(0.007)	(0.017)	(0.004)	(0.006)	(0.023)
Multi-unit property	− 0.070***	− 0.142***	0.001	0.002	− 0.095***	− 0.100
	(0.010)	(0.011)	(0.032)	(0.018)	(0.020)	(0.086)
Log assessed value	0.071***	0.051***	0.051**	0.014***	0.032***	− 0.047
	(0.008)	(0.008)	(0.021)	(0.005)	(0.006)	(0.030)
Property built before 1951	0.105***	0.143***		0.043***	0.065***	
	(0.009)	(0.011)		(0.004)	(0.005)	
Property built 1951–1960	0.100***	0.216***		0.016***	0.073***	
	(0.020)	(0.022)		(0.004)	(0.005)	
Assessor data unlinked	− 0.063***	− 0.119***	− 0.058	− 0.018***	− 0.063***	0.011
	(0.010)	(0.012)	(0.052)	(0.004)	(0.006)	(0.034)
Share Black	0.131***	0.045*	− 0.123*	− 0.006	− 0.097***	− 0.233***
	(0.023)	(0.027)	(0.069)	(0.009)	(0.011)	(0.051)
Share Hispanic	0.026	0.011	− 0.092	0.065***	0.034*	− 0.108
	(0.027)	(0.031)	(0.082)	(0.016)	(0.019)	(0.069)
Share under 5	− 0.152**	0.021	− 0.192	0.007	− 0.334***	0.395*
	(0.063)	(0.071)	(0.191)	(0.042)	(0.052)	(0.205)
Share over 64	− 0.046	0.239***	0.003	− 0.016	− 0.060**	0.242*
	(0.048)	(0.055)	(0.162)	(0.022)	(0.028)	(0.136)
Share college graduate	0.175***	0.231***	− 0.045	0.053***	0.015	− 0.035
	(0.044)	(0.049)	(0.128)	(0.013)	(0.016)	(0.078)
Share below poverty	0.096***	0.022	0.027	− 0.036	− 0.031	− 0.012
	(0.022)	(0.026)	(0.068)	(0.022)	(0.027)	(0.127)
Constant	− 0.114***	− 0.046	0.174**	− 0.043***	0.007	0.106**
	(0.028)	(0.032)	(0.083)	(0.011)	(0.014)	(0.053)
Observations	20,529	20,529	3098	35,388	35,388	2671
R^2	0.102	0.210	0.236	0.116	0.269	0.311

Linear probability models estimate the associations between outcomes (1)–(3) (urban sample) or (4)–(6) (suburban sample) and property and neighborhood characteristics. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t-tests

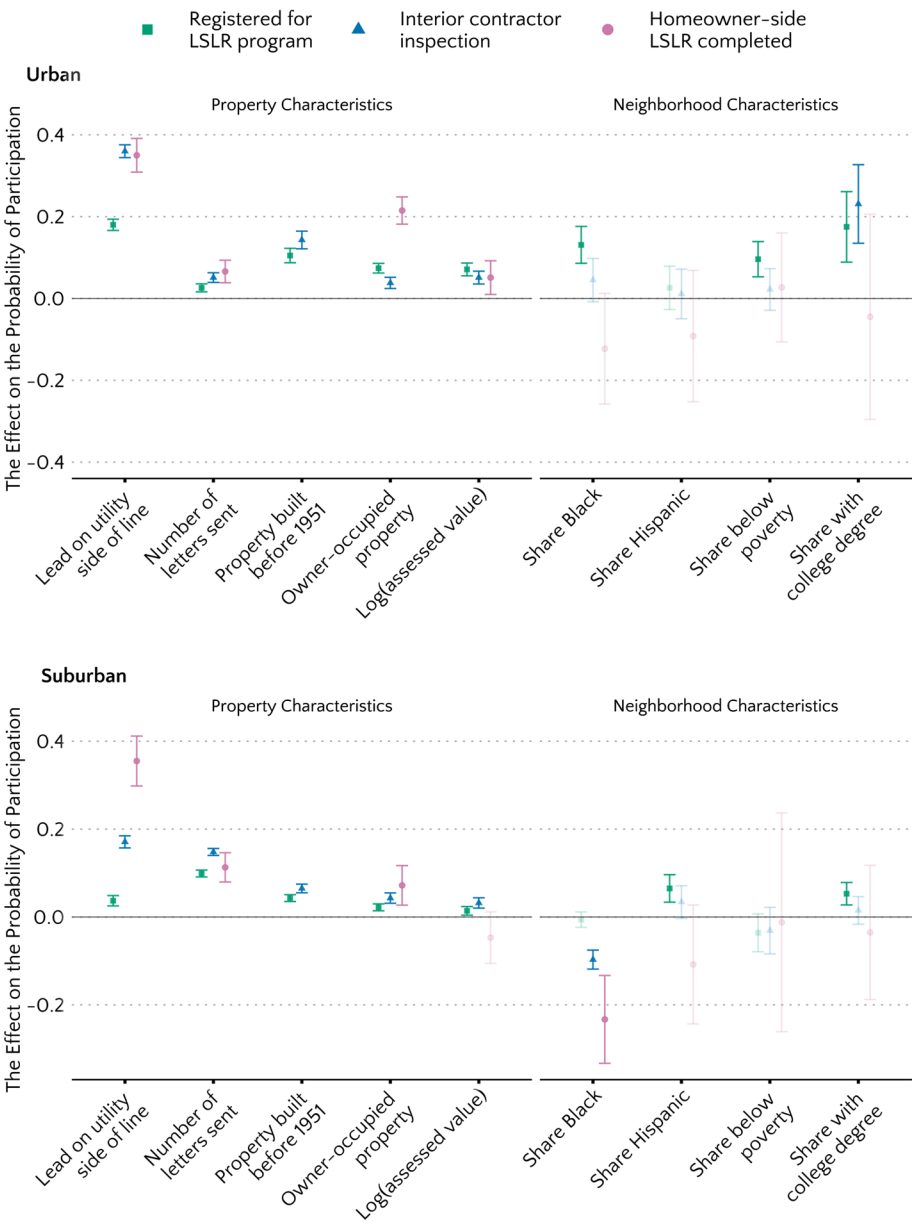

Fig. 3 Regression analysis of select determinants of LSLR program participation. The point estimates of the regression coefficients for each of the three outcome variables (1) registered for LSLR Program (green square), (2) interior contractor inspection (blue triangle), and (3) homeowner-side LSL replaced (purple circle) are plotted along with 95% confidence intervals. When the 95% confidence interval includes 0, the estimate is a lighter shade

have the line replaced compared to suburban neighborhoods with lower shares Black residents. Neighborhoods with a higher share of college graduates had higher rates of program registration and inspections, though not replacement of confirmed LSLs, than neighborhoods with lower shares of college graduates.

Pre-1951 properties were more likely to register and have inspections. (Almost all properties with confirmed homeowner-side LSLs—the relevant population for examining replacement—were built pre-1951, so this variable was excluded from the LSLR regressions.) Participation was also significantly correlated with having lead on the utility side of the service line. This result is consistent with the fact that contractors had incentives to work at homes with utility-side LSLs, where they would be guaranteed some construction work and would receive a higher payment if a full line replacement was needed.[12]

6.2 The Effect of Subsidies on LSLR Participation

Figure 4, Panel A presents the results of the quasi-experimental analysis evaluating the impact of the community-based grant program on LSLR participation in the urban municipality. The synthetic control approach ensures that pre-treatment participation rates are equal across the treatment and synthetic control groups. Property and neighborhood characteristics are also balanced across treatment and synthetic control groups (Appendix Table 6). The results shown in Fig. 4, Panel A were derived using the coefficient estimates from the synthetic control difference-in-difference regression reported in Appendix Table 7. After the grant was introduced, LSLR participation in the target community was substantially higher than in the synthetic control. Registration rates were twice as high, and inspection and replacement rates were more than 50% higher in the treatment group than in the synthetic control group, differences that are statistically significant.[13]

It is worth noting that LSLRs also increased significantly among the synthetic control group during the treatment period. An increase in the rate of LSLRs occurred over time throughout the study area as contractors gained experience and increased their capacity to implement replacements, and as residents' awareness of the program grew. The rate of replacements was also higher in the post-treatment period because restrictions related to the COVID-19 pandemic had been lifted. The increasing trend in replacements underscores the importance of identifying the effect of the subsidy program relative to a synthetic control group that experienced all other trends common to the study area except for

[12] Appendix Table 5 reports results from a regression in which we pooled the urban and suburban samples and included interaction terms between an urban indicator variable and all other explanatory variables to assess the heterogeneity across locations in associations between property and neighborhood characteristics and LSLR program participation. The results show that the magnitudes of the associations between property and neighborhood characteristics and LSLR participation are often significantly different across urban and suburban municipalities, even though Table 1 indicates that the sign and statistical significance of most characteristics' association with LSLR program association is similar.

[13] The unadjusted p-values of the effect of the community-based grant program on program registration, inspection, and replacement are 0.023, 0.055, and 0.032, respectively. When applying the Holm-Bonferroni adjustment to account for the possibility of a higher rate of falsely rejecting the null when testing multiple hypotheses, the p-values become 0.069, 0.055, and 0.064 (Holm 1979). While there is a marginal decline in statistical significance, the results remain statistically significant at the 10% level using a two-tailed *t*-test. Because the three LSLR program participation outcomes are highly correlated, and the Holm-Bonferroni approach does not account for correlation across outcomes, the approach has low power to detect false null hypotheses (List et al. 2019). We provide the adjusted p-values for illustrative purposes to demonstrate the robustness of our results to a conservative approach for addressing multiple hypothesis tests.

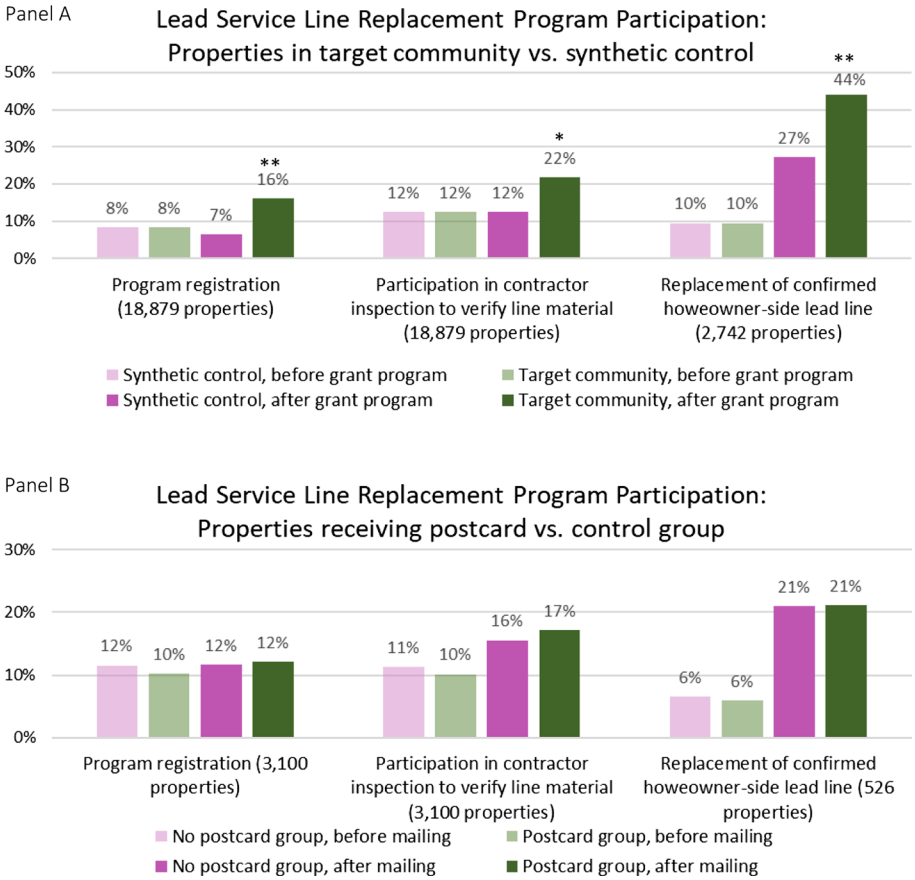

Fig. 4 The impact of community-based grant program (Panel **A**) and postcard experiment (Panel **B**) on LSLR registrations, inspections, and replacements. (Panel **A**) Because participation in the synthetic control before grant launch (light purple) and the target community before grant launch (light green) are identical, the treatment effect of the community grant program is the difference between the synthetic control community participation after grant program launch (dark purple) and the target community participation after grant program launch (dark green). Asterisks denote statistical significance of the treatment effect (***$p<0.01$, **$p<0.05$, *$p<0.1$). (Panel **B**) The treatment effect of the outreach study is the difference between the no-postcard group participation after mailing (dark purple) and the postcard group participation after mailing (dark green) minus the difference between the no-postcard group participation before mailing (light purple) and the postcard group participation before mailing (light green). There were no statistically significant treatment effects in this analysis

implementation of the community-based grant program.[14] It is also possible that the outreach conducted by the community-based organization had positive spillovers in the urban area outside of the target neighborhood, contributing to the increase in program participation over time throughout the study area. If such spillovers did occur, then our estimates of

[14] This upward trend is less apparent for the program registration outcome, which was less affected by the COVID-19 pandemic; the water system began registering customers in 2018 and continued to encourage sign-up throughout 2020, anticipating that the program would eventually resume. The water system slowed its efforts to register new properties later in 2021 and 2022 as it became clear that replacements would pause later in 2022 due to funding constraints. In addition, program registration was not strictly required for residents to have inspections and replacements.

the effectiveness of the community-based grant program are biased downward, and the true estimate would be even larger. This situation would not change our finding that all three participation outcomes were significantly higher in the target community after the grant program launch compared to the synthetic control.

The estimated difference-in-difference coefficients representing the impact of the community-based grant program are identical regardless of whether property characteristics and block group fixed effects are included in the regressions using the synthetic control approach (Appendix Table 7 and 8). The estimated impacts are also similar when using an unweighted difference-in-difference model including property characteristics and block group fixed effects in lieu of a synthetic control approach, indicating that the results are robust to alternative ways of constructing the control group (Appendix Table 9). However, pre-treatment inspection and replacement rates were notably higher without the synthetic control approach (Appendix Fig. 8), suggesting that the synthetic control provides a more appropriate counterfactual for the target community. Results were also qualitatively similar using coarsened exact matching (CEM) as an alternative matching approach used to construct the control group (Appendix Table 10). As with the unweighted data, pre-treatment participation outcomes were significantly different across the treatment and control groups when using CEM, confirming that the synthetic control approach provides a more appropriate control group.

Figure 4, Panel B shows the results of the postcard experiment promoting the housing department grant program. These results were derived using the coefficient estimates from the instrumental variables difference-in-difference regression (Appendix Table 12).[15] Program registration, inspection, and replacement of confirmed LSLs were similar across the treatment and control groups before the intervention.[16] After the postcards were sent, there was an increase in all three program participation measures in both the treatment and control groups, consistent with the increase in LSLR program activity during the study period. There were slightly larger increases in registration and inspections in the treatment group than the control group, and no differences in the change in replacement rates of confirmed LSLs across the two groups. None of the differences across treatment and control groups are statistically significant.[17] The treatment effects for registration and inspections are 1.7% and 2.8%, respectively. These effects are below the minimum detectable effect size of 3.2% and so are not statistically significant. The increase in replacement rates among properties with confirmed homeowner-side LSLs was nearly identical across the two groups. The effect size of 0.8% is near zero in a qualitative sense as well as not being statistically significant.

Furthermore, housing department staff confirmed that they did not receive a single application for LSLR under the grant program after the postcards were sent (personal communication, Farrah Gee, City of Trenton Department of Housing and Economic Development). Therefore, our null results are unlikely to be driven by spillover effects that could have occurred if residents who received postcards shared the information with neighbors

[15] The first stages of the IV regressions are reported in Appendix Table 13. They confirm that assignment to the postcard group is very strongly predictive of receiving a postcard.

[16] In addition, appendix Table 11 confirms that property and neighborhood characteristics are well balanced across the treatment and control groups.

[17] The unadjusted p-values of the effect of the housing department grant program on program registration, inspection, and replacement are 0.36, 0.17, and 0.87, respectively. Applying the Holm-Bonferroni adjustment for multiple hypothesis testing does not change our inability to reject the null hypothesis of no effect of the housing department grant program on LSLR program participation.

or friends who did not. These results suggest that the informational treatment about a grant opportunity was ineffective in boosting LSLR program participation.

Appendix Table 14 presents the coefficient estimates for the intent-to-treat difference-in-difference model that does not correct for the fact that 15% of observations in the assigned treatment group did not receive the postcards. While it is unnecessary to include control variables or block group fixed effects in this equation to estimate the treatment effect because the intervention was randomized within block groups and balanced on key observable characteristics related to LSLR program participation, Appendix Table 15 presents the coefficients for the intent-to-treat difference-in-difference model adding property-level control variables and block group fixed effects. The estimated treatment effects are identical to those in the intent-to-treat model that does not include the control variables and fixed effects. This result is consistent with the fact that the treatment and control groups are balanced in terms of property and neighborhood characteristics. Appendix Table 16 presents estimates using an as-treated approach in which properties that received a postcard are considered the treatment group and those whose addresses where undeliverable are considered part of the control group. The intent-to-treat, as-treated, and instrumental variables approaches all yield similar results: the effects of the postcard about the housing department grant program on all three measures of lead service line program participation are always negligible and indistinguishable from zero.

Because the postcards only included English-language information, they could have been less effective in areas with a higher concentration of non-English speakers. We tested this hypothesis by interacting the treatment variable with the share of Hispanic residents in the block group (Appendix Table 17). The impact of receiving a postcard interacted with the share of Hispanic residents is not statistically significant in any of the three LSLR program participation outcomes.

Overall, results from the two program evaluations are not consistent with the hypothesis that households offered either subsidy program are more likely to participate in the LSLR program, since there was no uptake of the housing department grant program, and the postcards about the housing department grant had no effect on LSLR participation. However, the differing results across the two grant programs is consistent with the hypothesis that subsidies are more effective when combined with outreach and trusted community messengers that can address non-financial barriers to participation.

7 Discussion

Our results show that LSLs were more prevalent in the urban municipality of the water system, which has a higher concentration of pre-1950 housing, Black and Hispanic residents, renters, and poverty than the suburban municipalities. Urban properties were also more likely to register for the program, have a homeowner-side service line inspection, and replace confirmed homeowner-side LSLs than suburban properties during the study period.

Notable results from the regression analyses of the full urban and suburban samples include the importance of tenancy, property values, and outreach. Replacement rates among urban properties with confirmed LSLs were 21 percentage points higher in owner-occupied than in renter-occupied properties. In suburban municipalities, this association is less pronounced but still statistically significant, with owner-occupied properties 7 percentage points more likely to replace their LSLs than renter-occupied properties. Registration and inspection rates were also significantly higher in owner-occupied than renter-occupied

properties in both urban and suburban municipalities. These results suggest that split incentives between landlords and renters pose a substantial barrier to LSL replacement. This finding is consistent with studies showing that owners are more likely than renters to make energy-efficient investments for residential buildings (Gerarden et al. 2017; Giraudet 2020).

Properties with higher assessed value were also significantly more likely to register and to have an inspection in both urban and suburban municipalities, and significantly more likely to replace a confirmed LSL in the urban municipality. This result suggests that barriers to participation were greatest among lower income households, which is consistent with hypothesis 1 and with existing literature showing higher uptake among relatively more advantaged households eligible for public assistance programs (Deshpande and Li 2019; Finkelstein and Notowidigdo 2019). It is important to note that the urban and suburban regression analyses cannot separately identify the role of financial and non-financial barriers since low-income households likely face both types of barriers disproportionately.

At the neighborhood level, registration rates were sometimes higher in areas with a higher share of Black or Hispanic residents, suggesting that non-financial barriers such as lack of information, mistrust, and language barriers were not necessarily higher in these neighborhoods. However, registration rates were also higher in neighborhoods with a higher share of college graduates. In addition, participation in replacement of confirmed LSLs—the ultimate outcome of interest—was lower in neighborhoods with a higher share of Black residents, which could reflect additional financial or non-financial barriers not reflected in tenancy and property values. These results generally support Hypothesis 1.

Another finding across urban and suburban municipalities is the importance of water system outreach, lending support to Hypothesis 2. The number of letters sent to encourage registration before program implementation was significantly associated with all three participation outcomes. This finding is consistent with studies of school choice and tax credit programs that found a boost in participation from information treatments (Linos et al. 2022; Bhargava and Manoli 2015; Hastings and Weinstein 2007). In the urban municipality, replacement conditional on having a confirmed LSL was 7 percentage points higher per letter sent, while in suburban municipalities, the effect was even larger at 11 percentage points per letter. Since letters were sent to properties rather than owners, the higher rental rate in the city could be a reason for the smaller magnitude of this coefficient. The water system targeted the letters to properties suspected to have homeowner-side LSLs, so we cannot make a strong causal claim about the impact of the letters because they are likely correlated with additional forms of outreach that were not tracked in our data, such as contractor and water system staff door-to-door visits. In addition, homeowners who suspected they had an LSL might have been more likely to register and sign up for an inspection even in the absence of outreach letters, though our analysis controlled for housing age, the primary determinant of LSL occurrence.

Our evaluations of two grant programs providing full-cost subsidies for certain urban residents yield insights about approaches that can motivate participation among residents facing a combination of financial and non-financial barriers. We find that the community-based grant program was highly effective in encouraging LSL replacement. Replacement rates among properties with confirmed homeowner-side LSLs increased by 17 percentage points more in the target community than in the synthetic control group. In contrast, the postcard informing residents about the housing department grant program was not effective. This finding is counter to what we posited in hypothesis 3

since both grant programs offered $1000 subsidies but only one had an impact on program participation.

However, the divergent results of the community-based and housing department grant programs are consistent with Hypothesis 4. The two programs differed substantially in mode of outreach and application process, which are factors relevant to non-financial barriers. The community-based grant program was accompanied by extensive outreach and education about LSLR, while the experiment promoting the housing department grant program involved a single postcard, and the program was not otherwise well publicized. In addition, the housing department grant program required an application including formal documentation of income to prove eligibility, while the community program had no income requirements. The results suggest that LSLR subsidy programs that address financial barriers alone are not sufficient to boost participation. Rather, consistent with past literature on encouraging uptake of programs such as disaster preparedness and college financial aid, approaches that use more intensive outreach and assistance to address non-financial barriers can greatly boost participation among low-income customers (Glik et al. 2014; Bettinger et al. 2012).

Participation in the neighborhood targeted by the community grant program, while much higher than in the control group, topped out at 54% of properties with confirmed LSLs during the study period even when the financial barrier to participation was removed for homeowners and substantially reduced for landlords. A representative from the community organization confirmed that the program could have funded more LSLRs had they received more applications, indicating that the shortfall was due to lack of customer demand rather than insufficient funding (personal communication, Caitlin Fair, East Trenton Collaborative). Considering that only 41% of residential properties in the target neighborhood were owner occupied and that only these properties were eligible for full $1000 subsidies, it is possible that the grant program could have had a larger effect if rental properties had also qualified for full rather than partial LSLR subsidies.

8 Conclusion

Our results confirm that water systems designing programs to identify and replace LSLs need to address both financial and non-financial barriers to achieve the goal of full and equitable replacement of LSLs. Our findings are consistent with past program evaluations showing that simple mailers providing information to households on how they can apply for assistance are not an effective way to boost participation. They also suggest that addressing financial barriers alone—even when those barriers are sizable—is not sufficient to induce participation among low-income customers. Rather, programs that are free, well publicized, and easy to access are likely needed to garner voluntary participation in programs to replace lead service lines.

9 Appendix: Factors Influencing Customer Participation in the Replacement of Lead-Contaminated Drinking Water Infrastructure

9.1 Expanded Study Area Background

Customer participation in the Trenton Water Works LSLR program was voluntary and open to all properties in Trenton and three suburban municipalities served by the water system with LSLs. Water system records indicate that there are no lead service lines in a fourth suburban municipality in the service area; we exclude this municipality from the analysis. Most service lines classified as lead by the water system are actually lead-lined galvanized steel and pose similar risks to water quality as solid lead service lines. The State of New Jersey also considers galvanized service lines as LSLs (https://www.nj.gov/dep/lead/notices.html, Accessed Jan. 17, 2023). We follow this definition and refer to lead-lined galvanized steel service lines as LSLs.

The water system began encouraging customers to register for the program in 2018. Program registration did not entail a formal commitment by the homeowner to pay the $1000 cost and proceed with replacement (if the line was confirmed to be lead), nor was registration required for an inspection or eventual replacement—it served simply as a signal of interest. The water system prioritized properties that registered for the program prior to 2020 for inspection and replacement efforts. It also distributed contractor work throughout the service area to ensure that inspections and replacements occurred in all four municipalities with LSLs. LSLR construction work started in February 2020, paused during March 2020 due to the COVID-19 pandemic, then resumed in July 2020. As of July 2022, Trenton Water Works had conducted some inspections and replacements in almost all neighborhoods in the service area. An interior inspection and/or replacement occurred in at least one home in 95% of Census block groups in the study area by the end of the study period. These block groups included over 99% of accounts in our final analysis sample.

The homeowner-side service line material was often unknown when customers registered for the LSLR program. Consequently, customer cooperation was necessary for material identification or verification, typically with residents needing to allow a water system contractor or employee inside the home to inspect the pipe. Alternatives to an interior contractor inspection included a self-inspection photo sent by the homeowner to the utility (an option introduced by the utility in 2020 in response to the COVID-19 pandemic), or an external excavation that required no cooperation from the homeowner. We consider the service line material to be "confirmed" if it was inspected by a utility employee or contractor (interior visual inspection or exterior excavation) or a homeowner visual inspection documented in a photo submitted to the utility. We do not consider historic utility records to be sufficient for homeowner-side service line material confirmation.

If an LSL was confirmed by the inspection, the homeowner was asked to sign a contract and right-of-entry agreement to replace the line. A resident was required to be present during the replacement, which typically took about half a day to complete. Construction included patching of walls or floors and restoration of pavement or sod damaged during the replacement. For properties confirmed to have lead on the utility-side of the line and a non-lead material on the homeowner-side, the water system could conduct a replacement on the utility side without accessing the property, so further homeowner participation was not needed.

See the Figs. 5, 6, 7, 8 and Tables 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17.

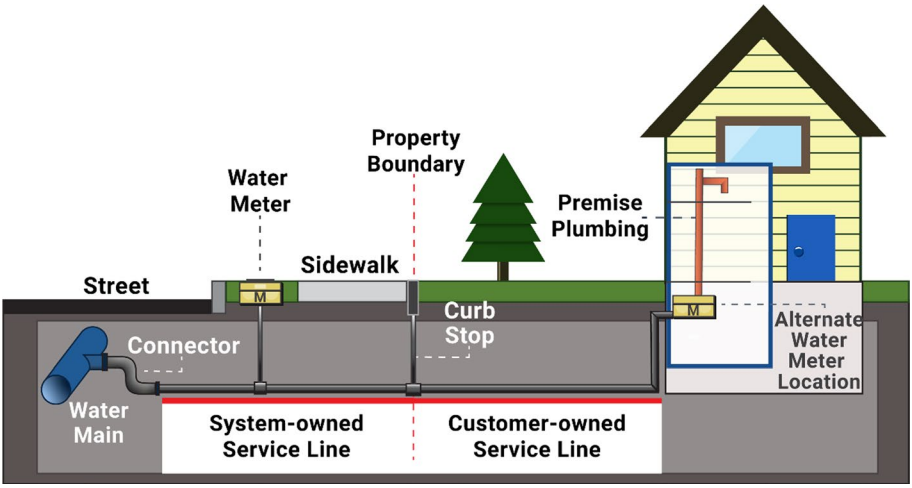

Fig. 5 Split ownership of water service lines. This illustrative service line configuration shows the typical split ownership of the service line between homeowner and utility. Either or both sides can contain lead. Public water systems may have records of where lead-containing utility-owned service lines are located. Inventory on the homeowner-side of the service line is typically less well documented and is a necessary step in replacing water service lines. *Source*: EPA 2022

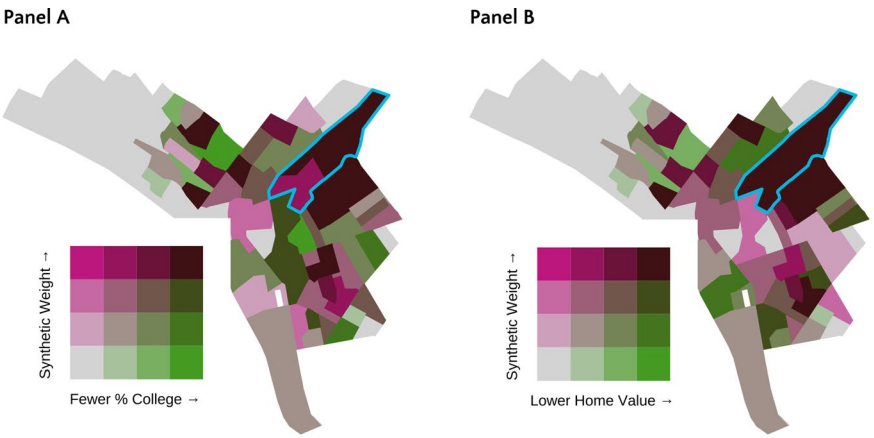

Fig. 6 Spatial distribution of neighborhood and property characteristics in the community-based grant program target neighborhood and corresponding synthetic control group. Synthetic weights were used to establish a balanced control group for the target neighborhood (outlined in light blue) for the evaluation of the community-based grant program. While all households in the target neighborhood received a weight of 1, there is some variation in the demographics within the neighborhood shown at the block group level (Panel **A**)

Fig. 7 Postcard sent to experimental sample

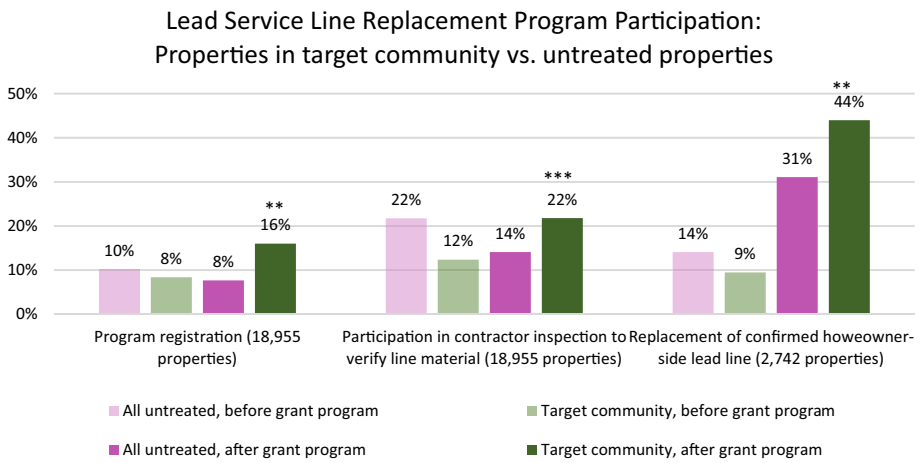

Fig. 8 Impact of community-based grant program on LSLR registrations, inspections, and completions using an unweighted average of all untreated properties instead of a synthetic control. Asterisks denote statistical significance of the treatment effect (***$p<0.01$, **$p<0.05$, *$p<0.1$)

Table 2 Full sample summary statistics disaggregated by homeowner-side service line material prior to any replacements

Observations	Confirmed lead		Confirmed non-lead		Material unknown	
	5769		15,761		34,387	
	Mean	SD	Mean	SD	Mean	SD
LSLR Participation						
Registered for LSLR program	40%	0.49	27%***	0.44	2%***	0.13
Interior contractor inspection	66%	0.48	67%	0.47	0%***	0.02
Replaced homeowner-side LSL[§]	43%	0.50	0%***	0.00	0%***	0.00
Property characteristics						
Lead on utility side of line	59%	0.49	58%	0.49	28%***	0.45
Number of LSLR letters[◊]	1.15	0.80	1.09***	0.82	0.60***	0.74
Account inactive	8%	0.27	7%	0.26	6%***	0.25
Street paving moratorium	10%	0.30	8%***	0.27	12%***	0.33
Suburban location	46%	0.50	59%***	0.49	68%***	0.47
Assessor data unlinked	4%	0.20	6%***	0.24	11%***	0.31
Property built before 1951†	87%	0.33	59%***	0.49	55%***	0.50
Property built 1951–1960[†]	10%	0.30	23%***	0.42	16%***	0.37
Owner-occupied property[†]	66%	0.47	71%***	0.46	71%***	0.45
Multi-family property[†]	5%	0.21	4%***	0.19	5%	0.21
Assessed value ($100,000)[†]	1.11	0.68	1.39***	0.78	1.58***	0.93
Neighborhood characteristics						
Share Black	36%	0.27	31%***	0.27	29%***	0.27
Share Hispanic	31%	0.24	23%***	0.23	22%***	0.22
Share under 5	7%	0.05	6%***	0.04	6%***	0.04
Share over 64	11%	0.06	13%***	0.07	13%***	0.08
Share college graduate	19%	0.13	25%***	0.16	25%***	0.17
Share below poverty	18%	0.14	15%***	0.14	15%***	0.13

[§]Data presented only for properties with confirmed LSL. [†]Data presented only for properties linked to assessor data. [◊]LSLR letters refers to mailings sent by the utility in 2018 and 2019 to properties suspected of having an LSL to encourage program registration rather than outreach conducted through the community-based grant program or the housing department grant program. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ denotes a statistically significant difference compared to properties with confirmed homeowner-side lead lines using two-tailed *t*-tests

Table 3 Urban sample summary statistics disaggregated by homeowner-side service line material prior to any replacements

Observations	Confirmed lead		Confirmed non-lead		Material unknown	
	3098		6429		11,002	
	Mean	SD	Mean	SD	Mean	SD
LSLR participation outcomes						
Registered for LSLR program	41%	0.49	36%***	0.48	2%***	0.12
Interior contractor inspection	70%	0.46	80%***	0.40	0%***	0.03
Replaced homeowner-side LSL[§]	44%	0.50	0%***	0.00	0%***	0.00
Property characteristics						
Lead on utility side of line	60%	0.49	65%***	0.48	32%***	0.47
Number of LSLR letters sent	1.32	0.72	1.30	0.72	0.97***	0.69
Account inactive	9%	0.29	10%	0.30	10%	0.30
Street paving moratorium	12%	0.33	11%	0.32	13%	0.34
Suburban location	0%	0.00	0%	0.00	0%	0.00
Assessor data missing	3%	0.17	6%***	0.23	10%***	0.30
Property built before 1951[†]	99%	0.12	92%***	0.27	90%***	0.30
Property built 1951–1960[†]	1%	0.09	5%***	0.21	2%***	0.14
Owner-occupied property[†]	51%	0.50	49%**	0.50	43%***	0.50
Multi-family property[†]	8%	0.27	8%	0.28	12%***	0.33
Assessed value ($100,000)[†]	0.64	0.35	0.64	0.37	0.61***	0.31
Block group characteristics						
Share Black	47%	0.28	49%***	0.29	49%***	0.29
Share Hispanic	40%	0.27	37%***	0.27	38%***	0.26
Share under 5	7%	0.05	7%	0.05	7%***	0.05
Share over 64	9%	0.06	9%***	0.06	9%	0.06
Share college graduate	12%	0.09	13%***	0.10	12%***	0.08
Share below poverty	25%	0.13	27%***	0.13	28%***	0.14

[§]Data presented only for properties with confirmed LSL. [†]Data presented only for properties linked to assessor data.[◊]LSLR letters refers to mailings sent by the utility in 2018 and 2019 to properties suspected of having an LSL to encourage program registration rather than outreach conducted through the community-based grant program or the housing department grant program. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ denotes a statistically significant difference compared to properties with confirmed homeowner-side lead lines using two-tailed *t*-tests

Table 4 Suburban sample summary statistics disaggregated by homeowner-side service line material prior to any replacements

Observations	Confirmed lead		Confirmed non-lead		Material unknown	
	2671		9332		23,385	
	Mean	SD	Mean	SD	Mean	SD
LSLR participation outcomes						
Registered for LSLR program	39%	0.49	20%***	0.40	2%***	0.14
Interior contractor inspection	61%	0.49	57%***	0.49	0%***	0.02
Replaced homeowner-side LSL§	42%	0.49	0%***	0.00	0%***	0.00
Property characteristics						
Lead on utility side of line	57%	0.50	53%***	0.50	26%***	0.44
Number of LSLR letters sent	0.96	0.85	0.95	0.85	0.43***	0.69
Account inactive	6%	0.24	5%*	0.22	5%**	0.22
Street paving moratorium	8%	0.27	6%***	0.23	12%***	0.33
Suburban location	100%	0.00	100%	0.00	100%	0.00
Assessor data missing	6%	0.24	6%	0.24	11%***	0.31
Property built before 1951†	74%	0.44	37%*	0.48	38%*	0.49
Property built 1951–1960†	22%	0.41	35%*	0.48	23%	0.42
Owner-occupied property†	84%	0.37	86%*	0.35	85%	0.36
Multi-family property†	1%	0.10	1%	0.08	1%	0.11
Assessed value ($100,000)†	1.68	0.54	1.91*	0.53	2.04*	0.75
Block group characteristics						
Share Black	24%	0.19	18%***	0.17	20%***	0.20
Share Hispanic	20%	0.16	13%***	0.12	14%***	0.14
Share under 5	7%	0.05	6%***	0.04	6%***	0.04
Share over 64	13%	0.06	16%***	0.06	15%***	0.08
Share college graduate	26%	0.14	33%***	0.14	32%***	0.16
Share below poverty	9%	0.08	8%***	0.07	9%*	0.08

§Data presented only for properties with confirmed LSL

†Data presented only for properties linked to assessor data.

◊LSLR letters refers to mailings sent by the utility in 2018 and 2019 to properties suspected of having an LSL to encourage program registration rather than outreach conducted through the community-based grant program or the housing department grant program. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ denotes a statistically significant difference compared to properties with confirmed homeowner-side lead lines using two-tailed *t*-tests

Table 5 Regression analysis of determinants of LSLR program participation with municipality heterogeneity

	(1) Registration	(2) Inspection	(3) Replacement of confirmed LSL
Number of letters sent	0.026***	0.051***	0.066***
	(0.005)	(0.006)	(0.014)
Account inactive	− 0.024***	0.002	− 0.027
	(0.008)	(0.010)	(0.028)
Lead on utility side	0.180***	0.360***	0.350***
	(0.007)	(0.008)	(0.021)
Street paving moratorium	0.001	− 0.061***	− 0.008
	(0.008)	(0.009)	(0.023)
Owner-occupied property	0.074***	0.038***	0.215***
	(0.006)	(0.007)	(0.017)
Multi-unit property	− 0.070***	− 0.142***	0.001
	(0.010)	(0.011)	(0.032)
Ln assessed value	0.071***	0.051***	0.051**
	(0.008)	(0.008)	(0.021)
Property built before 1951	0.105***	0.143***	
	(0.009)	(0.011)	
Property built 1951–1960	0.100***	0.216***	
	(0.020)	(0.022)	
Assessor data unlinked	− 0.063***	− 0.119***	− 0.058
	(0.010)	(0.012)	(0.052)
Share Black	0.131***	0.045*	− 0.123*
	(0.023)	(0.027)	(0.069)
Share Hispanic	0.026	0.011	− 0.092
	(0.027)	(0.031)	(0.082)
Share under 5	− 0.152**	0.021	− 0.192
	(0.063)	(0.071)	(0.191)
Share over 64	− 0.046	0.239***	0.003
	(0.048)	(0.055)	(0.162)
Share college graduate	0.175***	0.231***	− 0.045
	(0.044)	(0.049)	(0.128)
Share below poverty	0.096***	0.022	0.027
	(0.022)	(0.026)	(0.068)
Suburb	0.071**	0.053	− 0.068
	(0.030)	(0.035)	(0.099)
Number of letters sent × Suburb	0.072***	0.096***	0.047**
	(0.006)	(0.007)	(0.022)
Account inactive × Suburb	0.019*	− 0.019	0.015
	(0.011)	(0.013)	(0.043)
Lead on utility side × Suburb	− 0.142***	− 0.188***	0.005
	(0.009)	(0.010)	(0.036)
Street paving moratorium × Suburb	− 0.016*	− 0.066***	− 0.126***
	(0.009)	(0.010)	(0.038)

Table 5 (continued)

	(1) Registration	(2) Inspection	(3) Replacement of confirmed LSL
Owner-occupied property × Suburb	− 0.051***	0.004	− 0.143***
	(0.007)	(0.009)	(0.028)
Multi-unit property × Suburb	0.072***	0.047**	− 0.101
	(0.021)	(0.023)	(0.092)
Ln assessed value × Suburb	− 0.056***	− 0.019*	− 0.098***
	(0.009)	(0.011)	(0.037)
Property built before 1951 × Suburb	− 0.061***	− 0.078***	
	(0.010)	(0.012)	
Property built 1951–1960 × Suburb	− 0.085***	− 0.142***	
	(0.021)	(0.023)	
Assessor data unlinked × Suburb	0.044***	0.056***	0.068
	(0.011)	(0.013)	(0.062)
Share Black × Suburb	− 0.137***	− 0.142***	− 0.110
	(0.025)	(0.029)	(0.086)
Share Hispanic × Suburb	0.039	0.023	− 0.016
	(0.031)	(0.037)	(0.107)
Share under 5 × Suburb	0.159**	− 0.355***	0.587**
	(0.076)	(0.088)	(0.280)
Share over 64 × Suburb	0.031	− 0.300***	0.240
	(0.053)	(0.061)	(0.212)
Share college graduate × Suburb	− 0.122***	− 0.217***	0.011
	(0.045)	(0.051)	(0.150)
Share below poverty × Suburb	− 0.132***	− 0.053	− 0.039
	(0.032)	(0.037)	(0.144)
Constant	− 0.114***	− 0.046	0.174**
	(0.028)	(0.032)	(0.083)
Observations	55,917	55,917	5769
R^2	0.123	0.266	0.271

Linear probability models pooling the urban and suburban samples estimate the associations between outcomes (1)–(3) and property and neighborhood characteristics. Interaction terms between the property and neighborhood characteristics and an indicator for suburban location allow the coefficient estimates to vary by location. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t-tests

Table 6 Summary statistics for treated, untreated, and synthetic control groups in community-based grant program evaluation

Observations	Target community		Untreated urban properties		Synthetic control‡	
	1010		17,869		17,869	
	Mean	SD	Mean	SD	Mean	SD
LSLR participation outcomes						
Registered for LSLR program	24%	0.43	17%***	0.38	14%***	0.35
Interior contractor inspection	34%	0.47	36%	0.48	26%***	0.44
Homeowner-side LSLR completed	6%	0.24	7%	0.25	4%***	0.20
Property characteristics						
Lead on utility side of line	44%	0.50	46%	0.50	44%	0.50
Number of LSLR letters sent	1.07	0.73	1.12**	0.72	1.07	0.74
Account inactive	13%	0.34	10%***	0.30	13%	0.34
Street paving moratorium	25%	0.43	12%***	0.32	25%	0.43
Suburban location	0%	0.00	0%	0.00	0%	0.00
Assessor data missing	11%	0.32	7%***	0.26	11%	0.32
Property built before 1951†	91%	0.28	92%	0.28	91%	0.28
Property built 1951–1960†	0%	0.00	3%***	0.16	0%	0.02
Owner-occupied property†	41%	0.49	45%**	0.50	41%	0.49
Multi-family property†	4%	0.19	11%***	0.31	4%	0.19
Assessed value ($100,000)†	0.32	0.13	0.64***	0.33	0.32	0.11
Block group characteristics						
Share Black	50%	0.13	48%	0.29	50%	0.31
Share Hispanic	43%	0.14	38%***	0.27	43%	0.27
Share under 5	6%	0.02	7%***	0.05	8%***	0.06
Share over 64	5%	0.04	9%***	0.06	5%	0.03
Share rental	64%	0.10	59%***	0.20	64%	0.16
Share college graduate	6%	0.03	12%***	0.09	6%	0.05
Share below poverty	34%	0.15	27%***	0.13	34%	0.16

Robust standard errors in parentheses. †Data presented only for properties linked to assessor data. ‡Synthetic control summary statistics are calculated using entropy balancing weights. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ denotes a statistically significant difference compared to properties with confirmed homeowner-side lead lines using two-tailed *t*-tests

Table 7 Synthetic control difference-in-difference regression estimates of impact of community-based grant program on LSLR program participation

	(1) Registration	(2) Inspection	(3) Replacement (conditional on confirmed lead)
Target community	0.000	− 0.000	0.000
	(0.011)	(0.019)	(0.053)
After	− 0.019	0.000	0.176***
	(0.013)	(0.025)	(0.047)
Target community × After	0.096**	0.094*	0.169**
	(0.041)	(0.048)	(0.077)
Constant	0.084***	0.124***	0.095***
	(0.006)	(0.015)	(0.030)
Property characteristics	No	No	No
BG fixed effects	No	No	No
Observations	37,758	37,758	5484
R^2	0.015	0.012	0.117

Linear probability models estimate the impact of the community-based grant program on LSLR participation in the urban municipality. Regressions are weighted using entropy balancing weights. The synthetic control approach ensures that pre-treatment participation rates are equal across the treatment and synthetic control groups. These results shown graphically in Fig. 4, Panel A. Standard errors clustered by Census block group are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t-tests

Table 8 Synthetic control difference-in-difference regression estimates of impact of community-based grant program on LSLR program participation including block group fixed effects and property control variables

	(1) Registration	(2) Inspection	(3) Replacement (conditional on confirmed lead)
Target community	–	–	–
After	− 0.019	0.000	0.176***
	(0.013)	(0.025)	(0.047)
Target community × After	0.096**	0.094*	0.169**
	(0.041)	(0.048)	(0.078)
Constant	0.005	− 0.026	− 0.082
	(0.015)	(0.020)	(0.073)
Property characteristics	Yes	Yes	Yes
BG fixed effects	Yes	Yes	Yes
Observations	37,758	37,758	5484
R^2	0.083	0.103	0.266

Linear probability models estimate the impact of the community-based grant program on LSLR participation in the urban municipality. Regressions are weighted using entropy balancing weights. The synthetic control approach ensures that pre-treatment participation rates are equal across the treatment and synthetic control groups. These regressions also include Census block group fixed effects and all individual property characteristics included in Table 1. Standard errors clustered by Census block group are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t-tests

Table 9 Unweighted difference-in-difference regression estimates of impact of community-based grant program on LSLR program participation including block group fixed effects and property control variables

	(1) Registration	(2) Inspection	(3) Replacement (conditional on confirmed lead)
Target community	–	–	–
After	− 0.025**	− 0.077***	0.170***
	(0.011)	(0.025)	(0.048)
Target community	0.101**	0.171***	0.175**
	(0.040)	(0.048)	(0.078)
Constant	− 0.001	0.043**	− 0.053*
	(0.010)	(0.017)	(0.027)
Property characteristics	Yes	Yes	Yes
BG fixed effects	Yes	Yes	Yes
Observations	37,758	37,758	5484
R^2	0.056	0.102	0.149

Linear probability models estimate the impact of the community-based grant program on LSLR participation in the urban municipality. These regressions also include Census block group fixed effects and all individual property characteristics included in Table 1. Standard errors clustered by Census block group are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t tests

Table 10 CEM (coarsened exact matching) weighted difference-in-difference regression estimates of impact of community-based grant program on LSLR program participation including block group fixed effects

	(1) Registration	(2) Inspection	(3) Replacement (conditional on confirmed lead)
Target community	–	–	–
After	− 0.007	− 0.049**	0.203***
	(0.011)	(0.023)	(0.055)
Target community* × After	0.084**	0.144***	0.148*
	(0.040)	(0.046)	(0.086)
Constant	0.084***	0.183***	0.129***
	(0.005)	(0.011)	(0.026)
Property characteristics	No	No	No
BG fixed effects	Yes	Yes	Yes
Observations	32,612	32,612	4060
R^2	0.014	0.022	0.102

Linear probability models estimate the impact of the community-based grant program on LSLR participation in the urban municipality. Regressions are weighted using coarsened exact matching (CEM). CEM weights were derived by matching the treatment and comparison groups on all nine property-level covariates: lead on the utility-side of the line, number of LSLR letters sent, account inactive, street paving moratorium, assessor data missing, property built before 1951, property built 1951–1960, owner-occupied property, and assessed value. An exact match was used for all variables except assessed value, which was coarsened into five categories denoting $1–$25,000, $25,000–$50,000, $50,000–$75,000, $75,000–$100,000, and $100,000–$125,000. Census block group characteristics were not used as match variables; instead, the regressions include block group fixed effects. Standard errors clustered by Census block group are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t tests

Table 11 Summary statistics for postcard intervention treatment and control groups

Observations	Assigned treatment group		Assigned control group	
	3100		3100	
	Mean	SD	Mean	SD
LSLR participation outcomes				
Registered for LSLR program	22%	0.41	22%	0.41
Interior contractor inspection	27%	0.44	27%	0.44
Homeowner-side LSLR completed	4%	0.2	5%	0.21
Property characteristics				
Postcard received	85%	0.35	0%***	0.06
Lead on homeowner side of the line	47%	0.5	51%	0.50
Lead on utility side of line	51%	0.5	53%	0.50
Number of LSLR letters sent	1.09	0.73	1.13	0.73
Account inactive	4%	0.2	5%	0.23
Street paving moratorium	13%	0.33	14%	0.34
Assessor data missing	8%	0.28	9%	0.29
Property built before 1951[†]	94%	0.25	94%	0.23
Property built 1951–1960[†]	4%	0.19	4%	0.19
Owner-occupied property[†]	67%	0.47	65%	0.48
Multi-family property[†]	7%	0.25	8%	0.26
Assessed value ($100,000)[†]	0.85	4.02	0.80	1.52
Block group characteristics				
Share Black	52%	0.3	52%	0.30
Share Hispanic	36%	0.28	36%	0.28
Share under 5	7%	0.05	7%	0.05
Share over 64	9%	0.06	9%	0.06
Share rental	61%	0.19	61%	0.19
Share college graduate	12%	0.09	13%	0.09
Share below poverty	27%	0.13	27%	0.13

[†]Data presented only for properties linked to assessor data. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ denotes a statistically significant difference compared to properties with confirmed homeowner-side lead lines using two-tailed *t* tests

Table 12 Instrumental variables difference-in-difference regression estimates of impact of postcard on LSLR program participation

	(1) Registration	(2) Inspection	(3) Replacement (conditional on confirmed lead)
Received postcard	− 0.013	− 0.012	− 0.006
	(0.013)	(0.013)	(0.024)
After	0.001	0.042***	0.144***
	(0.012)	(0.012)	(0.029)
Received postcard × After	0.017	0.028	0.008
	(0.019)	(0.020)	(0.047)
Constant	0.116***	0.114***	0.065***
	(0.008)	(0.008)	(0.015)
Observations	6200	6200	1056
R^2	0.000	0.006	0.046

Linear probability models estimate the impact of the postcards about the housing-department grant program on LSLR participation in the experimental study sample within the urban municipality. Received postcard and received postcard x After were instrumented by indicator variables denoting that a property was assigned to the postcard treatment group and assignment to the postcard group x after. No additional control variables or fixed effects are included. These results shown graphically in Fig. 4, Panel B. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t tests

Table 13 First stage equations for instrumental variables estimates: effect of assignment to postcard group on receipt of postcard

	(1) Received postcard	(2) Received postcard	(3) Received postcard (conditional on confirmed lead)
Assigned to postcard treatment group After	0.850***	0.850***	0.884***
	(0.009)	(0.009)	(0.020)
	0.000	0.000	− 0.000
	(0.002)	(0.002)	(0.000)
Assigned to postcard treatment group × After Constant	0.000	0.000	0.000
	(0.013)	(0.013)	(0.029)
	0.003**	0.003**	0.000
	(0.001)	(0.001)	(0.000)
Observations R^2	6200	6200	1056
	0.73	0.73	0.80
	Received postcard x After	Received postcard x After	Received postcard x After
Assigned to postcard treatment group After	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
	0.003**	0.003**	0.000
	(0.001)	(0.001)	(0.000)
Assigned to postcard treatment X group After Constant	0.850***	0.850***	0.884***
	(0.009)	(0.009)	(0.020)
	− 0.000	− 0.000	− 0.000
	(0.000)	(0.000)	(0.000)
Observations R^2	6200	6200	1056
	0.73	0.73	0.80

First stages of instrumental variables equations in which received postcard and received postcard x after were instrumented by indicator variables denoting assignment to the postcard treatment group, the period after the postcard was sent, and an interaction between assignment to the postcard group x after the postcard was sent. No additional control variables or fixed effects are included. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t tests

Table 14 Intent-to-treat difference-in-difference regression estimates of impact of postcard on LSLR program participation

	(1) Registration	(2) Inspection	(3) Replacement (conditional on confirmed lead)
Assigned to postcard treatment group	− 0.011	− 0.010	− 0.005
	(0.011)	(0.011)	(0.021)
After	0.001	0.042***	0.144***
	(0.011)	(0.012)	(0.029)
Assigned to postcard treatment group × After	0.015	0.024	0.007
	(0.016)	(0.017)	(0.041)
Constant	0.115***	0.114***	0.065***
	(0.008)	(0.008)	(0.015)
Observations	6200	6200	1056
R^2	0.000	0.007	0.046

Linear probability models estimate the impact of the postcards about the housing-department grant program on LSLR participation in the experimental study sample within the urban municipality. Assignment to the treatment group is used as the treatment variable. No additional control variables or fixed effects are included. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t tests

Table 15 Intent-to-treat difference-in-difference regression estimates of impact of postcard on LSLR program participation including block group fixed effects and property control variables

	(1) Registration	(2) Inspection	(3) Replacement (conditional on confirmed lead)
Assigned to postcard treatment group	− 0.009	− 0.009	− 0.008
	(0.011)	(0.011)	(0.024)
After	0.001	0.042***	0.144***
	(0.011)	(0.012)	(0.027)
Assigned to postcard treatment group × After	0.015	0.024	0.007
	(0.016)	(0.017)	(0.039)
Constant	0.055	0.040	− 0.350***
	(0.043)	(0.044)	(0.125)
Observations	6196	6196	1056
R^2	0.080	0.098	0.194

Linear probability models estimate the impact of the postcards about the housing-department grant program on LSLR participation in the experimental study sample within the urban municipality. Assignment to the treatment group is used as the treatment variable. These regressions also include Census block group fixed effects and all individual property characteristics included in Table 1. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t tests

Table 16 As-treated difference-in-difference regression estimates of impact of postcard on LSLR program participation

	(1) Registration	(2) Inspection	(3) Replacement (conditional on confirmed lead)
Received postcard	− 0.003	− 0.001	− 0.007
	(0.011)	(0.011)	(0.021)
After	0.003	0.046***	0.150***
	(0.011)	(0.011)	(0.027)
Received postcard × After	0.011	0.019	− 0.006
	(0.016)	(0.018)	(0.042)
Constant	0.111***	0.109***	0.065***
	(0.007)	(0.007)	(0.014)
Observations	6200	6200	1056
R^2	0.000	0.007	0.047

Linear probability models estimate the impact of the postcards about the housing-department grant program on LSLR participation in the experimental study sample within the urban municipality. Receipt of the postcard is used as the treatment variable. These regressions do not include additional control variables or fixed effects. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t tests

Table 17 Intent-to-treat difference-in-difference regression estimates of impact of postcard interacted with percent Hispanic on LSLR program participation

	(1) Registration	(2) Inspection	(3) Replacement (conditional on confirmed lead)
Assigned to postcard treatment group	− 0.030	− 0.022	− 0.036
	(0.020)	(0.019)	(0.032)
After	− 0.012	0.063***	0.110**
	(0.021)	(0.021)	(0.045)
Assigned to postcard treatment group × After	0.022	0.037	0.079
	(0.029)	(0.030)	(0.065)
Share Hispanic	− 0.158***	− 0.059**	− 0.069
	(0.027)	(0.030)	(0.043)
Share Hispanic × After	0.037	− 0.060	0.105
	(0.040)	(0.044)	(0.099)
Share Hispanic × Assigned to postcard treatment group	0.054	0.032	0.090
	(0.038)	(0.040)	(0.064)
Share Hispanic × After × Assigned to postcard treatment group	− 0.019	− 0.036	− 0.209
	(0.056)	(0.062)	(0.140)
Constant	0.171***	0.135***	0.088***
	(0.015)	(0.014)	(0.024)
Observations	6200	6200	1056
R^2	0.011	0.012	0.049

Linear probability models estimate the impact of the postcards about the housing-department grant program on LSLR participation in the experimental study sample within the urban municipality. Assignment to the treatment group is used as the treatment variable. We include the share of the population of Hispanic ethnicity in the Census block group as a control variable and interaction terms between share Hispanic and the other independent variables to test whether response to the postcard varied by block group ethnic composition. No additional control variables or fixed effects are included. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$ using two-tailed t tests

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Author Contributions HK is the primary and coordinating author. KE and SK provided insight into the LSLR program, data, and local knowledge of the study region. All authors contributed equally to the research design and implementation of the RCT. HK, AW, and BP analyzed the data, estimated the regression models, and developed the analysis. HK, AW, BP, and SA wrote the paper.

Data Availability The account-level LSL program data used in this study were acquired under a non-disclosure data use agreement with Trenton Water Works. Deidentified data aggregated at the Census Block Group level and replication code for this paper are freely available at: https://github.com/bryanparthum/lslr-paper.git.

Declarations

Competing interests H.K., A.W., and B.P. declare they have no financial interests. S.K. and K.E. are employed at CDM Smith, which was a paid contractor of Trenton Water Works, and K.E. was employed at Trenton Water Works when the study was initiated. This study was not part of Trenton Water Works' contract with CDM Smith, nor did the results of the study affect the contract. S.A. worked at the University of Chicago Energy and Environment Lab, which received funding from the U.S. Environmental Protection Agency, when the study was initiated, and currently works at Accenture Federal Services, which also receives funding from the U.S. Environmental Protection Agency; that funding did not support this study, nor was it affected by this study's results.

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