

Lead Pipes, Prescriptive Policy and Property Values

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Abstract

Several recent incidences of severe waterborne lead exposure have public authorities and communities across the US rethinking their strategies to address aging water infrastructure. One common question: who should pay for updates? This paper provides evidence of positive property value capitalization effects following remediation of private lead service lines in Madison, WI. Using a 16-year panel of property transactions data and a universal and prescriptive policy change, I identify an average post-replacement price effect on the order of 3-4% of a property's value. This implies a more than 75% average return on public and private remediation costs, suggesting homeowners strongly value the benefits of lead reduction in publicly-supplied drinking water.

1 Introduction

Researchers have long linked lead pollution to a variety of adverse health effects. Findings from across academic fields has associated lead exposure with negative outcomes including increased mortality and morbidity, cognitive disability, and aggressive or violent behavior. The best known transmission pathways in post-industrial economies include airborne lead from transportation and industrial sources, lead-based paint in older housing units, and contaminated soil due to previous pollution. But water-borne transmission is also a major concern in the United States. Lead exposure from drinking water pipes was recently a national topic due to the crisis in Flint, MI, where a change in water source and treatment led to a severe increase in childhood lead poisoning cases.

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These concerns about water-borne lead are warranted. Research has dated public unease about lead pipes to as far back as Roman times, with some modern academics even speculating that widespread lead poisoning was a contributing factor to the fall of the empire (Nriagu, 1983). More recently, studies have noted that water was the dominant source of lead exposure in the United States during the first half of the twentieth century (Troesken, 2006), and in some areas the use of lead pipes in new construction continued into the 1980s (Renner, 2010). A legacy of this is that millions of US households still receive their water through full or partial lead pipe systems. By the most recent national estimates, as many as 22 million people consume water from these systems (Cornwell et al, 2016).

Given the widespread media coverage of the crisis in Flint and the undisputed scientific evidence on the hazards of lead pipes and an aging infrastructure, some policymakers in older US cities have recently broached the topic of complete lead pipe remediation in their water systems. There is little precedent for this in modern history: to date, only two major US cities have undertaken such a procedure. In 2000, through a costly and at-the-time controversial policy, Madison, WI enacted the nation’s first program to fully retrofit every known lead service line in its drinking water system. Today, this program is often hailed as a potential role model to other cities considering such an infrastructure update.

The nature of Madison’s program has proven of interest to public health advocates and policy-makers for a number of reasons. First, the program’s mandate covered both public and privately-owned pipes, meaning *all affected homeowners* were obligated by law to pay for the removal of pipes on their property. Second, in order to offset pipe replacement costs, the program offered partial subsidies to affected homeowners. While these subsidies were funded through a public revenue source¹ to incentivize participation in the program, economic theory suggests that subsequent community health benefits, educational and public safety outcomes, and property values could potentially aid in recompensing the program’s substantial costs. Third, the program occurred over the course of more than a decade, providing compelling, quasi-random variation in program rollout. Most importantly, the city’s utility maintained detailed data on the locations, replacement costs, and service dates of all private lead lines rehabilitated during the program.

¹Due to state law prohibiting rate-payer funds being used for private infrastructure upgrades, the utility raised money for this program by renting space on their water towers to cellular providers.

This paper’s primary aim is to quantify property value capitalization effects that resulted from a lead service line (LSL) replacement during Madison’s program. As city-specific laws in Madison allocate responsibility for water infrastructure to the utility only up to the private property line, affected homeowners were impacted materially at the time of the policy’s implementation. From 2000-2015, over 5700 homeowners in downtown Madison paid for the replacement of a privately-owned LSL on their property. I obtained data on the timing and private cost of each of these service line replacements from the Madison Water Utility. After merging this data with a 16-year panel of property sales transactions occurring during the program, I take an empirical approach grounded in hedonic and quasi-experimental methodological traditions. A robust analysis using cross-sectional difference-in-differences, repeated-sales, and matching techniques identifies the effect of a LSL replacement on a property’s value.

These empirical results imply that a sizable price capitalization effect exists. Across specifications and estimation strategies, estimates suggest the average size of this effect is on the order of 3-4.3% of a property’s value. Using the estimate from my preferred specification, I find that the median property treated during the program would have had a more than 300% return on private investment had they paid the average private cost of remediation, \$1,340. Taking into account the public capital costs of the program, the net return implied is still higher than 75%.

This research adds to a still-growing literature on the varied economic effects of lead exposure in daily life. Recent papers have underlined the costly health (Troesken (2008)), human development (Ferrie et al (2012)), and educational outcome (Rau et al (2015) and Aizer et al (2017)) effects of lead exposure. Others have provided correlational evidence on lead-reduction programs’ accrued benefits in the form of reduced anti-social behavior in adolescents and overall crime reduction (Reyes (2007) and Nevin (2007)). This work contributes perspective on another potential benefit of lead replacement: property value growth. To the best of my knowledge, this is the first study to examine how property markets capitalize the mandated replacement of lead plumbing.

In the property value literature, this paper is perhaps most directly related to papers by Gazze (2019), Bae (2012) and Billings and Schnepel (2017), all of which study the broad real estate effects of legislation targeting leaded paint in homes. The latter paper constructs a quasi-experimental framework around a lead-remediation program in Charlotte, North Carolina and finds a sizable premium for properties that undergo full remediation. The former papers make use of national

data and state-varying mandatory lead paint abatement policy changes to document evidence of heterogeneous policy incidence and potential sorting behaviors.

This work also adds evidence on the role of infrastructure quality to the literature examining consumers' valuation of drinking water and infrastructure quality. A recent working paper by Christensen et al (2019) finds evidence of housing price depreciation and averting behaviors following information shocks during the lead-in-water crisis in Flint, MI. Tanellari et al (2015) uses stated preference methods to analyze the determinants of positive consumer willingness-to-pay (WTP) for improved water quality and infrastructure updates in a region that previously faced its own lead-in-water crisis. My findings tie in nicely with results from these papers, and at least in the context of Madison, confirm above-cost consumer valuation of reduced lead piping in drinking water infrastructure.

The paper is structured as follows: Section 2 provides background information on modern waterborne lead regulation in the United States and spells out the specifics of policies instituted by Madison beginning in the year 2000. Section 3 describes the data used in the analysis and outlines the empirical framework and research design. Section 4 provides my primary result and confirms its robustness. I also discuss some possible mechanisms that could be behind this main finding. Finally, in Section 5, I conclude and propose potential directions for future research.

2 Background

US policies on lead in drinking water

Before discussing the specifics of Madison's program, a brief discussion of historic federal US policy on waterborne lead is warranted. In recent decades, local water providers and authorities have had jurisdiction and liability for attaining compliance with federal legislation. Since any large-scale replacement of water infrastructure tends to be expensive, it is perhaps unsurprising that no city before Madison undertook such a complete lead pipe replacement program.

Waterborne lead in US drinking water is rarely naturally-occurring, and most often finds its way into the public water supply via lead piping. The vast majority of remaining US lead water infrastructure was constructed up through the mid-20th century, when lead was favored for its malleability and durability. Until the mid-1980s, the use of new lead pipes in US public water

systems was legal, though decreasingly common over time. In 1986, the US government amended Section 1417 of the Safe Water Drinking Act (SWDA) to prohibit the use of lead pipes in the installation or repair of water systems used for human consumption. Five years later, the federal government issued the Lead and Copper Rule (LCR) as a subsection of the SWDA. The LCR regulates public water systems on the concentration of permissible lead in the water supply. Lead concentrations are measured at a random sample of customer taps for each public water system. The LCR stipulates that “lead concentration exceeding an action level of 15ppb (parts per billion)... in more than 10% of customer taps sampled must undertake a number of additional actions to control corrosion”.

Public water suppliers have substantial latitude in choosing a local strategy to initially satisfy this regulation and maintain compliance. Documented strategies range from preemptive use of corrosion control chemicals, to targeted replacement of problematic lead lines, to quasi-gaming of testing procedures (Edwards et al, 2009). In the case that compliance is not attained, however, the utilization of any number of water treatment strategies to manage pipe corrosion is recommended by the EPA as the water supplier’s initial response. As suggested above, such a strategy is typically more cost-effective than a large-scale pipe replacement effort and is employed in major cities across the US. Importantly, in the case of non-compliance, local authorities are also required to publicize sample results and perform outreach explaining waterborne lead risk to their community.

In the time since the water crisis in Flint, a substantial amount of media attention has been directed towards these federal policies and their effectiveness. A 2016 report found that over 18 million people, in over 45 states, were served by public water systems that reported violations of the LCR in 2015.² This media attention has created a sense that public awareness of residential waterborne lead risk is a recent phenomenon across the United States. In fact, several states have revised their property sale disclosure policies over the past decade, adding clauses that address issues of plumbing safety. As of 2017, before transacting on a property, nine states require mandatory disclosure of water piping material, while several others have voluntary disclosure forms that ask similarly targeted questions (EDF, 2017).³ Moving forward, more uniform and widespread

²A large proportion of these violations were for monitoring/reporting violations rather an exceedance of the LCR standard. See Olson and Fedinick (2016).

³At the time of writing, Wisconsin mandates the disclosure of lead water piping during a real estate transaction when the owner is aware of it. This law (Act 338) was only passed in the 2017 legislation session, however, so disclosure was not mandatory during the period studied in this paper.

disclosure legislation - similar to laws addressing lead paint in homes - seems likely.

Lead service replacement in Madison, WI

Madison, a small city with just under 250,000 inhabitants, has a nearly one-hundred-year track record of proactivity concerning lead pipes. Its inherently hard and non-acidic water does not naturally lend itself to rapid uniform pipe corrosion, but the city's utility ceased the installation of lead main and service lines in 1927 and by the 1970s was replacing lead lines during street construction projects (MWU, 2010). Despite these efforts, in 1992, only months after the LCR was passed, Madison was found to be in violation, testing at 16ppb in the 90th percentile of its sample. Some homes in the sample had lead levels of over 100ppb. The legislative fallout of this violation played out for nearly a decade, but by February 2000, after extensive scientific research and negotiation with the EPA, a team of engineers, chemists, and government officials concluded that the removal of aging lead water service lines was the optimal strategy to meet and surpass the requirements of the LCR.⁴ This decision was the catalyst of Madison's city-wide lead pipe removal program.

The program was fairly simple in its design.⁵ Any property with known lead in its service line was mandated to fully replace the lead piping. Property rights in Madison dictate that water supply pipes in the public domain - typically water mains running down streets and the section of the service line lying under the sidewalk- are the responsibility of the utility, while pipes lying within a private parcel - the remainder of the service line and interior plumbing - are the responsibility of the landowner. Thus, the program required buy-in from not only the city government, but also private citizens.

Refusal to comply with a mandated private-side removal upon request by the utility opened up the homeowner to daily fines of between \$50 to \$1000 and/or legal action from the city attorney's office.⁶ Furthermore, to incentivize additional reporting of lead service lines that were not present

⁴Interested readers can refer to MWU (2010) for a detailed description of the decade-long process that led to this conclusion. The short version: the sources and chemistry of Madison's groundwater made several potential adjustments not chemically or economically viable. Uniform corrosion of lead pipes was not the main cause of the city's LCR violation. As such, the use of phosphates - a group of chemicals which has elsewhere been shown to reduce lead pipe corrosion - was precluded due to ineffectiveness and widespread concern about damaging discharge into Madison's much-beloved lakes.

⁵See Madison city ordinances: Sec. 13.18 Cr. by Ord. 12,544.

⁶To further reiterate the point- households were only fined following a request from the utility to remove the private side service line. As I mention below, economies of scale were gained by replacing both public and private

in the utility’s database, the city government paired its fine-based “stick” with a “carrot” in the form of a partial subsidy. Upon presentation of documentation certifying that a LSL was removed at some cost, affected homeowners were awarded repayment for half the cost, up to an award threshold of \$1000. Of the treated households, more than 90% were paid less than this threshold—see Figures 1 and 2. The mean subsidy paid out by the utility was \$670, implying an average private-side replacement cost of more than \$1,340—though the censoring created by the subsidy cap implies uncertainty on the upper tails of the replacement cost distribution⁷.

At the program’s outset, the water utility possessed some records of likely lead pipe locations due to previous excavation or city works projects. However, the utility also strived to use the program as a mechanism to check for unknown lead pipes. The city pushed an extensive information campaign before and during the program, reaching out to homeowners and utility customers to provide specifics on the program and subsidy and to explain how to manually identify lead service lines and plumbing in their home’s water system. The utility also surveyed thousands of its customers to acquire information on material of their private side service line. This information campaign, in combination with the generous incentive structure in place, resulted in the remediation of around 8,000 total service pipes, of which around 5,700 were privately-owned, residential lines.⁸

Removal of lead services under the auspices of the program began in 2000. The vast majority of removals occurred before 2010, with a handful of properties servicing vulnerable populations receiving the earliest treatment. Lead pipes treated were service lines and their connectors at the property line; the renovation of interior lead plumbing was not covered by the program, nor is it observable in the data. When applicable, the utility and private-side contractors made conscious efforts to avoid partial service line replacements and aid in minimizing costs by replacing pipes in neighboring properties during scheduled water infrastructure construction and maintenance projects. This largely explains the ten-year rollout of the program, but does generate some local

pipes of neighboring properties—therefore, the utility often withheld requests for private service line removal until it was practical to concurrently replace the public side. Note that this also explains why there was not a rush to replace all private service lines at the program’s commencement.

⁷Household-level data on the utility’s costs for *public-side* service lines that were concurrently replaced during the program is not available. However, based on the capital budget allocated to utility-side replacements over the course of the program, and the number of service lines replaced, the utility estimates its average cost of a public-side LSL replacement was \$1997.

⁸To better illustrate the program’s scope, according to 2015 ACS data, there are 66,722 single or 2-4 unit housing structures in the entire city of Madison. From the same data source, there are 15,085 housing units in Madison that were built before 1940.

spatial correlation in the dates of pipe replacement during the earlier years of the program- see Figure 3 for visual evidence of this.

One natural question raised by the program’s rollout - especially when trying to draw conclusions about average price capitalization effects - is whether certain properties had a higher propensity for early treatment. Starting with the program’s outset in 2000, I perform a survival analysis, testing how observable property characteristics affect time to treatment. Using initial appraised value as a measure of property value in column (1) and a neighborhood sales price index in column (2), OLS results in Table 1 mostly suggest that time to treatment is uncorrelated with property characteristics. The only exceptions to this are for properties close to heavy automobile traffic and those with a basement garage; across the models, the parameter estimate on the former covariate is positive, large, and statistically significant, implying that properties next to traffic were treated, on average, later in the program, while the parameter estimates on the latter imply that properties with basement garages were, on average, remediated earlier. Aside from these covariates - perhaps related to long-term-scheduled water infrastructure updates - it seems that a property’s date of treatment is as good as random.

On that note, it’s worth mentioning that the service line replacement procedure varied by property; different techniques for replacement were used, and several property specific factors, namely cost-effectiveness, were used in the decision process. At a minimum, a private service line replacement required the excavation of access points at the curb stop and possibly the building’s plumbing entry. In cases where several neighboring properties were treated in procession, larger trenches were often used for access to public-side service lines and mains, disrupting sidewalk and street traffic until the completion of the project. It is difficult to know exactly what kind of approach was used for a given property, but the subsidy amount issued lends some idea of the procedure’s physical footprint on the private side of the replacement.

There have been broad reductions in waterborne lead content from public taps in Madison since the program’s debut. Figure 4 shows the distributional shift of lead concentration in Madison’s water from 1992 to 2014. Samples from the original 1992 LCR testing, as well as internal sampling by MWU in 1995 and 1997, possessed very long tails to the right: several properties possessed lead concentrations 2-10 times above the 15ppb EPA standard. Moreover, even as late as 2002, MWU internal tests recorded several sample concentrations well above 15ppb. In contrast, the

distribution drawn from the post-treatment, 2011 and 2014 LCR test samples show a noticeable shift to the left; the vast majority of the sample is not only below the 15ppb EPA benchmark, but is also below 5ppb. This substantial citywide variation over time illustrates that property owners who replaced service lines in Madison received a real reduction in waterborne lead exposure, rather than simply the perception of reduced risk.

3 Data and empirical framework

Description of data used in analysis

My empirical analysis makes use of property information acquired from several sources to build a set of transaction-based data for analysis. The primary source for property value data is Madison’s city assessor’s office. One of their publicly-available databases provides broad coverage of deeded property transactions in the city from the 1990s to the present day. Transactions on a given property are infrequent by nature, but there is a sizable subsample of properties for which I observe repeated sales.

These data are paired with residence and land characteristic information from the same office, and annual (2000-2015) parcel-level tax assessment data for Dane County, acquired from the University of Wisconsin-Madison geospatial data repository. Finally, I make use of linked geographic coordinates obtained from a parcel-level tax assessment database for the state of Wisconsin, constructed by CoreLogic. Together, this information provides a fairly complete picture of the property market in Madison during the lead pipe removal program. For each parcel in the city of Madison, there is information on date(s) of sale(s), transaction price(s), parcel-specific assessed land and improvement values, detailed housing structure characteristics as of 2015, parcel-specific land and infrastructure characteristics, neighborhood characteristics, owner information and geographic coordinates.

Restricted-access data on the lead pipe removal program were obtained from the Madison Water Utility. One key advantage of studying Madison’s replacement program - aside from the fact that it was the first to be fully completed - is the city’s size. It is large enough to ensure a robust and dynamic property market, but small enough for the utility to have a relatively high

degree of certitude in the accuracy of its infrastructure replacement records.⁹ Many public water providers in the US have historically maintained poor information on the location or removal date of lead services. MWU’s data contain both a unique parcel ID and the street address of properties with private lead service lines prior to the program; this allows for parcel-level matching with transaction or tax assessment observations in the property databases. The utility’s data also include information on the date of pipe removal and the amount paid to the property owner through the subsidy program. In all, the utility’s data on treated properties cover a little more than 5700 properties- see their spatial coverage in Figure 3.

With the specifics of Madison’s program in mind, it is reasonable to reduce the citywide transaction dataset from over 200,000 residential sales observations with a number of qualifications. First, the sample is restricted to only transactions between 2000 and 2015- the observed duration of the program. While the property data are usable back into the 1990s, the pipe replacement data begins only in the 2000s. To ensure that each property is accurately assigned the correct service line status, it is necessary to only consider the program period. For the baseline case, I assume the entire city of Madison is the extent of the market, but as I refine my sample to improve co-variate balance, results will largely focus on neighborhoods close to downtown.

To be conservative in analysis and ensure the study of only arm’s length sales, any transaction with CPI adjusted (2000\$) prices lower than 10,000 or larger than 5,000,000 are also dropped from the sample. Finally, since information on home characteristics are only available for the final year of the sample, I am unable to directly observe sizable property improvements over time through the use of characteristic variables. In order to account for properties that are flipped, or those that have substantially changed over time, properties that are transacted multiple times within a given year are dropped from the sample, as are properties with repeated sales that appreciate at a greater than 20% annual rate or a total rate of greater than 100%.

Table 2 provides summary statistics for the characteristic variables considered in the study, based on the transaction data. Columns labeled “Control” contain the statistics for homes that never have a lead pipe during the study period, while columns labeled “Treatment” contain the statistics for those properties treated by the policy. As can be seen in the first two columns, when

⁹To the best of the MWU’s knowledge, all lead services lines in the city of Madison have been removed. Previously unknown lead services are still occasionally found, but this is rare. For instance, the data show that only 15 lead services were replaced over the two year period, 2015-2016.

using the whole sample, there are substantial differences in the land, age, and style of homes that never had pipes versus homes that did. This makes intuitive sense- any newer homes assuredly do not have lead piping, and likely are characteristically different than the older properties that were treated during the program. These newer homes are also typically located further from downtown, and have larger lot sizes and a higher probability of central air conditioning.

Empirical Framework

Economic theory suggests that a lead pipe replacement - particularly a service line that is the responsibility of the property owner - should capitalize into a home's sale price. While infrastructure investments do not necessarily catch the eye, the mean pre-subsidy cost of a private pipe replacement was around 0.65% of a home's assessed value (see Figure 2), with a substantial portion of homeowners facing costs greater than 1% of their property's assessed value. These well-aged pipes were reaching the end of their natural lifetime during the replacement program, and as such would likely have required maintenance in the near future.

In practice, however, homeowners and buyers are heterogeneous in (a) their tolerance for waterborne lead risk and (b) income. Extremely cautious households or those with family members who may be more susceptible to lead's well-documented health impacts may be more wary of properties with lead service lines. More generally, the expected size of any capitalization effect hinges on transacting parties' awareness of the presence (or non-presence) of lead in the water infrastructure, the perception and tolerance of lead risk to a particular buyer/seller, and the potential trade-offs in replacing pipes now or in the uncertain future.

There are several ways in which the structure and rollout of Madison's prescriptive mandate was advantageous in ensuring homeowner salience about their property's exposure to lead service lines and the risks of waterborne lead. First, the city's approval of the program itself was contentious, generating headlines and much public debate in local and national media. Second, the utility proactively made use of outreach initiatives to disseminate the specifics of the city's program, notify homeowners if their property possessed a known lead service line, and to educate homeowners on visually identifying lead piping. Third, homeowners did not initially know how long the subsidy aspect of the program would be honored, and thus were incentivized to check, report, and replace lead services lines early. Finally, Madison's concurrent decision to not actively treat its drinking

water for lead corrosion meant that homeowners faced self-imposed consequences if they decided not to report or replace their service lines.

With these aspects of Madison’s program in mind, one can make a strong but contextually reasonable assumption that housing market participants in Madison circa 2000-2015 were particularly well-informed about the presence of lead service lines on properties of interest. In such a case, it seems natural to build an empirical framework around the decades-long price hedonic literature in its perception of housing as a differentiated good (Rosen, 1974). Buyers and sellers weigh the characteristics of each house in the market, with some function of these attributes and their respective implicit prices yielding the home’s value in competitive equilibrium; this provides the market’s equilibrium price schedule. The attribute of interest in this analysis is the presence of a lead service line on a given property.

My main analysis is constructed using the hedonic framework on cross-sectional variation in property sales prices; my setup treats Madison’s LSL replacement policy rollout as a natural experiment. Given the previously described data and nature of the program, a difference-in-differences approach can capture any capitalization of a service line treatment. Hedonic regressions include a rich set of property-specific characteristic covariates, spatial fixed effects, and temporal fixed effects in order to carefully capture any pre-treatment level differences in prices for homes with LSLs, as well as the post-treatment effect:

$$\ln P_{ijt} = \alpha TREAT_i + \gamma POST_t * TREAT_i + \beta \mathbf{X}_{it} + \tau_t + \delta_{jt} + \varepsilon_{ijt} \quad (1)$$

In this model, P_{ijt} is the sale price of home i in neighborhood j in year t . These prices are CPI-normalized to 2000 US\$, and following Billings and Schnepel (2017) and others in the property value literature, I impose a parsimonious log-linear specification¹⁰. The vector X_{it} contains property-specific characteristics including the age of home, lot size, interior square footage, finished basement area, total bedrooms, baths, fireplace openings, as well as indicators for central air, noticeable road traffic, heavy road traffic, airport noise, railroad noise, waterfront, wooded, outdoor porch, enclosed

¹⁰Appendix Table A1 contains estimation results for a handful of alternative specification assumptions. I estimate a Box-Cox model, whose left- and right-hand-side transformation parameter estimates suggest a log-linear specification is not inappropriate. I also estimate models with time-varying β_t parameters to allow for shifting hedonic equilibria and an alternative temporal fixed effects specification. Results from these alternatives are consistent with those from my preferred specification

porch, deck/balcony, patio, garage type, and architectural style. δ_{jt} is a neighborhood by year fixed effect, taken at the census block group level in my baseline, preferred analysis. τ_t is a vector of month fixed effects to capture the annual seasonality of housing sale prices (Ngai and Tenreyro, 2014). ε_{ijt} is the idiosyncratic error.

Lastly, $TREAT_{it}$ is a property-specific dummy marking the presence of lead service pipe at the start of the 2000-2016 study period. $POST_t$ is a dummy variable that signifies whether a property’s LSL has been replaced. More precisely, $POST_t = 1$ if a house is sold after its lead pipes have been removed under the program and $POST_t = 0$ if the home is sold before it has been treated. Thus, should the replacement of lead service piping appreciate home values conditional on other home-, time-, and neighborhood-specific characteristics, regressions of this class should yield a positive estimate for γ , my primary parameter of interest. If homes with LSLs suffer any stigma before replacement, α should be estimated as negative.

Of course, in order for a difference-in-differences identification strategy to accurately estimate γ , common pre-trends between treatment and control properties must hold. I address this necessary condition in two ways.

First, I construct a propensity-score matched subsample of the sales data to ensure control and treated properties are well balanced on observable characteristics in the panel. A balancing exercise of this type strengthens the argument that trends for treated properties would have been similar to the comparison group in the absence of Madison’s program. Research in both statistics and the social sciences has indicated that the pre-modeling use of matching techniques can reduce bias in regression analysis by improving the overlap of covariates (Ho et al, 2007).

I match each treatment property transaction during the program to a single property transaction that was not treated, restricting the potential control pool to only houses that are located in a census block group where a property had a pipe remediated. The matched pair is found by minimizing the distance between the treatment propensity scores¹¹, allowing replacement and estimating the score by logistic regression.

Columns (3) and (4) of Table 2 show the results of this balancing exercise. Indeed, this matching procedure engenders a set of control properties that looks much more similar to the set of treatment

¹¹To be precise, the propensity score equation is estimated as a linear function of lot size, square footage, bedrooms, baths, age of home, latitude, longitude, and indicators for porch, patio, deck/balcony, waterfront, wooded, airport noise, traffic noise, garage type, and importantly, year of property sale.

properties. While there is always a risk that misspecifying the propensity score function could bias pursuant causal effects, given the strong covariate balance of control and treatment groups illustrated in Table 2, I appeal to the practice described in Ho et al (2007): “If this procedure balances X , we use it.”

Secondly, Figure 5 provides visual evidence of these common pre-trends. Using the propensity-score matched subsample of sales, the figure plots residual log-prices for properties that were never treated during the program (control) and pre-treatment properties that were still in possession of lead service lines at the time of sale. After controlling for neighborhood-specific variation, residual sale prices for both groups follow a similar sales-weighted linear trend over the first decade of the program. A simple t-test of differential trends, discussed in the footnote of Figure 5, confirms the slope difference in linear trend between pre-treatment and control groups is highly insignificant.

With what appear to be common trends, my difference-in-difference estimates depend largely on neighborhood and time fixed effects to account for unobserved heterogeneity across properties, and therefore rely on variation within a neighborhood and year to identify any average price effect following treatment. This approach is advantageous in the context of Madison’s policy rollout. Since service lines were typically replaced neighborhood-by-neighborhood, any spillover price effects that may have accrued to neighboring properties’ values following a series of service line replacements should be mitigated by the tract-year fixed effect and diff-in-diff methodology. In addition, I also utilize two alternative estimation strategies to complement my baseline results, which I discuss here for completeness.

The first is a repeat-sales model; this model follows from a simple algebraic manipulation of equation (1):

$$\Delta \ln P_{ijtt'} \equiv \ln P_{ijt} - \ln P_{ijt'} = \gamma \Delta (POST * TREAT_i) + \tau_t + \tau_{t'} + \delta_j * t + \varepsilon_{ijtt'} \quad (2)$$

Here, both time-invariant observed and unobserved property-specific heterogeneity are differenced out, and the estimate of γ captures the average effect of lead pipe remediation on those properties that were treated. Thus, identifying price variation comes solely from sales on both side of a property’s LSL replacement. Time fixed-effects are richer- following Taylor et al (2016), the repeat-sales model also include information on the year of the home’s previous sale. Both algebraically

and intuitively, these richer time fixed effects ensure the comparison of properties that were on the market at similar times. Again, month-of-current-sale fixed effects are included to control for market conditions in different times of the year, and neighborhood time trends are included to allow for differential price trends across space. I estimate this model using only transactions on properties that fall in the spatial treatment area where the LSL replacement program occurred. Characteristic summary statistics for this subsample of sales are similar to the PS-matched sample used in my preferred, baseline results - see columns (5) and (6) of Table 1.

Secondly, as an alternative and nonparametric approach, I model the average treatment effect on treated properties (ATT) using a nearest-neighbor matching (NNM) estimator. The use of nonparametric matching techniques are advantageous in that they further relax parametric identification assumptions inherent to more traditional hedonic approaches; as such, they have grown increasingly common in the property valuation literature.¹²

In this application, the dependent variable is again the logged, CPI-adjusted sale price, with a discrete treatment variable being the presence of LSLs in a home at the time of sale. This implies that the “treatment” group contains transactions on properties that were affected by the policy, *before* the property’s service lines were replaced. Conversely, the “control” group consists of remaining transactions on homes that are free of lead service lines. Following the program evaluation literature, at least one matched property from the “control” group is imputed as the missing potential outcome for a treatment property. The estimated capitalization effect, then, is the average of differences in logged-sale price between the treatment observation and the imputed control observation. Should the resultant estimate be similar in magnitude to those from the parametric approaches, it would lend another layer of credibility to my findings.

The NNM algorithm matches a treatment property with its nearest neighbor, where the Mahalanobis distance is defined by the following covariates: total land cover, interior square footage, total number of bedrooms, total number of bathrooms, age of home, porch/patio/deck presence, dummies for wooded, waterfront, and noisy properties, longitude and latitude of a parcel, and *exact matching* on year of the sale transaction. The algorithm runs with replacement and I make use of the Abadie-Imbens (2011) bias-correction for continuous variables. This auxiliary regression includes the aforementioned property characteristics, as well as year of sale and census block group

¹²See Muehlenbachs et al (2015) or Abbott and Klaiber (2013) for recent applications related to housing markets.

fixed effects to help preclude potential unobserved confounders in the matching process.

4 Results and discussion

Results from my baseline cross-sectional diff-in-diff regressions of (1) are shown in Table 3. Parameter estimates for control variables and fixed effects are omitted, but universally possess the correct sign and are of sensible magnitudes.¹³ Column (1) displays coefficient estimates based on all transactions in the treatment area over the 16-year program period. This broad model, using all the market information in the dataset, finds a post-remediation effect averaging 3.8% of the property’s value.¹⁴ The treatment group dummy ($Treat_i$) has a small negative coefficient estimate, a result which holds across subsamples in Table 3.

Columns (2) and (3) aim to pare down the control sample in order to better compare like properties - in addition to better ensuring the fulfillment of the common trend assumption, one can imagine that some homebuyers are looking for certain qualities that exist in only older homes, and thus a slightly different hedonic market exists for such properties. In column (2), the sample is refined to only homes built before 1940 that fall inside census block groups where a LSL was replaced. Indeed, the vast majority of homes that had lead piping at the program’s outset were constructed before 1940- as were a substantial portion of the housing stock in the sections of Madison where this program occurred. As such, lead-pipe-free properties are more characteristically balanced with treatment properties in this limited subsample, and I find a capitalization effect following pipe remediation is of a slightly larger magnitude - around 3.9% of property’s value. Finally in column (3), the sample analyzed is restricted only to PS-matched properties. In this, my preferred specification, the capitalization effect is markedly similar in magnitude to that of column (2)- around 3.6% of a property’s value.

This is a sizable effect. For the median assessed price of a home with a lead service line in 2000, a 3.6% effect is equivalent to \$5,914. Using the average private remediation cost of \$1,340, homeowners benefits from a private return of over 300%. Taking into account the additional average

¹³Available on request, omitted for brevity. Across specifications: more space, rooms, and better housing/land characteristics increase property value. Noise decreases property value. Sale prices in summer months are stronger, as expected; a tumble in sales prices in the post housing crisis period are also evident.

¹⁴All reported estimates of price capitalization effects as a percentage of home value in the text make use of the Halvorson and Palmquist (1980) coefficient correction: $e^\gamma - 1$. As effect estimates are small (<5%), this correction is minute.

public remediation cost of \$1,997, this price effect estimate still implies an overall return on total investment of more than 75%. To confirm this result is not being driven by modeling decisions or spurious correlations in the data, I test the robustness and validity of my baseline identification strategy in several ways.

The first set of robustness tests for this identification strategy aim to determine whether spurious correlations might be driving my result. To ensure this is not the case, I run a series of randomization and placebo tests, the results of which can be seen in Figure 7 and Table 4. I begin by bootstrapping parameter estimates resulting from a complete randomization of treatment status across all sales included in my PS-matched subsample. The details of this procedure can be found in the caption of Figure 7, but results in panel (a) show that my parameter estimate from the true model is highly unlikely to be generated by chance.

Next, I test whether underlying temporal trends may somehow be spuriously driving my results. Panel (b) of the same figure shows nearly identical results from an analogous exercise where treatment status of a transaction is randomized within sale year. I also perform a temporal placebo test, where the date of service line remediation for treated properties is moved forward by 10-40 months, all treated transactions are dropped from the sample after their true remediation date, and the regression from column (3) of Table 3 is estimated. These placebo tests are designed to check whether an effect exists for a non-real treatment; a positive, statistically significant result would be of concern here. Results in columns (1) through (4) of Table 4 shows small, mixed-sign, and insignificant results across the differing placebo periods.

Finally, I test the potential for spatially-generated spurious results. Panel (c) of Figure 7 shows the resulting estimate distribution generated while bootstrapping randomized treatment status within census block group. Again, it is unlikely that my baseline parameter estimate was generated by chance. I also perform a spatial placebo test. Each treatment property transaction was matched with its nearest (physical distance) control property transaction that occurred in the same year. I then assigned each matched control property a placebo treatment corresponding to the replacement status of its treated neighbor, and drop all treated property transactions from the sample. Running my baseline specification, this placebo test shows small and statistically insignificant results for my parameters of interest, as seen in column (5) of Table 4.

Given this abundance of evidence against a purely spurious result, the next question must be

whether the baseline effect I am identifying suffers from any lingering omitted variable bias. In my preferred baseline result, the inclusion of δ_{jt} fixed effects and the use of a propensity-score-matched set of control homes suggest that the biggest remaining omitted variable threat to clean identification of the pipe replacements price effect would have to be some kind of unobserved improvement that *systematically* only occurs to treated properties following remediation.

To investigate this, I begin by looking for heterogeneity in post-replacement price effects by time since treatment. One may suspect that the potential for some kind of time-varying unobservable to bias my estimate is increasing in time since the property's treatment date. To examine this, I interact my post treatment indicator with dummies for whether a sale took place less than a year after replacement, 1 to 3 years after replacement, 3 to 5 years after replacement, or more than 5 years after replacement. Results in column (1) of Table 6 show this is unlikely to be the case here. The strongest post-replacement effect is measured for sales that take place within a year of replacement¹⁵.

A follow-up concern may be that a spike in sales occurring during the month of treatment (see Figure 7) is somehow indicative of unobserved concurrent improvements that are related to the pipe replacement. One straightforward example may be landscaping improvements that *only* treated properties receive following LSL retrofits¹⁶. I address this potential concern in two ways. First, column (2) of Table 6 re-estimates the baseline model, but drops all transactions that occur within a month of treatment. The parameter estimates increase in magnitude, suggesting the relative abundance of treated property sales close to the replacement date is not biasing the result upward. I also run a Cox survival model, formally testing whether properties are more likely to sell following a pipe replacement; results in column (3) of Table 1 show that is not the case.

As a second test, I interact the post-treatment indicator with quintile dummies for private replacement cost. Each property is assigned to a cost quintile based on the amount paid for remediation of its private-side LSL. If more costly LSL replacements are associated with more digging and construction, then one would speculate that unobserved landscaping improvements are

¹⁵For additional robustness, I also estimate my baseline model using only transactions on treated properties that fall within a 3-year or 5-year window of the LSL replacement. As shown in Appendix Table A2, the size of the post-treatment parameter (γ) is largely unaffected, though the magnitude of the negative parameter on treatment group (α) does increase when considering only samples close to the replacement date.

¹⁶In theory, this would be possible to directly test if landscaping improvements require building permits. Unfortunately, this is not the case in Madison.

most likely to occur at properties with larger remediation costs. The results in column (3) illustrate these heterogeneous post-replacement price effects by cost of replacement. Costs are highest in the 1st and 4th cost quintiles, which does not seem to support a narrative where unobserved landscaping costs are biasing my average price effect.

In addition to these baseline robustness checks, I consider several other model variations. To further mitigate any concerns of potential omitted variable bias in the cross-sectional approach, I turn to results from a repeat-sales analysis. The estimate in column (4), identified only off of pre- and post-treatment, within-property variation, is impressive in that it largely reflects the results from the repeated cross-section diff-in-diff regressions. Though smaller than the baseline estimate, it still imply a price capitalization effect of nearly 3%¹⁷. Finally, as a complement to the parametric analyses, I also estimate the replacement price effect using a non-parametric nearest-neighbor matching (NNM) approach. The result is shown in column (5)- when compared to their nearest neighbor match, properties with a lead service line sell for around 4.3% less. The striking similarity in direction and magnitude of results using both matching techniques and conventional parametric models indicates further robustness of the findings.

While the 3-4.3% effect I find across empirical strategies is large relative to the cost of the pipe replacement, it on the order of other relevant findings in the literature. For lead paint remediation, a procedure that is around 6 times more costly on average, Billings and Schnepel (2017) find an average post-treatment capitalization gain of around 32%. Using national data, upon the opening of a large-scale industrial plant within a mile of a home, Currie et al (2015) uncover an 11% drop in property values that they associate with pollution, including suspended lead particulate. In the aftermath of the 2015 Flint crisis, Christensen et al (2019) show that properties on the Flint water distribution network suffered strong depreciation relative to those off the network. Finally, in a related drinking water context, Muehlenbachs et al (2015) find that groundwater-dependent properties within 1.5km of shale gas development suffer devaluations of between 10-16%. When read in the context of this literature, a 3-4.3% price capitalization effect is certainly plausible, and likely captures premiums for both avoided health risk and the resolution of uncertainty about replacement cost.

¹⁷Again, in appendix Table A2, I estimate these models using only sales that occur in 3- or 5-year windows around treatment. Though I lose statistical power due to a small sample, the estimated magnitude of the effect only diminishes slightly.

5 Conclusion

Results across several estimation strategies show that homes treated by Madison’s lead pipe removal program from 2000-2015 profited from substantial average property value increases. Cross-sectional diff-in-diff, repeat sales, and matching estimates consistently find capitalization following lead service line remediation to be around 3-4.3% of a home’s value. These are sizable effects; the out-of-pocket cost for replacing a private service line during the program averaged less than 1% of a property’s assessed value, implying a return on investment of more than 300% for the median household treated during the program. Taking into account public costs as well, this still implies an average return of more than 75%.

Given the relative historical difficulty in acquiring accurate parcel-level service line replacement data, this is the first study to examine individual property value impacts following the replacement of private lead service lines. As this paper focuses simply on the average capitalization effect following remediation and remains speculative on the underlying causal mechanisms, there remain several open questions meriting further research.

Madison’s program was unique in several ways. First, during the program, homeowners arguably possessed higher-than-average salience about the presence of lead service lines on their property. As better data on water infrastructure becomes available and pre-sale piping disclosure laws increase in prevalence, it would be useful to examine how information disclosure on public and private drinking water infrastructure affects potential homebuyers in other contexts (Pope 2008).

A second non-universal aspect of Madison’s program is the aforementioned infrastructure property rights regime that exists in the city. Several cities have different legal structures surrounding the ownership of water piping. In such a city, one would expect a different capitalization effect following the removal of lead service lines- perhaps a purer measure of consumer valuation for lead-free water.

Thirdly, Madison’s mandate may have skewed incentives relative to those faced by a legally-unencumbered homeowner. In order to avoid partial-line-replacements, which are undesirable due to potential for *increased* waterborne lead concentration, the city had to find a way to coordinate their actions with those of private landowners. The city’s choice to pass a prescriptive policy is one regulatory approach. The counterfactual question is whether a less intrusive policy approach could

yield sufficient private participation for utilities to satisfy regulation and leave homeowners better off. As an example of a failed attempt at this, following the Washington, DC waterborne lead crisis in the early 2000s, the city tried to encourage concurrent public-private replacements without the use of mandates or incentives: uptake on the private side was negligible, and as a result, lead drinking water infrastructure remains common in the city.

Finally, there are potential policy implications that follow from this research. In conjunction with the reported capitalization effects, Madison’s use of a partial subsidy to incentivize reporting and removal of service lines appears to have yielded a double dividend to treated homeowners. To some, this could raise distributional concerns, as older homes in downtown Madison are often rented to students, young professionals and lower income households. Distributional concerns are compounded if the capitalization effect passes through to low-income households in the form of increased monthly rent. While there are surely other health, education, and public safety benefits resulting from the retrofitting of lead pipes, the findings in this paper indicate that low- or zero-interest loans may sometimes be preferable to subsidies in cities currently considering future programs.

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Figures and Tables

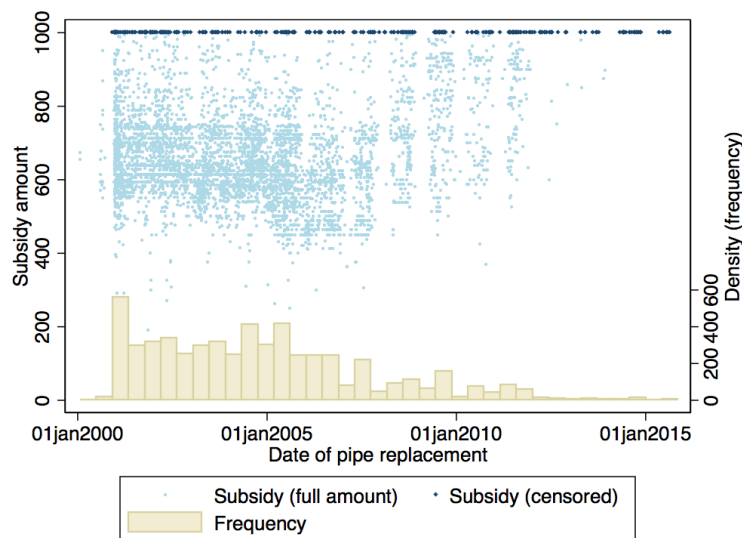


Figure 1: Distribution and temporal frequency of lead pipe removal reimbursements paid by MWU to homeowner recipients. Each point represents a month-year date and subsidy amount combination. The bulk of replacements occurred before 2011, when the program was initially scheduled for completion. These values must be doubled to reflect full private replacement costs; note that costs for just under 10% of the treated sample are censored due to the \$1000 repayment threshold.

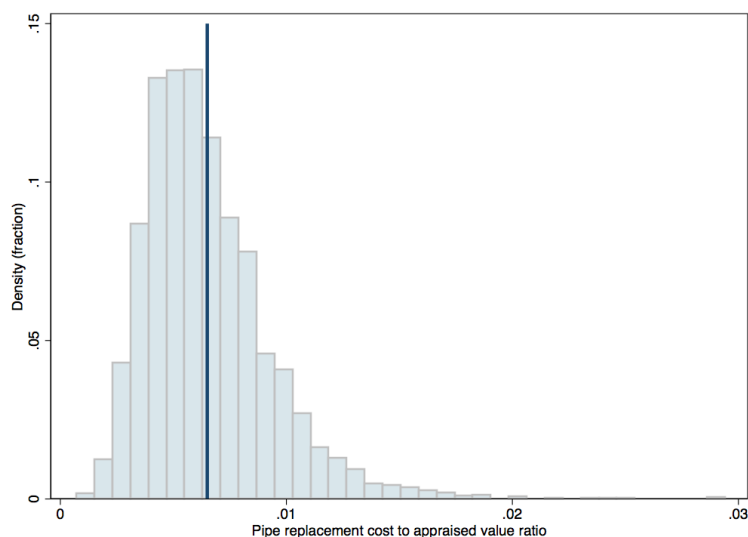


Figure 2: Histogram of replacement cost to appraised value ratio for properties treated during Madison's program. The ratio's mean value for treated properties was 0.63%, with a long right tail; the right-censoring on cost makes this estimated mean a lower bound on the true average cost ratio for a private side replacement during the program. Replacement cost calculated as twice the property's subsidy level. A property's appraised value is measured from the year of the service line replacement.

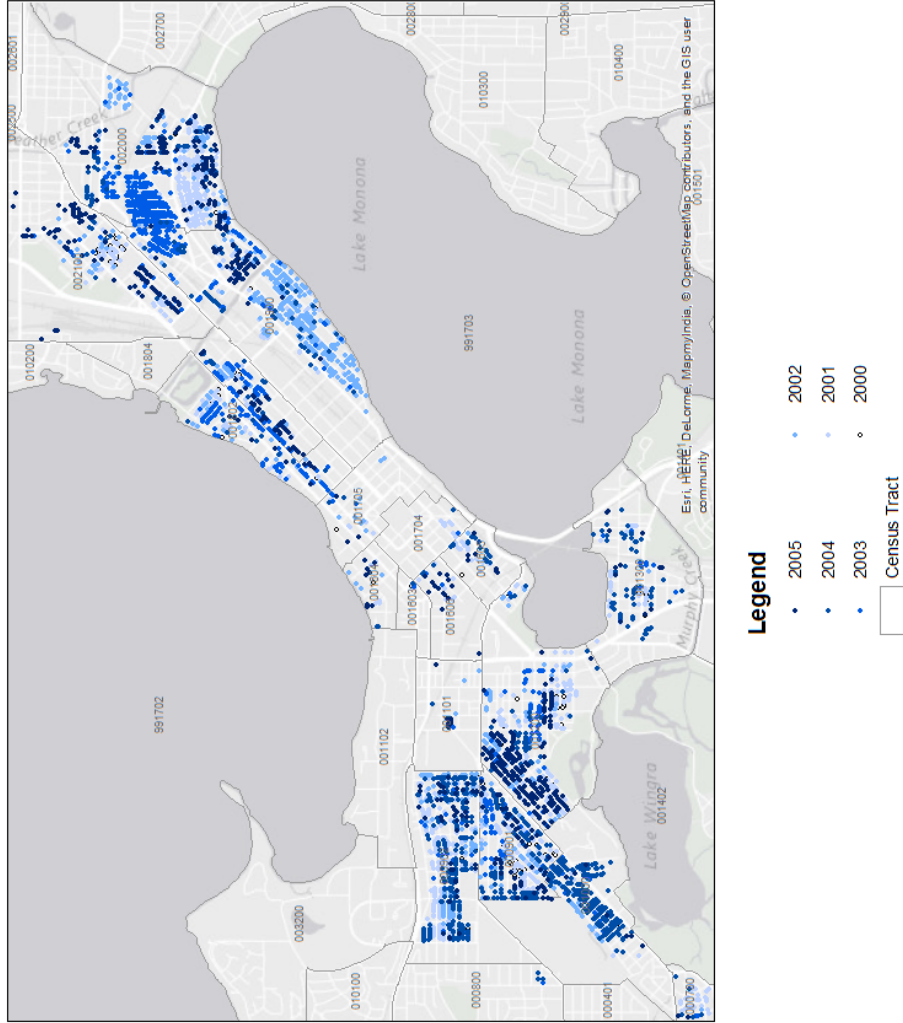


Figure 3: A map visualization of displaying a subsample of temporal and spatial variation in program coverage, year by year, over the replacement program's initial five years (2000-2005) of Madison's program. Replacements often occurred sequentially along a given street, resulting in neighborhood clustered treatment. The within neighborhood-year difference-in-differences identification strategy accounts for potential spillover effects.

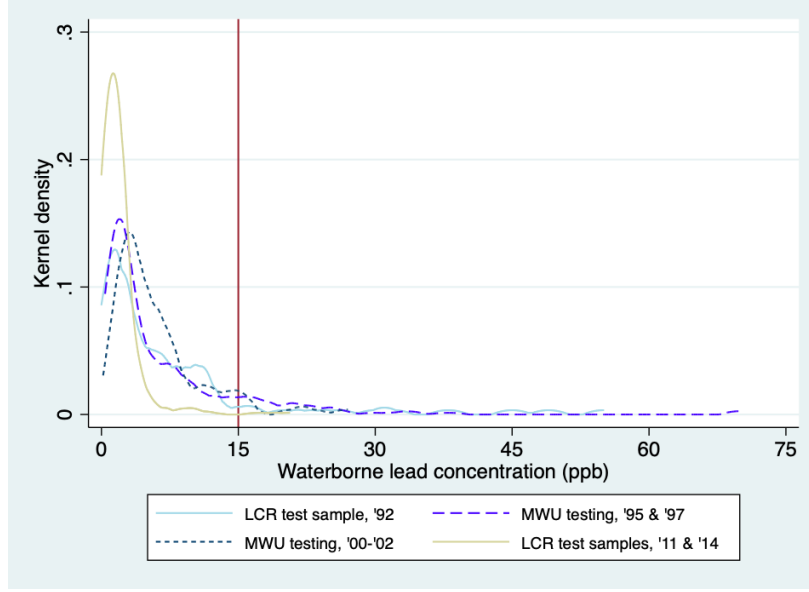


Figure 4: This density graphic demonstrates the real reduction in public waterborne lead concentration over the course of Madison’s LSL replacement program. In the initial 1992 LCR sample, the 90th percentile fell at 15ppb. This is confirmed by MWU internal testing from 1995 to 2002; for these samples, sizable parts of the distribution fell at or above the 15ppb EPA threshold, with some observations reaching levels as high as 150ppb. Following the LCL replacement program, the 2011-2014 LCR samples found only a single observation above the 15ppb threshold; even more importantly, the vast majority of the sample possessed concentrations <5ppb.

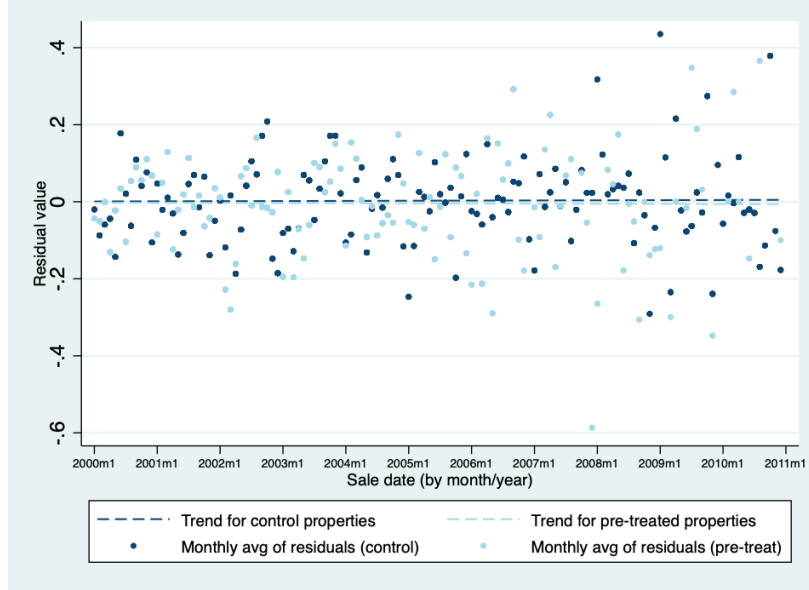


Figure 5: Logged sale prices were regressed on tract-year and month fixed effects. Resulting residuals are aggregated by treatment group and month, then plotted here, with a sales-weighted linear trend mapping over time for both groups. After controlling for city-wide time effects and neighborhood-specific characteristics, control and treatment properties possess common trends over the early years of the program. Note that plotted treatment residuals and trend contain **only pre-replacement** property sales. Using the residuals from above, a simple linear regression of the form $RESID_{it} = \alpha TREATGRP_i + \beta TREATGRP_i \times t + \gamma t + \varepsilon_{it}$ yields $\beta = -.0000341$ ($s.e. = .0003455$); the slope difference in linear trend between is highly insignificant.

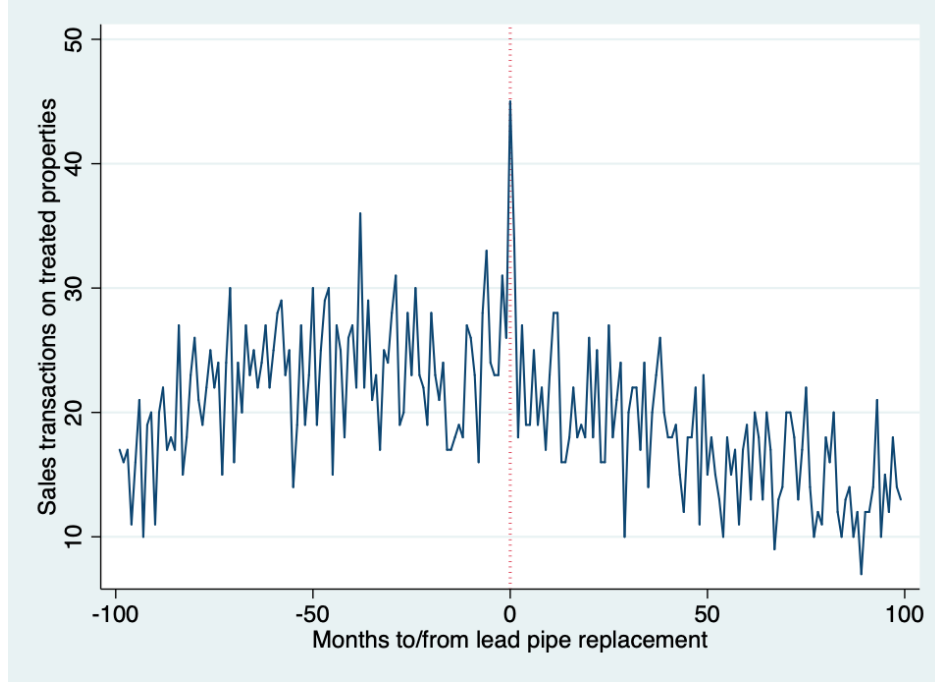
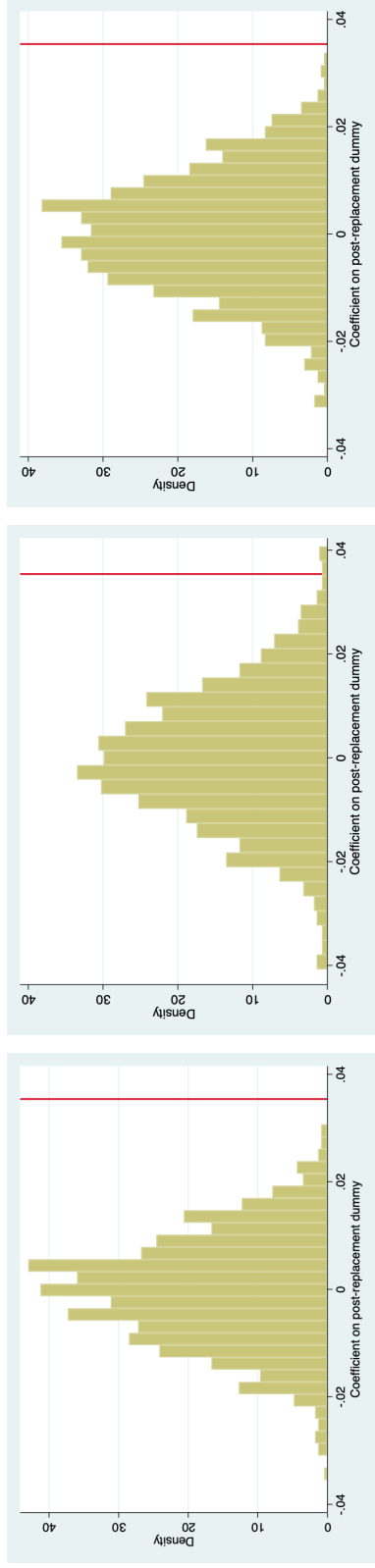


Figure 6: A running count of sales transactions on (eventually) treated properties is graphed over time during the study period (2000-2015). Time is measured as months to/from a property’s lead pipe replacement, with $t = 0$ indicating a sale in the month of treatment. Other than the evident spike during the month of treatment, there is little visual evidence suggesting treatment increases the frequency of sales transaction. If anything, the visual trend suggests sales on treated properties are less frequent after remediation, as is shown more formally through the survival model shown in column (3) of Table 1. To address a reviewer’s concerns about elevated risk of unobserved time-varying improvements occurring during treatment-month sales, I also show my main results are robust to the exclusion of transactions occurring in month before and after treatment - see column (2) of Table 5.



(a) Bootstrapped randomization of treatment - across all sales
(b) Bootstrapped randomization of treatment - within year of sale
(c) Bootstrapped randomization of treatment - within census block group

Figure 7: Randomization tests: I undertake three larger-scale randomization tests where I generate false data to make sure the main effects I find are not simply spurious. Across all three tests, the procedure followed a similar formula: (1) using the PS-matched subsample from the preferred specification, each observation is randomly reallocated a treatment status, with the newly-generated treatment and control groups being of identical size to those in the true data; (2) the regression of form (1) is run, and the resultant parameter estimate on $POST_t * TREAT_t$ is recorded; (3) this procedure is bootstrapped 1000 times, generating a distribution of parameter estimates, which is displayed in this figure. The first test (see left) is a complete randomization of treatment status across all observations in the sample. The second test is similar in nature, but randomizes treatment status *within sale year* to check whether larger time trends might be behind the result. And lastly, the third test randomizes treatment status *within census block group*, to ensure that the spatial structure of my panel is not spuriously yielding the result. The parameter estimate, $\gamma = 0.0354$, falls well into the right-hand tail of each of the generated distributions.

	(1)	(2)	(3)
	OLS estimation		Cox PH model
	Months to treatment	Months to treatment	Months to Sale
Initial appraised value (\$100k)	-2.960 (2.635)		
Neighborhood price index (\$100k)		-0.478 (3.935)	
Property sale price (\$100k)			0.221*** (0.030)
Size of parcel (sq ft)	0.000720 (0.000482)	0.000582 (0.000443)	-0.000008*** (0.0000001)
Size of home (sq ft)	0.000261 (0.00298)	-0.00224 (0.00241)	-0.0002*** (0.00002)
No. of bedrooms	-0.235 (0.728)	-0.356 (0.646)	0.025*** (0.009)
No. of bathrooms	-0.828 (1.067)	-0.450 (0.942)	-0.003 (0.012)
No. of fireplaces	-2.841* (1.447)	-1.632 (1.475)	-0.013 (0.011)
Area fin. basement (sq ft)	0.000546 (0.00325)	0.000749 (0.00343)	-0.00006** (0.00002)
Central air cond. (dummy)	-1.266 (0.828)	-1.801** (0.735)	-0.034** (0.015)
Noticeable traffic (dummy)	4.228 (3.636)	4.017 (3.674)	0.005 (0.018)
Heavy traffic (dummy)	11.23** (4.696)	15.13*** (5.247)	0.056** (0.026)
Year of home const.	0.0822 (0.0952)	0.0601 (0.0897)	-0.001 (0.0007)
Airport noise (dummy)	-1.289 (4.601)	-10.72** (4.093)	-0.043 (0.067)
Railroad noise (dummy)	-1.840 (3.280)	-1.648 (4.403)	0.048* (0.025)
Waterfront property (dummy)	-0.149 (13.30)	-2.073 (12.00)	-0.750*** (0.125)
Wooded property (dummy)	-3.257 (2.484)	-3.279 (2.379)	-0.035* (0.021)
Attached garage (dummy)	-2.414 (2.260)	-1.123 (2.075)	-0.034 (0.027)
Detached garage (dummy)	-4.762** (2.034)	-4.890** (1.856)	-0.040 (0.024)
Basement garage (dummy)	-10.77*** (2.867)	-9.178*** (3.009)	0.010 (0.032)
Post-replacement (dummy)			-0.584*** (0.031)
Observations	4,944	5,116	36,448
Adjusted R-squared	0.037	0.040	

Standard errors clustered by census block group.

*** p<0.01, ** p<0.05, * p<0.1

Table 1: Testing time to treatment and time to sale: In columns (1) and (2), using the full cross-section of treated properties with available property characteristic information, I formally test whether certain types of households were treated earlier during Madison's program. The dependent variable is date of treatment, measured as months since January 2000. Given the specifics of Madison's program and the fact that there is no censoring in this data - I see the date of service line replacement for each property, and consider January 2000 to be month 0 for all properties - I estimate the model by OLS. Column (1) uses initial appraised value as a measure of property price, while column (2) relies on a neighborhood price index calculated as the monthly average of neighborhood sales. Column (3) investigates whether receiving treatment in the program makes a property sale more or less likely to occur. Since these data are censored, I estimate a survival model of months to sale (beginning in January 2000) using a Cox proportional hazards model and following the counting process approach of Andersen and Gill (1982) that assumes repeated sales of a property are indistinguishable in type.

	Full market extent		PS-matched sample		Repeat sales sample	
	(1)	(2)	(3)	(4)	(5)	(6)
	Control mean/sd	Treatment mean/sd	Control mean/sd	Treatment mean/sd	Control mean/sd	Treatment mean/sd
log(sale price), (CPI-2000)	11.9327 (0.473)	12.140 (0.495)	12.042 (0.516)	12.141 (0.492)	12.037 (0.437)	12.147 (0.455)
Size of parcel (sq ft)	7690.176 (10462.21)	5128.322 (2371.8)	5136.455 (2518.094)	5127.557 (2370.295)	5127.751 (3233.92)	4907.659 (2120.14)
Size of home (sq ft)	1490.101 (584.422)	1562.897 (542.284)	1457.459 (582.433)	1558.285 (535.162)	1336.367 (495.189)	1477.917 (473.406)
No. of bedrooms	3.026 (0.932)	3.191 (1.004)	3.020 (1.078)	3.181 (0.992)	2.827 (0.986)	3.049 (0.922)
No. of bathrooms	2.100 (0.815)	1.769 (0.686)	1.710 (0.707)	1.760 (0.682)	1.666 (0.655)	1.697 (0.634)
No. of fireplaces	0.651 (0.678)	0.389 (0.561)	0.449 (0.670)	0.386 (0.554)	0.515 (0.674)	0.355 (0.275)
Area fin. basement (sq ft)	341.937 (380.468)	94.729 (180.479)	106.937 (193.093)	92.448 (176.198)	171.2948 (241.544)	90.870 (167.395)
Central air cond. (dummy)	0.884 (0.320)	0.602 (0.489)	0.632 (0.482)	0.601 (0.490)	0.749 (0.434)	0.629 (0.483)
Age of home (yrs)	30.161 (26.529)	90.844 (13.789)	85.162 (14.812)	90.994 (13.542)	67.127 (23.978)	91.935 (13.716)
Noticeable traffic (dummy)	0.121 (0.329)	0.205 (0.404)	0.193 (0.395)	0.205 (0.403)	0.200 (0.400)	0.217 (0.413)
Heavy traffic (dummy)	0.049 (0.217)	0.162 (0.368)	0.159 (0.365)	0.162 (0.368)	0.148 (0.355)	0.169 (0.375)
Year of home const.	1976.729 (26.293)	1916.087 (12.982)	1921.71 (14.812)	1915.944 (12.710)	1921.568 (23.957)	1916.529 (12.849)
Airport noise (dummy)	0.013 (0.114)	0.058 (0.234)	0.093 (0.291)	0.059 (0.235)	0.103 (0.304)	0.055 (0.228)
Railroad noise (dummy)	0.015 (0.124)	0.049 (0.218)	0.026 (0.158)	0.051 (0.220)	0.032 (0.177)	0.051 (0.221)
Waterfront property (dummy)	0.005 (0.068)	0.010 (0.098)	0.016 (0.125)	0.010 (0.098)	0.006 (0.083)	0.010 (0.051)
Wooded property (dummy)	0.145 (0.352)	0.092 (0.289)	0.113 (0.316)	0.091 (0.288)	0.175 (0.380)	0.091 (0.288)
Observations	49,877	3803	3132	3721	3046	1523

Table 2: Summary statistics for property characteristic variables in transaction data. Disaggregated by whether or not a property was treated during Madison’s LSL replacement program. First two columns are full sample- all sales between 2000-2015 in the city boundaries of Madison. I consider this the full extent of the Madison market, though the replacement program primarily affected properties in the downtown area. The middle two columns are the propensity-score matching (PSM) balanced sample used for my preferred analysis and results. A PSM matching procedure (described in text) created a more balanced panel of “control” and “treated” properties based on observable property characteristics. Of particular note - control and treatment properties are much more similar in age, lot size, probability of having air conditioning. Finally, columns (5) and (6) contain the summary statistics for transactions that are included in the repeat sales analysis in Table 5.

	(1)	(2)	(3)
log(sale price)	Full market extent	Pre-1940, treatment area	PS-matched sample
Treatment group	-0.0043 (0.0211)	-0.0008 (0.0142)	-0.0136 (0.0242)
Post-replacement	0.0376** (0.0152)	0.0384** (0.0144)	0.0354** (0.0161)
Observations	53,219	6,853	5,838
Adjusted R-squared	0.600	0.667	0.659
Controls	yes	yes	yes
Month FEs	yes	yes	yes
CBG x year FEs	yes	yes	yes

Standard errors two-way clustered by census block group and year of sale.

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Baseline difference-in-differences regressions on program sample covering 2000-2015. Columns (1) makes use of the complete data sample - best interpreted as the full extent of Madison's hedonic market during the LSL replacement program. Columns (2) uses a subsample of transactions: homes that were built before WWII and are located in the program's treatment area. Column (3), my preferred specification, is the PSM-balanced sample - by construction also limited to the replacement program's spatial footprint. Month and census block group-year fixed effects, as well as property-specific characteristics are included in all specifications- see summary statistics table for list of control variables.

	(1) 10 months before	(2) 20 months before	(3) 30 months before	(4) 40 months before	(5) Spatial placebo
log(Sale price)					
Treatment group dummy	0.0140 (0.0203)	0.0193 (0.0169)	0.0210 (0.0153)	0.0133 (0.0172)	0.0204 (0.0145)
Post (placebo) replacement	0.0130 (0.0205)	-0.0045 (0.0149)	-0.0064 (0.0180)	0.0061 (0.0206)	0.0153 (0.0119)
Observations	3,493	3,493	3,493	3,493	7,300
Adjusted R-squared	0.632	0.632	0.632	0.632	0.687
Controls	yes	yes	yes	yes	yes
Month FEs	yes	yes	yes	yes	yes
Tract x year FEs	yes	yes	yes	yes	yes

Standard errors two-way clustered by census tract and year of sale.

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Placebo tests: Using the PS-matched subsample from my preferred specification (column (3) of Table 3), in columns 1-4, I (falsely) alter the date of service line remediation for treated properties forward by 10-40 months, drop all sales for treated properties after their true remediation date, and run the cross-sectional diff-in-diff regression from Equation (1). Across these tests for temporally spurious results, there is no evidence of placebo effect. In column (5), as a spatial placebo test, I use the entire data sample to match each sale of a treated property with its closest neighboring control property that sold in the same year. I then (falsely) assign this control property its neighbor's treatment status, dropping the real sale observation for the treated property. I run the regression from equation (1), including only sales that occurred in the program's treatment area. Again, the resulting statistically insignificant parameters do not suggest the existence of a placebo effect.

VARIABLES	(1) Het. by sale date log(sale price)	(2) Drop sales during repl. log(sale price)	(3) Het. by repl. cost log(sale price)	(4) Repeat sales $\Delta \log(\text{sale price})$	(5) NN-matching log(sale price)
Treatment group	-0.0132 (0.0216)	-0.0215 (0.0239)	-0.0154 (0.0245)		
Post-replacement		0.0449*** (0.0146)		0.0298* (0.0157)	
Post x 0-12 months	0.0511** (0.0200)				
Post x 12-36 months	0.0227 (0.0232)				
Post x 36-60 months	0.0405 (0.0297)				
Post x >60 months	0.0336 (0.0309)				
Post x Lowest cost quint.			0.0418* (0.0206)		
Post x 2nd cost quint.			0.0348 (0.0215)		
Post x 3rd cost quint.			0.0290 (0.0185)		
Post x 4th cost quint.			0.0477** (0.0184)		
Post x Highest cost quint.			0.0257 (0.0236)		
Lead present					-0.0420*** (0.0148)
Observations	5,838	5,737	5,688	4,571	43,603
R-squared	0.659	0.661	0.654	0.391	
Controls	yes	yes	yes	no	
Block group x year FEs	yes	yes	yes	no	
Month FEs	yes	yes	yes	yes	
CBG time trends	no	no	no	no	
Year FEs	no	no	no	yes	
Previous year FEs	no	no	no	yes	

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Alternative specifications and estimation methods. Columns (1)-(3) are extensions of the preferred model (3) from Table 1. Column (1) measures heterogeneous post-treatment effects by time since LSL replacement. Column (2) drops all sales that occur in the month before/after a LSL replacement. Column (3) investigates potential heterogeneous price effects resulting from differing private replacement costs. Column (4) is a repeat sales model: dependent variable is $\ln(P_{it}) - \ln(P_{it'})$ for $t > t'$. Column (5) uses a nearest-neighbor matching algorithm to estimate the ATT for properties *possessing lead pipes* during the program. See text for specifics of matching procedure.

Standards errors: (1)-(4) two-way clustered by CBG and year of sale; (5) Abadie-Imbens robust.

Appendix

VARIABLES	(1)	(2)	(3)
	Box-Cox specification Sale price	Time-varying hedonic parameters log(Sale price)	CBG x Quarter-year FEs log(Sale price)
Treatment group dummy	2.710 (s-v)	-0.0084 (0.0244)	0.0067 (0.0221)
Post-replacement	4.101 (s-v)	0.0404* (0.0203)	0.0353* (0.0196)
Lambda	-0.044 (0.050)		
Theta	0.309*** (0.006)		
Observations	53,312	53,219	52,704
Log-likelihood	-653128		
Adjusted R-squared		0.610	0.589
Controls	yes	no	yes
Controls x year FEs	no	yes	no
Month FEs	yes	yes	no
Year FEs	yes	no	no
CBG FEs	yes	no	no
CBG x year FEs	no	yes	no
CBG x Quarter-year FEs	no	no	yes

Columns 2 & 3: Standard errors two-way clustered by census block group and year of sale.
*** p<0.01, ** p<0.05, * p<0.1

Table A1: This table shows results from alternative specifications for the hedonic model using the data sample that captures the full extent of the market. Column (1) is a left- and right-hand-side Box-Cox model, used primarily to test the validity of my primary log-linear specification. The estimated values of LHS transform parameter (λ) and RHS transform parameter (θ) suggest a log-linear specification is indeed reasonable, given the data. Column (2) follows Kuminoff et al (2010) in allowing the marginal implicit prices on control parameters to vary over time by interacting them with year of sale dummies. This relaxes the assumption of constant implicit covariate prices, but results show little effect on the magnitude of my parameters of interest. To account for unobserved, time-varying neighborhood characteristics in an alternative way, column (3) relies on CBG x quarter-of-year fixed effects instead of CBG x year and monthly fixed effects.

	(1)	(2)	(3)	(4)
	Cross-sectional models		Repeat sales models	
	3 year window	5 year window	3 year window	5 year window
log(Sale price)				
Treatment group dummy	-0.0291 (0.0199)	-0.0180 (0.0214)		
Post-replacement	0.0345** (0.0156)	0.0317 (0.0187)	0.0288 (0.0812)	0.0261 (0.0298)
Observations	4,122	4,530	686	1,892
Adjusted R-squared	0.652	0.649	0.067	0.229
Controls	yes	yes	no	no
Month FEs	yes	yes	yes	yes
CBG x year FEs	yes	yes	no	no
Year of sale FEs	no	no	yes	yes
Previous year of sale FEs	no	no	yes	yes
CBG time trends	no	no	yes	yes
Standard errors two-way clustered by census block group and year of sale.				
	*** p<0.01, ** p<0.05, * p<0.1			

Table A2: This table measures the robustness of baseline and repeat sales results to limiting the temporal window around a lead pipe replacement when constructing the study sample. This restricted sample should help limit the potential for unobservable, time-varying home improvements to occur. In columns (1) and (3), I limit the treatment sales sample to only transactions that occurred within 3 years of the LSL replacement. In columns (2) and (4), I expand this window to 5 years. While the restricted samples limit the statistical power of these results, the direction and magnitude of coefficient estimates remain consistent with the full-sample results.