The Effects of Lead Exposure on Violent Crime: Evidence from U.S. Cities in the Early Twentieth Century*

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December 29, 2014

Abstract

In the second half of the nineteenth century, many American cities built water systems using lead or iron service pipes. Municipal water systems brought significant public health improvements, but these improvements may have been partially offset by the damaging effects of lead exposure through lead water pipes. We study the effect of cities' use of lead pipes on homicide between 1921 and 1936. Lead water pipes exposed the entire city population to much higher doses of lead than have previously been studied in relation to crime. Our results suggest that cities' use of lead service pipes increased city-level murder rates.

*We thank Douglas Eckberg, Claudia Goldin, Andrew Hall, Richard Hornbeck, Lawrence Katz, Robert Margo, Jeffrey Miron, Andrew Papachristos, Werner Troesken, and Christopher Wildeman for detailed comments and suggestions, as well as seminar participants at the Harvard University Economic History Tea and Workshop in History, Culture, and Society. Price Fishback generously shared historical FBI data on homicide. This research has been supported by the NSF-IGERT Multidisciplinary Program in Inequality & Social Policy at Harvard University (Grant No. 0333403).

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At the end of the nineteenth century, large cities and small towns across the United States built and expanded municipal waterworks. The number of waterworks grew more than tenfold from 1870 to 1896, with more than one thousand new systems added between 1890 and 1896 alone (Cutler and Miller 2006). Sanitation engineers, city officials, and urban boosters alike greeted the new water systems with enthusiasm. The presence of waterworks signaled that one lived in a "respectable community" (Melosi 2000, p. 82). New water systems also promised to improve residents' quality of life. City dwellers were invited to "Think of the effect upon the standard of living caused by the introduction of a public water supply!" (Baker 1897). New waterworks reduced the labor required to draw well water and improved fire protection. They also marked the abandonment of badly polluted surface wells (Baker 1897). Contemporaries observed reductions in typhoid that modern social science has shown were causally linked to the introduction of waterworks (Cutler and Miller 2005; Alsan and Goldin 2014; Troesken 1999; Melosi 2000). These benefits, however, did not accrue evenly across all cities. In many cities, the very pipes installed to improve urban life and health leeched noxious particulates into the local water supply.

A growing body of evidence in the social and medical sciences traces high crime rates to lead exposure. Scholars have shown that lead exposure and crime are positively correlated using data on individuals, cities, counties, states, and nations. Reyes (2007) exploits the timing of states' compliance with the Clean Air Act to estimate the causal effect of lead emissions from gasoline on violent crime. She finds that reductions in childhood lead exposure in the 1970s and 1980s accounted for more than half of the violent crime decline of the 1990s.[1] Mielke and Zahran (2012) show that air lead and aggravated

[1] Cook and Laub (2002, p. 24, 28) note that the relative uniformity in the timing of the rise and fall of violent crime across cohorts is inconsistent with the conclusion that lead

assault rates were strongly associated in a panel of U.S. cities. Longitudinal studies of individuals document a positive relationship between pre- and post-natal lead exposure and delinquency (Dietrich et al. 2001) and arrests for offenses involving violence (Wright et al. 2008). Cross-sectional research on individuals (Denno 1990; Needleman et al. 1996, 2002) and counties (Stretesky and Lynch 2001, 2004), panel studies of nations (Nevin 2007), and analyses of national time-series (Nevin 2000) have yielded similar results.

To date, the strength of the literature on lead exposure and violent crime lies in the fact that its findings have been replicated at several scales. However, with the exception of Reyes (2007, 2014), few previous studies report estimates that can be considered causal, as researchers for obvious ethical reasons cannot randomly expose humans to lead. Moreover, it is difficult to find credible sources of exogenous variation in lead exposure today. In this paper, we set previous estimates of the lead-crime relationship on firmer causal footing by exploiting exogenous variation in the historical distribution of lead water pipes.

Because the mechanisms linking lead and crime are biochemical, lead's effects should be observable not only at different scales, but in different times as well. Reassuringly, scholars studying the historical effects of lead exposure on outcomes other than crime have found evidence that this is the case. Army enlistees who lived in cities whose water absorbed high levels of lead in 1930, for instance, scored comparatively low on the Army General Classification Test (AGCT) (Ferrie et al. 2012). Cities and towns with high concentrations of water lead at the turn of the twentieth century also had higher infant mortality rates than otherwise similar cities (Troesken 2008; Clay et al. 2014).

We study the lead-crime relationship using historical data on the water supply of U.S. cities between 1921 and 1936. Studying the historical effects

removal was a primary cause of the crime drop. However, this does not, they point out, mean that lead exposure has no effect on violence.

of lead has three advantages. First, lead levels in city water were determined by a plausibly exogenous source: a city's distance from a lead refinery. The dangers of lead were not widely known in the period we study, and there is little evidence that city officials used this information to decide whether to use lead pipes.[2] Second, city-dwellers were exposed to much higher doses of lead historically than they are today (Troesken 2006). Many of the cities we study had historical lead levels hundreds of times the modern Environmental Protection Agency (EPA) standard for water (Troesken 2006). The historical effects of lead consumed in drinking water contaminated by inflowing service pipes should consequently be larger than the contemporary effects of ingested lead paint chips or inhaled gasoline exhaust.

Third, lead exposure today is not uniformly distributed within cities. Poor children are more likely than middle-income or rich children to come into contact with lead (Brooks-Gunn and Duncan 1997). This empirical regularity makes it difficult to disentangle the effects of lead exposure from the effects of poverty. Studying cities in the early twentieth century enables us to circumvent this problem because the entire city population was exposed to lead through water. Our analysis compares cities that used lead water pipes to cities that did not rather than comparing individuals whose exposure to lead might be correlated with other causes of crime.

Because high-homicide cities, such as those in the South, selected out of lead pipe use, ordinary least squares estimates of the effect of lead exposure through water pipes are likely to be biased downward. Consequently, we report estimates of the effect of cities' use of lead pipes instrumented by their distance from the nearest lead refinery.[3] Our instrumental variables estimates suggest

[2]Tarr (1985) notes that the dangers of heavy metals, including lead, were not extensively studied in the early twentieth century. Maximum permissible standards for lead, copper, and zinc were not introduced until 1925 (Tarr 1985).

[3]Our data structure prohibits us from estimating the causal effect of lead using changes

that using lead service pipes increased cities' homicide rates considerably. Different estimation strategies and sample restrictions yield substantively similar results.

The paper proceeds as follows. In the next section we discuss the chemical and biological pathways through which lead exposure might increase violent crime rates. Next, we describe our data, devoting special attention to the advantages and disadvantages of different measures of historical homicides. The following section introduces our empirical strategy, presents our main results, and reports several additional falsification exercises and identification strategies. The final section concludes.

1 Mechanisms

There are three known pathways through which lead exposure might increase individuals' likelihood of committing violent crimes. The first is lead's effect on impulse control (Loeber et al. 2012). Several studies have shown that lead exposure is associated with a range of conduct problems (Marcus et al. 2010). Needleman et al. (1996), for instance, X-rayed the bones of 212 boys in Pittsburgh, finding that boys with high bone lead measurements exhibited more delinquent, aggressive, internalizing, and externalizing behavior than otherwise similar boys. Burns et al. (1999) replicated these findings in a study of children in Australia. Chen et al. (2007) report that bone lead concentration is positively correlated with externalizing and school problems at age 7, even conditional on IQ.

in pipe metals or city fixed effects. We observe whether cities used lead or iron pipes only once—as of 1897—and are able to generate a large enough sample of homicide rates beginning in 1921, long after lead service pipes were introduced in our sample cities. Likewise, we cannot use the removal of lead pipes to identify the effect of lead exposure because lead pipes often were replaced gradually with polyvinyl chloride (PVC) or iron pipes (Troesken 2006, p. 6).

The second and third ways lead exposure could affect individuals' probability of participating in violent crime are by increasing their likelihood of having ADHD and reducing their cognitive ability. A large literature connects lead exposure to the risk of ADHD (Goodlad et al. 2013). Braun et al. (2006), for example, document a significant dose-response relationship between childhood lead exposure and ADHD in a nationally representative sample of children. Nigg et al. (2010) show that blood lead was correlated with hyperactivity in a sample of 212 children. Children with ADHD, particularly those who exhibit early behavior problems, are more likely than comparable children to become involved in crime (Moffitt 1990a,b).

Finally, a large number of studies report a negative relationship between lead exposure and IQ, with no evidence of a threshold below which lead exposure does not affect cognition (Needleman and Gatsonis 1990; Bellinger et al. 1992; Pocock et al. 1994; Schwartz 1994; Lanphear et al. 2000; Canfield et al. 2003; Lanphear et al. 2005). IQ, in turn, is strongly related to the cluster of neuropsychological abilities known as "executive functions," whose impairment "will produce an inattentive, impulsive child who is handicapped at considering the future implications of his or her acts" (Lynam et al. 1993, p. 188).

In this paper, we estimate a reduced-form relationship between lead exposure and rates of violent crime at the city level. Our findings are consistent with any of the mechanisms previously documented in the medical literature. The causal identification strategy we use does not depend on the particular biological channel connecting lead exposure to historical violent crime rates.

2 Data

We collect information on the historical pipe metal used by all U.S. cities for which data are available, drawing on a variety of historical sources. In prior research, Clay et al. (2014) and Ferrie et al. (2012) compiled data on the pipe metals used in municipal water systems in the late nineteenth century. We supplement these data with additional information on water pipes drawn from *The Manual of American Water-Works* (Baker 1897). In all, we have information on the type of metal used in the water pipes of 591 cities.[4]

At the turn of the century, municipal water systems used one of three pipe metals: lead, galvanized iron, or wrought iron. Of the cities for which we can identify the pipe metal, 54% used lead or lead in combination with other metals, 40% used galvanized iron, 9% used wrought iron, and 9% used an unspecified type of iron.[5] The cities included in our sample are mapped in Figure 1.[6] In both the map and in our analysis, we divide cities into those using some lead pipes, marked on the map with triangles, and those using no lead pipes, marked on the map with circles, as reported in *The Manual of American Water-Works*. We include in the "some lead" group both cities with only lead pipes and cities with both lead and iron pipes because we are unable to determine the relative shares of each type of pipe in each city. This will bias our estimate of lead's effect towards zero, as we may include some cities with a very small share of lead pipes in the treatment group.

To conduct our analysis, we link the pipe data to city-level homicide data

[4]In the results presented below, the sample size varies due to missing homicide and demographic data. Our main specification includes cities that reported at least one homicide over the sixteen year period. We omit cities without a single reported homicide in the period because in these cities population variation creates spurious variation in the outcome variable. However, we also report results from regressions including these cities in Figure 7.

[5]These percentages sum to more than 100% because some cities used multiple types of pipe material.

[6]Figure A.3 in the Appendix includes detail maps of the urban Northeast and Midwest.

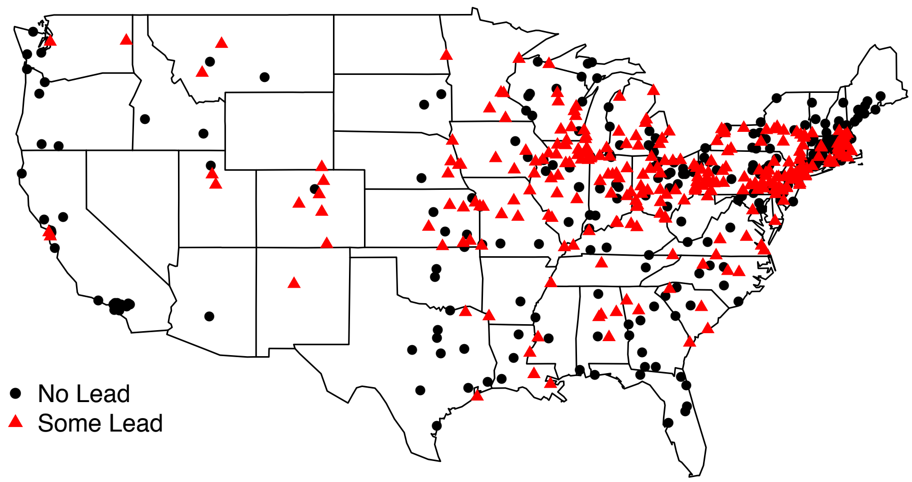


Figure 1: Location of sample cities and the type of water pipe they used as of 1897. Data are drawn from *The Manual of American Water-Works* (Baker 1897). Cities with only lead pipes and cities with a mix of lead and iron pipes are included in the "some lead" category and marked with triangles. Cities using either galvanized iron or wrought iron are included in the "no lead" category and marked with circles. See Figure A.3 in the Appendix for detail maps of the urban Northeast and Midwest.

culled from historical *Mortality Statistics* reports. These reports, produced contemporaneously by the Department of Commerce, record city-level mortality counts by cause of death. We digitize these data for the years 1921 to 1936.[7]

We begin measuring homicide in 1921 because early counts of violent crimes, homicides, and suicides in the *Mortality Statistics* are considered unreliable. Problems with the homicide counts were identified very early: in 1906, the Census Bureau deemed them "incorrect and absolutely misleading" (United States Census Bureau 1906, p. lv). Eckberg (2006) notes that the errors in early counts of murders were partially driven by the changing definition of homicide in the early twentieth century. For instance, police often recorded automobile accidents and other violent deaths as homicides. In addition, the *Mortality Statistics* rarely distinguished between felony murder and justifiable

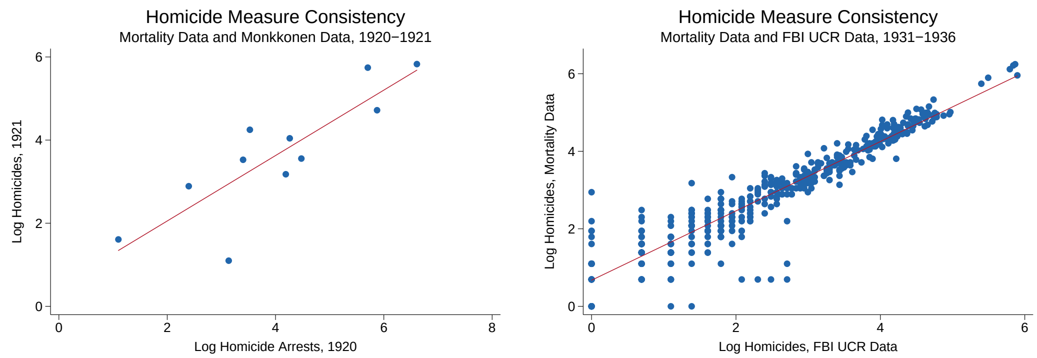
[7]We collect and digitize measures of total and cause-specific deaths per city by year, enabling us to minimize errors in our digitization.

homicide. Moreover, early death certificates, which were sent from localities to the Census Bureau, included no information about crime. This forced the Bureau to make an independent determination, based on very little evidence, of whether a death by poison was a murder, a suicide, or an accident.

These complications prompted the Census Bureau to develop a model death certificate in 1907. However, the categorization of violent deaths in the city-level *Mortality Statistics* remained inconsistent through 1920. In 1906 and 1907, the *Mortality Statistics* reported deaths from "other violence." This category was absent in 1908 and 1909, returning in 1910 under a slightly different name: "violent deaths (excluding suicide)." Starting in 1921, the *Mortality Statistics* dropped the "violent deaths" category and began reporting the number of homicides. Following Eckberg (1995), we believe that the reported deaths from other violence in 1906–1907 and 1910–1920 are likely to include homicides. However, these counts also include non-homicide deaths and may exclude some homicides. Accordingly, we limit our sample to homicides recorded after 1920. We follow Cutler and Miller (2005, p. 7) in ending our analysis before the introduction of a new data series in 1937.

To check the reliability of our homicide data after 1920, we match it to two other sources of data on homicide in the early twentieth century: homicide arrest data from 23 cities in 1920 collected by historian Eric Monkkonen (2005) and homicide data from the Uniform Crime Reports (UCR) of the Federal Bureau of Investigation (FBI).[8] The left panel of Figure 2 compares the 1920 homicide arrest data from Monkkonen (2005) with our 1921 measures of homicide from the *Mortality Statistics*. The correlation between the two samples is 0.848. The right panel compares FBI UCR data on homicides from 1930 to 1936 with our measure of homicides from the *Mortality Statistics* in the same

[8]We thank Price Fishback for sharing a digitized copy of the early UCR data with us (Fishback et al. 2010).



(a) Monkkonen Data **(b)** FBI UCR Data

Figure 2: The left panel shows homicide arrests in 1920 from the Monkkonen (2005) data against homicide deaths in 1921 from the *Mortality Statistics* data, both in logs. The Monkkonen (2005) data ends in 1920 and the mortality homicide data begins in 1921. The 11 overlapping cities included in both sources are graphed. The correlation between the two samples is 0.848. The right panel shows homicides from 1930 to 1936 from the FBI UCR data against homicide deaths from 1930 to 1936 from the mortality data, both in logs. The correlation between the two samples is 0.941. Comparing the mortality data on homicides with both datasets suggests that during our sample period of 1921 to 1936, the mortality data accurately measure homicides.

years. The correlation between these samples is 0.941. Both panels reveal a strong and tight relationship between the alternative measures and sources of homicide data and our measure of homicide from the *Mortality Statistics*, increasing our confidence in the accuracy of the mortality-based measures.[9]

A second known issue with the *Mortality Statistics* concerns sample selection. Although the U.S. Death Registration Area was created in 1880, it initially encompassed only two states—Massachusetts and New Jersey—and twenty cities (Eckberg 2006). While the registration area grew over time to include additional states, it did not cover the entire country until 1933. Fortunately, the lack of complete coverage at the state level is not relevant to our study. For our purposes, it is the completeness of coverage for cities that matters. Cities typically entered the registration area, and thus the *Mortality*

[9] Our variation in lead exposure is cross-sectional. This makes the Monkkonen data, which track a small number of cities, less useful for our purposes than the *Mortality Statistics*. The FBI data also have disadvantages: the FBI began releasing UCR data only in 1930, and the accuracy of these data "has long been questioned" (Eckberg 2006, p. 5-214). See also Gottschalk (2006, p. 24).

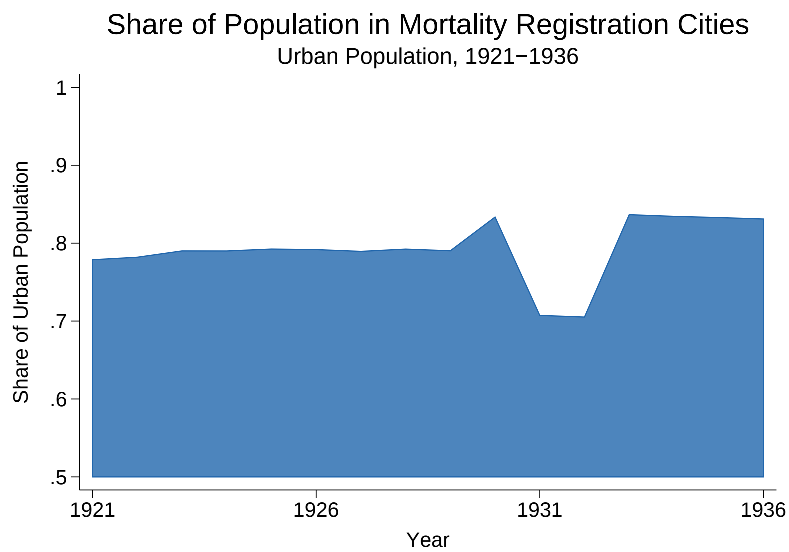


Figure 3: Share of the national urban population covered by the registration cities of the *Mortality Statistics*. Selection of states into the *Mortality Statistics* sample is a well-known problem. However, the coverage of cities in the sample is relatively complete and stable throughout our sample period. The 1931 and 1932 *Mortality Statistics* contain an abbreviated list of cities. The urban population is taken from the decennial census and linearly interpolated between 1920, 1930, and 1940.

Statistics, much earlier than entire states. In Figure 3, we plot the population of the cities in our sample against the national urban population.[10] The share of the urban population covered by the mortality data starts at 78% in 1921 and increases over time.[11] By the end of our sample period, approximately 83% of the national urban population lived in a registration city. Of the largest 100 cities in the country in 1920, 96 are recorded in our data in 1921.[12] Thus our sample encompasses the vast majority of American cities in the early twentieth century.

The changing registration area can complicate a time-series analysis, as increases in the national murder rate could be driven by new entrants to the

[10] Population data for both the cities and the nation are drawn from the decennial censuses and linearly interpolated between census years.

[11] The share decreases in 1931 and 1932, when the *Mortality Statistics* reported an abbreviated list of cities.

[12] The four cities missing from the Registration Data in 1921 are Des Moines, IA; Fort Worth, TX; Tusla, OK; and Sioux City, IA. All of the 100 largest cities in the country in 1930 are included in the 1930 Registration Data sample.

dataset rather than real changes in violence.[13] However, in our analysis of the effects of lead exposure on crime, the variation of interest comes not from changes over time but from differences between cities that did and did not use lead pipes. Moreover, as we demonstrate below, our results are robust to restricting our sample to a balanced panel of cities with mortality data in every year of the study period.

2.1 Which cities used lead pipes?

An important concern in a cross-sectional study such as ours is that the cities that adopted lead pipes in the nineteenth century might have differed from the cities that did not in ways that were correlated with their homicide rates. Based on available data on the characteristics of the cities in our sample, we find little evidence that this is the case.

In Table 1, we use data from the 1900 Integrated Public Use Microdata Series (IPUMS) sample of the census to predict which cities used lead pipes.[14] The table shows that there is no statistically significant relationship between a city's probability of using lead pipes and its black population share, its foreign-born population share, its average occupational score, its rate of homeownership, or its share of women. Although larger cities and cities with a greater share of men aged 18 to 40 were more likely to use lead pipes, we control for both of these variables in the regressions reported below.[15]

[13]The early states in the registration area tended to be from the Northeast and upper Midwest. In the early twentieth century, southern and western states had higher murder rates than northeastern and upper midwestern states. As southern states joined the registration area over time, the average homicide rate grew, driven in part by this composition effect. The extent of the composition effect has figured prominently in debates over the effect of national alcohol prohibition on the U.S. homicide rate (Miron 1999; Owens 2011).

[14]The IPUMS 1900 sample is a 5% sample of the census which indicates the city of residence of each respondent if the respondent lived in a city. We aggregate the demographic and economic data from each individual response to the city level to calculate city covariates.

[15]Men aged 18 to 40 are more likely than other groups to be involved in crime (Sampson and Laub 2003). Population size is typically positively correlated with crime rates (Blum-

There are two additional reasons why it is unlikely that the cities that used lead pipes would have had comparatively high homicide rates had they used iron instead. First, lead was more expensive than galvanized or wrought iron. City officials chose lead because it is more malleable and durable than iron (Troesken and Beeson 2003). Malleable pipes can be bent around obstacles, reducing labor and materials costs. Durable pipes require fewer repairs and need to be replaced less frequently than less durable pipes. Because of lead's comparatively high price, the cities that used lead typically were better off than the cities that did not.

Second, as shown in Figure 1, southern cities were less likely to use lead than other cities. Southern cities have had considerably higher homicide rates than comparable cities for as long as homicide statistics have been collected (Nisbett and Cohen 1996).[16] Thus we expect not only that the cities that used lead pipes would have had lower homicide rates than comparable cities in the absence of the treatment; we also expect lead to be positively correlated with violent crime only conditional on controls for latitude and longitude, which capture regional variation in homicide.

Figure 4 depicts the average residualized homicide rate in southern and non-southern cities over our sample period.[17] The figure makes two things clear. First, southern homicide rates are considerably higher than non-southern homicide rates. This can be seen by comparing the scales of the two panels. Second, cities that used lead water pipes had higher homicide rates than cities

stein 2000, p. 35–39), but only in cross-sectional studies (Rotolo and Tittle 2006). Less is known about the association of these covariates and homicide rates in the early twentieth century.

[16]Some scholars trace regional differences in homicide to the persistence of a "culture of honor" introduced by early migrants to the South (Nisbett and Cohen 1996; Grosjean 2014), although this explanation is not without critics (Elster 2007, p. 363).

[17]We generated the trend lines by capturing the residuals after regressing the logged homicide rate on controls for latitude and longitude. To ensure that the trend lines are comparable, we restricted the sample to a balanced panel of cities reporting homicides in every sample year.

Table 1: Characteristics of Cities Using Lead Pipes in 1897

	(1)	(2)	(3)	(4)	(5)
Log Population, 1900	0.109***			0.103***	0.109***
	(0.018)			(0.018)	(0.024)
City Latitude		0.014**		0.009	0.007
		(0.006)		(0.006)	(0.010)
City Longitude			0.004**	0.002	0.004**
			(0.002)	(0.002)	(0.002)
Black Population Share, 1900					−0.002
					(0.002)
Foreign-born Population Share, 1900					−0.002
					(0.002)
Occupation Score, 1900					−0.022
					(0.025)
Home Ownership Rate, 1900					−0.001
					(0.002)
Share Men 18-40, 1900					0.018*
					(0.011)
Share Women, 1900					0.002
					(0.011)
Observations	568	568	568	568	500
Adjusted R^2	0.060	0.009	0.008	0.065	0.062

Linear probability model predicting whether a city used lead pipes. The results are consistent with results from a probit or logit model. The lead pipe variable indicates that the city or municipal water supply used of lead pipes according the *1897 Manual of America Water-Works*, either exclusively or in addition to other pipe metals. In the sample included in the first four columns, 54.2% of cities are coded as using only lead pipes or some lead pipes. In the fifth column, 57.2% of the cities are coded as using only lead or some lead. Population in 1900 is measured using the Census. All control variables are calculated in 1900 by aggregating the IPUMS 5% census sample. All population share variables are measured as percentages. Increases in city latitude (longitude) indicate that a city is located farther north (east).

Sources: 1900 Census; 1900 IPUMS 5% Census Sample; Baker (1897)

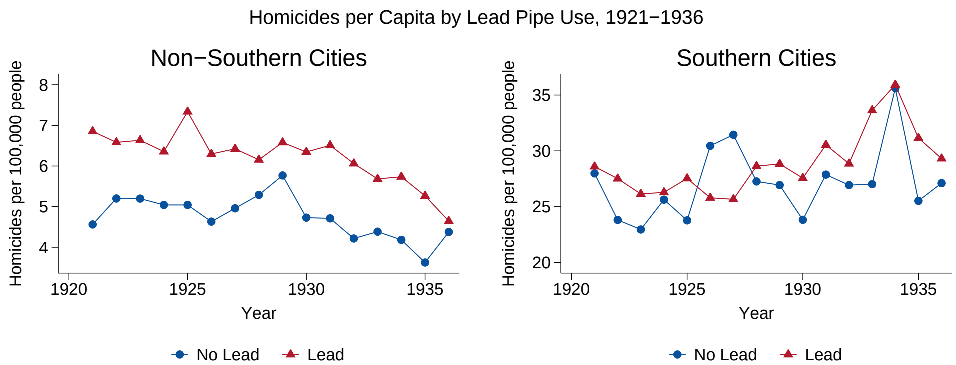


Figure 4: Residualized homicide rate in southern and non-southern cities. Outside of the South, cities with lead pipes had higher homicide rates than cities without lead pipes, as documented in the figure on the left. Homicide rates also trended down over the sample period outside of the South, both for cities with and without lead pipes. For southern cities, the differences between lead and non-lead cities are less striking and there are no clear time trends. The trend lines are constructed by capturing the residuals after regressing logged homicide rate on controls for latitude and longitude. The sample is restricted to a balanced panel of cities reporting data to the *Mortality Statistics* in each year between 1921 and 1936.

that did not, particularly in non-southern cities. In the South, the differences between the two sets of cities are less stark: although homicides rates are higher in cities using lead pipes in 13 of the 15 years in our sample, these differences are smaller relative to the average homicide rate in southern cities. It is possible that other causes of homicide in the South overwhelmed the effect of lead exposure. Because of regional variation in both the use of lead water pipes and in homicide rates, we include geographic controls in all of our regressions and introduce an instrumental variables identification strategy to account for the selection of high-homicide southern cities out of lead pipe use.

3 Results

3.1 Empirical Strategy

We begin our analysis by estimating regressions of the form

$$y_{i,t} = \lambda_t + \beta Lead_i + \delta Pop_{i,t} + \gamma X_i + \epsilon_{i,t}, \tag{1}$$

where y is the natural log of homicides plus one per capita in city i in year t, *Lead* is an indicator variable scoring one if the city's pipes contained some lead, *Pop* is the natural log of the city population in year t, λ is a vector of year fixed effects, and X is a column vector of city-level controls for the black population share, the foreign-born population share, the mean occupation score, the homeownership rate, the share of men 18–40, and the share of women, all measured in 1900, as well as the city's latitude and longitude. Homicide rates measured in levels are highly left-skewed. Using the natural log of homicides plus one per capita yields an approximately normal distribution.[18] With the outcome in logs, β can be interpreted as a semi-elasticity. We cluster our standard errors at the city level because we observe lead pipe or iron pipe use once, at the turn of the twentieth century (Baker 1897). Clustering adjusts our inference to account for the repeated observations of cities with a constant pipe type in multiple years.

If cities' adoption of lead pipes were uncorrelated with all other determinants of the homicide rate, β would capture the effect of lead exposure through water on the homicide rate. However, if the cities that had relatively high homicide rates typically selected out of lead-pipe use, β will be biased downward. For instance, southern cities, which had the highest murder rates of all regions, were less likely than other cities to use lead pipes. To address the potential downward bias in our estimates induced by this selection, we introduce an instrument that is correlated with lead pipe use but plausibly uncorrelated with other city characteristics related to crime.[19]

[18] See Figure A.1 in the appendix for the distribution of homicide rates.

[19] In the appendix, we show that our instrumental variables results are robust to controlling for whether a city was located within five miles of a lead refinery, which might have caused

As discussed in the previous section, the cities that used lead pipes did so despite lead's high price. Transportation costs rose with the distance from pipe supplier to city (Gross 2014).[20] In 1884, H. W. Richards, Superintendent of the New England Water Works Association, noted that the cost of pipe depended on "labor, freight, cartage, stop-box and paving, the cost of which will vary in different places" (New England Water Works Association 1885, p. 47). The local availability of smelted lead thus could have lowered its price and increased the likelihood that cities used lead pipes (Clay et al. 2014).

To test this proposition, we collect data from Ingalls (1908) on the locations of lead smelting facilities and refineries. We geolocate these refineries and calculate the distance of each city in our data from the nearest refinery. Figure 5 maps the 14 lead smelters and refineries in the United States as of 1899.[21] In Figure 6, we plot the relationship between a city's distance to the nearest lead refinery and its probability of using lead. The figure shows that the farther a city was from a lead refinery, the less likely it was to use lead water pipes. This relationship holds both unconditionally and conditional on geographic and demographic controls. In the next section, we report estimates of the effect of lead pipe use on homicide rates, using each city's distance from the nearest lead refinery as an instrumental variable.

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it to be polluted through means other than lead water pipes. We also experiment with an identification strategy introduced by Clay et al. (2014), wherein lead exposure is driven by the interaction of a city's use of lead pipes and the pH of its water. The results in the appendix confirm that the interaction of lead pipes and acidic water (lower pH levels) is positively correlated with higher homicide rates.

[20] Gross (2014), drawing on historical point-to-point railroad freight rates, shows that freight rates rose linearly with distance for both short- and long-haul routes in the early twentieth century.

[21] There was a smelter in both Newark, NJ and Perth Amboy, NJ, although the two points are difficult to distinguish on the map.

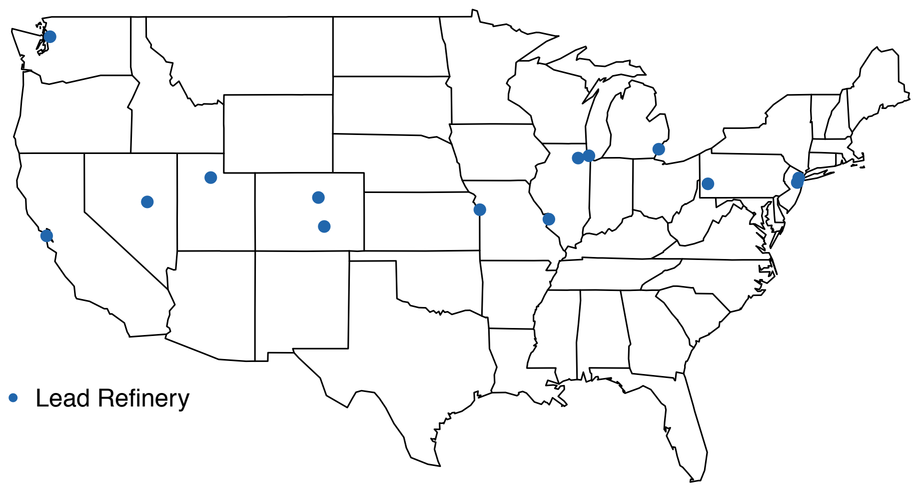

Figure 5: Location of lead smelters and refineries in 1899. We collect location data from Ingalls (1908) and identify 14 lead smelters and refineries. We expect the cost of lead pipes to be lower in cities located closer to the lead smelters and refineries because of reduced transport costs.

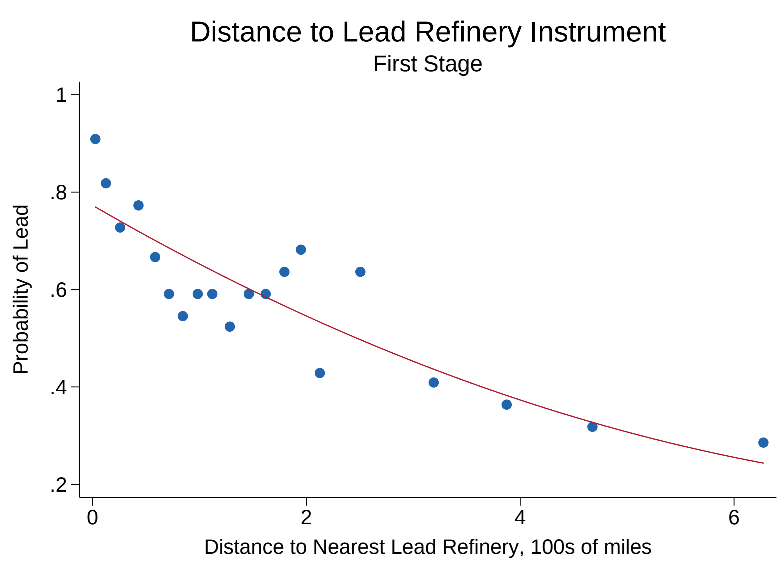

Figure 6: Binned scatter plot of the relationship between a city's probability of using lead and its distance to the nearest lead refinery in 1899. Each point represents 5% of the data, cut by the distance to the nearest refinery, and is plotted at the mean probability of using lead and of distance within the group. Cities farther from lead refineries were less likely to use lead pipes. 90% of cities with local lead refineries used lead pipes. Only half of the cities located approximately 200 miles from a refinery could be expected to use lead pipes. Lead refinery locations in 1899 were collected from Ingalls (1908).

3.2 Mortality Registration Area Data

Table 2 presents our main results. Column 1 shows the effect of lead pipe use on the log homicide rate, conditional on controls for the log population, latitude and longitude, and year fixed effects. We find that cities' lead pipe use increased their murder rates by 11.4% when controlling only for city size and geography. However, the effect of lead on the murder rate shrinks to 8.6% when we include a full set of controls for city demographics in 1900.

Southern cities selected out of lead pipe use, biasing downward the OLS estimates reported in columns 1 and 2. In columns 3–6, we present our instrumental variables estimates. With the full set of demographic controls included, these estimates should be compared to the OLS estimates reported in column two. Column 3 shows the the first stage of the instrumental variables regression using distance to a refinery to predict lead pipe use. The small standard error on the distance coefficient and large F-statistic suggest that distance is a strong instrument. The F-statistic with a linear distance instrument is 24.7.[22]

Figure 6 indicates that there might be a slight nonlinearity in the relationship between a city's distance to a refinery and its probability of using lead. In column 4 we include terms for both distance and distance squared. The quadratic specification of the instrument remains a strong predictor of lead pipe use, if slightly weaker than the linear specification. The F-statistic for the quadratic distance instrument is 13.6.

Columns 5 and 6 report the results of the second stage of the instrumental variables regressions. As expected, because of the selection of high-homicide cities out of lead pipe use, the instrumental variables estimates are much larger than the OLS estimates. The estimated effect, shown in columns 5 and 6,

[22] We present the partial F-statistics for our instruments as recommended by Angrist and Pischke (2009).

Table 2: OLS and IV Effects of Lead Pipes on Murder Rates from 1921 to 1936

	OLS		First Stage		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Lead Pipes	0.113**	0.086**			0.425**	0.476**
	(0.050)	(0.043)			(0.201)	(0.205)
Lead Refinery Distance (100 miles)			−0.102***	−0.136***		
			(0.020)	(0.041)		
Lead Distance Squared (100 miles)				0.007		
				(0.007)		
Log Population	−0.173***	−0.115***	0.056**	0.056**	−0.138***	−0.141***
	(0.028)	(0.030)	(0.022)	(0.022)	(0.031)	(0.031)
Black Population Share, 1900		0.023***	0.004	0.004*	0.023***	0.023***
		(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Foreign-born Population Share, 1900		−0.012***	0.001	−0.000	−0.012***	−0.012***
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Occupation Score, 1900		−0.017	0.002	0.004	−0.012	−0.011
		(0.026)	(0.029)	(0.030)	(0.028)	(0.028)
Home Ownership Rate, 1900		−0.001	0.002	0.002	−0.002	−0.002
		(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Share Men 18-40, 1900		0.019*	0.005	0.004	0.017	0.017
		(0.010)	(0.012)	(0.012)	(0.011)	(0.011)
Share Women, 1900		−0.024**	−0.011	−0.010	−0.020	−0.019
		(0.011)	(0.012)	(0.012)	(0.012)	(0.013)
Year Fixed Effects	Yes	Yes	No	No	No	No
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6542	6256	6256	6256	6256	6256
Clusters	464	436	436	436	436	436
F-Statistic			24.73	13.57		
Adjusted R^2	0.369	0.490	0.113	0.114	0.452	0.441

Standard errors are clustered at the city level. The murder rate is measured as the natural log of homicides plus one per capita. The lead pipe variable indicates that the city or municipal water supply consisted of lead pipes according the *1897 Manual of America Water-Works*, either exclusively or in addition to other pipe metals. Population, in logs, is measured contemporaneously to the murder rate. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. All population share variables are measured as percentages. Geographic controls include latitude and longitude. The instrument, distance to the nearest lead refinery, is measured as the great circle distance from the city to closest lead refinery operating in 1900, according to Ingalls (1908).

Sources: Vital Statistics of the U.S. from 1921 to 1936; 1900 Census; 1900 IPUMS 5% Census Sample; Baker (1897); Ingalls (1908)

suggests lead pipes increased the murder rate by 42% and 47%, respectively.

Because our variation of interest is cross-sectional, the fact that cities selectively entered the *Mortality Statistics* poses no problem for our analysis. Still, for robustness, in Table 3 we report the same results reported in Table 2, this time restricting our sample to those cities that contributed mortality data in every year of our sample. Reassuringly, these results mirror those estimated using the full sample, although the F-statistics are smaller in the balanced sample. Cities' distance to the nearest lead refinery was a strong predictor of whether they used lead pipes. Lead pipe use, in turn, increased cities' homicide rates by more than half.

Figure 7 shows the results of regressions estimated using different functional-form measurements of the murder rate—logs without adding one and square roots—as well as different sample restrictions. Dots show estimates from cities that reported at least one homicide over the entire sample period. Triangles depict estimates from the full sample of cities. Squares show the results when the outcome is logged, thereby excluding all homicide rates of zero. The error bars represent 95% confidence intervals. "IV1" and "IV2" refer to the linear and quadratic specifications, respectively, of the instrumental variables regressions. The figure shows that four of the fifteen estimates overlap slightly with zero, with p-values ranging from .06 to .13. However, the point estimates are all positive and generally of similar magnitudes within the OLS and IV specifications. According to these estimates, cities' use of lead water pipes increased their homicide rate by between 0.8 and 4.4 homicides per 100,000 people on average.

Table 3: OLS and IV Effects of Lead Pipes on Murder Rates from 1921 to 1936, Balanced Panel of Cities

	OLS		First Stage		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Lead Pipes	0.163**	0.076			0.562*	0.638**
	(0.070)	(0.058)			(0.313)	(0.317)
Lead Refinery Distance (100 miles)			−0.082***	−0.146***		
			(0.026)	(0.051)		
Lead Distance Squared (100 miles)				0.012		
				(0.009)		
Log Population	−0.045	0.013	0.052*	0.049*	−0.022	−0.027
	(0.036)	(0.034)	(0.028)	(0.028)	(0.041)	(0.041)
Black Population Share, 1900		0.025***	0.007*	0.007**	0.024***	0.024***
		(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Foreign-born Population Share, 1900		−0.013***	0.001	−0.000	−0.013***	−0.013***
		(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Occupation Score, 1900		−0.044	0.019	0.027	−0.043	−0.043
		(0.041)	(0.042)	(0.042)	(0.048)	(0.050)
Home Ownership Rate, 1900		−0.001	0.004	0.004	−0.004	−0.004
		(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Share Men 18-40, 1900		0.023*	0.008	0.006	0.021	0.020
		(0.014)	(0.017)	(0.017)	(0.018)	(0.018)
Share Women, 1900		−0.032**	−0.005	−0.005	−0.027	−0.026
		(0.016)	(0.018)	(0.017)	(0.020)	(0.021)
Year Fixed Effects	Yes	Yes	No	No	No	No
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4112	4000	4000	4000	4000	4000
Clusters	257	250	250	250	250	250
F-Statistic			9.99	6.74		
Adjusted R^2	0.373	0.525	0.110	0.115	0.457	0.435

Standard errors are clustered at the city level. In contrast to Table 2, this sample is restricted to those cities observed in the mortality data in every year from 1921 to 1936. The murder rate is measured as the natural log of homicides plus one per capita. The lead pipe variable indicates that the city or municipal water supply consisted of lead pipes according the *1897 Manual of America Water-Works*, either exclusively or in addition to other pipe metals. Population, in logs, is measured contemporaneously to the murder rate. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. All population share variables are measured as percentages. Geographic controls include latitude and longitude. The instrument, distance to the nearest lead refinery, is measured as the great circle distance from the city to closest lead refinery operating in 1900, according to Ingalls (1908).

Sources: Vital Statistics of the U.S. from 1921 to 1936; 1900 Census; 1900 IPUMS 5% Census Sample; Baker (1897); Ingalls (1908)

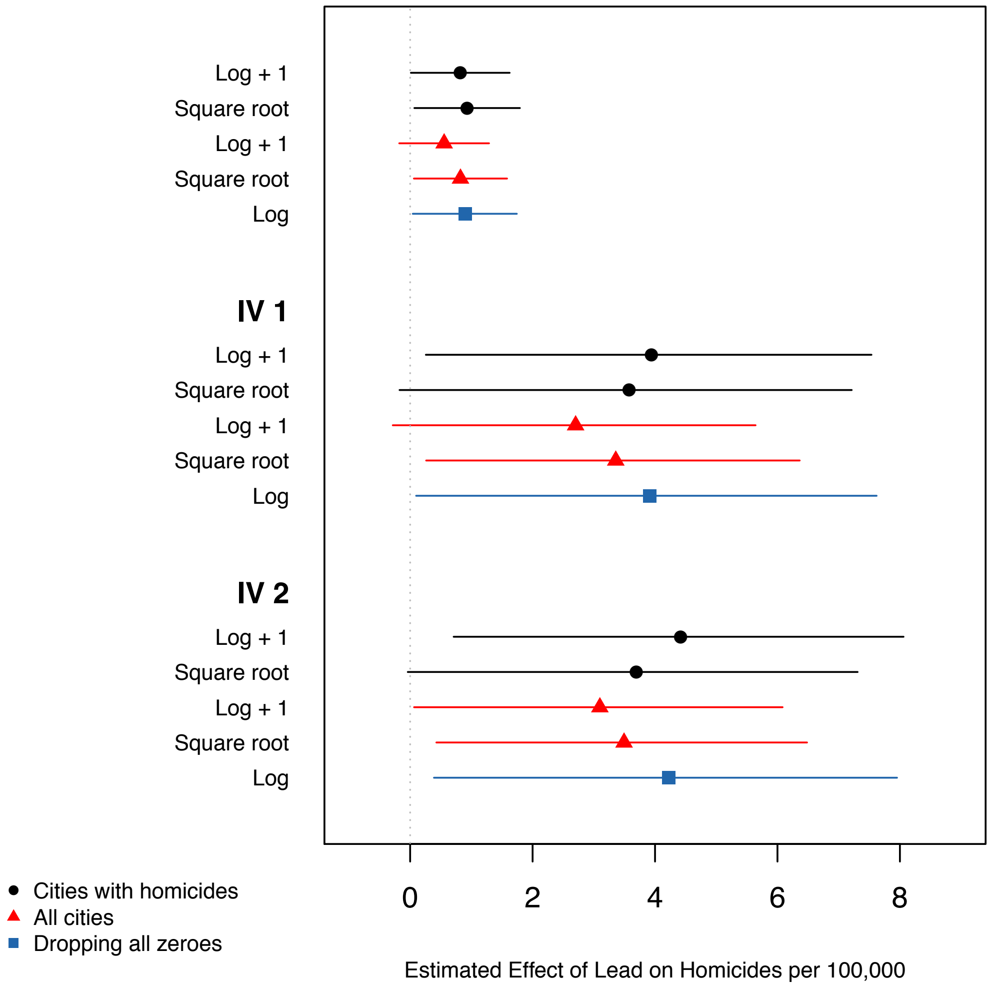

Figure 7: Estimated effect of lead on homicides per 100,000 people. Dots show estimates from cities that reported at least one homicide over the entire sample period. Triangles depict estimates from the full sample of cities. Squares show the results when the outcome is logged, thereby dropping all homicide rates of zero. The error bars represent 95% confidence intervals. **IV1** refers to instrumental variables regressions using cities' distance from a lead refinery as an instrument. **IV2** refers to instrumental variables regressions using cities' distance from a lead refinery and distance squared as instruments. All covariates are held at their sample means. Across all models and specifications, cities with lead pipes are estimated to have higher homicide rates than cities without lead pipes.

3.3 Falsification

A city's use of lead pipes should have increased its homicide rate by shifting the distribution impulsivity, ADHD, and IQ in the population. But lead water pipes should have had no bearing on death rates from causes unrelated to these individual-level effects. Table 4 shows the relationship between a city's use of lead pipes and its death rate from several causes: cirrhosis, suicide, heart disease, pneumonia, tuberculosis, auto accidents, influenza, diabetes, childbirth, syphilis, whooping cough, measles, typhoid, scarlet fever, train accidents, and malaria. As expected, many of these estimates are small and negative, and very few are statistically distinguishable from zero. Only the coefficients on death from cirrhosis and train accidents were large, positive, and statistically significant. Although these results could be due to the same underlying mechanisms that pushed up the homicide rate in cities with lead water pipes, they also could be the chance outcome of multiple statistical tests. In short, there is little evidence that lead exposure was positively correlated with mortality rates generally and thus is spuriously related to deaths from homicide.

We focus our analysis on lead exposure rather than lead poisoning, which was exceedingly rare. For example, in 1921, only 142 people in the entire registration area died of chronic lead poisoning—a rate of roughly 2 per million people. By the end of our sample period, 1936, that number had dropped to 132, despite growth in the population. With so few deaths, the *Mortality Statistics* do not report city-level deaths from lead poisoning, only national totals. Thus, we are unable to estimate the effects of lead use on deaths from lead poisoning.

An alternative falsification exercise entails examining the relationship between lead pipe use and homicide in the pre-treatment period. Most municipal

Table 4: Effects of Lead Pipes on Other Causes of Death, 1921 to 1936

	Estimated Effect of Lead Pipe Use				
Cause of Death	β	SE	N	Clusters	Deaths per 10,000
Cirrhosis of the Liver	0.106***	0.037	5932	498	0.909
Suicide	−0.008	0.026	6524	498	1.444
Circulatory Disease	−0.045	0.028	6940	498	23.200
Pneumonia	−0.024	0.030	6940	498	9.312
Tuberculosis of the Lungs	0.015	0.045	6898	498	4.723
Auto Accident	−0.021	0.040	5901	498	2.750
Influenza	0.064	0.040	6695	498	2.346
Diabetes	−0.037	0.031	6828	498	2.340
Death in Childbirth	−0.022	0.034	6480	497	1.648
Syphilis	−0.032	0.054	2401	479	1.013
Whooping Cough	−0.015	0.032	4601	488	0.615
Measles	−0.000	0.031	3430	489	0.532
Typhoid	−0.001	0.042	4280	476	0.522
Scarlet Fever	0.031	0.040	3605	475	0.388
Train Accident	0.105**	0.044	2304	449	0.316
Malaria	−0.001	0.105	1179	258	0.291
Total Mortality	−0.027	0.024	6940	498	134.055

Standard errors are clustered at the city level. Each row reports the result of a different OLS regression with the listed cause of death as the dependent variable. All death rates are measured as the natural log of deaths from the specified cause per capita. Deaths in child birth include puerperal fever. Deaths from heart disease include all deaths attributed to circulatory disease. The main independent variable is the lead pipe indicator, which reports whether the city or municipal water supply consisted of lead pipes according the *1897 Manual of America Water-Works*, either exclusively or in addition to other pipe metals. City-level demographic control variables include population, in logs, measured contemporaneously to the death rates, the black share of the population, the foreign-born share, the mean occupation score, the home ownership rate, the share of men between 18 and 40, and the share of women in the population. Other than population, all demographic control variables are measured in 1900 by aggregating the IPUMS 5% census sample. All population share variables are measured as percentages. Geographic controls include latitude and longitude. The sample size and number of clusters both fluctuate between different causes of death. Some causes of death are not reported in all sample years and other causes are never reported in some cities.

Sources: Vital Statistics of the U.S. from 1921 to 1936; 1900 Census; 1900 IPUMS 5% Census Sample; 1897 Manual of American Water-Works

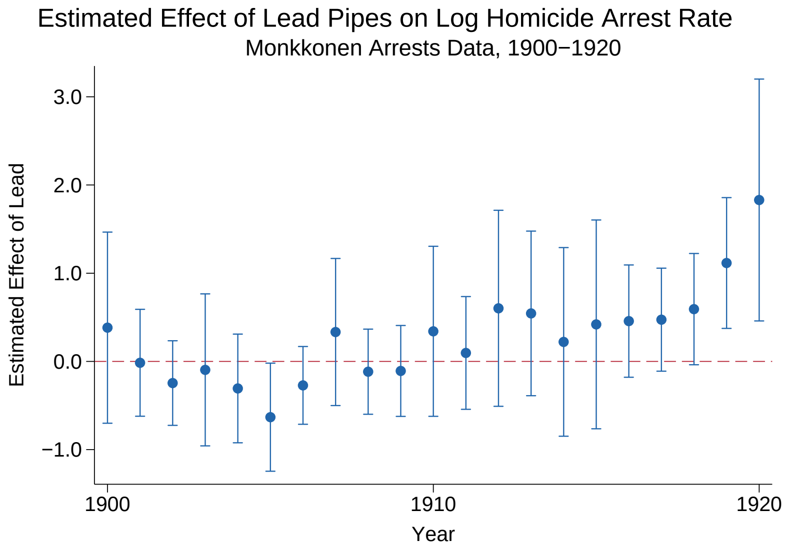

Figure 8: The effect of lead exposure on homicide in the pre- and post-treatment periods. Homicide arrest data are from Monkkonen (2005). Each point represents the estimated effect of lead on the annual homicide rate, controlling for log city population, latitude, longitude, city-level demographic controls as of 1900, and year fixed effects. In the 21-city sample included in the Monkkonen (2005) data, we see that lead effects are not present until later in the sample and only statistically significant in 1919 and 1920.

water systems were introduced in the late nineteenth century. Using data in Baker (1897), we identify the year of water system construction for 277 cities with lead service pipes.[23] Fewer than one-quarter of those cities (66) had installed any lead pipes before 1870. The plurality of lead pipe systems (108) were installed during the 1880s, with another 32 added in the 1890s.

The biochemical effects of lead are slow moving and should only be observable in our homicide outcomes after a considerable lag. Lead has the strongest effects on developing children, in whose bodies it builds up over time. In most cities, with lead pipe systems installed during the 1880s or 1890s, children young enough to have suffered the consequences of early lead exposure would have entered adulthood in roughly the first and second decades of the twentieth century. Thus we should observe the effects of lead exposure only in this

[23]Only slightly more than half of the water systems described in Baker (1897) include data on construction dates.

later period.

Figure 8 shows the effect of cities' use of lead pipes on homicide arrest rates in the small number of cities for which Monkkonen (2005) collected data. We regress homicide arrest rates on the lead water pipe indicator, controlling for log city population, latitude, longitude, city-level demographic controls as of 1900, and year fixed effects. As above, the standard errors are clustered at the city level. Consistent with our expectations, we find that a city's use of lead water pipes was strongly related to its homicide arrest rate only in the later years of the Monkkonen (2005) sample.[24] Although these results are suggestive, given the small number of cities included in the dataset, they should be treated with caution.

4 Conclusion

In this paper, we draw on exogenous variation in the use of lead service pipes in U.S. cities in the late nineteenth and early twentieth centuries to estimate the effect of lead exposure from water on city-level homicide rates. Our results suggest that cities' use of lead water pipes increased their homicide rates considerably.

The advantage of studying the effects of lead historically is threefold. First, city officials decided whether to use lead or iron pipes with little regard for the negative effects of lead on health. We find scant evidence that the cities we would expect to have high homicide rates for reasons unrelated to lead exposure selected into lead pipe use. Instead, it appears that the cities that adopted lead water pipes would otherwise have had comparatively low homicide rates. A city's distance from the nearest lead refinery, which we use as a proxy for

[24] Between 1874 and 1876, the effects of lead are also significant, but these effects are quite small and are likely driven by chance.

the cost of lead pipes, was a strong predictor of whether cities used lead pipes. For these reasons, we believe our results lend further support to the conclusion that lead exposure increases violent crime.

Second, the magnitude of our results is commensurate to the strength of the treatment. Individuals coming into contact with lead through city water suffered much higher average doses of lead than individuals exposed to lead paint chips or air lead in the late twentieth century. Our estimates imply that the cities that used lead water pipes at the dawn of the twentieth century had between 0.8 and 4.4 additional murders per 100,000 relative to comparable cities. Previous studies on prohibition (Miron 1999), "cultures of honor" (Grosjean 2014), and lead exposure from gasoline (Reyes 2007) report large effects on violent crime as well.

Third, a city-level analysis such as ours does not suffer from a common problem with individual-level studies, namely that individuals exposed to lead typically come from poor neighborhoods with both low-quality housing and high crime rates. Typical confounders of the lead-crime relationship in individual-level studies pose less of a problem in our city-level study, where the entire city population was exposed to lead through water.

Still, our study does have limitations. Our results were somewhat sensitive to our choice of models and samples. Four of the fifteen estimates reported in Figure 7 have p-values between .06 and .13, pushing them below statistical significance. Moreover, the precise mechanisms through which the individual-level effects of lead exposure contribute to aggregate homicide rates and interact with social determinants of violence have yet to be fully specified. Thus, before drawing strong conclusions about the causal effects of lead exposure on violent crime, scholars should conduct more research with strong identification strategies using data on individuals as well as aggregate units.

Finally, we stress that if lead exposure does increase violent crime, it is one cause among many. Future research should consider the importance of lead exposure relative to other causes discussed in the criminological, economic, and sociological literatures, as well as the ways that these causes might jointly affect violence and crime.

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A Appendix

A.1 Measuring Homicide

Our main outcome of interest in this paper is the murder rate. We measure murder rates annually for each city in our sample using *Mortality Statistics* reports collected by the Commerce Department.

Modeling crime rates can be difficult, especially for rare crimes like murders. There are some cities in our sample without a homicide recorded in a given year, and a subset of these cities do not report a homicide during the entire sixteen-year sample. In Tables 2 and 3, we restrict our sample to those cities that report more than one homicide over the sixteen-year period.

Figure A.1 shows that murder rates are distributed in a highly left-skewed manner, with a large number of observations at or close to zero. Once we log transform the data, the distribution is much closer to normal. A simple log transformation of the data, however, will drop city-year observations with zero recorded homicides. If the relationship between lead and crime is fundamentally different in this subset of dropped cities, our results will be biased in an unpredictable direction. An alternative logging method that is frequently used in data with zeros is to add one to the data before taking the log. In this case, we add one to the total number of homicides in a given year, divide by population to get a per capita murder rate, and then log the rate.[25] The bottom right panel of Figure A.1 shows the distribution of homicide rates using this measure, which we use as our outcome in Tables 2 and 3.

[25] The maximum homicide rate per capita in our sample is 0.0015 or 15 homicides per ten thousand people. Adding one to these rates as opposed to the number of homicides distorts and does not normalize the data.

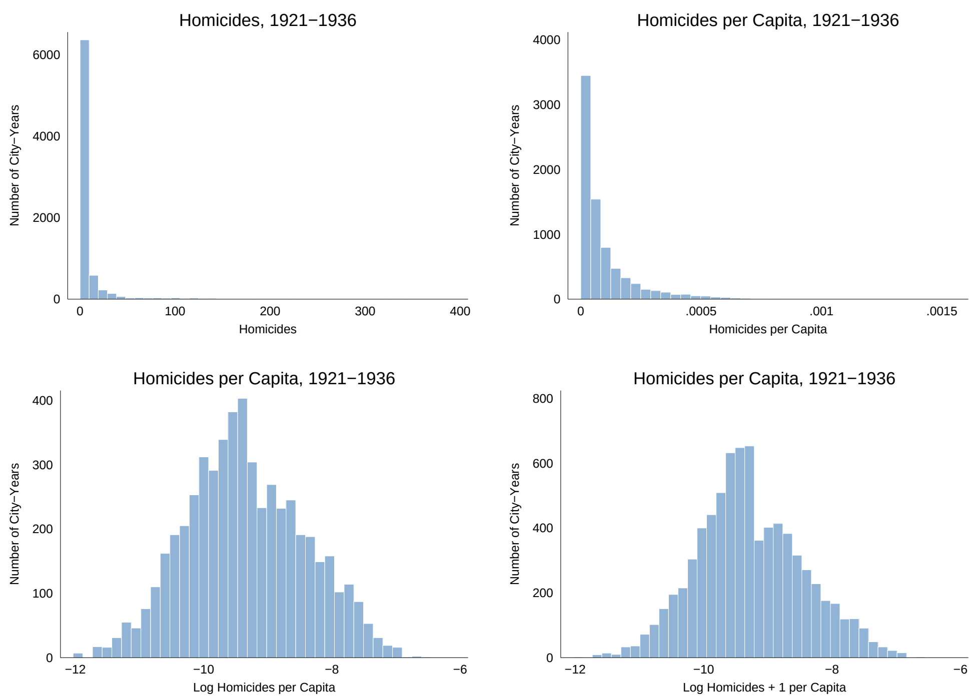


Figure A.1: Distribution of Homicides per Capita. When measured as the number of homicides or the number of homicides per capita, the distribution of outcomes is highly skewed. However, once the data are transformed with a log function, the distribution is approximately normal. Because there are cities in our sample that do not record a homicide in some years, we add one to the count of homicides before calculating the homicide rate and logging the variable. This transformation is also approximately normally distributed.

A.2 Lead Pollution from Refineries

One possible challenge to our instrumental variables estimates is that the exclusion restriction is violated. This would be the case if there were a direct effect of cities' distance from lead refineries on homicide above and beyond the effect of this distance on cities' probability of using lead water pipes. One way to address this concern is to include a control for whether a city was located within five miles of a lead refinery. If nearness to a lead refinery had a direct effect on a cities' homicide rates, this would invalidate our instrumental variables estimates. In Table A.1 we report results identical to those reported in Table 2, this time including a control for whether the city was located within five miles of a lead refinery. The table shows that our results are substantively unchanged by the inclusion of this control. Moreover, the coefficient on the variable indicating whether a city was within five miles of a lead refinery is not statistically distinguishable from zero in any of the specifications.

A.3 Alternative Identification: pH and Water Hardness

Clay et al. (2014) devise an alternative strategy for identifying the effects of lead exposure from water. Lead exposure was more severe in cities with acidic water, as more lead will leach into water with lower pH levels (Kim et al. 2011). Clay et al. (2014) collect data on the pH of city water in 1984 and find that, among cities with lead-only pipes, cities with higher pH had lower infant morality rates.

In this section, we replicate these results using homicide rates as our outcome. In the left panel of Figure A.2, we show that there is a negative relationship between pH and the log homicide rate in cities that used pipes with some lead.[26] This is consistent with the results for infant mortality reported in

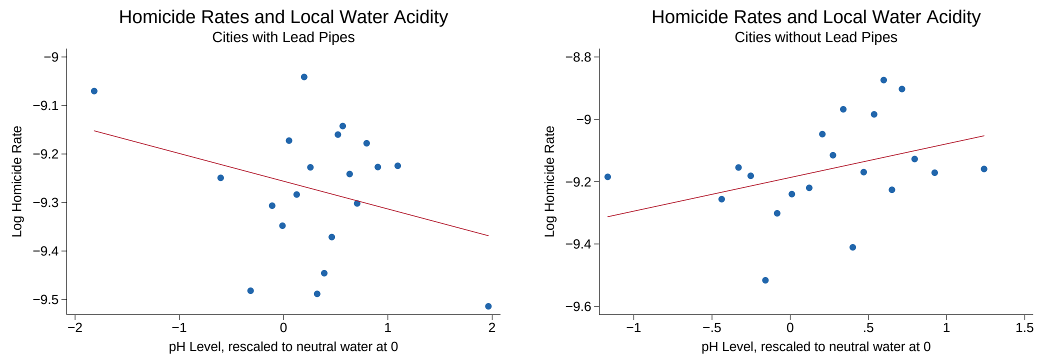
[26] In both binscatters, we control for the demographic and geographic variables included

Table A.1: OLS and IV Effects of Lead Pipes on Murder Rates from 1921 to 1936, Controlling for Cities with Lead Refineries

	OLS		First Stage		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Lead Pipes	0.108**	0.078*			0.357*	0.404*
	(0.051)	(0.043)			(0.214)	(0.216)
Lead Refinery Distance (100 miles)			−0.098***	−0.129***		
			(0.022)	(0.043)		
Lead Distance Squared (100 miles)				0.006		
				(0.007)		
Lead Refinery in City	0.157	0.225	0.085	0.066	0.154	0.142
	(0.193)	(0.162)	(0.093)	(0.095)	(0.187)	(0.189)
Log Population	−0.179***	−0.123***	0.053**	0.054**	−0.139***	−0.142***
	(0.028)	(0.029)	(0.022)	(0.022)	(0.030)	(0.030)
Black Population Share, 1900		0.023***	0.004	0.004	0.023***	0.023***
		(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Foreign-born Population Share, 1900		−0.013***	0.000	−0.000	−0.012***	−0.012***
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Occupation Score, 1900		−0.017	0.002	0.004	−0.013	−0.012
		(0.026)	(0.029)	(0.030)	(0.027)	(0.028)
Home Ownership Rate, 1900		−0.001	0.002	0.002	−0.002	−0.002
		(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
Share Men 18-40, 1900		0.019*	0.005	0.004	0.018*	0.018
		(0.010)	(0.012)	(0.012)	(0.011)	(0.011)
Share Women, 1900		−0.023**	−0.010	−0.010	−0.020*	−0.020
		(0.011)	(0.012)	(0.012)	(0.012)	(0.012)
Year Fixed Effects	Yes	Yes	No	No	No	No
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6542	6256	6256	6256	6256	6256
Clusters	464	436	436	436	436	436
F-Statistic			20.64	11.13		
Adjusted R^2	0.370	0.492	0.113	0.114	0.466	0.457

Standard errors are clustered at the city level. In contrast to Table 2, here we include a control for cities within 5 miles of a lead refinery. The murder rate is measured as the natural log of homicides plus one per capita. The lead pipe variable indicates that the city or municipal water supply consisted of lead pipes according the *1897 Manual of America Water-Works*, either exclusively or in addition to other pipe metals. Population, in logs, is measured contemporaneously to the murder rate. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. All population share variables are measured as percentages. Geographic controls include latitude and longitude. The instrument, distance to the nearest lead refinery, is measured as the great circle distance from the city to closest lead refinery operating in 1900, according to Ingalls (1908).

Sources: Vital Statistics of the U.S. from 1921 to 1936; 1900 Census; 1900 IPUMS 5% Census Sample; Baker (1897); Ingalls (1908)



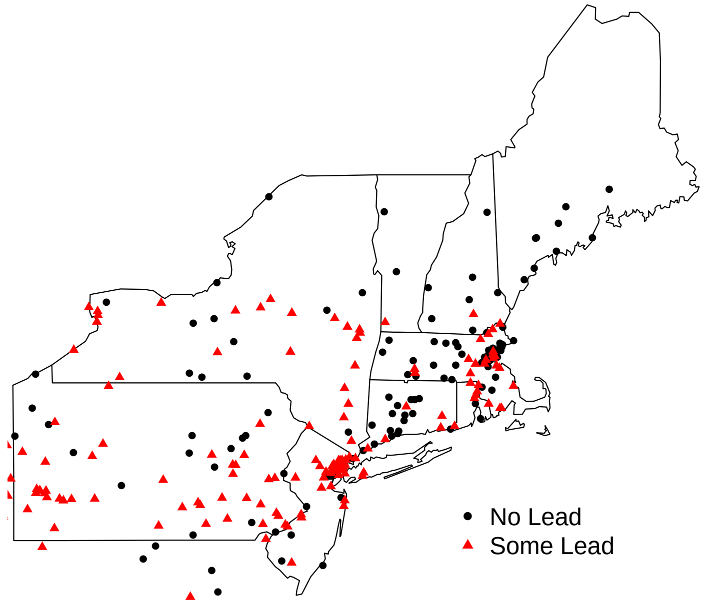
(a) Cities with Lead Pipes **(b)** Cities without Lead Pipes

Figure A.2: Binscatter of the relationship between pH levels and Homicide Rates

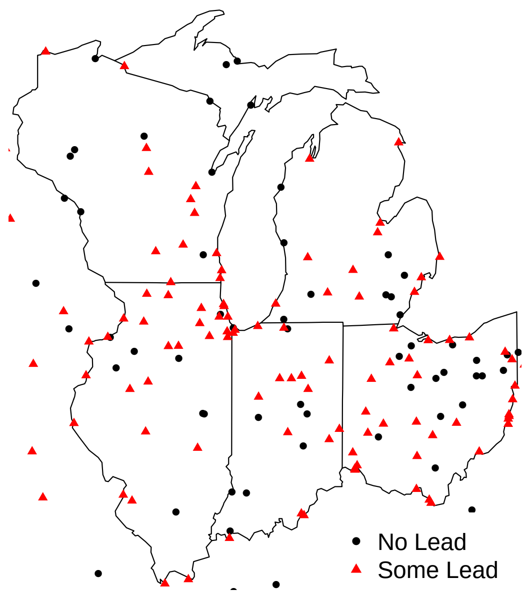
Clay et al. (2014). However, in the right panel of Figure A.2, we also observe a positive relationship between pH and homicide in cities with no lead pipes. Thus, in our data, the relationship between the cities' homicide rates and the interaction of their use of lead pipes and the pH of their water is driven by two separate relationships: the negative effect of pH in cities with pipes using some lead and the positive effect of pH in cities using pipes with no lead. Because we are unaware of any scientific literature to motivate the latter relationship, we prefer the identification strategy introduced earlier in the paper.

in our main specification.

A.4 Additional Maps



(a) Cities in the Northeast



(b) Cities in the Midwest

Figure A.3: Location of sample cities and the type of water pipe they used as of 1897. Data are drawn from *The Manual of American Water-Works* (Baker 1897). Cities with only lead pipes and cities with a mix of lead and iron pipes are included in the "some lead" category and marked with triangles. Cities using either galvanized iron or wrought iron are included in the "no lead" category and marked with circles.