Lead Exposure and Violent Crime in the Early Twentieth Century*

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March 22, 2016

Abstract

In the second half of the nineteenth century, many American cities built water systems using lead or iron service pipes. Municipal water systems generated 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 entire city populations to much higher doses of lead than have previously been studied in relation to crime. Our estimates suggest that cities' use of lead service pipes considerably increased city-level homicide rates.

JEL codes: Q53, N32, K42

*Authors are listed in alphabetical order and contributed equally. For detailed comments and suggestions, we thank Peter Bearman, Christopher Blattman, Douglas Eckberg, Neir Eshel, Claudia Goldin, Andrew Hall, Richard Hornbeck, David Hureau, Lawrence Katz, Shamus Khan, James Lee, Robert Margo, Jeffrey Miron, Suresh Naidu, Andrew Papachristos, Robert Sampson, Christopher Wildeman, Jon Zelner, and audience members at the Economic History Tea and Workshop in History, Culture, and Society at Harvard University, the University Seminar in Economic History and the Center for the Study of Wealth and Inequality Seminar at Columbia University, the Sociology Departments of Emory and Yale Universities, and the annual meetings of the Social Science History Association and the Robert Wood Johnson Foundation Health & Society Scholars Program. 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) and the Robert Wood Johnson Foundation Health & Society Scholars program.

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In the second half of the nineteenth century, cities and towns across the United States built and expanded municipal waterworks. The number of waterworks grew more than tenfold between 1870 and 1896, with more than one thousand new systems added from 1890 to 1896 alone (Cutler and Miller 2006, p. 169). Sanitation engineers, city officials, and urban boosters alike greeted the new water systems with enthusiasm. The presence of waterworks signaled that city dwellers lived in a "respectable community" (Melosi 2000, p. 82). Urban residents across the country witnessed "an immense change in the standard of living" (Baker 1897). The introduction of new waterworks eliminated the time and labor required to draw well water and improved fire protection. It also marked the abandonment of badly polluted surface wells (Baker 1897; Troesken 2006, p. 6). Contemporaries observed reductions in typhoid that recent scholarship 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 cities. In many cities, the very pipes installed to improve urban life and health leached noxious particulates into the local water supply.

A growing body of evidence in the social and medical sciences traces high crime rates to high rates of 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 state-specific reductions in lead exposure due to the Clean Air Act to estimate the effect of lead emissions from gasoline on violent crime. She reports 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] Stretesky and Lynch (2001) estimate that, from 1989 to 1991,

[1] Cook and Laub (2002, p. 24, 28) note that the relative uniformity of the timing of the rise and fall of violent crime across cohorts is inconsistent with the conclusion that lead removal was the primary cause of the crime drop. But this does not, they point out, mean that lead exposure has no effect on crime. Two recent studies reach different conclusions about the effect of reductions in gasoline lead on the crime decline. Using national time series data from 1973 to 2012, Lauritsen et al. (2016) find that lagged gasoline lead consumption is weakly correlated with national trends in homicide and serious violence, rape, robbery, and aggravated assault, as measured in the National Crime Victimization Survey. In contrast, using individual-level data from the National Longitudinal Survey of Youth, Reyes (2015) finds that the decline in air lead due to the Clean Air Act reduced childhood behavioral problems, teen pregnancy, aggression, and crime in adulthood. Given our historical focus, our conclusions are limited to determining whether lead exposure affects crime, not estimating how much that effect contributed to the late-twentieth-century reduction in crime.

counties with air lead levels equivalent to .17 μg/m^3 had homicide rates four times as high as counties with air lead levels equivalent to 0 μg/m^3. Mielke and Zahran (2012) show that air lead and aggravated 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 violent offenses (Wright et al. 2008). Cross-sectional research on individuals (Denno 1990; Needleman et al. 1996, 2002) and counties (Stretesky and Lynch 2004), studies using cross-national panel data (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 crime lies in the fact that its findings have been replicated at several scales. However, with the exception of Reyes (2007, 2015), few previous studies report estimates that can be considered causal, as researchers for obvious ethical reasons cannot randomly expose humans to lead.[2] Credible sources of exogenous variation in lead exposure, meanwhile, are difficult to find. 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 and the acidity of city water.

Because the mechanisms linking lead exposure and crime are biochemical, lead's effects should be observable not only in the late twentieth and early twenty-first centuries, but in earlier periods as well. Reassuringly, scholars studying the historical effects of lead exposure on outcomes other than crime have found evidence supporting this claim. 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 (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 in the late nineteenth century and data on homicide between 1921 and 1936, when

[2] We summarize below several experimental studies of the effect of lead exposure on non-human animals. For a discussion of the ethics of lead research involving humans, see Markowitz and Rosner (2013).

the first generation of children exposed to lead through water had reached adulthood. Lead exposure has the most detrimental effects on developing children, whose gut absorbs more lead than the adult gut and whose central nervous system is more sensitive to toxicants than the mature central nervous system (Silbergeld 1997, p. 191; Lidsky and Schneider 2003, p. 10; Needleman 2004, p. 212). For this reason, Mielke and Zahran (2012), Nevin (2000), Nevin (2007), and Reyes (2007, 2015), study the relationship between lead exposure and crime rates roughly 20 years later. The plurality of water systems in our data whose year of construction we know were installed in the 1880s and the 1890s. Thus, in most cities, the first children to have suffered the consequences of early lead exposure would have entered adulthood in roughly the first and second decades of the twentieth century. Water lead was the primary source of lead exposure in the early 20th century (Troesken 2006, p. 23; Clay et al. 2014, p. 459) and service pipes "were the primary source of lead in drinking water" (Troesken 2008, p. 555).

Studying the historical effects of lead exposure has three advantages. First, lead levels in city water were determined by two plausibly exogenous sources: a city's rail distance from the nearest lead refinery and the acidity of its water. The dangers of lead were not widely understood in the period we study, and there is little evidence that city officials used information about lead's effects on health or crime to decide whether to use lead pipes (Troesken 2006, p. 13; Clay et al. 2014, p. 458).[3] 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 far exceeding the current Environmental Protection Agency (EPA) standard for water (Troesken 2006, p. 5-6, 54-55, 71; Clay et al. 2014, p. 459).[4] 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

[3]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 in city water were not introduced until 1925 (Tarr 1985).

[4]Clay et al. (2014, p. 459) report that a 1900 investigation by the Massachusetts Board of Health found typical lead levels of 315 parts per billion in water after normal use and 870 parts per billion in water left standing overnight.

inhaled gasoline exhaust. Troesken (2008, p. 555) finds that the use of lead water pipes in cities with acidic water increased infant mortality rates three- to four-fold. "Such estimates are quite plausible," he concludes, "when one considers how much lead could be dissolved into household tap water as a result of lead service lines."

Third, lead exposure today is not uniformly distributed within cities. In the late twentieth and early twenty-first centuries, poor children were 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 individual or neighborhood poverty (Bellinger 2008). Studying cities in the early twentieth century enables us to circumvent this problem because in most cases the entire city population was exposed to lead through water (Troesken 2004, p. 39; Clay et al. 2014, p. 460). 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. Like Reyes (2007, 2015), we exploit exogenous variation in lead exposure to estimate the effects of lead on aggregate-level crime rates. However, our variation in lead exposure comes from a different source. The cities that used lead water pipes in the late nineteenth century are not the same cities that had the highest concentrations of lead deposited from gasoline in the mid-to-late twentieth century.[5]

To study the effect of lead exposure on homicide, we use two separate identification strategies.[6] First, we report estimates of the effect of cities' use of lead pipes instrumented by their distance by rail from the nearest lead refinery. Because transportation costs were a major consideration in city officials' decisions about whether to use lead or iron service

[5] Mielke et al. (2011) rank 90 urbanized areas according to their estimated lead aerosol inputs from 1950 to 1982. The correlation of cities' use of lead water pipes in the late nineteenth century and their rank for lead deposited from gasoline from 1950 to 1982 is $-.13$ ($p = .25$).

[6] Our data structure prohibits us from estimating the effect of lead exposure using changes 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 reliable sample of homicide rates beginning in 1921, long after lead service pipes were introduced in our sample cities. We also 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).

pipes (Clay et al. 2014, p. 460), cities' rail distance from the nearest lead refinery is a strong predictor of whether they used lead. Second, we adopt an identification strategy introduced by Troesken (2006) and Clay et al. (2014) that exploits the fact that more lead will leach into water with a low pH. Among cities using lead pipes, cities with more acidic water should consequently have had higher homicide rates than cities with more basic water. In addition, we show that cities that used lead pipes had higher rates of death from cirrhosis, infant diarrhea, influenza, and scarlet fever, but not other causes of death. Previous scholarship has linked lead exposure to cirrhosis (Troesken 2006, p. 45-46) and infant mortality (Troesken 2006, 2008; Clay et al. 2014), but not scarlet fever and influenza. With the exception of the latter results, these findings show that cities using lead water pipes had higher rates of death only from causes known to be linked to lead exposure, and not from causes associated with poverty, like tuberculosis and typhoid. These three strategies, along with our baseline results, yield a range of estimates of the effect of lead exposure on homicide. Taken together, they suggest that using lead service pipes increased cities' homicide rates considerably. Different estimation strategies and sample restrictions yield substantively similar results.

1 Lead exposure, cognition, and behavior

Lead exposure impairs brain development and interferes with neurotransmitter systems (Reyes 2015, p. 1583). Previous research has linked the biochemical and neurological effects of lead exposure to impulsivity, behavioral problems, hyperactivity, and impaired cognition, all of which are associated with crime. Cecil et al. (2008) find that childhood blood lead is negatively correlated with adult brain volume in regions associated with executive function, ADHD, and childhood behavioral problems. Lead's effect on impulse control stems in part from its ability to mimic calcium, which activates and mediates the transmission of nerve signals (Troesken 2006, p. 41; see also Finkelstein et al. 1998; Dietrich et al. 2001, p. 511; Lidsky and Schneider 2003, p. 5; and Needleman 2004, p. 210). "[L]ead's asymmetric effects on nerve conduction," writes Troesken (2006, p. 42), could "help explain the finding that lead levels are four times higher among convicted juvenile offenders than among non-delinquent high school students." Needleman et al. (2002, p. 715) note that abnormal functioning of the central nervous system is commonly reported among juvenile offenders.

The first pathway through which lead exposure might affect crime is by reducing exposed individuals' 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 scores.

Lead exposure could also increase individuals' probability of participating in crime by increasing their likelihood of having ADHD. A large literature connects lead exposure to 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. Mendelsohn et al. (1998) report a positive association between blood lead and hyperactivity, distractibility, and low frustration tolerance in very young children. Children with ADHD, particularly those who exhibit early behavioral problems, are more likely than comparable children to become involved in crime (Moffitt 1990a,b).

Finally, several studies report a negative relationship between lead exposure and IQ scores, 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).[7] IQ scores are strongly

[7] In related work using variation in children's exposure to household lead due to a policy change in Rhode Island, Aizer et al. (2015) find that lead exposure reduces children's reading and math test scores (see also Evens et al. 2015).

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). Measures of IQ, therefore, may indirectly capture the effects of impulse control on crime.

As the literature on lead exposure has developed, scholars have used increasingly extensive sets of controls and drawn on plausibly exogenous variation in lead exposure to try to rule out the possibility that the association between lead exposure and impulsivity, ADHD, and IQ scores is due to unobserved traits of individuals. Although there is no experimental evidence on lead exposure in humans, a large experimental literature on non-human animals has yielded results largely consistent with those reported in observational studies of humans. Silbergeld and Goldberg (1974), for instance, find that mice experimentally exposed to lead fought more often and had higher rates of hyperactivity than control mice. Sauerhoff and Michelson (1973) report similar results among lead-exposed rats. Early experimental studies of lead exposure and aggression yielded mixed results, with some finding that lead decreases aggression in mice (Cutler 1977; Ogilvie and Martin 1982) and rats (Drew et al. 1979). More recent studies report that lead increases aggression in mice (Burright et al. 1989; Hahn et al. 1991), cats (Li et al. 2003), hamsters (Delville 1999; Cervantes et al. 2005), and rats (Holloway and Thor 1987). Combined evidence from observational studies of humans and experimental studies of non-human animals prompted the Environmental Protection Agency to conclude that "there is a causal relationship between Pb exposure and effects on attention, impulsivity, and hyperactivity in children" and that "a causal relationship is likely to exist between Pb exposure and conduct disorders in children and young adults" (United States Environmental Protection Agency 2013, p. 22; see also Davis et al. 1990 and Cory-Slechta 2003).

Individuals' vulnerability to lead exposure depends in part on their social environment (Troesken 2006, p. 35). Crime and violence occur with greater frequency in some social situations (Collins 2008; Heller et al. 2015; Western 2015) and circumstances (Sampson et al.

1997; Meier et al. 2008) than others. By shifting the distribution of impulsivity, impaired judgment, and aggression in a city population, lead exposure may increase the likelihood that these situations and circumstances turn violent or fatal.

In this paper, we estimate a reduced-form relationship between lead exposure and homicide 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 channel connecting lead exposure to crime rates.

2 Measuring lead exposure and homicide in the early twentieth century

We collect information on the historical pipe metal used by all U.S. cities for which data are available. In prior research, Clay et al. (2014) compiled data on the pipe metals used in a sample of 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 total, we have information on the type of metal used in the water pipes of 591 cities. In the results presented below, the sample size varies over years due to missing homicide and demographic data. We have complete information on the pipe metal and covariates of 545 cities.

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.[8] We map the cities included in our sample in Figure 1.[9] 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. We include in the "lead" group 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 the effect of lead exposure towards

[8]These percentages sum to more than 100% because some cities used multiple types of pipe material.
[9]Figure A.4 in the Appendix includes detail maps of the urban Northeast and Midwest.

zero, as we may include some cities with a small share of lead pipes in the treatment group.

[Figure 1 about here.]

To conduct our analysis, we link the pipe data to city-level homicide data culled from historical *Mortality Statistics* reports. These reports, produced contemporaneously by the Department of Commerce, record city-level deaths by cause. We digitize these data for the years 1921 to 1936.[10]

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 homicides 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 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 homicide, 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 renamed "accident" in 1908 and 1909, changing in 1910 to 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. Like Eckberg (1995), we believe that the reported deaths from other violence in 1906–1920 probably include homicides. However, these counts also include

[10]We collect and digitize measures of total and cause-specific deaths per city by year. By checking that cause-specific deaths sum to total deaths, we are able to minimize errors in our digitization.

non-homicide deaths and may exclude some homicides. Accordingly, we limit our sample to homicides recorded after 1920. In Figure A.5 in the Appendix, we report estimates of the effect of lead exposure on violent deaths from 1906 to 1920 and homicides from 1921 to 1936. The discontinuity in the size of the effects suggests that the measure of violent deaths and the measure of homicides in the *Mortality Statistics* are not comparable. We follow Cutler and Miller (2005, p. 7) in ending our analysis before the introduction of a new data series in 1937.

[Figure 2 about here.]

We check the reliability of our homicide data by matching 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).[11] 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.[12] 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 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.[13]

A second known issue with the *Mortality Statistics* is sample selection. Although the U.S. Death Registration Area was created in 1880, it initially encompassed only two states—

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

[12] Twenty cities appear in both the Monkkonen (2005) data and the *Mortality Statistics* data. However, Monkkonen (2005) reports homicide arrests in 1920 for only 11 of these cities. We can expand the sample to include all 20 overlapping cities by using the homicide arrest data closest to 1920 for the 9 cities with missing data (1919 for 2 cities, 1918 for 1, 1916 for 1, and 1915 for 5). The correlation between the two samples in this case is 0.769.

[13] Our variation in lead exposure is cross-sectional. This makes the Monkkonen (2005) 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 and Lauritsen et al. 2016).

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 Statistics*, much earlier than entire states. In Figure A.1 in the Appendix, we plot the population of the cities in our sample against the national urban population.[14] The share of the urban population covered by the *Mortality Statistics* starts at 78% in 1921 and increases over time, with the exception of 1931 and 1932, when the *Mortality Statistics* reported an abbreviated list of cities. 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.[15] 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 changes in the national homicide rate could be driven by new entrants to the dataset rather than actual changes in violence.[16] However, in our analysis of the effect 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. Although limitations on the availability of homicide data prevent us from studying the relationship between lead exposure and homicide in earlier years, examining the effect of lead exposure from pipes installed in the late nineteenth century on homicides committed in the early twentieth century is consistent with previous research

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

[15]The four cities missing from the registration area in 1921 are Des Moines, IA, Fort Worth, TX, Tulsa, OK, and Sioux City, IA. All of the 100 largest cities in the country in 1930 are included in the 1930 registration area sample.

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

showing that lead exposure increases the crime rate as the first generations of exposed children enter adulthood.

3 Which cities used lead pipes?

An important concern in a cross-sectional study such as ours is that the cities that installed lead pipes in the nineteenth century might have differed from the cities that did not in unobserved 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 the cities that used lead pipes differed in observable ways from the cities that did not. Although larger cities, denser cities, and cities with comparatively low rates of home ownership were more likely to have used lead pipes, so were better-educated cities. High-homicide southern cities, meanwhile, were less likely to have used lead pipes. When we predict whether cities used lead pipes using all covariates, the only difference that remains statistically significant is the city population.

Using the Integrated Public Use Microdata Series (IPUMS) sample of the 1900 census, we calculate several city-level covariates that could be correlated with both lead pipe use and crime.[17] These include each city's population, population density, home ownership rate, and literacy rate, the share of each city's population composed of African Americans, foreign-born residents, and single men aged 18 to 40, and the share of each city's employed population working in manufacturing. We also create covariates for cities' latitude and longitude.

Southern cities have had higher homicide rates than cities in other regions of the United States for as long as homicide statistics have been collected (Nisbett and Cohen 1996).[18] As shown in Figure 1, southern cities were less likely to use lead pipes than other cities. This leads us to expect not only that the cities that used lead pipes would have had lower

[17]The IPUMS 1900 sample is a 5% sample of the census that indicates the city of residence of each respondent if the respondent resided in a city (Ruggles et al. 2010). We aggregate the demographic and economic data of each respondent to calculate city-level covariates.

[18]Some scholars trace regional differences in homicide to the persistence of a "culture of honor" introduced by early immigrants to the South (Nisbett and Cohen 1996; Grosjean 2014), although this explanation is contested (Elster 2007, p. 363).

homicide rates than comparable cities in the absence of the treatment, but also that lead exposure will be positively correlated with homicide only conditional on controls for latitude and longitude, which capture regional variation in homicide. Because of the selection of southern cities out of the treatment, unconditional estimates of the effect of lead exposure on homicide will be biased towards zero. In Figures A.10, A.11, and A.12 and Tables A.9 and A.10 in the Appendix, we reproduce our results in a smaller sample of non-southern cities.

Previous scholarship has shown that both population size and population density are positively correlated with crime rates (Blumstein 2000, p. 35–39; Sampson 1983). Accordingly, we adjust our estimates for both of these city attributes. Because comprehensive information on the land area of the majority of the cities in our sample does not exist, we create a proxy for population density using 1900 census data. We calculate the number of households per dwelling, excluding individuals residing in institutions. Families in single-family residences receive a value of 1; families residing in large apartment buildings receive much higher values. The cities with the top two rankings using our population density measure are Manhattan, NY, and Hoboken, NJ, two famously dense cities.[19] At the bottom of the population density dwelling rankings are small cities in California (Port Arthur and Sweetwater), as well as small cities in the Midwest (Newton, IA, Ashland, OH, New Castle, IN, and Adrian, MI).

Because lead pipes were more expensive and considered more desirable than iron pipes, we expect that wealthier and better-educated cities were more likely to use them than poorer and more poorly educated cities. City officials chose lead pipes because they are more malleable and more durable than iron pipes (Troesken and Beeson 2003, p. 190). 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. One trade journal noted that "The cost of lead pipe of sufficient thickness safely to withstand the pressure is more than the cost of many other materials used for services, but in a paved street the

[19]For the five boroughs of New York City, we collect mortality, pipe, and covariate data at the borough level and include each borough as a separate city.

greater duration of life probably more than compensates for the extra cost, and in places where the streets are occupied by other pipes and conduits the case of getting over and under these obstructions with a flexible pipe is a great advantage" (Engineering News 1916, p. 595, quoted in Troesken and Beeson 2003, p. 184–185). Because of lead's comparatively high price and favorable reputation among engineers, the cities that made the initial investment in lead pipes may have been better off and better educated than the cities that did not. With no sources of comprehensive city-level data on wealth, income, or inequality in 1900, we calculate the home ownership rate as a proxy for average city wealth and the literacy rate as a proxy for average city education.

In all years of our sample, southern cities and the largest non-southern cities report deaths from homicide separately for African Americans and whites. In these cities, African Americans consistently had higher rates of death from homicide than whites. Consequently, we adjust our estimates for the share of each city's population identified as African-American. The *Mortality Statistics* contain no information on rates of death from homicide among the foreign born. Moehling and Piehl (2009) find that foreign-born whites had lower rates of prison commitment for violent offenses and homicide than native-born whites in 1904 and 1930. Although prison commitments are weak indicators of involvement in crime, much less victimization, to address concerns that immigrants had different homicide victimization rates than native-born whites, we also control for the proportion of each city's population that was foreign-born in 1900.

Finally, we include controls for the proportion of single men aged 18 to 40 in each city's population and the share of its employed population working in manufacturing.[20] Unmarried men aged 18 to 40 are more likely than other groups to be involved in crime (Sampson and Laub 2003). Cities with more manufacturing might contain more lead or other industrial toxins related to brain development, impulse control, and ultimately crime.

We control for the 1900 values of these covariates rather than their contemporary values

[20]We use occupation codes 300 to 499 in the IPUMS sample to define manufacturing, which includes both durables and non-durables.

for two reasons. First, these city-level attributes are measurable only in decennial censuses. Annual data have to be interpolated. Second, because lead exposure could affect some of our covariates, using values of the covariates measured closer to the date of the treatment reduces the likelihood that including them will induce post-treatment bias. Appendix Figures A.6, A.7, A.8, and A.9 and Tables A.1, A.2, A.3, A.4, A.5, A.6, A.7, and A.8 show that our results are robust to controlling for covariates measured in 1880, 1910, 1920, and 1930 instead.[21]

[Table 1 about here.]

In Table 1, we compare the average value of all covariates in cities that did and did not use lead pipes (Columns 1–3). Then we regress an indicator for lead pipe use on our covariates (Columns 4–5). Columns 1–3 indicate that larger, denser, and better-educated cities were more likely to use lead pipes than smaller and less dense cities. As expected, western cities and southern cities were less likely to use lead pipes than cities farther east or north. Unexpectedly, cities that used lead pipes had lower rates of home ownership than cities that did not. This could mean that cities using lead pipes were poorer than cities that did not or that home ownership rates in the census are a weak proxy for city wealth. Among the other variables, there were no statistically significant differences between the cities that used lead pipes and the cities that did not. When we regress an indicator measuring lead pipe use on all covariates, we find that the only statistically significant difference between cities that used lead versus iron pipes was their size. Although the absence of other statistically significant differences in Columns 4 and 5 is reassuring, it does not rule out the possibility that the cities that used lead pipes differed from the cities that did not in unobserved ways. To address the concern that omitted variables correlated with both cities' use of lead pipes and with their homicide rates bias our estimates, we introduce two separate identification strategies, which we describe in the next section.

[21]Our results using 1880 and 1920 controls are less precisely estimated than our results using 1900, 1910, or 1930 controls because using 1880 and 1920 controls reduces the sample to 189 and 227 cities, respectively.

4 Estimating the Effect of Lead Exposure

We use count data regressions to model annual city-level homicides. Count models such as Poisson and negative binomial models are commonly used in crime and mortality research using non-negative integer data because they are more efficient than incorrectly specified linear models. Although the point estimates from linear models of count data will be consistent, the covariance matrix will not, making inference unreliable (Grogger 1990).

Both homicide counts and homicide rates are highly left-skewed. We plot the distribution of city-level homicide rates from 1921 to 1936 in Figure A.2 in the Appendix. Transforming homicide counts into logged homicide rates yields a distribution that is much closer to normal. However, transforming the outcome in this way forces us either to drop city-year observations with no homicides or to arbitrarily add a constant to the homicide count before calculating the logged homicide rate.[22]

In the Poisson distribution, the conditional variance equals the conditional mean. However, in our data, the variance is greater than the mean. This overdispersion will not change the consistency of our parameter estimates, but failing to account for it will bias our standard errors downward. Consequently, we model homicide using a negative binomial distribution, which includes a parameter—estimated directly—to account for overdispersion (Cameron and Trivedi 2013). The negative binomial distribution nests the Poisson distribution: if the variance equals the mean, it collapses to a Poisson distribution.[23]

We estimate negative binomial regressions with a stochastic component

$$y_i = \text{Negative-binomial (mean} = \theta_i, \text{overdispersion} = \omega), \tag{1}$$

[22]In Figure A.14 in the Appendix, we show that our negative binomial regressions of homicide counts generate similar estimates to OLS regressions of logged homicide rates when we drop all cities reporting no homicides.

[23]Imai et al. (2007, p. 339) provide a simple description of the negative binomial distribution. For a longer discussion, see Cameron and Trivedi (2013, p. 80–89). One alternative to using the negative binomial distribution is to use a pseudo-Poisson distribution, which scales the standard errors by the degree of overdispersion (Gelman and Hill 2007, p. 115). Although our results are robust to this alternative specification, we use the negative binomial distribution because the correct specification should yield more efficient estimates (Cameron and Trivedi 2013).

and a systematic component,

$$\theta_i = e^{(\log(Population_i)+\alpha+\beta Lead_i+\gamma X_i)}, \tag{2}$$

where y is the number of homicides in city i, *Lead* is an indicator variable scoring one if a city's pipes contained some lead, *Population* is the contemporaneous city population, and X is a column vector of city-level demographic and geographic controls. As noted above, these include latitude, longitude, population density, the literacy rate, the home ownership rate, the African-American population share, the foreign-born population share, the share of single men aged 18 to 40 in the employed population, and the share of the employed population working in manufacturing. We model homicide counts relative to the contemporaneous population, known in count models as the exposure. When the exposure input is logged, it is called an offset.[24] Coefficients estimated using negative binomial regression with a population offset can be interpreted in the same way as coefficients from a linear regression of a logged rate. Specifically, the coefficient of interest, β, is the semi-elasticity of lead pipe use: all else equal, a city that used lead pipes is expected to have $(100\times[e^{\beta} - 1])\%$ more homicides per capita than a city that used iron pipes.

Throughout the paper, we use two main specifications. First, we estimate annual cross-sectional regressions using Huber-White standard errors to account for heteroskedasticity, which is common in count data. Second, we fit pooled cross-sectional regressions for all years in our data. In these regressions, we cluster the standard errors at the city level because we observe lead pipe or iron pipe use once, at the turn of the twentieth century. Clustering adjusts our inference to account for the repeated observations of cities with a constant pipe type in multiple years. We also include year fixed effects to account for aggregate shocks or changes in national homicide reporting standards.[25] The annual results suggest that

[24]This is equivalent to including a parameter for the logged population with the coefficient constrained to one. In unconstrained regressions, we cannot reject that the true coefficient on the log of population is equal to one.

[25]We do not cluster over both cities and years because any within-year correlation of the errors is likely to be driven by a common shock which will be absorbed by the year fixed effects (Cameron and Miller 2015).

the effects are relatively stable over our sample period, but limiting ourselves to just one parameter for the lead effect simplifies the presentation of the results.

If cities' adoption of lead pipes were uncorrelated with all other determinants of their homicide rates, β would capture the effect of lead exposure through water on homicide. However, if the cities that had relatively high homicide rates for reasons unrelated to lead exposure typically selected out of lead-pipe use, or if there is measurement error in the treatment, β will be biased downward. For example, if better-educated, low-homicide cities were more likely to use lead pipes than less-educated, high-homicide cities, we would underestimate the effect of lead exposure on homicide. Measurement error could stem from the fact that we do not know the exact ratios of lead and iron pipes in the cities that used both pipe metals, as well as the usual concerns about measurement in historical data.

To address the potential bias in our estimates induced by selection or measurement error, we use two identification strategies. The first takes advantage of the fact that city officials' decisions about whether to use lead pipes were influenced by their cost. Transportation costs constituted a significant portion of the total cost of service pipes. Clay et al. (2014, p. 460) note that among the reasons city officials chose to use lead pipes, "Factors such as as the type of pipes produced by local firms were likely to have an impact. . . for both cost reasons (pipes were generally expensive to transport) and because local firms were likely to lobby engineers to use their pipes." Contemporaries issued similar judgments. For instance, 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). Transportation costs rose with the distance from pipe supplier to city (Gross 2014).[26] The local availability of smelted lead thus could have lowered its price and increased the likelihood that nearby cities used lead pipes (Clay et al. 2014).

To test this proposition, we collect data from Ingalls (1908) on the locations of all lead

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

smelting facilities and refineries in the United States as of 1899. We geolocate these refineries and calculate each city's distance from the nearest refinery via rail using data on the complete railroad network of the United States in 1900 (Atack et al. 2010; Donaldson and Hornbeck 2016). Although the correlation between the railroad distances and straight line distances is 0.988, for several cities the closest lead refinery changes depending on which measure we use. Nonetheless, our results are robust to either measure of distance.

[Figure 3 about here.]

Figure 3 maps the 14 lead smelters and refineries in the United States as of 1899 and the railroad network in 1900. Refineries and smelters were often drawn to their locations for idiosyncratic reasons. For instance, the first lead refinery in the country was constructed on the banks of the Passaic River because of its proximity to jewelry manufacturing in Newark, NJ (Ingalls 1908, p. 78).[27] Likewise, Selby & Naylor built a lead refinery in San Francisco to supply lead for a nearby shot tower built several years earlier (Ingalls 1908, p. 78).[28]

In Figure 4, we plot the relationship between a city's rail distance to the nearest lead refinery and its probability of using lead service pipes. 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 our controls. In the next section, we report estimates of the effect of lead pipe use on city-level homicides, using each city's rail distance from the nearest lead refinery as an instrumental variable (IV).

[Figure 4 about here.]

For rail distance from a lead refinery to be a valid instrument, it must satisfy the exclusion restriction. One concern with using a city's rail distance from the nearest lead refinery or smelter as an instrument is that there may have been a direct effect of living near a refinery

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

[28] Shot towers are used in the production of shot balls and other projectiles. Molten lead is dropped from the top of the tower into a dropping pan, forming balls and pellets when it hits water below (Sanders 1944).

or smelter on homicide. Warren (2000, p. 49–50), for instance, documents that smelter workers in the early twentieth century faced a considerable risk of chronic lead poisoning. Children living in very close proximity to a lead smelter are especially vulnerable. Landrigan et al. (1975) report that 43.2 percent of children one to nineteen years old living within 1.6 kilometers (1 mile) of a smelter in El Paso, Texas in 1972 had blood lead levels at or above 40 micrograms per deciliter. That number dropped 9.8 percent for those living 1.6 to 6.6 kilometers (1 to 4.1 miles) from the smelter.[29] Albalak et al. (2003) find that nearly 50 percent of children living within 1,500 meters (.9 miles) of a lead smelter in Torreón, Mexico in 2001 had lead levels greater than 10 micrograms per deciliter, but fewer than 3 percent of children living more than 6,000 meters (3.7 miles) from the smelter had lead levels in this range (see also García Vargas et al. 2001). In all of our regressions, we control for the proportion of the employed population working in manufacturing. However, to ensure that our results are not driven by a direct effect of living near a lead refinery, in separate regressions we also drop all cities located within ten miles of a city with a lead refinery. In Table A.11 and Figure A.13, we reproduce our results using a more restrictive sample excluding all cities within 20 miles of a lead refinery or smelter.

Our second identification strategy, introduced by Troesken (2006) and Clay et al. (2014), exploits the fact that more lead particulate will leach into water with low pH levels. If lead exposure has a causal effect on homicide, more acidic water should be correlated with higher homicide rates only in cities that used lead water pipes. Following Clay et al. (2014), we compile data on the pH of city water from the U.S Geological Survey report, *The Industrial Utility of Public Water Supplies in the United States, 1952* (Lohr and Love 1954a,b). The acidity of water is determined primarily by local geology and remains relatively stable over time (Clay et al. 2014, p. 459, 462). We fit three separate regressions. First, we report separate estimates of the effect of pH on homicide in cities that used pipes with some lead

[29] Landrigan's study was conducted at a time when blood lead levels from gasoline emissions were very high in the general population. From 1976 to 1980, a period when blood lead levels nationwide fell precipitously due to reductions in lead in gasoline, two percent of Americans six months through 74 years of age had blood lead levels at or above 30 micrograms per deciliter (Mahaffey et al. 1982).

versus cities that used pipes with no lead. Then we interact our lead pipe indicator with city-level pH in 1952 in all cities for which we have pH data. Cities with low pH levels should have higher homicide rates only if they used lead pipes. Because we lack pH data for 119 of the 545 cities in our sample, these estimates should be less precise than the estimates from our full sample.

Figure 5 depicts the residual variation in the homicide rate after regressing city-level homicide counts on all covariates except the lead indicator. The gap in the homicide rates of the cities that used lead pipes and the cities that did not is evident in the left panel, which uses data from all cities in our sample. This gap is much wider in the sample of cities whose water was acidic and negligible in the sample of cities whose water was basic.

[Figure 5 about here.]

5 Results

[Figure 6 about here.]

Figure 6 presents our main results including all covariates. The dots represent point estimates from negative binomial regressions of the number of homicides in each city on a variable indicating whether the city used pipes with some lead. We estimate these regressions separately in every year of our sample. Bars around the estimates represent 95 percent confidence intervals. Based on the lowest and highest point estimates, cities that used lead pipes had between 14 and 36 percent higher homicide rates than cities that did not. In four years of the sample, the confidence intervals overlap with zero, although we can reject a null effect at a 10% significance level. Taken together, these baseline estimates provide evidence that lead exposure through water pipes may have increased city-level homicide rates in the early twentieth century.

In the first column of Table 2, we estimate the lead effect in a pooled cross-sectional negative binomial regression, including all of the same covariates as in Figure 6, along with year fixed effects. The sample includes all 545 cities for which we have complete information on pipe metals, homicides, and city-level covariates in 1900. We cluster our standard errors at the city-level to account for the fact that we observe the city's pipe metal only once. In this specification, homicide rates are 24% higher in cities that used lead pipes, reassuringly close to the midpoint of our annual estimates. The signs on the control variables conform to our expectations based on the social science literature on homicide. Cities with higher African-American population shares, higher shares of single men aged 18 to 40, and higher population density had higher homicide rates. In contrast, cities with higher foreign-born population shares had lower homicide rates.

[Table 2 about here.]

[Figure 7 about here.]

In Figure 7, we present the yearly IV estimates. The top panel shows the baseline IV estimates. The bottom panel shows the IV estimates in a smaller sample excluding cities located within ten miles of a lead refinery. We estimate the IV negative binomial regressions following the control function procedure described by Cameron and Trivedi (2013, p. 401–406). We capture the residuals of the first stage OLS regression of our endogenous variable of interest (the lead pipe indicator) on the instrument (rail distance to a lead refinery) and the full set of controls and include those residuals as controls in the second stage negative binomial regression. To account for estimation error in the first stage, we bootstrap the standard errors.

The first-stage estimates of the effect of a city's rail distance from the nearest lead refinery on its probability of using lead pipes, which vary from year to year based on the sample size, are always negative and statistically significant in the baseline IV regressions. As the distance to the nearest lead refinery increases, cities are less likely to use lead pipes. Angrist-Pischke first-stage F-statistics from these regressions vary from 15.77 to 51.98.[30] In the second set

[30]The F-statistics vary only because the sample size and sample cities change from year to year. The smallest F-statistics are 15.77 and 15.81 in 1931 and 1932, when our sample shrinks to only 276 cities. In all other years, they never fall below 35.

of IV regressions, which exclude all cities within 10 miles of a lead refinery, there is also a negative and statistically significant relationship between a city's use of lead pipes and its rail distance from a lead refinery. First-stage F-statistics in these regressions range from 11.15 to 47.39.[31]

In the second through fifth columns of Table 2, we estimate the first stage and IV negative binomial regressions in our pooled cross-sectional data, corresponding to the annual estimates in Figure 7. As in the annual regressions, we estimate the IV negative binomial regressions with a control function and bootstrap the standard errors. The first stage F-Statistic is 45.98 in the full sample and 39.13 in the sample excluding cities within 10 miles of a lead refinery. The lead point estimate is smaller in the sample excluding cities within 10 miles of a refinery than in the full sample, but not statistically different.[32]

The IV estimates are considerably larger than the baseline negative binomial estimates. Based on the estimate reported in column 4 of Table 2, cities using lead pipes had homicide rates more than two and a half times the rates of cities that used iron pipes. One possible explanation for this difference is selection. Historical evidence suggests that wealthier and better-educated cities were more likely to install lead pipes. Troesken (2008, p. 564) notes that IV estimates of the effect of using lead pipes on infant morality are several times larger than the baseline estimates because the cities that used lead pipes were more "health conscious" than the cities that did not. "[I]f there were some sort of unobserved heterogeneity across towns using lead and nonlead pipes," he writes, "this heterogeneity imparts a downward bias" on the baseline estimates. Troesken and Beeson (2003, p. 184) point out that in the early twentieth century, most engineers believed lead pipes were a safe and "almost ideal" choice.

Assigning all cities using a mixture of lead and iron pipes to the treatment category could

[31] Noting the slight non-linearity in Figure 4, we also fit IV regressions using distance and distance-squared as instruments. We present these results, which are substantively identical, in Figure A.3 in the appendix.

[32] The number of clusters falls to 543 in the IV regressions because two cities in our sample, Eureka, CA and Port Angeles, WA, are too far, at 90 miles and 45 miles, respectively, from the nearest point on the railroad network for us to accurately calculate a rail distance. The sample falls to 521 in the set of non-refinery cities because we exclude the 22 cities within 10 miles of a lead refinery.

also generate measurement error. In this case, the IV estimates will reduce the noise in our lead indicator because we have precise measures of the railroad distance of each city to the nearest lead refinery. Measurement error will bias our estimated lead effect towards zero in the baseline negative binomial regressions, but not the instrumental variables regressions. Nonetheless, given their size, we consider the IV estimates to be an upper bound of the lead effect. If the IV estimates are too large and the baseline negative binomial estimates are too small, the true effect probably lies somewhere in between.

In Figure 8, we report estimates of the effect of lead exposure on homicide using our second identification strategy. Here we expect to observe a negative relationship between the pH of city water and the city homicide rate only in cities that used lead pipes. In contrast, there should be no relationship between pH and homicide in cities that used iron pipes. pH values are scaled between 0 and 14, with neutral water at 7. More acidic solutions have pH scores below 7, and more basic solutions have pH scores above 7. To ease the interpretation of our results, we subtract 7 from each city's original pH value, re-centering pH such that a city with neutral water has a pH of 0.

[Figure 8 about here.]

[Table 3 about here.]

The results of the second set of analyses are mixed. In the top panel of Figure 8, we present the effect of pH on city-level homicides separately for cities that used pipes with some lead content and cities that used pipes with no lead content. As expected, in every year of our sample, the pH point estimate, represented by the black dots, is negative only in the cities that used lead pipes. As the water in a city becomes more acidic, moving down the original pH scale, homicide rates rise accordingly. However, in this smaller sample, the 95 percent confidence intervals overlap with zero in all but five years. In contrast, the pH point estimates in cities that used iron pipes, represented by the white dots, are always positive.

Like the results for cities that used lead pipes, these are statistically significant in only six years of the sample.

In the bottom panel of Figure 8, we pool the data and estimate the effect of an interaction between city-level pH and the lead indicator, controlling for the lead indicator and pH directly. As expected, here too the point estimates are always negative: an increase in acidity, which corresponds to a decrease in pH, is positively correlated with homicide rates in cities using lead pipes. In nine years of the sample, the 95 percent confidence intervals do not overlap with zero, indicating that the difference in the effect of pH in cities that used lead pipes versus cities that did not is itself statistically significant in these years. These results are broadly consistent with the prediction that in cities using lead pipes, more acidic water increased the extent of lead exposure, and consequently the homicide rate. Although we know of no chemical or biological explanation for the fact that acidic water was associated with a reduction in homicides in cities using iron pipes, the results shown in the top panel of Figure 8 confirm that the effect of the lead-pH interaction is not driven by this association alone.

In Table 3, we report estimates of the effect of pH in the pooled cross-sectional data. In paired columns, we present specifications with pH in levels and in logs, following Clay et al. (2014).[33] In cities with lead pipes, we observe that a decrease of pH from 7 to 6 increases homicides by 10% (pH in levels) or by 9% (pH in logs), based on columns 1 and 2.[34] The negative coefficients on the interaction terms in columns 5 and 6 are also consistent with this pattern.

[33]We log the original pH values and then recenter the logged pHs around ln(7), once again setting a city with neutral water to be zero. This simplifies the interpretation of the lead coefficients in the interaction regressions in columns 5 and 6 of Table 3.

[34]As noted above, in a negative binomial regression, the coefficients can be interpreted as elasticities when the independent variable is logged. A decrease from 7 to 6 is a 14.28% decline in pH and $0.1428 \times 0.594 = 0.0848$.

5.1 Falsification

As discussed above, lead exposure could increase city-level homicide rates by shifting the distribution of impulsivity and aggression in the population. If instead the cities that used lead pipes had higher homicide rates than the cities that used iron because they were poorer than those cities, we would expect to observe an association between lead-pipe use and deaths from diseases associated with poverty, such as tuberculosis and typhoid. The absence of such an association should increase our confidence that the relationship between lead pipe use and homicide is not driven by city-level poverty.

However, two additional causes of death should be related to lead exposure. The first is cirrhosis. According to Troesken (2006, p. 45-46), "lead adversely affects liver and kidney function" and "might indirectly impair liver function by stimulating alcohol consumption." Dietrich et al. (2001, p. 516) show that postnatal blood lead levels in a prospective longitudinal birth cohort were positively associated with teenage alcohol consumption. Reyes (2015) replicates these findings in a study using exogenous changes in gasoline lead. Experimental studies of non-human animals yield similar results. Nation et al. (1986) and Virgolini et al. (1999), for instance, find that rats fed a diet containing lead acetate consumed more ethanol than control rats.

The second cause is infant diarrhea. Troesken (2006, p. 34, 106) notes that lead exposure can lead to diarrhea and culminate in death among infants. According to Ferrie and Troesken (2008, p. 8), in the early twentieth century, "diarrheal illness was probably the leading cause of death for the very young" (see also Troesken 2006, p. 112). As noted above, Troesken (2008) and Clay et al. (2014) show that lead exposure through water pipes increased infant mortality from 1900 to 1920.

[Table 4 about here.]

Table 4 reports the relationship between a city's use of lead pipes and its death rate from several causes: homicide, circulatory disease, cancer, nephritis, cerebral hemorrhage, pneumonia, congenital malformations, tuberculosis, influenza, appendicitis, diabetes, infant diarrhea, childbirth, hernia, suicide, violent accidents, cirrhosis, unknown causes, diphtheria, bronchitis, whooping cough, typhoid, measles, rheumatism, scarlet fever, erysipelas, malaria, meningitis, and smallpox.[35] We also estimate the effect of lead exposure on total mortality. To simplify the presentation of the results, we pool the data over years, cluster the standard errors at the city-level, and include year fixed effects. We order the causes according to how many people per 10,000 died of them in the full sample and report the baseline homicide results in the first row for reference.

As expected, we observe large, positive, and statistically significant relationships between a city's use of lead pipes and its rates of death from cirrhosis and infant diarrhea. Unexpectedly, we find that cities that used lead water pipes had higher rates of death from scarlet fever and influenza. Cities that used iron pipes, in contrast, had higher rates of death from circulatory disease, cancer, and cerebral hemorrhage. We know of no scientific literature to motivate these latter relationships.[36] If lead exposure causes more infant deaths and deaths from homicide and cirrhosis, it could reduce the size of the population at risk of dying from circulatory disease and cancer late in life. It is also possible that these relationships reflect the chance result of multiple statistical tests. For all other causes, there is no statistically significant difference between rates of death in cities that used lead pipes versus cities that did not. In short, there is little evidence that lead exposure was positively correlated with mortality rates generally or with deaths from tuberculosis and typhoid.

We focus our analysis on lead exposure rather than lead poisoning, which was much rarer. 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, 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.

[35]The average age at death from these causes varies. Deaths from some causes may consequently be relatively more responsive than others to the introduction of lead pipes in the late nineteenth century.

[36]It is possible that deaths from scarlet fever reflect a misdiagnosis of another cause of death related to lead exposure, but we know of no historical medical literature addressing this point.

Thus, we are unable to estimate the effects of lead pipe use on deaths from lead poisoning.

6 Conclusion

In this paper, we draw on variation in the use of lead service pipes in the late nineteenth century to estimate the effect of lead exposure from water on city-level homicide rates from 1921 to 1936. Our baseline negative binomial estimates imply that cities that used lead water pipes had homicide rates that were twenty-four percent higher than cities that did not. These results are robust to instrumenting a city's use of lead pipes with its rail distance from the nearest lead refinery. We found evidence broadly consistent with the prediction that more lead will leach into more acidic water and increase the homicide rates of cities with low-pH water and lead service pipes. We also found that, with two exceptions, cities that used lead pipes had higher rates of death only from causes known to be related to lead exposure and not from causes associated with poverty. Although the size and precision of our estimates vary across identification strategies, the weight of the evidence suggests that cities' use of lead water pipes considerably increased their homicide rates.

The advantage of studying the effects of lead exposure in the past is threefold. First, the city officials who decided whether to use lead or iron pipes did so with little regard for the negative effects of lead exposure on health or crime. We found scant evidence that the cities we would expect to have high homicide rates for reasons unrelated to lead exposure selected into lead pipe use. Historical studies instead conclude that it was the best educated and most "health conscious" cities that used lead pipes (Troesken and Beeson 2003; Troesken 2008). A city's rail distance from the nearest lead refinery, which we use as a proxy for the transportation cost of lead pipes, strongly predicts whether cities used lead pipes.

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 exposure than individuals exposed to lead paint chips or air lead in the late twentieth century. The IV estimates indicate that the effect of lead exposure on homicide in the early twentieth century was potentially much larger than the baseline estimates suggest.

Although the true effect of lead exposure on homicide mostly likely lies between the baseline negative binomial estimates and the IV estimates, our largest estimates are similar to those found in previous studies on lead exposure and crime and infant mortality (Reyes 2007; Stretesky and Lynch 2001; Troesken 2008).

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. Common confounders of the lead-crime relationship in individual-level studies pose less of a problem in our city-level study, where in most cases the entire city population was exposed to lead through water. Our use of city-level homicide data from the early twentieth century adds new variation to the literature on lead exposure and crime because city-level lead water pipe use in the late nineteenth century is weakly correlated with city-level lead deposited from gasoline in the mid-to-late twentieth century.

Still, our study does have limitations. The precise mechanisms through which the individual-level effects of lead exposure contribute to aggregate homicide rates and interact with the social determinants of crime have yet to be fully specified. Our conclusions are based on the combined results of several identification strategies rather than a single experimental test. More research with strong identification strategies using data on individuals as well as aggregate units would permit stronger conclusions about the causal effect of lead exposure on crime, both historically and today.

Finally, we stress that if lead exposure does increase 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. Scholars studying the causes of crime should also bear in mind the different social costs associated with being wrong about a particular cause. If lead exposure increases crime, then the solution is to invest in lead removal (Needleman 2004, p. 218). Even if lead removal will not reduce crime, it will remove a dangerous toxin from the environment. Other strategies to reduce crime may not have similarly positive side effects.

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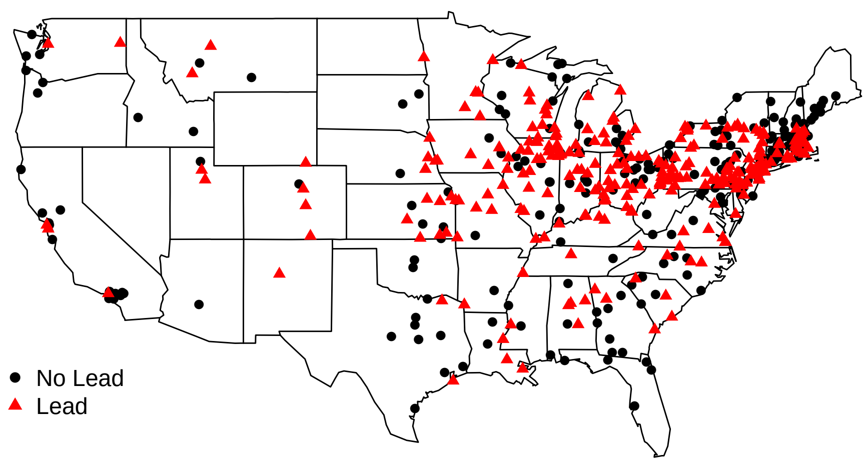
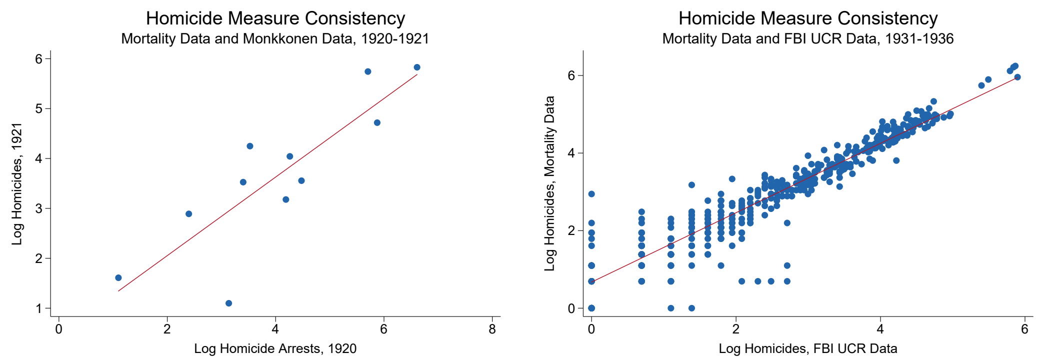


Figure 1: Location of sample cities and the type of water pipe they used as of 1897. The data are drawn from *The Manual of American Water-Works* (Baker 1897). Cities with only lead pipes and cities with a mix of lead and iron pipes are included in the "lead" category and marked with triangles. Cities using either galvanized iron or wrought iron are included in the "no lead" category and marked with circles. See Figure A.4 in the Appendix for detail maps of the urban Northeast and Midwest.



(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 Statistics* homicide data begins in 1921. The figure shows the 11 overlapping cities included in both sources. 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 Statistics* data, both in logs. The correlation between the two samples is 0.941. Comparing the *Mortality Statistics* data on homicides to both alternative measures suggests that during our sample period of 1921 to 1936, the *Mortality Statistics* data accurately measure homicides.

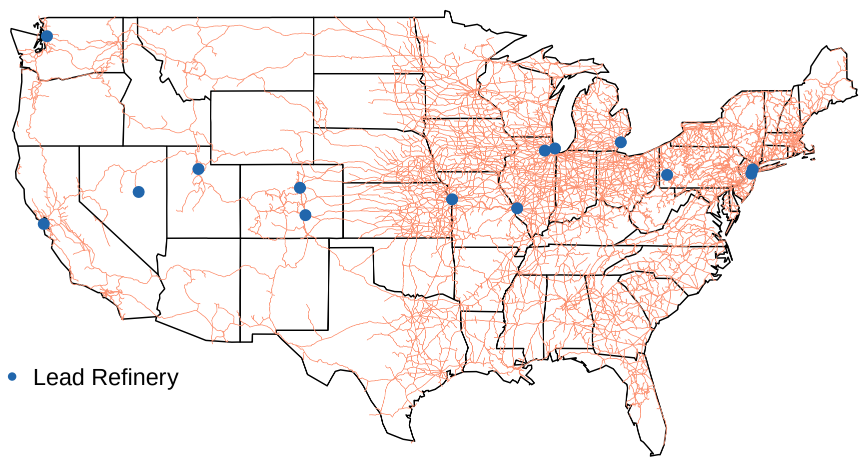


Figure 3: Location of lead smelters and refineries in 1899 and the railroad network in 1900. Data on the location of the 14 lead smelters and refineries in the United States are from Ingalls (1908). The transportation cost of lead pipes should be lower in cities located closer to lead smelters and refineries via rail. The 1900 railroad network map was digitized by Donaldson and Hornbeck (2016).

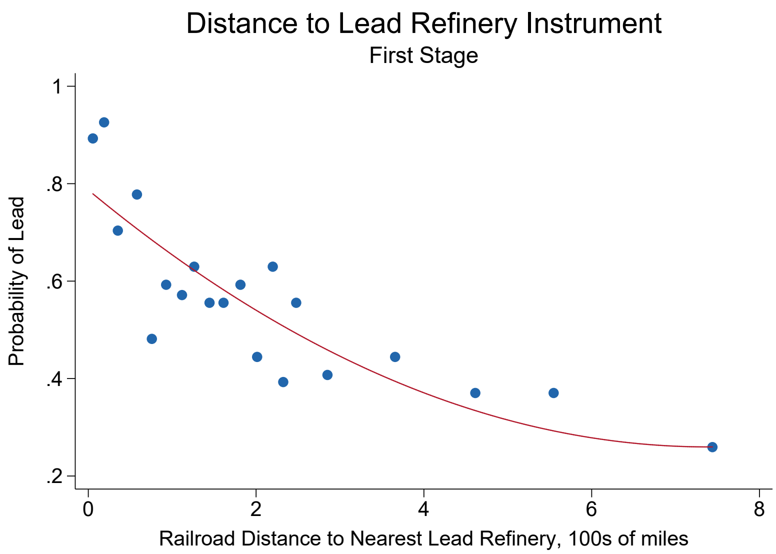


Figure 4: Binned scatter plot of the relationship between a city's probability of using lead and its distance to the nearest lead refinery in 1899. Each point represents 5% of the data, cut by distance, and is plotted at the mean probability of using lead and of distance within the group. Cities farther from lead refineries were less likely to use lead pipes. Ninety percent 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 are drawn from Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).

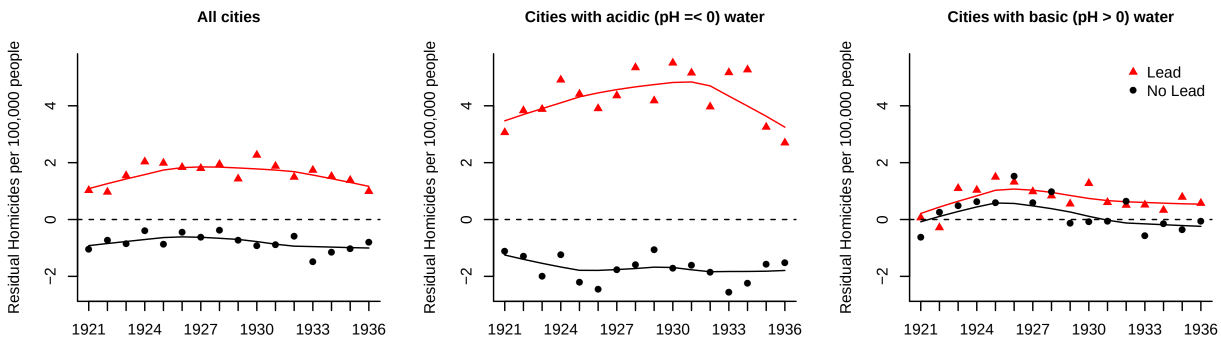


Figure 5: Residual homicide rate in US cities, 1921-1936. Homicide rates were consistently higher in cities using lead pipes than in cities using non-lead pipes. The gap between the lead and the non-lead cities is much wider in the sample of cities whose water was acidic and negligible in the sample of cities whose water was basic. The trend lines are constructed by capturing the residuals after regressing the homicide rate on all covariates other than the lead pipe indicator. The homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. The population data are interpolated using decennial census data. Cities with only lead pipes and cities with a mix of lead and iron pipes are included in the "lead" category. Cities using either galvanized iron or wrought iron are included in the "no lead" category.

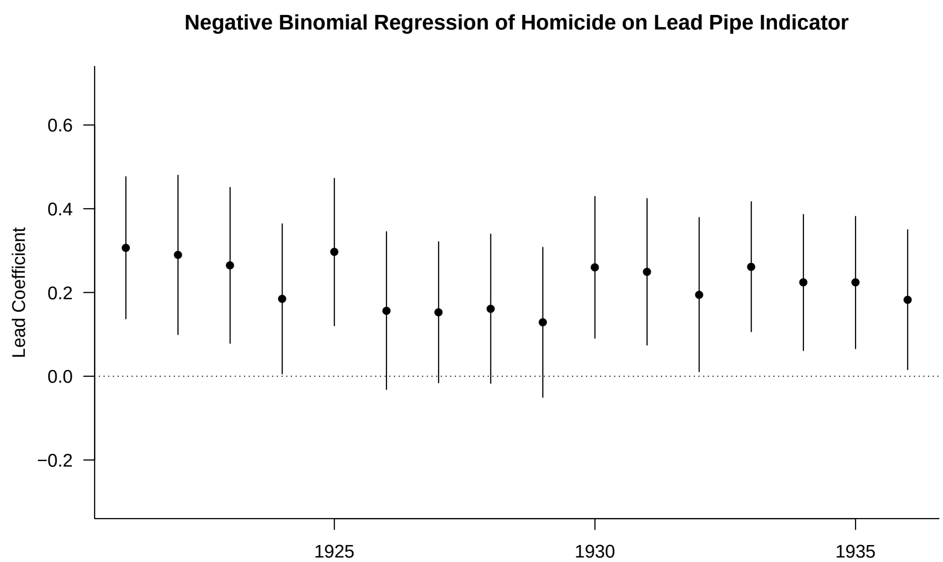
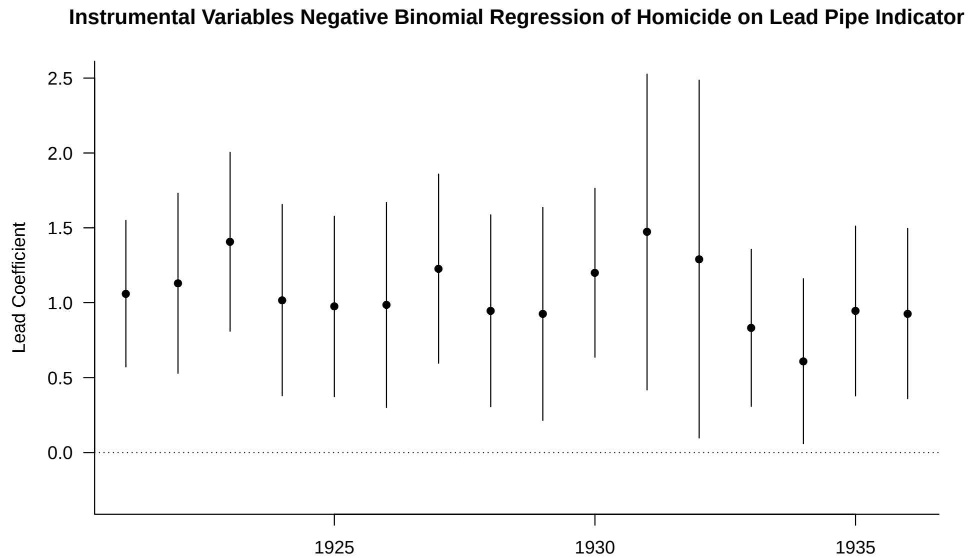


Figure 6: Estimated lead coefficients from yearly negative binomial regressions with all controls and a population offset. Black dots represent the negative binomial point estimates. Based on the lowest and highest point estimates, cities that used lead pipes had between 14 and 36 percent higher homicide rates than cities that did not. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample.



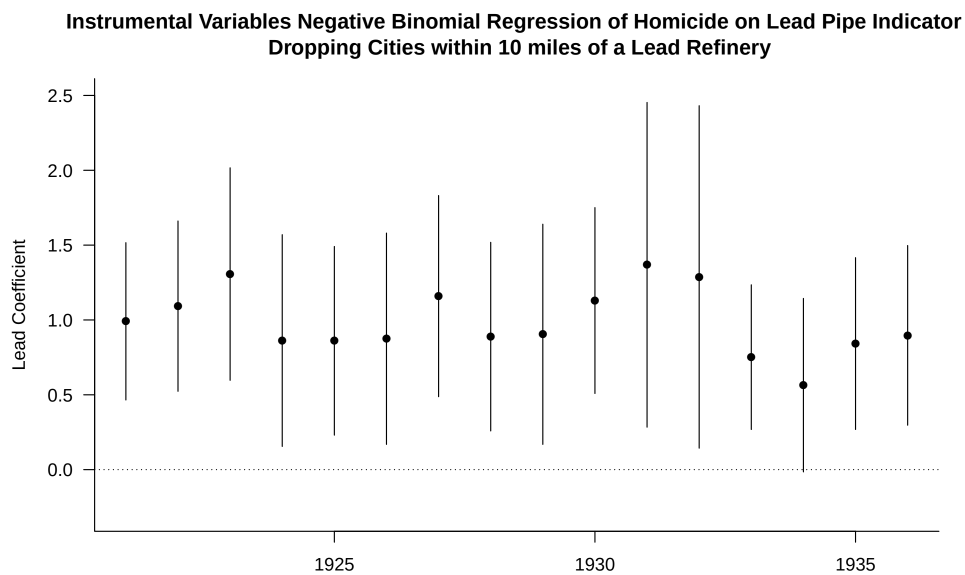
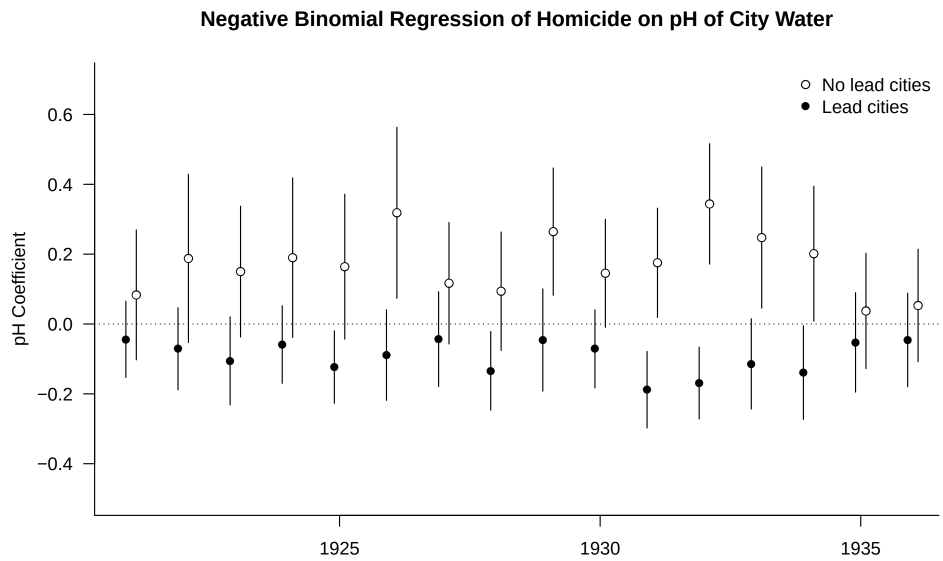


Figure 7: Estimated lead coefficients from yearly IV negative binomial regressions with all controls and a population offset. The top panel depicts the baseline IV estimates. The bottom panel depicts the IV estimates from a sample of cities located more than 10 miles from a lead refinery. Black dots represent the IV negative binomial point estimates. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. The instrument, distance to the nearest lead refinery, is measured as shortest path distance along the 1900 railroad network from the city to closest lead refinery, as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).



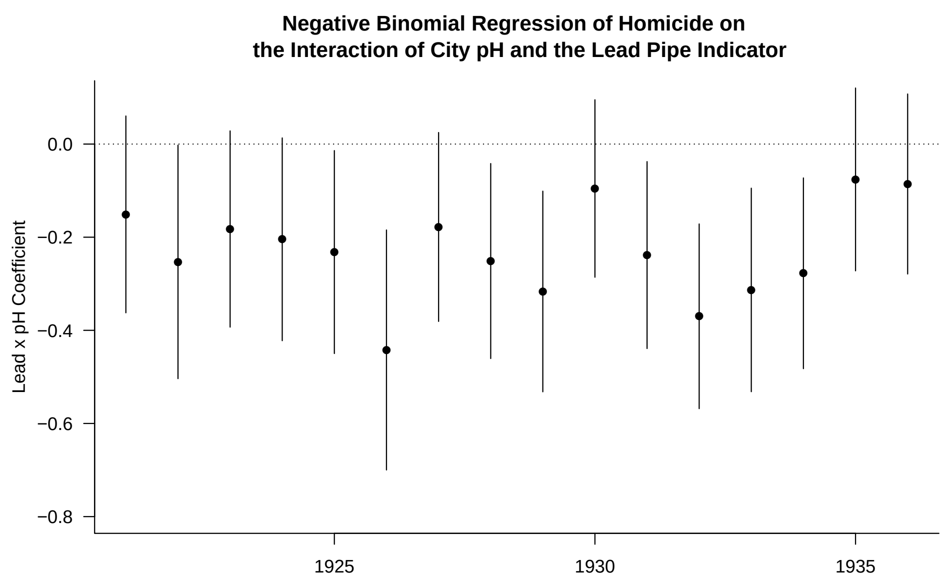


Figure 8: Estimated pH coefficients from yearly negative binomial regressions with all controls and a population offset. In the top panel, black dots represent pH point estimates from the sample of cities using lead pipes and white dots represent pH point estimates from the sample of cities using iron pipes. In the bottom panel, black dots represent point estimates from the interaction of pH with the lead pipe indicator. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. pH is measured according to *The Industrial Utility of Public Water Supplies in the United States, 1952.*

Table 1: Characteristics of Cities Using Lead Pipes in 1897

	Unconditional Means			Linear Probability Regression	
	Lead	No Lead	Difference	Does the City Use Lead?	
City Latitude	40.47	39.94	0.53*	0.0065	0.0036
	(2.80)	(4.21)	(0.30)	(0.0061)	(0.0098)
City Longitude	−82.91	−84.87	1.96*	0.0016	0.0024
	(9.82)	(14.80)	(1.06)	(0.0017)	(0.0021)
Log Population, 1900	9.89	9.37	0.52***	0.0984***	0.0906***
	(1.24)	(0.86)	(0.09)	(0.0188)	(0.0229)
Black Population Share, 1900	7.28	9.32	−2.05		−0.0010
	(13.67)	(17.09)	(1.32)		(0.0034)
Foreign-born Population Share, 1900	18.30	17.13	1.17		−0.0025
	(11.83)	(11.81)	(1.02)		(0.0028)
Literacy Rate, 1900	93.64	92.51	1.13*		0.0041
	(6.18)	(8.16)	(0.61)		(0.0060)
Home Ownership Rate, 1900	39.77	42.51	−2.74**		−0.0014
	(12.94)	(12.68)	(1.11)		(0.0022)
Share Single Men 18-40, 1900	11.47	11.08	0.39		0.0072
	(3.56)	(3.07)	(0.29)		(0.0068)
Share Employed in Manufacturing 1900	26.80	25.05	1.76		−0.0002
	(14.85)	(15.85)	(1.32)		(0.0017)
Population Density (Average Dwelling 1900)	7.06	6.51	0.54**		0.0024
	(3.12)	(1.66)	(0.22)		(0.0104)
Observations	303	242	545	545	545

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Cities included in the lead pipe subsample are cities with a municipal water supply using lead pipes according the 1897 *Manual of America Water-Works*, either exclusively or in addition to other pipe metals. Columns (1) and (2) give the unconditional means of city characteristics in the lead and non-lead sample, with standard deviations in parentheses below. Column (3) reports the difference from a t-test of the means with the standard error in parentheses below. Columns (4) and (5) report the coefficients from a linear probability model with the lead indicator variable as the outcome. 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).

Table 2: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Railroad Distance to Refinery Instrument

	Negative Binomial	First Stage		Instrumental NB	
	(1)	(2) All Cities	(3) Non-Refinery Cities	(4) All Cities	(5) Non-Refinery Cities
Lead Pipes	0.219***			1.022***	0.953***
	(0.064)			(0.257)	(0.263)
Lead Refinery Distance (100 miles)		−0.099***	−0.098***		
		(0.015)	(0.016)		
Log Population		0.077***	0.090***		
		(0.022)	(0.024)		
Black Population Share, 1900	0.026***	0.006*	0.006*	0.022***	0.022***
	(0.005)	(0.003)	(0.004)	(0.006)	(0.006)
Foreign-born Population Share, 1900	−0.016***	−0.001	−0.001	−0.016***	−0.015***
	(0.004)	(0.003)	(0.003)	(0.005)	(0.006)
Share Employed in Manufacturing 1900	0.001	0.000	0.000	0.001	0.001
	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
Literacy Rate 1900	0.009	0.003	0.002	−0.001	−0.000
	(0.011)	(0.006)	(0.006)	(0.012)	(0.012)
Share Single Men 18-40, 1900	0.044***	0.011	0.011	0.042***	0.043***
	(0.011)	(0.007)	(0.007)	(0.012)	(0.012)
Population Density (Average Dwelling 1900)	0.019**	0.003	0.001	0.007	0.007
	(0.010)	(0.007)	(0.007)	(0.020)	(0.027)
Home Ownership Rate, 1900	−0.004	−0.000	−0.000	−0.001	−0.000
	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	7434	7415	7080	7415	7080
Clusters	545	543	521	543	521
F-Statistic		45.98	39.13		
Log Likelihood	-15647	-4788	-4637	-15514	-14490

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. Column 1 presents the negative binomial regression with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. Column 2 presents the first stage OLS regression of lead pipe usage on the rail distance of the city to the nearest lead refinery. Column 4 presents the IV negative binomial regression using control function estimation, which controls for the residuals from the first stage. Standard errors for the IV estimates are calculated using a bootstrap resampling at the city level. Columns 3 and 5 replicate Columns 2 and 4, but for the sample of cities more than 10 miles by rail from a lead refinery. 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 homicide 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 is measured as shortest path distance along the 1900 railroad network from the city to the closest lead refinery as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).

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

Table 3: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Negative Binomial Regressions

	Lead Pipe Cities		Non-lead Pipe Cities		All Cities with pH Data	
	(1)	(2)	(3)	(4)	(5)	(6)
Lead Pipes					0.256***	0.242***
					(0.063)	(0.063)
pH in 1952, USGS	−0.094*		0.164**		0.134**	
	(0.050)		(0.067)		(0.065)	
Lead × pH (1952)					−0.224***	
					(0.076)	
Log pH in 1952, USGS		−0.594**		1.071***		0.895**
		(0.272)		(0.394)		(0.386)
Lead × log pH (1952)						−1.477***
						(0.448)
Black Population Share, 1900	0.023***	0.023***	0.026***	0.026***	0.023***	0.023***
	(0.008)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)
Foreign-born Population Share, 1900	−0.004	−0.004	−0.025***	−0.025***	−0.012**	−0.012**
	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)
Share Employed in Manufacturing 1900	0.001	0.001	0.004	0.004	0.001	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Literacy Rate 1900	0.008	0.008	0.011	0.011	0.004	0.004
	(0.021)	(0.020)	(0.010)	(0.010)	(0.012)	(0.012)
Share Single Men 18-40, 1900	0.043***	0.043***	0.043**	0.043**	0.041***	0.041***
	(0.013)	(0.013)	(0.020)	(0.019)	(0.011)	(0.011)
Population Density (Average Dwelling 1900)	−0.033	−0.035	−0.021	−0.021	−0.025	−0.026
	(0.029)	(0.029)	(0.025)	(0.024)	(0.022)	(0.022)
Home Ownership Rate, 1900	−0.005	−0.005	−0.010*	−0.010*	−0.007*	−0.007*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3367	3367	2597	2597	5964	5964
Clusters	233	233	193	193	426	426
Log Likelihood	-8336	-8334	-5112	-5111	-13500	-13496

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. All columns present negative binomial regressions with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. 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 homicides. pH is measured according to *The Industrial Utility of Public Water Supplies in the United States, 1952*. We recenter pH, in levels, such that neutral water, with a pH of 7, is zero. Acidic water, with a pH less than 7, is negative and basic water, with a pH greater than 7, is positive. For the log of pH, we take the log of the raw pH values and recenter around ln(7), such that neutral water would take a zero. 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.

Sources: Mortality Statistics of the U.S. from 1921 to 1936; 1900 Census; 1900 IPUMS 5% Census Sample; Baker (1897); Ingalls (1908); Lohr and Love (1954b,a).

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

	Lead Coefficient		
Cause of Death	Beta	SE	Deaths per 10,000
Homicide	0.219***	0.064	0.93
Total Mortality	−0.036	0.026	138.04
Circulatory Disease	−0.080**	0.031	25.27
Cancer and other malignant tumors	−0.063**	0.029	12.54
Nephritis	0.001	0.030	10.67
Cerebral hemorrhage	−0.070***	0.027	10.41
Pneumonia	−0.002	0.031	10.23
Congenital malformations	−0.026	0.028	8.03
Tuberculosis (All)	0.013	0.056	7.11
Tuberculosis of the Lungs	0.019	0.061	6.01
Influenza	0.078**	0.039	3.06
Appendicitis	0.061	0.043	2.50
Diabetes	−0.018	0.029	2.46
Diarrhea (under age 2)	0.150***	0.054	2.17
Childbirth	0.002	0.035	1.81
Hernia	0.012	0.036	1.68
Suicide	0.003	0.026	1.56
Tuberculosis (other)	0.013	0.044	1.11
Violent Accidents	0.046	0.035	1.00
Cirrhosis of the Liver	0.163***	0.040	0.91
Unknown or Ill-Identified	−0.114	0.136	0.86
Diphtheria	0.037	0.049	0.71
Bronchitis	0.047	0.044	0.63
Whooping cough	−0.016	0.038	0.57
Typhoid	0.025	0.067	0.50
Syphilis	0.050	0.067	0.46
Measles	0.049	0.047	0.44
Rheumatism	0.025	0.040	0.42
Scarlet fever	0.157***	0.054	0.29
Erysipelas	−0.073	0.045	0.26
Malaria	−0.004	0.217	0.13
Meningitis	0.062	0.085	0.12
Smallpox	0.219	0.186	0.02

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. Each row reports the result of a separate negative binomial regression with the count of the listed cause of death as the dependent variable. Each regression has a sample size of 7434 with 545 city clusters. 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. Other control variables include population, the African-American share of the population, the foreign-born share, the literacy rate, the home ownership rate, the share of single men aged 18 to 40, the share of employment in manufacturing, latitude, and longitude. Population, in logs, is measured contemporaneously to the death rates. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. All population share variables are measured as percentages.

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

A Appendix

A.1 Additional Figures

[Figure A.1 about here.]

Our outcome is the city-level homicide rate. We measure homicide 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 homicide (Grogger 1990). There are some cities in our sample without a homicide recorded in a given year and a subset of these cities that do not report a homicide during the entire sixteen-year sample. Figure A.2 shows that homicide rates are distributed in a highly left-skewed manner, with a large number of observations at or close to zero. Negative binomial regressions are more suitable than linear regressions for modeling outcomes distributed in this way (Cameron and Trivedi 2013).

[Figure A.2 about here.]

Figure 4 suggests that the relationship between a city's rail distance from the nearest lead refinery and its probability of using lead pipes could be non-linear. Figure A.3 shows that our results are robust to using both a city's rail distance and its rail distance squared as instrumental variables for lead pipe use. F-statistics from these regressions range from 8.71 to 27.02 in the top panel and 6.17 to 24.66 in the bottom panel.[37]

[Figure A.3 about here.]

A.2 Additional Maps

[Figure A.4 about here.]

[37] As in the main IV specification, 1931 and 1932 are the low-outliers in F-statistics because we observe a much smaller sample of cities in the mortality data. In all other years the F-statistics are greater than 15 and often greater than 20.

A.3 Alternative Specifications

A.3.1 Violent Deaths, 1906-1920

The *Mortality Statistics* did not begin reporting homicides at the city level until 1921. From 1906 to 1920, deaths by homicide were included in the categories "accidents," "other violence," "violent deaths (excluding suicide)," or "violent deaths." Because we are uncertain about whether these categories include deaths from homicide, poison, or accidents, we do not present them in the body of the paper.

Figure A.5 compares the effects of lead exposure on homicide from 1921 to 1936 to the effects of lead exposure on "violent deaths" from 1906 to 1920. The discontinuity in the effect size before and after 1920 suggests that counts of "violent deaths" and counts of homicide in the *Mortality Statistics* are not comparable. Because most municipal water systems were built in the final decades of the 19th century, it is possible that deaths from violence were no higher in cities using lead pipes than in cities using iron pipes because in many cities the first children exposed to lead through water had not yet entered adulthood. However, because we know neither which causes of death were included in the "violent deaths" categories nor the relative share of each cause within the total number of violent deaths, we hesitate to place a strong substantive interpretation on the estimates of the effect of lead exposure on deaths from violence.

[Figure A.5 about here.]

A.3.2 1880 Controls

In the results presented in the body of the paper, we control for city-level demographic and economic covariates using microdata from the the 1900 census. We use 1900 controls rather than contemporaneous controls to avoid inducing post-treatment bias. However, we observe whether cities used lead pipes as of 1897, and the majority of cities built water systems in the three decades preceding that year. It may therefore be preferable to use controls measured in earlier censuses.

We cannot use controls measured in 1890 because microdata from the 1890 census were destroyed. Using controls from the 1880 IPUMS 10% sample, meanwhile, has a major drawback: rather than 545 cities, we have complete data for only 189. In addition, data on home ownership rates are unavailable in the 1880 census sample.

With a sample 35% as large as the sample presented in the body of the paper, we expect that our results will be considerably noisier when we use 1880 controls rather than 1900 controls. Figure A.6 shows that this expectation is borne out. Consistent with Figure 6, the point estimate on the lead indicator is positive in every year. However, in this smaller sample of 189 cities, the 95% confidence intervals overlap with zero in half of the years. Tables A.1 and A.2 show our results in the pooled cross-sectional data. The lead effect remains positive and significant in this smaller sample with alternative pre-treatment controls in both the baseline negative binomial results and with the railroad distance instrument. Table A.2 shows that the results are robust to using the pH identification strategy as well: in cities with lead pipes, more basic water decreased homicides. In sum, although the results using 1880 controls are statistically weaker due to the smaller sample size, they are broadly consistent with the results using controls measured in 1900.

[Figure A.6 about here.]

[Table A.1 about here.]

[Table A.2 about here.]

A.3.3 1910, 1920, and 1930 Controls

In our main analysis, we use covariates measured in 1900 because lead exposure could affect covariates measured in later years. Including contemporary controls in our analyses could therefore induce post-treatment bias. Using contemporary controls is further complicated by the availability of decennial census samples. We calculate cities' black population share, foreign-born population share, literacy rate, home ownership rate, share of single men ages 18 to 40, manufacturing share, and population density in 1900 by aggregating the 1900 IPUMS 5% sample. However, in 1910 and 1920, only 1% IPUMS samples are available. We are less confident in the precision of our calculated control variables for these two sources because they are based on fewer observations per city in our sample.

In Figures A.7 and A.8 and Tables A.3, A.4, A.5, and A.6, we reproduce our results using controls from 1910 and 1920. As shown in Figure A.7, the point estimates using controls from 1910 are consistently positive and statistically distinguishable from zero in all but two years. The estimates using controls from 1920 are less precise, as we can generate 1920 covariates for only 227 cities, but they are always positive and statistically significant in nine years of our data. Tables A.3, A.4, A.5, and A.6 show that our results are robust in the pooled cross-sectional regressions as well.

[Figure A.7 about here.]

[Table A.3 about here.]

[Table A.4 about here.]

[Figure A.8 about here.]

[Table A.5 about here.]

[Table A.6 about here.]

One alternative to generating contemporaneous controls is to use the 1930 census, for which we have a sufficiently large 5% IPUMS sample. In Figure A.9 we report yearly estimates of homicides from 1930 to 1936 regressed on the lead pipe indicator and controls measured using the 1930 IPUMS 5% sample. The point estimates from these regressions are positive and statistically distinguishable from zero in all but two years. Tables A.7 and A.8 show estimates using 1930 controls in the pooled cross-sectional data. Here too the results are robust both our rail distance and pH identification strategies.

[Figure A.9 about here.]

[Table A.7 about here.]

[Table A.8 about here.]

A.3.4 Dropping the South

In Figures A.10, A.11, and A.12, and Tables A.9 and A.10, we show our results using a restricted sample of non-southern cities. As expected, the estimates are less precise, given the smaller sample size, but larger. This is consistent with our observation that southern cities, which had higher homicide rates than cities elsewhere in the country for reasons other than lead exposure, typically used iron pipes instead of lead. This empirical regularity exerts a downward bias on our baseline estimates of the effect of lead exposure. In our full sample, as shown in Figure 6, cities that used lead pipes had between 14 and 36 percent higher homicide rates than cities that did not. In the sample of non-southern cities, in contrast, cities using lead pipes had homicide rates that were between 15 and 52 percent higher. All but two of these estimates are statistically distinguishable from zero. Table A.9 shows that across all years, non-southern cities that used lead pipes had roughly 35 percent higher homicide rates than cities that used iron, as compared to 24 percent higher homicide rates in our full sample. The same pattern holds for our instrumental variables estimates, as shown in Figure A.11 and Table A.9. We also find that pH is negatively related to the homicide rate only in cities that used lead, but that the coefficients, as in the full sample, are statistically distinguishable from zero in just five years. One difference between our estimates of the pH effect in the full sample versus the non-South sample is that in the non-South sample the positive effect of pH in cities using iron pipes is apparent in only two years.

[Figure A.10 about here.]

[Table A.9 about here.]

[Figure A.11 about here.]

[Figure A.12 about here.]

[Table A.10 about here.]

A.3.5 Excluding Cities Close to Refineries

In the main results, we present our instrumental variables estimates using the full sample of cities and excluding cities located less than 10 miles by rail from a lead refinery or smelter. We drop these cities to rule out the possibility that lead pollution from a refinery or smelter had a direct effect on homicide. The literature on lead exposure from refineries and smelters suggests that elevated blood levels are concentrated within a few miles of where the lead is emitted (Landrigan et al. 1975; Albalak et al. 2003; García Vargas et al. 2001). In Figure A.13 and Table A.11, we reproduce our results using a smaller sample of cities located more than 20 miles from a lead refinery. Although our estimates are less precise in this smaller sample, we continue to observe large and statistically significant coefficients describing the relationship between lead pipe use and homicide.

[Figure A.13 about here.]

[Table A.11 about here.]

A.3.6 Linear Regression

Coefficients estimated using negative binomial regression with a population offset, like coefficients estimated using OLS regression of a logged rate, can be interpreted as semi-elasticities. In Figure A.14, we show that, when we drop all cities reporting zero homicides, our negative binomial regressions of homicide counts produce estimates that are very similar to OLS estimates of logged homicide rates.

[Figure A.14 about here.]

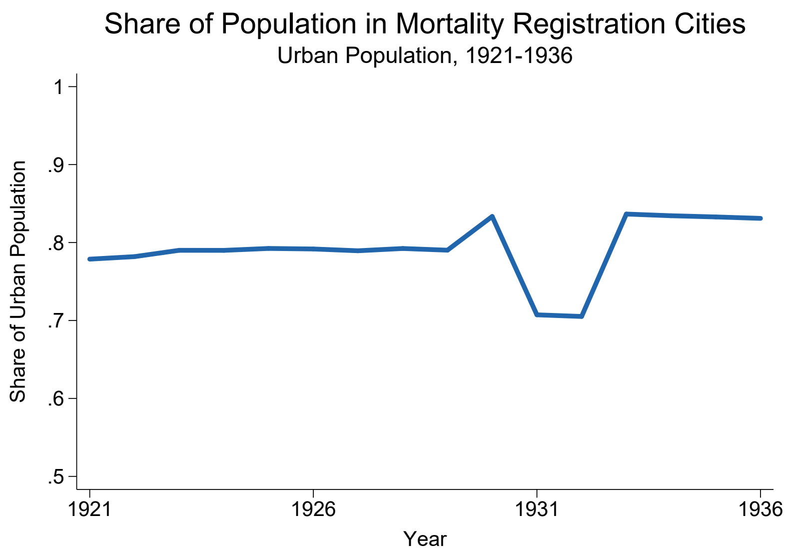


Figure A.1: Share of the national urban population covered by the registration cities of the *Mortality Statistics*. The selection of states into the *Mortality Statistics* sample is a well-known problem. However, the coverage of cities in the sample is relatively complete and stable throughout our sample 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.

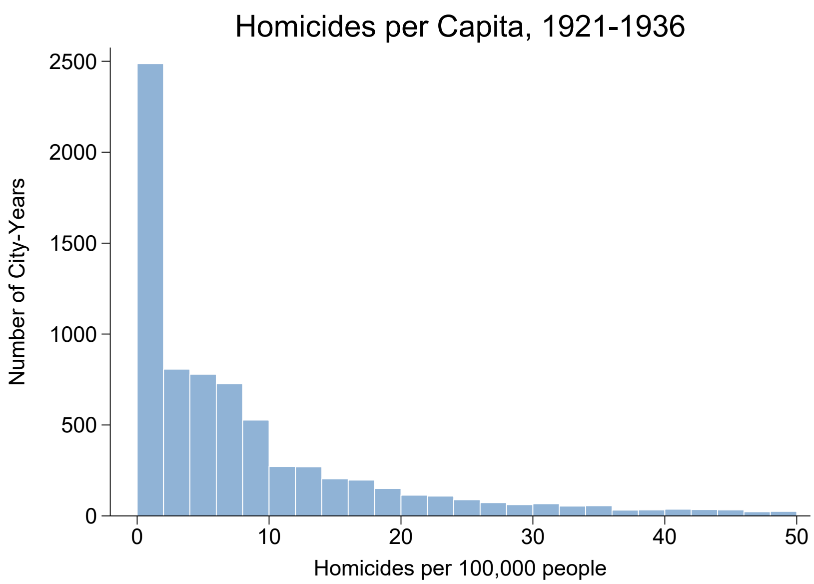
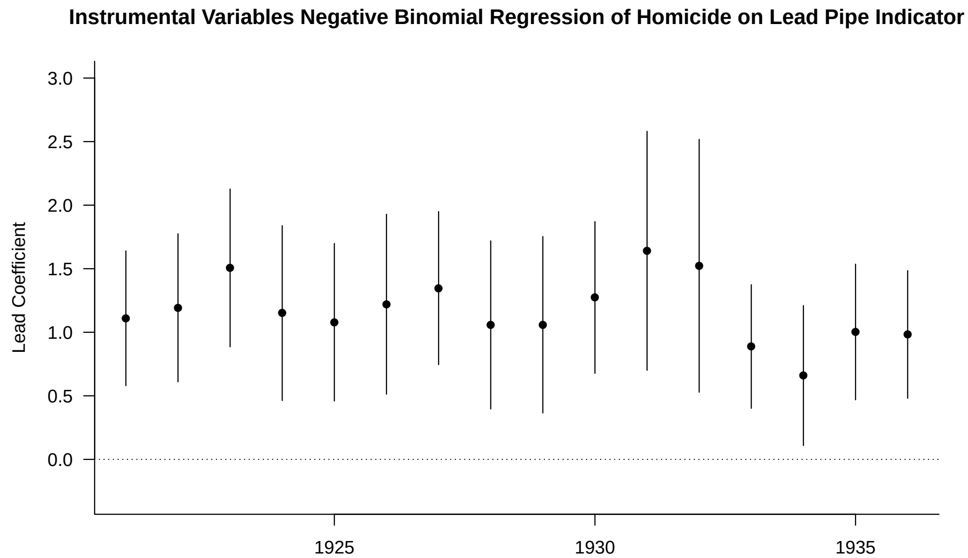


Figure A.2: Distribution of homicides per 100,000 people. When measured as the number of homicides per capita, the distribution of the outcome is highly left-skewed. We omit the 258 city-year observations with more than 50 homicides per capita from the histogram. The maximum observation is 125.9 homicides per 100,000 (17 homicides) in Greenville, MS in 1926.



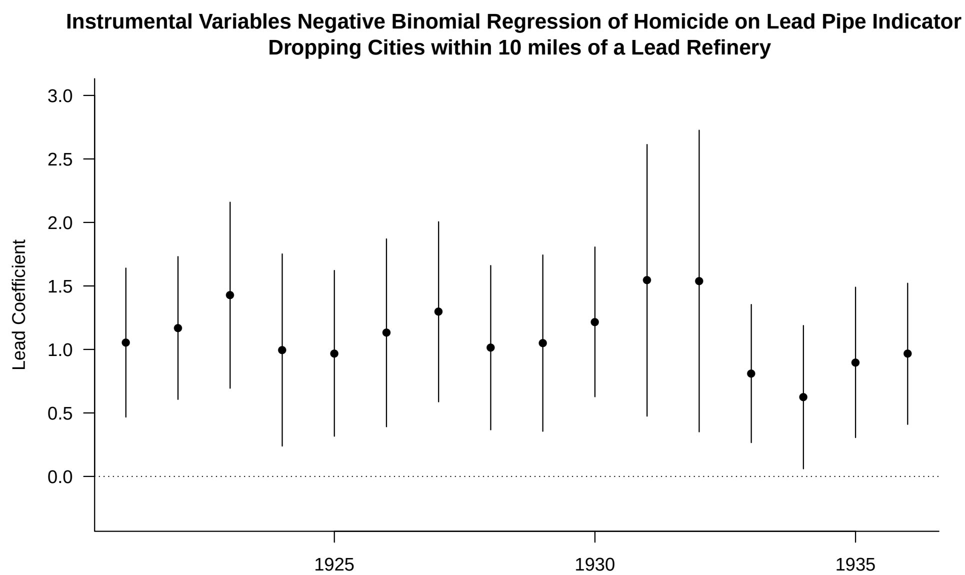
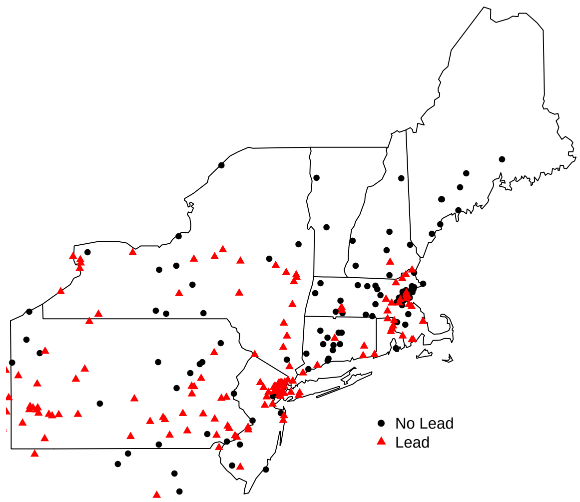
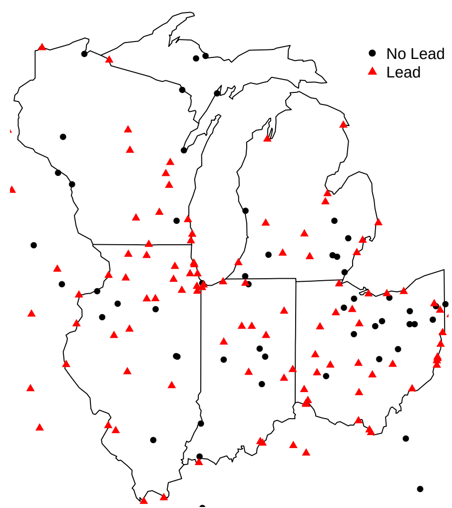


Figure A.3: Estimated lead coefficients from yearly IV negative binomial regressions with all controls and a population offset. Here the instrument is the rail distance and rail distance squared of each city from the nearest lead refinery. The top panel depicts the baseline IV estimates. The bottom panel depicts the IV estimates from a sample of cities located more than 10 miles from a lead refinery. Black dots represent the IV negative binomial point estimates. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. The instrument, distance and distance squared to the nearest lead refinery, is measured as shortest path distance along the 1900 railroad network from the city to closest lead refinery, as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).



(a) Cities in the Northeast



(b) Cities in the Midwest

Figure A.4: 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 "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.

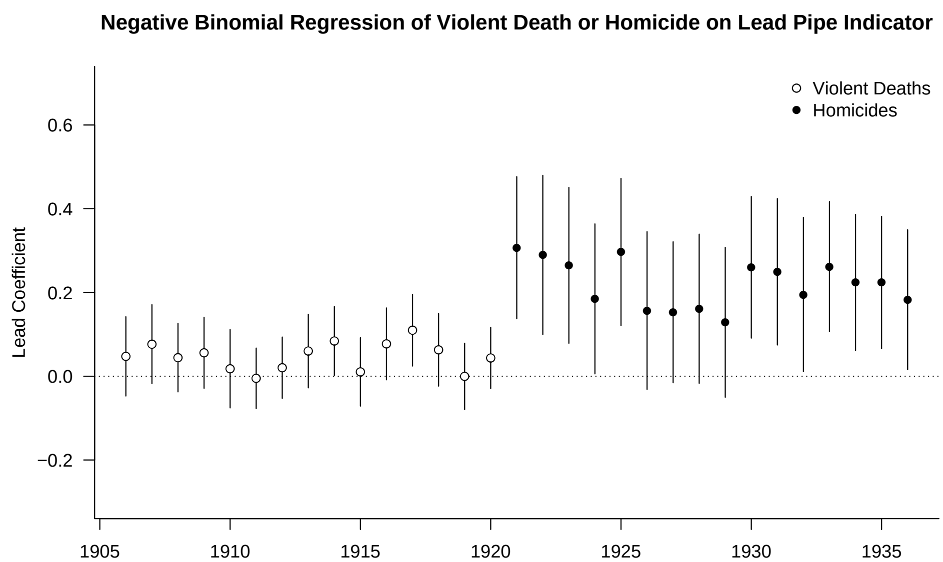


Figure A.5: Estimated lead coefficients from yearly negative binomial regressions with all controls and a population offset. White dots represent negative binomial point estimates from regressions of violent death counts on the lead pipe indicator. Black dots represent negative binomial point estimates from regressions of homicide counts on the lead pipe indicator. Bars around the estimates represent 95% confidence intervals. Violent death and homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1906 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample.

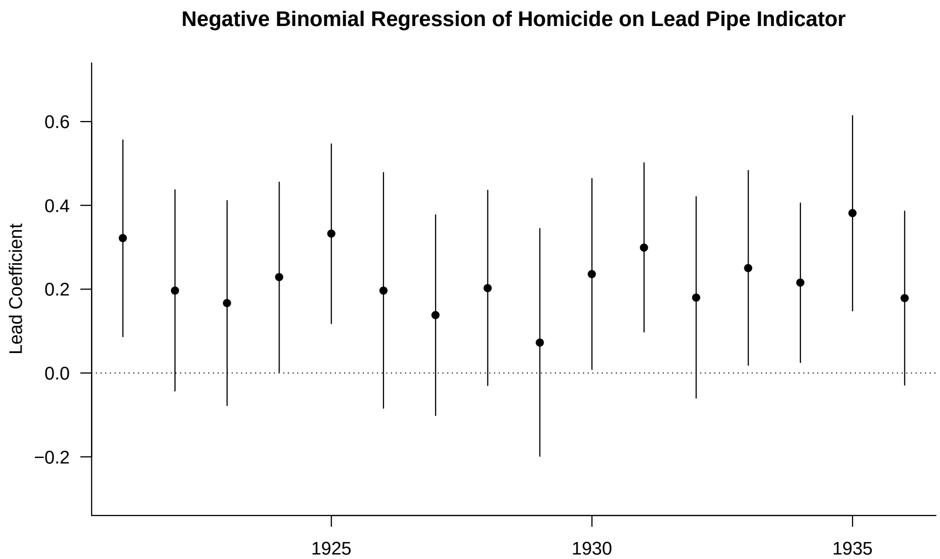


Figure A.6: Estimated lead coefficients from yearly negative binomial regressions with all 1880 controls and a population offset. Black dots represent the negative binomial point estimates. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1800 by aggregating the IPUMS 10% census sample. Using 1880 controls restricts the sample to 189 cities.

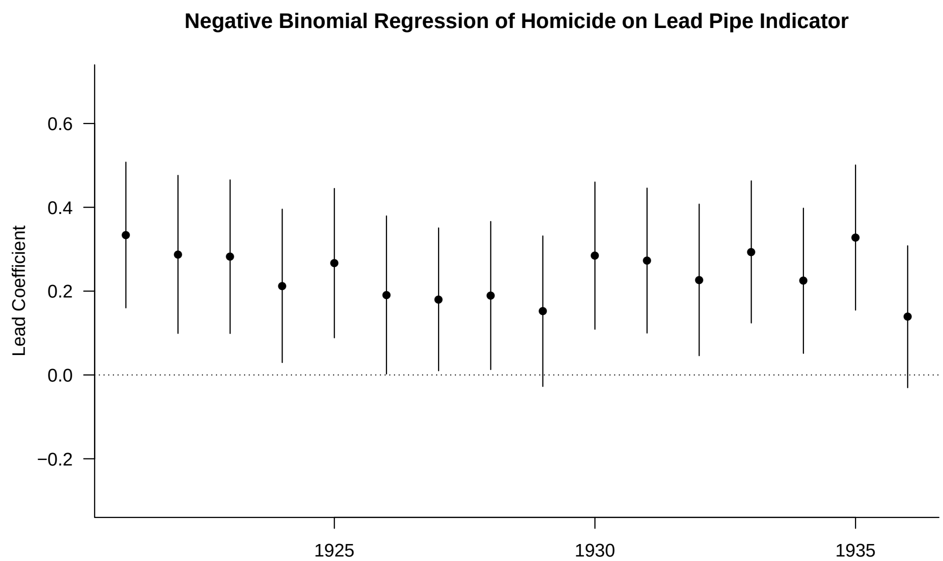


Figure A.7: Estimated lead coefficients from yearly negative binomial regressions with all 1910 controls and a population offset. Black dots represent the negative binomial point estimates. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1910 by aggregating the IPUMS 1% census sample.

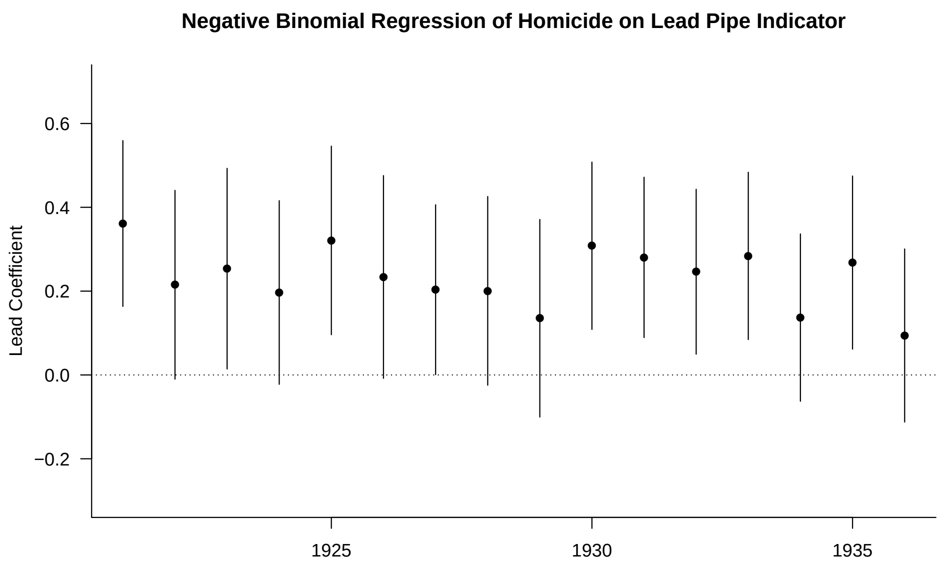


Figure A.8: Estimated lead coefficients from yearly negative binomial regressions with all 1920 controls and a population offset. Black dots represent the negative binomial point estimates. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1920 by aggregating the IPUMS 1% census sample.

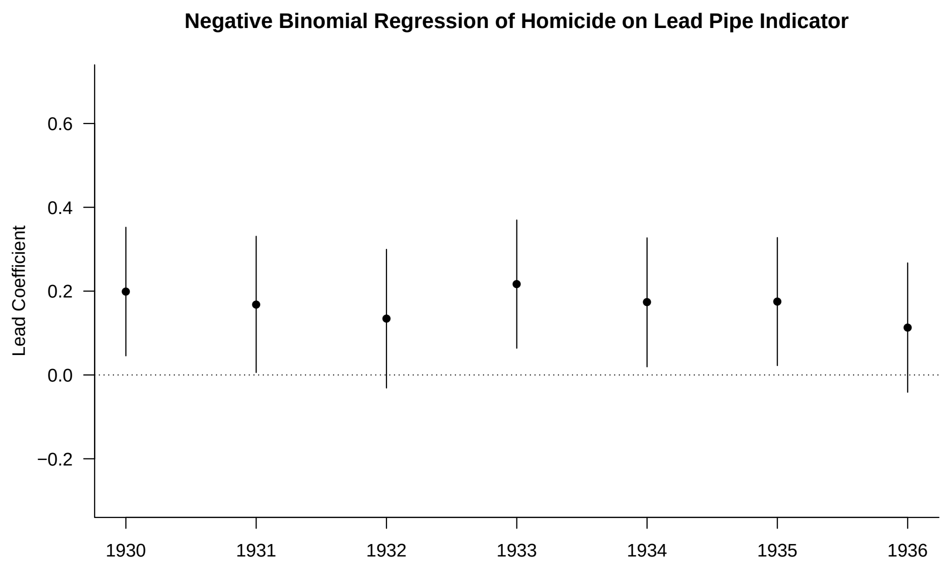


Figure A.9: Estimated lead coefficients from yearly negative binomial regressions with all 1930 controls and a population offset. Black dots represent the negative binomial point estimates. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1930 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1930 by aggregating the IPUMS 5% census sample.

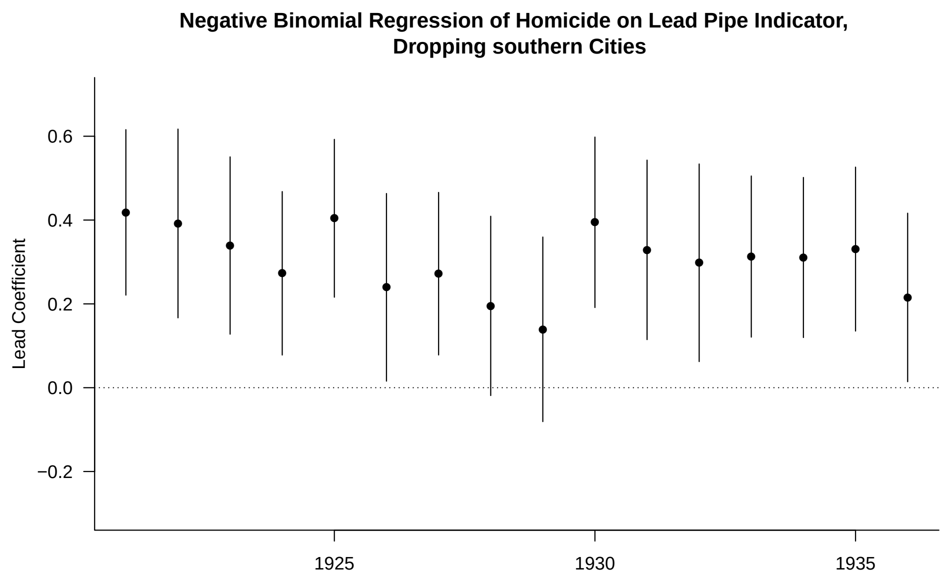
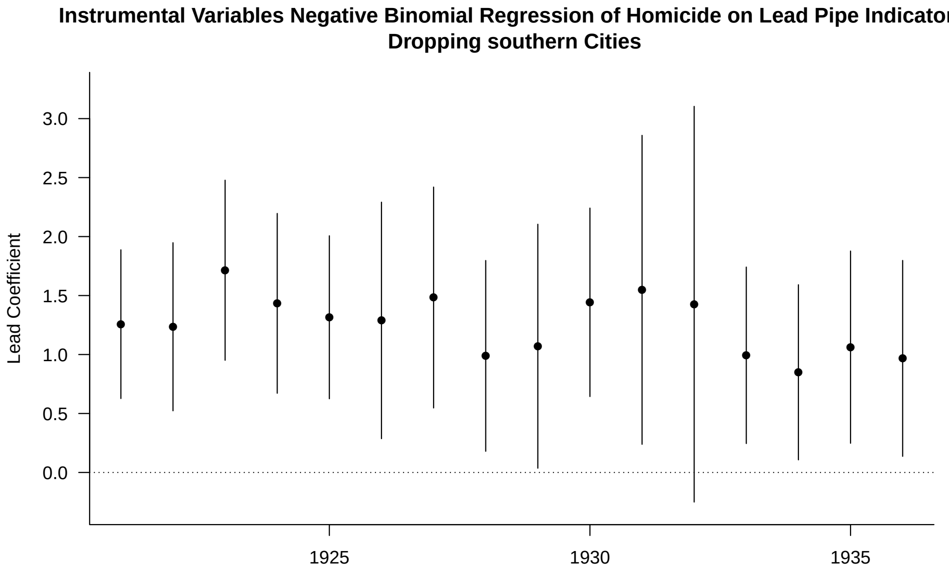


Figure A.10: Estimated lead coefficients from yearly negative binomial regressions with all controls and a population offset, excluding cities located in southern states (Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, Missouri, North Carolina, South Carolina, Tennessee, Texas, Virginia, and West Virginia). Black dots represent the negative binomial point estimates. Based on the lowest and highest point estimates, cities that used lead pipes had between 15 and 52 percent higher homicide rates than cities that did not. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample.



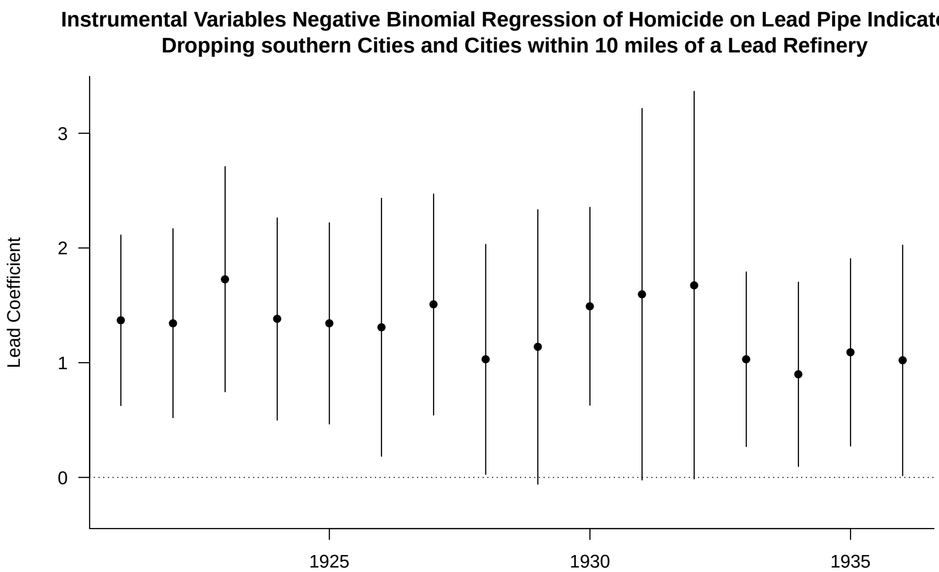
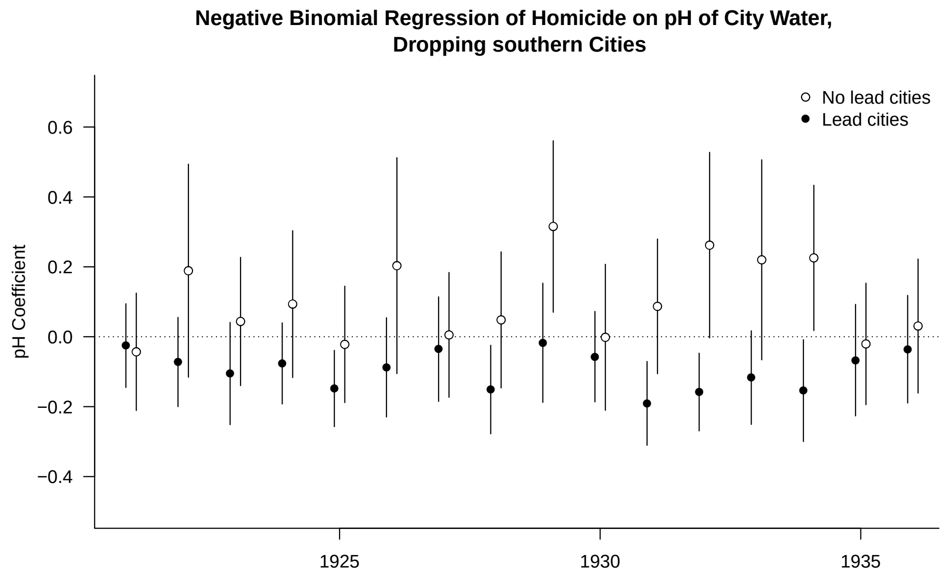


Figure A.11: Estimated lead coefficients from yearly IV negative binomial regressions with all controls and a population offset, excluding cities located in southern states (Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, Missouri, North Carolina, South Carolina, Tennessee, Texas, Virginia, and West Virginia). The top panel depicts the baseline IV estimates. The bottom panel depicts the IV estimates from a sample of cities located more than 10 miles from a lead refinery. Black dots represent the IV negative binomial point estimates. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. The instrument, distance to the nearest lead refinery, is measured as shortest path distance along the 1900 railroad network from the city to closest lead refinery, as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).



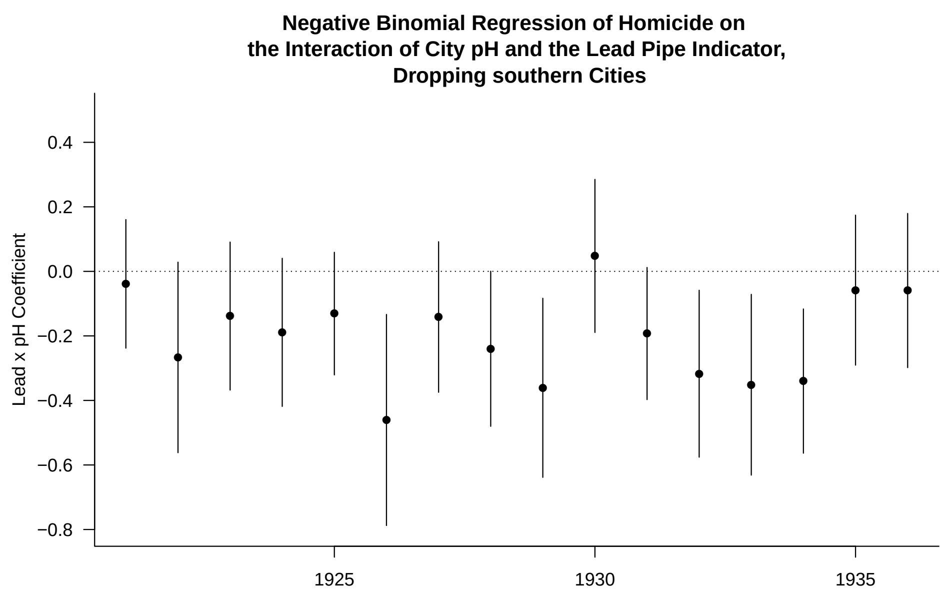
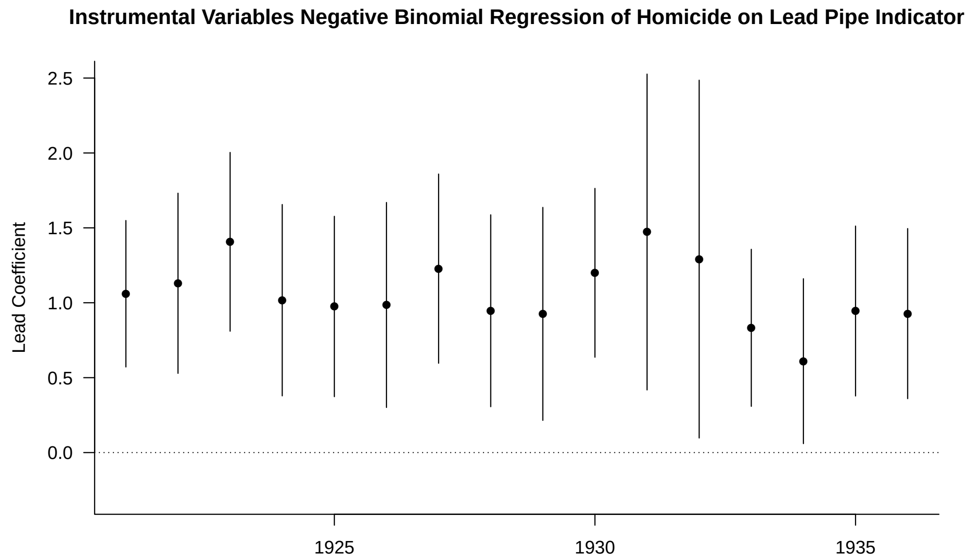


Figure A.12: Estimated pH coefficients from yearly negative binomial regressions with all controls and a population offset, excluding cities located in southern states (Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, Missouri, North Carolina, South Carolina, Tennessee, Texas, Virginia, and West Virginia). In the top panel, black dots represent pH point estimates from the sample of cities using lead pipes and white dots represent pH point estimates from the sample of cities using iron pipes. In the bottom panel, black dots represent point estimates from the interaction of pH with the lead pipe indicator. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. pH is measured according to *The Industrial Utility of Public Water Supplies in the United States, 1952.*



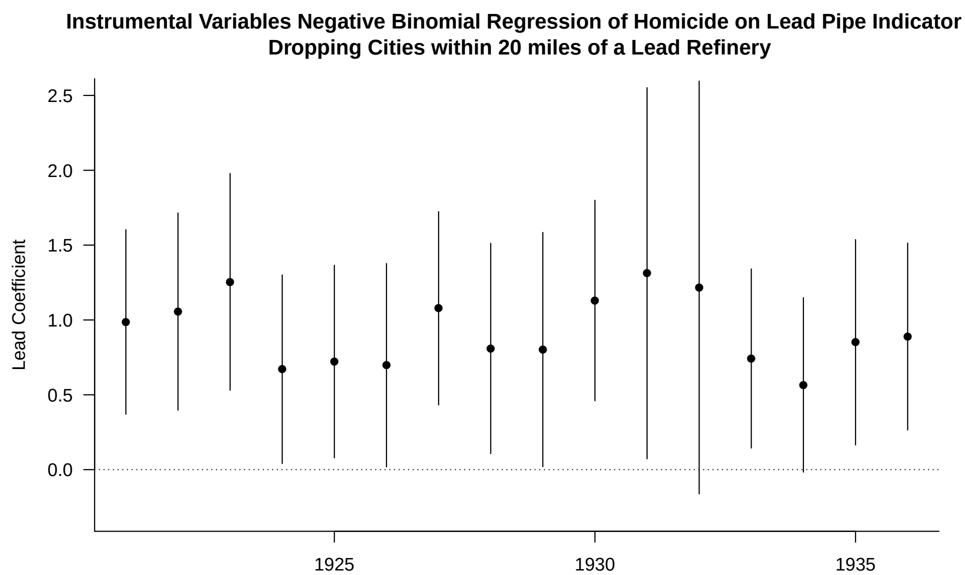


Figure A.13: Estimated lead coefficients from yearly IV negative binomial regressions with all controls and a population offset. The top panel depicts the baseline IV estimates. The bottom panel depicts the IV estimates from a sample of cities located more than 20 miles from a lead refinery. Black dots represent the IV negative binomial point estimates. Bars around the estimates represent 95% confidence intervals. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample. The instrument, distance to the nearest lead refinery, is measured as shortest path distance along the 1900 railroad network from the city to closest lead refinery, as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).

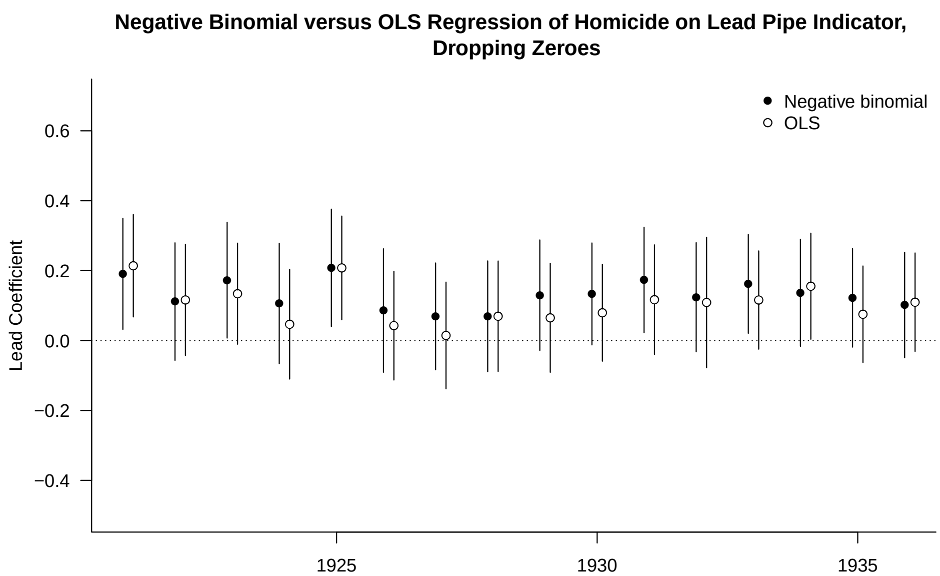


Figure A.14: Estimated lead coefficients from yearly negative binomial regressions with all controls and a population offset and OLS regressions of a logged rate. Black dots represent the negative binomial point estimates. White dots represent the OLS estimates. Bars around the estimates represent 95% confidence intervals. Negative binomial regressions of homicide counts generate similar estimates to OLS regressions of logged homicide rates when we drop all cities reporting no homicides. Homicide data are drawn from cities reporting data to the *Mortality Statistics* between 1921 and 1936. Population data are interpolated using decennial census data. All other control variables are measured in 1900 by aggregating the IPUMS 5% census sample.

Table A.1: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Railroad Distance to Refinery Instrument with 1880 Controls

	Negative Binomial	First Stage		Instrumental NB	
	(1)	(2) All Cities	(3) Non-Refinery Cities	(4) All Cities	(5) Non-Refinery Cities
Lead Pipes	0.225**			1.630***	1.427***
	(0.090)			(0.569)	(0.383)
Lead Refinery Distance (100 miles)		−0.091***	−0.107***		
		(0.028)	(0.030)		
Log Population		0.066**	0.070**		
		(0.028)	(0.032)		
Black Population Share, 1880	0.035***	−0.002	−0.000	0.032*	0.028**
	(0.011)	(0.006)	(0.006)	(0.017)	(0.014)
Foreign-born Population Share, 1880	−0.000	0.002	0.004	−0.008	−0.008
	(0.009)	(0.006)	(0.006)	(0.013)	(0.013)
Literacy Rate 1880	0.026**	−0.007	−0.006	0.017	0.012
	(0.013)	(0.008)	(0.009)	(0.021)	(0.018)
Share Employed in Manufacturing 1880	−0.006	−0.003	−0.004	−0.001	−0.000
	(0.004)	(0.003)	(0.003)	(0.007)	(0.006)
Share Single Men 18-40, 1880	−0.004	−0.029**	−0.028*	0.037	0.020
	(0.019)	(0.015)	(0.017)	(0.041)	(0.034)
Population Density (Average Dwelling 1880)	0.018	0.013	0.013	−0.011	−0.006
	(0.015)	(0.012)	(0.013)	(0.036)	(0.043)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	2981	2981	2805	2981	2805
Clusters	190	190	179	190	179
F-Statistic		10.65	13.06		
Log Likelihood	-7883	-1755	-1667	-7041	-7041

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. Column 1 presents the negative binomial regression with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. Column 2 presents the first stage OLS regression of lead pipe usage on the rail distance of the city to the nearest lead refinery. Column 4 presents the IV negative binomial regression using control function estimation, which controls for the residuals from the first stage. Standard errors for the IV estimates are calculated using a bootstrap resampling at the city level. Columns 3 and 5 replicate Columns 2 and 4, but for the sample of cities more than 10 miles by rail from a lead refinery. 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 homicide rate. All other control variables are measured in 1880 by aggregating the IPUMS 10% 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 shortest path distance along the 1900 railroad network from the city to closest lead refinery, as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).

Sources: Mortality Statistics of the U.S. from 1921 to 1936; 1880 Census; 1880 IPUMS 10% Census Sample; Baker (1897); Ingalls (1908).

Table A.2: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Negative Binomial Regressions with 1880 Controls

	Lead Pipe Cities		Non-lead Pipe Cities		All Cities with pH Data	
	(1)	(2)	(3)	(4)	(5)	(6)
Lead Pipes					0.296***	0.278***
					(0.086)	(0.088)
pH in 1952, USGS	−0.115**		0.229***		0.168*	
	(0.045)		(0.077)		(0.094)	
Lead × pH (1952)					−0.293***	
					(0.108)	
Log pH in 1952, USGS		−0.697***		1.390***		0.972*
		(0.224)		(0.448)		(0.550)
Lead × log pH (1952)						−1.735***
						(0.618)
Black Population Share, 1880	0.033***	0.033***	0.001	0.001	0.032***	0.032***
	(0.012)	(0.012)	(0.021)	(0.021)	(0.011)	(0.011)
Foreign-born Population Share, 1880	0.003	0.003	−0.026	−0.027	−0.001	−0.001
	(0.011)	(0.011)	(0.018)	(0.018)	(0.010)	(0.010)
Literacy Rate 1880	0.023	0.022	−0.014	−0.013	0.020	0.020
	(0.017)	(0.017)	(0.024)	(0.023)	(0.013)	(0.014)
Share Employed in Manufacturing 1880	−0.006	−0.006	−0.002	−0.002	−0.004	−0.004
	(0.005)	(0.005)	(0.008)	(0.008)	(0.004)	(0.004)
Share Single Men 18-40, 1880	−0.022	−0.022	0.059*	0.061*	−0.002	−0.001
	(0.030)	(0.030)	(0.035)	(0.035)	(0.021)	(0.021)
Population Density (Average Dwelling 1880)	−0.002	−0.002	−0.121**	−0.122**	−0.013	−0.013
	(0.030)	(0.031)	(0.057)	(0.057)	(0.028)	(0.028)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1746	1746	933	933	2679	2679
Clusters	111	111	60	60	171	171
Log Likelihood	-5122	-5121	-1973	-1973	-7145	-7144

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. All columns present negative binomial regressions with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. 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 homicides. pH is measured according to *The Industrial Utility of Public Water Supplies in the United States, 1952*. We recenter pH, in levels, such that neutral water, with a pH of 7, is zero. Acidic water, with a pH less than 7, is negative and basic water, with a pH greater than 7, is positive. For the log of pH, we take the log of the raw pH values and recenter around ln(7), such that neutral water would take a zero. All other control variables are measured in 1880 by aggregating the IPUMS 10% census sample. All population share variables are measured as percentages. Geographic controls include latitude and longitude.

Sources: Mortality Statistics of the U.S. from 1921 to 1936; 1880 Census; 1880 IPUMS 10% Census Sample; Baker (1897); Ingalls (1908); Lohr and Love (1954b,a).

Table A.3: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Railroad Distance to Refinery Instrument with 1910 Controls

	Negative Binomial	First Stage		Instrumental NB	
	(1)	(2) All Cities	(3) Non-Refinery Cities	(4) All Cities	(5) Non-Refinery Cities
Lead Pipes	0.238***			0.933***	0.827***
	(0.066)			(0.235)	(0.244)
Lead Refinery Distance (100 miles)		−0.107***	−0.102***		
		(0.016)	(0.018)		
Log Population		0.076***	0.092***		
		(0.022)	(0.024)		
Black Population Share, 1910	0.024***	0.005	0.005	0.023***	0.024***
	(0.005)	(0.003)	(0.003)	(0.006)	(0.006)
Foreign-born Population Share, 1910	−0.014***	−0.003	−0.005	−0.012**	−0.012*
	(0.005)	(0.003)	(0.003)	(0.006)	(0.006)
Literacy Rate 1910	−0.002	−0.008	−0.010	−0.004	−0.003
	(0.011)	(0.007)	(0.007)	(0.012)	(0.013)
Share Employed in Manufacturing 1910	0.000	−0.001	−0.001	0.000	−0.000
	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)
Share Single Men 18-40, 1910	0.048***	0.007	0.008	0.041***	0.044***
	(0.010)	(0.006)	(0.006)	(0.012)	(0.012)
Population Density (Average Dwelling 1910)	0.016**	0.000	−0.000	0.010	0.012
	(0.007)	(0.005)	(0.005)	(0.018)	(0.023)
Home Ownership Rate, 1910	0.002	0.000	0.000	0.003	0.005
	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	6313	6299	5969	6299	5969
Clusters	423	422	401	422	401
F-Statistic		42.62	33.57		
Log Likelihood	-14311	-3995	-3843	-13155	-13155

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. Column 1 presents the negative binomial regression with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. Column 2 presents the first stage OLS regression of lead pipe usage on the rail distance of the city to the nearest lead refinery. Column 4 presents the IV negative binomial regression using control function estimation, which controls for the residuals from the first stage. Standard errors for the IV estimates are calculated using a bootstrap resampling at the city level. Columns 3 and 5 replicate Columns 2 and 4, but for the sample of cities more than 10 miles by rail from a lead refinery. 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 homicide rate. All other control variables are measured in 1910 by aggregating the IPUMS 1% 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 shortest path distance along the 1900 railroad network from the city to closest lead refinery, as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).

Sources: *Mortality Statistics* of the U.S. from 1921 to 1936; 1880 Census; 1880 IPUMS 10% Census Sample; Baker (1897); Ingalls (1908).

Table A.4: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Negative Binomial Regressions with 1910 Controls

	Lead Pipe Cities		Non-lead Pipe Cities		All Cities with pH Data	
	(1)	(2)	(3)	(4)	(5)	(6)
Lead Pipes					0.263***	0.251***
					(0.065)	(0.064)
pH in 1952, USGS	−0.118***		0.138*		0.078	
	(0.044)		(0.079)		(0.083)	
Lead × pH (1952)					−0.187**	
					(0.092)	
Log pH in 1952, USGS		−0.694***		0.915**		0.554
		(0.242)		(0.438)		(0.467)
Lead × log pH (1952)						−1.220**
						(0.522)
Black Population Share, 1910	0.027***	0.027***	0.020***	0.020***	0.026***	0.026***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)
Foreign-born Population Share, 1910	0.002	0.002	−0.025***	−0.026***	−0.006	−0.006
	(0.008)	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)
Literacy Rate 1910	0.020	0.019	−0.019*	−0.019*	0.006	0.005
	(0.016)	(0.016)	(0.010)	(0.010)	(0.012)	(0.012)
Share Employed in Manufacturing 1910	0.002	0.002	−0.001	−0.001	0.000	0.000
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
Share Single Men 18-40, 1910	0.053***	0.052***	0.026**	0.026**	0.042***	0.042***
	(0.014)	(0.014)	(0.012)	(0.012)	(0.010)	(0.010)
Population Density (Average Dwelling 1910)	−0.025	−0.025	−0.017	−0.017	−0.023	−0.023
	(0.024)	(0.024)	(0.023)	(0.023)	(0.017)	(0.017)
Home Ownership Rate, 1910	0.004	0.003	−0.002	−0.002	0.002	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3132	3132	2159	2159	5291	5291
Clusters	206	206	148	148	354	354
Log Likelihood	-8013	-8013	-4476	-4475	-12580	-12578

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. All columns present negative binomial regressions with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. 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 homicides. pH is measured according to *The Industrial Utility of Public Water Supplies in the United States, 1952*. We recenter pH, in levels, such that neutral water, with a pH of 7, is zero. Acidic water, with a pH less than 7, is negative and basic water, with a pH greater than 7, is positive. For the log of pH, we take the log of the raw pH values and recenter around ln(7), such that neutral water would take a zero. All other control variables are measured in 1910 by aggregating the IPUMS 1% census sample. All population share variables are measured as percentages. Geographic controls include latitude and longitude.

Sources: Mortality Statistics of the U.S. from 1921 to 1936; 1880 Census; 1880 IPUMS 10% Census Sample; Baker (1897); Ingalls (1908); Lohr and Love (1954b,a).

Table A.5: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Railroad Distance to Refinery Instrument with 1920 Controls

	Negative Binomial	First Stage		Instrumental NB	
	(1)	(2) All Cities	(3) Non-Refinery Cities	(4) All Cities	(5) Non-Refinery Cities
Lead Pipes	0.233***			1.056***	0.834**
	(0.085)			(0.362)	(0.348)
Lead Refinery Distance (100 miles)		−0.081***	−0.078***		
		(0.022)	(0.024)		
Log Population		0.085***	0.104***		
		(0.033)	(0.036)		
Black Population Share, 1920	0.022***	0.012**	0.011**	0.014	0.017*
	(0.006)	(0.005)	(0.005)	(0.010)	(0.010)
Foreign-born Population Share, 1920	−0.018***	0.003	0.001	−0.020**	−0.017*
	(0.007)	(0.005)	(0.005)	(0.009)	(0.009)
Literacy Rate 1920	−0.021	−0.011	−0.016	−0.018	−0.017
	(0.019)	(0.012)	(0.013)	(0.026)	(0.024)
Share Employed in Manufacturing 1920	−0.004	0.000	−0.000	−0.004	−0.004
	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)
Share Single Men 18-40, 1920	0.060***	−0.021	−0.017	0.072***	0.066***
	(0.016)	(0.015)	(0.015)	(0.023)	(0.022)
Population Density (Average Dwelling 1920)	0.008	−0.002	−0.004	0.004	0.007
	(0.008)	(0.004)	(0.004)	(0.014)	(0.016)
Home Ownership Rate, 1920	0.002	0.001	0.001	0.002	0.004
	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	3604	3604	3348	3604	3348
Clusters	227	227	211	227	211
F-Statistic		13.78	10.10		
Log Likelihood	-10146	-2131	-2024	-9185	-9185

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. Column 1 presents the negative binomial regression with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. Column 2 presents the first stage OLS regression of lead pipe usage on the rail distance of the city to the nearest lead refinery. Column 4 presents the IV negative binomial regression using control function estimation, which controls for the residuals from the first stage. Standard errors for the IV estimates are calculated using a bootstrap resampling at the city level. Columns 3 and 5 replicate Columns 2 and 4, but for the sample of cities more than 10 miles by rail from a lead refinery. 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 homicide rate. All other control variables are measured in 1920 by aggregating the IPUMS 1% 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 shortest path distance along the 1900 railroad network from the city to closest lead refinery, as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).

Sources: *Mortality Statistics* of the U.S. from 1921 to 1936; 1880 Census; 1880 IPUMS 10% Census Sample; Baker (1897); Ingalls (1908).

Table A.6: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Negative Binomial Regressions with 1920 Controls

	Lead Pipe Cities		Non-lead Pipe Cities		All Cities with pH Data	
	(1)	(2)	(3)	(4)	(5)	(6)
Lead Pipes					0.266***	0.250***
					(0.077)	(0.078)
pH in 1952, USGS	−0.096**		0.233**		0.145	
	(0.046)		(0.101)		(0.097)	
Lead × pH (1952)					−0.237**	
					(0.106)	
Log pH in 1952, USGS		−0.576**		1.469***		0.947*
		(0.237)		(0.527)		(0.516)
Lead × log pH (1952)						−1.519***
						(0.570)
Black Population Share, 1920	0.031***	0.031***	0.008	0.007	0.023***	0.023***
	(0.009)	(0.009)	(0.008)	(0.008)	(0.006)	(0.006)
Foreign-born Population Share, 1920	0.000	−0.000	−0.028**	−0.028**	−0.009	−0.010
	(0.010)	(0.010)	(0.011)	(0.011)	(0.008)	(0.008)
Literacy Rate 1920	−0.001	−0.001	−0.024	−0.023	−0.012	−0.012
	(0.027)	(0.027)	(0.028)	(0.028)	(0.021)	(0.021)
Share Employed in Manufacturing 1920	0.001	0.001	−0.010*	−0.010**	−0.004	−0.004
	(0.004)	(0.004)	(0.005)	(0.005)	(0.003)	(0.003)
Share Single Men 18-40, 1920	0.026	0.026	0.074***	0.075***	0.048***	0.048***
	(0.023)	(0.023)	(0.022)	(0.022)	(0.016)	(0.016)
Population Density (Average Dwelling 1920)	−0.031	−0.031	−0.046	−0.047	−0.029	−0.029
	(0.028)	(0.028)	(0.038)	(0.038)	(0.023)	(0.023)
Home Ownership Rate, 1920	0.002	0.002	−0.012	−0.011	0.001	0.001
	(0.007)	(0.007)	(0.009)	(0.009)	(0.005)	(0.005)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2038	2038	1102	1102	3140	3140
Clusters	128	128	70	70	198	198
Log Likelihood	-6057	-6056	-2881	-2880	-9018	-9016

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. All columns present negative binomial regressions with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. 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 homicides. pH is measured according to *The Industrial Utility of Public Water Supplies in the United States, 1952*. We recenter pH, in levels, such that neutral water, with a pH of 7, is zero. Acidic water, with a pH less than 7, is negative and basic water, with a pH greater than 7, is positive. For the log of pH, we take the log of the raw pH values and recenter around ln(7), such that neutral water would take a zero. All other control variables are measured in 1920 by aggregating the IPUMS 1% census sample. All population share variables are measured as percentages. Geographic controls include latitude and longitude.

Sources: Mortality Statistics of the U.S. from 1921 to 1936; 1880 Census; 1880 IPUMS 10% Census Sample; Baker (1897); Ingalls (1908); Lohr and Love (1954b,a).

Table A.7: Effects of Lead Water Pipes on Homicides from 1930 to 1936, Railroad Distance to Refinery Instrument with 1930 Controls

	Negative Binomial	First Stage		Instrumental NB	
	(1)	(2) All Cities	(3) Non-Refinery Cities	(4) All Cities	(5) Non-Refinery Cities
Lead Pipes	0.171***			0.792***	0.751***
	(0.059)			(0.176)	(0.183)
Lead Refinery Distance (100 miles)		−0.091***	−0.088***		
		(0.014)	(0.015)		
Log Population		0.101***	0.112***		
		(0.020)	(0.021)		
Black Population Share, 1930	0.033***	0.001	0.001	0.031***	0.031***
	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)
Foreign-born Population Share, 1930	−0.035***	−0.009***	−0.010***	−0.032***	−0.031***
	(0.005)	(0.003)	(0.003)	(0.006)	(0.006)
Literacy Rate 1930	0.003	−0.030**	−0.031**	0.005	0.006
	(0.018)	(0.012)	(0.013)	(0.020)	(0.020)
Share Employed in Manufacturing 1930	−0.000	−0.002	−0.002	−0.000	−0.000
	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Share Single Men 18-40, 1930	0.075***	0.019*	0.017*	0.056***	0.059***
	(0.015)	(0.010)	(0.010)	(0.019)	(0.017)
Population Density (Average Dwelling 1930)	0.004	0.002	0.002	−0.003	−0.002
	(0.005)	(0.002)	(0.002)	(0.009)	(0.009)
Home Ownership Rate, 1930	−0.013***	0.003	0.003	−0.015***	−0.014***
	(0.004)	(0.002)	(0.003)	(0.004)	(0.004)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	3308	3298	3152	3298	3152
Clusters	552	550	528	550	528
F-Statistic		43.52	35.95		
Log Likelihood	-6852	-2074	-2003	-6383	-6383

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. Column 1 presents the negative binomial regression with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. Column 2 presents the first stage OLS regression of lead pipe usage on the rail distance of the city to the nearest lead refinery. Column 4 presents the IV negative binomial regression using control function estimation, which controls for the residuals from the first stage. Standard errors for the IV estimates are calculated using a bootstrap resampling at the city level. Columns 3 and 5 replicate Columns 2 and 4, but for the sample of cities more than 10 miles by rail from a lead refinery. 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 homicide rate. All other control variables are measured in 1930 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 shortest path distance along the 1900 railroad network from the city to closest lead refinery, as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).

Sources: Mortality Statistics of the U.S. from 1930 to 1936; 1930 Census; 1930 IPUMS 5% Census Sample; Baker (1897); Ingalls (1908).

Table A.8: Effects of Lead Water Pipes on Homicides from 1930 to 1936, Negative Binomial Regressions with 1930 Controls

	Lead Pipe Cities		Non-lead Pipe Cities		All Cities with pH Data	
	(1)	(2)	(3)	(4)	(5)	(6)
Lead Pipes					0.180***	0.173***
					(0.062)	(0.061)
pH in 1952, USGS	−0.116***		0.034		0.014	
	(0.045)		(0.074)		(0.071)	
Lead × pH (1952)					−0.117	
					(0.082)	
Log pH in 1952, USGS		−0.681***		0.311		0.177
		(0.245)		(0.431)		(0.425)
Lead × log pH (1952)						−0.803*
						(0.485)
Black Population Share, 1930	0.037***	0.037***	0.025***	0.025***	0.033***	0.033***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)
Foreign-born Population Share, 1930	−0.021***	−0.021***	−0.050***	−0.050***	−0.030***	−0.031***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.005)	(0.005)
Literacy Rate 1930	0.022	0.021	−0.006	−0.007	0.011	0.010
	(0.022)	(0.022)	(0.027)	(0.028)	(0.018)	(0.018)
Share Employed in Manufacturing 1930	−0.000	−0.000	−0.000	−0.000	−0.000	−0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Share Single Men 18-40, 1930	0.068***	0.067***	0.069**	0.069**	0.066***	0.066***
	(0.016)	(0.016)	(0.029)	(0.029)	(0.014)	(0.014)
Population Density (Average Dwelling 1930)	−0.016**	−0.017**	0.023	0.023	−0.010	−0.011
	(0.007)	(0.007)	(0.039)	(0.039)	(0.008)	(0.007)
Home Ownership Rate, 1930	−0.007	−0.007	−0.013*	−0.013*	−0.011***	−0.011***
	(0.005)	(0.005)	(0.007)	(0.007)	(0.004)	(0.004)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1464	1464	1162	1162	2626	2626
Clusters	234	234	197	197	431	431
Log Likelihood	-3552	-3552	-2317	-2317	-5891	-5890

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. All columns present negative binomial regressions with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. 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 homicides. pH is measured according to *The Industrial Utility of Public Water Supplies in the United States, 1952.* We recenter pH, in levels, such that neutral water, with a pH of 7, is zero. Acidic water, with a pH less than 7, is negative and basic water, with a pH greater than 7, is positive. For the log of pH, we take the log of the raw pH values and recenter around ln(7), such that neutral water would take a zero. All other control variables are measured in 1930 by aggregating the IPUMS 5% census sample. All population share variables are measured as percentages. Geographic controls include latitude and longitude.

Sources: Mortality Statistics of the U.S. from 1930 to 1936; 1930 Census; 1930 IPUMS 5% Census Sample; Baker (1897); Ingalls (1908); Lohr and Love (1954b,a).

Table A.9: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Railroad Distance to Refinery Instrument, Southern States Excluded

	Negative Binomial	First Stage		Instrumental NB	
	(1)	(2) All Cities	(3) Non-Refinery Cities	(4) All Cities	(5) Non-Refinery Cities
Lead Pipes	0.300***			1.197***	1.250***
	(0.070)			(0.350)	(0.375)
Lead Refinery Distance (100 miles)		−0.110***	−0.103***		
		(0.018)	(0.019)		
Log Population		0.087***	0.097***		
		(0.023)	(0.025)		
Black Population Share, 1900	0.047***	0.002	0.002	0.038***	0.037***
	(0.008)	(0.006)	(0.006)	(0.011)	(0.011)
Foreign-born Population Share, 1900	−0.008	−0.001	−0.002	−0.013*	−0.011
	(0.006)	(0.003)	(0.003)	(0.007)	(0.007)
Literacy Rate 1900	0.011	−0.001	−0.003	0.000	0.003
	(0.016)	(0.007)	(0.007)	(0.017)	(0.018)
Share Employed in Manufacturing 1900	0.003	−0.000	0.000	0.002	0.002
	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
Share Single Men 18-40, 1900	0.059***	0.007	0.007	0.059***	0.063***
	(0.012)	(0.008)	(0.009)	(0.014)	(0.016)
Population Density (Average Dwelling 1900)	0.021**	0.000	−0.002	0.013	0.011
	(0.010)	(0.007)	(0.007)	(0.023)	(0.030)
Home Ownership Rate, 1900	0.002	0.000	0.001	0.004	0.004
	(0.004)	(0.003)	(0.003)	(0.006)	(0.005)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	6249	6230	5927	6230	5927
Clusters	452	450	430	450	430
F-Statistic		37.14	28.04		
Log Likelihood	-11625	-4043	-3932	-10664	-10664

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. Column 1 presents the negative binomial regression with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. Column 2 presents the first stage OLS regression of lead pipe usage on the rail distance of the city to the nearest lead refinery. Column 4 presents the IV negative binomial regression using control function estimation, which controls for the residuals from the first stage. Standard errors for the IV estimates are calculated using a bootstrap resampling at the city level. Columns 3 and 5 replicate Columns 2 and 4, but for the sample of cities more than 10 miles by rail from a lead refinery. 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 homicide 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 shortest path distance along the 1900 railroad network from the city to closest lead refinery, as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).

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

Table A.10: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Negative Binomial Regressions, Southern States Excluded

	Lead Pipe Cities		Non-lead Pipe Cities		All Cities with pH Data	
	(1)	(2)	(3)	(4)	(5)	(6)
Lead Pipes					0.327***	0.312***
					(0.075)	(0.074)
pH in 1952, USGS	−0.093*		0.093		0.096	
	(0.054)		(0.060)		(0.059)	
Lead × pH (1952)					−0.201***	
					(0.076)	
Log pH in 1952, USGS		−0.602**		0.631**		0.624*
		(0.306)		(0.311)		(0.343)
Lead × log pH (1952)						−1.297***
						(0.436)
Black Population Share, 1900	0.054***	0.054***	0.028**	0.027**	0.044***	0.044***
	(0.015)	(0.015)	(0.013)	(0.013)	(0.010)	(0.010)
Foreign-born Population Share, 1900	0.007	0.007	−0.013*	−0.014*	−0.001	−0.001
	(0.009)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)
Literacy Rate 1900	0.016	0.015	−0.002	−0.002	0.008	0.008
	(0.027)	(0.027)	(0.018)	(0.018)	(0.021)	(0.020)
Share Employed in Manufacturing 1900	0.003	0.003	0.000	0.000	0.002	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Share Single Men 18-40, 1900	0.057***	0.058***	0.057***	0.057***	0.055***	0.056***
	(0.016)	(0.016)	(0.019)	(0.019)	(0.013)	(0.013)
Population Density (Average Dwelling 1900)	−0.039	−0.041	−0.053*	−0.053*	−0.038	−0.039
	(0.033)	(0.033)	(0.028)	(0.028)	(0.026)	(0.026)
Home Ownership Rate, 1900	−0.001	−0.001	−0.002	−0.002	−0.000	−0.000
	(0.005)	(0.005)	(0.007)	(0.007)	(0.005)	(0.005)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2759	2759	2044	2044	4803	4803
Clusters	189	189	146	146	335	335
Log Likelihood	-6215	-6213	-3311	-3311	-9562	-9559

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. All columns present negative binomial regressions with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. 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 homicides. pH is measured according to *The Industrial Utility of Public Water Supplies in the United States, 1952*. We recenter pH, in levels, such that neutral water, with a pH of 7, is zero. Acidic water, with a pH less than 7, is negative and basic water, with a pH greater than 7, is positive. For the log of pH, we take the log of the raw pH values and recenter around ln(7), such that neutral water would take a zero. 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.

Sources: Mortality Statistics of the U.S. from 1921 to 1936; 1900 Census; 1900 IPUMS 5% Census Sample; Baker (1897); Ingalls (1908); Lohr and Love (1954b,a).

Table A.11: Effects of Lead Water Pipes on Homicides from 1921 to 1936, Railroad Distance to Refinery Instrument

	Negative Binomial	First Stage		Instrumental NB	
	(1)	(2) All Cities	(3) Non-Refinery Cities	(4) All Cities	(5) Non-Refinery Cities
Lead Pipes	0.219***			1.022***	0.880***
	(0.064)			(0.257)	(0.276)
Lead Refinery Distance (100 miles)		−0.099***	−0.092***		
		(0.015)	(0.017)		
Log Population		0.077***	0.093***		
		(0.022)	(0.025)		
Black Population Share, 1900	0.026***	0.006*	0.004	0.022***	0.023***
	(0.005)	(0.003)	(0.004)	(0.006)	(0.006)
Foreign-born Population Share, 1900	−0.016***	−0.001	−0.003	−0.016***	−0.014**
	(0.004)	(0.003)	(0.003)	(0.005)	(0.006)
Share Employed in Manufacturing 1900	0.001	0.000	−0.000	0.001	0.003
	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
Literacy Rate 1900	0.009	0.003	−0.001	−0.001	−0.000
	(0.011)	(0.006)	(0.006)	(0.012)	(0.012)
Share Single Men 18-40, 1900	0.044***	0.011	0.013*	0.042***	0.046***
	(0.011)	(0.007)	(0.007)	(0.012)	(0.012)
Population Density (Average Dwelling 1900)	0.019**	0.003	0.004	0.007	−0.029
	(0.010)	(0.007)	(0.016)	(0.020)	(0.027)
Home Ownership Rate, 1900	−0.004	−0.000	−0.001	−0.001	−0.001
	(0.004)	(0.002)	(0.003)	(0.004)	(0.004)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	7434	7415	6827	7415	6827
Clusters	545	543	500	543	500
F-Statistic		45.98	30.68		
Log Likelihood	-15647	-4788	-4530	-15514	-13833

* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Standard errors are clustered at the city level. Column 1 presents the negative binomial regression with the count of homicide deaths as the dependent variable and the contemporaneous log of the city population as the offset. Column 2 presents the first stage OLS regression of lead pipe usage on the rail distance of the city to the nearest lead refinery. Column 4 presents the IV negative binomial regression using control function estimation, which controls for the residuals from the first stage. Standard errors for the IV estimates are calculated using a bootstrap resampling at the city level. Columns 3 and 5 replicate Columns 2 and 4, but for the sample of cities more than 20 miles by rail from a lead refinery. 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 homicide 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 is measured as shortest path distance along the 1900 railroad network from the city to the closest lead refinery as reported in Ingalls (1908). The railroad network was digitized by Donaldson and Hornbeck (2016).

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