

The Value of Piped Water and Sewers: Evidence from 19th Century Chicago^{*}

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Abstract: We estimate the impact of piped water and sewers on property values in late-19th century Chicago. The cost of sewer construction depends sensitively on imperceptible variation in elevation, and such variation delays water and sewer service to part of the city. This delay provides quasi-random variation for causal estimates. We extrapolate ATE estimates from our natural experiment to the area treated with water and sewer service during 1874–1880 using a new estimator. Water and sewer access increases property values by more than a factor of about 2.8. This suggests that total benefits are much larger than the value of averted mortality or construction costs.

JEL: O18, R3, L97, N11

Keywords: Piped water and sewer access, Infrastructure, Extrapolation

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1 Introduction

We estimate the impact of piped water and sewers on land values in late-19th century Chicago. To conduct this estimation, we rely on novel, purpose-collected data describing land transactions and detailed annual maps of piped water and sewer networks. To identify the causal effect of water and sewer infrastructure, we rely on a spatial discontinuity in the timing of construction; parcels south of Congress Street received sewer and water service about three years later than those to the north. This policy discontinuity was motivated by nearly imperceptible differences in elevation that, because of the sensitivity of gravity sewers to elevation, had important implications for construction costs. We propose a new estimator to extrapolate treatment effects from the region where we can defend our natural experiment to a region that is relevant for cost-benefit analysis.

We find that access to piped water and sewers increased residential land prices in Chicago by a factor of 2.8. Four comparisons help to put this estimate in perspective. First, an average Chicago household paid about 36% of its income for a parcel with water and sewer access versus 12% without. Second, during the decade containing our study period, land prices in Chicago first decreased by half, and then doubled. Third, if we aggregate the benefits of water and sewer expansion over affected parcels and compare to construction costs, then benefits exceed costs by about a factor of 60. Finally, a back of the envelope calculation suggests the benefits of water and infrastructure exceed the value of averted mortality by about a factor of 12. Thus, our estimate of the effects of sewers on land prices is modest when compared to wages or to business cycle variation, but it is large compared to construction costs and the value of averted mortality.

These results are of interest for several reasons. According to the World Bank, about 15% of the world's urban population did not have access to safely managed drinking water in 2020, and about 40% did not

have access to safely managed sanitation facilities.¹ Given the likely impact of safely managed water and sanitation on health and mortality, the provision of such services would seem to be a priority. Yet, many cities also lack other basic services such as decent roads, sufficient public transit, adequate schooling, and reliable electricity. Thus, trade-offs inevitably arise. By providing estimates of the benefits of piped water and sewer access, we hope to inform policy makers facing such trade-offs. Our primary outcome variable is land price rather than a measure of health or mortality. Thus, we estimate the total private value of water and sewer access and provide a basis for cost benefit analysis without the intermediate and challenging appeal to estimates of the value of a statistical life.

Our estimates also inform us about an important aspect of the development of the American economy during the late 19th and early 20th centuries. Economic historians have long emphasized the importance of public health infrastructure for the development of American cities (Ferrie and Troesken, 2008). The existing literature on sanitation investments during this period relies almost entirely on time series or panel data relating city-level changes in health and mortality to changes in the availability of particular public health interventions (e.g., Cutler and Miller (2005), Alsan and Goldin (2019)). However, this time period also saw changes in food purity laws, widespread acceptance of the germ theory of disease, and dramatic increases in income that could confound estimates based on time-series variation. Results in Anderson et al. (2018) suggest that this concern is not purely hypothetical. Our cross-sectional identification strategy is not subject to this problem, and so provides new evidence for the importance of capital-intensive public health interventions.

The effects of sewer access on the development of cities and the well-being of their inhabitants have been much less studied than have the

¹<https://data.worldbank.org/indicator/SH.H2O.SMDW.UR.ZS> and <https://data.worldbank.org/indicator/SH.STA.SMSS.UR.ZS>, Accessed December 15, 2021.

effects of other types of infrastructure, such as water treatment, electrification, or transportation. This partly reflects the intrinsic difficulty of observing underground pipes. But it also reflects the difficulty of devising a compelling identification strategy. We pioneer a new identification strategy and hope that intuition underlying our research design will prove portable, and will facilitate research on the effects of sewer and water infrastructure in cities of the modern world.

Finally, building on the marginal treatment effects model proposed by Carneiro et al. (2011), we give conditions under which an estimate of marginal treatment effects may be extrapolated from a sample where quasi-random assignment to treatment may be defended, to a sample where no source of such quasi-random variation is available. Reliance on carefully constructed samples to identify the effects of specific treatments is common, and our hope is that our technique will permit researchers using such designs to extrapolate their results to more relevant samples in a principled way.

2 Literature

The effect of late 19th and early 20th century municipal water treatment on mortality rates has been widely studied. Using a sample of US cities from about 1900 to 1940, Cutler and Miller (2005), Anderson et al. (2018), Anderson et al. (2019) and Cutler and Miller (2020)), estimate the relationship between water filtration, chlorination and various mortality rates. Ferrie and Troesken (2008) consider the effect of various public works projects to improve drinking water quality in Chicago from 1852 to 1925 on the crude death rate and disease specific mortality rates. Alsan and Goldin (2019), Beach et al. (2016), Ogasawara and Matsushita (2018), and Knutsson (2020) also study the effect of improvements in water quality on measures of mortality. Cain and Rotella (2001) consider the effect of expenditures on water works on mortality.

Both the details and conclusions of these studies differ. For example, Alsan and Goldin (2019) examine the effect on infant mortality rates of interventions to protect drinking water quality in the Boston harbor

watershed. These interventions included public works projects to direct the outflow of sanitary sewers away from drinking water supplies, the creation of municipal reservoirs, and efforts to protect the sources of these reservoirs. They estimate that these interventions interacted to cause a 26% decline in infant mortality rates between 1880 and 1920. Using a sample of 25 US cities between 1900 and 1940, Anderson et al. (2018) examine the effect on infant and total mortality rates of municipal water filtration, chlorination, the development of protected drinking water sources, and treatment or diversion of sewer outflows. They have two main findings. First, efforts to manage sewage outflows have no effect on infant mortality. Second, although water filtration leads to an 11% decline in infant mortality, the joint effect of all water quality related interventions is only 4%. Alsan and Goldin (2019) and Anderson et al. (2018) do not report identical estimands, but they are close, and the nearly order of magnitude difference in estimated effects for similar interventions is striking, especially if we note that Boston is also part of Anderson et al.'s sample.

Uncertainty about magnitudes remains, but the literature suggests that improvements to municipal water quality and the management of sewer outflows had important implications for health. The literature is less informative about the effects of expansions of residential piped water and sewer networks. Although, like us, Anderson et al. (2018) and Alsan and Goldin (2019) both consider improvements in water and sewer infrastructure, we consider qualitatively different interventions. Anderson et al. (2018) and Alsan and Goldin (2019) consider municipal level efforts to manage the outflow of sanitary sewer networks and to improve municipal water supplies, while we consider the extension of piped water and a combined sanitary and storm sewer system to residential neighborhoods. The literature evaluating expansions of residential sewer networks during this period consists of just a few papers. Kesztenbaum and Rosenthal (2017) examine the effect of the increasing availability of sewers in Paris between 1880 and 1915 and find that a 10% increase in

neighborhood sewer connections increases neighborhood mean life expectancy, conditional on reaching age one, by 0.13 years. Troesken (2004) documents the role that water and sewer service played in narrowing the black-white life expectancy gap in the US during the first half of the 20th century.

The literature also investigates the effects of municipal water quality improvement in the modern developing world. Ashraf et al. (2017) find that interruptions to piped water supplies in urban Lusaka significantly increase the incidence of diarrhea and typhoid, and for young women, increase time at chores and decrease time at study. Galiani et al. (2005) examine the effects of privatizing the provision of municipal water supplies in Argentina in the 1990s and conclude that the resulting improvements in service quality reduced child mortality by 8%. Bhalotra et al. (2021) examine the effect of a large expansion of water treatment in Mexico between 1991-5 and find that improved access to piped water led to a large reduction in childhood mortality from diarrheal illness. Devoto et al. (2012) find that randomly assigned help obtaining credit for piped water connections significantly increases time allocated to leisure activities in an RCT conducted in Tangiers in 2007.

The intervention evaluated in Gamper-Rabindran et al. (2010) is probably closest to the one we study. Gamper-Rabindran et al. (2010) study Brazil between 1970 and 2000, and like us, investigates the the effects of expanded access to piped water and sewers. During this period, the share of Brazilian households with piped water increased from 15% to 62% and the infant mortality rate fell from 125/1000 to 34/1000. On the basis of a panel data estimation, they conclude that each percentage point increase in piped water access decreases infant mortality by 0.48/1000, about 25% of the total effect.² Gamper-Rabindran et al. (2010) also examine the effects of increased sewer access and find no effect.

Our analysis makes several contributions. First, the historical

²The realized expansion in piped water access decreased infant mortality by $(62 - 15) \times 0.48 \approx 22/1000$, about 25% of the total decrease of 91/1000.

literature focuses on the effects of municipal water quality and the management of sewer system outflows. Only Kesztenbaum and Rosenthal (2017) and Troesken (2004) explicitly analyze expansions in household sewer provision. Expansions of piped water access are still less studied. Among papers studying the modern developing world, only Gamper-Rabindran et al. (2010) explicitly studies expansions in residential access to piped water and sewers.

Second, our analysis of the relationship between public health infrastructure and land rent is nearly unique.³ The literature makes clear that public health infrastructure has complicated effects on the lives of those it touches. Not only does it affect current mortality and morbidity rates, it may affect time allocated to leisure (Devoto et al., 2012), time spent at school (Ashraf et al., 2017), and future mortality rates (Ferrie and Troesken, 2008)). It follows that an evaluation of the benefits of public health infrastructure requires an effort to aggregate and monetize all of these different effects, an exercise complicated by the difficulty of calculating the value of a statistical life from historical data (Costa and Kahn, 2004). In contrast, land rent is a revealed preference measure summarizing the value of all of the effects of piped water and sewer service. As such, it provides a simple basis for valuing all of the private benefits of piped water and sewer service.

Third, the literature studying 19th century public health initiatives relies on comparisons of mortality rates before and after an innovation (e.g., Ferrie and Troesken (2008) or on difference-in-differences designs (e.g., Cutler and Miller (2005) or Alsan and Goldin (2019)). However, the late 19th and early 20th century saw the widespread adoption of vaccination, the development of the germ theory of disease, the increasing availability of refrigeration, and the widespread adoption of food purity

³We know of only one other paper that investigates the relationship between water quality and rents. In a recent paper Ambrus et al. (2020) show that housing prices are persistently lower in London neighborhoods with a history of cholera contamination in their water supply. Notably, they consider the contamination of neighborhood wells, not residential piped water.

standards (Haines, 2001). It is natural to suspect that estimators based on time series variation may confound the effects of these innovations with those of water treatment. Efforts to control for improvements in milk quality in Alsan and Goldin (2019) are reassuring in this regard, but those in Anderson et al. (2018) are not.⁴ By construction, our cross-sectional research design is not subject to this problem.

Fourth, although the disease environments in modern developing world cities and late 19th century Chicago are different (see Henderson and Turner (2020) and Haines (2001)), rates of infant mortality, the effects of water treatment on infant mortality and income levels are all similar. Alsan and Goldin (2019) estimate that between 1870 and 1930, the infant mortality rate in Massachusetts declined from about 160 per 1000 births to about 40, and that 26% of this decline was due to improvements in the management of sewage and drinking water. Gamper-Rabindran et al. (2010) find an infant mortality rate of 125/1000 for Brazil in 1970 and estimate that water and sewer access reduces this rate by about 25%. For Mexico between 1991 and 1995, Bhalotra et al. (2021) find an infant mortality rate of 28/1000 and that this rate declines by about one half with water chlorination. That is, infant mortality rates and the effects of improved water quality on infant mortality are both large in turn of the century US and modern day Brazil and Mexico. Finally, average annual incomes for laborers in Chicago during our study period were about \$17,000 in 2021 dollars, close to modern Brazil and Mexico.⁵ Absent studies based on modern data, these similarities suggest the reasonableness of using our estimates as a starting point for evaluating policies in modern day developing countries.

In addition to our primary object of estimating the effects of piped

⁴See (Anderson et al., 2018, table 7). The total effect of water quality related interventions falls by about half when controls for sewage treatment and milk purity are included.

⁵From estimates of wages per non-agricultural worker for the state of Illinois taken from (Easterlin, 1960, 73-140) (\$627 per year) and Hoyt's (2000, pp.118-119) estimates of wages for workers in the city of Chicago during the 1870s (\$3 a day for unskilled laborers). We inflate to 2021 price levels using CPI estimates from Sahr (2009) for 1880-1912 and the BLS CPI series for 1913-.

water and sewer infrastructure on land prices, we develop a new method for extrapolating estimates based on a quasi-experiment to a sample for which quasi-random assignment of the treatment is not available. Our approach builds on the marginal treatment effects estimator developed by Heckman and Vytlacil (2005) and Carneiro et al. (2010) but extrapolates to units not in the original estimation sample. The possibility of extrapolation from quasi-experimental samples using marginal treatment effect estimates has not been considered.⁶

3 Data

Our main empirical exercise requires two main types of data, a measure of land values and a measure of piped water and sewer access. For econometric purposes, we require a description of the attributes of transacted parcels. To complete our cost benefit analysis, we must measure construction costs. We here describe the data we use for each purpose.

Between 1874 and 1889, the Chicago Tribune often reports land parcel transactions filed with the municipal title office on the previous day. We collect all transactions listed in the Sunday edition. This results in about 700 observations per year in the 1870s and 1000 per year in the 1880s.⁷ We restrict attention to Sunday transactions for three reasons. First, the

⁶Other methods for extrapolating causal effects outside the sampled population include Hotz et al. (2005), Angrist and Fernández-Val (2013), and Dehejia et al. (2021). Related to this, Angrist and Rokkanen (2015), Rokkanen (2015), and Cattaneo et al. (2020) consider extrapolating treatment effects estimated using an RDD design to points away from the discontinuity.

We also note the related series papers, Mogstad and Torgovitsky (2018), Mogstad et al. (2018), and Brinch et al. (2017). These papers consider extrapolation and interpolation of marginal treatment effects for units in the estimation sample. The analysis of policy relevant treatment effects considered in Heckman and Vytlacil (2001, 2005), Carneiro et al. (2011) concerns the impact of a counterfactual policy that influences individual's treatment choice through, for instance, manipulated assignments of excluded instruments. Our analysis differs in two ways. First, we consider the problem of extrapolating marginal treatment effects to units not in the estimation sample. Second, we do not observe the instrument in the Relevant sample.

⁷The Tribune still published parcel transactions after 1889, but the coverage is limited to parcels with a value of at least \$1000 (nominal value).

Tribune always reports real estate transactions on Sundays and irregularly on other days. We suspect that this reflects a weekday page limit. Second, the Sunday paper consistently reports the largest volume of transactions, even in weeks when transactions are reported on other days. Finally, resource constraints precluded the collection of transactions from all days.

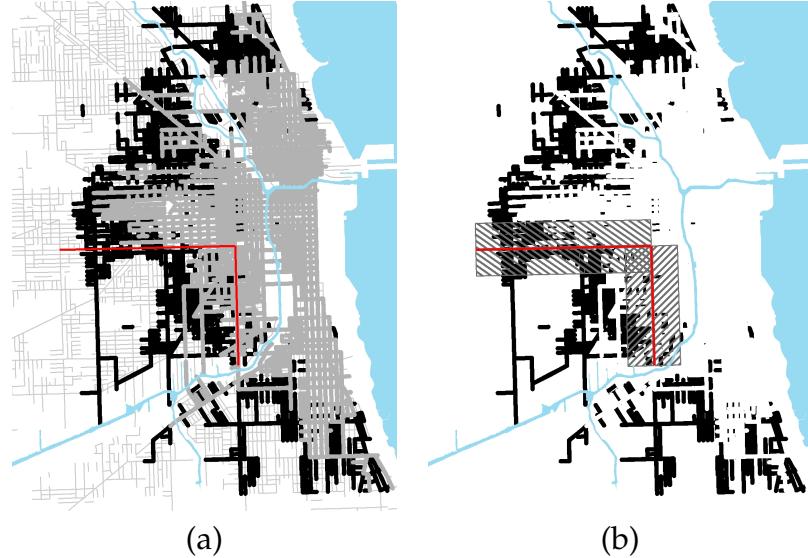
Each of our transactions is characterized by three dates: the date of the paper where it is reported, the date the transaction is filed at the courthouse, and the date the transaction occurred. Although we restrict attention to transactions filed at the courthouse on Saturday and reported in the newspaper on Sunday, transaction dates occur throughout the week. Saturday transactions outnumber Monday transaction about 5 to 2 in our sample, suggesting that real estate agents sometimes filed the week's transactions with the courthouse on Saturday. Citywide, there is no difference in either the likelihood of water and sewer access or average transaction price by day of the week. We experiment with controlling for transaction day of week in estimations reported below.

The Tribune consistently reports price, parcel dimensions, either a street address or the nearest intersection, and whether the parcel is on a corner. Because the Tribune separately indicates transactions with a "premises", i.e., a house, we are confident that our data describe land transactions only.

We geocode our sample parcels in two steps. First, we attempt to match the "nearest intersection" reported by the Tribune to an intersection in the contemporary street grid described by the Google Maps API. If this fails, we attempt to match the parcel to an intersection in a circa 1880 street map (Logan et al., 2011). In this way, we geocode 77% of transactions by assigning them the coordinate of their nearest intersection. Appendix A provides a more complete description of how we collect and geocode transactions.

We rely on historical GIS maps to describe the block-by-block expansion of the sewer network from 1830-1930 (Fogel et al., 2014). These

Figure 1: Extent of piped water and sewer network, Southwest Triangle, and Quasi-experimental samples



Note: (a) Sewers before 1874, during 1874-1880, after 1880, and boundaries of the Southwest triangle. (b) “Relevant” sample area (1874-1880 expansion) and “Quasi-experimental” sample areas.

maps derive from the annual reports of the Chicago Department of Public Works, and report both the location and opening date for each segment of the sewer network. Water and sewer service were almost always installed simultaneously, and so we rely exclusively on sewer maps.

We say a transaction “has water and sewer access” if the nearest intersection to the transaction is within 75 feet of an operating sewer line in the transaction year. Visual inspection of the matching process confirms accurate matching of *intersections* to sewers. One can imagine situations in which a *parcel* without access to sewer and water matches to an *intersection* where access is available, though such situations should be rare.⁸ False negatives are harder to imagine.

Figure 1a illustrates the expansion of piped water and sewer access. Thick, light gray lines indicate water and sewer lines pre-dating our

⁸A parcel on a street without water and sewer service could match to an intersection where the cross-street has water and sewer access.

1874-1880 study period. These lines tend to be close to the center of the city. Thick black lines indicate water and sewer lines constructed during our 1874-1880 study period. These lines are mostly located on the periphery of the previous network. Finally, the fine gray lines indicate sewer and water lines built after the end of our study period; these lines are peripheral to the 1880 network and often extend beyond the boundary of the figure.

We collect demographic data from the 1880 census. In the late 19th century, in-person enumeration was organized on the basis of enumeration districts, the unit of spatial aggregation available to us. As we describe below, our research design relies on variation over spatial scales that are too small to be measured by the relatively large enumeration districts. That is, the spatial variation in late 19th century censuses is too coarse to be useful in our research design. Therefore, we restrict discussion of these data to the appendix.

We calculate a number of control variables from GIS data layers. For each parcel, we calculate distance to the CBD as the distance to City Hall in 1873 (now known as the Rookery Building). We calculate distance to the lake as distance to the modern lakeshore⁹ and calculate distance to the Chicago River similarly. Finally, we calculate distance to the nearest horsecar line and major street using contemporaneous maps of the two networks.¹⁰

To estimate the cost of piped water and sewer expansion, we rely on reports of annual expenditures on water and sewer construction in the Annual Reports of the Chicago Department of Public Works (accessed through Hathi Trust). Expenditures generally increase in the early 1870s and decrease during the recession of the late 1870s. Waterworks, including pumping stations, were typically the largest category of expenditure, with sewer construction second. Sewer operation and

⁹Cook County Government Open Data, <https://datacatalog.cookcountyil.gov/GIS-Maps/Historical-ccgisdata-Lakes-and-Rivers-2015/kpef-5dtn>.

¹⁰The 1880 horse-drawn streetcar routes were digitized using a map from the Illinois State Grain Inspection Department. The 1880 street network is from Logan et al. (2011).

maintenance costs were consistently stable and relatively small. Expansions to the sewer and water system were primarily financed by bonds, and nineteenth-century Chicago had a large tax base of valuable land on which to levy the property taxes that were the primary source of revenue to service these bonds.¹¹

4 Background

The Census reports Chicago's population as 300,000 in 1870 and above one million in 1890. The Great Fire of 1871 destroyed the central business district and much of the city, but barely checked this growth. The city continued to expand throughout the 1870s and 1880s, particularly in the band of mostly unsettled land a few miles from the downtown where our study area lies. This rapid growth was driven by immigrants from Europe and by internal migration. Chicago provided relatively high-wage employment opportunities for unskilled workers. The average income for laborer Chicago was about \$650 in 1880 dollars or \$17,000 in 2021 dollars.¹²

Hoyt (2000) describes Chicago's land market between 1830 and 1930. Land values grew rapidly in the early 1870s, declined by 50% between 1873 and 1877, and then recovered over the following five years. (Hoyt, 2000, p. 140). Thus, our 1874-1880 study period spans a major recession (1873-1877) and recovery (1878-1882). Figure B1 demonstrates that our data show this same pattern of decline and recovery.

In the 1850s, most residents drank from backyard wells. These wells were often near privy vaults and these vaults were seldom tight. Households with access to the city water system found it contaminated by industrial pollutants and minnows from Lake Michigan. Water quality improved as the city moved the water intakes further out into Lake Michigan and reduced the volume of waste dumped in the lake, although

¹¹Special assessments and connection fees also helped to finance sewer and piped water infrastructure. However, the Sewerage Board was reluctant to rely too heavily on fees and user charges because the resulting negotiations with building owners slowed down the expansion process (Melosi, 2000, p. 98).

¹²See footnote 5.

there were no major changes to municipal water quality during our 1874-1880 study period.¹³

Chicago's infant mortality rate in the 1870s was 74 per 1000 (Ferrie and Troesken, 2008). This is similar to contemporaneous rates reported in other US cities (Alsan and Goldin, 2019, Haines, 2001). Most deaths were of infants or children, and were caused by infectious disease (Ferrie and Troesken, 2008).

The condition of the City's poorly drained streets was grim. Ashbury's well-known Chicago history reports that the "gutters [run] with filth at which the very swine turn up their noses in supreme disgust..." (Ashbury, 1940, p.23). When storms washed these wastes into Lake Michigan or private wells, cholera and dysentery epidemics followed. Such events killed hundreds of people in both 1852 and 1854, precipitating plans to improve water and sewer infrastructure.

Chicago is famously flat and grades shallower than 1:1000 are common in our study area. Ordinary gravity fed sanitary sewers require a grade of about 1:200 to prevent suspended solids from settling and blocking the pipe (Mara, 1996). To design a sewer system capable of operating in Chicago's flat topography, Chicago hired noted engineer Ellis Chesbrough and substantially followed the proposal he submitted in 1855.

Chesbrough's plan calls for continuous flushing of sewer mains. This allows them to operate at a grade of 1:2500; far shallower than conventional sewers, and shallow enough to be practical in Chicago.¹⁴ Importantly, Chicago's sewers manage both household sewerage and storm water runoff; in modern parlance, a 'combined' sewer system. Thus, expansions of the sewer system improve both the management of household waste and the drainage of affected streets.

¹³Water quality improved with the completion of the Two Mile crib (1867), the Four Mile crib (1892), and the permanent reversal of the Chicago River in 1900 (Ferrie and Troesken, 2008). Our study period (1874-1880) falls entirely within the Two Mile crib period.

¹⁴Chesbrough's plan originally called for mechanical flushing, but the city ultimately flushed sewer mains using water delivered by horse-drawn carts. As late as 1940, horse-drawn tanks were still used to manually flush certain sewer lines in Chicago (Cain, 1978, p. 32).

Chesbrough's sewers required large enough flows of water that they were only practical if piped water was available. This meant that sewers could not be installed before piped water. On the other hand, drainage in Chicago was so poor that the increased volume of wastewater that accompanied piped water caused cesspools to overflow (Melosi, 2000, p. 91), so that installing piped water without sewer access was also impractical. Thus, the provision of piped water and sewer access almost always coincided.

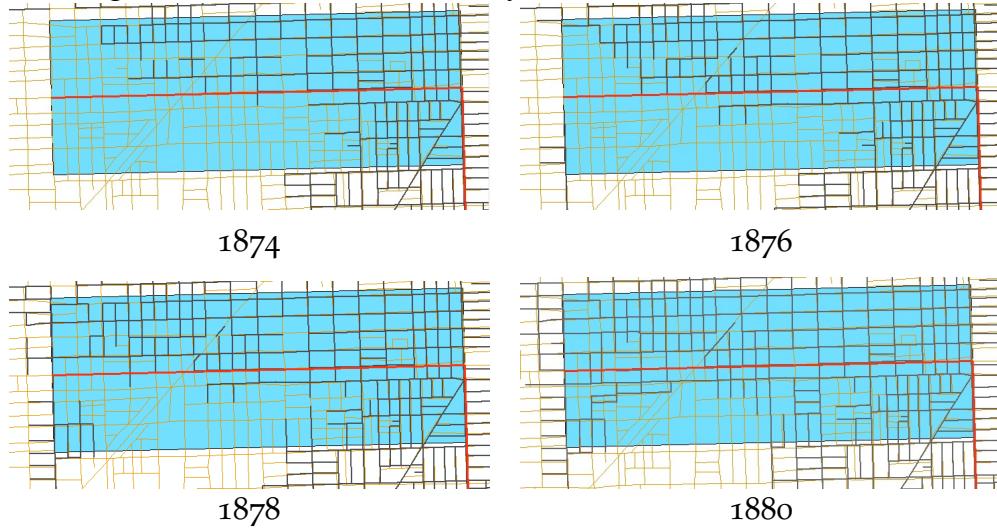
Constructing sewers involved first laying sewer and water pipes at the required grade, whether above or below ground, filling in the space above or around them and, sometimes, paving the newly sewerized road.¹⁵ Our estimates should be understood as the total effect of this process. That is, our estimated effect will reflect improved access to water and sewer service, and access to the better drained streets that result from the construction of the combined sewer.

Chicago issued its original plan for sewerage in 1855. This document describes the grades required to accommodate the proposed sewer system in each region of the city (Plan of Sewerage, Chicago Board of Sewerage Commissioners, 1855). Subsequent ordinances were issued at regular intervals as the sewer system expanded. The sewer ordinances describe the details of the regrading operation and list, block by block, the planned elevation of each intersection in hundredths of an inch. To understand the scale of this project, consider that the 1855 plan states, “[i]t will be necessary to raise the grades of streets an average of eighteen inches per 2500 feet going West.” Raising a 2,500 foot segment of a 20 foot wide street by 18 inches requires 8300 cubic yards of fill. At 1.5 tons per cubic yard, this is 12,450 tons of fill.

Municipal authorities knew which streets had the worst drainage and were anxious to sewer them as soon as the network reached them. From

¹⁵Buildings, particularly those built out of stone and brick, were raised in the downtown to match the new street level as the sewer system expanded. These well-known feats of engineering pre-date our 1874-1880 study period. Our analysis focuses on vacant lots in outlying areas.

Figure 2: Sewer extent in study area between 1874 and 1880



Note: Tan indicates the 1930s street network and red indicates boundaries of the Southwest Triangle. Light blue indicates the area within 2000 feet of Congress Street running west from Halsted Street for two miles to Western Avenue. Black lines indicate the sewer network. There is more sewer coverage in the Northern half of our study area than the southern half during the 1874-80 study period.

the Chicago Tribune (June 25th, 1873, page 4): “*The Mayor points out the various localities where this sewerage is the most needed. It so happens that the unsewered portion of the city is that which, of all others, most needs it. ... These neighborhoods are densely populated by people who have not the means to adopt any sanitary measures.*” Thus, there is no reason to believe that the assignment of sewers to neighborhoods and streets was independent of land value.

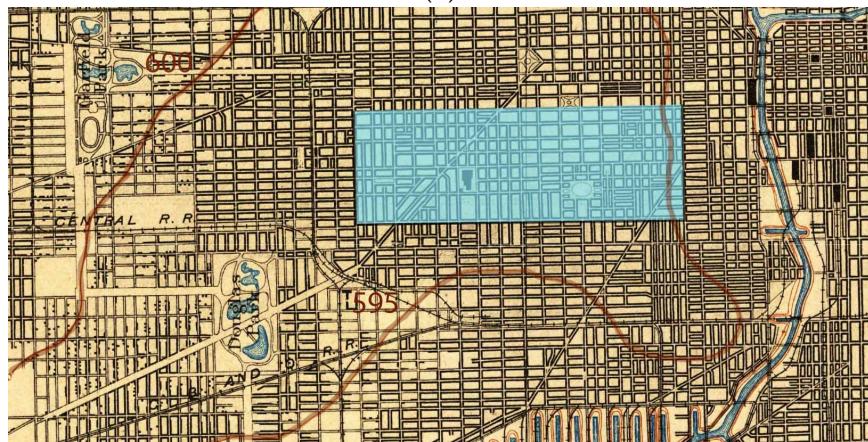
5 Research design and description of data

The 1855 plan and subsequent ordinances prescribe the more-or-less radially symmetric expansion of the sewer system illustrated in figure 1, with an important exception. The 1855 ordinance describes a “triangle”, South of Congress Street and West of Halsted Street, where sewer service is to be delayed because the area is slightly lower than adjacent areas.

Figure 3: Study area in 1852 and topography in 1901



(a)



(b)

Note: (a) An 1853 drawing of Chicago with the appproximate boundaries of the Quasi-experimental area highlighted. This area is completely undeveloped just two years before the 1855 plan. (b) A section of a 1901 USGS topographic map with study area and the 595' and 600' contours highlighted. Note that Halsted street closely follows the 595 foot contour line in our study area, i.e., it is perfectly flat, while the entire two mile length fits easily in between the 595 and 600 foot contours, so that East to West elevation gain is strictly less than 5 feet in 1901.

Because the 1855 plan specifies only northern and eastern borders of the Southwestern triangle, we draw a western boundary near the limit of the 1880 sewer network, at Western Avenue, two miles west of Halsted Street, and a southern boundary at the Chicago River.

Chesbrough writes of this region: "The extreme south-west part of the city [is] too low [to sewer] ... and the depth of filling required to raise streets over it would average two feet" (p. 16). Because of this, Chesbrough delays service to this region; "[a]s this part of the city may not be improved for several years, it is deemed sufficient for present purposes to state the general depth of filling that would be required" (p. 15). Because the plan calls for streets to be raised "an average of eighteen inches per 2500 feet going West", this means that Chesbrough delayed sewer service in the Southwest triangle because of six marginal inches of fill.

We rely on variation in sewer access across the boundaries of the southwest triangle to address the endogenous assignment of sewer and water service to parcels. For this purpose, define the Quasi-experimental area as the area within 2000' of Congress Street, west of Halsted Street and two miles east to Western Ave. Our primary analysis will be based on a set of 351 transactions occurring in this area between 1874-1880. We exclude parcels exactly on Congress Street, i.e., those matching to intersections within 75' of Congress Street, because the 1855 plan is ambiguous about whether or not Congress Street lies inside or outside the Southwest Triangle. In robustness checks, we consider an Extended-quasi-experimental area by adding transactions that occur within 2000' of Halsted street, south of Congress. In total, 533 transactions occur in this larger area during 1874-80.

Red lines in figure 1 illustrate the northern and eastern border of the Southwest Triangle: Congress and Halsted Streets. Hatched areas in panel (b) describe the Quasi- and Extended-quasi-experimental areas. The black areas in panel (b) indicate the entire region that received water and sewer access during our 1874-80 study period. This is the region for which we

observe construction costs and is the relevant area for the purpose of policy evaluation. We will refer to a sample drawn from this area as a 'Relevant' sample. Appendix A provides further details and illustrates the distribution of transactions across these regions.

Figure 2 highlights the evolution of the sewer network in the Quasi-experimental region. This figure establishes that, even 20 years after the adoption of the 1855 sewer ordinance, the construction of sewers south of Congress Street lags the northern side of the street.

Table 1 presents sample means for the Quasi-experimental sample. Column 1 describes transactions inside the Southwest Triangle, i.e., south of Congress Street. Column 2 describes transactions outside the Triangle, i.e. north of Congress Street. Column 3 reports a *t*-statistic testing the equality of the first two columns. In the first row, we see that piped water and sewer incidence is lower inside the Southwest Triangle than outside, just as the 1855 plan prescribes. The second row provides preliminary evidence for the importance of piped water and sewer access for land values. Consistent with a large effect of water and sewer access on value, unconditional prices are 73 log points (108%) higher outside of the Southwest Triangle than inside.

Figure 4(a) shows changes in sewer incidence across the Congress Street border of the Southwest Triangle during 1874-80 as a function of distance to Congress Street. The *x*-axis in this figure is distance from Congress Street. Negative distances indicate displacement south into the Southwest Triangle and conversely. The *y*-axis indicates piped water and sewer share relative to the share in the bin just inside the Southwest Triangle. Consistent with Table 1, we see that piped water and sewer incidence and land prices are lower in the Southwest Triangle and, as prescribed by the 1855 plan, the drop in sewer incidence occurs at Congress street, i.e., $x = 0$ in the figure. The anomalously high incidence of sewers in the far left bin reflects transactions in the southwest corner of the study area that rely on the separate, southward draining sewer main along Western avenue.

According to the 1855 plan, the elevations of parcels on opposite sides of the boundary of the southwest triangle differ from each other by about six inches on average. To get a sense for the implied grade, note that the quasi-experimental area is 4000' north-to-south. Achieving a six inch average difference between the northern and southern portions of Quasi-experimental area requires an average drop of 3 inches per 1000 feet, a grade of 1:4000. The logic of gravity fed sewers implies that this tiny difference in elevation has a large effect on construction costs and leads to the delay in sewer provision prescribed by the 1855 plan and observed in figure 2.

Despite of their importance for sewer construction, such shallow grades are nearly imperceptible. Aldous (1999) reports that people begin to perceive a playing field as sloped at a grade of about 1:70, while a grade of 1:1000 is close to the precision of a present day contractor's laser level.¹⁶ In short, the variation in grade that determines the timing of sewer construction in our study area is beyond unaided human perception; is at the limits of what a carpenter can measure; and, can only be accurately measured with a surveyor's tools. Our estimation strategy is based on the proposition that the variation in elevation that precipitated the demarcation of the southwest triangle only affected real estate prices through its effect on the timing of sewer and water access.

The validity of this strategy depends on whether the boundary of the southwest triangle is drawn because of the small change in elevation that is its ostensible basis in the 1855 plan, or if it reflects some unobserved difference in land quality. Several pieces of evidence suggest that the boundaries of the southwest triangle were drawn solely in response to the small change in elevation across the border.

Figure 3(a) shows an illustration of Chicago drawn in 1853, just before the publication of the 1855 plan (Allison, 1974). We superimpose the approximate boundaries of the quasi-experimental study area on the upper left corner of the image. This area is uniform and undeveloped.

¹⁶A Bosch GLL25-10 Laser Level has a precision of 5/16 of inch over 30 feet, that is, 1:1152.

Similarly, Hoyt (2000) maps the extent of the developed area of Chicago in 1857 and 1873. The entire Extended-quasi-experimental area was undeveloped in 1857. The Quasi-experimental sample, along Congress street remains undeveloped in 1873, although much of the area around Halsted street was developed at this time. This is confirmed by our transaction data. Between 1874 and 1880, at most 3% of real estate transactions reported in the Quasi-experimental area involved a parcel with a building in place. When Chesbrough described the southwest triangle in 1855, it lay in a uniform, undeveloped area, and the quasi-experimental area remained undeveloped until the beginning of our study period.

Figure 3(b) shows our quasi-experimental study area superimposed on a 1901 USGS topographical map (U.S. Geological Survey, 1901). The entire two mile length of the quasi-experimental study fits easily in the region between 595 foot contour line, highlighted on the right of the image, and the 600 foot contour, highlighted on the left. Thus, the entire two mile east-to-west extent of this region involves strictly less than five feet of rise, a grade of less than 1:1056. Looking North-to-south, we see similar contour spacing, and hence grade. In fact, Halsted Street follows the 595 foot contour, so it is flat to limits of the map's precision. This map reports topography in 1901, after the regrading associated with sewer construction has occurred. Because regrading generally increases the grade of the city, grades in this map are likely steeper than for the pre-sewer topography. In all, the topography of our study area was remarkably uniform.

Table 1 presents sample means for observable covariates inside and outside of the southwest triangle during our 1874-80 study period. The share of corner parcels, mean distance to the CBD, and mean transaction year are statistically identical for parcels included and excluded from the southwest triangle. However, parcels included in the southwest triangle are about one city block farther from a horsecar line, less than half a block farther from a major street, and may be slightly larger. If transactions are

randomly assigned to the southwest triangle, we would expect all covariate distributions to be independent of this status.

Failing a balance test poses a threat to the validity of our research design if two additional conditions obtain. First, that the covariates in question have an independent effect on the outcome, and second, that treatment effect estimations do not condition on the problematic covariates. Given this, we will control for covariates when we estimate treatment effects. We will also provide evidence against the hypothesis that the unbalanced covariates have an independent effect on outcomes.

Data limitations prevent us from checking that land prices across Congress are constant in a prior period when sewer and water service is uniformly unavailable.¹⁷ However, we can check that land prices across this boundary after sewer service is universal both north and south of Congress Street.

Table B1 describes transactions occurring in the Quasi-experimental region during 1886-9, six to nine years after the end of our main study period. This table replicates the first three columns of Table 1 for this later time period. Piped water and sewer access is universal and the difference between prices inside and outside the Southwest Triangle that shows so clearly in Table 1 is no longer present. Differences in covariates are similar for the two samples. That differences in covariates persist, but not differences in price, when sewer access is universal is not consistent with the hypothesis that the unbalanced covariates in Table 1 have an independent effect on outcomes.

Figure 5 and Table B2 refine this conclusion. Figure 5(a) considers 1886-9 instead of 1874-80, but is otherwise like figure 4. Panel (a) shows that sewer incidence is constant across the boundary by 1886. Panel (b) reports mean log price controlling for year indicators, $\ln(\text{area})$, and $\ln(\text{mi. to CBD})$. Parcel prices are constant across the border during 1886-9. Table B2 conducts regressions similar to those on which Figure 5 is based, but with a single indicator for ‘outside the southwest triangle.’ These

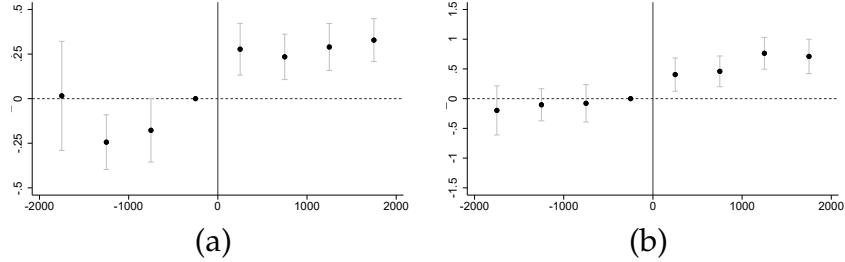
¹⁷The Herald Tribune did not publish parcel transactions until October of 1873.

regressions confirm that by 1886-9 the cross-border difference in property prices cannot be distinguished from zero and that that this conclusion is robust to permutation of control variables. Estimates most like those of Figure 5(b) in column (2) allow us to reject the hypothesis that prices of parcels north of Congress Street are more than 36 log points greater than those to the south. This establishes a bound on the importance of unobservable determinants of cross-border changes in prices.

Summing up, three pieces of evidence weigh against the hypothesis that land prices change across Congress Street for reasons unrelated to the sewer and water access. First, the quasi-experimental area consists of uniform, undeveloped land when the plan is drawn, and it remains undeveloped until the start of our study period. Second, the price difference that arises across Congress Street during 1874-80 is erased by 1886-9 when sewer access is also equalized across the border. This imposes strong restrictions on possible confounders: they must arise sharply at Congress street, must affect land prices, and must disappear over the same short period during which piped water and sewer construction is completed. We satisfy a balance test on several observed covariates, and there is no evidence that problematic covariates have the independent effect on the outcome that could invalidate our estimation strategy.

Evidence provided so far suggests this narrative. Parcels in the Southwest Triangle were less likely to have access to piped water and sewers in the 1870s because of Chesbrough's response to a nearly imperceptible change in elevation that affected costs of constructing gravity fed sewers. All available evidence suggests that the study area was uniform, flat, undeveloped land in 1855, and remained this way until the start of our study period. Average prices on both sides of the border were the same nine years after our main study period when sewer and water access was universal. Therefore, there is no reason to suspect that parcels on opposite sides of Congress Street are systematically different, except that parcels inside the Southwest Triangle are more remote from

Figure 4: Sewer incidence and land price by distance to boundary, 1874-80



Note: (a) *x*-axis is distance to Congress Street boundary, with *x* < 0 displacement South, “inside” and conversely. *y*-axis is share of transactions sewered between 1874-80, controlling for year indicators, $\ln(\text{Area})$, and $\ln(\text{mi. to CBD})$ by 500’ long bins. (b) Same as left panel but *y*-axis is $\ln(\text{Price})$, controlling for the same set of covariates. The incidence of sewer access and prices are both higher north of Congress Street than south.

horsecar lines and major streets.

This suggests that conditional on controls, a comparison of changes in prices and sewer access across Congress Street should yield an unconfounded estimate of the effect of water and sewer access on prices. Indeed, because distance to horsecar lines and major streets do not appear to have an important independent effect on outcomes, the case for controlling for these variables is debatable.

Figure 4 performs this comparison. Panel (a) shows changes in sewer incidence across the Congress Street border of the Southwest Triangle and panel (b) shows the corresponding changes in log price. These figures illustrate the variation on which our estimates are based. The left panel is a first-stage regression, the right panel is a reduced form. The ratio of the two cross-boundary gaps, averaged over the four interior and exterior bins, yields (approximately) a local average treatment effect for the whole Quasi-experimental sample.

Discussion

Table 1 shows that parcels in the Southwest Triangle were less valuable during our study period. There is evidence that such initial

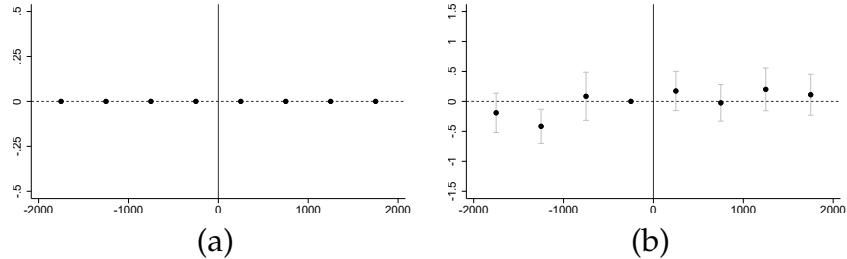
Table 1: Summary Statistics 1874-1880

	(1) SW $\Delta = 1$	(2) SW $\Delta = 0$	(3) <i>t</i> -test	(4) Relevant
Share Sewered	0.47 (0.50)	0.92 (0.27)	10.84	0.70 (0.46)
Log Price	7.68 (0.85)	8.41 (0.75)	8.48	7.39 (0.89)
Log Distance to CBD	9.13 (0.39)	9.10 (0.38)	-0.82	9.49 (0.25)
Log Area	-9.02 (0.62)	-8.88 (0.69)	1.88	-8.97 (0.54)
Share Corner	0.10 (0.30)	0.12 (0.33)	0.56	0.13 (0.34)
Distance to Horsecar	0.17 (0.11)	0.08 (0.06)	-9.61	0.34 (0.26)
Distance to Major Street	0.11 (0.08)	0.09 (0.07)	-2.27	0.09 (0.07)
Year	1877.21 (2.20)	1877.42 (2.19)	0.87	1877.60 (2.26)
Unsewered parcel time to Sewer	3.38 (2.12)	2.69 (1.08)	-1.26	2.96 (1.66)
Observations	146	205		1298

Note: Means and standard deviations of parcel characteristics. Column 1 reports on parcels in the Quasi-experimental sample (within 2000' of Congress St. west of Halsted) that are in the Southwest Triangle (south of Congress Street). Column 2 reports on parcels that are not in the Southwest Triangle (north of Congress Street). Column 3 reports the *t*-statistic for the difference between the first two columns. Column 4 presents parcel means and standard deviations for all parcels in the Relevant sample. In all columns, we restrict attention to parcels transacted during 1874-1880.

disadvantages often “lock-in” and lead to long run differences between places (Bleakley and Lin, 2012, Ambrus et al., 2020). Given this, our finding that price differences largely disappear with the elimination of the difference in sewer access is surprising. The available evidence suggests that path dependence works against the price equalization that we see in Figure 5. Poor places stay poor and rich places stay rich. Our finding that

Figure 5: Sewer and water share and price by distance to boundary, 1886-9



Note: Same as Figure 4, but for transactions occurring between 1886-9. Piped water and sewer access and prices are both the same at the border after sewer and water provision is completed in the Southwest Triangle.

lock-in does not operate likely reflects particular features of the Chicago land market: the city was growing rapidly; our transactions are entirely of undeveloped land; and structures in our study area were generally cheap, hastily constructed wooden houses.

In our research design a parcel is ‘treated’ when sewer and water pipes are installed through the nearest intersection. This means that any treated parcel will also have treated neighbors. Thus, we should think of estimated price effects as reflecting the amelioration of neighborhood level externalities. To the extent that piped water and sewer access resolves more distant external effects, like the transmission of diarrheal disease to households in remote parts of the city, these effects are invisible to our research design. In theory, external effects that operate at an intermediate scale, say of a few blocks, could be detected in our data. In this case, parcels close to Congress Street could experience smaller effects of treatment than parcels farther south because these parcels have the benefit of more neighbors with water and sewer access. In practice, our sample is too small to allow us to investigate this issue.

We note an unresolved ambiguity in the mechanism underlying the different rates of sewer and water access across the boundaries of the Southwest triangle. The exclusion of the Southwest triangle from the 1855 sewer plan reflects tiny variations in elevation and grade that cause differences in sewer construction costs. However, it is also possible that

excluding the Southwest Triangle from the 1855 plan had an effect on piped water and sewer provision independent of construction costs. One could resolve this ambiguity by relying on variation in initial elevation as a source of identifying variation. Because the 1855 plan reports finished rather than initial elevations, and because no record of earlier surveys exists, this is not possible.

Figure 4 suggests the possibility of implementing a fuzzy-RD design. Given our already small sample, and the discrete nature of the street grid, this research design would rely heavily on a tiny set of observations. To avoid this, we abstract from the spatial structure of the data and base our estimates on an instrumental variable design using the whole Quasi-experimental sample. To the extent our sample allows, we investigate the possibility of confounding spatial trends below.

The last row of Table 1 gives mean years until water and sewer access for transactions without sewer and water access. This is about 3.4 years for transactions in the southwest triangle, about 2.7 for those outside, and the difference is not statistically significant at conventional thresholds. This is important for two reasons. First, it means that our estimate of "the effect of water and sewer access on land prices" is really the effect a three year delay of this access. We will ultimately want to convert the value of this flow of services to the value of sewer and water service in perpetuity. Second, these statistics motivate our decision to organize our econometric analysis as a model of a binary treatment. Being north or south of Congress Street affects the likelihood of a three year delay of water and sewer access, but this delay is about the same on both sides of the border.¹⁸

Finally, the fourth column of table 1 highlights an important econometric challenge. It reports sample means from the Relevant

¹⁸An alternative approach to this problem would be to define treatment as a "years until water and sewer access". This raises two problems. First, this variables will be highly correlated with the year the transaction occurs, and hence with important business cycle variation in prices. Second, it requires that we consider an econometric model that permits multiple treatments. This would complicate our econometric problem dramatically.

sample. On average, these parcels are less expensive and further from the CBD than parcels in the Quasi-experimental sample. If we are to apply estimates of the effects of water and sewer access based on the Quasi-experimental study region to the Relevant area, we must consider the possibility that treatment effects may vary systematically between the two samples. We return to this issue below.

6 Estimation

Let Y_i be the log of parcel i 's transaction price observed in the data. Let X_i denote a vector of observable parcel attributes drawn from, *transaction year indicators, $\ln(\text{miles to CBD})$, $\ln(\text{Parcel Area})$, Corner indicator, distance to horsecar line and distance to major street*. Let D_i be a treatment indicator, with $D_i = 1$ if and only if parcel i has piped water and sewer access. Let Z_i be a binary variable with $Z_i = 1$ if and only if the parcel is *not* in the Southwest Triangle. We view Z_i as an instrumental variable and assume that it shifts the cost of access to piped water and sewers without directly affecting the land price, fixing controlling covariates. We define Z so that $Z_i = 1$ outside of the Southwest Triangle to assure a conventional positive relationship between instrument and treatment.

We adopt the convention of indicating potential outcomes with a subscript, so that Y_{1i} is the price of parcel i in a state of the world where it is treated, and Y_{0i} is the untreated price. Let U_1, U_0, U_D denote three error terms to be defined later. Finally let P denote our Quasi-experimental sample and, abusing notation slightly, the joint distribution of $(Y_1, Y_0, X, Z, D, U_1, U_0, U_D)$ drawn from this sample.

We are also interested in the corresponding quantities drawn from the Relevant sample, all transactions in the area receiving water and sewer access during 1874-80. We indicate these quantities with an asterisk. For example, Y_i^* is a transaction price drawn from this sample, and P^* denotes the distribution of $(Y_1^*, Y_0^*, X^*, Z^*, D^*, U_1^*, U_0^*, U_D^*)$.

We would like to estimate the average treatment effect on the economically relevant sample, that is, $\text{ATE}^* \equiv E(Y_1^* - Y_0^*)$. This treatment

Table 2: OLS, First Stage, Reduced form, and TSLS estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: OLS.								
Sewer=1	.354*** (.089)	.413*** (.086)	.407*** (.087)	.379*** (.144)	-.080 (.105)	.265*** (.084)	.276*** (.081)	.25*** (.079)
R^2	0.032	0.386	0.405	0.436	0.536	0.391	0.376	0.458
B: Red. Form								
SW $\Delta = 0$.731*** (.088)	.657*** (.072)	.805*** (.077)	.543*** (.106)	.338* (.173)	.457*** (.072)	.336*** (.063)	.322*** (.059)
R^2	0.171	0.486	0.527	0.504	0.540	0.447	0.397	0.478
C. 1st Stage								
SW $\Delta = 0$.449*** (.045)	.432*** (.039)	.440*** (.042)	.320*** (.056)	.187* (.097)	.432*** (.039)	.259*** (.031)	.259*** (.031)
R^2	0.252	0.451	0.451	0.454	0.469	0.451	0.333	0.334
F-stat	97.543	119.729	107.237	32.917	3.727	119.729	71.711	72.171
D. IV.								
Sewer=1	1.626*** (.265)	1.522*** (.220)	1.831*** (.244)	1.699*** (.425)	1.805 (1.323)	1.058*** (.195)	1.296*** (.277)	1.242*** (.263)
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y	Y	Y
H.car & Maj. St.& Corner			Y		Y			Y
Distance to Congress St.					Y			
1886-1889 Trend Correction						Y		
Sample	Q.E.	Q.E.	Q.E.	Q.E. 1k'	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Observations	351	351	351	172	351	351	533	533

Note: All results based on transactions during 1874-80. Columns 1-3, 5 rely on the Quasi-experimental sample, 7 and 8 on the Extended-quasi-experimental sample, and column 4 restricts attention to the subset of the Quasi-experimental sample within 1000' of Congress Street. (A) Reports OLS regressions of log transaction price on the treatment indicator. (B) Reports reduced form regressions log transaction price on the instrument. (C) Reports first stage regressions of treatment on instrument. (D) Reports TSLS estimate of the effect of water and sewer access on log parcel price. The bottom panel of the table indicates controls for all regressions in the column above. Robust standard errors in parentheses.
*, **, *** indicate 10%, 5%, 1% significance.

effect permits an immediate evaluation of a realized policy and matches neatly to available data on costs. Estimating ATE* requires that we address the conventional problem of estimating ATES rather than LATES. In addition, we must find a way to extrapolate our estimated treatment effect from the Quasi-experimental to the Relevant sample.

We first estimate local average treatment effects of piped water and

sewer access with TSLS.¹⁹ We next implement the local IV framework proposed by Carneiro et al. (2010). This framework offers a simple way to control for multi-dimensional X , allows the explicit calculation of an average treatment effect, and allows tests for heterogeneity of treatment effects with respect to observable and unobservable characteristics. The LIV/MTE framework also provides a foundation for a novel, principled approach to the extrapolation of treatment effects. We develop and implement this method in the final stage of our analysis.

Local Average Treatment Effects Table 2 presents four sets of estimates. For reference, Panel A presents OLS regressions of the form,

$$Y_i = A_0 + A_1 D_i + A_2 X_i + \varepsilon_i.$$

These regressions show a significant positive association between piped water and sewer access, and transaction prices. In the first column, we present a specification without controls. In the second column, we add year indicators and controls for lot size and log miles to the CBD. The coefficient estimate remains stable even as the controls add considerable explanatory power. In the third column, we add controls for corner properties, distance to horsecar, and distance to a major street. In each case, transaction prices are about 30 log points higher for parcels with water and sewer access. We postpone a discussion of the remaining columns.

Panel B presents the corresponding reduced form regressions of transaction price on the instrument,

$$Y_i = A_0 + A_1 Z_i + A_2 X_i + \varepsilon_i.$$

We see in column 1 that being in the Southwest triangle decreases transaction prices by about 70 log points. This effect is estimated precisely

¹⁹In addition to instrument exclusion, exogeneity, and monotonicity (no-defier condition) conditional on X , if the conditional expectation of D given X is linear, we can interpret the estimand of TSLS as a weighted average of the local average treatment effects aggregating compliers' conditional average causal effects given X . See Abadie (2003), Kolesár (2013), and Słoczyński (2021) for further detail.

and varies only slightly as we add control variables in columns 2 and 3. Column 2 uses the same controls as we used in Figure 4b, and so the estimated effect approximately corresponds to the average price difference between inside and outside parcels that we see in this figure.

Panel C presents first stage regressions,

$$D_i = B_0 + B_1 Z_i + B_2 X_i + \mu_i.$$

Conditional on control variables, being in the Southwest triangle reduces the probability of piped water and sewer access by about 40%. Again, this effect corresponds approximately to the mean difference in sewer access between inside and outside parcels in Figure 4a. First stage *F* statistics are above critical values for conventional weak instrument tests.

Panel D presents TSLS estimates of the effect of piped water and sewer access on transaction prices. IV estimates range between about 124 and 183 log points, estimated precisely.

In unreported results, we also add day of week indicators to each specification reported in table 2. This results in TSLS estimates that, except for column 5, are never statistically different from those in table 2.

Comparing IV to OLS results suggests that the equilibrium process assigns piped water and sewer service to parcels that are less valuable after conditioning on observable controls. This is consistent with anecdotal evidence presented earlier.

Figure 4 illustrates an increase in piped water and sewer access and transaction prices that occurs when we cross Congress Street to leave the Southwest triangle. These changes appear to occur sharply in the figure. Nevertheless, we are concerned that this increase may reflect a confounding spatial trend correlated with treatment and transaction prices. To address this concern, in column 4 of table 2 we restrict the sample to a narrower window that includes only parcels within 1000 ft. of Congress Street. The magnitudes of the reduced form and first stage are reduced, but the IV estimate is unchanged. In column 5, we include controls for distance to Congress Street in our regression of column 2 and

allow the slope of this trend to change at Congress Street. Once again these controls reduce the magnitude of first stage and reduced form effects by about half, but leave the iv point estimate unchanged, although the standard error increases to just above the 10% significance threshold.

To refine this test, we consider the impact of a hypothetical confounding trend in land prices across Congress Street, the trend that we observe across the Congress Street boundary during 1886-9, after piped water and sewer access is universal on both sides of the border. Implicitly, we suppose that the entire (small) trend we observe in 1886-9 is due to confounding unobservables rather than path dependence on an otherwise homogeneous landscape. Appendix Table B2 is similar to panel B of table 2, and reports this trend in column 3. We then subtract this trend from transaction prices, the dependent variable, in our 1874-80 sample in column 6 of table 2. Unsurprisingly, this leads to a smaller estimated treatment effect, but one that is estimated precisely and is still around 100 log points. We reach a similar conclusion more directly if we subtract the 36 log point upper confidence bound of the cross-border price difference estimated in column 2 of table B2 from the treatment effects estimated in table 2.

Summing up, the validity of our research design rests on four pieces of evidence. First, inclusion in the southwest triangle reflects variation in elevation that affects the cost of sewers but is otherwise practically imperceptible. This supports the a priori argument that the instrument affects outcomes only through its effect on the likelihood of treatment. Second, the disappearance of price differences across Congress Street after water and sewer access equalizes across this boundary suggests that, except for piped water and sewer access, the distribution of parcel prices is the same on both sides of the boundary. Third, the difference between ols and iv estimates is consistent with what one would predict from anecdotal evidence about the assignment process; the equilibrium assignment process favors cheaper parcels than does quasi-random assignment. Finally, the robustness of results to various choices of control

variables, and to correction for a confounding spatial trend, suggests that omitted variables correlated with the instrument and outcome are not confounding our estimates.

The estimates in panel D of Table 2 are LATES for our Quasi-experimental sample. We now turn our attention to whether this estimate differs from the ATE in this sample and whether we can extrapolate to the Relevant sample.

To begin, columns 7 and 8 of Table 2 re-estimate the specifications of columns 1 and 2 on the Extended-quasi-experimental sample. That is, the sample of transactions drawn from within 2000' of the northern or eastern boundary of the Southwest Triangle.

A Local Average Treatment Effect coincides with the Average Treatment Effect if treatment effects are the same for all units. By expanding our sample, we change the set of compliers, and hence the sample of units over which the LATE is estimated. We observe that coefficients in columns 7 and 8 are statistically indistinguishable from their counterparts estimated on the smaller Quasi-experimental sample. This suggests either that treatment effects are not heterogeneous, or that the distributions of treatment effects in the two samples of compliers are similar.

We would ultimately like to extrapolate our estimate to the Relevant sample. The Extended-quasi-experimental sample has a larger support for X and, presumably, a larger support for unobservable determinants of treatment and potential outcomes. In this sense, less extrapolation is required from the Extended-quasi-experimental sample to the Relevant sample, than from the smaller Quasi-experimental sample.

We note that the validity for our research design is easier to defend on the smaller Quasi-experimental sample than the Extended-quasi-experimental. Figure B2 in the appendix reproduces the border plots of Figure 4 for the larger sample. Neither prices nor sewer access change as sharply at the boundary of the Southwest Triangle in the larger sample. This is because, 20 years after the 1855 ordinance, both

sides of the eastern boundary of the Southwest Triangle have sewer service, see Figure 1. This increases our concern about the possibility of a confounding trend across the border and motivates our preference for estimates based on the smaller Quasi-experimental sample.

The choice of specifications presented in Table 2 reflects our interest in extrapolating estimates. We do not consider more flexible specifications for the effect of distance to CBD for two reasons. Extrapolation to the larger and more remote Relevant sample based on (e.g.) polynomials in distance to the CBD, is sensitive to functional form. Moreover, prior evidence provides strong support for our simple specification.²⁰

In a similar spirit, we do not include measures of distance to the Chicago River in the results presented in Table 2. Because the Chicago River runs approximately parallel to our Quasi-experimental sample, and approximately perpendicular to the Relevant sample, extrapolating this effect is hard to defend. Finally, we do not consider demographic variables from the 1880 census as controls for three reasons. First, the spatial resolution of these data is poor enough relative to the scale of our analysis that we are skeptical of their explanatory power. Second, these data are decennial cannot reflect the higher frequency changes in demographics relevant for our sample. Finally, demographic variables probably depend on sewer and water access, and so are bad controls.

With these caveats in place, Table B3 presents supplementary results that allow for more flexible effects of distance to the CBD, include a control for distance to the Chicago River, and include demographic controls. Broadly, the results presented in Table 2 are robust to these changes, although the effect of treatment falls modestly with the inclusion of demographic variables. Appendix B provides details.

²⁰Ahlfeldt and McMillen (2018) find that land prices in the entirety of late 19th century Chicago track the logarithm of distance from the CBD closely. This basic conclusion is confirmed in French and Japanese cities (Combes et al. (2019), Lucas et al. (2001)).

Table 3: LIV Regression Test Statistics

	(1)	(2)	(3)	(4)
χ^2	220	235	243	251
H0: $\delta_1 - \delta_0, \gamma_1, \gamma_2, \gamma_3 = 0$	0	0	.005	0.000
H0: $\delta_1 - \delta_0 = 0$.108	.141	.298	0.0002
H0: $\gamma_2, \gamma_3 = 0$.002	.002	.656	.056
H0: $\delta_1 - \delta_0, \gamma_2, \gamma_3 = 0$.001	.001	.15	0.000
ATE	1.04*** (.40)	1.00*** (.36)	1.31* (.69)	1.41** (.70)
ATE*	1.04*** (.31)	1.10*** (.40)	1.05** (.46)	1.04** (.47)
Carr & Kitagawa	0.286	0.252	0.866	0.374
Year FE & ln(Area)	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y
H.car & Maj. St.& Corner		Y		Y
Sample	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Observations	351	351	533	533

Note: Various test statistics based on estimates of the LIV model of equation (3) and estimates of ATE and ATE* based on equations (5) and (8). Complete report of coefficient estimates is in table B4. All estimations based on transactions during 1874-80. Columns 1, 2, and 3 rely on the Quasi-experimental sample, 4 and 5 on the Extended-quasi-experimental sample. Bottom panel indicates controls for the regression above. Bootstrapped standard errors in parentheses. *, **, *** indicate 10%, 5%, 1% significance.

Marginal and Average Treatment Effects

The LIV/MTE framework developed in Heckman and Vytlacil (2005) and Carneiro et al. (2010) offers a method to estimate treatment effect heterogeneity and a framework to evaluate the difference between LATES and ATES. Moreover, as we will show, this framework provides a foundation for extrapolating our estimates from the Quasi-experimental to the Relevant sample under a weaker assumption than “no heterogeneous treatment effects”.

The LIV/MTE framework recasts the potential outcomes framework described in Angrist et al. (1996) as a Roy model. Each unit selects into treated or untreated status on the basis of a third selection equation.

Formally,

$$\begin{aligned} Y_1 &= X'\delta_1 + U_1 \\ Y_0 &= X'\delta_0 + U_0 \\ D &= \mathbf{1}[v(X,Z) - U_D \geq 0], \end{aligned} \tag{1}$$

where Y_1 denotes a treated potential outcome and Y_0 is not treated. We assume that the controls enter the potential outcome equations linearly with coefficients δ_1 and δ_0 , and make the “practical independence” assumption as in Carneiro et al. (2010),

$$(X,Z) \perp (U_1, U_0, U_D) \tag{2}$$

U_D measures unobserved “resistance to treatment,” in our context, unobservable determinants of the cost of piped water and sewer access for each parcel. We assume that U_D is continuously distributed.

Let $p = F(X,Z) \equiv P(D = 1|X,Z)$ be the propensity score in the Quasi-experimental sample. Let \tilde{U}_D denote U_D normalized by its cdf. That is, $\tilde{U}_D = F_{U_D}(U_D) \sim Unif(0,1)$. This transformed unobserved heterogeneity ranks units in the population P according to the unobservable cost of access to piped water and sewage, i.e., \tilde{U}_D is smaller as unobserved costs of piped water and sewer access are smaller. On the basis of arguments in Carneiro et al. (2011), we state our estimating equation and subsequent derivations in terms of this transformed variable.

Define marginal treatment effects, MTE, for each conditioning covariate value X and $\tilde{U}_D \in [0,1]$ as

$$\text{MTE}(X, \tilde{U}_D) \equiv E(Y_1 - Y_0 | X, \tilde{U}_D)$$

That is, MTE describes how causal effects vary with observable characteristics, X , and with the unobservable resistance to treatment, \tilde{U}_D .

To estimate MTEs, we run the local IV regression

$$\begin{aligned} p &\equiv \Pr(D = 1|X,Z) = F(X,Z), \\ Y &= X'\delta_0 + \hat{p}X'(\delta_1 - \delta_0) + K(\hat{p}) + \varepsilon. \end{aligned} \tag{3}$$

The first equation is a first stage binary regression of treatment status on the instrument and controls. In our case, a Logit regression with linear index in (X, Z) . The second equation is a structural equation with a control function in \hat{p} , where the additive functional form follows from our specification (1) and the practical exogeneity restriction (2). In light of our small sample size, we restrict attention to the case with a parametric cubic specification for $K(\cdot)$,

$$K(\hat{p}) = \gamma_1 \hat{p} + \gamma_2 \hat{p}^2 + \gamma_3 \hat{p}^3.$$

Heckman and Vytlacil (2005) show that the derivative of the local IV regression with respect to the propensity score identifies the marginal treatment effect, and that taking the expectation of MTE over (X, \tilde{U}_D) identifies the average treatment effect. That is,

$$\text{MTE}(X, \tilde{U}_D) = X'(\delta_1 - \delta_0) + \gamma_1 + 2\gamma_2 \tilde{U}_D + 3\gamma_3 \tilde{U}_D^2 \quad (4)$$

$$\text{ATE} = E(X)'(\delta_1 - \delta_0) + \gamma_1 + \gamma_2 + \gamma_3. \quad (5)$$

Equation (4) allows explicit tests for heterogeneity of treatment effects. If $\delta_1 - \delta_0 \neq 0$ then the marginal treatment effects vary with unit observables. If γ_3 or $\gamma_2 \neq 0$ then the marginal treatment effects vary with unobserved resistance to treatment. Rejecting both sorts of treatment heterogeneity means that LATE, any weighted average of MTES, and ATE are all equal. In this case, the conventional linear TSLS estimator for the coefficient of endogenous D is a consistent estimator of ATE.

We estimate equation (3) for specifications corresponding to those in columns 2, 3, 7, and 8 of Table 2. Because equation (3) is quite long, we relegate a complete report of parameter estimates and bootstrapped standard errors to appendix Table B4. Table 3 reports estimates of ATE derived from these regressions, along with several hypothesis tests.

The first row of Table 3 reports a χ^2 test of the significance of our instrument in the first stage Logit regression. As in our TSLS estimations, we easily reject the hypothesis that our instrument does not affect treatment.

The second row of Table 3 reports p-values of the tests of the hypothesis that all terms involving the propensity for treatment are zero. That is, that treatment effects are different from zero. This is rejected in all specifications. Piped water and sewer almost surely affect land prices in our Quasi-experimental and Extended-quasi-experimental samples.

The third row tests the hypothesis of homogeneity of effects by observables. The fourth row tests whether the hypothesis of homogeneity of effects by unobservables. The fifth row tests the joint hypothesis of either sort of treatment effect homogeneity.

The results of these tests vary with sample. In our Quasi-experimental sample, columns 1 and 2, we see clear evidence of treatment heterogeneity on unobservables, somewhat weaker evidence for treatment effects on observables, and clearly reject the hypothesis of treatment effect homogeneity. Columns 3 and 4 consider the larger Extended-quasi-experimental sample. Here, we reject the hypothesis of treatment effect homogeneity at the 15% level in Column 3, but we cannot reject treatment effect homogeneity by observables or unobservables alone. We can reject the hypothesis of treatment homogeneity by both observables and unobservables when adding additional controls in Column 4. Inspection of appendix Table B4 suggests that treatment effects likely vary by year in all specifications, though there is no clear pattern in the coefficients across years.

The sixth row of Table 3 calculates the average treatment effect given in equation (5) along with bootstrapped standard errors. Comparing to the LATES estimated in Table 2 we see that ATES are marginally smaller than corresponding TSLS LATES in the Quasi-experimental sample, and both are estimated precisely. In the larger Extended-quasi-experimental sample, ATE and LATE are statistically indistinguishable. Even the smallest of these ATE estimates is still very large; $e^{1.00} \approx 2.7$, so these estimates indicate that piped water and sewer access at least doubles land values.

The differences between LATE and ATE estimates are consistent with other results in rows 3 to 5 of table 3. Heterogeneous treatment effects are

necessary if ATE and LATE are to diverge.

Figure B3 presents a standard diagnostic for the LIV regression presented in column 2 of Tables B4(a) and (b). This figure is a histogram showing the frequency of treated and untreated transactions as a function of \hat{p} . As we expect from Table 1, the distribution of parcels is heavily skewed toward “treated”; 0.47 of the Quasi-experimental sample South of Congress Street has piped water or sewer access, and this share is even higher to the North. With this said, conditional on this skewed distribution, the histograms for treated and untreated parcels are similar, although there is more mass left of 0.6 for untreated parcels. The corresponding histograms for other specifications reported in Table B4 (not reported) are qualitatively similar.

Figure B4 is a second standard diagnostic figure. Figure B4 plots marginal treatment effects as a function of resistance to treatment, \tilde{U}_D , and lets us visualize the importance of treatment heterogeneity on unobservables. In light of the hypothesis test presented in column 2, row 4 of Table 3, that this figure suggests marginal treatment effects change with unobservables is unsurprising. Because most of the probability mass of treated and untreated parcels has \hat{p} of at least 0.6, the region of Figure B4 to the left of 0.6 should be understood as extrapolation from the larger values. To investigate the importance of this issue, in unreported results we also consider a quadratic formulation of $K(\cdot)$. This change in specification does not qualitatively change our results.²¹

The final row of Table 3 presents the p -value for the instrument validity test proposed in Carr and Kitagawa (2021). This test evaluates the joint null hypothesis of practical exogeneity (2), instrument monotonicity, and the functional form specification for the potential outcome equations (1). p -values consistently above 15% indicate that the data do not reject the

²¹Identification of $MTE(X, \tilde{U}_D)$ without a parametric control function $K(\cdot)$ is possible for values of \tilde{U}_D supported by the distribution of propensity scores. Figure B3 indicates that observations with propensity scores near 1 largely contribute to the estimation of cubic $K(\cdot)$. MTE estimates for the range of \tilde{U}_D 's without much probability mass extrapolate using the functional form of $K(\cdot)$.

assumptions on which our MTE and ATE estimates rely.²²

Extrapolation to Relevant sample While our LIV estimation does not offer conclusive evidence for the importance of heterogeneous treatment effects, neither does it offer much reassurance that they are not important. Given this, we consider the problem of extrapolating our ATE estimates under both assumptions, that treatment effects are heterogeneous, and that they are not.

In the absence of treatment heterogeneity, extending our treatment effect estimates from the Quasi-experimental to the Relevant sample is straightforward. Estimates in Table 2 can be interpreted as Average Treatment Effects, and provided treatment effects remain constant on the larger support of the Relevant sample, these estimates apply immediately to units in the larger sample.

However, Table 3 suggests that concern about treatment heterogeneity is warranted. Given this, we develop a method for extrapolating treatment effects in the presence of treatment heterogeneity.

This extrapolation requires that equations (1) and (2) continue to hold on the Quasi-experimental sample. In addition, we assume

$$\begin{aligned} Y_1^* &= X^{*\prime} \delta_1 + U_1^* \\ Y_0^* &= X^{*\prime} \delta_0 + U_0^* \\ D^* &= \mathbb{1}[v(X^*, Z^*) - U_D^* \geq 0]. \end{aligned} \tag{6}$$

and that

$$P_{U_1^*, U_0^*, U_D^*}^* = P_{U_1^*, U_0^*, U_D^*}. \tag{7}$$

²²We also apply the IV validity test of Mourifié and Wan (2017). This test evaluates the strict exogeneity of instrument (i.e., Z is also independent of X) rather than conditional exogeneity. We do not reject the null of instrument validity at 5% significance level for the Quasi-experimental sample. However, we do reject the null at the same level for the Extended-quasi-experimental sample. Taken together with the results of the Carr & Kitagawa test reported in Table 3, this means that we reject the strict exogeneity of our instrument, but fail to reject conditional exogeneity. It follows that controlling for conditioning covariates is important for the estimation of causal effects in our model, particularly in the Extended-quasi-experimental sample.

In words, we assume that the same econometric model governs the effects of treatment in the Relevant sample as in the Quasi-experimental sample and that the marginal distribution of unobserved heterogeneities is the same across the two samples. These conditions would be satisfied, for example, if the mechanism and magnitude of the causal effect are the same in both samples, and unobserved resistance to receiving the treatments is identically distributed between them.

In our data, the cost shock Z is observed on the Quasi-experimental sample and latent on the Relevant sample. In addition, we can credibly assume that Z is randomized in the Quasi-experimental sample, but Z^* is probably not randomized in the Relevant sample, even if it could be observed. Our approach to extrapolation does not require that the joint distributions of observable characteristics and the instrument are identical for the Quasi-experimental and Relevant samples.

Assuming equations (1), (2), (6) and (7), we can extrapolate MTE estimates from the Quasi-experimental to the Relevant sample and use them to calculate an average treatment effect on the Relevant sample as follows,

$$\text{ATE}^* = E(X^*)'(\delta_1 - \delta_0) + \gamma_1 + \gamma_2 + \gamma_3. \quad (8)$$

Appendix C provides a proof.

In words, the average treatment effect for the Relevant sample is the same as for the Quasi-experimental sample, except that we must adjust for differences in the distributions of observable controls between the two samples. If the structural equations that govern treatment effects and assignment are the same across samples, and if the distribution of unobservables is the same, then we can extrapolate MTE estimates. This result holds even if the instrument is latent or dependent on the unobservables in the Relevant sample, or if the support of observable controls differs across samples. This result seems intuitive and, to our knowledge, no similar result exists in the literature.

The seventh row of Table 3 presents our estimates of ATE^* for each of

our specifications, along with bootstrapped standard errors. All are estimated precisely enough that they may easily be distinguished from zero. These estimates of ATE^* range from 1.04 to 1.10, across all samples and specifications. There is even less variation in ATE^* across samples and specifications than we saw for ATE , but in no case is the ATE^* statistically distinguishable from the corresponding ATE .

Conditional on the validity of our estimates of ATE , the validity of our estimates of ATE^* hinges on equations (6) and (7). Ideally, we would be able to test whether these equations hold in our data. We have not been able to define such a test, and our investigations suggests that a test may not exist except in the uninteresting case where there is no treatment heterogeneity. In the absence of a formal test, we provide informal evidence that the Quasi-experimental and Relevant samples are both governed by the same basic economic logic.

Figure B5 compares the Quasi-experimental and Relevant samples. Panel (a) of Figure B5 reports mean log prices by year in the Relevant and Quasi-experimental samples, conditional on: $\ln(\text{Area})$, $\ln(\text{miles to CBD})$, and corner. Panel (b) reports mean log prices by parcel area in both samples, conditional on year indicators, $\ln(\text{miles to CBD})$, and corner. Finally, panel (c) gives counts of transactions by year and sample. Other than the differences in levels, the two samples show similar patterns and suggest no contradiction to the hypothesis that the same basic economic forces are at work determining prices in the Quasi-experimental and Relevant samples.

7 The value of piped water and sewer access

We can now calculate the effect of piped water and sewer access on land values in the relevant area. We proceed in four steps. First, we calculate the area affected by the piped water and sewer expansion of 1874-80. Second, we calculate average price per square foot of an untreated parcel in this region. Third, we calculate the increase in price per square foot that results from piped water and sewer access. Fourth, multiplying this increase by the area affected gives the total increase in

land value resulting from piped water and sewer expansion during 1874-80.

An average residential lot in any of our samples is about 125 feet deep. If we assume that every sewer serves lots on both sides of one street, then each linear foot of sewer serves 250 ft² of land area. Our shapefiles of the sewer network then allow us to calculate that about 138m ft² of land received piped water and sewer access during 1874-80.

During 1874-80, 384 untreated parcels transacted in the Relevant sample area. The total area of these parcels was about 1.8m ft², and their aggregate value was about 0.81m 1880 dollars. Dividing, the average price per ft² of untreated land in the Relevant area was about 0.45 dollars.

We must now decide whether to apply an estimated ATE that does or does not allow for heterogeneous treatment effects. Our LIV estimates do not strongly support either hypothesis, and so we proceed using the smallest estimate, 1.04, from column 1 of table 3.

Applying this treatment effect to the price per square foot of untreated land in the Relevant sample area, we calculate that piped water and sewer access increases the value of land in this area by $0.45 \times (e^{ATE^*} - 1) = 0.82\$/ft^2$, about 180%. Multiplying this increase by the area affected, the total value of the piped water and sewer expansion is slightly above 113m 1880 dollars.

This estimate requires several comments. First, this calculation reflects our smallest estimate of the average treatment effect. If, as we might do on the basis of column 3 of table 3, we reject the hypothesis of heterogeneous treatment effects, then the LATES we estimate in Table 2 can be defended as ATES and extended to the relevant sample. In this case, using column 7 in table 2 (the analog of column 3 of table 3) we have ATE = 1.3. Using this estimate to value piped water and sewer access gives about 164m 1880 dollars.

Second, an average parcel in the Quasi-experimental sample receives piped water and sewer service about three years after it is sold. Thus, our estimates reflect the flow value of three years of piped water and sewer

access, not the full asset value. Hoyt (2000) reports that interest rates were about 8% during our study period. If we denote our estimated aggregate value by V^* and assume that this flow value arrives every three years in perpetuity, then the full asset value of piped water and sewer access is $\sum_{t=0}^{\infty} \left[\left(\frac{1}{1.08} \right)^3 \right]^t V^* \approx 4.9V^*$. Thus, we should multiply by about 4.9 to scale up our three year flow value to an asset value. Applying this adjustment to our 113m dollar estimate of the three year flow value, we have an asset value of about 554m 1880 dollars.

Third, as we noted earlier, piped water and sewer expansions were largely paid for with bonds that were serviced by property taxes (Chicago Board of Public Works, 1873). Capitalization construction costs into transaction prices would bias our estimates of treatment effects downward.

Finally, while it seems reasonable to ignore general equilibrium effects in our estimates of treatment effects based on the relatively small Quasi-experimental sample, this assumption seems difficult to defend when we extend our estimates to the Relevant area, the entire area that received piped water and sewer access between 1874-80. Given this, our estimates of the value of piped water and sewer expansion should be understood as a basis for evaluating a marginal counterfactual change in the extent of the Relevant area, or as being net of general equilibrium effects.

With our estimates of the value of piped water and sewer access in place, we turn to estimates of its cost. We digitize expenditures on water and sewer for the 1874-80 period (Chicago Board of Public Works, 1873). Sewer and water works expenditures during this time were \$1.5m and \$2.4m. Maintenance expenditure was about \$0.4m per year. Assuming maintenance costs constant in perpetuity and discounting at the same 8% rate we use above, the discount present value of maintenance is \$5.0m. Summing, total expenditure on water and sewer access is \$8.9m.

Our estimate of the three year flow value of piped water and sewer

access was about \$113m, about 13 times the total cost of the water and sewer system. Our estimate of the total asset value piped water and sewer access is \$554m, about 62 times as large as costs.

We would like to compare our estimate of the benefits of water and sewer access based on land prices to those based on health outcomes for two reasons. First, finding that purely health related benefits exceed the value reflected in land price would suggest a problem with one of the two estimates. Second, the difference between the two estimates will give us some insight into the value of non-health related effects of water and sewer infrastructure.

Alsan and Goldin (2019) estimate that all water and sewage related public health interventions were jointly responsible for a 26% reduction in infant mortality in Boston between 1880 and 1920. In 1896, the infant mortality rate in Boston was about 163/1000. From the 1880 census, there were 3014 infants living in the Relevant sample area in 1880. Elementary calculations using these numbers suggests that water and sewer access would prevent about 127 infant deaths per year. Costa and Kahn (2004) estimates that the value of statistical life in 1900 was about 516,000 USD2011, or 23,200 USD1880.²³ Multiplying, we have an annual value of averted infant deaths of about 3m dollars. Recall that our estimate of treatment effects is a three year effect, suggesting that we multiply this by three to compare it with our 113m dollar estimate for the value of piped water and sewer access. This suggests that the value of water and sewer access was about 12 times as large as the value of averted infant mortality and, therefore, that non-mortality related benefits of water and sewer access are economically important. This is consistent with anecdotal evidence.

We can also compare our estimates to the likely ability of residents to pay. Average income for Chicago laborers were about \$650 in 1880²⁴

²³We adjust prices using indices from Sahr (2009) for the period 1880-1912 and the BLS CPI series for 1913-.

²⁴See footnote 5 for details.

From Table 1 we have that the average log value of a property in the Quasi-experimental region north of Congress street was 8.4, or about 4,500 dollars. Almost all of these parcels had water and sewer access, so this is effectively an estimate of the price of a parcel with water and sewer access. A treatment effect of 1.04 log points means that an untreated parcel is worth about one third as much as a treated one. Thus, we have that water and sewer access increases the value of a parcel by about 2,910 dollars, or around five years income for an average unskilled laborer. If a household financed its parcel with a 10 year note at 8% interest, then payments would be about 250\$ per year for an average parcel without water and sewer access, and about 710\$ with. Thus, for a household with three people working at the average income of 650\$/year, the cost of a parcel without water and sewer access would have been about 12% of annual household income, versus about 36% for a parcel with water and sewer access.

8 Conclusion

While tremendous progress has been made in providing safe water and modern sanitation for the relatively poor recent immigrants to developing world cities, access is far from universal. A large body of evidence suggests that in the absence of modern public health and sanitation infrastructure, urban density causes disease. Increasing access to high quality drinking water and modern sanitation would seem to call for a crisis response. However, relatively poor developing world cities face a portfolio of crises. Not only do their residents need more and better water and sewer infrastructure, they also need more and better roads, public transit, electricity supply and distribution, education, and housing. Trade-offs must be evaluated and made.

With this in mind, piped water and sewer access are conspicuously understudied. There is now a large active literature evaluating various improvements to transportation infrastructure, both in the developed and developing world. Electricity generation and distribution has also received attention. The literature on piped water and sewer access is

much less developed. Indeed, as a result of divergent estimates in Alsan and Goldin (2019) and Anderson et al. (2018), recent research has increased our uncertainty about the importance of public health policy. In this light, our results are doubly important. We are the first to evaluate the effect of piped water and sewer access on land prices, a comprehensive revealed preference measure of value, and our results suggest a high value for piped water and sewer access.

This generally supports a high priority for water and sewer infrastructure. It also highlights the importance of further research on the issue. Infant mortality rates and the benefits of water treatment appear to be of about the same magnitude in late 19th century as in parts of the modern developing world. However, the disease environment in modern Latin American and African cities is different than it was in 19th century Chicago (see Henderson and Turner (2020)), so studies conducted in developing world cities are desirable. An important obstacle to such research has been the difficulty of devising a research design to estimate causal effects. We are hopeful that a variant of the research design we develop can help to address this issue.

Our results also inform the ongoing inquiry into the development of the American economy. Up until now, almost all evidence for or against the importance of piped water and sewer infrastructure reflects changes in mortality rates, and is estimated by comparing outcomes before and after a particular intervention. By offering a novel research design and a different outcome, we provide independent evidence for the importance of piped water and sewer infrastructure. Our estimate indicates that piped water and sewer access more than doubled land prices. A back of the envelope comparison suggests that the increase in aggregate land rent is a multiple of the value of foregone infant mortality caused by water and sewer access, and hence that benefits of water and sewer access that are not narrowly related to mortality and health are economically important.

Finally, we propose a technique for the principled extrapolation of treatment effects from a quasi-experimental study area to an area that is

more relevant for economic analysis. The practice of restricting attention to small populations or areas, carefully chosen so that a quasi-experimental research design may be defended, is a pervasive practice in applied micro-economic analyses. Thus, so to is the problem of extrapolating to more economically interesting samples. We hope that our technique for extrapolating treatment effects will, therefore, find wide use among other applied researchers.

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The Value of Piped Water and Sewers: Evidence from 19th Century Chicago

Online Appendices

Appendix A Data construction and description

Transaction data: We digitize the entire set of house and land transactions reported in every Sunday *Tribune* starting in October 1873 when reporting began, and ending in April 1889 when the *Tribune* stopped reporting transactions below \$1000 in order to limit the size of the column. Figure A1 shows a few sample transaction listings. Because we have just three months of data in 1873, and because transaction volumes were low in the these three months, we begin our analysis in 1874, our first complete year of transaction data.

The *Tribune* reports both vacant parcels and parcels with a house. Parcels with a house are denoted by an address, or "Premises Number" and are easily distinguished from transactions without a house. About 97% of the transactions reported in the Tribune are land transactions. The location of vacant parcels is given by an intersection, that of the street the parcel fronts and the nearest cross-street. The intersection is a useful georeference. While street names are not permanent, they are persistent, and several digitized maps exist recording the street names as they existed in the late 1800s. For reference, figure A2 reports street names in an area around our Quasi-experimental study area from the Urban Transitions project (Logan et al., 2011). The Tribune's reporting of intersections, together with the persistence of street names and the availability of digital street maps motivates our strategy for geocoding land transactions by matching them to the nearest intersection.

House transactions report a regular street address rather than the nearest intersection. Geocoding these addresses is not feasible for two reasons. First, the city of Chicago renumbered all of its house addresses in 1909. Second, this renumbering was motivated by the prevailing disorder

of street numbering:

Prior to the 1909 street renumbering, Chicago street numbers were chaotic. There were several separate and distinct numbering systems. The baseline for street numbers varied from street to street. The location of a number on one street thus did not correspond to the location of the same number on another street running in the same direction. Critics often complained that the city's street numbers were without system. - The Chicago Directory Company (1909).

The changes in the numbering system and the difficulty of establishing a correspondence between the old and new numbering systems rules out the use of modern geocoders.

We digitize 5751 land transactions between 1874 and 1880. Of these, we successfully geocode 4421. Figure A1 illustrates the distribution of these transactions across the intersections in the whole city (a) and in the area around our Quasi-experimental area (b). Each transaction is represented by a circle. Since transactions are matched to intersections, many intersections match to many transactions, and a darker circle on an intersection indicates that more transactions match to that intersection. We record transactions all over the city. Panel (a) gives a sense for the magnitude of this data collection effort. Panel (b) shows that transactions are distributed fairly uniformly in our Quasi-experimental area, although this is not true for regions outside this area.

As a check, we investigate the location of 20 ungeocoded land transactions manually. Of the 11 for which we could establish locations, nine were outside the 1880 city limits. Our geocoding is based in part on the 1880 Chicago street map (Logan et al., 2011) that entirely covers our study area. That is, within city limits and between one and three miles from the CBD. In contrast, the *Tribune* reports transactions beyond city limits but within seven miles of the county court house. This range includes outlying towns such as Forest Park, Evanston, and Hyde Park, that are not covered by our street map. This suggests that most of the transactions that we could not geocode lie outside of our study area.

Figure A1: Land transactions in the Chicago Tribune

SATURDAY'S TRANSFERS.	
The following instruments were filed for record Saturday, April 10:	
CITY PROPERTY.	
Walnut st, 120 ft e of Western av, s f, 30x 128 ft, dated April 10 (A. E. and C. M. Hemler to John T. Shannon).....	\$ 2,025
West Superior st, 49 4-10 ft e of Lincoln, n f, 25x128 ft, dated April 10 (B. F. Crosby to O. B. Olson).....	600
Cleaver st, 225 ft s of Bradley, w f, 37½ x125 ft, dated April 8 (Mat Schillo et al. to M. Kufei et al.).....	750
West Madison st, 428 ft w of Staunton, s f, undivided $\frac{1}{2}$ of 24x126 ft, dated April 6 (Mary J. Seymour to C. L. Wehe).....	2,400

Note: *An example of listings of land transactions in the Chicago Tribune. Our land transaction data results from digitizing all transactions filed on Saturday between 1873 and 1889. Note that each record reports the nearest intersection, price, and area. Records also report if the parcel is on a corner.*

Table A1 compares transactions that we did and did not successfully geocode. Year, frontage, and depth are approximately the same across geocoded and ungeocoded parcels. Ungeocoded parcels are slightly larger. The large price difference between geocoded and ungeocoded parcels probably reflects the fact that ungeocoded parcels tend to be far from the CBD.

1880 Census: It is natural to suspect that the demographic characteristics of residents will affect and be affected by sewer assignment and land prices. To investigate this process, we incorporate the 1880 census into our data.

The 1880 census reports data aggregated to the level of the 'enumeration district'. Figure A4 superimposes a map of these regions on our Quasi-experimental area. In total, 21 enumeration districts intersect our quasi-experimental study area. Of these 21; 5 span Congress St., 3 are entirely north of Congress St, within the study area, 2 are entirely south of Congress St, within the study area, 7 have some part of the ED

Figure A2: Map of Study Area with Street Names



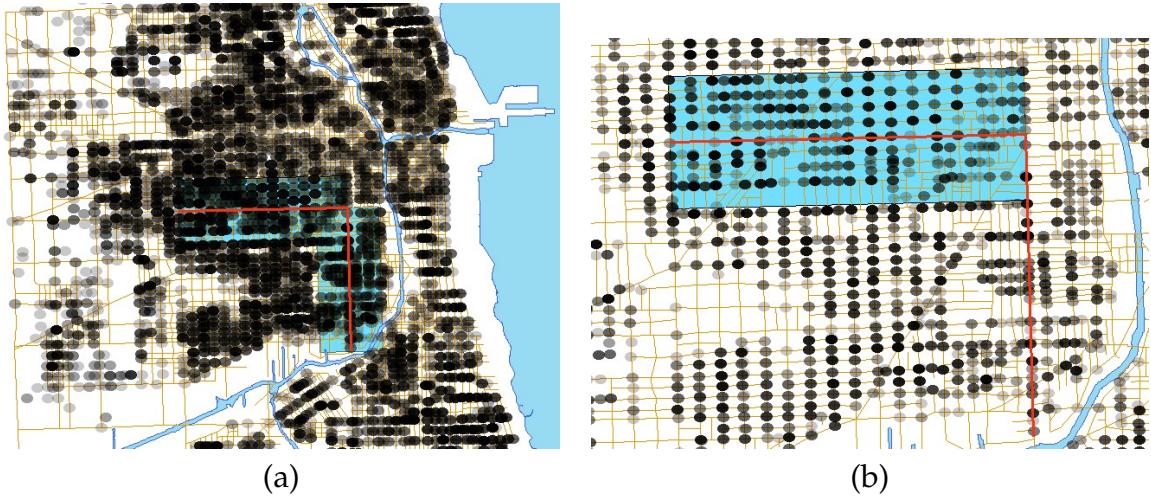
Note: Illustration of street map with street names in the Quasi-Experimental Area (Logan et al., 2011).

intersecting the study area north of Congress St. and 4 have some part of the ED intersecting the study area south of Congress St. Prorating on the basis of area, we are able to use these data to construct estimates of demographic characteristics for our Quasi-experimental and Relevant sample areas.

Table A2 reports means of demographic characteristics from the relevant area, from the quasi-experimental area, and from the whole city. Although the spatial resolution of these data is poor relative to the size of our quasi-experimental study area, they suggest that the quasi-experimental area was relatively specialized in professional and tradespeople and that the foreign born were marginally less common than in the other areas.

While these variables are of obvious interest to our analysis, we make limited use of them. Their spatial resolution is too coarse to permit them to register changes at the spatial scale we use in our research design and their decennial frequency prevents them from registering changes at the annual frequency of the rest of our data.

Figure A3: Map of Geocoded Parcels

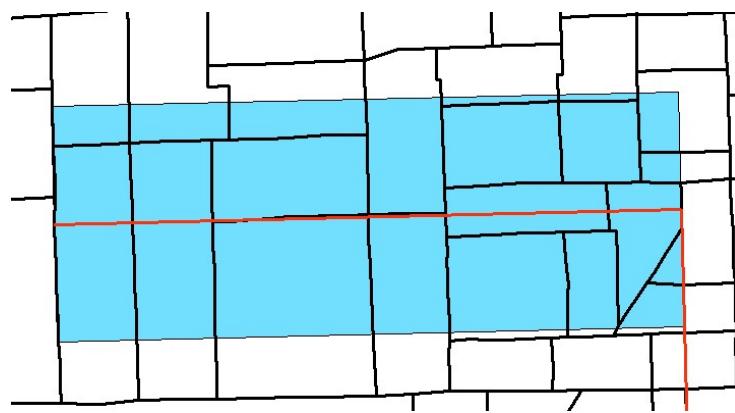


Note: *Geocoded parcels. Entire city (a) and zoom to study area (b). In both panels a disk indicates an intersection to which we match a transaction. Darker disks indicate that we match more transactions to that intersection.*

Table A1: Comparison of Geocoded and Ungeocoded Parcels, 1874-1880

	Ungeocoded	Geocoded	T-test
Price	3065.82 (5157.86)	4459.59 (10402.74)	-4.71
Year	1877.57 (2.18)	1877.59 (2.19)	-0.21
Frontage	34.66 (19.25)	33.18 (17.34)	2.66
Depth	124.59 (44.36)	121.15 (27.80)	3.39
Observations	1330	4421	

Figure A4: Map of Study Area with Overlaying 1880 Enumeration Districts



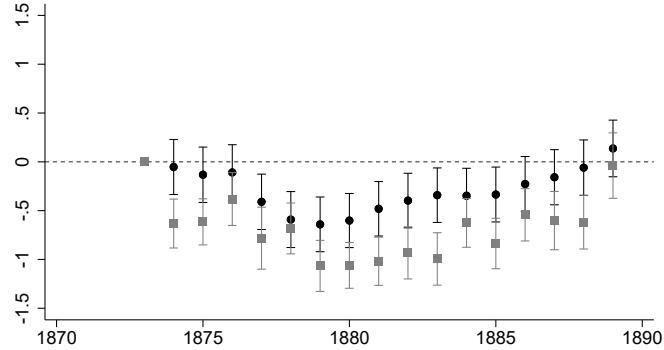
Note: 1880 Census enumeration districts overlaying Quasi-Experimental Area
(Logan et al., 2011).

Table A2: Demographics from the 1880 Census

	Relevant	Quasi-Experimental	Citywide
Total Population	93,394	35,367	503,489
Share White	0.996	0.996	0.987
Share Black	0.004	0.003	0.013
Share Foreign Born	0.431	0.302	0.407
Share Canada	0.023	0.059	0.028
Share Czechoslovakia	0.040	0.004	0.024
Share England	0.023	0.046	0.026
Share German	0.169	0.045	0.147
Share Ireland	0.080	0.100	0.088
Share Norway	0.028	0.005	0.019
Share Poland	0.020	0.001	0.014
Share Scotland	0.007	0.015	0.008
Share Sweden	0.018	0.006	0.026
Share Professional	0.096	0.110	0.117
Share Trade	0.083	0.124	0.102
Share Manufacturing	0.159	0.134	0.153

Note: Columns one and two contain demographic information for the Relevant and Quasi-Experimental regions respectively. These values are constructed through areal interpolation of enumeration districts from the 1880 full count census. Column 3 contains the full count demographics for the city of Chicago.

Figure B1: Land prices in Chicago and Quasi-experimental sample



Note: Mean $\ln(\text{Price})$ by year, relative to 1873, in Quasi-experimental sample (Gray) and all of Chicago (Black). Controls: $\ln(\text{miles to CBD})$, corner, $\ln(\text{Area})$.

Appendix B Supplemental results

Our Quasi-experimental sample is a set of 351 transactions occurring between 1874-1880 within 2000' of Congress Street, west of Halsted. Gray squares in figure B1 report mean log transaction price by year (after controlling for corner status, log of parcel area, and log miles to the CBD), for all transactions falling in the Quasi-experimental region at any time between 1873 and 1889. Black points show the corresponding prices calculated for the entire city of Chicago. Whiskers indicate 95% confidence intervals.

This figure shows the same basic patterns described in Hoyt (2000). Prices fall between 1873 and 1880, before beginning a slow recovery. Figure B1 also shows that prices in the Quasi-experimental region follow those in the city as a whole. That is, the Quasi-experimental region is a small part of a large, liquid land market. This suggests that the assignment of sewers and piped water (or not) to parcels in the Southwest Triangle should not affect prices outside of the Southwest Triangle. On the basis of this observation, we ignore the general equilibrium price effects in

Table B1: Summary Statistics 1886-1889, after piped water and sewer construction

	(1) SW Δ = 1	(2) SW Δ = 0	(3) <i>t</i> -test
Share Sewered	1.00 (0.00)	1.00 (0.00)	.
Log Price	8.38 (0.94)	8.57 (0.74)	1.37
Log Distance to CBD	9.06 (0.36)	8.97 (0.48)	-1.12
Log Area	-8.86 (0.67)	-8.95 (0.51)	-0.99
Share Corner	0.10 (0.30)	0.11 (0.32)	0.33
Distance to Horsecar	0.13 (0.10)	0.07 (0.06)	-4.86
Distance to Major Street	0.10 (0.08)	0.08 (0.07)	-1.05
Year	1887.19 (0.95)	1887.34 (1.08)	0.85
Observations	63	80	

Note: Means and standard deviations of parcel characteristics. Column 1 reports on parcels in the Quasi-experimental sample (within 2000' of Congress Street west of Halsted) that are in the Southwest Triangle (south of Congress Street). Column 2 presents corresponding values for parcels that are not in the Southwest Triangle (i.e., north of Congress Street). Column 3 reports the *t*-statistic for the difference between the first two columns. In all columns, we restrict attention to parcels transacted during 1886-1889.

our analysis of the Quasi-experimental sample.

Table B2: Reduced form regressions after completion of piped water and sewer network.

	(1)	(2)	(3)	(4)	(5)
Reduced Form					
SW Δ = 0	.174 (.119)	.115 (.125)	-.124 (.254)	.146 (.1)	.115 (.095)
Miles to Boundary			0.630 (.621)		
R^2	0.364	0.454	0.458	0.330	0.433
Year FE & ln(Area)	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y
Horsecar and Major Street		Y	Y		Y
Sample	Q.E.	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Observations	143	143	143	213	213

Note: All results based on transactions during 1886-9. Columns 1-3 rely on the Quasi-experimental area, 4 and 5 on the Extended-quasi-experimental area.

Regressions are reduced form regressions of log transaction price on the instrument and, in column (3), distance to the Congress Street. Bottom panel of the table indicates control variables. Unlike the 1874-80 period, the entire Southwest Triangle has piped water and sewer access by 1886-9 and the price difference across the Congress Street boundary is small economically and statistically. Robust standard errors in parentheses. *, **, *** indicate 10%, 5%, 1% significance.

Table B3: Main 2SLS Results, Additional Controls

	(1)	(2)	(3)	(4)
A: OLS.				
Sewer=1	.388*** (.083)	.083 (.091)	.242*** (.101)	.433*** (.084)
R^2	0.448	0.513	0.422	0.413
B. Reduced Form				
$SW\Delta = 0$.608*** (.068)	.355*** (.097)	.824*** (.112)	.648*** (.071)
R^2	0.531	0.532	0.510	0.506
C. 1st Stage				
$SW\Delta = 0$.429*** (.041)	.424*** (.061)	.330*** (.048)	.438*** (.039)
R^2	0.472	0.463	0.461	0.465
F-stat	107.904	48.113	48.075	124.096
D. IV.				
Sewer=1	1.417*** (.205)	.838*** (.262)	2.496*** (.488)	1.479*** (.209)
Year FE & $\ln(\text{Area})$	Y	Y	Y	Y
$\ln(\text{mi. CBD})$.	Y	Y	Y
Cubic mi. to CBD	Y	.	.	.
Corner	Y	Y	Y	Y
ED % Foreign Born and Mean SES	.	Y	.	.
Miles to River	.	.	Y	.
Near River Indicator	.	.	.	Y
Sample	Q.E.	Q.E.	Q.E.	Q.E.
Observations	351	351	351	351

Table B4: (a) LIV Regression Results

	(1)	(2)	(3)	(4)				
	1 st Stage	2 nd Stage						
Z	3.95*** (.49)	5.41*** (.73)	2.76*** (.36)	3.32*** (.44)				
ln(Area)	-.08 (.29)	.72*** (.22)	-.02 (.35)	.66*** (.21)	-.34 (.23)	.72*** (.20)	-.29 (.26)	.62*** (.20)
1(Year = 1875)	.56 (.64)	.45** (.2)	.55 (.71)	.35* (.19)	.21 (.54)	.38* (.23)	.23 (.55)	.34 (.23)
1(Year = 1876)	.95 (.66)	.39 (.26)	.86 (.74)	.30 (.28)	.42 (.54)	.35 (.32)	.38 (.55)	.23 (.31)
1(Year = 1877)	1.41* (.72)	.52 (.36)	1.55** (.78)	.51 (.38)	1.00* (.57)	.42 (.37)	.94 (.58)	.39 (.39)
1(Year = 1877)	3.06*** (.83)	.32 (.43)	3.24*** (.86)	.34 (.41)	1.58*** (.66)	.29 (.5)	1.75*** (.67)	.26 (.47)
1(Year = 1879)	2.45*** (.73)	-.08 (.49)	2.63*** (.77)	0 (.51)	1.15** (.56)	-.38 (.58)	1.27** (.58)	-.44 (.59)
1(Year = 1880)	3.65*** (.71)	-.63 (.63)	3.89*** (.75)	-.83 (.66)	2.72*** (.53)	-1.54 (.94)	2.67*** (.54)	-1.36* (.81)
ln(mi. CBD)	-5.83*** (.91)	.31 (.64)	-8.19*** (.129)	.24 (.62)	-5.41*** (.71)	.85 (.79)	-5.75*** (.75)	.49 (.78)
ln(to Horsecar)		9.39*** (2.63)		.15 (.88)			3.63*** (1.36)	1.53* (.8)
ln(to Major Street)			-3.09 (2.56)	1.11 (1.08)			-1.18 (2.03)	2.69** (1.34)
1(Corner)				-.54 (.7)	.43 (.29)		-.03 (.50)	.39 (.35)
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y	Y	Y
Horsecar and Major Street, Corner			Y	Y		Y	Y	Y
Sample	Q.E.	Q.E.	Q.E.	Q.E.	E.Q.E.	E.Q.E.	E.Q.E.	E.Q.E.
Observations	351	351	351	351	533	533	533	533

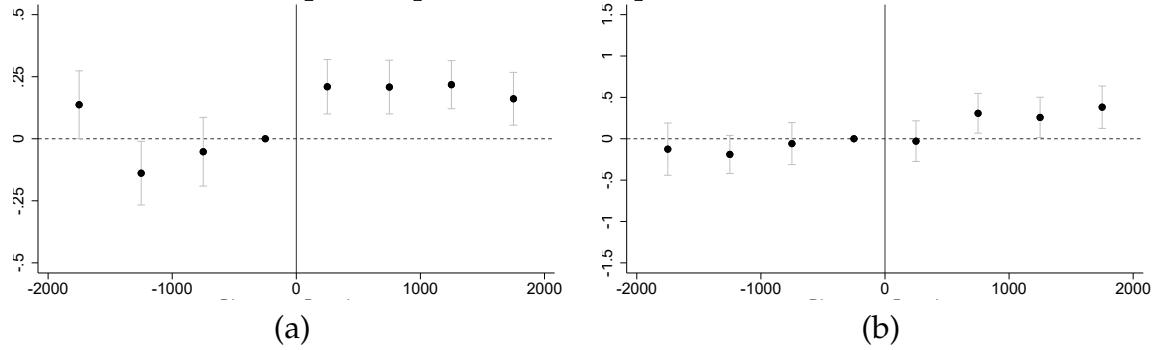
Note: Table continued next page

Table B4: (b) LIV Regression Results

	(1) 1 st Stage	2 nd Stage	(2) 1 st Stage	2 nd Stage	1 st Stage	2 nd Stage	(3) 1 st Stage	2 nd Stage	(4) 1 st Stage	2 nd Stage
\hat{p}		.74 (2.84)		.86 (2.83)		2.39 (2.91)			5.08* (3.07)	
\hat{p}^2		-3.56 (4.83)		-3.38 (4.37)		-.94 (4.51)			-7.23 (4.57)	
\hat{p}^3		3.81 (3.03)		3.65 (2.73)		1.05 (2.72)			5.14* (2.67)	
$\hat{p}\ln(\text{Area})$		-.10 (.23)		-.08 (.23)		.09 (.23)			.02 (.20)	
$\hat{p}\mathbb{1}(\text{Year} = 1875)$		-.97*** (.33)		-.80*** (.30)		-.66* (.37)			-.67** (.34)	
$\hat{p}\mathbb{1}(\text{Year} = 1876)$		-.64* (.39)		-.43 (.38)		-.35 (.46)			-.28 (.42)	
$\hat{p}\mathbb{1}(\text{Year} = 1877)$		-1.40*** (.54)		-1.30*** (.51)		-.93* (.50)			-.94* (.52)	
$\hat{p}\mathbb{1}(\text{Year} = 1878)$		-1.24** (.54)		-.98** (.49)		-1.04* (.60)			-1.00* (.55)	
$\hat{p}\mathbb{1}(\text{Year} = 1879)$		-1.09* (.59)		-1.00* (.59)		-.36 (.67)			-.32 (.67)	
$\hat{p}\mathbb{1}(\text{Year} = 1880)$		-.51 (.72)		-.10 (.70)		.78 (1.01)			.62 (.87)	
$\hat{p}\ln(\text{mi. CBD})$		-.11 (.68)		-.06 (.67)		-.57 (.85)			-.11 (.82)	
$\hat{p}\ln(\text{to Horsecar})$.19 (1.42)					-3.34*** (.86)	
$\hat{p}\ln(\text{to Major Street})$				-2.34 (1.46)					-4.47*** (1.59)	
$\hat{p}\mathbb{1}(\text{Corner})$				-.03 (.37)					.02 (.40)	
Year FE & $\ln(\text{Area})$	Y	Y	Y	Y	Y	Y	Y	Y	Y	
$\ln(\text{mi. CBD})$	Y	Y	Y	Y	Y	Y	Y	Y	Y	
Horsecar, Major Street, Corner			Y	Y			Y	Y		
Sample		Q.E.		Q.E.		E.Q.E.		E.Q.E.		
Observations		351		351		533		533		

Note: Estimates of the LIV model of equation (3). Column headings indicate Logit first stage coefficients and corresponding second stages, so that the table reports two columns per specification. Specifications and samples match those reported in the same columns of table 3. Bottom panel indicates controls for the regression above. Bootstrapped standard errors in parentheses. *, **, *** indicate 10%, 5%, 1% significance.

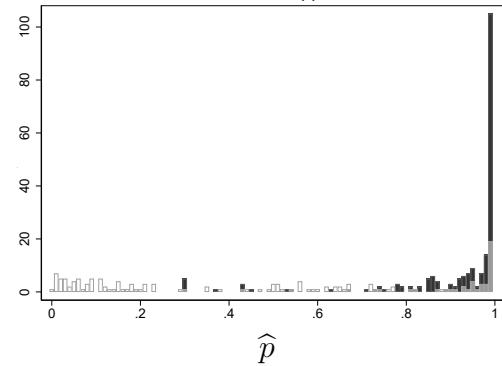
Figure B2: Sewer incidence and land price by distance to boundary, 1874-80, for the Extended-quasi-experimental sample



Note: (a) Share of parcels sewered 1874-80 by 500' bins of distance to SW \triangle boundary, $x < 0$ is "inside". $x \in [-500, 0]$ is y intercept. Conditional on year, $\ln(\text{area})$, $\ln(\text{mi. to CBD})$. (b) Same as left panel but y -axis is $\ln(\text{Price})$.

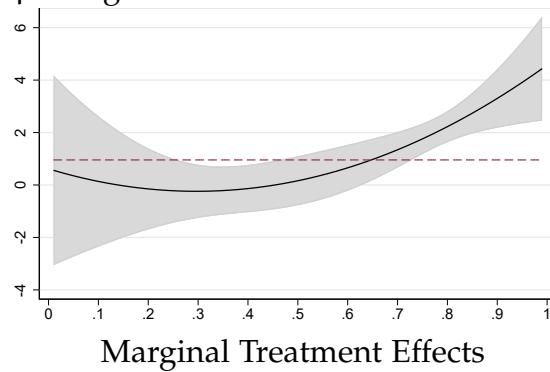
Table B3 shows main results using alternative specifications. We prefer to control for distance using $\ln(\text{mi. CBD})$ because it provides a more plausible basis for extrapolation to the Relevant area. Enumeration district-level population controls are not measured at a sufficiently fine level. There are only five EDs contained entirely within either the north or south sides of Congress street inside the experimental area, so these coarse controls rely heavily on areal interpolation. We also choose not to control for distance to river in our preferred specification, as it is almost entirely colinear with distance to CBD in the experimental region, and there are exceptionally few parcels located in close proximity to the river.

Figure B3: Density of treatment by \hat{p}



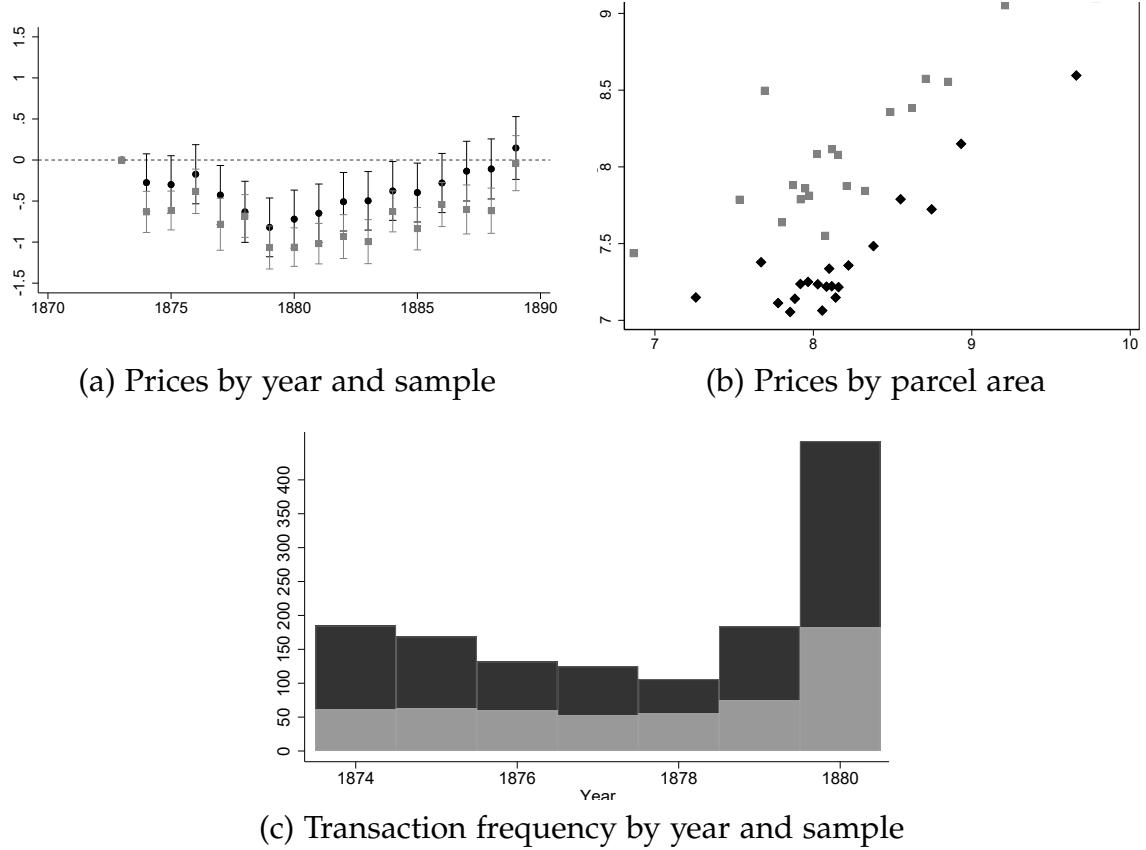
Note: Density of treated and untreated parcels by propensity score. The propensity score distribution is skewed toward one, but conditional on a mass of propensity scores, treated and untreated parcels both occur. Based on column 2 of table 3.

Figure B4: Marginal Treatment Effect as a function of \tilde{U}_D



Note: Expected MTE as a function of \tilde{U}_D . Dashed line shows ATE for this sample/specification and sample average X's. Based on column 2 of Table 3.

Figure B5: Comparison of Quasi-experimental and Relevant samples.



Note: (a) Mean log transaction price by year in the main Quasi-experimental (gray) sample and the Relevant (black) sample. Conditional on: $\ln(\text{Area})$, $\ln(\text{miles to CBD})$, year. Means and variances of Y in the two samples are similar conditional on year. (b) Mean log transaction price by parcel area. (c) Transactions by year and sample. The Relevant sample is larger, but the distribution of transactions across years is similar for the Quasi-experimental and Relevant samples. The spike in 1880 reflects a change in sampling effort, not in transaction volume.

Appendix C Derivation of equation (8)

We maintain the MTE model with semiparametric potential outcome equations introduced in the main text; see (1) in the main text. We also maintain the key restriction of practical exogeneity; see (2) in the main text. With propensity score $p = F(x,z) = P(D = 1|X = x, Z = z)$ introduced in the main text and the normalized unobserved heterogeneity in the selection process, $\tilde{U}_D \sim Unif[0,1]$, the selection equation can be represented as

$$D = 1\{\tilde{U}_D \leq F(X,Z)\}. \quad (\text{Appendix C.1})$$

Under the cubic polynomial specification of the control function $K(p)$ in (3), MTE at each conditioning covariate value X and $\tilde{U}_D \in [0,1]$ is given as in (4), and averaging (X, \tilde{U}_D) for the population of the Quasi-experimental sample leads to ATE in the Quasi-experimental sample (5).

Our interest is to obtain an estimate for ATE for the population of the Relevant sample P^* as denoted by ATE^* in the main text. We assume that a unit in the Relevant sample admits the same structural equations (6) with the same parameter values as a unit in the Quasi-experimental sample. Importantly, even though we assume that a binary cost shifter Z^* is present and measures the cost of access to sewage in the same scale for each unit in the Relevant as in the Quasi-experimental sample, Z^* is not observed for any unit of the Relevant sample. In addition, unlike in the Quasi-experimental sample, Z^* need not be randomly assigned and the analogue of the instrument exogeneity assumption $Z^* \perp (U_1^*, U_0^*, U_D^*)$ may fail in P^* .

The following assumption describes what is necessary, and what is not, for feasible extrapolation from P to P^* .

Assumption EX: (The relationship between P and P^*)

1. The equations of potential outcomes and selection given in (1) are identical between the Quasi-experimental and Relevant samples (other than that Z^* is not observed in P^*). Furthermore, the distributions of (U_1, U_0, U_D) and (U_1^*, U_0^*, U_D^*) are common.

2. The joint distribution of observable covariates X and cost shifter (instrument) Z in the Quasi-experimental sample and the joint distribution of X^* and Z^* in the Relevant sample can be different.

Under (EX1), we can normalize U_D^* of (6) to define the uniform random variable $\tilde{U}_D^* = F_{U_D^*}(U_D^*)$ such that for \tilde{U}_D defined in (Appendix C.1), $\tilde{U}_D^* = \tilde{U}_D$ is equivalent to $U_D^* = U_D$. In other words, a unit in the Relevant sample and a unit in the Quasi-experimental sample that share the values of \tilde{U}_D^* and \tilde{U}_D have identical unobservables in the selection equation. Assumption EX1 also implies that the control function term $K(\cdot)$ in the LIV regression (3) is common between the two samples, because the control function term is determined only by the distribution of $(U_1, U_0) | U_D$ and this does not vary between the two samples. As a result, for MTE in the Relevant sample $MTE^*(X^*, \tilde{U}_D^*)$, $MTE(X, \tilde{U}_D) = MTE^*(X, \tilde{U}_D^*)$ holds whenever $X = X^*$ and $\tilde{U}_D = \tilde{U}_D^*$ hold. We hence obtain

$$MTE^*(X^*, \tilde{U}_D^*) = (X^*)'(\delta_1 - \delta_0) + \gamma_1 + 2\gamma_2 \tilde{U}_D^* + 3\gamma_3 \tilde{U}_D^{*2}. \quad (\text{Appendix C.2})$$

Taking the expectation with respect to X^* and $\tilde{U}_D^* \sim Unif[0,1]$, we obtain equation of (8) in the main text, where $E(X^*)$ is directly identified by the data of the Relevant sample. Note that this argument does not require Z^* to be independent of the unobservables (U_1^*, U_0^*, U_D^*) .