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Working Paper 29718
http://www.nber.org/papers/w29718

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2022, Revised July 2022

We are grateful to Werner Troesken, who sparked the data collection underlying this paper before he passed away in 2018. We also gratefully acknowledge helpful comments and discussions with seminar participants at Brown, Buffalo, Case Western Reserve, Oxford, Syracuse, and Yale, and with Caitlin Brett, Peter Hull and Michael Hahneman. We thank Thomas Carr for excellent research assistance. Any errors are our responsibility alone. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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JEL No. L97,N11,O18,R3

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 grade, and such variations in grade delay 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 two. This exceeds costs by almost a factor of 40.

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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 causal effects, we exploit the fact that the construction cost for sewers varies sensitively with variations in grade that are otherwise imperceptible and, therefore, affect land values only through their effect on the timing of piped water and sewer access. We propose a new estimator to extrapolate treatment effects from the small region where we can defend our natural experiment to a region that is more relevant for cost-benefit analysis. In our most conservative estimate, we find that access to piped water and sewers more than doubles the value of residential land in Chicago. Aggregating this increase over affected parcels and comparing to construction costs, we find that the benefits of piped water and sewer infrastructure exceed costs by almost a factor of 40.

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 estimates 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 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

[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.

food purity laws, improvements in water and sewer access and quality, 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. Uniquely in the literature, our primary outcome variable is land price rather than a measure of health or mortality. Since land cannot move in response to location specific policies, this permits the use of granular spatial data without raising issues related to sorting. In addition, changes in land prices provide a natural basis for cost benefit analysis without the intermediate and challenging appeal to estimates of the value of a statistical life.

We pioneer a new identification strategy for estimating the causal effects of sewers. 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 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 lack of a compelling identification strategy. We hope that 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 develop a method for extrapolating treatment effects from a quasi-experimental region to a more economically relevant region. 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 quasi-random variation in treatment assignment is available. The reliance on small, 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 is well studied. Using samples 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) examine the effect of measures to improve water quality in the Boston Harbor watershed on infant and child mortality between 1880 and 1915. Beach et al. (2016), Ogasawara and Matsushita (2018), and Knutsson (2020) also study the effect of improvements in water quality on measures of mortality. Finally, Cain and Rotella (2001) consider the effect of expenditures on water works on mortality.

While the details of the particular studies differ, the results in Alsan and Goldin (2019) are fairly typical. They examine the effect on infant mortality rates of a series of interventions to protect drinking water quality in the Boston Harbor watershed between 1880 and 1915. They estimate that these interventions caused a decline in infant mortality rates of 0.21 log points, 19%, from an 1880 level of 163/1000.

Expansions of sewer access during this period are less well studied. Anderson et al. (2018) finds no effect of sewage treatment on various measures of mortality. However, 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. Beach (2021) argues that the various innovations in municipal sanitation and water supply were responsible for the elimination of typhoid in American cities between 1900 and 1930. Finally, Cain and Rotella (2001) find that a 1% increase in sewer expenditure is associated with a 2% decrease in waterborne disease death rate.[2]

The literature also investigates the effects of municipal water quality

[2] Cutler and Miller (2005) control for the presence of a sewage treatment plant and for chlorination of sewage. However, these facilities are only present in three and one of their sample cities, respectively.

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 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.

Finally, Gamper-Rabindran et al. (2010) investigate the relationship between increased access to piped water and sewers in Brazil between 1970 and 2000. During this period, the share of 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.[3] 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 literature focuses on the effects of water treatment. Only Kesztenbaum and Rosenthal (2017), Cain and Rotella (2001) and Anderson et al. (2018) explicitly analyze sewer provision, and the expansion of piped water access is still less studied. Among papers studying the modern developing world, only Gamper-Rabindran et al. (2010) explicitly studies expansions in water and sewer availability.

Second, our analysis of the relationship between public health infrastructure and land rent appears to be unique. 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

[3]The 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.

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 to the people to whom the service is made available. 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 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. The results in Anderson et al. (2018) justify this suspicion.[4] By construction, our cross-sectional research design is not subject to this problem.

Fourth, the disease environment in modern developing world cities is clearly different from late 19th century Chicago (see Henderson and Turner (2020) and Haines (2001)). However, the available evidence suggests that rates of infant mortality and the effects of water treatment are similar.[5] While the comparison is imprecise, raw infant mortality rates and the effects of improved water quality are large in both turn of the century US and modern day Brazil and Mexico. This suggests that, absent studies based on modern data, our estimates of the value of piped water and sewer in late 19th century Chicago can serve as a starting point for evaluating policies in modern day developing countries.

[4]See (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.

[5]Between 1900 and 1940, the infant mortality rate in major US cities declined from 38/1000 to 8/1000 and between 4% and 10% of this decline was due to water treatment and filtration Anderson et al. (2018). Similarly, Alsan and Goldin (2019) find an infant mortality rate of 163/1000 for Boston in 1880, and that interventions to protect drinking water quality caused a decline of about 19%. Gamper-Rabindran et al. (2010) find an infant mortality 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.

In addition to our primary object of estimating the effects of piped water and sewer infrastructure on land prices, we develop a new method for extrapolating estimates based on a quasi-experiment to a more economically relevant sample for which quasi-random assignment of the treatment is not available. Our approach to this problem 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. Other methods for extrapolating causal effects to populations other than the sampled population include Hotz et al. (2005), Angrist and Fernández-Val (2013), Andrews and Oster (2019), and Dehejia et al. (2021). There is also a small literature (Angrist and Rokkanen (2015), Rokkanen (2015), and Cattaneo et al. (2020)) considering the related question of extrapolating treatment effects estimated using an RDD design to points away from the discontinuity. The possibility of extrapolation from quasi-experimental samples to more economically relevant samples based on marginal treatment effect estimates has not been previously 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 also require a description of the attributes of transacted parcels. To complete our cost benefit analysis, we must also measure construction costs. We here describe the data we use for each purpose.

Between 1873 and 1889, the Chicago Tribune reports every land parcel transaction filed with the municipal title office on the previous day. We collect all transactions listed in the Sunday edition, which is usually the day of the week with the largest number of listings. This results in about 700 observations per

[6] 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 of 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 extrapolation analysis differs from these works in the following aspects. First, we consider the problem of extrapolating marginal treatment effects to units not in the estimation sample. Second, we do not observe the assignment of instruments (cost-shifter for sewage access) in the Relevant sample and the instrument and unobserved heterogeneities can be correlated in the Relevant sample.

Figure 1: 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 126 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. Kufel et al.)..................... 750

West Madison st, 428 ft w of Staunton, s f, undivided ⅗ 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 reported on Saturday between 1873 and 1889. Note that each record reports the nearest intersection, price, and area. Most records also report if the parcel is "improved" or "corner."*

year in the 1870s and 1000 per year in the 1880s.[7]

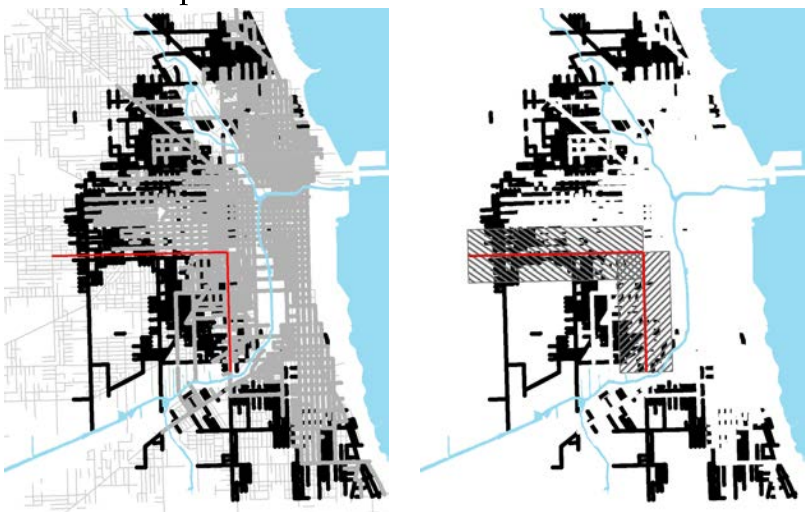
The Tribune consistently reports price, parcel dimensions, either a street address or the nearest intersection, and whether the parcel is "improved." Figure 1 illustrates a sample of transaction listings. Because the Tribune separately indicates transactions with a "premises", i.e., a house, we are confident that our data describe land transactions only. The newspaper does not define improved, but the term typically indicates that the parcel has some improvement that makes it suitable for residential use. It is clear from the data that improved in the transaction record is not equivalent to water and sewers because we observe improved parcels both with and without access.[8]

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. When we cannot match a reported intersection to the contemporary street grid, we attempt to

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

[8]Because the Annual Reports of the Chicago Department of Public works routinely refer to paved streets as "improved", we believe that improved indicates that the parcel fronts a paved road. Most streets were paved after a sewer line was installed. Thus, the including improved as a control allows for a separate effect of fronting a paved road.

Figure 2: Extent of piped water and sewer network, Southwest Triangle, and Quasi-experimental samples



(a) (b)

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.*

match it to an intersection in the circa 1880 street map created by Logan et al. (2011). This process allows us to geocode about 77% of transactions by assigning them the coordinate of their nearest intersection. Appendix A provides a more complete description of how we collected and geocoded these transactions.

We rely on historical GIS maps describing the block-by-block expansion of the sewer network from 1830-1930 Fogel et al. (2014). These 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 indicated that this rule resulted in an 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.[9] False negatives are harder to imagine.

Figure 2a illustrates the expansion of piped water and sewer access during the post-Civil War period. In this figure, the thick, light gray lines indicate water and sewer lines pre-dating our 1874-1880 study period. Unsurprisingly, 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. Also unsurprisingly, 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 also peripheral to the 1880 network and often extend beyond the boundary of the figure.

We collect demographic data from the 1880 census. Our identification strategy relies on highly disaggregated spatial and temporal data describing sewer and water access. In contrast, the historical census is available decennially at level of the enumeration district, the units used by the Census Bureau to organize in-person enumeration in the late 19th century. In consequence, the 1880 census is too coarse to inform our estimations and we largely restrict our 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 lakeshore[10] 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.[11]

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 vary year to year but are increasing in the early 1870s and decline

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

[10]The hydro file was obtained from Cook County Government Open Data, see https://datacatalog.cookcountyil.gov/GIS-Maps/Historical-ccgisdata-Lakes-and-Rivers-2015/kpef-5dtn.

[11]The 1880 horse-drawn streetcar routes were digitized using a map from the Illinois State Grain Inspection Department. The street network in 1880 was digitized by John Logan, see https://s4.ad.brown.edu/Projects/UTP2/39cities.htm

during the recession of the late 1870s. Waterworks, including pumping stations, were typically the largest category of expenditure, with sewer construction second. Sewer maintenance costs, including manual flushing (discussed below), were stable and relatively small throughout the period. 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.[12]

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 per laborer in the city of Chicago was as high as $650 in 1880 dollars or $17,000 in 2021 dollars.[13]

Hoyt (2000) describes Chicago's land market between 1830 and 1930. He reports rapid growth in the value of land in the early 1870s. Prices declined from their peak after the panic in 1873 and the value of the land within city limits declined 50 percent by 1877. Economic conditions improved in the early 1880s and, by 1882, Chicago's land values had recovered to their 1873 peak (Hoyt, 2000, p. 140). In short, our 1874-1880 study period spans a major recession (1873-1877) and recovery (1878-1882). Population growth was robust throughout the whole period from 1870-1890.

Chicago's infant mortality rate in the 1870s was 74 per 1000. This is similar to contemporaneous rates reported in other US cities, e.g., Alsan and Goldin (2019)

[12] Special 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).

[13] From 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). These values were inflated to 2021 price levels using CPI estimates from Sahr (2009) for 1880-1912 and the BLS CPI series for 1913-.

or Haines (2001), and to current rates in poor developing countries like Sierra Leone or Somalia.[14] Most deaths were caused by infectious disease and occurred predominantly among the young (Ferrie and Troesken, 2008).

In the 1850s, the quality of Chicago's drinking water was notably poor. 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. Specifically, water 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). Importantly, our study period (1874-1880) is located entirely within the Two Mile crib period.

The condition of the City's poorly drained streets was grim. Ashbury's well-known Chicago history, (Asbury, 1940, p.23) reports that the "gutters [run] with filth at which the very swine turn up their noses in supreme disgust...". 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, prompting the city to begin planning the improvements to its water and sewer infrastructure that we discuss below.

Typical gravity fed sanitary sewers require a grade of about 1:200 to prevent suspended solids from settling and blocking the pipe. The precise required grade is sensitive to the details of the system; the rate of flow, pipe size and cross-sectional shape, and the smoothness of interior walls. For details see, e.g., Mara (1996). Importantly, variation in grade that is critical for sewer construction is practically beyond human perception. Aldous (1999) reports that people begin to perceive a playing field as sloped at a grade of about 1:70. Variation in grade is less relevant to piped water networks.

Our research design will be organized around transactions that occurred in the area around Congress Street, currently the Eisenhower Expressway, and extending West about three miles from Halsted Street. The present day corner of

[14]Estimate for Chicago taken from Ferrie and Troesken (2008) and for Africa from the UN Interagency Group for Child Mortality Estimation (UNICEF, WHO, World Bank, UN DESA Population Division) at childmortality.org.

Halsted and Congress Streets is about two miles from and twelve feet above the level of Lake Michigan, a grade of about 1:880. This is much too flat for conventional gravity-fed sanitary sewers. Indeed, such grades are so flat that water generally does not drain away. Rainfall either evaporates or is absorbed into the ground. Chicago's unusually flat terrain contributes to the benefits of sewers as well as to the difficulty of constructing them.

Chicago hired noted engineer Ellis Chesbrough to design a sewer system capable of operating in Chicago's flat topography, and substantially followed the proposal he submitted in 1855. Chesbrough proposed what is now known as a "combined" sewer system to manage household sewerage and street runoff. Chesbrough's plan called for continuous mechanical flushing, although the city ultimately adopted a system under which sewer mains were manually flushed using water delivered by horse-drawn carts.[15] This systematic manual flushing allowed sewer mains to operate at a grade of 1:2500, far shallower than conventional sewers.

To function, even Chesbrough's sewers require large enough flows of water that they are only practical if piped water is available. For this reason, sewers could not be installed before piped water. In fact, 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.

Because water and sewer service are almost always provided together, we estimate their joint value. With this said, the discussion above points out that water and sewer service were highly complementary, so that providing one without the other would probably have had much less value.

Construction of Chesbrough's sewers required a massive program of regrading to raise streets to the required grades. The process for constructing sewers involved first laying sewer and water pipes at the required grade, whether above or below ground, and then filling in the space around them with earth as required. The newly raised streets were then sometimes paved over to conclude the process. Because street paving could independently contribute to

[15]As late as 1940, horse-drawn tanks were still used to manually flush certain sewer lines in Chicago (Cain, 1978, p. 32).

property values, this raises the possibility that our estimates reflect the joint value of water, sewer and street paving. We address this possibility by controlling for improved status in our estimations.

Buildings, 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.

Chicago issued its original plan for sewerage in 1855. This document describes the street grades in each region of the city required to accommodate the proposed sewer system (Plan of Sewerage, Chicago Board of Sewerage Commissioners, 1855). Subsequent ordinances were issued at regular intervals as the sewer system expanded beyond the streets covered in this initial report. The sewer ordinances describe the details of the regrading operation and list, block by block, the planned elevation of each street intersection relative to the level of the lake. The 1855 plan states, "It will be necessary to raise the grades of streets an average of eighteen inches per 2500 feet going West." To get a sense for the scale of this undertaking, it requires about 8300 cubic yards of fill to raise a 2,500 foot segment of a 20 foot wide street by 18 inches. At about 1.5 tons per cubic yard, this is almost 12,500 tons of fill per 2500 foot segment of road.

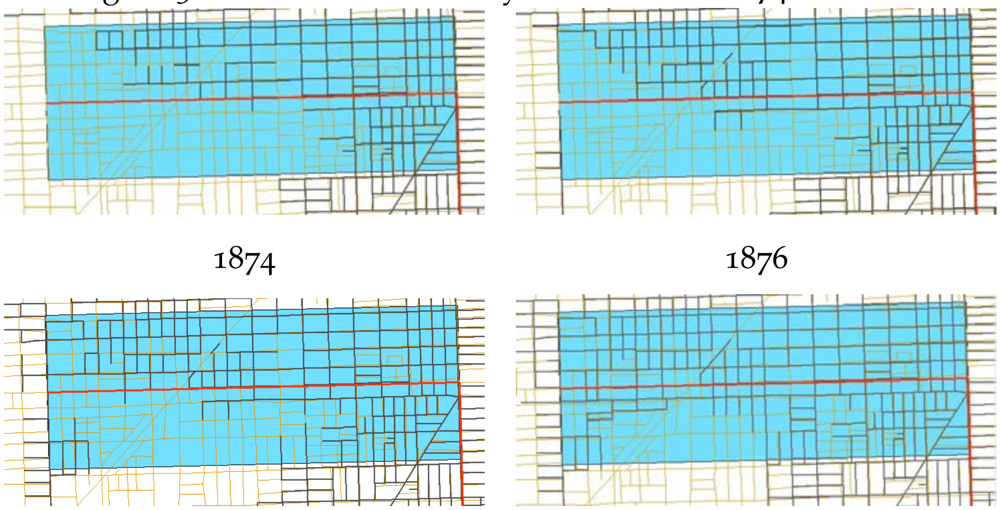
The historical record suggests that municipal authorities knew which streets had the worst drainage and were anxious to sewer them as soon as the network reached them. From the Chicago Tribune (June 25th, 1873, page 4):

> "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.

The 1855 ordinance describes a "triangle" southwest of the downtown that was at a slightly lower elevation than the rest of the city. Chesbrough wrote of this region, South of Congress Street and West of Halsted Street: "The extreme south-west part of the city [is] too low [to sewer], "as the depth of filling

Figure 3: Sewer extent in study area between 1874 and 1880



1874 1876

1878 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 14,000 feet West from Halsted Street. 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.*

required to raise streets over it would average two feet" (p. 16). Recalling that the plan calls for streets to be raised "an average of eighteen inches per 2500 feet going West", this means that the marginal 6 inches of fill required in this region was decisive. Chesbrough concludes by writing, "[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).

Figure 2 illustrates the expansion of the Chicago sewer system that occurred between 1870 and 1890. In both panels, thick light grey lines indicate the extent of the sewer network prior to 1874, thick black lines indicate the expansion that occurred between 1874 and 1880, and, thin light gray lines indicate post-1880 expansion. Red lines indicate the northern and eastern border of the Southwest Triangle, Congress, and Halsted Streets.

While the 1855 plan refers to "a triangle", it specifies only northern and eastern borders. We draw a western boundary near the limit of the 1880 sewer network, 14,000 feet west of Halsted Street, and a southern boundary at the Chicago River. We exclude parcels exactly on Congress Street, i.e., those matching to intersections within 75′ of Congress Street, for two reasons. First, the 1855 plan is ambiguous about whether or not Congress Street lies inside or outside the Southwest Triangle. Second, our data does not allow us to determine whether parcels matching to Congress Street lie north or south of the Street. Thus, we cannot determine whether parcels matching to Congress Street are inside or outside the Southwest Triangle.

The black region in Figure 2b illustrates the entire region that received sewer and water access between 1874 and 1880. This is the region for which we observe construction costs and it is the economically relevant area for the purpose of policy evaluation. We often refer to a sample drawn from this area as a "Relevant sample." Our estimation of causal effects is primarily based on the region within 2000 feet of the northern boundary of the Southwest Triangle, Congress Street. We often refer to a sample drawn from this area as a "Quasi-experimental sample". We sometimes consider the effect of sewers in the area within 2000′ of the northern *or* eastern boundary of the Southwest Triangle, Congress and Halsted Streets. We often refer to a sample drawn from this area as an "Extended-quasi-experimental sample." Figure 2b illustrates all three regions. Appendix A provides further details and illustrates the distribution of transactions across these regions.

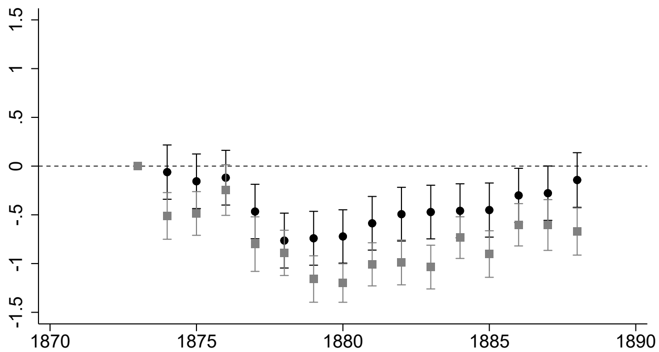
Figure 3 highlights the evolution of the sewer network in the Quasi-experimental sample. This figure makes it clear that, even 20 years after the adoption of the 1855 sewer ordinance, the construction of sewers south of Congress Street lags the northern side of the street. It is this north-south difference in sewer assignment on which we base our estimates of the causal effects of piped water and sewer access.

5 Description

Our Quasi-experimental sample is a set of 351 transactions occurring between 1874-1880 within 2000′ of Congress Street, west of Halsted. This is the sample where the case for quasi-random assignment of sewer and water access as a function of membership or exclusion from the Southwest Triangle is strongest.

Gray squares in figure 4 report mean log transaction price by year (after controlling for improved and corner status, log of parcel area, and log miles to

Figure 4: Land prices in Chicago and Quasi-experimental sample



Note: *Mean* $\ln(Price)$ *by year, relative to 1874, in Quasi-experimental sample (Gray) and all of Chicago (Black). Controls:* $\ln(miles\ to\ CBD)$*, improved, corner,* $\ln(Area)$.

the CBD), for all transactions falling in the Quasi-experimental region at any time between 1873 and 1880. Black points show the corresponding prices calculated for the entire city of Chicago. Whiskers indicate 95% confidence intervals. Unsurprisingly, annual means are more precise for the whole city than for the smaller sample drawn from the Quasi-experimental region.

This figure shows the same basic patterns described in Hoyt (2000). Prices fall between 1873 and 1880, before beginning a slow recovery. Figure 4 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 our analysis of the Quasi-experimental sample.

Table 1 presents sample means for the Quasi-experimental sample. The first column describes transactions inside the Southwest Triangle, i.e., south of Congress Street, the second, transactions outside the Triangle, i.e. north of Congress Street. As the 1855 Ordinance prescribes, and as figure 3 shows, piped water and sewer incidence is lower inside the Southwest Triangle than outside. About half the transactions in the Southwest Triangle have water and sewer access during 1874-80 and access is almost universal outside. Consistent with a large effect of water and sewer access on value, unconditional prices are 0.72 log points or 105% higher outside of the Southwest Triangle than inside. The

Table 1: Summary Statistics 1874-1880

	(1) SW$\triangle = 1$	(2) SW$\triangle = 0$	(3) t-test	(4) Relevant
Share Sewered	0.47	0.92	11.04	0.70
	(0.50)	(0.27)		(0.46)
Log Price	7.70	8.42	8.44	7.41
	(0.86)	(0.76)		(0.91)
Log Distance to CBD	9.13	9.10	-0.89	9.49
	(0.38)	(0.38)		(0.25)
Log Area	8.12	8.26	1.88	8.17
	(0.62)	(0.69)		(0.54)
Share Improved	0.11	0.23	2.99	0.15
	(0.31)	(0.42)		(0.36)
Share Corner	0.11	0.13	0.42	0.14
	(0.32)	(0.33)		(0.34)
Distance to Horsecar	884	427	-9.53	1757
	(573)	(335)		(1351)
Distance to Major Street	564	475	-2.13	441
	(427)	(363)		(372)
Year	1877.18	1877.45	1.14	1877.60
	(2.19)	(2.17)		(2.26)
Time to Sewer	3.39	2.65	-1.42	2.93
	(2.09)	(1.06)		(1.64)
N	150	211		1358

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.*

frequency of corner parcels is the same on both sides of the boundary. Improved parcels are more frequent outside the Southwest Triangle indicating the importance of this control. Parcels outside the Southwest Triangle are at most slightly larger than those inside. Parcels outside the Southwest triangle are on average one city block closer to the nearest horsecar line, though both sides of Congress Street are well integrated with the horsecar network. Major streets in

Chicago occur at one mile intervals, or every eight blocks. Parcels on either side of Congress Street are on average one to two blocks from the nearest major street. The region inside the Southwest Triangle is marginally further from the CBD than the region outside, and so transactions outside are nearer the CBD than those inside by construction.

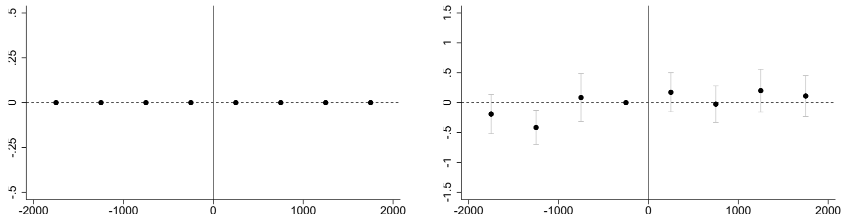
The final row of Table 1 gives mean years until water and sewer access for parcels without such access at the transaction date. This is about 3.4 years for transactions in the southwest triangle, about 2.7 for those outside, and the difference is not statistically significant at conventional thresholds. 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 of obtaining this access about three years sooner. 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 make clear that our natural experiment is operating as it should. 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. This accords precisely with the econometric model that we present later.[16]

The fourth column of table 1 highlights one of our main econometric challenges. It reports sample means from the Relevant 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 this larger policy relevant area, we should consider the possibility that treatment effects may vary systematically between the two samples.

Ideally, to check that unobservable determinants of value are the same on both sides of Congress Street, we would check land prices before piped water and sewer service was available on either side of the border. However, such data

[16] An 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, and this would complicate our econometric problem dramatically.

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



(a) (b)

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 1886-89, controlling for year indicators, $\ln(Area)$, and $\ln(mi.\ to\ CBD)$) by 500′ long bins. (b) Same as left panel but y-axis is $\ln(Price)$, controlling for the same set of covariates. 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.*

are not available.[17] Instead, we compare land prices on either side of Congress Street a short time after our study period when piped water and sewer access was universal.

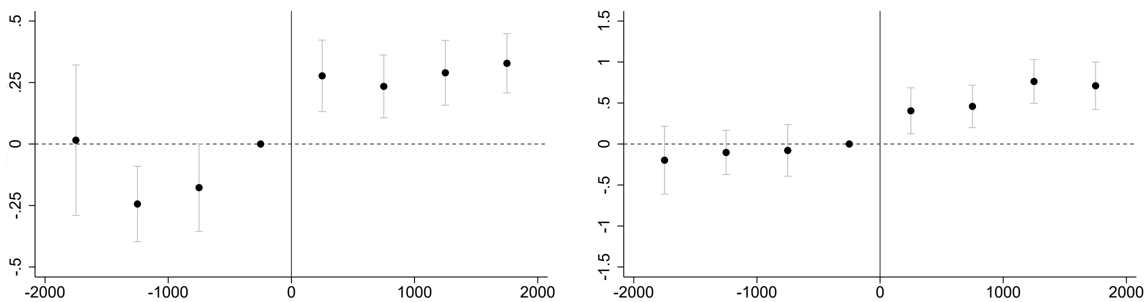
Table B1 describes transactions occurring in the Quasi-experimental region during 1886-9, six to nine years after the end of our main study window. This table replicates the first three columns of Table 1 for the later time period. This table indicates that the same basic patterns present in the data during 1874-80 largely persist into 1886-9, with two notable exceptions. Piped water and sewer access is universal during the later period, and the difference between prices inside and outside the Southwest Triangle that shows so clearly in Table 1 is no longer present in the later period.

Figure 5a illustrates piped water and sewer access in our experimental study area during 1886-9 as a function of distance to Congress Street. The x-axis of this figure is distance from Congress Street. Negative distances indicate displacement into the Southwest Triangle, and conversely for positive values. The y-axis indicates piped water and sewer share relative to the share in the bin just inside the Southwest Triangle. Sewerage is universal across the boundary by 1886.

Figure 5b is similar, but reports on transaction prices. The y-axis indicates log price relative to the bin just inside the Southwest Triangle. Mean log price in

[17]The Tribune began reporting transactions only in 1873, and 1860 census did not ask about home values or about the value of vacant land.

Figure 6: Sewer incidence and land price by distance to boundary, 1874-80



(a) (b)

Note: *Same as Figure 5, but for transactions occurring between 1874 and 1880.*

each bin is calculated controlling for year indicators, ln(area), and ln(mi. to CBD). Whiskers indicate 95% confidence intervals. Table 1 indicates a 105% difference in prices across this boundary during 1874-80. Figure 5 indicates that this difference is completely erased in less than 9 years, once sewer incidence across the border equalizes. This confirms what we see in the unconditional means presented in Table B1.

Table 1 shows that parcels in the Southwest Triangle were less valuable during our study period. There is evidence that such initial disadvantages often "lock-in" and lead to long run differences between places (e.g., Bleakley and Lin (2012) or Ambrus et al. (2020)). Poor places stay poor and rich places stay rich. 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. This may reflect the dynamic nature of the Chicago real estate market, the pervasiveness of cheap, short-lived structures, and our focus on vacant lots.

The descriptive evidence provided so far is consistent with the following narrative. Parcels in the Southwest Triangle were less likely to have access to piped water and sewers in the 1870s because of a nearly imperceptible change in elevation that affected costs of constructing gravity fed sewers. There is no a priori reason to suspect that parcels on opposite sides of Congress Street are systematically different, except that parcels inside the Southwest Triangle are slightly more remote from the CBD. 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.

Figure 6 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. The construction of this figure is the same as Figure 5, except that it is based on data from our main study period, 1874-1880. Consistent with the unconditional means presented in Table 1, we see that piped water and sewer incidence and land prices are lower in the Southwest Triangle. 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.

We note that Figure 6 suggests the possibility of implementing a fuzzy-RD design. Given our already small sample, 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. Note that our Quasi-experimental study region is narrow enough to walk across in 20 minutes and lies in an a priori homogeneous landscape. We can reasonably hope to have restricted attention to parcels with on average identical unobserved determinants of land price. To the extent our sample allows, we investigate the possibility of confounding spatial trends in unobservables in our regression analysis.

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*, $\ln($*miles to CBD*$)$, $\ln($*Parcel Area*$)$, *Corner* and *Improved* indicators, *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 indicating $Z_i = 1$ 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 sewage without directly affecting the land price, fixing controlling covariates. By defining Z so that $Z_i = 1$ outside of

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: OLS.								
Sewer=1	.413***	.390***	.400***	.328***	-.018	.194***	.276***	.239***
	(.086)	(.082)	(.084)	(.139)	(.101)	(.080)	(.081)	(.078)
R^2	0.386	0.502	0.504	0.567	0.598	0.505	0.376	0.439
B: Red. Form								
$SW\triangle = 0$	.657***	.568***	.714***	.439***	.292*	.300***	.336***	.332***
	(.072)	(.069)	(.073)	(.093)	(.151)	(.068)	(.063)	(.059)
R^2	0.486	0.568	0.591	0.606	0.602	0.527	0.397	0.462
C. 1^{st} Stage								
$SW\triangle = 0$	.432***	.443***	.451***	.323***	.194**	.443***	.259***	.259***
	(.039)	(.040)	(.043)	(.057)	(.097)	(.040)	(.031)	(.031)
R^2	0.451	0.455	0.455	0.456	0.474	0.455	0.333	0.335
F-stat	119.729	125.018	110.664	32.311	3.992	125.018	71.711	71.283
D. IV.								
Sewer=1	1.522***	1.283***	1.582***	1.360***	1.501	.678***	1.296***	1.283***
	(.220)	(.191)	(.209)	(.352)	(1.067)	(.164)	(.277)	(.266)
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y	Y	Y
Imp. & Corner		Y	Y	Y	Y	Y		Y
H.car & Maj. St.			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.*

the Southwest Triangle, we 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, ATE$^* \equiv E(Y_1^* - Y_0^*)$. This treatment 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.[18] We next implement the local IV framework proposed by Carneiro et al. (2010). This framework offers a simple way to control multi-dimensional X, and allows the explicit calculation of an average treatment effect and 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 control for year indicators and log miles to the CBD. In the second column, we add indicators for corner lot and improved status. In the third column, we add controls for distance

[18] In 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.

to horsecar and distance to a major street. In each case, transaction prices are about 0.4 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 0.6 log points. This effect is estimated precisely and varies only slightly as we add control variables in columns 2 and 3. Column 3 uses the same controls as we used in Figure 6b, 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 6a. First stage F statistics are above critical values for conventional weak instrument tests (e.g., Stock and Yogo (2002)).

Panel D presents TSLS estimates of the effect of piped water and sewer access on transaction prices. IV estimates range between about 1.3 and 1.5 log points, estimated precisely. This treatment effect is enormous: a 1.3 log point increase in parcel price is a factor of 3.7.

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 6 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 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, where we 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 D 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 nearly 0.7 log points.

Summing up, the validity of our research design rests on four pieces of evidence. First, the sensitivity of sewer construction costs to otherwise imperceptible changes in grade supports the a priori argument that the instrument affects outcomes only through its effect on the likelihood of treatment. Second, the near 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. 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 very heterogeneous, or that the distributions of treatment effects in the two samples of compliers are similar.

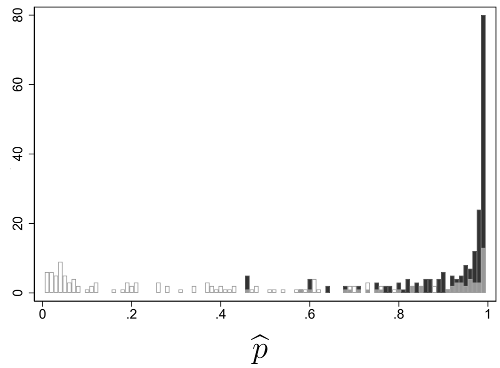
We would ultimately like to extrapolate our estimate to the Relevant sample. The Extended-quasi-experimental sample has a larger support for X and presumably, a larger support for unobservable determinants of treatment and potential outcomes. In this sense, less extrapolation is required from the Extended-quasi-experimental 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 6 for the larger sample. Neither prices nor sewer access change as sharply at the boundary of the Southwest Triangle in the larger sample.[19] 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 to the larger Relevant sample. 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, leads to extrapolations that are highly sensitive to functional form. Moreover, Ahlfeldt and McMillen (2018) find that

[19]This is because, 20 years after the 1855 ordinance, both sides of the eastern boundary of the Southwest Triangle have sewer service, see Figure 2.

Figure 7: Density of treatment by $\widehat{p}$



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.*

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)). That is, the prior evidence in support of our simple specification of the effect of distance to the CBD is compelling. In a similar spirit, we do not include measures of distance to the Chicago River in the results presented in Table 2. Since 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. The spatial resolution of these data is poor enough relative to the scale of our analysis that we are skeptical of their explanatory power. These data are available only for 1880 and cannot reflect the higher frequency changes in demographics that likely occurred. Finally, demographic variables probably depend on sewer and water access, and so the case that they are bad controls is easy to make.

With these caveats in place, Table B4 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.

Table 3: LIV Regression Test Statistics

	(1)	(2)	(3)	(4)	(5)
χ^2	220	221	237	243	245
H0: $\delta_1 - \delta_0, \gamma_1, \gamma_2, \gamma_3 = 0$	0	0	0	.005	.002
H0: $\delta_1 - \delta_0 = 0$	.108	.07	.074	.298	.205
H0: $\gamma_2, \gamma_3 = 0$	.002	0	.001	.656	.498
H0: $\delta_1 - \delta_0,\ \gamma_2, \gamma_3 = 0$	.001	.001	.001	.15	.076
ATE	1.04^{***}	$.72^{**}$	$.8^{***}$	1.31^{*}	1.31^{**}
	(.4)	(.35)	(.32)	(.69)	(.65)
ATE*	1.04^{***}	$.75^{***}$	$.89^{***}$	1.05^{**}	$.87^{**}$
	(.31)	(.27)	(.36)	(.46)	(.41)
Carr & Kitagawa	0.156	0.154	0.434	0.792	0.916
Year FE & ln(Area)	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y
Improved and Corner		Y	Y		Y
Horsecar and Major Street			Y		
Sample	Q.E.	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Observations	351	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 B3. 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 outcome 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,

$$\begin{aligned} Y_1 &= X'\delta_1 + U_1 \\ Y_0 &= X'\delta_0 + U_0 \end{aligned} \tag{1}$$

$$D = \mathbb{1}[v(X,Z) - U_D \geq 0],$$

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 $\widetilde{U}_D$ denote U_D normalized by its cdf. That is, $\widetilde{U}_D = F_{U_D}(U_D) \sim Unif(0,1)$. This transformed unobserved heterogeneity ranks units in the population P according to the unobservable cost of access to piped water and sewage, i.e., $\widetilde{U}_D$ 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 $\widetilde{U}_D \in [0,1]$ as

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

That is, MTE describes how the causal effects vary with observable characteristics, X, and with the unobservable $\widetilde{U}_D$.

To estimate MTEs, we run the local IV regression

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

The first equation is a first stage binary regression of treatment status on the instrument and controls. In our case, we specify a Logit regression with linear index in (X,Z) for the first stage. The second equation is a structural equation with a control function in $\widehat{p}$, 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 $K(\cdot)$,

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

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 $(X, \widetilde{U}_D)$ identifies the average treatment effect. That is,

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

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

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, we can interpret the conventional linear TSLS estimator for the coefficient of endogenous D as a consistent estimator for ATE.

We estimate equation (3) for specifications corresponding to those in columns 1,2,3, 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 B3. 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 1,2 and 3, we see clear evidence of treatment heterogeneity on unobservables, somewhat weaker evidence for treatment effects on observables, and clearly reject the hypothesis of effect homogeneity at all. Columns 4 and 5, we consider the larger Extended-quasi-experimental sample. Here, we reject the hypothesis of treatment effect homogeneity at the 7 or 15% level, depending on specification, but we cannot reject treatment effect homogeneity in terms of observables or unobservables alone. Inspection of appendix Table B3 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 LATEs in the Quasi-experimental sample, [0.72,1.04] versus [1.28,1.52] 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; $e^{0.72} \approx 2$, 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 7 presents a standard diagnostic for the LIV regression presented in column 2 of Tables B3 (a) and (b). This figure is a histogram showing the frequency of treated and untreated transactions as a function of $\widehat{p}$. 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 B3 (not reported) are qualitatively similar.

Figure B1 is a second standard diagnostic figure. Figure B1 plots marginal treatment effects as a function of resistance to treatment, $\widetilde{U}_D$, 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 $\widehat{p}$ of at least 0.6, the region of Figure B1 to the left of 0.6 should be understood as extrapolation from the larger values.[20]

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 assumptions on which our MTE and ATE estimates rely.[21]

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.

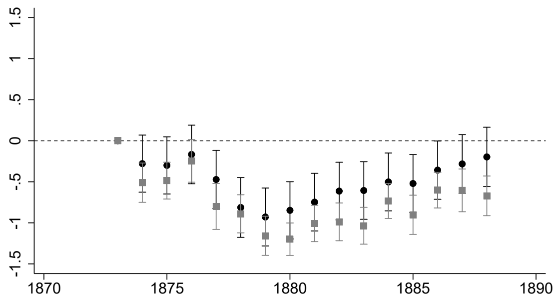
In the absence of treatment heterogeneity, extending our treatment effect estimates from the Quasi-experimental to the Relevant sample is straightforward. Estimates in Table 2 can be interpreted as Average Treatment Effects, and provided treatment effects remain constant on the larger support of the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

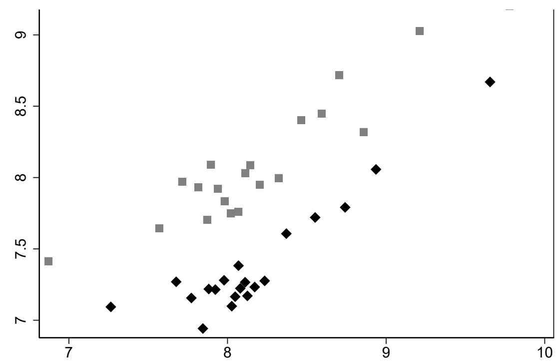
[20] Identification of MTE$(X,\widetilde{U}_D)$ without a parametric control function $K(\cdot)$ is possible for values of $\widetilde{U}_D$ supported by the distribution of propensity scores. Figure 7 indicates that observations with propensity scores near 1 largely contribute to the estimation of cubic $K(\cdot)$. MTE estimates for the range of $\widetilde{U}_D$'s without much probability mass extrapolate using the functional form of $K(\cdot)$.

[21] We 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 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.

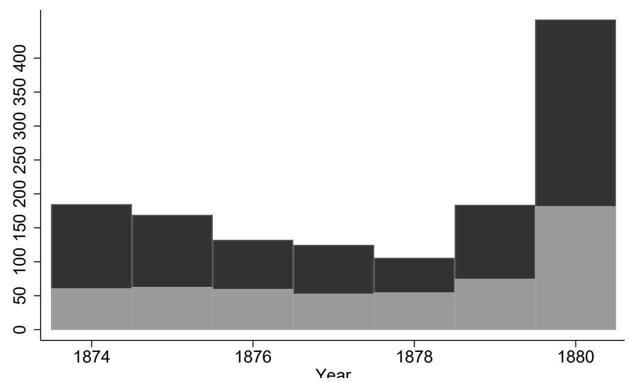
Figure 8: Comparison of Quasi-experimental and Relevant samples.



(a) Prices by year and sample



(b) Prices by parcel area



(c) Transaction frequency by year and sample

Note: *(a) Mean log transaction price by year in the main Quasi-experimental (gray) sample and the Relevant (black) sample. Conditional on:* $\ln(Area)$, $\ln(miles\ to\ CBD)$, *improved, corner. 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.*

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

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

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 joint 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,

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

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 0.75 to 1.04, 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 8 compares the Quasi-experimental and Relevant samples. Panel (a) of Figure 8 reports mean log prices by year in the Relevant and Quasi-experimental samples, conditional on: ln(Area), ln(miles to CBD), improved and corner. Panel (b) reports mean log prices by parcel area in both samples, conditional on year indicators, ln(miles to CBD), improved 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 ft^2 of land area. Our shapefiles of the sewer network then allow us to calculate that about 138m ft^2 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 ft^2, and their aggregate value was about 0.81m 1880 dollars. Dividing, the average price per ft^2 of 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 estimates, 0.75, from column 4 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 $0.45 \times (e^{ATT^*} - 1) = 0.50\$/\text{ft}^2$. That is, using our most conservative estimate, piped water and sewer access increases the value of land by about 110%. Multiplying this increase by the area affected, the total value of the piped water and sewer expansion was slightly above 69m 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 8 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 8 of table 3) we have $\text{ATE} = 1.3$. 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 for 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 69m dollar estimate of the three year flow value, we have an asset value of about 338m 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). If there is any sort of capitalization of piped water and sewer construction costs into transaction prices, then this 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 Construction costs 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 applied to treatment effects above, the discount present value of maintenance is $5.0m. Summing, we have a total expenditure water and sewer access of $8.9m.

Our estimate of the three year flow value of piped water and sewer access was about $69m, about 8 times the total cost of the water and sewer system. Our estimate of the total asset value piped water and sewer access is $338m, about 38 times as large as costs. Both of these calculations are based on our smallest estimate of average treatment effects. If we use one of our larger (but still defensible) estimates of ATE, these ratios approximately triple.

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.

Anderson et al. (2018) estimate that all water related public health interventions (water, sewer, and water treatment) were jointly responsible for a reduction in infant mortality of 0.43 log points, or about a 35% reduction. Alsan and Goldin (2019) estimate that infant mortality in Boston between 1880 and

1915 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 172 infant deaths. Costa and Kahn (2004) estimates that the value of statistical life in 1900 was about 516,000 USD2011, or 23,200 USD1880.[22] Multiplying, we have an annual value of averted infant deaths of about 4m dollars. Recall that our estimate of treatment effects is a three year effect suggests that we multiply this by three to compare it with our 69m dollar estimate for the value of piped water a sewer access. This suggests that the value of water and sewer access was about five times as large as the value of averted infant mortality. This suggests that non-health related benefits of water and sewer access are probably economically important.

We can also benchmark our estimates against the likely ability of residents to pay. Average incomes in Chicago were as high as $650 in 1880 [23] From Table 1 we have that the average log value of a property in the Quasi-experimental region north of Congress street was 8.4, or about 4,500 dollars. Almost all of these parcels had water and sewer access, so this is effectively and estimate of the price of a parcel with water and sewer access. A treatment effect of 0.75 log points means that an untreated parcel is worth about half as much as a treated one. Thus, we have that water and sewer access increases the value of a parcel by about 2,250 dollars, or around four years income for an average unskilled laborer. If a household financed its parcel with a 10 year note at 8% interest, then payments would be about 355$ 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 incremental cost of water and sewer access would have been about 18% of annual income for a parcel without water and sewer access, and about 36% with.

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

[23] From 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).

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 will inevitably need to 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 conflicting conclusions suggested by Alsan and Goldin (2019) and Anderson et al. (2018), recent research has served to increase our uncertainty about the importance 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 value of piped water and sewer access that is large, even relative to the large estimates of Cutler and Miller (2005).

This generally supports a high priority for water and sewer infrastructure. It also highlights the importance of further research on the 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 clearly different than it was in 19th century Chicago (see Henderson and Turner (2020)), so the desirability of studies conducted in developing world cities is high. An important obstacle to such research has been the absence of a credible research design for estimating causal effects. We are hopeful that some 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 piped water and sewer infrastructure. Our most conservative 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 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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Appendix A Supplementary Description of Data Construction and Summary Statistics

Transaction data

We digitize the entire set of house and land transactions reported in every Sunday *Tribune* starting in 1873 and ending in April 1889 when the *Tribune* stopped reporting transactions below $1000 in order to limit the size of the column. We restrict attention to Sundays for two reasons. The *Tribune* always reports real estate transactions on Sundays but reports them 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. We suspect this reflects a tendency for real estate agents to file the week's transactions with the courthouse on Saturday.

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 A1 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).

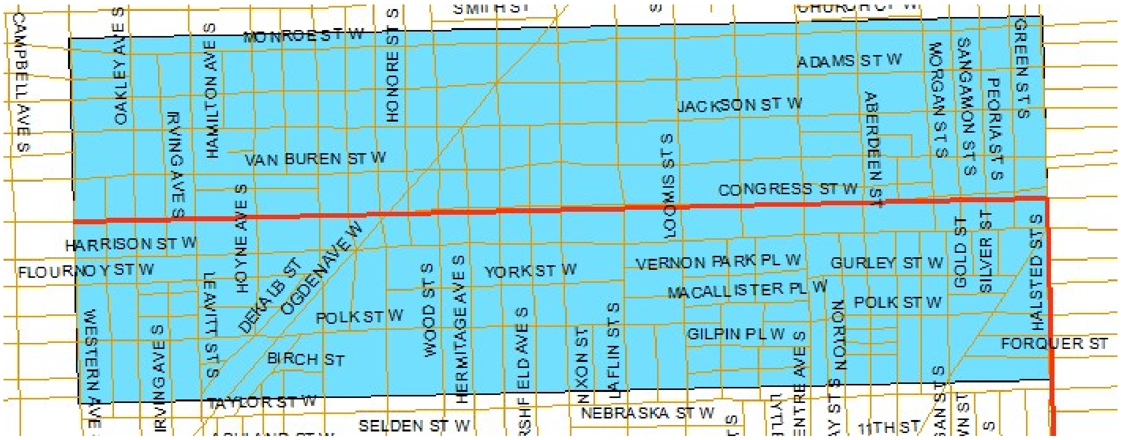
The changes in the numbering system and the difficulty of establishing a correspondence between the old and new numbering systems rules out the use of modern geocoders.

We digitize 5751 land transactions between 1874 and 1880. Of these, we successfully geocode 4421. Figure 1 illustrates the distribution of the 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.

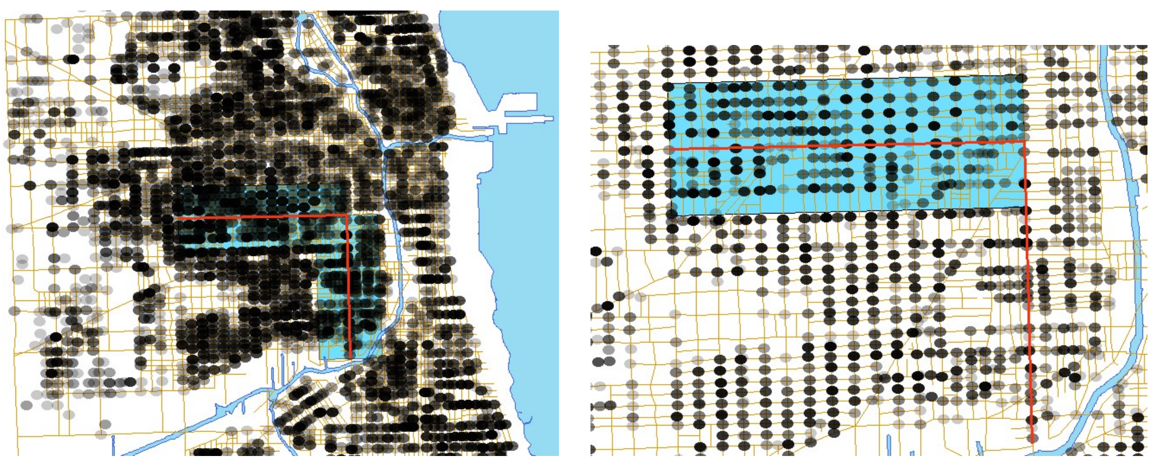
Table A1 compares transactions that we did and did not successfully geocode. Year, frontage, and depth are approximately the same across geocoded and ungeocoded parcels. Ungeocoded parcels are slightly larger. The large price difference between geocoded and ungeocoded parcels probably reflects the fact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

Figure A1: Map of Study Area with Street Names



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

Figure A2: Map of Geocoded Parcels



(a) (b)

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 interstection.*

1880 Census

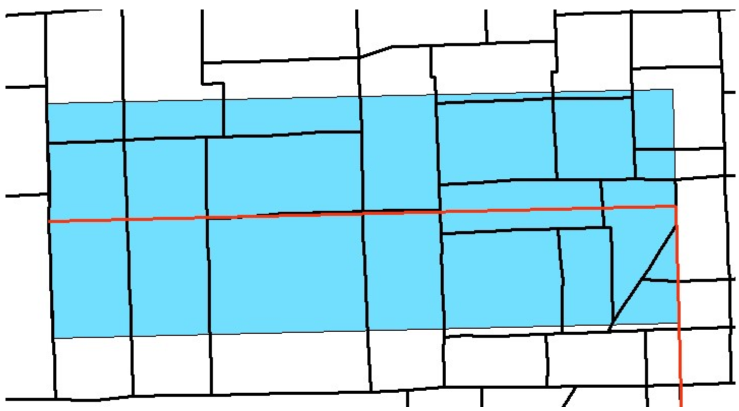
It is natural to suspect that the demographic characteristics of residents will affect and be affected by sewer assignment and land prices. To investigate this process, we incorporate the 1880 census into our data.

The 1880 census reports data aggregated to the level of the 'enumeration

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

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

Figure A3: Map of Study Area with Overlaying 1880 Enumeration Districts



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

district'. Figure A3 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 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

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.*

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.

Appendix B Supplemental Results

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

	(1) $\text{SW}\triangle = 1$	(2) $\text{SW}\triangle = 0$	(3) t-test
Share Sewered	1.00	1.00	.
	(0.00)	(0.00)	
Log Price	8.35	8.56	1.56
	(0.94)	(0.78)	
Log Distance to CBD	9.08	8.98	-1.46
	(0.35)	(0.48)	
Log Area	8.29	8.19	-0.99
	(0.67)	(0.51)	
Share Improved	0.22	0.15	-1.11
	(0.42)	(0.36)	
Share Corner	0.09	0.10	0.34
	(0.29)	(0.31)	
Distance to Horsecar	751	374	-5.50
	(527)	(314)	
Distance to Major Street	512	438	-1.11
	(431)	(390)	
Year	1887.19	1887.35	0.95
	(0.95)	(1.07)	
Observations	68	86	

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.*

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

	(1)	(2)	(3)	(4)	(5)	(6)
Reduced Form: ln(Price)						
SW $\triangle = 1$	-.174	-.233***	.165	-.183*	-.146	-.164*
	(.119)	(.096)	(.225)	(.105)	(.1)	(.09)
Miles to Boundary			1.03			
			(.539)			
R^2	0.364	0.580	0.590	0.598	0.330	0.454
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y
Improved and Corner		Y	Y	Y		Y
Horsecar and Major Street				Y		
Sample	Q.E.	Q.E.	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Observations	143	143	143	143	213	213

Note: *All results based on transactions during 1886-9. Columns 1-4 rely on the Quasi-experimental area, 5 and 6 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: (a) LIV Regression Results

	(1)		(2)		(3)		(4)		(5)	
	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
Z	3.95***		4.08***		5.55***		2.76***		2.74***	
	(.49)		(.52)		(.76)		(.36)		(.36)	
ln(Area)	-.08	.72***	.01	.63***	-.02	.63***	-.34	.72***	-.33	.67***
	(.29)	(.22)	(.33)	(.21)	(.35)	(.21)	(.23)	(.2)	(.25)	(.2)
$\mathbb{1}$(Year = 1875)	.56	.45**	.6	.42**	.57	.35*	.21	.38*	.24	.42*
	(.64)	(.2)	(.65)	(.19)	(.72)	(.19)	(.54)	(.23)	(.53)	(.22)
$\mathbb{1}$(Year = 1876)	.95	.39	.99	.37	.89	.29	.42	.35	.44	.38
	(.66)	(.26)	(.68)	(.27)	(.75)	(.28)	(.54)	(.32)	(.54)	(.31)
$\mathbb{1}$(Year = 1877)	1.41*	.52	1.59**	.58	1.73**	.47	1*	.42	.89	.38
	(.72)	(.36)	(.74)	(.39)	(.8)	(.38)	(.57)	(.37)	(.58)	(.33)
$\mathbb{1}$(Year = 1878)	3.06***	.32	3.31***	.38	3.6***	.23	1.58***	.29	1.38**	.21
	(.83)	(.43)	(.89)	(.44)	(.93)	(.38)	(.66)	(.5)	(.69)	(.43)
$\mathbb{1}$(Year = 1879)	2.45***	-.08	2.66***	.03	2.86***	-.03	1.15**	-.38	1.05*	-.27
	(.73)	(.49)	(.76)	(.44)	(.81)	(.49)	(.56)	(.58)	(.57)	(.53)
$\mathbb{1}$(Year = 1880)	3.65***	-.63	3.86***	-.26	4.09***	-.59	2.72***	-1.54	2.6***	-1.21
	(.71)	(.63)	(.75)	(.51)	(.79)	(.57)	(.53)	(.94)	(.54)	(.74)
ln(mi. CBD)	-5.83***	.31	-5.93***	.03	-8.3***	.09	-5.41***	.85	-5.38***	1.2
	(.91)	(.64)	(.93)	(.57)	(1.32)	(.58)	(.71)	(.79)	(.71)	(.76)
$\mathbb{1}$(Improved)			-.6	.43	-.7	.51			.66	.52
			(.63)	(.52)	(.64)	(.46)			(.5)	(.66)
$\mathbb{1}$(Corner)			-.52	.53*	-.6	.43			.12	.35
			(.64)	(.29)	(.7)	(.29)			(.49)	(.34)
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Improved and Corner			Y	Y	Y	Y			Y	Y
Horsecar and Major Street					Y	Y				
Sample	Q.E.	Q.E.	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	351	351	533	533	533	533

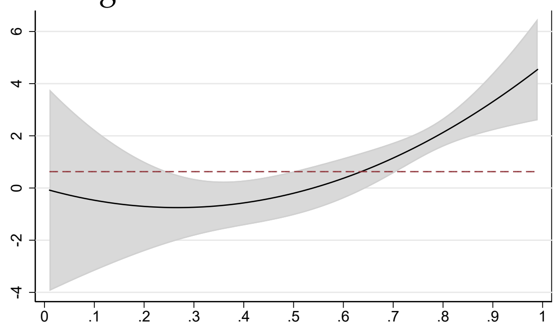
Note: *Table continued next page*

Table B3: (b) LIV Regression Results

	(1)		(2)		(3)		(4)		(5)	
	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
$\widehat{p}$		.74		1.21		1.3		2.39		3.59
		(2.84)		(2.73)		(2.8)		(2.91)		(2.92)
$\widehat{p}^2$		-3.56		-3.04		-2.74		-.94		-1.71
		(4.83)		(4.41)		(4.23)		(4.51)		(4.1)
$\widehat{p}^3$		3.81		3.65		3.26		1.05		1.59
		(3.03)		(2.77)		(2.62)		(2.72)		(2.5)
$\widehat{p}\,\ln(\text{Area})$		-.1		.02		.02		.09		.16
		(.23)		(.23)		(.22)		(.23)		(.23)
$\widehat{p}\mathbb{1}(\text{Year} = 1875)$		-.97***		-.93***		-.77***		-.66*		-.69*
		(.33)		(.32)		(.29)		(.37)		(.36)
$\widehat{p}\mathbb{1}(\text{Year} = 1876)$		-.64*		-.6		-.39		-.35		-.38
		(.39)		(.4)		(.38)		(.46)		(.46)
$\widehat{p}\mathbb{1}(\text{Year} = 1877)$		-1.4***		-1.66***		-1.4***		-.93*		-1.02**
		(.54)		(.56)		(.49)		(.5)		(.46)
$\widehat{p}\mathbb{1}(\text{Year} = 1878)$		-1.24**		-1.58***		-1.18***		-1.04*		-1.19**
		(.54)		(.55)		(.44)		(.6)		(.53)
$\widehat{p}\mathbb{1}(\text{Year} = 1879)$		-1.09*		-1.43***		-1.17**		-.36		-.64
		(.59)		(.54)		(.55)		(.67)		(.61)
$\widehat{p}\mathbb{1}(\text{Year} = 1880)$		-.51		-1.2*		-.62		.78		.21
		(.72)		(.62)		(.62)		(1.01)		(.83)
$\widehat{p}\,\ln(\text{mi. CBD})$		-.11		.14		.07		-.57		-.92
		(.68)		(.61)		(.62)		(.85)		(.81)
$\widehat{p}\mathbb{1}(\text{Improved})$				.38		.28				0
				(.56)		(.51)				(.69)
$\widehat{p}\mathbb{1}(\text{Corner})$				-.14		-.01				-.05
				(.36)		(.34)				(.39)
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Improved and Corner			Y	Y	Y	Y			Y	Y
Horsecar and Major Street					Y	Y				
Sample		Q.E.		Q.E.		Q.E.		E.Q.E.		E.Q.E.
Observations		351		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.*

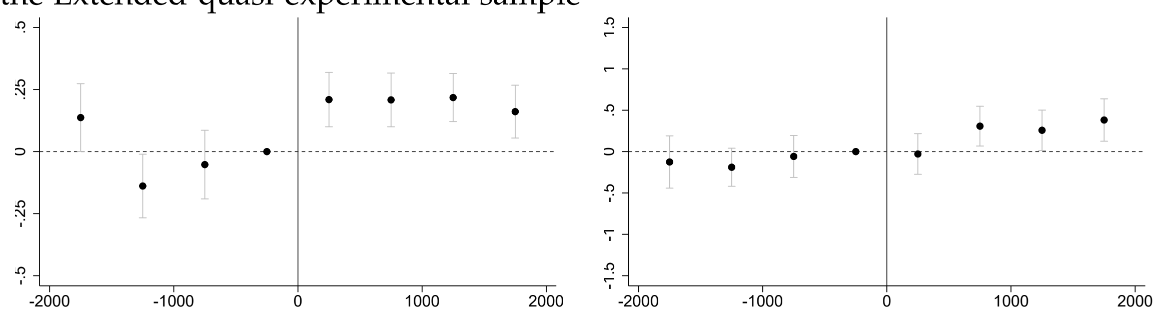
Figure B1: Marginal Treatment Effect as a function of $\widetilde{U}_D$



Marginal Treatment Effects

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

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



(a) (b)

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

Table B4 shows main results using alternative specifications. We prefer to control for distance using ln(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

Table B4: Main 2SLS Results, Additional Controls

	(1)	(2)	(3)	(4)
Panel 1: OLS.				
Sewer=1	.361***	.102	.274***	.404***
	(.082)	(.086)	(.095)	(.082)
R-squared	0.539	0.588	0.511	0.505
Panel 2: Reduced Form				
SW Triangle=0	.524***	.273***	.737***	.567***
	(.067)	(.089)	(.104)	(.068)
R-squared	0.594	0.597	0.576	0.569
Panel 3: First Stage				
SW Triangle=0	.438***	.434***	.341***	.447***
	(.042)	(.061)	(.048)	(.04)
R-squared	0.475	0.467	0.466	0.468
Panel 4: IV.				
Sewer=1	1.195***	.63***	2.159***	1.269***
	(.183)	(.223)	(.418)	(.187)
F-stat	110.773	50.939	49.711	127.599
Year FE & ln(Area)	Y	Y	Y	Y
ln(mi. CBD)	.	Y	Y	Y
Cubic mi. to CBD	Y	.	.	.
Imp. & Corner	Y	Y	Y	Y
ED % Foreign Born and Mean SES	.	Y	.	.
Miles to River	.	.	Y	.
Near River Indicator		.	.	Y
Sample	EW 2k	EW 2k	EW 2k	EW 2k
Observations	351	351	351	351

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.

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, $\widetilde{U}_D \sim Unif[0,1]$, the selection equation can be represented as

$$D = 1\{\widetilde{U}_D \leq F(X,Z)\}. \tag{Appendix C.1}$$

Under the cubic polynomial specification of the control function $K(p)$ in (3), MTE at each conditioning covariate value X and $\widetilde{U}_D \in [0,1]$ is given as in (4), and averaging $(X,\widetilde{U}_D)$ 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 $\widetilde{U}_D^* = F_{U_D^*}(U_D^*)$ such that for $\widetilde{U}_D$ defined in (Appendix C.1), $\widetilde{U}_D^* = \widetilde{U}_D$ 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 $\widetilde{U}_D^*$ and $\widetilde{U}_D$ 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 $\text{MTE}^*(X^*, \widetilde{U}_D^*)$, $\text{MTE}(X, \widetilde{U}_D) = \text{MTE}^*(X, \widetilde{U}_D^*)$ holds whenever $X = X^*$ and $\widetilde{U}_D = \widetilde{U}_D^*$ hold. We hence obtain

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

Taking the expectation with respect to X^* and $\widetilde{U}_D^* \sim Unif[0,1]$, 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^*).