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

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

JEL: O18, R3, L97, N11

Keywords: Piped water and sewer access, Infrastructure, Extrapolation

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1 Introduction

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

We find that access to piped water and sewers increased residential land prices in Chicago by a factor of about 2.8. Four comparisons help to put this estimate in perspective. First, an average Chicago household paid about 36% of its income for a parcel with water and sewer access versus 12% without. Second, during the decade containing our study period, land prices in Chicago first decreased by half and then doubled. Third, a simple benefit-cost analysis suggests that the benefits of sewer expansion exceed costs by about a factor of 60. This ratio is, for example, somewhat higher than that for the TransMilenio BRT system and much higher than the Los Angeles Metro. Finally, a back-of-the-envelope calculation suggests that the benefits of water and infrastructure exceed the value of averted mortality by about a factor of 7. Thus, our estimate of the effects of sewers on land prices is modest when compared to wages or to business cycle variation, but it is large compared to construction costs, the value of averted mortality, and other infrastructure projects.

According to the World Bank, about 15% of the world's urban population lacked

access to safe drinking water in 2020, and about 40% to safely managed sanitation facilities. Given the impact of safely managed water and sanitation on health and mortality, their provision would seem to be a priority. Yet, many cities also lack other basic services such as decent roads, sufficient public transit, adequate schooling, and reliable electricity. Thus, trade-offs inevitably arise. By providing estimates of the benefits of piped water and sewer access, we hope to inform policy makers facing such trade-offs. Our primary outcome variable is land price rather than a measure of health or mortality. Thus, we estimate the total private value of water and sewer access and provide a basis for cost-benefit analysis without the intermediate and challenging appeal to estimates of the value of a statistical life.

Our estimates also inform us about the development of the American economy during the late 19th century. Economic historians have long emphasized the importance of public health infrastructure for American cities (Ferrie & Troesken, 2008). The existing literature on sanitation investments during this period relies almost entirely on time series or panel data relating city-level changes in health and mortality to changes in the availability of particular public health interventions (e.g., Anderson et al. (2018), Alsan and Goldin (2019)). However, this period also saw changes in food purity laws, acceptance of the germ theory of disease, widespread vaccination, and increases in income, that could all 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 public health interventions.

Finally, building on the marginal treatment effects model proposed by Carneiro et al. (2011), we give conditions under which an estimate of marginal treatment effects may be extrapolated from a sample where quasi-random assignment to treatment may be defended to a sample where no quasi-random variation is available. Reliance on carefully constructed samples to identify treatment effects is common, and our

technique permits researchers to extrapolate results in a principled way.

2 Literature

The effect of municipal water treatment on mortality rates in late 19th and early 20th century is well studied (e.g., Anderson et al. (2018); Alsan and Goldin (2019)), but disagreements over effect size remain. Alsan and Goldin (2019) estimate the effect on infant mortality rates in the Boston Harbor watershed of diversion of sewer outflows and protection of drinking water from 1880 to 1920. They conclude that these interventions interacted to cause a 26% decline in infant mortality rates. On the other hand, using a sample of 25 U.S. cities between 1900 and 1940, Anderson et al. (2018) conclude that efforts to manage sewage outflows have no effect on infant mortality, water filtration leads to an 11% decline in infant mortality, and the joint effect of all water-quality related interventions is only 4%. The two papers do not report identical estimands, but they are close, and so the nearly order of magnitude difference in effect size is puzzling.

The literature evaluating public health initiatives in the late 19th and early 20th centuries 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., Alsan and Goldin (2019) and Anderson et al. (2018)). However, the late 19th and early 20th centuries 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). Estimators based on time series variation may confound the effects of these innovations with those of water treatment. Efforts to control for improvements in milk quality in Alsan and Goldin (2019) are reassuring, but those in Anderson et al. (2018) are not.¹ Our cross-sectional research design is not subject to this problem.

¹See 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.

The literature estimating effects of expansions of residential piped water and sewer networks consists of just a few studies. Kesztenbaum and Rosenthal (2017) examine the effect of the increasing availability of sewers in Paris between 1880 and 1915, while Troesken (2004) documents the role that water and sewer service played in narrowing the Black-White life expectancy gap in the U.S. during the first half of the 20th century.

The effects of municipal water quality improvement in the modern developing world is also well studied (e.g., Galiani et al. (2005); Bhalotra et al. (2021)). Although methods, interventions and outcomes differ across studies, all support the conclusion that water quality has economically important implications. Gamper-Rabindran et al. (2010) is most relevant. They estimate that in Brazil, between 1970 and 2000, the expansion of piped water explains about 25% of the decline in infant mortality, but find no effect of improved sewer access.² We know of only one other investigation of the relationship between water quality and rents (Ambrus et al., 2020).

Water infrastructure has complicated effects on the lives of those it touches. It affects current mortality and morbidity rates, and may affect time at leisure (Devoto et al., 2012), time at school (Ashraf et al., 2017), and future mortality (Ferrie & Troesken, 2008). Using these estimates for policy evaluation requires a complicated effort to aggregate and monetize these different effects. In contrast, land rent is a revealed preference measure summarizing the value of piped water and sewer service.

Efforts to extrapolate treatment effects outside the sampled population include Hotz et al. (2005), Angrist and Fernández-Val (2013), and Dehejia et al. (2021), Angrist and Rokkanen (2015), and Rokkanen (2015), while Cattaneo et al. (2021) consider extrapolation of treatment effects in a regression discontinuity design. We develop a new method for extrapolating estimates based on a quasi-experiment to a sample for

²From Gamper-Rabindran et al. (2010), between 1970 and 2000 household access to piped water increased from 15% to 62% and infant mortality fell from 125/1,000 to 34/1,000. They estimate that each percentage increase in piped water access decreases infant mortality by 0.48/1000. Thus, piped water access decreases infant mortality by $(62 - 15) \times 0.48 \approx 22/1,000$, about 25% of the total decrease of 91/1,000.

which quasi-random assignment of the treatment is not available. Our approach builds on the marginal treatment effects estimator developed by Heckman and Vytlacil (2005) and Carneiro et al. (2010).

3 Data

We require data to measure land values, and piped water and sewer access. We also require a description of other attributes of transacted parcels. To complete our cost-benefit analysis, we also measure construction costs. We here describe the data we use for each purpose.

Between 1874 and 1889, the *Chicago Tribune* often reported land parcel transactions filed with the municipal title office on the previous day. We collected all transactions listed in the Sunday edition using the website newspapers.com. This process yielded about 700 observations per year in the 1870s and 1,000 in the 1880s. The *Tribune* reported transaction date, price, parcel dimensions, either a street address or the nearest intersection, and whether the parcel is on a corner. The *Tribune* separately indicated transactions with "premises," i.e., a house, so we are confident that our data describe land transactions only. Appendix A gives more detail and describes our geocoding algorithm.

We rely on historical GIS maps to describe the block-by-block expansion of the sewer network (Fogel et al., 2014). These maps derive from the annual reports of the Chicago Department of Public Works and report the location and opening date for each segment of the sewer network. Water and sewer service were almost always installed simultaneously, and so we rely exclusively on sewer maps.

We say a transaction "has water and sewer access" if the nearest intersection to the transaction is within 75 feet of an operating sewer line in the transaction year. Visual inspection of the matching process confirms accurate matching of intersections to sewers.

We collect demographic data from the 1880 and 1900 censuses.³ Late 19th century census enumeration was organized around enumeration districts, the smallest spatial unit available to us. Our research design relies on variation over spatial scales that are small relative to enumeration districts, so using both the 1880 and 1900 censuses in our research design requires heroic downscaling. Because of this, we restrict discussion of these data to the appendix.

We calculate a number of control variables from GIS data layers. For each parcel, we calculate distance to the central business district (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 lakeshore⁴ and calculate distance to the Chicago River similarly. Finally, we calculate distance to the nearest horsecar line and major street from contemporaneous maps.⁵

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 HathiTrust).

4 Background

Chicago's population grew from about 300,000 in 1870 to more than one million in 1890. The Great Fire of 1871 destroyed the CBD and much of the city, but 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 internal migration. In this regard, late 19th-century Chicago resembles the rapidly growing developing world cities of contemporary Asia and Africa more than the slow growing contemporary cities of Latin America (Henderson & Turner, 2020).

³The 1890 census was lost and microdata no longer exist.

⁴Cook County Government Open Data, <https://datacatalog.cookcountyil.gov/GIS-Maps/Historical-ccgisdata-Lakes-and-Rivers-2015/kpef-5dtn>.

⁵The 1880 horse-drawn streetcar routes were digitized using a map from the Illinois State Grain Inspection Department. The 1880 street network is from Logan et al. (2011).

Land values grew rapidly in the early 1870s, declined by 50% between 1873 and 1877, and then recovered over the following five years. (Hoyt, 2000, p. 140). Thus, our 1874-1880 study period spans a major recession (1873-1877) and recovery (1878-1882). Figure B2(a) shows this same pattern of decline and recovery in our data.

In the 1850s, most residents drank from backyard wells. These wells were often near leaky privy vaults. Households with access to the city water system found it contaminated by industrial pollutants and minnows from Lake Michigan. Infant mortality in the 1870s was 74 per 1,000 (Ferrie & Troesken, 2008), similar to contemporaneous rates reported in other U.S. cities (Alsan & Goldin, 2019; Haines, 2001). Water quality improved as the city moved the water intakes further out into Lake Michigan and reduced the volume of waste dumped in the lake, although there were no major changes to municipal water quality during our 1874-1880 study period. Asbury's well-known Chicago history reports that the "gutters [run] with filth at which the very swine turn up their noses. . ." (Asbury, 1940, p. 23). When storms washed these wastes into Lake Michigan or private wells, cholera and dysentery epidemics followed. These epidemics killed hundreds of people in 1852 and 1854, precipitating plans to improve water and sewer infrastructure.

Expansions of the sewer and water system were primarily financed by bonds serviced by property tax revenue.⁶ These taxes may cause construction costs to be capitalized into property prices. If so, this biases our estimates downward.

Chicago is famously flat and grades shallower than 1:1,000 are common in our study area. Gravity fed sanitary sewers require a grade of about 1:200 to prevent suspended solids from settling and blocking the pipe (Mara, 1996). To design a sewer system capable of operating in Chicago's flat topography, Chicago hired noted engineer Ellis Chesbrough and substantially followed the proposal he submitted in 1855. This plan called for continuous flushing of sewer mains, which allowed them to operate at a

⁶The Sewerage Board was reluctant to impose fees and user charges because the resulting negotiations with building owners slowed down the expansion process (Melosi, 2000, p. 98).

grade of 1:2,500, shallow enough to be practical in Chicago. Importantly, Chicago's sewers manage both household sewerage and storm water runoff; in modern parlance, a 'combined' sewer system. Thus, expansions of the sewer system improve both the management of household waste and the drainage of affected streets.

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

Constructing sewers involved first laying sewer and water pipes at the required grade, whether above or below ground, filling in the space above or around them and, sometimes, paving the newly sewer road. We estimate the total effect of this process. Note that the sewer ordinances called for streets to be raised, but *not* the adjacent lots. Even today, one finds parcels below the level of the street throughout Chicago.⁷

Chicago issued its original plan for sewerage in 1855 (Chicago Board of Sewerage Commissioners, 1855) and later ordinances were issued at regular intervals as the system expanded. Ordinances were approved by everyone from the canal trustees to the sewerage commissioners. They list, block by block, the planned elevation of each intersection in hundredths of a foot. To understand the scale of this project, consider that the 1855 plan states, "[i]t will be necessary to raise the grades of streets an average of eighteen inches per 2,500 feet going West." Raising a 2,500 foot segment of a 20 foot-wide street by 18 inches requires 8,300 cubic yards or about 12,450 tons of fill.

⁷Chesbrough's 1855 plan explicitly considers the implications of the sewer elevations that are high relative to the parcels they serve (Plan of Sewerage, Chicago Board of Sewerage Commissioners, 1855, p. 19). Some buildings, particularly those 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.

The build-out of the system was rapid and predictable. Between 1874 and 1888, the mean miles of new sewer per year was 20 and only three years saw new mileage less than 14 or greater than 30. Variance in the rate of construction followed municipal expenditures and the business cycle: an annual regression of miles of new sewer per year on total municipal expenditure, city average real estate price, and lagged city average real estate price has an R^2 of 0.81.

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, p. 4): “The Mayor points out ... 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.” On the other hand, Hoyt (2000, p. 91) reports that just prior to our study period “Groups of promoters and politicians combined to secure improvements ... on the streets they had selected for development as fashionable sections.” The historical record makes clear that the assignment of sewers to neighborhoods and streets was not independent of land value but offers conflicting evidence on the details of this assignment process.

On the other hand, the principles of wastewater engineering limit the ability of politicians to privilege one neighborhood over another. The sewer system must be built out continuously; disfavored neighborhoods cannot be skipped. Figure 1a illustrates the expansion of piped water and sewer access. Thick gray lines indicate water and sewer lines pre-dating our 1874-1880 study period. Thick black lines indicate construction during our 1874-1880 study period. Fine gray lines indicate later construction. The orderly radial expansion of the network is obvious. This is consistent with contemporaneous sewer provision in southern cities. Southern cities provided sewer service to Black neighborhoods, in part because it was impractical to construct the network to avoid them (Troesken, 2004).

The 1873 ordinance in effect during our study period covered dozens of streets.

Politicians may have affected the timing of construction on streets covered by the ordinance. However, the importance of engineering considerations in the expansion process suggests that any exercise of political discretion that required deviation from an orderly radial expansion of the network was likely rare.

5 Research design

The 1855 plan and subsequent ordinances prescribed the more-or-less radial expansion of the sewer system, with an important exception. The 1855 plan described a “triangle,” south of Congress Street and west of Halsted Street, where sewer service was to be delayed because the area was slightly lower than adjacent areas. Chesbrough writes of this region: “The extreme south-west part of the city [is] too low [to sewer] ... and the depth of filling required to raise streets over it would average two feet” (Plan of Sewerage, p. 16). Because of this, Chesbrough delayed service to this region; “[a]s this part of the city may not be improved for several years, it is deemed sufficient for present purposes to state the general depth of filling that would be required” (Plan of Sewerage, p. 15). Because the plan also calls for streets to be raised “an average of eighteen inches per 2,500 feet going West,” this means that Chesbrough delayed sewer service in the Southwest Triangle because of six marginal inches of fill.

We rely on variation in sewer access across the boundaries of the Southwest Triangle to address the endogenous assignment of sewer and water service to parcels. For this purpose, we define our Quasi-experimental area as the area extending 2,000 feet north and south of Congress Street, and for two miles west from its eastern limit at the corner of Halsted and Congress. The boundaries of this region closely follow Halsted and Western in the east and west, and Monroe and Taylor in the south and north. For reference, Congress Street is now the Eisenhower Expressway.

Our primary analysis will be based on 351 transactions occurring in this area between 1874-1880. We exclude parcels exactly on Congress Street, i.e., those matching to intersections within 75 feet of Congress Street because the 1855 plan was ambiguous

about whether or not Congress Street lies inside or outside the Southwest Triangle. In robustness checks, we consider an Extended-quasi-experimental area by adding transactions that occur within 2,000 feet of Halsted Street, south of Congress. In total, 533 transactions occur in this larger area during 1874-1880.

The white-and-black checked lines in Figure 1 illustrate the northern and eastern border of the Southwest Triangle: Congress and Halsted Streets. Hatched areas in panel (b) describe the Quasi- and Extended-quasi-experimental areas. The black areas in panel (b) indicate the entire region that received water and sewer access during our 1874-1880 study period. This is the region for which we observe construction costs and is the relevant area for the purpose of policy evaluation. We refer to a sample drawn from this area as a Relevant sample. Appendix A provides further details and illustrates the distribution of transactions across these regions.

Figure 1(c) highlights the evolution of the sewer network in the Quasi-experimental region. This figure establishes that, even 20 years after the adoption of the 1855 sewer ordinance, the construction of sewers south of Congress Street trailed the north.

Table B1 presents sample means for the Quasi-experimental sample. Column (1) describes transactions inside the Southwest Triangle, i.e., south of Congress Street. Column (2) describes transactions outside the Triangle, i.e., north of Congress Street. Column (3) reports a t -statistic testing the equality of the first two columns. Piped water and sewer incidence is lower inside the Southwest Triangle than outside, just as the 1855 plan prescribes. Consistent with a large effect of water and sewer access on value, prices are 73 log points (108%) higher outside of the Southwest Triangle than inside.

Figure 2(a) shows changes in sewer incidence across the Congress Street border of the Southwest Triangle during 1874-1880 as a function of distance to Congress Street. The x -axis in this figure is distance from Congress Street. Negative distances indicate displacement south into the Southwest Triangle and conversely. The y -axis indicates piped water and sewer share relative to the share in the bin just inside the Southwest

Triangle. As in Table B1, piped water and sewer incidence and land prices are lower in the Southwest Triangle. As prescribed by the 1855 plan, the drop in sewer incidence occurs at Congress Street, i.e., $x = 0$.⁸

According to the 1855 plan, the elevations of parcels on opposite sides of the boundary of the Southwest Triangle differ from each other by about six inches on average. The Quasi-experimental study area is 4,000 feet north-to-south, so a six-inch average difference between the northern and southern portions of the Quasi-experimental area requires an average drop of 3 inches per 1,000 feet, a grade of 1:4,000. The logic of gravity fed sewers implies that this tiny difference in elevation had a large effect on construction costs and led to the delay in sewer provision prescribed by the 1855 plan and observed in Figure 1(c).

Despite their importance for sewer construction, such grades are nearly imperceptible. People begin to perceive a playing field as sloped at a grade of about 1:70 (Aldous, 1999), while a grade of 1:1,000 is close to the precision of a present day contractor's laser level. The variation in grade that determines the timing of sewer construction in our study area is beyond unaided human perception, is at the limits of what a carpenter can measure, and can be accurately measured only with a surveyor's tools.

Our estimation strategy is based on the proposition that the variation in elevation that precipitated the demarcation of the Southwest Triangle only affected real estate prices through its effect on the timing of sewer and water access. The validity of this strategy depends on whether the boundary of the Southwest Triangle is drawn because of the small change in elevation that is its basis in the 1855 plan, as Chesbrough explicitly claimed, or if it reflects some other unobserved difference.

Because the area south of Congress Street is lower than the area to the north, if imperceptibly so, it may be 'swampier' than the area to the north of Congress Street.

⁸The high incidence of sewers in the far left bin reflects transactions in the southwest corner of the study connected to a separate, southward-draining main.

Appendix C presents evidence from historical maps and surveys that this was not the case. Our study area appears to have been remarkably flat, uniform, dry, undeveloped prairie before 1855. The Quasi-experimental area remained undeveloped up until the start of our study period, although development along Halstead Street meant that the Extended quasi-experimental area was partly developed by 1874.

Table B1 presents sample means for observable covariates inside and outside of the Southwest Triangle during our 1874-1880 study period. The share of corner parcels, mean distance to the CBD, and mean transaction year are statistically identical for parcels included and excluded from the Southwest Triangle. However, parcels included in the Southwest Triangle are about one city block farther from a horsecar line, are less than half a block farther from a major street, and may be slightly larger. If transactions are randomly assigned to the Southwest Triangle, all covariate distributions should be independent of this status. Failing this balance test poses a threat to the validity of our research design if the covariates in question have an independent effect on the outcome and if we do not condition on the problematic covariates. Given this, we will control for covariates when we estimate treatment effects. We also provide evidence against the hypothesis that the unbalanced covariates have an independent effect on outcomes.

Data limitations prevent us from checking that land prices across Congress were constant in a prior period when sewer and water service is uniformly unavailable; the *Tribune* first publishes parcel transactions in October 1873. However, we can check that land prices were equal across this boundary after sewer service was universal both north and south of Congress Street.

Table B2 describes transactions occurring in the Quasi-experimental region during 1886-1889, six to nine years after the end of our main study period. This table replicates the first three columns of Table B1 for this later time period. Piped water and sewer access is universal and the difference between prices inside and outside the Southwest Triangle is gone. Differences in covariates are similar for the two samples. That

differences in covariates persist, but not differences in price, when sewer access is universal is not consistent with the hypothesis that the unbalanced covariates in Table B1 have an independent effect on outcomes.

The top panels of Figure 2 and Table B3 refine this conclusion. Panel (a) of Figure 2 shows changes in sewer incidence across the Congress Street border of the Southwest Triangle and panel (b) shows the corresponding changes in log price. Panel (a) shows that sewer incidence was constant across the boundary by 1886. Panel (b) reports mean log price controlling for year indicators, $\ln(\text{area})$, and $\ln(\text{mi. to CBD})$. Parcel prices were constant across the border during 1886-1889. Table B3 conducts regressions similar to those on which Figure 2 is based, but with a single indicator for 'outside the Southwest Triangle.' These regressions confirm that by 1886-1889 the cross-border difference in property prices cannot be distinguished from zero and that this conclusion is robust to the permutation of control variables. Estimates most like those of Figure 2(b) in column (2) allow us to reject the hypothesis that prices of parcels north of Congress Street are more than 34 log points greater than those to the south. This establishes a bound on the importance of unobservable determinants of cross-border changes in prices.

Summing up, the Quasi-experimental area consisted of uniform, undeveloped prairie when the plan was drawn, and it remained undeveloped until the start of our study period. The price difference across Congress Street during 1874-1880 is erased by 1886-1889 when sewer access was also equalized across the border. We satisfy a balance test on several observed covariates, and there is no evidence that unbalanced covariates have the independent effect on the outcome that could invalidate our estimation strategy. This evidence all weighs against the hypothesis that land prices changed across Congress Street for reasons unrelated to the sewer and water access.

This suggests that, conditional on controls, a comparison of changes in prices and sewer access across Congress Street should yield an unconfounded estimate of the effect of water and sewer access on prices. Indeed, because distance to horsecar lines

and major streets do not have an important independent effect on outcomes, the case for conditioning on these variables is debatable.

The bottom panels of Figure 2 perform this comparison. These figures consider 1874-1880 instead of 1886-1889, but are otherwise like the two top panels. Panel (c) shows changes in sewer incidence across the Congress Street border of the Southwest Triangle and panel (d) shows the corresponding changes in log price. This is the variation on which our estimates are based. Panel (c) is a first-stage regression. Panel (d) 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.

6 Discussion

Table B1 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)). Given this, our finding that price differences disappear with the elimination of the difference in sewer access is surprising and likely reflects particular features of the Chicago land market: the city was growing rapidly; our transactions are of undeveloped land; and structures in our study area were generally cheap, hastily constructed wooden houses.

We are concerned about several possible threats to the validity of our research design. First, it may be that transactions in different years, and hence different parts of the business cycle, have different rates of treatment and are systematically different in unobservable ways that confound our estimates. Second, it is possible that other municipal services are bundled with water and sewer provision. If so, this would require a reinterpretation of our results. Third, it is also possible that sewer access served in part to improve the management of industrial waste or to shift land use towards industrial activity. If true, this would also require a reinterpretation of our estimates. Appendix C discusses each of these each of these possibilities in detail. None

appears to find support in our data or the historical record.

Because all parcels eventually receive sewer service, our treatment is “accelerated arrival of sewer and water access.” Interpreting this treatment effect requires care and an appeal to theory. Let R denote the annual rent on an unsewered parcel, S the increment to annual rent if sewer service is available, and δ the market discount factor. A treated transaction is one with water and sewer access at the time of sale. If Y_1 is the asset price of such a parcel, then $Y_1 = \sum_{t=0}^{\infty} \delta^t (R + S)$. Now let Y_{0k} be the asset price of a parcel that receives sewer service k years in the future. In this case, $Y_{0k} = \sum_{t=0}^{k-1} \delta^t R + \sum_{t=k}^{\infty} \delta^t (R + S)$. Hence, the value of treating a parcel with a k year acceleration of the arrival of water and sewer service is $Y_1 - Y_{0k} = \sum_{t=0}^{k-1} \delta^t S$, the present value of k years of water and sewer access. An implication of this logic is that the value of a parcel should be decreasing in the length of time until the arrival of water and sewer service. We confirm that this occurs in results presented below.

Of 93 unsewered transactions in the Quasi-experimental sample, 25, 15, 13, and 16 receive water and sewer service in 1, 2, 3, and 4 years, respectively. To simplify our problem, we consolidate different delay times into a single treatment by pooling the population of unsewered transactions. This means that our treatment effect reflects an average of the treatment effects of the different delay times in the population.⁹ This simplification is motivated by practical and econometric concerns. First, there is limited variation in years of delay in our population of unsewered transactions. Second, because our single instrument is binary, our research design cannot inform us about the effect of more than one treatment, i.e., one year delay, two year delay, etc. Third, the econometrics of estimating models with multiple or continuous treatments is much more difficult, and the marginal treatment effects framework that forms the basis for

⁹This intuition extends easily to the case where delay k is stochastic. For example, if sewers arrive with probability p at $k = 1$ and are available with certainty for $k \geq 2$, then $Y_0 = \delta p S + \delta^2 \sum_{t=0}^{\infty} \delta^t S + \sum_{t=0}^{\infty} \delta^t R$. If each unsewered parcel has its own lottery over the arrival times of sewer and water service, then our estimand reflects an average over the population of lotteries rather over the population of deterministic arrival times.

our approach to extrapolation does not apply.

7 Estimation

Let Y_i be the log of parcel i 's transaction price observed in the data. Let X_i denote a vector of observable parcel attributes drawn from *transaction year indicators*, $\ln(\text{miles to CBD})$, $\ln(\text{Parcel Area})$, *corner indicator*, *distance to horsecar line* and *distance to major street*. Let D_i be a treatment indicator, with $D_i = 1$ if and only if parcel i has piped water and sewer access. Let Z_i be a binary instrument with $Z_i = 1$ outside of the Southwest Triangle. This definition of Z_i assures a conventional positive relationship between instrument and treatment.

We indicate potential outcomes with a subscript, so that Y_{1i} is the price of treated parcel i , and Y_{0i} its 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 1874-1880. We indicate these quantities with an asterisk, e.g., Y_i^* is a transaction price drawn from this sample.

We would like to estimate the average treatment effect on the 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 ATE s rather than $LATE$ s, and 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 $TSLS$.¹⁰ We next implement the local IV framework proposed by Carneiro et al.

¹⁰In addition to instrument exclusion, exogeneity, and monotonicity conditional on X , if the conditional expectation of D given X is linear, we can interpret the $TSLS$ estimand as a weighted average of the local average treatment given X . See, e.g., Słoczyński (2021) for details.

(2010). This framework offers a simple way to control for multi-dimensional X , allows the explicit calculation of an average treatment effect, and allows tests for heterogeneity of treatment effects with respect to observable and unobservable characteristics. The LIV/MTE framework also provides a foundation for the approach to extrapolation of treatment effects that we present in the final stage of our analysis.

Local Average Treatment Effects Table 1 presents four sets of estimates. For reference, panel A presents OLS regressions of the form,

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

These regressions show a significant positive association between piped water and sewer access, and transaction prices. In the first column, we present a specification without controls. In the second column, we add year indicators and controls for log lot size and log miles to the CBD. The coefficient estimate remains stable even as the controls add considerable explanatory power. In the third column, we add controls for corner properties, distance to horsecar, and distance to a major street. In each case, transaction prices are about 30 log points higher for parcels with water and sewer access. Because parcel level water and sewer access is determined by the presence of a sewer in the nearest intersection, throughout Table 1 we report standard errors clustered by intersection in parentheses. For reference we also present unclustered errors in brackets. Clustered errors are generally larger than robust errors, but never by enough to qualitatively change the interpretation of our results.

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

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

We see in column 1 that being in the Southwest Triangle decreases transaction prices by about 70 log points. This effect is estimated precisely and varies only slightly as we add control variables in columns (2) and (3). Column (2) uses the same controls as we used

in Figure 2(b), 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 2(a). First-stage F statistics are above critical values for conventional weak instrument tests.

Panel D presents TSLS estimates of the effect of piped water and sewer access on transaction prices. IV estimates range from between 124 to 183 log points, estimated precisely. In unreported results, we also add day of week indicators to each specification reported in Table 1 columns (1-4). The resulting TSLS estimates are not statistically different from Table 1.

Figure 2(c)(d) illustrates an increase in piped water and sewer access and transaction prices when we cross Congress Street to leave the Southwest Triangle. These changes appear to occur sharply in the figure. Nevertheless, we worry that this increase may reflect a spatial trend correlated with treatment and transaction prices. In column 4 of Table 1 we restrict the sample to a narrower window that includes only parcels within 1,000 ft. of Congress Street. The magnitudes of the reduced form and first stage are reduced, but the IV estimate is unchanged. In column 5, we include controls for distance to Congress Street in our regression of column 2 and allow the slope of this trend to change at Congress Street. Once again these controls reduce the magnitude of first-stage and reduced form effects by about half, but leave the IV point estimate unchanged, although the standard error increases to just above the 10% significance threshold.

To refine this test, we consider the impact of a hypothetical confounding trend in land prices across Congress Street, the trend that we observe across the Congress Street boundary during 1886-1889, after piped water and sewer access was universal on both sides of the border. Implicitly, we suppose that the small spatial trend we observe in 1886-1889 is due to confounding unobservables. Table B3 is similar to the middle panel of Table 1, and reports this trend in column 3. We then subtract this trend from transaction prices, the dependent variable, in our 1874-1880 sample in column 6 of Table 1. Unsurprisingly, this leads to a smaller estimated treatment effect, but one that is estimated precisely and is still around 100 log points. We reach a similar conclusion more directly if we subtract the 34 log point upper confidence bound of the cross-border price difference estimated in column 2 of Table B3 from the treatment effects estimated in Table 1. The robustness of results to permutations of control variables and to correction for a spatial trend suggests that omitted variables are not confounding our estimates.

We conduct two further exercises to investigate whether our estimates are confounded by the business cycle. First, for the first-stage and reduced form estimates reported in Table B3, we report the partial R^2 that results when all year indicators are restricted to be zero (the omitted year is 1874). We see that these partial R^2 s are close to the unrestricted R^2 s: the year dummies explain little of the total variation in land prices. Second, in Table B4 column 5 we add a quarterly indicator as a control, and in column 6 we include as a control city-wide mean quarterly transaction prices (based on our entire sample of transactions). Neither control changes our estimates. Like the evidence we present in Figure B2(c) and (d), these results do not support the hypothesis that our results are confounded by the business cycle.

Columns 7 and 8 of Table 1 re-estimate the specifications of columns 1 and 2 on the Extended-quasi-experimental sample, i.e., the sample of transactions drawn from within 2,000 feet of the northern or eastern boundary of the Southwest Triangle. We

observe that coefficients in columns 7 and 8 are statistically indistinguishable from their counterparts estimated on the smaller Quasi-experimental sample. This suggests either that treatment effects are not heterogeneous or that the distributions of treatment effects in the two samples of compliers are similar.

We note that the validity of our research design is easier to defend on the smaller Quasi-experimental sample than the Extended-quasi-experimental sample. Figure B1 reproduces the border plots of Figure 2 for the larger sample. Neither prices nor sewer access changes as sharply at the boundary of the Southwest Triangle in the larger sample. This is because, 20 years after the 1855 ordinance, both sides of the eastern boundary of the Southwest Triangle have sewer service (see Figure 1). The absence of a sharp break in sewer share across the boundary of this sample 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 1 reflects our interest in extrapolating estimates. We do not consider more flexible specifications for the effect of distance to CBD for two reasons. First, extrapolation to the larger and more remote Relevant sample based on, for instance, polynomials in distance to the CBD is sensitive to functional form. Second, prior evidence provides strong support for our simple specification.¹¹ In a similar spirit, we do not include measures of distance to the Chicago River in the results presented in Table 1. Because the Chicago River runs approximately parallel to our Quasi-experimental sample, and approximately perpendicular to the Relevant sample, extrapolating this effect is hard to defend. With these caveats in place, Table B4 presents supplementary results that allow for more flexible effects of distance to the CBD and include a control for distance to the Chicago River. Broadly, the results presented in Table 1 are robust to these changes.

The interpretation of our treatment effect as the increment to land value resulting

¹¹Ahlfeldt and McMillen (2018) find that land prices in the entirety of late 19th-century Chicago track the logarithm of distance from the CBD closely.

from a three year delay (or acceleration) of access to water and sewer access relies on conventional logic for converting flows of rents into asset prices. An implication of this logic is that transactions for which water and sewer access is more remote should have a lower value than those for which it is imminent. We test whether this is the case in column 7 of Table B4 by duplicating the regression performed in Table 1 column 2, but using years to access in place of our usual binary treatment indicator. As expected, parcel value is decreasing in time until sewer and water access.

Given results in Bayer et al. (2007) and Caetano and Maheshri (2021), we also consider the possibility that our treatment effect reflects homophilic racial or demographic preferences and ethnic sorting in response to sewer and water access. The 1880 (1900) census reports the foreign-born share north of Congress at 24.8% (22.8%) and south at 36.2% (30.1%). Thus, the foreign-born share is slightly higher south of Congress throughout the whole period, but the north to south gap decreases only slightly, from 9% to 7%. This is consistent with Alsan and Goldin (2019), who find that that ethnic composition changes only slightly in response to water quality.

Recall that prices north and south of Congress Street are different before and the same after sewer construction. If the initial north-to-south price differences reflected ethnic sorting, a 2% change in cross-Congress Street foreign-born share between 1874-1880 and 1886-1890 can only account for the disappearance of an about 100% price gap if the land price elasticity of ethnic share is on the order of 50. To our knowledge, there are no estimates of the effect of immigrant share on real estate prices in late 19th century U.S. However, Bayer et al. (2007) consider this issue in contemporary real estate markets. They find ample evidence for the importance of racial preferences and racial sorting in residential real estate markets, but conclude that racial sorting *per se* does not affect real estate prices.¹² In all, the evidence seems to weigh against the hypothesis that the ethnic sorting into unsewered neighborhoods is an important contributor to

¹²This surprising conclusion obtains because “households are able to sort themselves across neighborhoods on the basis of race without the need for price differences to clear the market” (Bayer et al., 2007).

our treatment effects.

In Table B3, we include the foreign-born share from the 1880 census as a control. This reduces the magnitude of our treatment effect from about 1.5 in Table 1 to about 0.8. Because the spatial resolution of the 1880 census is poor, we regard this estimation as speculative. Taken together with the evidence above, these estimates suggest 0.7 as an upper bound on the contribution of homophilic ethnic sorting to land prices.

Marginal and Average Treatment Effects The LIV/MTE framework developed in Heckman and Vytlacil (2005) and Carneiro et al. (2010) permits estimation average treatment effects in environments with parametric controls. This framework also provides a foundation for extrapolating our estimates from the Quasi-experimental to the Relevant sample under a weaker assumption than “no heterogeneous treatment effects.”

The LIV/MTE framework recasts the potential outcomes framework as a Roy model. Each unit selects into treated or untreated status on the basis of a third selection equation. Formally,

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

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

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

where 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 \tilde{U}_D denote U_D normalized by its cdf. That is, $\tilde{U}_D = F_{U_D}(U_D) \sim Unif(0,1)$. This transformed unobserved heterogeneity ranks units in the population P according to the

unobservable cost of access to piped water and sewage, i.e., \tilde{U}_D is smaller as unobserved costs of piped water and sewer access are smaller. On the basis of arguments in Carneiro et al., 2011, we state our estimating equation and subsequent derivations in terms of this transformed variable.

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

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

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

To estimate MTEs, we run the local IV regression:

$$p \equiv \Pr(D = 1 | X, Z) = F(X, Z), \quad (3)$$

$$Y = X'\delta_0 + \hat{p}X'(\delta_1 - \delta_0) + K(\hat{p}) + \varepsilon.$$

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

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

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

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

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

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

We estimate equation (3) for specifications corresponding to those in columns 2, 3, 7, and 8 of Table 1. Because equation (3) is quite long, we relegate complete reporting to Table B5. Table 2 reports estimates of ATE and hypothesis tests derived from these regressions.

The first row of Table 2 reports an 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 2 reports p -values of the tests of the hypothesis that all terms involving the propensity for treatment are zero. This is always rejected. Piped water and sewer almost certainly 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 the hypothesis of homogeneity of effects by unobservables. The fifth row tests the joint hypothesis of either sort of treatment effect homogeneity.

The results of these tests vary with sample. In our Quasi-experimental sample, columns 1 and 2, we see clear evidence of treatment heterogeneity on unobservables, and somewhat weaker evidence for treatment effects on observables, and we clearly reject the hypothesis of treatment effect homogeneity. Columns 3 and 4 consider the larger Extended-quasi-experimental sample. Here, we reject the hypothesis of treatment effect homogeneity at the 15% level in Column 3, but we cannot reject treatment effect homogeneity by observables or unobservables alone. We can reject the hypothesis of treatment homogeneity by both observables and unobservables when adding

additional controls in Column 4. Inspection of Table B5 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 2 calculates the average treatment effect given in equation (5), along with bootstrapped standard errors. Comparing to the LATES estimated in Table 1 we see that ATEs are marginally smaller than corresponding TSLS LATES in the Quasi-experimental sample, and both are estimated precisely. In the larger Extended-quasi-experimental sample, ATE and LATE are statistically indistinguishable. Even the smallest of these ATE estimates is large; $e^{1.00} \approx 2.7$, so these estimates indicate that piped water and sewer access at least doubles land values. The differences between LATE and ATE estimates are consistent with other results in rows 3 to 5 of Table 2. Heterogeneous treatment effects are necessary if ATE and LATE are to diverge.

The final row of Table 2 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. We do not cluster errors by intersection in our MTE estimations because clustering had only modest effects in our TSLS results and because no extension of the Carr and Kitagawa test is available for this case.

We can also use the MTE estimations to evaluate the importance of heterogeneous treatment effects. In unreported results, we re-evaluate the ATE for each of the four specifications reported in Table 2 for the counterfactual case when all transactions occurred in 1874 (i.e., all year indicators zero). This leads to marginally larger point estimates of the treatment effect that cannot be distinguished from the baseline case at ordinary levels of confidence. This does not support the hypothesis that the business cycle plays an important role in determining the magnitude of treatment effects.

Extrapolation to Relevant sample In the absence of treatment heterogeneity, extending our treatment effect estimates from the Quasi-experimental to the Relevant sample is straightforward. Estimates in Table 1 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 2 suggests that concern about treatment heterogeneity is warranted. Given this, we develop a method for extrapolating treatment effects in the presence of treatment heterogeneity. This extrapolation requires that equations (1) and (2) continue to hold on the Quasi-experimental sample. In addition, we assume

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

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 marginal distribution of unobserved heterogeneities is the same across the two samples. These conditions would be satisfied, for example, if the mechanism and magnitude of the causal effect are the same in both samples, and unobserved resistance to receiving the treatments is identically distributed between them.¹³

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

¹³More concretely, (6) would be violated if the hedonic prices of the X 's were different in the two samples, and (7) would be violated if a parcel with idiosyncratically high sewer value was more likely to have idiosyncratically high unsewered value in the Relevant than the Quasi-experimental sample.

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:

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

Appendix E 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 2 presents our estimates of ATE^* for each of our specifications, along with bootstrapped standard errors. All are estimated precisely enough that they may easily be distinguished from zero. These estimates of ATE^* range from 1.04 to 1.10, across all samples and specifications. There is even less variation in ATE^* across samples and specifications than we saw for ATE , but in no case is the ATE^* statistically distinguishable from the corresponding ATE .

Conditional on the validity of our estimates of ATE , the validity of our estimates of ATE^* hinges on equations (6) and (7). We have not been able to construct a test of whether these equations hold in our data. 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 B2 compares the Quasi-experimental and Relevant samples. Panel (a) of Figure B2 reports mean log prices by year in the Relevant and Quasi-experimental samples, conditional on: $\ln(\text{Area})$, $\ln(\text{miles to CBD})$, and corner. Panel (b) reports mean log prices by parcel area in both samples, conditional on year indicators, $\ln(\text{miles to CBD})$, and corner. Finally, panel (c) gives counts of transactions by year and sample. Other than the differences in levels, the two samples show similar patterns and suggest no contradiction to the hypothesis that the same basic economic forces are at work determining prices in the Quasi-experimental and Relevant samples.

8 Externalities

The value of water and sewer access likely has both a private and an external component. Most likely, a parcel is more valuable if it has water and sewer access and if nearby parcels have water and sewer access. As the external share of benefits increases, the case for subsidies or public provision is stronger. We have focused on the effect of a binary treatment: a parcel is ‘treated’ when sewer and water pipes are installed through the nearest intersection. This implicitly aggregates private benefits and external benefits that operate on the scale of a city block. In Appendix D we attempt to disentangle the two effects.

Our investigation suggests the following. First, the value of water and sewer is primarily private or external effects operate over a scale of a single city block. Second, to the extent that there are external effects operating over greater distances, they are asymmetric. Neighborhoods with sewers are harmed by nearby unsewered neighborhoods but neighborhoods without sewers are not.

9 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 1874-1880. 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 1874-1880.

An average residential lot in any of our samples is about 125 feet deep. If every sewer serves lots on both sides of one street, then each linear foot of sewer serves 250 ft² of land area. Our shapefiles of the sewer network then allow us to calculate that about 138m ft² of land received piped water and sewer access during 1874-1880. During 1874-1880, 384 untreated parcels transacted in the Relevant sample area. The total area of these parcels was about 1.8m ft², and their aggregate value was about 0.81m (1880\$). Dividing, the average price per ft² of untreated land in the Relevant area was about \$0.45.

We must now decide whether to apply an estimated ATE that does or does not allow for heterogeneous treatment effects. Our LIV estimates do not strongly support either hypothesis, and so we proceed using the smallest estimate, 1.04, from column (1) of Table 2. Applying this treatment effect to the price per square foot of untreated land in the Relevant sample area, we calculate that piped water and sewer access increases the value of land in this area by $0.45 \times (e^{ATE^*} - 1) = 0.82\$/ft^2$, about 180%. Multiplying this increase by the area affected, the total value of the piped water and sewer expansion is slightly above \$113m. Using the standard error of this estimate of ATE^* and applying the same logic, we get a 95% CI [33m,261m].

This estimate requires several comments. First, this calculation reflects our smallest estimate of the average treatment effect. If, as we might do on the basis of column (3) of Table 2, we reject the hypothesis of heterogeneous treatment effects, then the LATES we estimate in Table 1 can be defended as ATEs and extended to the Relevant sample. In this case, using column (7) in Table 1 (the analog of column 3 of Table 2) we have $ATE = 1.3$. Using this estimate to value piped water and sewer access gives about

\$164m.

Second, an average parcel in the Quasi-experimental sample receives piped water and sewer service about three years after it is sold. Thus, our estimates reflect the flow value of three years of piped water and sewer access, not the full asset value. Hoyt (2000) reports that interest rates were about 8% during our study period. If we denote our estimated aggregate value by V^* and assume that this flow value arrives every three years in perpetuity, then the full asset value of piped water and sewer access is $\sum_{t=0}^{\infty} \left[\left(\frac{1}{1.08} \right)^3 \right]^t V^* \approx 4.9V^*$. Thus, we should multiply by about 4.9 to scale up our three year flow value to an asset value. Applying this adjustment to our 113m dollar estimate of the three year flow value, we have an asset value of about 554m, and using our earlier calculation, the 95% CI is [162m,1274m] (all in 1880 dollars).

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 1874-1880. 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. Expenditures on water and sewer during 1874-1880 were \$1.5m and \$2.4m. Maintenance expenditure was about \$0.4m per year (Chicago Board of Public Works, 1873). Assuming maintenance costs are constant in perpetuity and discounting at the same 8% rate as above, the discount present value of maintenance is \$5.0m. Summing, total expenditure on water and sewer access is \$8.9m.

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

costs.

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

Alsan and Goldin (2019) estimate that all water and sewage-related public health interventions were jointly responsible for a 26% reduction in infant mortality in Boston between 1880 and 1920. In 1896, the infant mortality rate in Boston was about 163/1,000. From the 1880 census, there were 3,014 infants living in the Relevant sample area in 1880. Elementary calculations suggest that water and sewer access resulting from the 1874-1880 expansion prevented about 127 infant deaths per year. Per U.S. Environmental Protection Agency (2015) the value of a statistical life in 2000 is 6.3m USD₂₀₁₅. Converting to 1880 dollars¹⁴ and adjusting for the change in per capita income using Viscusi and Aldy (2003), we have a VSL of \$127,000 with 95% CI [33,000, 261,000].¹⁵ Multiplying, we have an annual value of averted infant deaths of about \$16.m with 95% CI [4.1, 33.1] (1880 dollars). Recall that our estimate of treatment effects is a three year effect, suggesting that we multiply this by three to compare it with the corresponding 113m dollar estimate for the value of piped water and sewer access. Comparing point estimates suggests that the value of water and sewer access was about 7 times as large as the value of averted infant mortality and, therefore, that non-mortality-related benefits of water and sewer access are economically important. This multiple is dramatically larger if we consider VSL values near the bottom of their 95% CI or values of water and sewer near the top of that 95% CI. Only in the

¹⁴We adjust prices using indices from Sahr (2009) for the period 1880-1912 and the Bureau of Labor Statistics (BLS) CPI series for 1913-.

¹⁵Viscusi and Aldy (2003) report income elasticity of VSL of 0.50, with 95% CI [0.2,0.8]. Converting to USD₂₀₀₀, we have 2000 per capita GDP of 35,880 and 1880 per capita GDP of 3,930 Census Bureau (1975).

improbable case that we draw a VSL near the top of its CI and a value of water sewer near the bottom of its 95% CI can we conclude that the two quantities are of close to the same magnitude.

Per Table B1 the average log value of a property in the Quasi-experimental region north of Congress Street was 8.4, or about 4,500 dollars. Almost all of these parcels had water and sewer access, so this is effectively an estimate of the price of a parcel with water and sewer access. A treatment effect of 1.04 log points means that an untreated parcel is worth about one third as much as a treated one. Thus, we have that water and sewer access increases the value of a parcel by about 2,910 dollars, or around five years income for an average unskilled laborer.¹⁶

We can also compare the costs and benefits of the 1874-1880 Chicago sewer expansion to other infrastructure projects. Tsivanidis (2019) estimates the ratio of benefits to costs for the Bogota TransMilenio bus rapid transit network to be between 5:1 and 20:1. Severen (2023) performs a similar evaluation of the Los Angeles Metro Rail in 2000 and estimates that ratio of benefit to costs is at most 1:1, and possibly as small as 1:8. Allen and Arkolakis (2014) estimate the benefit of incremental inter-city trade caused by the U.S. Interstate Highway System and find a cost-benefit ratio of about 3:2. Duranton and Turner (2012) consider several hypothetical expansions of metropolitan portions of the U.S. Interstate Highway System and conclude that such expansions always fail cost-benefit tests. Lewis and Severnini (2020) examine extensions of rural power lines in mid-20th century U.S. and conclude that, for an average county, costs and benefits were about equal. Finally, Clay et al. (2016) examine policies to reduce emissions from mid-20th century coal fired power plants and conclude that the benefits of these policies exceeded costs by a factor of about six. Recent estimates of the

¹⁶Average income for Chicago laborers was about \$650 in 1880, from estimates of wages per non-agricultural worker for the state of Illinois found in Easterlin (1960, pp. 73–140) (\$627 per year) and Hoyt's (2000, pp. 118-119) estimates of wages for workers in the city of Chicago during the 1870s (\$3 a day for unskilled laborers). We inflate to 2021 price levels using CPI estimates from Sahr (2009) for 1880-1912 and the BLS CPI series for 1913 onward.

costs and benefits of the U.S. Clean Air Act have the cost-benefit ratio at about 30:1. The benefit to cost ratio for Chicago's 1874-1880 expansion of water and sewer access is probably large relative to other large public infrastructure projects, but is close to that of the U.S. Clean Air Act.

10 Conclusion

Access to safe water and modern sanitation for the relatively poor immigrants to developing world cities is far from universal, and a large body of evidence suggests that without it, urban density causes disease. Thus, increasing access to high quality drinking water and modern sanitation would seem to call for a crisis response. However, relatively poor developing world cities face a portfolio of crises. Not only do their residents need more and better water and sewer infrastructure, they also need more and better roads, public transit, electricity supply and distribution, education, and housing. Trade-offs must be evaluated and made.

With this in mind, piped water and sewer access are conspicuously understudied. There is now a large active literature evaluating various improvements to transportation infrastructure, both in the developed and developing world. Electricity generation and distribution has also received attention. The literature on piped water and sewer access is much less developed. Indeed, as a result of divergent estimates in Alsan and Goldin (2019) and Anderson et al. (2018), recent research has increased our uncertainty about the importance of public health policy. In this light, our results are doubly important. We are the first to evaluate the effect of piped water and sewer access on land prices, a comprehensive revealed preference measure of value, and we provide new evidence in support of a high value for piped water and sewer access.

This generally supports a high priority for water and sewer infrastructure. It also highlights the importance of further research on the issue. Both infant mortality rates and the benefits of water treatment appear to be of about the same magnitude in late 19th-century U.S. cities as in parts of the modern developing world. However, the

disease environment in modern Latin American and African cities is different than it was in 19th century Chicago (see Henderson and Turner (2020)), so studies conducted in developing world cities are desirable.

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

Finally, we propose a technique for the principled extrapolation of treatment effects from a quasi-experimental study area to an area that is more relevant for economic analysis. The practice of restricting attention to small populations or areas, carefully chosen so that a quasi-experimental research design may be defended, is pervasive in applied micro-economic analyses. Thus, so too 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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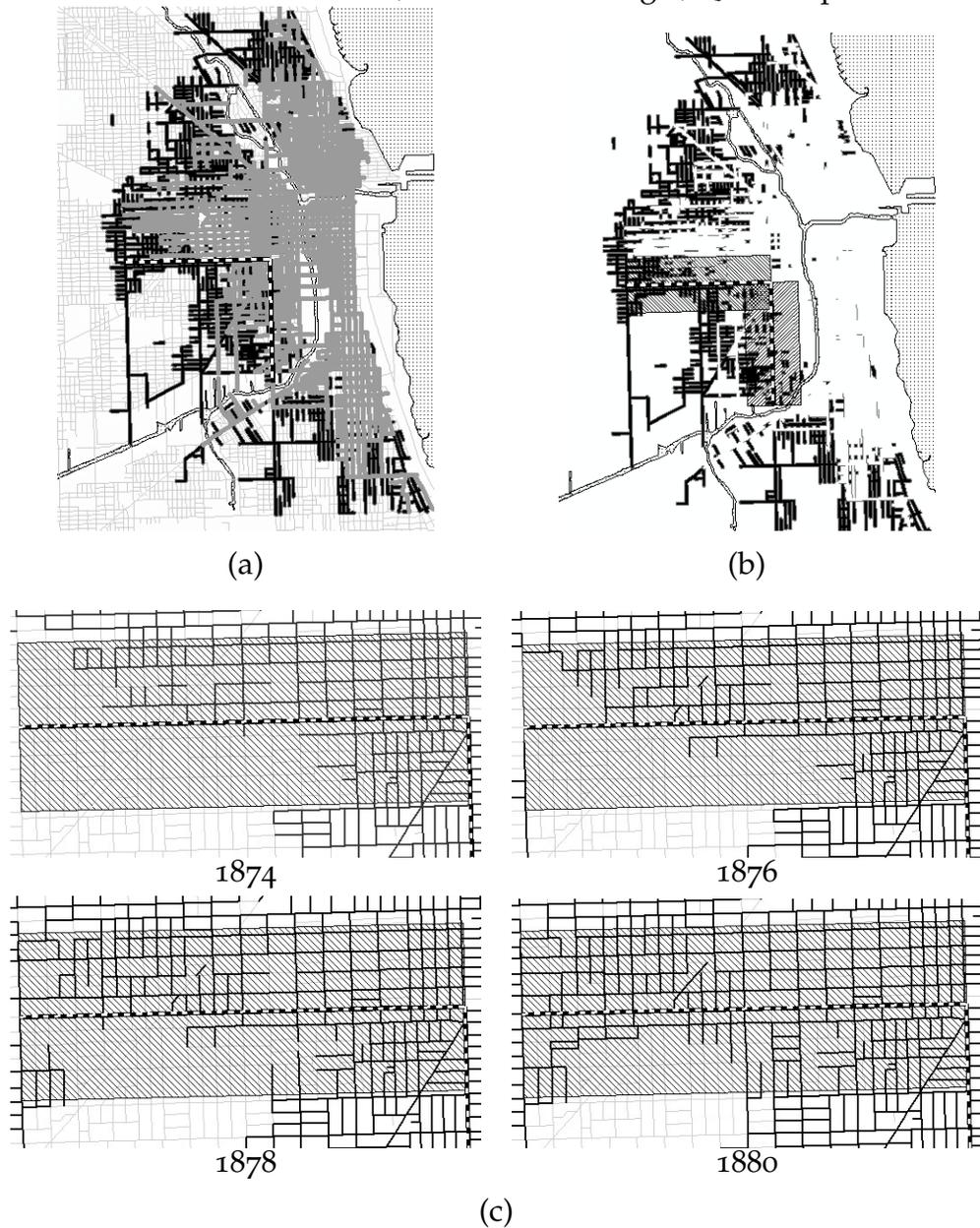
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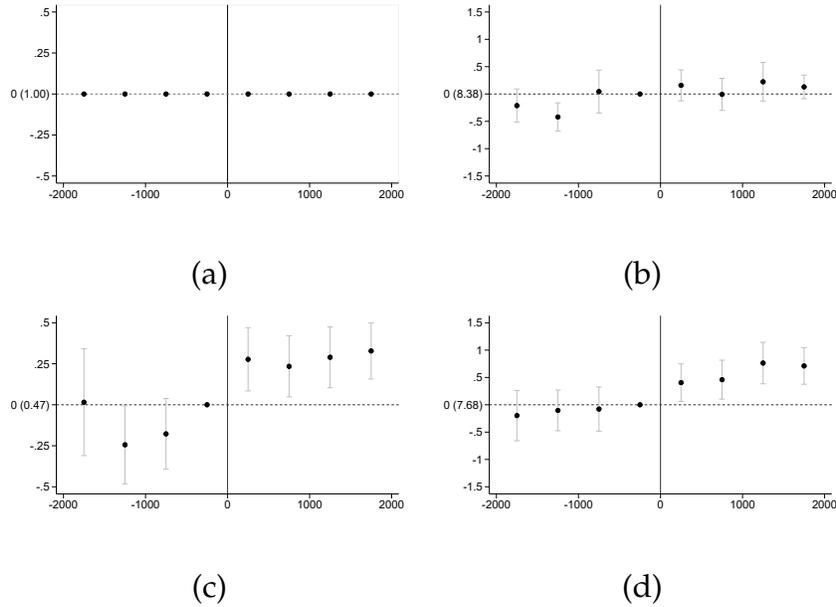
Figure 1: Water and sewer network, Southwest Triangle, Quasi-experimental samples



Note: (a) Sewers pre-1874, 1874-1880, post-1880, and boundaries of the Southwest Triangle.

(b) Relevant and Quasi-experimental sample areas. (c) Light gray is the 1930s streets. The dashed line is the boundary of the Southwest Triangle. The Quasi-experimental (hatched) area is the area within 2000 feet of Congress Street running west from Halsted Street for two miles to Western Avenue. Black lines indicate the sewer network. There is more sewer coverage in the northern than southern half of our study area during the 1874-1880 study period.

Figure 2: Sewer and water share and price by distance to Congress St.



Note: In each panel, x -axis is distance to Congress Street boundary, with $x < 0$ displacement south, “inside” and conversely. Top (bottom) panels are based on transactions during 1886-1889 (1874-1880). In the left two panels y -axis is share of transactions sewered between 1874-1880 by 500 foot bins (controlling for year indicators, $\ln(\text{Area})$, and $\ln(\text{mi. to CBD})$). The y -axis in the right two panels is $\ln(\text{Price})$, controlling for the same covariates. The second label for the y -origin gives the value for the reference ($[-500\text{m},0]$) bin. During 1874-1880, the incidence of sewer access and prices are both higher to the north. During 1886-1889, the incidence of sewer access and prices are the same on both sides of Congress Street.

Table 1: OLS, Reduced Form, First Stage, and TSLs estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: OLS.								
Sewer=1	.35*** (.117) [.089]	.41*** (.104) [.086]	.41*** (.107) [.087]	.38** (.181) [.144]	-.08 (.125) [.105]	.27*** (.084) [.1]	.28*** (.101) [.081]	.25*** (.098) [.079]
R^2	0.03	0.39	0.41	0.44	0.54	0.39	0.38	0.46
B: Red. Form								
SW Δ = 0	.73*** (.117) [.088]	.66*** (.094) [.072]	.80*** (.094) [.077]	.54*** (.128) [.106]	.34* (.202) [.173]	.46*** (.072) [.094]	.34*** (.087) [.063]	.32*** (.081) [.059]
R^2	0.17	0.49	0.53	0.50	0.54	0.45	0.40	0.48
R^2 Restricted Model		0.42	0.46	0.43	0.48	0.38	0.34	0.43
C. 1 st Stage								
SW Δ = 0	.45*** (.069) [.045]	.43*** (.062) [.039]	.44*** (.062) [.042]	.32*** (.072) [.056]	.19 (.126) [.097]	.43*** (.062) [.039]	.26*** (.049) [.031]	.26*** (.048) [.031]
R^2	0.25	0.45	0.45	0.45	0.47	0.45	0.33	0.33
R^2 Restricted Model		0.41	0.41	0.36	0.43	0.41	0.28	0.29
F-stat	42.44	49.09	49.74	19.63	2.22	49.09	28.27	28.82
D. IV.								
Sewer=1	1.63*** (.342) [.265]	1.52*** (.292) [.22]	1.83*** (.324) [.244]	1.70*** (.484) [.425]	1.80 (1.571) [1.323]	1.058*** (.254) [.195]	1.30*** (.369) [.277]	1.24*** (.343) [.263]
Year FE & ln(Area)		Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)		Y	Y	Y	Y	Y	Y	Y
H.car & Maj. St.& Corner			Y		Y			Y
Distance to Congress St.					Y			
Distance to Boundary Control					Y			
1886-1889 Trend Correction						Y		
Sample	Q.E.	Q.E.	Q.E.	Q.E. 1k'	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Intersections	129	129	129	71	129	129	224	224
Observations	351	351	351	172	351	351	533	533

Note: Results for transactions from 1874-1880. (1-3, 5) Quasi-experimental sample; (7,8) Extended-quasi-experimental sample; (4) Quasi-experimental sample within 1,000 feet of Congress Street. (A) OLS regressions of log transaction price on the treatment indicator. (B) Reduced-form regressions of log transaction price on the instrument. (C) First-stage regressions of treatment on instrument. (D) TSLs estimate of the effect of water and sewer access on log parcel price. The bottom panel indicates controls. Standard errors clustered by intersection in parentheses (). *, **, *** indicate 10%, 5%, 1% significance. Robust errors in brackets [].

Table 2: LIV Regression Test Statistics

	(1)	(2)	(3)	(4)
χ^2	220	235	243	251
H0: $\delta_1 - \delta_0, \gamma_1, \gamma_2, \gamma_3 = 0$	0	0	.005	.000
H0: $\delta_1 - \delta_0 = 0$.108	.141	.298	.0002
H0: $\gamma_2, \gamma_3 = 0$.002	.002	.656	.056
H0: $\delta_1 - \delta_0, \gamma_2, \gamma_3 = 0$.001	.001	.15	.000
ATE	1.04***	1.00***	1.31*	1.41**
	(.40)	(.36)	(.69)	(.70)
ATE*	1.04***	1.10***	1.05**	1.04**
	(.31)	(.40)	(.46)	(.47)
Carr & Kitagawa	.286	.252	.866	.374
Year FE & ln(Area)	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y
H.car & Maj. St.& Corner		Y		Y
Sample	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Observations	351	351	533	533

Note: Various test statistics based on estimates of the LIV model of equation (3) and estimates of ATE and ATE* based on equations (5) and (8). Complete report of coefficient estimates is in Table B5. All estimations based on transactions during 1874-1880 period. Cols. 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.

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

Online Appendices

Appendix A Data construction and description

Transaction data: We digitize the entire set of house and land transactions reported in every Sunday *Tribune* starting in October 1873 when reporting began, and ending in April 1889 when the *Tribune* stopped reporting transactions below \$1,000 in order to limit the size of the column.

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

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

Figure A1 shows a few sample transaction listings. Because we have just three months of data in 1873, and because transaction volumes were low over these three months, we begin our analysis in 1874, our first complete year of transaction data.

The *Tribune* reports both vacant parcels and parcels with a house. Parcels with a house are denoted by an address, or "Premises Number," and are easily distinguished from transactions without a house. About 97% of the transactions reported in the *Tribune* are land transactions.

To geocode our transactions we first attempt to match the "nearest intersection" reported by the *Tribune* to an intersection in the contemporary street grid described or to an intersection in a circa 1880 street map (Logan et al., 2011). The location of vacant parcels is given by an intersection, that of the street the parcel fronts and the nearest

Figure A1: Land transactions in the *Chicago Tribune*

SATURDAY'S TRANSFERS.	
The following instruments were filed for record Saturday, April 10:	
CITY PROPERTY.	
Walnut st, 120 ft e of Western av, s f, 30x 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 ½ of 24x126 ft, dated April 6 (Mary J. Seymour to C. L. Wehe).....	2,400

Note: Land transactions in the *Chicago Tribune*. Our land transaction data results from digitizing all transactions filed on Saturdays between 1873 and 1889. Each record reports the nearest intersection, price, area, and if the parcel is on a corner cross-street. The intersection is a useful georeference. While street names are not permanent, they are persistent, and several digitized maps exist recording the street names as they existed in the late 1800s. For reference, Figure A2 reports street names in an area around our Quasi-experimental study area from the Urban Transitions project (Logan et al., 2011). The *Tribune's* reporting of intersections, together with the persistence of street names and the availability of digital street maps, motivates our strategy for geocoding land transactions by matching them to the nearest intersection.

House transactions report a regular street address rather than the nearest intersection. Geocoding these addresses is not feasible for two reasons. First, the city of Chicago renumbered all of its house addresses in 1909. Second, this renumbering was motivated by the prevailing disorder of street numbering: "Prior to the 1909 street renumbering, Chicago street numbers were chaotic.... Critics often complained that the city's street numbers were without system" (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 6,044 land transactions between 1874 and 1880. Of these, we successfully geocode 4,616 (the observation counts in Table A1 are smaller because some are missing depth or frontage information). Figure A1 illustrates the distribution of these transactions across the intersections in the whole city (a) and in the area around our Quasi-experimental area (b). Each transaction is represented by a circle. Since transactions are matched to intersections, many intersections match to many transactions, and a darker circle on an intersection indicates that more transactions match to that intersection. We record transactions all over the city. Panel (a) gives a sense for the magnitude of this data collection effort. Panel (b) shows that transactions are distributed fairly uniformly in our Quasi-experimental area, although this is not

Table A1: Comparison of geocoded and ungeocoded parcels, 1874-1880

	Ungeocoded	Geocoded	T-test
Price	2962.51 (6057.77)	4063.03 (9549.75)	-4.09
Year	1877.70 (2.18)	1877.71 (2.20)	-0.22
Frontage	32.63 (16.50)	31.95 (15.13)	1.45
Depth	123.67 (42.82)	121.43 (28.85)	2.17
Observations	1428	4323	

Note: This table compares the characteristics of parcels from the full sample that could be geocoded versus those that could 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 courthouse. 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.

Our Quasi-experimental sample is a set of 351 transactions occurring between 1874-1880 within 2000 feet of Congress Street, west of Halsted. Gray squares in Figure B2(a) report mean log transaction price by year (after controlling for corner status, log of parcel area, and log miles to the CBD) for all transactions falling in the Quasi-experimental region at any time between 1873 and 1889. Black points show the corresponding prices calculated for the entire city of Chicago. Whiskers indicate 95% confidence intervals.

Figure A2: Map of study area with street names



Note: Illustration of street map with street names in the Quasi-experimental area from Logan et al. (2011).

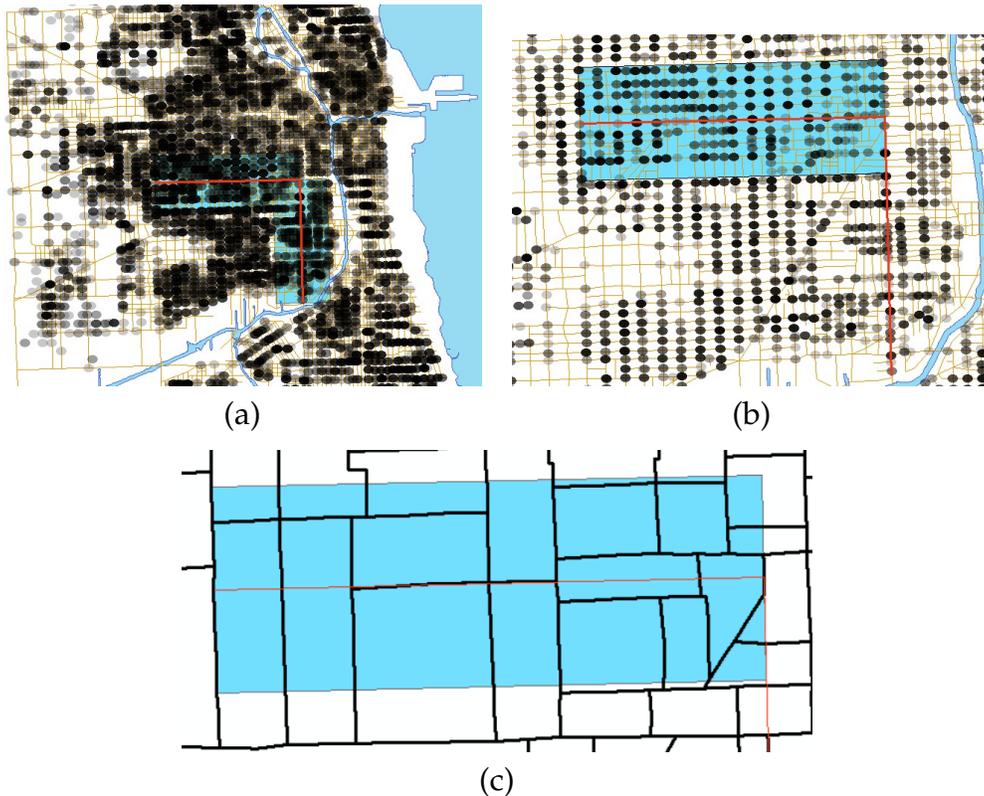
This figure shows the same basic patterns described in Hoyt (2000). Prices fell between 1873 and 1880 before beginning a slow recovery. Figure B2 also shows that prices in the Quasi-experimental region followed those in the city as a whole. That is, the Quasi-experimental region is a small part of a 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.

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

The 1880 census reports data aggregated to the level of the “enumeration district” (ED). 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 area, from

Figure A3: Map of Geocoded Parcels and 1880 Enumeration Districts



Note: (a) and (b) show geocoded transactions. A disk indicates an intersection to which we match a transaction. Darker disks indicate that we match more transactions to that intersection. (a) shows the entire city and (b) is a close up of the Quasi-experimental area (b). (c) shows 1880 Census enumeration districts overlaying the Quasi-experimental area Logan et al. (2011).

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 with professionals 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.

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

Note: Columns (1) and (2) contain demographic information for the Relevant and Quasi-experimental regions respectively. These values are constructed by interpolation of enumeration districts from the 1880 full count census. Col. (3) reports the full count demographics for the city of Chicago.

Appendix B Supplemental results

Tables B1 and B2 present summary statistics and are discussed in the main text. Table B3 is similar to Table 1, but is based on transactions occurring between 1886-1889 instead of 1874-1880. Table B4 shows main results using alternative specifications. We prefer to control for distance using $\ln(\text{mi. CBD})$ because it provides a more plausible basis for extrapolation to the Relevant area. Enumeration district-level population controls are not measured at a sufficiently fine level. There are only five EDs contained entirely within either the north or south sides of Congress street inside the experimental area, so these coarse controls rely heavily on areal interpolation. We also choose not to control for distance to river in our preferred specification, as it is almost entirely collinear with distance to CBD in the Quasi-experimental region, and there are exceptionally few parcels located in close proximity to the river. Table B5 provides a more exhaustive reporting of results summarized in the main text in Table 2. Figure B1 repeats Figure 2 on the larger Extended-quasi-experimental sample. Figure B2 presents comparisons of the Quasi-experimental and Relevant samples. Both are discussed in the main text.

Table B1: Summary statistics 1874-1880

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

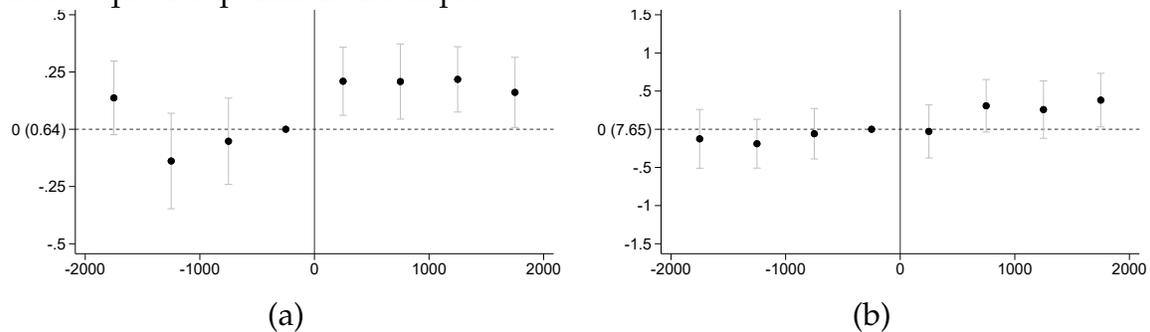
Note: Means and standard deviations of parcel characteristics for 1874-1880. Col. (1) reports on parcels in the Quasi-experimental sample (within 2,000 feet of Congress Street west of Halsted) that are in the Southwest Triangle (south of Congress Street). Col. (2) reports on parcels that are not in the Southwest Triangle (north of Congress Street). Col. (3) reports the *t*-statistic for the difference between the first two columns. Col. (4) presents parcel means and standard deviations for the Relevant sample.

Table B2: Summary Statistics 1886-1889, after piped water and sewer construction

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

Note: Means and standard deviations of parcel characteristics. Col. (1) reports on parcels in the Quasi-experimental sample (within 2,000 feet of Congress Street west of Halsted) that are in the Southwest Triangle (south of Congress Street). Col. (2) presents corresponding values for parcels that are not in the Southwest Triangle (i.e. north of Congress Street). Col. (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.

Figure B1: Sewer incidence and land price by distance to boundary, 1874-1880, for the Extended-quasi-experimental sample



Note: (a) Share of parcels sewered 1874-1880 by 500 foot bins of distance to SW Δ boundary, $x < 0$ is "inside." $x \in [-500,0]$ is *y* intercept. Conditional on year, $\ln(\text{area})$, $\ln(\text{mi. to CBD})$. (b) Same as left panel but *y*-axis is $\ln(\text{Price})$.

Table B3: Reduced form regressions after completion of piped water and sewer network

	(1)	(2)	(3)	(4)	(5)
Reduced Form					
SW $\Delta = 0$.174 (.131)	.115 (.130)	-.124 (.257)	.146 (.112)	.115 (.105)
Miles to Boundary			0.630 (.614)		
R^2	0.364	0.454	0.458	0.330	0.433
Year FE & ln(Area)	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y
Horsecar and Major Street		Y	Y		Y
Sample	Q.E.	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Intersections	101	101	101	132	132
Observations	143	143	143	213	213

Note: All results based on transactions during 1886-1889. Col. (1-3) rely on the Quasi-experimental area, (4) and (5) on the Extended-quasi-experimental area. Regressions are reduced-form regressions of log transaction price on the instrument and, in Col. (3), distance to Congress Street. The bottom panel of the table indicates control variables. Unlike during the 1874-1880 period, the entire Southwest Triangle had piped water and sewer access by 1886-1889 and the price differences across the Congress Street boundary are small economically and statistically. Robust standard errors in parentheses. *, **, *** indicate 10%, 5%, 1% significance.

Table B4: Main 2SLS Results, Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A: OLS.							
Sewer=1	.39***	.08	.29***	.24**	.43***	.41***	-.10***
R^2	(.101)	(.098)	(.120)	(.119)	(.106)	(.099)	(.031)
	0.45	0.51	0.44	0.42	0.42	0.40	0.38
B. Red. Form							
SW Δ = 0	.61***	.36***	.71***	.82***	.71***	.66***	.66***
R^2	(.089)	(.114)	(.136)	(.148)	(.095)	(.093)	(.094)
	0.53	0.53	0.51	0.51	0.53	0.50	0.49
C. 1 st Stage							
SW Δ = 0	.43***	.42***	.52***	.33***	.43***	.43***	-1.51***
R^2	(.066)	(.092)	(.073)	(.062)	(.062)	(.062)	(.286)
	0.47	0.46	0.46	0.46	0.47	0.45	0.40
D. IV.							
Sewer=1	1.42***	.84***	1.38***	2.5***	1.64***	1.52***	-.436***
F-stat	(.268)	(.327)	(.310)	(.576)	(.290)	(.290)	(.102)
	42.2	21.2	49.9	28.3	48.4	48.8	27.8
Sample Observations	EW 2k 351						
ln(Area)	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	.	Y	Y
Quarter by year FE	Y	.	.
Citywide Quarterly Price Index	Y	.
ln(mi. CBD)	.	Y	Y	Y	Y	Y	Y
Cubic mi. to CBD	Y
Corner	Y	Y	Y	Y	Y	Y	Y
ED % Foreign Born and Mean SES	.	Y
1900 ED % Foreign Born and Mean SES	.	.	Y
Near River Indicator	.	.	.	Y	.	.	.
Treatment=Years to Sewer	Y
Sample Intersections	Q.E. 129						
Observations	351	351	351	351	351	351	351

Note: This table presents the main 2SLS results with additional controls. Column (1) uses a cubic polynomial to control for distance to the CBD. Column (2) controls for ED-level demographics from the 1880 Census, Column (3) controls for demographics from the 1900 Census. Column (4) adds an indicator for parcels within 0.1 miles of the river. Column (5) replaces year fixed effects with quarter-by-year fixed effects. Column (6) controls for the citywide quarterly price index. Column (7) defines treatment as years until sewer service is received. Parcels north of Congress St. received sewers 1.5 years before parcels to the south (Column 7, first stage), and each year delay in receiving sewers reduced land value by .436 log points (Column 7, IV). Robust standard errors in parentheses clustered by intersection. *, **, *** indicate 10%, 5%, 1% significance.

Table B5: (a) LIV Regression Results

	(1)		(2)		(3)		(4)	
	1 st Stage	2 nd Stage						
Z	3.95***		5.41***		2.76***		3.32***	
	(.77)		(.94)		(.56)		(.84)	
ln(Area)	-.08	.72***	-.02	.66***	-.34	.72***	-.29	.62***
	(.48)	(.22)	(.56)	(.21)	(.30)	(.20)	(.34)	(.20)
1 (Year = 1875)	.56	.45**	.55	.35*	.21	.38*	.23	.34
	(.49)	(.20)	(.53)	(.19)	(.44)	(.23)	(.41)	(.23)
1 (Year = 1876)	.95	.39	.86	.30	.42	.35	.38	.23
	(.59)	(.26)	(.59)	(.28)	(.50)	(.32)	(.45)	(.31)
1 (Year = 1877)	1.41	.52	1.55	.51	1.00	.42	.94	.39
	(1.17)	(.36)	(1.16)	(.38)	(.78)	(.37)	(.75)	(.39)
1 (Year = 1878)	3.06***	.32	3.24***	.34	1.58**	.29	1.75**	.26
	(.82)	(.43)	(.77)	(.41)	(.79)	(.5)	(.79)	(.47)
1 (Year = 1879)	2.45***	-.08	2.63***	0.00	1.15	-.38	1.27	-.44
	(.88)	(.49)	(.85)	(.51)	(.86)	(.58)	(.88)	(.59)
1 (Year = 1880)	3.65***	-.63	3.89***	-.83	2.72***	-1.54	2.67***	-1.36*
	(.79)	(.63)	(.75)	(.66)	(.61)	(.94)	(.58)	(.81)
ln(mi. CBD)	-5.83***	.31	-8.19***	.24	-5.41***	.85	-5.75***	.49
	(1.58)	(.64)	(1.40)	(.62)	(1.13)	(.79)	(1.20)	(.78)
ln(to Horsecar)			9.39**	.15			3.63	1.53*
			(4.39)	(.88)			(2.93)	(.80)
ln(to Major Street)			-3.09	1.11			-1.18	2.69**
			(3.10)	(1.08)			(2.80)	(1.34)
1 (Corner)			-.54	.43			-.03	.39
			(.74)	(.29)			(.49)	(.35)
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y	Y	Y
Horsecar and Major Street, Corner			Y	Y			Y	Y
Sample	Q.E.	Q.E.	Q.E.	Q.E.	E.Q.E.	E.Q.E.	E.Q.E.	E.Q.E.
Intersections	129	129	129	129	224	224	224	224
Observations	351	351	351	351	533	533	533	533

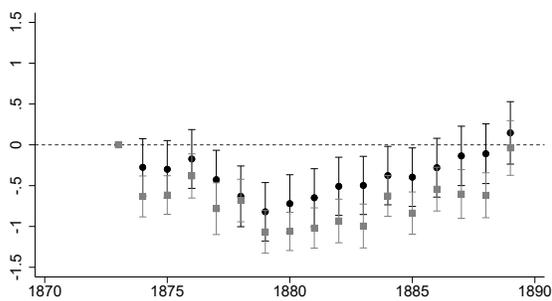
Note: (Table continued next page)

Table B5: (b) LIV Regression Results

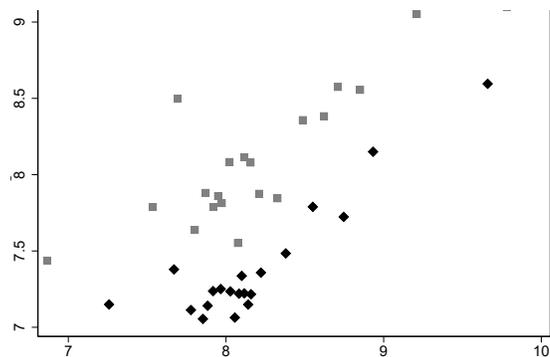
	(1)		(2)		(3)		(4)	
	1 st Stage	2 nd Stage						
\hat{p}		.74 (2.84)		.86 (2.83)		2.39 (2.91)		5.08* (3.07)
\hat{p}^2		-3.56 (4.83)		-3.38 (4.37)		-.94 (4.51)		-7.23 (4.57)
\hat{p}^3		3.81 (3.03)		3.65 (2.73)		1.05 (2.72)		5.14* (2.67)
$\hat{p}\ln(\text{Area})$		-.10 (.23)		-.08 (.23)		.09 (.23)		.02 (.22)
$\hat{p}1(\text{Year} = 1875)$		-.97*** (.33)		-.80*** (.30)		-.66* (.37)		-.67** (.34)
$\hat{p}1(\text{Year} = 1876)$		-.64* (.39)		-.43 (.38)		-.35 (.46)		-.28 (.42)
$\hat{p}1(\text{Year} = 1877)$		-1.40*** (.54)		-1.30*** (.51)		-.93* (.50)		-.94* (.52)
$\hat{p}1(\text{Year} = 1878)$		-1.24** (.54)		-.98** (.49)		-1.04* (.60)		-1.00* (.55)
$\hat{p}1(\text{Year} = 1879)$		-1.09* (.59)		-1.00* (.59)		-.36 (.67)		-.32 (.67)
$\hat{p}1(\text{Year} = 1880)$		-.51 (.72)		-.10 (.70)		.78 (1.01)		.62 (.87)
$\hat{p}\ln(\text{mi. CBD})$		-.11 (.68)		-.06 (.67)		-.57 (.85)		-.11 (.82)
$\hat{p}\ln(\text{to Horsecar})$.19 (1.42)				-3.34*** (.86)
$\hat{p}\ln(\text{to Major Street})$				-2.34 (1.46)				-4.47*** (1.59)
$\hat{p}1(\text{Corner})$				-.03 (.37)				.02 (.40)
Year FE & $\ln(\text{Area})$	Y	Y	Y	Y	Y	Y	Y	Y
$\ln(\text{mi. CBD})$	Y	Y	Y	Y	Y	Y	Y	Y
Horsecar, Major Street, Corner			Y	Y			Y	Y
Sample		Q.E.		Q.E.		E.Q.E.		E.Q.E.
Intersections	129	129	129	129	224	224	224	224
Observations		351		351		533		533

Note: Estimates of the LIV model of equation (3). Column headings indicate Logit first-stage coefficients and corresponding second stages so that the table reports two columns per specification. Specifications and samples match those reported in the same columns of Table 2. The bottom panel indicates controls for the regression above. Bootstrapped standard errors in parentheses. *, **, *** indicate 10%, 5%, 1% significance.

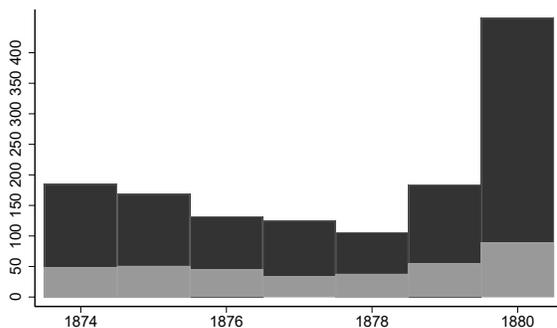
Figure B2: Comparison of quasi-experimental and relevant sample



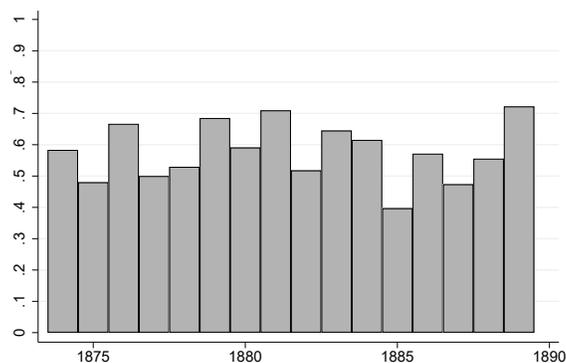
(a) Prices by year and sample



(b) Prices by parcel area



(c) Transaction frequency by year and sample



(d) Transaction Share north of Congress in QE

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

Appendix C Discussion of instrument validity

Because the area south of Congress Street is lower than the area to the north, if imperceptibly so, it may be “swampier” than the area to the north of Congress Street. Figure C1(a) is an excerpt from a map describing land cover in the Chicago area around 1840 (Illinois Department of Natural Resources, August, 2003) with our Quasi-experimental study area highlighted. This map aggregates information about Illinois land cover from federal and private survey maps and supporting documentation from the early 19th century. This map describes many landscape features that are small relative to our study area, and distinguishes between eight classes of “swamp” land cover: bayou, low land, marsh, slough, slash, swamp, wet land, and wet prairie. In spite of this, only a single land cover classification appears in our study area, “prairie.” Related to this, although Chesbrough’s plan called for what are now known as “combined sewers” that handle both household sewerage and run-off, nothing in the plan suggested that the system was intended to drain swamps, or that it had the capacity do so. This buttresses what we learn from Figure C1(a): there is no need to drain swamps where none exist.

Figure C1(b) shows Chicago in 1853, just before the publication of the 1855 plan (Kurz & Allison, 1974). We draw the approximate boundaries of the Quasi-experimental study area on the upper left corner of the image and see that it is uniform, undeveloped prairie. Maps in Hoyt (2000) for 1857 and 1873 confirm that the entire Extended-quasi-experimental area was undeveloped in 1857. The Quasi-experimental sample, along Congress Street, remained undeveloped in 1873, although much of the area around Halsted Street was developed at this time. When Chesbrough described the Southwest Triangle in 1855, it lay in a uniform, undeveloped, dry prairie, and the Quasi-experimental area remained undeveloped until the beginning of our study period.

Finally, Panel (c) shows our Quasi-experimental study area on a 1901 USGS topographical map (U.S. Geological Survey, 1901). The entire two-mile length of the Quasi-experimental area fits easily in the region between the 595 foot contour line, highlighted on the right, and the 600 foot contour, highlighted on the left. Thus, the entire two-mile east-to-west extent of this region involves strictly less than five feet of rise, a grade of less than 1:1,056. Looking north-to-south, we see similar contour spacing, and hence grade. This map reports topography in 1901, after the regrading associated with sewer construction had occurred. Because regrading generally increases the grade of a city, grades in this map are likely steeper than for the pre-sewer

topography. In all, the topography of our study area was remarkably uniform.

We are concerned that transactions in different years, and hence different parts of the business cycle, may have different rates of treatment and be systematically different in unobservable ways that confound our estimates. Figure B2(c) reports the annual count of transactions in the Quasi-experimental and Relevant samples. Except for a spike in 1880 that reflects increased sampling effort, transactions in the Quasi-experimental sample are about constant across years during 1874-1880. Figure B2(d) reports the share of Quasi-experimental sample transactions that are south of Congress Street. This share is also about constant over our 1874-1880 study period. While the the business cycle did affect the level of prices, neither the number of transactions nor their distribution north and south of Congress street shows any obvious relationship to the business cycle. This does not suggest that variation related to the business cycle is a threat to our identification strategy. We present further evidence on this issue below.

It is possible that other municipal services were bundled with water and sewer provision and, if so, this would require a reinterpretation of our results. This does not appear to have been the case. Anderson et al. (2018) date other public health interventions in Chicago: chlorination of municipal water supply, 1912-1917; milk purity standard and tuberculin testing of cows, 1909. Per Ferrie and Troesken (2008), the two-mile crib and associated development occurred around 1870. The four-mile crib was finished in 1893. The complete reversal of the Chicago River was in 1900. None of these interventions intersects our 1874-1880 study period.

To investigate the possibility of bundled municipal services more comprehensively, we digitize municipal budgets between 1872 and 1882.¹⁷ Municipal expenditures during our study period generally follow the business cycle. During our main 1874-1880 study period, waterworks, including pumping stations, were typically the largest single category of expenditure, with sewer construction second. Sewer operation and maintenance costs were stable and relatively small. Taken together, water and sewer were by far the single largest category of municipal expenditure. Other important categories were police, schools, public works and the health department.

Within 'Public works' spending, the largest categories of expenditure appear to have been bridges and viaducts. It is hard to see how this could have different effects north and south of Congress Street. This, in turn, makes it difficult to argue that bridge or viaduct spending could account for observed patterns in real estate prices.

¹⁷These budgets were taken from the Annual Reports of the Comptroller of the City of Chicago, accessed through HathiTrust at <https://catalog.hathitrust.org/Record/100557907>.

As a rule, the municipality provided schools, police, and fire protection to an area immediately after its annexation. Our study area is well within the city's 1870 boundaries (Keating, 2004) and there was no large annexation of territory until 1886. No part of our study area changed its eligibility for municipal services during our study period.

To explain the large north-south difference in land price during our study period, and its later disappearance, as functions of municipal expenditure unrelated to water and sewer service, we require that schools, police, or fire protection were differentially supplied across Congress Street during 1874-1880, and then equally supplied during 1886-1890. This seems *a priori* unlikely. Moreover, annual expenditures on schools, police, and fire protection were only two or three dollars per person, while our estimates will suggest that households were willing to pay much more for water and sewer access. To explain our results would require a dramatically unequal distribution of schools, police, and fire protection across Congress Street during 1874-1880, followed by a dramatic geographic equalization from 1886-1890. We can find no mention of such a massive reallocation of public services in the historical record.

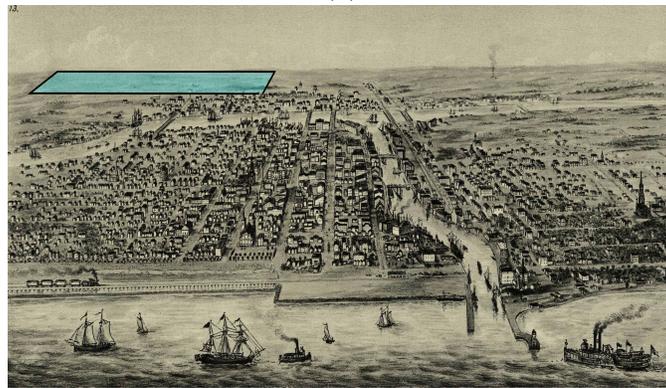
It is also possible that sewer access served in part to improve the management of industrial waste or to shift land use towards industrial activity. If true, this would also require a reinterpretation of our estimates. This does not seem to have been the case. Although no spatially disaggregated industrial census exists during this time period, Industrial World Company (1886) maps the locations of rail lines and large factories in Chicago around 1886. This map shows that the level of industrial investment in our study area was uniformly low in our Quasi-experimental study area, both north and south of Congress Street.

Further evidence comes from Shertzer et al. (2018), who digitize the 1922 Chicago land use survey describing industrial facilities and their locations. There are 34 industrial facilities in our Quasi-experimental study area in 1922. Of these, 19 are north of Congress Street, and 15 south. If industrial facilities were assigned to the north and south at random with equal probabilities, then the probability of drawing a distribution in which 19 of 34 facilities are north of Congress is about 0.3. These data do not support the hypothesis that sewers affect land prices by improving the management of industrial waste or promoting industrial land use.

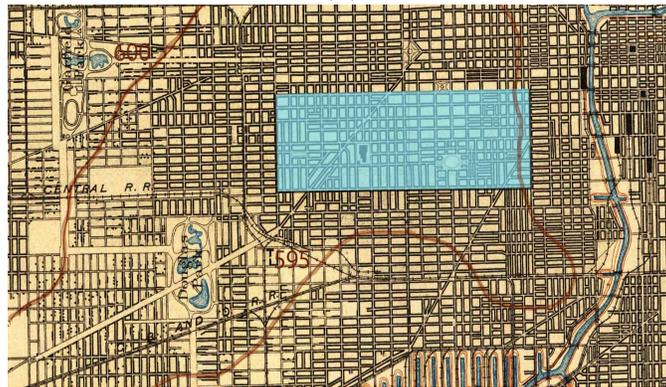
Figure C1: Study area landcover ca. 1840 and 1852 and topography in 1901



(a)



(b)



(c)

Note: (a) From Illinois Department of Natural Resources (August, 2003), showing the Quasi-experimental study area uniformly in “prairie” landcover classification. (b) An 1853 drawing of Chicago with the approximate boundaries of the Quasi-experimental area highlighted. This area is completely undeveloped just two years before the 1855 plan. (c) 1901 USGS topographic map with study area and the 595 foot and 600 foot contours highlighted. Halsted Street closely follows the 595 foot contour line in our study area, i.e., it is flat, while the entire two-mile length fits between the 595 and 600 foot contours, so that east-west elevation gain is less than 5 feet in 1901.

Appendix D Externalities

The value of water and sewer access likely has both a private and an external component. Most likely, a parcel is more valuable if it has water and sewer access and if nearby parcels have water and sewer access. The relative magnitude of the two components has important implications for policy. As the external share of benefits increases, the case for subsidies or public provision is stronger. So far, we have focused on the effect of a binary treatment: a parcel is “treated” when sewer and water pipes are installed through the nearest intersection. This implicitly aggregates private benefits and external benefits that operate on the scale of a city block. We here attempt to disentangle the two effects.

We first ask whether the value of a parcel varies with the share of neighboring areas with water and sewer access, holding its own access constant. Define d_i^{200} (d_i^{400}) as the share of sewered intersections within 200 (400) yards of transaction i . To begin, define the corresponding instruments z_i^{200} and z_i^{400} as the areal share of the relevant disk that is north of Congress Street. We can then conduct OLS and TSLS/IV regressions similar to those reported in Table 1, up to the change in the definition of the treatment and instrument. The mean and variance of d_i^{200} and d_i^{400} are 0.88(0.06) and 0.85(0.04), respectively, and these treatment variables are highly correlated ($R^2=0.84$).

Columns 1-4 of Table D1 report OLS and TSLS regressions of log transaction price on d_i^{200} and d_i^{400} . Like the results reported in Table 1, the TSLS/IV coefficient on treatment is larger than OLS. Second, the coefficients on d_i^{200} and d_i^{400} are large relative the effect of sewer access in the nearest intersection. These larger estimates appear to be a mechanical consequence of the smaller variances of d_i^{200} and d_i^{400} . Finally, the effects of d_i^{200} and d_i^{400} on transaction prices are close and statistically indistinguishable.

This final result seems surprising. To the extent that water and sewer access in one place had external effects on another, one expects this impact to decay with distance, and hence for the effect d_i^{200} to be larger than d_i^{400} . Sampling error aside, we see two resolutions of this puzzle. First, the external effects of sewers are important and operate over scales that are large compared to the 200-400 yard scale of our analysis. Second, the external effects of sewers are close to zero, and therefore do not decay with distance. Column 5 includes both d_i^{200} and d_i^{400} as treatments and uses both z_i^{200} and z_i^{400} in an attempt to parse the effects of the two treatments. That this regression is obviously uninformative suggests that we not discount the role of sampling error.

Column 8 also includes two treatments, this time an OLS regression of the log of transaction price on our main treatment, sewer access in nearest intersection, and d_i^{200} .

Table D1: External effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	IV	OLS	IV	IV	IV (Sewer=1)	OLS
d_i^{200}	0.00601*** (0.00192)	0.0233*** (0.00586)			-0.471 (1.097)	0.106*** (0.0260)	0.00263 (0.00224)
d_i^{400}			0.0107*** (0.00284)	0.0272*** (0.00532)	0.507 (1.104)		
Sewer=1							0.314** (0.128)
R^2	0.377		0.401				0.389
Intersections	129	129	129	129	129	108	129
Observations	351	351	351	351	351	258	351

Note: Standard errors clustered by intersection in parentheses. *, **, *** indicate 10%, 5%, 1% significance.

Somewhat surprisingly, we see that only sewers in the nearest intersection matter, and the coefficient on this variable is practically unchanged from the corresponding column in Table 1. This suggests that d_i^{200} and d_i^{400} are important primarily for their ability to predict whether a transaction has water and sewer service, not because they measure the intensity of external effects. Note that the unreported TSLS/IV specification of Column 8 is as uninformative as column 5.

Finally, columns 6 and 7 repeat the TSLS/IV regression of column 2, but partition the sample according to whether the nearest intersection has water and sewer access. These results are striking. Nearby water and sewer access has a tiny effect on prices for parcels that themselves lack water and sewer access (column 5), but a large effect for those with water and sewer access.

These estimations suggest the following. First, the value of water and sewer is primarily private or external effects operate over a scale of a single city block. Second, to the extent that there are external effects operating over greater distances, they are asymmetric. Neighborhoods with sewers are harmed by nearby unsewered neighborhoods, but neighborhoods without sewers are not.

Appendix E 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 the propensity score

$p = F(x,z) = P(D = 1|X = x, Z = z)$ introduced in the main text and the normalized unobserved heterogeneity in the selection process, $\tilde{U}_D \sim Unif[0,1]$, the selection equation can be represented as

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

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

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

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

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

1. The equations of potential outcomes and selection given in (1) are identical between the Quasi-experimental and Relevant samples (other than that Z^* is not observed in P^*). Furthermore, the distributions of (U_1, U_0, U_D) and (U_1^*, U_0^*, U_D^*) are common.
2. The joint distribution of observable covariates X and cost shifter (instrument) Z in the Quasi-experimental sample and the joint distribution of X^* and Z^* in the Relevant sample can be different.

Under (EX1), we can normalize U_D^* of (6) to define the uniform random variable $\tilde{U}_D^* = F_{U_D^*}(U_D^*)$ such that for \tilde{U}_D defined in (Appendix E.1), $\tilde{U}_D^* = \tilde{U}_D$ is equivalent to $U_D^* = U_D$. In other words, a unit in the Relevant sample and a unit in the Quasi-experimental sample that share the values of \tilde{U}_D^* and \tilde{U}_D have identical unobservables in the selection equation. Assumption EX1 also implies that the control function term $K(\cdot)$ in the LIV regression (3) is common between the two samples,

because the control function term is determined only by the distribution of $(U_1, U_0)|U_D$ and this does not vary between the two samples. As a result, for MTE in the Relevant sample $\text{MTE}^*(X^*, \tilde{U}_D^*)$, $\text{MTE}(X, \tilde{U}_D) = \text{MTE}^*(X, \tilde{U}_D^*)$ holds whenever $X = X^*$ and $\tilde{U}_D = \tilde{U}_D^*$ hold. We hence obtain

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

Taking the expectation with respect to X^* and $\tilde{U}_D^* \sim \text{Unif}[0,1]$, we obtain equation (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^*) .