ARTICLE IN PRESS
JID: YJUEC [m5G;April 14, 2016;16:52]
Journal of Urban Economics 000 (2016) 1–13
Contents lists available at ScienceDirect
Journal of Urban Economics
journal homepage: www.elsevier.com/locate/jue

Sewers' diffusion and the decline of mortality: The case of Paris, 1880–1914☆

Lionel Kesztenbaum[a,b], Jean-Laurent Rosenthal[c,*]

[a] Institut National d'Etudes Démographiques (INED), 133 Boulevard Davout, F-75980 Paris Cedex 20, France
[b] Paris School of Economics (PSE), Paris, France
[c] Division of the Humanities and Social Sciences, California Institute of Technology, Pasadena, CA 91125 USA

ARTICLE INFO

Article history:
Received 8 July 2014
Revised 11 February 2016
Available online xxx

JEL classification:
I14
I15
N33
N93

Keywords:
Differential mortality
Wealth
Urbanization
Paris
Sanitation

ABSTRACT

It is common to argue that water infrastructure innovations improve life expectancy. Yet the benefits of clean water depend on a mechanism to dispose of waste water. We draw on the historical experience of a large industrial city to estimate the impact of the spread of the sewer system. Using a longitudinal data set on mortality and rents for each of Paris' 80 neighborhoods we show that sanitation contributed several years to life expectancy. These results point out the multiplicity of infrastructure needed to help decrease mortality.

1. Introduction

Despite recent progress, water-borne diseases—preeminently diarrhea—remain major killers in the developing world (Gawtkin and Guillot, 1999; WHO, 2014). In fact, the incomplete and unequal deployment of sanitation infrastructure designed to provide clean water remains an important problem in large parts of the world today (Banerjee and Duflo, 2007; Baisa et al., 2010; Günther et al., 2014) just as it was 100 years ago in Europe and North America. In cities of the developing world in particular, the diffusion of these technologies continues to be uneven (Galiani et al., 2009). Clearly, clean water saves lives (Fewtrell et al., 2005; Watson, 2006; Cairncross et al., 2010; Kremer et al., 2011). Yet the benefits of clean water alone are limited (Bennett, 2012). There are several reasons for this. The first is that health improvements diffuse slowly. Thus the short-term impact on mortality substantially understates the value of clean water infrastructure. Second, clean water's impact will be at best muted unless there are sewers to carry the waste water away. Without sewers, household members can be contaminated by contact with soiled water (Curtis et al., 2000; Aslan and Goldin, 2015). In this paper we will discuss both effects but focus mainly on the second one as we ask by how much sewage systems improve life expectancy.

We do so by taking advantage of excellent data that detail both mortality and access to water infrastructure for each of Paris' 80 neighborhoods from 1880 to 1914. In 1880 none of Paris' buildings had direct connections to the sewer. By then, however, two-thirds of all buildings were connected to the city's clean water network and the rest had access to free neighborhood taps (*fontaines*). In the absence of a direct connection to the sewers, buildings were equipped with a variety of systems that held or filtered soiled water and human waste. By 1913, 68% of all buildings in Paris had direct connections to the sewer. We establish the large and positive impact of sewers on mortality using within-year neighborhood-level variation in mortality and sewer connections.

Paris at the end of the nineteenth century, like many cities in the developing world today, was very unequal in wealth, income,

☆ The authors would like to thank Leah Brooks, David Canning, Jonathan Chapman, Tracy Dennison, Edward Glaeser, Timothy Guinnane, Philip Hoffman, Samantha Myers, Jim Oeppen, Paul Rhode, Paula Scott, William Summerhill and the participants at seminars at All-UC Huntington conference, INED, Michigan, PAA annual conference, PSE, Stanford, UCLA, Université Paris-Dauphine, University of Pennsylvania and Yale for helpful comments.
* Corresponding author.
E-mail addresses: lionel.kesztenbaum@ined.fr (L. Kesztenbaum), rosentha@caltech.edu (J.-L. Rosenthal).

http://dx.doi.org/10.1016/j.jue.2016.03.001
0094-1190/

Please cite this article as: L. Kesztenbaum, J.-L. Rosenthal, Sewers' diffusion and the decline of mortality: The case of Paris, 1880–1914, Journal of Urban Economics (2016), http://dx.doi.org/10.1016/j.jue.2016.03.001

and life expectancy. It was also residentially segregated by income. In this context, the fact that water infrastructure is an excludable local service and not a pure public good becomes central to any analysis. In particular, the rich have little interest in subsidizing access to water infrastructure for poor neighborhoods. In one common scenario, they use their political control to deploy water infrastructure based on user fees rather than public subsidies. At one extreme, the rich can even deprive the poor of access to the water infrastructure as a way to prevent them from coming in the city (Feler and Henderson, 2011). In turn, faced with significant fees, the poor opt to go without the benefits of this infrastructure (Devoto et al., 2012). Thus, an important part of the debate today involves who should pay for expanding infrastructure: users of water, landlords, or the rich more generally (Galiani et al., 2005). The same was true for Paris and other major cities at end of the 19th century. And it is no surprise that sewer connection rates were always higher in rich Parisian neighborhoods than in poor ones; it is also true that mortality fell faster in rich neighborhoods than in poor ones.

The Parisian experience we analyze here extends a large prior literature that examines the evolution of mortality at the city level. The decline of mortality between 1870 and 1914 was widespread in large cities across the North Atlantic economies (Costa and Kahn, 2015). The sharp reduction of mortality occurred in the U.S. (Cutler and Miller, 2005; Troesken, 1999), in Germany (Brown, 1989), and in the U.K. (Szreter, 1988) among other countries. This decline initially allowed urban mortality to reach parity with that of rural areas, which had long enjoyed a health advantage (Woods, 2003). Urban mortality then continued to fall, finally giving cities the life expectancy advantage over rural areas they currently enjoy (including in the developing world, see Bocquier et al., 2011).

One simple reason for this is that over time increasing incomes offset the environmental risks presented by large cities. In the early nineteenth century, cities had little infrastructure and high mortality (Cain and Hong, 2009). Brown (1988) shows that German cities with higher incomes were earlier adopters of water infrastructure. Overall, large scale improvements such as clean water contributed to the fall in urban mortality (Ferrie and Troesken, 2008). Most studies use variations across cities to estimate the benefits of water purification. For instance Cutler and Miller (2005) use variation in the timing of two clean water technologies – filtration and chlorination – to assess their causal influence on mortality decline. These technologies diffused quickly and fully within a city once adopted. In each case scholars examined the impact of these measures city-wide – in effect treating them as public goods.

These are informative approaches but they also have some limitations, the first being the elision of the huge variations that occurred *within* cities. In each of the major cities (New York, London, or Paris) mortality was both high and uneven (the inter-quartile range among districts ranges between 25% and 30% of the mean).[1] In most U.S. cities at the end of the nineteenth century (before the advent of large scale infrastructure to provide clean water) mortality in the worst areas was three to fourth times that in the best ones (Floud et al., 2011, pp. 328–329). Paris here presents an advantage because we can carry out the analysis at the neighborhood level. As we will show, the variation in take up of infrastructure across neighborhoods was large and closely connected with variations in mortality.

We also show that these results are robust to including average rent by neighborhood. We do so because we want to control for a variety of characteristics of neighborhoods that change over time. First, rents will obviously control for the average quality of housing (including the perceived value of sewer connections). But, as we discuss below, the expenditure share of housing is less than 30% and the income elasticity of housing expenditures is less than one; thus households living in a neighborhood with twice the average rent of another will have more than twice the income of their counterparts. The denizens of the higher rent neighborhoods will spend more—both in absolute and relative terms—on other aspects of consumption that enhance life expectancy. That the mortality reduction from increased sewer connection is robust to controlling for average rents only strengthen confidence in our results.

In the next section we review the dataset we assembled and make the case that it is critically important to control for income differences over time and across space within the city. In Section 3, we discuss the relationship between life expectancy and sewers and review the history of sewer diffusion in Paris. In Section 4, we provide our baseline statistical model and show that sewers increased life expectancy substantially. Section 5 includes robustness tests that confirm the findings of Section 4. In Section 6, we disaggregate life expectancy into mortality risk by sex and by age and show that sewers have similar benefits for women and men. These benefits are largest at early ages. In the last section, we conclude and examine the obstacles to the provision of excludable networks such as sewer systems in the developing world.

2. Paris as a laboratory

Nineteenth century Paris is an ideal laboratory for studying differential mortality: first, because administrative boundaries within the city have not changed since 1860; and second, because the municipal statistical office was staffed by individuals obsessed with collecting and publishing detailed demographic and infrastructure data. As a result, we have access to good data on mortality, rents, and sewers adoption by neighborhood.

The Paris statistical bureau's publications allow us to track the evolution of mortality between 1880 and 1913 for each of the 80 neighborhoods (*quartiers*) of the city.[2] On the demographic side, the statistical office published death totals by sex, broken down into six age categories for each year and neighborhood, and in 1880 it added a series of detailed population abstracts for the city drawn from the national censuses from 1881 to 1911. Taken together, these two datasets allow us to compute mortality rates and life expectancy at the neighborhood level (see Appendix B for details). Unfortunately we cannot compute infant mortality because until late in the nineteenth century middle- and lower-class Parisians very frequently sent newborns to wet nurses who lived some distance from the capital (Rollet-Echalier, 1982; Preston and van de Walle, 1974). Since scholars generally agree that the largest benefits from water infrastructure go to infants, our results clearly understate the benefits of sewers.

Although we do not have access to a panel data set for income at the neighborhood level, we do have excellent data on the distribution of rents across the city derived from real estate censuses for 1878, 1890, 1900, and 1910. For each neighborhood, the censuses distribute housing units in two dozen categories of rent levels, including two for those dwellings below the 300 franc threshold of the *taxe mobilière*. This was a direct tax assessed on the basis of occupation and of the rental value of the household's dwelling. The top category in 1890 comprised 521 dwellings, each assessed at more than 16,000 francs in rent.[3] Although these data provide

[1] London, New York and Paris all reported aggregate death rates by neighborhood (General Register Office, 1881–1901; US Census Office, 1894). Only in Paris are neighborhood boundaries fixed overtime.

[2] The city was divided into twenty administrative districts (*arrondissements)* that were each split into four neighborhoods (*quartiers*).

[3] With French per-capita income below 600 francs in 1890 (Lévy-Leboyer and Bourguignon, 1990), rent of 16,000 francs would correspond today to housing units

ample evidence of the correlation between rent and life expectancy, they are too infrequent for our purposes. To supplement the censuses, we collected neighborhood level fiscal data for every five years from 1876 to 1911 from the summary registers of the *taxe mobilière* in the archive of the finance ministry. These data include the number of households that paid a rent above 300 francs (the threshold at which they were liable for the tax) and the total rent they paid.

From the fiscal data set, we compute the average rent paid by households above the threshold, the average fiscal rent. It turns out to be a good statistic for average rent. The correlation between the average rent from any of our four real estate censuses and the average fiscal rent is never less than 0.97. Though truncated, the fiscal rent data are an effective statistic for average rent at the neighborhood level.

Rents are clearly the result of market transactions, and their variation reflects changes in supply and demand. On the supply side, there are two issues to consider. The first is the cost of creating additional units of housing. Here the advantage of Paris's small size is that the cost of creating a unit of housing was similar throughout the city because wages and the price of materials would not have varied across neighborhoods. Paris did expand, but not at all rapidly: there were 20% more buildings and 40% more dwellings in 1911 than in 1876, which translates to very slow growth (for instance dwellings grew at barely 1% a year). The second issue involves the supply of land, and here the specific history of Paris helps us: the boundaries of the city were fixed in 1860. Further, there was relatively little growth of suburban housing prior to WWI. Indeed people living in the metropolis preferred to live within the fortifications that encompassed Paris after 1860. As a result the supply of parcels of land was largely fixed. Thus, housing grew because of increases in density (building multistory units and subdividing large parcels of land into smaller ones). We can therefore take the supply of housing as fixed in the short run and only moderately elastic in the longer run.

On the demand side, the rent of a housing unit is the price that a family is willing to pay to live in that unit. It thus reflects the value of receiving a particular set of housing services (defined by the characteristics of the housing unit) at that location. For our purposes, rent will therefore be high in good apartments in good neighborhoods. In our case "good" would likely involve spacious apartments with running water in healthy neighborhoods. In a sorting equilibrium like that in Rosen (1974), higher income families will live in high quality neighborhoods and rents in such neighborhoods will tend to be higher. This suggests that higher neighborhood rent is a good proxy for perceived variation in neighborhood quality—including attributes that preserve life. In a cross section, we could therefore include rent in our regressions to take into account neighborhood quality, which might be correlated with sewer diffusion. Doing so, however, will bias downward any estimate of the impact of sewers if they too are capitalized in rents.

We also have to worry about the extent to which buildings were renovated or rebuilt when they were connected to sewers—and thus the extent to which neighborhood characteristics evolved over time. We have to keep in mind another important effect: as neighborhoods evolve, and in particular gentrify, rents and incomes go up together. Including rents in the panel analysis allows us to control for this effect as well, and we can place limits on it by considering the budget share and income elasticity of housing. In both historical and contemporary studies, the share of housing in the budget lies somewhere between 15% and 25% (Haines, 2015; Davis and Ortalo-Magné, 2011; Fahey et al., 2004). As for the income elasticity of housing, although the range of estimates is wide, they are always less than one, with historical estimates falling between 0.3 and 0.7 (Haines and Goodman, 1992). Taken together these findings imply that on average denizens of high rent districts have non-housing consumption that is proportionally much larger than that of individuals living in poor neighborhoods. As a result, the positive impact of higher rent is not due to the better characteristics of housing alone; it also stems from increases in other expenditures. This is yet another reason to include rent in our regressions. In short, the diffusion of sewers will be at least partly capitalized in rents, which means our findings on infrastructure will be biased downward.

Beyond the theoretical arguments above, we would like to compare rents and income across neighborhoods. Unfortunately, we do not have detailed income data for Paris before WWI, and there are no data on incomes by neighborhood. But we can estimate income for the mid-1890s at the district (*arrondissement*) level and compare it with rents. To do so we combine information on wealth from estate documents (for capital income), information on labor income from the industrial survey of 1896, and information on occupational distribution at the district level to produce a cross section of Parisian incomes (see Appendix C). The procedure involves some assumptions but whatever choices we make always produce a set of average incomes that are very strongly correlated with average rents. Indeed the correlation is at least 0.8, despite the fact that we had to omit the within-occupation wage variations. Overall, rents appear to be good statistic for income.

How variable were rents? The real estate census of 1878 provides a striking image of the city's inequality (Fig. 1). The wealthy (paying annual rents over 1000 francs) comprised less than 10% of households. The poor (who paid less than 300 francs rents) made up 68% of households. These different groups lived in different places and rents reflect these contrasts: rents in the Champs Élysées neighborhood averaged 3200 francs, nearly 20 times the 179 francs of the mean rent in Charonne. This difference in part reflects pure location rents. The high rent districts were clustered around the financial center (the Bourse) and its political counterpart (the Élysée). But this difference also reflects the massive differences in the quality of the housing units (the size of apartments, amenities like running water, toilets inside the apartment rather than in the hallway or on the ground floor, in air quality, etc.).

We also have information on access to clean water and how waste water was dealt with. By 1885 two-thirds of Parisian buildings were connected to the city's water supply (Cebron de Lisle, 1991, p. 547), and the vast majority of homes received pure (spring) water brought in by aqueduct (Deligny, 1883, Annexe no. 1, p. 49). After that date access to clean water was not an issue (Goubert, 1986, pp. 90–92; Bocquet et al., 2008). But the diffusion and increased use of clean water (for whatever purpose) worsened the problem of removing the soiled water.

As elsewhere in Europe or the U.S., the clean water likely did play a role in decreasing mortality, especially infant mortality (Preston and van de Walle, 1978), although we lack the data to analyze its impact within Paris. At the same time, improvements in water infrastructure may also have indirect effects, the so-called Mills–Reincke phenomenon (Ferrie and Troesken, 2008). As Preston and van de Walle (1978) show, the mortality decline in Paris featured strong cohort effects. This earlier study, however, cannot establish if the cohort effects were connected with water infrastructure, better nutrition, or any other factors. Here we narrow the focus to variation within Paris and concentrate our analysis on the 30 years at the turn of the century. It is the period when we can observe precisely variations in mortality, income (rents), and the water infrastructure. To do so, we take advantage of annual reports on the fraction of buildings that had a direct

with rentals values of 1 million dollars or more in the U.S. and 650,000 euros or more in France.

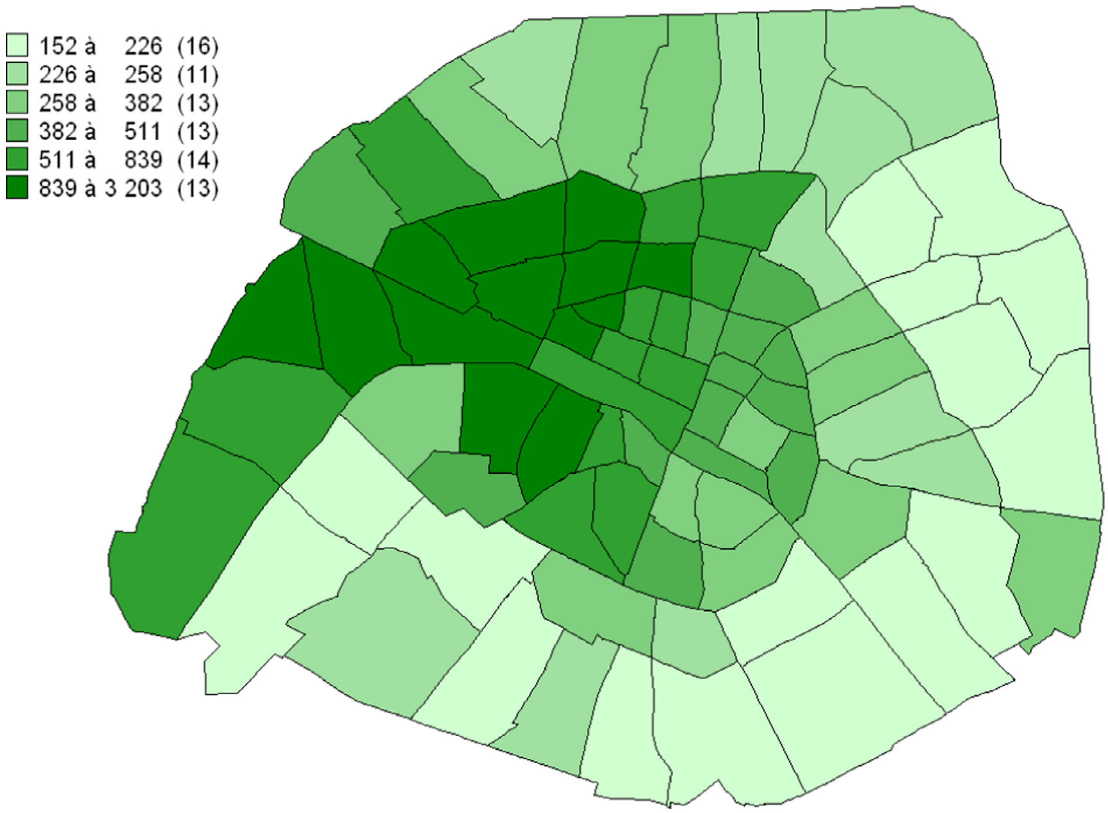

Fig. 1. Average rents by neighborhoods in Paris, 1878.

connection to the sewer by neighborhood. But our time frame precludes any cohort analysis.

The halcyon days of the statistical office ended abruptly in 1913. Afterwards, and despite a massive increase in the city's involvement in sanitation, it stopped preparing detailed reports on mortality. After WWI data were summarized only at the more aggregate level of districts; the city ceased publishing abstracts from the population censuses or any real estate information; and even the treasury stripped its internal reports of useful information. So we limit our analysis to the period before 1914; fortunately, that was the period when the most important improvements occurred.

3. The diffusion of sewers in time and space

Fig. 2 presents the average life expectancy at age 1 for Paris (the black line) and for France (the dotted black line).[4] The figure also shows the life expectancy for the worst eight (the red line) and the best eight (the dotted red line) neighborhoods in the capital. The variation within Paris dwarfs the difference between Paris and France. Individuals in the worst neighborhoods in Paris had a life span that was some seven years less than the city average and 10–15 years below that of the French people as a whole. By contrast, in the early 1880s life expectancy in the best neighborhoods was 13 years higher than the rest of the city and four years better than the rest of France. Over the next three decades life expectancy in Paris rose quickly for everyone and surpassed 52 years. The life expectancy deficit of Paris relative to the rest of France fell by half. Both the rise of life expectancy and the convergence towards French levels would continue in the interwar period. The increased longevity was one of the more widely distributed benefits of long-term economic growth (Birchenall, 2007; Peltzman, 2009; Becker et al., 2005; Soares, 2007). And although the timing was specific, the mortality decline in Paris was part of the general epidemiological transition in North Atlantic countries, when victory over infectious diseases eliminated the urban mortality penalty (for the U. S., see Haines, 2001; for the U.K., see Woods, 2003).

Before addressing the relationship between mortality and access to sanitation, we briefly review the history of sanitation in Paris. Many Parisian sewers date back to Roman times and the Middle Ages. The network of pipes began to expand dramatically in the mid-nineteenth century when Baron Haussmann renovated Paris and its infrastructure (Gandy, 1999). In fact, 67% of all lines in place by 1913 had been built by 1885. But, by law, sewers could only accommodate liquid waste (Chevallier, 2010, pp. 244–246). Engineers feared that the water flow was insufficient to move solid waste down the network. Buildings were equipped with different systems to capture waste solids. In the most basic system, residents emptied their waste water into pits or tanks whose contents were then taken away by night soil companies. More often, buildings had waste pipes (they were often installed at the same time as running water) that emptied into septic systems that captured solids while the liquids drained to sewers or the street. These septic systems also had to be emptied regularly. In either case, the residents of buildings were exposed to contaminants of waste water. In 1886, finally, the city allowed landlords to connect their buildings' waste water pipes directly to the sewer (Jacquemet, 1979, p. 517).

Landlords were slow to take advantage of the direct connection option, but not because of a lack of sewer lines. In 1885, had the number of buildings connected per kilometer of line been the same as in 1913, 45% of all buildings would have adopted the improvement. In fact, in 1885 only 100 buildings out of 65,000 were

[4] The share of Paris in the French population was 4.5% at the beginning of our period and 7% at the end.

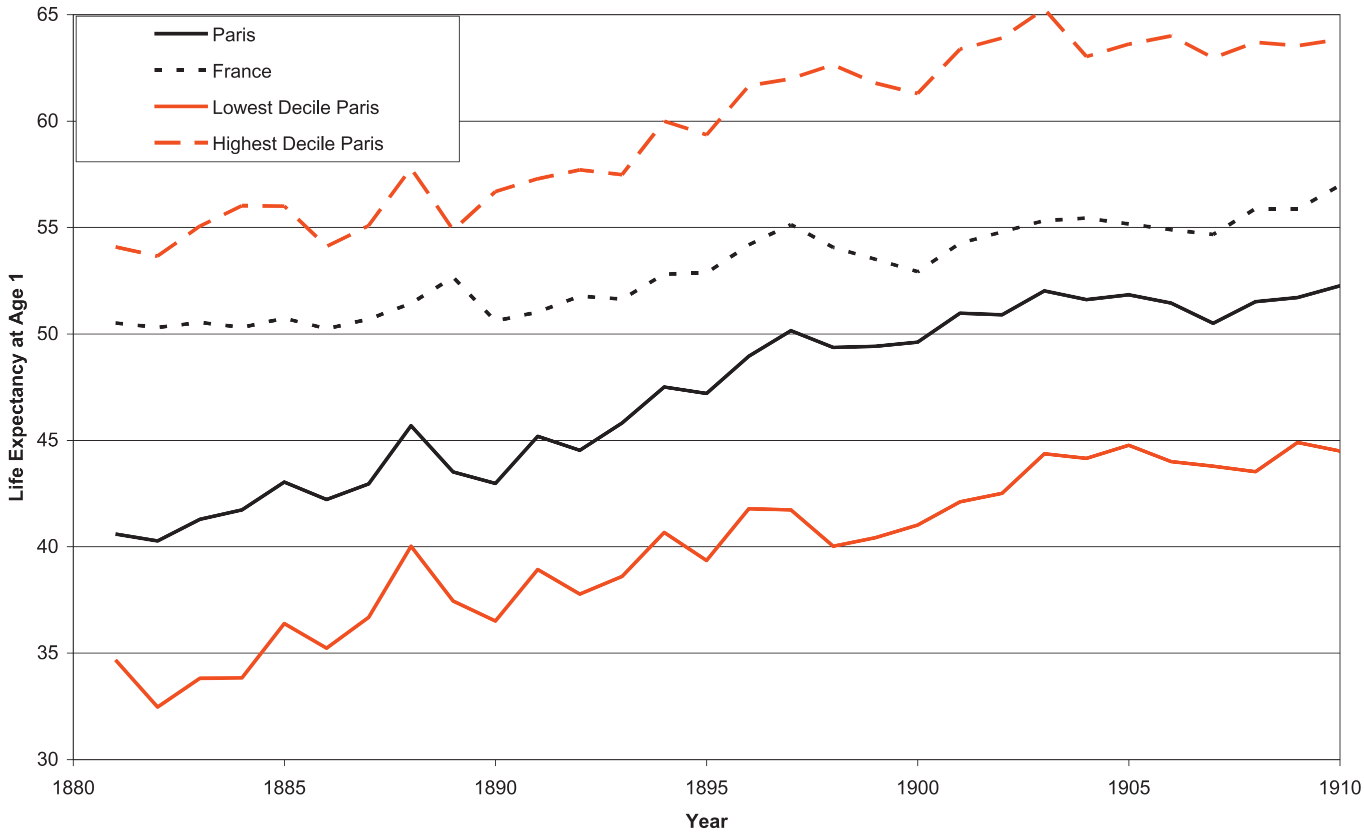

Fig. 2. Life expectancy at age 1 within Paris, compared to France. (For interpretation of the references to color in this figure, the reader is referred to the web version of this article.)

connected. In 1895, had the number of buildings connected per kilometer of line been the same as in 1913, the connection rate would have been 55% instead of 10%.

The reason very few building owners chose to connect their property was cost. After 1886, landlords had to decide whether to retrofit their buildings and pay an annual fee of 60 francs per downpipe that was connected to the sewer. Given an average rent of 300 francs per apartment in 1876, this fee was sizeable. To encourage owners of buildings in poor neighborhoods to connect, where over 90% of households paid less than 300 francs in rent, the city lowered the feed for buildings that rented for less than 500 francs to 30 francs per year. But the lower fee was still unattractive to the landlords in poor neighborhoods because they anticipated that rents on their buildings would not raise enough to pay for the improvement. In 1894 the city did make connection mandatory, but the law was selectively enforced. Older buildings were in effect grand-fathered in, and their owners decided whether or not to connect, making the lawing binding only for new constructions. By the end of 1904, 10 years after connections were mandated, only half (37,342) of all buildings in Paris were directly connected to the sewers. Nearly all structures built after 1894 were directly connected to the sewer, but connections in the old *arrondissements,* where there was nearly no new construction, show no sharp jump after 1894.

Beyond its own efforts at improving the worst areas of Paris (*Ilots insalubres*), and the price discounts detailed above, the city did little to promote sewers (Jacquemet, 1979). Nevertheless sewer connections grew with two inflections, an early acceleration in the mid-1890s and then a slowdown in the mid-1900s (Fig. 3). By 1906, the rate of sewer adoption seems to have settled into some long-term process (slightly faster in the poorer, less connected, neighborhoods; slightly slower in the richer ones). Over time, there were steady gains. By 1913 almost 70% of the buildings were connected, although the 12th, 13th, and 20th districts on the eastern edge of the city had yet to pass 60%. By 1928 when the detailed reporting ends, the connection rate topped 85% in the quartile of most favored districts and ranged between 67% and 77% in the bottom quartile. Sewers were therefore a technological change whose endogenous adoption favored rich neighborhoods over poor ones and thus actually furthered the spatial inequality within the city.

Fig. 3 shows clearly that the most affluent neighborhoods in the city always had the highest connection rates. A simple linear regression confirms that rent is a strong predictor of connection to sewers (Table 1).[5] Beyond the obvious idea that those who can pay more will get the improvement first, we need to specify why the most affluent neighborhoods adopted direct connection the fastest. A little theory helps frame the decisions of three sets of actors (renters, landlords, and the city's sanitation department). To begin with, each renter must decide how much to bid for an apartment in a building directly connected to the sewers. It seems likely that the willingness of households to pay for a direct sewer connection increases with income and that the direct connection is a normal good. Because sewer connections are costly, there will be a threshold income above which households are willing to pay at least the average cost of connecting to the sewer.

Second, each landlord must choose whether to connect his or her building to the sewer. We focus on landlords because before WWI each building had at most one owner, so that at least 82%

[5] Given the fast increase in sewer connection rates, it is clear that we need to use the full yearly sample from 1885 to 1913 if we are to understand the phenomenon. To do so, we linearly interpolate fiscal rents at the neighborhood level between census years (every five years). The data on the fiscal rents available yearly at the district level allow us to control that it is quite a good approximation.

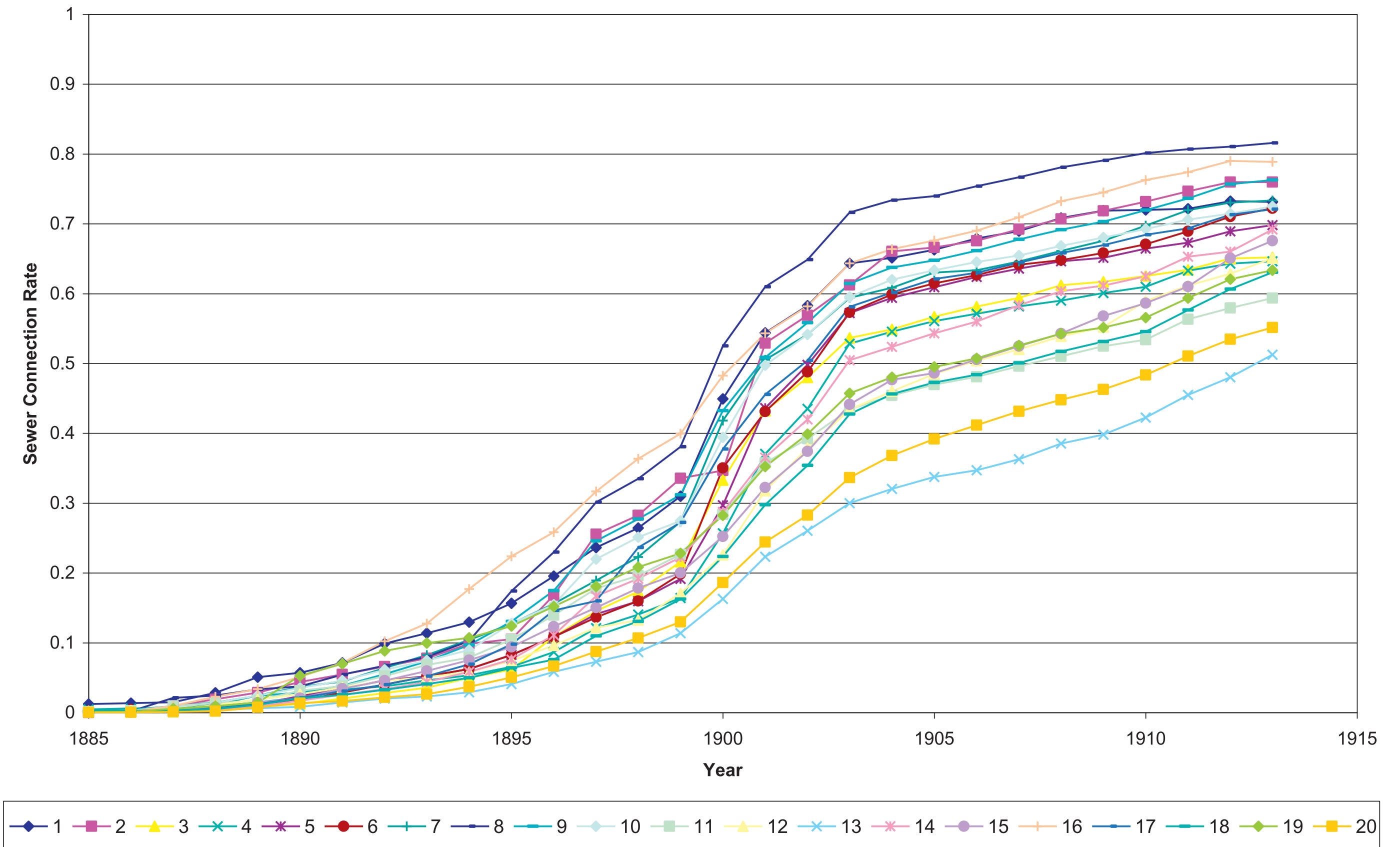

Fig. 3. Share of buildings connected to the sewer by districts.

Table 1
Cross section regressions of sewer connection rate on rents.

	Share of buildings connected to sewer		
	OLS [1]	OLS [2]	OLS [3]
Neighborhood rent	0.065***	0.106***	0.071**
	(0.007)	(0.020)	(0.033)
Constant	0.320***	0.408***	0.406***
	(0.005)	(0.007)	(0.007)
Neighborhood fixed effects	No	Yes	Yes
Time fixed effects	No	Yes	Yes
Time trend × neighborhood fixed effects	No	No	Yes
Adjusted R-square	0.06	0.96	0.98
Observations	2320	2320	2320
No. of neighborhoods	80	80	80

Sources: The share of buildings connected to the sewer was collected in *Annuaires statistiques de la ville de Paris* (yearly from 1881 to 1914). Neighborhood rent is computed from the *Etats annuels du montant des rôles généraux des contributions directes* (every five years from 1876 to 1911) available at the French national center for financial and economic archives in Savigny (Centre des archives économiques et financières, CAEF).
Note: The dependent variable is the share of buildings connected to sewer in a neighborhood; the independent variables are standardized. Robust standard errors, clustered at the neighborhood level, reported in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

of the 883,871 housing units existing in 1900 were rented. The true proportion of units rented was no doubt higher because buildings in poor neighborhoods were owned by individuals who were renters in nicer buildings and because rich individuals owned multiple buildings, if the estate tax data are any indication (Piketty et al., 2006). In the end, the decision to connect to the sewer was made by landlords who wanted to maximize rental income, and their decision depended on how much their tenants would bid up rents if units were directly connected to the sewer.

As long as the demand for sewer connection is an increasing function of income, rent will increase more in absolute value for an expensive apartment than for a cheap one. Thus landlords' incentives to provide the improvement will increase with the quality of their buildings. The initial 30 or 60 francs per connected down pipe fee made it a costly investment – by some account double the costs of traditional septic tanks. It is thus not surprising that connections rose more quickly in richer than in poorer areas – and that income and infrastructure were correlated. As long as the connection decision was left within private hands, there was bound to be a delay in connecting dwellings in poor neighborhoods.

There was a third reason why poor neighborhoods would be slow to connect—a political one. The city's sanitation department has to decide on how much to charge owners, and whether to price discriminate. The city could have levied a tax (on buildings or consumption) and connected all buildings in short order. Yet in a highly unequal society like Paris at the time, political economic considerations will get in the way of any such scheme. Any such compulsory scheme would feature either a subsidy from landlords to poor tenants or from the top part of the income distribution towards the bottom. Because the size of the subsidy rises with inequality, the rich's opposition to any such scheme also grows with inequality. In any case, Parisian landlords were publicly opposed to any legal requirement that they connect their buildings to the sewer. They waged a long judicial and political battle to delay the passage and implementation of the 1894 ordinance that made connection to sewer mandatory (Jacquemet, 1979). Owners of buildings in the Champs Élysées neighborhood did adopt the new technology with great alacrity, because doing so led tenants to bid up the value of their rents by more than the cost of implementing

Table 2
Life expectancy and the diffusion of sewers.

	Life expectancy at age 1						
	Main results				Robustness		
	OLS [1]	OLS [2]	OLS [3]	OLS [4]	OLS [5]	OLS [6]	TSLS [7]
Connected to sewer	3.927***	3.025***	1.331***	1.012**	1.067	0.941	1.307*
	(0.178)	(0.141)	(0.442)	(0.430)	(0.712)	(0.689)	(0.704)
Neighborhood rent		3.746***		1.310**		2.190*	1.198*
		(0.447)		(0.654)		(1.186)	(0.642)
Constant	50.149***	50.101***	51.957***	52.120***	51.774***	51.916***	52.027***
	(0.544)	(0.359)	(0.254)	(0.264)	(0.302)	(0.312)	(0.348)
Neighborhood fixed effects	No	No	Yes	Yes	Yes	Yes	Yes
Time fixed effects	No	No	Yes	Yes	Yes	Yes	Yes
Time trend × neighborhood fixed effects	No	No	No	No	Yes	Yes	No
Adjusted R-square	0.34	0.64	0.90	0.90	0.92	0.92	
First-stage statistic							96.5
Observations	2320	2320	2320	2320	2320	2320	2320
No. of neighborhoods	80	80	80	80	80	80	80

Sources: Life expectancy is compiled from various sources, see Appendix B for details. The share of buildings connected to the sewer was collected in *Annuaires statistiques de la ville de Paris* (yearly from 1881 to 1914). Neighborhood rent is computed from the *Etats annuels du montant des rôles généraux des contributions directes* (every five years from 1876 to 1911) available at the French national center for financial and economic archives in Savigny (Centre des archives économiques et financières, CAEF).
Note: Dependent variable is life expectancy at age 1. Both independent variables are standardized. Robust standard errors, clustered at the neighborhood level, reported in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

the new technology. Tenants in poorer neighborhoods would still desire the improvements but, with a smaller budget, they could only offer smaller increases in rent to their landlord–not enough to induce him or her to retrofit buildings.

Given the externalities inherent in water infrastructure, it makes sense to subsidize sewer connections. One way to do so would be to price discriminate and charge high-rent buildings more than low-rent buildings and use the proceeds to expand the sewer network. This is precisely the mechanism used by the city with variations over time. In 1888, when connection was voluntary, owners faced 30 or 60 francs fees per connected pipe. But by the end of the century, when connections were mandatory, the price schedule was more complex, with 12 different fee levels ranging from 10 to 1500 francs annually per building (Préfecture de la Seine, 1899, p. 9). Overall, however, diffusion remained slow because, as noted above, there were relatively few rich housing units available to subsidize the vast number of housing units rented by the poor. It was also slow due to the hostility of building owners and the political obstacles the city encountered in enforcing the 1894 ordinance (Jacquemet, 1979, pp. 535–545).

To pay for new sewer lines that buildings could connect to, the city could borrow, as long as user charges covered interest and maintenance. Given this constraint, it would make sense to equip richer (high willingness to pay) neighborhoods faster than poorer ones, and not surprisingly the correlation between rents and the ratio of street to sewer length per arrondissement is positive. It is largest early on (0.65 in 1880) and then declines over time as more and more neighborhoods become better equipped (0.47 in 1906). Overall, the length of installed sewers grew much faster than building connections. The city built new sewers at a high rate in the last three decades of the 19th century (85% of the total sewer-line length was built by 1895 when only 10% of all buildings were connected). More important, the high rate of sewer completion implies that what limited the share of buildings connected to the sewers was not the political economy of sewer construction but rather the political economy of connection pricing.

4. Sewers and life expectancy

The diffusion of sewers was clearly a social phenomenon, and as such far from a quasi-experimental situation where we could just regress the sewer connection rate on life expectancy. There are two issues that our statistical model should address. The first is the considerable variation in *ex-ante* life expectancy, income, and housing quality across neighborhoods. The other is that adoption of direct connection might be endogenous. We may imagine that neighborhoods would adopt sewer connections at higher rates following outbreaks of water-borne diseases (see Troesken, 1999). Additionally, some neighborhoods might derive greater benefits from the adoption of sewers than others. But because neighborhoods share the same water technology and environmental conditions vary little across Paris, it is likely that the mortality impact of sewer connections was similar everywhere. If anything the fact that rich neighborhoods adopted early would underestimate the impact of sewer connections since they already enjoyed a higher life expectancy. Thus the key issues involve initial conditions and income rather than endogeneity. We estimate the following panel model:

$$le_{it} = \alpha + \beta SCR_{it} + \gamma Rent_{it} + \delta_t^1 + \delta_i^2 + \varepsilon$$

where le_{it} is life expectancy in neighborhood i for a given year t; SCR_{it} and $Rent_{it}$ are, respectively, the sewer connection rate and the average rent in neighborhood i in year t, δ_t^1 is a year fixed effect, and δ_i^2 is a neighborhood fixed effect. We thus estimate the impact of sewers net of any city wide demographic shocks (as well as the general trend of improvement in mortality) and net of any permanent differences between different parts of Paris. Standard errors are clustered at the neighborhood level. As a robustness check we will use an instrumental variable strategy in the next section.

Table 2 below reports regressions of life expectancy on the fraction of buildings connected to the sewers and average rents by neighborhood; both with and without fixed effects. The data set includes one observation per neighborhood per year from 1885 to 1913 (2320 observations in all). We begin with a straightforward correlation without fixed effects. Sewers seem to have had significant positive benefits, adding nearly four years to Parisians' life expectancy. The impact of increasing sewers by one standard deviation (28%) is a bit less than doing the same for rents when we run the regression with one variable and omit the other (regressions for rents without sewers are not reported). The impact of increased sewer connection rates is also robust to including rents. Overall, neighborhoods that are one standard deviation below the mean in either rents or sewer connections have a life expectancy three years lower than those at the mean.

The impact of both rents and sewer connections fall by about two thirds when we add the full set of fixed effects, but they remain highly statistically significant (columns 3 and 4 of Table 2). The smaller coefficients when year and neighborhood fixed effects are added are not surprising. First of all, much of the variance across neighborhoods is relatively constant over time. Neighborhoods that had high rents and low mortality in 1880 also had high rents and low mortality in 1913. The same neighborhoods were also most likely to adopt direct connection to sewers early and have large mortality declines. All of that variation is absorbed by the neighborhood fixed effects. Second, there are regular trends as the decline of mortality, increase in sewer connection, and rise in income all occur monotonically over time. So, much of the variation over time is absorbed by year fixed effects. It is thus remarkable that the coefficients on sewers and rents remain precisely identified. The explanatory power of the regression that includes both sewers and rent is significantly higher than with sewers alone, which suggests each has an independent effect. More important, the coefficient of sewers changes little when the rent variable is included (and the same is true for the coefficient of rents when we include sewers) and remains statistically significant. Overall, sewers do seem to have had an important and significant impact in prolonging life.

Finally we can add neighborhood specific linear time trends (columns 5 and 6 of Table 2). Doing so does not change the magnitude of the coefficients or the explanatory power of the regressions. But the standard error of the most monotone variable (sewers) increases enough that we lose statistical significance. It is clear that we cannot sustain statistical significance in regressions with both neighborhood fixed effects and neighborhood time trends. Nevertheless, in any other combination of fixed effects, time trends, and rents, the effect of sewers is significant at the 5% level or better. This implies that we cannot rule out another variable that would be monotone in time and highly correlated with sewer diffusion at the neighborhood level (but not correlated with rents at the neighborhood level) as the cause of a significant increase in life expectancy in Paris. We think this is highly unlikely; instead the evidence is consistent with sewer connections increasing life expectancy by several years.

5. Robustness checks

We have established that sewers have a positive and large connection to the fall in mortality. Yet we might be concerned that even if sewer adoption is not a proxy for income growth, it is endogenous. One might worry that the neighborhoods that adopt sewers first might do so because omitted factors that make sewers more efficient there than elsewhere. Such factors would need to vary over time within a neighborhood (since either spatial or time invariant factors would be controlled for by our space and time fixed-effects). Moreover it is important to keep in mind that high rent neighborhoods adopted first and fastest (the correlation between rents in 1876 and the sewer connection rate in 1913 is 0.48 and significant at the 1% level). And these were the low mortality neighborhoods (the correlation between mortality in 1880 and the sewer connection rate in 1913 is −0.32 and significant at 1% level). The rich neighborhoods were adopting even though they already enjoyed low mortality. Moreover these neighborhoods had better substitutes for direct sewer connections since they were the most likely to use the next best system, namely septic tanks with the soiled water routed to the sewers.[6] So the bias will probably underestimate the true effect of adopting sewers on life expectancy, because late adopters were poorer neighborhoods where life expectancy had more to gain from direct sewer connection. Nevertheless, an instrumental variable approach is a good robustness check, in particular because local shocks in mortality (e.g. spikes in water borne diseases) might spur the adoption of sewer connections. If we are correct, however, that the primary endogeneity problem causes downward biases, then the IV estimate should produce larger coefficients for sewer connection than OLS.

We propose to take the average share of buildings that are connected to the sewers in adjoining neighborhoods and use it lagged one year as an instrument. Our primary worry in terms of endogeneity is reverse causality. One might well worry that high rates of water borne morbidity in a given neighborhood would spur adoption nearby.[7] Our instrument addresses that issue because, by construction, it is independent of neighborhood specific mortality shocks. The instrument is measured outside the neighborhood of interest—in both time and space—thus independent of mortality shocks specific to that neighborhood. To justify our choice of an instrument, we must show that the average sewer adoption rate in adjoining neighborhoods does induce adoption in the neighborhood under consideration, but is not related to mortality shocks there. The spatial correlation in adoption could come about either through demand or supply side issues. On the supply side, we might think that some areas of Paris saw earlier expansion on their adoption because they were equipped with local pipes at higher rates. The reason for this would be that the neighborhoods closest to the main collectors would be cheapest to equip. For Paris at least, this argument is not plausible. As we argued above, the length of installed sewer pipes (the main pipes) meant that far more buildings could have been connected to the sewer than actually were in every year before WW1.

The spatial correlation in adoption comes in fact from the demand side. First, a high sewer connection rate among its neighbors will cause the neighborhood under consideration to adopt more direct connections via a keeping up with the Jones' effect. Second, the spatial correlation is also likely to reflect the fact that the inhabitants of adjoining neighborhoods learned about the benefits of sewers at the same rate. The result in both cases will be that the adoption of sewers in a given neighborhood will positively influence adoption in its neighbors. On the other side, we might worry that sewer adoption in a given neighborhood was related to changes in adjacent neighborhoods' mortality because of contamination fears. Such fears could have arisen through three channels. First, contaminated individuals might have directly infected their neighbors as they moved in the city. Second, sick individuals might have contaminated both the water supply where they lived but also wherever they went throughout the city. Third, contaminated water from one neighborhood might have seeped into the water supply of the nearby neighborhoods. During our period Parisians' sources of water did not depend on ground water so most contamination with soiled water would come from (very) local sources and both the second and the third possibilities above appear highly unlikely. But we cannot exclude the first one as movement between neighborhoods occurred daily. Even though contamination from person to person is rather limited for water borne diseases, contaminated individuals (in particular those infected but healthy)

[6] In addition, apartments in higher income neighborhoods were more spacious and had more rooms so that sick members of the household could be isolated from healthy ones. They also had many servants who could do far more cleaning than would have been possible in poor households. See Bertillon (1887, pp. 125–128).

[7] Another concern we already mentioned would be differential adoption of other health enhancing practices between neighborhoods (e.g. better hygiene in the wealthy neighborhoods). As there is no reason for the improvements in hygiene to be related from one neighborhood to the next, our instrument would also control for that issue.

may facilitate the dispersion of these diseases from one neighborhood to the next.

Our instrument is not perfect as we cannot rule out that individuals learned about mortality shock in adjacent neighborhoods and that such news increased their fear of contagion and thus their demand for sewer connections. But, we regard this possibility as both unlikely and of limited overall effects. As noted above, the behavior of Parisians is consistent with an increasing awareness of water borne diseases but not of serious concerns with either person to person contamination or any other form of increased risk from neighbors, otherwise there would have been more pressure for mandatory adoption of sanitation.

Our instrument is directly related to the adoption of sewer connections in a given neighborhood for a given year (as indeed demonstrated in the first stage) and there are good reasons to think it satisfies the exclusion restriction. The results from the IV estimation are consistent with the main results we presented in the previous section (columns 7 of Table 2). As we expected, the coefficient on sewers is larger than it was without the IV but the estimation is less precise. Overall, the change is relatively limited and the IV estimates mostly confirm our previous estimates.

We perform other tests of robustness including removing neighborhoods with either very high or very low connections. Those tests leave the effect of sewers a little lower but still important (almost a year of additional life expectancy) and significant (Table 3). We also implemented a placebo test which reallocates the sewer connection rate randomly across neighborhoods. This does not produce any effect of sewers connection on life expectancy. Thus the sewer effect is not simply an aggregate decline in mortality that coincides with the diffusion of sewers in Paris. Finally we test non-linear effects, introducing variables that account for each additional 20% of sewer connections. The effects are slightly non-linear with the gain in life expectancy higher in the middle of the distribution (when half the building are connected to sewers) and declining at the end (once 80% of the buildings are connected). One might want to interpret the non-linearity as evidence of externalities at the neighborhood level but it might also simply be that improvements in life expectancy begin once a minimum adoption rate has been achieved for the whole neighborhood. In all cases, the robustness tests confirm our main results.

Table 3
Additional robustness controls.

	Life expectancy at age 1		
	Excluding extremes	Placebo	Non-linear connection to sewers
	[1]	[2]	[3]
Connected to sewers	0.873**	0.001	
	(0.424)	(0.465)	
Neighborhood rent	1.514**	1.691***	1.498**
	(0.726)	(0.599)	(0.663)
Between 20% and 40% of buildings connected to sewers			0.653**
			(0.254)
Between 40% and 60% of buildings connected to sewers			1.730***
			(0.365)
Between 60% and 80% of buildings connected to sewers			2.399***
			(0.472)
Between 80% and 100% of buildings connected to sewers			1.995***
			(0.509)
Constant	52.195***	52.776***	51.134***
	(0.247)	(0.392)	(0.346)
Neighborhood fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Adjusted R-square	0.90	0.90	0.90
Observations	2233	2320	2320
No. of neighborhoods	77	80	80

Sources: see Table 2.
Note: Dependent variable is life expectancy at age 1. Model [1] excludes the most advanced (Champs-Elysées) and the two least advanced (Montparnasse and Santé) neighborhoods in sewer connection. Model [2] is estimated after redistributing randomly sewer connection rates (the whole sequence) among neighborhoods. The figures shown are the average over 500 replications. Of those, 22 shows a statistically significant effect of sewer at 5% level. Both rents and constant vary slightly but are always significant at 1% level. Model [3] is estimated using 4 dummy variables indicating different levels of connection to sewers, in place of a single continuous variable.
Robust standard errors, clustered at the neighborhood level, reported in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

6. Age and gender effects

Because the mortality data are broken down by age and sex, we can examine the impact of sewers on age-specific mortality risk. We do so for five age categories: 1–4, 5–19, 20–39, 40–59, and 60–79. Given that the IV estimates are similar to those of the OLS but less precise, we only report the OLS results. When looking at mortality risk, if the sewer connection rate is beneficial its coefficient ought to be negative.

Table 4 reports results for men. The impact of sewers is particularly large for the very young and for men in their twenties and thirties. The first finding is consistent with the literature and the idea that water borne diseases took a particularly heavy toll on the young. The second finding, a high impact for the 20–39 years old, might well be related to on-the-job sanitary conditions.

Estimates of the impact of sewers on women's mortality risks are a bit smaller and statistically less precisely estimated than those of men's (Table 5). The relatively higher sensitivity of women 5–19 may be due to with the greater participation of girls and young women in domestic chores. Prior to direct connections they may have been far more exposed to waste water than boys and young men either at home or in domestic service.

In economic terms it seems that the impact on both sexes was similar. In both cases the impact is largest for the youngest group and declines with age. The proportional reduction in mortality risk at age 1 is five times what it is for 40 years olds both because the baseline mortality is higher for the young and because the impact of sewers is larger for them. Finally, there is no discernable effect for those older than 60 years.

Overall the regularity of the decline in the importance of sewers as age increases make us more confident that direct connection to sewers played an important role in reducing mortality. As noted we do not have proper mortality data for infants and causes of deaths were not reported by both neighborhood and age. Nevertheless we do have data on causes of deaths by neighborhood which reveal that, as could be expected, much of the mortality in Paris in the nineteenth century was related to infectious diseases (Kuagbenou and Biraben, 1998). But even deaths due to infectious diseases were not equally distributed over time and between neighborhoods. During the period of sewer diffusion, death from water-borne diseases (typhoid, cholera, and diarrhea) fell dramatically. It fell earlier in the high adopting neighborhoods (from between 4% and 5% of deaths between 1885 and 1894 to about 2% in 1900 and about 1% in 1913). The later adopters started with much higher rates of death from water borne diseases (between 10% and 14% before 1894) and those rates fell by half by 1913.

Table 4
Regressions of mortality risks by age and sex on sewer connection rate – men.

	log (mortality rate) 1–4 years old [1]	log (mortality rate) 5–19 years old [2]	log (mortality rate) 20–39 years old [3]	log (mortality rate) 40–59 years old [4]	log (mortality rate) over 60 years old [5]
Connected to sewer	−0.308***	−0.126**	−0.128***	−0.082***	−0.039
	(0.062)	(0.063)	(0.036)	(0.025)	(0.029)
Neighborhood rent	−0.323***	−0.172	−0.200**	−0.087**	0.017
	(0.123)	(0.105)	(0.079)	(0.035)	(0.036)
Constant	4.687***	3.873***	5.242***	5.995***	6.521***
	(0.044)	(0.064)	(0.021)	(0.015)	(0.018)
Neighborhood fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Adjusted R-square	0.68	0.41	0.73	0.78	0.45
N	2320	2320	2320	2320	2320
No. of neighborhoods	80	80	80	80	80

Sources: See Table 2.
Note: Dependent variable is log (mortality rate) for five different age group. Both independent variables are standardized.
Robust standard errors, clustered at the neighborhood level, reported in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Table 5
Regressions of mortality risks by age and sex on sewer connection rate – women.

	log (mortality rate) 1–4 years old [1]	log (mortality rate) 5–19 years old [2]	log (mortality rate) 20–39 years old [3]	log (mortality rate) 40–59 years old [4]	log (mortality rate) over 60 years old [5]
Connected to sewer	−0.211***	−0.198***	−0.063*	−0.074**	−0.038
	(0.071)	(0.052)	(0.036)	(0.029)	(0.035)
Neighborhood rent	−0.399***	−0.148***	−0.223***	−0.094**	0.009
	(0.096)	(0.053)	(0.062)	(0.043)	(0.041)
Constant	4.579***	4.107***	4.965***	5.523***	6.393***
	(0.047)	(0.041)	(0.028)	(0.022)	(0.019)
Neighborhood fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Adjusted R-square	0.67	0.36	0.80	0.78	0.56
N	2320	2320	2320	2320	2320
No. of neighborhoods	80	80	80	80	80

Sources: See Table 2.
Note: Dependent variable is log (mortality rate) for five different age group. Both independent variables are standardized.
Robust standard errors, clustered at the neighborhood level, reported in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

7. Conclusion

This paper measures the contribution of public infrastructure to the decline of mortality in the historical experience. We examine the connection between the pace at which sewers were adopted across Parisian neighborhoods between 1885 and 1913 and the decline of mortality. Controlling for invariant neighborhoods features, the aggregate pace of mortality decline, and variations in average rents, sewers had a large and positive effect on life expectancy. A one standard deviation rise in the share of buildings directly connected to the sewers increased life expectancy by one year, or around 2%, at age one. This effect is about as large as a one standard deviation increase in rents. In other words, the average difference in life expectancy between living in an affluent neighborhood (with 1700 francs in average rents) and an average one (around 900 francs in rents) is the same as the difference between living in a neighborhood with 32% of dwellings connected to the sewer and one with 60%. Sewers saved lives.

More important, the gain in life expectancy related to sewers was not evenly distributed, because the owners of buildings in the more affluent neighborhoods—measured here through rents—were the first to adopt the new infrastructure. Less affluent neighborhoods connected far more slowly; three decades after the technology was made available, fewer than two-thirds of all the buildings in the poorer eastern neighborhoods were directly connected to the sewers.

That delay had significant social costs. As an illustration, we estimate a counterfactual: what would life expectancy have been if poor districts had achieved their 1928 sewer connection rate much earlier in 1900? The acceleration in connections would have tripled their connection rates from just about a quarter of buildings to more than three quarters. Using our smallest coefficients (Table 2, column 4) such an increase in connections would have raised life expectancy by three years. There are three ways to consider how substantial this gain might have been. First, this jump would have been enough to propel life expectancy in the worst decile of neighborhoods all the way to the level experienced by the median neighborhoods for Paris as a whole. Second, to achieve the same effect by increasing income (or rents) would have required doubling them; at 2% growth (which is twice the rate of growth of rents and likely exceeds the growth rate of wages in Paris) that would have taken 35 years. Finally, since life expectancy at age 1 was about 47 years, the increased life span coming from sewers would have mostly involved extra years of work for workers with valuable skills.

In the developed world, the diffusion of infrastructure has been extensive and has contributed to a remarkable reduction in the inequality of life expectancy. In Paris today, for instance, differences in life expectancy are a third of what they were a century ago. By contrast, many cities of the developing world still face the extraordinary range of living conditions that once characterized Paris. In such cities, to be poor is to die young, not only because of low income but also because of a lack of basic infrastructure. In the extreme version of this inequality, a large fraction of the urban population in many developing countries lives in shanty towns. While it is relatively easy to bring a clean water tap to the shanty towns, connecting the homes to sewers is a far more complex problem.

The problem facing developing countries is complex for several reasons. First of all, although placing a clear-water tap at every street is complicated, it pales relative to the investment required to provide sanitation. Good sanitation has to capture waste water wherever it is produced. Doing so requires conjoined individual and collective investments. Individual households have to direct their waste water to pipes and thus eliminate alternative and cheaper approaches such as outhouses or simply throwing the waste water into the street. In addition, poor individuals today, as in 1880s Paris, may be credit constrained and thus reluctant to invest in life improving technologies. At the same time, there has to be a sewer to accept households' waste water. Cities in the developing world often find it difficult to finance the large scale investment needed to bring sewers to everyone due to political factors, as in Paris. Furthermore, cities also find it difficult to collect fees for providing sanitation services, particularly in shanty towns where the lack of formal titles certainly makes the collection of user fees extremely difficult. Here turn-of-the-century Paris had two advantages over cities in developing countries: it had access to abundant financing and a high degree of formality in real estate. Both made it easy to charge owners of buildings for connecting to the sewer.

It seems likely that politics explains why a great deal of the social gains were foregone even in Paris, because building owners and the wealthy were successful in blocking the large scale redistribution that would have been necessary to realize the social gains. Because sanitation and many other investments that prolong life are excludable network goods, they are frequently delivered through user fees that charge close to average costs. That was historically the case in North Atlantic countries and the practice continues nowadays in developing countries. When societies are highly unequal, the use of user fees slows diffusion and keeps mortality high and life spans unequal, just as they were in Paris on the eve of World War I. In sewers, as in many other things, the trickle down is slow.

Appendix A. Descriptive statistics of the longitudinal sample

Panel A: all

	N	Year	Mean	SD total	SD between	SD within	Rank
Life expectancy at age 1 (years)	2640	1881–1913	49.35	7.02	5.54	4.36	0.73
Average rents – complete (francs)	320	1878, 1890, 1900, and 1910	656.42	606.81	595.09	131.97	0.93
Average rents – fiscal (francs)	2640	1881–1913	896.12	797.48	789.75	140.82	0.94
Sewer connection rate (SCR) (%)	2320	1885–1913	32.05	27.97	0.07	27.05	
Building permits (%)	1440	1896–1913	16.83	22.61	17.07	14.93	

Note: All data are for 80 neighborhoods. "Rank" gives the linear correlation between neighborhoods ranking in 1881 and in 1911 (1876 and 1910 for complete rents).

Appendix B. Computing mortality risk and Life expectancy

Our goal is to compute life expectancy at age one. Implicitly this is a simple procedure that integrates age specific mortality risk. Yet because the age categories reported at the neighborhood (*quartier*) level are not stable over time and do not necessarily accord between the *Annuaires* – that give the deaths – and the Censuses – that report the number of living –, we must make corrections. We proceed in three steps.

First, we adjust both the mortality and population reports in order to obtain the number of deaths and the number of living for the same age intervals: 1–4; 5–19; 20–39; 40–59; and finally 60 or more years old. For each year we also have the report that breaks down deaths by sex and five year age groups for Paris as a whole. We use it to correct the coarser *quartier* level reports. Take for instance the death reports between 1881 and 1893: instead of giving total deaths for age groups 5–19, 20–39, and 40–59, the *Annuaires*' table uses the age intervals 5–14, 15–34, and 35–59. So we estimate, from the data for Paris as a whole, the share of deceased aged 15–19 among those aged 15–34. We apply this share to the groups defined at the neighborhood level to get the number of deaths between 15 and 19 years old. We add this number to total deaths in the age group 5–14 and subtract it from those in the age group 15–34. We proceed in the same way for the age groups 15–34 and 35–59. Finally, we estimate smaller age-interval for the older ages using the distribution of death for Paris as a whole: we subdivide both 40–59, and 60 and over intervals into five-years age groups.

Second, we need the population at risk. We estimate intercensus populations for every year. The standard way to do so is to evaluate the change in population between census years by combining the effect of mortality and net migration. In the case of a closed population, such estimates are (almost) immediate given the population total by age in a census year and the number of deaths each year (one just needs to make hypotheses about the relationship between birth cohorts and calendar years). At the other extreme, if migration rates are very high, then the flow of new people in the city determines the size of a given age group. This is the case for Paris and we use a linear interpolation of the size of the population of a given age between the two adjoining censuses. Such a procedure neglects both mortality shocks and variation in migration patterns that might affect one age group more severely than another in a given inter-census year. Given the rather coarse nature of our data we could not try to capture the differentiated consequence of either effects at the neighborhood level without making heroic assumptions.

Third, we compute a life table for each year and neighborhood: to do so we compute a set of age-specific death rates (m) for each year and neighborhood by dividing the number of death in the age group by the number of individuals living in that age group. We can then produce probabilities of dying (q) using the standard formula $q = n^*m/(1+(n-a)*m)$, n and a being the average number of person-years lived in the interval by, respectively, those who survived that age group and those dying in that age group. Given that we do not have the exact age at death, the value of a, the average number of person-years lived by the deceased, is borrowed from another population, e.g. Keyfitz and Fliegler (1968, p. 491). The step from death probabilities to mortality tables and life expectancy at each age is then straightforward (Preston et al., 2001, pp. 42–50).

Overall, we have tried to make the simplest assumptions in these computations to avoid biasing our results. When these assumptions matter, they do so in ways that tend to understate differential mortality. In particular, the average number of person-years lived by those dying in the last age group (older than 80) comes out to just under eight years which is perhaps too optimistic. More importantly it seems likely that this number varied across neighborhood: even among the old, mortality was probably more severe for the poor than for the rich. In this case we would be underestimating mortality in the poorer neighborhoods and as a consequence understating the actual mortality

Please cite this article as: L. Kesztenbaum, J.-L. Rosenthal, Sewers' diffusion and the decline of mortality: The case of Paris, 1880–1914, Journal of Urban Economics (2016), http://dx.doi.org/10.1016/j.jue.2016.03.001

differential. Yet it seems logical, at least to start, to make the same assumptions for all the neighborhoods so as to insure we do not produce differential mortality by construction. In the end, our computations probably understate mortality differences across neighborhoods, but the extent of the bias is limited. After all the life expectancies we compute for the census years (when we have the exact population) are very similar to those for inter-census years. Varying the average life span per interval or the maximal age in the life table has some impact on life expectancy but very little on differences among neighborhoods in the city.

Appendix C. Estimating the rent to income gradient for Paris

The analysis in this paper relies on rents because average rents are reported by neighborhood for each year in tax reports. Using the real estate censuses of 1890, 1900, and 1911 we show that average rents are good statistic for a variety of other moments of the distribution by neighborhood. What we lack is any detailed information about the distribution of income by neighborhood. Yet by collating different piece of information we can generate a distribution of income by *arrondissement* for the 1890s.

We start with the population census of 1891. For each district (*arrondissement*) it distributes the population into 240 different occupational groups (within 32 branches). For each group it gives separately and by sex the number of owners/managers (*patrons*), white collar workers and supervisors (*employés*) and unskilled workers (*ouvriers*). It also totals the number of family members not gainfully employed and the numbers of servants (*domestiques*) employed by the households in each group. Thus to each occupational group corresponds 10 categories of population (6 categories for gainfully employed individuals and 4 for their dependents) from female owner managers to male servants. Considering the fact that households with higher incomes are more likely to employ more servants, and that unskilled workers most likely have lower incomes than owners/managers, one can ask what is the correlation between the number of servants per owners/managers and rents (0.96), or the correlation between the share of the gainfully employed that are unskilled workers and rents (−0.87).

The next step is to estimate income for the six gainfully employed categories. For about half the occupational groups we can use the 1896 industrial survey (which reports income for both *employés* and *ouvriers*). To keep things simple we apply the average income over Paris by occupational group (most of the variance in reported income is across gender, category, and rank). That still leaves out large chunks of the population who were not surveyed: all occupational groups in services; all owner/managers; those who lived from capital income (*rentiers*); and domestics.

For *rentiers* and owner managers we rely on estate tax filings for 1892 (see Piketty et al., 2014). These individual data gives both wealth at death and occupation. We assign all the estates whose owners where reported as without occupation or retired to one category (*rentiers*) and all those who reported a current profession to another (employed). The *rentiers* category has on average two and half time the wealth of those who are currently in an occupation. *Rentiers* are also extremely spatially differentiated with those in the rich 8th district having nearly 30 times the wealth of those in the poor 20th district. We assume that wealth produces a 4% return to compute income from wealth. We then estimate the mean by district and apply it to the total of individuals who declared living off their capital in the 1891 census.

For owners/managers in industry, we sum the income estimated from the wealth at death of the employed category with the labor income of white collar workers in industry. For a few branches in services, smaller surveys provide some income information but we lack income for most service occupations. In the absence of further data we give these occupations the mean gender-category Parisian wage. Hence a white collar worker in a department store receives the same income as a bank clerk. Considering that service occupations range from butchers and hairdresser to bankers and stock brokers, their occupation are likely to have had an income variance much larger than manufacturing but smaller than that of *rentiers*. As a result, we are suppressing part of the variance in income between districts. Nevertheless we do preserve part of the between districts variance because the distribution of employment by sex and categories was rather systematic (in wealthier districts there are more men, more *employés* and fewer *ouvriers*). Finally, given that servants did not make decision about housing we leave them aside.

This procedure produces a set of incomes that are tightly correlated with rents as shown in Table C.1. The correlation is 0.84 which means that rents and incomes were closely matched. Moreover, although that procedure is our favorite specification, other ways to order the various information we gather (income in the industrial sector, rents, share of servants, etc.) produce the similar results. For instance, if we only look at industrial occupations we get a correlation of 0.83; if we ignore *rentiers*, the correlation drops to 0.78; if we impute arrondissement average incomes to occupations for which we do not have data, the correlation becomes 0.85.

Table C.1
Imputed incomes and measured rents.

District (*arrondissement*)	Mean income	Mean rent	Ratio
1	3428.5	1127.5	0.33
2	2589.3	846.0	0.33
3	2362.7	613.8	0.26
4	2481.4	666.4	0.27
5	3103.3	675.5	0.22
6	3433.0	959.9	0.28
7	4502.1	1564.1	0.35
8	22450.3	2654.1	0.12
9	5590.5	1224.7	0.22
10	2681.7	735.5	0.27
11	2157.6	495.3	0.23
12	2125.4	469.9	0.22
13	1907.1	330.1	0.17
14	2050.9	432.5	0.21
15	2101.5	375.3	0.18
16	4146.5	1640.2	0.40
17	2724.2	896.4	0.33
18	1965.6	400.6	0.20
19	1872.7	379.1	0.20
20	1770.7	257.3	0.15

Appendix D. Estimating the rent to income gradient for Paris

Table D.1.

Table D.1
IV first stage.

	First stage
Adjacent sewers	3.208***
	(0.325)
Rents	0.058
	(0.074)
Constant	−1.125***
	(0.148)
Neighborhood fixed effects	Yes
Time fixed effects	Yes
R^2	0.98
N	2320

Note: Dependent variable is the standardized share of buildings connected to the sewer. Adjacent sewers is the standardized one-year lagged average sewer connection rate for adjacent neighborhoods; rents is the standardized average value of rent per apartment.
Robust standard errors, clustered at the neighborhood level, reported in parentheses.
*** Significant at 1% level.

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Please cite this article as: L. Kesztenbaum, J.-L. Rosenthal, Sewers' diffusion and the decline of mortality: The case of Paris, 1880–1914, Journal of Urban Economics (2016), http://dx.doi.org/10.1016/j.jue.2016.03.001