Public Health and Mortality: What Can We Learn from the Past?[1]

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Introduction

City life in the nineteenth and early twentieth century was dirty and dangerous (Melosi 2000). The water and milk supply of cities was contaminated with bacteria causing typhoid fever, dysentery, and diarrhea. Cities did not remove sewage and their streets were filled with garbage and carrion. The influx of migrants from abroad and from rural areas crowding into dank and dark urban tenements provided new foci of infection and new victims, and the rapid transmission of disease from host to host increased its virulence. Among infants the excess urban mortality was 88 percent in 1890 and 48 percent in 1900 (Haines forthcoming) and nowhere was the urban mortality penalty as large as in the poor areas of town where crowding was greater and where parents could not afford to buy clean water and milk (Rochester 1923). City life left those who survived to age 60 permanently scarred, shortening their lives at older ages even controlling for later residential moves (Costa 2003; Costa and Lahey 2003). But, by 1940, the urban mortality penalty had disappeared and life in a city was in many ways healthier than life in the countryside (Haines forthcoming). Between 1902 and 1929, the urban waterborne death rate had fallen by 88% (Cain and Rotella 2001).

This paper focuses on this mortality transition in American city life between 1910 and 1930, a change that was only possible because of very expensive investments in city infrastructure. These investments swamped all other forms of public assistance. In 1913, the United States was spending twice as much on hospitals and health as it was on public poor relief and welfare (Lindert 2004). In contrast, in 1980 the United States was spending three times as much on public poor relief and welfare as hospitals and health for the poor. Although later public policies, such as those of the New Deal, were also effective in reducing mortality (Fishback, Haines, and Kantor 2002), the reduction in mortality prior to 1930 was perhaps the foremost public policy success of the twentieth century.

The paper begins by investigating the determinants of state and local generosity in this time period (the federal government played only a minor role). An intriguing puzzle emerges. In the present day, several empirical studies of the determinants of local generosity such as Orr (1976), Luttmer (2001), Bahl, Martinez-Vazquez and Wallace (2002), Alesina and Glaeser (2004) have documented that support for redistribution (typically welfare payments) is lower in areas where more minorities live and higher in areas with greater ethnic and racial homogeneity (e.g. Luttmer 2001; Poterba 1997). Unlike in the present day, we find that in the early 20th century U.S, support for redistribution was higher in areas with more blacks and immigrants. We do not argue that the middle class has become less altruistic over time. Instead, below, we focus on the self interest of the middle class as a motivating factor in supporting large public health investments.

Increased government expenditure for the poor is intended to increase their quality of life. Such expenditures can have unintended consequences. Some look at San Francisco's large homeless population and wonder whether this tolerant city's generosity has acted as a magnet attracting more homeless to move there. Public finance economists have conducted analyses of "crowding out" to test whether increased government expenditure causes reduced private donations to charity. The vast majority of tests related to the unintended consequences of government redistribution have focused on modern data. We use our historical data to test for whether generous cities are immigrant magnets and to test for whether there is a negative correlation between city charity expenditure and private charity.

The third part of this paper assesses the effectiveness of government expenditure in improving the health of the population. In short, do public health investments save lives? Whose lives? Urban blacks faced much higher death rates than urban whites. Did public health investments help close this racial mortality gap? Our findings contribute to a growing urban economic history literature that measures the health benefits from increased public expenditure (Cain and Rotella 2001, Haines 2003, Troesken 2004). We estimate individual level and city level health productions to test whether, holding other factors constant, cities that spend more on public health have lower death rates from diseases with a public health component. Complicating answering this important public policy issue is the potential endogeneity of public health spending. Cain and Rotella (2001) argue that city public health investment is likely to be high in cities that had a public health epidemic in previous years. In this case, ordinary least squares regression estimates of the city level health production function could yield the surprising finding that increased government expenditure *raises* a city's death rate! Below, we present instrumental variable strategies for addressing this issue.

This section's contribution to the urban historical public health literature is that we examine the effectiveness of public spending using a larger sample, using individual level data to control for individual covariates, and extending our analysis to 1940 when the urban penalty had disappeared. By examining individual level data we can determine whether the poor benefited more than the middle class did from increased public spending.

By estimating individual level infant mortality regressions, we provide new evidence on whether blacks benefited more from public health expenditures as argued by Troesken (forthcoming) or whether they benefited less as argued by Higgs (1980). In addition to examining micro data, we also use a large city panel data set covering the years 1912 to 1925 and a state/year panel data set with death rates by race from 1910 to 1940 to provide a more comprehensive analysis of the effectiveness and incidence of public spending.

Our paper addresses some of the issues that have been of life-long interest to Eugene Smolensky. An ongoing challenge in designing programs that improve the quality of life of the poor is to provide resources for this group without creating perverse incentives that discourage work or human capital accumulation. Today's welfare reform debate wrestles with this issue (Smolensky, Evenhouse and Reilly 1997). As compared to public assistance, public health investment is a likely example of a program that benefits the poor without distorting incentives.

The paper first examines the determinants of city and state public health expenditures in the past. We use cross-city level data to test for what are the correlates of urban redistribution in the past. We then present two tests of the unintended consequences of these expenditures. We then investigate the public health gains achieved through public health investments. This section utilizes a combination of individual level data, city level and state level data. All three data sources are used to estimate forms of a health production function to test for whether death rate decline when local government spends more on public health. In the final section, we use "value of life" estimates and new compensating differential estimates to value the benefits of public spending. We find that the average city was under-investing in public health. We conclude with some conjectures for why this could take place.

City and State and Local Redistributionary Expenditure in the Early 20th Century

The United States has traditionally spent little on social transfers. In 1910, the United States was below the OECD country median in terms of social transfers as a percentage of GDP (Lindert 2004). The United States redistributed 0.56 percentage points while Denmark's social transfers equaled 1.75 percentage points of GDP. In 1995, even though the US share spent on social transfers ballooned to 14 percent of GDP, the United States was still below the median. Examining redistribution within the United States can help us understand why the United States has always been low spender.

We first seek to study the determinants of redistribution differentials. Our measures of government redistribution in the past are 1) combined state and local (county and incorporated places) government per capita expenditures in 1913 on the two categories charities, hospitals, and corrections and recreation, health, and sanitation; 2) city per capita expenditures on health, sanitation, and charities in 1907; and, 3) city per capita board of health expenditures in 1930. Although these measures are not strictly comparable, they are all indicators of government generosity. The 1907 data, which can be broken down into its subcomponents, show that the largest component of health expenditures was expenditures on sanitation. In 159 cities, median per capita expenditure in 1907 dollars on health, sanitation, and charities combined was $1.59 (the maximum was $6.47). Median per capita expenditures on the individual categories of health, sanitation, and charities were 17 cents, 40 cents, and 82 cents, respectively. The greater the expenditure on a single category, the greater the expenditure on all categories. The correlations between per capita spending on health and sanitation, sanitation and charities, and health and charities were 0.42, 0.39, and 0.20, respectively.

We examine what local attributes are correlated with relative state and city generosity and whether the political variables that we hope to later use as instrumental variables have any explanatory power by running OLS regressions of the form

(1) $$\log(E_{lt}) = b_0 + b_1 X_{lt} + b_2 P_{lt} + u_{lt}$$

where E is expenditures per capita in city l at time t, X is a vector of demographic and socioeconomic characteristics, and P is a vector of political variables. We run three OLS regressions; one for combined state and local expenditures on charities and health in 1913; a second for 1907 city expenditures on health, charities, and sanitation; and, a third for city health board expenditures in 1930.

Table One shows that both demographic characteristics and city heterogeneity matter. Larger cities are more generous. The population elasticity in 1907 is 0.22 and in 1930 it is 0.19. Locations with older residents spend more on redistribution but this coefficient is only statistically significant in the state level regression. Richer locations, as proxied by the Duncan Index (which in turn is based upon occupation), spend more (Orr 1976; Lindert 1994).[2] Cities where income fragmentation (as proxied by the

[2] Our results are not driven by outlier cities. We have re-estimated these regressions using quintile regressions and find similar findings.

standard deviation of the Duncan Index) was high distributed less, consistent with Costa and Kahn's (2003) results of the importance of income fragmentation to such social capital proxies as volunteering. Surprisingly, at the state level greater income fragmentation predicted greater spending.[3] Also surprisingly, given that the literature on modern spending finds less spending when ethnic and racial fragmentation is high, in cities and states with a higher fraction of blacks and foreign-born, expenditures were greater. A 10 percentage point increase in the foreign-born increased 1907 city expenditures by 38 percent. A 10 percentage point increase in the city's black population increases spending by 16%. Breaking out total spending into each of the three sub-categories, charities, health, and sanitation, yields the same finding. In cities that have a larger share of the population that are black, redistributionary spending per-capita is higher.

While a benevolent planner might allocate greater spending per-capita in areas where there are greater numbers of needy poor people, why would self-interested middle class tax payers be so generous?[4] In the present day, studies of the determinants of cross-state differences in AFDC generosity such as Orr (1976) report that a state's generosity is negatively correlated with its minority population share. Is it a puzzle that cities were more generous towards minorities in the past than in the present? One plausible explanation is the ongoing decline in transportation costs over the last 100 years. When the vast majority of a city's employment was located in its downtown and

[3] State spending on redistribution is surprisingly persistent over time. The correlation between a state's 1990 average monthly AFDC payment to a recipient and its 1913 redistribution per-capita is 0.65.

[4] The correlation between the proportion of a city's population in 1907 that was black and the proportion that was illiterate was 0.82. When we included the proportion of the city that was illiterate in our 1907 regression, the coefficient on the proportion black became small and insignificant whereas the coefficient on the proportion illiterate was 3.844, statistically significant at the 5 percent level.

the suburbs were not developed, the rich and poor lived in much closer physical proximity. While these groups lived in separate communities, there was a greater potential for a public health shock in the poor's community to have a contagion effect on the richer community. Middle class and rich tax payers might view public health investments as a type of insurance policy. Today as employment has suburbanized, and transportation costs have fallen, the middle class and rich have a greater physical distance "a moat" separating them from the day to day life of the poor and less of an incentive to vote for redistribution that benefits the poor. "In a world, where blacks and whites lived in close proximity 'sewers for everyone' was an aesthetically sound strategy. Failing to install water and sewer mains in black neighborhoods increased the risk of diseases spreading from black neighborhoods to white ones Troesken (2004 page 10)."

Chinatown in San Francisco offers an interesting case study (Craddock 2000). Within the city, typhoid rates were highest in the immigrant Chinatown area. To reduce the prospects of a public health crisis emerging from there, pro-active steps were taken to invest in public health. Civic leaders recognized that this community interacted with the native community and hence there existed the possibility of disease contagion. A rather large percentage of Chinese immigrants who lived in Chinatown worked outside of Chinatown in laundries, as cooks and domestic workers. Many also traveled to outlying farm areas transporting produce and other commodities from truck farms to the city of San Francisco (personal communication with Susan Craddock). "The Chinese were in the very center of the city, strategically located to infect the rest of San Francisco with their diseases (Craddock 2000 page 135)." In Table One we reported a fairly large positive population elasticity in raising per-capita redistribution rates. This positive population coefficient may partially reflect an urban density effect.

Political variables also predict state and city spending. We hypothesize that states and cities whose congressmen and senators are Democrats and who have greater seniority spend more on redistribution. Democrats (controlling for regional fixed effects) have traditionally had a more redistributionary ideology and seniority positively correlates with more money for the home state. We find that both 1913 state and 1907 city generosity is positively correlated with the share of Democrats in the House and with average years of service. Surprisingly, the same was not true of the Senate and in 1930 our political variables were poor predictors.

Unintended Consequences of Public Expenditure for the Poor

Local public redistribution can affect the locational decisions of poor households and the charity decisions of well-off households. An ongoing public policy debate focuses on whether state and local generosity triggers "welfare magnet" effects and a "race to the bottom". The literature on welfare magnets has examined whether in the present day the poor migrate and seek out more generous places (Borjas 1999; Blank 1988). Borjas (1999) has argued that international migrants have the largest "welfare arbitrage" responses, disproportionately moving to high welfare benefit states such as California relative to native poor people, because they have already made the decision to move. We test this hypothesis in the past by implementing a simple test. Using micro data from the 1900 and 1920 Micro census data, for each city we count the total number of immigrants over the age of 18 who have moved to the U.S in the last 10 years. We then estimate a cross-city regression where the dependent variable is the log of the count of immigrants in the city in 1920. We regress this on the log of the count of immigrants in that city in 1900, the log of city population in 1907, and the log of that city's redistribution per-capita in 1907, controlling for 9 region fixed effects.

$$\underset{}{\text{Immigrants in 1920}} = \underset{(0.118)}{0.229}\text{*(City Pop)} + \underset{(0.095)}{0.706}\text{*(Immigrant in 1900)} + \underset{(0.135)}{0.078}\text{*Redistribute}$$

N=132, and R^2=0.77

We conclude that there is no statistically significant evidence that in the past immigrants migrated to cities redistributing more.

The second hypothesis we test is whether public generosity crowds out private generosity or whether the two are complements. We study whether people living in generous cities contribute less to private charity using micro data from the 1917-1919 Consumer Expenditure Survey. This survey provides detailed information on expenditures, including charitable expenditures (which were less than one percent of total expenditures for the mean household), and also includes geographical identifiers for cities. We first regress the share of total expenditures spent on charity, Charity, for family i in city j on the logarithm of total expenditures and on demographic characteristics, X, and on city fixed effects, City,

(2) $$Charity_{il} = b_0 + b_1 X_{il} + b_2 (City_l) + u_{il}$$

where u is an error term. We recover the city fixed effects from our estimated regression and merge these city fixed effects to our data on city expenditures. Finally, we graph the relationship between private generosity within cities and per-capita public expenditures (see Figure One). The negative and statistically significant relationship between the private charity city fixed effects and public city generosity suggests that private charity and city expenditures were substitutes.[5]

Did Money Matter in Improving Public Health?

Death rates offer us an important, measurable outcome indicator for determining whether public expenditure improved the poor's quality of life. There are two different empirical strategies for measuring the benefits of greater public health expenditure. One approach looks within specific cities on a community by community basis to establish whether investments in sewage and water supplies reduced typhoid fever, dysentery, and diarrhea mortality (Condran and Cheney 1982). A second type of evidence focuses on cross-city analysis. In this section, we will estimate health production regressions at the individual, city and state level, using new data sets, each with its own strengths and weaknesses. All else equal, does greater expenditure on public health reduce the urban death rate? Details of the data construction are provided in the Appendix.

Individual Level Data

We use the 1910 and 1940 micro data from the Census of Population and Housing to study the probability that a mother experienced an infant death as a function of her

[5] We recognize that our finding is simply based on cross-sectional data. Lindert (2004, Chapter 3) argues that history rejects the notion that government aid to the poor crowds out private aid. "Back in the late 1920s, when government aid to the poor was only 1/6 of one percent of national product, private charity to the poor was the same. The subsequent rise of government "welfare" aid to around four percent of GNP by

household's characteristics, city size, and either the city's expenditures on health and sanitation or such health characteristics of the city such as water filtration. Following Preston and Haines (1991), we calculate a mortality index for each married woman equal to the number of child deaths experienced divided by the expected number of deaths for her marital duration. We calculate number of deaths in 1910 as the difference between the number of children ever born and the number of children surviving. In 1940 we calculate the number of deaths as the difference between the number of children ever born and the number of own children in the household. We limit the sample to women whose marital duration was less than 15 years.

Our health production functions, estimated separately for whites and for blacks, allow us to determine whether, all else being equal, death rates are lower in cities that spend more on health and sanitation. The functions we estimate are of the form,

(3) $$m_{ilt} = b_0 + b_1 \log(E_{lt}) + b_2 C_{lt} + b_3 X_{ilt} + u_{ilt}$$

where m is the mortality index for each individual i in city j at time t , E is per capita city level health expenditures, C is a vector of dummies indicating city size (greater than 1,500,000, between 300,000 and 1,500,000, between 100,000 and 300,000, and less than 100,000), X is a vector of socioeconomic and demographic characteristics, and u is an error term. We report estimates of equation (3) using OLS and instrumental variables.

Our OLS estimates for whites indicate that controlling for a range of household attributes, white child death rates declined as the city spent more on redistribution (see Table Two). The mean mortality index in the white sample is 0.88, which implies an infant mortality rate of roughly 0.11 in a Model West life table. Increasing expenditures

1995 could not just crowd out private charity because there was only 1/6 of one percent of GNP in private philanthropy that could have been crowded out in the first place."

by one standard deviation therefore would decrease the mortality index by 0.08 and the infant mortality rate by roughly 0.01. In contrast, black children did not benefit from increased city expenditure. Consistent with Preston and Haines' (1991) results, we find that a large city population raised death risk for both white and black children and disproportionately raised it for blacks. Note that for black children the effect of being in one of the largest cities was five times worse than for white children (a coefficient of 1.053 versus 0.242).

We recognize that city level health expenditure is unlikely to be randomly assigned. Cities are likely to spend more if in the past they have had a health crisis (Cain and Rotella 2001). If the error term is serially correlated then this means that OLS estimates of β_1 are biased toward zero. We therefore instrument for city expenditures using the city level variables in Table One, that is, a city's demographic and socioeconomic characteristics and the political characteristics of the state. In the white sample, our estimated coefficient on city spending increases (in absolute value) from -0.127 to -0.172 and is still statistically significant. In the black sample the coefficient on city spending increases and becomes statistically significant, but its positive sign implies that higher spending increases black child mortality. We suspect that because large cities spent more, our estimated coefficients on spending in part reflect city size. Our suspicion is re-inforced by the much smaller coefficient on city size in the IV regression.

The results in Table Two raise a puzzle. As shown in Table One, per-capita redistribution is higher in cities with a larger black population. Table Two shows that in 1910 black mortality was not declining with respect to this expenditure. Troesken (2004) argues that black health gains occurred more slowly in more segregated cities. Using the

Cutler, Glaeser, Vigdor (1999) disimilarity measure of racial residential segregation for 64 cities, we find that controlling for city's population and its percent black, more segregated cities spend more on redistribution. This finding is borderline statistically significant.

City expenditures measured in dollars may represent different "treatments" in different cities. Large expenditures may translate into little tangible improvements in the poor's health if expenditures are high because of urban patronage. Cities obtaining their water from wells or mountain springs instead of lakes or rivers would need to make fewer health investments. Still other cities may have invested before the year 1907 in fixed cost infrastructure with little variable cost. Based on our "flow" data from 1907, we would classify them as low expenditure cities when in fact they have made their health investments in the past.

We therefore turn to "stock" indicators of city public health infrastructure investment. Our two stock indicators are the fraction of the city population whose dwelling had a sewer connection and a dummy variable indicating whether the city filtered its water by 1905. We estimate equation (2) substituting these "real" investments for the expenditure variable results reported in Table Two.

Table Three shows that child mortality among whites was lower in cities where a high proportion of the population had a sewer connection and in cities that filtered their water by 1905. The effects of water filtration were particularly strong, probably because there was much more variation among cities. When we interact whether or not a city filtered its water by 1905 with a dummy variable indicating home ownership, we find that the poor (the non-owners) were the primary beneficiaries of water filtration, perhaps because they could take fewer steps to protect themselves. Blacks benefited very little from city health investments. Our coefficients in Table Three are almost all positive (but insignificant). The interaction of water filtration with home ownership suggests that black home owners were the primary beneficiaries of water filtration, perhaps because water service and water filtration had not yet come to the poorer black neighborhoods. As in our previous regressions, it may not be possible to disentangle the effects of city health investments from those of city size. When we exclude city size indicators from our regressions, we find that in the black sample the coefficient on the fraction of the city population with a sewer connection becomes -0.115 ($\hat{s}$ = 0.093). Although the coefficient is still statistically insignificant, the point estimate implies that blacks benefited more than whites from city investments in sewage connections.

Table Four presents our results for 1940. Note that neither city health board expenditures in 1930 nor city size was a significant predictor of child mortality for either whites or blacks. Our findings on city size are consistent with Haines' (2003) account of the disappearance of the urban mortality penalty. By 1930 most cities had solved their sanitation problems. Health problems, however, did remain. A survey conducted by the White Conference on Child Health revealed that only 51 percent of the pre-school children surveyed in cities and 37 percent of the pre-school children surveyed in rural areas had ever had a health exam (a preventive check-up) and that only 13 percent of children in both urban and rural areas had ever had a dental exam. Among children in this age group only 21 percent of those in cities were vaccinated against smallpox and diphtheria (White House Conference on Child Health and Protection 1931).

Table Five shows that child mortality among whites was lower in cities where a higher percentage of children had had a health exam. Generally this health examination was given prior to age 1 (and none were given after age one) and roughly 10 percent of all children who had had an exam got one from a dispensary (White House Conference on Child Health and Protection 1931). However, health examinations may still have been valuable in lowering child mortality because most child mortality was below age one and because information about child health may have been transmitted to mothers. A high percentage of health examinations, however, could also reflect the availability of children's health services in dispensaries. We do not believe that it reflects general health consciousness, because vaccination and dental examinations should also be indicators of health consciousness and these are not statistically significant predictors of child mortality. Cities with a greater percentage of children who had had health exams also spent more; although aggregate health board expenditures may not have been beneficial, at least spending on public dispensaries was effective.

Table Five shows that only whites benefited from health examinations; the greater the proportion of health examinations in the city, the higher the black child mortality rate. We also found that among whites non-owners benefited more than owners. When we interacted our home ownership dummy variable with the logarithm of the percentage of children having health examinations, we found that in the white sample the coefficient on health examinations was -0.200 ($\hat{s}$ =0.078) and the coefficient on the interaction between home ownership and health examinations was 0.121 ($\hat{s}$ =0.113). In contrast, when we used the same specification in the black sample the coefficient on health examinations was 0.635 ($\hat{s}$ =0.281) and the coefficient on the interaction between health and home ownership was -0.383 ($\hat{s}$ =0.434), providing some suggestive evidence that if there were any benefits to blacks, the benefits accrued to the better-off.

City Level Data

We recognize that there is a 30 year gap between our two micro data sets. Detailed City level death rate data are available between the years 1912 and 1925. Such data allow us to "fill in the blanks." For each city and year between 1912 and 1925 (with the exception of 1918) we observe the case and death rate for diphtheria, measles, polio, smallpox, tuberculosis, and typhoid and link these cities to our 1907 redistribution data for 130 major cities.[6]

We study whether cities with greater health expenditures in 1907 have a steeper negative time trend in mortality and case rates for our six major diseases controlling for a city specific intercept. That is, we estimate OLS regressions for each of the six diseases,

(4) $$\log(m_{lt}+0.01)=b_0+b_1T_{lt}+b_2T_{jt}\log(E_j)+b_3City+u_{lt}$$

(5) $$\log(c_{lt}+0.01)=b_0+b_1T_{lt}+b_2T_{jt}\log(E_j)+b_3City+u_{lt}$$

where m is the mortality rate and c is the case rate, T is a time trend, City is a vector of city fixed effects, u is an error term, and the subscript l indexes the city and the subscript t indexes time t. Note that case and death rates may be higher in cities with better public health offices because the better offices may have been able to enforce more precise diagnoses on the part of physicians. We are therefore likely to underestimate the effect of city expenditures on case and death rates.

Table Six reports the predicted time trend for each disease for a city that spends the sample mean on redistribution and the predicted time trend for each disease for a city that spends one standard deviation above the mean on redistribution. For measles, we find statistically significant evidence that the case rate and the death rate time trend is steeper for cities that spend more on redistribution. The average city during this time period had a 4.2% annual decline in its measles death rate while a city whose redistributionary spending was a standard deviation above the mean had time trend of – 5.3% per year in its measles death rate. One surprise that emerges is for typhoid. When we population weight the regressions, we find that cities that spend more on redistribution have a less steep reduction in their death rates from typhoid then cities that spend the average. This result is driven by New York City. When we do not weight the regression, this "wrong sign" vanishes.

We also examine the effect of city expenditures in 1907 on infant mortality in 1910 for 120 cities, for all races combined, for whites, and for blacks. That is, we run OLS regressions of the form,

$$\log(m_l) = b_0 + b_1 \log(E_l) + b_2 X_l + u_l \qquad (6)$$

where m is the mortality rate (deaths per 100 children under age 1), E is per capita health expenditures, X is a vector of city demographic characteristics, u is an error term, and l indexes the city. We also run IV regressions in which we instrument for per capita health expenditures using our state political variables. Since reverse causality will bias OLS

[6] The case rate is the number of diagnosed cases per hundred people and the death rate is the number of deaths per 100 people. The data for 1918 were unavailable at the time of writing. However, because of the influenza pandemic, 1918 may be an unusual year.

estimates towards zero, we expect that IV estimates of equation (6) will yield a larger negative coefficient estimate of β_1 than OLS estimates.

Table Seven shows that when we instrument for city expenditures, the coefficient on the logarithm of city expenditures is both strongly negative and is statistically significant for all races combined and for whites. An increase of a standard deviation in city expenditures lowers total infant mortality rates from a mean of 14.9 per 100 to 11.5 per 100. Although city expenditures do not have a statistically significant effect on black mortality rates, the magnitude of the coefficient on expenditures implies that blacks benefited as much as whites from city spending. The contrast with our micro-data results suggests that perhaps the sample of blacks in the micro-data was too small to draw reliable conclusions. As in our regressions using the census micro-data, the urban penalty for blacks is much higher than the urban penalty for whites. In larger cities in 1910, blacks were living in more segregated areas.[7]

State Level Data

State level data allows us to further investigate the effect of expenditures on mortality rates by race and by cause. We link total 1913 expenditures on the broad categories of charities, hospitals, and corrections and recreation, health, and sanitation by state and local governments to an unbalanced panel on death rates for all ages at every 5 year interval fom 1910 to 1940 for 10 different conditions.[8] The conditions that we examine are all causes, typhoid, scarlet fever, whooping cough, diphtheria, dysentery,

[7] For 64 cities based on the Cutler, Glaeser, Vigdor (1999) 1910 measure of residential racial segregation (the disimilarity index), the correlation of the log of city population and this disimilarity index is 0.42.

tuberculosis, bronchitis, measles, pneumonia, influenza, diarrhea, and hernia, where we use hernia as a placebo because while expenditures on hospitals towards the end of the time period may well have reduced deaths from hernias, most public health expenditures would have only a very small causal impact. We examine the effect on death rates of only 1913 state expenditures because the expenditure data are not comparable over time. Expenditures should therefore be interpreted as more of a rank ordering.

The regressions that we estimate are of the form

$$\log(m_{st}) = b_o + b_1 T_{st} + b_2 \log(E_s) + b_3 X_{st} + u_{st} \tag{7}$$

where m is the mortality rate for each state s at time t, T is a time trend, E is per capita state and local government expenditures in 1913, X is a vector of demographic characteristics, and u is an error term. We estimate separate regressions by disease and by race. In addition to OLS regressions, we also estimate IV regressions in which we instrument for expenditures using our political variables. Because states with health problems in the past were likely to be spending more, our OLS coefficients are lower bound estimates of the effectiveness of state expenditures in reducing death rates.

Tables Eight and Nine show that state expenditures were mainly effective in reducing death rates from typhoid fever, diphtheria, and from dysentery. Expenditures had a statistically significant effect in reducing white deaths from typhoid and diphtheria and a statistically significant effect in reducing black deaths from diphtheria and dysentery. However, the magnitude of the coefficient on expenditures suggests that expenditures also played a role in reducing white deaths from dysentery and in reducing

[8] This measure of total 1913 local government per-capita expenditure is highly positively correlated with Chapin's (1915) ranking of state public health department's quality.

black deaths from typhoid. In addition, the coefficients on expenditures are quite large for both white and black deaths from pneumonia. Expenditures have no effect on death rates from hernias, our placebo, for whites, but raise deaths from hernias for blacks, perhaps because states that spent more were more likely to attribute accurately cause of death to hernias. State expenditures appear to have played a slightly larger role in lowering white deaths from typhoid fever, diphtheria, and pneumonia than in lowering black deaths. In addition, the time trend in deaths from diphtheria and pneumonia is bigger for whites than for blacks.

Valuing Public Health Investments in the Early 20th Century

We have shown that on the whole government expenditures played an important role in lowering mortality rates, particularly in the first few decades of the twentieth century. But, what were the dollar benefits of these expenditures? To answer this, we must combine our estimates of how much extra health was produced through greater public health expenditure with estimates of how much the population valued improvements in health. We answer this question in two ways. We first estimate a rental hedonic using the 1917-1919 Consumer Expenditure Survey and city level infant mortality rates in 1920. We then use estimates of the value of life calculated from wage hedonics and industry risk to value the statistical lives saved.

The rental regression that we estimate is

$$\log(r_{il}) = b_0 + b_1 \log(m_l) + b_2 X_{il} + u_{il} \tag{8}$$

where r is the yearly rent (imputed for home owners) for dwelling i in city l, m is the infant mortality rate in city l, X is a vector of housing characteristics, and u is an error term. Assuming that migration costs are low and that people not living in a city are aware of the attributes of the city, the coefficient estimate on the infant mortality rate represents the "compensating differential" to living in a high mortality city (Williamson 1981). If preferences over risk exposure and consumption are homogenous, then this hedonic sketches out the representative agent's indifference curve. It is important to note that in estimating equation (8), we are assuming that the disease environment proxied for by m_j varies across cities but not within cities. Table Ten shows that apartment dwellers paid higher rents for a lower city level infant mortality rate, controlling for city population and dwelling characteristics.

We seek to measure how much a city's residents would value the health benefits of increased public health expenditure. Recall that in Table Seven, a standard deviation increase in per capita city expenditures (roughly $19.66 in 2002 dollars), decreased total infant mortality from 14.9 per 100 to 11.5 per 100. This decrease of 3.4 deaths per 100 would have raised yearly rents by approximately $127.36 in 2002 dollars at a time when average rents in the sample were $2,264 in 2002 dollars. The implied value of a statistical infant's life was only $51,585 in 2002 dollars, a very small number. We believe that this very small estimate is due to intra-city variation in community disease exposure. Within a city, there are safer low density communities and riskier, high density communities. This introduces measurement error in the explanatory variable which in turn biases toward zero the estimate of the value of a statistical life. Craddock's (2000) map of San Francisco's typhoid rates across communities supports this "Hot Spots" hypothesis. Two additional negative results further support the intra-city variation hypothesis. We find no evidence that city level infant mortality rates were capitalized into the rents of non-apartment dwellers. We also find no evidence that city level infant mortality rates were capitalized into wages. We expected that cities with high mortality rates would pay higher wages as a compensating differential.

Given that we do not fully trust the estimates in Table Ten for recovering the historical value of a statistical life, we pursue an alternative strategy of valuing the benefits of health investments. We use estimates of the value of life derived from hedonic wage regressions on industry fatality risk. Costa and Kahn (2003; forthcoming) used micro-census data from 1940 to 1980 to estimate changes in the value of life over this period and concluded that the income elasticity of the value of life ranged between 1.5 and 1.7. Using an elasticity of 1.7 and interpolating back to 1920 yields an estimated value of life of $895,000 in 2002 dollars (Costa and Kahn 2003). Thus the decrease of 3.4 deaths per hundred infants gained from an increase in per capita expenditures of $19.66 in 2002 dollars would yield benefits of at least $30,430 in 2002 dollars. Using our 1980 estimate of the value of a statistical life of $7,393,000 yields a benefit of $251,362 in 2002 dollars. Both of these estimates underestimate the benefits of city expenditures because they only account for changes in infant mortality, not for changes in child and adult mortality.

Were expenditures in reducing mortality worth it, to cities? Because average population size in the cities for which we estimated a health production function was 181,778, total city expenditures would have had to rise by $3,573,765 in 2002 dollars to save 3.4 infant lives per 100 and some unknown number of child and adult lives. The average number of infant in our cities was 4,265, implying that 145 infants would have been saved. Using the value of life of $895,000 interpolated from Costa and Kahn's

(2003; forthcoming) wage regressions implies that the total benefit was $3,817,175,000 in 2002 dollars to city expenditures of $3.5 million. Using the value of an infant life of $51,585 derived from our rental hedonic yields total benefits of $7,479,825 in 2002 dollars, suggesting that under a broad range of value of life estimates cities were underinvesting in health.

Conclusion

How effective were public health expenditures in lowering mortality rates at the beginning of the twentieth century? Early work (summarized in United Nations 1953 and 1973) emphasized the importance of public health reforms together with advances in medical technology and in living standards in lowering infectious disease rates. McKeown (1976), arguing by a process of elimination, upset this consensus view and claimed that because mortality declines began prior to any changes in medical technology or in public health reforms, the primary explanation had to be improved nutrition. But, as Fogel (1997) pointed out, what matters is net nutrition, that is the difference between food intake and the demand made on that intake by disease, climate, and work. Those with parasitic diseases suffer depletion of iron supplies despite their consumption of an otherwise healthy diet. Recurrent sufferers from gastrointestinal diseases cannot digest all of the ingested nutrients.

This paper has emphasized the efficacy of public health reforms. We have shown that state expenditures on public health lowered mortality rates from typhoid, dysentery, and diphtheria between 1910 and 1940 and that city public health expenditures circa

1910, particularly those on sewage and water filtration, were very effective in reducing childhood and infant mortality. By 1940, however, cities had solved their sanitation problems and the biggest gains in mortality begin to come from spending on preventive medical care. We find some evidence that the poor benefited disproportionately from early public health spending. Renters, who lived in higher density areas with a more severe disease environment and whose income gave them the fewer self-protection options, benefited from water filtration in the early 1900s, whereas home-owners did not. Renters also disproportionately benefited from city expenditures on child health exams in the early 1930s. Such improvements in health capital could help to reduce poverty by increasing economic opportunities for this group (Wolfe 1994).

Our evidence on the relative importance of city spending to blacks and whites is mixed. Our micro data suggests that blacks did not benefit whereas our state and city level data suggest that they benefited as much as whites. Furthermore, the disappearance of the very large urban penalty for blacks in both the micro and city level data suggests that changes within cities benefited blacks more than whites. We may not find very large effects for blacks because the extension of water filtration and sewage connections to black neighborhoods generally lagged service provision to white neighborhoods by about 5 to 7 years (Troesken forthcoming). It is possible that blacks did eventually benefit from the extension of services into their communities but that our 1910 data samples “too soon” before the benefits of these infrastructure expansions were realized.

The public health expenditures undertaken by cities circa 1910 were very low relative to the value of the lives saved, under a wide range of plausible value of life estimates. Why didn’t cities increase their public expenditures? Perhaps, it was because the poor were getting the greater benefits from such investments as water filtration and publicly financed child health exams. Alternatively, it may have taken time for cities to learn how to reduce mortality. Cleaning sewage, water, and the milk supply, establishing disease reporting and quarantining systems, disseminating health information to citizens, and ensuring that all babies and children have medical exams and vaccinations required setting up new organizations and co-operation between citizens, doctors, private philanthropists, and city public health departments.

Our results speak to trends in inequality in overall well-being in the early 20th century. More comprehensive measures of economic inequality should incorporate the value of government services, unpaid services in the home, leisure, natural environment and work satisfaction (Reynolds and Smolensky 1978). Our estimates of the health gains from public expenditure provide a guide to the value of government services.

Data Appendix

City level data: We use city level data on spending, sewer connections and water filtration from the 1907, 1909, and 1916 *Social Statistics of Cities,* respectively. We use reported infant deaths for the death registration cities in 1919 as published by the Census Bureau and calculate mortality rates using 1920 population. We use reportable disease cases and deaths as published by the Public Health Service between 1912-1925 and calculate mortality rates and case rates using estimated populations. Reportable disease cases and deaths for 1918 were not available at the time of writing. We use health board spending and the percent of children under age 6 who had ever had a health examination, a dental examination, diphtheria immunization, and smallpox immunization from the

1931 White House Conference on Child Health and Protection. The 1909 and 1916 *Social Statistics of Cities* and reportable disease cases and deaths for 1912-1925 are available from http://www.cpe.uchicago.edu. We thank Michael Haines for his files on city deaths and populations for 1909-1911 and 1919-1920. We obtain demographic and socioeconomic characteristics of cities from the integrated public use census samples, http://www.ipums.umn.edu.

State level data: We obtain information on state and local (county and incorporated place) expenditures on charities, hospitals, and corrections and recreation, health, and sanitation from Sylla, Legler,and Wallis' State and Local Government: Sources and Uses of Funds (ICPSR 6304). We aggregate all of these expenditures into total state expenditures per capita. We obtain state mortality rates by cause for 5 year intervals from 1910 to 1940 from *Vital Statistics of the United States, 1900-1940*. We obtain demographic and socioeconomic characteristics of cities from the integrated public use census samples. The source for the politics data used in Table One as a set of explanatory variables and used throughout the other tables as instrumental variables is http://voteview.uh.edu/icpsr.htm.

Micro-level data: We use the 1910 and 1940 integrated public use census samples (http://www.ipums.umn.edu) to estimate the effect of city spending and city health infrastructure on child mortality. We restrict both samples to currently married women whose husband is in the household and who ever had children. We restrict the sample to women who were married for 15 years (using the variable on marriage duration in 1910 and the variable on age at first marriage in 1940). We excluded from the analysis observations where the number of children ever born was greater than the duration of the marriage. The 1910 census had questions on both the number of children ever born and the number of children surviving. The 1940 census only had a question on the number of children ever born. We therefore imputed the number of children surviving from the number of own children present in the household. We further restricted the 1940 census to women who had not moved across counties within the last five years and excluded 11 observations where the number of children ever born was greater than 8 and there were no children in the household. Our dependent variable is a mortality index calculated as the total number of deaths for every women divided by the expected number of deaths for women within that marital duration category, where the marital duration categories are 0 to 4 years, 5 to 9 years, and 10 to 14 years. The expected number of deaths is simply the mean number of deaths per woman within each census, calculated over all races and over all urban and rural areas.

We use the 1917-1919 Consumer Expenditure Survey (*Cost of Living in the United States, 1917-1919*, ICPSR 8299) to estimate the effect of city-level infant mortality rates on yearly rental prices. Families were selected from employer records and were restricted to families in which both spouses were present and where there was at least one child in the household, where salaried workers did not earn more than $2,000 a year ($13,245 in 1982-84 dollars), families had resided for a year in the same community prior to the survey, families did not take in more than three boarders, families were not classified as either slum or charity, and non-English families had been in the United States five or more years. We restrict the sample to whites.

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Table One: Determinants of Per Capita State and City Expenditure Generosity

City/State Characteristics	Log(1913 Combined State and City Expenditures)	Log(1907 City Health, Charities, And Sanitation Expenditures)	Log(1930 City Health Board Expenditures)
Log(Population)	0.056	0.223***	0.185*
	(0.035)	(0.047)	(0.094)
Mean age	0.086***	0.047	0.027
	(0.016)	(0.033)	(0.038)
Duncan index	0.029	0.053***	0.002
	(0.020)	(0.020)	(0.025)
Std Dev of Duncan index	0.070*	-0.087**	-0.042
	(0.041)	(0.036)	(0.046)
Fraction black	1.081*	1.675***	3.849***
	(0.614)	(0.621)	(1.484)
Fraction foreign-born	5.980***	3.838**	2.500***
	(0.701)	(1.666)	(1.098)
State share of Democrats, US House	0.047***	1.138***	-0.270
	(0.013)	(0.339)	(0.373)
State share of Democrats, US Senate	-0.009	-0.747***	0.312
	(0.008)	(0.226)	(0.239)
Average Years of Service of State Representatives:			
House	0.047***	0.149**	0.011
	(0.013)	(0.060)	(0.019)
Senate	-0.009	-0.008	0.031*
	(0.008)	(0.144)	(0.015)
R^2	0.895	0.587	0.300
Observations	48	132	116

Ordinary least squares regressions are of state and city health care and sanitation spending on state and city characteristics, including region fixed effects (4 regions) and a constant. See Equation 1 in the text. Robust standard errors (clustered on state in the city regressions) in parentheses. The symbols *, **, and *** indicate that the coefficient is statistically different from 0 at the 10, 5, and 1 percent level respectively.

Table Two: Effect of City Population and City Expenditures on Child Mortality, 1910 Census Microdata

	OLS		IV	
	White	Black	White	Black
Dummy=1 if city population				
> 1,500,000	0.242***	1.053*	0.286***	0.726
	(0.068)	(0.562)	(0.101)	(0.594)
300,000-1,500,000	0.264***	0.617*	0.316***	0.526
	(0.089)	(0.327)	(0.118)	(0.455)
100,000-300,000	0.091	-0.176	0.107	-0.287
	(0.075)	(0.375)	(0.079)	(0.416)
< 100,000				
Log(per capita expenditures on health, sanitation, and charities in city) in 1907	-0.127**	0.351	-0.172*	0.878*
	(0.059)	(0.269)	(0.104)	(0.512)
R^2	0.027	0.144	0.026	0.512
Observations	7,061	372	6,693	352
Number of cities	143	67	142	66

Estimated from the 1910 census integrated public use data sets for all married women whose husband was present in the household, who had ever had children, whose marital duration was less than 15 years, and for whom the number of children ever born was no greater than marital duration. Health expenditures are from the 1907 *Statistics of Cities*. Mean per capita health expenditures in 1907 dollars in cities were $2.69 in the white sample and $2.50 in the black sample. Ordinary least squares regressions are of the mortality index on city health expenditures controlling for the logarithm of city population. Additional control variables include the woman's age, a dummy variable equal to one if the household owned it's own home, dummies for the husband's occupational class (professional, managerial, clerical and sales, crafts, service, operative, laborer, and no occupation), a dummy equal to one if the mother worked, dummies for the mother's place of birth if white (United States, Canada, Scandinavia, Britain, Ireland, Germany, Poland or Russia, Italy, other southern Europe, other eastern Europe, and other), average July temperature in the state, and 9 region dummies. See Equation 3 in the text. Instruments in the IV regressions are the fraction of the city population that is black and the fraction that is foreign born, the city's average Duncan socio-economic index, the city's standard deviation in the Duncan socio-economic index, the state's share of democrats in the US Senate, the state's share of democrats in the US House, the average number of years of service of the state's representatives in the US Senate, and the average number of years of service of the state's representatives in the US House. Washington DC is excluded from the IV regression. Robust standard errors clustered on city in parentheses. The symbols *, **, and *** indicate that the coefficient is significantly different from 0 at the 10, 5, and 1 percent level.

Table Three: Effect of City Health Characteristics on Child Mortality, 1910 Census Microdata

		White		Black	
	No. of Regional Dummies	Coef-icient	R^2	Coef-icient	R^2
Independent variable is sewer connection:					
1) Log(fraction of city with sewer connection)	4	-0.058** (0.029)	0.025	0.048 (0.122)	0.116
2) Log(fraction of city with sewer connection)	9	-0.036 (0.031)	0.026	0.021 (0.107)	0.148
Observations		7,226		372	
Number of cities		157		69	
Independent variable is water filtration:					
1) Dummy=1 if city filtered water by 1905	4	-0.202*** (0.070)	0.028	0.115 (0.402)	0.113
2) Dummy=1 if city filtered water by 1905	9	-0.196*** (0.079)	0.030	0.234 (0.393)	0.135
Independent variables are water filtration and interaction:					
1) Dummy=1 if city filtered water by 1905	9	-0.247*** (0.089)	0.030	0.294 (0.409)	0.137
(City filtered water by 1905)*(dummy=1 if owned home)		0.154 (0.088)		-0.988 (0.806)	
Observations		6,562		351	
Number of cities		147		59	

Estimated from the 1910 census integrated public use data sets for all married women whose husband was present in the household, who had ever had children, whose marital duration was less than 15 years, and for whom the number of children ever born was no greater than marital duration. Information on sewer connections and on water filtration comes from the 1909 and 1916 *Social Statistics of Cities*, respectively. The mean percentage of the population with a sewer connection in the city was 81 percent in the white sample and 70 percent in the black sample. The mean percentage of the population in a city that filtered water by 1905 was 33 percent in the white sample and 32 percent in the black sample. Regressions are ordinary least squares regressions of the mortality index on city health characteristics controlling for city size. (Regressions are similar except city health characteristics are substituted for city expenditures.) In examining water filtration the sample is restricted to cities with information on their water supply system. Additional control variables include the woman's age, a dummy variable equal to one if the household owned it's own home, dummies for the husband's occupational class (professional, managerial, clerical and sales, crafts, service, operative, laborer, and no occupation), a dummy equal to one if the mother worked, dummies for the mother's place of birth if white (United States, Canada, Scandinavia, Britain, Ireland, Germany, Poland or Russia, Italy, other southern Europe, other eastern Europe, and other), and average July temperature in the state. Robust standard errors clustered on city in parentheses. The symbols *, **, and *** indicate that the coefficient is significantly different from 0 at the 10, 5, and 1 percent level.

Table Four: Effect of City Population and City Expenditures on Child Mortality, 1940 Census Microdata

	OLS		IV	
	White	Black	White	Black
Dummy=1 if city population				
> 1,500,000	0.010	-0.113	0.013	-0.187
	(0.071)	(0.339)	(0.071)	(0.339)
300,000-1,500,000	-0.000	-0.412	0.008	-0.475
	(0.072)	(0.291)	(0.075)	(0.285)
100,000-300,000	0.022	-0.086	0.024	-0.133
	(0.080)	(0.334)	(0.080)	(0.319)
< 100,000				
Log(per capita health expenditures) in 1930	0.032	0.085	0.016	0.061
	(0.020)	(0.082)	(0.036)	(0.064)
R^2	0.021	0.138	0.029	0.150
Observations	4,364	289	4,318	281
Number of cities	64	39	63	38

Estimated from the 1940 census integrated public use data sets for all married women whose husband was present in the household, who had ever had children, whose marital duration was less than 15 years, and for whom the number of children ever born was no greater than marital duration. Health expenditure information is from *the White Conference on Child Health and Protection*. Mean per capita health expenditures (including those on hospitals, medical poor relief, and plumbing) were $1.17 in 1930 dollars in the white sample and $1.13 in 1930 dollars in the black sample. Regressions are ordinary least squares regressions of the mortality index on city health expenditures controlling for the logarithm of city population. Additional control variables include the woman's age, a dummy variable equal to one if the household owned it's own home, dummies for the husband's occupational class (professional, managerial, clerical and sales, crafts, service, operative, laborer, and no occupation), a dummy equal to one if the mother worked, dummies for the mother's place of birth if white (United States, Canada, Scandinavia, Britain, Ireland, Germany, Poland or Russia, Italy, other southern Europe, other eastern Europe, and other), average July temperature in the state, and 9 region dummies. See Equation 3 in the text. Instruments in the IV regressions are the fraction of the city population that is black and the fraction that is foreign born, the city's average Duncan socio-economic index, the city's standard deviation in the Duncan socio-economic index, the state's share of democrats in the US Senate, the state's share of democrats in the US House, the average number of years of service of the state's representatives in the US Senate, and the average number of years of service of the state's representatives in the US House. Washington DC is excluded from the IV regression. Robust standard errors clustered on city in parentheses. The symbols *, **, and *** indicate that the coefficient is significantly different from 0 at the 10, 5, and 1 percent level. Population weights are used in all regressions.

Table Five: Effect of City Health Characteristics on Child Mortality, 1940 Census Microdata

	White	R^2	Black	R^2
Log(percent of children in city who had had health exam by 1930)	-0.169**	0.022	0.594**	0.136
	(0.063)		(0.260)	
Log(percent of children in city who had had diphtheria immunization by 1930)	-0.027	0.021	0.028	0.127
	(0.028)		(0.116)	
Log(percent of children in city who had had smallpox vaccination by 1930)	-0.019	0.021	0.151	0.128
	(0.031)		(0.208)	
Log(percent of children in city who had had dental exam by 1930)	-0.022	0.021	0.097	0.127
	(0.040)		(0.157)	
Observations	4,427		307	
Number of cities	67		41	

Estimated from the 1940 census integrated public use data sets for all married women whose husband was present in the household, who had ever had children, whose marital duration was less than 15 years, and for whom the number of children ever born was no greater than marital duration. Health information is from the White Conference on Child Health and Protection and is based upon city surveys. The mean percentage of children in the city who had had a health examination by 1930 was 53 percent in the white sample and 50 percent in the black sample. The mean percentage of children who had had a diphtheria immunization by 1930 was 24 percent in the white sample and 21 percent in the black sample. The mean percentage of children who had been vaccinated for smallpox by 1930 was 25 percent in the white sample and 21 percent in the black sample. The mean percentage of children who had had a dental examination by 1930 was 12 percent in both the white and black samples. Ordinary least squares regressions are of the mortality index on city health characteristics. (The regression is a variant of Equation 3 in the text in which city health characteristics are substituted for city expenditures.) Additional control variables include dummies for city population, the woman's age, a dummy variable equal to one if the household owned it's own home, dummies for the husband's occupational class (professional, managerial, clerical and sales, crafts, service, operative, laborer, and no occupation), a dummy equal to one if the mother worked, dummies for the mother's place of birth if white (United States, Canada, Scandinavia, Britain, Ireland, Germany, Poland or Russia, Italy, other southern Europe, other eastern Europe, and other), average July temperature in the state, and 9 region dummies. Robust standard errors clustered on city in parentheses. The symbols *, **, and *** indicate that the coefficient is significantly different from 0 at the 10, 5, and 1 percent level. Population weights used in all regressions.

Table Six: Time Trends in City Case and Death Rates for Reportable Diseases by City Expenditure Class, 1912-1925

City Illness Indicator	Time trend for city spending mean amount	Time trend for city spending 1 standard deviation above mean amount
Diphtheria Case Rate	-0.028	-0.030
Diphtheria Death Rate	-0.043	-0.044
Measles case rate	-0.031	-0.044 (10% level)
Measles death rate	-0.042	-0.053 (10% level)
Polio case rate	-0.021	-0.014
Polio death rate	-0.018	-0.014
Small pox case rate	0.010	0.011
Small pox death rate	0.020	0.016
TB case rate	-0.038	-0.044 (10% level)
TB death rate	-0.052	-0.056
Typhoid case rate	-0.125	-0.119
Typhoid death rate	-0.103	-0.099
Typhoid case rate (unweighted regression)	-0.119	-0.126 (5% level)
Typhoid death rate (unweighted regression)	-0.102	-0.106

The unit of analysis is a city/year. The dependent variable differs by row and is the logarithm of the case or death rate plus 0.01. See Equations 4 and 5 in the text. The control variables are a city fixed effect, time trend, and time trend interacted with city per-capita redistribution expenditure in 1907. All regressions, except where indicated, are weighted by population. The table give time trends predicted for mean city spending and one standard above mean city spending. 130 observations from 1912-1925, excluding 1918. Statistical significance levels are for the interaction of the logarithm of per capita health expenditures times the time trend.

Table Seven: Effect of City Population and City Expenditures on City Infant Mortality, 1910 City Level Data

	Total		White		Black	
	OLS	IV	OLS	IV	OLS	IV
City size						
Within top 10%	0.162***	0.462***	2.720***	2.954***	4.210***	4.346***
	(0.047)	(0.156)	(0.422)	(0.494)	(0.693)	(0.868)
Within next 50-90%	0.084*	0.162***	1.005***	0.989***	1.683***	1.669***
	(0.044)	(0.062)	(0.349)	(0.376)	(0.547)	(0.598)
Log(city expenditures)	-0.014	-0.351**	-0.194	-0.606*	-0.367	-0.621
	(0.039)	(0.167)	(0.212)	(0.307)	(0.392)	(0.694)
R^2	0.531	0.162	0.898	0.890	0.709	0.702
Observations	120	119	62	61	60	59

The infant mortality rate is calculated as the total number of deaths divided by the total population below age one. City expenditures include expenditures on health, sanitation, and charities. City size percentiles are calculated within the sample of 120 cities. Additional control variables include mean age, the fraction black, the fraction foreign-born, the fraction illiterate, the Duncan socioeconomic index, and 8 regional dummies. See Equation 6. Instrumental variables are the state's share of democrats in the US Senate, the state's share of democrats in the US House, the average number of years of service of the state's representatives in the US Senate, and the average number of years of service of the state's representatives in the US House. Washington DC is excluded from the IV regression. Robust standard errors clustered on the state are in parentheses. The symbols *, **, and *** indicate that the coefficient is significantly different from 0 at the 10, 5, and 1 percent level, respectively.

Table Eight: Effect of State Expenditures on State Mortality by Cause by Race, Ordinary Least Squares Regressions, 1910-1940

	White			Black		
	Coefficient on			Coefficient on		
Log(mortality rate)	Log(Expenditures	Time Trend	R^2	Log(Expenditures	Time Trend	R^2
All causes	0.023 (0.045)	-0.014*** (0.001)	0.490	0.130** (0.060)	-0.017*** (0.002)	0.326
Typhoid fever	-0.396* (0.212)	-0.088*** (0.006)	0.864	-0.416* (0.212)	-0.097*** (0.007)	0.757
Scarlet fever	0.220** (0.101)	-0.038*** (0.005)	0.692	0.047 (0.098)	-0.023*** (0.004)	0.479
Whooping cough	-0.172 (0.115)	-0.056 (0.004)	0.739	-0.097 (0.180)	-0.054*** (0.006)	0.487
Diphtheria	-0.462*** (0.169)	-0.092*** (0.007)	0.851	-0.293** (0.115)	-0.062*** (0.005)	0.598
Dysentery	-0.191 (0.174)	-0.044*** (0.006)	0.753	-0.263* (0.134)	-0.042*** (0.005)	0.780
Tuberculosis	0.256 (0.169)	-0.052*** (0.005)	0.805	0.144 (0.189)	-0.032*** (0.010)	0.491
Bronchitis	-0.045 (0.116)	-0.064*** (0.004)	0.816	0.389 (0.180)	-0.081*** (0.007)	0.703
Measles	0.065 (0.152)	-0.054*** (0.007)	0.541	0.169 (0.169)	-0.039*** (0.006)	0.255
Pneumonia	-0.855 (0.640)	-0.058*** (0.017)	0.340	-0.571 (0.368)	-0.049*** (0.007)	0.421
Diarrhea	-0.286 (0.188)	-0.086*** (0.006)	0.861	-0.086 (0.110)	-0.070*** (0.005)	0.736
Hernia	-0.015 (0.056)	-0.011*** (0.001)	0.610	0.371*** (0.056)	-0.008*** (0.002)	0.378

Ordinary least squares regressions are of state mortality rates by cause and by race on a time trend and on the logarithm of per capita expenditures on charities, hospitals, and corrections and recreation, health, and sanitation by state and local governments within a state. Each row reports two regressions, one in which the dependent variables is the logarithm of the mortality rate for whites and one in which the dependent variable is the logarithm of the mortality rate for blacks. These state mortality rates are for the years 1910, 1915, 1920, 1925, 1930, 1935, and 1940 for the death registration states. Per capita expenditures are for the year 1913. Additional control variables include the age distribution of the population, the Duncan socio-economic index, and four regional dummies. See equation 7. Robust standard errors, clustered on the state, in parentheses. The symbols *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. All regressions are weighted by state population.

Table Nine: Effect of State Expenditures on State Mortality by Cause by Race, Instrumental Variables Regressions, 1910-1940

	White			Black		
	Coefficient on			Coefficient on		
Log(mortality rate)	Log(Ex-penditures	Time Trend	R^2	Log(Ex-penditures	Time Trend	R^2
All causes	-0.049	-0.015***	0.483	0.207*	-0.016***	0.138
	(0.104)	(0.002)		(0.108)	(0.002)	
Typhoid fever	-0.941**	-0.098***	0.850	-0.424	-0.097***	0.757
	(0.009)	(0.009)		(0.351)	(0.008)	
Scarlet fever	0.095	-0.040***	0.692	0.070	-0.022***	0.478
	(0.226)	(0.005)		(0.118)	(0.004)	
Whooping cough	-0.154	-0.056***	0.739	-0.087	-0.054***	0.487
	(0.195)	(0.005)		(0.262)	(0.006)	
Diphtheria	-0.613**	-0.095***	0.850	-0.459***	-0.063***	0.591
	(0.252)	(0.006)		(0.177)	(0.005)	
Dysentery	-0.530	-0.051***	0.743	-0.596**	-0.044***	0.761
	(0.341)	(0.009)		(0.290)	(0.006)	
Tuberculosis	-0.114	-0.059***	0.786	0.053	-0.033***	0.488
	(0.292)	(0.007)		(0.263)	(0.010)	
Bronchitis	0.262	-0.059***	0.808	0.106	-0.083	0.431
	(0.241)	(0.005)		(0.326)	(0.006)	
Measles	0.039	-0.054***	0.541	0.197	-0.039***	0.593
	(0.172)	(0.007)		(0.302)	(0.007)	
Pneumonia	-1.316	-0.067***	0.588	-0.455	-0.048***	0.493
	(1.057)	(0.023)		(0.350)	(0.008)	
Diarrhea	-0.276	-0.086***	0.861	-0.289	-0.071***	0.727
	(0.352)	(0.009)		(0.201)	(0.005)	
Hernia	-0.024	-0.011***	0.610	0.381***	-0.008***	0.377
	(0.099)	(0.002)		(0.115)	(0.002)	

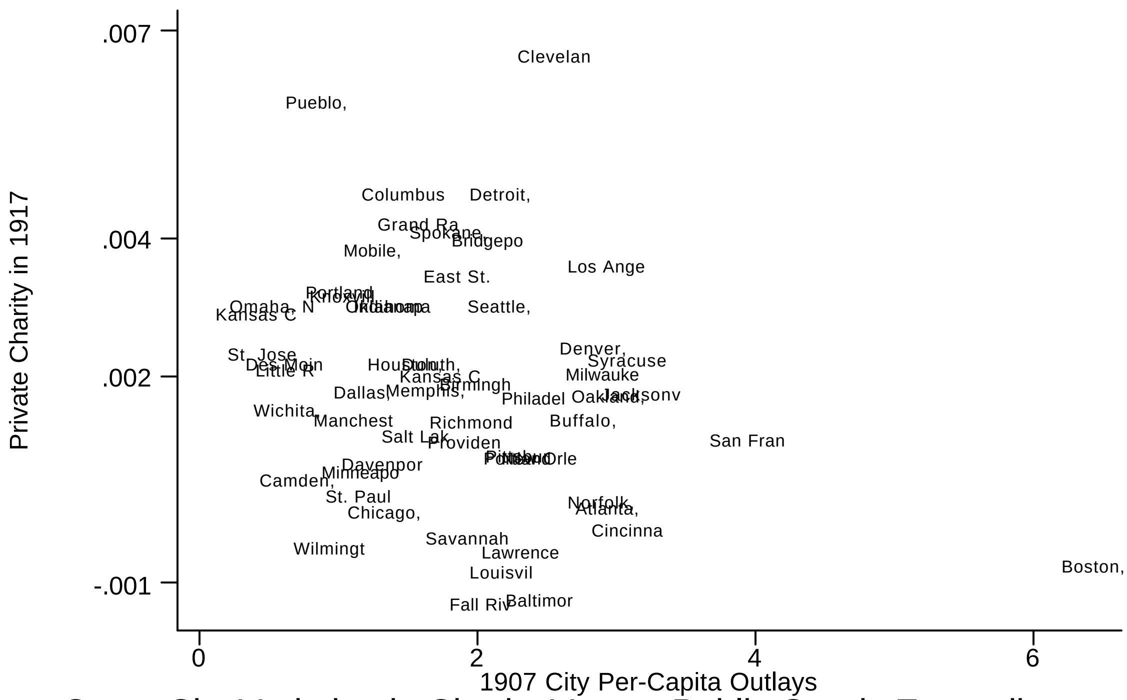
Instrumental variables regressions are of state mortality rates by cause and by race on year and on the logarithm of per capita expenditures on charities, hospitals, and corrections and recreation, health, and sanitation by state and local governments within a state. Each row reports two regressions, one in which the dependent variables is the logarithm of the mortality rate for whites and one in which the dependent variable is the logarithm of the mortality rate for blacks. These state mortality rates are for the years 1910, 1915, 1920, 1925, 1930, 1935, and 1940 for the death registration states. Per capita expenditures are for 1913. Additional control variables include the age distribution of the population, the Duncan socio-economic index, and four regional dummies. See Equation 7 in the text. Instrumental variables are the share of the state's democrats in the house, the share of the state's democrats in the senate, the average number of years of seniority of the state's representatives in the house, and the average number of years of seniority of the state's representatives in the senate. Robust standard errors, clustered on the state, in parentheses. The symbols *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. All regressions are weighted by state population.

Table Ten: Compensating Differential for Infant Mortality Risk

	Apartments		Non-apartments	
Log(city population in 1,000s)	0.048***	0.051***	0.015	0.017
	(0.018)	(0.016)	(0.020)	(0.021)
Log(city infant mortality)	-0.198	-0.227**	0.086	0.073
	(0.170)	(0.107)	(0.119)	(0.105)
With 4 region dummies?	Y	N	Y	N
Probability dummies are jointly significant, from F-test	0.860		0.456	
R^2	0.519	0.518	0.476	0.472
Observations	3,128	3,128	6,437	6,437
Number of cities	94	94	112	112

Estimated from the 1917-1919 Consumer Expenditure Survey. Regressions are of the logarithm of rental value (imputed by homeowners for owned properties) on the logarithm of city infant mortality controlling for city population. Infant mortality is 1919 mortality for the registration cities. Average yearly rent in July 1918 dollars was $190 in the apartment sample and $198 in the non-apartment sample. The mean city infant mortality rate was 0.123 in both samples. Additional control variables include the number of rooms, the number of windows, the number of windows squared, whether the dwelling had a bathroom, whether the dwelling had a WC inside, whether the dwelling had a sewer connection, whether the dwelling had a pantry, whether the dwelling had an attic, whether the dwelling had a cellar, and whether the dwelling contained stationary laundry tubs. See Equation 8 in the text. Robust standard errors in parentheses. The symbols *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively.

Figure One



Cross-City Variation in Charity Versus Public Goods Expenditure