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Reexamining the Contribution of Public Health Efforts to the Decline in Urban Mortality: Comment†

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We address points raised by Anderson, Charles, and Rees (2022b), which comments on our prior work. After correcting unambiguous data mistakes, our revised estimates suggest that municipal water disinfection (filtration) explains 38 percent of the total mortality rate decline in our sample cities and years—a result not very different from our original estimate of 43 percent. However, effects on infant mortality rates are smaller than in our original analysis. Much of the difference between their analyses and ours is due to the coding of partial intervention years and to differences in population denominators, for which ideal data are difficult to find. (*JEL* H75, I12, I18, J13, Q18, Q51, Q53)

In this comment, we address points raised by D. Mark Anderson, Kerwin Kofi Charles, and Daniel I. Rees's paper "Reexamining the Contribution of Public Health Efforts to the Decline in Urban Mortality" (hereafter, ACR). ACR is, in part, an examination of our 2005 *Demography* article "The Role of Public Health Improvements in Health Advances: The Twentieth-Century United States" (Cutler and Miller 2005a—hereafter, CM). During the summer of 2018, we shared data and code from CM with ACR. They replicated our original results. In conducting new analyses of both milk purification and water and sanitation technologies in American cities in the early twentieth century, they also identified differences between our original analysis and their new analysis. They communicated with us about these issues in a helpful and collegial manner. We very much appreciate their constructive feedback. In light of their results, we have evaluated the issues further. We report our findings here.

The issues raised by ACR generally fall into three categories: transcription errors, the assignment of differing dates to clean water interventions (including coding of partial intervention years), and the population denominators used in constructing mortality rates. Several of the transcription errors identified in ACR

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are in fact mistakes in the original paper, and we are grateful to have these identified.[1] Otherwise, a large share of the discrepancy in the estimates between CM and ACR is due to the coding of partial intervention years and to the construction of population denominators for mortality rates when such denominators are not known for certain. This comment focuses on the role of differing clean water intervention dates (using corrected CM total mortality and infant mortality rates), and a longer comment available online discusses all three issues in greater detail.

I. Assignment of Clean Water Intervention Dates

ACR identify a number of differences in clean water intervention dates between their analysis and those used by CM. Reviewing the data more closely, differences in dates generally appear to be the result of two factors: differences in dates reported in various historical sources and differences in coding when an intervention was introduced in a phased manner over multiple years. All told, out of 13 total cities, ACR use water filtration dates for 4 cities and water chlorination dates for 7 cities that are different from the CM dates.[2]

Different historical sources give different dates for clean water interventions. When writing the original paper, we addressed this inconsistency by making phone calls to individual waterworks to verify intervention dates through their own records. Reaching some confidence in intervention dates through discussions with waterworks employees, in part, motivated our choice of dates (and cities) to incorporate. ACR use historical articles providing intervention dates. Table 1, columns 1 and 2 show the effect on our total mortality results of using the alternative dates put forward by ACR. The filtration coefficient falls from -0.13 (p-value $= 0.027$) in the CM analysis to -0.09 (p-value $= 0.036$) using ACR intervention dates.[3]

Philadelphia provides an illustrative example of the differences due to coding. Philadelphia adopted filtration technology incrementally between 1902 and 1909: filtration systems were installed in Lower Roxborough in 1902, in Kittanning in 1905, and in Lancaster in 1906. However, the largest facility, Torresdale, which provided the majority of Philadelphia's drinking water (and was the largest facility in the world at the time), was not completed until 1909. ACR use 1906 as the date of filtration, while CM use 1908. Upon reconsideration of Philadelphia's history, we would actually be inclined to think that 1909 is the most appropriate date to use in this case. We do not see a case for 1906 being the best choice of year.[4]

[1] The dataset is available online and includes updates to correct these errors (Cutler and Miller 2005b). In this comment, we use the corrected CM total mortality rate and infant mortality rate estimates as the basis for comparison rather than those originally reported in CM (2005).

[2] When allowing for fractional coding for years in which interventions were partially available, CM and ACR clean water technology variables differ in value for all 13 cities.

[3] Results obtained using ACR intervention dates throughout this comment differ slightly from those reported in Tables 14 and 15 of an earlier working paper version (see ACR 2020) because we recode lagged intervention variables to correspond to the ACR intervention dates.

[4] When adjusting Philadelphia's water filtration date to 1909, point estimates for total mortality increase to -0.14 (p-value $= 0.027$) using CM (2005) dates for all other cities, -0.11 (p-value $= 0.032$) using ACR (2020) dates for all other cities, and -0.16 (p-value $= 0.006$) when using ACR (2020) dates recoded as indicators. When adjusting Philadelphia's date to 1909, point estimates for infant mortality are to -0.13 (p-value $= 0.05$) using CM

Table 1—Sensitivity of Total and Infant Mortality Rate Estimates to Alternative Intervention Dates

Mortality rate source	Total mortality, CM (corrected)			Infant mortality rate, corrected		
Intervention date source	CM (1)	ACR fractional coding (2)	ACR as indicators (3)	CM (4)	ACR fractional coding (5)	ACR as indicators (6)
Filtration	−0.13 (0.053)	−0.09 (0.040)	−0.15 (0.050)	−0.13 (0.059)	−0.05 (0.055)	−0.15 (0.039)
Chlorination	−0.01 (0.024)	−0.02 (0.025)	−0.01 (0.026)	0.02 (0.043)	0.03 (0.039)	−0.03 (0.039)
Filtration × chlorination	0.03 (0.026)	0.03 (0.027)	0.02 (0.025)	0.07 (0.048)	−0.00 (0.043)	−0.01 (0.041)
Filtration within 5 years	−0.07 (0.048)	−0.04 (0.032)	−0.08 (0.043)	−0.03 (0.028)	0.01 (0.026)	−0.04 (0.023)
Chlorination within 5 years	0.01 (0.014)	−0.01 (0.014)	−0.01 (0.018)	0.03 (0.028)	−0.05 (0.028)	−0.07 (0.034)
Observations	410	410	410	410	410	410
R^2	0.963	0.962	0.964	0.977	0.978	0.978
F-test	2.883	1.965	3.151	2.827	0.589	6.457
$\Pr > F$	0.0798	0.173	0.0647	0.0835	0.634	0.00753

Notes: Table shows sensitivity to the use of alternative water filtration dates in equation (1) for total mortality and infant mortality rates. Total mortality rates are contemporaneously reported as described in Cutler and Miller (2005), with data entry errors corrected. Infant mortality rates used are corrected as described in Anderson, Charles, and Rees (2020). Columns 1–3 show results for total mortality, with column 1 showing results using intervention dates described in CM (2005), column 2 fixing all dates at those shown in ACR (2020), and column 3 showing results using dates in ACR (2020) recoded as indicator variables. Columns 4–6 show results for infant mortality rates, with column 4 showing results fixing all intervention dates to those described in CM (2005), column 5 showing results fixing all dates to those proposed in ACR (2020) as originally coded, and column 6 showing results fixing all intervention dates to those proposed in ACR (2020), recoded as indicators. All specifications include sewage treatment dummy variables, lagged mortality, year and city dummy variables, city trends, and demographic characteristics (population share by gender, race, birthplace, and age). Standard errors are clustered at the city level.

Finally, CM code water disinfection and sanitation interventions using indicator variables (taking a value of 1 if an intervention was active at any point during a given year), while ACR use, in some instances, a partial/fractional intervention coding for a subset of intervention years (in monthly increments, or twelfths).[5] Though seemingly minor, this coding difference matters for the results. Table 1, column 3 shows that simply recoding partial years of an intervention to full years (but otherwise using ACR dates) increases the magnitude of the ACR filtration estimate from −0.09 (p-value = 0.036) to −0.15 (p-value = 0.010), a value greater in magnitude than our original estimate. In principle, the idea of using partial-intervention-year coding is a good one. However, the ideal coding should be based on the share of clean water provided to people weighted by how likely they were to die of waterborne disease. Without this information, we hesitate to use partial-year data.

(2005) dates for all other cities, −0.05 (p-value = 0.475) using ACR (2020) dates for all other cities, and −0.14 (p-value = 0.012) when using ACR (2020) dates recoded as indicators.

[5] The first year of water filtration interventions is coded as partial/fractional years for eight cities. The first year of water chlorination is coded as partial/fractional years for four cities. Finally, three cities have partial/fractional coding for more than one intervention year (two cities with two years each of fractional coding and one city with three years; in two cases, these values decrease over time before taking a value of 1).

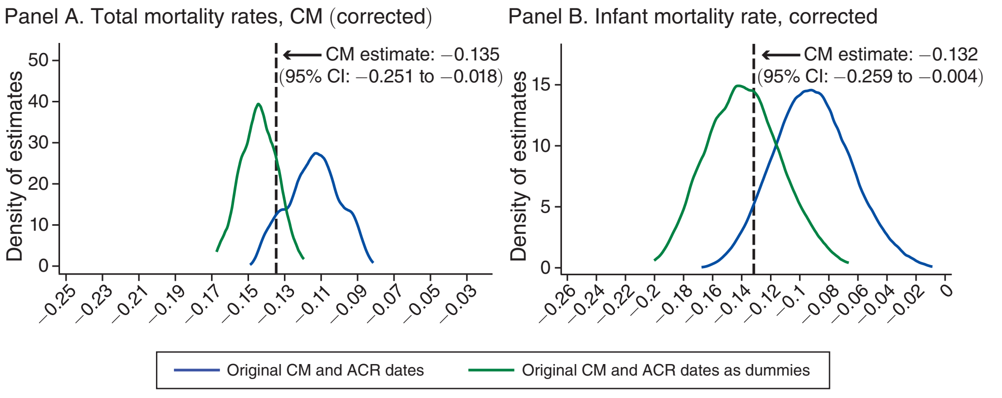

Figure 1. Total Mortality and Possible Intervention Date Combinations

Notes: Figure 1 assesses the sensitivity of the CM (2005) total mortality rate and infant mortality rate results to alternative intervention dates. Total mortality rates are contemporaneously reported, as described in CM (2005), with data entry errors corrected. Infant mortality rates used are corrected as described in ACR (2020). We reestimate equation (1) using all possible unique pair-wise combinations of city-level intervention dates used in CM (2005) and ACR (2020). When allowing for fractional year intervention coding, CM and ACR intervention variables differ in at least some years for all 13 cities, implying a total of $2^{13} = 8{,}192$ possible unique pair-wise combinations of city-level intervention variables. When recoding ACR dates to indicators (not allowing for partial years), intervention variables using CM and ACR differ for only nine cities, leading to a total of $2^9 = 512$ possible unique combinations of city-level dates. Panel A shows the results of this analysis for the total mortality rate (using the corrected CM (2005) approach to population denominators); dashed line indicates point estimates and 95 percent confidence intervals produced using CM intervention dates. Panel B shows the results for infant mortality rate using ACR (2020) rates, which correct data errors in CM (2005); dashed line shows the point estimates and 95 percent confidence intervals produced using CM intervention dates. Panels show the distribution of each set of resulting point estimates. All specifications include sewage treatment dummy variables, lagged mortality, year and city dummy variables, city trends, and demographic characteristics (population share by gender, race, birthplace, and age).

Ultimately, because some degree of judgment is required, we take an empirical approach to assessing the sensitivity of the CM and ACR results to intervention dates. Specifically, we reestimate our original specification using all 8,192 possible combinations of CM and ACR intervention dates.[6] We also repeat this exercise changing ACR's coding of fractional intervention years to indicator variables for all cities. Focusing first on total mortality rates, Figure 1, panel A shows that no combination of dates using either partial-intervention-year coding or exclusive indicator variable coding produces an estimate outside the confidence interval using our original dates.

To further explore sensitivity, we also adopt a "leave-one-out" strategy, starting with CM dates, ACR dates, and ACR dates recoded as indicators, and we omit one city at a time from the analysis. Figure 2, panel A shows that the results are generally robust to excluding each city and that no estimates are outside of the confidence intervals of the original CM or ACR estimates, respectively.

[6] When allowing for partial intervention years, CM and ACR intervention variables differ in at least some years for all 13 cities, implying a total of $2^{13} = 8{,}192$ possible unique combinations of city-level intervention variables. When recoding ACR dates to indicators (not allowing for partial years), intervention variables using CM and ACR differ for only 9 cities, leading to a total of $2^9 = 512$ possible unique combinations of city-level dates.

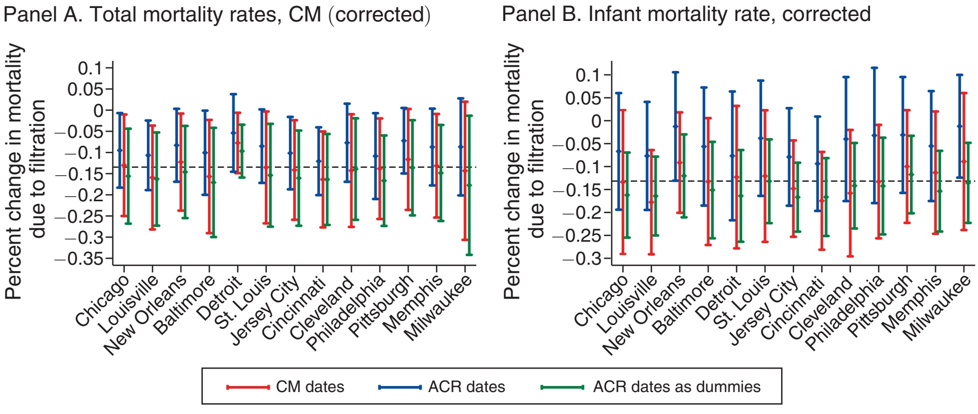

Figure 2. Leave-One-Out Analysis

Notes: Figure 2 assesses the sensitivity of the CM (2005) results for total mortality rate and infant mortality rate to alternative intervention dates for each city using a "leave-one-out" approach. Using corrected CM (2005) total mortality rates (panel A) and corrected infant mortality rates as reported in ACR (2020) (panel B), we estimate equation (1) starting with each alternative set of intervention dates (CM dates, ACR dates, and ACR dates recoded as indicators), and omitting one city at a time from the analysis. Panels A and B show the resulting point estimates and corresponding 95 percent confidence intervals. All specifications include sewage treatment dummy variables, lagged mortality, year and city dummy variables, city trends, and demographic characteristics (population share by gender, race, birthplace, and age). Standard errors are clustered at the city level.

Unlike total mortality rates, estimates for infant mortality rates appear more sensitive to the choice of dates and, in particular, sensitive to the coding of partial intervention years. Table 1, columns 4–6 show this using dates and coding choices from both CM and ACR, yielding -0.13 (p-value $= 0.03$) using CM dates, -0.05 (p-value $= 0.45$) using ACR dates, and -0.15 (p-value $= 0.003$) using ACR dates but with partial intervention years recoded as indicators.

Analogous to our sensitivity analysis for total mortality rates, Figure 1, panel B shows distributions of estimates for infant mortality rates using all possible combinations of CM and ACR dates (both as originally coded and recoding ACR partial intervention year variables as indicators). No combination of dates falls outside the confidence interval using our original dates, including when recoding partial intervention years to indicator variables.

Finally, Figure 2, panel B shows the leave-one-out analysis, leaving each city out of the analysis, one at a time, using each set of intervention dates. As with total mortality rate estimates, the results are generally robust to excluding each city, and no estimates are outside of the confidence intervals of full-sample estimates using CM rates or ACR rates, respectively.[7]

[7] This is true for the revised CM infant mortality rate estimates (after correcting the unambiguous errors described in Section III), but not for the original CM (2005) infant mortality rate estimates.

II. The Role of Population Denominators

Although CM and ACR collect mortality counts and rates from the same sources (US Census Bureau 1909–1940), ACR identify slight differences in total mortality rates for years 1901–1909 and more substantial differences for years 1910–1917. (There are no differences for years after 1917.)[8] These differences are due to differences in methods for estimating population denominators. CM use mortality rate information as published contemporaneously by the US Census Bureau. For years prior to 1917, vital registration systems reported estimated mortality rates, dividing death counts reported by localities by population denominator estimates. Such denominators are only known with near certainty in population census years. ACR recalculate mortality rates for intervening years using mortality counts and population denominators interpolated between census years. For a full discussion of these differences, see the longer version of this comment posted online.

While these differences in population denominators seem arcane, they have a considerable impact on the results. Columns 1 and 2 of Table 2 show that changing the method of calculating population denominators cuts the estimated effect of water filtration on total mortality rates from −0.13 (p-value 0.03) to −0.08 (p-value 0.014). However, the 95 percent confidence interval around the CM estimates in column 1 includes the estimate with different population denominators, and the confidence interval around the estimate with different population denominators in column 2 includes the CM (2005) estimate as well. Online Appendix Figure 1 considers this issue further, reproducing Figure 1, panel A using ACR population denominators. Doing so shifts the resulting distributions to the right of those in Figure 1, panel A.

The "gold-standard" approach to constructing population denominators would be to build city-specific life tables for generating intercensal population projections (Wunsch, Mouchart, and Duchêne 2002). However, the data required to do so is not generally available.[9]

III. Conclusion

We are grateful to ACR for the careful reanalysis of our earlier paper and deeply appreciate both the constructive nature of our exchanges with them and their identification of several mistakes in our original paper. Many of the other discrepancies identified, including those that substantively and quantitatively matter most for the results, are ones which we believe require judgment. We have done our best to evaluate these issues, especially the coding of city intervention dates and the construction of population denominators.

[8] Cause-specific mortality rates do not differ between the two sources.

[9] Building these life tables would require data (or estimates) on four types of population flows: births, deaths, immigration, and emigration. With annual measures of each, the process would be a relatively straightforward population accounting exercise. Annual measures of births and deaths are generally available, but to the best of our knowledge, annual information on immigration and emigration are not. Nonetheless, methods for estimating immigration and emigration may be possible (and cohort sizes in intercensal years could be adjusted accordingly).

Table 2—Sensitivity of Total Mortality Rate Estimates to Choice of Population Denominator

All cause mortality rate source	CM (corrected)	ACR
Intervention date source	CM (1)	CM (2)
Filtration	−0.13	−0.08
	(0.053)	(0.028)
Chlorination	−0.01	−0.04
	(0.024)	(0.026)
Filtration × chlorination	0.03	0.05
	(0.026)	(0.024)
Filtration within 5 years	−0.07	−0.02
	(0.048)	(0.013)
Chlorination within 5 years	0.01	0.02
	(0.014)	(0.011)
Observations	410	410
R^2	0.963	0.970
F-test	2.883	2.843
Pr > F	0.0798	0.0824

Notes: Table shows the results of alternate approaches to population denominators used to calculate total mortality rates, fixing intervention dates at those used in CM (2005). Column 1 shows results using contemporaneously reported mortality rates from CM, with data entry errors corrected, and column 2 shows results using total mortality rates proposed in ACR (2020). All specifications include sewage treatment dummy variables, lagged mortality, year and city dummy variables, city trends, and demographic characteristics (population share by gender, race, birthplace, and age). Standard errors are clustered at the city level.

Overall, correcting the unambiguous mistakes in our earlier paper yields the finding that municipal water disinfection explains 38 percent of the total mortality rate decline in our sample cities and study years—a result not materially different from the 43 percent estimated in the original paper. However, effects on infant mortality rates appear more sensitive to these adjustments and markedly smaller than in our original analysis—although we believe the evidence still supports significant, and quantitatively meaningful, effects of clean water on infant mortality as well. Otherwise, a large share of the discrepancy between our analysis and ACR's is, somewhat surprisingly, due to the coding of partial intervention years and to the construction of population denominators for mortality rates. The population is not known for certain, and city populations were changing rapidly in this time period.

More generally, based on the findings of other papers studying municipal water and sanitation interventions in similar historical contexts—some of which were inspired by our paper—we believe that these technologies have been substantively important for historical urban mortality decline (Alsan and Goldin 2018; Anderson et al. 2019; Anderson, Charles, and Rees 2022b; Cain and Rotella 2001; Ferrie and Troesken 2008; Ketzenbaum and Rosenthal 2014; Knutsson 2018; Knutsson 2020; Ogasawara, Shirota, and Kobayashi 2015) and appear to be an important determinant of health in contemporary lower-income cities as well (Ashraf et al. 2017; Bhalotra et al. 2021; Galiani, Gertler, and Schargrodsky 2005).

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